Traffic Safety Program Evaluation: The Empirical Bayes Model and Mean Reversion Bias

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Abstract

Motor vehicle fatalities are increasing in the US. The US Department of Transportation emphasizes the need to implement "proven safety countermeasures," to reverse the "crisis on America's roadways." Separating the causal safety effect of a traffic policy from the natural variation in crash rates is challenging. The empirical bayes (EB) model was developed to correct for regression to the mean estimation bias and is a standard, widely used methodology in the traffic safety engineering literature. We show that the EB model does not correct for regression to the mean bias. We show this analytically, via a placebo Monte Carlo experiment, and in reference to the traffic safety literature. We demonstrate the shortcomings of the EB model and our recommendations for how to improve the reliability of traffic safety studies using 20 years of data (2003-2022) on all vehicle crashes in San Antonio, TX.

Keywords: Traffic Safety, Mean Reversion, Empirical Bayes, Red-light Cameras

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"The empirical Bayes (EB) method for the estimation of safety increases the precision of estimation and corrects for regression-to-mean bias."
- Hauer et al. (2002), *Transportation Research Record*, p126

"The EB method pulls the crash count towards the mean, accounting for RTM bias."
- US Department of Transportation (2023), *Highway Safety Improvement Program Manual - Safety*, p4

1 Introduction

Motor vehicle fatalities are increasing in the US. Fatalities increased by 10.5% from 2020 to 2021. The fatality rate is 18% higher than in 2019 (National Highway Traffic Safety Administration [2022]). The US Transportation Secretary referred to this reversal in roadway safety as a "crisis on America's roadways" (National Highway Traffic Safety Administration [2022b]). The US Department of Transportation (USDOT) emphasizes the need to implement "proven safety countermeasures" to meet this crisis (USDOT [2023]).

Regression to the mean (RTM) is a well-known challenge in measuring roadway risk and in evaluating the effectiveness of safety countermeasures (USDOT [2023b]). Naturally occurring variation in the number of crashes can be misinterpreted as evidence that a traffic intervention improves safety (e.g., Gallagher and Fisher [2020]). Roadway safety interventions often occur following years when there is an unusually high number of crashes. We would expect the number of crashes to be lower the following year, regardless of whether there is any safety intervention.

Separating the causal safety effect of a traffic policy from the natural variation in crashes is challenging. The empirical bayes (EB) model was developed to correct for RTM estimation bias (e.g., Abbess et al. [1981]; Hauer [1986]). The EB model is a standard, widely used methodology in the traffic safety engineering literature. The popularity of the EB model stems from the belief that the model "corrects" for RTM bias (e.g., Hauer et al. [2002], p126). The EB model is the preferred research methodology for both the USDOT and the American Association of State Highway and Transportation Officials (Highway Safety Manual [2023], Section 3). The USDOT recommends the EB model to "control for RTM" (USDOT [2007], p3) and asserts that the EB method "account[s] for RTM bias" (USDOT [2023c], p4).

We show that the EB model does not correct for RTM bias. The EB model can lessen RTM bias, but never fully account for the bias. Moreover, the EB model, as commonly applied in the transportation and safety engineering literature, corrects for very little RTM bias. We first show this analytically. Next, we run a placebo Monte Carlo policy experiment. Our Monte Carlo analysis shows that the EB model performs particularly poorly when there is a high amount of unexplained variation in the crash data and when researchers select modeling weights to produce the most efficient estimator.

In Section 5, we evaluate a placebo traffic intersection safety program using twenty years of data (2003-2022) on all vehicle crashes in San Antonio, TX, the 12th largest city in North America. We refer to this non-existent safety intervention as a "placebo red-light camera program," although the placebo policy could be any intersection safety countermeasure. Red-light camera (RLC) programs are common in US cities. San Antonio is one of the few large cities never to have had a RLC program (Insurance Institute for Highway Safety [2023]). Meta-analyses, which almost exclusively summarize studies that use the EB model, conclude that RLC programs dramatically reduce the number of total crashes (Høye [2013]; Goldenbeld et al. [2019]). We show that a simple before-after model implies a 12% reduction in total crashes, when intersections are selected for the placebo treatment using selection criteria similar to that used in other cities with actual RLC programs. We then show that the standard EB model does not correct for mean reversion in this setting. The EB model is severely biased towards finding that RLC programs improve safety.

The USDOT rightly states that "program and project evaluations help agencies determine which countermeasures are most effective in saving lives and reducing injuries" (USDOT [2023c], p16). However, policy evaluations are only helpful if they provide unbiased estimates for the causal impact of the safety measure being studied. The final section provides recommendations for how to improve the reliability of traffic safety studies, given that researchers can not rely on the EB model to correct for RTM bias. We propose diagnostic tests that will help researchers gauge the potential RTM bias from using an EB model in a particular setting. We also draw a parallel to the program evaluation literature in labor economics, which confronted a similar sample selection problem (e.g., Ashenfelter [1978]; Ashenfelter and Card [1985]; LaLonde [1986]). The solutions proposed by this literature are illustrative: the importance of transparent analysis of the raw data, recognition that conventional models do not solve RTM (and sample selection) bias, an emphasis on careful sample construction, and the development of new research designs.

2 The Empirical Bayes Model

Road locations targeted for safety countermeasures are not randomly selected. The main selection concern is that roadway locations (i.e. road segments or intersections) where safety measures are implemented ("treated" locations) are often chosen based on their underlying crash risk, or because of recent trends in the number of crashes. Model estimates for how a safety measure impacts the number of crashes will generally be biased when the underlying risk differences are not taken into account, or when naturally occurring variation in the number of crashes is misinterpreted as part of the causal safety treatment effect.

Treated roadway locations are often selected due to the large number of recent crashes. A simple before-after model that evaluates the effectiveness of the safety countermeasure using the time period just before the treatment as the baseline level of crashes will lead to a safety treatment estimate that is too large. The reason is that the baseline level of crashes is artificially high due to naturally occurring variation in the number of crashes. The EB model (e.g., Abbess et al. [1981]; Hauer [1986]; Hauer et al. [2002]) seeks to account for this bias by adjusting the baseline level of crashes downwards at treated locations, so as to eliminate RTM bias and to estimate an unbiased treatment effect.

Below we highlight the key steps to estimate the EB model. In the next section, we provide details for our two main criticisms of the EB model: (1) the EB model does not correct for RTM bias, and (2) as applied in the literature, the EB model will usually adjust for only a small amount of RTM bias.

The first step to estimate the EB model is to specify a structural model of vehicle crashes, referred to as the Safety Performance Function (SPF).[1] The SPF is supposed to accurately characterize the deterministic relationship between the number of crashes and the variables that cause these crashes. The SPF is estimated using out-of-sample data. The traffic engineering literature usually estimates the SPF with a negative binomial regression. The estimated SPF parameters are then applied to the sample locations to generate predictions for the pre-treatment and post-treatment number of crashes, $\widehat{E(k_i)}$ and $\widehat{E(\lambda_i)}$, respectively. Note that in the EB model, there is no difference between sample locations and treated locations. The entire sample is comprised of locations where the safety measure is implemented.

Next, use the predicted number of crashes for the sample locations in the pre-treatment period to adjust the actual crashes observed at these locations. This is the key modeling step. The adjustment is supposed to account for random variation in the number of crashes at sample locations that would otherwise lead to RTM bias. In practice, this is done by calculating a weighted average of the actual and predicted number of crashes (Equation 1). K_i is the actual number of crashes at the sample locations during the pre-period. $\hat{w}_i \in (0, 1)$ is the weight.

$$\widehat{E(k_i|K_i)} = \hat{w}_i \widehat{E(k_i)} + (1 - \hat{w}_i)K_i \tag{1}$$

[1] Throughout our discussion we assume that all roadway locations are a constant length (e.g. an intersection or a 1km road segment). See Hauer [1997], Hauer et al. [2002], and Highway Safety Manual [2023] for detailed discussions on how to estimate the model, including: framing the estimated treatment effect as the safety index, and adjustments that are necessary when sample locations are of varying lengths.

Equation 2 shows that $\hat{w}_i$ is a ratio of two components that are estimated using the SPF. $\widehat{V(k_i)}$ is the estimated variance in the number of pre-treatment crashes for sample location i. $\widehat{V(k_i)} = \frac{\widehat{E(k_i)}^2}{\phi}$ when estimating the model using a negative binomial regression, where ϕ is the estimated overdispersion parameter.

$$\hat{w}_i = \frac{\widehat{E(k_i)}}{\widehat{E(k_i)} + \widehat{V(k_i)}} = \frac{1}{1 + \frac{\widehat{E(k_i)}}{\phi}} \tag{2}$$

Equation 3 calculates the predicted crashes at each sample location in the post-treatment period in the absence of a safety intervention. The assumption is that $\hat{q}_i = \frac{\widehat{E(\lambda_i)}}{\widehat{E(k_i)}}$ reflects how the number of crashes would have evolved over time if the location was never treated.

$$\hat{\pi}_i = \hat{q}_i \widehat{E(k_i|K_i)} \tag{3}$$

The estimated causal effect of a new traffic policy or engineering safety measure on the number of crashes is calculated as $\hat{\delta} = \lambda - \hat{\pi}$. λ is the sum of the actual number of crashes at sample locations in the post-treatment. $\hat{\pi}$ is the sum of predicted crashes at sample locations in the post-treatment. The average causal effect of implementing a safety measure at a sample location is $\bar{\hat{\delta}} = \bar{\lambda} - \bar{\hat{\pi}}$, where $\bar{\hat{\delta}} = \frac{1}{N}\sum_{i=1}^{N} \hat{\delta}_i$ (and similarly for $\bar{\hat{\pi}}$ and $\bar{\lambda}$). $\bar{\hat{\delta}}_i$ is an unbiased causal estimate for a safety intervention if $\hat{\pi}_i$ is an unbiased estimate for the predicted crashes at each sample location in the absence of an intervention.

Figure 1: **EB Model Causal Effect for a Safety Countermeasure**

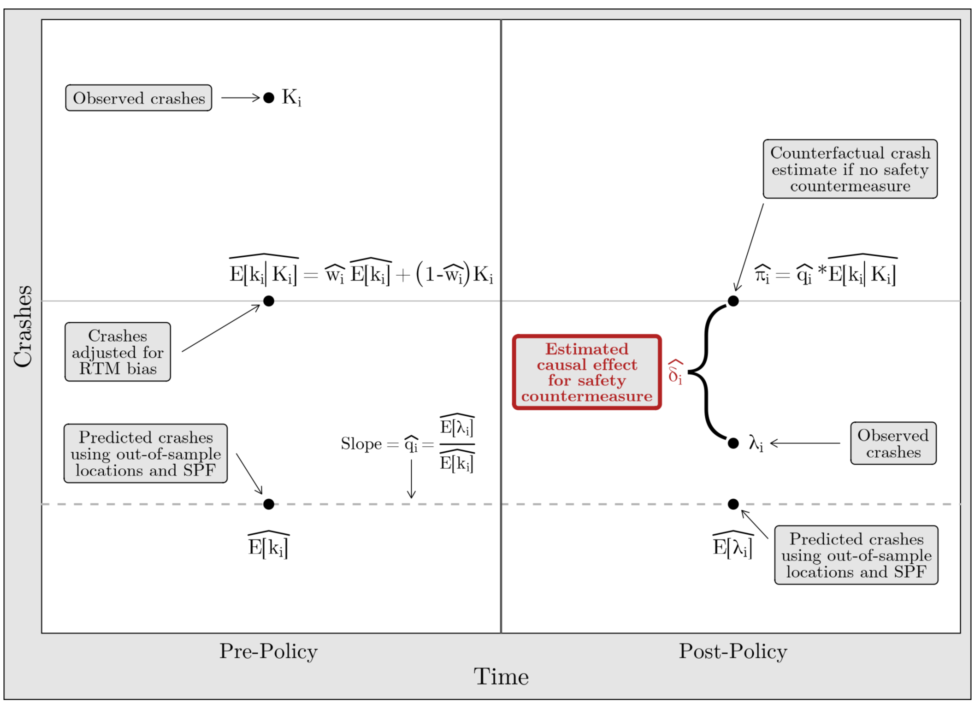


Figure 1 depicts the EB model for sample location i. The y-axis measures the number of crashes, while the x-axis is time. The vertical line in the middle of the figure indicates the introduction of a safety countermeasure (treatment). The points to the left (right) of the vertical line are calculated over the entire pre-treatment (post-treatment) period. The estimated causal effect of the safety countermeasure is represented by the vertical distance between $\hat{\pi}_i$ and λ_i. We illustrate the case where there is no change in the expected number of crashes (i.e. $\hat{q}_i = 1$) and $\hat{w}_i$ is approximately 0.5.

3 The Empirical Bayes Model and RTM Bias

The EB model does not correct for RTM bias. There are two related reasons. First, the modeling of the SPF. Second, how the EB model ostensibly corrects for mean reversion. In our discussion, we reference a sample of forty research papers from the published traffic safety engineering literature. We select the papers based on citation counts to highlight the large variety of safety interventions evaluated using the EB model (Table 1).

3.1 Modeling the Safety Performance Function

The EB model relies on the correct specification of the SPF. Estimates from the SPF are used as a way to reduce RTM bias, and to adjust the observed crash data for calendar time trends. Numerous driver, vehicle, and roadway factors correlate with crash risk and fatality rates (e.g., World Health Organization [2022]). However, many EB models estimate a very parsimonious SPF. The median number of independent crash risk variables included in the SPF models for the papers in our literature review is two.

In our view, there are two likely reasons why researchers tend to include so few explanatory variables when modeling crashes. First, methodological papers often demonstrate the EB model using a parsimonious SPF. Hauer et al. [2002], a highly cited tutorial, specifies the SPF using just a single independent variable. Second, in most research settings, detailed data are unavailable for many factors that correlate with crash rates.

A direct consequence of estimating a SPF with few explanatory variables is that the SPF is likely to do a poor job explaining the observed variation in crashes. Only 30% of the studies in our literature review report the amount of crash variation explained by the SPF model. For example, only around half the crash variation is explained by the SPF models in Montella et al. [2015]. This is a concern because the SPF is the main modeling tool to reduce RTM bias.

Roadway locations selected for safety interventions tend to be more risky, on average, than unselected locations. The EB model applies SPF coefficient estimates that are typically estimated from lower risk out-of-sample locations to predict crash levels at riskier sample

Table 1: **Empirical Bayes Model Literature Review, EB Model Language**

Study	Topic	EB Model and RTM bias language
Himes et al. (2016)	Accident Prevention Tech.	"this methodology is considered rigorous in that it <u>accounts</u> for regression to the mean" p9
Rahman et al. (2020)	Accident Prevention Tech.	"the present study used the EB method [...] as it <u>accounts</u> for the regression-to-mean bias" p51
Claros et al. (2015)	Alternative Intersections	"to <u>account</u> for [...] resulting regression to the mean, the *Highway Safety Manual* recommends [...] empirical Bayes" p6
Edara et al. (2015)	Alternative Intersections	"an empirical Bayes analysis to <u>account</u> for regression to the mean" p12
Gross et al. (2013)	Alternative Intersections	"empirical Bayes (EB) methodology to <u>control</u> for regression-to-the-mean" p235
Le et al. (2017)	Cost-Benefit Analysis	"EB method is considered rigorous in that it <u>accounts</u> for regression to the mean" p81
Jung (2017)	Driving Behavior	"EB and FB methods can increase the precision of the estimation and <u>correct</u> for the regression-to-the-mean bias" p358
Montella (2010)	Hot Spot Identification	"EB method increases the precision of the safety estimation and <u>corrects</u> for the regression-to-mean bias" p577
Persaud et al. (2010)	Methodology	"this procedure <u>accounts</u> for regression-to-the-mean effects" p38
Yu et al. (2014)	Methodology	None
Brewer et al. (2012)	Passing Zones	None
Park et al. (2012)	Passing Zones	"empirical Bayes (EB) method [...] is superior to other methods because it can <u>address</u> the regression-to-the-mean bias" p38
Persaud et al. (2013)	Passing Zones	"because of changes in safety [...] from regression to the mean [...] the count of crashes before a treatment by itself is not a good estimate" p60
Gouda et al. (2020)	Pavement Quality	"the before-and-after EB technique [...] <u>accounts</u> for critical analysis limitations [...] such as regression-to the-mean (RTM)" p93
Park et al. (2017)	Pavement Quality	"CMFs [developed using empirical Bayes] can <u>account</u> for the regression-to-the-mean threat" p78
Peel et al. (2017)	Pavement Quality	"empirical Bayes [...] accurately reflects anticipated changes in crash frequencies [...] that may be <u>attributable</u> to regression to the mean bias" p12
Park et al. (2015)	Pedestrian, Cyclist Safety	"The main advantage of the EB method is that it can <u>account</u> for [...] regression-to-the-mean (RTM) effects" p181
Strauss et al. (2015)	Pedestrian, Cyclist Safety	None
Strauss et al. (2017)	Pedestrian, Cyclist Safety	None
Goh et al. (2013)	Public Transportation	"[empirical Bayes method] key strength is its ability to <u>account</u> for regression to the mean (RTM) effects" p43
Naznin et al. (2016)	Public Transportation	"using the empirical Bayes (EB) method [...] <u>account[s]</u> for wider crash trends and regression to the mean effects" p91
Ahmed et al. (2015)	Red Light Cameras	"to <u>account</u> for the possible regression-to-the-mean (RTM) bias [...] Empirical Bayes (EB) method should be adopted" p133
Ko et al. (2017)	Red Light Cameras	"this study uses an EB methodology to <u>address</u> regression-to-mean (RTM) bias" p118
Pulugurtha et al. (2014)	Red Light Cameras	"EB method [...] <u>minimizes</u> the regression-to-mean bias in a sample and gives valid results even for small samples" p11
Park et al. (2019)	Road Signage	"EB methods have been regarded as statistically defensible methods that can <u>cope</u> with [...] regression-to-the-mean bias" p3
Wood et al. (2020)	Road Signage	None
Wu et al. (2020)	Road Signage	"before-and-after evaluation with the EB method, as proposed by Hauer, which explicitly <u>addresses</u> the RTM effect" p423
Cafiso et al. (2017)	Road Shoulders, Barriers	"The methodology [empirical Bayes] has the great advantage to <u>account</u> for the regression to the mean effects" p324
Chimba et al. (2017)	Road Shoulders, Barriers	"The EB uses before-and-after procedures which properly <u>account</u> for regression to the mean bias"
Park et al. (2012)	Road Shoulders, Barriers	"The EB method can <u>account</u> for the effect of regression-to-the mean" p318
Park et al. (2016)	Road Shoulders, Barriers	"main advantage of the EB method is that it can <u>account</u> for [...] regression-to-the-mean effects" p33
Khan et al. (2015)	Rumble Strips	"[EB method] <u>address[es]</u> the limitation of the Naïve and CG Methods by <u>accounting</u> for the regression-to-the-mean effect" p36
Park et al. (2015)	Rumble Strips	"main advantage of the EB method is that it can <u>account</u> for [...] regression-to-the mean (RTM) effects" p313
Park et al. (2014)	Rumble Strips	"main advantage of the EB method is that it can <u>account</u> for [...] regression-to-the mean (RTM) effects" p170
De Pauw et al. (2018)	Speed Limits, Enforcement	"EB approach increases the precision of estimation and <u>corrects</u> for the regression-to the-mean (RTM) bias" p85
Montella et al. (2012)	Speed Limits, Enforcement	"the empirical Bayes methodology [...] properly <u>accounts</u> for regression to the mean" p17
Montella et al. (2015)	Speed Limits, Enforcement	"this [empirical Bayes] methodology is rigorous and properly <u>accounts</u> for regression-to-the-mean" p168
Jin et al. (2021)	Turn Lanes, Traffic Signals	None
Khattak et al. (2018)	Turn Lanes, Traffic Signals	"The E-B method <u>eliminates</u> regression to the mean (RTM) bias" p124
Srinivasan et al. (2012)	Turn Lanes, Traffic Signals	"state-of-the-art before-after empirical Bayes method because it has been shown to properly <u>account</u> for regression to the mean" p109

Studies were selected based on topic and citation count from the following journals (2010-2022): *Accident Analysis & Prevention, Analytic Models in Accident Research, Journal of Safety Research, Traffic Injury Prevention, Transportation Research Record, Transportation Research Part A-F, Transportation Science.*

locations. The sample and out-of-sample locations are likely to differ based on one or more of the many crash risk factors omitted from the SPF. This will generally lead the crash risk parameters estimated on the out-of-sample locations to be biased estimators for the sample locations (e.g., LaLonde [1986]; Greene [2003]). EB model adjustments for mean reversion and the passage of time that rely on the SPF will also be biased.

3.2 Adjusting for Mean Reversion

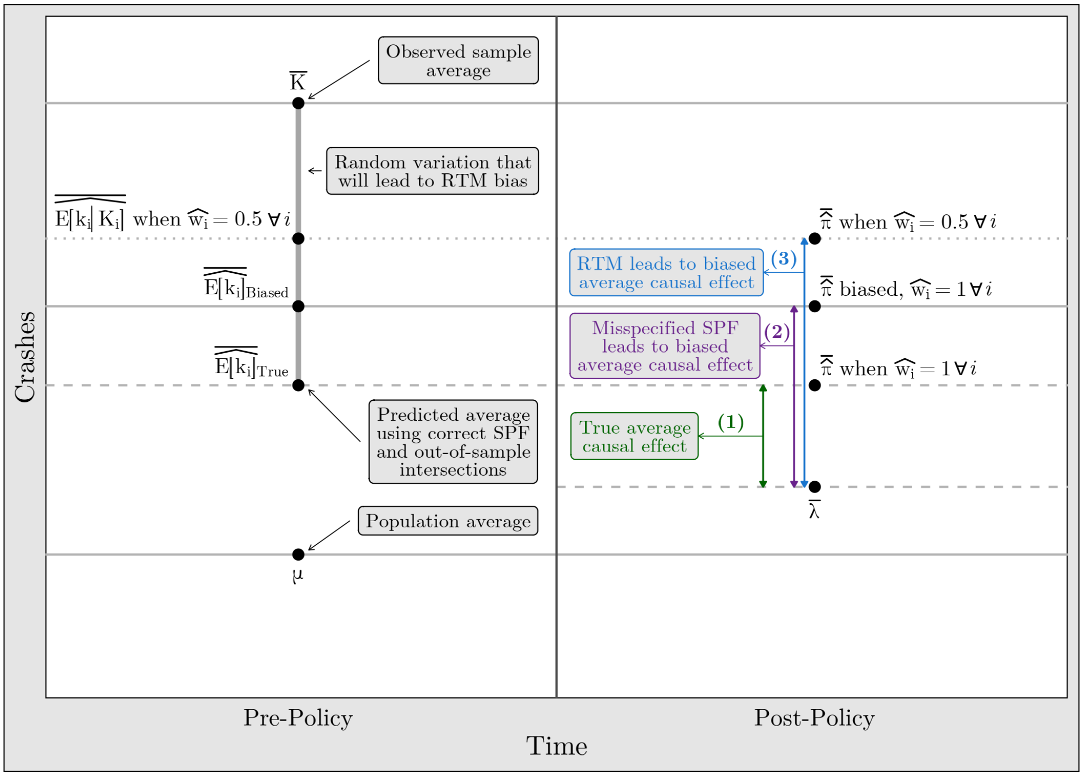
The vast majority of EB studies in our literature review include statements claiming that the model "corrects" or "accounts" (or similar) for mean reversion (Table 1). These statements repeat the language used by leading EB model advocates (Hauer et al. [2002]; USDOT [2023c]).

Equation 1 is the key modeling step that adjusts for mean reversion. Equation 1 calculates a weighted average of the actual and predicted number of pre-treatment crashes at sample locations. The assumption is that the SPF will provide an estimate, $\widehat{E(k_i)}$, that is non-mean reverting and unbiased. Even if this assumption is true, Equation 1 will only eliminate mean reversion when all of the weight, $\hat{w}_i$, is on the predicted number of crashes. The weighted average will be mean reverting when the pre-treatment crash levels are mean reverting and $\hat{w}_i \in (0, 1)$.

Figure 2 depicts the two potential sources of bias (SPF misspecification and RTM) in the EB model. The figure is similar to Figure 1, except that the plotted points are for a sample of locations, rather than for a single location and the points in the figure are averages across the sample.

The vertical distance between $\bar{K}$ and $\overline{\widehat{E(k_i)}}_{True}$ in Figure 2 shows the anticipated mean reversion that will occur in years following a safety intervention at treated locations when the SPF is correctly estimated, regardless of the actual effectiveness of the safety intervention. The figure shows three estimates for the average causal effect for a safety countermeasure using the EB model. The true causal effect is shown by the vertical line (1). The EB model only provides an unbiased estimate when the SPF is correctly specified and all of the weight in Equation 1 is on the SPF estimate for all sample locations. Vertical line (2) shows how the causal effect will suffer from RTM bias, even when the SPF is correctly specified. Vertical line (3) shows how the causal effect can be biased from SPF misspecification. Note that we illustrate the case where there is no change in the expected number of crashes (i.e. $\hat{q}_i = 1$) and $\hat{w}_i$ is 0.5 for all sample locations. Our critique of the EB model holds for any value of $\hat{q}_i$ and for $\hat{w}_i \in (0, 1)$.

Figure 2: **EB Model RTM and SPF Misspecification Bias**



3.3 EB Model Only Reduces a Small Amount of RTM Bias

Equation 2 shows how the weight, $\hat{w}_i$, is calculated. This equation is used to calculate the weight because it minimizes the variance of $\widehat{E(k_i|K_i)}$ (e.g., Hauer [1997]). In practice, the EB model is generally estimated with a small $\hat{w}_i$. The reason is that high crash locations are most likely to be selected for an intervention. High crash locations have a larger expected crash to overdispersion parameter ratio ($\frac{\widehat{E(k_i)}}{\phi}$), as compared to other potential locations.

A small $\hat{w}_i$ implies that the (mean-reverting) number of crashes is weighted more heavily when calculating the expected number of crashes at sample locations (see Equation 1). None of the studies from our detailed review specify the average weights used in the SPF. For example, Ko et al. [2017] evaluate the city of Houston's red-light camera program. RTM bias is a serious concern in this setting. Houston officials selected signal intersections for the red-light camera safety intervention based on the unusually high number of crashes at these intersections in the years just prior to the start of the program (e.g., Gallagher and Fisher [2020]; Stein et al. [2006]). The number of crashes at these Houston intersections would be expected to decrease in subsequent years due to mean reversion regardless of the camera program.

We estimate the EB model on our own sample of Houston red-light camera intersections, using a parsimonious SPF model similar to that of Ko et al. [2017]. We calculate an average $\hat{w}_i$ less than 0.1. The EB model only eliminates a very small amount of RTM bias.

4 Monte Carlo Simulation Experiment

We generate crash data and simulate a placebo experiment to evaluate the effectiveness of the EB model at correcting for RTM bias.

4.1 Data Generating Process

We follow the literature and model crashes using a negative binomial distribution. We generate crash data using Equation 4. The number of crashes at a location is positively correlated with Average Daily Traffic (ADT). ADT_i is constant over time at the location level, drawn from an exponentiated uniform distribution, and can be interpreted as the number (in hundreds of thousands) of vehicles passing through a location each day.

$$Crashes_{it} = poisson(gamma\Big(\frac{exp(\alpha + 0.05ADT_i)}{\phi}\Big), \phi) \tag{4}$$

The overdispersion parameter, ϕ, allows for the variance in the number of crashes to be larger than the variance for a poisson distribution with the same mean (e.g., Cameron and Trivedi [2005]). The overdispersion parameter is a key input into the recommended weight commonly used in the EB model literature (Equation 2). Typically, greater overdispersion implies a larger crash variance and a smaller $\hat{w}_i$.

In our simulations, we vary ϕ to isolate how $\hat{w}_i$ affects RTM bias in the EB model, while holding the crash variance constant across the generated datasets.[2] We select α conditional on ϕ to generate weights, while holding the crash variance constant. We simulate crash data using 56 different values of the overdispersion parameter that lead to $\hat{w}_i \in (0, 0.97]$, and create one hundred datasets for each of the 56 parameterizations. Each dataset includes 10,000 locations (i) and six time periods (t) for a total sample size of 60,000.

4.2 Placebo Safety Policy Experiment

We estimate the effect of a placebo safety policy with a true effect of zero using a before-after model and the EB model. The Monte Carlo experiment focuses on potential RTM bias, since the estimated SPF matches the true crash data generating process. We assume that a placebo safety policy is implemented following period three. The first three time periods are considered “pre-policy”, while periods four to six are “post-policy”. The placebo treatment

[2] We also run a Monte Carlo simulation that allows the crash variance to differ between datasets. The (poor) performance of the EB model in eliminating RTM bias is very similar.

is assigned to the 500 locations with the highest number of crashes in the three pre-policy periods. These 500 locations comprise our treatment sample in each dataset, while the other 9,500 locations are the out-of-sample locations used to estimate the SPF.

We chose the panel length based on the recommendation in the literature that using the three most recent pre-policy years are sufficient to reliably estimate the EB model. In fact, confidence in the EB model to eliminate RTM bias, combined with a concern that earlier years may not reflect a location's current underlying crash risk, lead many researchers to avoid the use of crash information for earlier years (e.g., Hauer et al. [2002]).

The USDOT motivates the EB model as best practice by stating that the EB model "account[s] for RTM bias" that would otherwise occur when using a "naive" before-after model (USDOT [2023c]). In our setting, estimates from a before-after model wrongly suggest that the placebo policy reduces vehicle crashes. This follows from the fact that there is no actual safety policy, and because the sample locations are selected based on an unusually high number of pre-policy crashes. The number of crashes is lower after the placebo policy due to mean reversion. The average before-after model estimate across our Monte Carlo samples is approximately -80%.

Figure 3a shows the results of our simulation. We follow the USDOT and use a before-after model as a baseline to measure whether the EB model corrects for RTM bias (USDOT [2023c]). The vertical axis plots the ratio of the EB estimate to the before-after model estimate for the same sample and simulation. We interpret the y-axis as the percentage of mean reversion remaining when using the EB model. The x-axis measures the average SPF weight ($\bar{w}_i$) used for each sample and simulation. There is a clear negative correlation between the SPF weight and the level of RTM bias. As expected, EB models that use small weights correct for very little RTM bias.

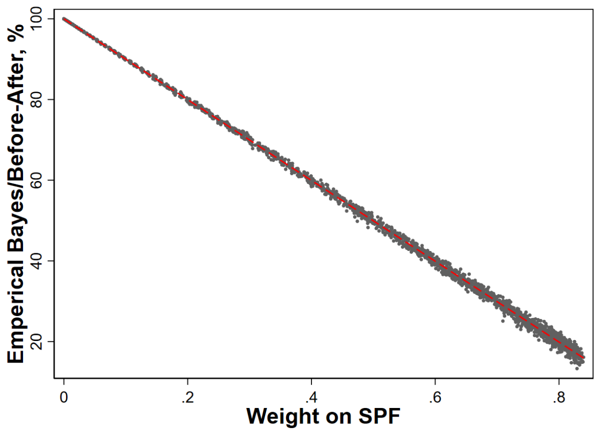
5 San Antonio Crash Analysis

The San Antonio analysis is motivated by the EB modeling literature, which concludes that red-light camera programs are highly effective in reducing the total number of crashes (e.g., Høye [2013]; Goldenbeld et al. [2019]). We demonstrate the shortcomings of the EB model in correcting for RTM bias using 20 years (2003-2022) of data on all reported vehicle crashes in San Antonio, TX.

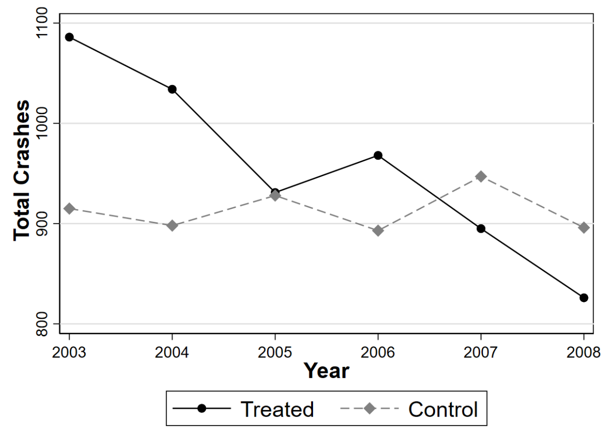
5.1 Data

The crash data are from the Texas Department of Transportation's Crash Records Information System (CRIS) database. CRIS stores information on all TX vehicle crashes. The

Figure 3: **Monte Carlo and San Antonio Placebo Policy Analyses**



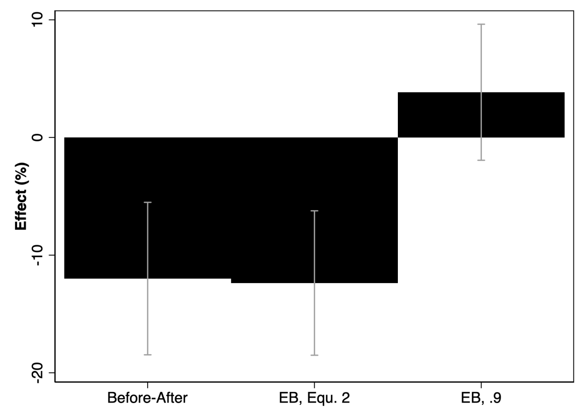
(a) Remaining RTM Bias



(b) Crash Trends in San Antonio, TX

Dependent Variable:	Total Crashes
Average Daily Traffic	-0.0000026
	(0.0000067)
Risk Proxy	0.077
	(0.021)
Psuedo R-Squared	0.042
Overdispersion	0.15
Avg weight, treated obs	0.139
Observations	44

(c) SPF Model for San Antonio, TX



(d) EB Model Placebo Treatment Effect

information in CRIS is compiled by law enforcement personnel and includes the crash location (latitude and longitude) and whether the crash was “in or related to” an intersection. We use GIS software to identify the yearly count of crashes that are “in or related to” one of 624 major San Antonio intersections. Next, we spatially join all georeferenced crashes that are within 200 feet of a major intersection and within 50 feet of an intersecting road that passes through the intersection. We define an intersection crash as within 200 feet based on evidence that the number of crashes within 200 feet of an intersection in Houston, TX during the same time period is several times higher than at any distance greater than 200 feet (Gallagher and Fisher [2020]).

We also collect average daily traffic (ADT) information for each intersection (North Central Texas Council of Governments [2016]). Not all roadways are surveyed each year.

ADT, calculated as the average across all intersecting roads at an intersection, is available once for each intersection during the first three years of our panel. The final panel for our analysis is a yearly intersection-level database that sums the number of crashes at each intersection.

5.2 Placebo Safety Policy

San Antonio is one of the few large North American cities never to have had a red-light camera program (Insurance Institute for Highway Safety (2023)). RLC programs use video cameras to monitor signalized intersections for red-light running. The aim is to reduce the total number of crashes, and especially injury-related crashes, through a reduction in collisions involving vehicles running a red light. We focus our analysis on 2003-2008 because ADT is available for each intersection 2003-2005. We evaluate a placebo intersection safety countermeasure that we call a RLC program. We assume the policy is implemented in 2006. We assign the placebo safety intervention to the 50 intersections with the largest number of crashes 2003-2005. This selection criteria closely follows how Houston selected intersections for its RLC program (Stein et al. [2006]; Gallagher and Fisher [2020]). The effect of the program is analyzed using three post-treatment years 2006-2008.

Figure 3b plots the total number of crashes at the "treated" and "control" intersections by year. The number of crashes is stable at the control intersections before and after the placebo safety policy. The average total number of yearly crashes at the control intersection is 914 before the policy and 912 after the policy. In sharp contrast, the number of crashes at treated intersections decreased by 12% from 1,017 pre-policy to 896 post-policy.

5.3 Model Results

We evaluate the placebo program using a simple before-after model and the EB model. The before-after model implies that the program led to a 12% reduction in crashes (Figure 3d). The entire estimated reduction in the before-after model is due to mean reversion. There is no evidence of an overall trend in crashes at the non-treated intersections (Figure 3b).

Our review of two recent meta-analyses on the safety effectiveness of RLC programs reveals that most of these studies use the EB model and a parsimonious SPF ([Høye, 2013]; [Goldenbeld et al., 2019]). The median number of independent variables in the SPF is two. ADT is included in every SPF. We follow the literature in our analysis and also specify an SPF with two independent variables and include ADT as one of the variables. The second variable in our SPF is a proxy for crash risk, measured as the yearly mean number crashes at the intersection from 2003-2022. Twenty years of actual crash data provides a good measure of the true crash risk at each intersection. The variable is a proxy because it does not reflect changes to the roadways or city-wide driving patterns that may have altered the underlying crash risk. We do not claim to have modeled a correct SPF. Our aim is to specify a SPF model in line with the current literature.

A critical decision in any EB model analysis is how to specify the control group when estimating the SPF. The crash-related characteristics of the control group should closely match those of the treated group. We use the risk proxy variable to limit out-of-sample San Antonio control intersections to those with a risk proxy in the same range as the treated intersections. All potential control intersections with an average number of yearly crashes (2003-2022) of less than 5.0 or greater than 29.4 are excluded from the analysis. We estimate the SPF on a sample of 44 control intersections.

Figure 3c shows results from using a binomial regression to estimate our SPF. The table lists parameter estimates and standard errors (in parentheses) for the independent variables. The estimated coefficients can be interpreted as semi-elasticities. We estimate that a one crash per year increase in the risk proxy increases the average number of yearly crashes at the intersection in the placebo pre-period (2003-2005) by approximately 8 percent (probability value < 0.01).

Figure 3d displays the before-after and EB model point estimates. We estimate the EB model using two different sets of model weights ($\hat{w}_i$). The recommended approach in the literature is to use Equation 2 to determine $\hat{w}_i$ for each treated intersection. When we use the recommended approach, we estimate an average weight of 0.14 (Figure 3c), and a placebo policy treatment effect of -12% (Figure 3d, middle column). The figure shows the 95% confidence interval in brackets.[3] The EB model estimate is very similar to the before-after estimate. Both estimates suffer from substantial RTM bias. In the third column of Figure 3d we estimate the EB model using a fixed weight of 0.9 for all intersections. The estimated average placebo safety treatment effect is small in magnitude and not statistically different from zero.

6 Discussion and Recommendations

We provide the following recommendations for researchers and practitioners who continue to use the EB model, or who seek alternative models to evaluate traffic safety interventions.

6.1 Better Selection of Intervention Sites

Safety interventions should target high risk road locations to most improve roadway safety. However, a treatment selection process where the selection criteria emphasize recent spikes

[3]We calculate bootstrapped standard errors using a non-parametric bootstrap procedure and 1,000 bootstrap samples (e.g., Efron and Tibshirani [1993]; Cameron and Trivedi [2005]).

in crashes, rather than the underlying crash risk, will lead to mean reversion and complicate evaluation of the safety intervention.

Mean reversion induced by the site selection method can be reduced by limiting the role that year-to-year variation in crash outcomes plays in selecting treatment locations. First, select locations for a safety intervention using more years of crash data (e.g. 10 years rather than 2-3 years as is common). Second, practitioners can minimize selection based on mean reverting temporal trends by *excluding* the most recent crash data (e.g., Ashenfelter [1978]). This is in direct contrast to the current recommendation in the EB modeling literature (e.g., Hauer et al. [2002]). Third, avoid using infrequent crash outcomes, such as fatalities, to measure roadway risk when assigning safety interventions. Even the most dangerous locations often have years with no fatal crashes. An assignment criteria that includes all crashes, while weighting the more severe crashes, will provide a more stable measure of risk over time and reduce mean reversion. The most effective method for site selection is to randomize the intervention across a sample of (pre-selected) locations, and then to evaluate the treatment effect using a randomized controlled trial (e.g., Elvik [2021]).

Practitioners can evaluate whether the selection process is likely to lead to treated locations characterized by mean reversion. Use the site selection criteria to create proposed lists of treated locations using different pre-treatment time periods. If the proposed treatment locations vary substantially from period to period, then the selection criteria is mainly picking up on temporal trends and doing a poor job measuring the true underlying crash risk. Locations assigned safety interventions using these criteria will likely be characterized by mean reversion and assessing the true impact of the safety intervention will be difficult.

6.2 Improved Application of the Empirical Bayes Method

We recommend updating language to better reflect the ability of the EB method to reduce RTM bias, reporting additional statistics regarding mean reversion, and selecting a high weight ($\hat{w}_i$).

Researchers frequently describe the EB model as "correcting", "accounting", "addressing", or "eliminating" RTM bias (Table 1). The Bayesian intuition of combining existing and new sample information to improve model estimates does not correct for RTM bias. The EB model will only eliminate RTM bias when the SPF model is specified correctly and when all weight is placed on the out-of-sample prediction (i.e. $\hat{w}_i = 1$). We recommend that researchers are transparent about these limitations of the model. Moreover, researchers should report the average $\hat{w}_i$, which provides an indication of the remaining RTM bias relative to the simple before-after model.

Researchers should set a large weight in the SPF model when there is concern over mean reversion. The standard approach is to estimate Equation 2, which selects $\hat{w}_i$ to minimize the estimated variance of $\widehat{E(k_i|K_i)}$ and thereby maximizes the precision of the EB model. However, there is a trade-off between the duel objectives of eliminating RTM bias and maximizing precision. We recommend using a fixed weight of 0.9 in settings where there is suspected mean reversion. A weight of 1.0 would eliminate RTM bias, provided the SPF is correctly specified, but completely relies on out-of-sample prediction. Moreover, a weight of 1.0 would no longer reflect the Bayesian approach of combining existing crash risk information with new crash risk information from the sample locations targeted for a safety intervention. A weight of 0.9 allows the EB model to largely reduce RTM bias in expectation (Figure 3a) and in practice (Figure 3d), while still incorporating location specific information not captured by the SPF.

6.3 Consider Alternative Models for Analysis

The difference-in-differences (DD) model is an attractive alternative to the EB model (e.g., Angrist and Pischke [2008]; Roth et al. [2023]). The DD model estimates the effect of a safety intervention by comparing the change before and after intervention for treated sites to the same before and after change for the control sites. The DD model allows the researcher to adjust for mean reversion by selecting control locations that have similar patterns of mean reversion as treated locations.

The most important part of the DD model is to choose control locations that would have a similar change in the number of crashes without the intervention. There are several strategies to choose control locations. First, if researchers know the criteria used by policy makers to select the treated locations, then similar control locations can be selected using the same criteria. For example, there may be control locations that meet the same selection criteria, but were excluded for reasons unrelated to crash risk (e.g. political or budget considerations). Second, use propensity score matching and trimming to identify sites with similar features as those receiving the intervention (e.g., Abdulsalam et al. [2017], Gallagher and Fisher [2020]). Third, use eligible non-selected locations when the organization implementing the intervention randomizes the treated locations.

7 Conclusion

The empirical bayes model was developed to correct for regression to the mean estimation bias and is a standard, widely used methodology in the traffic safety engineering literature. We show that the EB model does not correct for RTM bias. We first show this analytically using the EB modeling equations. The EB model will lead to causal safety measure estimates that suffer from RTM bias, unless the EB model completely ignores crash data at the treatment locations. Our Monte Carlo analysis shows that the EB model performs particularly poorly when there is a high amount of unexplained variation in the crash data and when researchers select modeling weights to produce the most efficient estimator.

We apply the EB model to analyze a placebo traffic safety program using twenty years of data (2003-2022) on all vehicle crashes in San Antonio, TX. We refer to this non-existent safety program as a red-light camera program. We show that a simple before-after model implies a 12% reduction in total crashes, when intersections are selected for the placebo treatment using selection criteria similar to that used in other cities with actual RLC programs. We then show that the standard EB model does not correct for mean reversion in this setting.

Our recommendations for how to improve the reliability of traffic safety studies include recognizing that the EB model does not correct for RTM bias, proposed diagnostic tests to gauge the amount of potential RTM bias from using an EB model, a stronger emphasis on careful sample construction, and the application of statistical models that are more likely to correct for RTM bias.

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