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A comparative analysis of accident modification functions for traffic law enforcement

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ABSTRACT

Traffic law enforcement is a road safety measure whose effects on accidents or injuries is best described by means of a function rather than a point estimate. An informative function should comprise both increases and decreases in enforcement. Currently available accident modification functions cannot serve this need. A fruitful approach to developing accident modification functions covering both increases and decreases in enforcement is differences-in-differences estimates based on multivariate accident prediction models. The paper explains how to develop such estimates and illustrates them. The interpretation of the results of empirical studies can be informed by a game-theoretic model of the effects of enforcement, previously published in Accident Analysis and Prevention (Bjørnskau and Elvik 1992, 507–520).

1. Introduction

Traffic law enforcement is a road safety measure which can be introduced in large or small doses. In principle, its use can be varied continuously, by varying police manpower by the hour. One would expect its effect on accidents and injuries to vary according to the extent of its use. Elvik (2011A, 2015A) attempted to develop dose–response curves to describe the effects of varying amounts of enforcement on the number of accidents, but these attempts were not very successful. The studies included were quite heterogeneous and outlying data points was a problem in many of them. Despite extensive editing of data, the accident modification functions were not very satisfactory.

This paper studies accident modification functions for traffic law enforcement from a different perspective. Rather than trying to develop such a function by fitting curves to data points based on different studies, functions that have been developed in different studies are compared. Each of the studies reviewed in this paper has developed one or more accident modification functions for traffic law enforcement, in most cases by applying a count regression model like Poisson regression or negative binomial regression. These functions can be compared in terms of the shape of the accident modification functions and the size of the effects of enforcement implied by them. More specifically, the paper focuses on the following questions:

1. Do accident modification functions developed in different studies give consistent results?
2. Is there a relationship between the range of levels of enforcement covered by the functions and the size of the effect on accidents or injured road users?
3. Do the estimated effects of enforcement vary according to accident or injury severity?
4. Do the estimated effects of enforcement vary according to the target for enforcement (alcohol use, speeding, etc.)?
5. Which is the best approach for developing accident modification functions for traffic law enforcement?

The studies included were retrieved by searching Google Scholar and Science direct, using "traffic law enforcement" as search term. No formal synthesis of the results of the studies is made in this paper. The main focus is on comparing the accident modification functions, not synthesising them. It is therefore not very important if all relevant studies have been included. As will become clear, the studies included amply illustrate the many differences between studies that make a formal synthesis of their results difficult.

2. Theory of effects of enforcement

Traffic law enforcement is a measure whose effects to some extent can be predicted theoretically. Bjørnskau and Elvik (1992) argue that the effects of police enforcement can be modelled by means of game theory. The logic of the game taking place between drivers and the police can be explained by examining Table 1.

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Table 1
Game-theoretic model of traffic law enforcement.

		The police	
		Enforce	Not enforce
Drivers	Violate speed limit	–10000 –300	–20000 50
	Not violate speed limit	–10000 –50	0 –50

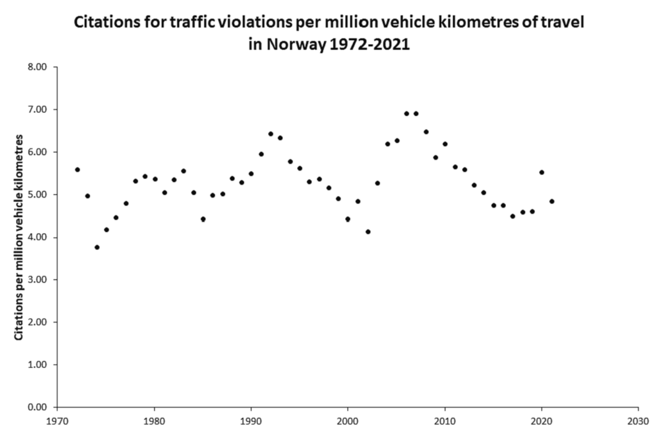

Fig. 1. Citations for traffic violations per million vehicle kilometres of driving in Norway 1972–2021.

Table 1 shows the game in normal form. In each cell of the Table, the payoff to drivers is shown in the bottom left corner, the payoff to the police is shown in the upper right corner. The payoffs to drivers may be interpreted as the monetary valuation of travel time plus the expected value of a traffic ticket if caught speeding. The payoffs to the police may be interpreted as the cost of enforcement plus the societal cost of the accidents that are prevented if enforcement ensures that there is no speeding.

The most widely accepted definition of the solution of a game is Nash equilibrium. There is Nash equilibrium if no participant in a game can gain a higher payoff by unilaterally making a different choice. A unilateral choice means that the other participants in the game do not change their choices. Does the game shown in Table 1 have a Nash equilibrium?

Starting in the upper left cell, it is seen that drivers can get a higher payoff by changing from violating the speed limit to complying with it, given that the police do not change their choice of enforcing. Once this change is made, the game moves to the lower left cell. However, from this cell, the police can make a gain by not doing enforcement. It makes sense that there is no need for enforcing speed limits if drivers comply with them anyway. Once enforcement ceases, the game moves to the lower right cell. It now becomes clear that drivers can gain by violating the speed limit. There is no risk of getting caught if there is no enforcement. Drivers therefore start speeding again and the game moves to the upper right cell. The police can now gain (or more precisely, produce a societal gain) by doing enforcement. The game moves to the upper left cell, where it started. This cycle can go on forever. The game has no solution, no Nash equilibrium, in pure strategies.

However, the game has a solution in so called mixed strategies. A mixed strategy means that each of the pure strategies – to speed or not to speed, and to enforce or not enforce – is chosen with a certain probability. Given the payoffs in Table 1, Bjørnskau and Elvik (1992) estimated that drivers should speed with a probability of 0.5 and the police enforce with a probability of 0.2857. These results are obviously sensitive to the values of the payoffs and should therefore be regarded as illustrative only. Moreover, any such a mixed strategy equilibrium is unlikely to be stable in the long run, as neither drivers nor the police will be able to determine the probabilities very precisely.

Table 2
Studies developing accident modification functions for traffic law enforcement.

Study	Country	Period	Main target	Observations
Tay, 2005	Australia	1994–2001	Alcohol	94
Yannis et al., 2008	Greece	1998–2002	Alcohol	245
Cameron et al., 2016	Australia	1995–2002	Speed	Not stated
Cameron et al., 2016	Australia	1983–2011	Alcohol	Not stated
Cameron et al., 2016	Australia	2005–2009	Drugs	Not stated
Feng et al., 2020	China	2018	All violations	365
Elvik et al., 2022	Norway	2007–2019	All violations	13
Elvik, 2023	Norway	2008–2020	Vehicle defects	13
Elvik (this paper)	Norway	1980–2021	All violations (mainly speed)	41
Elvik (this paper)	Sweden	1981–2004	All violations	23

Elvik (2015B) tested the game-theoretic model empirically, using Norwegian data for 2004–2013. He found support for the model: drivers responded to increased enforcement by complying better with speed limits, and the police changed the amount of enforcement depending on whether the rate of violations in year N – 1 increased or decreased. However, the response of the police to changes in the rate of violations was not statistically significant.

Nevertheless, time series data on the number of citations for traffic law violations per million vehicle kilometres of driving in Norway clearly show a cyclical pattern. This is shown in Fig. 1.

The number of citations can reasonably be interpreted as an indicator of the amount of enforcement. Most violations of road traffic law go undetected (Elvik, 2012), and the number of citations could be ten times, or even hundred times, greater than it is if there was more enforcement. Hence, the variation over time in the number of citations mainly reflects changes in enforcement, not changes in the rate of violations. The variation over time in the number of citations per million vehicle kilometres of driving can be exploited to determine if the number of accidents or injured road users change in response to changes in the amount of enforcement.

3. A review of accident modification functions

Table 2 lists the studies that have been identified. Each study contains one or more estimates of an accident modification function for traffic law enforcement. All studies have been published after 2000. The studies were made in Australia, China, Greece, Norway and Sweden. The data cover periods of different lengths and the main targets for enforcement vary. The number of data points used to estimate the accident modification functions also varies between the studies.

Table 3 provides details about the accident modification functions fitted in each study. A total of 17 accident modification functions have been identified. For each function, Table 3 gives the following information:

1. The statistical technique used to estimate the function. Different techniques have been used but negative binomial regression is the most common.
2. The main target for enforcement. Targets include alcohol, drugs, speed, vehicle defects and all types of violations.
3. The severity of accidents or injuries included when developing accident modification functions. In studies that include more than one

Table 3
Characteristics of accident modification functions for traffic law enforcement.

Study	Model of analysis	Main target for enforcement	Accident or injury severity	Coefficient for enforcement	Standard error of coefficient	Range of levels of enforcement (lowest = 1.00)	Accident modification factor at highest level
Tay, 2005	Poisson regression	Alcohol	Killed or seriously injured	−0.164	0.052	4.50	0.781
Yannis et al., 2008	Negative binomial regression	Alcohol	Killed road users	−0.0041	0.0024	5.12	0.933
Yannis et al., 2008	Negative binomial regression	Alcohol	Injury accidents	−0.025	0.004	5.12	0.654
Cameron et al., 2016	Power function fitted to data	Speed	Injury accidents	−0.0461	0.0019	2.01	0.968
Cameron et al., 2016	Power function fitted to data	Alcohol	Killed road users	−0.115	0.096	2.50	0.900
Cameron et al., 2016	Power function fitted to data	Alcohol	Seriously injured road users	−0.0184	0.0024	2.50	0.983
Cameron et al., 2016	Power function fitted to data	Alcohol	Injury accidents	−0.0132	0.0027	2.50	0.988
Cameron et al., 2016	Power function fitted to data	Drugs	Killed drivers	−0.1328	0.0177	2.12	0.905
Feng et al., 2020	Structural vector autoregressive	All violations	Not stated	−0.15	Not stated	2.33	0.881
Elvik et al., 2022	Negative binomial regression	All violations (drivers of heavy vehicles)	Injury accidents	−0.008	0.0042	4.34	0.870
Elvik, 2023	Negative binomial regression	Vehicle defects (heavy vehicles)	Injury accidents	−0.070	0.0251	4.67	0.901
Elvik, 2023	Negative binomial regression	Vehicle defects (heavy vehicles)	Injury accidents	−0.339	0.2262	1.30	0.905
Study	**Model of analysis**					**Range of levels of enforcement (lowest = 1.00)**	**Accident modification factor at highest level**
Elvik and Nævestad, 2023	Negative binomial regression	All violations (mostly speed)	Killed road users	−0.0493	0.0288	1.67	0.872
Elvik and Nævestad, 2023	Negative binomial regression	All violations (mostly speed)	Killed or seriously injured road users	−0.0489	0.0156	1.67	0.873
Elvik, 2023 #	Negative binomial regression	All violations	Killed road users	−0.0159	0.0031	3.04	0.742
Elvik, 2023 #	Negative binomial regression	All violations	Killed or seriously injured road users	−0.0133	0.0035	3.04	0.779

These models were developed for this paper.

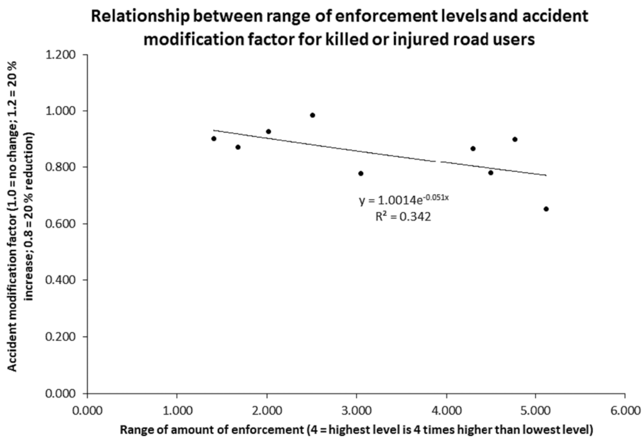

Fig. 2. Relationship between range of enforcement levels and accident modification factor for killed or seriously injured road users.

level of accident- or injury severity, separate functions have been fitted for each level.

4. The estimated regression coefficient for enforcement. A negative coefficient means that increased enforcement is associated with a reduction of the number of accidents or injured road users.
5. The standard error of the estimated regression coefficient. In *meta*-analysis, this would form the basis for estimating an inverse-variance statistical weight to each coefficient ($1/SE^2$).
6. The range of levels of enforcement. A value of, for example, 5.12 means that the highest level of enforcement was 5.12 times higher than the lowest.
7. The estimated accident modification factor associated with the highest level of enforcement. Thus, for injury accidents in Yannis et al., 2008, the value of 0.654 shows that at a level of 5.12 times the lowest, the number of injury accidents was reduced by 34.6 percent.

How these statistics were obtained will be briefly explained for each study. Tay (2005) relied on data for January 1994–October 2001 for the state of Queensland in Australia. Enforcement was measured in terms of the number of random breath tests per month. This number fluctuated around 50,000 in the first 35 months of the period, but then increased to a peak of 290,943 per month. Fig. 2 of the paper by Tay suggest an average of about 225,000 breath tests per month for months 70–94. A Poisson regression model was fitted using the natural logarithm of the number of random breath tests per month as indicator for enforcement. Applying the coefficient in Table 3, gives a predicted number of accidents of $e(\ln(50{,}000) \bullet -0.164) = 0.170$ for the low level of enforcement and of $e(\ln(225{,}000) \bullet -0.164) = 0.130$ for the high level of enforcement. The ratio of these numbers ($0.130/0.170 = 0.781$) gives the accident modification factor associated with going from the lowest to the highest level of enforcement (i.e. an increase by a factor of 4.5 (225,000/50,000) can be expected to reduce the number of accidents involving fatal or serious injury by 22 %).

Yannis et al. (2008) developed several regression models to estimate the effects of an increase in police enforcement in Greece from 1998 to 2002. The indicator showing the largest increase in enforcement was the number of drink-drive controls, which probably refers to the number of drivers who were tested for alcohol. This number increased (uniformly) from 202,161 in 1998 to 1,034,502 in 2002. In the models, Yannis et al. used the number of controls in thousands. Model 12 was chosen for representing the results of the study in this paper. Model 12 was a multilevel, multivariate extra-Poisson model. The term extra-Poisson indicates that the model allowed for overdispersion of the residual terms. To obtain the value of 0.654 in Table 3, the coefficient of −0.025 for injury accidents presented in Table 8 of the paper by Yannis et al. was used. At the lowest level of enforcement, $e(-0.025 \bullet (202/49)) = 0.902$ accidents were predicted. The term (202/49) is the lowest number of controls in thousands (2 0 2) divided by the number of counties in Greece (49). At the highest level of enforcement, the predicted number of accidents was: $e(-0.025\ (1034/49)) = 0.590$. The ratio of these numbers $= 0.590/0.902 = 0.654$, indicates the reduction in the number of injury accidents associated with increasing enforcement from its lowest level to the highest level (i.e. by a factor of 5.117).

Cameron et al. (2016) estimated elasticities to describe how changes in enforcement were related to changes in the number of accidents. An elasticity is a power function: Change in dependent variable = (Change in independent variable)$^{\text{Power}}$. Several elasticities were estimated. The first, −0.0461, refers to speed enforcement and is a weighted average based on several studies. It is not stated how much enforcement changed, but a report by Diamantopoulou and Cameron (2002) shows that the number of police hours doubled. The next three estimates refer to alcohol enforcement. Again, no data are given on the changes in enforcement, but a report by Newstead et al. (1998) indicates that random breath tests per month in Melbourne increased from about 40,000 per month in 1990 to about 100,000 per month in 1996, i.e. an increase by a factor of 2.5. Thus, for killed drivers, the accident modification function was: $2.5^{-0.115} = 0.900$. The final estimate extracted from Cameron et al. (2016) refers to drug enforcement. Enforcement was slightly more than doubled, as indicated by the number of roadside oral fluid tests. The number of killed drivers was reduced by 9.5 %.

Feng et al. (2020) fitted a structural vector autoregressive model (a form of time-series model) to data for Shanghai, China, in order to estimate the effects of police enforcement. Presumably enforcement was targeted at all types of violations. Fig. 2(B) in the paper by Feng et al. (2020) suggests that enforcement, stated as police patrol time, varied by a factor of roughly 2.33. It is stated that the number of accidents was reduced by 0.15 % when patrol time increased by 1 %. This suggests an accident modification factor of $2.333^{-0.15} = 0.881$ (12 % accident reduction).

Elvik et al. (2022) studied police checks of drivers of heavy goods vehicles in Norway. The amount of enforcement was stated in terms of the number of drivers checked per million kilometres driven per year, which varied between 5.267 and 22.624 (a factor of 4.295). Negative binomial regression was used to estimate the effects of police checks. A coefficient of −0.008 was estimated. Hence, at for the lowest number of drivers checked, the predicted number of accidents was $e(-0.008 \bullet 5.267) = 0.959$. For the highest number of drivers checked, the predicted number of accidents was $e(-0.008 \bullet 22.624) = 0.834$. The ratio of these number ($0.834/0.959 = 0.870$) shows the accident modification factors associated with increasing enforcement by a factor of 4.3.

Elvik (2023) evaluated the effects on accidents of technical inspections of heavy goods vehicles in Norway. These inspections are not performed by the police but should nevertheless be regarded as part of traffic law enforcement, since technical defects on heavy goods vehicles are known to be quite common and contribute to an increased risk of accident. Negative binomial regression models were fitted to data for two periods (1985–1997 and 2008–2020). The coefficient for technical inspections was negative for both periods. Accident modification factors were obtained, as already shown for other studies, by taking the exponential function of the coefficient multiplied by the highest and lowest value for the enforcement variable.

Elvik and Nævestad (2023) evaluated whether the Safe System approach to road safety policy is associated with an improved road safety performance. Negative binomial regression models were developed. These models included enforcement, stated as the number of citations per million vehicle kilometres of travel. The coefficient was negative both for killed road users and for killed or seriously injured road users. Applying it, suggested an accident modification factor of 0.872 for fatalities and 0.873 for killed or seriously injured road users associated with an increase in enforcement by a factor of 1.67.

Finally, Elvik developed a model for Sweden specifically for this paper, based on data for 1981–2004. These data were collected in a previous study (Elvik et al., 2009). An inquiry about more recent data, in particular for the enforcement variable, did not produce more recent data. The enforcement variable is the number of drivers checked by the police per million kilometres of travel. As shown in Table 3, the coefficient for enforcement was negative both for killed road users and for killed or seriously injured road users. It implied accident modification factors of 0.742 for killed road users and 0.779 for killed or seriously injured road users, associated with an increase in enforcement by a factor of 3.042.

All regression coefficients referring to variables indicating the amount of enforcement are negative. This means that all studies have found that an increased amount of enforcement is associated with a reduced number of accidents or injured road users. The size of the estimated reduction in the number of accidents or injured road users associated with increased enforcement varies between less than 5 % and more than 30 %. One would expect to find a systematic variation of effect. More specifically:

1. There should, for a given increase in enforcement, be a larger percentage reduction of fatal accidents or killed road users than of less serious accidents or injuries. The reason for expecting this, is that traffic law violations have been found to contribute more to the risk of fatal accidents or injuries than to less severe accidents or injuries (Elvik, 2011B). When the results for which a statistical weight can be computed (all except Feng et al., 2020) are combined, the weighted (fixed-effects model) mean accident modification factor was 0.857 (14 % accident reduction) for killed road users and 0.917 (8 % accident reduction) for injured road users.
2. A large increase in enforcement, or a wide range between the highest and lowest levels observed in a study, should be associated with a larger reduction of the number of accidents or injured road users than a small increase in enforcement or a narrow range of levels.

Fig. 2 shows the relationship between the range of enforcement and the value of the accident modification factor.

The data points in Fig. 2 are unweighted. There is a tendency for the accident modification factor to become lower (i.e. show a larger accident reduction) as there is a larger increase in the amount of enforcement. However, the data points are quite widely spread around the trend line fitted to them. A similar relationship was not found when results for killed road users were plotted separately. However, there was only five results for killed road users.

As far as targets for enforcement are concerned, the only clear finding was that a larger effect on accidents was associated with enforcement targeted at all types of violations than enforcement with more specific targets, like speeding or drink-driving.

These comparisons are not as rigorous as one would want them to be. The regression coefficients listed in Table 3 are not capable of describing the effects of changes in the level of enforcement with sufficient detail and precision. To understand why, it is useful to examine the data shown in Fig. 1 once more. These data show a cyclical pattern. There are periods of increasing enforcement, followed by periods of declining enforcement. The effects of enforcement are usually assumed to be instantaneous (Bjørnskau and Elvik, 1992), although Vaa (1997) found a time-halo effect up to eight weeks after a period of increased enforcement. However, the halo effect gradually diminished during these eight weeks and was zero at the end of the period. When a year is used as the unit of observation, no great error is made by assuming instantaneous effects of enforcement (eight weeks is just 15 % of a year).

One would expect the annual variations in the amount of enforcement seen in Fig. 1 to be associated with corresponding annual variations in the number of accidents or injured road users. In years when enforcement increased, there ought to be a larger decline in the number of accidents or injured road users than in years when enforcement did not increase. In years when enforcement was reduced, one would expect the number of accidents or injured road users to increase, or to decline at a slower rate than if enforcement had not been reduced. A regression coefficient cannot capture this pattern of annual changes. However, an analysis employing differences-in-differences estimates of effect can capture such annual fluctuations.

4. Differences-in-differences estimates of accident modification functions

Differences-in-differences estimates of effect have not been widely applied in road safety research. The logic of a differences-in-differences research design is identical to a before-and-after study with a comparison group. The latter design is well-known and widely applied in road safety evaluation. The usual estimator of effect in before-and-after studies with a comparison group is the odds ratio:

$$\text{Odds ratio estimate of effect} = \frac{\left(\frac{\textit{Number of accidents in treated group after}}{\textit{Number of accidents in treated group before}}\right)}{\left(\frac{\textit{Number of accidents in comparison group after}}{\textit{Number of accidents in comparison group before}}\right)}$$

Differences-in-differences estimates of effect are based on differences, rather than ratios (Buckley and Shang, 2002; Angrist and Pischke, 2008). However, differences-in-differences estimates can be converted to relative changes, akin to odds ratios.

To develop differences-in-differences estimates of the effects of changes in enforcement, one starts by fitting accident prediction models like those reviewed in section 3. These models usually contain several variables, not just enforcement. The key question in any evaluation study is: did the measure produce changes that would otherwise not have happened? More specifically: did changes in enforcement from one year to the next produce changes in the number of killed or injured road users that would not have occurred if there had been no change in enforcement? To answer questions of this kind, the counterfactual, i.e. what would otherwise have happened must be established. In before-and-after studies with a comparison group, the comparison group serves this function. How can the counterfactual be established in a multivariate accident prediction model?

The simplest way of doing it, is to estimate the model-predicted number of killed or injured road users by applying a model in which the variable whose effects a study aims to evaluate is omitted, while all other coefficients of the full model are retained with unchanged values.

As an example, applying the model for killed or seriously injured road users for 1980–2021 developed by Elvik and Nævestad (table 5 of their paper), 2408.06 killed or seriously injured road users was predicted for 1980. For 1981, 2438.14 killed or seriously injured road users was predicted. These predicted values included the effects of all independent variables, including citations per million vehicle kilometres of driving. When the citations variable is omitted but all other variables included with the same coefficient values, the predicted numbers become 3065.47 for 1980 and 3059.05 for 1981. These predicted values reflect the influence of all independent variables except for enforcement. These values can be interpreted as those that might have occurred had there been no change in enforcement from 1980 to 1981.

The difference between 1981 and 1980 when all variables are included is 2438.14 – 2408.06 = 30.08. The difference between 1981 and 1980 when the enforcement variable is omitted is 3059.05 – 3065.47 = −6.42. The differences-in-differences is 30.08 – (−6.42) = 36.50. This is the net contribution to the change in the number of killed or seriously injured road users from changes in enforcement from 1980 to 1981. The relative change in the number of killed or seriously injured road users attributable to change in enforcement is (2438.14 + 36.50)/2438.14 = 1.015. Similarly, enforcement changed from 5.37 citations per million vehicle kilometres in 1980 to 5.05 citations per million vehicle kilometres in 1981, a relative change of 5.05/5.37 = 0.940.

By developing similar estimates year-by-year two time series are generated: one shows annual changes in enforcement, the other shows

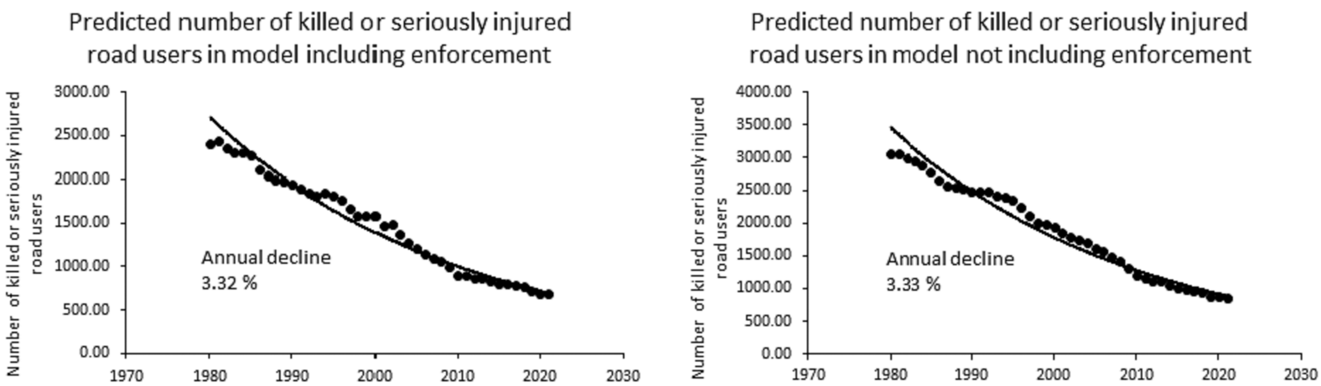

Fig. 3. Actual and counterfactual development of killed and seriously injured road users in Norway 1980–2021.

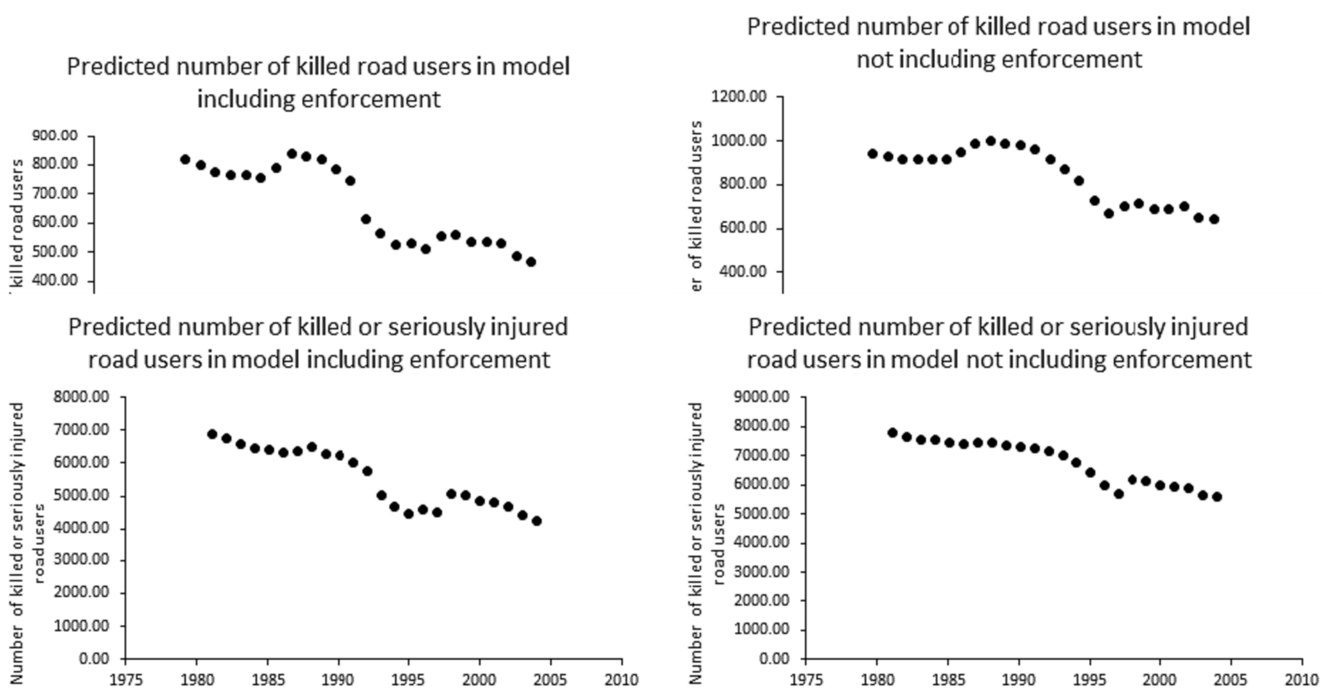

Fig. 4. Actual and counterfactual development of killed and of killed and seriously injured road users in Sweden 1981–2004.

the estimated contributions to the changes in the number of killed or injured road users attributable to changes in enforcement as estimated by the differences-in-differences estimator. For these estimates to be valid an important assumption must be fulfilled. This assumption is that the changes over time when the treatment variable (in this case enforcement) is included should be the same as the counterfactual changes over time when the treatment variable is omitted. The validity of this assumption is tested for Norway in Fig. 3.

The similarity of the time series is striking. The curves look almost identical. One has an annual mean decline of 3.32 %, the other an annual mean decline of 3.33 %. Clearly, the assumption that the counterfactual changes over time (right figure) should be the same as the actual changes (left figure) is valid in this case. Fig. 4 reports similar tests for Sweden. The upper half show curves for killed road users, the lower half shows curves for killed or seriously injured road users.

The curves for killed road users are quite similar. These curves are not as similar as the two curves for Norway shown in Fig. 3, but nevertheless sufficiently similar that one may conclude that actual (left) and counterfactual (right) changes over time are, if not perfectly identical, then at least highly similar. The curves for killed or seriously injured road users are also similar, but not as similar as the curves for killed road users. However, both curves show a slow decline in the number of killed or seriously injured road users in the beginning, accelerating from about 1990 to 1997, followed by an increase from 1997 to 1998 and a decline in the years after. On the whole, the assumption of similar changes over time in the actual and counterfactual time series is judged to be fulfilled.

Table 4 shows differences-in-differences estimates of the effects on the number of killed or seriously injured road users of annual changes in the amount of enforcement in Sweden from 1981 to 2004. The two columns to the right in Table 4 have been plotted in Fig. 5.

Fig. 5 shows the relationship between annual changes in enforcement and annual changes in the number of killed or seriously injured road users in Sweden from 1981 to 2004. A function has been fitted to the data. It fits the data points quite well.

There are 23 data points. 21 data points show either an increase in the number of killed or seriously injured road users when enforcement is reduced or a decrease in the number of killed or seriously injured road users when enforcement is increased. This pattern can only be found by using a differences-in-differences estimator and does not emerge when the effects of enforcement are shown simply by a regression coefficient. If the regression coefficient is negative, applying it will indicate a

Table 4
Developing differences-in-differences estimates of effect on killed road users of annual changes in enforcement in Sweden 1981–2004.

	Model-predicted number of fatalities in full model and counterfactual model and annual differences						
Year	**Full model**	**Counterfactual model**	**Differences (full model)**	**Differences (counterfactual model)**	**Differences-in-differences**	**Relative change in enforcement**	**Relative change in fatalities**
1981	819.66	948.20					
1982	800.03	932.03	−19.62	−16.17	−3.45	1.048	0.996
1983	777.97	918.39	−22.06	−13.64	−8.42	1.087	0.989
1984	766.48	919.32	−11.50	0.92	−12.42	1.096	0.984
1985	768.00	916.24	1.52	−3.08	4.60	0.971	1.006
1986	758.41	917.80	−9.59	1.56	−11.15	1.081	0.985
1987	792.84	953.82	34.43	36.03	−1.59	0.969	0.998
1988	838.85	988.71	46.01	34.89	11.12	0.889	1.013
1989	830.05	1002.00	−8.80	13.29	−22.10	1.145	0.973
1990	819.41	990.58	−10.63	−11.43	0.79	1.008	1.001
1991	786.31	987.23	−33.11	−3.34	−29.76	1.200	0.962
1992	747.18	967.03	−39.12	−20.21	−18.91	1.133	0.975
1993	615.98	917.72	−131.20	−49.31	−81.89	1.546	0.867
1994	567.95	876.49	−48.03	−41.22	−6.81	1.088	0.988
1995	527.68	822.05	−40.27	−54.44	14.17	1.022	1.027
1996	533.85	733.02	6.17	−89.03	95.20	0.715	1.178
1997	513.22	674.07	−20.63	−58.95	38.33	0.860	1.075
1998	555.79	705.77	42.57	31.71	10.86	0.876	1.020
1999	561.50	715.76	5.71	9.99	−4.28	1.016	0.992
2000	540.36	693.18	−21.14	−22.58	1.44	1.026	1.003
2001	536.57	690.89	−3.78	−2.29	−1.49	1.015	0.997
2002	535.29	704.38	−1.28	13.49	−14.77	1.086	0.972
2003	489.70	655.11	−45.60	−49.27	3.67	1.060	1.007
2004	467.81	647.90	−21.89	−7.22	−14.67	1.119	0.969

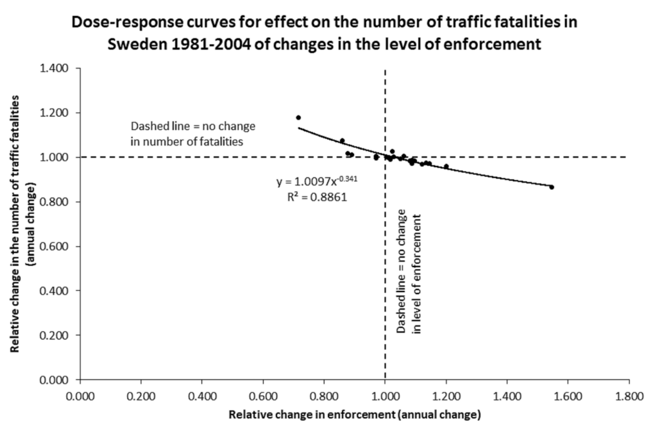

Fig. 5. Differences-in-differences estimates of effects of changes in enforcement and function fitted to the data points.

reduction of the number of accidents or injured road users at any level of enforcement except zero.

5. Comparing differences-in-differences estimates to regression coefficients

Differences-in-differences estimates have been developed in the studies of Elvik, Pasnin and Nævestad (2022), Elvik (2023), Elvik and Nævestad (2023) and the data for Sweden analysed by Elvik for this paper. In each of the studies, scatter plots of the results were produced, and a function fitted to the data points, as shown in Fig. 5 for killed or seriously injured road users in Sweden during 1981–2004. Table 5 compares these estimates to those based on regression coefficients.

The column for largest decrease in enforcement shows that largest reduction of enforcement from one year to the next during the period covered by the data. A value of, for example, 0.85 indicates at 15 % reduction. With respect to increase in enforcement, a value of 1.27 indicates an increase of 27 %. The differences-in-differences estimates of effect are based on such annual changes. The next column shows the form of the function that fitted best to the differences-in-differences data points. A logarithmic function fitted best in three cases, a power function in four cases. The next two columns show parameter estimates for the functions fitted to the differences-in-differences data. In these data, no change in enforcement has the value 1.00 and no change in accidents or injured road users has the value of 1.00. Ideally speaking, a function should pass through the (1, 1) data point and therefore have a constant term of 1.00. The slope term should be negative. As can be seen from Table 5, all functions are consistent with these predictions.

The final two columns of Table 5 compare accident modification functions estimated by relying on the regression coefficients to the functions fitted to the differences-in-differences data points. Using the study by Elvik, Pasnin and Nævestad as example, the accident modification factor based on the regression coefficient was obtained as follows:

The predicted number of accidents for the highest level of enforcement was $e(-0.008 \bullet 22.624) = 0.834$. The predicted number of accidents for lowest level of enforcement was $e(-0.008 \bullet 5.267) = 0.959$. The ratio of these number $(0.834/0.959) = 0.870$ is the accident modification factor associated with the range of levels of enforcement found in this data set.

For the differences-in-differences estimates, the accident modification factor was found as follows: The largest annual increase in enforcement was by a factor of 2.49. The change in the number of accidents associated with this change was: $1.006 + (-0.100 \bullet \ln(2.49)) = 0.915$. The largest annual decrease in enforcement was 0.61. The accident modification factor associated with this was

$1.006 + (-0.100 \bullet \ln(0.61)) = 1.056$. The ratio of these numbers $(0.915/1.056) = 0.867$ is the accident modification factor. All other accident modification factors were developed the same way. In general, the accident modification factors based on the regression coefficients are close to those based on functions fitted to differences-in-differences data.

6. Discussion

It is fruitful to base the evaluation of the effects of traffic law enforcement on road user behaviour and on accidents on the game-theoretic model described in section 2. This model makes several predictions that can be tested empirically:

Table 5
Comparing estimates of effect based on regression coefficients and differences-in-differences.

Study	Dependent variable	Largest relative decrease of enforcement from one year to next	Largest relative increase of enforcement from one year to next	Form of function fitted to differences-in-differences data points	Constant term (standard error)	Slope term (standard error)	Accident modification factor based on model	Accident modification factor based on differences-in-differences
Elvik et al., 2022	Injury accidents	0.61	2.49	Logarithmic	1.006 (0.005)	−0.100 (0.014)	0.870	0.867
Elvik, 2023 (1985–1997)	Injury accidents	0.32	1.45	Logarithmic	1.000 (0.006)	−0.071 (0.015)	0.901	0.901
Elvik, 2023 (2008–2020)	Injury accidents	0.83	1.21	Logarithmic	1.009 (0.004)	−0.306 (0.032)	0.905	0.892
Elvik, Nævestad 1980–2021	Fatalities	0.85	1.27	Power	1.010 (0.003)	−0.255 (0.036)	0.872	0.903
Elvik Nævestad 1980–2021	Fatal and serious injury	0.85	1.27	Power	1.009 (0.001)	−0.249 (0.014)	0.873	0.905
Elvik Sweden 1981–2004	Fatalities	0.72	1.55	Power	1.010 (0.004)	−0.341 (0.027)	0.742	0.770
Elvik Sweden 1981–2004	Fatal and serious injury	0.72	1.55	Power	1.006 (0.003)	−0.275 (0.019)	0.779	0.810

1. Effects on road user behaviour or accidents are generated by changes in the amount of enforcement
2. A stable (unchanged) level of enforcement maintains a stable number of accidents (or a stable long-term trend).
3. An increase in enforcement will be associated with a reduction of traffic violations and a reduction of the number of accidents.
4. The larger the increase in enforcement, the larger the reduction of violations and accidents.
5. A decrease in enforcement will be associated with an increase in traffic violations and the number of accidents.
6. The larger the decrease in enforcement, the larger the increase in violations and accidents.
7. The amount of enforcement will tend to fluctuate over time, as road users respond to increased enforcement by reducing violations, and the police respond to a reduced rate of violations by reducing enforcement.

In order to test these predictions empirically, it is useful to develop differences-in-differences estimates of effect based on annual changes in enforcement and annual changes in the number accidents or killed or injured road users. Both the direction and size of changes can then be determined.

If most data points are consistent with the predictions of the game-theoretic model, this indicates that the data points mainly reflect the effect of changes in enforcement. If, on the other hand, the data points are widely scattered and/or a high proportion of them are inconsistent with the predictions of the game-theoretic model, this indicates either that this model is wrong or that there are confounding factors not sufficiently well-controlled for by the analysis.

7. Conclusions

The main conclusions of the analyses presented in this paper are as follows:

1. All accident modification functions developed for traffic law enforcement have found that increased enforcement is associated with a reduction of the number of accidents or injured road users.
2. A large increase in enforcement is associated with a larger change in the number of accidents or injured road users than a small increase in enforcement.
3. A given change in the amount of enforcement is associated with a larger change in fatal injuries than in less serious injuries.
4. Enforcement targeted at all types of violations is associated with larger changes in the number of accidents or injured road users than enforcement with more specific targets (e.g. speed)
5. By developing differences-in-differences estimates of effect, it is possible to evaluate both decreases and increases of enforcement in the same study. A decrease in enforcement is associated with an increase in the number of accidents or injured road users.

CRediT authorship contribution statement

Rune Elvik: Conceptualization, Data curation, Formal analysis, Writing – original draft, Writing – review & editing.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Data availability

Data will be made available on request.

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