PETITION FOR RECONSIDERATION

FILED WITH THE

FEDERAL MOTOR CARRIER SAFETY ADMINISTRATION

REGARDING THE ORDER ISSUED ON HOURS OF SERVICE OF DRIVERS; FINAL RULE

49 C.F.R. PARTS 385 AND 395
Docket No. FMCSA-2004-19608
73 Fed. Reg. 69567 (Nov. 19, 2008)

Public Citizen, Advocates for Highway and Auto Safety, the International Brotherhood of Teamsters, and the Truck Safety Coalition submit this petition for reconsideration of the final rule promulgated by the Federal Motor Carrier Safety Administration ("FMCSA") establishing the hours of service ("HOS") for drivers of commercial motor vehicles ("CMVs"), published at 73 Fed. Reg. 69567 (Nov. 19, 2008) ("final rule").

Petitioners have already provided an exhaustive analysis of the rule FMCSA has now adopted as final. Their critique of the Interim Final Rule ("IFR"), 72 Fed. Reg. 71247 (Dec. 17, 2007) was comprehensive, and they refer FMCSA to their March 2008 submission, Comments filed by Advocates for Highway and Auto Safety et al. (2008) ("Petitioners' Comments"), as well as to their earlier comments filed in connection with the first two rulemakings, for a more complete discussion of how FMCSA has failed to comply with its health and safety statutory mandates in issuing an HOS rule that both increases permissible consecutive driving hours per shift from 10 to 11 hours and adopts a 34-hour restart provision that permits truck drivers to drive and/or work significantly more hours on a weekly and long-term basis than under the pre-2003 HOS rule.

This petition for reconsideration will focus on FMCSA's reliance on inadequate research findings and crash data to justify its determination to re-adopt the 11-hour driving shift and 34-hour restart. This is now the third time that FMCSA has adopted these increases in permissible driving and working hours, notwithstanding two decisions by the U.S. Court of Appeals for the D.C. Circuit, both highly critical of the agency, that vacated both the 2003 and 2005 HOS rules. *See Owner-Operator Indep. Drivers' Ass'n v. FMCSA*, 494 F.3d 188 (D.C. Cir. 2007); *Public Citizen v. FMCSA*, 374 F.3d 1209 (D.C. Cir. 2004). FMCSA third effort is unlikely to fare any better than its first two. Petitioners urge the agency to abandon the 11-hour consecutive driving limit and the 34-hour restart now.

I. FMCSA's Decision to Adopt an HOS Rule Increasing Permissible Driving Hours Per Shift from 10 to 11 Consecutive Hours Is Unsupported by Either Scientific Research or Crash Data.

More than 35 years of research have documented not only that crash risk mounts with hours driven but that performance of long-haul truck drivers diminishes long before the pre-2003

10-hour driving limit is reached.[1] Despite its earlier protestations that the 11th driving hour was unlikely to be used often, FMCSA reports that the 11th driving hour is now used in 27 to 46 percent of runs, depending on the survey claims cited by the agency. 73 Fed. Reg. at 69570. FMCSA cites no research demonstrating either that 11 consecutive hours of driving is safe or that requiring drivers to take two additional off-duty hours off between shifts (compared to the pre-2003 rule) enables truck drivers to drive safely for longer periods at a stretch than they were previously capable of. Furthermore, although drivers can now work and drive for far longer hours over the same number of days as compared to the pre-2003 HOS rule, the agency has not demonstrated that this dramatic increase in risk exposure results in no more crashes, including fatal crashes, than the fewer hours driven under the previous HOS regulation—much less that the final rule improves truck safety and protects truck-driver health.

A. The Hanowski Study

FMCSA relies heavily on the study by Hanowski et al. at the Virginia Tech Transportation Institute ("VTTI")[2] for the proposition that 11 hours of driving is no less safe than 10 hours of driving, *see* 73 Fed. Reg. at 69576; 72 Fed. Reg. at 71260-61, preferring this limited investigation to field studies of actual crashes. The agency declares that Hanowski's study "allowed the Agency to answer the question whether there is an increase in risk associated with driving into the 11th hour," and that the answer is that "[t]here is no measurable increased risk for drivers driving in the 11th hour as compared to the 10th hour or any other driving-hour," 73 Fed. Reg. at 69576—a breathtaking claim that even the study's reviewers agreed the study could not and did not establish.[3]

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[1] *See, e.g.*, National Transportation Safety Board, *Factors That Affect Fatigue in Heavy Truck Accidents, Volume 1: Analysis* (1995); F.F. Saccomanno et al., *Effect of Driver Fatigue on Commercial Vehicle Accidents* (1996); W.J. Frith, *A Case Control Study of Heavy Vehicle Drivers' Working Time and Safety* (1994); Heikki Summala & Timo Mikkola, *Fatal Accidents Among Car and Truck Drivers: Effects of Fatigue, Age, and Alcohol Consumption* (1994); Tzuoo-Ding Lin et al., *Time of Day Models of Motor Carrier Accident Risk* (1994); Tzuoo-Ding Lin et al., *Modeling the Safety of Truck Driver Service Hours Using Time-Dependent Logistic Regression* (1993); Ian S. Jones & Howard S. Stein, *Effect of Driver Hours of Service on Tractor-Trailer Crash Involvement* (1987); Robert R. Mackie & James C. Miller, *Effects of Hours of Service Regularity of Schedules, and Cargo Loading on Truck and Bus Driver Fatigue* (1978); William Harris & Robert R. Mackie, *A Study of the Relationships Among Fatigue, Hours of Service, and Safety of Operations of Truck and Bus Drivers* (1972).

[2] Hanowski et al., *Analysis of Risk as a Function of Driving Hour: Assessment of Driving Hours 1 Through 11*, Draft Preliminary Letter Report (Nov. 20, 2007) ("Hanowski, *Analysis of Risk*"); Hanowski et al., *Critical Incidents That Occur in the 10th and 11th Hour of Driving in Commercial Vehicle Operations: Does Risk Increase in the 11th Hour?*, Final Letter Report (Nov. 9, 2007) ("Hanowski, *Critical Incidents*").

[3] The peer review report placed in the docket, *Integrated Report: Peer Review of R.J. Hanowski et al.*, "Analysis of Risk as a Function of Driving Hours: Assessment of Driving Hours 1 through 11" ("Peer Review Report"), reflects that the reviewers were evaluating a different and longer version of the Hanowski study than the two November 2007 papers that are in the docket. For example, there are multiple citations in the Peer Review Report to "chapters" and to pages higher than page 38, the last page of the longer Hanowski 2007 paper in the docket. FMCSA should place the version of the report discussed by the peer review team into the docket. The Peer Review Report has also been "integrated," presumably by FMCSA, raising the question whether the agency has edited the individual peer review reports. The original, unedited versions of those individual reports, if different from what is included in the Integrated Report, should also be placed into the docket.

The Hanowski study cannot bear the weight FMCSA assigns to it. First and foremost, the study used a total of 103 subjects (reduced to 98) operating 46 trucks covering just over 2 million miles. Hanowski, *Critical Incidents*, at 1. The sample used is far too small for generalizability.[4] The overall fatal large-truck vehicle involvement rate per 100 million vehicle-miles-traveled ("VMT") was just above 2 in 2006. The large-truck involvement rate for injury crashes per 100 million VMT was just above 35. NHTSA, *Traffic Safety Facts – 2007 Data*. It should be obvious, then, that even with events of interest expanded to include "critical incidents" (crashes, crash: tire strikes, near-crashes, and crash relevant conflicts), a study of trucks driving only 2 million miles is not sufficiently large or finely grained to discern a difference in crash risk between 10 and 11 hours of driving. The updated literature review commissioned by FMCSA, Greg Belenky & Lora J. Wu, *Literature Review on Fatigue and Health Issues Associated with Commercial Motor Vehicle Drivers Hours of Service: Update from 2004* (Jan. 2008), made this exact point about the Hanowski study: "[T]he sample size is not large enough to assess if allowing an 11th hour of driving is safe." *Id.* at 17. The fact that the study found no statistical difference between the 2d through 11th hour of driving should have been a dead giveaway that the study lacks the statistical power to support generalizing to the larger trucking population—much as getting "heads" 8 times out of 10 flips of a quarter does not mean that the odds of flipping "heads" are 4 times greater than "tails," but that many times this number of flips are necessarily to reveal the odds. In short, FMCSA's heavy reliance on the Hanowski study flies in the face of the agency's claim "to rely primarily" on studies that are "sufficient in scope" to answer the research questions posed. 73 Fed. Reg. at 69575.

The problems with FMCSA's dependence on the Hanowski study do not stop there, however. As another threshold matter, the study suffers from its reliance on a surrogate measure (critical incidents), which is not an acceptable proxy for the number and percentage of fatigue-related truck *crashes* that occur in actual truck driving. After all, the major purpose of the Motor Carrier Safety Improvement Act of 1999, which established FMCSA, was "to reduce the number and severity of large-truck involved crashes," Pub. L. 106-159, § 4 (1999) (49 U.S.C. § 113 note), not the occurrence of "critical incidents." Indeed, Hanowski's lumping together of the various categories of "critical incidents" obscures the fact that the number of crashes recorded by investigators for the 11th driving hour is actually greater than the number in the 10th hour of driving. Hanowski, *Analysis of Risk*, at 9 (Figure 3). The study further attempts to buttress its conclusion that there were no more "critical incidents" in the 11th hour of driving than in the 10th hour by eliminating "critical incidents" based on a video review and judgments of whether a subject driver was "at fault." *Id.* at 10-12.[5]

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[4] Compare, *e.g.*, FMCSA's disclaimed reliance on the 9 fatal truck crashes that occurred in the 11th hour of driving in the 1991-2002 Trucks in Fatal Accidents ("TIFA") database as too few to allow the agency to "make a reasonable choice between a 10- and an 11-hour driving limit," 70 Fed. Reg. 49978, 50012 (2005); its rejection of P.P. Jovanis et al., *Factors Affecting Motor Carrier Crash Risk*, Final Report (Sept. 2005), which found a significant increase in crash risk in the 11th driving hour versus the 10th, because of the "small sample size in the 11th hour of driving," 73 Fed. Reg. at 69576; and the agency's rejection of C.D. Wylie et al., *Commercial Motor Vehicle Driver Rest Periods and Recovery of Performance* (1997), demonstrating that neither a 36-hour or 48-hour off-duty period was sufficient for driver recovery, because of that study's "small subject sample." 70 Fed. Reg. at 49994.

[5] No independent reviewer or member of the public can evaluate the judgments made by the viewers of the videos to determine how and why critical incidents were sometimes judged to be the "fault" of a driver in the study.

But that's not all. The drivers in the study were using a drowsy driving warning system ("DDWS") intended to alert them when signs of drowsiness were detected. The presence of the instrumentation makes it impossible to isolate the effects of an additional hour of driving or to extrapolate the study's findings to the larger population of CMV drivers. Recognizing this flaw, Hanowski restricted some analyses to periods when the warning system was not activated, and indeed, the Peer Review Report stated that "we believe that the non-DDWS data is the most generalizable in this data set." Peer Review Report at 12. Note, however, that these non-DDWS instances make up only a small portion of what is already an inadequate sample size for investigation. With the warning system deactivated, critical incidents in which a trucker was judged at-fault revealed a higher risk in the 11th versus 10th hour (1.90 odds ratio). Hanowski, *Analysis of Risk*, at 15 (Table 10). Hanowski dismisses this finding as not statistically significant, but statistical significance is not the only basis for testing whether effects are real. The best estimate from Hanowski's study is that risk of an at-fault incident (under the study's subjective criteria) nearly doubles between the 10th and 11th hour of driving. Similarly, the Peer Review Report notes (at 13), citing Hanowski, Chapter 3, that "[w]hen DDWS is controlled for, the rates for hour 11 are more similar to the rates for hour 1," and that "there is a significant increase for hour 9 and that appears to persist through hour 11."[6] FMCSA's conclusion that there is no difference in "critical incidents" between the 10th and 11th driving hours appears to be more a matter of selective packaging than of substance.

Apart from these difficulties, confounding or unrepresentative factors further limit the usefulness of this study in assessing any difference in crash risk between the 10th and 11th driving hours. As the Peer Review Report recognized, the fact that drivers were being continuously monitored by sensors and video recordings may have influenced their behavior (the Hawthorne Effect). "If a driver knows s/he is to be observed via video and their critical incidents documented, they are probably less likely to proceed with dangerous or drowsy driving." Peer Review Report at 12. The reviewers' greatest concern was that "this type of observation study yields data that is not representative of the at large CMV driver population because of the presumed high rate of HOS compliance during the study due to continuous monitoring." *Id.*

Furthermore, Hanowski's results based on driving hours are hopelessly confounded by the fact that "[e]mbedded in breaks were non-driving work; there was no way to separate non-driving work from rest breaks as there was only a record of driving." Hanowski, *Analysis of Risk*, at 6. In other words, there is no way to know when drivers who had *driven* fewer hours had, in fact, *worked* longer total hours than drivers who had *driven* longer hours. There is ample

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These judgments constitute a major part of the data relied on, but the videos and criteria that were used to make these judgments are not provided for public review. This same problem plagued Wylie, et al., *Commercial Motor Vehicle Driver Fatigue and Alertness Study* (1996) ("DFAS"), which similarly relied on judgments of observers viewing segments of video tapes regarding whether the recorded faces of U.S. and Canadian drivers, the subjects of the investigation, were "drowsy." Although Advocates for Highway and Auto Safety long ago sought the DFAS videotapes because these ultimately were the "data" relied upon by investigators and, in turn, the agency, for its judgments about the prevalence of fatigue among the subject drivers, the agency never turned over the data.

[6] Because the version of the study subject to peer review is not in the docket, however, we cannot see the data to which the Peer Review Report is referring here.

support in the rulemaking record regarding the fatiguing effects of total duty time, not merely consecutive driving time. *See, e.g.*, 65 Fed. Reg. 25540, 25556 (2000).

One of Hanowski's findings was that the first hour of driving presented the highest risk. But because most of the truck drivers in the study drove at night, the first hour of driving may have occurred at night and the 11th hour during daylight, confounding the effects of driving long hours with the effects of time of day.[7]

But neither the VTTI research team nor FMCSA is deterred by the study's raft of limitations. While noting that caution is in order because of the small sample of drivers, Hanowski declares that "the results from this study do not support the hypothesis that there is an increased risk resulting from CMV drivers driving in the 11th hour as compared to the 10th hour, or any hour." Hanowski, *Analysis of Risk*, at 34. The study also claims that its "results are not consistent [with] the contention that crash risk increases as hour of driving increases." *Id.* at 35. FMCSA took it one step further, claiming that the study concluded that "[t]here is no measurable increased risk for drivers driving in the 11th hour as compared to the 10th hour or any other driving-hour. The finding that TOT is a poor predictor of crashes is consistent with other well-conducted research in this domain . . . ." 73 Fed. Reg. at 69576.[8]

These conclusions are specifically rejected in the Hanowski Peer Review Report. As one reviewer points out, the study's claim that its results "*do not* support the hypothesis that there is an increased risk resulting from CMV drivers driving in the 11th driving-hour as compared to the 10th driving-hour, *or any hour*" is "misleading." Peer Review Report at 6 (emphasis in original). The reviewer recommended that the discussion be expanded "to explain in some detail that while the performance decrement due to time on task was not proven by this study, the study did not

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[7] By contrast, as frequently cited by petitioners and the Insurance Institute for Highway Safety ("IIHS"), several studies controlling for time-of-day effects have found crash risk to increase with time on task. *See, e.g.*, Lin at 7 (1994); Lin at 9-10 (1993); Jones & Stein at 11-18; Saccomanno at 157, 167-72; Frith at 24-28.

[8] As an example of such "other well-conducted research," FMCSA cites the DFAS study, but it is unfathomable how the agency can continue to rely on it. *See* 73 Fed. Reg. at 69576; 72 Fed. Reg. at 71256. That study (in contrast to those cited in note 7, *supra*, not only confounded the time-on-task and time-of-day variables, *see* David Shinar & Robert M. Nicholson, *Peer Review Report of Commercial Driver Fatigue Research* 2, 6 (1995), but it also did not keep track of or analyze the effect of driver naps, DFAS at ES-12, 4-19, and it performed only an incomplete comparison of drivers on 10- and 13-hour schedules, omitting evaluation of their performances at night. *Id.* at ES-9. FMCSA's attempted defense of the study and disparagement of some of its peer reviewers, *see* 73 Fed. Reg. at 69575-76, rings hollow. The DFAS Peer Review Report stated in its blistering general conclusions that the study "suffered from poor design and an inappropriate statistical approach to address its major objectives," Peer Review Report at 2, and it concluded that the study's "analyses look like a fishing expedition rather than a properly designed plan for a test of predetermined hypotheses," *id.* at 6, that it failed to meet objective #1 of "[m]easur[ing] loss of alertness due to fatigue," *id.* at 4, 5, and the *best* rating DFAS received from *any* reviewer was "fair." *Id.* at 5. DFAS's findings are neither sound nor credible. Accordingly, FMCSA discounted the study in its 1999 literature review, *An Annotated Literature Review Relating to Proposed Revisions to the Hours-of-Service Regulation for Commercial Motor Vehicle Drivers* 11-12 (1997) ("DOT Annotated Literature Review"), emphasizing instead that "[n]umerous studies have documented performance and alertness decrements after periods of driving far shorter than 13 or even 10 hours," *id.* at 12, and disregarded the study in the 2003 rule. *See* 68 Fed. Reg. 22456 (Apr. 28, 2003). The agency resurrected the DFAS study only when desperate to find support for the 11-hour driving limit after the 2003 and 2005 rules were vacated by the court.

prove that it doesn't exist either . . . ." *Id.* The reviewer also takes the study to task for stating that its results "are *not consistent* with the contention that crash risk increases as hours of driving increases." Instead, he comments that "[i]t would be more correct to say the results of this study are *inconclusive* relative to that contention . . . ." *Id.* (emphasis in original). Indeed, the reviewer emphasized that "[i]t's not only counterintuitive but contrary to past research findings to say that crash risk does not increase *at all* with hours of driving." *Id.* (emphasis in original). Yet FMCSA persists in relying on the Hanowski study, which fails to demonstrate the counterintuitive proposition that 11 consecutive hours of driving is as safe as 10, while continuing to ignore both the numerous scientific studies indicating that crash risk increases sharply after driving more than 8-10 hours, as well as newer research, such as Jovanis's 2005 case-control study, involving 5,050 drivers and a dataset encompassing 16 million miles of travel, which not only validated the finding of increased risk in driving hours 9-10, but in hour 11 as well.

B. Other Research and Data Cited by FMCSA

With the Hanowski study producing no findings demonstrating the safety of 11 consecutive hours of driving, FMCSA has little else to rely on. It continues to defend its reliance on T.R. O'Neill et al., *Effects of Operating Practices on Commercial Driver Alertness* (1999), for the proposition that a work schedule of 14 hours on-duty per day and 10 hours off-duty does not produce fatigue. 73 Fed. Reg. at 69576-77; *see also* 72 Fed. Reg. at 71261 (citing O'Neill study for proposition that time-on-task is not a good predictor of driving degradations).

FMCSA persists in ignoring the fact that, as its own commissioned literature reviews recognized, the O'Neill study's conclusions, based on just 10 drivers operating simulators under unrealistic conditions (another illustration of FMCSA's embrace of studies with tiny samples when it suits the agency's purposes), cannot be generalized to the broader truck-driver population.

The drivers in O'Neill's study followed strictly daytime schedules, took 3 scheduled breaks throughout the day, rested for 10 consecutive hours at night in an apartment (not on the road), and received a weekly 58-hour recovery period allowing 3 separate sleep periods between 12:00 and 6:00 a.m. O'Neill at 2-3, 16-18. In short, the drivers worked under ideal conditions far removed from the realities of long-haul trucking. For that reason, the study's authors, the Transportation Research Board, and the agency itself have each recognized that the study's results are not generalizable to operations that are not day shifts, have shorter post-shift off-duty periods, few or no breaks during the duty period, or are irregular—in short, the bulk of long-haul trucking. DOT Annotated Literature Review at 116; Transportation Research Board, *Literature Review on Health and Fatigue Issues Associated with Commercial Motor Vehicle Driver Hours of Work* 76-80 (2005) ("TRB, Health and Fatigue Panel"); O'Neill at 2-3, 34, 40-41, 48.

FMCSA also claims that ATRI, *Safety and Health Impacts of the New Hours-of-Service* (2006), authored by the American Trucking Associations' research arm, further supports the conclusion that "overall safety of the motor carrier industry has been maintained since the 2003 and 2005 HOS rules became effective." 73 Fed. Reg. at 69577. This study consists of private

data gathered from truck fleets, the kind of motor carrier survey results that are self-selected and immune to independent, objective evaluation by the public. The agency's reliance on this information yet again flouts FMCSA's alleged "principle" of relying on data "that can be independently verified and tested." *Id.* at 69575. In any event, petitioners' challenge to the 2003, 2005, and now 2008 HOS rule does not address the "overall safety of the motor carrier industry," which is influenced by a multitude of factors. Our challenge is to the HOS rule, which indefensibly increases the daily and weekly hours that truck drivers are permitted to drive and work. Even the ATRI report's authors admit (while FMCSA does not) that its study could not isolate the effects of the HOS rule schedule changes and indeed could not attribute improvements claimed in its survey results to reduced driver fatigue. ATRI at 36.

And although FMCSA claims that the rate of fatigue-related large truck crashes in the Department of Transportation's Fatality Analysis Reporting System ("FARS") database has remained "relatively stable" since the 2003 HOS rule went into effect, 73 Fed. Reg. at 69583, an unenlightening and dramatically understated statistic (as discussed in the next Section), the 2007 rate of 1.9 percent of fatal truck crashes coded for a fatigued driver, *id.*, is at its highest reported level since 2000. *See* 72 Fed. Reg. at 71259 (Table 1). Tellingly, FMCSA ignores the analysis of Quality Control Systems ("QCS"), prepared for petitioners, which demonstrates that the percentage of fatigue-coded, fatal crashes that did *not* involve any large trucks—and thus would be unaffected by the HOS rule—fell *faster* over the period of 2000-2006 than the comparable rate for fatal, large truck crashes that were affected by the HOS rule. QCS, *Report on the FMCSA Interim Final Rule for Hours of Service of Truck Drivers* at 3 (2008). These facts suggest that any overall trend of declining fatigue-coded fatal crashes is attributable to factors other than the HOS rule and at least raises the question whether the smaller drop in the rate for fatigue-coded truck crashes than for non-truck crashes might be attributable to the changes in the HOS rule. *Id.*

C. The Regulatory Impact Analysis of 11 Hours of Driving

Lacking new research to counter decades of scientific findings that crash risk increases sharply after 8 hours of driving, FMCSA decided to go ahead and increase permissible consecutive driving from 10 to 11 hours anyway. The agency relies heavily on its byzantine modeling set out in the Regulatory Impact Analysis (Nov. 2008) ("RIA"), which purports to assess the increased risk of a fatigue-related crash after a trucker has driven for 11 consecutive hours. The RIA fails to take into account either the health impact on truck drivers of driving for longer shifts or the combined impact of driving for 11 hours in a shift that may include up to 14 total working hours. FMCSA is on record as conceding that TOT fatigue effects accrue not only from driving, but from the adverse impact on alertness and performance from *all* duties performed by a driver over the course of a daily shift. *See, e.g.*, 65 Fed. Reg. at 25556 ("The research suggests that performance degrades and crash risk increases markedly after the 12th hour of *any duty time* during a work shift . . . .") (emphasis added). Thus, because the RIA's model fails to take into account all hours that a driver might work in a shift, the model *still* would be insufficient to justify the increase in permissible driving time—even if it accurately predicted driver performance after 11 hours of driving (which it does not).

The RIA model predicting performance after 11 hours of driving relies heavily on SAFTE/FAST data, developed in a laboratory based on a sleep-dose study, as augmented by TIFA data, to assess the relative safety impact of driving for 11 consecutive hours. Without repeating all the criticisms petitioners and IIHS have already leveled at the RIA, we focus here on one fundamental flaw that renders the statistical model, on its own restricted terms, neither useful nor credible for gauging the safety of driving for 11 hours: The modeling is only as good as its underlying data, and TIFA data is unreliable in assessing the relationship between time-on-task ("TOT") and fatigue. FMCSA rejects petitioners' extensive criticism of its reliance on TIFA data for its TOT modeling, but its back-of-the-hand dismissal of petitioners' critique fails even the most superficial scrutiny. *See* 73 Fed. Reg. at 69577-78.

Whereas the FARS database consists of an annual census of all fatal crashes that occur on a public road, the TIFA augmentation of a sampling of the FARS database allows for analysis of the number of hours driven at the time of a fatal truck crash. As is well known, TIFA data collection is based on telephone interviews with individuals who have knowledge of the truck, driver, or crash, such as the driver (if s/he survived), the truck owner, the safety director of the motor carrier, the reporting police officer, and other involved parties. This interview approach to gaining critical information makes TIFA a hit-or-miss system of fact gathering. The uncertainty inherent in the recollection of events and circumstances by the individuals questioned, the unavailability of parties, problems of concealment and hindsight bias, and the substantial length of time that may elapse—often up to a year—before interviews are conducted, render central reliance on TIFA data highly questionable, especially for the number of hours that the driver had driven at the time of the fatal crash.

Accordingly, Campbell noted in his final report analyzing the TIFA data from 1991-2002: "The information in the TIFA file on hours driving at the time of the fatal collision is much less complete than other variables because this information is usually not on the police accident report and the driver/carrier often declines to respond to this question." Kenneth L. Campbell, *Estimates of the Prevalence and Risk of Fatigue in Fatal Crashes Involving Medium/Heavy Trucks from the 1991-2002 TIFA Files*, Final Report 9 (Aug. 2005). The number of hours of driving at the time of the fatal crash was not determined for about 31 percent of the truck drivers, *id.* at 10—biasing the data in important respects, as discussed below. Indeed, Campbell observed: "One might expect that drivers/carriers would be reluctant to report driving beyond the legal limits," *id.*, raising the possibility that some fatal crashes reported as having occurred after driving for 10 or fewer hours (when 10 hours was the legal limit) actually occurred in the 11th or later driver hours, which were illegal.

But there is an equally fundamental problem with relying on TIFA as the source of data to model the relative risks of driving for longer hours: As repeatedly acknowledged by FMCSA, fatigue is significantly underreported in the FARS database. *See, e.g.*, 73 Fed. Reg. at 69578; 65 Fed. Reg. at 25545-46. The agency has discussed at length the problems inherent in relying on police officers to detect that a truck driver was fatigued at the time of a crash. *See, e.g.*, *Preliminary Regulatory Evaluation and Regulatory Flexibility Act Analysis, Hours of Service NPRM* 21-22, 25 (2000) ("PRE"). For starters, the responding officer's primary concern is assisting accident victims and restoring the flow of traffic. Investigating the causes of the

accident is often a secondary concern. Second, few police officers are trained in accident reconstruction; thus, they often lack the experience to conduct a detailed investigation of the evidence and instead, wind up relying on eyewitness and other oral evidence. By the time an officer interviews surviving drivers, any signs of fatigue are likely to have worn off. The stress of a crash produces an adrenaline surge, eliminating any traces of fatigue. 65 Fed. Reg. at 25545; PRE at 21. In addition, FMCSA has agreed that "inattention, distraction, or other driver failures may be related to fatigue, as fatigued drivers are more prone to various types of mental error." 65 Fed. Reg. at 25545. Thus, fatigue crashes are likely underestimated for the additional reason that other driver errors indicated on a PAR may have been attributable to fatigue. "Unfortunately, there is no objective measure of fatigue that can be used by an investigating officer." Campbell at 9.

Even if a police officer recognizes fatigue as a contributing factor, idiosyncrasies in state reporting significantly hinder the information making its way into the FARS database. The agency's literature review explains: "Some police crash reporting forms do not contain a check-off block for driver fatigue or drowsiness—if the driver is not coded as "asleep," no alternative classification to indicate a lesser degree of drowsiness is available to the enforcement official." DOT Annotated Literature Review at 17. As Campbell's 2005 report makes clear, the underreporting of fatigue in the FARS database is not at the margins but is systemic: Overall, no contributing factor is recorded in FARS for about 60 percent of truck drivers involved in fatal crashes, and five states recorded no truck driver fatigue or only a few cases in the 12 years of data analyzed. Campbell at 1-2. Given the poor state of the FARS database with respect to fatigue coding, Campbell was forced to conclude "that the incidence of fatigue in the FARS file underestimates the true incidence of fatigue in fatal crashes." *Id.* at 2.

Oddly, the RIA explains FMCSA's rejection of other highway crash database (such as NHTSA's General Estimates System or FMCSA's Motor Carrier Management Information System) because "reporting practices varied by state, and data were often missing." RIA at 39. But inconsistent coding of fatigue among states is rampant in the FARS database, fundamentally undermining the FARS/TIFA dataset as a source of information regarding the prevalence of fatigue on long driving shifts. As the Campbell-Belzer Report warned FMCSA in 2000:

> To the extent that the reporting of fatigue varies from state to state, it is probably a reflection of the availability of coding or information on the original police report. . . . Fatigue, of course, is particularly difficult to assess, even with in-depth investigations, since there is no physical evidence of fatigue. The assessment is usually based on statements of the involved parties or witnesses, or inferred from the sequence of events.[9]

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[9] Kenneth L. Campbell & Michael H. Belzer, *Hours of Service Regulatory Evaluation Analytical Support* 3 (2000).

This subjectivity, variability from state to state, and underreporting of fatigue in large truck crashes makes reliance on FARS fatigue coding highly problematic.[10] Indeed, TIFA classified only 9 fatal crashes that allegedly occurred in the 11th hour of driving as fatigue-related in the twelve-year period of 1991 to 2002. As FMCSA previously recognized: "Whatever the statistical risk of driving in the 11th hour, FMCSA cannot make a reasonable choice between a 10- and an 11-hour driving limit on the basis of only 9 fatal crashes over a 12-year period." 70 Fed. Reg. at 50011-12. Yet the agency has rested its regulatory impact analysis on this very dataset.

FMCSA's response to petitioners' and QCS's discussion in their March 2008 comments of the immense disparities in fatigue-coding in the TIFA dataset, both by state and by year, is to assert that "FMCSA uses the national level FARS/TIFA data precisely to minimize the potential impact of State-by-State differences in the coding of fatigue by each State's officers. There is no reason to believe that the coding of fatigue in large truck crashes would change across all States simultaneously, such that the national level estimates would vary significantly from one year to the next." 73 Fed. Reg. at 69578. That response is nonsensical. The "national level" FARS/TIFA data is only as good as its inputs.

The Campbell-Belzer Report points out repeatedly that many states are underreporting large truck fatigue-related crashes and that several states have reported no or virtually no truck driver fatigue in their fatal truck crashes. *See* Campbell-Belzer at 2-11. Figure 2 shows the states of North Carolina, Wisconsin, New Hampshire, the District of Columbia, and Hawaii as having reported no large truck crashes in TIFA 1991-1996, while several other states, including large and populous states such as Ohio, Michigan, and Florida, reported very few fatigue crashes despite heavy truck traffic. *Id.* at 4. The significance of this disparity is underscored by NHTSA's NCSA Analysis of large truck crashes by state.[11] NHTSA's NCSA analysis shows that Florida, Ohio, and North Carolina were, respectively, the 3rd, 4th, and 6th highest ranked states in 1996-2000 for the number of large truck fatal crash involvements. Moreover, these three states alone accounted for *nearly 14 percent of all large truck fatal crashes*. It is impossible to believe that no or virtually no fatigue-related large truck crashes occurred in these states. The dramatic under-coding of fatigue crashes in these and other states significantly lowered the national average.

Perhaps an even more telling marker regarding the inadequacies of the TIFA dataset is the significant disparity between states that report very high percentages of fatigue present in

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[10] The problems of subjectivity and underreporting of fatigue also afflicted the Large Truck Crash Causation Study ("LTCCS"), a failed FMCSA enterprise severely criticized by the National Academy of Sciences and the Centers for Disease control for its major defects in research design and data collection. Interestingly, even the flawed LTCCS found fatigue present for 12 percent of drivers of large trucks involved in crashes with injuries or death—notably higher than the 7 percent figure the agency relies on here. 73 Fed. Reg. at 69578; *see* FMCSA, *Analyses of Fatigue-Related Large Truck Crashes, the Assignment of Critical Reason, and Other Variables Using the Large Truck Crash Causation Study* 5-6 & Table 3 (2008).

[11] NHTSA, National Center for Statistics and Analysis, *An Analysis of Fatal Large Truck Crashes* 18 (Table 10) (June 2003) ("NCSA").

fatal crashes resulting in the deaths of the truck drivers and those that report none or very low percentages. As FMCSA has recognized, crashes involving truck driver deaths are particularly likely to involve truck-driver fatigue. 73 Fed. Reg. at 69578. For example, Figure 4 of the Campbell-Belzer Report demonstrates that in 1991-1996, Arizona reported 43 percent and Maine 40 percent of fatal-to-the-truck-driver large truck crashes as attributable to fatigue, along with other states reporting high percentages, while 8 states reported that *0 percent* of their truck-driver fatalities were ascribed to fatigue. Campbell-Belzer at 9. Adjacent states with similar operating conditions show enormous differences in fatigue coding for these truck-driver fatalities. For instance, North Carolina reported *no* truck-driver fatalities attributable to fatigue, but Tennessee, a neighboring state, reported approximately 16-17 percent of such crashes attributable to fatigue. *Id.* This phenomenon of bordering states assigning vastly different fatigue percentages as a driver factor in truck-driver crash fatalities is repeated several times. These disparities are indisputably artifacts of each state's reporting system and not an accurate reflection of the actual proportions of fatigue among truck drivers in fatal crashes. Petitioners' March 2008 comments to the docket discuss in detail the extreme variability in fatigue coding from state to state. Petitioners' Comments at 42-46.

FMCSA is also wrong when it asserts that there is no reason to believe that fatigue coding "would vary significantly from one year to the next." 73 Fed. Reg. 69578; *see also* 72 Fed. Reg. at 71259 (expressing doubt that underreporting of fatigue "varied from year to year"). As the data reproduced in petitioners' March 2008 comments (at 44-45) reflects, fatigue coding of truck drivers in fatal crashes is not only extremely variable from state to state, but also from year to year. For example, the percentage of fatigue-coded fatal truck crashes changed by 50 percent in Texas in just one year, from 2000 to 2001. Petitioners' Comments at 44. According to NHTSA's 2003 analysis, Texas, ranked 1st in fatal large truck crashes, accounted for 8.9 percent of total fatal large truck crashes in 1996-2000. NCSA at 18 (Table 10). An examination of individual state reporting of fatigue-coded fatal truck crashes from year to year shows that state fatigue coding is highly erratic and manifestly underreported, with some states not even reporting any truck driver fatigue-related fatal crashes for some years, and several states reporting percentages of fatigue-related fatal crashes that differ from other states by well over 2 orders of magnitude. *See* Petitioners' Comments at 43-46.

Thus, FMCSA is wrong to think that it can rely on the "national level" FARS/TIFA data to smooth out the enormous variations in fatigue coding both state-to-state and year-by-year. *See* QCS at 4 (widely varying levels of fatigue coding between states and differences in accident report design are important to statistical analyses of relationship between fatigue and hours of driving "*insofar as such problems may confound the basic association* by calendar year") (emphasis in original). FARS simply is not a credible source of information for determining the proportions of truck driver fatigue-related fatal truck crashes, and, as recognized by Campbell-Belzer in 2000 and Campbell in 2005, reliance on FARS, even amplified by TIFA, amounts to underestimating the contribution of truck driver fatigue to fatal truck crashes at a national level. As the Campbell-Belzer Report asserts: "It seems clear that the overall proportion in fatal accidents, 1.86 percent from FARS/TIFA, underestimates the true value." Campbell-Belzer at 11; *see also* Campbell at 2. Yet this is almost exactly the percentage relied on by FMCSA in the final rule, the IFR, and the RIA as the extent of fatigue present in the FARS/TIFA database. *See*

73 Fed. Reg. at 69583; 72 Fed. Reg. at 71259; RIA at 42-43.[12] (As discussed below, scaling the model to produce an average 7 percent fatigue figure does not cure the problem.)

FMCSA's reliance on FARS/TIFA, however, is flawed for yet another reason that was highlighted in the QCS Report at 4-10: The subset of TIFA data used by FMCSA was biased in a way likely to underestimate the elevated risk for fatigue coding with each additional hour of driving. Individuals responding to interviews by TIFA investigators understandably are reluctant to admit that the truck driver drove a number of hours that was then illegal, especially if the respondent is the truck driver or a potentially liable truck owner. *Id.* at 5; *see also* Campbell at 9-10. The potential bias affects not only how hours of driving are reported, but also whether or not a respondent can even be found to report hours of driving. QCS at 5. The RIA's TOT analysis is based on only a subset of the TIFA dataset. It omits vehicles with missing data. The hours of service for truck drivers are unknown for 36 percent of the total. *Id.*

QCS's report showed that this exclusion of incomplete data affected the TOT fatigue crash risk analysis presented in the RIA and that "the underlying data are biased by differential non-response." *Id.* The distribution of cases with missing hours of driving data in the full 1991-2004 TIFA database is not the same for the crashes with fatigue-coded drivers as it is for drivers with no fatigue code. *Id.* Inclusion of TIFA data with incomplete information, but reflecting whether driving hours were or were not within legal limits, QCS's analysis showed, raised the estimate of relative risk of driving illegal hours for fatigue-coded crashes by approximately 28 percent. *Id.* at 9. As the report explains: "The apparent bias in the relative risk of driving illegal hours for fatigue-coded crashes indicates that the fitted logistic model relied upon for the TOT adjustment equation is also biased by the missing data. This bias is likely to underestimate the increasing risk for fatigue coding with each additional hour of service. This underestimate is due to high rates of non-response (i.e., missing data for hours of driving) that differ by fatigue coding." *Id.* The problem is not addressed, the report continues, by FMCSA's use of a bootstrap simulation, which relies on the same data, to estimate the uncertainty of relative risk of fatigue in the 11th hour. *Id.* at 9-10.

FMCSA's answer to this analysis makes no sense: First, the agency asserts that its calculations in the 2005, 2007, and 2008 RIAs, based only on records with full data, "are not significantly different, statistically speaking, from those that use the dataset that includes records with partial HOS data," as QCS suggested. 73 Fed. Reg. at 69578. What does that mean? There

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[12] Inexplicably, FMCSA still fails to see the error in the RIA's narrative at 42, discussing Exhibit 5-1. *See* 73 Fed. Reg. at 69579. Petitioners appreciate that the vertical axis in the bar chart reflects the relative risk of fatigue compared to the average for all driving hours (1.9% overall fatigue coding), not compared to the first hour. The error lies in this statement: "In the 11th hour of driving, the relative risk per involvement in a fatigue-related crash is roughly five times higher than that in the first hour." RIA at 42. FMCSA overlooked the fact that the relative risk in the first hour, according to its calculations, is less than 1. Using the agency's methodology, the relative risk in the 11th hour ($9 \div 94 \div 0.019 = 5$ percent) is still *nearly 10 times higher*, not 5 times higher, than the relative risk in the 1st hour ($102 \div 10{,}142 \div 0.019 = 0.52$ percent). Similar errors in other parts of RIA, Chapter 5, which petitioners brought to FMCSA's attention, Petitioners' Comments at 53-54, have also gone uncorrected and raise questions regarding the validity of the agency's claims and calculations regarding the relative risk of fatal truck crashes for each consecutive hour of driving.

is no showing in the final rule or the RIA that FMCSA performed a formal analysis of the statistical significance of the difference. No specific hypothesis is stated. No critical test statistic is calculated. No probability value is cited for a test statistic. Does the agency mean that it agrees that its data are biased, but it is not impressed with the level of bias? Second, FMCSA states that "the standard errors from imputing missing information, whatever the approach, must be integrated into the model. Given that the difference between the partial and full data is not statistically significant, the net result would be to degrade the performance of the model, not enhance its precision." *Id.* FMCSA's response again is indecipherable. What does its reference to "standard errors" mean in this context? And how could incorporating missing data into its analysis to eliminate bias in the data—a bias the agency does not contest—"degrade the performance" of the model? Incorporating missing data to eliminate bias would only improve the model.

The agency cannot salvage the faults in the FARS/TIFA dataset by scaling the results of its model to produce an overall average fatigue-related value of 7 percent. *See* 73 Fed. Reg. at 69578; RIA at ES-5, 65, V-2, V-15. In 2000, FMCSA estimated fatigue-related crashes at 15 percent, 65 Fed. Reg. at 25546, but then reduced it in 2003 to 8.15 percent (for operations under the pre-2003 rule) with little justification. *See* Regulatory Impact Analysis (2003) at 8-9 to 8-15 ("2003 RIA").

Apart from the lack of research support for the proposition that fatigue contributes to only 7 percent or 8.15 percent of truck crashes, FMCSA's derivation of these figures was flawed. The 2003 RIA derived the 8.15 percent figure for operations under the pre-2003 rule from its analysis of FARS data for 1997-2000, and thus, the estimate suffers from the same flaws discussed above because of the inability of FARS to shed light on the proportion of truck crashes attributable to fatigue. Moreover, the agency's correction of the fatigue-coding percentage from FARS by adding less than 1 percent for inattention crashes, 73 Fed. Reg. at 69578; 2003 RIA at 8-14, is both trivial and inconsistent with the agency's previous recognition that "[c]rashes involving inattention, distraction, or other driver failures may be related to fatigue, as fatigued drivers are more prone to various types of mental error. These errors are major causal factors in crashes." 65 Fed. Reg. at 25545.

FMCSA also calculated the 8.15 percent figure in part by editing the FARS data to exclude cases in which "primary fault appeared to lie with other vehicles (not trucks) involved in the crash, and with certain hazardous weather conditions." RIA at 40; 2003 RIA at 8-14. But no explanation is provided in any of the RIAs of precisely how FMCSA scrubbed the data to reach a figure of 8.15 percent (under the pre-2003 rule), or the figure of 7 percent under the 2003 and later rules.[13] The 2003 RIA indicates that the trivial increase for inattention crashes it included was derived from a study of short-haul drivers conducted by Hanowski, 2003 RIA at 8-14, without providing any explanation for why data from short-haul operations would be applicable to long-haul operations.

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[13] As discussed in our comments, the removal of crash data based on a judgment from FARS codes regarding driver "fault" is highly problematic. Petitioners' Comments at 50 n.174.

Then, in the 2003 RIA, FMCSA predicted that the percentage of fatigue-related crashes would decline from 8.15 percent for operations under the pre-2003 rule to 7 percent under the 2003 final rule by relying on its Walter Reed sleep-dose model (the predecessor to the SAFTE/FAST model), which predicted performance without regard to time-on-task. 2003 RIA at 8-22; *see id.* at 8-30 to 8-33. The 2003 RIA's exclusion of TOT provoked serious criticism by the U.S. Court of Appeals for the D.C. Circuit in its first decision, which is what led the agency to embark upon the enterprise of creating TOT adjustment factors for the 2005 rule, IFR, and 2008 rule. In other words, FMCSA assumed that the fatigue percentage would drop under the 2003 rule and derived the lower 7 percent fatigue figure by *using the very model* the court criticized for *assuming away* TOT effects. In short, FMCSA's prediction that the fatigue percentage would fall to 7 percent under the 2003 (and later) rules—a figure the agency than used to scale its TOT crash risk analysis—is completely circular. The agency's prediction, moreover, is unfounded, defying logic and common sense. Truck drivers were subject to stricter HOS limits, both per shift and per week, under the pre-2003 rule than the 2003 rule. Yet FMCSA predicted in the 2003 RIA that the percentage of fatigue-related crashes would *decline* post-2003. In other words, the logic offered by the agency is that *greater* exposure on the road from longer hours of driving leads to a *lower* risk of fatigue-related large truck crashes.

FMCSA defends its reliance on the 7 percent figure, 73 Fed. Reg. at 69578, without coming to grips with these criticisms. Instead, it lamely offers that its 7 percent figure has been validated by recent TIFA data in 2004 through 2006 reflecting that in *only 1 of 45* fatal truck crashes (2.2 percent) that occurred in the 11th driving hours in 2004-2006 was the truck driver coded as fatigued. *Id.*; *see also id.* at 69583 (Table 1). It is incredible that the agency would cite the very dataset whose conceded underrepresentation of fatigue triggered the enterprise of calculating an adjusted, higher fatigue percentage in the first place to verify that the adjusted fatigue percentage is not too low. That only 1 out of 45 fatal truck crashes in the 11th driving hour was coded for fatigue is all the proof FMCSA needs that the FARS/TIFA dataset cannot be used to calculate the risk of permitting 11 consecutive driving hours.

II. FMCSA's Decision to Adopt the 34-Hour Restart, Significantly Increasing Permissible Driving Hours Per Week Is Unjustified.

In both the final rule and the RIA, FMCSA continues to ignore both the safety and health implications of cumulative fatigue from the substantially elevated hours (both weekly and long term) allowed by the 34-hour restart.[14] Petitioners have discussed at length, both in their most recent submission, Petitioners' Comments at 8-14, 67-76, and in their comments filed in connection with the previous HOS rulemakings, the adverse effects of cumulative fatigue, from both health and safety standpoints.

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[14] The restart provision amends the pre-2003 60- and 70-hour maximum limits on weekly driving hours by permitting drivers to take as little as 34 hours off-duty before they "restart" their weekly tally of driving or on-duty hours, thereby increasing the number of hours a truck driver may either drive or work per week (and over the longer term). Drivers operating under this new "floating" work week who maximize the use of driving hours under the final rule can drive 77 rather than 60 hours in 7 consecutive days, a 28% increase, while drivers on an 8-day schedule can drive up to 88 hours instead of 70, a 26% increase. Similarly, drivers operating on the nominal 8-day tour of duty schedule can work up to 40% more non-driving hours over 8 consecutive calendar days than allowed by

Whether or not the "average" truck driver is driving longer hours than under the pre-2003 rule, it is indisputable that a significant percentage of drivers are driving and/or working more hours than they did before. These longer hours have adverse consequences both for highway safety and for truck driver health, but the agency continues to ignore them, as well as the effect of the interaction between the HOS rule's increased hours of driving allowed per shift and increased hours of both work and driving allowed per week and over the longer term.

No scientific evidence supports the safety of the driving and working schedules permitted by the 34-hour restart. FMCSA makes a half-hearted attempt to invoke studies it claims support a 34-hour restart, 73 Fed. Reg. at 69569 (citing 70 Fed. Reg. at 49994), but the few studies FMCSA cites in this final rule and the 2005 rule regarding the recovery period's duration—none of which address the recovery period drivers would need after following the driving/work schedules that the 34-hour restart makes possible and lawful—do not support adopting a 34-hour restart.

FMCSA contends that 4 of 5 studies identified by TRB's Health and Fatigue Panel supported recovery periods of 34 or even 24 hours, while only one supported a recovery period longer than 34 hours. 73 Fed. Reg. at 69569; 70 Fed. Reg. at 49994. The one study FMCSA agrees does not support a 34-hour restart is C.D. Wylie, et al., *Commercial Motor Vehicle Driver Rest Periods and Recovery of Performance* (1997), which assessed different length recovery periods for truckers in real-world driving conditions, and found "there was no objective evidence of driver recovery of performance" after 36 hours off-duty or even after 48 hours. Wylie at vii-viii, 25-26. FMCSA dismissed Wylie's findings, however, because the study involved a "small subject sample" of 25 drivers, 70 Fed. Reg. at 49994, 50024, although all the studies cited by FMCSA involved small samples, with the O'Neill (1999) study based only on 10 truckers operating driving simulators. O'Neill at 16.

The other cited studies do not support FMCSA's contention that 34 hours (or less) is sufficient for weekly recovery. One of these is O'Neill's study. 70 Fed. Reg. at 49994. But as discussed above, this study is inapposite, as it studies the performance of 10 drivers operating simulators under ideal conditions (daytime driving, breaks throughout the day, night sleep in apartments, 58-hour weekly off-duty periods) far removed from normal long-haul driving conditions. O'Neill did not purport to study the recovery period that would be needed to restore performance for truck drivers following anything like realistic driving conditions under schedules permitted by the final rule.

Another study cited by FMCSA, A.-M. Feyer et al., *Balancing Work and Rest to Combat Driver Fatigue: An Investigation of Two-Up Driving in Australia* (1997), *see* 70 Fed. Reg. at 49994, studied 37 drivers—another small sample—to compare fatigue levels in single and two-up (team) operations on a 4 to 5-day roundtrip in Australia and found that two-up drivers who

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the pre-2003 HOS rule. The increased driving or working hours directly correspond to a reduction in available hours of off-duty time under the current HOS rule, as FMCSA was compelled to admit. *See* 70 FR 3339, 3348 (2005).

stopped for an overnight rest mid-trip recovered alertness afterwards. Feyer at 541, 552. No conclusions can be drawn from the study regarding the effectiveness of a single overnight break. For starters, the study was conducted in Australia, which, according to FMCSA, limits its value because, as the agency said in dismissing a different study conducted in Australia, "Australian drivers . . . operate under very different operational and regulatory environments than in the United States." 73 Fed. Reg. at 69577.

In addition, Feyer's finding for two-up drivers cannot be generalized to single drivers because the latter spent more time driving than the two-up drivers. *Id.* at 544. The authors also observed that the drivers used breaks throughout the trip as a primary tool to recover from fatigue, *id.* at 546, 552, diluting any assessment of the effectiveness of an overnight break. No details were provided regarding how much sleep drivers obtained either overnight or during the breaks throughout the trip. Significantly, the study's judgments about fatigue levels relied heavily on subjective self-reports. *Id.* at 547. Yet FMCSA has rejected multiple study findings cited by petitioners because they were based on subjective self-reports. *See, e.g.*, 73 Fed. Reg. at 69575 ("The first principle that the Agency uses in evaluating research is that studies based on quantifiable, objective data that can be independently verified and tested are preferable to those based on subjective data such as individuals' opinions or perceptions."); *id.* at 69573 (rejecting many studies showing adverse effects on truck driver health from long driving and working hours because "many of the studies involved self-evaluations, which cannot be independently verified").

Feyer's objective performance measures suggested that drivers actually were suffering from mounting cumulative fatigue over the trip. *Id.* at 549 ("Performance on the . . . critical flicker fusion task [a cognitive test] deteriorated linearly across the trip, for both single and two-up drivers, consistent with increasing fatigue."). Alertness, as reflected in steering control, decreased for all two-up drivers at the end of the trip. *Id.* at 551. "The pre-break fatigue experienced by the two-up drivers showed an increasing trend over the later breaks in the trip, suggestive of accumulating fatigue." *Id.* at 548. Breaks that occurred later in the trip did not restore these drivers' alertness. *Id.* Finally, the authors observed: "Some loss of alertness was seen at the end of the trip among two-up drivers with a long overnight stop . . . this result indicated that the loss of alertness seen at the beginning of the homeward leg of the trip was not recovered." *Id.* at 551. Feyer actually shows that performance degradation could *not* be eliminated after a single overnight stop, even with many other breaks.

FMCSA's citation to E.A. Alluisi, *Influence of Work-Rest Scheduling and Sleep Loss on Sustained Performance* (1972), 73 Fed. Reg. at 69569; 70 Fed. Reg. at 49994, which, in fact, was *not* cited by TRB's Health and Fatigue Panel (perhaps because of the study's apparent lack of relevance to truck driving), is equally unavailing. Alluisi's findings are terribly out of date. He maintained that humans "can do remarkably well in working 12 hours a day 7 days a week for 20 to 60 days, or even 16 hours a day 7 days a week for 15 to 30 days." *Id.* at 205. No serious researcher believes that any more, and the statement has been disproved repeatedly in NIOSH studies.[15] The laboratory schedules studied by Alluisi (4-4-4-12, 4-4, or 4-2 work regimes), *id.* at

[15] *See, e.g.*, Rosa, *Extended Workshifts and Excessive Fatigue* (1995); Rosa & Bonnet, *Performance and Alertness on 8 h and 12 h Rotating Shifts at a Natural Gas Utility* (1993); Rosa, *Performance, Alertness, and Sleep*

205-07, bear little resemblance to the schedules followed by long-haul truckers, and the test demands of providing computation/coding responses, *id.* at 200-01, are far removed from the physical exertion and vigilance necessary to operate a multi-ton truck safely for 11 hours per day and/or 80-90 hours per week. Until this HOS rulemaking, FMCSA (and its predecessors) evidently did not credit this study because, in the 1990s, the agency found no scientific support for a 24-hour restart. *See* 58 Fed. Reg. 6937, 6938 (Feb. 3, 1993); *accord* DOT Annotated Literature Review at 96; *see also* A. Smiley & R. Heslegrave, *A 36-Hour Recovery Period for Truck Drivers: Synopsis of Current Scientific Knowledge* vii, 14 (1997) (research does not support a 36-hour reset that would allow 92 hours on-duty within 7 days); Sang-Woo Park, et al., *Safety Implications of Multi-Day Driving Schedules for Truck Drivers: Comparison of Field Experiments and Crash Data Analysis* 14 (2005) (crash data suggests that 24 and perhaps 48 hours' recovery is insufficient).

Finally, T. Balkin, et al., *Effects of Sleep Schedules on Commercial Motor Vehicle Driver Performance* (2000), the last study cited, *see* 70 Fed. Reg. at 49994, confirms the inadequacy of 34-hour and even longer recovery periods. Balkin found that performance in the 7-hours-in-bed group was consistently reduced across the 7-day study period and that even after 3 nights' recovery sleep (8 hours in-bed), driver performance on the PVT improved, but failed to return to baseline levels. Balkin at 5-7 to 5-8; Gregory Belenky et al., *Patterns of Performance Degradation and Restoration During Sleep Restriction and Subsequent Recovery: A Sleep Dose-Response Study* 10 (2003); *see also* TRB, Health and Fatigue Panel, at 144.

In sum, no scientific research supports either the short duration of the 34-hour restart or the working/driving schedules that the restart provision made possible and lawful.

CONCLUSION

FMCSA should grant the petition for reconsideration and adopt an HOS rule that restores the pre-2003 10-hour limit on consecutive driving hours and eliminates the 34-hour restart provision.

After 3-5 Years of 12 h Shifts: A Follow-up Study (1991); Rosa et al., *Extended Workdays: Effects of 8-hour and 12-hour Rotating Shift Schedules on Performance, Subjective Alertness, Sleep-Patterns, and Psychosocial Variables* (1989); *see also* Comments filed by NIOSH (Dec. 2000).

Respectfully submitted,

Judith Lee Stone
President
Advocates for Highway
and Auto Safety

LaMont Byrd
Director, Safety and Safety Department
International Brotherhood of Teamsters

Joan Claybrook
President
Public Citizen

John Lannen
Executive Director
Truck Safety Coalition

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ADVOCATES
FOR HIGHWAY
AND AUTO SAFETY

750 First Street, NE
Suite 901
Washington, DC 20002

Honorable John Hill, Administrator
Federal Motor Carrier Safety Administration
Suite W60-308
United States Department of Transportation
1200 New Jersey Avenue, S.E.
Washington, D.C. 20590-0001