REPLIES TO LOWER (2011) COMMENTS

DRI-TR-12-05

J. W. Zellner
R. M. Van Auken
S. A. Kebschull

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EXECUTIVE SUMMARY

This report provides replies to comments made regarding previous research into Rollover Protection Systems (ROPs) and Crush Protective Devices (CPDs) proposed for use on All Terrain Vehicles (ATVs), conducted and published by Dynamic Research, Inc. (DRI) beginning in 1997. The comments replied to herein are contained in the document assembled by Lower (2011) entitled "Summary – Review of Dynamic Research Inc. Data." The Lower (2011) document contains 50 specific comments, which are stated to be based on the findings of John Lambert, Shane Richardson and Geoff McDonald.

In this report, the term "CPD" refers to devices like the Quadbar which have an overturn protective structure and no restraint belts. The term "ROPS" refers to systems which have an overturn protective structure and one or more restraint belts. The term "overturn" is considered to include rollovers, pitchovers and combined axis overturns of a vehicle. The term "misuse" refers to using a product in a way that is contrary to the warning labels that are required on ANSI/SVIA-1 compliant ATV's, for example, not wearing a helmet, use by children, etc.

DRI's replies to the Lower (2011) comments, presented herein, are somewhat detailed, as the Lower (2011) comments themselves are relatively detailed.

Unfortunately, most of the Lower (2011) comments were observed to be not accurate and somewhat misleading, primarily because they did not take into proper account the extensive documentation provided in the context of the Victorian Coronial inquests into deaths associated with ATV's or other past DRI research on ROPS/CPDs. In addition, many of those comments can be shown to be inaccurate by reference to further, contemporaneous DRI research published in August 2012 (i.e., Zellner, et al. (2012).

Examples of comments which were inaccurate and misleading because they did not have proper regard to previously published data include:

- The fact that "terrain discontinuities" (e.g., dirt next to grass, as in a dirt road running through a field) do appear in many of the original UK/US case descriptions and in the corresponding simulation model summaries in the DRI reports, whereas the Lower (2011) comments claim that there was "no justification" for why the "terrain types…have very different surface characteristics adjacent to each other"(see reply to Comment B.4));
- The fact that nearly 700 sets of 100 page computer simulation input files that describe suspension, tyre and ATV surface characteristics *were* provided by DRI, whereas the Lower (2011) comments state that there were "no…characteristics listed" (see replies to Comments A.1, B.1, B.2);
- The fact that for each case, all of the relevant UK/US case information that was available was used, and that information was directly used or considered in each general "type" of

overturn that was defined for simulation and potential test purposes (see reply to Comment B.5);

- The fact that the UK case information did indicate that ATV's *overturned* on 45 degree slopes (five UK cases explicitly mentioning 40 to 45 degree slopes), which is very different from the statement in the Lower (2011) comments that the simulations have ATV's "operating on 45 degree slopes, a physical impossibility" (Comment B.7);
- The fact that predicted "crush" injuries to the chest and abdomen (calculated according to ISO 13232 (2005)) and to the skull and face *were* included in the simulation model, which is very different from the assertion in the Lower (2011) comments that crush injuries were not included (see reply to Comment C.6);
- The fact that head bouncing at 4 cycles per second *is* visible in the independently published response data for the MATD crash dummy that was used for the DRI tests and simulations, whereas the Lower (2011) comments state that such bouncing is a "physical impossibility" (Comment D.5);
- The fact that the overturning ATV's in the simulation animations did not in fact bounce "like a beach ball", which is descriptive only of an object with very low mass and density, and for which aerodynamic forces can have a relatively large effect. ATV's (as well as cars, light trucks and dune buggies) have been observed and filmed in full-scale tests to "bounce" to some extent as they undergo overturning motions, particularly on steep slopes and/or at high speeds. The DRI ATV model is based on characteristics that have been directly measured, and/or dynamically matched based on high speed digitized films of actual full-scale tests of ATV overturns and subsequent ATV motions (Comment D.6);
- The DRI simulations are based solely on Newtonian physics, as implemented in the US Air Force/US Department of Transportation Articulated Total Body Simulation that was used by DRI. That simulation has been in existence and has been widely used and validated by many researchers during the last 40 years. Further, DRI's input parameter values are based on measured or estimated mechanical characteristics, and the simulated ATV motions have been mathematically compared with and correlated against digitized high speed films of real ATV/dummy overturn tests and have been found to be in very close agreement (e.g., r^2=0.91 correlation coefficient). For each of these reasons, the claim in the Lower (2011) comments that DRI's simulations "def[y] physics" is without basis (Comment C.8);
- The DRI-published high speed film digitized data, which are accurate and calibrated, indicate that the slope where the ATV overturned in the DRI uphill calibration tests was indeed 43 to 45 degrees, whereas the claim made in the Lower (2011) comments that the slope was instead "32-34 degrees" is based on the video images, which have distorted aspect ratios (see replies to Comments E.2 and E.6);
- The fact that in the uphill calibration tests, a wide-open throttle input was applied to the ATV while it was headed uphill on a 45 degree slope in order to induce a rearward

overturn, however the Lower (2011) comments mistakenly claim that the "bounce of the [ATV]" (presumably referring to the sudden pitch-up of the ATV, due to that throttle input, which is visible in the test video) is evidence that the test surface was not a "uniform surface" (see reply to Comment E.7);

- The Lower (2011) comments both wrongly assert that it is possible to calculate Net Benefit percentage from Risk/Benefit percentage and then report erroneous values of Net Benefit. However, as is set out in ISO 13232 (2005), it is in fact *not* possible to calculate Net Benefit percentage from Risk/Benefit percentage (see reply to Comment F.6);
- The fact that the DRI 2007 report states that the Quadbar injury risks "were much greater than those in ISO 13232-5 *guidelines* for occupant protection systems. ISO 13232-5 *recommends* that "the risk/benefit percentage *should* be less than 7 percent and *should not* be more than 12 percent"", which is very different from the Lower (2011) comments, in which it is stated that "DRI *required* that the increase in injury…had to be 7% or less" (see reply to Comment G.7);
- The fact that there are no published data, worldwide, indicating that there are any acceptable automotive safety devices that have an injury or fatality risk/benefit percentage greater than 7%, which is very different from the Lower (2011) comments say that this [risk/benefit] criterion "fails any test of reasonableness" (see reply to Comment G.7).

The previous lack of agreement between the injuries observed in the 113 simulated general "types" of overturn and those observed in the 113 actual cases to which the Lower (2011) comments refer was primarily due to: a) the impossibility of matching the outcomes of simulated general "types" of overturn to specific overturn cases, which is not appropriate and is beyond both the state of technology and the quality of the accident data; b) the previous use of estimated rather than measured dummy/soil friction coefficients; and c) the previous inclusion of actual injuries that cannot be monitored by crash dummies. Of these, b) and c) were expedients taken at the time by DRI in response to the requests of authorities urgently requesting the relevant DRI reports. However, the most recent updated DRI research on the Quadbar, done when more time was available, and in which refinements addressing these and other points were made, indicates very close agreement between the aggregated injury severity distributions in the simulated overturn types and the actual overturn cases, and found that the overall conclusions regarding the Quadbar were not affected by these refinements. The statements in the Lower (2011) comments that the previous lack of agreement was a "bias" that was "systematic" that "some may say [was] deliberately fraudulent" were baseless (see replies to Comments F.3 and F.4).

In addition, some of the Lower (2011) comments appear to be based on false premises. For example:

- Incorrectly assuming that crash dummies should be “active”, when no such crash dummy technology exists (see replies to Comment B.2 and E.9):
- Incorrectly assuming that the DRI simulations were intended to model detailed causes of ATV overturns, or “stability calibrations”, or ATV “operating” conditions, whereas the actual objective of those simulations was to model the *consequences* (i.e., predicted injury outcomes) of an overturn, assuming that an overturn occurred (see replies to Comments B.3, B.7, E.2, E.5 and E.8);
- Incorrectly assuming that the objective was to recreate each UK/US overturn case in detail, including actual injuries, whereas it was never possible to have done this because of the lack of detailed case information. Instead, the DRI objective was to define 113 general “types” of ATV overturn, where each specific case resided within or adjacent to the corresponding general “type” of overturn (see replies to Comment B.5, B.6, C.1, C.2, C.3, F.1);
- Incorrectly assuming that case-by-case matching of injuries between simulation and actual accidents was the objective, which is beyond the state of both technology and the information. Comparing aggregated injury distributions, between model and actual cases, *is* within the state of technology, and DRI’s latest results indicate very close agreement in that regard (see replies to C.2, C.3, C.4, C.7, F.1);
- Incorrectly assuming that it is possible to use crash dummies to monitor for all injuries sustained by humans, including all types of trunk injuries, which is beyond the state of crash dummy technology (see reply to Comment E.2).

Importantly, some of the Lower (2011) comments are now out of date ~~now out of date~~ because DRI’s latest updated research on the Quadbar, which takes into account all known, valid comments in making the methodology updates, extensions and refinements described in Section IV of Zellner, et al. (2012), indicates that for the helmeted condition, the injury risk/benefit percentage for the Quadbar is 108% [69%, 168%],[1] which does not change the previous overall conclusions about the Quadbar. In fact, on average, for the helmeted condition, the injury risk is greater than the injury benefit in the simulation sample (and not statistically significantly different than the injury benefit when projected to the population of all overturns) (see replies to Comments G.8 and G.9).

In addition, DRI’s latest research indicates:

- A preliminary index of asphyxiation (i.e., breathing difficulty) can be included in the simulations, and when it is, the previous conclusions about the Quadbar are not

[1] Square brackets indicate 95 percentile upper and lower confidence intervals on the population estimate. When 100% lies within this interval, the outcome is not statistically significantly different from 100% for the population of all overturns, i.e., the injury risks are effectively equal to the injury benefits)

affected, i.e., the Quadbar causes as many potential asphyxiations as it prevents (see replies to Comment C.6);

- In terms of probability of fatality, the risk/benefit percentage of the Quadbar for the helmeted condition is 134%, within a 95% confidence interval of [79%, 219%]. The 134% value means that the Quadbar fatality risks are, on average, greater than the fatality benefits in the simulation sample of 770 overturns, but the range in square brackets (which includes 100%) means that this outcome is not statistically significantly different from 100% for the population of all overturns, i.e., the Quadbar has equal fatality risks and fatality benefits (see reply to Comment F.6);
- The most recent DRI animations of the Quadbar simulations, which have been refined in order to take into account all known, valid comments, exhibit no "head…bouncing on the ground…at 4 cycles per second" and like the previous DRI research, no "bouncing like a beach ball", and no motion that "defies physics" (see replies to comments D.5, D.6, D.8);

In addition, some of the Lower (2011) comments are not relevant, because the motions described rarely or never occurred in the DRI simulations, and *never* occurred in the previous or most recent refined simulations used to evaluate the Quadbar risk/benefits (see replies to Comments D.3, D.4, D.5, D.8).

Finally, many of the Lower (2011) comments are unclear, as it is unknown to which DRI or other work it is referring. For example, the source is unknown for the Lower (2011) statement that "if we take no action, 13 deaths will occur; fit modification, 13 deaths will be prevented, [and] 1 death will occur as a result of the modification." There is no valid basis for such a statement in any of the DRI work, and no valid basis in any other research of which DRI is aware.

Overall, 47 of the 50 Lower (2011) comments were found to have one or more significant issues pertaining to them. The remaining three comments were valid but warranted comment.

In view of the extent and the nature of the issues concerning the validity of the summary provided by Lower (2011), which is stated to be based on the findings of Lambert, Richardson and McDonald, that summary cannot be relied upon.

SECTION I

INTRODUCTION

This report provides replies to comments made regarding previous research on Rollover Protection Systems (ROPs) and Crush Protective Devices (CPDs) proposed for use on All Terrain Vehicles (ATVs), conducted and published by Dynamic Research, Inc. (DRI) beginning in 1997. The comments replied to herein are contained in the document assembled by Lower (2011) entitled “Summary – Review of Dynamic Research Inc. Data.” The Lower (2011) document states that it was assembled by Lower, based on the findings of John Lambert, Shane Richardson and Geoff McDonald.

In this report, the term "CPD" refers to devices like the Quadbar which have an overturn protective structure and no restraint belts. The term "ROPS" refers to systems which have an overturn protective structure and one or more restraint belts. The term "overturn" is considered to include rollovers, pitchovers and combined axis overturns of a vehicle. The term "misuse" refers to using a product in a way that is contrary to the warning labels that are required on ANSI/SVIA-I compliant ATV's, for example, not wearing a helmet, use by children, etc.

This report proceeds by presenting each comment from Lower (2011), followed by DRI’s reply to that comment.

SECTION II

REPLIES TO COMMENTS

A. INTRODUCTORY COMMENT IN LOWER (2011)

1. ***"The Dynamic Research Inc. (DRI) work is based on a sample of 59 HSE - UK and 54 US quad bike rollovers (n=113) cases, along with some computer modelling and video simulations."***

This comment is accurate as far as it goes, but it is also somewhat incomplete and misleading because the previous DRI work was far more extensive than implied here. The DRI work includes what is mentioned here, but is also based on data, standards and analysis other than what is implied by this sentence. The DRI work is in part contained in documents provided to counsel for Australian ATV importers who, it is understood, submitted them to the Victorian State Coroner and of which copies were provided to Richardson's business, DV Experts International Pty Ltd, in the context of the 2002 to 2009 Coronial inquests, including 16 volumes of reports (including but not limited to Zellner, et al. (2004) and Munoz, et al. (2007)), comprising 1636 pages prepared for the Coronial inquests, based on analysis of 1612 computer simulation runs; 678 computer simulation input files of approximately 100 pages each; a set of 5 computer simulation software programs provided to DV Experts under license; ~~and~~ example videos of computer simulation animations involving ATV overturns; constructing a MUARC ROPS and conducting human factors, ergonomics and mobility tests with it; obtaining and measuring a prototype Quadbar device. These reports and documentation are not specifically referenced by the Lower (2011) or McDonald (2010) summaries, and the Lambert (2011a~~b~~) document only vaguely refers to the Munoz, et al. (2007) report and to the Zellner, et al. (2008) paper.

The aforementioned DRI reports and extensive supporting documentation, as well as extensive supporting testimony, were generated and provided specifically for and in the context of the Coronial inquests. It is possible that the commentators did not review that extensive documentation or testimony.

In addition, the Lower (2011) document does not identify precisely which Lambert, MacDonald and Richardson documents describe the findings to which it refers; nor is it clear whether or when any such source documents have ever been published or circulated to the relevant parties, including DRI.

Also submitted to the Coronial inquests, but not cited in the Lower (2011) document or in the Lambert (2011a) report or in the McDonald (2010) summary on which Lower (2011) is apparently partly based, were: a previous, extensive, four volume DRI report, Van Auken, et al. (1998), comprising 569 pages, involving high speed camera motion analysis of 12 full-scale ATV overturn calibration tests with a fully instrumented ISO-MATD crash dummy, and analysis of 354 computer simulations runs; a narrated video of the 12 full-scale ATV overturn calibration tests; one internationally published DRI paper (i.e., Zellner, et al. (2008)) on ATV ROPS, comprising 23 pages and analysis of 15,820 computer simulation runs; and another internationally published DRI paper (i.e., Kebschull, et al. (1998)), comprising 19 pages describing 39 laboratory calibration tests with the ISO MATD dummy.

The only DRI work referenced in the Lambert (2011a) report is the relatively early Van Auken (1997a) "preliminary analysis.", and the Munoz, et al. (2007) report on the Robertson V-Bar.

Also submitted to the Coronial inquests were at least four internationally published papers by other researchers, on the topics of risk-benefit analysis of vehicle safety devices (i.e., Osendorfer (2001), Iijima, et al. (1998) and Thompson, et al. (2001)) and comparison of actual to computer simulated injury distributions based on aggregated data (i.e., Kuchar (2001)).

Since the documented research of DRI in the area of ATV Rollover Protection Systems (ROPS) and Crush Protection Devices (CPDs) is in fact extensive, as many of the comments contained in the Lower (2011) document do not provide specific references to data or statements within the DRI work, they are unfortunately excessively vague as to their exact subject, and it is often unclear to which DRI report, data, video or result a given comment is referring. Answers to many of the comments are contained in the aforementioned large body of DRI research.

B. MODELING (LAMBERT)

1. ***"The quad bike model is deficient – there are no front axle characteristics listed, no tire vertical and horizontal factors lists; no shock absorber characteristics listed; no deformation values for quad bike surfaces listed; and no information on how fuel tank fluid is modelled;"***

This statement is not accurate, because each of the 708 computer simulation input files mentioned in Item 1 (each of which consisted of approximately 100 pages of input values) contained extensive details of the front axle (i.e., independent suspension) characteristics, tyre vertical and horizontal factors, shock absorber characteristics, and force-deflection and deformation characteristics of ATV surfaces.

The fuel tank was assumed to be full, so that for simplicity, fluid slosh was not modelled (although DRI has modelled fluid slosh in other, e.g., tanker truck applications where the large volume of fluid can have a substantial effect).

2. ***"The Hybrid III dummy is incorrectly modelled, principally because it is passive – there is no active movement of the torso to lean up a slope or lean to the inside when undertaking a turn;"***

   This statement is not accurate, because the dummy model in both the computer simulation and the full-scale overturn calibration tests) is not a Hybrid III dummy but, rather, an ISO 13232 MATD dummy, which includes 28 crucial modifications to a Hybrid dummy (e.g., to enable the dummy to sit on a straddle seat, grip the handlebars, retain a helmet, monitor lateral and vertical as well as fore-aft forces and motions) which are relevant to ATVs.

   In addition, this statement is based on a false premise, because it implies that the dummy model should be active; however in actuality the dummy model must model the mechanical MATD dummy, which like all crash dummies in existence, is fundamentally passive. There are no crash dummies that have "active" torso movement.

   It is improper and invalid to model an active human in a crash/rollover test, because it is impossible to calibrate/validate such a model, as it is unethical to use humans in crash tests. Only mechanical, passive crash dummies are used in crash/rollover tests, worldwide, as they are repeatable and reproducible, and ethically appropriate; and that is what was both modelled and tested by DRI.

3. ***"The friction factors assumed are unreal;"***

   If what is being referred to are the tyre/soil friction values listed in, e.g., Van Auken, et al. (1997a), then this comment is not accurate, somewhat misleading and based on a false premise as these values were not intended to be indicative of typical or normal operating conditions but, rather, as being representative of values that can be associated with overturns. This is appropriate because all of the n=113 cases did involve overturns. Typical, lower friction values encountered in normal usage, examples of which are cited by Lambert (2011a, page 9~~s 3 and 5~~), usually or very often do not result in ATV overturn, either in the DRI computer simulations or in the real world.

   In any case, as stated in the various DRI reports, the purpose of the computer simulations was to measure what injuries may occur when the vehicle overturns, not when it is operating in typical conditions. Nor was their purpose to attempt to study the detailed *causes* of an overturn. These are quite different types of studies.

   DRI has portable tyre/soil dynamic measurement machines, and has done extensive ATV tyre/soil measurements under diverse conditions across North America, and is aware of the range of tyre/soil friction coefficients that can occur. Although relatively low friction coefficients are typical, in the absence of a large obstacle or discontinuity, it is relatively difficult to overturn an ATV under low friction coefficient conditions. On the other hand, relatively high friction coefficients can occur on dry, hard, compacted, clayey soils, for example, or whenever there is substantial tyre sinkage (i.e., when the tyre sinks into the soil) or in deep vegetation or when the tyre lugs mechanically engage the soil, in which cases the effective side force or longitudinal force can be very large, corresponding to “effective” coefficients of friction that can easily exceed 1.0. The tyre/soil coefficient values used in the simulation models and associated with overturns were mostly 0.75 and 1.0, with a few instances of greater (i.e., sinkage or engagement type) coefficients.

These coefficients of friction are not unrealistic for overturn conditions, and moreover, the exact values used are not crucial, as the purpose was, as noted above, to evaluate the effects - not the causes - of overturns.

4. ***Four out of the 6 terrain types (21 out of 59 cases) have very different surface characteristics adjacent to each other mostly with no justification based on HSE descriptions;"***

   This comment is not accurate, as there is justification in the HSE case information for having very different surface characteristics adjacent to each other.

   The primary reason for this is that the HSE case descriptions indicate that many of the accident terrains have discontinuities, e.g., “ditch”, “rut”, “gulley”, “embankment”, “kerb”, “rutted road”, “pothole”, “track”, “culvert”, “ramp”. It is plausible, reasonable and typical in the real world that such discontinuities often involve different surface characteristics on either side of the discontinuity. For example, a dirt road involves one set of characteristics for an assumed, typical, hard, compacted, dirt road, and a very different set of characteristics for an adjacent, assumed, typical, grassy or vegetation covered field. A ditch or culvert would often or typically have different surface characteristics than the immediately adjacent field. Similar justification applies to the other common types of terrain discontinuities that were reported in the HSE case files and modeled in the simulations.

5. ***"Only 29 (13%) of the 215 data values inputted into the models are supported by HSE case data;"***

   This comment is based on the false premise that the goal was to somehow recreate each of the HSE (and CPSC) cases in detail. But this was not the goal, and would indeed have been impossible, given the extremely limited detail available in the relevant case files. Instead, the goal was to create a set of general "types" or "categories" (also referred to in the DRI reports as “configurations” or “scenarios”) of overturn, within which each specific case resided. In practice, this meant using all of the relevant variables that were available in the case file (from the list of 17 variables needed to describe an ATV overturn); and then, for those variables that were not available in the case file, assuming standardized, plausible values that would represent the general "type" of overturn. In this way, 113 "types" (or "categories", configurations" or "scenarios") of overturn were modelled, not 113 specific cases.

In case this point was not clearly apparent to some readers in the earlier DRI reports, the latest revised DRI report in Zellner, et al (2012) clarifies this distinction.

6. ***"Only 1 of the 59 HSE cases is correctly modelled – that is the HSE description is correctly reflected by the DRI data in only one case;"***

This comment is based on a false premise that the goal was to somehow recreate each of the HSE (and CPSC) cases in detail. This was not the goal and would have been impossible as there was not enough case information to do so. The goal was to define 113 general "types" of overturn (See reply in Item 6 above).

Also, it is unclear to what the expression "correctly modelled" in this comment refers.

If "correctly modelled" is referring to the 17 input variables noted in the reply above, this statement is not accurate, as in all 59 HSE cases, each of those 17 variables that were available in each specific case correspond to those used in the simulation of the corresponding general "type" of rollover.

If "correctly modelled" is referring to the outcomes of the rollover, it is based on a false premise, as each simulation was never intended to represent a specific accident outcome. For this to be done, a semi-infinite number of input variables would have been needed, e.g., the position of each rock, tussock, 3D terrain feature, as well as complete steering, braking, throttle, and body positioning time histories of the rider, as well as rider helmet use, stature, weight, girth, age, exact path and speed along the terrain before the rollover, etc., etc., etc.

Instead, the goal was to define 59 general "types" (also referred to as "configurations" or "scenarios") of HSE-reported overturns, within which each of the 59 UK/HSE cases reside; and 54 general "types" of CPSC reported overturn, within which each of the 54 US/CPSC cases reside.

7. ***"Of the 59 cases, 10 have quad bikes operating on slopes of 45 degrees, a physical impossibility, and another 11 are crossing a slope of 30 degrees sideways (heading within 30 degrees of being directly across slope) which is also close to impossible based on friction and stability calculations"***

This comment is not accurate and somewhat misleading, as seven (rather than 10) of the general types of HSE-reported overturns that were modelled (for which there was one HSE case each) involved ATV's “overturning on” (rather than "operating on" (e.g., in a steady state manner)) steep or 45 degree slopes. For these seven types, five of the associated HSE cases explicitly report slopes of 40 to 45 degrees, and one explicitly reports a "steep" slope. All seven slopes were defined in the simulation models to be 45 degrees because: 1) 45 degrees produced a highly likely dynamic overturn (as overturns were known to have occurred in all of the example HSE cases); and 2) all slopes used in the simulation models (with the exception of the loading "ramp"[2]) were rounded to the nearest 15 degree increment, in order to simplify and to minimize the number of types of terrain modelled, and in order to provide a more “rounded” (i.e., standardized) value (i.e., analogous to the way in which the impact angles associated with each general type of impact have been rounded to the nearest 45 degree increment in ISO 13232).

There are numerous cases in ATV accident files which involved riding onto and down slopes steeper than 45 degrees (e.g., falling into culverts, the walls of which are vertical, etc.). It is misleading to claim that it is impossible to “operate” on such a “slope”, when it is not “operating on” but rather “encountering” or “overturning on” such surfaces that is the relevant phenomenon.

For 30 degree slopes, seven (rather than 11) of the general “types” of HSE-reported overturns that were modelled (for which there was one HSE case each) involved ATV's overturning on 30 degree slopes. For these seven types, two of the associated HSE cases explicitly mention cross slopes of 24 to 30 degrees, two mentioned overturning sideways on a bank or hillside, and one mentions overturning on “hilly” ground. In general, ATV overturn is more likely to be in the lateral direction than in the fore or aft directions, and in the absence of other inputs, is more likely to occur on slopes of 30 degrees than on slopes of 15 degrees. All seven of these intermediate slopes were defined as 30 degrees because: 1) 30 degrees gave a more certain overturn in the lateral direction than would 15 degrees, as overturns were known to have occurred in all of the example HSE cases; and 2) as in the case of 45 degree slopes, all slopes (with the exception of the loading "ramp") were rounded to the nearest 15 degree

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[2] A truck loading ramp, which appears in several HSE and CPSC cases, was modeled to have a slope which was midway between 15 degrees and 30 degrees as, for typical portable loading ramps suitable for utility trucks, 15 degrees produces a ramp which is unrealistically long; and 30 degrees produces a ramp which is too steep.

increment, in order to simplify the number of types of terrains modelled, and to provide a more rounded (i.e., standardized) value.

This comment is also based on a false premise regarding the purpose of the simulations. The actual purpose of the simulations was to evaluate the effects of ROPS/CPDs on overturn consequences (i.e., post-overturn motions and injuries), and not to evaluate the effects of ROPS/CPDs on ATV operation or overturn causation. This is because overturn was a "given", for the baseline (non-ROPS/CPD) ATVs, as overturns were observed to have occurred in all 59 of the HSE cases. An overturn was therefore modelled to occur in all of the associated general "types" of overturn. A different type of model, and suitable calibration tests, focusing on the differences between non-overturns and overturns, the details of rider control inputs and body position, more detailed terrain conditions and other aspects would have been needed to assess overturn causation, which this comment and some of the other comments seem to be focused on, but which was not the purpose of the DRI studies.

C. PREDICTION OF INJURIES (MCDONALD AND RICHARDSON)

1. ***"Assessment of the 113 cases comparing the actual injuries with those predicted by the simulation indicates that <10% of these are valid from the view of:"***

This introductory comment is based on a false premise, as it wrongly assumes that the goal was or should have been case-by-case matching of injuries for large collections of cases, whereas such a goal exceeds the state-of-technology worldwide, as well as the content of the available accident data.

Matching the "aggregated" injury distribution (between modelled distribution and the actual distribution) is the state-of-technology in safety research, as illustrated for example by Kuchar (2001) in Figure 1 below.

The latest DRI results, reported in Zellner, et al. (2012), as shown in Figure 2 and Table 1, indicate a close agreement between modelled and actual aggregated injury distributions (correlation coefficient r^2 = 0.97 on average, and in excess of 0.99 for some body regions, between modeled and actual frequency of injury, across all body regions and overturn cases/types, which indicates very close agreement). This is despite the fact that the simulated aggregated injury distribution and the actual aggregated injury distribution, though generally very similar, fundamentally would not be expected to be identical because of the inherent dissimilarities in human, vehicle, environment and accident variables between the simulation sample of general "types" of overturn (for which all of the simplified details are known), and the sample of much more complex actual accidents (for which many details are unknown).

Table 1. Correlation Coefficients between Actual and Simulated aggregated Abbreviated Injury Scale (AIS) Injuries, Unhelmeted, DRI Latest Results

Body Region or Index	Correlation Coefficient
MAIS	0.880617
Head	0.940405
Neck	0.994150
Chest	0.996379
Abdomen	0.999619
Femur	0.999267
Knee	0.996978
Tibia	0.999438
Asphyxia	0.999558
Aggregate	0.974958

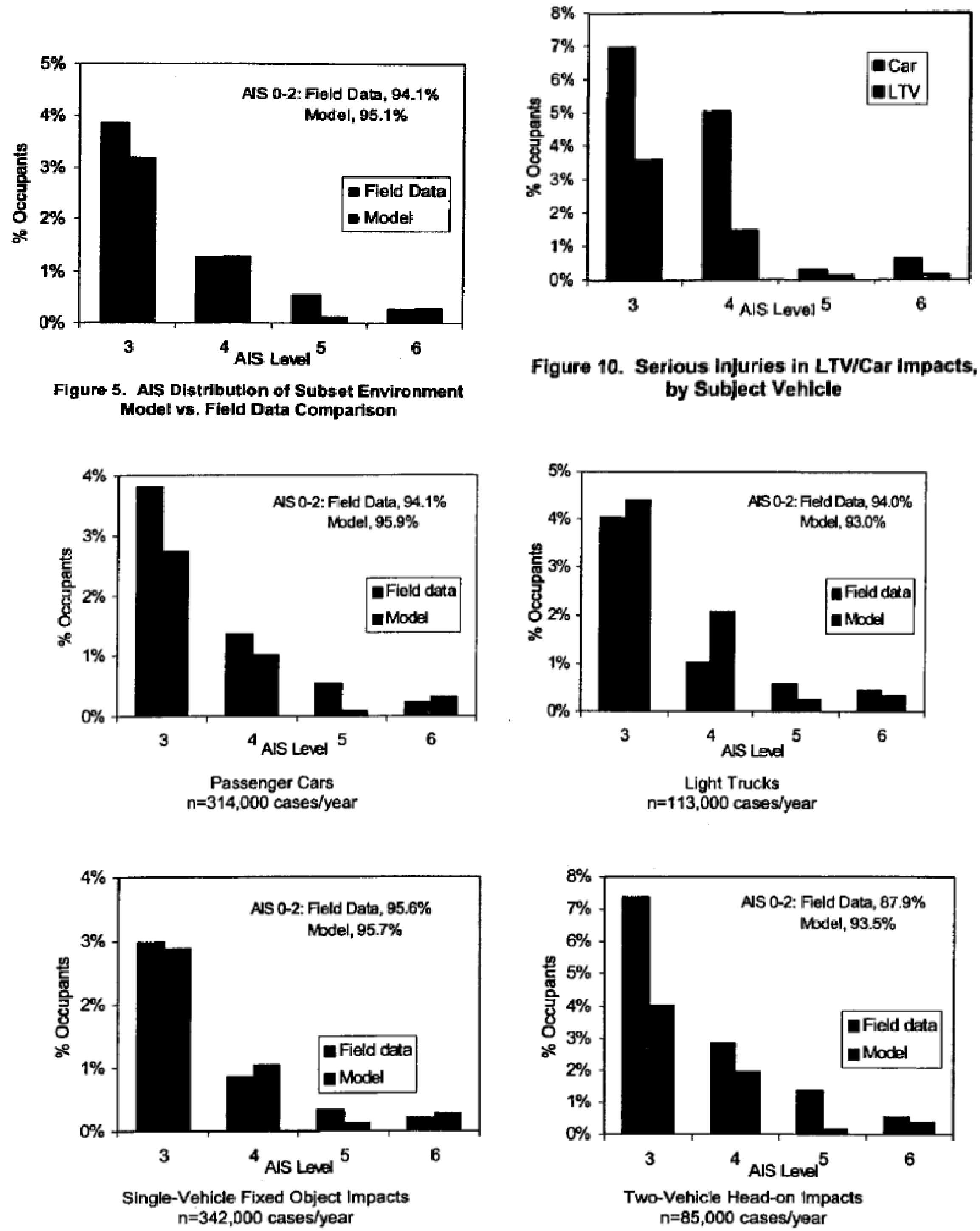


Figure 1. Example distributions of modeled versus actual injury severity, from Kuchar (2001)

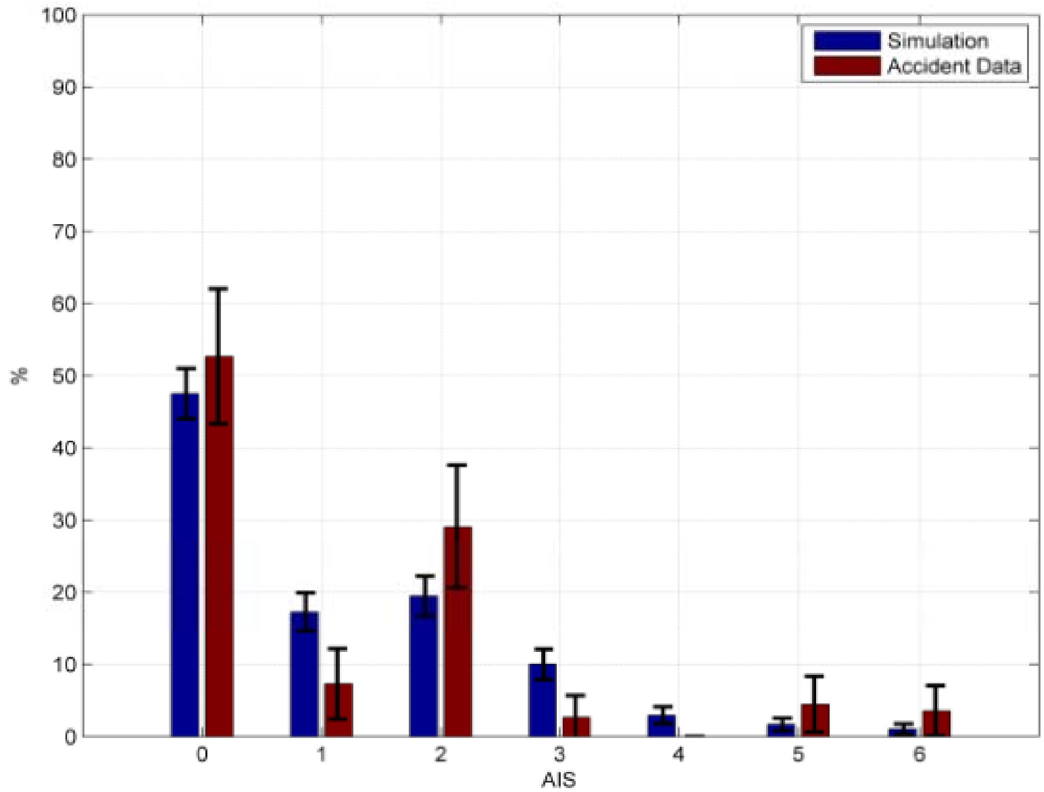


Figure 2. Example comparison of injury severity distributions from DRI's most recent simulations, Baseline ATV (simulation assuming a helmeted dummy in this example)

2. *"Categorising injury according to severity (using the Abbreviated Injury Scale - AIS)*

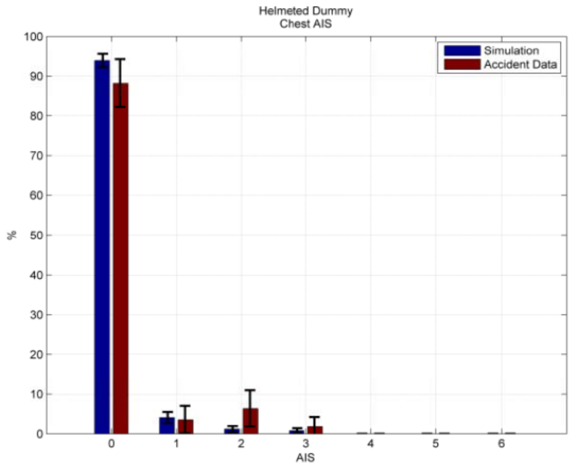
a. The ratio of trunk to head injuries is inexplicably modified
i. For Critical AIS 5& 6, the actual is 3: 1 (75%); in the simulation it is 0: 25 (0%).
ii. For all AIS 1-6, the ratio of the actual data is 2.4: 1 (41%); in the simulation it is 1: 25 (4%)."

This comment is based on a false premise, as described in the reply to Comment C.1.

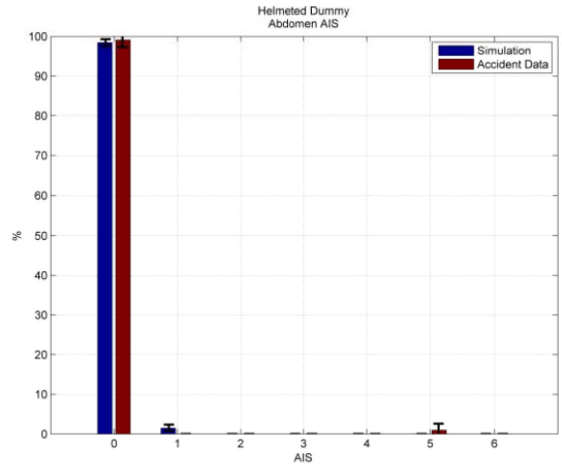
Case-by-case matching of individual injuries for large collections of cases exceeds the state-of-technology, as well as the state of available accident data required for such finely detailed modelling. The commentators are attempting to establish an impossibly high criterion which no one in the injury simulation field has ever achieved, and which is beyond the state of technology. As noted in the reply to Comment C.1, aggregated injury distribution matching is the current state of technology.

Fundamentally, one would not expect a close match when comparing the injury in each overturn case with the injuries in each overturn "type", as each case involves 17 variables, some or many of which are unknown, whereas each overturn "type" involves 17 variables which have been set to a value which is either not inconsistent with the corresponding case variables (to the extent the latter variables were recorded) or is a plausible typical value. More importantly, in the specific overturn case, there are a semi-infinite number of other unknown, unmodelled variables that are likely to have influenced the injuries in the individual cases, which cannot be modeled in the simulation. Case-by-case matching of injuries as between models and actual data in large samples of accident cases is beyond the state of technology, and no examples of it are found in the published literature.

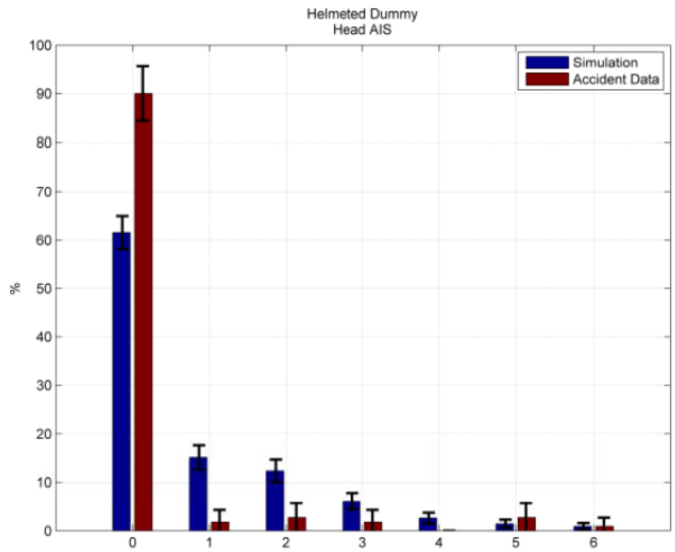
Again, matching the aggregated injury distribution (between modelled and actual accidents) is the state-of-technology in safety research, as illustrated for example by Kuchar (2001). The latest DRI results indicated a close agreement between modelled and actual aggregated injury distributions for body regions, as illustrated in Figure 3.a and 3.b (for trunk injuries), Figure 3.c (for head injuries) and in all three Figures for AIS 5 and 6. Note that AIS 1 head injuries (e.g., headaches) are well known to be under-reported in vehicle accidents. Also note that other categories of trunk injury (e.g., thoracic spine and lumbar spine injuries at various AIS levels) are beyond the state of technology for injury monitoring crash dummies, and must therefore be excluded from such comparisons.



a) chest injuries



b) abdomen injuries



c) head injuries

Figure 3. Example comparison of injury severity distributions from DRI's most recent simulations, Baseline ATV (simulation assuming a helmeted dummy in this example)

3. ***"Categorising injury according to body part by AIS: - The trunk's percentage of injuries, (AIS 1-6) changes from 71% (actual) to 4% (simulation). - The percentage change in head injuries (AIS 1-6), is from 29% (actual) to 96% (simulation)."***

See replies immediately above. The latest DRI results in Zellner, et al. (2012), which include the effects of measured (rather than estimated) head-, limb- and body-to-soil friction coefficients, indicate a close agreement between modelled and actual aggregated injury distributions, as illustrated in Figures 3.a and b (for trunk injuries) and Figure 3.c (for head injuries).

This comment greatly overstates the number of relevant trunk injuries in the UK/US sample as it includes many trunk injuries that are not able to be monitored by contemporary crash dummies (e.g., thoracic spine and lumbar spine injuries, and other injuries). Although of course it may be desirable to monitor such injuries, it is simply not technically possible to do so at the current time. Figure 3 properly excludes those injuries that are beyond the current state of crash dummy monitoring technology.

4. ***"Categorising body part injured according to a comparison of Actual and Simulation Predictions***

   a. ***Head only injury increased from a total of 17 (actual) to 99 (simulated) i.e.5.8 times increase***
   b. ***Trunk injury reduced from 50 (actual) to 4 (predicted) i.e. a 12.5 times decrease.***
   c. ***Face injury increased from 7 (actual) to 27 (predicted) i.e. a 3.8 times increase."***

   See replies immediately above. The latest DRI results indicate a close agreement between modelled and actual aggregated injury distributions, as illustrated in Figures 3.a and b (for trunk injuries), and Figure 3.c (for head injuries).

5. ***"The simulations are based on ISO 13232 which is applicable to testing for motorcycles (specifically impact tests), however these guidelines give no process for simulating rollovers..."***

   This comment is true, but somewhat misleading, and the reasons for applying some (but not all) of the ISO 13232 procedures to ATV overturns have already been extensively described in the DRI reports.

   It is true that ISO 13232 is for motorcycles. There is currently no such impact research standard, or rollover research standard, for ATVs. However, ISO 13232 is the only standard, worldwide, for which there is a crash dummy compatible with straddle seat, handlebar vehicles where the rider may be helmeted, and where the rider is subject to potential injuries from any and multiple directions. Both motorcycles and ATV's are in this category. None of the existing car test methodologies and crash dummies are suitable or able to address these areas.

   It is also true that ISO 13232 methods "give no process for simulating rollovers", and DRI has therefore developed an extension of the methodology, described in numerous documents and in use since 1996, for analyzing ATV rollovers. The extension involves the same general accident analysis and categorization methodologies as were used in ISO 13232 to define, in that instance, a generalized set of motorcycle impact configurations, in order to define, in this instance, a generalized set of ATV overturn configurations. More specifically, in both instances, a set of specific accident cases, which involve limited descriptive data, were used to define a set of generalized "types" of testable event.

6. ***"(including crush / asphyxiation), which account for 50% of the fatalities."***

This comment is not accurate. Also, its subject matter has been addressed in subsequent research.

As for "crush" injuries, ISO 13232 (2005) contains indices describing chest compression (up to AIS 6 level), and abdominal penetration (up to the limits of the AIS scale for this injury type) which are two categories of injury directly related to internal organ "crush", as described in the respective literature on those measurements. In addition, DRI adopted in its ATV ROPS/CPD analysis published in the Munoz, et al. (2007) report on the V-Bar (aka Quadbar) and in the Zellner, et al. (2008) FISITA paper and in its most recent Zellner, et al. (2012) simulations, indices for skull fracture/crush and for face fracture/crush, as recently published by Van Auken, et al. (2011). Other "crush" indices, though they might be desirable, are beyond the current state of crash dummy technology.

As for "asphyxiation" and associated biomechanical injury criteria, there is very little (if any) scientific literature on this topic, worldwide. Nevertheless, DRI has, since the Victorian Coronial inquests, investigated this area, and has formulated a preliminary and conservative potential asphyxiation (or, more specifically, a "breathing difficulty") criterion, and has applied this in its most recent Zellner, et al. (2012) simulations of the Quadbar.

The latest simulation results indicate that including this preliminary asphyxiation index, as well as the aforementioned crush indices, does not change the overall conclusions about the Quadbar, and in fact, for example, the Quadbar was found to cause approximately as many potential "breathing difficulties" (i.e., 11 in 770 overturns) as it prevents (i.e., 10 in 770 overturns), as illustrated in, e.g., Figure 4.[3] In Figure 4, which is one of 11 examples where there was potential asphyxiation/breathing difficulty when the Quadbar was fitted, the ATV with Quadbar comes to rest on its side on the dummy's chest.

[3] This rate of breathing difficulty for the simulated baseline ATV (i.e., 10 per 770 overturns) is somewhat similar to that observed in the original UK/US cases (i.e., 3 per 113 overturns) although the latter involved one highly intoxicated adult where the tyre came to rest on the face, and two children, who may have had very different chest force tolerances from that used in the assumed asphyxiation criterion.

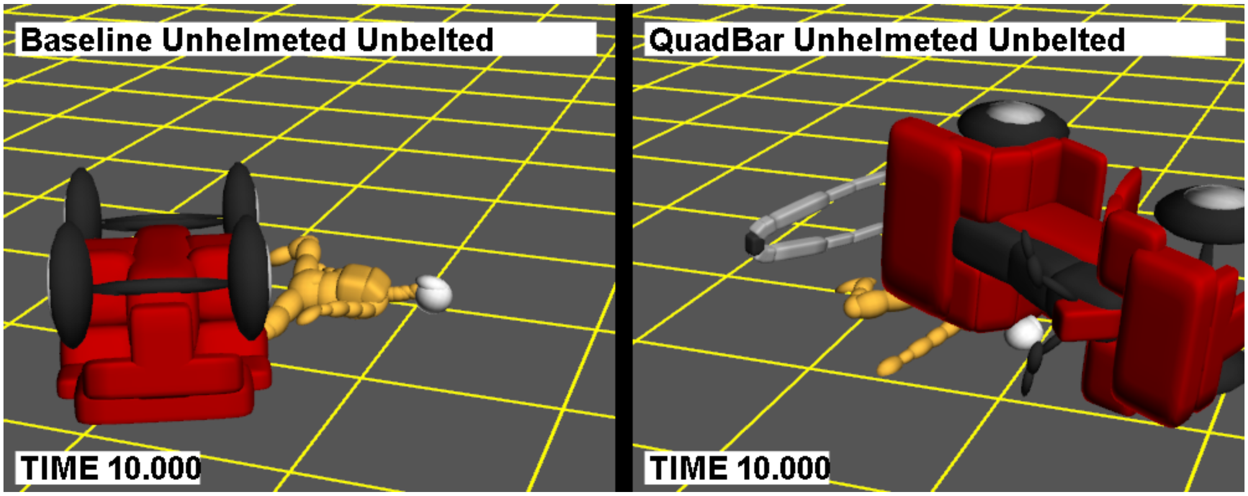


Figure 4. Example still image of QuadBar CPD injury risk. Potential asphyxia (i.e., predicted "breathing difficulty") occurred with Quadbar. Baseline ATV in same overturn had no monitor-able injuries

7. ***"The summary of injury predicted by the simulation of the UK/US cases in terms of the actual injuries, shows a poor relationship and a severe bias increasing emphasis on the head and decreasing, virtually eliminating, trunk injuries."***

   This comment, to the extent it is referring to a case-by-case comparison, is based on a false premise as discussed above. If, on the other hand, it is referring to aggregated injury distributions, it is not accurate and, moreover, now obsolete as the latest DRI results in Zellner, et al. (2012), which have taken into account all of the known, valid comments of reviewers in making the methodology updates, extensions and refinements, indicate a close relationship between the modelled and the actual injuries, for all body regions and injury severities, as illustrated in Figures 1, 2 and 3.

D. COMPUTER MODELLING (LAMBERT)

1. ***"The writer was provided with two videos showing modelling a UK HSE quad bike cases, and modelling the performance of a quad bike with and without a Quadbar fitted. The writer reviewed these and found the following."***

   This comment and the next five comments ~~appear to be related to are statements~~ were made in a ~~draft~~ report by Lambert (201~~0~~1a). ~~Version 4 of that report, dated August 2011, was requested from Mr. Lambert, but as of the date of this current report, he had declined to provide it. Comparison of the still images from the Lambert (20101a) report with animations that had been previously provided by DRI to others facilitated the identification of the specific simulation images discussed in the latter report, and which~~ That report, however, did not specify which simulations were depicted in the videos obtained by Mr. Lambert, and he subsequently declined to provide a copy of or any details of the videos. DRI has since identified the videos from comparison with still images included in Lambert (2011a), and those identified videos and associated simulations form part of the bases for the following replies.

2. ***"In a situation where a quad bike is travelling up a slope, when the quad bike hits an object, instead of the torso being forced forward against the handle bars, the rider is thrown backwards;"***

This comment appears to refer to a baseline simulation from an evaluation of a MUARC ROPS, which is located in a section entitled "Video Evidence – Part 1 – Modelling – Case 1" in Lambert (2010).

This comment is based on a false premise as it wrongly assumes that the only input to the ATV is the "object", and it ignores the large, sudden throttle input to the vehicle, which is described in the table given in, e.g., Zellner, et al. (2004), Appendix E.

In simulations (and in dummy tests and in real world situations) that are similar to this video description, the dummy (or rider) can be accelerated forward, or alternatively the dummy can be "thrown backwards", depending on the relative magnitudes (and shapes) of the obstacle, the terrain and the control inputs that are involved. Of course, the dummy (or rider) can be accelerated forward by impacting a vertically oriented surface or obstacle. Alternatively, the dummy (or rider) can be "thrown backwards" by: a) the front wheels of the ATV rising upward and rolling over large, rounded obstacles; b) a sudden wide open throttle input (which was used in most of the simulations involving this general type of overturn, and in the corresponding calibration tests, in order to induce ATV pitch-up, which was involved in the example HSE cases) which accelerates the ATV forward and tends to throw the rider backward relative to the ATV; and c) gravity, as the ATV itself pitches nose-up.

The dummy motions that are described are as would be expected and observed in a situation where the throttle is suddenly opened, and the ATV front wheel rises up and over a rounded object. The dummy is demonstrably not thrown forward in these situations, but rather it tends to slide rearward.

3. ***"When the 250 kg quad bike lands on the riders back with his body supported on its arms and legs, the rider does not crumble to the ground but successfully supports the quad;"***

This comment appears to refer to a baseline simulation from an evaluation of a MUARC ROPS, which is located in a section entitled "Video Evidence – Part 1 – Modelling – Case 1" in Lambert (2010).

This comment is not accurate and in any case is irrelevant, as the effect described is exceedingly rare among the thousands of preliminary and refined simulations run since 1996, and it was not observed among any of the runs used to evaluate the Quadbar.
Although what is described in this comment is extremely rare in DRI's simulations or tests, it occurred in this case because the dummy's torso is inverted and perpendicular to the ground, and at this point in the simulation is in compression, due to contact to the buttocks from above by the ATV, and contact to the shoulders from below by the ground. The actual MATD dummy's thoracic spine is rigid, and the lumbar spine, though more flexible than the Hybrid III car dummy spine, is relatively stiff in bending, which means the dummy spine can support a vertical load, and this is reflected in the model. In this particular case, the dummy's arms and legs were not supporting the ATV in any way, and the dummy's spine only supports the ATV for a period of approximately 0.060 seconds (roughly the time of an eye blink). Therefore, this comment is not accurate.

4. ***"With the quad bike on top of them, the rider manages to roll over onto their back without significant relative movement between the riders body and the quad bike;"***

This comment appears to refer to a baseline simulation from an evaluation of a MUARC ROPS, which is located in a section entitled "Video Evidence – Part 1 – Modelling – Case 1" in Lambert (2010).

This comment is irrelevant, as the effect described was not observed in the Lambert (2010) images, or among any of the simulations. In the actual Lambert (2010) images and in the corresponding simulation, the dummy does not pitch rearward onto its back until the ATV separates from the dummy.

5. ***"With the rider prone. Their head is observed bouncing on the ground at a rate of 4 cycles per second, a physical impossibility;"***

This comment appears to refer to a baseline simulation from an evaluation of a MUARC ROPS, which is located in a section entitled "Video Evidence – Part 1 – Modelling – Case 1" in Lambert (2010).

The statement that this is a "physical impossibility" is not accurate as the simulation is modeling the physical dummy, and the physical dummy head/neck assembly has many vibrational modes (i.e., ISO 13232-5, Annex P, Figures P.2, P.4, and P.6), including one vibrational mode at approximately 4 cycles per second.

While earlier simulations occasionally exhibited this vibration, which occurred when the dummy head encountered relatively high values of head/soil friction coefficients used in those earlier analyses (i.e., which had previously only been estimated), in the most recent Zellner, et al. (2012) simulations, which used lower, measured (rather than estimated) values of head/soil friction coefficients, this phenomenon is not apparent.
In any case, the physical (MATD) dummy closely matches the head-neck response of a live volunteer Navy test subject who was subjected to large forward, rearward and lateral accelerations, as indicated in e.g., ISO 13232-3 (2005) and in Withnall, et al. (2003).

The vibrational modes of a live human's head and neck in such a situation are unknown, and are not relevant. To state that a 4 cycle per second vibration is an "impossibility" is not accurate if it is referring to the physical dummy on a high friction surface as described above; and it is not relevant if it is referring to a human, as it is unethical to conduct rollover tests with live humans - with the "head…bouncing on the ground" - and it is not possible to validate and calibrate a simulation model of a live human in such a situation.

6. *"The quad bike is observed "'bouncing down a slope like a beach ball';"*

This comment appears to refer to a baseline simulation from an evaluation of a MUARC ROPS, which is located in a section entitled "Video Evidence – Part 1 – Modelling – Case 1" in Lambert (2010).

This comment is not accurate and somewhat misleading, as in no cases do the simulated ATV's "bounce down a slope like a beach ball". This is descriptive of an object with very low mass and density, and for which aerodynamic forces can have a relatively large effect. This is not the case for ATVs, or for DRI's simulation models of ATVs. In addition, the ATV's limited "bouncing" down the slope in this particular simulation occurs long after the rider has separated from the ATV, and it has no effect on rider injuries.

ATV's (as well as cars, light trucks and dune buggies) have been observed and filmed in full-scale tests to "bounce" to some extent as they undergo overturning motions, particularly on steep slopes and/or at high speeds, but not "like a beach ball". The extent of bouncing is governed by the mass of the ATV itself and the force-deflection characteristics of the particular soil and the various ATV components, including the tyres, seat, handlebars, ROPs, etc. These characteristics, used in the simulation model, have been directly measured and/or dynamically matched based on high speed digitized films of actual full-scale tests of ATV overturns.

The most recent version of the simulation model as described in Zellner, et al. (2012), which involves “low energy” overturns, exhibit relatively small amounts of “bouncing”, and no "bouncing... like a beach ball". This reduction in “bouncing” did not have a statistically significant effect on Quad Bar injury risks and benefits for the (helmeted condition) population of overturns. This comment is therefore not relevant.

7. ***In identical scenarios of a quad bike travelling up a slope and hitting an obstacle: i. With a Quadbar not fitted the rider is propelled backwards down the slope even though with the same momentum as the quad bike, they should continue to travel up the slope; and ii. With a Quadbar fitted, through an inexplicable mechanism when the obstacle is hit and the rider’s torso should be propelled into the handlebars, the rider’s torso is inexplicable thrown backwards and their head becomes jammed in the Quadbar."***

This comment appears to refer to simulations related to the Munoz, et al. (2007) evaluation of the Quadbar.

As stated in the reply to Comment D.2, the first portion of this comment is somewhat misleading as it is based on a false premise, as the dummy *can* be "thrown backwards" by: a) the front wheels of the ATV rising upward and rolling over a rounded obstacle; b) a sudden throttle input (which was used in most of the simulations and tests in order to induce pitch-up, which was involved in the original HSE cases) that accelerates the ATV (but not the dummy) forward, causing the dummy to tend to move rearward on the ATV; and c) gravity, as the ATV pitches nose-up. These dummy motions are as would be expected in a situation where the throttle is suddenly opened, and the ATV front wheel rises up and over the object.

Both the first and second portions of this comment are somewhat misleading, as they are based on a false premise in that the dummy is demonstrably not thrown forward in these situations. The mechanism is what would be expected given the wide open throttle input and it is not "inexplicable". In addition, real riders have been known to slide off the back of ATV's in some pitch-up situations.

8. ***"In summary the DRI quad bike output videos display quad bike and rider behaviour that defies physics and or reality. This reflects that the computer simulation model outputs are of no value."***

This comment is not accurate and it is unsupported. As explained in the immediately preceding replies, the motions (including in particular those in the most recent Zellner, et al. (2012) ATV simulations) do not defy physics or reality in any way.

In fact, by definition, the motions cannot defy physics, as the simulations are based solely on Newtonian physics, as implemented in the US Air Force/US Department of Transportation Articulated Total Body Simulation that was used by DRI. That simulation has been in existence and has been widely used and validated by many researchers during the last 40 years, and is a predecessor of the widely used MADYMO commercial simulation software (which Richardson's company, DV Experts, routinely uses). Similarly, the input parameters do not "def[y] physics" (as could theoretically be the case if, say, a negative damping coefficient was used in the model, which it was not), as they were values based on measured or estimated mechanical characteristics.

In addition, the output videos have been mathematically compared with and correlated against digitized high speed films of real ATV/dummy overturn tests, and have been found to be in very close agreement (e.g., r^2=0.91 correlation coefficient), so they are in close agreement with, rather than "def[ying] physics or reality".

Further, the 113 "types" of overturn (based on the 113 UK/US cases) have recently been revised to reflect "low energy" overturns, and to address and remove other small anomalies. None of the most recent simulations used to evaluate the Quadbar have any unusual or inexplicable motions.

Finally, this comment is somewhat misleading and not relevant; as, for the helmeted (intended use) condition, the most recent simulations and analyses do not produce injury risk/benefit percentages that differ in a statistically significant way from the previous results, and the overall conclusions based on those results have not changed as a result of DRI having addressed all known comments.

E. CALIBRATION VIDEOS (LAMBERT)

1. ***"The writer was provided with a number of actual videos showing actual tests of quad bikes with Hybrid III dummies on board. The videos included spoken commentaries. The writer reviewed three of these to some degree and found the following."***

This comment is not accurate as the videos were of an ISO 13232 MATD dummy, not a Hybrid III dummy. A Hybrid III dummy cannot sit astride a straddle seat, cannot have realistic head and lower back posture on a motorcycle or ATV, cannot grip the handlebars and cannot properly retain a motorcycle helmet or ATV helmet.

2. ***"In respect of side stability calibration on a slope: To get a quad bike to do the impossible and sit stationary sideways on a 45 degree slope, DRI personal tethered the quad to the slope;"***

This comment is somewhat misleading as it is based on a false premise: it is *not* “impossible” for an ATV to be at rest sideways on a 45 degree cross slope if, for example, the rider shifts his buttocks and/or torso uphill, and/or there is significant soil penetration, ruts, berms, tussocks, rocks, stumps or other objects pressed against the downhill sidewalls of the ATV tyres, or the ATV body itself.

Use of a substantially smaller cross slope for this condition, as perhaps impliedly proposed by this comment, or somehow dynamically leaning the dummy downhill, would have produced less repeatable ATV overturns and dummy motions in testing, would have required other inputs to the vehicle, would have been beyond the state of crash dummy technology, would have been outside any dummy positioning standard that exists and would likely have had adverse effects on experimental variability.

The goal was not to get the ATV “to do the impossible”, but to set up and initiate in an reasonably repeatable manner for test purposes (for this general type of rollover) a stationary rollover event, in both the computer simulation and in the corresponding full-scale rollover test (which was used to calibrate the simulation) that would not require a robotic rider, or some other capability or input that would be either unrealistic (e.g., a catapult or a rigid ramp) or beyond the current state of technology.

This comment is also based on a false premise, as the purpose for doing the simulations and tests was not ATV “side stability calibration”, but, rather, “post-overturn motion calibration” of the ATV and dummy, as well as observation of the effects of various devices on predicted injuries, for the given overturn event.

In general, when an ATV with 50^{th} percentile male dummy in a centered position is placed on a 45 degree slope (which was a convenient, geometrically rounded reference value), with a heading angle that is pointed directly across the slope, for most ATVs, a rollover event is generally "highly likely" to occur (in the absence of significant dummy body shift uphill, soil penetration or any object (e.g., rocks, vegetation, etc.) pressing against the downhill sides of the tyres, or any other inputs to the vehicle). A tether was merely a simple means to represent the effects of such shifts, penetration or pressing, which if modelled or tested would have been more complex in nature.

On the other hand, a 30 degree slope (which provided a second, convenient, geometrically rounded reference value used to simulate other "types" of ATV overturn) may or may not produce a reliable rollover event in a full-scale test, depending on many factors including details of dummy size, weight and positioning, extent of soil penetration or adjacent objects, the specific ATV used, presence or absence of a ROPS or CPD, and so on. As a result, it can be said that an ATV may be "conditionally stable" on a 30 degree cross slope; and rollover on that slope may or may not require other inputs such as forward motion, steering, braking or throttle inputs, heading angle and track angle across the slope, or "lean downhill" body motion (the latter of which is not possible to repeatedly reproduce with current crash dummy technology).

Note that for all of the overturn simulations and for all of the full-scale overturn tests, "rounded" slope angle values were used to represent the different general types of overturn condition. For standardization purposes, these were initially chosen to be in 15 degree increments (i.e., 0, 15, 30, 45 degrees). This grouping of slope angles is analogous to the grouping of impact angles used in ISO 13232 (which uses 45 degree impact angle increments). Generally, with such groupings, the simulation outcomes (or test outcomes) can be considered to be generally representative of the "type" of overturn that may occur for a range of angles about the specified angle, i.e., in the case of 15 degree increments, $\pm$ 7.5 degrees around the specified angle. Generally, larger vehicle inputs (i.e., steering, throttle, braking, obstacle size or ground roughness) are required to induce a rollover at the smaller slope angles, and conversely for the larger slope angles (i.e., not much vehicle input is needed for overturn to occur on larger slope angles).

Of course, as with any off-road vehicle, operators can attempt to ride an ATV over slopes even steeper than 45 degrees, which if maintained for more than a very short time interval, has a high likelihood of resulting in an overturn. (Stunt riders or other expert riders, who through use of momentum and other means, may briefly climb, traverse or descend very steep (even vertical) slopes or steeply banked curves, are not the focus here).

Note that for the actual specific HSE reported accident associated with this "stationary steep cross slope" overturn type (i.e., HSE Case 62), which was one of the 113 types of overturn that were simulated, it was reported that the ATV was "parked across a steep slope" and that the ATV overturned on the rider as "he leant over to access [a] toolbox", which downhill leaning, again, is not something that can be feasibly or repeatedly recreated with existing crash dummy technology. Note that many or most of the other variables were unknown for this specific accident, i.e., the actual slope angle, the rider's size, weight and positioning, the extent of soil penetration, whether adjacent objects (e.g., ruts, rocks, vegetation) were pressing against the downhill sides of the tyres, etc. As discussed previously, the full-scale tests and simulations were not intended to recreate specific accidents, about which many or most details are unknown; but rather to recreate a repeatable general "type" of overturn, similar to the one for which a few details are provided in the HSE (or CPSC) case file.

Therefore, to represent this general type of “stationary/steep cross slope” overturn, a suitably steep and rounded value (i.e., standardized angle) cross slope was used (i.e., 45 degrees) that would result in a reliable overturn under these conditions; the vehicle was initially tethered (which represents in the real world factors such as an uphill rider body position, ruts, soil penetration, adjacent objects pressing against the downhill sides of the tyres of the vehicle, etc.); and the tether was released to initiate what is a reasonably repeatable overturn event (representing in this particular example HSE case, the rider leaning downhill). In effect, this same procedure was used in the full-scale test and in the corresponding computer simulation. A lesser slope (e.g., 30 degrees) could have been used, but overturn occurrence and motions would have been more influenced by (i.e., conditional on), e.g., the exact position of the dummy, exact soil condition and soil penetration, presence/absence of ROPS/CPD, etc., and would have been less repeatable and reproducible.

3. ***"The Hybrid III dummy was positioned as expected for level ground – that is the torso was perpendicular to the seat when a real rider would be leaning up the slope to maximise stability;"***

This comment is somewhat misleading.

First, a real rider perhaps would also have shifted his buttocks uphill on the seat, as well as leaning his torso uphill. Second, it implies that it is an easy matter to lean or shift the dummy position uphill. However, this is not easy to do, and no standardized, repeatable method exists for doing so. As those familiar with crash dummies are well aware, in order to run a reasonably repeatable test, the dummy (whether it is MATD or Hybrid III) has <u>dozens</u> of joints, each one of which typically has several degrees of freedom, and each one of which, before a test, must be (e.g., according to the detailed dummy positioning procedures in ISO 13232, or the applicable standard for all other crash dummies): a) adjusted and calibrated in tension; b) adjusted and measured in terms of its initial pre-seated angular positions (i.e., there are multiple rotations in many of the joints, and the rotation angles of each portion of the joint must be set to a reference value before placing the dummy on the seat; and, c) flexed to a specified position or angle in the process of placing it on the seat, footrests and handgrips, in accordance with specific rules and measurements, all with an approximately 80 kg dummy. ISO 13232 provides elaborate, multi-annex, multi-page procedures for suspending and then lowering the MATD dummy into a centred position on a motorcycle that can be applied to other straddle seat, handlebar vehicles such as ATVs, and this rigorous, standard and repeatable procedure was used in each of the ATV full-scale overturn tests. This process for a centered seating position can typically require approximately one hour per test to complete. No standardized test procedures currently exist for positioning the dummy in an offset, uphill leaning and uphill shifted riding position.

Moreover, the UK/HSE investigator reported that the rider leaned downhill prior to the ATV overturning. Therefore, it is unclear whether positioning the rider uphill, or centred, or downhill would be most relevant for this particular test for it to be considered to represent this "type" of overturn event. DRI did not want to seek to invent new standardized procedures and technology for this limited series of tests, on an issue that was not so clear and for which no test procedures exist, so it elected to position the dummy using the existing standard ISO 13232 centred method, which is standardized and repeatable.

In addition, this comment is of uncertain relevance, as no data exist indicating that there would be a statistically significant effect on injuries, or on ROPS/CPD injury risk/benefit percentage, if such dynamic rider lean effects were somehow included (though including them is not considered technically feasible at the current state of technology).

4. ***"When much of the weight of the quad bike lands on the hybrid dummy, the spoken commentary advises that no injury was recorded."***

This comment is somewhat misleading as it implies that the video's spoken commentary is somehow inaccurate or implausible. However, ISO 13232 monitors for approximately 17 locations, types, severities and directions of injury, and in this particular test, there were in fact no (AIS 1 or greater) predicted injuries recorded by the dummy in any of these locations, types and axes of injury, so the narrative is accurate.

This is not to say that additional locations, types and axes of injury would not occur in a human rider in these circumstances, but this is unknown, unknowable and fundamentally not a repeatable or reproducible outcome. At the current time, crash dummy technology (including sensors and injury criteria) does not exist for recording and predicting other locations, types, severities and directions of injury.

5. ***"In respect of limits to stability calibration for travel up a slope:…"***

This comment is somewhat misleading, as it is based on a false premise.

The purpose of the calibration tests was not "limits of stability calibration" but, rather, "post-overturn ATV and dummy motion calibration". See discussion above and below.

6. ***"a. The video clearly shows the limit to stability under full throttle was in the range 32-34 degrees, a value that is much less than the 45 degrees DRI assumed in 10 modelled cases;"***

This comment is false and highly inaccurate.

The videos that this comment are based upon, and videos in general, can distort the aspect ratio of the image and the relative vertical and horizontal scaling of the image, and therefore cannot be relied upon for estimating terrain slope or ATV pitch angle. In addition, the video camera perspective angle cannot be accounted for in the video.

The digitized high speed film data, which were recorded with a camera positioned to be essentially perpendicular to the position of overturn initiation, which were calibrated and reduced frame by frame, and which are presented in Van Auken et al. (1998), Volume III, Annex 7, pages 10, 11 and 13, indicate that the slope of the ATV wheel hubs' path (which followed the ground slope as the ATV travelled up the slope) at the time the front wheel lifted off the surface, ranged between 43 and 45 degrees for the three full-scale tests being discussed. This is very close to the 45 degrees used in the modeled cases.

7. ***"b. The surface is obviously neither a uniform slope or a uniform surface as evidenced by its increasing slope and the bounce of the quad bike;"***

This comment is mostly not accurate and, in any case, it is not relevant.

Both the slope and the surface were virtually uniform in the region of the pitch-up event, though the slope angle further down the hill was less than where ATV pitch-up occurred. In addition, the initial so-called upward "bounce" of the ATV front wheels, said to be visible in the test video, was not a "bounce" at all, but rather the response of the ATV to a step-like, wide open throttle input, intended to initiate the ATV pitch-up and rearward overturn. This is documented in the Van Auken, et al. (1998) test report.

The physical surface varied from an initial nearly level slope at the bottom of the hill (where the dummy was positioned on the stationary ATV using ISO 13232 procedures) and it gradually increased over a substantial distance up the hill to the 45 degree slope (which was directly measured via inclinometer, and verified by wheel hub high speed film analysis (see reply to Comment E.6 above)). The ATV was remotely controlled so that it propelled itself up the increasing slope at a low speed (e.g., 2 mi/h), until it reached the 45 degree slope, where the throttle was remotely commanded to be in a wide open position, accelerating the ATV and causing it to pitch-up, and then to somersault backward.

The computer simulation for the motion calibration modelled the ATV and the dummy as they climbed the hill at an initial low speed, and then, reaching the 45 degree slope, inputting a step-like wide open throttle, resulting in rearward overturn and the ATV and dummy motions calculated by the simulation model. These were then compared to (i.e., calibrated against) the motions measured by means of high speed film in the full-scale tests, with the result being a strong correlation (i.e., r^2=0.91 on average). As noted in the reply to Comment E.6, the section of the slope where the pitch-up, somersault backward and dummy ejection occurred was on an essentially uniform 45 degree slope, as indicated by the data in Van Auken, et al. (1998), Volume III, Annex 7, pages 10, 11 and 13.

8. ***"In respect of limits to stability calibration for travel around a curve:"***

This comment is somewhat misleading, as it is based on a false premise.

The purpose of the calibration tests was not to identify "limits of stability calibration" but, rather, "post-overturn ATV and dummy movement calibration". See discussion above and below.

9. ***"a. The video clearly shows the hybrid III dummy does not lean towards the inside of the curve as a real rider would. In fact its arms are restricted in moving relative to its torso so that as the torso moves outwards under centripetal forces, the handlebars are turned so as to increase the tightness of the turn, and so far that the steer wheel begins to plough. In real life a rider would lean to the inside of the curve and either maintain the turning radius or if possible increase it to minimise the risk of rollover."***

This comment is somewhat misleading as it is based on a false premise that implies that a crash dummy should, or somehow could, lean as a human would lean.

This comment is also misleading and based on a false premise in that the purpose of the calibration test was not to reproduce what a particular human rider does, or might or might not do, in a particular situation, but to calibrate (i.e., correlate) the simulation against the full-scale test, in terms of the post-overturn motion of the passive MATD crash dummy, riding on an ATV, in a situation that resulted in an ATV overturn.

If, in a turn, a human rider sufficiently reduced the steer input and/or the speed, and/or shifted his body "into" the turn, then such an overturn would not have occurred, which is what this comment is describing. However, this was not the purpose of the tests and simulations, as the tests and simulations were intended to represent situations where these appropriate rider actions did *not* occur.

First of all, the physical dummy being tested and modelled is inherently "passive", there being no "active" crash dummies in existence anywhere in the world.

Second, fundamentally, models must be able to be validated experimentally, by means of instrumentation, high speed film or video, and the like. This means that one must use for testing, and one must model, crash dummies. It is not possible, ethically or otherwise, nor is it repeatable or reproducible, to use and model live humans involved in overturning vehicle maneuvers, be they with cars, motorcycles or ATVs.

Third, certainly, one can mathematically model "lean up a slope" or "lean to the inside" behaviours, and DRI has done so in other studies for other purposes, but if that is done, the behaviours and the results cannot be experimentally verified, as no existing crash dummy can provide such behaviours, and it is unethical and generally not repeatable or reproducible to use live humans in a vehicle crash or overturn test.

Fourth, although a "passive rider" behaviour might not be the most typical case, and although an "active dummy" is beyond the state of technology, a "passive dummy" (physical and model) might be considered to model, as a first approximation, some types of plausible "passive rider" behaviours, some of which appear in accident case files or which are otherwise observable, e.g., riders who are highly intoxicated, experiencing a heart attack or other impairment, temporary disability, etc.

10. ***"In summary the quad bike calibration videos have next to no value as a means of calibrating a computer model. In addition where it is visually obvious there would have been injury none is recorded. And finally the videos prove the limit to stability travelling up a slope is in the range 32 – 34 degrees – yet DRI ignore that fact."***

This comment is not accurate, without basis, and is misleading as it is based on a false premise about the purpose of the methods involved in the calibration tests and simulations, as discussed in the preceding replies. As discussed above, the videos were not used to calibrate the model but rather the far more accurate high speed films were used for this purpose. This comment infers, as discussed in the replies above, injuries that were not apparent and that were unable to be recorded by any existing crash dummy technology. The claimed slope angles are erroneous and are very different from measured slope angles.

F. QUADBAR (MCDONALD AND RICHARDSON)

1. ***"The [simulated] V-Bar injuries were compared with those predicted by simulation [for the baseline ATV], not with the original UK/US injuries."***

This comment is somewhat misleading as it is based on a false premise.

It would have been highly inappropriate to compare the simulated V-Bar injuries with the original actual UK/US injuries because: a) DRI used (as in ISO 13232) a "paired comparison" methodology, where all conditions (i.e., as modeled in the simulation) are exactly the same for the baseline ATV and for the ATV with device, except for the presence of the device). Comparing the outcomes of a simulated V-Bar "type" of rollover to an actual baseline ATV overturn case would be like comparing "apples to oranges", and would violate the "paired comparison" principle; and, b) as discussed above, DRI did not model the original, specific UK/US accidents, but rather modelled 113 general "types" (referred to as "configurations" and "scenarios") of ATV overturn (which are based on the generally very limited set of key variables provided by each UK/US investigator for each of the 113 specific cases, plus a series of plausible, systematic and consistent assumptions regarding the unknown variables, so that each case is generalized to represent a general "type" of overturn, rather than a specific overturn, for which many of the variables were unknown.

It is not valid and highly inappropriate and violates the "paired comparison" methodology to compare the outcomes of a real accident case (that has largely unknown conditions) to a simulated case that has fully known and specified conditions. Instead, a simulated "type" of rollover with the baseline ATV must be compared to the same simulated "type" of rollover with the device-fitted ATV (i.e., fitted with a V-Bar in this instance).

Modelling specific UK/US cases, which is not what was done, would have required a semi-infinite number of input variables, e.g., the position of each rock, tussock, 3D terrain feature, detailed steering, braking, throttle, body positioning time histories of the rider, plus rider stature, weight, girth, age, physical condition, injury tolerance, exact path and speed along the terrain before the rollover, what the rider landed on after rollover, etc., etc., etc., which were not recorded by the government investigators, and therefore was not feasible.

The use of paired simulations of generalized "types" of accident configuration (rather than comparing a simulation outcome to a specific accident outcome) is the same approach used in ISO 13232, except in that case it is for motorcycle impacts, rather than for ATV overturns.

2. ***"However, as the simulation greatly increases head injuries and virtually eliminates trunk injuries, the V-Bar (Quadbar) does not have much opportunity to gain credit for reducing trunk injuries because the simulation has removed 46 of the 50 trunk injuries including the 5 coded AIS 5 and 6."***

The most recent, updated, refined simulations, run since the 2004 to 2007 DRI Coronial inquest reports were submitted, and reported in Zellner, et al. (2012), have addressed this issue.

These latest simulations, which take into account all known, valid comments in making the methodology updates, extensions and refinements described in Section IV of Zellner, et al. (2012), found that the previous dummy-soil friction coefficients, which were only estimated in the previous analysis, were somewhat greater than actual values which were measured more recently on soil. These previous, higher coefficients of friction produced generally greater frequencies of head and leg injuries than were observed in the real accidents.

In addition, further review of the UK/US case files enabled refinement of the AIS coding for some of the HSE- and CPSC-reported injuries, by including only those injuries which can feasibly be monitored by the MATD crash dummy (or any other crash dummy) and that therefore can be modelled.

In particular, trunk injuries that can be monitored by crash dummies were found to be relatively infrequent in the original UK/US accidents, as reflected in the revised AIS codings, and as reported in Zellner, et al. (2012), and as illustrated in Figures 3.a and 3.b.

Together, these appropriate refinements to the simulation model and to the DRI injury codings resulted in a closer agreement between the frequency distributions of the modelled injuries (for the 113 general "types" of overturn) and the actual injuries from the 113 specific cases), as illustrated in Figures 2 and 3 above.

However, these and all of the other recent refinements to the models, which take into account all known, valid comments in making the methodology updates, extensions and refinements described in Section IV of Zellner, et al. (2012), did not change the overall conclusions regarding the Quadbar, e.g., after these refinements to the simulation model, the Quadbar caused approximately as many new injuries and fatalities as it prevented.

3. ***"This bias appears to be systematic (some may say deliberately fraudulent)."***

Reference is made to the response to Comment F.2 with respect to the allegation of "systematic" bias.

This comment is false, defamatory and without basis in regard to the suggestion of a "deliberately fraudulent" bias in the previous DRI research.

For the reasons discussed above, the comment is in any event no longer of any relevance, as the most recent simulation analyses reported in Zellner, et al. (2012), which have eliminated these over-and under-predictions of injuries, indicate that the Quadbar would cause approximately as many injuries (and deaths) as it would prevent.

Previously, the use of estimated (rather than measured) dummy-soil friction coefficients was an expedient taken in order to respect the “urgent” requests for reports by the Victorian State Coroner, and by the UK/HSE during an earlier period. Use of estimated rather than measured dummy/soil friction coefficient values did have a systematic effect on the frequency of head and leg injuries. In addition, previous inaccuracies in the coded real injuries (which in some cases involved including injuries that were not measurable with an MATD dummy or any other crash dummy) affected the number of trunk injuries coded for the real accidents. These likewise may have had systematic effects on the comparison of the actual injury distribution of the specific accident sample, with the simulated injury distribution of the general overturn "types" sample. However, clearly, neither of these effects was "deliberate" in any way, and they were only taken as expedients in the interest of time in response to requests from the relevant authorities.

Most importantly, the recent corrections to these dummy/soil friction coefficients have not resulted in any change to the overall conclusions regarding Quadbar injury risks and benefits.

4. ***"Given this scenario there may be some other models of device that have also been systematically biased (not just the V-Bar)."***

   See reply to Comment F.3 above.

5. ***"Notwithstanding this bias the claim that "There is 'clear evidence' shows 'the device is…more likely to cause than ameliorate harm to ATV users' is not credible for the following reasons:"***

   Note: This quote is not taken from a DRI report. It is presumed that it is referring to the Quadbar.

6. ***" The DRI report showed:***
   - ***a. For helmeted riders, the overall reduction in injuries was 1%.***
   - ***b. For unhelmeted riders, the overall reduction in injuries was 29%.***
   - ***c. In both cases, the increase in injury was less than the decrease in injury, indicating a net overall reduction in damage."***

   It is not clear on what data this comment is based. If this comment is based on the Munoz et al. (2007) DRI report, then it is not accurate, as the risk/benefit percentages presented in that report cannot be used to calculate net benefit.

   The risk/benefit percentage is a ratio between two numbers, while the "overall reduction", i.e., net benefit, is the difference between two numbers divided by a third number, as defined in ISO 13232-5 (2005), clauses 5.9.4.2 and 5.9.4.3. A risk/benefit of 99% does not mean and cannot be interpreted as a net benefit of 1%; and in general, the net benefit for a 99% risk/benefit can be any value between 0.0% (i.e., no net benefit) and 1.0%, and is by no means necessarily the maximum value of 1.0%. Likewise, the net benefit for a risk/benefit of ~~8~~71% (which was the value reported in Munoz, et al. (2007)) can be any value between 0% and 29%, and is by no means necessarily the maximum value of 29%.

   In general, for a risk/benefit percentage of X% (when X is equal or less than 100), the "net benefit" can range between 0% (when the benefits of the device are much smaller than the total baseline vehicle injuries) and 100 – X% (in the rare case where the benefits are equal to (i.e., the device eliminates all of) the total baseline vehicle injuries).

As for the bullet points (i.e., referring to “helmeted riders” and “unhelmeted riders”), these do not appear in the DRI Munoz, et al. (2007) report, and they are a misinterpretation of what does appear in the DRI report.

The DRI report states that (p 11) the “injury risk/benefit percentages [emphasis added] of the V-Bar ROPS…are:− With helmet, 99 [53, 192] percent, which means that the injury risks from the ROPS are excessively large and nearly equal to (i.e., 99 percent of) the injury benefits from the ROPS; and that any net benefit is <u>not statistically significant</u>, and − With no helmet, 71 [41, 137] percent, which means that the injury risks from the ROPS are excessively large in comparison to the injury benefits from the ROPS (i.e., ISO 13232-5 guidelines are that the risk/benefit percentage for safety devices should be less than 7 percent) and not more than 12 percent.”

First, the phrase “99 [53, 192] percent” is not equivalent to “the overall reduction in injuries was 1%.” This is an incorrect statement as, first of all, the “average net benefit” or reduction in injuries, according to ISO 13232, is not a percentage but is a difference expressed in injury units. On the other hand, the “99%” in the DRI Munoz, et al. (2007) report relates to the ratio, expressed as a percentage, not the difference, between the injury risks and the injury benefits of the device in comparison to the baseline ATV injuries. It is impossible to infer that “the overall reduction in injuries was 1%” as, in order to do this, one would need to calculate the total baseline injury units, which calculation was not done in the Zellner, et al. (2004) or Munoz, et al. (2007) reports.

Second, the report states that the square brackets [ ] in the above quote from the DRI report “denote 95% confidence interval [i.e., statistical uncertainty bounds]…. If the confidence interval includes 100%, then the Risk/Benefit ratio is not statistically significantly different from 100%, i.e., statistically, there is no difference between the magnitude of the risk and the magnitude of the benefit, and therefore there is no statistically significant net benefit”.

Therefore, the results from the DRI report for both the helmeted and unhelmeted conditions are that the V-Bar (i.e., Quadbar) has “no statistically significant net benefit”. This is very different to the incorrect statements that “the overall reduction in injuries was 1%” (for the helmeted condition) and “the overall reduction in injuries was 29%” (for the unhelmeted condition).

The third bullet is not from the DRI report either, and misinterprets the results, as it ignores the statistical confidence bounds, which indicate that the V-Bar has “no statistically significant net benefit". The DRI report indicates that the injury risk/benefit percentages from the Quadbar device (i.e., 99% which was found (due to large variability in outcome) to be not statistically significantly different from 100%, and ~~8~~71% which was found (due to variability in outcome) to be not statistically significantly different from 100%) and far above the guidelines in International Standard ISO 13232 (i.e., 7%). A 100% risk/benefit percentage means that a device is as likely to increase injuries as it is to decrease injuries. Moreover, from a designer's viewpoint, one has to design a safety device or system primarily for its intended (i.e., helmeted) use, which places greater importance on the 99% value.

In any case, the subject matter of this comment is no longer current because the latest simulation results reported in Zellner, et al. (2012) indicate that the Quadbar causes approximately as many injuries and deaths as (and in fact slightly more injuries and deaths than) it prevents.

7. ***"The FCAI's ongoing statements that the devices create more injuries cannot be substantiated, even by their own data."***

Various previous DRI reports have found statistically significant increases in injuries from some devices in some conditions, and so a statement that some types of devices "create more injuries" can be substantiated by the past research.

For the Quadbar, the latest simulation results reported in Zellner, et al. (2012), which take into account all known, valid comments in making the methodology updates, extensions and refinements, indicate that, for the helmeted condition, the Quadbar causes approximately as many injuries and deaths as (and in fact slightly more injuries and deaths (though not statistically significantly more when projected to the population of all overturns)) than it prevents.

G. RISK/BENEFIT (MCDONALD)

1. ***"The DRI report (and sometimes though not always the FCAI statements), suggest that the difference was not statistically significant (though it never says that the Quadbar created more injuries than it prevented)."***

Although this comment is accurate for the previous Munoz et al. (2007) report, its relevance to the most recent simulation analyses, which take into account all known, valid comments in making the methodology updates, extensions and refinements described in Section IV of Zellner, et al. (2012), warrants a note.

The most recent simulation results reported in Zellner, et al. (2012) indicate that, for the helmeted (intended use) condition, the injury risk/benefit percentage of the Quadbar is 108%, within 95% confidence interval of [69%, 168%]. The 108% value means that the Quadbar injury risks for that condition are on average greater than the injury benefits for the simulation sample of 770 overturns, but the range in square brackets (which includes 100%) means that this outcome is not statistically significant when projected to the population of overturns, i.e., the injury risks for the helmeted condition are essentially statistically equal to (i.e., not different from) the injury benefits, which was exactly the same statistical outcome as was reported in the 2007 report.

2. ***"In plain English, DRI required that the increase in injury from fitting a protective device had to be 7% or less than the decrease in injury from fitting the device."***

This comment is not accurate and somewhat misleading because DRI did not (and cannot) "require" anything, but rather stated that the Quadbar risk/benefit is greater than the ISO guideline values that say the risk/benefit of a protective device "should be less than 7% and should not be more than 12%".

Note: in order to be accurate, according to ISO 13232-5 (2005) Annex E, this comment should say “7% or less of the decrease…,” (i.e., Risk is less than or equal to 0.07 X Benefit) as defined in ISO 13232-5. This statement has an entirely different meaning than “7% or less than the decrease in injury” (i.e., Risk is less than or equal to (Benefit - 0.07 X Benefit), which is equal to 0.93 X Benefit), which is incorrect and not in accordance with the ISO 13232 guideline.

3. ***"7% can be taken as 1 in 14, 1 in 13 is 7.7% which would not according to the 7% be acceptable. This unacceptability is attributed to Annex E of ISO 13232.5. Annex E of ISO 13232.5 does not require compliance with the 7%."***

This comment is not accurate and somewhat misleading because ISO 13232-5 (2005) Annex E, footnote 2 states that 12 percent (not 7.7%) "is presumed to be an unacceptable risk/benefit percentage".

In addition, DRI has never stated that ISO 13232-5 "required compliance". In fact, ISO 13232-5 (2005) Annex E, p 56 of the Standard, states that it is a "suggested reference guideline" and that "the risk/benefit percentage should be less than seven percent and should not be more than 12 percent". Footnote 2 on p 56 states that this is "Based on the results for automobile seat belts (7 percent) based on Malliaris, et al. (1982), as described by Rogers (1996), which is presumed to be an acceptable risk/benefit percentage; and the results for pre-1998 automobile passenger airbags (12 percent), based on Iijima, et al. (1998), which is presumed to be an unacceptable risk/benefit percentage."

In addition, an extensive worldwide literature search and review has indicated that there are no known acceptable automotive safety devices for which there are published risk and benefit data that have a risk/benefit percentage greater than 7%.

4. ***"In its own words, Annex E is 'informative', 'potentially useful', 'a suggested reference guideline' and invites users 'to develop other guidelines, or have no particular guideline, depending on the research'."***

This comment is true.

As a matter of clarification, DRI has never said that ISO 13232 Annex E was a requirement or "required compliance", or was anything more than a "guideline". DRI said (i.e., Munoz, et al. (2007)) that "The injury risks of fitting the V-Bar ROPS (99 percent risk/benefit percentage for helmeted riders, 71 percent for unhelmeted riders) were much greater than those in ISO 13232-5 guidelines for occupant protection systems. ISO 13232-5 recommends that "the risk/benefit percentage should be less than seven percent and should not be more than 12 percent".

5. ***"Furthermore, 'ISO 13232 does not apply to testing for regulatory or legislation purposes.'"***

This comment is true, and DRI has never said that ISO 13232 should apply to testing for regulatory or legislative purposes. ISO 13232 is a standard for research into motorcycle rider protective devices and, for the purposes of its research, DRI has applied it to ATV rider protective devices.

6. ***"What this means is that if we:***
   - ***a. Take no action -13 deaths will occur***
   - ***b. Fit modification -13 deaths will be prevented -1 death will occur as a result of the modification"***

This basis of this comment is unclear and unknown.

McDonald (2010) refers to a hypothetical situation in which there might be some "device which eliminated 13 fatalities and created 1 [fatality]". However, this appears to be a purely hypothetical example, not based on any simulation or testing of ATVs.

It is conceivable that these numbers come from some other MacDonald or Lambert report, one of which (i.e., Lambert (2011~~0~~c)) invalidly attempts to extrapolate ROPS outcomes for heavy agricultural tractors to ATVs.

If these numbers are based on data contained in the DRI Munoz, et al. (2007) report on the Quadbar, this comment is not accurate as the number of simulated fatalities with or without Quadbar is not reported in that document. If the commentators wrongly assumed that "AIS 6", which is an index that is included in the Munoz, et al. (2007) report, is a fatal injury, such an assumption is not accurate. AIS 6 is a "maximal" injury, as defined in, e.g., AIS (2005), and only represents a less than certain (e.g., approximately ~~81~~79 percent, as indicated in AIS (2005), page 5) probability of fatality. The index termed "probability of fatality" (POF), which involves an independent analysis which can be calculated according to ISO 13232 (2005), is the appropriate method to assess effects on fatalities.

In analyzing the probability of fatality, DRI's most recent ATV simulation results reported in Zellner, et al. (2012), which takes into account all known, valid comments in making the methodology updates, extensions and refinements described in Section IV of Zellner, et al. (2012), indicate that, for the helmeted condition, the probability of fatality risk/benefit of the Quadbar is 134%, within a 95% confidence interval of [79%, 219%]. The 134% value means that the Quadbar fatality risks are on average greater than the fatality benefits, but the range in square brackets (which includes 100%) means that this outcome is not statistically significant, i.e., the fatality risks are essentially statistically equal to the fatality benefits. However, this is completely different from what is stated in this comment, which claims that 13 deaths (e.g., most or all of the Australian deaths associated with ATV's in one year) would be prevented by the Quadbar. There are no scientifically valid data or analyses that support such a claim; and certainly neither the previous nor the latest DRI reports and data support such a claim. The DRI reports, including the most recent updated Zellner, et al. (2012) report, have found that, for the helmeted condition, statistically, the Quadbar would cause as many fatalities as it would prevent.

7. ***"This formula fails any test of reasonableness. Any net benefit is a positive and there is no requirement in law for it to meet any artificial risk/benefit formula."***

This comment is not accurate and somewhat misleading, as the ISO 13232 criterion does not fail a test of reasonableness in comparison to all known risk and benefit data for automotive safety devices (which indicate risk/benefit percentages less than 7 to 12%), and because DRI has never said that there was any "requirement in law". ISO 13232-5 (2005) Annex E is only a guideline.

The ISO 13232 guideline passes the test of reasonableness because widely accepted (i.e., nearly universally accepted) automotive safety devices such as safety belts, head restraints and airbags demonstrably have very low risk/benefit percentages, all below 7 percent (Ref

Iijima, et al. (2007), Thompson, et al. (2001), Malliaris, et al. (1983)). This observation is based on the only known, published data for automotive protective device risks and benefits.

In addition, the commentators, who participated in the HWSA/TEG meetings and reviewed the TEG report, should clarify what the TEG report means when it states (as reported by a WorkSafe staff member during the TEG meetings) that "in the regulatory policies of several of the states, the benefits needed to be at least 2 times the risks" (i.e., 2 deaths prevented for every 1 new death caused by a device, or 50 percent risk/benefit, TEG report, p32, footnote 7, November 2010).

Although the latter criterion, if accurate, would be an astonishingly high risk/benefit criterion, higher than any known and measured risk/benefit percentage of any automotive safety device, the Quadbar risk/benefit percentages of 99% (2007 DRI report) or 108% (in the latest refined DRI results) are far above even this possible "requirement [or policy] in law".

8. ***"Any net benefit is a positive and there is no requirement in law for it to meet any artificial risk/benefit formula."***

This comment is somewhat misleading, and arguably not accurate if one accepts the apparent H&S state policies (see reply to Comment 7 above).

This comment is somewhat misleading because the net benefit is not "positive" but, rather, according to both DRI's 2007 and latest simulation results, for the helmeted condition, the Quadbar in fact has no statistically significant net injury benefit or net fatality benefit. For the unhelmeted condition, the Quadbar has a statistically significant negative net benefit.

Moreover, this comment is now obsolete because, according to DRI's latest simulation results reported in Zellner, et al. (2012),the Quadbar *average* net benefit is negative for the simulation sample, i.e., it has a net injury risk and net fatality risk (though for the helmeted condition, not statistically significantly so when projected to the population of all overturns).

This comment is arguably not accurate because, as noted in the reply to Comment F.7, it is understood that one WorkSafe staff member stated that several of the Australian states have risk/benefit policies of at least 2 to 1 (i.e., 2 deaths prevented for every 1 new death caused by a device, or 50 percent risk/benefit, TEG report, p32, footnote 7, November 2010) If so, this could potentially be a requirement in law, or at least in government policy, which the Quadbar would not be close to meeting.

9. ***"Additionally, irrespective of any benefit to risk formula, if the benefits outweigh the risks to any degree (as shown by the DRI data) – than surely that complies with making plant "as safe as reasonably practicable".***

This comment is misleading and not accurate; as well as obsolete.

The DRI data do not show that the Quadbar "benefits outweigh the risks to any degree", as the data indicate that there is no statistically significant net benefit, and the risk/benefit is not statistically significantly different from 100%. In other words, it is impossible to say within a 95% confidence interval (the most widely used standard of statistical confidence worldwide) that the Quadbar injury benefits are less, more or equal to its injury risks. Therefore, to state that the "benefits outweigh the risks to any degree" is not accurate in regard to the 2007 DRI results, and ignores the most basic standard of statistical confidence.

This comment is now out of date and categorically not accurate when considering the most recent DRI refined results for the Quadbar <u>for the helmeted condition</u> (i.e., 108% risk/benefit percentage, within a 95% confidence interval of [69%, 168%]), i.e., if anything, the Quadbar injury risks exceed its injury benefits (though not statistically significantly so).

10. ***"If this is indeed the case – and given that the manufacturers have known this since at least 2007, have they been in breach of the OHS Act?"***

This comment is misleading as it is based on a false premise, as the "benefits outweighing the risks to any degree" is categorically not the case, as indicated in the reply to Comment G.9.

If anything, the Quadbar, in not meeting the possible "regulatory policies of several of the states, the benefits need[ing] to be at least 2 times the risks" (i.e., 2 deaths prevented for every 1 new death caused by a device, or 50 percent risk/benefit, TEG report, p32, footnote 7, November 2010), might be "in breach" of several states' H&S policies, based on both the 2007 injury evaluation data submitted to the Victorian State Coroner, as well as the latest 2012 simulation results.

SECTION III

SUMMARY

This report has provided replies to comments made in Lower (2011) regarding previous research on Rollover Protection Systems (ROPs) and Crush Protective Devices (CPDs) proposed for use on All Terrain Vehicles (ATVs), conducted and published by Dynamic Research, Inc. (DRI) beginning in 1997. Overall, these comments are contained in a series of documents, summarized in the Lower (2011) document addressed in this report, which was entitled "Summary – Review of Dynamic Research Inc. Data". The Lower (2011) document states that it was based on the findings of John Lambert, Shane Richardson and Geoff McDonald.

This report has presented each comment from Lower (2011), followed by DRI's reply to the comment.

Unfortunately, as discussed, many of the Lower (2011) comments were observed to be not accurate and somewhat misleading, because they did not take into account the extensive documentation provided in the context of the Victorian Coronial inquests, of past and contemporaneous DRI research on ROPS/CPDs. In addition, as discussed, some of the Lower (2011) comments appear to be based on false premises.

Importantly, the lack of validity of many of the Lower (2011) comments has been confirmed by DRI's latest updated research on the Quadbar, which take into account all known, valid comments in making the methodology updates, extensions and refinements described in Section IV of Zellner, et al. (2012), and which, for the helmeted condition, indicates that the injury risk/benefit percentage for the Quadbar is 108% [69%, 168%],[4] which does not change the previous overall conclusions about the Quadbar. In fact, on *average*, the injury risk is greater than the injury benefit for the simulation sample of overturns (though for the helmeted condition, not statistically significantly so when projected to the population of all overturns). In addition, DRI's latest research indicates:

- A preliminary index of asphyxiation (i.e., breathing difficulty) can be included in the simulations, and when it is, the previous conclusions about the Quadbar are not affected, i.e., the Quadbar causes as many potential asphyxiations as it prevents;
- In terms of probability of fatality, the risk/benefit percentage of the Quadbar for the helmeted condition is 134%, within a 95% confidence interval of [79%, 219%]. The 134% value means that the Quadbar fatality risks are on average greater than the fatality benefits for the simulation sample, but the range in square brackets (which includes 100%) means that this outcome is not statistically significantly different

[4] Square brackets indicate 95 percentile upper and lower confidence intervals on the population estimate. When 100% lies within this interval, the outcome is not statistically significantly different from 100%, i.e., the injury risks are effectively equal to the injury benefits)

from 100% when projected to the population of overturns, i.e., the Quadbar has essentially equal fatality risks and fatality benefits;

– The most recent DRI animations of the Quadbar simulations, which have been refined in order to take into account all known comments, exhibit no “head…bouncing on the ground…at 4 cycles per second”, no “bouncing like a beach ball”, and no motion that “defies physics”.

In addition, some of the Lower (2011) comments are not relevant, because the motions described rarely or never occurred in the DRI simulations, and *never* occurred in the previous or most recent refined simulations used to evaluate the Quadbar risk/benefits.

Finally, many of the Lower (2011) comments are unclear, as it is unknown to which DRI or other work it is referring. For example, the source is unknown for the Lower (2011) statement that “if we take no action, 13 deaths will occur; fit modification, 13 deaths will be prevented, [and] 1 death will occur as a result of the modification." There is no valid basis for such a statement in any of the DRI work, and no valid basis in any other research of which DRI is aware.

In view of the extent and the nature of the issues concerning the validity of the summary provided by Lower (2011), which is stated to be based on the findings of Lambert, Richardson and McDonald, that summary cannot be relied upon.

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