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Motorcycle helmets and traffic safety

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ABSTRACT

Between 1997 and 2005, the number of annual motorcyclist fatalities doubled. Motorcyclist fatalities now account for over 10 percent of all traffic-related fatalities. However, over the last three decades, states have generally been eliminating laws that require helmet use among all motorcyclists. This study examines the effectiveness of helmet use and state laws that mandate helmet use in reducing motorcyclist fatalities. Within-vehicle comparisons among two-rider motorcycles indicate that helmet use reduces fatality risk by 34 percent. State laws requiring helmet use appear to reduce motorcyclist fatalities by 27 percent. Fatality reductions of this magnitude suggest that the health benefits of helmet-use laws are not meaningfully compromised by compensating increases in risk-taking by motorcyclists.

1. Introduction

Traffic-related fatalities are a leading cause of mortality in the United States, particularly among children and young adults. There are typically over 40,000 traffic fatalities in the United States every year. However, the relative stability of these fatality counts over the last two decades, combined with the fact that the number of drivers and the amount of driving has increased dramatically over this period, implies that there have actually been meaningful improvements in traffic safety over time.[1] In fact, the Centers for Disease Control and Prevention (CDC, 1999) has characterized improvements in traffic-related safety as one of the 10 great public-health achievements of the 20th century.

However, these improvements in overall traffic-safety risk obscure an increasingly prominent source of traffic-related fatalities: motorcycles. Between 1997 and 2005, annual motorcyclist fatalities more than doubled, increasing from 2116 to 4553 (Fig. 1). Motorcyclist fatalities currently account for just over 10 percent of all traffic fatalities. This dramatic growth is undoubtedly due in part to the increased popularity of motorcycles. Between 1997 and 2005, the number of registered motorcycles grew from 3.8 million to 6.2 million (NHTSA, 2006a).

However, motorcyclist fatalities also increased relative to both the number of registered motorcycles and the miles traveled by motorcyclists. This growth in fatality risk may be due in part to changes in the prevalence of helmet use among motorcyclists. Between 1998 and 2006, helmet use among motorcyclists fell from 67 percent to 51 percent (Glassbrenner and Ye, 2006). Furthermore, there have been dramatic reductions in state laws that mandated helmet use among adult motorcyclists. Between 1966 and 1976, virtually all states introduced such laws because of a Federal statute that threatened state access to some highway-construction funds. However, only 20 states currently require helmet use among all motorcyclists (Jones and Bayer, 2007).

In this study, I present new evidence on the effectiveness of both motorcycle helmets and state laws that mandate their use. More specifically, I first examine the *technological* effectiveness of helmet use (i.e., effectiveness net of associated risk-compensating behaviors) in reducing the probabilities of fatal and non-fatal injuries. I identify the technological effectiveness of helmet use by constructing regression-

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[1] Between 1985 and 2005, the number of vehicle miles traveled has increased by 68 percent (NHTSA, 2006a).

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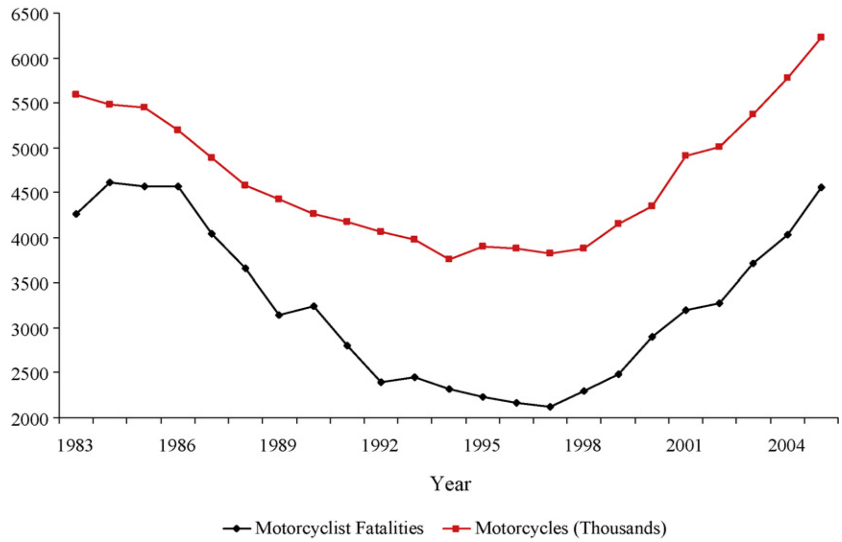

Fig. 1. Registered motorcycles and motorcyclist fatalities, 1983–2005.

adjusted, within-vehicle comparisons of the injury outcomes among the riders of two-rider motorcycles that are involved in fatal accidents. This basic identification strategy is similar to the seminal "double-pairs" comparisons introduced by Evans (1986). However, a generalized, regression-based version of this approach provides a framework for assessing the internal validity of these within-vehicle comparisons by making it possible to condition on the potentially confounding variation in observed, individual traits that influence both helmet use and injury outcomes (e.g., age, sex, driver status and alcohol use). Similarly, within-vehicle comparisons have been used in other recent research on the effectiveness of seat belts and car seats (Levitt and Doyle, in press). I also present evidence on the critical question of whether the inferences based two-rider motorcycles involved in fatal accidents have external validity for other types of motorcycle accidents.

Second, I present indirect evidence on whether there are empirically meaningful risk-compensating behaviors in response to helmet use and regulation. This evidence is based on comparing updated estimates of the effects of state-level motorcycle helmet laws on motorcyclist fatalities to explicitly constructed empirical benchmarks that assume the absence of behavioral responses to changes in helmet use and regulation. I conclude with a discussion of the implications of these positive results for public policy towards the regulation of helmet use.

2. The technological effectiveness of helmets

2.1. Prior literature

The notion that wearing a motorcycle helmet is effective at reducing the prevalence and severity of injuries might seem uncontroversial. However, a number of behavioral and technical factors could actually complicate the anticipated health benefits of helmet use and related regulations. For example, the well-known "Peltzman hypothesis" suggests that the health benefits of helmet use might be attenuated by compensating changes in other risky driving behaviors (Peltzman, 1975). That is, if drivers choose to (or are compelled to) wear a helmet, they may adjust their risk-taking on other margins (e.g., speed and braking distance). A second factor that may attenuate the health benefits of helmet laws is that, in the absence of such regulations, motorcyclists may already tend to use helmets in circumstances where they are most effective. In other words, helmet laws may encourage helmet use on margins where they are least effective. Third, the use of helmets could compromise the vision and reaction time of motorcyclists and so increase both the likelihood and the severity of injuries. Fourth, helmet use may be ineffective at preventing injuries in the most serious crashes simply because the bodies of motorcyclists are otherwise so exposed. And, fifth, it is also possible that the weight of a helmet could, in an accident, exacerbate certain types of injuries (i.e., those in the neck).

A study of motorcycle crashes in the Los Angeles area presented some evidence consistent with these concerns. More specifically, Goldstein (1986) concluded that helmet use was associated with reductions in the severity of head injuries but was ineffective at preventing fatalities and actually increased the severity of neck injuries at higher impact speeds. However, there are a number of reasons to suspect that cross-sectional comparisons of this sort may lead to biased inferences. For example, a sample-selection problem is created by the fact that the typical data set only includes those who were in an accident that meets some criteria for severity (e.g., going to an emergency room). This type of selection implies a downward bias in the effectiveness of helmets because those for whom helmets effectively prevented injuries are underrepresented in the available data.

Another potentially important complication is that helmet use may be correlated with individual-level, vehicle-level, and accident-level traits that influence injury risks but are inherently difficult, if not impossible, to observe. For example, helmet users may be more prone to engage in other complementary, precautionary behaviors (e.g., driving at lower speeds and the regular mechanical maintenance of their vehicle), which would imply an upward bias in estimates of helmet effectiveness. However, it is also reasonable to suspect that naïve cross-sectional estimates of helmet effectiveness are biased downwards because helmet users are overrepresented in particularly risky conditions (e.g., poor road conditions and riding at night). Similarly, a compensating behavioral response to helmet use implies that the estimated technological effectiveness of a helmet would be biased downward.

The evaluation literature from the field of public health has focused on a creative, panel-based approach to estimating the effectiveness of helmets in the presence of these identification challenges. The "double pairs" approach is based on analyzing a data set that consists only of accidents involving *two-rider* motorcycles in which there was at least one fatality. This approach then identifies the effectiveness of motorcycle helmets by comparing the ratio of fatality counts from cases where one rider was helmeted and another was not to the corresponding ratio from cases in which either both or none of the riders were helmeted. This approach is clearly in the spirit of panel-based econometric specifications that condition on vehicle fixed effects in that it turns on comparisons of the differences among the two occupants of each observed motorcycle. The seminal double-pairs study of motorcycle helmets by Evans and Frick (1988), which was based on data from 1975 to 1986, found that helmets reduced fatality risks by 28 percent. A more recent application of this technique, using data from 1993 to 2002, suggests that helmets have become more effective, reducing fatality risks by 37 percent (Deutermann, 2004).

However, this approach also has possibly important shortcomings relative to a more generalized panel-based regression analysis. One is that it does not accommodate the introduction of controls for traits varying among paired motorcycle riders. And the absence of such controls could be a source of confounding bias. For example, if the younger riders tended to be more likely to be unhelmeted but also more likely to survive a crash, the basic double-pairs method would understate the benefits of motorcycle helmets. Similarly, if females were more likely to wear a helmet but less likely to survive a crash, the benefits of helmets would also be biased downward. Another valuable benefit of a generalized, panel-data version of this approach is that it does not require unusually ad hoc assumptions about the relevant sampling variation (e.g., Evans and Frick, 1988, p. 451) and can accommodate accident-level or vehicle-level clustering in the error term.

In fairness, Evans and Frick (1988) acknowledge the potential biases due to the sex and age of the passengers and addressed these issues by providing sex-specific estimates of helmet effectiveness and by limiting some of their primary analysis to paired passengers who were within 3 years of age. However, these adjustments to the sample under study also suggest an additional drawback of the basic double-pairs approach, and one that has apparently received little attention: the issue of "external validity." Motorcycle accidents involving paired riders on a single motorcycle are only a small subset of overall motorcycle accidents. Furthermore, the vehicle-level and accident-level circumstances in which two-rider accidents occur appear to differ meaningfully from those of the majority of accidents. Therefore, the relevance of an estimate of helmet effectiveness based only on two-rider accidents for the majority of motorcycle riders is an open, empirical question.

In this section, I present new evidence on the effectiveness of motorcycle helmets, using a within-vehicle panel data specification that effectively generalizes the double-pairs methodology. This evidence makes several contributions to the extant literature. First, it is based on the most recently available data (i.e., through 2005), which may be relevant given the technological advances that may influence helmet quality. Second, a regression-based version of this approach provides a flexible and conventional framework for identifying how the effectiveness of helmets may vary by driver traits (i.e., driver status, sex, and age) and for calculating the standard errors necessary for statistical inference. Third, this study also uses this framework to examine the possibly heterogeneous effects of helmets on *non-fatal* injuries in addition to their effects on fatalities. Fourth, I also examine the empirical relevance of the "external validity" issue. Specifically, I examine how the observed traits of two-rider accidents compare to a nationally representative sample of police-reported accidents from the same time period. I then examine how the apparent effectiveness of helmet use differs in samples that are defined by these observable traits.

2.2. Fatality Analysis Reporting System (FARS)

The data used to evaluate helmet effectiveness were drawn from the Fatality Analysis Reporting System, which is managed by the National Highway Traffic Safety Administration (NHTSA). FARS contains data on a full census of traffic crashes that occurred on generally public roads in the United States, the District of Columbia and Puerto Rico and that resulted in at least one fatality within 30 days of the crash (NHTSA, 2006a). NHTSA coordinates the consistent construction of the FARS variables with trained state employees who have access to a variety of relevant administrative data (e.g., police accident reports, state driver licensing, state vehicle registration, death certificates and coroner reports).

This study is based on the 18 annual FARS files that describe the fatal motor-vehicle accidents that occurred between 1988 and 2005. These pooled files include information on over 1.8 million persons. Just over 60,000 of these individuals were riding motorcycles. Of these motorcyclists, nearly 57,000 observations have valid data on injury status, helmet use, age, and driver/passenger status. Approximately 28 percent of these observations were riding on a motorcycle with one other person. The observations from these two-rider vehicles – 15,710 riders from 7855 vehicles – constitute the analytical sample used here to evaluate the effectiveness of helmet use.[2] However, in order to be clear about the observations that effectively identify helmet efficacy, some evaluations also focus on the subset of observations (1426 observations from 713 vehicles) with within-vehicle variation in helmet use (i.e., $\bar{H}_j = 0.5$).

It should be noted that the FARS variable for helmet use does not identify whether the helmet was compliant with Federal safety standards (e.g., coverage and thickness). Data from the 2006 National Occupant Protection Use Survey (Glassbrenner and Ye, 2006) indicate that 51 percent of motorcyclists use compliant helmets while 14 percent use non-compliant helmets. Because these evaluations cannot discriminate among different types of helmets, they may understate the true health benefits of using helmets that are compliant with Federal standards. However, this homogeneous measure of helmet use may be appropriate for the empirical benchmarks used in this study to assess whether risk-compensating behaviors attenuate the health benefits of state helmet-use laws. Some states that require helmet use do not stipulate that these helmets should be compliant with Federal safety standards and, in states that do make that requirement, not all motorcyclists respond by using helmets that are compliant.

The FARS coding protocol categorizes the injuries of individuals involved in fatal accidents according to the "KABCO" scheme. More specifically, individuals are categorized, often by responding police officers, as belonging to one of five categories: a fatality (K), an incapacitating injury (A), a non-incapacitating injury (B), a possible injury (C), and no injury (O). While the coding of fatalities is subject to little

[2] An additional 72 persons were identified as riding a motorcycle with 3 or 4 passengers. These observations were excluded from the 2-rider sample, though including them has little effect on the results.

Table 1
Variables and descriptive statistics, 1988–2005 FARS two-rider and GES samples.

Variable	Sample		
	FARS	FARS $\mid \bar{H}_j = 0.5$	GES
Injury outcomes			
Fatality	0.572	0.575	0.030
Incapacitating injury or worse	0.857	0.856	0.351
Non-incapacitating injury or worse	0.962	0.961	0.874
Possible injury or worse	0.985	0.984	0.936
Other individual traits			
Helmet use	0.451	0.500	0.588
Driver	0.500	0.500	0.891
Male	0.639	0.678	0.877
Age	32.3	29.0	32.8
BAC ≥ 0.08	0.170	0.177	n/a
Vehicle traits			
Engine ≥ 750 CC	0.401	0.307	0.249
Model year ≥ 1990	0.434	0.435	0.518
Front impact	0.568	0.575	0.354
Side impact	0.189	0.163	0.174
Accident traits			
1 vehicle	0.420	0.466	0.499
2 vehicles	0.500	0.467	0.470
Nighttime (6:00 p.m. to 5:59 a.m.)	0.746	0.783	0.585
Weekend (Saturday or Sunday)	0.508	0.489	0.383
Speed Limit ≥ 55	0.402	0.337	0.193
Sample size	15,710	1426	23,889

This FARS 2-rider sample consists of motorcycle drivers and passengers from two-rider motorcycles involved in fatal traffic accidents between 1988 and 2005 (n = 15,710). The second FARS sample refers to the subset of the two-rider sample, which had within-vehicle variation in helmet use (i.e., $\bar{H}_j = 0.5$). The GES sample consists of motorcycle riders in police-reported accidents from 1998 to 2005.

ambiguity, it should be noted that the other injury classifications may be somewhat crude proxies relative to more sophisticated injury scores that require more extensive medical information and expertise (Compton, 2005).

Table 1 presents descriptive statistics on the injury outcomes and other observed traits of the FARS two-rider sample. Not surprisingly, given the purposively selective nature of the FARS data collection, fatalities are quite common in this sample. Over 57 percent of the observed motorcyclists were killed. And nearly 86 percent had an incapacitating injury or were killed. The unconditional probabilities of having a non-incapacitating (or worse) or a possible injury (or worse) were even higher (i.e., about 96 and 98 percent, respectively). The average motorcyclist in the full two-rider sample was 32 years old. Furthermore, approximately 17 percent of these motorcyclists had a recorded blood-alcohol concentration of 0.08 or higher, the legal limit in most states.[3] Roughly 45 percent of these motorcyclists were wearing helmets and 64 percent were male. However, the average motorcyclist was more likely to be male (i.e., 67.8 percent) and somewhat younger (i.e., 29 years old) in the sub-sample of observations from vehicles where only one helmet was in use. By construction, exactly half of the observations were drivers instead of passengers in both FARS samples.

The remaining rows in Table 1 identify the mean values of other vehicle and accident-level traits. These include whether the motorcycle had a relatively large engine (i.e., greater than 750 cubic centimeters of displacement) and whether the motorcycle was a relatively recent model (i.e., after 1990). Two dummy variables identify whether the vehicle was involved in a frontal or side collision. Two other dummy variables identify whether the number of vehicles involved in the accident were 1 or 2 where the reference category is 3 or more. The remaining variables identify whether the accident occurred at nighttime, on a weekend and on a road with a speed limit of 55 MPH or higher.

The third column in Table 1 provides information on how the two-rider sample compares to a representative sample of accidents from the same time period. More specifically, I used data from the pooled 1988–2005 General Estimates Survey (GES). The GES is an annual, nationally representative survey of police-reported accidents that uses data-coding protocols similar to those used in FARS. The GES data are based on a 3-stage, stratified sample of police accident reports (NHTSA, 2006b). The pooled files from this period include information on nearly 2.5 million individuals. However, only about 27,000 of these individuals were riding a motorcycle. And, of this group, roughly 24,000 have valid data on injury severity, age, gender, driver status and helmet use.

The third column in Table 1 reports the sample means for the observed individual, vehicle and accident-level traits of the motorcyclists observed in the GES surveys.[4] A comparison of the observed traits of the FARS and GES data in Table 1 indicates that two-rider accidents observed in the FARS differ from the typical motorcycle accident in a number of ways. For example, the two-rider accidents are more likely to involve a motorcycle with a more powerful engine as well as an older motorcycle. The motorcycles observed in the two-rider sample are also more likely to be observed in a frontal impact and in an accident that occurs either at nighttime or on a weekend. The two-rider accidents are also more likely to occur on a road with a speed limit of 55 MPH or higher. As suggested earlier, it is possible that inferences

3 Riding under the influence of alcohol is a potentially important determinant of both injury outcomes and helmet use. However, whether BAC is measured is likely to be endogenous to the injury outcome. Nonetheless, some of the specifications reported here condition on this BAC measure as a robustness check.

4 These means were not adjusted for the design effects implied by the GES sampling design. However, means that do adjust for these effects are similar to those reported here.

about the effectiveness of helmets that are based on the two-rider sample may not accurately generalize to other accidents (e.g., those on weekdays or on roads with lower speed limits). The analysis of the FARS sample presented below addresses these issues by examining how the apparent effectiveness of helmet use varies by such traits.

2.3. Specification

The basic econometric specification used to analyze the determinants of injury outcomes in the two-rider sample takes the following form:

$$Y_{ij} = \beta X_{ij} + \gamma H_{ij} + \alpha_j + \varepsilon_{ij}$$

where Y_{ij} is a binary indicator for a particular injury outcome of individual i in vehicle j, X_{ij} reflects the observed traits of each motorcyclist (e.g., male, driver, and age), and H_{ij} is a binary indicator for helmet use. The term, α_j, represents fixed effects unique to each motorcycle observed in the FARS two-rider sample. The effects of helmet use on injury outcomes are identified in this specification by the variation in helmet use among the riders of each given motorcycle (i.e., the *within-vehicle* variation in helmet use and injury outcomes). As suggested previously, this fixed-effect specification and its implied identification strategy generalizes the simple "double-pairs" comparisons introduced by Evans and Frick (1988). However, this generalized regression improves upon that approach by providing a framework for evaluating threats to the internal validity of this research design. Specifically, this approach allows for the estimated effects of helmet use to be evaluated conditional on various observed traits (i.e., X_{ij}) that vary among the riders of a given motorcycle and may be correlated both with helmet use and injury outcomes.

This generalized regression-based version also improves upon the prior double-pair comparisons by allowing for the conventional estimation of standard errors and for classical hypothesis testing. The error term in this model, ε_{ij}, is assumed to reflect heteroscedasticity clustered at the vehicle level, a conservative approach which appears to increase the standard errors appreciably. The results of linear probability models based on this specification are presented here. However, it should be noted that alternative specifications (e.g., conditional logits) return similar results. Furthermore, in order to provide comparative evidence on the cross-sectional identification strategy used in some prior studies (e.g., Goldstein, 1986; Weiss, 1992), some specifications exclude the vehicle fixed effects but condition on the vehicle and accident-level observables listed in Table 1.

2.4. Baseline results

Table 2 presents the key results from alternative specifications where the fatality indicator is the dependent variable. In model (1), which excludes vehicle fixed effects, helmet use is associated with a small but statistically significant reduction in fatality risk. More specifically, these cross-sectional comparisons suggest that helmet use reduces fatality risk by only 3 percent (i.e., a 1.8 percentage point shift relative to a mean of 57.8 percent). However, in the models that condition on vehicle fixed effects, the estimated reduction in fatality risk implied by wearing a helmet is substantially larger (as well as statistically significant). These comparative results indicate that regression-adjusted cross-sectional comparisons can be highly biased against the effectiveness of helmet use. Furthermore, this pattern implies that the helmet use is correlated with the unobserved determinants that contribute to fatality risk (e.g., driving in riskier conditions).

The effect sizes implied by these point estimates are quite large. For example, the full-sample result that conditions on driver status, sex, and age as well as vehicle fixed effects suggests that helmet use reduces the probability of dying by 24.1 percentage points, a 42 percent reduction relative to the fatality probability among unhelmeted motorcyclists (i.e., 57.8 percent). However, this approach appears to overstate the effectiveness of helmet use simply because the benchmark fatality rate for those not wearing helmets (i.e., $\bar{Y}|H_{ij} = 0$) is larger when motorcycles without within-vehicle variation in helmet use are included. A model based only on observations where $\bar{H}_j = 0.5$ suggests that helmet use reduces fatality risk by 34 percent (i.e., a 24 percentage point shift relative to a baseline risk of 70.7 percent). This estimate is larger than that reported by Evans and Frick (1988) for earlier years but somewhat smaller than that reported by Deutermann (2004) using more recent data.

The regression estimates in Table 2 suggest that the prior double-pairs estimates have produced roughly accurate estimates of the technological effectiveness of helmets. However, both those earlier results and the panel-data estimates presented here turn on the key identifying assumption that the within-vehicle variation in helmet use can be viewed as conditionally random. The validity of that assumption cannot be definitively confirmed. Nonetheless, a number of factors suggest that the estimates in Table 2 can be reliably interpreted as causal.

For example, it may be that the helmeted riders on a given motorcycle are more or less likely to wear other protective gear (e.g., jackets and gloves). However, the use of protective gear is an unlikely source of bias (at least with regard to fatalities and more serious injuries) because the available evidence suggests that they are only effective at reducing rarely serious injuries like abrasions and lacerations (Hurt et al., 1981). Furthermore, the observed individual traits that do clearly predict the within-vehicle variation in helmet use (e.g., drivers, females, and younger riders are more likely to use a helmet) are at best weakly related to fatality outcomes (e.g., model (7) in Table 2). This pattern of selection on observables suggests that selection on unobserved traits is an unlikely source of bias. Similarly, the estimated effect of helmet use was quite similar across specifications that conditioned on fully general interactions between sex, driver status and age. Furthermore, there were no statistically significant interactions between these observed traits and helmet use.

However, patterns of alcohol use are a potential source of bias in these evaluations. Motorcyclists with a recorded blood-alcohol concentration (BAC) of 0.08 or higher are both less likely to wear a helmet and more likely to die in a crash. This pattern suggests that naïve within-vehicle estimates that do not control for alcohol use may overstate the true efficacy of helmets. To examine the empirical relevance of this issue, some of the results in Table 2 (i.e., models (4) and (8)) condition on BAC status. The results indicate that controlling for this potentially endogenous measure has relatively little effect on the estimated efficacy of helmet use. The robustness of the estimated helmet-use effect reflects the fact that, though drunk riders are significantly less likely to use a helmet, the magnitude of this effect (i.e., 2.8 percentage points) is small relative to the fatality consequences of being drunk and the quite large effects of helmet use on the probability

Table 2
OLS estimates of fatality determinants, 1988–2005 FARS 2-rider samples.

Independent variable	Full sample ($n = 15,710$)				$\bar{H}_j = 0.5$ ($n = 1426$)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Helmet	−0.018‡ (0.006)	−0.264‡ (0.045)	−0.241‡ (0.045)	−0.212‡ (0.045)	−0.255‡ (0.033)	−0.264‡ (0.045)	−0.240‡ (0.048)	−0.220‡ (0.049)
Driver	0.094‡ (0.013)	–	0.121‡ (0.024)	0.039 (0.024)	0.050 (0.041)	–	0.021 (0.074)	−0.032 (0.075)
Male	−0.057‡ (0.013)	–	−0.153‡ (0.028)	−0.137‡ (0.028)	0.026 (0.039)	–	0.030 (0.090)	−0.038 (0.090)
Age	0.0025‡ (0.0003)	–	0.013‡ (0.001)	0.012‡ (0.001)	0.002* (0.001)	–	0.008* (0.004)	0.008* (0.004)
BAC $\geq$ 0.08	–	–	–	0.450‡ (0.026)	–	–	–	0.277‡ (0.091)
$\bar{Y}\|H_{ij} = 0$	0.5781				0.7069			
R^2	0.0155	0.2240	0.2474	0.2902	0.0811	0.2561	0.2671	0.2832
Vehicle traits	Yes	No	No	No	Yes	No	No	No
Accident traits	Yes	No	No	No	Yes	No	No	No
Vehicle fixed effects	No	Yes	Yes	Yes	No	Yes	Yes	Yes

Standard errors, adjusted for vehicle-level clustering, are reported in parentheses.
* Statistically significant at the 10-percent level.
‡ Statistically significant at the 1-percent level.

Table 3
Estimated effect of helmet use by injury outcome, cross-sectional and within-vehicle comparisons, 1988–2005 FARS 2-rider sample.

Dependent variable	(1)		(2)	
	$\hat{\gamma}$	R^2	$\hat{\gamma}$	R^2
Fatality	−0.018‡ (0.006)	0.0155	−0.241‡ (0.045)	0.2474
Incapacitating injury or worse	−0.007 (0.006)	0.0183	−0.115‡ (0.026)	0.5194
Non-incapacitating injury or worse	−0.006* (0.003)	0.0111	−0.041‡ (0.013)	0.6042
Possible injury or worse	−0.004* (0.002)	0.0109	−0.018† (0.008)	0.6609
Vehicle traits	Yes		No	
Accident traits	Yes		No	
Vehicle fixed effects	No		Yes	

All models condition on driver status, sex, and age. Standard errors, adjusted for vehicle-level clustering, are reported in parentheses. The sample size is 15,710.
* Statistically significant at the 10-percent level.
† Statistically significant at the 5-percent level.
‡ Statistically significant at the 1-percent level.

of a fatality. However, a potential drawback with this approach to controlling for alcohol use is that BAC status is not uniformly ascertained for individuals involved in fatal accidents. To assess the empirical relevance of this measurement-error problem, the effects of helmet use were also estimated when the two-rider sample was limited to accidents where at least one rider has a recorded BAC (i.e., state and police practices were such that this was being measured). The estimated effects of helmet use in this sample were quite similar to those reported here.

Table 3 presents the estimated effects of helmet use along different margins of injury severity (i.e., the four injury indicators described in Table 1). In order to provide some continuity with respect to prior cross-sectional evaluations, these results are based on the larger two-rider sample and two specifications: one that omits vehicle fixed effects but includes controls for individual, vehicle and accident-level observables and the preferred specification that includes vehicle fixed effects and the other individual-level observables (i.e., driver status, sex, and age). These results uniformly suggest that helmet use is associated with reductions in injury severity. For example, helmet use implies a reduction in the probability of an incapacitating injury or worse of roughly 11 percentage points. Furthermore, as with the fatality results, these estimated effects are larger and statistically significant in the specifications that control for accident and vehicle unobservables through the use of fixed effects. These comparative results suggest that cross-sectional comparisons that attempt to control for the observed traits of accidents and vehicles are likely to understate the effectiveness of helmets in preventing injuries of varying severity.

The corresponding effects of helmet use on the probabilities of a non-incapacitating injury (or worse) and a possible injury (or worse) are 4.1 and 1.8 percentage points, respectively. Interestingly, these results suggest that helmet use is noticeably less effective in preventing less serious injuries. This is not entirely surprising given that the KABCO injury coding does not distinguish between injuries to the head and to other parts of the body. However, it should also be noted that the FARS necessarily consists of accidents serious enough for there to have been at least one fatality. The effectiveness of helmet use may differ in accidents that are less serious.

2.5. External validity

Overall, these results imply that helmets are effective at preventing injuries, particularly so with respect to fatalities. However, one potentially substantive caveat to these results is that these inferences may be valid only for vehicles and accident circumstances similar to those that characterize the FARS two-rider sample. And, as the comparative data in Table 1 indicate, the FARS two-rider sample differs from a representative sample of motorcycle accidents in a number of ways. For example, the FARS two-rider accidents are more likely to involve motorcycles with larger engines, earlier model years, and frontal or side impacts. The FARS two-rider sample is also more likely to involve accidents that occur at nighttime, on weekends and on roads with speed limits in excess of 55 MPH.

Table 4 presents evidence on the external validity of the FARS two-rider results by estimating helmet effectiveness in samples defined by traits that more like those in the general population of motorcycle accidents. All of these models are based on motorcycles with within-vehicle variation in helmet use ($n = 1426$) and these specifications condition on vehicle fixed effects and the available individual-level traits (i.e., driver status, age, and sex).

As a point of comparison, the first row of Table 4 presents results based on the full sample of 1426 observations. Overall, the remaining results in Table 4 indicate that the estimated effectiveness of wearing a helmet is quite similar across these samples. In particular, helmet use is similarly effective in preventing fatalities on later-model motorcycles, on motorcycles with smaller engines and in accidents that occur on weekdays or on roads with lower speed limits. Furthermore, helmet use appears even more effective for those on motorcycles that were not involved in a frontal or side impact (i.e., reducing fatality risk by 51 percent relative to the mean fatality rate among unhelmeted riders).

These results suggest that, if anything, the inferences based on the FARS two-rider sample are likely to provide a lower bound on the overall effectiveness of helmet use. The one consistent exception to this pattern is that, in daytime accidents, the effects of helmet use, though negative, are smaller and statistically insignificant. It should be noted that the daytime sample is relatively small and the 95 percent confidence intervals for γ are correspondingly wide and include the point estimates from the full sample results. Nonetheless, some of the simple policy simulations discussed below assume that helmets are effective only in nighttime crashes.

Another potentially relevant type of heterogeneity to explore is how helmet efficacy changes with respect to the introduction of a mandatory helmet law. The concern here is that the health benefits of helmet laws may be attenuated if, in the absence of helmet regulations, riders already tended to wear helmets in the most risky circumstances. This sensible scenario implies that the increases in helmet use created by helmet regulations will be on margins where they convey fewer health benefits. To examine the empirical relevance of this

Table 4
Estimated effect of helmet use on injury outcomes by vehicle and accident traits, 1988–2005 FARS 2-rider sample.

Sample definition	Dependent variable								Sample size
	Fatality		Incapacitating injury or worse		Non-incapacitating injury or worse		Possible injury or worse		
	$\hat{\gamma}$	$\bar{Y}\|H_{ij}=0$	$\hat{\gamma}$	$\bar{Y}\|H_{ij}=0$	$\hat{\gamma}$	$\bar{Y}\|H_{ij}=0$	$\hat{\gamma}$	$\bar{Y}\|H_{ij}=0$	
Full sample	–0.240‡ (0.048)	0.707	–0.112‡ (0.027)	0.917	–0.044‡ (0.014)	0.982	–0.019† (0.009)	0.993	1426
Engine < 750 CC	–0.227‡ (0.059)	0.692	–0.089‡ (0.032)	0.921	–0.044‡ (0.016)	0.988	–0.018† (0.009)	0.998	988
Model year ≥ 1990	–0.242‡ (0.072)	0.706	–0.110‡ (0.041)	0.910	–0.057† (0.024)	0.987	–0.019 (0.013)	0.994	620
No frontal or side impact	–0.382‡ (0.091)	0.749	–0.189‡ (0.059)	0.914	–0.099‡ (0.034)	0.989	–0.050† (0.023)	1.000	374
Daytime	–0.063 (0.110)	0.626	–0.044 (0.062)	0.884	–0.043 (0.037)	0.968	–0.017 (0.017)	0.987	310
Weekday	–0.220‡ (0.068)	0.714	–0.132‡ (0.038)	0.929	–0.056‡ (0.020)	0.986	–0.017* (0.009)	0.995	728
Speed limit < 55	–0.245‡ (0.059)	0.702	–0.107‡ (0.034)	0.907	–0.040† (0.018)	0.976	–0.007 (0.008)	0.992	946
Helmet law in effect	–0.198‡ (0.074)	0.697	–0.088† (0.044)	0.904	–0.032 (0.024)	0.973	–0.019* (0.011)	0.996	522
No helmet law in effect	–0.282‡ (0.062)	0.712	–0.133‡ (0.034)	0.924	–0.058‡ (0.019)	0.987	–0.022* (0.013)	0.991	904

All models condition on vehicle fixed effects, driver status, sex, and age. Standard errors, adjusted for vehicle-level clustering, are reported in parentheses.

* Statistically significant at the 10-percent level.

† Statistically significant at the 5-percent level.

‡ Statistically significant at the 1-percent level.

Table 5
Estimated lives and potential lives saved annually by use of motorcycle helmets.

Helmet effectiveness	Helmets effective for all motorcyclists		Helmets effective only at nighttime	
	Lives saved annually	Additional lives saved by 100% use	Lives saved annually	Additional lives saved by 100% use
10%	281	173	165	119
20%	631	347	372	237
30%	1082	520	637	356
34%	**1301**	**590**	766	403
39%	1614	676	**951**	**463**
40%	1683	694	991	474
50%	2525	867	1487	593
60%	3788	1040	2231	712

These calculations are based on 2005 data and calculations described in the text. The estimates in bold are based on the preferred estimates of helmet effectiveness.

phenomenon, I separated the two-rider sample into two samples defined by whether a mandatory helmet-use law was in effect in the given state and year. The estimated effects of helmet use across these two samples are reported in the bottom two rows of Table 4. The results are somewhat consistent with the conjectured heterogeneity in that helmet use appears modestly but consistently less effective in accidents that occur when helmet regulations are already in effect. In other words, these results are consistent with the view that the idiosyncratic variation in helmet use occurs on less effective margins when helmet use is required. Nonetheless, even in this sample, the estimated effectiveness of helmets is quite large, implying a 28 percent reduction in the risk of a fatality (i.e., 0.198/0.697).

2.6. Calculating lives saved

The results based on within-vehicle comparisons of motorcyclists indicate that helmet use is highly effective at reducing injury risk, particularly fatalities. Furthermore, the estimates based on this sample appear to have external validity for the more typical motorcycle accident, with the possible exception of those that occur in the daytime. From a policy perspective, a useful way to frame this evaluation evidence is to identify what it suggests about the number of lives that are saved by current helmet use and, more important, how many could be saved by increases in the prevalence of helmet use.

The conventional approach to this question has used estimates of the technological effectiveness of helmets and ignored the possible ways in which helmet use (and corresponding regulations) might also influence risk-taking behaviors (e.g., Deutermann, 2005). More specifically, if we assume that there is some potential number of fatalities among helmeted motorcyclists (i.e., F_{PH}), the number of lives saved by helmet use, L, is simply

$$L = F_{PH}E$$

where E is the effectiveness of motorcycle helmets in reducing fatality risk. Of course, F_{PH} is not observed. However, the number of fatalities observed among helmeted motorcyclists (i.e., F_H) equals

$$F_H = F_{PH}(1 - E).$$

Combining these equations indicates that the number of lives saved by helmet use, L, can be estimated using the effectiveness of helmet use, E, and the observed number of fatalities where helmets were used:

$$L = F_H \frac{E}{(1 - E)}$$

The number of *additional* lives that could be saved through universal use of motorcycle helmets is given by the number of unhelmeted motorcycle fatalities (i.e., F_{UH}) multiplied by helmet effectiveness: EF_{UH}. In 2005, the most recent year for which the FARS data are currently available, there were 2525 helmeted fatalities and 1734 unhelmeted fatalities. Table 5 uses these annual data for F_H and F_{UH} along with different assumptions about helmet effectiveness to identify the number of lives saved annually by helmet use as well as the additional lives that could be saved through universal helmet use. The preferred estimate of E, 34 percent (i.e., Model (7), Table 2), implies that, in 2005, helmet use saved 1301 lives and that an additional 590 lives would have been saved through universal use.

One caveat to these calculations is that they assume that motorcycle helmets are effective in all circumstances. However, the results in Table 5 suggested that helmets may only be effective in reducing fatalities that occur at nighttime. The assumption that helmets are only effective in a subset of accidents could obviously lower the estimated number of lives saved. In particular, during 2005, there were 1487 nighttime fatalities among helmeted motorcyclists and 1186 nighttime fatalities among unhelmeted motorcyclists. However, a specification that includes vehicle fixed effects and the other individual-level observables also suggests that helmet use is more effective at nighttime, reducing fatality risk by 39 percent (i.e., a 28.4 percentage-point effect relative to 72.9 percent fatality rate among unhelmeted nighttime motorcyclists).

The final two columns of Table 5 combine this information to identify the number of lives saved annually by helmet use and the number of additional lives that could be saved through universal usage, under the assumption that helmets are only effective at nighttime. Using the preferred estimate for helmet effectiveness in nighttime accidents (i.e., 39 percent) implies that helmet use saved 951 lives in 2005 and that an additional 463 lives could have been saved through universal use.

3. The effects of motorcycle helmet laws

The conventional lives-saved calculations illustrated in the previous section may not correspond to the actual effects of regulations that influence helmet use for a number of reasons. The most well-known complication is the so-called Peltzman hypothesis, which suggests that the live-saving effects of regulations like mandatory helmet use may be attenuated by compensating increases in risk-taking among drivers (Peltzman, 1975). Furthermore, as noted in the previous section, any regulation-induced increases in helmet use may occur on margins in which those helmets are least effective.

However, there are also at least two ways in which laws that require helmet use may reduce fatalities by *more* than what would simply be implied by increases in helmet use and the technological effectiveness of helmets. For example, a binding helmet law would necessarily focus police-enforcement efforts on motorcyclists to some increased degree. This increased police attention could promote traffic safety in ways that are unrelated to helmet use (e.g., less reckless or speedy driving). Second, the public debate over motorcyclist helmet laws suggests that some non-trivial number of motorcyclists emphatically prefer not to wear them. Therefore, in the presence of a mandatory helmet law, some motorcyclists might choose to ride less, or even not at all.[5] This reduction in risk exposure would reduce observed fatalities in a way that has nothing to do with the direct effect of helmet use.

Between 1966 and 1976, the Federal government encouraged states to mandate helmet use among all motorcyclists through the threat of withholding highway construction funds and virtually all states complied. Since the Federal government changed this policy, states have been dramatically reducing legal requirements related to helmet use. In 1977, 47 states had mandatory helmet laws that applied to all riders. Currently, only 20 states have such requirements (Jones and Bayer, 2007).

A number of studies have examined the fatality consequences of state-level laws that mandate helmet use. One of the most credible studies (Sass and Zimmerman, 2000) examined the effect of mandatory helmet use laws using state-level panel data on fatalities from the 1976 to 1997 period and a two-way fixed effect specification. They found that helmet use was associated with a 29–33 percent reduction in fatalities. Two more recent studies that used a similar two-way fixed effect specification (Houston and Richardson, 2008; French et al., 2008) applied to data from different time periods found that state helmet-use laws reduced fatalities by at least 22 percent.

In this section, I present new estimates of the effects of state laws that mandate motorcycle helmet use for adults. This evidence adds to the extant literature in a number of ways. First, unlike the other recent studies with the exception of Houston and Richardson (2008), this analysis focuses on the most recent data and state law changes. In particular, this analysis uses data from the 1988–2005 period, which corresponds to this study's evidence on helmet effectiveness. Eleven states varied their adult motorcycle helmet laws over this period. Four states repealed their mandates (AR, FL, KY and PA) and five states introduced a new motorcycle helmet law (CA, OR, WA, NE, MD). Louisiana repealed their law over this period and reinstated it again while Texas reinstated their law over this period and then repealed it.

A second contribution of this evidence is that the estimated law's effects are compared to explicit benchmark values constructed under the assumption that there are compensating changes in risk-taking in response to mandatory helmet use. A comparison of the laws' actual estimated effects to these benchmark values provides an indirect way to assess the empirical relevance of the Peltzman hypothesis and of alternative mechanisms for the laws' effects. Third, this evidence also explores in a number of ways the robustness of the prior evidence suggesting the effectiveness of these laws. One is by introducing controls for state-specific linear, quadratic, and cubic trends in motorcyclist fatalities. The results presented here correct for the possible serial correlation common to panel-based specifications of this type (Bertrand et al., 2004). Furthermore, this study provides evidence on whether helmet use laws were changed endogenously with respect to trends in motorcyclist fatalities.

3.1. Benchmarking the effect size

Evans (1987) outlines a straightforward way to identify the percent reduction in fatalities that could be anticipated from the technological effectiveness of helmets. Specifically, consider a population of n motorcyclists in which h identifies the fraction that use helmets, E identifies the percent effectiveness of helmets in reducing fatalities and c identifies the rate of potentially fatal crashes (i.e., crashes that are potentially fatal to those with helmets and fatal to those who are not). The assumption that there is no risk-compensating behavior associated with helmet use implies that the crash rate is the same among both helmeted and unhelmeted motorcyclists. Under these assumptions, the number of motorcyclist fatalities, F, can be expressed as

$$F = nc(h(1-E) + (1-h)) = nc(1-Eh)$$

A change in the rate of helmet use of Δh implies that the number of fatalities would change by $ncE\Delta h$. Dividing this expression by the number of fatalities above implies that the percent reduction in fatalities implied by a given change in helmet use and the absence of risk-compensating behavior is

$$\%\Delta F = \frac{E\Delta h}{(1-Eh)}$$

Data from the 2006 National Occupant Protection Use Survey (NOPUS) indicate that the rate of motorcycle-helmet use in states that do not require helmet use is approximately 50 percent (Glassbrenner and Ye, 2006). Given this information, what is the percent fatality reduction that could be expected if such states introduced mandatory use laws that were not compromised by behavioral changes? The answer depends both on the effectiveness of helmets (i.e., E) and the corresponding effectiveness of mandatory state laws in actually increasing helmet use (i.e., Δh). Table 6 uses the formula above to identify the percent reduction in motorcyclist fatalities that could be expected under different assumptions about E and Δh, the assumption that h equals 0.5, and the presumed absence of compensating behaviors.

The results vary substantially along with the underlying assumption about the effects of helmet laws on use rates as well as about the effectiveness of helmets. More specifically, based on the parameters, any law-induced reduction in fatalities that is below 33 percent could be consistent with the existence of some degree of compensating risk taking. However, under the preferred assumption about the effectiveness of motorcycle helmets (i.e., 34 percent), mandatory helmet use laws could be expected to reduce fatalities by no more than

[5] A recent study by Carpenter and Stehr (2007) finds evidence of this sort in the context of bicycle-helmet laws (namely, that helmet laws lead to less bicycle riding).

Table 6
Expected percent reduction in fatalities due to mandatory helmet use law.

Helmet effectiveness	Change in rate of helmet use due to law			
	0.20	0.30	0.40	0.50
20%	4%	7%	9%	11%
30%	7%	11%	14%	18%
34%	**8%**	**12%**	**16%**	**20%**
40%	10%	15%	20%	25%
50%	13%	20%	27%	33%

The estimates based on the preferred estimate of helmet effectiveness are in bold. The calculations for this table are described in the text. The presumed baseline rate of helmet use is 50 percent (Glassbrenner and Ye, 2006).

20 percent. Furthermore, the expected fatality reduction due to a mandatory helmet law would only be 14 percent if we assume that the increase in helmet use due to a helmet-use law (i.e., Δh) is similar to the simple cross-state difference observed in the 2006 NOPUS (Glassbrenner and Ye, 2006) across states with and without mandatory laws (i.e., 33 percentage points).

3.2. Data and specifications

The evidence from earlier years indicating that helmet laws reduce motorcyclist fatalities by 29–33 percent (Sass and Zimmerman, 2000) suggests not only the absence of empirically relevant risk-taking responses to these laws but also the possibility that the laws are more effective than expected (e.g., because they reduce the popularity of motorcycles). This section explores these issues using annual data from the 48 contiguous states during the period from 1988 to 2005 ($n = 864$), which corresponds to the evaluation of helmet effectiveness. Specifically, I constructed state-year counts of motorcyclist fatalities for this period using the FARS data described previously. I also identified counts of daytime and nighttime fatalities to assess whether motorcycle helmet laws differed over this period.

The basic specification used to evaluate the effect of laws that require helmet use takes the following form:

$$Y_{st} = \beta X_{st} + \delta H_{st} + \alpha_s + \mu_t + \varepsilon_{st}$$

where Y_{st} is the natural log of motorcyclists fatalities in state s during year t, X_{st} reflects a variety of observables for state s in year t, and H_{st} is a variable that indicates the fraction of the calendar year that a mandatory adult motorcycle-helmet law was in effect. The terms, α and μ, represent state and year fixed effects respectively while ε is a mean-zero error term. Count-data models such as negative binomial regressions return results similar to those reported here. The standard errors reported here are adjusted for clustering at the state level (Bertrand et al., 2004).

In every specification, X_{st} includes the natural log of the state population as a measure of exposure. Other specifications introduce state-by-year observables such as the state unemployment rate, dummy variables for maximum speed limits (one for 65 MPH, one for 70 MPH and up), for mandatory seat-belt laws (one for primary enforcement, one for secondary enforcement) and for graduated driver licensing (Dee et al., 2005). Additional state-year control variables identify specific drunk-driving policies: administrative per se laws, illegal per se laws (one for 0.08 BAC, another for 0.10 BAC or higher), and one for zero-tolerance laws. Some specifications also introduce state-specific linear, quadratic and cubic trend variables to control for the unobservables varying within states over time.

3.3. Results

The key identifying assumption in the basic panel-based specification used here is that the timing of helmet-law changes within states is unrelated to trends in the unobserved determinants of motorcyclist fatalities. However, this assumption would be violated under the plausible scenario in which states introduced or removed mandatory helmet laws partially in response to trends in motorcyclist fatalities. For example, if states with idiosyncratic trends towards higher motorcyclist fatalities (e.g., through increased ownership) were more likely to revoke mandatory helmet laws, the basic "difference in differences" approach would overstate the health benefits of these laws.

One heuristic way to check for this sort of "policy endogeneity" would be to examine how leads and lags in helmet-law changes relate to motorcyclist fatalities. Fig. 2 summarizes the key results from this sort of exercise. More specifically, Fig. 2 identifies the point estimates for dummy variables on leads and lags of law changes in a model where the natural log of motorcyclist fatalities is the dependent variable and the independent variables include state fixed effects, year fixed effects, and the natural log of the state-year population. Separate dummy variables identify whether each state-year observation is up to 3 years before a law change and up to 6 or more years after the change. These dummy variables are defined separately for states that introduced and revoked helmet laws over this period.[6] This approach makes it possible to assess whether the removal and introduction of a helmet law appear to have symmetrical effects, which is another assumption implicitly made by the regression model. The reference category for these dummy variables is being 4 or more years prior to a law change or not having within-state variation in helmet laws over this period. A longer set of "pre-treatment" dummies was not included simply because most new state helmet laws were introduced within the first few years of the study period.

The results in Fig. 2 indicate that changes in state helmet laws led to distinct changes in motorcyclist fatalities. More specifically, both the introduction and revocation of helmet laws are associated with clear and respective decreases and increases in motorcyclist fatalities. Furthermore, the fatality consequences of introducing and removing a helmet law appear roughly symmetrical.[7] However, the results in

[6] The two states that both removed and introduced helmet laws over this period (i.e., TX and LA) are excluded. However, including them does not substantively change the results.

[7] This result is confirmed in a hypothesis test. The null hypothesis that the effects of helmet laws are the same in states that introduced them and those that removed them cannot be rejected (p-value = 0.6641).

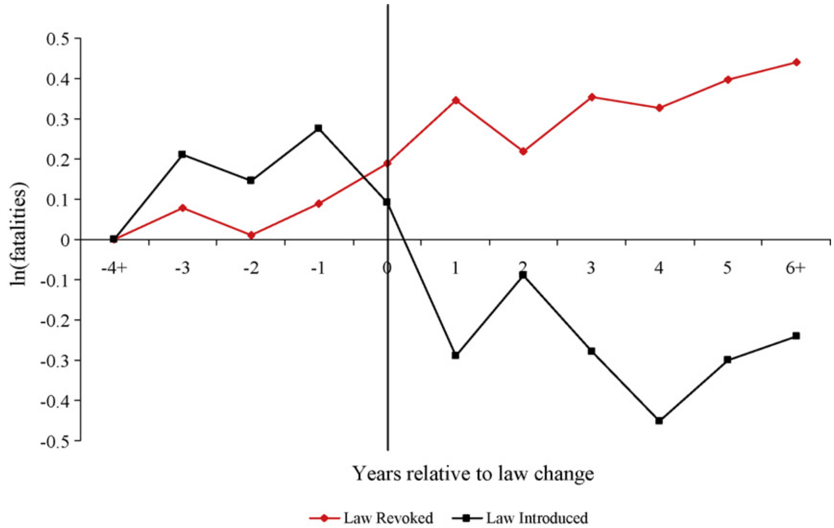

Fig. 2. Motorcyclist fatalities and the timing of helmet law changes, 1988–2005.

Table 7
Estimated effect of state mandatory motorcycle-helmet laws on motorcycle registrations.

Specification	Estimate	R^2
State fixed effects, year fixed effects, and ln(population)	−0.249‡ (0.053)	0.9341
Previous model and state-year observables	−0.250‡ (0.056)	0.9370
Previous model and state-specific linear trends	−0.111† (0.045)	0.9582
Previous model and state-specific quadratic trends	−0.036 (0.052)	0.9643
Previous model and state-specific cubic trends	−0.057 (0.044)	0.9668

The dependent variable is the natural log of registered motorcycles ($n = 864$). Standard errors, adjusted for state-level clustering, are reported in parentheses.
† Statistically significant at the 5-percent level.
‡ Statistically significant at the 1-percent level.

Fig. 2 also suggest that both the revocation and, to a greater extent, the introduction of helmet laws were preceded by trends towards increased motorcyclist fatalities. The possible bias created by such trends motivates examining the robustness of the results by introducing state-specific trend variables as controls in some reduced-form specifications.

Another potential complication to interpreting the reduced-form effect of helmet laws on fatalities is that these laws may reduce fatalities by reducing the amount of motorcycle riding in addition to their more direct effects through the prevalence of helmet use. In order to assess this issue, I estimated fixed effect specifications where the natural log of state-year motorcycle registrations was the dependent variable.[8] Table 7 summarizes the key results from this exercise. Some of these specifications suggest that helmet laws led to large and statistically significant reductions in motorcycle ownership. For example, helmet laws imply an 11 percent reduction in motorcycle registrations in models that include linear state-specific trends and the other state-year observables. However, in models that introduce non-linear state-specific trends, this effect, though still negative, is smaller and statistically indistinguishable from zero.

Nonetheless, this finding complicates an assessment of the Peltzman hypothesis based on comparing the reduced-form effect of helmet laws on fatalities to the empirical benchmarks in Table 6. In particular, if risk-compensating behavior attenuates the life-saving benefits of helmet laws, that attenuation could be obscured when these laws also reduce motorcycle travel and ownership. As an ad hoc way to assess the relevance of this concern, some of the results identify the effect of helmet laws on fatalities conditional on the natural log of state-year motorcycle registrations.

Table 8 presents the key results from two-way fixed-effect specifications that examine the effects of state motorcycle-helmet laws on motorcyclist fatalities. In a specification that controls only for population, state and year fixed effects, the estimated value of δ is −0.364. Given the size of this and the other point estimates reported in Table 8, the semi-log coefficient is a fairly poor approximation to the percent change in fatalities. Therefore, percent change is identified here as $(\exp(\hat{\delta}) - 1)$. For example, conditional on the state-year observables, the implied reduction in motorcyclist fatalities due to helmet laws is 32 percent (i.e., $\exp(-0.393) - 1$). This basic result is quite robust to introducing additional controls for linear, quadratic, and even cubic, state-specific trends as well as to introducing the natural log of motorcycle registrations. For example, model (7), which includes all of these controls, indicates that helmet laws reduced motorcyclist fatalities by 27 percent (i.e., $\exp(-0.312) - 1$).

The second and third rows in Table 8 report the estimated effects of helmet laws on nighttime and daytime motorcyclist fatalities. Helmet laws were associated with large and statistically significant reductions in both types of fatalities. However, the fatality

[8] I would like to thank Kitt Carpenter for providing these data, which are available from editions of NHTSA's annual *Highway Statistics* volume. Measures of person-miles traveled by motorcycle would be a preferred measure of utilization. However, state-year estimates of motorcycle travel are not consistently available. Fortunately, the national time-series variation in motorcycle registrations appears to track estimates for miles traveled by motorcycle well. Similarly, the correspondence between motorcycle registrations and motorcyclist fatalities (Fig. 1) suggests that it is a valid proxy for utilization.

Table 8
Estimated effect of state mandatory motorcycle-helmet laws on motorcyclist fatalities and registrations, 1988–2005 FARS.

Dependent variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Total motorcyclist fatalities	−0.364‡ (0.049)	−0.393‡ (0.045)	−0.370‡ (0.046)	−0.342‡ (0.060)	−0.299‡ (0.056)	−0.314‡ (0.056)	−0.312‡ (0.055)
R^2	0.9376	0.9405	0.9410	0.9478	0.9523	0.9562	0.9562
Nighttime motorcyclist fatalities	−0.362‡ (0.049)	−0.387‡ (0.048)	−0.361‡ (0.049)	−0.308‡ (0.076)	−0.193‡ (0.069)	−0.195* (0.103)	−0.192* (0.103)
R^2	0.9161	0.9184	0.9190	0.9253	0.9322	0.9375	0.9376
Daytime motorcyclist fatalities	−0.369‡ (0.078)	−0.414‡ (0.060)	−0.396‡ (0.062)	−0.436‡ (0.096)	−0.533‡ (0.112)	−0.598‡ (0.207)	−0.599‡ (0.208)
R^2	0.8644	0.8688	0.8691	0.8811	0.8895	0.8964	0.8964
State-year observables	No	Yes	Yes	Yes	Yes	Yes	Yes
ln(motorcycle registrations)	No	No	Yes	No	No	No	Yes
State-specific trends (linear)	No	No	No	Yes	Yes	Yes	Yes
State-specific trends (quadratic)	No	No	No	No	Yes	Yes	Yes
State-specific trends (cubic)	No	No	No	No	No	Yes	Yes

The dependent variable is the natural log of motorcyclist fatalities ($n = 864$). All models condition on the natural log of the state-year population, state fixed effects and year fixed effects. Standard errors, adjusted for state-level clustering, are reported in parentheses.

* Statistically significant at the 10-percent level.

† Statistically significant at the 5-percent level.

‡ Statistically significant at the 1-percent level.

reductions were particularly large for daytime fatalities, a pattern that becomes starker in the specifications that include more controls.

The percent reduction in motorcyclist fatalities associated with these laws exceeds (or is in the upper range) of the magnitudes that would be expected based solely on the technological efficacy of helmets and reasonable changes in usage rates. These surprisingly large fatality reductions are inconsistent with the presence of empirically relevant increases in risk-taking behavior due to the laws. Furthermore, the robustness of these reduced-form results to controlling for changes in motorcycle registrations suggests that changes in miles traveled on motorcycles are not complicating these comparisons.

4. Discussion

Motorcycles are an increasingly prominent source of annual traffic-related fatalities, more than doubling since 1997 and now accounting for more than 10 percent of all traffic-related deaths. This trend corresponds not only with the growing popularity of motorcycles but also with decreases in the prevalence of helmet use. In this study, I presented new empirical evidence on the effectiveness of helmet use and state laws that mandate their use. This evidence contributed to the extant literature in this area in a number of ways; perhaps most notably, by examining fundamental concerns about the internal and external validity of inferences about the effects of helmet use and helmet laws.

The within-vehicle estimates based on two-rider motorcycles in fatal accidents indicate that helmets are highly effective in reducing the risk of dying (i.e., reducing fatality risk by 34 percent). Helmet use also implies statistically significant reductions in the risk of non-fatal injuries among those involved in motorcycle accidents. I also found that these within-vehicle estimates appear to have fairly broad external validity for the typical motorcycle accident. Furthermore, recent changes in state laws that require helmet use appear to reduce fatalities in magnitudes (i.e., 27 percent) that are roughly consistent with the effectiveness of helmets and reasonable changes in helmet use. This evidence is not consistent with the hypothesis that the life-saving effects of helmet laws are appreciably attenuated by risk-compensating behavioral changes. Instead, the magnitude of the laws' effect actually suggests these policies may promote traffic safety in apparently unintended ways (e.g., through the unintended safety-related effects of police enforcement).

This evidence of the apparent life-saving benefits of helmets and helmet laws does not speak directly to the distinctly normative question of whether these regulations are desirable public policies. In particular, it should be noted that the often fierce opposition to motorcycle helmet laws suggests that they lead to a loss in personal utility that is non-trivial. Because these costs cannot be easily quantified, it is not possible to credibly compare the costs and benefits of these regulations. However, these results do make it possible to frame the empirical magnitudes of the benefits of helmet laws in a way that may be useful for informed policy discourse.

More specifically, in 2005, the 28 continental states without mandatory helmet laws experienced 2387 motorcyclist fatalities. A conservative estimate suggests that a mandatory helmet law would reduce these fatalities by 27 percent or about 644 deaths per year (an estimate quite similar to the simulation results in Table 5). Using a quite conservative estimate for the value of a statistical life ($2 million in 1998 dollars; Mrozek and Taylor, 2002), the annual benefit of saving 644 additional lives would be roughly $1.6 billion in 2005 dollars. On the cost side, there were roughly 3.6 million registered motorcycles in states without mandatory helmet laws in 2005. Roughly half of motorcyclists in these states wear helmets despite the absence of a legal requirement (Glassbrenner and Ye, 2006). Therefore, approximately 1.8 million motorcyclists would be constrained by the universal expansion of helmet laws.

Combining these results, the life-saving effect of helmet laws would amount to a $888 benefit *annually* for each motorcyclist constrained by a new law. Assuming a real discount rate of 5 percent and a 30-year time horizon, the present discounted value of this social benefit is roughly $14,000 for each motorcyclist who would be required to wear a helmet because of a legal requirement. These figures provide a rough sense of how to compare the health benefits of motorcycle-helmet laws with their social costs. However, it should be noted these calculations ignored both the direct cost of purchasing a helmet and the external benefits of helmet use. The external benefits of helmet use could include both their effects on insurance premiums and their long-run personal costs (i.e., an "internality") if those costs are ignored in current decision-making.

A broader point that merits further consideration is that there may be creative policy compromises that move beyond narrow debates over the costs and benefits of simply mandating helmet use. The discourse driving recent changes in state helmet use laws has reflected the tension between public-health concerns and political ideologies that emphasize the value of individual choice (Homer and French, 2009). However, both public-health advocates and motorcyclists who oppose helmet regulations might be willing to agree to a regulatory compromise that mandates helmet use (subject to secondary enforcement) but only for those who have not paid a Pigouvian fee that reflects the external costs of not using a helmet. Alternatively, the regulatory fee for non-use of a helmet could be a non-pecuniary act that makes a contribution to public health (e.g., becoming an organ donor). Alternative policies like this may provide a politically feasible and normatively attractive way to balance the external costs of not wearing a motorcycle helmet with other health-related or fiscal benefits.

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