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The effects of Electronic Stability Control (ESC) on fatal crash rates in the United States

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ABSTRACT

Problem: Electronic Stability Control (ESC) is believed to be among the most efficient vehicle safety interventions with reported effects around 50% for fatal single and rollover crashes. However, such estimates have used sample data, which have not controlled for the possibilities of self-selection, behavioral adaptation, increased access to the technology by less safe drivers, and the calculation of effects on very specific categories of crashes. Effects of ESC in the population can therefore be expected to be smaller than is currently believed. *Method:* National U.S. data for fatal crashes, driving exposure and other control factors, and market penetration of ESC over 1991–2021 were used to calculate whether the trends in fatalities over time in crash rates for singles, rollovers, and fatal crashes in general matched projections from estimates of effectiveness. *Results:* It was found that downward trends in the relevant crash types were generally present before ESC was introduced, and that the trends thereafter were weaker. Although some trends were consistent with effects of ESC, they were markedly smaller than the projected ones, and could be explained by other factors such as the number of vehicles per capita. At best, the effect for rollovers could be up to two-thirds of previous estimates, no effect was detected for singles, while for all fatal crashes results depended upon the type of analysis performed. These results conflict with conclusions in all published ESC crash sample studies, which have compared vehicles with and without ESC. This discrepancy can be explained by methodological errors in the previous studies using induced exposure methods and self-selected samples. *Practical applications:* Traffic safety may not be as much improved by technological interventions as believed. Alternative approaches to traffic safety are needed, which do not rely on technology that interferes with driver behavior.

1. Introduction

1.1. Electronic Stability Control and traffic safety

Vehicle manufacturers have been striving to increase the safety of their products perhaps since the car was invented, and some inventions have seen some successes, like the seatbelt. In most countries, a strong decline in the number of crashes (especially fatalities) have resulted from this work and other interventions (e.g., emergency medicine, laws, policing, driver education, better roads, airbags, and deformation zones), although the exact contribution of different interventions and other factors are difficult to estimate.

Over time, the complexity of vehicle safety systems has increased exponentially and is currently at the level of automated features, which change the input of the driver in various ways, or rarely even need any human input at all. These systems are expected to increase traffic safety very strongly (Ashley, 2008; NHTSA, 2016; NTC, 2018), just as their somewhat simpler predecessors were expected to do. The question can be raised, however, have previous vehicle safety systems delivered the expected safety benefits? Is there reason to believe that forecasts of traffic safety benefits are flawed, and strongly over-estimate the possible benefits?

Forecasted benefits of vehicle safety systems are based on a few different methods that try to estimate safety effects before national crash data are available. Early forecasts mainly use crash analysis (e.g., which crashes could potentially be prevented), but also on-road driver behavior and simulator studies. It is only after the safety systems have achieved a certain level of market penetration that it is possible to analyze whether there is indeed an effect on crashes. This type of analysis is most often undertaken by comparing vehicles of the same model before and after a safety feature is added (hereafter called crash sample effect studies). When exposure data are not available (which is

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usually the case for passenger vehicles), some sort of induced exposure technique is applied to hold differences in exposure constant (Haight, 1973; Lyles, Stamatiadis, & Lighthizer, 1991; af Wåhlberg & Dorn, submitted for publication).

The remainder of this study will investigate Electronic Stability Control (ESC) as a special case of a vehicle safety technology that is predicted and claimed to lead to large traffic safety gains (e.g., NHTSA, 2011; 2017). If it can be concluded that various forecasts of ESC effects have indeed been overly optimistic, it could be suspected that effects of other vehicle safety features may have also been over-estimated.

ESC crash sample studies have shown very strong effects on some types of crashes (see meta-analyses by Erke, 2008; Høye, 2011; af Wåhlberg & Dorn, submitted for publication). Many traffic safety stakeholders have concluded from this evidence that there are huge safety benefits taking place at the national level (e.g., Goldsmith et al., 2017). However, there are several reasons to question the belief that ESC delivers about a 40–50% reduction for crash types such as singles and rollovers, reasons which will be described in the following sections.

1.2. Self-selection of samples

The standard method for estimating the effects of ESC on crash involvement in previous studies is to compare vehicles with and vehicles without ESC (see the meta-analyses by Erke, 2008; af Wåhlberg & Dorn, submitted for publication). However, in a population of private drivers, this method cannot control for self-selection (also called adverse recruitment; Harless & Hoffer, 2003) of drivers beyond statistical adjustment for proxy variables such as sex and age. It is suggested here that drivers who are more safety-conscious will tend to buy cars with more safety features (Koppel, Clark, Hoareau, Charlton, & Newstead, 2013; Koppel, Charlton, Fildes, & Fitzharris, 2008), and, as they are probably safer drivers than others, inflate the possible effect of ESC. Such a mechanism could probably also explain results such as those of Williams and O'Neill (1974), who found that racing drivers (with presumably high driving skills) had more crashes and violations on record than matched drivers. Furthermore, as income is inversely related to rates of traffic deaths (Zlatoper, 1991), it would seem to be reasonable to assume that people at the high income end buy more expensive alternatives (i.e., vehicles with more features). Self-selection effects have also been found in other areas of research (e.g., Bornehag, Sundell, Sigsgaard, & Janson, 2004; Jonas-van Dijk, Zebel, Claessen, & Nelen, 2020).

On the other hand, it has recently been reported by Vertlib, Rosenzweig, Rubin, and Steren (2023) that drivers who buy ADAS systems that alert the driver to danger, tend to receive more speeding tickets. Although the authors interpreted this as due to behavioral adaptation (see next section), it could also be due to adverse self-selection (i.e., worse drivers buying these systems). Such an interpretation could seem to be contrary to the present proposition, but as ESC actively interferes with driving (while the systems studied by Vertlib et al. do not), this does not have to be a contradiction.

Furthermore, it is possible that drivers buying new cars, with or without technological safety features, are different from those who do not. When second-hand cars with ESC (and other systems) reach this different market segment, it is possible that these drivers do not react to these features as the early adopters do. Therefore, even if there was a substantial safety effect of ESC for the early adopters, this might be much smaller for other groups. The mechanism involved could, for example, be that of behavioral adaptation, as described in the next section.

1.3. Behavioral adaptation

Within traffic safety research, the problem of unintended reactions to safety interventions is well known (e.g., Evans, 1985; Hedlund, 2000; Smiley & Rudin-Brown, 2020; Vrolix, 2006), although they are difficult to predict. What is known is that many safety interventions have turned out not to increase safety at all, apparently due to changes in behavior, which counteracts the effect of the intervention. Reports of such behavioral adaptations (also called risk compensation, risk homeostasis, moral hazard, compensating behavior, danger compensation, utility maximizing, offsetting behavior, etc.) include studded tires (Rumar, Berggrund, Jernberg, & Ytterbom, 1976), seatbelts (Janssen, 1994), adaptive cruise control (Hoedemaeker & Brookhuis, 1998), pedestrian rules (Thulin, 2007), automated vehicles (Soni, Reddy, Tsapi, van Arem, & Farah, 2022) and many others (see Vrolix, 2006, for a review). Sometimes such effects have been reported for driver behavior, sometimes for crash involvement, and it should be noted that these do not necessarily need to be the same thing. Similarly, effects reported using simulator-based studies need not transfer into real driving behavior. Finally, it can be noted that not all researchers agree about the evidence concerning behavioral adaptation (e.g., Radun, Radun, Esmaeilikia, & Lajunen, 2018).

Theoretically, there is a plethora of models to choose between (Rudin-Brown & Jamson, 2013; Vrolix, 2006). However, none of them would seem to offer any exact guidance on how long it would take for behavioral adaptation to appear. The Visibility Rule of Hedlund (2000) would seem to include not only how apparent the change/feature is, but also how often it is encountered. However, even if this principle does indicate that safety features that are seldom noticed will result in less behavioral adaptation, this does not transfer into any explicit frequencies or time periods, and not even whether the effect will actually be smaller, or just more delayed. Still, this is the most relevant theoretical statement found for the present study.

The problem with behavioral adaptation has apparently not been studied concerning ESC. This contrasts with its precursor anti-lock braking (ABS; Vaa, Penttinen & Spyropoulou, 2007), where several papers reported negative changes in driver behavior when they started using ABS (Aschenbrenner & Biehl, 1994; Sagberg, Fosser & Sätermo, 1997), and probably a net null effect on crashes (Evans, 1995; Evans & Gerrish, 1996; Kahane, 1994).

ESC is a technology which, like ABS, improves the handling qualities of vehicles. However, it is also a function that is rarely noticed by most drivers, because it only interferes under very special circumstances. This probably means that if driver behavior changes due to ESC, it will be a gradual shift, probably happening over several years. Unfortunately, empirical research on long-term driver adaptation to vehicle features is rare (Saad, 2004), but a shift in driving behavior in response to perceived vehicle features could explain phenomena such as increased safety with increased size of trucks over many years (af Wåhlberg, 2008).

From available evidence and theory, it could therefore be expected that over a longer timeframe (probably years) drivers get used to the ESC system and the increased stability it offers, and take less care in their driving, especially in challenging conditions. This could lead to shorter headways, higher speeds, and stronger braking, and therefore crashes.

1.4. Creating the effect

An unusual aspect of ESC research is the concept of target and non-target crashes, where the former is expected to be influenced by the technology, while the others are believed not to be. First, researchers have failed to agree on what these categories consist of in terms of crash characteristics (af Wåhlberg & Dorn, submitted for publication). Second, researchers sometimes create very specific crash sub-categories, and when these are investigated, they might yield very high effects. However, as the percent of crashes affected is usually very small, this does not translate into a large effect for crashes in general. Unfortunately, this discrepancy is often ignored when effects are interpreted.

In a meta-analysis of ESC effects (af Wåhlberg & Dorn, submitted for publication), an attempt to investigate this kind of effect was undertaken, although sample size is a crude measure of the proposed mechanism (information about sub-sample sizes is often missing for such calculations). It was found that sometimes effect sizes were smaller when the sample sizes were larger. However, this effect could also be due to dissemination bias, which would also inflate the effects. However, the findings were not wholly systematic, and difficult to interpret, due to the heterogeneity of the data. Further analysis to consider how restrictive the definition of a crash was versus the ESC effect would be required to understand whether this mechanism has been at play. For the present study, the mechanism just described is a further reason to predict that effects of ESC in national data will be smaller than in samples, however large.

1.5. Investigating ESC effects in national crash data

All previous attempts to estimate effects of ESC have used crash analysis, crash samples, surveys, and other methods that may not be fully representative of the driving population, and, as described for the crash sample method, seem to have serious shortcomings. No study has been sourced that uses national crash data to investigate effects of ESC, probably because this has not really been possible up until now, because market penetration has been too low to make it possible to detect any effect. However, in the United States, there was not only more than 50% market penetration of ESC for passenger vehicles by 2020, but also excellent crash databases and other transport-related information that can be used to investigate whether ESC is having the expected effect.

When investigating the effects of ESC, it is important to consider exactly what ESC is expected to achieve, and what cannot be expected. Most researchers who have published on ESC would seem to agree that the primary types of crashes to be influenced are singles, rollovers, and running-off-road (Erke, 2008; af Wåhlberg & Dorn, submitted for publication). These types would seem to have quite some overlap, as a single crash could probably feature all these characteristics. Some researchers have also included multi-vehicle crashes in their studies, but effects have been very much smaller than for singles, rollovers, and running-off-road (af Wåhlberg & Dorn, submitted for publication). Changes in rear-end crashes are usually assumed to be caused by other factors than those targeted by ESC. However, it should be remembered that some studies have reported slight increases in rear-ended crashes and crashes involving pedestrians and animals (Høye, 2011). Effects have in general been slightly larger for fatal as compared to injury crashes (af Wåhlberg & Dorn, submitted for publication).

Furthermore, it is important to ponder exactly how an effect of ESC would show up in national crash data over a period of time in which there is increased market penetration of ESC. The raw, absolute numbers per year would be affected by many factors that can be grouped into two main factors: amount and quality of exposure. The amount would include the number of vehicles and drivers on the roads, and mileage, while the quality would be where and how vehicles are being driven. For example, an increase in sheer numbers of licensed drivers and registered vehicles on the roads would be a quantitative shift, while a qualitative shift could be different drivers being on the road in response to shifts in the economy (Maheshri & Winston, 2016). Quantity of exposure can be controlled for in several different ways, while quality is more difficult, as data on such factors are usually not available. However, there are some possible proxy variables that have been found to strongly influence the number of fatal crashes in the United States, although the reasons why this happens may still be somewhat obscure.

When testing for the impact of ESC in national crash data, it is necessary to control for other factors, as there is already a long-term downward trend in crash data in most developed countries, which could otherwise be confused with a specific intervention effect (Dee, Grabowski, & Morrisey, 2005; Fell, Fisher, Voas, Blackman, & Tippetts, 2009; Fell, Jones, Romano, & Voas, 2011; Lim & Chi, 2013). Some of these factors will be reviewed here, with an emphasis on the United States. First, and possibly most important among predictors of crash numbers in a nation are the numbers of registered vehicles and population (which is called Smeed's law). Although the original formulation (Smeed, 1949) has been shown not to fit modern data very well (Borsos, Koren, Ivan, & Ravishanker, 2012), these parameters should still be considered in an analysis such as that attempted here.

Yet another predictor of crash numbers is the economic development of countries (Maheshri & Winston, 2016), for example industrial production (Joksch, 1984). This association has been found to be non-linear, with an initial positive correlation turning to negative at a point of economic development that the United States passed in the early 1970s (van Beeck, Borsboom & Mackenbach, 2000).

However, these associations are for absolute numbers, while in the present paper, the analytical approach is for rates (crashes/VMT). For such variables, less evidence is available, but it has been shown that there is a very reliable decrease in crashes over time per vehicle miles travelled (VMT), interpreted as mainly an effect of experience (Stipdonk, 2020). Similarly, the percentage of licensed drivers in the population could be expected to be negatively correlated with accident rates in the United States in the last decades, because the percentage of young drivers has decreased (Sivak & Schoettle, 2012) and VMT increased.

In general, the variables described should not be interpreted as causative, but rather as proxy predictors that are highly correlated with each other and the actual causes of changes, such as behavior, road infrastructure, and crashworthiness of vehicles. However, due to their strong associations with crashes, they can be used as controls when estimating the effects of safety interventions in national data.

1.6. Design of study

The current design took advantage of the fact that ESC has been available in the U.S. market since at least 1997 and has become increasingly common in the vehicle population over time. This means that decreases in the rates of single, rollover, and general fatal crashes could be expected at about half the rate as ESC has increased, while rear-ends would not be following this trend.

There are several ways in which exposure can be controlled for in national data, two of which will be applied in the current study; VMT and rear-end crashes. Both methods should be used, as the non-target method is more like the crash sample method, and therefore could be expected to yield results that are more similar to previous results than those of the more direct exposure control of VMT.

Several control factors were applied to control for trends in the data, and two different types of statistical analyses.

2. Method

2.1. General

To investigate whether ESC have had the expected safety effects at the national level, fatal crash data from the United States were used, as these are freely available online, of high quality, from a long time period, and very numerous. ESC market penetration data have also been sourced for light four-wheel vehicles, as well as estimates of yearly miles traveled.

The first analytic principle applied was to follow the trends of different types of crashes over time from before the introduction of ESC up to the current day and compare these to predicted effects given the market penetration levels of ESC and various estimated safety effects. Second, a multi-variate regression analysis was applied where the trend in the crash data was represented by one of the known factors for crashes at this level of aggregation. In this analysis, the logic was to test whether there was any residual variance that ESC market penetration could explain after known predictors had been entered into the regression.

2.2. ESC market penetration data

Market penetration values for ESC were gathered from HLDI Bulletin 37:11 (2020), Fig. 3. The percentages extracted from this source included only vehicles with ESC as standard equipment, while excluding those where ESC was an option. This means that prevalence of ESC in the population of vehicles was probably under-estimated by several percent of the total at any one time. The values given in the HLDI Bulletin were only for vehicles insured for personal use, and excluding motorcycles (Moore, 2023, personal communication).

2.3. Exposure and trend data

Estimates of the total number of miles traveled by all kinds of vehicles on U.S. roads were used to control for differences in risk exposure between years. As the crash data were for passenger vehicles, while the exposure data were for all vehicles, this method assumes that there is a fairly constant ratio between the total and the passenger vehicle miles variables. Induced exposure data were extracted as number of rear-end crashes per year (see the next section). This variable correlated 0.73 with VMT per year ($N = 30$) and was thus not hugely different.

Several variables were tested as controls for trends in the data; industrial production, number of licensed drivers, number of vehicles registered, and total population of the United States, all per year. See the Appendix for details.

2.4. Crash data

Crash data were retrieved from the U.S. Department of Transportation's National Highway Traffic Safety Administration's File downloads, using the Fatal Accident Reporting System (FARS) subcategory. This is further described in the Appendix.

The dependent variables in this study were all derived from fatal crashes per year in the United States from 1991 up to 2021. The starting point was chosen as yielding a sufficiently long period for the establishment of a reliable trend, but also because coding changed at that time. The numbers for crashes are slightly smaller than the total number of fatalities, but it was expected that the strongest associations would be for crashes, as the number of fatalities would also be dependent upon factors that may have nothing to do with driver behavior.

2.5. Variables; target and non-target

The aim of the crash data extract was to try to include events for which forecasts and empirical studies had previously indicated certain expected effects. However, this undertaking was not straightforward, as different authors have had somewhat different views on what this would entail (af Wåhlberg & Dorn, submitted for publication), and their methodologies have therefore differed. The main problem was to ascertain whether an expected reduction in crashes related only to the cause of a crash or if it included its outcomes. In the case of rollovers (a major target for ESC), the difference in the FARS data was large. If counted as the first harmful event (which would usually mean that it is a single crash), the numbers were about 60% less than if all rollovers were included (i.e., those that happened due to something else). For the present analysis, both codes 1 and 2 were included, as the definitions of those have changed over time, but taken together they have the same meaning.

Another problem with forecasts and empirical sample studies is that they often only discuss crashes, without stating what kind of vehicles are involved. It would often seem to be the case that even if no limitations are described, some remarks indicate that the analysis is only for light vehicles for personal use. In the present study, it was assumed that all the studies included in af Wåhlberg and Dorn (submitted for publication) were for light passenger vehicles, and that the meta-analytic estimates from that study would be applicable for this kind of vehicle.

The fatal crashes were subdivided into categories that were available in the FARS data tables, with the expectation that those with a clear theoretical connection with ESC (singles, rollovers, and all fatal crashes) would be most strongly associated with the trend in ESC market penetration (while there should be no effect for rear-ends).

The variables were thus gathered from FARS vehicle files, using selection rules described in detail in the Appendix. The rules used the principle of taking as wide a capture of events/data as possible, as definitions and codes have changed during the time period studied, and several possible sub-categories of data would only have been available for certain parts of this period. Only light vehicles were included, thus excluding vehicles weighing more than 10,000 lb.

2.6. Prediction of safety effects

The variables and ESC safety effect estimates included were chosen based upon the meta-analytic findings of af Wåhlberg and Dorn (submitted for publication), representing the most up to date estimates of ESC safety effects from crash samples. The targets were fatal single vehicle crashes (−48%), fatal rollovers (−63%), and all fatal crashes (−35%), while non-target was rear-end crashes, which were not expected to change due to ESC.

To test whether the empirical data agreed with the forecasted effects of ESC, predicted effects for each year were calculated, based upon the effects stated in the previous paragraph, the estimated market penetration of ESC in the United States over time, and a linear trend that was estimated from the years 1991–1998. The last factor thus considered other ongoing traffic safety work. The assumption of a linear trend and this specific time period was based in the work of Elvik (2010), where it was shown that for crash numbers (not rates) there were small differences between different ways of estimating trends and using different time periods for this end.

However, as the trend in data calculated in this way rests upon the assumption of linearity of development in the data and uses the same kind of data as the dependent variable, an alternative approach was also used. Different predictors of national crash numbers were harvested from the literature, as described, and national U.S. sources for such data were used to test for their associations with the trend in fatal crashes per VMT 1991–1998. The goal was to find a variable that could be used as a proxy for the trend before ESC was introduced. This could be entered into a regression analysis along with ESC market penetration, predicting the fatal crash rates of the different categories of crashes.

2.7. Analysis

The most commonly used design in ESC research is that of comparing samples of crashes of cars with and without ESC. In the present study, the numbers of crashes per VMT were compared in time series instead, where 1998 and previous years were assumed to have no ESC effect, and therefore could act as the baseline. However, this is not a static baseline, but rather a dynamic one (i.e., the trends before ESC were introduced need to be taken into account).

If ESC has an effect, the rate of target crashes should show a decline over time that is strongly associated with the increase in ESC-equipped cars. This effect need not be exactly as strong as predicted, but the decline should follow a strict pattern, even if it is small in absolute terms. Therefore, the correlations between actual and predicted rates were calculated. This association does not tell us what the size of the effect of ESC is, but it can indicate whether ESC or other factors are the stronger determinants of the crash rate.

To estimate the effect of ESC on crash rates in terms of percent reduction, on the other hand, the predicted rate can be manipulated using different percentage reductions and testing which level yields the best fit to the actual rate. This can be done by calculating the averages of each over years.

In the present analysis, the variables were all continuous, while their distributions were rather disparate (but none was even close to being Poisson-distributed or similar). A social science standard statistical approach was therefore used, applying the robust techniques of Pearson correlations and multiple regressions. These may not be optimal for all variables but do have the advantage of yielding effect sizes that are comparable to others, and possible to convert to other metrics for meta-analysis.

After testing which proxy factor had the strongest association with crashes per VMT for 1991–1998, this was entered into a forward step-wise regression analysis along with percent market penetration of ESC as predictor. This analysis tests whether ESC could explain any variation in the data beyond that of known factors, which should be the case if ESC was having a safety effect. Furthermore, the amount of explained variance, beta and B values could be used to estimate the size of this effect.

3. Results

3.1. Descriptive results

Table 1 displays the descriptive values for the variables used in the analyses. The number of cars per person in the United States has declined over the period of study, while VMT and licensed drivers per capita have increased, apart from during the periods of recession and Covid-19.

3.2. Trends in crashes per mile

Fig. 1 shows the number of fatal crashes per 10 billion miles per year. Also, the percent of light vehicles without ESC in the population is given. These variables are thus on different scales and the absolute numbers are not comparable, only the trends. However, these trends would seem to be rather different for different crash types. What is most striking is that there are strong downward trends for all fatalities and single fatal crashes even before ESC was introduced. Furthermore, there is a levelling off in these trends around 2010, and at the end of the time series, when the rate of non-ESC equipped cars is at its lowest, these crashes start to increase again.

Rollover crashes, on the other hand, would seem to have a trend that is more aligned with the increased market penetration of ESC, while rear-end crashes are remarkably stable over time, with a slight increase.

3.3. Trends in target crashes per non-target

In Fig. 2 crashes using a different exposure denominator are displayed; rear-end crashes, with the trend in ESC in the vehicle population. The crash trend shapes seem to be very different from decrease in non-ESC vehicles in the United States, although all trend in the same general downward direction.

3.4. Prediction of expected trends with mileage exposure

To calculate more precisely how well the actual trend aligned with what could be expected to happen given the effect sizes for ESC reported in previous studies, the expected number of crashes per mile were calculated. The first formula took the value of 51.0 single crashes per 10 billion miles in 1998 as the starting point, and then calculated the number for each year that would result if the risk of fatal single crashes was reduced by 48% for vehicles with ESC, as reported in af Wåhlberg and Dorn (submitted for publication). This predicted trend was thereafter compared to the actual one. It can be seen in Fig. 3 that the actual reduction is larger than expected.

However, this calculation does not consider that single crashes per VMT were in decline even before ESC was introduced. If this (assumed linear) trend (-1.45 per year) is factored in, the picture is very different. In the same figure, crashes are also predicted from the pre-1998 trend and a combination of ESC and previous trend. The previous trend yielded the best (relative) fit to the actual one (see Table 2). This result still held up if the analysis was restricted to the values for 1998–2021. From this figure it can also be seen that although there were fewer crashes per VMT than could be expected from the introduction of ESC, the previous trend predicted an even lower value. If the trend is factored in, there was no reduction in crashes that ESC could explain.

This analysis was repeated for rollovers (starting point 24.7 crashes per 10 billion miles in 1998, ESC effect 63% and previous trend −0.20) and all fatal crashes (starting point 135.9 crashes per 10 billion miles, ESC effect 35% and previous trend −1.99).

Fig. 4 shows the results for the fatal rollovers per VMT predictions. Here, the trends were more complicated, as the actual crashes per VMT first declined at a modest rate, if at all, then took a sharp dip between 2007 and 2014, after which they took an uneven course with hardly any average change at all. Still, the correlations (see Table 2) are very high. The average rate of actual crashes from 1998 onwards was almost 10% lower than predicted from the previous trend, which means that ESC could have some effect. This was calculated to be about 40% instead of the meta-analytically derived 63%.

The results for all fatal crashes in Fig. 5 are like those for singles (which are about 40% of all crashes), but with more variation in the actual crashes trend. After the steady decline in crash rate until about 2007, the dip is so strong it undershoots all the projections, after which there is a rebound that overshoots all predictions. As with singles, it was the previous trend that had the slightly better fit with the actual trend 1998–2021. With trend accounted for, there was still a discrepancy between actual and predicted trend, which is consistent with an ESC effect of about 25% instead of the meta-analytically derived 35%.

3.5. Prediction of expected trends with induced exposure

The analysis in the previous section was repeated with fatal rear-end crashes as exposure measure instead of miles traveled. For singles, the values used were 5.1 per rear-end, 48% effect and a trend of −0.186 per year, while for rollovers, these were 2.46, 63%, and −0.036, and finally all fatal crashes; 13.6, 35% and trend −0.295.

Table 6 shows that the actual trend follows the prediction from ESC very closely, but that the previous trend indicates that values should have been even lower. The relative reduction in singles versus rear-end crashes changes at the point when ESC was introduced, but in the opposite direction of what could have been expected.

Fig. 7 is more difficult to interpret, because the trend crosses the predictions in some places. However, the average value of the prediction from the trend was slightly lower than the actual, indicating that ESC could only have an effect if the trend is disregarded.

Finally, Fig. 8 can be interpreted in the same way as Fig. 6; as the prediction from trend is consistently lower than the actual trend, there is no room left for ESC to have any effect. The result can also be described as a change in trend in the wrong direction when ESC was introduced.

Table 1
Descriptive statistics for numbers of crashes per VMT and ESC market penetration for the US population of passenger cars. Also,

Variable	Mean/std	Max/min	Skewness/kurtosis
Number of fatal crashes per ten billion miles traveled 1991–2021	116.0/21.7	157.2/87.2	0.15/−1.35
Number of fatal single crashes per ten billion miles traveled 1991–2021	45.1/7.9	63.6/35.2	0.42/−0.78
Number of fatal rollover crashes per ten billion miles traveled 1991–2021	21.2/4.3	27.9/13.8	−0.31/−1.57
Number of fatal rear-end crashes per ten billion miles traveled 1991–2021	9.5/1.1	12.7/7.4	0.41/2.00
Percent ESC in passenger vehicles 1998–2021	23.2/23.7	70.0/0	0.71/−0.97
Number of passenger vehicles per capita 1991–2020	0.438/0.070	0.561/0.317	−0.23/−1.04
Percent licensed drivers per capita 1991–2020	67.7/0.93	69.6/66.3	0.50/−0.36

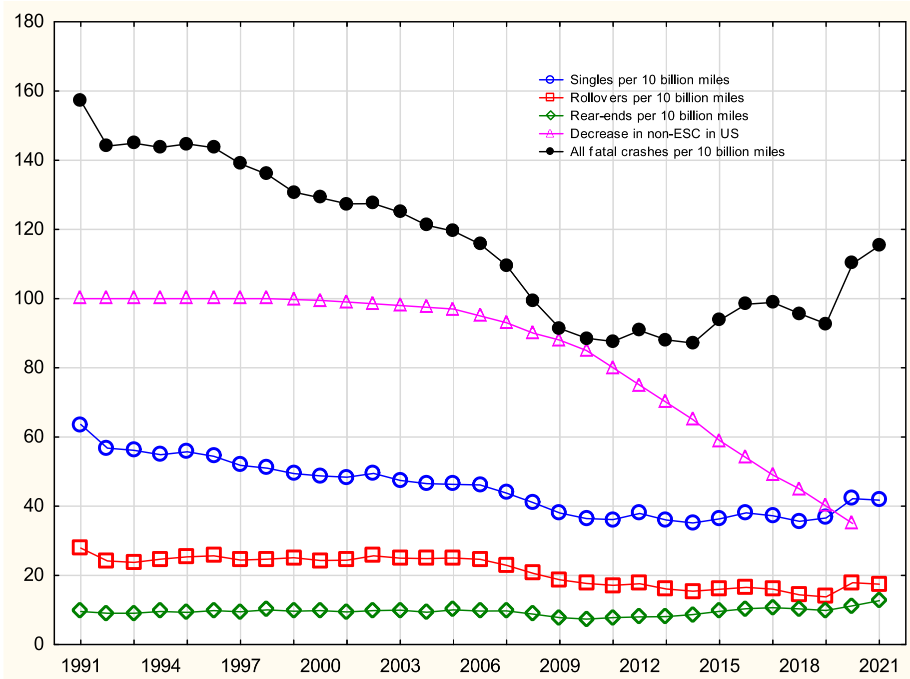

Fig. 1. Single, rollover and rear-end crashes in FARS data 1991–2021 per 10 billion miles traveled. On a different scale (percent) is shown the percent light passenger vehicles without ESC in the US population.

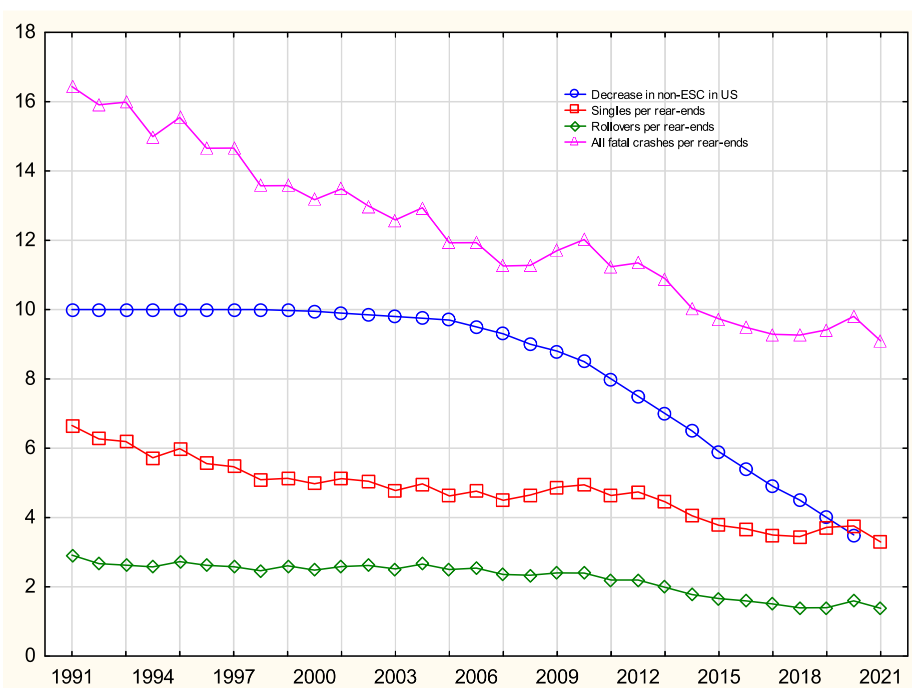

Fig. 2. The numbers of singles and rollovers per year divided by the number of rear-end crashes per year in FARS data. On a different scale (tens of percent) is shown the percent light passenger vehicles without ESC in the US population.

3.6. Correlation and regression analysis

In Table 3, the correlations are displayed between the crash variables per VMT and control parameters per capita. It can be noticed that rear-end crashes are very different from the other crash categories, being only weakly correlated with the predictors, and in three cases out of four with a different sign. Results were similar if the analysis was restricted to 1991–1998.

As the number of passenger cars per capita was the strongest predictor of crashes (excluding rear-ends) per VMT, this variable was entered along with ESC into forward stepwise multiple regressions as predictors of the crash variables. The results can be seen in Table 4. It is noteworthy that for singles and all fatal crashes, the sign changes from the correlation to the regression, meaning that when the percent of automobiles per population is controlled for, these crashes increase with increased ESC market penetration. For rollovers, the ESC effect could be

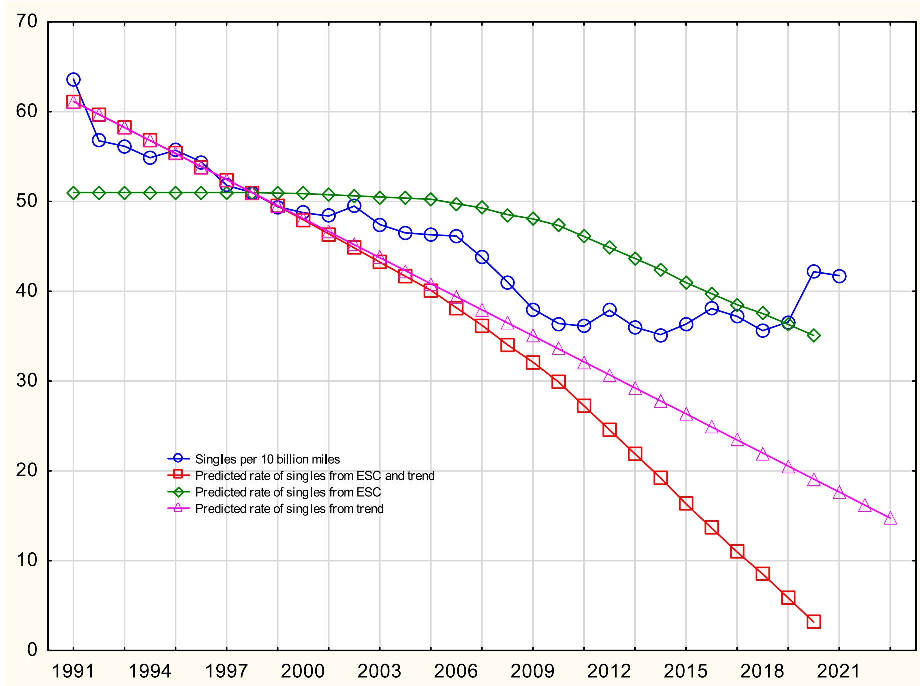

Fig. 3. Predicted and actual trends for fatal single crashes per 10 billion miles in the US.

Table 2
The correlations between predicted and actual crash rates per mile traveled per year. The predicted rate is different between columns. All correlations significant at $p < .001$. N = 30.

	Singles per VMT	Rollovers per VMT	All fatal crashes per VMT
Predicted rate from ESC and trend	0.90	0.91	0.85
Predicted rate from ESC	0.73	0.88	0.69
Predicted rate from trend	0.93	0.89	0.91

calculated to be about 40% with automobiles per capita held constant. Results were very similar if the analysis was restricted to 1999–2020.

4. Discussion

4.1. Results

The present study found that for crashes per VMT there is no evidence for an effect of ESC on fatal singles in the U.S. population, while for fatal rollovers and all fatal crashes, the effect appears to be about two-thirds of what could have been expected from sample studies. When calculating crashes per rear-end (a popular exposure measure in sample studies of ESC), there was no evidence of any safety effect at all, if the previous trend was considered. Finally, when holding one control factor constant, only rollovers showed a safety effect (about 40%). These results are similar to those of Lim and Chi (2013), who found that when previous trend was controlled for, the effect of a ban on cell phone use when driving was no longer significant.

The results in this study can therefore be interpreted in different ways. One interpretation is that there is little or no safety effect at all from ESC. In this view, there were factors present before ESC that had a positive effect, and that this has continued after the late nineties, but with diminishing returns. However, it could also be claimed that ESC is having an effect, although maybe not as large as predicted, but that whatever was causing the previous decline has had a strongly waning effect at about the same time as when ESC was introduced. Both explanations suffer from the problem of not knowing what the previous cause(s) was.

A clear uptick in crashes, in absolute and relative numbers, in 2020–2021 was visible in the data. As early 2022 data have shown a strong decrease in fatal crashes (NHTSA, 2022), the increase was probably due to Covid-19 (Ruhm, 2022). However, this anomaly is still relevant for the present study. If some sort of behavioral change can have such a strong effect in a population where ESC is present, it is very feasible that behavioral adaptation might have nullified the effects of ESC. Furthermore, whether the trends will continue to follow the same decline as before Covid-19 is an open question, and the development in the next few years will be crucial for the interpretation of the effect of ESC on safety.

4.2. Limitations

The main problem encountered in this study is the calculation and interpretation of trends in the data. First, the trend calculations could have been undertaken on a longer time series of numbers. Second, the choice of starting year for ESC could be shifted slightly, according to what is considered a relevant percentage of ESC in the population to calculate effects on. Third, different kinds of curves could have been fitted, possibly with different results. All these factors could influence the calculated trend before ESC, and possibly alter the results. Fourth, there are many different statistical methods that could have been applied, but there seem to be no consensus on what would be the most correct one (see for example Dee, Grabowski, & Morrisey, 2005; Fell, Fisher, Voas, Blackman, & Tippetts, 2009; Fell, Jones, Romano, & Voas, 2011; Lim & Chi, 2013).

The present study has presented three (or four) different hypotheses that predict that estimates of ESC effects from samples are probably too high. However, the analysis did not attempt to disengage these effects, in contrast to, for example, the study by Harless and Hoffer (2003). The aim of the study was thus not to disentangle such effects, but rather to adjust the estimates of ESC safety effects, and in the end to indicate that safety forecasts for technological safety systems are liable to be overly optimistic.

The FARS data have been used by several authors to identify ESC-

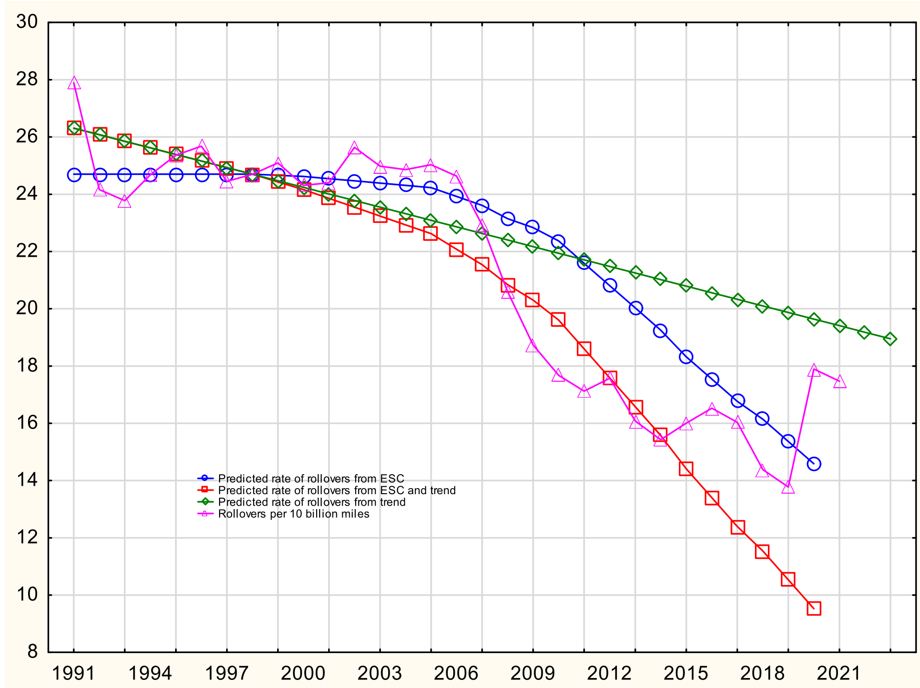

Fig. 4. Predicted and actual trend for fatal rollovers per 10 billion miles in the US.

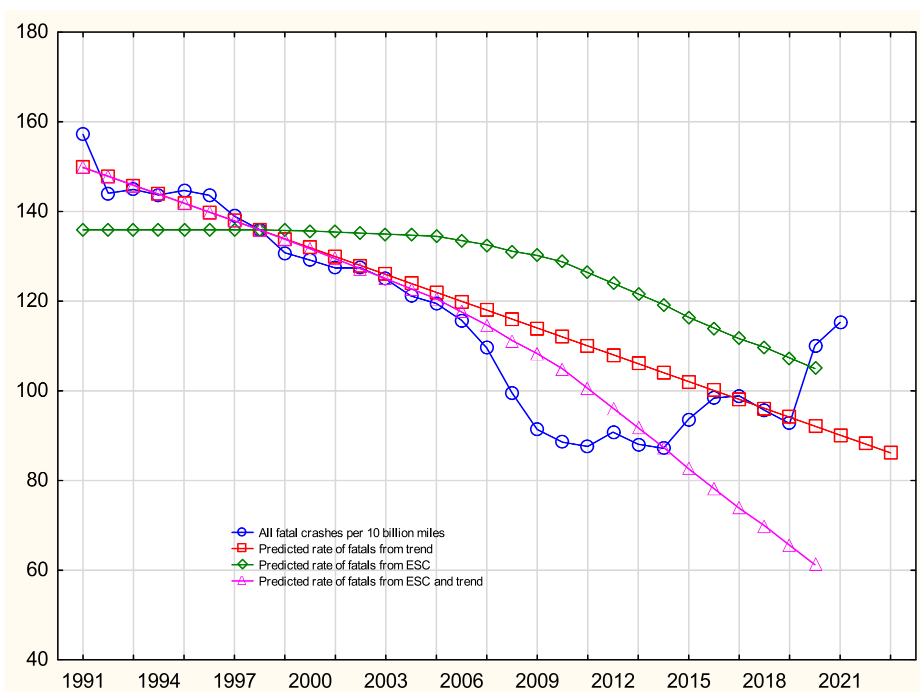

Fig. 5. Predicted and actual trends for fatal crashes per 10 billion miles in the US.

equipped vehicles and compare their crash rates to those of other vehicles, using the Vehicle Identification Numbers (VINs). This method could in principle have been used here, comparing the percent ESC in the population to that of the FARS data. However, analysis of the vpicdecode files from 2017 to 2021, which contain the information from the VINs, indicate that ESC is not reliably identified by this number. ESC has been mandatory on all new passenger vehicles sold in the United States since 2012, but about 10% of such vehicles are not identified for model years 2017–2021, while older vehicles have hardly any hits at all, in the vpicdecode files. The reasons for this problem have not been found in any of the supporting documents from NHTSA, or in their VIN decoder (https://www.nhtsa.gov/vin-decoder).

The market penetration data from HLDI and the FARS data were from slightly different populations, as the HLDI estimates excluded commercial vehicles. It can be assumed that commercial light vehicles are a few years younger on average as compared to the population of private vehicles.

The main emphasis of this paper has been the comparison between

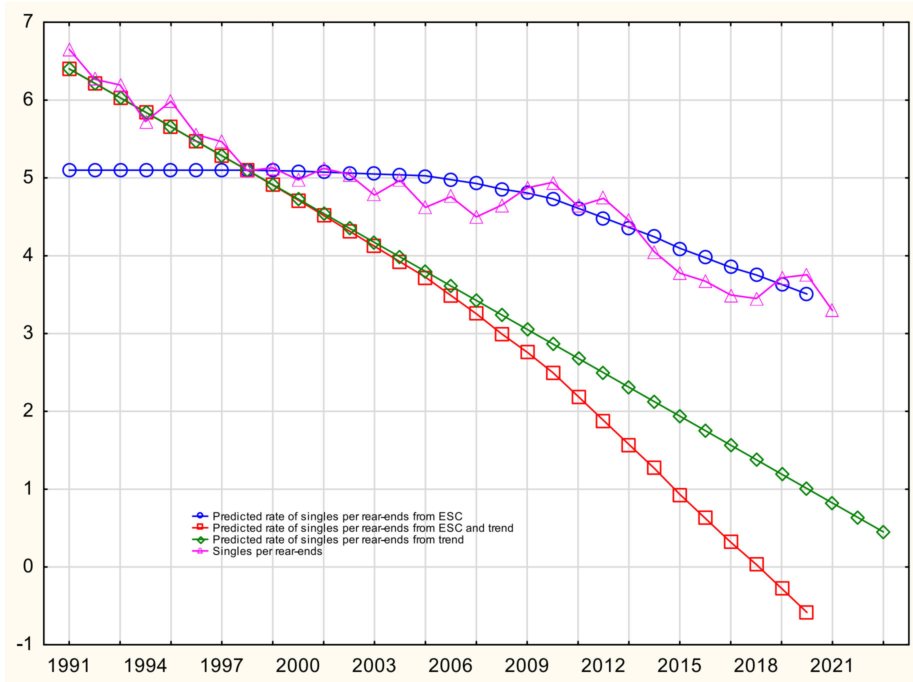

Fig. 6. Predicted and actual trends in number of fatal singles per number of fatal rear-ends in the US.

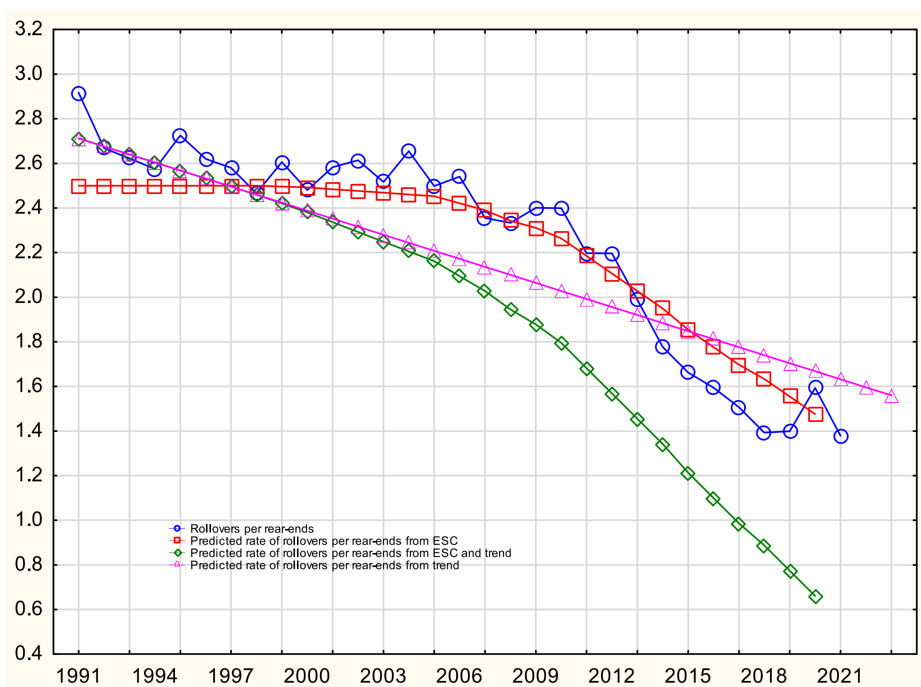

Fig. 7. Predicted and actual trends in number of fatal rollovers per number of fatal rear-ends in the US.

the results of sample-based studies and a population-based estimate of the effects of ESC on crashes thought to be directly influenced by this technology. This excludes measuring the possibility of ESC having transfer effects, causing other types of crashes (for example with pedestrians). In this sense, the present study is not system-comprehensive, and the total safety effect might be even smaller than estimated here (Hedlund, 2000).

5. Conclusions

This study appears to be the first to test whether nationwide accident rates have been influenced by the introduction of active safety systems (but see Lim & Chi, 2013). More research of this kind is therefore needed, especially as the conclusions here are rather different from those of crash sample-based studies.

It would seem to be evident that there is no evidence of an effect of ESC on fatal single crashes in the United States, while for fatal rollovers

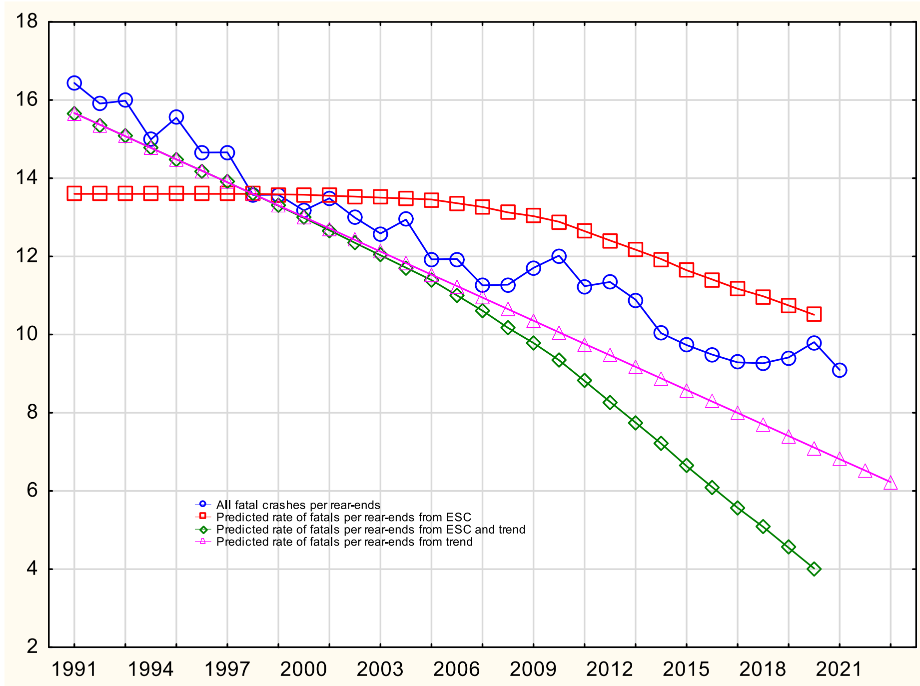

Fig. 8. Predicted and actual trends in number of fatal crashes per number of fatal rear-ends in the US.

Table 3
Correlations between crashes per VMT and control variables over the years 1991–2020 (number of licensed drivers and registered vehicles not available for 2021). N = 30.

Variable	Fatal singles	Fatal rollovers	Fatal rear-ends	All fatal
Industrial production/ capita	-0.58***	-0.25	0.11	-0.53**
Licensed drivers/capita	-0.73***	-0.67***	0.27	-0.70***
Passenger cars/capita	0.90***	0.90***	-0.08	0.86***
ESC market penetration (%)	-0.73***	-0.88***	0.25	-0.69***

** p < .01, *** p < .001.

and all types of fatal crashes, the effect could possibly be about two thirds of what has been calculated in sample-based studies using induced exposure. This discrepancy between effects on variables that have been expected to be similar would need an explanation.

The current results would seem to agree with hypotheses of an over-estimation of the effect of ESC in sample studies, possibly due to (a) self-selection of drivers, (b) behavioral adaptation to ESC over long time periods, (c) a diminishing effect of ESC in driver groups who are late to adopt such features (or simply prefer older vehicles for various reasons), and (d) the use of very specific crash samples. All four mechanisms would lead to lower-than-expected effects in the population. However, as individual drivers could not be traced in these data, and ESC status could not be reliably determined, it was not possible to disentangle these possible mechanisms.

Many empirical studies on new vehicle safety features have used the same kind of methodology as the one described here for ESC (e.g., Fildes et al., 2015; Rizzi, Kullgren, & Tingvall, 2014). Given the current results, it could be suspected that these too have been somewhat over-estimated.

Similarly, projections of safety effects for ESC from other methods such as crash analysis, simulators, and simulations, have yielded effects of the same magnitude as sample studies (e.g., Erke, 2008). Apparently, such methods and their results should also be questioned, especially as these and other similar methods are currently being applied to new vehicle safety features (such as lane departure warning; see the review by Sohrabi, Khodadadi, Mousavi, Dadashova, and Lord (2021) and promising effects not very different from those that were expected concerning ESC (e.g., Harper, Hendrickson, & Samaras, 2016; Houser, Murray, Shackelford, Kreeb, & Dunn, 2009; Kitajima, Shimono, Tajima, Antona-Makoshi, & Uchida, 2019). It would seem that the advice of Hedlund (2000) is warranted; “Don’t over-predict benefits” (p. 88).

The present results call for new research, utilizing different methods, into the effects of ESC, ABS, and other driver assistance systems. Apart from replications of the present study in other datasets, a meta-analytic comparison between results for ESC in private and professional driving should be undertaken. Given that individual self-selection and exposure measurement would be less of a problem, while behavioral adaptation would still be operating, ESC effect sizes could be expected to be smaller for professional drivers (including light vehicles). Furthermore, results

Table 4
Results of forward stepwise multiple regressions with crashes per VMT as dependent variables. N = 30. F value to enter 1.

Crash variable	Passenger vehicles per capita (beta, R2 change)	Market penetration of ESC (beta, R2 change)	Total amount of variance explained (adjusted)	Standard error of estimate	Intercept	Standard error of intercept
Singles per VMT	1.42*, 80.3%	0.57*, 5.2%	84%	3.2	−29.0	10.1
Rollovers per VMT	0.58*, 81.6%	−0.35, 2.0%	82%	1.8	6.9	5.8
All fatal crashes per VMT	1.38*, 73.2%	0.57*, 5.3%	77%	10.6	106.3	4.2
Rear-ends per VMT	0.90*, 13.2%	1.08*, 6.2%	14%	0.82	8.66	0.3

* p < .05.

of sample and population studies should be more similar for light vehicles than what has been reported here. However, this prediction only holds if the methods used avoid the problems of self-selection at company level (safer companies install more safety features), and subjective methodology such as crash analysis (e.g., Hickman et al., 2015).

There is much activity in traffic safety, and many safety claims for the future are being forwarded, especially for various automated features of vehicles (see the review by Tafidis, Farah, Brijs, & Pirdavani, 2022). However, many of these predictions seem to be suspiciously large. If all of them were as effective as claimed, there would hardly be any crashes at all when market penetration become high (e.g., Vaa, Penttinen, & Spyropoulou, 2007). An analysis of a study by Anderson et al. (2010) found that even without ESC, the claimed safety effect would be 75% for various automated features (af Wåhlberg, unpublished).

When calculating the effects of ESC in the present data, the previous trends in crash rates were considered. These could be due to developments in crashworthiness of vehicles, improvements in emergency medicine response times (Cruz & Ferenchak, 2020; Liu, 2022), graduated licensing (McCartt & Teoh, 2015), deterrents, speeding countermeasures, seat belts (Venkatraman, Richard, Magee & Johnson, 2021), and so forth in the United States during the time period studied here. If ESC is to have effects similar in size to the forecasts, it would have to be assumed that most other traffic safety interventions was having little effect during this period.

Traffic safety intervention effects would seem to be a crowded place indeed, where each claim is largely made without reference to any other. However, "Two men saying they are Jesus - one of them must be wrong" (Knopfler, 1982).

5.1. Practical applications

It would seem like ESC could have the same fate as ABS; after an initial hype and reports of strong safety gains, the final result would seem to yield little difference, because although some crash categories may be positively influenced, others are negatively influenced, and the net result is nil (e.g., Burton, Delaney, Newstead, Logan, & Fildes, 2004). In the current climate of strong advocacy for automated vehicle features and fully automated cars, these results are very pertinent. Has any technological safety intervention actually fully delivered the expected benefits? Given the vast resources that are put into the development of automated systems for vehicles, the current results would seem to indicate that it would be better to divert some of these into alternatives with better proven effects, such as graduated licensing (Williams, Tefft, & Grabowski, 2012).

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.

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Appendix

Data sources

Market penetration of ESC

The estimated percent of light four-wheel vehicles with ESC in the US population of such vehicles was extracted from HLDI Bulletin 37:11 (2020) Fig. 3. The HLDI data had been compiled from all registered vehicles in the US, except commercial vehicles and motorcycles. The value for 2021 was extrapolated from the previous trend of a 5 percent unit increase per year.

Crash data

Data download. NHTSA FARS data files for 1991–2021 were downloaded from the National folder at https://www.nhtsa.gov/file-downloads?p=nhtsa/downloads/FARS/.

These files contain data for all fatal crashes in the US by year, from 1975 and onwards (NHTSA, 2023).

The Vehicle file includes data on all vehicles involved in fatal crashes each year in the US, and the numbers are therefore larger than the number of crashes themselves in the Accident file.

Vehicles included in dataset. Vehicle types included in the present study: All light (<10,000 lbs) four-wheel vehicles with the same coding over the time period studied.

Variable names and numbers in FARS manual:

BODY_TYP (changing variable number over time), which denotes the type of vehicle (body) in the crash. This variable was used to restrict the data extracted to light passenger vehicles only.

FARS codes: 1–7, 10–11, 14–20, 22, 33, 39. These codes had the same definition for the period 1991–2021.

Crash types. For the vehicle categories defined in the previous each year 1991–2021 were extracted the numbers of the following crashes:

SINGLE
File: Vehicle
Variable names and numbers in FARS manual: C4A Number of Vehicles in Transport (MVIT) (p. 47).
Variable name in files: VE_FORMS
FARS code: 1
ROLLOVER
File: Vehicle
Variable names and numbers in FARS manual: ROLLOVER.
FARS codes: 1, 2, 9
Comments: The ROLLOVER variable was chosen instead of the First Harmful Event (HARM_EV). Therefore, all types of rollover were included, regardless of when and how this happened in the crash sequence.
REAR END
File: Vehicle
Variable names and numbers in FARS manual: MAN_COLL, C20.
FARS code: 1
ALL FATALS
File: Vehicle
Variable names and numbers in FARS manual: Any could be used.
FARS codes: Any.

VMT data

Summary data for miles travelled by the total US population of vehicles per year was downloaded from https://cdan.nhtsa.gov/tsftables/tsfar.htm#, Table 2 (Ch 1 Trends General) and https://www.bts.gov/content/us-vehicle-miles.

Industrial production data

https://www.federalreserve.gov/releases/g17/ipdisk/ip_nsa.txt

Number of licensed drivers and four-wheel passenger vehicles registered in the US

https://www.bts.gov/content/automobile-profile

Comment: Data for 1991–1994 was not available, and these numbers were therefore extrapolated as a linear trend from 1990 to 1995, for which data was available.

US population data

https://data.oecd.org/pop/population.html

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