

**Automobile Safety Regulation and the Incentive to Drive Recklessly:
Evidence from NASCAR**

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Auto Safety and Reckless Driving in NASCAR

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Abstract

When safety regulation makes automobiles safer, drivers may drive more recklessly, creating partially or completely offsetting effects on the overall level of safety. Evidence of these offsetting effects has been hard to find, however, primarily because of the aggregate nature of accident data. In this paper we explore how changes in the safety of automobiles used in NASCAR has altered the incentive of drivers to drive recklessly. This unique data set allows more accurate and objective measurement of the necessary variables to test for these effects at a micro-level. Our results strongly support the presence of these offsetting behavioral effects.

1. Introduction

Does automobile safety regulation (such as mandatory airbags) cause drivers to drive more recklessly? Economists have been fond of this idea since it was originally proposed by Peltzman (1975). Today this argument appears in almost every mainstream economics textbook and popular press book (e.g., Steven Landsburg's *Armchair Economist*). However, for a theory so frequently presented as a basic insight of economics, the empirical evidence in its favor is rather unconvincing. In fact, the vast majority of empirical studies attempting to test for this 'Peltzman effect' have rejected it in its entirety.¹ Because of this, most economists largely discard the data and previous empirical studies, and attempt to prove the argument logically. In verbal argument, for example, Armen Alchian and Gordon Tullock have made famous the hypothetical question of how drivers would react to the installation of large metal daggers protruding from steering wheels coupled with the removal of all restraint devices.²

In this paper we pose and test the question: How do drivers react to having cars so safe that they can generally walk away with no injuries when they crash it into a concrete wall or another car at very high speeds? The answer is that they race them at 200 miles per hour around tiny oval racetracks only inches away from other automobiles—and have lots of wrecks. We employ both individual driver and individual race level data from the National Association for Stock Car Auto Racing (NASCAR) to test for the presence of these offsetting behavioral effects.³

NASCAR data is uniquely suited to test for this 'Peltzman effect' because, by its very nature, NASCAR imposes most of the *ceteris paribus* conditions necessary to isolate these behavioral responses. We are essentially able to test how the same drivers, on the same tracks

and in the same weather conditions, alter their behavior in response to changes in automobile safety. The use of NASCAR data also overcomes the aggregation and measurement problems faced by other authors with state and county level accident and fatality data. Even more advantageous is that in NASCAR both safety and recklessness can be objectively measured using individual data on driver injury and fatality rates, and data on car speed and traffic volume.

Finally, unlike data on street-level seatbelt use, our results are not plagued by non-compliance issues, as NASCAR enforcement policies restrict the race participants to only those drivers whose automobiles pass a pre-race inspection. Because of these advantages, our empirical analysis provides the strongest test to date for these offsetting behavioral effects. We are directly testing for individual human responses to safety improvements within a well-controlled environment. Our results also have policy implications for NASCAR itself, particularly given the increased emphasis on safety since the death of seven-time NASCAR champion Dale Earnhardt in the 2001 Daytona 500 – the fourth driver killed on a NASCAR track since May 2000.

2. The Peltzman Effect

It is important at the outset to clarify the two distinct parts of Peltzman's (1975) hypothesis using Equation (1).

$$\# \text{ injuries} = \text{prob}(\text{injury}/\text{accident}) \times \# \text{ accidents} \quad (1)$$

?
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↑

Equation (1) shows that the total number of injuries is equal to the probability of injury, *conditional on being in an accident*, multiplied by the number of accidents. Automobile safety

regulations, such as mandatory seatbelts or airbags, reduce the probability of injury conditional on being in an accident. But, given that it is now less costly for an individual to be in an accident, drivers will expend fewer resources to avoid being in an accident (by, for example, driving more recklessly), and thus the number of accidents will increase. Whether or not the incentive effect occurs is the first issue. The second issue is whether the effect is large enough to entirely offset the reduction in the probability of injury so that the total number of injuries actually increases as automobile safety is improved.

Following Peltzman (1975), authors generally have looked at the issue of automobile safety by estimating some measure of injuries or fatalities as the dependent variable and some measure of safety as the independent variable, *as opposed to* directly testing whether recklessness (or in equation 1, accidents) is a function of these same safety measures. The lack of empirical consensus from the previous literature is partially due to this problem. However, even if this weren't a problem, the severe limitations inherent in aggregated street-level data make it doubtful, even if these studies had all found similar results, that there would be convincing evidence of the underlying behavioral effect. There are simply too many complicating factors reflected in the underlying data that can not be removed, such as compliance, enforcement, insurance, and weather conditions. For example, Merrell, Poitras, and Sutter (1999) have shown that mandated vehicle safety inspections have no significant impact on accident injury and fatality rates.⁴ On closer examination, however, Poitras and Sutter (2001) find that the reason for the lack of a relationship is not because of offsetting behavioral effects, but rather because of evasion and lack of enforcement of the law.⁵ Thus, equations that only test the second effect cannot be used to prove the existence of the first behavioral effect. This is

why it is worth examining the relationship directly as we do here.

3. Automobile Safety in NASCAR

Modern safety standards in NASCAR are far removed from the early days of racing in the 1950s when race cars were essentially supercharged street cars with no special safety features (and some factory safety features were often removed to reduce weight), running on dirt tracks with little protection for fans or drivers (in fact, many of the cars were convertibles). Modern race cars are equipped with a host of safety features including roll cages, five-point harnesses, window nets, Lexan windshields, special fuel cells, roof-flaps, and in response to the death of Dale Earnhardt, NASCAR now mandates the use of an approved head-and-neck restraint system. In addition, since 1988, both Daytona and Talladega have required the use of restrictor plates that significantly lower average speeds, and recently the New Hampshire International Speedway adopted restrictor plates following the deaths of Adam Petty and Kenny Irwin within months of each other at that track.

NASCAR introduces literally hundreds of rule changes each season regarding safety and performance issues. NASCAR drivers, like ordinary street drivers, adjust their driving habits in a predictable way according to perceived risk.⁶ To measure the combined effect of all of these varying safety changes we calculate the actual probability of injury conditional on being in an accident for NASCAR drivers. We use hand-coded race-level data compiled from Fielden (1989, 1990, 1994) and Golenbock and Fielden (1997) to obtain this, and other necessary, variables for our analysis. These sources allow us to acquire data on injuries, cautions and accidents, speed, race distance, number of cars, and prize money for every season between 1972

and 1993.⁷ These 22 years of data provide us with a more than adequate sample size of over 600 observations.

Because driver behavior is influenced by their own perceptions of risk, our probability of injury variable must reflect the drivers' perception of the probability of driver injury conditional on being in an accident. To do so, we calculate a backward-looking moving average of the actual realized proportion of racetrack accidents resulting in injury (more precisely the number of drivers injured divided by the number of cars involved in accidents), as the perception of risk is largely influenced by the recently observed conditional probability of injury. The length of this moving average (110 races) is determined statistically, specifying the necessary sample size for a reliable measure of this probability.⁸ However, our results are robust to both different length moving averages and alternate measures of the variable.⁹

We use four different dependent variables to measure reckless driving, all involving data on the number of accidents or cautions in the race. For readers unfamiliar with NASCAR racing, a caution is declared any time the track is deemed dangerous, which almost always results from debris from an accident. Under caution the competitors circle the track at a reduced speed and cannot compete for position until the track is once again suitable for racing. Our four measures are: (1) percent of cars eliminated from the race due to an accident, (2) percent of laps run under a caution, (3) number of caution laps, and (4) the number of race miles run under caution.

[Figure 1 about here.]

Prior to presenting the results from a more sophisticated regression model, it is worth pointing out that even the simple correlations between the conditional probability of driver injury and our measures of recklessness in the raw data are very strong. Figure 1 shows one of these

relationships graphically using season-level average data. In the figure, each point represents the average values for one season of racing. Plotted are the number of caution laps (a proxy for the number of accidents), and our ex post calculated probability of injury conditional on being in an accident for NASCAR drivers that season. In the figure it is clear that as NASCAR safety has improved, lowering the probability of injury conditional on being in an accident, the number of accidents (here measured by cautions) has fallen. This relationship is not specific to our use of the caution laps variable, and a similar relationship exists for our other dependent variables even in the raw data. However, other factors might be at work here, necessitating the use of a multiple regression model to accurately control for these other variables. Nonetheless, the strength of the relationship, even in the raw data, is encouraging.

4. Empirical Analysis

Turning to our more formal econometric estimation, our model takes the general form

$$\# \text{ Accidents} = \beta_1 + \beta_2 * P(\text{injury} | \text{accident}) + \beta * \Gamma + \varepsilon \quad (2)$$

where $P(\text{injury} | \text{accident})$ is the probability of driver injury conditional on being involved in an accident and Γ is a matrix of control variables. For each of our four dependent variables we run two specifications. In specification [1], Γ includes race distance, cars per mile of track, and the prize differential between the first and second place finishers (in constant 2000 dollars). In specification [2], we add pole qualifying speed and the percentage of cars that lead the race to the matrix of control variables.¹⁰ Descriptive statistics for all of our variables can be found in Appendix Table A1.

In total, we run eight specifications of the model using race-level data from the 1972-1993 NASCAR seasons (631 races), and again run these eight specifications using season-level average data (22 years).¹¹ The race-level model is a fixed effects model with dummy variables for each track.¹² We cannot include year dummy variables because the majority of the safety changes occur at the beginning of the season, and this variable would mostly steal the explanatory power away from our probability of injury variable.¹³

Our priors suggest that cars per mile, the 1st-2nd prize differential, the percentage of cars that lead the race, and pole speed should all be positively related to the number of accidents. Explanations for our priors follow. Cars per mile of track should vary positively with the number of accidents because the number of accidents should rise with heavier traffic on the raceway. An increase in the prize differential gives drivers more incentive to win the race, and thus to take more risks. The percentage of cars that lead the race is a measure of how competitive the cars are relative to each other. As the cars become more competitively equal, they will tend to not spread out as much across the track, increasing the odds of an accident. Finally, driving at greater speeds makes it more difficult to avoid an accident (this variable is particularly important considering some tracks require cars to use restrictor plates that limit car speeds while others do not). The relationship between the distance of the race and the number of accidents depends on which measure we use for the dependent variable. For instance, longer races should tend to have a greater number of caution laps, simply because there are more total laps in the race (similarly for caution miles). On the other hand because of attrition throughout the course of the race, there probably will a smaller percentage of laps run under caution in longer races.

In order to determine the presence of offsetting behavior we are mainly concerned with the relationship between the number of accidents and the probability of driver injury conditional on being in an accident. If offsetting behavior is present, we expect the coefficient on the probability of driver injury to be negative and significant. The results of our model using race-level data are presented in Table 1 and the results using season average data are presented in Table 2.

[Tables 1 and 2 about here.]

In Tables 1 and 2, the coefficient on the probability of driver injury is negative and significant in all 16 specifications. Furthermore, the probability of driver injury is significant at the 1 percent level in 13 of the 16 specifications; the exceptions are the specifications using the percent of cars involved in crashes, where the variable is significant at the 5 percent level. The R^2 for the race-level model ranges from 0.14 to 0.54, which is typical of micro-level data. The R^2 for the season-level model rises, as is to be expected from aggregated data that averages out random variance, and ranges from 0.27 to 0.79. The control variables in the regressions generally performed as expected in sign, although they were not always statistically significant. The results from our estimations strongly support the idea that NASCAR drivers drive more recklessly (as measured by the number of accidents and cautions) as the probability of driver injury has fallen in NASCAR.¹⁴

Our results suggest that increased safety results in offsetting behavior on the part of drivers. However, the question remains as to whether this offsetting behavior is large enough to result in total injuries rising in response to safety improvements, rather than falling as might be expected if one ignored the presence of these behavioral effects. It is possible to answer this

question through total differentiation of Equation (1). In order for the behavioral effect of driving more recklessly to completely offset the direct effect of increased safety, the total differential of injury with respect to the conditional probability of injury must be less than or equal to zero. This derivation can be found in the Appendix. Through substitution of the mean values of the variables and the necessary coefficients, we can conclude that the behavioral effects are not large enough to be completely offsetting. That is, an increase in safety still leads to a decline in the number of injuries, but the effect is not as large as would be predicted in the absence of these behavioral effects. Thus, making cars safer does result in more accidents, but total injuries still decrease.

Perhaps the most intuitive way to understand these magnitudes is to calculate the elasticities of our reckless driving variables with respect to the conditional probability of injury. If the elasticity is less than one, an increase in safety will lower injuries because the indirect behavioral offset is a smaller percentage change than is the direct impact. An elasticity greater than one would suggest that safety improvements will lead to such a large increase in reckless driving that total injuries will instead rise. In this manner, the elasticity is interpreted similarly to the way a price elasticity would be used to conclude about the impact of a price change on total consumer expenditure (or firm revenue). The elasticities computed from all eight of our race-level specifications are uniformly less than one, and in fact are almost identical. For the eight models shown in Table 2, the respective elasticities are 0.28, 0.21, 0.24, 0.21, 0.23, 0.19, 0.22, and 0.18; all within a narrow range of 0.18 to 0.28. Thus, a 10 percent improvement in NASCAR automobile safety results in approximately a 2 percent increase in reckless driving (regardless of how it is measured). This is not large enough to result in more total injuries, but is

clearly large enough to demonstrate the existence of an offsetting behavioral response—something that has proven illusive for previous empirical literature on auto safety.

5. A Driver-level Empirical Analysis

The above analysis attempts to estimate the effect of improved safety on the incentive to drive recklessly using data on *all* drivers within each race. However, there are often a few drivers that change from race to race due to lack of funding for the entire season, inadequate preparation preventing a driver from qualifying, or injury, among other factors. In order to address this issue and attempt to go even more micro-level in our analysis, we now turn to estimating our model for a specific subset of individual drivers to see if the negative relationship between perceived safety and reckless driving still holds. In this manner we can see whether specific individual drivers were involved in more accidents as the conditional probability of injury was lessened through time.

We selected our sample by finding the 5 drivers who were in the most number of races together. Our five drivers (Cale Yarborough, Benny Parsons, Bobby Allison, Dave Marcis, and Richard Petty) were in 275 races together as a complete group throughout our sample (these 275 races span the period August 20, 1972 through May 29, 1988). For each of these 275 races, we recalculated our accident/caution data using only accidents involving one or more of these five drivers. In this new sample we are simply looking at these five drivers and how the number of accidents they are involved in has changed through time. There were only a few races in which more than one of the group members were in an accident, so we decided to code our dependent variable as a one if at least one member of this group had an accident, and zero otherwise. We

then repeated our empirical analysis using this race-level data on our new dependent variable using both probit and logistic regression techniques in our estimation. The models are run both with and without track dummies (fixed effects). The results of this analysis are presented in Table 3.

[Table 3 about here.]

We find that the probability of injury is significant and negative in three of our four models. The coefficient estimate is almost identical across all four specifications; it is the slightly higher standard error that results in one of the estimates being insignificant. These results suggest that even when we only consider this specific group of five drivers, they were involved in more accidents through time as the probability of injury fell with added safety features on the cars. While the degrees of freedom are substantially lower here than in our previous analysis, the fact that the results still hold among this small subset of drivers is a substantial robustness check of our results.

Our results not only add to the literature on automobile safety but they also have policy implications for NASCAR itself. This is particularly true given the increased emphasis on safety in NASCAR since the death of Dale Earnhardt. Our results suggest that increased automobile safety results in not only more accidents but also a reduced number of total injuries. If it is true that NASCAR viewership is increased by more accidents (as has been claimed by sports commentators), then the safety improvements are a win-win situation because they not only increase the number of accidents (which increases viewership) but also lower the total number of driver injuries. Thus, increased safety measures can serve both profit- and safety-enhancing motives in NASCAR. The more likely case is that there is an optimal number of accidents that

the audience wants to see (less than the maximum number of accidents due to clean up time), and there is an optimal level of safety that maximizes NASCAR's profits.¹⁵ However, there also exists the possibility that some safety improvements could reduce the aesthetic quality of races to fans (as has sometimes been claimed with restrictor plates), lowering viewership. Another implication concerns the profitability of the individual race teams. The monetary costs of the safety innovations may be quite large, especially since offsetting behavior increases the number of accidents and, thus, the cost of repairs, while the benefits of such innovations may be very small.¹⁶ Thus, race teams may be most profitable under a lower level of safety than NASCAR as a whole.

6. Conclusion

Our results suggest that the inability of previous empirical studies to arrive at a definitive conclusion regarding the existence and degree of offsetting behavior in response to increased automotive safety is primarily the result of poor data. The aggregate nature of street-level accident data simply leads to inconsistent results as other variables such as compliance, enforcement, weather, and insurance complicate the relationship. Furthermore, an overwhelming majority of the previous literature estimates some measure of injuries or fatalities as a function of a measure of driver safety, which only gets at the behavioral effects indirectly, leading to interpretation problems and, in some cases, the wrong conclusion.

Our study improves on the previous literature by avoiding most, if not all, of these issues that plagued prior studies. Because NASCAR inherently controls for problems of enforcement and weather and requires the same safety devices be installed in all vehicles, the use of our data

virtually eliminates all problems associated with aggregated data. We test for the presence of offsetting behavior directly by estimating the relationship between accidents and the probability of injury, leaving room for no misinterpretation. Our results clearly support the existence of offsetting behavior in NASCAR—drivers do drive more recklessly in response to increased safety of their automobiles. Total injuries, however, still fall because this effect is not large enough to completely offset the direct impact of increased automobile safety.

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Endnotes

1. While Peltzman's (1975) original empirical tests yielded mixed results, the vast majority of follow-up studies have entirely rejected the empirical presence and significance of offsetting behavior, see for examples, Robertson (1977), Peltzman (1977), Graham and Garber (1984), Crandall and Graham (1984), Graham (1984), Evans and Graham (1991), Loeb (1995), and Lund and O'Neill (1986), Orr (1982), Chirinko and Harper (1993), Risa (1994), Peterson and Hoffer (1994), Hoffer and Millner (1992), Lund and Hazelbaker (1993).
2. The author of Herman Comics, Jim Unger, also depicted a similar idea in a newspaper comic.
3. O'Rourke and Wood (2003) employ NASCAR data to study the impact of restrictor plates, which reduces both the average speed and the variance of speed across drivers. They find that although the plates, through their impact on speed variance, tend to increase the number of accidents, there is no evidence that they have led to more driver injuries. Thus, their result is consistent with the idea of offsetting behavioral responses for the specific case of restrictor plates. Also see von Allmen (2001) for an interesting study on the efficiency of the reward system in NASCAR.
4. For further studies of the effects of mandated automobile safety inspections see Garbacz (1990) and Loeb (1990).
5. See Lee (1985) and Graves, Lee, and Sexton (1989, 1993) for interesting studies that take enforcement into account in examining the issue of optimal speed limits.
6. Although professional drivers are arguably less risk averse than the common driver, they

respond to incentives in the same manner—as they feel safer, they will take more risks. Risa (1992) offers a theoretical proof of this proposition. Risa shows that while the direction of the offsetting incentive effect will be the same for both risk-loving and risk-averse drivers, the magnitude of the effect will differ. In particular, the incentive response will grow in magnitude as the preference for risk increases. Thus, to the extent that NASCAR drivers are more risk loving than ordinary street drivers, our results suggest that an increase in automobile safety will lead to an increase in accidents but a decrease in total injuries for both NASCAR and ordinary street drivers. There are other complications, however, such as perhaps a wider variation in driver skill levels.

7. 1972 is chosen as the beginning point for our sample because NASCAR rules became more clearly defined and enforced, and records better recorded, in response to Winston becoming the primary sponsor of the circuit in that year. Winston also limited the number of NASCAR Winston Cup sanctioned races to one per week, whereas there were often two or more races at different venues on the same day prior to Winston's involvement. We would have liked to extend the sample further than 1993, but Fielden (1994), our primary source of data, is the last volume of the series, and there is no current publication that gives the detailed data necessary to calculate our variables of interest after the 1993 NASCAR season. Races shortened due to weather and races with missing data were excluded from our sample.

8. We tested for the appropriate number of accidents observed in a sample for a population proportion at the 95% confidence level and a maximum allowable error of .03: $n = p(1-p)(z/E)^2$, where p is the probability of injury conditional on being in an accident, z is the standard normal

value for a 95% confidence interval (1.96), and E is the maximum allowable error. With an average of 2.78 accidents per race, we are able to calculate that about 110 races should be observed for a reliable measure of the probability of injury conditional on being in an accident. We were able to obtain from Fielden (1989) data for the necessary variables for the 110 races previous to the 1972 season to construct this moving average and avoid throwing out observations.

9. For robustness, sample sizes of 50 and 100 races were used for the calculation of the moving average of the probability of injury conditional on being in an accident, replacing the 110-race measure of probability of injury. We also attempt a 2-season and 3-season moving average for the probability of injury conditional on an accident. Results for the 100-race average and 3-season average perform nearly identically to the 110-race average, while the 50-race average and 2-season average perform similarly in the race-level model only. We also explored whether a dummy variable reflecting the presence of a recent fatality (in the last 10, 20, or 30 races) should be included, but it was insignificant in the full specification of the model as an additional variable, and always fit worse than our true probability of injury variable (there were only 6 deaths during this period).

10. For readers unfamiliar with NASCAR, the pole speed is the speed for the fastest qualifying car. We also ran the estimation replacing pole speed with average race speed and found similar results. However, average race speed is correlated with cautions since caution lap speeds are included in the average. Thus, it is a biased measure of true race speed because it is pulled down by accidents, and pole speed is a better measure of the race speed for our purposes. We also

included a variable to measure the percent of drivers that were rookies, but it was insignificant and did not alter the findings, so was excluded from the final model.

11. Season averages are found by averaging the values for all race-level variables across all races within the season.

12. White's matrix was used to correct for heteroskedasticity in both the race- and season-level models. The track dummy variables were jointly significant at better than the 1 percent level in all specifications. Corresponding F-statistics for track dummy joint significance were: 5.92, 4.55, 4.38, 3.50, 3.57, 3.63, 5.76, and 4.26.

13. We did include a time trend in early specifications of the model, where it was significant in some specifications and not in others. Although our probability of injury variable remained significant even when including the time trend, we exclude the trend from our final analysis due to concerns that it might be picking up some of the effect of improved safety through time.

14. We also ran these models using a logarithmic and censored tobit specifications and found similar results. The log specification has the disadvantage that any observations with a zero must be omitted. The tobit model explicitly handles the censored nature of the variable, but made little difference with only 9 zero observations for cautions (percent, laps, and miles) and 134 for percent of cars involved in crashes.

15. The relationship can be depicted as a typical Laffer curve with a particular level of accidents maximizing NASCAR profits.

16. For a cost-benefit analysis of automotive safety regulation see Crandall, Keeler, and Lave

(1982) and Lave and Webber (1970).

Table 1. Race-Level Track Fixed Effects Model, 1972-1993

Dependent Variable:	Percent of Cars Involved in Crashes		Percent of Laps Run Under Caution		Number of Caution Laps		Number of Race Miles Under Caution	
	[1]	[2]	[1]	[2]	[1]	[2]	[1]	[2]
Conditional Probability of Injury	-0.28 *** (3.13)	-0.21 ** (2.49)	-0.40 *** (3.70)	-0.35 *** (3.43)	-1.13 *** (3.60)	-0.96 *** (3.27)	-1.39 *** (3.66)	-1.18 *** (3.40)
Constant	8.07 *** (3.00)	-12.18 ** (2.47)	20.55 *** (7.27)	25.11 *** (4.02)	1.05 (0.17)	0.68 (0.05)	10.05 (1.58)	30.82 * (1.69)
Race Distance [x10 miles]	0.02 (0.34)	-0.05 (1.01)	-0.14 ** (2.49)	-0.22 *** (4.03)	0.47 *** (5.61)	0.24 *** (2.98)	0.12 *** (7.43)	0.88 *** (6.34)
Cars per Mile of Track	0.21 (1.57)	0.21 (1.58)	0.24 ** (2.23)	0.22 * (1.94)	0.84 * (1.87)	0.78 * (1.70)	0.85 ** (2.38)	0.77 ** (2.02)
1st–2nd Prize Differential [2000 dollars] [x\$10,000]	0.03 * (1.85)	0.03 * (1.84)	-0.01 (0.29)	0.001 (0.04)	-0.03 (0.28)	-0.01 (0.07)	-0.05 (0.36)	-0.02 (0.15)
Percent of Cars that Led Race		0.23 *** (5.97)		0.34 *** (9.97)		1.00 *** (9.86)		1.34 *** (10.37)
Pole Speed for Race		0.09 *** (3.45)		-0.05 (1.40)		-0.06 (1.21)		-0.20 ** (2.21)
R-squared	0.14	0.22	0.20	0.31	0.38	0.46	0.45	0.54
Observations	631	631	631	631	631	631	631	631

Notes: *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level. Absolute t-ratios appear in parenthesis and have been corrected for heteroskedasticity using White's matrix. All regressions include dummy variables for each track, which have been suppressed from the table. Full results are available from the authors upon request.

Table 2. Season-Level Model, 1972-1993

Dependent Variable:	Percent of Cars Involved in Crashes		Percent of Laps Run Under Caution		Number of Caution Laps		Number of Race Miles Under Caution	
	[1]	[2]	[1]	[2]	[1]	[2]	[1]	[2]
Conditional Probability of Injury	-0.30 ** (2.42)	-0.19 ** (2.76)	-0.65 *** (8.28)	-0.43 *** (3.95)	-1.66 *** (9.07)	-1.25 *** (4.44)	-2.56 *** (9.28)	-1.95 *** (4.39)
Constant	26.32 (1.16)	-55.57 *** (3.95)	79.02 *** (3.78)	34.22 (1.21)	173.81 *** (4.21)	104.58 (1.70)	291.98 *** (4.45)	150.93 (1.55)
Race Distance [x10 miles]	-0.59 (0.95)	0.09 (0.25)	-1.04 ** (2.27)	-0.27 (0.66)	-2.33 ** (2.54)	-0.95 (0.95)	-3.82 ** (2.74)	-1.55 (0.96)
Cars per Mile of Track	0.13 (0.33)	0.45 (1.52)	-0.66 ** (2.49)	0.04 (0.13)	-1.10 ** (2.14)	0.22 (0.30)	-2.24 ** (2.66)	-0.25 (0.21)
1st–2nd Prize Differential [2000 dollars] [x\$10,000]	0.44 (1.45)	-0.307 * (1.89)	-0.10 (0.64)	-0.08 (0.56)	0.10 (0.24)	-0.34 (0.95)	0.61 (0.95)	0.52 (0.88)
Percent of Cars that Led Race		0.16 (1.27)		0.50 *** (4.20)		0.97 ** (2.62)		1.42 ** (2.27)
Pole Speed for Race		0.31 *** (3.50)		-0.13 (1.68)		-0.34 (1.46)		-0.29 (0.73)
R-squared	0.27	0.79	0.50	0.74	0.55	0.69	0.54	0.68
Observations	22	22	22	22	22	22	22	22

Notes: *** indicates statistical significance at the 1% level, ** at the 5% level, and * at the 10% level. Absolute t-ratios appear in parenthesis and have been corrected for heteroskedasticity using White's matrix.

Table 3. Binomial Probit & Logit Models. Marginal Effects Reported.

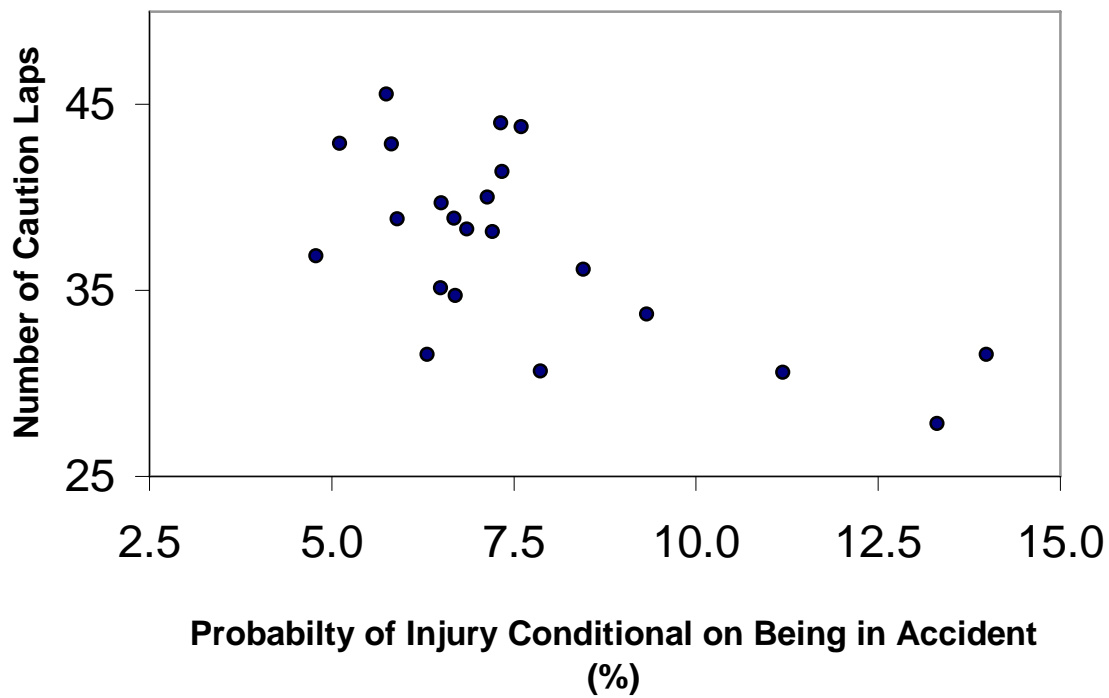
Dependent Variable = 1 if at least one driver in group was involved in an accident

Variable	Probit		Logit	
Conditional Probability of Injury	-0.015 * (1.746)	-0.015 (1.628)	-0.015 * (1.739)	-0.015 * (1.664)
Constant	0.002 (0.008)	-0.093 (0.179)	-0.005 (0.025)	-0.091 (0.177)
Race Distance [x10 miles]	-0.004 (1.290)	-0.006 (0.629)	-0.004 (1.204)	-0.006 (0.572)
Cars per Mile of Track	0.0005 (0.225)	0.011 (0.656)	0.001 (0.316)	0.011 (0.655)
1st–2nd Prize Differential [2000 dollars] [x\$10,000]	0.019 (1.637)	0.014 (0.993)	0.018 (1.639)	0.013 (0.940)
Track Fixed Effects	No	Yes	No	Yes
Log-likelihood Ratio Test Occurrences	10.41 ** 275	19.99 275	10.33 ** 275	19.99 275

Notes: The group of drivers used in the regressions includes C. Yarborough, B. Parsons, B. Allison, D. Marcis, and R. Petty. ** indicates statistical significance at the 5% level and * at the 1% level. Absolute t-ratios appear in parenthesis. The fixed effects regressions include dummy variables for each track, which are suppressed from the table. Full results are available from the authors upon request.

Figure 1. Season-Level Relationship between Probability of Driver Injury and Reckless Driving Behavior (measured by caution laps)

Figure 1:



Appendix.

Table A1. Descriptive Statistics, 1972-1993

Variable	Race-Level Data				Season-Level Data			
	Mean	Minimum	Maximum	St. Dev.	Mean	Minimum	Maximum	St. Dev.
Conditional Probability of Injury	7.63	3.65	14.69	2.50	7.62	4.79	13.98	2.39
Percent of Cars Involved in Crashes	7.62	0.00	36.67	7.24	7.50	4.06	10.71	2.08
Percent of Laps Run Under Caution	12.77	0.00	46.00	7.22	12.81	9.91	16.27	2.10
Number of Caution Laps	38.26	0.00	133.00	23.90	37.43	27.84	45.56	5.00
Number of Race Miles Under Caution	49.12	0.00	169.50	31.81	57.12	41.78	69.60	8.11
Race Distance [x10 miles]	38.42	12.50	60.00	13.31	38.06	36.93	39.23	0.66
Cars per Mile of Track	32.33	10.40	68.57	16.29	32.08	30.50	35.39	0.96
1st–2nd Prize Differential [2000 dollars] [x\$10,000]	3.26	0.00	170.16	7.28	3.27	1.79	8.41	1.68
Percent of Cars that Led Race	20.44	2.94	65.00	7.82	20.11	12.11	25.23	3.36
Pole Speed for Race	145.63	84.12	212.23	34.97	145.14	136.44	151.66	5.22

Sources: Fielden (1989, 1990, and 1994) and Golenbock and Fielden (1997)

Derivation of Partial Offsetting Behavior Result

Equation (1) is restated below as Equation (A1) with simpler notation to facilitate this derivation. We have substituted I for the number of injuries, P for the conditional probability of injury, and A for the number of accidents.

$$I = P \cdot A \quad (A1)$$

Taking the total differential of Equation (A1) yields,

$$dI = A \cdot dP + P \cdot \frac{\partial A}{\partial P} \cdot dP \quad (A2)$$

Solving for $\frac{dI}{dP}$ yields,

$$\frac{dI}{dP} = A + P \cdot \frac{\partial A}{\partial P} \quad (A3)$$

Because $\frac{\partial A}{\partial P}$ is equal to the slope coefficient, β , on the conditional probability of injury from the regression results, Equation (A3) can be rewritten in terms of β as:

$$\frac{dI}{dP} = A + P \cdot \beta \quad (A4)$$

Equation (A4) indicates that the impact of a change in the conditional probability of injury influences the number of injuries through two channels. First, a reduction in the conditional probability of injury will reduce the total number of injuries from any fixed number of accidents (shown by the first term in A4). Second, a reduction in this probability will work behaviorally to increase the number of accidents, increasing the number of injuries (shown by the second term in A4).

If there were no behavioral effect ($\beta = 0$), the direct effect (A) would be all that remains and the relationship would necessarily be positive. However, as our regression results have shown, offsetting behavior does occur, $\beta < 0$. Thus, total injuries could theoretically either increase or decrease with an improvement in safety depending on which effect is larger.

To determine whether the number of injuries rises or falls with an increase in safety, we can evaluate the above equation at the mean values of our four measures of A and of our conditional probability of injury variable and substituting in the values of β from our regression results. For example, using the percent of cars involved in crashes as the measure of accidents and the slope coefficient of the conditional probability of injury from specification [1] from the race-level

results:

$$\frac{dI}{dP} = 7.62 + 7.63 \cdot (-0.28) = 5.48 > 0 \quad (\text{A5})$$

Since this relationship is positive, it is clear that there is a direct relationship between injuries and the conditional probability of injury. The safety improvements in NASCAR cause a decline in the conditional probability of injury, which implies that the number of injuries falls. Similar results are found using the other three measures of accidents and their corresponding estimated values for β . In our results, there is always a positive relationship between the number of injuries and the conditional probability of injury—the behavioral effect only partially offsets the direct benefits of an increase in safety.