U.S. Department
of Transportation

National Highway
Traffic Safety
Administration

NHTSA

DOT HS 812 529 April 2018

A Preliminary Comparison of Seat Belt Use Coded in Crash Databases and Reported By Event Data Recorders

Suggested APA Format Reference:

Kahane, C. J. (2018, April). *A preliminary comparison of seat belt use coded in crash databases and reported by event data recorders* (Report No. DOT HS 812 529). Washington, DC: National Highway Traffic Safety Administration.

Technical Report Documentation Page

1. Report No.	2. Government Accession No.	3. Recipient's Catalog No.
DOT HS 812 529		

4. Title and Subtitle	5. Report Date
A Preliminary Comparison of Seat Belt Use Coded in Crash Databases and Reported by Event Data Recorders	April 2018
	6. Performing Organization Code
7. Authors Charles J. Kahane, Ph.D.	8. Performing Organization Report No.
9. Performing Organization Name and Address Bowhead Logistics Solutions, LLC 4900 Seminary Road, Suite 1200 Alexandria, Virginia 22311-1858	10. Work Unit No. (TRAIS) 11. Contract or Grant No.
12. Sponsoring Agency Name and Address National Highway Traffic Safety Administration 1200 New Jersey Avenue SE. Washington, DC 20590	13. Type of Report and Period Covered NHTSA Technical Report 14. Sponsoring Agency Code

15. Supplementary Notes

16. Abstract

Event data recorders that record the status of the driver's seat belt (buckled or unbuckled) began to appear on production cars in 1994. By 2016 almost all new passenger cars, light trucks, and vans sold in the United States were equipped with EDRs. The EDR records drivers' belt use accurately (with a few possible exceptions) at the moment the crash occurs. That contrasts with traditional investigations of low-severity crashes, where drivers have likely left their seats before police arrive: investigators must rely primarily on the driver's own statement of whether they were belted. With laws that require belt use, drivers have a disincentive to admit they were unrestrained.

The availability of EDR data for selected crashes allows for checking the accuracy of the belt use of drivers and right front seat passengers reported in crash databases. In the Crashworthiness Data System, starting in 2002, investigators have requested permission from the owners of crash-involved vehicles to download the EDR for research purposes only, as part of a database without personal identifiers. This report studies the 7,786 CDS case vehicles from CY 2002 to 2015 for which drivers' belt use determined by CDS investigators can be compared to belt use reported by the EDR. They include 7,033 cases where CDS also coded the belt use listed on the police report, and 411 cases were cross-referenced to the Fatality Analysis Reporting System for comparison to the belt use reported in FARS.

The EDR data indicates that FARS and police-reported data over-reported belt use throughout 2002 to 2015, especially for drivers with minor or no injury. CDS initially over-reported belt use but began to employ EDR data to refine their assessments of belt use on selected cases starting circa 2006 and for almost all cases from 2011 – but the data suggests that CDS continued to over-report drivers' belt use in cases without EDR downloads. When over-reporting erroneously transfers people with minor or no injury from the "unrestrained" to the "belted" column, it reduces the observed fatality and injury risk for belted occupants and increases it for unrestrained. Consequently, the fatality- and injury-reducing effectiveness of drivers' belts is exaggerated when it is estimated with the belt use reported on the crash database, but can potentially be more accurately estimated with EDR-reported belt use. However, the number of FARS cases linked to EDR data is not yet sufficient for a new estimate of fatality reduction.

Since the 1980s, NHTSA has estimated that seat belts reduce fatality risk by approximately 45 percent for drivers of passenger cars and by 60 percent for drivers of light trucks and vans. The agency's method for computing the estimates included a hypothesis that belt use was over-reported in crash data and a correction factor to scale back the effectiveness from what was estimated directly from FARS. The 45- and 60-percent estimates remain unchanged; this report does not have a new estimate of fatality reduction. But the analyses of this report reconfirm the agency's hypothesis of over-reporting and corroborate the need for a correction factor like the one the agency currently uses.

17. Key Words	18. Distribution Statement
Double-pair comparison, NASS, fatality reduction, belt effectiveness, evaluation, occupant protection	Document is available to the public from the National Technical Information Service www.ntis.gov.

19. Security Classif. (Of this report)	20. Security Classif. (Of this page)	21. No. of Pages	22. Price
Unclassified	Unclassified	69	

Form DOT F 1700.7 (8-72) Reproduction of completed page authorized

TABLE OF CONTENTS

LIST OF ABBREVIATIONS

AIS	abbreviated injury scale; the levels of this scale are: 0 = uninjured, 1 = minor, 2 = moderate, 3 = serious, 4 = severe, 5 = critical, and 6 = maximum
ANOVA	analysis of variance
CDR	Crash Data Retrieval system of Robert Bosch GmbH, tool for reading EDR
CDS	Crashworthiness Data System of NASS
CFR	Code of Federal Regulations
CISS	Crash Investigation Sampling System
CUV	crossover utility vehicle
CY	calendar year
df	degrees of freedom
DLR	difference in the logarithm of the ratio of the belted fatality odds to the unrestrained fatality odds, for non-equipped minus EDR-equipped vehicles
EDR	event data recorder
FARS	Fatality Analysis Reporting System, a census of fatal crashes in the United States since 1975
Fed. Reg.	Federal Register
GM	General Motors
GmbH	Gesellschaft mit beschränkter Haftung [limited liability corporation]
LTV	light trucks and vans, including pickup trucks, SUVs, CUVs, minivans, and full-size vans
MAIS	a person's maximum-severity injury on the abbreviated injury scale (AIS)
MY	model year
NASS	National Automotive Sampling System, a probability sample of police-reported crashes in the United States since 1979, investigated in detail
NOPUS	National Occupant Protection Use Survey
PAR	police accident report
RF	right front [seating position]
SAS	statistical and database management software produced by SAS Institute, Inc.
SDM	sensing and diagnostic module of the GM EDR
UEF	universal exaggeration factor for belt effectiveness estimates for drivers and right front seat passengers after seat belt laws
VIN	Vehicle Identification Number

EXECUTIVE SUMMARY

"Black boxes" that store critical information about events culminating in crashes have long been features of aircraft, ships, and locomotives. Event data recorders that record the status of the driver's seat belt (buckled or unbuckled), compute and store the velocity change during the crash, and record performance data for the frontal air bag if it deployed began to appear on GM production cars in 1994. By model year 2016, almost all new passenger cars, light trucks, and vans sold in the United States were equipped with EDRs readable by a commercially available tool and meeting NHTSA's regulation (49 CFR Part 563) for performance and accessibility of EDR systems.[1]

The EDR records drivers' belt use directly and (with a few possible exceptions) accurately at the moment the crash occurs. That contrasts with traditional investigations of low-severity crashes, where drivers have likely left their seats before police arrive: investigators must rely primarily on the driver's own statement of whether they were belted. With laws that require belt use, drivers have a disincentive to admit they were unrestrained. In severe crashes, drivers might be injured to the extent of not leaving their seats, or belt use/nonuse might leave physical tell-tales – but these tell-tales are not common in low-speed, non-injury, or low-injury crashes.

During the 1980s, NHTSA estimated that seat belts reduce fatality risk by approximately 45 percent for drivers of passenger cars and by 60 percent for drivers of light trucks and vans – and these have continued to be the agency's effectiveness estimates.[2] However, these estimates were more conservative than the fatality reductions observed in analyses of crash data at that time.[3] The agency hypothesized that analyses based on contemporary crash data overestimated effectiveness because, with belt use laws, many unrestrained crash survivors with minor or no injury are coded as "belted." Transferring crash survivors from the "unrestrained" to the "belted" column would reduce the observed fatality rate per 100 "belted" occupants and increase the observed fatality rate per 100 "unrestrained" occupants, thereby exaggerating belt effectiveness. Therefore, the agency adopted more conservative effectiveness estimates. NHTSA's 2000 evaluation of fatality reduction by seat belts,[4] based on data from the Fatality Analysis Reporting System reviewed the issue. It found that, indeed, the observed effectiveness of belts increased abruptly for drivers and right front seat passengers in CY 1986, after 9 of the 10 most populous

[1] 71 Fed. Reg. 51043 (August 28, 2006); 73 Fed. Reg. 2179 (January 14, 2008); 76 Fed. Reg. 47486 (August 5, 2011); 49 CFR, Part 563. The regulation went into effect on September 1, 2012; it does not obligate manufacturers to equip new vehicles with EDRs, but if so equipped, the EDR has to be readable by a commercially available tool and it must meet various performance standards, including a requirement that it record the driver's and right front passenger's belt use.

[2] NHTSA. (1984). *Final regulatory impact analysis, amendment to Federal Motor Vehicle Safety Standard 208, passenger car front seat occupant protection*. (NHTSA Report No. DOT HS 806 572). Washington, DC: Author. Pp. IV-1 - IV-16; NHTSA. (1990). *Final regulatory impact analysis, extension of the automatic restraint requirements of FMVSS 208 to trucks, buses and multi-purpose passenger vehicles*. (NHTSA Docket No. 74-14-N70-001). Washington, DC: Author. P. 23.

[3] Partyka, S. C. (1988, May). Belt effectiveness in pickup trucks and passenger cars by crash direction and accident year. In *Papers on Adult Seat Belts – Effectiveness and Use* (Report No. DOT HS 807 285). Washington, DC: National Highway Traffic Safety Administration.

[4] Kahane, C. J. (2000, December). *Fatality reduction by safety belts for front-seat occupants of cars and light trucks: Updated and expanded estimates based on 1986-99 FARS data*. (Report No. DOT HS 809 199). Washington, DC: National Highway Traffic Safety Administration. Available at crashstats.nhtsa.dot.gov-/Api/Public/ViewPublication/809199

States had enacted belt laws for those two seating positions. The evaluation proposed that analyses based on 1985 and earlier data, before belt use laws affected reporting, estimate effectiveness more accurately. It developed a specific **correction factor** to adjust downward any estimate based on 1986 and later data to make it more consistent with results from 1985 and earlier. This correction factor has been applied in subsequent NHTSA analyses of belt effectiveness for drivers and right front passengers.

The availability of EDR data for selected crashes allows testing these hypotheses and checking the accuracy of belt use reporting in more recent crash data than 1986. In the Crash Investigation Sampling System and its predecessor, the Crashworthiness Data System of the National Automotive Sampling System, starting in 2002, investigators have requested permission from the owners of crash-involved vehicles to download the EDR for research purposes only, as part of a database without personal identifiers. This report studies the 7,786 CDS case vehicles in CY 2002 through 2015 for which the EDR reported the driver's belt use ("yes" or "no," not "unknown") and CDS also coded belt use as determined by the CDS investigator – permitting analysis of when the EDR-reported and the CDS-investigator-determined belt use agree or disagree. For 7,033 of these cases, the CDS file also codes the belt use listed on the police report, allowing comparison of EDR-reported and police-reported belt use. We cross-referenced 411 FARS vehicle cases to CDS records with EDR downloads and contrasted FARS-reported belt use with the EDR.

The analysis potentially has two goals:

1. Compare EDR-based **belt use** to what is coded in the crash data, to see if belt use is indeed over-reported in the crash data, especially for occupants with minor or no injury, consistent with NHTSA's past hypotheses; if so, it would reconfirm the agency's position that belt effectiveness estimates based directly on crash data are overstated and it would reconfirm the rationale for a correction factor to lower those estimates.

2. Furthermore, with each of these crash databases, **belt effectiveness** might be estimated with the EDR-reported belt use and compared to the corresponding estimate based on the file's own reported belt use. This could eventually furnish new estimates of fatality reduction as well as a new estimate of the correction factor for the discrepancy between actual effectiveness (the estimate based on EDR-reported belt use) and the effectiveness computed directly from the crash database.

This report accomplishes the first analysis goal. The EDR data confirm that crash databases have over-reported belt use for drivers and, consequently, would have exaggerated the fatality- and injury-reducing effectiveness of belts for drivers if it had been directly estimated with the belt use reported in the database, without any correction factor such as the one used by NHTSA:

- CDS initially over-reported belt use, especially for drivers with minor or no injury. However, CDS investigators, who have been downloading EDRs since 2002, began to employ this EDR data to refine their assessments of belt use on selected cases starting circa 2006 and for almost all cases from 2011 (after CDS personnel had received extensive training on the interpretation of EDR data). Consequently, belt use is no longer over-reported for the vehicles with EDR data in CDS. The injury-reducing effectiveness of belts, when computed based on the CDS-reported belt use, appears to be realistic for the vehicles with EDR data after CY 2009, but it is exaggerated for vehicles with EDR data in the earlier years – and throughout CY 2002 to 2015 for the vehicles where CDS did not download the EDR.

- Police download EDRs to support their investigations in selected crashes, but as of 2015, this was apparently still a negligible percentage of all reported crashes in FARS and in State crash files. Consequently, police reports overstated belt use throughout 2002 to 2015, especially for drivers with minor or no injury. Estimates of fatality reduction in FARS and of injury reduction in other police-reported data, based directly on the belt use coded on those databases, are exaggerated throughout 2002 to 2015.

- The analyses of this report corroborate the rationale for the correction factor that NHTSA has employed since 2000 to adjust FARS-based belt effectiveness estimates downwards for outboard front seat occupants.

However, the limited (411 cases) FARS data cross-referenced to EDR downloads to date is insufficient to accomplish the second analysis goal – namely, a statistically meaningful estimate of fatality reduction based on EDR-reported belt use and a new computation of the correction factor. The number of cases would need to grow by an order of magnitude to achieve this goal and allow finalizing this "preliminary" report. It is unlikely, though, that a sufficient number of additional cases will be accumulated in the next two or three years. Nevertheless, the current data is enough for pilot analyses that suggest the new correction factor will be directionally similar to the factor NHTSA currently uses and likely of a similar magnitude, too.

Until fatality reduction can be definitively estimated with EDR-based belt use data – and this is unlikely to happen for some years to come – it seems appropriate to continue with the existing estimates that belts reduce overall fatality risk by 45 percent for drivers and right front passengers of cars and by 60 percent in light trucks and vans. Effectiveness can change over time as technologies evolve and/or the distribution of crashes shifts, but analyses over the past 30 years have shown little net change. Likewise, it is appropriate to continue using the existing correction factor for more detailed estimates based directly on analyses of FARS data. The analyses of this report corroborate the rationale for the correction factor and support the plausibility of the current 45- and 60-percent effectiveness estimates.

1. Rationale for estimating belt use and effectiveness in crashes based on EDR data

1.1 Event data recorders, a resource to determine if people buckled up: "Black boxes" that store critical information about events culminating in a crash have long been features of aircraft, ships, and locomotives. In 1974 EDR systems began to appear on production cars. A crucial milestone in the 1990s was the development by General Motors of an EDR with a sensing and diagnostic module, which computed and stored the velocity change during the crash, recorded performance data for the frontal air bag if it deployed, and – most relevant to this report – recorded the status of the driver's belt switch (buckled or unbuckled) at the time of the crash.[5] GM installed this type of EDR in several of its MY 1994 production cars: Buick's Roadmaster; Cadillac's DeVille, Seville, and Fleetwood; Chevrolet's Caprice; and Pontiac's Grand Prix. A key feature of this EDR is its accessibility to crash investigators by a commercially available tool, the crash data retrieval system of Robert Bosch GmbH.[6] Subsequent EDRs added the capability to record vehicle systems status (speed, throttle and brake status, and driver belt use) for several seconds preceding a crash and even for selected events earlier in the vehicle's history. Some later EDRs also recorded the right front passenger's belt use. EDRs began to appear on Ford and Toyota vehicles in 2000. As of MY 2016, almost all new passenger cars and LTVs sold in the United States were equipped with EDRs readable by the CDR system or another commercially available tool and meeting NHTSA's regulation (49 CFR Part 563) for performance and accessibility of EDR systems.[7] The regulation went into effect on September 1, 2012; it does not obligate manufacturers to equip new vehicles with EDRs, but if so equipped, the EDR has to be readable by a commercially available tool and it must meet various performance standards, including a requirement that it record the driver's and right front passenger's belt use. Appendix A of this report lists all makes and models of passenger cars and LTVs that have been equipped with EDRs that can be read by a commercially available tool and record belt use (at least for the driver), indicating the first model year when such EDRs were standard equipment. Table 1-1 shows that the share of new vehicles equipped with such EDRs has increased from 2 percent in MY 1994 to nearly 100 percent by MY 2015; however, the percentage of all vehicles on the road (including older vehicles) equipped with EDRs had only reached 57 percent by CY 2015.

---

[5] Chidester, A., Hinch, J., Mercer, T. C., & Schultz, K. S. (1999). *Recording automotive crash event data.* Proceedings of the International Symposium on Transportation Recorders: Transportation Recording: 2000 and Beyond. Washington, DC: National Transportation Safety Board. Pp. 85-98. Available at www.nhtsa.gov/cars/problems/studies/record/chidester.htm

[6] Crash Data Group. (2017). *CDR vehicle list, CDR software 17.2*. Temecula, CA: Author. Available at crashdatagroup.com/software/versions/CDR_v17.2_Vehicle_Coverage_List_R1_0_0.pdf

[7] 71 Fed. Reg. 51043 (August 28, 2006); 73 Fed. Reg. 2179 (January 14, 2008); 76 Fed. Reg. 47486 (August 5, 2011); 49 CFR, Part 563.

Table 1-1: Percentage of Vehicles Equipped With EDRs
(Source: Weighted CDS Data, CY 2002-2015; tabulates percentage of vehicles known to be equipped with EDRs; however, the EDR was not necessarily accessed by the investigators)

MY	Percentage of New Vehicles	CY	Percentage of Vehicles on the Road
1994	2		
1995	10		
1996	18		
1997	24		
1998	28		
1999	32		
2000	31		
2001	50		
2002	51	2002	20
2003	54	2003	22
2004	56	2004	26
2005	58	2005	31
2006	54	2006	30
2007	56	2007	39
2008	54	2008	42
2009	52	2009	39
2010	59	2010	40
2011	72	2011	43
2012	77	2012	45
2013	92	2013	46
2014	92	2014	54
2015	> 99	2015	57

The EDR offers, for the first time, an opportunity to record drivers' belt use directly and accurately at the moment the crash occurs. That contrasts with traditional investigations of typical crashes of low-to-moderate severity, where drivers have likely left their seats to inspect their vehicles and/or talk to other drivers before police arrive: the police must rely primarily on the driver's own statement of whether they were belted. Given that 49 States and the District of Columbia have had belt-use laws since 1995 or earlier, drivers have a disincentive to admit they were unrestrained. In more severe crashes, drivers might be injured to the extent of not leaving their seats, or belt use/nonuse might leave physical tell-tales such as a distinctive injury pattern, occupant contact points within the vehicle, or stretched belt webbing – but these tell-tales are not common in low-speed, non-injury, and low-injury crashes. Also, in crash databases such as NASS-CDS created purely for research purposes, drivers may have less of a disincentive to report they were unrestrained – but we are still relying on the driver's statement after the fact rather than direct, real-time observation.

Nevertheless, one important caveat throughout this report is that the EDR is itself not a foolproof recorder of belt use. Some of the earlier EDR systems, if the impact was so severe that it cut power to the control module, caused the belt status to default to “not used” even if the occupant was belted. An EDR only reports whether or not it received a signal that the latch was buckled; this does not necessarily mean the belt was worn – e.g., if occupants buckle the belts behind their backs. It is also unclear how the various EDR systems react when a buckle extender has been added to accommodate a large occupant.

These caveats aside, our intuition is that belt use is over-reported in crash databases because, in the presence of belt use laws, drivers have a disincentive to report they were unrestrained. Here is some hard evidence that belt use is, indeed, overreported.

1.2 Crash databases have over-reported seat belt use: NHTSA’s National Occupant Protection Use Surveys of 1994, 1996, 1998, and every year since 2000 accurately estimate belt use by drivers of cars and LTVs on the Nation’s streets, roads and highways during daylight hours, based on direct observation of a probability sample of vehicles and roadways.[8] NOPUS

---

[8] Bondy, N., & Utter, D. (1997, April). *Observed safety belt use in 1996*. Washington, DC: National Highway Traffic Safety Administration. Available at www-nrd.nhtsa.dot.gov/Pubs/97820.PDF;

Bondy, N., & Utter, D. (2001, February). *Observed safety belt use, fall 2000 National Occupant Protection Use Survey*. Washington, DC: National Highway Traffic Safety Administration. Available at www-nrd.nhtsa.dot.gov/Pubs/01010.pdf;

Glassbrenner, D. (2002, September). *Safety belt and helmet use in 2002 – Overall results*. (Report No. DOT HS 809 500). Washington, DC: National Highway Traffic Safety Administration. Available at www-nrd.nhtsa.dot.gov/Pubs/809500.pdf;

Glassbrenner, D. (2003, September). *Safety belt use in 2003.* (Report No. DOT HS 809 646). Washington, DC: National Highway Traffic Safety Administration. Available at www-nrd.nhtsa.dot.gov/Pubs/809646.pdf;

Glassbrenner, D. (2004, September). *Safety belt use in 2004 – Overall results.* (Report No. DOT HS 809 783). Washington, DC: National Highway Traffic Safety Administration. Available at www-nrd.nhtsa.dot.gov/Pubs/809783.pdf;

Glassbrenner, D. (2005, August). *Safety belt use in 2005 – Overall results.* (Report No. DOT HS 809 932). Washington, DC: National Highway Traffic Safety Administration. Available at www-nrd.nhtsa.dot.gov/Pubs/809932.pdf;

Glassbrenner, D., & Ye, J. Y. (2006, November). *Seat belt use in 2006 – Overall results.* (Report No. DOT HS 810 677). Washington, DC: National Highway Traffic Safety Administration. Available at www-nrd.nhtsa.dot.gov/Pubs/810677.pdf;

Glassbrenner, D., & Ye, J. Y. (2007, September). *Seat belt use in 2007 – Overall results.* (Report No. DOT HS 810 841). Washington, DC: National Highway Traffic Safety Administration. Available at www-nrd.nhtsa.dot.gov/Pubs/810841.pdf;

Pickrell, T. M., & Ye, J. Y. (2008, September). *Seat belt use in 2008 – Overall results.* (Report No. DOT HS 811 036). Washington, DC: National Highway Traffic Safety Administration. Available at www-nrd.nhtsa.dot.gov/Pubs/811036.pdf;

Pickrell, T. M., & Ye, J. Y. (2009, September). *Seat belt use in 2009 – Overall results.* (Report No. DOT HS 811 200). Washington, DC: National Highway Traffic Safety Administration. Available at www-nrd.nhtsa.dot.gov/Pubs/811200.pdf;

Pickrell, T. M., & Ye, J. Y. (2010, September). *Seat belt use in 2010 – Overall results.* (Report No. DOT HS 811 378). Washington, DC: National Highway Traffic Safety Administration. Available at www-nrd.nhtsa.dot.gov/Pubs/811378.pdf;

Pickrell, T. M., & Ye, J. Y. (2011, November). *Seat belt use in 2011 – Overall results.* (Report No. DOT HS 811 544). Washington, DC: National Highway Traffic Safety Administration. Available at www-nrd.nhtsa.dot.gov/Pubs/811544.pdf;

Pickrell, T. M., & Ye, T. J. (2012, November). *Seat belt use in 2012 – Overall results*. (Report No. DOT HS 811 691). Washington, DC: National Highway Traffic Safety Administration. Available at www-nrd.nhtsa.dot.gov/Pubs/811691.pdf;

results can have sampling error; 95 percent confidence bounds were initially ± 4 percent but in recent years have shrunk to less than ± 2 percent. The left column of Table 1-2 shows that 59 percent of drivers in daytime traffic actually buckled up during 1994. Belt use reached 70 percent in 1998, 80 percent in 2003, and then gradually climbed through the 80s, reaching 89 percent by 2015.

Table 1-2: Drivers' Seat Belt Use (%) in the United States by Calendar Year, 1994 to 2015: Observed on the Road Versus Reported in Crashes

		Vehicles W/O EDR Download		Vehicles With EDR Download		
Calendar Year	Observed in NOPUS	Reported by Police	CDS Investigator-Determined	Reported by EDR	Reported by Police	CDS Investigator-Determined
1994	59	83	77			
1996	62	86	79			
1998	70	90	85			
2000	72	89	83			
2001	74	93	86			
2002	76	94	80	60	94	80
2003	80	94	85	70	95	87
2004	81	94	87	62	95	87
2005	83	95	86	56	97	86
2006	82	95	87	67	93	81
2007	83	95	87	82	96	91
2008	84	96	87	74	93	87
2009	85	96	91	73	95	88
2010	86	95	92	72	98	90
2011	84	95	93	66	97	69
2012	87	97	94	76	98	77
2013	88	96	94	77	95	79
2014	87	97	95	70	95	67
2015	89	95	88	75	97	83

Pickrell, T. M., & Liu, C. (2014, January). *Seat belt use in 2013 – Overall results*. (Report No. DOT HS 811 875). Washington, DC: National Highway Traffic Safety Administration. Available at crashstats.nhtsa.dot.gov/Api/Public/ViewPublication/811875;
Pickrell, T. M., & Choi, E.-H. (2015, February). *Seat belt use in 2014 – Overall results*. (Report No. DOT HS 812 113). Washington, DC: National Highway Traffic Safety Administration. Available at crashstats.nhtsa.dot.gov/Api/Public/ViewPublication/812113;
Pickrell, T. M., & Li, R. (2016, February). *Seat belt use in 2015 – Overall results*. (Report No. DOT HS 812 243). Washington, DC: National Highway Traffic Safety Administration. Available at crashstats.nhtsa.dot.gov/Api/Public/ViewPublication/812243

We would expect on-the-road belt use observed in NOPUS to be somewhat higher than the actual belt use of crash-involved drivers, for two reasons: (1) NOPUS is limited to daytime observations, while crash databases include both daytime and nighttime incidents: when seat belt use has been observed at the same locations day and night it has averaged 6 percentage points lower at night;[9] and (2) Many of the drivers involved in crashes are probably less careful than the average driver on the road, and nonuse of belts might be a behavior associated with careless drivers.

Contrary to the preceding expectations, drivers' belt use reported in police accident reports (PAR) is substantially higher than in NOPUS. NASS-CDS is a probability sample of towaway crashes in the United States. The PARUSE variable in CDS is the belt use specified on the PAR associated with the crash. Thus, the distribution of PARUSE provides an unbiased estimate of police-reported belt use in the nation's crashes. The second column of Table 1-2 shows that police-reported belt use in crashes has exceeded NOPUS belt use every year. It was 83 percent in 1994, when NOPUS showed belt use on the road was actually 59 percent. By 2001, when NOPUS reached 74 percent, police-reported belt use was 93 percent. In 2015, when NOPUS said 89 percent, police-reported belt use was 95 percent in vehicles not equipped with EDRs (column 2) and 97 percent in vehicles equipped with EDRs (column 5).

NASS began to access and download EDR data in CY 2002, for vehicles equipped with EDRs that had been involved in crashes sampled and investigated by CDS. In every year from 2002 through 2015, belt use at the time of the crash according to the EDR is much lower than the police-reported belt use and, for that matter, also lower than NOPUS. For example, Table 1-2 shows that belt use in 2002 was 76 percent in NOPUS (column 1), 94 percent in the police reports for the EDR-equipped vehicles (column 5), but only 60 percent according to the EDR (column 4). Throughout the 14 years, 2002 to 2015, belt use in crashes reported by EDRs is lower than NOPUS in each year: the median difference is 14 percentage points lower. That is a plausible difference, considering many of the crashes happened at night whereas NOPUS is daytime-only and many of the crash-involved drivers are less careful than the typical on-the-road driver; however, it is conceivable that the difference might to some extent also reflect the occasional inaccuracies of EDR systems discussed earlier.

The CDS variables MANUSE and ABELTUSE represent the CDS investigator's final determination of belt use. CDS tends to be more skeptical about belt use than the police, presumably thanks to the acquisition of detailed vehicle-inspection and injury data and possibly more candid self-reporting of belt use by drivers. Nevertheless, CDS-investigator-determined belt use has historically been considerably higher than NOPUS. For example, in 1994, NOPUS reported 59 percent belt use on the road (column 1 of Table 1-2), the police reported 83 percent belt use in crashes (column 2), and CDS reported 77 percent in those same crashes (column 3): CDS is closer to the PARs than to NOPUS. By 2001, these percentages were 74, 93, and 86, respectively (similar pattern). From 2002 onwards, in the vehicles not equipped with EDRs or where CDS did not retrieve an EDR readout, CDS-investigator-determined belt use continues to exceed NOPUS (except in 2015) and in 2010 through 2014 was just 2 or 3 percentage points

[9] Vasudevan, V., Kachroo, P., & Bandaroo, N. (2015, March). Nighttime seatbelt usage data collection: When and how long? *IATSS Research, 38, 2*, pp. 149-156. Available at www.safetylit.org/citations/index.php?-fuseaction=citations.viewdetails&citationIds%5B%5D=citjournalarticle_466118_14

below police-reported belt use. In the vehicles with EDR data, until perhaps as late as 2010, CDS-investigator-determined belt use (column 6) is usually as high as in the non-EDR vehicles (column 3); it is usually well above NOPUS and the EDR-based belt use. But starting in 2011 (or perhaps earlier), CDS-investigator-determined belt use for the EDR-equipped vehicles drops down to the EDR-based levels, below NOPUS and far below the police-reported belt use – because CDS investigators in recent years have incorporated the EDR readouts as a crucial part of their evidence for assessing belt use on the vehicles where the readouts are available – while continuing to assess belt use by similar methods as in the past for vehicles without EDR information.

1.3 Over-reported belt use may cause inaccurate effectiveness estimates: During the mid-1980s, NHTSA and others estimated that seat belts reduce fatality risk by approximately 45 percent for drivers of passenger cars. These estimates derived from double-pair comparison analyses of FARS data (which will be described in the next section) and other methods.[10] Later in that decade, NHTSA continued to monitor belt effectiveness and noticed that estimates rose substantially as more recent FARS data were fed into the analyses – e.g., analyses of 1982-87 FARS data produced a belt effectiveness estimate of 55 percent for passenger cars.[11] The agency hypothesized that the new analyses overestimated effectiveness because, with belt use laws, people had begun over-reporting belt use in crashes. As a consequence, the agency concluded that effectiveness estimates needed to be more conservative than what was directly computed from the data; specifically, the agency scaled back belt effectiveness in LTVs from an observed 69 percent reduction in the crash data down to a best estimate of 60 percent.[12] These have continued to be the agency's effectiveness estimates for seat belts for drivers and RF passengers: 45 percent fatality reduction in passenger cars and 60 percent in LTVs.

Inaccurate reporting of belt use will not necessarily bias estimates of fatality or injury reduction by seat belts in a particular direction; it depends on how the extent of inaccurate reporting is associated with fatality or injury risk. Table 1-3 considers, for example, a hypothetical database where, if belt use had been accurately reported, there would have been 100 unrestrained and 100 belted drivers (50% belt use) and the fatality rate for unrestrained drivers would have been double the rate for belted drivers (50% fatality reduction):

[10] NHTSA (1984), pp. IV-1 - IV-16; Evans, L. (1986b). The effectiveness of safety belts in preventing fatalities. *Accident Analysis and Prevention*, *18*, pp. 229-241.
[11] Partyka (1988, May); Kahane (2000, December), pp. 1-4.
[12] NHTSA (1990), p. 23.

Table 1-3: Hypothetical Computation of Fatality Reduction Based on Actual Belt Use

	Unrestrained	Belted	Fatality Reduction
Fatalities	20	10	
Survivors	80	90	
Total	100	100	
Fatality rate	.20	.10	50%

If, however, one of every four unrestrained drivers had been incorrectly reported as belted – regardless of whether that driver was a fatality or a survivor – "belt use" would have been over-reported as 62.5 percent (125 of 200 drivers). Table 1-4 shows the estimated fatality reduction would actually have decreased from 50 percent to 40 percent because the reportedly belted population includes unrestrained drivers:

Table 1-4: 25 Percent of Unrestrained (Fatalities and Survivors) Reported as Belted

	Unrestrained	Belted	Fatality Reduction
Fatalities	15	15	
Survivors	60	110	
Total	75	125	
Fatality rate	.20	.12	40%

Table 1-4, however, is not the expected pattern of over-reporting. Section 1.1 proposed that drivers with little or no injury have likely left their seats before police arrive and self-report belt use, whereas drivers with eventually fatal injuries may still be in their seats, with the belts still buckled or unbuckled as they were before the crash. In Table 1-5 one of every four unrestrained **surviving** drivers has been incorrectly reported as belted – but belt use in all the fatality cases has been correctly reported. "Belt use" is again over-reported as 60 percent (120 of 200 drivers), but now fatality reduction is also substantially overestimated (67% rather than 50%):

Table 1-5: 25 Percent of Unrestrained Survivors Reported as Belted

	Unrestrained	Belted	Fatality Reduction
Fatalities	20	10	
Survivors	60	110	
Total	80	120	
Fatality rate	.25	.083	67%

1.4 Estimation of belt effectiveness by double-pair comparison: Since the mid-1980s, NHTSA's estimates of fatality-reducing effectiveness for seat belts (and also for child safety seats) have usually been based on **double-pair comparison** analyses of FARS data. NHTSA started FARS, a census of the fatal traffic crashes in the United States, in 1975. Double-pair comparison is valuable because it allows the direct use of FARS data, which has a much higher number of fatalities than any other crash database. A second major advantage is that double-pair comparison implicitly "adjusts" or "controls" for the differences in the severity of crashes involving belted and unrestrained occupants. Under the right circumstances, it can separate belt effectiveness from other factors that influence fatality risk, such as an occupant's age, the type and severity of the crash, or the overall crashworthiness of the vehicle.[13]

For example, NHTSA's 2000 evaluation report on fatality reduction by seat belts analyzes fatality reduction for drivers and RF passengers of passenger cars, based on FARS data from CY 1977 through 1985,[14] the last year that the vast majority of States still did not have belt use laws.[15] Records of passenger cars of MY 1975 to 1986 are extracted (1975 is the first model year with "Type 2" 3-point belt systems, not counting 1974, where cars were also equipped with the ignition interlock). The analysis is limited to:

- Cars with a driver and a RF passenger (and perhaps other passengers);
- The driver, or the RF passenger, or both were fatally injured;
- The driver and the RF passenger both have known reported belt use; and
- The driver and the RF passenger are both 14 to 97 years old.[16]

There are 30,665 cars in CY 1977 to 1985 with a driver and a RF passenger, at least one fatal, both with belt use reported to be "yes" or "no" (i.e., not "unknown") and 14 to 97 years old. Table 1-6 tallies the vehicle cases, based on each occupant's belt use and survival:

---

[13] Partyka, S. C. (1984). *Restraint use and fatality risk for infants and toddlers*. Washington, DC: National Highway Traffic Safety Administration; Evans, L. (1986a). Double pair comparison – a new method to determine how occupant characteristics affect fatality risk in traffic crashes. *Accident Analysis and Prevention*, *18*, pp. 217-227;

Evans, L. (1986b);

Kahane, C. J. (1986, February). *An evaluation of child passenger safety: The effectiveness and benefits of safety seats*. (Report No. DOT HS 806 890). Washington, DC: National Highway Traffic Safety Administration. Available at crashstats.nhtsa.dot.gov/Api/Public/ViewPublication/806890;

Partyka, (1988, May).

[14] Kahane, (2000, December), pp. 5-10.

[15] Belt use laws went into effect in New York in December 1984, New Jersey in March 1985, Michigan in July, Missouri and Texas in September, North Carolina in October, and Hawaii and the District of Columbia in December;

NHTSA. (1999, October). *Traffic safety facts 1998* (Report No. DOT HS 808 983). Washington, DC: Author. P. 186. Available at crashstats.nhtsa.dot.gov/Api/Public/ViewPublication/808983

[16] Within the timespan of the data for that analysis, 14 was the minimum legal driving age in several States; 97 was the oldest age reportable on FARS; the same age range was set for the RF passengers.

Table 1-6: Passenger Cars by Driver and RF Belt Use and Survival Status (FARS 1977 to 1985)

Vehicles	Driver Died RF Survived	Driver Survived RF Died	Both Died
Both unrestrained	11,186	11,469	5,317
Driver unrestrained, RF belted	300	152	74
Driver belted, RF unrestrained	186	487	102
Both belted	497	653	242

Table 1-7 tallies fatality counts rather than vehicle cases by adding the "both died" column to each of the preceding columns:

Table 1-7: Fatalities by Belt Use and Seating Position (FARS 1977 to 1985)

Fatalities	Driver Fatalities	RF Fatalities	Driver/RF Risk Ratio
Both unrestrained	16,503	16,786	0.983
Driver unrestrained, RF belted	374	226	1.655
Driver belted, RF unrestrained	288	589	0.489
Both belted	739	895	0.826

In CY 1977 to 1985, it is clear that (1) the overwhelming majority of people killed in crashes were unrestrained; (2) unrestrained drivers and RF passengers are at nearly equal risk in the same crash; and (3) whoever buckled up substantially reduced their risk.

The four rows of data allow a total of four double-pair comparisons, two for computing the effectiveness of belts for drivers, and two for RF passengers. The first comparison for the driver is based on the first and third rows of data:

		Driver Fatalities	RF Fatalities	Driver/RF Risk Ratio
Driver unrestrained	RF unrestrained	16,503	16,786	0.983
Driver belted	RF unrestrained	288	589	0.489

In both pairs, the driver's fatality risk is compared to the same control group: the unrestrained RF passenger. The unrestrained driver has essentially the same fatality risk as the unrestrained RF in the same crash, the belted driver about half. The fatality reduction for belts is:

$$1 - (0.489/0.983) = 50.3 \text{ percent.}$$

The other comparison for the driver is based on the second and fourth rows of data:

		Driver Fatalities	RF Fatalities	Driver/RF Risk Ratio
Driver unrestrained	RF belted	374	226	1.655
Driver belted	RF belted	739	895	0.826

Here, the control group is the belted RF passenger. The unrestrained driver has higher fatality risk than the belted RF in the same crash, the belted driver, lower. The fatality reduction is:

$$1 - (0.826/1.655) = 50.1 \text{ percent.}$$

It is important that the effectiveness estimates are nearly identical with the two control groups: it suggests the estimates are robust and not affected by the choice of control group.

The first double-pair comparison for estimating belt effectiveness for the RF passenger is obtained by using the first two rows of data, reversing the order of the columns and computing the RF/Driver rather than the Driver/RF risk ratio:

		RF Fatalities	Driver Fatalities	RF/Driver Risk Ratio
RF unrestrained	Driver unrestrained	16,786	16,503	1.017
RF belted	Driver unrestrained	226	374	0.604

The control group is the unrestrained driver. Fatality reduction for the belted RF passenger is:

$$1 - (0.604/1.017) = 40.6 \text{ percent.}$$

The second estimate uses the last two rows of data:

		RF Fatalities	Driver Fatalities	RF/Driver Risk Ratio
RF unrestrained	Driver belted	589	288	2.045
RF belted	Driver belted	895	739	1.211

The control group is the belted driver. The fatality reduction for the belted RF passenger is:

$$1 - (1.211/2.045) = 40.8 \text{ percent.}$$

Again, the two control groups produce nearly identical estimates. Also, belt effectiveness is lower for the RF passenger than for the driver.

The next task is to develop a weighting procedure that combines the two driver estimates into a single number, and likewise for the two RF estimates. In the 1977-85 FARS data, the actual number of driver fatalities is

Actual driver fatalities = 16,503 + 374 + 288 + 739 = 17,904

The first two numbers in that sum are unrestrained drivers, the last two, belted. However, if every driver had been unrestrained, that sum would have increased to

All-unrestrained driver fatalities = 16,503 + 374 + (0.983 x 589) + (1.655 x 895) = 18,937

(Here, 589 was the number of unrestrained RF fatalities that accompanied the 288 belted drivers and 0.983 is the risk ratio of unrestrained driver to unrestrained RF fatalities; 895 is the number of belted RF fatalities that accompanied the 739 belted drivers and 1.655 is the risk ratio of unrestrained drivers to belted RF fatalities.)

On the other hand, if every driver had buckled up, the sum would have dropped to

All-belted driver fatalities = (0.489 x 16,786) + (0.826 x 226) + 288 + 739 = 9,421

The overall effectiveness of belts for drivers is

(18,937 - 9,421) / 18,937 = 50.25 percent,

which is between the results of the two separate double-pair comparisons for drivers (50.1 and 50.3 percent).

Similarly, the actual number of RF passenger fatalities is

Actual RF fatalities = 16,786 + 226 + 589 + 895 = 18,496

If every RF passenger had been unrestrained, that sum would have increased to

All-unrestrained RF fatalities = 16,786 + (1.017 x 374) + 589 + (2.045 x 739) = 19,267

(Here, 374 was the number of unrestrained driver fatalities that accompanied the 226 belted RF passengers and 1.017 is the risk ratio of unrestrained RF to unrestrained driver fatalities; 739 is the number of belted driver fatalities that accompanied the 895 belted RF and 2.045 is the risk ratio of unrestrained RF to belted driver fatalities.)

But if every RF passenger had buckled up, the sum would have dropped to

All-belted RF fatalities = (0.604 x 16,503) + 226 + (1.211 x 288) + 895 = 11,442

The overall effectiveness of belts for RF passengers is

(19,267 - 11,442) / 19,267 = 40.61 percent,

which is between the results of the two separate double-pair comparisons for RF passengers (40.6 and 40.8 percent).

Finally, for an estimate of the overall effectiveness of 3-point belts for outboard front seat occupants of passenger cars, we must note that drivers have over the years typically outnumbered RF passengers by very close to 3 to 1 in the general crash-involved population (as opposed to these special cases that were limited to cars with the RF seat occupied). The preceding statistics for drivers need to be weighted by 3 and the statistics for RF passengers by 1. If all drivers and RF passengers were unrestrained, that sum would have increased to

All-unrestrained outboard front seat fatalities = (3 x 18,937) + 19,267 = 76,078

If they had all buckled up, the sum would have dropped to

All-belted outboard front seat fatalities = (3 x 9,421) + 11,442 = 39,706

The overall effectiveness of 3-point belts for outboard front seat occupants is

(76,078 - 39,706) / 76,078 = 47.81 percent,

which is between the estimates for drivers and RF passengers, but closer to the driver estimate, as it should be, given the higher weight factor for drivers.

1.5 Belt effectiveness has been overestimated in crash data since 1986: NHTSA's 2000 report repeats the preceding double-pair comparison analysis with more recent FARS data, specifically CY 1986 through 1999, involving passenger cars of MY 1975 through 1999.[17] Table 1-8 tallies fatalities, analogous to Table 1-7 for the earlier data:

Table 1-8: Fatalities by Belt Use and Seating Position (FARS 1986 to 1999)

Fatalities	Driver Fatalities	RF Fatalities	Driver/RF Risk Ratio
Both unrestrained	23,476	23,579	0.996
Driver unrestrained, RF belted	3,934	1,622	2.425
Driver belted, RF unrestrained	1,815	4,820	0.377
Both belted	11,225	12,901	0.870

The effect of belts appears more dramatic at first glance in Table 1-8 than in Table 1-7. The ratio of unrestrained driver to belted RF fatalities increased from 1.655 to 2.425 while the ratio of

[17] Kahane (2000, December), pp. 10-19; cars with 2-point automatic belts are excluded; cars with only a driver air bag are excluded to preserve the symmetry (nearly equal fatality risk) of the driver and the RF positions in the analysis.

belted driver to unrestrained RF decreased from 0.489 to 0.377. Working through the double-pair comparisons and weighted averages generates fatality reduction estimates:[18]

Fatality Reduction	CY 1977 to 1985	CY 1986 to 1999
Drivers	50.25%	63.26%
RF passengers	40.61%	57.71%
All outboard front seat occupants	47.81%	61.89%

Table 1-9 shows the observed overall fatality reductions for belts when a separate double-pair comparison analysis is run on each individual calendar year of FARS data:[19]

Table 1-9: Observed Fatality Reduction by Calendar Year

1977	49 percent	1986	61	1993	60
1978	28	1987	58	1994	64
1979	44	1988	61	1995	63
1980	38	1989	63	1996	65
1981	52	1990	69	1997	58
1982	53	1991	62	1998	62
1983	38	1992	60	1999	59
1984	46				
1985	55				

During 1977 through 1985, observed belt effectiveness varies a fair amount from year to year, due to the small numbers of belted cases in FARS, but arguably centers on the average effect, 47.8 percent with little or no time trend (except perhaps an increase in 1985, as belt laws began to take effect in a few States). In 1986, the first year with belt use laws covering a large proportion of occupants (including 9 of the 10 most populous States) the fatality reduction has already reached 61 percent, essentially the 1986-99 average, and it stayed close to that year after year, with no evidence of any time trend within 1986-99.

Furthermore, Table 1-10 indicates that when the vehicles are subdivided into model-year cohorts, observed belt effectiveness is about the same in each cohort, but is consistently higher in the later calendar years of FARS:[20]

[18] *Ibid*., p. 10.
[19] *Ibid*., p. 11.
[20] *Ibid*., p. 13.

Table 1-10: Observed Fatality Reductions by MY and CY Groups

	Effect in 1977-85 FARS	Effect in 1986-99 FARS
Manual belts in MY 1975-79 cars	48	63
Manual belts in MY 1980-85 cars	47	63
Manual belts in MY 1986-90 cars		60
Automatic 3-point belts (MY 1987-95)		64
Manual belts in cars with dual air bags (MY 1987-99)		63

The data suggests that true belt effectiveness stayed the about the same throughout MY 1975 to 1999, but observed belt effectiveness increased abruptly in CY 1986 with the advent of belt use laws, and it stayed at that new, higher level, at least through CY 1999. Since the belts themselves had not changed much (pretensioners and load limiters were just beginning to appear in the late 1990s), NHTSA's 2000 report proposed that the original CY 1977-to-1985 effectiveness, when people had no tangible incentive (avoidance of a ticket and fine) to over-report belt use, is close to the true value, whereas the higher CY 1986-to-1999 effectiveness is an exaggeration associated with inaccurate belt use reporting, specifically, a portion of unrestrained crash survivors self-reporting that they had worn belts.

1.6 Definition of the "universal exaggeration factor": 3-point belts reduced fatality risk in passenger cars by 47.81 percent in CY 1977-to-1985 FARS data and were observed to "reduce" fatality risk by 61.89 percent in CY 1986-to-1999 FARS data. With the hypothesis that the first estimate is unbiased, whereas the second is biased upwards by inaccurate belt use reporting, NHTSA's 2000 report defined the "Universal Exaggeration Factor" to be the relative difference of the two estimates:[21]

$$UEF = (100 - 47.81) / (100 - 61.89) = 1.369$$

It is the adjustment factor that has to be applied to the inappropriately low CY 1986-to-1999 ratio of "belted" to "unrestrained" fatality risk to obtain the purportedly accurate CY 1977-to-1985 ratio of actual belted to unrestrained fatality risk:

$$1.369 \times (100 - 61.89) = 100 - 47.81; \quad 47.81 = 100 - [1.369 \times (100 - 61.89)]$$

The report further proposed that this same UEF = 1.369 might also be **empirically valid for other double-pair comparison analyses based on CY 1986 and later FARS data**, for drivers and RF passengers, including other types of vehicles or belts, subgroups of crashes or occupants, and more complex weighted averages of double-pair comparisons (at least through CY 1999). In other words, if the analysis of CY 1986+ data yields an effectiveness estimate E*, the true effectiveness E is close to

[21] *Ibid.*, pp. 13-14.

$$E = 100 - [1.369 \times (100 - E^*)]$$

NHTSA's report placed fairly high confidence in the UEF because it was quite robust, varying relatively little when it was separately computed for quite disparate subsets of the FARS database – various crash types; driver age, gender, and behavior; locations (10 States with the most fatal crashes); and specific post-1985 calendar year – as shown in Table 1-11:[22]

[22] *Ibid*., pp. 17-19 (except the UEF by CY, which was computed but not tabulated in the report); some variation is to be expected because effectiveness is calculated for relatively small subsets of the data, which are limited, especially in CY 1977 to 1985 (the estimated 1.369 UEF for the entire database has a standard error of .259).

Table 1-11: Belt Effectiveness Exaggeration Factors for Various Subgroups

	Effect in 1977-85 FARS	Effect in 1986-99 FARS	Exaggeration Factor
Single-vehicle crashes	63.77	71.32	1.26
Multivehicle crashes	35.29	51.90	1.35
Frontal impacts	43.31	63.52	1.55
Side impacts	34.46	47.96	1.26
First-event rollovers	75.31	82.07	1.38
Driver & RF ≤ 30	55.37	63.85	1.23
Driver ≤ 30, RF ≥ 31	38.65	65.15	1.76
Driver ≥ 31, RF ≤ 30	35.10	58.98	1.58
Driver & RF ≥ 31	40.24	56.00	1.36
Male driver & RF	56.28	64.09	1.22
Male driver, Female RF	36.18	56.43	1.46
Female driver, Male RF	56.23	65.81	1.28
Female driver & RF	50.51	63.65	1.36
Drinking or other antisocial[23] behavior	53.97	66.08	1.36
No antisocial behavior	42.98	58.80	1.38
California	67.21	68.17	1.03
Texas	42.05	60.43	1.46
Florida	45.15	58.44	1.32
New York	52.01	66.10	1.42
Pennsylvania	26.34	62.85	1.98
Illinois	49.35	66.71	1.52
Michigan	43.99	60.27	1.41
Ohio	54.38	58.70	1.10
North Carolina	36.66	62.95	1.71
Georgia	36.22	62.06	1.68

[23] Driving under the influence of drugs, driving without a valid license, a history of violations or crashes, reckless driving, attempting to escape police, hit-and-run, and/or racing.

Table 1-11 (concluded): Belt Effectiveness Exaggeration Factors for Various Subgroups

	Effect in 1977-85 FARS	Effect in 1986-99 FARS	Exaggeration Factor
1986[24]	47.81	60.93	1.34
1987	47.81	58.32	1.25
1988	47.81	60.82	1.33
1989	47.81	62.57	1.39
1990	47.81	69.10	1.69
1991	47.81	61.82	1.37
1992	47.81	59.86	1.30
1993	47.81	60.49	1.32
1994	47.81	63.69	1.44
1995	47.81	62.91	1.41
1996	47.81	65.22	1.50
1997	47.81	58.38	1.25
1998	47.81	62.47	1.39
1999	47.81	59.04	1.27

Specifically, the UEF was fairly invariant over time (at least through 1999). The UEF also appears to be insensitive to the absolute level of belt use – e.g., it is about the same in the late 1990s, when national belt use was around 70 percent as in 1986, when it was still just 37 percent.[25] Similarly, the UEF is nearly the same for drinking drivers (low belt use) as sober drivers (higher belt use).

The key adjectives for the UEF, however, are "hypothetical" and "empirical." The UEF derived from the hypothesis that belt use reporting was accurate through 1985, not accurate starting in 1986, and that the difference in observed effectiveness is due to the less accurate reporting of belt use. The proposal that the UEF is fairly invariant over time and for widely different groups of drivers and crashes is based on the empirical results presented in Table 1-11. It is not based on a theoretical argument; the UEF is a complicated function of numerous highly correlated statistics – the actual and reported belt use of drivers and RF passengers, surviving and fatal – and the actual belt use is unknown (at least, prior to EDRs) and can only be surmised. Specifically, the UEF is an arithmetic adjustment factor; it is not the actual percentage of over-reporting, nor can that percentage be readily calculated from the UEF.

Recent NHTSA evaluation reports have continued to use the UEF to adjust observed estimates of belt effectiveness for drivers and RF passengers downwards, even in analyses of FARS data

[24] Includes all types of crashes, but compares effect in CY 1986 alone to effect in CY 1977 through 1985.

[25] Kahane, C. J. (2015, January). *Lives saved by vehicle safety technologies and associated Federal Motor Vehicle Safety Standards, 1960 to 2012 – Passenger cars and LTVs – With reviews of 26 FMVSS and the effectiveness of their associated safety technologies in reducing fatalities, injuries, and crashes* (Report No. DOT HS 812 069). Washington, DC: National Highway Traffic Safety Administration. P. 103. Available at crashstats.nhtsa.dot.gov/Api/Public/ViewPublication/812069

from calendar years later than 1999, in fact as recently as CY 2011.[26] The UEF was computed from a phenomenon that could only be witnessed in the data collected during the transition to belt laws from 1984 to 1986. It has become the basis for an effect projected indefinitely into the future and assumed to remain constant over time. But each passing year takes us a little further from the data supporting the original hypothesis and adds a little more uncertainty to that hypothesis. Now, finally, EDRs provide an opportunity to compare actual to reported belt use in crash data more recent than the CY 1984-to-1986 transition to belt laws. Furthermore, whereas the original computation of the UEF relied on the mere **assumption** that belt use was correctly reported before 1986, the EDR (to the extent it is accurate) tells us the true, actual belt use. Whereas the original computation of the UEF only allowed comparisons of aggregate belt use in one database versus another, the EDR allows individual comparisons of actual and reported belt use, case-by-case.

In summary, the analyses of this report have two potential goals:

1. Compare EDR-based **belt use** to what is coded in the crash data, to see if belt use is indeed over-reported in the crash data, especially for occupants with minor or no injury, consistent with NHTSA's past hypotheses; if so, it would reconfirm the agency's position that belt effectiveness estimates based directly on crash data are overstated and it would reconfirm the rationale for the UEF.

2. Furthermore, **belt effectiveness** might be estimated with the EDR-reported belt use and compared to the corresponding estimate based on the file's own reported belt use. This could eventually furnish updated estimates of fatality reduction as well as an updated estimate of the UEF.

As we shall see, this "preliminary" report will accomplish the first analysis goal and reconfirm the rationale for the UEF, but there will not be enough FARS cases cross-referenced to EDR data for a statistically meaningful update of the fatality reduction estimates or for a specific updated value of the UEF.

2. Belt use and effectiveness in crashes, based on EDR data

2.1 CDS database with belt use reported by EDR: By CY 2014, according to Table 1-1, 55 percent of the 255,000,000 registered cars and LTVs – approximately 140,000,000 vehicles were equipped with an EDR.[27] From a purely technological standpoint, it would be possible to download EDR information on any of those vehicles involved in crashes, providing an almost unlimited database. In reality, downloads have been far fewer. The tools to read EDR data are

[26] Kahane, C. J. (2013, May). *Injury vulnerability and effectiveness of occupant protection technologies for older occupants and women* (Report No. DOT HS 811 766). Washington, DC: National Highway Traffic Safety Administration. Pp. 213-222. Available at crashstats.nhtsa.dot.gov/Api/Public/ViewPublication/811766;
Kahane, C. J. (2013, November). *Effectiveness of pretensioners and load limiters for enhancing fatality reduction by seat belts* (Report No. DOT HS 811 835, pp. 1-3, 37-50). Washington, DC: National Highway Traffic Safety Administration. Available at crashstats.nhtsa.dot.gov/Api/Public/ViewPublication/811835

[27] Cars and LTVs registered in 2014: NHTSA. (2016). *Traffic safety facts 2014* (Report No. DOT HS 812 261). Washington, DC: Author. Pp. 30-32. Available at crashstats.nhtsa.dot.gov/Api/Public/ViewPublication/812261

not owned by as many organizations or individuals as, say, the tools to read vehicle diagnostic codes. Furthermore, vehicle owners think of the information on their EDR as private, and there are issues about what organizations have authority or permission to access it.

As of 2018 the Crash Investigation Sampling System of NHTSA's National Automotive Sampling System – and its predecessor through 2015, the Crashworthiness Data System – are the only databases accessible to the public that contain EDR data on a substantial number of crash-involved vehicles. Identification of individual drivers or vehicles has been removed from the data. CISS/CDS investigators request vehicle owners' permission to download the EDR for research purposes only, as part of a database without personal identifiers. Downloading began in CY 2002. Through CY 2015, CDS had received permission to download EDR data for nearly 12,000 crash-involved vehicles. This data may be accessed by the public via the NHTSA/NCSA website, where it is included in the individual CDS case files available for download. The tally of CDS cases from CY 2002 through 2015 includes:

67,183 CDS case vehicles (MY 1985-2014) with a decodable VIN and a driver whose age is known ("case vehicles" have to be towaways and, starting CY 2009, have to be less than 10 years old); of which

27,265 CDS case vehicles (MY 1994-2014) that are equipped with an EDR that can be read by commercially available tools;[28] of which

9,156 CDS case vehicles for which CDS received permission and downloaded the EDR;[29]

2,620 of which also had a RF passenger;

7,810 CDS case vehicles for which the EDR specifies the driver's belt use (yes or no);[30]

985 where the EDR specifies the RF passenger's belt use

The 7,810 CDS cases where the EDR specifies whether or not the driver was belted constitute the initial database for our analysis. The 985 cases where the EDR also specifies the RF passenger's belt use might at best allow a limited analysis; the number is much smaller than for drivers because: (1) The RF seat is occupied in fewer than one-third of the crash-involved case vehicles (2,620/9,156) and (2) Many of the earlier EDR systems only recorded belt use for the driver.

Some of the earlier EDR systems retain data only on the single most recent crash event, but most current systems can retain data on multiple events, including events in the sequence constituting the CDS crash, but also perhaps substantially earlier events unrelated to the CDS crash. For example, one widely used system records two types of crash events. A "non-deployment event" records data but does not deploy any air bag(s). This EDR can store a non-deployment event for

[28] Based on the list of make-models in Appendix A of this report.
[29] CDS investigators received permission and downloaded EDRs for 11,991 vehicles involved in CDS crashes, but 2,835 of them were not "case" vehicles because they were not towed away and/or (in CY 2009+) they were 10 or more years old.
[30] In other words, for 1,346 vehicles (9,156 – 7,810), the EDR specified "unknown" belt use or specified a known belt use only for an earlier event that was not part of the CDS crash.

approximately 250 ignition cycles[31] or until there is another non-deployment event with an even greater longitudinal velocity change. A "deployment event" deploys at least one of the available air bags. This type of EDR can store up to two events: two deployment events if they occur within 5 seconds of one another; or one deployment event and one non-deployment event if there was no second deployment event and if the non-deployment event happened during the 5 seconds before the deployment event. Five seconds after the first deployment event, this type of EDR "locks" and cannot record any other events.

When it comes to assessing drivers' belt use in CDS crashes, it is important to distinguish between events that happened during or immediately before the crash sequence (which are relevant) and other events that happened long before the CDS crash (where the driver's belt use is irrelevant to the CDS crash; in fact, it might even have been a different person driving the vehicle). CDS data provides two types of data elements to distinguish the type of event. One type, stored within the EDR, is the "ignition cycle" number of the event. An ignition cycle consists of somebody turning the ignition on, possibly starting the engine and driving, and then turning it off. The vehicle's computer keeps a running count of how many times the ignition has been turned on and off since the vehicle first emerged from the assembly line. When the EDR saves data on an event, it will save the ignition cycle number for that event (a data element called EVCYCLES in the SAS database). CDS also identifies on which ignition cycle the EDR was downloaded by CDS investigators (INVCYCLE in the SAS database).

The second type of data element, supplied by CDS investigators (and called EACCSEQ in the SAS database), places the recorded events that are part of the CDS crash sequence in chronological order: "1" is the first recorded event, "2" the second, etc.; however, a code of "97" for this data element indicates that the event was not related to the CDS crash.

All the recorded events that are part of the CDS crash should have the same value of EVCYCLES; moreover, this number should be close to INVCYCLE, the latter being just a few cycles higher (because the CDS investigation usually takes place not long after the crash, and the vehicle, being a towaway, would not have been driven many times between the crash and the investigation).

When all of these data elements are fully and accurately recorded, it is not difficult to determine the driver's belt use in the CDS crash, as in the following hypothetical example (which is not the "widely used" type of EDR described above, because it retains more than two events):

---

[31] One "ignition cycle" consists of turning the ignition on, possibly starting the engine and driving, and then turning it off.

EV-CYCLES	INV-CYCLE	EACCSEQ	Deployment Event?	Driver Belt Use
740	976	97 (Event not related to the CDS crash)	No	No
974	976	1 (1st recorded event in CDS crash)	No	**Yes**
974	976	2 (2nd recorded event in CDS crash)	Yes	**Yes**

This CDS crash included two EDR-recorded crash-sequence events that occurred on the 974th ignition cycle of the vehicle's lifetime: a non-deployment event (1st) followed by a deployment event (2nd). The EDR consistently indicates that the driver was belted during the two CDS crash events. CDS personnel investigated the crash and downloaded the EDR on the 976th cycle, presumably soon after the crash took place (974th cycle). This EDR, however, also recorded an earlier event, on the 740th cycle, long before and unrelated to the CDS crash (event sequence code 97). The fact that whoever was driving the vehicle at that time was unrestrained is irrelevant to the belt use in the CDS crash and it may be disregarded.

However, there are many cases where one or both of the data elements are not fully reported. The ignition cycle number may be blank (or occasionally have an implausible value) for some or all EDR events and/or for the CDS investigation cycle. The event sequence number might be coded 98 or 99, which, depending on the context, might indicate that the investigator does not even know if it was part of the CDS crash or, alternatively, that the investigator knows it is part of the CDS crash, but merely cannot sequence the events within that crash.

Nevertheless, it is not always necessary to have full data on these variables. For example, if the ignition cycle number is known for each crash event, it is perfectly clear that only the events on the highest cycle can be part of the CDS crash, even if the event sequence numbers are unknown. Conversely, if the event sequence numbers are all 1, 2, 3… or 97, it is clear that the former are part of the CDS crash, even if the ignition cycle is unknown for those events. Even if data is missing on both of those variables but there is a single deployment event, we may assume that this event was part of the CDS crash, because EDR systems generally lock up soon after a deployment.

When two or more events are part of the CDS crash sequence (e.g., as evidenced by having the same, maximum ignition cycle among the recorded events and by event sequence numbers 1, 2, 3…) a potential complication is that these events might have conflicting values for driver belt use according to the EDR: one says "belted" and another says "unrestrained." Presumably, this could be an artifact of the recording system (e.g., defaulting to "unrestrained" if the system loses power, as discussed in Section 1.1) or a malfunction of the belt itself – because it is unlikely that the drivers themselves would buckle or unbuckle the belts during the few seconds between the events that constitute the CDS crash. Another possibility: it is not always crystal-clear if an event is part of the crash sequence (see the discussion in Appendix B) and it is conceivable that one of the two events with conflicting belt use was not actually part of the crash sequence.. In such cases, if any EDR event **that this report classifies as part of the CDS crash** says "belted," this report will assume that the driver was belted at the beginning of the CDS crash sequence and it will disregard other events, even within the CDS crash sequence, that say "unrestrained." This complication is infrequent. Among 1,676 vehicles with multiple crash-sequence readings of belt use for the driver, only 44 have a mix of "yes" and "no" readings, while all readings are in agreement for 1,632 of the vehicles. Among 781 vehicles with multiple readings for the RF passenger, only 12 have a mix of "yes" and "no" readings.

Appendix B documents the "decision tree" that this report uses to establish belt use depending on whether there are single or multiple EDR events and whether the ignition cycle numbers, event sequence numbers, and/or other variables are known or unknown. The CDS database for this report includes all 104,944 vehicle-level records in CDS from CY 2002 through 2015 and MY 1985 to 2016, even if no EDR data was acquired, of which 67,183 are CDS "case vehicles" with a decodable VIN and a driver whose age is known, of which 7,810 have an assessment of belt use from the EDR ("yes" or "no," not "unknown"). Because CDS includes the assessment of belt use in the police report for that crash (PARUSE) as well as the CDS investigator's assessment (MANUSE), our vehicle-level database includes belt use for the driver and for the RF passenger (if that seat is occupied) according to the EDR, the CDS investigator, and the police report.

2.2 FARS database with belt use reported by EDR: It is sometimes possible to cross-reference CDS cases that are fatal crashes to FARS cases, based on similar fields in the two databases – e.g., if the first 12 characters of the VIN, the driver's age and gender, and the CY and month of the crash match up.[32] The 104,944 vehicle records on our CDS database include 6,476 vehicles involved in fatal crashes (as evidenced by ATREAT = 1); 5,349 of them were cross-referenced to FARS vehicle records – including 433 that have an assessment of the driver's belt use from the EDR ("yes" or "no," not "unknown"), but only 40 cases where the RF seat is occupied and there is an assessment of the RF passenger's belt use from the EDR. The 433 FARS cases with an EDR assessment of the driver's belt use are the database for the principal analyses of this report.

2.3 Belt use in CDS crashes: EDR-reported versus CDS-investigator-determined: The two left columns of Table 2-1 (which is excerpted from Table 1-2) compare the EDR-reported and CDS investigator-determined belt use in CDS crashes by calendar year, for CDS case vehicles where the EDR information was downloaded and stored by CDS personnel. For comparison purposes, the right column shows CDS-investigator-determined belt use for the vehicles that were not equipped with an EDR or where CDS did not download the EDR. The percentages are based on weighted CDS data.

[32] Mynatt, M., Bean, J. D., Kahane, C. J., Rush, C. J., Traube, E., & Wiacek, C. (2011, June). A study of NMVCCS to identify critical precrash factors in fatal crashes. *Proceedings of the Twenty-Second International Technical Conference on the Enhanced Safety of Vehicles*. (Paper No. 11-0168). Washington, DC: National Highway Traffic Safety Administration. Available at www-nrd.nhtsa.gov/pdf/esv/esv22/22ESV-000168.pdf

Table 2-1: Drivers' Seat Belt Use (%) in CDS Crashes by Calendar Year, 2002 to 2015: EDR-Reported Versus CDS-Investigator-Determined

	Vehicles With EDR Download		Vehicles Without EDR Download
Calendar Year	EDR-Reported	CDS Investigator-Determined	CDS Investigator-Determined
2002	60	80	80
2003	70	87	85
2004	62	87	87
2005	56	86	86
2006	67	81	87
2007	82	91	87
2008	74	87	87
2009	73	88	91
2010	72	90	92
2011	66	69	93
2012	76	77	94
2013	77	79	94
2014	70	67	95
2015	75	83	88

As discussed in Section 1.2, EDR-reported belt use was substantially lower than CDS-investigator-determined in the same vehicles from 2002 through 2010, with gaps ranging from 9 percentage points (2007) up to 30 percentage points (2005). But from 2011 onwards, EDR-reported and CDS-investigator-determined use rates were quite similar: CDS-investigator-determined belt use dropped substantially from 2010 (90%) to 2011 (69%). By contrast, in the vehicles without EDR downloads, CDS-investigator-determined belt use continued rising slightly after 2010, at least through 2014.

In other words, CDS investigators in recent years have incorporated the EDR readouts as a crucial part of their evidence for assessing belt use on the vehicles where the readouts are available – while continuing to assess belt use by similar methods as in the past for vehicles without EDR information.

Table 2-2 focuses on the individual CDS driver records where the EDR has been downloaded, indicating whether the EDR-reported and CDS-investigator-determined belt use disagree. It is based on a subset of the 7,810 CDS cases with EDR downloads where DRVBELT = 1 or 2, namely the 7,786 driver cases where, additionally, CDS reports the driver's belt use as "yes" or "no," not unknown (i.e., MANUSE = 0,1: unrestrained; MANUSE = 2 through 5: belted; all other drivers excluded). Table 2-2 is based on unweighted CDS data, because its purpose is not to estimate national rates, but to examine the data on a case-by-case basis; the right column shows the number of cases involved in each year. From CY 2002 through 2005, the EDR disagrees with the CDS-investigator-determined belt use for about 20 percent of the drivers. Disagreement percentages taper down to the mid- or lower teens during 2006 through 2010 and then abruptly fall to almost zero in 2011 through 2015:

Table 2-2: Percentage of Drivers Where EDR-Reported and CDS-Investigator-Determined Belt Use Disagrees (Vehicles with EDR download, unweighted CDS, CY 2002 to 2015)

Calendar Year	% EDR and CDS Disagree	Number of Driver Cases
2002	22	417
2003	18	552
2004	21	719
2005	21	550
2006	14	608
2007	18	493
2008	17	431
2009	13	390
2010	12	345
2011	2	621
2012	2	636
2013	1	715
2014	1	702
2015	1	607

From 2011 onward, CDS investigators almost always relied on the EDR data, when available, for the belt use coded in CDS (MANUSE). The few cases of disagreement after 2010 in Table 2-2 might be due to the investigator (1) having reason to believe the EDR data is inaccurate, (2) having overwhelming physical evidence to supersede the EDR data, or (3) interpreting data from multiple EDR events in a slightly different way than the algorithm in Appendix B of this report. The numbers in Table 2-1 show that CDS investigators continued reporting high belt use in vehicles without EDR data after 2010, even as they obtained substantially lower rates of belt use in the vehicles with EDR downloads. The years 2006 through 2010 are a transitional period, where MANUSE was sometimes based primarily on the EDR data, but sometimes not, even when an EDR had been downloaded.

CDS investigators have had the option to consider EDR data since CDS began downloading EDRs in 2002. All CDS coding and editing manuals published after 2002 (the earliest in 2006[33] and the most recent, as of August 2017, in 2014[34]) list the EDR download variables as part of the

[33] NHTSA. (2006, January). *National Automotive Sampling System, Crashworthiness Data System 2006 coding and editing manual*. Washington, DC: Author. Available at crashstats.nhtsa.dot.gov/Api/Public/ViewPublication/CDS06

[34] NHTSA. (2015, October). *National Automotive Sampling System, Crashworthiness Data System 2014 coding and editing manual* (Report No. DOT HS 812 195). Washington, DC: Author. Available at crashstats.nhtsa.dot.gov/Api/Public/ViewPublication/812195

"Vehicle Exterior Form," which is, in turn, a subsection of the "vehicle inspection." All of them identically tell investigators to first code belt use on the "Safety Systems Form" using "only the evidence in the vehicle at the time of inspection"; subsequently, to code MANUSE on the "Occupant Assessment Form" based on this "vehicle evidence along with police report information, interviews, relationship of contact points to seat position relative to the PDOF(s) assigned to the vehicle, presence of belt-caused occupant injuries, and presence or absence of ejection."[35] But the use of this EDR data soared after 2010; NHTSA's NASS staff verified that CDS personnel received extensive training in 2010 in the interpretation of EDR codes.

In summary, EDR data may have been used in coding MANUSE just occasionally through CY 2005 and still not too frequently through 2008, but ever more often after that.

Next, let us concentrate on the CDS data for 2002 through 2008, where the EDR was infrequently consulted in coding MANUSE, and take a closer look at how EDR-reported and CDS-investigator-determined belt use disagree. Here is a table of the 3,630 [unweighted] driver cases where the EDR indicated whether or not the driver was belted (DRVBELT = 1 or 2) and so did MANUSE (i.e., MANUSE = 0,1: unrestrained; MANUSE = 2 through 5: belted; all other drivers excluded). Table 2-3 indicates the EDR agreed with MANUSE in 2,962 cases and disagreed in 668 cases:

Table 2-3: EDR-Reported Versus CDS-Investigator-Determined Driver Belt Use, CY 2002-2008

		EDR-REPORTED		
		UNRESTRAINED	BELTED	Total
INVESTIGATOR-DETERMINED	UNRESTRAINED	648	55	703
	BELTED	613	2314	2927
	Total	1261	2369	3630

Moreover, the disagreements are skewed in one direction, with 613 cases where CDS said "belted" and the EDR said "unrestrained," but only 55 cases vice-versa. As a consequence, CDS-investigator-determined belt use is higher, overall, than the EDR-reported use. The pattern becomes clearer when drivers with moderate or greater injuries are tabulated separately from the drivers with minor or no injuries. "Drivers with moderate or greater injury" include fatalities and drivers with MAIS = 2, 3, 4, or 5, where the MAIS is a person's maximum-severity injury on the abbreviated injury scale (AIS).[36] Table 2-3a shows that the largest group of disagreements, by far, is the 493 drivers with minor or no injury that CDS coded as belted, but the EDR reported to be unrestrained:

---

[35] *Ibid*., pp. SS-1, EV-204, and OF-167.
[36] Gennarelli, T. A., & Wodzin, E. (2006). AIS 2005: a contemporary injury scale. *Injury*, *37*, pp. 1083-1091.

Table 2-3a: EDR-Reported Versus CDS-Investigator-Determined Driver Belt Use, CY 2002-2008

Drivers With Moderate or Greater Injuries

		EDR-REPORTED		
		UNRESTRAINED	BELTED	Total
INVESTIGATOR-DETERMINED	UNRESTRAINED	289	18	307
	BELTED	120	388	508
	Total	409	406	815

Drivers With Minor or No Injury

		EDR-REPORTED		
		UNRESTRAINED	BELTED	Total
INVESTIGATOR-DETERMINED	UNRESTRAINED	359	37	396
	BELTED	**493**	1926	2419
	Total	852	1963	2815

Table 2-4 presents the same data as side-by-side univariate tabulations, making it easier to see the absolute and proportional net change in each category:

Table 2-4: EDR-Reported and CDS-Investigator-Determined Driver Belt Use by Injury Severity, CY 2002-2008

	CDS Belt Use	EDR Belt Use	Absolute Change	Proportional Change
Belted MAIS 2+	508	406	- 102	- 20 %
Unrestrained MAIS 2+	307	409	+ 102	+ 33 %
Belted MAIS 0 or 1	2,419	1,963	- 456	- 19 %
Unrestrained MAIS 0 or 1	396	852	+ 456	+ 115 %

By far the largest change in relative terms (and also largest in absolute terms) is the 115-percent increase, from 396 to 852, of unrestrained drivers with minor or no injury. Directionally, this is exactly the tendency hypothesized in NHTSA's 2000 evaluation of seat belts[37] as a basis for defining the UEF (defined in Sections 1.5 and 1.6 of the current report). Moving a substantial portion of the drivers with minor or no injury from the "belted" to the "unrestrained" column will

[37] Kahane (2000), pp. 10-19.

increase the observed moderate-injury risk for belted drivers and reduce it for unrestrained drivers. This should, in turn, make observed belt effectiveness lower with EDR-reported belt use than with CDS-investigator-determined belt use in CY 2002 through 2008.

In a 2009 report, Swanseen studied a similar CDS database and likewise noted that many of the CDS-EDR disagreements were drivers coded as belted but identified by the EDR as unrestrained; that resulted in lower effectiveness estimates with EDR-reported belt use.[38]

2.4 Injury reduction by belts – EDR-reported versus CDS-investigator-determined belt use: CDS, being a probability sample of the nation's towaway crashes, permits computation of unbiased injury rates per 100 towaway-involved drivers, using the nationally weighted data (i.e., weighting each case by the national weight factor, RATWGT). An effectiveness estimate for belts may be computed simply by comparing the injury rates for belted and unrestrained drivers.[39] We will work with rates of moderate or greater injury (MAIS 2+) because this is the most inclusive type of non-minor injury, offering the greatest hope of statistically meaningful results, given the limited CDS data with EDR downloads. Table 2-5, based on the same 3,630 CDS driver cases with EDR downloads for CY 2002 through 2008 as in the preceding section, compares the weighted injury rates for belted and unrestrained drivers, based on the CDS-investigator-determined belt use:

Table 2-5: MAIS 2+ Injury Rate Per 100 Drivers, Based on CDS-Investigator-Determined Belt Use (CY 2002-2008 CDS towaway vehicles with EDR downloads, weighted data)

CDS-Investigator-Determined Belt Use	Drivers With MAIS ≥ 2	Drivers	Injury Rate (%)	Injury Reduction
Unrestrained	38,201	242,794	15.73	
Belted	85,694	1,472,590	5.82	63 %

The injury rate is 15.73 percent for the drivers coded as unrestrained, 5.82 percent for belted: an observed 63-percent injury reduction for belts. Table 2-6 is based on the same data, but now belt use is classified based on what the EDR reported:

[38] Swanseen, K. D. (2009, December). *Effect of belt usage reporting errors on injury risk estimates*. (Report No. etd-12172009-175352). Blacksburg, VA: Virginia Tech. Available at theses.lib.vt.edu/theses/available/etd-12172009-175352

[39] This estimate does not control for possible differences in the demographics, vehicles, or crash severities of belted and unrestrained drivers.

Table 2-6: MAIS 2+ Injury Rate Per 100 Drivers, Based on EDR-Reported Belt Use
(CY 2002-2008 CDS towaway vehicles with EDR downloads, weighted data)

EDR-Reported Belt Use	Drivers With MAIS ≥ 2	Drivers	Injury Rate (%)	Injury Reduction
Unrestrained	67,088	581,715	11.53	
Belted	56,807	1,133,670	5.01	57 %

The number of unrestrained crash-involved drivers is much larger in Table 2-6 than in Table 2-5 (581,715 versus 242,794), because the EDR reported as unrestrained many of the drivers that CDS had coded as belted. This increase is partially offset by a proportionately smaller increase in the injured unrestrained drivers (67,088 versus 38,201). As a consequence, the unrestrained injury rate is substantially lower in Table 2-6 (11.53%) than in Table 2-5 (15.73%). The belted injury rate is slightly lower in Table 2-6 (5.01%) than in Table 2-5 (5.82%). The observed injury reduction for belts is 63 percent, based on CDS-investigator-determined belt use, but only 57 percent, based on EDR-reported belt use.

Similarly, for drivers with serious injury (MAIS ≥ 3), the observed injury reduction with belts is 74 percent, based on CDS-investigator-determined belt use, but only 54 percent, based on EDR-reported belt use. For drivers with life-threatening or fatal injury (MAIS ≥ 4), the observed injury reduction with belts is 73 percent, based on CDS-investigator-determined belt use, but only 24 percent, based on EDR-reported belt use. The observed fatality reduction for belts in this CDS database is 84 percent, based on CDS-investigator-determined belt use, but only 50 percent, based on EDR-reported belt use. Thus, at every level of injury severity, the observed belt effectiveness is lower with the EDR-reported belt use – although it is difficult to quantify how much, because injury rates have a lot of sampling error, given the limited number of CDS cases.

For a wider perspective, let us consider only the rate of moderate or greater injury (MAIS ≥ 2) based on CDS-investigator-determined belt use, but for our entire CY 2002-to-2015 CDS database of 67,183 case vehicles, subdivided into four quadrants:

- CY 2002-to-2008 cases with EDR downloads [that reported the driver's belt use];
- CY 2009-to-2015 cases with EDR downloads;
- CY 2002-to-2008 cases without EDR downloads; and
- CY 2009-to-2015 cases without EDR downloads

The first quadrant is identical to the data we have just analyzed. In the other three quadrants, the EDR-reported belt use is usually identical to the CDS-investigator-determined (second quadrant) or unavailable (third and fourth quadrant); thus, the analysis that follows is based exclusively on the CDS-investigator-determined belt use, which is available in all four quadrants. Table 2-7 computes belt effectiveness:

Table 2-7: MAIS 2+ Injury Rate Per 100 Drivers, Based on CDS-Investigator-Determined Belt Use (CY 2002-2015 CDS towaway vehicles, weighted data)

CDS-Investigator-Determined Belt Use	Drivers With MAIS ≥ 2	Drivers	Injury Rate (%)	Injury Reduction
	CY **2002-2008**: vehicles **with** EDR downloads			
Unrestrained	38,201	242,794	15.73	
Belted	85,694	1,472,590	5.82	63 %
	CY **2009-2015**: vehicles **with** EDR downloads			
Unrestrained	57,164	449,506	12.72	
Belted	131,906	1,530,593	8.62	32 %
	CY **2002-2008**: vehicles **without** EDR downloads			
Unrestrained	270,197	1,306,428	20.68	
Belted	607,345	11,320,935	5.36	74 %
	CY **2009-2015**: vehicles **without** EDR downloads			
Unrestrained	100,092	468,892	21.35	
Belted	300,990	5,780,912	5.21	76 %

For the vehicles with EDR downloads, the observed injury reduction was 63 percent in CY 2002 through 2008, when CDS investigators for the most part did not use EDR data to inform their coding of MANUSE. But observed effectiveness fell sharply to 32 percent in CY 2009 through 2015, when the EDR increasingly became a primary source for assessing MANUSE. By contrast, in the vehicles without EDR downloads, where CDS investigators could only rely on their earlier methods to assess belt use, the observed injury reduction stayed almost the same: a too-good-to-be-true 74 percent in 2002 through 2008 and 76 percent in 2009 through 2015. In other words, Table 2-7 confirms the preceding analysis that belt effectiveness is overestimated if EDR data is not employed or not available to correct other sources of information on drivers' belt use.

2.5 EDR-reported versus police-reported belt use in CDS crashes: The CDS variable PARUSE is the belt use specified on the police report associated with the crash. Thus, the distribution of PARUSE in CDS provides an unbiased estimate of police-reported belt use in the nation's towaway crashes. The two left columns of Table 2-8 (which is excerpted from Table 1-2) compare the EDR-reported and police-reported belt use in CDS crashes by calendar year, for CDS case vehicles where the EDR information was downloaded by CDS personnel. The right column shows police-reported belt use for the vehicles that were not equipped with an EDR or where CDS did not download the EDR. The percentages are based on weighted CDS data.

Table 2-8: Drivers' Seat Belt Use (%) in CDS Crashes by Calendar Year, 2002 to 2015: EDR-Reported Versus Police-Reported

	Vehicles With EDR Download		Vehicles Without EDR Download
Calendar Year	EDR-Reported	Police-Reported	Police-Reported
2002	60	94	94
2003	70	95	94
2004	62	95	94
2005	56	97	95
2006	67	93	95
2007	82	96	95
2008	74	93	96
2009	73	95	96
2010	72	98	95
2011	66	97	95
2012	76	98	97
2013	77	95	96
2014	70	95	97
2015	75	97	95

As discussed in Section 1.2, police-reported belt use in crashes has consistently been close to 95 percent since 2002, well above any percentage reported in crashes by EDRs or observed on the road in NOPUS. Unlike the CDS-investigator-determined belt use (Table 2-1), no visible downward trend starts in 2011 or in any other recent year – suggesting that, in the majority of crashes, police have continued to use their existing methods of assessing belt use rather than downloading EDRs for that purpose.

As of February 2018, there do not appear to be any national statistics on the use of EDR data by police agencies. Nevertheless, there is evidence from literature and Internet searches that a moderate number of police agencies have acquired tools to download EDRs and limited evidence of police incorporating EDR data in their crash investigations:

- The 2016 online brochure of the Crash Data Group,[40] the subsidiary of Robert Bosch GmbH that supplies the CDR tool, asserts that "EDR capabilities have become increasingly important in accident investigations because they store potentially important data that may be very useful in evaluating car crashes… There are currently **hundreds** [emphasis added] of law enforcement agencies using the Bosch CDR tool to obtain

[40] Available at www.crashdatagroup.com/learnmore/lawenforcement.html

valuable crash data during their investigation." (There were a total of 15,388 local, county, and State law enforcement agencies in the United States in 2013.[41])

- A 2015 article in the Portland, Maine *Press Herald* discusses the value of EDR data in crash investigation (including detection of belt use), noting that five police officers in Maine had been trained to download and analyze EDR data as of 2015 (three State police officers, one county, one municipal). The three State police officers downloaded a total of 67 EDRs in 2014 and 68 in 2013, whereas the county officer downloaded 15 EDRs in 2014.[42] (There were 31,880 crashes in Maine during 2014.[43])
- Similarly, a 2017 article in the Battle Creek, Michigan *Enquirer* informs us that local police have by now pulled EDRs from 250 crash-involved vehicles. They "always obtain court-ordered search warrants before obtaining the data."[44]
- The Philadelphia Police Foundation is currently (February 2018) soliciting donations to purchase one CDR tool and train 10 officers to download EDRs; this would enable Philadelphia police to download EDRs themselves rather than relying on the State Police (who already have the tool and training) to do it for them.[45]

This anecdotal evidence suggests that selected police agencies have been downloading EDR data for some time, but probably for a negligible percentage of the nation's police-reported crashes, if crashes of all severity levels are included.

Table 2-9 focuses on the individual CDS driver records where the EDR has been downloaded, indicating whether the EDR-reported and police-reported belt use disagree. The first column of Table 2-9 shows that the absolute percentage of disagreements has gradually declined from around 30 percent to 15 percent. That may be a misleading statistic. Year after year, the police report that almost everybody (approximately 95%) buckles up in crashes. But as actual belt use on the road steadily increased from 76 percent in 2002 to 89 percent in 2015 (see Table 1-2), reality is gradually "catching up" with what is reported, and there will be ever fewer disagreements. (For example, given 95% police-reported belt use, if true belt use is zero, the disagreement rate would be 95%, but if true belt use is 100%, the disagreement rate would be only 5%.) Perhaps a more meaningful statistic would be the number of police-EDR disagreements **relative to** the proportion of drivers on the road who are unrestrained, as observed in NOPUS. That statistic is shown in the right column of Table 2-9. It has stayed fairly close to 1.50 from 2002 through 2015. It has changed little over time, consistent with the anecdotal evidence that police, as of 2015, are using EDR data to assess belt use in, at most, a small percentage of crash reports.

---

[41] Reaves, B. A. (2015, May). *Local police departments, 2013: personnel, policies, and practices*. Washington, DC: Bureau of Justice Statistics. Available at www.bjs.gov/content/pub/pdf/lpd13ppp.pdf

[42] Hench, D. (2015, March 2). 'Black boxes' in cars capture data, and the truth. *Portland Press Herald*. Available at www.pressherald.com/2015/03/02/black-boxes-in-cars-capture-data-and-the-truth

[43] Maine Department of Transportation. (2017). *2016 Maine highway safety facts*. Augusta, ME: Author. P. 44. Available at maine.gov/mdot/safety/docs/2017/MaineHighwaySafetyFacts_12.2016.pdf

[44] Christenson, T. (2017, April 20). Car data recorders aid police investigations. *Battle Creek Enquirer*. Available at www.battlecreekenquirer.com/story/news/local/2017/04/20/car-data-recorders-aid-police-investigations/100573746/

[45] Available at phillypolicefoundation.org/projects/crash-data-retrieval-system

Table 2-9: Percentage of Drivers Where EDR-Reported and Police-Reported Belt Use Disagrees (Vehicles with EDR download, unweighted CDS, CY 2002 to 2015)

Calendar Year	% EDR and CDS Disagree	% Unrestrained (NOPUS)	Disagreements / Unrestrained
2002	32.7	24	1.36
2003	29.5	20	1.48
2004	29.6	19	1.56
2005	30.6	17	1.80
2006	25.7	18	1.43
2007	24.4	17	1.43
2008	24.3	16	1.52
2009	20.0	15	1.33
2010	20.2	14	1.44
2011	25.2	16	1.57
2012	20.2	13	1.56
2013	19.2	12	1.60
2014	16.8	13	1.29
2015	15.9	11	1.45

Tables 2-10 through 2-11 take a closer look at how EDR-reported and police-reported belt use disagrees in CY 2002 through 2015. Table 2-10 includes the 7,033 [unweighted] driver cases where the EDR indicated whether or not the driver was belted (DRVBELT = 1 or 2) and so did PARUSE (i.e., PARUSE = 0,1: unrestrained; PARUSE = 2 through 5: belted; all other drivers excluded). The EDR agreed with PARUSE in 5,382 cases and disagreed in 1,651 cases:

Table 2-10: EDR-Reported Versus Police-Reported Driver Belt Use, CY 2002-2015

		EDR-REPORTED		
		UNRESTRAINED	BELTED	Total
POLICE-REPORTED	UNRESTRAINED	448	90	538
	BELTED	1561	4934	6495
	Total	2009	5024	7033

The disagreements are skewed in one direction, with 1,561 cases where police said "belted" and the EDR said "unrestrained," but only 90 cases vice-versa. As a consequence, police-reported belt use is higher, overall, than the EDR-reported use. Table 2-10a lists drivers with moderate or greater injuries (as coded during the subsequent CDS investigations of these cases) separately from the drivers with minor or no injuries. The largest group of disagreements, by far, is the

1,216 drivers with minor or no injury that police coded as belted, but the EDR reported to be unrestrained:

Table 2-10a: EDR-Reported Versus Police-Reported Driver Belt Use, CY 2002-2015

Drivers With Moderate or Greater Injuries

```
                         EDR-REPORTED

                  |UNRESTRA|BELTED  |  Total
                  |INED    |        |
     -------------+--------+--------+
     UNRESTRAINED |    284 |     32 |    316
POLICE-REPORTED  -------------+--------+--------+
     BELTED       |    345 |    774 |   1119
     -------------+--------+--------+
     Total             629      806     1435
```

Drivers With Minor or No Injury

```
                         EDR-REPORTED

                  |UNRESTRA|BELTED  |  Total
                  |INED    |        |
     -------------+--------+--------+
     UNRESTRAINED |    164 |     58 |    222
POLICE-REPORTED  -------------+--------+--------+
     BELTED       |   1216 |   4160 |   5376
     -------------+--------+--------+
     Total            1380     4218     5598
```

Table 2-11 presents the same data as side-by-side univariate tabulations, displaying the absolute and proportional net change in each category:

Table 2-11: EDR-Reported and Police-Reported Driver Belt Use by Injury Severity, 2002-2015

	Police-Reported Belt Use	EDR Belt Use	Absolute Change	Proportional Change
Belted MAIS 2+	1,119	806	- 313	- 28 %
Unrestrained MAIS 2+	316	629	+ 313	+ 99 %
Belted MAIS 0 or 1	5,376	4,218	- 1,158	- 22 %
Unrestrained MAIS 0 or 1	222	1,380	+ 1,158	+ 422 %

By far the largest change in relative terms (and also largest in absolute terms) is the 422-percent increase, from 222 to 1,380, of unrestrained drivers with minor or no injury. Moving a substantial portion of the drivers with minor or no injury from the "belted" to the "unrestrained" column will increase the observed moderate-injury risk for belted drivers and reduce it for unrestrained drivers, making the observed belt effectiveness lower with EDR-reported belt use than with police-reported belt use in CY 2002 through 2015.

2.6 Injury reduction by belts – EDR-reported versus police-reported belt use: Effectiveness is estimated by simply comparing the belted and unrestrained MAIS 2+ injury rates per 100 drivers (weighted data), as in Section 2.4. Table 2-12, based on the same 7,033 CDS driver cases with EDR downloads for CY 2002 through 2015 as in the preceding section, compares the weighted injury rates for belted and unrestrained drivers, based on the police-reported belt use:

Table 2-12: MAIS 2+ Injury Rate Per 100 Drivers, Based on Police-Reported Belt Use
(CY 2002-2015 CDS towaway vehicles with EDR downloads, weighted data)

Police-Reported Belt Use	Drivers With MAIS ≥ 2	Drivers	Injury Rate (%)	Injury Reduction
Unrestrained	39,044	138,749	28.14	
Belted	254,348	3,365,323	7.56	73 %

The injury rate is 28.14 percent for the drivers coded as unrestrained, 7.56 percent for belted: an observed 73-percent injury reduction for belts. Table 2-13 is based on the same data, but now belt use is classified based on what the EDR reported:

Table 2-13: MAIS 2+ Injury Rate Per 100 Drivers, Based on EDR-Reported Belt Use
(CY 2002-2015 CDS towaway vehicles with EDR downloads, weighted data)

EDR-Reported Belt Use	Drivers With MAIS ≥ 2	Drivers	Injury Rate (%)	Injury Reduction
Unrestrained	111,249	959,194	11.60	
Belted	182,143	2,544,879	7.16	38 %

The number of unrestrained crash-involved drivers is much larger in Table 2-6 than in Table 2-5 (959,194 versus 138,749), because the EDR reported as unrestrained many of the drivers that police had reported as belted. This increase is partially offset by a proportionately smaller increase in the injured unrestrained drivers (111,249 versus 39,044). As a consequence, the unrestrained injury rate is substantially lower in Table 2-13 (11.60%) than in Table 2-12 (28.14%). The belted injury rate is slightly lower in Table 2-13 (7.16%) than in Table 2-5 (7.56%). The observed injury reduction for belts is 73 percent, based on police-reported belt use, but only 38 percent, based on EDR-reported belt use.

Similarly, for drivers with serious injury (MAIS ≥ 3), the observed injury reduction with belts is 88 percent, based on police-reported belt use, but only 58 percent, based on EDR-reported belt use. For drivers with life-threatening or fatal injury (MAIS ≥ 4), the observed injury reduction with belts is 91 percent, based on police-reported belt use, but only 50 percent, based on EDR-reported belt use. The observed fatality reduction for belts in this CDS database is 96 percent, based on police-reported belt use, but only 61 percent, based on EDR-reported belt use. Thus, at every level of injury severity, the observed belt effectiveness is far too good to be true with the police-reported belt use, but fairly realistic with the EDR-reported belt use (taking into account that the estimates may have substantial sampling error and do not control for possible differences in the demographics, vehicles, or crash severities of belted and unrestrained drivers).

As in Section 2.4, Table 2-14 offers a wider perspective by considering the rate of moderate or greater injury (MAIS ≥ 2) based on police-reported belt use for the entire CY 2002-to-2015 CDS database, subdivided into four quadrants:

Table 2-14: MAIS 2+ Injury Rate Per 100 Drivers, Based on Police-Reported Belt Use
(CY 2002-2015 CDS towaway vehicles, weighted data)

Police-Reported Belt Use	Drivers With MAIS ≥ 2	Drivers	Injury Rate (%)	Injury Reduction
	CY **2002-2008**: vehicles **with** EDR downloads			
Unrestrained	20,110	79,974	25.15	
Belted	96,846	1,542,401	6.28	75 %
	CY **2009-2015**: vehicles **with** EDR downloads			
Unrestrained	18,934	58,776	32.21	
Belted	157,502	1,822,922	8.64	73 %
	CY **2002-2008**: vehicles **without** EDR downloads			
Unrestrained	184,952	584,029	31.67	
Belted	681,308	12,594,948	5.41	83 %
	CY **2009-2015**: vehicles **without** EDR downloads			
Unrestrained	59,387	206,650	28.74	
Belted	343,450	6,922,210	4.96	83 %

Unlike Table 2-7, we do not see a sharp drop in observed effectiveness in the second quadrant; it is unrealistically high in all four quadrants. Table 2-7 showed much lower effectiveness in the second quadrant because CDS investigators largely relied on EDR data, when it was available, to code belt use in 2009 to 2015. Because effectiveness based on police-reported belt use is about the same in CY 2009 to 2015 as in 2002 to 2008, we may surmise that, as of 2015, police had not yet begun to extensively rely on EDR data to code belt use, but at most only in a small proportion of their crash investigations.

2.7 Belt use in FARS – EDR-reported versus FARS-reported: As discussed in Section 2.2, 433 FARS vehicle cases in CY 2002 through 2015 have been cross-referenced, via CDS, to an assessment of the driver's belt use from the EDR ("yes" or "no," not "unknown"). Only 40 cases where the RF seat is occupied have been cross-referenced to an assessment of the RF passenger's belt use from the EDR, too few for meaningful statistics. The analyses here are based on a subset of the 433 cases, namely the 411 driver cases where, additionally, FARS reports the driver's belt use as "yes" or "no" – i.e., excluding the 22 cases where FARS reports unknown belt use. (REST_USE = 1, 2, 3, 8, or 13 is "yes"; REST_USE = 0 or 7 [starting CY 2010] is "no"; all others REST_USE codes indicate unknown belt use and would be excluded from the 411 cases).

Table 2-15 focuses on these 411 cases, indicating whether the EDR-reported and FARS-reported belt use disagree. The first column of Table 2-15 lists the number of FARS cases available each year: the numbers are small, which will result in considerable year-to-year fluctuation. The second column shows that the absolute percentage of disagreements appears to have declined from percentages typically around 25 in the early years to about 15 more recently. As in Table 2-9, that may be a misleading statistic. The steady increase of on-the-road belt use shrinks the pool of cases where drivers who were actually unrestrained could be misreported as belted (and the disagreements are much less frequent in the other direction). Perhaps a more meaningful statistic would be the number of FARS-EDR disagreements **relative to** the proportion of drivers on the road who are unrestrained, as observed in NOPUS. This statistic is shown in the right column of Table 2-15 and it has averaged approximately 1.15 from 2002 through 2015, with little or no evidence of a decreasing trend over time: the average value is 1.12 for CY 2002 through 2008 and 1.22 for 2009 through 2015; the correlation of this statistic with CY (weighted by number of cases in each CY) is not statistically significant (r = -.08, p = .80).

Table 2-15: Percentage of Drivers Where EDR-Reported and FARS-Reported Belt Use Disagrees
(FARS vehicle cases cross-referenced with EDR downloads, CY 2002 to 2015)

CY	Number of Cases	% EDR and FARS Disagree	% Unrestrained (NOPUS)	Disagreements / Unrestrained
2002	28	28.6	24	1.19
2003	38	21.1	20	1.05
2004	47	27.7	19	1.46
2005	31	22.6	17	1.33
2006	46	21.7	18	1.21
2007	27	11.1	17	.65
2008	17	0.0	16	.00
2009	17	29.4	15	1.96
2010	13	15.4	14	1.10
2011	25	20.0	16	1.25
2012	34	14.7	13	1.13
2013	28	17.9	12	1.49
2014	28	10.7	13	.82
2015	32	12.5	11	1.14

Thus, the data in Table 2-15 (based on the 411 FARS cases cross-referenced with EDRs) suggests that, as of 2015, EDR data is not yet extensively employed to refine assessments of belt use in FARS.

There are two stages in the development of a FARS case at which EDR data might potentially be employed to assess belt use: (1) Police, while investigating the crash, might consult the EDR data in their assessment of belt use; or (2) FARS analysts might review EDR data that is in a case file, obtained from the police or possibly other sources, to reassess and perhaps modify the police-reported belt use.

Section 2.5 presented anecdotal evidence that police, as of 2015, occasionally used EDR data to assess belt use, but in no more than a small percentage of crash reports, if crashes of all severities are taken into account. Nevertheless, if most of these EDR downloads occurred in fatal crashes (which tend to be investigated the most thoroughly), they might conceivably amount to an appreciable percentage of the fatal crashes (which constitute only 0.5% of all police-reported crashes[46]).

To shed more light on the use of EDR data within FARS, NHTSA staff inspected the police reports and other documents supporting the 65 FARS cases of CY 2014 and 2015 that we have cross-referenced with EDR data downloaded within CDS (namely, the 60 cases included in the 2014 and 2015 rows of Table 2-15 plus 5 cases in which the EDR reported belt use or non-use but FARS said REST_USE was unknown). The purpose of the review was to ascertain if the FARS analysts (whose work is completely independent of the CDS teams) had themselves gleaned any information from the EDR, either from the police report or from subsequent or ancillary material. NHTSA found that the police reports and documentation do not mention EDRs in even a single one of these 65 cases (even though 7 of them have detailed narratives describing how belt use was ascertained from various sources unrelated to EDRs). This is an ambiguous finding because it does not preclude that EDR data may have been accessed by somebody but then not mentioned in the police report or appended to the documentation – but it is certainly not positive evidence that EDR information was widely used in coding FARS during 2014 or 2015.

NHTSA's FARS staff provided the following information regarding the use of EDR data by FARS analysts: inclusion of EDR data in the case materials does not appear to be prevalent. As of early 2018, NHTSA has not directed or advised FARS analysts to independently review EDR data, because of the complexity of interpreting this data.

We will take one more look at this issue in the next section, when we track the trend in belt effectiveness in the entire 2002-to-2015 FARS database, not just the 433 cases cross-referenced with CDS downloads of EDRs.

Tables 2-16 through 2-17 compare EDR-reported and FARS-reported belt use in CY 2002 through 2015 for the 411 cases where the EDR and FARS (REST_USE) indicated whether or not

[46] NHTSA (2016), p. 17.

the driver was belted. Table 2-16 shows that the EDR agreed with REST_USE in 333 cases and disagreed in 78 cases:

Table 2-16: EDR-Reported Versus FARS-Reported Driver Belt Use, CY 2002-2015

FARS-REPORTED	EDR-REPORTED: UNRESTRAINED	EDR-REPORTED: BELTED	Total
UNRESTRAINED	126	14	140
BELTED	64	207	271
Total	190	221	411

The disagreements are skewed in one direction, with 64 cases where police said "belted" and the EDR said "unrestrained," but only 14 cases vice-versa. As a consequence, FARS-coded belt use is higher, overall, than the EDR-reported use. Table 2-16a lists fatally injured drivers separately from the survivors. The largest group of disagreements is the 37 surviving drivers that FARS coded as belted, but the EDR reported to be unrestrained:

Table 2-16a: EDR-Reported Versus FARS-Reported Driver Belt Use, CY 2002-2015

Fatalities

FARS-REPORTED	EDR-REPORTED: UNRESTRAINED	EDR-REPORTED: BELTED	Total
UNRESTRAINED	114	13	127
BELTED	27	101	128
Total	141	114	255

Survivors

FARS-REPORTED	EDR-REPORTED: UNRESTRAINED	EDR-REPORTED: BELTED	Total
UNRESTRAINED	12	1	13
BELTED	**37**	106	143
Total	49	107	156

Table 2-17 presents the same data as side-by-side univariate tabulations, displaying the absolute and proportional net change in each category:

Table 2-17: EDR-Reported and FARS-Reported Driver Belt Use by Survival Status, 2002-2015

	FARS-Reported Belt Use	EDR Belt Use	Absolute Change	Proportional Change
Belted fatalities	128	114	- 14	- 11 %
Unrestrained fatalities	127	141	+ 14	+ 11 %
Belted survivors	143	107	- 36	- 25 %
Unrestrained survivors	13	49	+ 36	+ 277 %

By far the largest change in relative terms (and also largest in absolute terms) is the 277-percent increase, from 13 to 49, of unrestrained survivors. Moving a substantial portion of the survivors from the "belted" to the "unrestrained" cohort will increase the observed fatality risk for belted drivers in potential analyses and reduce it for unrestrained drivers, making the observed belt effectiveness lower with EDR-reported belt use than with FARS-reported belt use in CY 2002 through 2015.

The 411 cases also permit identification of driver subgroups especially prone to inaccurate reporting of belt use, as evidenced by the frequency of disagreements between FARS-reported and EDR-reported belt use. For example, Table 2-18 confirms the preceding discussion, indicating that survivor cases have a significantly higher proportion of disagreements (24.36%) than fatality cases (15.69%) – despite the fact that belt use is higher for survivors than fatalities, resulting in a relatively smaller pool of cases for potential disagreements (i.e., cases where the drivers are actually unrestrained). The chi-square statistic for the 2x2 table is 4.73, which exceeds the 3.84 required for statistical significance at the two-sided .05 level:

Table 2-18: FARS-EDR Agreement or Disagreement on Driver Belt Use
By Driver's Survival Status

	DISAGREE	AGREE	Total
SURVIVOR	38	118	156
Row %	24.36	75.64	
FATALITY	40	215	255
Row %	15.69	84.31	
Total	78	333	411

Chi-Square = 4.73 ($p < .05$)

Table 2-19 indicates that FARS-EDR disagreements are significantly ($\chi^2 = 8.67$) more common for drivers of MY 1994-to-2002 vehicles (23.93%) than MY 2003-to-2016 vehicles (12.43%).

Table 2-19: FARS-EDR Agreement/Disagreement on Driver Belt Use
By Vehicle's Model Year

	DISAGREE	AGREE	Total
1994-2002	56	178	234
Row %	23.93	76.07	
2003-2016	22	155	177
Row %	12.43	87.57	
Total	78	333	411

Chi-Square = 8.67 ($p < .05$)

However, this may be partly due to the earlier-model vehicles having a larger pool of potential disagreements because (1) They are preponderant in the earlier calendar years, where belt use is lower; and (2) By the last calendar years they have become fairly old vehicles, and belt use decreases with each year that a vehicle ages.[47]

The day of the week when the crash occurred has a borderline-significant association with the proportion of FARS-EDR disagreements: they were more common in Saturday and Sunday crashes (24.29%) than Monday through Friday (16.24%). The difference is not an artifact of one group having a larger pool of potential disagreements, because belt use is nearly the same on weekdays and weekends.[48] Table 2-20 indicates the conventional chi-square is 3.89, which just exceeds the 3.84 needed for statistical significance at the two-sided .05 level; however, three widely-used alternatives for the conventional chi-square (likelihood ratio χ^2, continuity adjusted χ^2, and Fisher's exact test) fall just short of statistical significance, while only one (Mantel-Haenzel χ^2) reaches it.

[47] Vehicle age has consistently negative coefficients in logistic regression equations to impute 3-point belt use as a function of various parameters: Kahane (2015, January), Pp. 375, 376, 379, and 380.
[48] Pickrell & Li (2016, February).

Table 2-20: FARS-EDR Agreement or Disagreement on Driver Belt Use
By Day of Week

	DISAGREE	AGREE	Total
MON-FRI	44	227	271
Row %	16.24	83.76	
SAT SUN	34	106	140
Row %	24.29	75.71	
Total	78	333	411

Chi-Square = 3.89 ($p < .05$); Likelihood ratio χ^2 = 3.78 ($p > .05$); Continuity adjusted χ^2 = 3.38 ($p > .05$); Mantel-Haenszel χ^2 = 3.88 ($p < .05$); Fisher's exact test $p > .05$

The FARS-EDR disagreement rate was somewhat lower for female drivers (13.89%) then males (21.72%); also for drivers at least 38 years old (16.91%) than for younger drivers (21.08%). Neither difference is statistically significant (conventional χ^2 = 3.73 and 1.16, respectively; however, the former comes close to 3.84 and even reaches 3.88 on the likelihood-ratio χ^2). These differences could be, to some extent, an artifact of the pool of potential disagreements being smaller for female and older drivers, because they historically have had high rates of belt use.[49]

Passenger cars and LTVs had approximately the same FARS-EDR disagreement rates. So did drinking and non-drinking drivers; so did vehicles involved in single-vehicle crashes, frontal impacts into another vehicle, and non-frontal impacts by another vehicle.

2.8 Fatality reduction by belts – EDR-reported versus FARS-reported belt use: Double-pair comparison analysis, as discussed in Section 1.4, is valuable for estimating belt effectiveness directly from FARS data, but a paucity of data precludes its use for estimating fatality reduction based on EDR-reported belt use. Table 2-21 presents the actual data, comparable to Tables 1-7 and 1-8, for the subset of the 411 vehicles occupied by a driver **and** a RF passenger, at least one of them died, and the EDR specified a belt use ("yes" or "no," not "unknown") for both of them:

[49] Driver age and female gender have consistently positive coefficients in logistic regression equations to impute 3-point belt use as a function of various parameters: Kahane, (2015, January), pp. 375, 376, 379, and 380.

Table 2-21: Fatalities by EDR-Reported Belt Use and Seating Position (FARS 2002 to 2015)

Fatalities	Driver Fatalities	RF Fatalities
Both unrestrained	6	5
Driver unrestrained, RF belted	2	0
Driver belted, RF unrestrained	1	2
Both belted	6	10

The smallest cell in Table 1-7 was 226 and in Table 1-8, 1,622; Table 2-21 has a zero cell and no fatality count exceeds 10. The paucity of data is due to: (1) Many of the early EDR systems did not record the RF belt status; and (2) The requirement, in double-pair comparison, that the RF seat be occupied, which excludes approximately two-thirds of the vehicle cases because people were driving alone. We will return to double-pair comparison in this section's last group of analyses, which cover the entire CY 2002-to-2015 FARS and are not limited to the 411 cases cross-referenced to EDR data; however, these analyses will be based on FARS-reported, not EDR-reported belt use.

Furthermore, FARS data cannot be used to compute effectiveness based on simple fatality rates per 100 crash-involved drivers, as in Tables 2-5 and 2-6 (CDS data) or Tables 2-12 and 2-13 (CDS cases with police-reported belt use). FARS provides the numerator for such rates (the number of fatalities), but not the denominator (the number of drivers involved in crashes of any type, fatal or non-fatal), because crashes are only entered in FARS if they resulted in a fatality.

For a preliminary analysis, however, one driver fatality "rate" that can be readily computed from FARS is the number of fatalities per 100 drivers involved in FARS-reported crashes (which are fatal to somebody, but not necessarily this driver – perhaps a passenger, the driver of another vehicle, and/or a pedestrian or bicyclist). It is also possible to separately compute these "rates" for belted and unrestrained occupants, as long as the resulting ratio is not construed as a measure of belt effectiveness. (For example, in the subgroup of single-vehicle, non-pedestrian crashes involving unaccompanied drivers, the fatality rate internal to FARS is 100% for unrestrained drivers and likewise 100% for belted drivers – there being no other parties to the crash, this driver has to be a fatality, or the crash would not be in FARS.) The point is that these rates and their ratios may be calculated based on FARS-reported belt use and then recalculated based on EDR-reported belt use, to obtain at least a qualitative or directional indication of how the source of belt use information might affect the observed fatality reduction.

Table 2-22, based on the 411 FARS driver cases cross-referenced with EDR downloads (via CDS) for CY 2002 through 2015, compares the fatality "rates" for belted and unrestrained drivers, based on the FARS-reported belt use:

Table 2-22: Fatality Rate Per 100 FARS Driver Records, Based on FARS-Reported Belt Use (CY 2002-2015 FARS vehicle cases cross-referenced to EDR downloads)

FARS-Reported Belt Use	Fatalities	FARS Driver Records	Fatality "Rate" (%)
Unrestrained	127	140	90.71
Belted	143	271	47.23

The fatality rate is 90.71 percent for the drivers listed in FARS and coded as unrestrained, 47.23 percent for belted. Table 2-23 is based on the same 411 FARS cases, but now belt use is classified based on what the EDR reported:

Table 2-23: Fatality Rate Per 100 FARS Driver Records, Based on EDR-Reported Belt Use (CY 2002-2015 FARS vehicle cases cross-referenced to EDR downloads)

EDR-Reported Belt Use	Fatalities	FARS Driver Records	Fatality "Rate" (%)
Unrestrained	141	190	74.21
Belted	114	221	51.58

The number of unrestrained crash-involved drivers is larger in Table 2-23 than in Table 2-22 (190 versus 140), because the EDR reported as unrestrained many of the drivers that FARS had coded as belted. This increase is partially offset by a proportionately smaller increase in the unrestrained fatalities (141 versus 127). As a consequence, the unrestrained fatality "rate" is lower in Table 2-23 (74.21%) than in Table 2-22 (90.71%). But the belted fatality rate is slightly higher in Table 2-23 (51.58%) than in Table 2-22 (47.23%).

The **exaggeration factor** for the rates in Table 2-22 versus Table 2-23 may be calculated the same way as the universal exaggeration factor (UEF) in Section 1.6:

$$(R_{EDR,belted}/R_{EDR,unr})/\ (R_{FARS,belted}/R_{FARS,unr}) = (51.58/74.21)\ /\ (47.23/90.71) = 1.335$$

It is quite close to the UEF, 1.369 that NHTSA computed in its 2000 evaluation of belt effectiveness and also used in subsequent reports[50] to adjust downwards estimates of fatality reduction based on FARS-reported belt use after 1986, when many States had enacted belt use laws.

A possible question is why the exaggeration factor continues to be high when the absolute percentage of disagreements between actual and police- or FARS-reported belt use has declined as belt use has increased into the 90-percent range (see Tables 2-9 and 2-15). However, the evidence presented in Table 1-11 and discussed in Section 1.6 suggests that the exaggeration

[50] Kahane (2000, December; 2013, May; 2013, November).

factor is not proportional to the absolute percentage of disagreements and, in fact, is relatively invariant even as belt use increases or decreases – e.g., in Table 1-11, the UEF stayed nearly the same from 1986 through 1999 even though belt use increased substantially during that period.

A more pertinent issue is that the UEF (1.369) and the exaggeration factor computed from Tables 2-22 and 2-23 (1.335) are not directly comparable. The UEF was computed for effectiveness estimates based on **double-pair comparison** analysis: it compares the estimates based on pre-1985 FARS and post-1985 FARS data for basically the same vehicle population, under the working assumption that the difference in the two estimates is due to over-reporting of belt use after 1985 (because the law deters people from reporting they were unrestrained). NHTSA's 2000 evaluation proposed that the UEF could be applied to future estimates based on double-pair comparison, but it might be quantitatively incorrect for other, arithmetically different types of effectiveness computations such as the ratios of the simple driver fatality "rates" in Tables 2-22 and 2-23. It may be somewhat more appropriate to compare our 1.335 exaggeration factor to a pre-1985/post-1985 comparison of similarly computed fatality "rates." Table 2-24 compares these fatality rates, based on FARS-reported belt use, for the entire CY 1980-to-1984 FARS to the entire CY 1986-to-1990 FARS (consistent with the data in Tables 2-22 and 2-23, vehicles are limited to passenger cars and LTVs, crashes involving pedestrians and/or bicyclists are excluded, and the driver's seat was equipped with 3-point belts):

Table 2-24: Fatality Rate Per 100 FARS Driver Records, Based on FARS-Reported Belt Use
(CY 1980-to-1984 FARS versus CY 1986-to-1990 FARS)

CY 1980 to 1984 (Before Belt Laws)

FARS-Reported Belt Use	Fatalities	FARS Driver Records	Fatality "Rate" (%)
Unrestrained	37,905	66,802	57.74
Belted	1,657	4,650	35.63

CY 1986 to 1990 (After Belt Laws)

FARS-Reported Belt Use	Fatalities	FARS Driver Records	Fatality "Rate" (%)
Unrestrained	47,670	72,658	65.61
Belted	14,395	41,328	34.83

The exaggeration factor in Table 2-24 is:

$$(R_{8084,belted}/R_{8084,unr})/ (R_{8690,belted}/R_{8690,unr}) = (35.63/57.74) / (34.83/65.61) = 1.183$$

It is lower than the 1.369 UEF computed for corresponding double-pair comparison analyses.

Confidence bounds may be estimated for the 1.335 exaggeration factor based on FARS-reported versus EDR-reported belt use (the data in Tables 2-22 and 2-23) by a jackknife technique.[51] This relatively complex technique is appropriate because the four fatality "rates" in Tables 2-22 and 2-23 are not statistically independent – e.g., the unrestrained EDR-reported and FARS-reported rates are based on the same driver cases. The logarithm of 1.335 is 0.289.[52] The FARS cases are subdivided into 10 systematic random subsamples, based on the last digit of the case number, ST_CASE. The logarithm of the exaggeration factor is recomputed 10 times, each based on the 9/10 of the FARS data that remain after one of the subsamples is removed. (The subsample is then replaced before the next subsample is removed.) For example, the exaggeration factor might become 0.289 + h when all FARS cases are used except those with ST_CASE ending in zero. A "pseudo-estimate" 0.289 – 9h is generated for the subsample including only the FARS cases with ST_CASE ending in zero (because if the factor could have been computed using only these cases, it would have to be 0.289 – 9h in order for it and the 0.289 + h generated from the other 9/10 of the data to average out to 0.289). The standard error of these 10 pseudo-estimates is 0.046. The sampling error can be treated as a t-distribution with 9 degrees of freedom (df). The 95-percent confidence bounds for the logarithm of the exaggeration factor are 0.289 ± 2.262 x 0.046; the confidence bounds for the factor itself range from 1.203 to 1.482.

Confidence bounds may be estimated for the 1.183 exaggeration factor based on CY 1980-to-1984 versus 1986-to-1990 FARS (the data in Table 2-24) by noting that the four fatality "rates" in Table 2-24 (57.74, 35.63, 65.61, and 34.83) are independent statistics. The relative variance of the exaggeration factor, which is a ratio of ratios, is the sum of the relative variances of the four rates – which are derived from binomial distributions and each have relative variance (1-p)/np. The relative variance for the exaggeration factor is 0.000452; the standard deviation of the factor is 1.183 x √0.000452 = 0.0252; the 95-percent confidence bounds are 1.183 ± 1.96 x 0.0252; they range from 1.134 to 1.232.

Thus, the confidence bounds, 1.203-to-1.482 and 1.134-to-1.232 overlap to some extent, but in purely numeric terms, the 1.335 factor may be considered significantly larger than the 1.183.[53] Possible explanations for the quantitative difference might include:

- Perhaps the numbers are simply not directly comparable, because the exaggeration factor for these simple driver "rates" might not be as robust as the UEF based on double-pair comparison, but prone to change as belt use rates or the fatality rates themselves change;
- Over-reporting of belt use might have increased in relative terms (e.g., as a percentage of the actually unrestrained) since the 1980s; or
- The original computations based on pre- versus post-1985 FARS data might have understated the effect because there may have been some over-reporting even before 1985.

---

[51] Efron, B. (1982). *The jackknife, the bootstrap, and other resampling plans.* Philadelphia: Society for Industrial and Applied Mathematics.

[52] The logarithm of the exaggeration factor is more suitable than the factor itself for sampling error calculations, because its sampling distribution is more symmetric and closer to a normal distribution.

[53] 1.135 – 1.183 = 0.152; the standard deviation of the difference is approximately $\sqrt{(.0617^2 + .0252^2)}$ = .0666, where .0617 is the standard deviation of the 1.335 factor; .152/.0666 = 2.28, which exceeds the 97.5th percentile of a t-distribution with 9 df (2.262) and also the 97.5th percentile of the normal distribution (1.96).

But in qualitative, directional terms, both exaggeration factors are telling us the same thing: belt use has been over-reported in crash databases, especially for crash survivors with minor or no injuries, leading to exaggerated estimates of belt effectiveness if the data on belt use are taken literally. Both indicate that a correction to the effectiveness estimates based on some kind of data with more accurate belt use will yield a more realistic effectiveness estimate. Furthermore, the exaggeration factors based on the EDR data, if not directly comparable to the UEF, suggest there should be a correction of about the same order of magnitude as the UEF.

Three alternatives may be considered at this point:

- Stop correcting estimates of belt effectiveness with the UEF, or any other factor;
- Revise the UEF based on the EDR findings to date; or
- Continue, for now, to use the existing UEF correction for estimates of belt effectiveness based on double-pair comparison with FARS data.

We may reject the first alternative because the EDR analyses of this report strongly corroborate the rationale for the UEF, namely that belt use is over-reported (in the absence of EDR data) in crash databases, resulting in exaggerated estimates of effectiveness that need to be corrected. Furthermore, the exaggerations seen in the recently corrected EDR data are similar in direction and order of magnitude as the UEF based on FARS data from 1977 through 1999. The UEF is the right concept, even if perhaps the number is no longer exactly right today.

On the other hand, the EDR data that can be cross-referenced with FARS to date (411 driver cases through 2015, and only 40 RF passenger cases) is simply too limited to update the correction factor and replace it with a new one that is applicable to effectiveness analyses based on double-pair comparison. The issue may be revisited – and this "preliminary" report updated with a "final" report – when and if EDR data becomes available on a large enough number of FARS driver and RF passenger cases to allow statistically meaningful double-pair comparison analyses. This would likely require an order of magnitude more FARS cases linked to EDR data. The cases could derive from cross-referencing additional CDS (or its successor CISS) cases with FARS, or from other sources, or both. It is unlikely that a sufficient number of cases can be accumulated in the next two or three years.

Thus, the best alternative, more by default than as a positive conclusion, is to continue using the UEF = 1.369 for correcting effectiveness analyses based on FARS, at least until EDR data becomes more widely available. We hope, however, that the analyses of this report have corroborated that belt use continues to be over-reported in crash databases not based on EDR data and that some correction to effectiveness estimates such as the UEF or something similar to it continues to be needed.

It likewise seems appropriate to continue with the agency's existing estimates that belts reduce overall fatality risk by 45 percent for drivers and right front passengers of cars and by 60 percent in LTVs (until fatality reduction can be definitively estimated with EDR-based belt use data – and this is unlikely to happen for some years to come). We believe, moreover, that this report reinforces the continuing plausibility of the current 45- and 60-percent effectiveness estimates.

The last analysis in this report estimates fatality reduction by seat belts on the entire FARS file, based on FARS-reported belt use, calendar year by calendar year, separately for vehicles equipped with EDRs and non-equipped vehicles. Its objective is to see if there are any time trends in the observed effectiveness estimates. A trend of gradually decreasing effectiveness might suggest substantial and growing use of EDR data by FARS analysts to help them ascertain belt use. The analysis is comparable to Table 2-7 for CDS-reported belt use. Table 2-7 showed a substantially lower estimate of injury reduction for belts in CY 2009 through 2015, but only for the vehicles where CDS investigators had downloaded the EDR – demonstrating that CDS had begun relying on EDR data, when available, for more accurate assessment of belt use, resulting in more realistic effectiveness estimates. The analysis also resembles Table 2-14 for police-reported belt use. Table 2-14, unlike Table 2-7, did not show diminished injury reduction for these vehicles – suggesting that police had not yet as of 2015 begun relying on EDR data in any substantial portion of their investigations.

This analysis will differ from Tables 2-7 and 2-14, however, in that:

- Thanks to the much larger number of cases in FARS, separate estimates may be obtained for each individual CY from 2002 through 2016, rather than merely comparing 2002-to-2008 with 2009-to-2016;
- Separate estimates for cars and LTVs will be statistically meaningful thanks to the ample data;
- There will be estimates for RF passengers as well as for drivers;
- Effectiveness is estimated by double-pair comparison rather than ratios of simple casualty rates per 100 crash-involved drivers; and
- Vehicles are divided into two groups based on whether or not they are equipped with an EDR that records belt use (based on the make-model lists in Appendix A), not on whether the EDR was downloaded (which is something known only for CDS cases, not for FARS generally).

The analysis is limited to vehicle records with decodable VINs and in which both the driver's and RF seating positions are equipped with 3-point belts and frontal air bags, but neither is equipped with a factory-installed on-off switch for the air bags.

Table 2-25 presents the estimates of fatality reduction from the double-pair comparison analyses. Appendix C contains the fatality counts that produced these estimates (analogous to Table 1.7), based on the methods explained in Section 1.4.

For example, in the CY 2002 FARS, for the passenger cars equipped with EDRs that were involved in fatal crashes in that year, the observed effectiveness is a 61.17-percent fatality reduction for belted relative to unrestrained drivers; in the cars not equipped with EDRs, the fatality reduction is 55.51 percent. The corresponding effectiveness estimates for RF passengers of cars are 61.00 and 57.63 percent. Belt effectiveness is higher in LTVs: 73.72 and 85.31 for drivers, 75.97 and 75.36 percent for RF passengers.

Table 2-25: Belt Effectiveness by CY, FARS 2002-2015, Based on Double-Pair Comparison
Vehicles Equipped with EDRs Versus Non-Equipped Vehicles

F A T A L I T Y   R E D U C T I O N (%)

	PASSENGER CARS				LTVs			
	DRIVERS		RF PASSENGERS		DRIVERS		RF PASSENGERS	
CY	EDR-Eqpd	Not Eqpd	EDR-Eqpd	Not Eqpd	EDR-Eqpd	Not Eqpd	EDR-Eqpd	Not Eqpd
2002	61.17	55.51	61.00	57.63	73.72	85.31	75.97	75.36
2003	60.53	58.51	57.69	55.93	78.20	70.21	74.27	71.48
2004	59.15	68.43	55.53	59.85	82.82	81.77	76.49	71.86
2005	52.33	63.24	55.34	64.06	86.72	82.59	72.70	71.17
2006	63.96	58.32	53.87	58.72	77.13	74.78	82.84	71.92
2007	61.63	60.06	66.54	56.73	83.83	80.02	77.63	74.01
2008	65.25	62.73	52.49	61.85	81.54	89.45	67.71	80.13
2009	64.77	63.04	55.91	59.69	79.42	78.42	74.64	74.16
2010	57.46	59.52	55.73	53.18	81.27	79.61	83.29	71.14
2011	62.00	60.55	48.84	59.49	82.53	82.99	76.06	78.71
2012	66.84	71.25	60.26	65.38	73.73	82.38	64.96	74.15
2013	62.18	57.59	55.55	53.90	73.04	71.37	74.56	75.06
2014	51.30	67.62	63.28	60.55	80.81	78.33	79.00	77.60
2015	64.52	52.29	49.25	62.31	75.28	74.42	74.23	75.44
2016	67.34	59.81	63.36	56.03	74.70	69.49	75.52	77.18

The telltale pattern that would suggest FARS has begun to rely extensively on EDR data for assessing belt use would be comparable to the pattern in Table 2-7. We would see a steady diminution of observed belt effectiveness in the later CY in the EDR-equipped vehicles (as coding of belt use becomes more accurate thanks to the EDRs), whereas belt effectiveness stays about the same in the non-equipped vehicles throughout 2002 to 2015.

Certainly, an “eyeball” inspection of Table 2-25 does not reveal an obvious trend of that type. The observed belt effectiveness has remained fairly close to 60 percent in the passenger cars year after year (with some variation as might be expected with calculations based on limited data). Effectiveness is higher in LTVs, but here, too, it has stayed reasonably close to 75 percent year after year. We see neither an abrupt nor a steady diminution of effectiveness in the EDR-equipped vehicles.

Statistical techniques might help identify trends that are not readily visible. Table 2-25 has 120 observations of effectiveness. They constitute 60 matched pairs (15 CY x 2 vehicle types x 2 seating positions): the vehicles equipped with EDRs and the corresponding vehicles not equipped with EDRs. For example, the first two entries in the CY 2002 row: drivers of cars equipped with EDRs (belt effectiveness 61.17%) and drivers of cars not equipped with EDRs (55.51%). The effectiveness estimates E (expressed as numbers between 0 and 1, not as percentages) are transformed to the log-odds ratios, LR = log(1 – E). The LR variables are more suitable for statistical analyses because they have sampling distributions that are symmetric and similar to a normal distribution, and so do linear combinations of them (e.g., sums, differences, or averages). The values of the LR variables are shown in Appendix C. For each of the 56 matched pairs, DLR is defined to be the LR for the non-equipped vehicles minus LR for the corresponding EDR-equipped vehicles. Note that the higher the effectiveness, the more negative the LR. Thus, if the observed fatality reduction in non-equipped vehicles were to remain high (strongly negative LR) while observed effectiveness in the EDR-equipped vehicles diminished (less and less negative LR), DLR would become more and more negative.

Table 2-26: Average Value of DLR by Calendar Year

2002	- 0.0844
2003	0.1265
2004	- 0.0303
2005	- 0.0379
2006	0.1563
2007	0.1647
2008	- 0.2985
2009	0.0062
2010	0.1595
2011	- 0.0849
2012	- 0.2460
2013	0.0480
2014	- 0.0377
2015	- 0.0039
2016	0.1268

Table 2-26 tracks the arithmetic average of the four DLR readings for each CY (car drivers, car RF, LTV drivers, and LTV RF). Table 2-26 does not show an obvious trend. The distribution of positives and negatives is fairly uniform throughout 2002 to 2016.

Next, a correlation analysis between CY and DLR for the 60 matched pairs produces a negative correlation coefficient (i.e., DLR becoming more negative over time), but it is quite weak ($r = -.017$) and it is not statistically significant ($p > .05$, in fact $p = .90$).

The timespan from 2002 to 2015 can be split into two epochs and DLR can be compared in the earlier and later epoch. If the timespan is split after 2008, as in Table 2-7, the 28 values of DLR for CY 2002 through 2008 averaged -0.001, whereas the 32 values for 2009 through 2016 averaged -0.004. Thus, the average point estimate has become slightly more negative, but analysis of variance (PROC ANOVA) shows that the difference is not statistically significant ($F = 0.00$, $df = 58$, $p > .05$). Similarly, if the timespan is split one year later, the 32 values of DLR for CY 2002 through 2009 averaged close to zero, whereas the 28 values for 2010 through 2016 averaged -0.005, but this, too, is not a statistically significant difference ($F = 0.01$, $df = 58$, $p > .05$). With the division yet another year later, the 36 values of DLR for CY 2002 through 2010 averaged +0.018, whereas the 24 values for 2011 through 2016 averaged -0.033, but this is still not a statistically significant difference ($F = 0.73$, $df = 58$, $p > .05$). Finally, with the division after 2011, the 40 values of DLR for CY 2002 through 2011 averaged +0.008 and the 20 values for 2012 through 2016 averaged -0.23, again not a significant difference ($F = 0.24$, $df = 58$, $p > .05$).

All of these analyses fall short of a “controlled experiment” because the vehicle distribution changes from year to year. As additional makes and models became equipped with EDRs (see Appendix A), an ever greater share of the new-vehicle population moved from the non-equipped to the EDR-equipped cohort. In recent calendar years, the non-equipped cohort has increasingly become a population of older vehicles. Thus, even if one of the analyses had shown statistically significant trends in DLR (and none of them did), the trends might have been due to other factors such as the changing vehicle populations, rather than the effect of EDR on the reporting of belt use in FARS.

In other words, the results in Table 2-25 do not suggest that EDR data, as of 2016, was employed by police or FARS analysts to ascertain belt use in a large proportion of FARS cases, but they do not preclude the possibility that EDR data may have occasionally facilitated some investigations.

Appendix A: Initial Model Year for EDR, MY 1994-2016

There were no EDRs readable by commercially available tools in production vehicles in the United States before MY 1994. The following table lists the first year that a make-model was equipped with some kind of EDR (and, unless shown otherwise, continued to be equipped with them in all subsequent years).

More relevant to this report, the right column of the table also lists the first year that a make-model was equipped with an EDR that, at a minimum, recorded the driver's belt use and could be read by a commercially available tool. Many of these EDRs, additionally, were certified to meet NHTSA's Part 563 regulation (whose requirements include that the EDR record driver's and RF passengers' belt use and that it be readable by a commercially available tool). As of September 1, 2012 (generally equivalent to the beginning of MY 2013), every EDR must meet Part 563.

Since 2006, NHTSA has requested the manufacturers to provide annual information on the availability and characteristics of EDRs in their new cars and LTVs. For MY before 2006, information is available from CDS data and the Bosch CDR vehicle list (available at crashdatagroup.com/software/versions/CDR_v17.2_Vehicle_Coverage_List_R1_0_0.pdf). The table lists nameplates in alphabetical order and, within nameplates, the EDR installation dates for the various models, in chronological order. Make-models not listed here were discontinued before 2016 and never had EDRs. As of MY 2016 NHTSA's information suggests all make-models from the nameplates listed below had EDRs except Porsche (no EDR) and, perhaps, Tesla and Sprinter (not specified).

Make-Model	First MY with EDR	First 563-compliant or tool-readable/ belt-use-recording EDR
Acura, all models	2006 or earlier	
Acura RL, TSX		2012
Acura MDX, ZDX		2012 or 2013
Acura all others		2013
Audi, all models	2015	2015
BMW, 300 sedan, 500, 600, 700, X3	2013	2013
BMW all others except X1, X6, Z4, 1-series CV	2014	2014
BMW X1, X6, Z4, 1-series CV	2015	2015
Buick Roadmaster	1994	1994
Buick Regal, LeSabre, Park Avenue	1995	1995
Buick Riviera, Skylark, etc.	1996	1996
Cadillac DeVille, Seville, Fleetwood	1994	1994
Cadillac all others except Catera	1995	1995
Chevrolet Caprice	1994	1994

Make-Model	First MY with EDR	First 563-compliant or tool-readable/ belt-use-recording EDR
Chevrolet Lumina, Monte Carlo, Geo Metro	1995	1995
Chevrolet Cavalier, Camaro, Express, Geo Tracker, Astro	1996	1996
Chevrolet Tahoe, Venture, Suburban, Malibu, Corvette, Silverado, C/K	1997	1997
Chevrolet all others	1998	1998
Chrysler 300, PT Cruiser	2006	2010
Chrysler Pacifica, Sebring	2007	2010
Chrysler Town & Country	2008	2010
Chrysler all others	2009	2010
Dodge Ram 1500 pickup, Magnum, Charger	2006	2010
Dodge Caliber, Caravan, Grand Caravan, Nitro	2007	2010
Dodge Avenger, Ram 2500 pickup, Viper	2008	2010
Dodge others (exc. Sprinter, listed separately)	2009	2010
Fiat, all models	2012	2012
Ford Taurus	2000	2000[54]
Ford, all others	2001	2001
GMC Safari, Savana	1996	1996
GMC Yukon, Suburban, Sierra	1997	1997
GMC all others	1998	1998
Honda, all models	2006 or earlier	
Honda CR-V, Fit, Ridgeline		2012
Honda CR-Z, Insight, Odyssey, Pilot		2012 or 2013
Honda, all others		2013
Hummer, all models	2003	2003
Hyundai, all models	2007	2011 (record belt use) 2013 (fully 563)
Infiniti QX	2006 or earlier	2013
Infiniti all others	2007	2013
Isuzu Hombre, Ascender	1998	1998
Jaguar	2006/earlier-2012	
Jaguar, all except XK		2012
Jaguar XK		no EDR 2013-2015
Jeep Commander, Grand Cherokee	2006	2010

[54] www.sae.org/events/gim/presentations/2007wheelock.pdf

Make-Model	First MY with EDR	First 563-compliant or tool-readable/ belt-use-recording EDR
Jeep Wrangler, Patriot, Compass	2007	2010
Jeep all others	2008	2010
Kia, all models	2007	2011 (record belt use) 2013 (fully 563)
Land Rover, all models	2006 or earlier	
Land Rover Evoque		2012
Land Rover Range Rover (excl Sport)		2013
Land Rover Range Rover Sport		2014
Land Rover, all except LR2		2015
Land Rover, all models		2016
Lexus LS, GS	2000	2000
Lexus ES, SC	2001	2001
Lexus LX, GX	2002	2002
Lexus HS, RX	2003	2003
Lexus all others	2005	2005
Lincoln, all models	2001	2001
Mazda, all models	2006 or earlier	
Mazda 2, 3, 6, CX-9		2011
Mazda 5, CX-5		2012
Mazda, all except MX-5		2013
Mazda MX-5		2016
Mercedes E, GLK, S, SL, SLK	2014	2014
Mercedes all others	2015	2015
Mercury Sable	2000	2000
Mercury Sable, Grand Marquis	2001	2001
Mercury all others	2003	2003
Mini-Cooper, all models	2014	2014
Mitsubishi, all except Raider	2006 or earlier	2013
Mitsubishi Raider	2009	never
Nissan Armada, Frontier, Titan, Xterra	2006 or earlier	2013
Nissan, all others	2007	2013
Oldsmobile 88, 98, Supreme	1995	1995
Oldsmobile Achieva, Aurora	1996	1996
Oldsmobile Silhouette	1997	1997
Oldsmobile all others	1998	1998
Pontiac Grand Prix	1994	1994

Make-Model	First MY with EDR	First 563-compliant or tool-readable/ belt-use-recording EDR
Pontiac Bonneville	1995	1995
Pontiac Sunfire, Grand Am, Firebird	1996	1996
Pontiac all others	1997	1997
Porsche	none as of 2016	none as of 2016
Saab 9-7x	2005	2005
Saab, all others	2006	
Saab 9-4x		2011
Saab, all others		never(?)
Saturn, all models	1995	1995
Scion, all models	2003	2003
Smart, all models	2015	2015
Sprinter	none through 2014, then unknown	
Subaru Impreza excluding WRX	2012	2012 or 2013
Subaru BRZ, Legacy, Outback, Crosstrek	2013	2013
Subaru Forester	2014	2014
Subaru all	2015	2015
Suzuki Swift	1995	1995
Suzuki Sidekick	1996	1996
Suzuki all others	1999	1999 (record belt use) 2013 (fully 563)
Tesla	unknown	unknown
Toyota Camry	2001	2001
Toyota Avalon, Corolla, Echo, Land Cruiser, Matrix	2002	2002
Toyota Highlander, Prius, Camry Solara, RAV4, 4Runner, Sienna	2003	2003
Toyota all others	2005	2005
VW Routan	2009	2010
VW, all others	mid-2015	mid-2015
Volvo, all models	2006 or earlier	
Volvo S60		2012
Volvo, all others		2013

Appendix B: Creation of CDS Database With Belt Use From EDR

CDS data from CY 2002 through 2015 has records of 11,991 crash-involved cars and LTVs for which the EDR was downloaded. There are a total of 15,454 events recorded in the EDRs.

Step 1: Delete the 354 vehicle records with a single "placeholder" event that has basically no data – i.e., EACCSEQ (event sequence number) = -9990, EVCYCLES (ignition cycle) = -9990, and DRVBELT = 7 (no event recorded). There remain 11,637 vehicles with 15,110 events.

Step 2: Subdivide into single-event and multiple-event vehicles. There are 8,713 vehicles with a single event. There are 2,924 multiple-event vehicles, with a total of 6,397 events recorded on them.

Step 3: Processing of the 8,713 single-event vehicles

> Step 3.1: Subdivide according to whether this event is definitely part of the CDS crash sequence (EACCSEQ = 1,2,3…; 7,402 vehicles), definitely not part of the CDS crash (EACCSEQ = 97; 231 vehicles), or unknown (EACCSEQ = 98,99; 1,080 vehicles).
>
> Step 3.2: For each of the 7,402 vehicles where the recorded event is part of the CDS crash sequence, accept the driver and RF belt use (DRVBELT and PASBELT) for that event as the EDR-based belt use in the crash.
>
> Step 3.3: For the 231 vehicles where the recorded event was not part of the CDS crash – if the ignition cycle is known for this event, the ignition cycle is also known for the investigation, and the event is at most 4 cycles earlier but no later than the investigation ($1 \leq$ EVCYCLES ≤ 900000 and $1 \leq$ INVCYCLE ≤ 900000 and $0 \leq$ INVCYCLE-EVCYCLES ≤ 4), it seems plausible that recorded event happened on the same ignition cycle as the crash and would have had the same belt use. There are only 13 vehicles meeting that criterion; we accept the EDR belt use for those 13 vehicles but delete the remaining 218 vehicle records.
>
> Step 3.4: For the 1,080 vehicles where it is unknown if the recorded event is part of the CDS crash sequence, it is plausible that in most cases this event actually is part of the CDS crash and the investigator merely could not pinpoint its exact location in the sequence of events: in general, if there is one event on an EDR, it is likely to have been the most recent and most severe event, namely the CDS crash. Unless we know that the recorded event happened more than 20 cycles before the investigation (likely quite a while before the crash) or at any time after the investigation (indicates a data problem, $1 \leq$ EVCYCLES ≤ 900000 and $1 \leq$ INVCYCLE ≤ 900000 and [INVCYCLE-EVCYCLES < 0 or INVCYCLE-EVCYCLES > 20]), we accept the EDR belt use for this event. We accept 997 of the 1,080 vehicles and delete the remaining 83.
>
> Step 3.5: Gather the 8,412 (7,402 + 13 + 997) vehicle records for which we accept the EDR-read belt use to be the belt use in the CDS crash.

Step 4: Initial processing of the 2,934 multiple-event vehicles and their 6,397 events. The objectives are to classify the events and to order them chronologically based on the ignition cycle (if known).

Step 4.1: Strip away the 562 events that are not part of the CDS crash and where we do not know on which ignition cycle they occurred. In the absence of positive information about when they occurred, we must assume they happened on an earlier cycle than the CDS crash, and the belt use at that time is irrelevant to the CDS crash. That leaves 5,835 still potentially relevant events, distributed among 2,916 remaining vehicles (18 vehicles had only the stripped-away events).

Step 4.2: Classify the remaining events as (1) non-CDS-crash (if EACCSEQ = 97; EVCYCLES is known for all of these, because we just stripped away the cases where it is unknown); (2) crash-known-cycle (if EACCSEQ = 1,2,3… and EVCYCLES is known); (3) crash-unknown-cycle (if EACCSEQ = 1,2,3… and EVCYCLES is unknown); (4) other-known-cycle (if EACCSEQ = 98,99 and EVCYCLES is known); and (5) other-unknown-cycle. Furthermore, for categories 2 through 5, classify if it is a deployment or non-deployment event. The tally of events is:

	Deployment	Non-Deployment	Total
1. Non-CDS-crash			719
2. Crash-known-cycle	2,090	1,202	3,292
3. Crash-unknown-cycle	393	538	931
4. Other-known-cycle	138	487	625
5. Other-unknown-cycle	21	247	268
			5,835

Step 4.3: For each of the 2,916 vehicles, count how many events there are of each of the 5 preceding types. Count how many of the events of types 2 to 5 are deployment events. For event types 1, 2, and 4, find the highest ignition cycle among the events of that type.

Step 5: Deletion of events that certainly or most likely occurred on an ignition cycle prior to the CDS crash. Process each of the 2,916 vehicles, depending on what event types exist for that vehicle.

Step 5.1: Ideal situation – this vehicle has at least 1 crash-known-cycle event; find the max ignition cycle for these events; keep every event on this cycle and also keep any crash-unknown-cycle events. Each of the events we are keeping is part of the CDS crash or happened on the same ignition cycle as the CDS crash: the belt use is relevant to the CDS crash. Discard the other events.

Step 5.2: This vehicle has at least 1 crash-unknown-cycle event but no crash-known-cycle events; keep all the crash-unknown-cycle events; also, if this vehicle a single other-known-cycle or other-unknown-cycle event with deployment, keep it too (because the deployment event would usually be the last recorded event, thus likely part of the CDS crash, even if not given a sequence number 1,2,3…).

Step 5.3: This vehicle has no crash-known-cycle or crash-unknown-cycle events, but it does have at least 1 other-known-cycle event; find the max ignition cycle for these other-known-cycle events; keep every event on this cycle – unless this cycle is more than 30 before the investigation cycle (probably not part of the CDS crash) or after the investigation cycle (indication of likely error in the cycle numbering).

Step 5.4: This vehicle has no crash-known-cycle, crash-unknown-cycle, or other-known-cycle events, but it does have at least 1 other-unknown-cycle event; if one of those other-unknown-cycle events is a deployment, we will assume it is part of the CDS crash and keep it; all other events are discarded because we have no information as to whether they occurred on the same cycle as the CDS crash.

Step 5.5: This vehicle only has non-CDS-crash events, but the ignition cycle is known for each of them; keep only the event(s) on the max ignition cycle, and then only if this cycle is at most 4 cycles earlier and no later than the investigation cycle.

Step 5.6: Gather the remaining vehicles (2,861) and events (4,752). We have rejected 1,083 events because they were certainly or most probably recorded on an ignition cycle prior to the CDS crash (most of these are non-CDS-crash events or non-deployment other-unknown-cycle events). We have eliminated 55 vehicles because we doubt that any of their reported events occurred on the CDS crash. The tally of the remaining events is:

	Deployment	Non-Deployment	Total
1. Non-CDS-crash			62
2. Crash-known-cycle	2,082	1,153	3,235
3. Crash-unknown-cycle	393	538	931
4. Other-known-cycle	132	378	510
5. Other-unknown-cycle	14	0	14
			4,752

Step 6: Determine the driver's and RF passenger's belt use in the vehicle during the CDS crash. Gather the 8,412 + 2,861 = 11,273 vehicles and 8,412 + 4,752 = 13,164 events remaining after Steps 3 and 5. Disregard the events for which the EDR says belt use is unknown or not reported. For any given vehicle, if the EDR says the belt was in not use during each of the remaining events, code the EDR-based belt use as "no." If the EDR says the belt was in use during each of the remaining events, code the EDR-based belt use as "yes." Finally, if the EDR says "belted" for some events and "unrestrained" for the other events that are part of the CDS crash sequence, we will assume that the occupant was, in fact, belted and that the "unrestrained" readings are most likely an artifact of the recording system (e.g., defaulting to "unrestrained" if the system loses power) or a malfunction of the belt itself, and we will code the EDR-based belt use as "yes." Here are the tallies of vehicle-level codes, and their underlying event-level belt use coding. Disagreements are infrequent – e.g., there are 1,632 (536 + 1,096) vehicles with multiple events and the EDR belt use for the driver agrees on all events, versus 44 vehicles with disagreements. Also, when the RF seat is unoccupied, an EDR system that records data on the RF belt will code it as "not belted"; RF belt use is low in the following table because the data has not yet been filtered to exclude unoccupied seats.

Event-Level Belt Codes	Drivers	RF Passengers	Vehicle-Level Code
Single event: belt not used	2,523	2,568	Not belted
Single event: belt used	5,740	771	Belted
Multiple events: all "not used"	536	594	Not belted
Multiple events: all "used"	1,096	175	Belted
Some "used," some "not used"	44	12	Belted
	9,939	4,120	
All events: unknown belt use	1,334	7,153	Unknown
	11,273	11,273	

Step 7: Merge the vehicle-level EDR-based belt use codes with a vehicle-level file, derived from basic CDS, which includes information on the crash, the vehicle, and the demographics and injury severity of the driver and the RF passenger (if there is one). Although all the vehicles with EDR downloads were involved in CDS crashes, they might not have been "case vehicles" because they were not towed and/or they were 10 or more years old (if CY is 2009 or later). Of the 9,939 vehicles where the preceding analysis generated a known EDR-based belt use for the driver (see above table), 7,810 are CDS "case vehicles." Of the 4,120 vehicles where the EDR specified the status of the RF seat belt, 985 are "case vehicles" in which the RF seat was occupied by a passenger. Basic CDS includes the assessment of belt use in the police report (PARUSE) as well as the CDS investigator's assessment (MANUSE). Thus, our vehicle level file includes belt use for the driver according to the EDR, the police, and CDS (DRVBELT, MANUSE1, and PARUSE1, respectively) and for the RF passenger (PASBELT, MANUSE3, and PARUSE3).[55] The merged database includes all CDS vehicle records from CY 2002 through 2015 and MY 1985 to 2016, even if no EDR data was acquired, a total of 104,944 vehicle records, of which 67,183 are CDS "case vehicles" with a decodable VIN and a driver whose age is known.

[55] Because the driver's seat is numbered 1 or 11 in most crash databases and the RF seat 3 or 13, numerals 1 and 3 will be used on our vehicle-level file for data elements pertaining to the driver and the RF passenger, respectively.

Appendix C: Supporting Data for Table 2-25

Belt Effectiveness by CY, FARS 2002-2016, Based on Double-Pair Comparison
Vehicles Equipped With EDRs Versus Non-Equipped Vehicles

	FATALITY REDUCTION (%)		LOG(FATALITY REDUCTION)		DRIVER FATALITY COUNTS				RF PASSENGER FATALITY COUNTS			
					DRV: UNR	UNR	BELT	BELT	UNR	UNR	BELT	BELT
CY	DRIVER	RF	DRIVER	RF	**RF:** UNR	BELT	UNR	BELT	UNR	BELT	UNR	BELT
Passenger Cars Not Equipped With EDRs												
2002	55.51	57.63	-0.8100	-0.8587	356	137	52	517	343	70	141	548
2003	58.51	55.93	-0.8796	-0.8195	300	148	63	538	300	64	149	564
2004	68.43	59.85	-1.1530	-0.9124	350	162	59	517	344	56	161	589
2005	63.24	64.06	-1.0007	-1.0233	355	164	58	532	386	65	174	571
2006	58.32	58.72	-0.8751	-0.8848	358	161	60	558	377	72	156	593
2007	60.06	56.73	-0.9179	-0.8377	295	144	62	538	310	61	152	583
2008	62.73	61.85	-0.9869	-0.9636	276	122	51	475	263	54	158	533
2009	63.04	59.69	-0.9953	-0.9085	218	119	55	418	233	47	146	458
2010	59.52	53.18	-0.9043	-0.7589	210	119	63	427	210	50	140	458
2011	60.55	59.49	-0.9300	-0.9036	205	123	49	388	190	51	127	396
2012	71.25	65.38	-1.2464	-1.0607	189	122	46	374	195	39	148	427
2013	57.59	53.90	-0.8577	-0.7743	182	104	44	372	178	49	106	408
2014	67.62	60.55	-1.1275	-0.9302	173	87	37	383	179	33	110	456
2015	52.29	62.31	-0.7401	-0.9758	169	98	37	368	161	53	111	372
2016	59.81	56.03	-0.9116	-0.8216	165	110	45	358	162	51	118	405
Passenger Cars Equipped With EDRs												
2002	61.17	61.00	-0.9460	-0.9417	194	62	30	288	191	27	86	310
2003	60.53	57.69	-0.9295	-0.8600	185	71	39	351	184	32	105	393
2004	59.15	55.53	-0.8953	-0.8104	230	103	47	420	219	45	113	445
2005	52.33	55.34	-0.7408	-0.8061	217	101	46	416	211	51	109	420
2006	63.96	53.87	-1.0205	-0.7738	242	115	48	434	233	43	108	473
2007	61.63	66.54	-0.9580	-1.0949	238	96	35	422	218	47	132	476
2008	65.25	52.49	-1.0569	-0.7443	239	111	51	435	218	38	106	458
2009	64.77	55.91	-1.0432	-0.8188	195	94	46	380	192	39	123	453
2010	57.46	55.73	-0.8547	-0.8148	160	86	42	385	172	48	124	485
2011	62.00	48.84	-0.9677	-0.6703	197	88	48	381	183	33	93	403
2012	66.84	60.26	-1.1039	-0.9229	180	132	42	414	171	46	111	443
2013	62.18	55.55	-0.9724	-0.8109	165	102	42	463	159	40	98	489
2014	51.30	63.28	-0.7195	-1.0018	167	99	26	447	174	56	83	478
2015	64.52	49.25	-1.0363	-0.6782	193	128	61	526	184	49	130	598
2016	67.34	63.36	-1.1191	-1.0040	204	114	45	547	202	44	145	639

CY	FATALITY REDUCTION (%) DRIVER	RF	LOG(FATALITY REDUCTION) DRIVER	RF	DRIVER FATALITY COUNTS **DRV:** UNR **RF:** UNR	UNR BELT	BELT UNR	BELT BELT	RF PASSENGER FATALITY COUNTS UNR UNR	UNR BELT	BELT UNR	BELT BELT
LTVs Not Equipped With EDRs												
2002	85.31	75.36	-1.9179	-1.4008	112	80	22	164	87	12	92	176
2003	70.21	71.48	-1.2109	-1.2545	119	73	24	200	135	23	89	213
2004	81.77	71.86	-1.7024	-1.2681	126	83	27	214	123	13	83	207
2005	82.59	71.17	-1.7483	-1.2438	119	88	28	211	112	15	96	227
2006	74.78	71.92	-1.3774	-1.2700	149	98	25	227	127	25	90	226
2007	80.02	74.01	-1.6103	-1.3476	163	88	27	240	149	16	95	232
2008	89.45	80.13	-2.2488	-1.6161	159	79	17	193	114	9	93	216
2009	78.42	74.16	-1.5334	-1.3532	164	71	23	220	128	15	87	213
2010	79.61	71.14	-1.5901	-1.2427	113	76	27	208	126	14	85	212
2011	82.99	78.71	-1.7713	-1.5471	94	97	19	182	81	17	92	189
2012	82.38	74.15	-1.7363	-1.3529	98	68	20	163	102	12	78	178
2013	71.37	75.06	-1.2508	-1.3886	115	49	21	178	97	15	88	172
2014	78.33	77.60	-1.5294	-1.4960	94	65	14	168	67	15	66	167
2015	74.42	75.44	-1.3635	-1.4040	117	68	19	183	98	16	71	163
2016	69.49	77.18	-1.1872	-1.4774	105	82	22	188	95	24	92	165
LTVs Equipped With EDRs												
2002	73.72	75.97	-1.3363	-1.4257	71	28	14	91	47	7	53	75
2003	78.20	74.27	-1.5234	-1.3575	86	47	18	119	80	9	62	111
2004	82.82	76.49	-1.7612	-1.4479	100	69	20	170	84	9	65	140
2005	86.72	72.70	-2.0192	-1.2984	115	72	24	161	97	10	94	187
2006	77.13	82.84	-1.4755	-1.7626	136	83	22	228	112	16	107	180
2007	83.83	77.63	-1.8218	-1.4976	153	95	25	199	136	16	117	215
2008	81.54	67.71	-1.6898	-1.1306	171	94	26	193	137	18	84	217
2009	79.42	74.64	-1.5809	-1.3721	167	89	24	206	132	18	93	202
2010	81.27	83.29	-1.6751	-1.7894	150	84	14	210	126	16	85	202
2011	82.53	76.06	-1.7447	-1.4297	149	90	27	224	114	15	108	218
2012	73.73	64.96	-1.3366	-1.0487	131	81	28	239	139	20	75	248
2013	73.04	74.56	-1.3110	-1.3689	137	87	25	252	116	25	104	252
2014	80.81	79.00	-1.6506	-1.5606	143	93	26	287	129	17	118	275
2015	75.28	74.23	-1.3974	-1.3559	142	104	32	331	127	27	130	340
2016	74.70	75.52	-1.3744	-1.4075	176	128	33	351	152	34	141	352

DOT HS 812 529
April 2018

U.S. Department
of Transportation

**National Highway
Traffic Safety
Administration**

NHTSA

13578-042018-v3