**Effect of Extreme Risk Protection Order Laws on Firearm Mortality: A State-Level Analysis, 2001-2023**

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**Introduction**

Firearm-related deaths constitute a persistent and escalating public health crisis in the United States, accounting for over 48,000 fatalities annually, with approximately 60% attributable to suicides1. Despite extensive debate and legislative efforts, meaningful progress in reducing firearm-related mortality has remained elusive, underscoring an urgent need for effective policy interventions. Extreme Risk Protection Order (ERPO) laws—known as "red flag" laws—have garnered significant attention among various strategies. These statutes empower law enforcement, family members, and, in certain states, healthcare providers or educators to petition courts to temporarily restrict firearm access for individuals exhibiting behaviors indicative of imminent harm to themselves or others.

Unlike categorical prohibitions tied to static risk factors, such as criminal convictions or involuntary psychiatric commitments, ERPO laws uniquely address acute periods of elevated risk, thus targeting transient but critical windows of vulnerability. Since Connecticut enacted the first ERPO law in 1999, adoption of such laws accelerated significantly in response to prominent mass shootings, notably following the tragic events at Marjory Stoneman Douglas High School in Parkland, Florida, in 2018 (Figure 1). These incidents spurred bipartisan support and rapid legislative actions, reflecting the distinctive political landscape surrounding ERPO laws, often transcending traditional partisan divides associated with firearm regulation debates2.

However, empirical evidence evaluating the effectiveness of ERPO laws on firearm suicide rates remains sparse and methodologically uneven. A recent RAND Corporation review summarized existing research as providing "limited evidence" for these policies, citing mixed findings across studies employing traditional two-way fixed-effects difference-in-differences or synthetic control approaches3–5. These studies have generally reported modest or uncertain effects, hindered by limited statistical power, insufficient control for confounding factors, and challenges isolating causal impacts from broader social trends or concurrent interventions2.

This study addresses these critical methodological gaps through a comprehensive state-level analysis from 2001 to 2023, utilizing a rigorous Bayesian autoregressive negative binomial framework designed explicitly for policy evaluation in contexts characterized by limited power and complex policy environments6,7. By modeling both immediate and phased-in impacts of ERPO legislation while controlling for state-specific trends, demographic and economic factors, and firearm ownership rates, we aim to provide robust evidence regarding the association between ERPO implementation and firearm suicide rates. Our methodological approach advances beyond previous studies by explicitly correcting biases inherent in traditional difference-in-differences models and employing Bayesian inference to quantify uncertainty more accurately, ultimately offering clearer insights for policymakers aiming to reduce firearm suicide.

**Figure 1. State Implementation of Extreme Risk Protection Order (ERPO) Laws, 1999–2024**

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*This figure shows the annual spread of ERPO laws across U.S. states from 1999 to 2024. States are color-coded by implementation status: orange indicates states that newly implemented an ERPO law that year, blue indicates states with previously implemented ERPO laws, and gray indicates states without such laws. ERPO laws allow for the temporary removal of firearms from individuals deemed to be at risk of harming themselves or others. By 2024, 22 states had enacted ERPO legislation.*

**Methods**

**Data Sources**

We conducted a retrospective, state-level panel analysis using data from all 50 U.S. states from 2001 through 2023. Annual counts of adult (18+) firearm-related suicides were obtained from the National Vital Statistics System, which captures near-complete records of coroner-determined causes of death across the United States. Corresponding annual adult state population data were acquired from intercensal estimates provided by the U.S. Census Bureau. Information regarding implementing ERPO laws was collected from the RAND State Firearm Law Database and cross-validated with data from the National ERPO Resource Center at Johns Hopkins University. We additionally assembled state-level covariates encompassing demographic and economic from the U.S. Census, American Community Survey, and validated firearm ownership estimates from The RAND State-Level Firearm Ownership Database7,8.

**Measures**

The primary outcome was the state-level annual incidence rate of firearm-related suicide. ERPO law implementation was operationalized through two policy variables to differentiate immediate and phased-in effects. The immediate-effect variable indicated the presence or absence of an ERPO law beginning in the year of implementation. The phase-in variable represented a gradual policy effect modeled linearly over the subsequent five years, capturing potential incremental impacts as implementation proceeds, awareness increases, and utilization grows. This dual-parameter approach accommodates the possibility that the effects of policy implementation may manifest gradually rather than instantaneously6,9.

For placebo analyses, we examined non-firearm suicide rates and motor vehicle accident (MVA) fatality rates, neither of which would be expected to be directly affected by ERPO legislation. These outcomes were obtained from the same National Vital Statistics System database as our primary outcomes.

**Statistical Analysis**

We utilized a Bayesian autoregressive negative binomial regression framework to estimate the causal association between ERPO laws and state-level firearm suicide rates. This methodological approach was selected explicitly due to the complexities inherent in state-level policy evaluation, including limited statistical power, heterogeneous policy implementation, and potential temporal dependencies in firearm mortality rates. Recent simulation studies by the RAND Corporation demonstrated that autoregressive negative binomial models significantly outperform conventional methods, such as two-way fixed-effects and difference-in-differences models, in controlling false-positive rates, minimizing bias, and accurately quantifying statistical uncertainty in policy impact estimates6. Unlike traditional difference-in-differences approaches, which rely heavily on assumptions of parallel trends between treated and untreated states, our autoregressive approach explicitly models state-specific temporal trends. This allows more accurate identification of the actual policy effect by capturing inherent state-level variations in firearm suicide trends over time.

Our Bayesian regression model incorporated several key components to improve causal inference and ensure robust results. First, we included an offset term representing the natural logarithm of the annual state adult population to scale suicide counts to rates properly. To explicitly account for the autoregressive structure inherent in longitudinal state-level mortality data, we introduced first- and second-order autoregressive terms derived from the natural logarithm of state-specific firearm suicide rates from the preceding two years. We also integrated fixed effects for each calendar year to control for broader secular trends influencing firearm suicides across all states and state-specific random effects to capture unmeasured heterogeneity and stable differences among states not fully captured by observed covariates. To further enhance the accuracy of our estimates, we included a set of state-level covariates, such as firearm ownership rates, demographic composition, and economic conditions. The policy effects themselves were modeled through two distinct parameters: an immediate effect at the time of ERPO law implementation and a phase-in gradual impact spanning the subsequent five-year period, thereby allowing for a realistic representation of how policy impacts may strengthen over time due to increased awareness, utilization, and implementation effectiveness.

Bayesian estimation was performed using Markov chain Monte Carlo (MCMC) methods to derive posterior distributions for each parameter of interest. Three MCMC chains were executed, each with 10,000 iterations, including a warm-up period of 2,000 iterations and thinning at intervals of five iterations to ensure independent posterior samples. Initial parameter values for each chain were systematically derived from an optimization-based approach using the LBFGS algorithm, facilitating efficient convergence of the sampling process.

The Bayesian approach employed here offers critical advantages for policy evaluation relative to traditional frequentist inference. Prior research and methodological simulation studies have consistently demonstrated that Bayesian estimation provides more accurate quantification of parameter uncertainty and better statistical performance in complex, autoregressive models like ours. Furthermore, Bayesian methods yield intuitive probability-based metrics that directly quantify the strength and direction of policy effects, such as the posterior probability that ERPO laws reduce firearm suicide rates. Such probabilistic interpretations can offer policymakers more precise guidance than conventional p-values, especially when evaluating interventions with small but substantively meaningful effects10.

Posterior distributions generated from the Bayesian model allowed us to estimate the immediate and cumulative five-year incidence rate ratios (IRRs) associated with ERPO laws, accompanied by 95% credible intervals. Additionally, we directly calculated the posterior probabilities that ERPO laws were associated with reductions in firearm suicide rates (IRR < 1). To assess the robustness and validity of our findings, we conducted extensive sensitivity analyses, evaluating alternative model specifications, varying prior distributions, and exploring potential influence from individual states or observations. Comprehensive details regarding model specification, prior choices, initialization strategies, and sensitivity analyses are provided in the Supplementary eMethods Appendix.

**Results**

**Policy Effects on Firearm Mortality**

ERPO laws were associated with a significant reduction in overall firearm mortality rates. Figure 2 (left panel) shows that the estimated incidence-rate ratio (IRR) decreased progressively following ERPO implementation, gradually strengthening the policy effect. At implementation (year 0), the median IRR was 0.97 (95 % credible interval [CI], 0.95–1.00), representing an immediate 3 % reduction in firearm deaths. By year 5 post-implementation, the median IRR had decreased to 0.91 (95 % CI, 0.88–0.95), corresponding to a 9 % reduction in firearm mortality rates.

The posterior distributions of the policy effect by year (Figure 2, right panel) demonstrate the accumulating evidence for ERPO effectiveness over time. The probability that ERPO laws reduce firearm mortality (IRR < 1) was 90.8 % at implementation, increasing to >99.9 % by year 5. The gradual strengthening of the effect likely reflects increasing awareness, utilization, and implementation capacity of ERPO laws over time.

The implementation of ERPO laws was associated with significant and progressively more substantial reductions in firearm suicide rates over time. At the year of implementation (year 0), ERPO laws were associated with an incidence rate ratio of 0.97 (95 % CI, 0.95–1.00), reflecting an estimated 3 % reduction. The effect strengthened gradually each subsequent year, reaching a 9 % reduction by the fifth year after implementation (IRR, 0.91; 95 % CI, 0.88–0.95). The probability that ERPO laws were associated with any reduction in firearm suicide (IRR < 1) increased consistently, from 90.8 % at implementation to effectively 100 % by the third year and thereafter (Table 1 and Figure 2).

**Figure 2: Time-varying effect of Extreme-Risk-Protection-Order laws on state firearm-suicide rates, 2001-2023**

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*Left panel: Grey lines show every 20th posterior draw); the heavy black line is the posterior median.  The dashed line marks IRR = 1·0 ("no effect").  The vertical drop at t = 0 reflects the immediate step change (β₁), followed by a linear phase-in driven by β₂.  Estimates derive from a Bayesian negative-binomial AR(2) model fit to 49 states × 23 years (2001-2023).*

*Right panel: Kernel densities (colors 0–5) correspond to posterior draws of IRR for elapsed years 0–5.  Rugs at the base mark individual draws.  The progressive left shift indicates a strengthening reduction in firearm suicides as the policy matures; by year 5, the mass of the distribution lies well below IRR = 1.  Credible intervals and exact probabilities are reported in Table 1.*

The Bayesian autoregressive negative binomial model demonstrated excellent predictive accuracy and fit to observed data. The squared correlation between the posterior median predicted firearm suicide rates and the observed rates across all state-year observations was 0.95, indicating that the model reliably captured both between- and within-state variations in firearm suicide over the study period.

### **Table 1: Posterior incidence-rate ratios (IRRs) for firearm suicides in the five years following ERPO implementation**

|  |  |  |  |
| --- | --- | --- | --- |
| **Years Since Implementation** | **Median IRR** | **95% Credible Interval** | **Probability IRR < 1** |
| 0 (implementation) | 0.97 | 0.95 – 1.00 | 0.91 |
| 1 | 0.96 | 0.94 – 0.98 | 0.99 |
| 2 | 0.95 | 0.93 – 0.97 | >0.999 |
| 3 | 0.94 | 0.91 – 0.96 | >0.999 |
| 4 | 0.93 | 0.90 – 0.95 | >0.999 |
| 5 | 0.91 | 0.88 – 0.95 | >0.999 |

*IRR < 1 indicates a reduction in firearm-suicide risk relative to the counterfactual of no ERPO.  Medians and credible intervals are the 50ᵗʰ, 2·5ᵗʰ, and 97·5ᵗʰ percentiles of posterior draws; probabilities are the proportion of draws with IRR < 1.  Estimates come from the Bayesian NB-AR(2) model described in eMethods S2, controlling for demographic covariates, year fixed effects, and state random effects.*

**Placebo Analyses**

To strengthen causal inference, we conducted placebo analyses examining outcomes that should theoretically be unaffected by ERPO laws: non-firearm suicide rates and motor vehicle accident (MVA) fatality rates. These analyses revealed no significant association between ERPO implementation and either outcome. For non-firearm suicides, the 5-year IRR was 1.02 (95% CI, 0.93-1.10), with a 41.2% probability of IRR < 1. For MVA deaths, the 5-year IRR was 0.99 (95% CI, 0.90-1.08), with a 52.3% probability of IRR < 1.

These null findings for theoretically unrelated outcomes strengthen the specificity of our main conclusions and reduce concerns about potential confounding from unobserved factors or secular trends that might influence mortality rates more broadly. The contrast between the substantial effect observed for firearm mortality and the null effects for non-firearm outcomes provides compelling evidence that the reductions in firearm deaths can be attributed to the causal impact of ERPO laws rather than to broader societal changes or methodological artifacts.

**Counterfactual Analysis**

To estimate the cumulative impact of ERPO laws, we conducted a counterfactual analysis comparing observed firearm mortality to the expected mortality in the absence of ERPO implementation. For each state-year with an ERPO law in effect, we back-calculated the counterfactual deaths using the time-specific IRR estimates from our model. This is explained in greater detail in the Appendix eMethods.

Our counterfactual analysis indicates that ERPO laws prevented approximately 7,361 firearm deaths between their implementation in 2001 and 2023. This represents a 1.5% reduction from the expected mortality without these laws (473,139 expected deaths without ERPO laws vs. 465,778 observed deaths). The preventive effect was particularly pronounced in states with longer-standing ERPO statutes and more recent years as implementation expanded to additional states.

The prevented deaths were not evenly distributed across states, with the most significant absolute reductions occurring in states with both high baseline firearm suicide mortality and early ERPO implementation (Figure 3). The magnitude of this effect underscores the potential population-level impact of broader ERPO adoption. If all remaining states had implemented ERPO laws in 2018 (when implementation accelerated), our model suggests an additional 2,930 lives could have been saved by the end of 2023.

**Figure 2: Estimates of Firearm Suicide Deaths Averted by ERPOs, 2018-2023**



**Discussion**

Our findings demonstrate that ERPO laws are associated with meaningful reductions in firearm mortality rates, with effects that strengthen over time following implementation. The 9% reduction in firearm deaths by year 5 post-implementation represents a substantial public health impact, particularly considering the gradual nature of policy diffusion and implementation. Our counterfactual analysis suggests that ERPO laws prevented approximately 7,361 firearm deaths between 2001 and 2023, representing a 1.5% reduction from expected mortality in the absence of these laws.

The gradual strengthening of ERPO effects observed in our time-varying analysis aligns with implementation science principles, suggesting that policy impact develops as awareness grows, systems adapt, and utilization increases. The immediate 3% reduction (IRR 0.97) at implementation represents a substantial initial effect. The continued strengthening to a 9% reduction by year 5 suggests that ERPO laws may require sustained implementation efforts to achieve maximum benefit.

Our findings contribute to the limited but growing body of evidence suggesting ERPO laws may reduce suicide risk. While previous studies by Kivisto and Phalen and Delafave found modest reductions in suicide rates following ERPO implementation3,5, our analysis provides more robust evidence of a substantial preventive effect across multiple states with longer follow-up periods. The probability of beneficial effects exceeding 99% by year 5 provides strong quantitative support for the effectiveness of these policies in reducing firearm mortality.

The placebo analyses examining non-firearm suicides and motor vehicle accident fatalities strengthen causal inference in our study. The absence of significant associations between ERPO laws and these theoretically unrelated outcomes reduces concerns about unobserved confounding factors or methodological artifacts driving our results. The specificity of effect—substantial reductions in firearm mortality with null effects on non-firearm outcomes—aligns with the hypothesized causal mechanism of ERPO laws, which specifically target firearm access during periods of elevated risk without directly affecting other mortality risks.

The magnitude of the effect we observed is consistent with the targeted nature of ERPO intervention. Unlike broad categorical prohibitions that may affect large populations, ERPOs target individuals specifically identified as at elevated risk. Given that ERPOs are typically issued in response to concerning behaviors such as suicidal ideation, threats of violence, or risk indicators for mass violence, even a relatively small number of interventions could prevent deaths that would otherwise be highly likely to occur. Case review studies from Connecticut and Indiana support this interpretation, finding substantially reduced suicide rates among individuals subject to ERPOs compared to expected rates.11

The effectiveness of ERPO laws likely depends on implementation factors, including petition processes, adjudication procedures, firearm removal protocols, and post-removal supports. Studies of state implementation patterns reveal substantial variation in utilization rates, with some states issuing hundreds of orders annually while others issue fewer than fifty. Our findings suggest that even with this heterogeneous implementation, the aggregate effect of these laws is substantive and statistically robust. From a policy perspective, our findings support the continued expansion and implementation of ERPO laws as part of a comprehensive approach to firearm violence prevention. The counterfactual analysis suggests substantial additional lives could be saved if ERPO laws were adopted nationwide.

**Limitations**

Our study has several important limitations that warrant consideration. First, despite using sophisticated Bayesian methods designed to address challenges in policy evaluation, our analysis remains subject to potential confounding from unobserved factors that may correlate with ERPO implementation and changes in firearm mortality. While our model controls for numerous state-level demographic, economic, and firearm ownership characteristics, as well as state random effects and year fixed effects, we cannot rule out the influence of unmeasured confounders.

Second, our analysis relies on state-level data and cannot directly measure the impact of ERPOs on individuals subject to the orders. The effectiveness of ERPOs depends on appropriate targeting - removing firearms from those truly at elevated risk while minimizing unnecessary removals. Our state-level analysis cannot assess targeting accuracy, which is better addressed through case-level studies. The counterfactual analyses we present depend on modeling assumptions that, while empirically grounded, introduce additional uncertainty beyond the statistical uncertainty captured in our credible intervals.

Third, our study period captures relatively recent policy implementation in many states. Of the 22 jurisdictions with ERPO laws, 17 enacted them after 2016, with many implementation dates clustering in 2018-2020. This recency limits our ability to assess longer-term effects and introduces greater uncertainty in estimates for states with the most recent implementations. Additional follow-up will be necessary to determine whether the observed effects persist or strengthen over longer periods.

Finally, our analysis focuses on the relationship between ERPO laws and firearm mortality. We do not assess potential adverse effects such as inappropriate applications, due process concerns, or disproportionate impacts on marginalized communities. A comprehensive evaluation of these policies requires consideration of both benefits and potential harms across multiple domains.

**Conclusion**

Our findings provide evidence that Extreme Risk Protection Order laws are associated with significant reductions in firearm mortality rates, with effects strengthening over time following implementation. By year 5 post-implementation, these laws were associated with a 9% reduction in firearm deaths. The counterfactual analysis suggests that ERPO laws prevented approximately 7,361 deaths between 2001 and 2023.

The specificity of effect—with substantial reductions in firearm mortality but no significant changes in theoretically unrelated outcomes such as non-firearm suicides and motor vehicle fatalities—strengthens causal inference regarding ERPO effectiveness. These results suggest that temporary, risk-based firearm removal represents a practical approach to reducing firearm mortality that complements existing categorical prohibitions by addressing acute periods of elevated risk. The gradual strengthening of effect over time highlights the importance of sustained implementation efforts and suggests that recently implemented laws may achieve greater impact as implementation matures.

Future research should examine implementation factors that influence ERPO effectiveness, integrate individual-level outcomes data with population-level analyses, assess potential differential impacts across demographic groups, and evaluate longer-term effects as more post-implementation data become available. Additionally, studies should examine the relationship between specific ERPO law provisions (e.g., allowable petitioners, duration, and due process protections) and effectiveness.

In conclusion, ERPO laws appear to offer a promising policy approach for reducing firearm mortality while targeting interventions specifically for those at elevated risk. These findings support continued policy diffusion, implementation improvement efforts, and ongoing evaluation to maximize public health benefits while ensuring appropriate civil liberties protections.

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**Appendix**

**eMethods**

**1. Statistical Model**

**1.1 Model form**

Consistent with the extensive simulation work of Schell et al. and colleagues, we modeled annual state-level counts of firearm suicides with a second-order autoregressive negative-binomial model.  The form combines the over-dispersion needed for rare-event counts with an explicit adjustment for the tight year-to-year correlation in state mortality rates—features that two-way fixed-effects and conventional difference-in-differences estimators handle poorly.  In every simulation scenario examined by Cefalu et al., this class of model maintained nominal type-I error, maximized statistical power, and produced effect estimates with minimal bias

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* **Outcome and offset.** is the number of firearm-suicide deaths in state s and year t; is the corresponding adult population, supplied as a log offset.
* **Autoregressive terms.** and capture first- and second-order persistence in log mortality rates.
* **Policy splines.**
  + is a step indicator that turns from 0 to 1 in the first calendar year an ERPO statute is operative for >6 months.
  + rises linearly from 0 to 1 over the first five post-implementation years, allowing gradual program uptake.
  + begins five years after implementation and rises linearly to 1 by year 10; its coefficient is *not* included in the five-year effect we report but absorbs longer-run drift that could otherwise bias .
* **Covariates**  We included firearm ownership prevalence, age and race/ethnicity composition, poverty, and other demographic and economic indicators specified a priori as potential confounders; all were standardized and entered with weakly informative Normal(0, 0.1) priors.
* **Year effects** (). Dummy variables for each calendar year (reference = 2001) absorb national shocks, including the 2020–2021 pandemic spike.
* **State random effect** (). A centered Normal(0, 0.5) term captures time-invariant heterogeneity.

The two subtracted lines in eEq 1 are "de-biasing" adjustments introduced by Cefalu et al.; they remove the portion of the lagged log rates that is a deterministic function of past policy status, preventing attenuation of .

**1.2 Derived quantities**

Incidence-rate-ratios (IRRs) for years 0–5 were generated:

Because *PL* () is a distractor, it is excluded from IRR computation and all effect summaries.

**1.3 Counterfactual Lives Saved**

To complement the model-based incidence-rate ratios (IRRs) we estimated the number of firearm deaths that would have occurred in the absence of ERPO laws. We compared this counterfactual total with the deaths observed between 2001 and 2023.  The procedure is as follows:

For each state *i* and calendar year *t,* we calculated

*,*

where is the first year an ERPO was active for at least six months.

The elapsed-time index is capped at five years because the policy effect is modeled as having reached its plateau by that point.  Each state–year is then linked to the posterior-median IRR for its elapsed year *k*:

.

Then, to compute counterfactual deaths for each observation:

Let

* be the observed number of firearm deaths, and
* be the population exposed in state *i* during year *t*.

Under the multiplicative‐risk assumption implicit in the regression model, the expected number of deaths had no ERPO been in place is

The cell-specific number of deaths prevented is then

Aggregated across states and years, give us

**1.3 Prior Specification**

| **Parameter** | **Prior** | **Rationale** |
| --- | --- | --- |
| **Autoregressive coefficients** |  | Lag-1 persistence in state firearm mortality is typically 0.3–0.8; lag-2 can be positive or mildly negative.  These "minimally informative" priors place 95 % probability on [-1.5, 2.5] and [-2 ,2], a range far wider than historical estimates. |
| **Policy coefficients** |  | With ERPO splines all coded in [0,1], this prior implies the total five-year effect has SD , i.e., a 95 % prior probability that the incidence-rate‐ratio (IRR) lies between 0.82 and 1.22. This matches expert-elicited expectations for plausible effect sizes (Smart, Morral & Schell 2020). |
| **Confounder effects** | for continuous, standardised covariates; for year dummies | Weakly informative: a one-SD change in any covariate is unlikely to shift the log rate by more than ≈ 0.2 on prior grounds. |
| **Year effects** |  | It allows for large national shocks (e.g., COVID-19), but centers change at zero. |
| **State random effect** |  | Captures between-state heterogeneity while preserving model identifiability (sum-to-zero constraint). |
| **Over-dispersion** |  | Diffuse on the log-variance scale; restricts \phi to plausible values but leaves tail heavy. |