Daytime Running Lights (DRL): A review of the reports from the European Commission

by I Knight, B Sexton, R Bartlett, T Barlow, S Latham & I McCrae

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Executive summary

The European Commission (EC) has published the results of research into the effectiveness, costs and benefits of introducing Daytime Running Lights (DRL). This research suggested that a substantial number of casualties could be prevented if this was introduced across the EU. Further, it showed a positive benefit to cost ratio when the costs of fitting lamps and the environmental cost of running them was considered, (i.e. benefit/cost ratios >1). On the basis of this research the EC has now entered a consultation exercise on how best to implement DRL.

In the past, a number of researchers have found flaws with some of the studies carried out into the effectiveness of DRL and some road safety groups are concerned that they could potentially have adverse affects for some road user groups, particularly for motorcyclists. For this reason, the DfT commissioned TRL to carry out a critical review of the research carried out for the EC in order to inform the DfT’s response to the EC consultation exercise. The work has involved TRL experts reviewing specific parts of the research reports produced for the EC, comparing with other related research and carrying out a sensitivity analysis on the cost benefit model used in the EC research.

Overall, the research reported in the EC DRL reports represents a thorough and comprehensive analysis of the available data. Although it is possible to be critical of several specific aspects of the work, very substantial evidence has been presented that the introduction of DRL would result in a net casualty reduction effect. However, there appears to be greater scientific uncertainty concerning the size of the expected effect. Some of the parameters in the statistical analysis were not found to be statistically significant and should, therefore, be treated with some caution. In particular, the evidence for assuming a 15% improvement on fatal accidents is weak and it was considered that it would be more technically defensible to assume that a mean effect of between 3.9% and 5.9% (depending on which biases and assumptions are considered) applied to accidents of all injury severities and that there would be no effect on damage-only accidents.

The investigation of the effect of DRL for passenger cars on the conspicuity of vulnerable road users appeared, in general, to be a well controlled experiment. It is possible to criticise several specific aspects of the research but, in most cases, it was not considered likely that these would have substantially affected the main results and conclusions of the work. However, a few more serious concerns with this work were identified:

- The conspicuity of motorcycles in the presence of differing intensities of DRL and different ambient lighting conditions was not investigated.
- There was some concern that the photographic methods used *may potentially* not have replicated the real world environment sufficiently realistically.
- The relative positions of cars and motorcycles that were evaluated by the work did not include situations at a junction where the motorcycle was approaching from the side and was positioned in front of a car equipped with DRL. All road scenes considered appeared to place the motorcycle to the side of the car such that daylight was visible between the two to physically separate them in the image.

However, when this research was compared with other experiments carried out in this area, it was considered that, if the limitations of scope of Interim Report 3 (Brouwer *et al*, 2004) were accepted, then the three studies actually presented consistent conclusions. These were that DRL with high light intensities could impair the conspicuity of motorcyclists but it was possible to design DRL that could improve the conspicuity of cars in the dim ambient light conditions of most relevance without adversely affecting the conspicuity of motorcyclists. This shows that it is very important that the technical details of the implementation of DRL are considered very carefully since it may be that a policy option which involved the use of existing passing beam headlights (or high intensity dedicated DRL) as DRL could have an adverse effect on motorcyclist conspicuity. Further research to assess the concerns identified above will be necessary to gain confidence that implementation of any particular DRL policy option would not have an adverse affect for motorcyclists.

There was very little evidence presented in the EC reports on the justification of the estimates of environmental dis-benefits in terms of increased fuel consumption and emissions. However, an independent assessment of those effects using a sophisticated computer modelling technique has suggested that the values of 0.5% to 1.5% increase in fuel use and carbon dioxide emissions appear reasonably accurate and possibly even slightly higher than justified.

The topic where the greatest scientific uncertainty was found was in the cost benefit analysis. It was noted that the cost benefit analysis presented showed benefit/cost ratios considerably in excess of 1. However, the confidence limits calculated reported for these ratios were not statistically significantly different from a ratio of 1. It had been assumed that the accident reduction effect of DRL would be 15% of fatal accidents, 10% of serious accidents and 5% of slight accidents. This review suggests that the relationship between size of effect and accident severity was very weakly supported by the statistical studies of accidents and it was considered more technically defensible to assume that the mean value of 5% (or perhaps 5.9%) applied to all injury accidents. It can be seen that changing the assumptions from the very weakly supported estimate of effect in relation to accident severity (15%, 10%, 5%) to the more technically defensible 5.9% mean effect for all severities has a very large effect on the conclusions, changing the benefit/cost ratios from substantially greater than one to substantially less than one (i.e. a negative cost benefit). In addition, this review found that the statistical analysis also acknowledged a publication bias in the data and analysis has shown that this would in fact reduce the size of the mean benefit by 2% to 3.9%.

The size of effect estimates discussed above were found to be crucial to the cost benefit argument in favour of DRL. It seems clear that in terms of the cost benefit analysis, considerable technical uncertainty remains.

One further issue that was considered noteworthy was the fuel cost assumptions. The analysis had been carried out using a cost per litre for fuel that excluded tax, which is the technically correct approach to an analysis of the effect on “Europe plc”. However, if the increased cost to UK motorists were considered the fuel cost would be approximately four times that assumed in the cost benefit analysis.

In summary, the review reached the following conclusions:

1. There is substantial evidence that the mandatory use of DRL would provide a net accident reduction. However, the evidence concerning the magnitude of the effect and particularly the relationship with accident severity is considerably weaker.
2. The estimates of the fuel and emissions increases as a result of implementing DRL are reasonable and possibly slightly conservative (high).
3. The research into the potential of DRL on cars to impair the conspicuity of motorcyclists and other vulnerable road users was well controlled but limited in scope and did not consider some important variables. However, some consistent conclusions could be drawn which were that it should be possible to design dedicated DRL of low intensity (e.g. about 200cd) that are beneficial to the conspicuity of cars without adversely affecting the conspicuity of motorcyclists. However, DRL of higher intensity (potentially including standard passing beam headlights) could have an adverse effect on motorcyclist conspicuity in some circumstances.
4. There is considerable scientific uncertainty inherent in the values of the benefit to cost ratios presented in the EC work. The key variable is the assumption that the accident benefits would be considerably greater for fatal accidents (15%) than for serious (10%) or slight (5%) accidents. This assumption was very weakly supported by the available data and changing it to a more technically defensible assumption that the mean effect of 5.9% remained the same for all accident severities reduced the benefit to cost ratios to much less than 1 indicating that the costs would be greater than the benefits.
5. It was considered that it would be more technically valid to present a range of possible benefit to cost ratios within which there could be confidence that the true answer would lie, thus

reflecting the technical uncertainty. The analysis showed that a ratio of 1 would fall within this range meaning that, although an accident reduction potential exists, it is not possible to say with certainty whether the benefits of implementing DRL would outweigh the costs.

1 Introduction

The European Commission (EC) has published the results of research into the effectiveness, costs and benefits of introducing Daytime Running Lights (DRL). This research suggested that a substantial number of casualties could be prevented if this was introduced across the EU. Further, it showed a positive benefit to cost ratio (i.e. >1), when the costs of fitting lamps and the environmental cost of running them was considered. On the basis of this research the EC has now entered a consultation exercise on how best to implement DRL.

In the past, a number of researchers have found flaws with some of the studies carried out into the effectiveness of DRL and some road safety groups are concerned that they could potentially have adverse affects for some road user groups, particularly for motorcyclists. For this reason, the DfT has commissioned TRL to carry out a critical review of the research carried out for the EC in order to inform the DfTs response to the EC consultation exercise. The work has involved TRL subject matter experts reviewing specific parts of the research reports produced for the EC, comparing with other related research and carrying out a sensitivity analysis on the cost benefit model used in the EC research.

This report describes the findings of the review in full.

2 Methods

The analysis was predominantly based on critical review of the reports published by the EC but has also involved comparison with other published literature, statistical analysis of data provided in the EC reports and sensitivity testing on the cost benefit model.

3 Review of Interim Report 2 – Statistical analysis of safety benefits

Interim Report 2 (Elvik *et al*, 2003) describes a meta-analysis of 25 individual studies of the effects of DRL fitted to passenger cars and 16 studies considering the effects for motorcyclists. The meta-analysis was intended to provide a weighted mean estimate of the effects based on the combined results of the individual studies.

The statistics used to assess the safety effects of DRL were reproduced for each study as Appendix 1 to Interim Report 2. There are four pages of information extracted from 25 studies, which together supply 111 rows of data. The data include the basic accident counts by type for multi-party and single vehicle, during the day and at night, for periods before and after the introduction of DRL. Estimates of the kilometres covered by the multi-party journeys corresponding to the accident period reported are also included where available.

These data permit the calculation of the three measures of effectiveness used in the meta-analysis. However, inevitably there are some data missing and so some calculations of the safety effect measures have been included which use other data, for example they may be derived from a statistical model. As such, it is not possible to replicate fully the statistics used to assess the safety measures presented within the appendix. This was not seen as a major problem and there was no suggestion that data have been manipulated in any sense, it was simply that the data in Appendix 1 of Elvik *et* al (2003) could not be reproduced from the information available within the report, which limited the additional analyses that could be carried out as part of this review.

The DfT work specification asked for several specific aspects of the meta-analysis to be reviewed and these are considered in turn in the sub-sections below.

3.1 Estimator of the safety effect of DRL

Elvik *et al* (2003) used three measures within their report to assess the safety effect of DRL. These estimators have all been developed for the purpose of controlling for confounding factors when estimating the effect of DRL on accidents. Confounding factors are considered to be other factors that may have influenced any changes in the accident patterns within the sample studied and, for example, may include changes in the exposure to risk, the effect of other safety changes or initiatives that may have been occurring at the same time as the measure studied, or changes in driver behaviour. The estimators are:

- The accident rate ratio (ARR) – is the ratio of accidents per kilometre of driving for cars using DRL to accidents per kilometre of driving for cars not using DRL. i.e.

  $ARR = (MD_a / KMT_a) / (MD_b / KMT_b)$ [1]

- The odds ratio (OR) – is calculated as the ratio of (multi-party daytime accidents with DRL to all other with DRL accidents) divided by (the ratio of multi-party daytime accidents before DRL to all other before DRL accidents).

  $OR = [MD_a / (MN_a+SD_a+SN_a)] / [MD_b / (MN_b+SD_b+SN_b)]$

- The ratio of odds ratios (ROR) – is calculated as the ratio of (multi-party daytime with DRL accidents by single-vehicle daytime with DRL accidents) by (multi-party night-time with DRL accidents by single-vehicle night-time with DRL accidents) all divided by the same double ratio but calculated for accidents before DRL was introduced.

  $ROR = [(MD_a / SD_a) / (MN_a / SN_a)] / [(MD_b / SD_b) / (MN_b / SN_b)]$

If any of these measures is less than 1.0, the use of DRL is associated with an improvement in road safety. If they are greater than 1.0, the use of DRL is associated with a deterioration of road safety. They attempt to control for a number of differing confounding situations.

The following illustrative examples, Table 1, show how the three safety effect measures are influenced by potentially confounding scenarios. The examples look at the three measures and how they are affected by actual changes to all accidents, just multi-party accidents, or just single vehicle accidents during the day and/or at night.

The expected DRL effect is one which should only affect the multi-party accidents during the daytime, and all three measures should show a consistent pattern for this scenario – i.e. provided that the three measures are showing a very similar result then one can be confident of the safety benefit. This is a reflection of the way in which the three safety measures have been developed.

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[1] MD = multi-party daytime accidents, SD = single vehicle daytime accidents, MN = multi-party accidents at night, SN = single vehicle accidents at night, a = using DRL, or after a law requiring the use of DRL has been passed, b = not using DRL, or before a law requiring the use of DRL has been passed. KMT = vehicle kilometres of travel.

Table 1 Effect on safety effect measures for different scenarios

Accident type	Change after DRL introduced		ARR	OR	ROR
	On accident type(s)	On multi-vehicle mileage			
All	0%	No change	1.000	1.000	1.000
	-10%	No change	0.900	1.000	1.000
	+10%	No change	1.100	1.000	1.000
All multi-party	-10%	No change	0.900	0.955	1.000
	+10%	No change	1.100	1.041	1.000
All single vehicle	-10%	No change	1.000	1.045	1.000
	+10%	No change	1.000	0.959	1.000
All night time	-10%	No change	1.000	1.094	1.000
	+10%	No change	1.000	0.921	1.000
Daytime multi-party	-10%	No change	0.900	0.900	0.900
	+10%	No change	1.100	1.100	1.100
Daytime single vehicle	-10%	No change	1.000	1.014	1.111
	+10%	No change	1.000	0.986	0.909
All	0%	10% decrease	1.111	1.000	1.000
	0%	10% increase	0.909	1.000	1.000
Daytime multi-party	-10%	10% decrease	1.000	0.900	0.900
	+10%	10% increase	1.000	1.100	1.100

The above table was constructed by assuming the following numbers of accidents and kilometres driven before DRL was introduced and these are similar to some of the meta-data used in the analysis. Using different numbers would have generated slightly different figures in the cells:

- Multi-party daytime accidents = 4,000
- Multi-party night accidents = 2,000
- Single-vehicle daytime accidents = 500
- Single-vehicle night accidents = 1,000
- Kilometres driven = 20,000 Km

The ARR (Accident Rate Ratio) measure has an attraction because it takes into account exposure. However, one difficulty is obtaining a good estimate of the exposure. Ideally it would be the distance travelled by those vehicles involved in multi-party collisions. In practice it will be necessary to use an estimate of the exposure for all vehicles of interest (cars), and this is likely to be for the whole day not just during daylight hours. The source of such data is usually from surveys of drivers (National Travel Survey and General Household Survey, ONS) and/or traffic counts and may be subject to considerable sampling noise.

Total annual mileage for cars is also available from road tax renewal data, but is not split by day and night. It is possible that the noise in any exposure measure data exceeds the effect of DRL. It can be seen from the scenarios in Table 1 that a change in the mileage figure, where there is no underlying change in accidents, will lead to a change in the ARR measure. This may be an accurate reflection of the multi-party accident rate changing, but would not be reflected in the OR nor ROR measures. This raises a fundamental point about the interpretation of these different measures.

The OR (odds ratio) measure indicates a change in the proportion of multi-party daytime accidents as compared to all other accident types, once DRL is in use compared to before its introduction. Hence it is a measure of the impact of DRL on multi-party daytime accidents controlling for overall trends. Thus, as can be seen in Table 1, if all accident types change then the OR value remains constant. However, if just multi-party or single-vehicle accidents change then the OR will also change – but not by the underlying rate of change for that vehicle type. That is, if all multi-party accidents increase by 10% from the 'before' to 'after' period then the OR increases by just 4.1% in Table 1 (the 4.1% reflects the accident numbers used in the example). An increase in single-vehicle accidents produces a decrease in the OR value. This affect may be important when considering the potential influence of other safety changes that may have been happening at a similar time to the introduction of DRL.

The ROR (Ratio of Odds Ratios) measure is complex to describe; it is a ratio of odds ratios where each ratio reflects the relative number of daytime multi-party to single-vehicle accidents compared to night-time multi-party to single-vehicle accidents. The ratios are computed and are compared for accidents with DRL against accidents before DRL was introduced. The measure thus allows for trend or other changes between daytime and night-time accidents and so any change in the ratio is more likely to be due to a change in daytime multi-party or single-vehicle accidents. This can be seen in Table 1 where an increase of 10% in daytime multi-party accidents shows a 10% change in ROR and a 10% increase in daytime single-vehicle accidents shows a decrease of 9.1% in the ROR.

These three measures thus control in differing ways for potentially confounding factors. Provided that the actual change is just daytime multi-party accidents then all three measures will show the same degree of change. Interpreting situations where they change by different amounts is complex, but at-least if the change is always in the same direction then some confidence can be had that the daylight multi-party accidents have changed relative to other types of accident. A similar study is reported in section 5.1 of the report (Elvik *et al,* 2003*)* where similar conclusions are reached.

The reasons why different types of accident may have changed are worth considering. Newer cars tend to be stiffer and also have electronic devices which assist the driver by preventing skids (ABS) and losing control (ESC). The drivers of these newer cars may thus be able to avoid a collision, which may result in fewer single-vehicle accidents or multi-party collisions where avoiding action was possible. Provided the influence of ABS, ESC and other such driver aids reduce both single-vehicle and multi-party accidents in similar proportions then they will not confound the OR or ROR measures, but could affect the ARR measure if mileage covered remained the same. This effect will only matter if the increase in such aids corresponds to the introduction of DRL.

As an example, research in the USA (Kahane, 1994) suggested that the use of ABS:

- reduced the frequency of multi-party accidents in wet conditions by 14%
- had no effect in dry conditions,
- reduced the frequency of collisions with pedestrians and cyclists by 27 % in all weather conditions
- increased the frequency of single vehicle accidents involving running off the road and/or colliding with fixed objects by 19%.

The net effect on all accidents was close to zero. The fitment of ABS to passenger cars has been increasing steadily since about 1985 and if the fitment of ABS was increasing significantly during the period of any study of the influence of DRL could have substantial affects on the results. The ARR would not be expected to be affected (close to zero overall influence) but the OR could be substantially affected because of a decrease in multi-party accidents and an increase in the single vehicle accidents forming part of the control measure.

Another example may be that newer cars may have xenon headlights, which may help to prevent night-time accidents because drivers are better able to see the road geometry and potential hazards.

Reducing all night-time accidents will have no effect on the ARR and ROR measures, but will increase the OR measure. Similarly, campaigns or other driver aids which reduce the night-time accidents will have similar effects, but they will only matter if introduced at the same time as DRL.

The number of accidents is related to the distance and type of driving. Hence if there was some reason why the exposure to risk was reduced because of overall changes of driving patterns, perhaps due to an increase in fuel charges then the numbers of accidents would fall. Provided this was a proportionate effect across all accident types, assuming that it coincided with the introduction of DRL, then there would be no impact on the three measures being considered.

3.2 The study quality assessment approach

Each of the studies included in the meta-analysis have been given a study quality rating. This has been defined as a weighted combination of assessments on four criteria, each of which were judged on a 3 or 4-point scale:

- Specification of accident severity (relative weight=0.15)
- Specification of types of accident (relative weight=0.15)
- Control for confounding factors (relative weight=0.60)
- Information on DRL use (relative weight=0.10)

The maximum score possible was 2.9, and the minimum was zero. The computed scores were normalised to a 0 to 1 scale by dividing by 2.9. As stated in the interim report 2, the factors and weightings used to derive the study quality scale are arbitrary but transparent.

Section 4.4.2 of the Interim Report 2 (IR2) describes a sensitivity analysis where the data were either weighted by the study quality or were not. The results from this analysis showed very little overall difference in the size of DRL effect, regardless of which of the three measures was used. The maximum difference was 1 percentage point, with an increase in effect size for ARR and a reduction of effect size for OR. The ROR measure showed no change. The conclusion from this sensitivity study was that applying the study quality rating, as defined, makes very little difference to the conclusion suggested by any of the three measures of effectiveness.

The fact that the application of the study quality measure makes little difference to the results, suggests that the application of alternative (but probably similar) quality weightings would also make little difference.

The study quality rating focuses very much on the ‘control for confounding factors’ (relative weight of 0.60). This is clearly an important consideration because, as seen above, confounding factors can influence one or more of the measures being used. There are an infinite number of factors that could be included when devising a quality scale, however the authors have selected four very relevant factors and it is difficult to think of others which may be of greater importance. Given the relative insensitivity of the quality measure on the results, then it is difficult to consider anything which would be more informative.

It is worth recalling that the meta-analysis takes into account a statistical weight which is determined from the size of the data set and the variance of the data. The statistical quality of the data is always taken into account in any analysis. The sensitivity analysis combined both the statistical quality and the study quality measure. The fact that the inclusion of the study quality measure made little difference to the results suggests that the statistical quality is of much greater importance.

3.3 The quality of level of injury data

The meta data presented in Appendix 1 of interim report 2 identifies the type of accident severity where it is available. However, for 42% of data records the level of injury is not known and the data may include fatal accidents, injury accidents and/or property damage only accidents. Most of the other data records relate to injury accidents (44%). Just 6% of records used within the meta-analysis are for accidents where there was a fatality and 8% of analysis records were for property damage only

Table 2 Count of all car and m/c records by accident severity and model factor

Factor	Value	Severity				Total
		Fatal	Injury	Not known	Property damage only	
Level	Aggregate	0	29	1	1	31
	Intrinsic	5	8	44	8	65
Period	Pre-1990	0	13	6	4	23
	1990+	5	24	39	5	73
Location	Non-USA	0	13	6	4	23
	USA	5	24	39	5	73
Total	Total	5	37	45	9	96

Note: Aggregate studies are defined as those where the original individual research report reported data as an aggregated (national) level estimate of the effect of a DRL law or campaign. Intrinsic studies reported the intrinsic findings of a specific study which may have been based on a sample and may be vehicle type related.

The type of accident was included in a multivariate-analysis of aggregate effects on cars or motorcycle as reported in section 4.3.4 and as shown in Table 5 of interim report 2. The numbers for fatal and property damage only are small and it is likely that the associated confidence intervals on the estimates in Table 5 would be very wide. As was stated in interim report 2, these confidence intervals could not be derived by the statistical package being used (LIMDEP). This is commented on in section 5.6 where it is stated that is was not possible to quantify the effect on fatal accidents with any great precision.

According to the counts of records available, as shown in Table 2 above, there were no aggregate data available for fatal accidents; hence the results reported in Table 5 (see Appendix B) must have been derived from the multivariate model analysis which included intrinsic and aggregate data and where an adjustment has been made in order to derive aggregate effect sizes. It is stated that the derivation of these effects involved a *'fair amount of extrapolation'* and so it is unfortunate that the quality of the estimates (as judged by their respective confidence intervals) could not be supplied. The derivation of the aggregate effect has been justified by Elvik *et* al (2003) because it is these values which are required for use in the cost benefit analysis.

The fact that for 47% of the data the accident severity is not known means that this data cannot be used to provide more precise estimates for the DRL effect on fatal, injury or damage only accidents which would be the case if the severity was known. The estimates of the size of effect for the not known severity cases are in the same direction as those for the DRL effects on fatal and injury accidents, i.e. a reduction, whereas the DRL effect on the OR and ROR measures for damage only accidents is estimated as an increase.

It would be interesting to repeat the multivariate analysis but to exclude the 'not known' severity accidents, this would reduce the sample size but would show how these cases may have affected the parameter estimates. If the not known cases were not strongly confounded with other model parameters, then there should be little impact on the severity effects as estimated from this reduced model. However, it is possible that excluding the 42% of records which had not recorded injury level may change the estimated parameters and so change the results given by the multivariate analysis.

It can be seen in Table 2 that most of the not known accidents are for intrinsic effects, post 1990 and from USA studies. Hence excluding the not known accidents in the multivariate model may affect the estimated parameters associated with the intrinsic/aggregate, the period and the location. This may then affect the estimated effect sizes as given in table 5 of the report. It is not possible to say by how much these effects will change without producing the reduced model.

3.4 Size of effects for severity of accident as used for the Cost Benefit Analysis (CBA)

Section 6.1.3 of interim report 2 states that, given the evidence, it will be assumed that mandatory use of DRL will be associated with a:

- 15% reduction in multi-party daytime fatal accidents
- 10% reduction in multi-party daytime serious injury accidents
- 5% reduction in multi-party daytime slight injury accidents
- 0% reduction in multi-party daytime damage only accidents

These figures are used in the cost benefit analysis, and hence it is important that they can be justified. Their use is defended by the fact that the weighted average figure, of 5.9%, is similar to a summary figure from the meta-analysis.

It is not clear if these figures are related to the ARR, OR or ROR effect estimates, it is also not clear where the overall summary figure is reported. It is assumed that they are probably based on the ROR measure and that the intrinsic effect for all multi-party daytime accidents is about 6% - reference interim report 2 Table 3 - random effects model (see Appendix B). Furthermore, the split between serious and slight injury is not based on data examined in the meta-analysis, but presumably comes from a logical consideration of accident severity. The reductions assumed do, however, seem intuitively reasonable and clearly bear some relation to the meta-study analysis.

However, the suggested 15% reduction for fatal accidents is poorly based. There are only five studies which contribute to car fatal accident meta-analysis (see Table 3, below). Four of these have looked at the intrinsic effect and are all from the same NHTSA study in the USA. These all have a low quality rating. The other study has a reasonable quality rating and is from a French study where the aggregate effect is reported, but the study is based on fairly small numbers of accidents and so has a relatively low statistical weight.

Table 3 Data on fatal accident studies, with estimates of the mean benefit

Level	Country	Quality	OR	ROR	Statistical weight for OR	Statistical weight for ROR
I	USA	0.210	1.011	1.107	175.61	44.17
I	USA	0.210	0.949	1.280	119.64	16.49
I	USA	0.210	1.003	0.947	147.06	36.72
I	USA	0.210	0.948	1.183	99.91	14.10
A	FRA	0.600	0.413	0.413	13.90	n/k
% decrease for fatals due to DRL						
Simple mean %			13.5%	1.4%		
Quality weighted mean			25.8%	16.9%		
Statistical weighted mean*			3.1%	0.9%		
Quality & statistical weighted mean*			5.5%	0.9%		

* these estimates for ROR exclude the FRA study because of no statistical weight

Table 3 shows the data from the 5 studies and has estimated the average reduction in fatal accidents by weighting the data in different ways. It can be seen that there is considerable variation in the different estimates, The French study had by far the largest effect size and clearly has a strong influence on the estimates (it could not be included for the ROR estimate which used the statistical weight due to lack of sample size data). The difference between intrinsic and aggregate level estimates could not be taken into account in this simple analysis, whereas it was within the multivariate analysis as reported in Table 5 of the report.

It is suggested that the ’best’ estimate will take into account the statistical weight and should also take into account the quality weight, hence a figure of 5.5% for the OR measure provides the ‘best’ estimate. (The ROR measure excluded the influential French study and so is not so representative). This is somewhat different from the 15% being proposed, even allowing for the fact that no allowance was made for the intrinsic/aggregate difference which at most is about 2% (reference section 4.3.2).

It is accepted that this simple analysis of studies relating to the effect of DRL on fatal accidents is somewhat limited, and that the multivariate analysis as reported should be more precise. However, given that the estimate for the effect on fatal accidents is determined by the 5 cases listed above, then it can be argued that the assumption of 15% is a little inconsistent. It is unfortunate that the report did not supply the standard errors or confidence intervals associated with the model parameters estimated in the multivariate analysis; this may have helped to assess the quality of the 15% figure. A copy of the spreadsheet including the various parameters was provided to TRL by the authors of Interim Report 2 but unfortunately this also did not include the standard errors etc. It had been hoped that the parameters in the spreadsheet could be used to simulate a wide variety of scenarios and so build up a distribution of effect sizes. This may have helped to judge the quality of the results presented but has unfortunately not been possible.

A similar simple analysis could be conducted for the size of the effect of DRL on injury accidents, but not for serious and slight injury accidents separately. This has not been done, because there are more sophisticated analyses within the report. Analyses of intrinsic effects are not reported by injury level, but the summary statistics show diversity between different measures. Generally the ARR and the ROR measures show a benefit in DRL whereas the OR measure suggests the opposite. This is of concern because, as was stated earlier, only a consistency between these three measures indicates that one can be confident of the effect on daylight multi-party accidents.

The reported analyses for aggregate effects are consistent in the direction of the effect of DRL, albeit the size of effect does vary (and it is not always statistically significantly different from zero). Table 5 (p 67) is the key table, but does not contain confidence intervals for reasons reported earlier. It is also produced by extrapolating the results from the multivariate modelling where there are no underlying supporting meta data, thus some of the extrapolations will be poorly based. However, the estimated effect of DRL on injury accidents is reasonably consistent regardless of the analytical approach used, i.e. for the OR and ROR measures it varies between 4% and 6% with an average of 5%.

In conclusion, examination of the data and of the analyses suggests that the benefit effect of DRL on fatal or injury accidents is 5%, and there may be a small increase in property damage accidents but it is probably acceptable to take it as 0%. It would thus be worth while recalculating the CBA using a value of 5% for all the accident severities, or perhaps a 5.9% effect which is the weighted average as previous discussed. It should be noted that logical arguments could be presented to support or contradict the presence of a different effectiveness for accidents of different injury severity. However, there is no evidence available to support either argument.

3.5 Inflated size of effects

One conclusion reached in section 5.1 of interim report 2 suggests that there is a tendency to give inflated estimates of the effects of DRL on accidents. This conclusion was reached by looking at a range of simulated scenarios where the “true case” is known and assessing what the ARR, OR and ROR show. The difficulty with simulating different scenarios is that they are, by definition, limited and may be ‘too artificial’. There are also an infinite number of possible simulations that can be generated. It is thus difficult to generalise from artificial simulations.

Table 4 Simulated scenarios in the report IR2

% change for accidents after DRL				% change in measures	
Multi-party daytime	**Single-vehicle daytime**	**Multi-party night**	**Single-vehicle night**	**OR**	**ROR**
20%	20%	20%	20%	0%	0%
0%	0%	50%	50%	23%	0%
0%	100%	0%	100%	38%	0%
20%	0%	20%	0%	-11%	0%
0%	0%	0%	0%	0%	0%
0%	50%	0%	0%	17%	33%
0%	0%	50%	0%	17%	33%
0%	0%	0%	60%	11%	-60%
-14%	0%	0%	0%	14%	14%
-14%	50%	0%	0%	29%	43%
-14%	0%	50%	0%	29%	43%
-14%	0%	0%	60%	23%	-37%

The simulations presented in Table 1 earlier in this review were based on a specific change to certain accident types. They indicated that if all three measures provided a very similar size of effect, then it was true and could be defended. If all three estimates are similar then they will not over- nor under-inflate the effect size, it is only when they do not agree that some will be under- or over-inflated to varying degrees which depends on the simulation data assumptions. The simulations in the report are summarised in Table 4, above. Some of the % changes to specific accident types are difficult to conceive, e.g. why should single vehicle daytime accidents change by 50%?

Ideally it would be preferable to define a distribution for accidents for each accident type being considered, i.e. multi-party daytime, single-vehicle daytime etc. A simulation allowing for the stochastic variation about a known DRL effect could then be generated across a range of scenarios. The average across a number of the simulations for the same scenario would then provide a central estimate of the effect size and of the expected variability of this estimate. In this way a level of confidence could be derived for any specific scenario. For example, if the actual DRL effect was a decrease of 5% then the simulation may also show an average of 5% but with a 95% confidence interval from 1% to 9%.

It was suggested that any bias in the estimated benefit was due to the set of simulation scenarios used and that they served a very useful function in showing how bias could be obtained and that this could be quite substantial. It would be feasible to produce simulations which produced different biases and they would be quite tenable scenarios. It is not possible to estimate the size of any bias from actual data, nor perhaps wise to generalise from a limited number of simulated results.

3.6 Novelty effects of DRL

The overall effectiveness rate suggested for use in the CBA (Cost Benefit Analysis) assumes that the effectiveness is constant over 12 years and that it is a reduction of 15% for fatal, 10% for serious and 5% for slight accidents. This was suggested because the conclusion reached within the report was that evidence for a novelty effect was inconclusive.

However, the report suggests that the size of novelty effect for different time periods following the introduction of DRL can be estimated by taking a simple mean of the data for those periods. The effect size for multi-party daytime accidents was thus estimated as a reduction of 9% in the 1st 12-months, a 7% reduction in the 2nd 12-months and a 1% reduction in the 3rd 12-months (albeit the 3rd year estimate is poorly based), see section 5.3, p82 of interim report 2.

Those studies contributing to the estimates for the effect on accidents of the introduction of DRL are shown in Table 5. These are studies from Europe and all have a fairly good quality rating. It is clear from the table that there is no consistent pattern and the simple means as calculated do not really reflect the considerable spread of results. Overall there does appear to be a smaller effect for the second (and third) years after the introduction of DRL, but there is also a large degree of uncertainty.

Table 5 Reported reduction in accidents for 1st and 2nd years after DRL introduction

Quality	Year	Country	Measure	Reduction in accidents 1st year	 2nd year
0.60	1976	Finland	ROR	25%	13%
0.64	1981	Sweden	?	3%	-2%
0.60	1993	Norway	?	0%	0%
0.86	1995	Demark	ROR	13%	11%
0.60	1998	Hungary	?	10%	18%
Simple average as computed from above				10%	9%
Simple average as reported				9%	7%

The sensitivity of the CBA can easily be tested by assuming a diminishing effect, but if it proves sensitive (which is quite likely) then it remains difficult to reach a conclusion. The average rates for those studies available to judge a ‘novelty’ effect are limited, and have an average effect size somewhat higher than the average suggested for the CBA, i.e. 5.9%.

The averages from these 5 studies have an associated 95% confidence interval of ±8% (pooling the data), or ±12% and ±11% respectively for the first and second year. These wide confidence intervals enforce the view that these estimates are poorly based and that it is difficult to be conclusive about any novelty effect.

Logically if the use of DRL makes other vehicles easier to see, and hence avoid, while driving this should not change with time. Drivers may become used to seeing vehicles with DRL and may develop driving habits under the assumption that other vehicles will use DRL, This may not be true if some drivers do not conform, but this is unlikely to become worse with time, more the reverse. Hence it can be argued there are no strong reasons why DRL may have a ‘novelty’ effect.

3.7 Publication bias for aggregate effects

Sections 4.2.2 and 4.4.1 of interim report 2 describes the funnel analysis for DRL-campaigns or DRL-laws for cars. The analysis finds that there are probably 2 studies missing and that there is some publication bias. Studies are considered ‘missing’ if the funnel plot shows a lack of symmetry and the likely number missing is determined from a visual inspection of the plot. The analysis used the trim-and-fill approach and showed that if the missing studies were included this would result in a reduction from a 5% effect to a 3% effect, which is then not statistically significantly different from zero. The funnel plot shows a clear bias in that there are relatively few studies with a negative effect and a low weighting, i.e. there is a cluster of positive effects with statistical weights less than 200 which is not balanced by negative effects. (The summary estimate for this funnel plot suggests a 6.3% reduction, i.e. ‘summary estimate = 0.937’, however this is at variance with the 5% figure given in the text and is identical to a previous funnel plot and so one or the other may simply be an error.

Assuming that the average effect from the funnel plot is 5%, then this is less than the 5.9% average identified from the multivariate analysis. This difference is probably due to the multivariate analysis taking into account factors such as year of study, location of study and its use of intrinsic and aggregate data. The average from the funnel plot is just based on aggregate data and does not include data from other factors. If one is taking the 5.9% as the basis for the CBA analysis, then the publication bias found by the funnel analysis suggests that this should be reduced to 3.9%, i.e. by simply assuming that the bias has increased the estimate by 2%.

3.8 Other possible biases

It was noticeable that most recent studies (with one exception in France) were based in the USA or Canada and were estimating intrinsic effects. Most of the other studies were based on aggregate effects and were mainly from European countries. Given that the types of roads and road traffic laws, especially with respect to speed limits, do differ between North America and Europe then there may be biases in the data due to these confounding factors.

The analysis may also be influenced to a large degree by one or more large study. For example, the Farmer and Williams study in the USA, 2002 had a statistical weight 4 times larger than any other study, although it had a low quality rating. An analysis which excluded each study in turn found this particular study to be very influential and without it the conclusions would have changed considerably. Excluding this one study changed the intrinsic effect for cars from a 1% increase to a 12% decrease.

The multivariate analysis takes into account a factor for intrinsic/aggregate effects, USA/other and pre/post 1990 and as such makes some attempt to control for these factors. However, as is stated in the report, the estimates involve a *“fair amount of extrapolation”*. In the UK context the estimates obtained are as representative as possible, being based on non-USA, post-1990 to a declared accident severity and as an aggregate effect. It is unfortunate that confidence intervals for the estimates were not available because then at least the precision could be judged, if not the bias.

3.9 Rear impact accidents

There were 8 studies reported from European countries that investigated the aggregate effect of DRL on rear-end injury accident collisions. The estimates vary from -32% to +88% change on the effect of DRL and most estimates from the European studies suggest an increase in the accident measure. The statistically weighted average varies from +7% for the ARR measure to +30% for the OR measure.

There were 7 reported studies, mainly from North America, on the intrinsic effect of DRL on rear-end collisions (not known, injury and damage only). The effect size varies from -56% to +18% with the statistically weighted average varying from -14% to -19%. Most of the U.S. studies suggest a reduction in the accident measure due to DRL.

These summary statistics confusingly suggest that there is a reduction of the intrinsic effect and a dis-benefit to the aggregate effect on rear-end collisions with the introduction of DRL. However, these effects are largely confounded with USA or European studies respectively, i.e. the intrinsic effects relate to an effect seen in USA studies and the aggregation effect to European studies. Thus the difference in results may be due to fundamental differences between these different road systems and traffic laws.

If the aggregate figure is correct, then it would be necessary to allow for this in any CBA, i.e. the proportion of total accidents which were rear-end collisions would need to be estimated and these would have to be differentially factored into any CBA calculation. This would have the effect of reducing the CBA ratio.

A paper by Boudewijn van Kampen (2003) shows that the proportion of rear-end accidents has been increasing within many European countries between 1992 and 1998. He shows that it is mainly due to an increase in the proportion of injury accidents compared to fatal or serious accidents. However, when looking at individual European countries (where IRTAD/CARE data were available) there was some variability. The most marked growth in the proportion of rear end accidents occurred in Italy, Netherlands, Austria and Portugal. Examining accident rates per km showed even more variation, and a noticeable growth in rates was seen for France and Greece. There was a decrease in rates for Sweden, Netherlands and to a degree Ireland. The risk rates include both daytime and night time accidents and so do not necessarily reflect use of DRL.

It would be interesting to conduct a world-wide study looking at the accident rate per km over the last 20 years, for accidents of different types and severities and relate them to changes in legislation, especially for the introduction of DRL. In this way a noticeable intervention effect may be detected and associated with specific accident types. This would be an approach which would complement the existing meta-analysis as reported in Interim Report 2.

3.10 Dose-response

If a dose response relationship exists then it would show that an increase in the usage level of a safety feature resulted in an increased benefit. Figure 7 in interim report2 shows the increase in DRL and the effect size and it illustrates that there is no discernable relationship, i.e. there is no dose-response. However, the x-axis is the change in DRL from before to after introduction – it does not show at what level DRL are now operating. The data as presented in the appendix shows that the after level varies from 22% to 100% and the before level from 0% to 50%. There is, therefore, a variety of scenarios which may be confusing the dose-response relationship.

Ideally, this relationship would be best illustrated by an increase for the after DRL-use compared to a fixed before-DRL use value. This would then show the benefit from a common start-point. However, with the available studies for this meta-analysis, this is not tenable. The lack of a dose response relationship in this study may, therefore, be entirely due to limitations in the available data and does not necessarily mean that a relationship does not exist.

3.11 Multivariate analysis model

The multivariate model used in Interim Report 2 was provided to TRL but TRL had only very limited time remaining available in order to carry out analysis with it. The LIMDEP software, as used by Elvik et al (2003) in Interim Report 2, was not available to replicate fully their analysis, however a weighted least squares regression on the log of the OR measure was conducted. It is accepted that this approach could only give approximate results, but it did enable standard errors and confidence intervals to be estimated for derived parameters. There was evidence of very significant collinearity between the USA/Europe factor and the intrinsic/aggregate factor which means that these factors are highly correlated and so violate the underlying assumption of independence in the analytical method, which can result in an incorrect solution being found. Excluding the USA/European factor from the analysis largely solved this problem and only then could an acceptable solution be obtained. It is observed that the Elvik et al (2003) analysis did not address this collinearity problem.

However, the estimated DRL benefit on aggregate, post 1990, European fatal and injury accidents were similar to those derived by Elvik et al. The confidence intervals for these odds ratio estimates spanned 1.0 indicating that they were not statistically significantly different from a zero benefit. This combined with the finding that the regression parameters were not statistically significantly different from zero suggests that the results from this exercise, and by association those of Elvik et al, should be treated with some caution.

It was also observed that the Elvik et al (2003) multivariate analysis included a parameter for the quality of the study. Only a weak negative relationship exists between quality and the odds ratio, see Figure 1. However, the estimation of the odds ratio for aggregate, post-1990, European for each class of accident severity includes the quality measure. The estimates generated from the multivariate model assume that the data quality has a value of 1.0, i.e. the data fully meet all of the quality requirements. It is difficult to understand quite what this means, but assuming the data is of 100% quality makes a significant contribution to generating a DRL benefit as compared with a DRL dis-benefit as measured by the odds ratio.

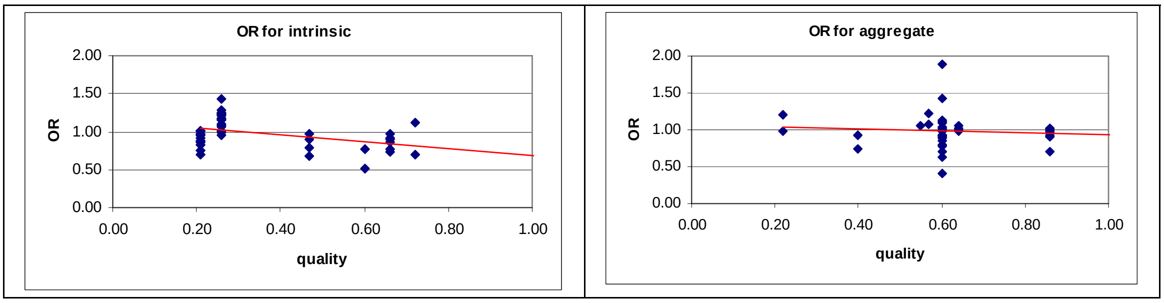

Figure 1. Relationship between quality parameter and odds ratio

3.12 Summary

The following points were identified in Section 3.

- If the estimates for the safety effect of DRL for the three metrics (ARR, OR and ROR) agree then one can be confident that it is a ‘real’ effect.
- It is tenable that changes not due to DRL can influence the measured effect size, but probably not in a consistent way across all indicator measures.
- There is confounding of factors in the meta-data, i.e. most USA studies identify intrinsic effect whereas most European measure aggregate effects, also most of the not known severity of injury category is from post 1990, USA intrinsic effect studies.
- The size of effect by injury category is poorly based and it is difficult to defend the benefit effects used in the reported cost benefit analysis. This is especially true for the benefit suggested for fatal accidents.
- There is no strong evidence for novelty effects of DRL or of a ‘dose-response’.
- There is some evidence of publication bias, which suggest an over-estimation of about 2% on the estimated effect size.
- The results using the multivariate analysis required a considerable degree of extrapolation; confidence limits on these results were not available. Results from a similar (though more simplistic) analysis suggest that these estimates are probably not statistically significant.

4 Environmental effects of DRL

4.1 Introduction

The final report (TNO, 2003) of the EU daytime running lights (DRL) project comments on the effect of DRL on fuel consumption:

> *“For both aspects, an increase in the order of 0.5-1.5% was estimated*”

These summary findings are not expanded on within the final report. However further information was contained within the interim report 2 (Elvik *et al*, 2003). This interim report presented data drawn from elsewhere. Of specific interest in relation to the environmental effects was the comment:

> "*For small cars using petrol, overall* [fuel] *consumption per year has been estimated to increase by 1.6% when DRL are used*"

This section of this report aims to review these findings and determine the effect of DRL on fuel consumption and exhaust emissions.

4.2 Background

The Swiss research institute EMPA investigated the effect of electrical ancillaries on the emissions and fuel consumption from a sample of petrol cars (Soltic *et al*, 2003). Table 6 gives the typical electrical power consumption of various electrical ancillaries commonly used during normal driving. Six vehicles, of between 44 kW and 150 kW rated power and 983 kg to 1621 kg mass, were driven over various driving cycles, intended to replicate typical Swiss driving conditions and ranged from motorway to urban stop go driving situations, on a chassis dynamometer. Vehicles were given a preliminary warm up before emissions were measured over the test cycles.

Table 6. Typical power consumption of electrical ancillaries

Electrical consumer	Power (W)
Dipped headlights#	160
Full beam headlights#	170
Full beam headlights + additional lights#	330
Front fog lights	110
Rear fog lights	42
Radio	15
Rear-window demister	150
Seat heating	150
Fan	60
Rear wiper (without contact)	40

When the lights are turned on, in addition to the front headlights (~55W each), there is also power demand from the front sidelights (~5W each), tail-lights (~5W each) and various dashboard illumination and warning lights.

Emissions were measured under three conditions of varying electrical load:

- Basic – with basic electrical load (fuel pump, injection, ignition *etc*)
- Minimum – with basic electrical load and the dipped headlights switched on (called "passing lights" in the paper). This used an *additional* 160W of electrical power
- Maximum – with basic electrical load and all permanent electrical auxiliaries active except for the front wipers (due to safety reasons) and with the rear wipers not contacting the window (presumably to avoid wear/damage that might occur using a dry window). This used an *additional* 632W of electrical power. (Note air conditioning was not included in electrical ancillaries)

The results of this investigation, which were subject to relatively poor repeatability, are summarised in

Table 7. Unfortunately only simple summary statistics (mean and standard deviation) were provided, and no data were presented on the standard error or number of tests, so the significance of the changes cannot be definitively established. However, based on the wide standard deviations, high statistical significance is doubtful. The possibility of electrical charge entering or emanating from the battery was considered as a possible explanation for this variability. Emission results usually exhibit poor repeatability and determining the effect of such small load changes on emissions or even fuel consumption would be expected to be difficult.

Table 7. Effect of electrical load on relative emissions and fuel consumption

Pollutant	Basic load		Basic load plus passing lights[2] (160W)		Basic load plus all ancillaries (632W)	
	Mean % basic load	SD/mean +/-% basic load	Mean % basic load	SD/mean +/-% basic load	Mean % basic load	SD/mean +/-% basic load
Carbon monoxide (CO)	100	82	104	91	94.33	90
Hydrocarbons (HC)	100	85	77.9	117	80.44	>120
Oxides of nitrogen (NO_X)	100	77	93.63	80	115.3	75
Fuel consumption (FC) & carbon dioxide (CO_2)	100	9	101.6	9	107.6	8

Standard deviations (SD) are estimated.

The emission measurements indicated that NO_X emissions would increase by 15%, HC emissions decrease by 20% and CO emissions decrease by 10% under the 632W electrical load condition. This trend would be consistent with theory, since higher engine loads are associated with an increase in NO_X formation and lower HC emissions due to the higher combustion and exhaust temperatures. However, at the lower load of 160W these trends were not replicated, with NOx decreasing by 6%, HC increasing by 17% and CO decreasing by 9%. However, the estimation of emission changes associated with these relatively small changes in electrical load are uncertain, particularly given the poor repeatability of regulated emission results. The most consistent emission measurements are associated with CO_2 emissions (which can be considered to be proportional to fuel consumption). An average increase of 1.6% was measured from a 160W electrical load and an average increase of 7.6% from a 632W electrical load, although it is unclear if even these changes were statistically significant. The conversion efficiency from the chemical energy contained in the fuel (based on the lower calorific value) to the electrical power developed by the alternator to run these ancillaries can be determined from these data. The overall ‘real world’ efficiency for all the driving cycles was calculated to be 23.2%.

4.3 Increased fuel consumption and CO_2 due to use of DRL in the UK.

If it is assumed that these test vehicles, test cycles and resultant measurements are broadly representative of the UK situation, the CO_2 emission results could potentially be used as a basis for estimate of the influence of DRL on fuel consumption, after allowance for the engine load differences. There are three possible approaches to calculating the effect of DRL on fuel and CO_2:

Method 1:

The most straightforward method is to divide the DRL electrical load by the ‘real world’ conversion efficiency and lower net calorific value of the fuel to calculate the extra fuel used.

$$fuel\,used\,(g/s) = \frac{DRL\ power\,(J/s)}{(\text{conversion efficiency x lower calorific value of gasoline (J/g)})}$$

The DRL power ratings are assumed to be:

- Using 21W dedicated DRL, a total additional power requirement of 42W (two 21W lights) – assuming that no other lights turn on when the DRL are in use

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[2] definition of passing lights: “In Austria, the daytime use of passing lights (dipped headlights) became mandatory on the 15th November 2005 for all vehicles”. http://66.249.93.104/search?q=cache:ZZleNUUJKfEJ:www.theaa.com/motoring_advice/touring_tips/AA_Austria.pdf+what+are+%22passing+lights%22&hl=en&gl=uk&ct=clnk&cd=20

- Using 55W dipped headlights, a total additional power requirement of 160W (two 55W headlights, two 5W tail-lights plus various dash lights)
- The net (lower) fuel calorific value is 42.7 MJ/kg.
- The overall 'real world' efficiency is 23.2% (taken from the EMPA study).

Using this method, the 42W (dedicated DRL) and 160W (dipped headlights) loads would produce an increase of 4.24 mg/s and 16.15 mg/s of extra fuel.(what units are mg/s? is it possible to express as a % increase?)

Using conversion factors of 2.31 for converting litres petrol to kg CO_2 and a petrol density of 0.737 kg/litre (an overall conversion factor of 3.13) these 42W and 160W loads would produce an increase in CO_2 emissions of 13.3 mg/s and 50.6 mg/s respectively.

This approach can be used across a wider range of cycles and vehicles to that used in the EMPA study, since it uses the efficiency of the conversion process to calculate an absolute emission. It is however still dependent on the results from the study itself being reasonably accurate and statistically significant.

Method 2:

An alternative method of calculating the increased CO_2 emissions from DRL is to establish the proportionate increase in CO_2 per unit of electrical load from the EMPA study and to multiply this by the DRL power use.

$$\text{DRL } CO_{2\ \%\ \text{increase}} = \text{DRL}_{\text{watts}} \text{ x}[(CO_{2\ \%\ \text{increase from 160w}} /160 + CO_{2\ \%\ \text{increase from 623w/}}/632)/2]$$

The increase in CO_2 for the 160w and 632w electrical load used in the study was 1.6% and a 7.6% respectively. This produces an average increase of 0.011 % in CO_2 emissions per watt of electricity used. Using this figure, additional loads of 42W and 160W would produce an increase in CO_2 emissions of 0.46% and 1.76% respectively.

However, this estimate is sensitive to the average power used in a typical driving cycle. Therefore, if this is to be used for estimating the effect of DRL, it is important that the cycles and vehicles used in this study are broadly representative of the UK situation which is far from clear. To illustrate this, the average power of these cars, if they were driven over the New European driving cycle (NEDC), would have been only 3.3 kW[3]. This is the regulatory cycle for petrol cars in the UK, but is sometimes regarded as being untypical of real driving situations due to its light load conditions. The 42 and 160 watts of electrical power required for DRL would use 84 and 320 watts of engine power assuming an alternator efficiency of 50% (Bosch, 1987). This translates to a 2.55% and 9.70% increase in the power used over the NEDC cycle, substantially more than the CO_2 increases in the typical Swiss conditions noted above. It is difficult to translate this figure to CO_2 emissions without a more detailed study, but this implies that expressing increased emissions as a proportion of the total could lead to substantial errors unless the test conditions are typical.

Method 3:

Whilst the assessment of fuel consumption changes may be measured under controlled laboratory conditions, the short duration and limited budget of this project prohibited this approach. Therefore the effect of additional electrical load on exhaust emissions was assessed using a third modelling approach.

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[3] also quoted from Soltic and Weilenmann (2002), unfortunately the power of the composite Swiss cycle was not provided as a comparison.

The EU fifth framework ARTEMIS[4] project and the COST Action 346[5] provided new insight into the emission behaviour of modern vehicles. One of the main outputs of these projects was the development of a model capable of accurately simulating emission factors for all types of vehicles over any driving cycle and for various vehicle loads and gradients. The resulting tool – PHEM (**P**assenger car and **H**eavy-duty **E**mission **M**odel) **-** estimates fuel consumption and emissions (CO, THC, NO_x and PM) based on the instantaneous engine power demand and engine speed during a driving cycle specified by the user. The model combines steady-state engine maps with correction functions for transient operation (Rexeis *et al.*, 2005).

With a given driving cycle and road gradient the effective engine power is calculated at one second intervals from the driving resistances and losses in the transmission system. The actual engine speed is simulated by the transmission ratios and a driver's gear shift model. The emissions are then interpolated from engine maps. This method was originally developed for Heavy Duty Vehicles (HDVs), where typically engines are measured on engine test beds. The application of this approach to passenger cars required the development of a suitable method to derive engine emission maps from engines and emission measurements routinely undertaken on a chassis dynamometer. Steady state engine maps can be measured on the roller test bed with sufficient accuracy, but these measurements were not included in the basic input test programmes (ARTEMIS) for the model development. Therefore, an approach, already used for established instantaneous models, was applied to derive engine emission maps from transient vehicle tests (Weilenmann *et. al.*, 2002). To ensure a more reliable basis, these measured instantaneous emissions were corrected for analyser response times and the variable transport time in the measurement system (Zallinger *et. al.*, 2005).

Based on the driving resistances and the transmission losses, the engine power is simulated second per second. The actual engine speed is calculated from the transmission ratios, the wheel diameter and the gear shift rules from the actual test cycle. The resulting instantaneous data on engine power, speed and the exhaust gas emissions are used as engine maps within the model.

From TRL's database of in-service driving cycles, 122 typical passenger car driving cycles were selected to represent a range of average speed driving conditions between 5 and 120 km/h. Emission and fuel consumption estimates were derived using PHEM over each of these 122 cycles for:

- typical petrol and diesel passenger cars (see brief vehicles specification in Table 8, which was used as input data for the PHEM model),
- a range of vehicle Euro emission classes (Euro 0 to Euro IV),

The PHEM model was then re-run for two scenarios, firstly with the additional power requirements (160W) of dipped headlights, and secondly with the additional power requirements (42W) for the use of dedicated DRL. It was assumed that the efficiency of the alternator was 50%, hence the additional engine loads of 320W and 84W were used respectively.

Table 8. Vehicle specifications used in the PHEM model

Vehicle	Rated power (kW)	Vehicle weight (kg)
Petrol car	65.9	1173
Diesel car	78.8	1421

4.4 Results

For each fuel type (petrol and diesel), for each Euro class (Euro 0 to Euro IV), for each scenario and for each pollutant, 122 results were produced (one for each driving cycle). Figure 2 shows an example

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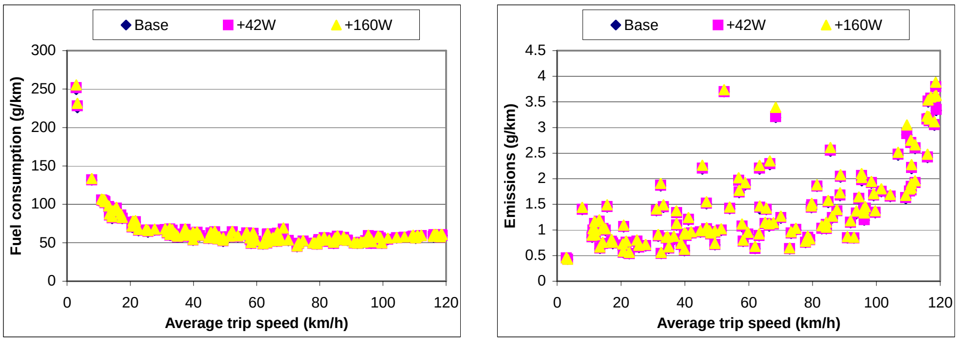
[4] http://trl.co.uk/artemis

[5] http://www.cordis.lu/cost-transport/src/cost-346.htm

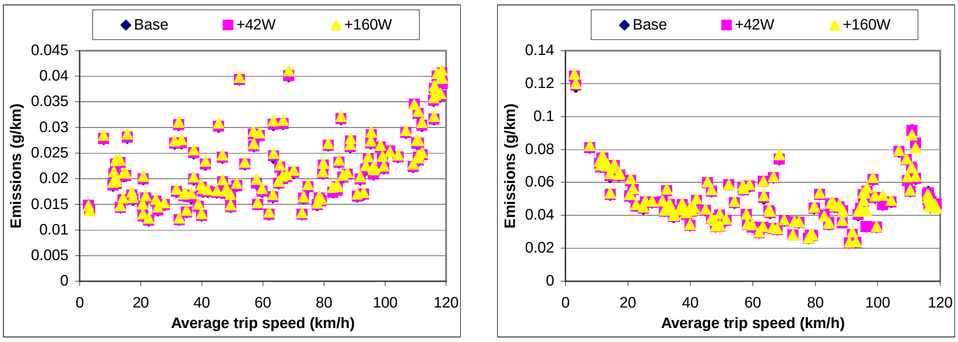
of the predicted emissions plotted against average trip speed. The presented data are for a Euro III petrol car. The figure show the predicted fuel consumption, CO, HC and NO_x emissions plotted on separate graphs. Each graph contains three data sets – the base case, with the addition of 42W lights (*i.e.* dedicated DRL) and with the addition of 160W lights (*i.e.* dipped headlights). However, the predicted values for each scenario are very similar (the data points are plotted almost on top of one another), making it difficult to distinguish the three sets of data.

These same data are used for the graphs shown in Figure 3, but this time they are shown as the relative change with respect to the base case. This time the effect of the two ‘use of lights’ scenarios can be clearly seen. The use of dedicated DRL produce a very small increase in fuel consumption and emissions while the use of dipped headlights produces a larger increase. Most of the driving cycles resulted in similar relative changes in fuel consumption and, CO and NO_x emissions. However, there are a few outliers – some with very low or negative changes (*i.e.* an improvement) and some with very high increases. This is likely to be due to the characteristics of that particular driving cycle. The effects of a constant additional load on a driving cycle with a very high power demand (*i.e.* lots of high accelerations) on the engine is likely to be very low, whereas the effect will be more significant on a driving cycle with a low power demand (*i.e.* near-constant speed cruising with minimal accelerations).



a. Fuel consumption

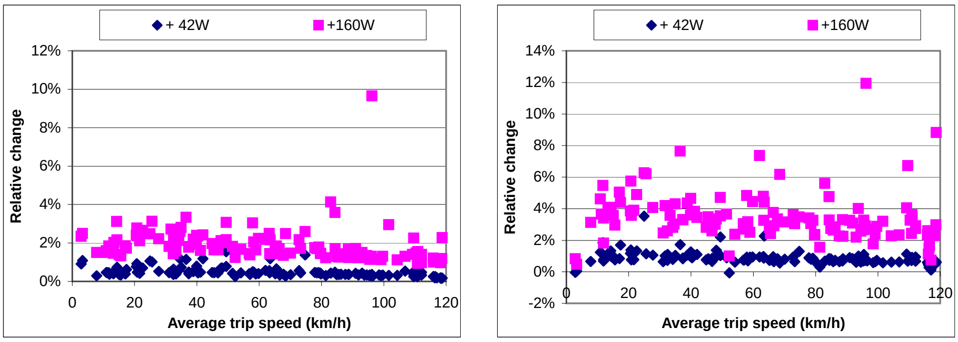
b. CO emissions



c. HC emissions

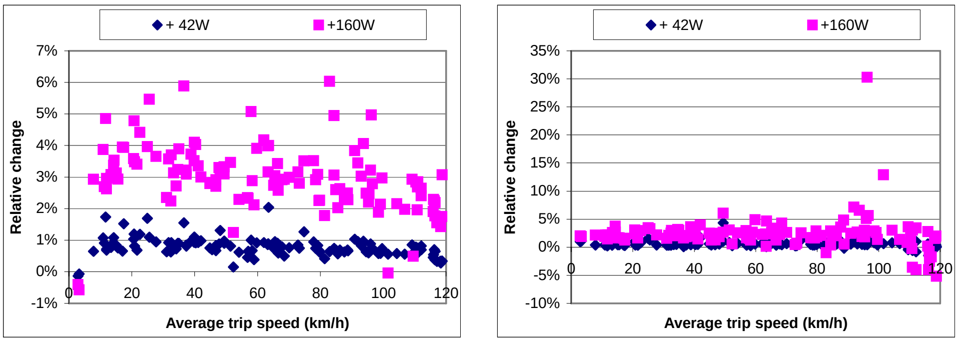
d. NO_x emissions

Figure 2. Example of the estimated emission and fuel consumption rates derived from the PHEM model.
Vehicle class: Euro III Petrol car



a. Fuel consumption

b. CO emissions



c. HC emissions

d. NO_x emissions

Figure 3. Example of the relative changes in fuel consumption and emissions
Vehicle class: Euro III Petrol car

The average relative changes for each vehicle category are shown in Table 9 and Table 10, for dedicated DRL and dipped headlights respectively. Dedicated DRL result in about a 0.5% increase in fuel consumption whereas dipped headlights increase fuel consumption by 1.8 to 1.9%. There is also an increase in the emissions. Of most concern, due to their air quality impacts, are NO_x and PM. The use of dipped headlights increases the emissions of NO_x by over 2% and increase PM emissions from diesel cars by between 0.8 and 1.8%. However, it should be noted that variations in driving styles/speed *etc.* produce much greater variations in emissions.

Tables containing the actual emission estimates, together with the 5th, 50th and 95th percentiles are presented in Appendix A. The 5th and 95th percentiles show the range of changes that may occur (while excluding the outliers at either extreme – very low and very high). These are also present graphically in Appendix A.

Table 9. Relative change in average emissions: Dedicated DRL (+42W)

Vehicle type	FC	CO	HC	NOx	PM
Petrol Euro 0	0.21%	0.54%	0.21%	0.76%	
Petrol Euro I	0.52%	0.77%	0.73%	0.57%	
Petrol Euro II	0.52%	0.77%	0.73%	0.63%	
Petrol Euro III	0.52%	0.77%	0.73%	0.57%	
Petrol Euro IV	0.50%	0.44%	0.07%	0.49%	
Diesel Euro 0	0.46%	0.30%	0.12%	0.51%	0.20%
Diesel Euro I	0.46%	0.30%	0.12%	0.51%	0.20%
Diesel Euro II	0.46%	0.31%	0.11%	0.51%	0.20%
Diesel Euro III	0.47%	0.97%	0.36%	0.59%	0.44%
Diesel Euro IV	0.48%	0.97%	0.37%	0.60%	0.44%

Table 10. Relative change in average emissions: Dipped headlights (+160W)

Vehicle type	FC	CO	HC	NOx	PM
Petrol Euro 0	1.11%	1.97%	0.80%	2.85%	
Petrol Euro I	1.89%	3.26%	2.78%	2.15%	
Petrol Euro II	1.89%	3.26%	2.78%	2.30%	
Petrol Euro III	1.89%	3.27%	2.79%	2.18%	
Petrol Euro IV	1.82%	2.10%	0.86%	1.56%	
Diesel Euro 0	1.82%	1.16%	0.48%	2.01%	0.84%
Diesel Euro I	1.82%	1.16%	0.48%	2.01%	0.84%
Diesel Euro II	1.82%	1.16%	0.48%	2.01%	0.84%
Diesel Euro III	1.89%	3.70%	1.50%	2.32%	1.84%
Diesel Euro IV	1.89%	3.71%	1.50%	2.32%	1.83%

4.5 Conclusions

Method 2 predicts that dedicated DRL and dipped headlights would increase CO_2 emissions by 0.46% and 1.76%, respectively. Due to a near linear relationship between fuel consumption and CO_2 emissions, this would also produce similar increases in fuel consumption.

The analysis using the PHEM emission model predicted similar increases in fuel consumption. Dedicated DRL result in about a 0.5% increase in fuel consumption whereas dipped headlights increase fuel consumption by 1.8 to 1.9%. There is also a small increase in all exhaust emissions.

However, these increases occur on a per trip basis. Using the assumption that 55% of travel in terms of distance occurs during daylight hours (Koornstra *et al*, 1997), this means that these derived increase should be multiplied by a factor of 0.55. From the PHEM results, this gives an average annual increase in fuel consumption and CO_2 emissions of 0.28% from the use of dedicated DLRs and an increase of 1.0% from the use of dipped headlights.

5 Experimental study of the effects of DRL on the conspicuity of vulnerable road users

The EC research interim report 3 (Brouwer *et al*, 2004), describes a literature review and experiment to assess what affect the use of DRL on passenger cars had on the conspicuity of motorcyclists and pedestrians. The DfT has asked TRL to review this report with particular reference to:

- The benefits and disbenefits of the methods used in the experiment, with particular reference to the use of photographic slide images of road scenes rather than real vehicles
- Compare the results of the work with research by Cobb (1992) and submissions to GRE from the Japanese expert.

5.1 Review of the methodology used in Interim Report 3

The experiment described by Interim Report 3 was a laboratory experiment where a range of human subjects were asked to view a photographic slide image of various road scenes and quickly identify the vehicles that were present in those scenes. The presence and relative positions of cars, motorcycles and pedestrians were varied, as were both the use of DRL on cars and motorcycles and the background of the image. The time that each subject took to correctly identify the vehicles in the scene was used as the main measure of their conspicuity in order to determine the effects of DRL on motorcyclist and pedestrian conspicuity in a range of conditions.

In general, interim report 3 described research that was well controlled and analysed using generally accepted techniques. However, there were some areas that could be open to criticism, generally relatively minor, but it was often not clear whether there were genuine short comings in the research or whether the detail had simply not been reported in order to keep the report concise. There was one shortcoming identified that appeared more fundamental and all areas are discussed in more detail below.

In very general terms, laboratory test methods have the advantages of enabling more control over the experiment and being quicker and cheaper to carry out. Their main disadvantage is that their ability to realistically represent the real world situation they are simulating can be less than that of full scale experiments with real vehicles in an outdoor test environment. However, laboratory tests are routinely and effectively used in human factors research and can be a very powerful research tool (Castro and Horberry, 2004).

One important measure that can be taken to validate the results of laboratory testing, or at least to quantify the effects of the reduced realism, is to carry out directly comparable full scale experiments with real vehicles for at least a representative proportion of the scenarios which the laboratory tests aim to evaluate. Such a validation exercise does not appear to have been carried out as part of the research programme described by interim report 3. As such, a laboratory test of this type can produce a like-for-like comparison between different variables (such as the presence/absence of daytime running lights), but the absolute validity of such laboratory measures is unproven. The interim report 3 makes reference to other research literature (Rumar, 1981; Hole & Tyrell, 1995) that also used slide images of traffic scenes in evaluations of DRL, but it is not known whether either of these experiments involved an evaluation of the method in comparison with full scale tests. However, it should be noted that even full scale tests with real vehicles on a test track may not fully replicate the demands of the driving task in real traffic environments.

Given that no evidence has been presented that directly correlates performance in the laboratory test with performance in a full scale test or real traffic situations, it is important to consider analytically each aspect of the laboratory test environment that could be considered to be different to a real environment and assess how well the experiment has controlled for this variation.

5.1.1 Visual scanning behaviour

The conspicuity of motorcycles is most frequently reported to be an issue at junctions such as T-junctions or roundabouts and each of the images presented in the trial were taken at junctions. For a driver to negotiate a T-junction successfully he or she must scan the whole forward 180 degrees of the visual scene to ensure no traffic is approaching from any direction. At junctions where the line of sight is restricted this must take place very quickly to ensure that no vehicles have come into view in the direction first scanned by the time the scan is complete and the decision has been taken to pull away from the junction. Although such scanning can in part be undertaken by peripheral vision, the active scanning can involve up to 90 degree rotation of the neck as well as movement of the eyes. In the trial reported by Brouwer *et al* (2004) the whole scene is recorded in a single photograph projected onto a flat screen that was 2.19 metres wide and was viewed from a distance of 3.6 m. In this situation the observer does not have to scan a large area to find the object that they are asked to recognise, which makes the visual task considerably easier than in real traffic situations.

Brouwer *et al* (2004) have controlled for this variation by using a method known as occlusion. In effect, the slide is only projected onto the screen for a duration of 0.2 seconds. Research has shown that 0.2 seconds is approximately the amount of time that the eye will fixate on any one particular point when scanning the road scene (Sanders and McCormick, 1993) and so the experiment shows the relevant items (i.e. car, motorcycle, and pedestrian) only for the amount of time that the eye would be expected to rest on them while scanning the full junction. This is a well accepted control measure that is used in many experiments involving object detection and has been shown in other research to show correlation with the results obtained in more realistic situations (Horberry, 1998).

There are two relatively minor criticisms of this aspect of the method. Firstly, no mask was applied after presentation of the slide. A mask would generally be a random pattern of colours/shapes that is approximately the same colour and brightness contrast as the image in the slide. The image of the slide will persist in an observer’s iconic memory for a time after the presentation of the slide has ceased and this time will vary depending on subsequent visual stimuli, meaning that the presentation time is less well controlled than it first appears. Presenting a “mask” immediately after the image would have helped to control this affect.

Secondly, the time for which the slide was presented changed from 0.2 seconds in the main experiment to 0.35 seconds in the additional experiment assessing whether observers visual limitations changed the conclusions. This additional presentation time is likely to make the identification task easier, thus meaning that results are not directly comparable to the main experiment, at least in absolute terms.

In general, in this type of experiment, the degree of difficulty of the task can have an important effect on the results. If the task is very easy (for example, if unlimited time was allowed to view the scene) then all observers would get virtually all responses correct and it would appear that there was no difference between the conditions being evaluated. This is equally true if the task is so difficult that very few observers correctly identify the objects presented. In order to gain the optimum sensitivity in the experiment it should be designed such that ‘ceiling’ effects are avoided (for example, by designing the baseline condition so that approximately 50% of the observers could correctly identify the features presented). It is worth noting that the data on error rates presented in interim report 3 shows that the variation is only between 3.8% and 14.5%. This shows that in all conditions a large majority of observers found the task sufficiently easy that they were able to correctly identify the vehicles presented.

However, there is a counter argument that a more difficult experiment does not reflect reality because it is unlikely that there is, for example, a 50% error rate in detection of motorcyclists at junctions in the current baseline conditions of no DRL. It can be very difficult to design an experiment that is sufficiently sensitive to identify differences between different configurations and involves a degree of difficulty that is representative of a real driving task. As such, this is not a direct criticism of the experiment described in interim report 3.

5.1.2 The use of reaction time as the principal dependent variable

The experiment described in interim report 3 was carried out by presenting the image to the observer and asking them to press one of two buttons, representing “yes” or “no”, in answer to the question *“was another road user other than a car present in the scene”*. The subjects were asked to respond as quickly as possible and then indicate what kind of other road user was present after the button was pressed. In this way, the researchers could identify whether the observer had correctly identified the presence and type of the other road user and also the reaction time between presentation of the slide and pushing the button.

Although both dependent variables were recorded, most of the results presented in interim report 3 show the findings with respect to mean reaction times. A logical analysis of the task of correctly identifying the presence of vulnerable road users at a junction would suggest, perhaps, that the frequency of correct identification was of more importance to safety than the reaction time. When a driver is scanning the visual scene at a junction and their eyes are fixating on individual points for very short durations of time (approximately 0.2 seconds), it is very important that in the short time the eyes are looking towards a vulnerable road user that they correctly identify their presence. The time that the participant takes to process this information and activate a response (in the case of the trial pressing a button, in a real situation perhaps looking back to confirm the presence, pressing the brake to stop vehicle movement, or simply taking the decision not to move) may be less important (at least, within certain limits). If the driver fails to correctly identify the presence of the other road user in the original scan the time taken to process that information and act becomes largely irrelevant because the information based on which he/she is acting is false.

In the context of the trial, the image is presented for only 0.2 seconds yet the typical reaction times were slightly less than a second. Based on the data presented in Table 2 of the interim report, the mean reaction time varied from a minimum of 0.809 to a maximum of 1.048, a range of 0.239 seconds. This range in the means is in excess of the duration of the slide presentation and the total range of responses will, by definition, have been greater still. Most of the reaction time therefore occurs after the presentation has disappeared from view and it is not entirely clear how the use of this measure directly relates to the level of safety in a real driving task..

Two alternative methods could possibly be considered to be more logical. If reaction time was considered the main dependent variable then it may possibly have been better to present the slide for an unlimited amount of time and measure how quickly the observers responded with a correct answer. In this situation observers would be asked to ensure they gave the correct answer and very few incorrect identifications would be expected. This method would also mirror ‘real’ driving in which a driver may choose to fixate on a particular section of the road scene (for example, a potential hazard) for a longer period of time.

Alternatively, the slide could be presented for the 0.2 second glimpse only but subjects could be instructed to try to respond correctly without the instruction to respond as quickly as possible, thus reducing the chance of forcing incorrect identifications because of the time constraint.

Given that two qualitatively different response measures were used (speed and accuracy), it seems that the precise experimental instructions may have a large effect on whether a participant responds quickly or accurately. It must be noted that the report states that *“the pattern of errors followed the pattern of reaction times”* and that *“these results therefore suggest that there is no speed-accuracy trade-off”*. Although this is undoubtedly the correct conclusion from the results they obtained, having different experimental instructions (for example, emphasising the accuracy component, as would be done in ‘real life’), might have produced different results.

There is relatively little information presented in the report on the error rates found so it is difficult to independently assess the influence of this dependent variable. However, Table 2 of the interim report did present data on both dependent variables in aggregate form. Although no statistical analysis was possible based on the information presented it was possible to do a very simple comparison of the effects measured by each variable on the basis of simple ratios of the results for each of the factors and conditions presented. This is summarised in Table 11, below.

Table 11. Comparison of reaction time and errors as dependent variables based on simple ratios of information presented in Table 2 of Interim Report 3

Factor	Conditions compared	Ratio of reaction times	Ratio of errors
DRL	Off/on	1.014	1.18
Other Road user (OR)-expectancy	20%/80%	1.300	1.32
Other road user	Cyclist/motorcyclist light on	1.013	1.22
	Cyclist/motorcycle light off	0.984	0.73
	Cyclist/pedestrian	0.900	0.55
Distance to car	Close/distant	1.014	2.33

It can be seen that both the reaction time and the proportion of errors always produce consistent results in terms of direction of change for each condition. That is, if the reaction time is longer for one condition (e.g. DRL off) there are also more errors for that condition and this is shown by a ratio in excess of 1. For example, the reaction time for DRL off is 1.014 times longer than when it is on (i.e. the reaction time is 1.4% longer when DRL is off) and the number of errors is also greater when DRL are off with a ratio greater than one. However, for the data presented, the magnitude of the effects seen is measured very differently by the two variables and the difference is not consistent for all comparisons of independent variables. For example when considering the level of expectancy of Other Road users (OR-expectancy) the mean reaction time when only 20% of slides have other road users in the view is 30% longer than when 80% of slides have other road users. In this case there are also 32% more errors when only 20% of slides have other road users in view. However, when comparing the conspicuity of different types of road user it can be seen that the mean reaction time for detecting a pedal cyclist was only 90% of that required to detect a pedestrian (suggesting the pedestrian was slightly less conspicuous than the cyclist) but the number of errors in detection of the cyclist was only 55% of the number of errors made in detection of the pedestrian (suggesting that the pedestrian was a lot less conspicuous than the cyclist).

This variation in the size of effect measured by the two dependent variables is most graphically demonstrated by analysis of the last two factors shown in Table 2 of the interim report; distance to car and background. The mean reaction time was increased by 1.4% when the other road user was close to the car rather than distant but the number of errors increased by 233% when the other road user was close. For the background factor there was an increase of 382% in the number of errors when the background was homogenous but no significant difference was found in the reaction time.

Although these data do show some large variations in the size of the effect measured by the two dependent variables the fact that they always seem to show the same direction of effect is very important for the interpretation of results. The more detailed analysis presented subsequent to Table 2 of the interim report are based on reaction time and, with one exception, all show a positive change or little effect with DRL on. The omission of the error data from the detailed analyses does leave some cause for concern but it does seem likely that the conclusions of the report would remain similar if these analyses had been based on the proportion of errors instead.

5.1.3 The use of object recognition as a sole task

Interim report 3 suggests that the sole task that the observers were asked to carry out was to identify whether or not road users other than the car were present in the road scene. In a real driving situation, drivers would not only have to identify the presence of other road users but assess their position, speed and direction and hence the level of hazard that they represent. They will have to be at least conscious of other driving tasks such as changing gear, operating the handbrake, holding the clutch at biting point ready to pull away. In addition to this, they may also be subject to indirect pressures such as concern at running late or at selecting the correct direction to take at a junction. This will have meant that the observers used in the experiment were subject to considerably less mental and physical workload than they may have been in a real driving environment. There is considerable research (e.g. Probst, 1985; Miura, 1986) to suggest that such competition for the drivers cognitive and motor resources can have a substantial affect on their reaction time. Miura (1986) also found that competition for motor and cognitive resources led to a decrease in the peripheral vision performance.

In recognition of this, it is quite commonplace to ask the observers involved in this type of research to carry out a surrogate or simplified driving task at the same time as being asked to identify objects. In this way the competition for physical and cognitive resources that is experienced in real driving is simulated, at least to an extent. The use of a secondary task in the experiment would have been likely to have the effect of making the road user identification task more difficult. As discussed previously, this would have been likely to increase the error rate in the baseline condition, thus potentially making the experiment more sensitive to different configurations of road users. It is possible that the use of a secondary task could have identified effects that did not exist in the simpler experiment and this could potentially have changed the results and conclusions from the work. Despite this, the experiment did conduct a controlled like-for-like comparison, so the relative effects of the different variables seems reasonably valid. However, without carrying out a further experiment using a secondary/ surrogate driving task it is not possible to predict what would have happened, however, it is possible that the conclusions would remain unchanged.

5.1.4 The use of photographic slides instead of real vehicles

Interim report 3 clearly states that the images were presented to observers in the form of photographic slides and that calculations had suggested that there was sufficient dynamic range available in a slide to cope with the differing light intensities whereas there was not with a modern computer “beamer”. However, no further technical details about the method of photography, the camera or the projector used were available in the interim report.

In order to use slide photos in an experiment such as this, the researcher is relying on the use of a camera and projection system as an accurate sensor of light intensity, colour and contrast. It is possible to achieve this but there are many pitfalls that need to be avoided, particularly with modern “user friendly” cameras. In order to get directly comparable photographs for use in the research it would have been essential to use a manually controlled camera with fixed exposure (i.e. fixed shutter speed, aperture, focal length etc.) for every photo taken. This means that the dynamic range (the difference between the intensity of light resulting in total black in the photo and the amount resulting in total white) must be sufficient to capture the full range of light intensities present in the scene being photographed without saturating the film (or CCD in the case of a digital camera).

If a photograph is taken of a car on a dull overcast day without any headlamps on then the exposure needs to be relatively sensitive in order to produce a clear accurate image. If a modern camera is set with automatic functions this will happen automatically. However, if the ambient light changes or the vehicle headlights are switched on then the automatic camera functions will change the aperture and shutter speed to compensate for this. Because more light is present, the camera will adjust such that the level of absolute white in the resulting photograph actually needs a greater light intensity. This means that other objects will become darker in colour in the photograph than in the original, lights off, photograph.

Although there is nothing obvious about the photographs shown in the appendix to interim report 3 to suggest that this type of problem has taken place it, there is no confirmation of the photographic equipment or methods used and there is, therefore, a possibility that the experiment could have been influenced by this.

When the photographic slide is projected on to the screen the relative intensity of different parts of the image should be reasonably accurate, subject to the photographs being correctly taken as described above and the slight non-linearities in the slide film development/print process. However, the absolute level of light intensity falling on the observers eyes will be solely dependent on the intensity of the light source within the projector and the ambient light level and the visibility of the image itself and features within it will be at least partially dependent on the contrast between the projector light intensity and the ambient light intensity. It is possible that these issues would prove sufficiently linear that only the absolute numbers of errors would be affected and that comparison between different slides would be valid.

Another factor related to the light intensities in the laboratory and of the projector that could potentially influence the results is the speed of reaction of the human eye. As previously discussed the eyes of a human driver scanning a traffic scene typically fixate on different areas of the visual scene for a time of very approximately 0.2 seconds. If a driver is scanning an area of the scene where there is only relatively low light intensity and suddenly moves their eyes to focus on a vehicle with lights then initially there will be too much light falling on the eye and it may be subject to glare effects. In steady state conditions the eyes will quickly adjust to this increased light but this takes a finite amount of time. If the eyes only fixate on this area for 0.2 seconds the eye may not adapt to the increased light level sufficiently quickly, especially for older drivers (Sanders and McCormick, 1993). If this was the case darker objects near to the lights may be less conspicuous during that brief 0.2 seconds.

If the level of light intensity from the projector is lower than the light intensity of the headlights (after factoring for viewing distances) then there is a chance that the eyes of the observers do not have to adjust quickly in the same way as they would have in the real scene. This may increase the chance of another road user being seen compared with the use of real vehicles.

Ideally, the projector light source would be of an intensity such that the level of light intensity coming from the headlights of the slide image was equivalent to the light intensity of the actual vehicle headlights, factored using the inverse square law to account for differing viewing distances in the real scene and the laboratory. Equally, the ambient lighting in the laboratory should be set to be equal to the ambient light level measured on the day the photographs were taken. Combined with the manual control of the camera settings in the photographs this would minimise the possibility that the use of photographic slides instead of real vehicles would affect the validity of the findings. It is quite possible that all of this was accounted for in the experimental methods, but interim report 3 only states that the photographs were taken on an overcast day, that the laboratory where the experiment was carried out was dimly lit and that a Kodak slide projector was used.

5.1.5 Location of road users within the scene

All of the example photographs shown in the appendix to interim report 3 showed scenes at junctions where the car and the other road users were directly in front of the camera view. In effect, this simulated a driver observing vehicles facing in the opposite direction on the other side of a cross-road. In all of the relevant examples shown, the other road user was positioned to the side of the car, thus physically separated from the car by the background view. There was no evidence presented in the report to suggest that different relative positions had been evaluated.

One of the most common and severe accident types, for motorcyclists in particular, is where a car turning 90 degrees from a side road onto a main road pulls out in front of the motorcyclist such that the motorcycle collides with the side of the car. This is the situation that most motorcycle conspicuity research and education campaigns have focussed on. It is also the situation of most concern in relation to daytime running lights because from the view of the car driver pulling out of the side road, the motorcyclist can be in front of a car with lights on such that the headlight of the motorcycle coincides exactly with the headlight of the car. This potential accident mechanism has not been investigated at all in the research reported by interim report 3.

It may potentially be more difficult to assess the sideways viewpoint required to directly simulate this accident mechanism using the photographic slide measurement because the point of view of the image would always be straight ahead. However, it would have been possible to draw at least some conclusions by positioning the car with DRL on and off behind the other road users rather than to one side. Of particular interest would have been a situation where a motorcycle with DRL was positioned in front of a car with and without DRL such that the line of sight from the camera lens travelled through the motorcycle headlight and one of the car headlights.

The conclusions of interim report 3 acknowledge this to some extent because they conclude that there is no adverse effect *"at least over the range of situations studied in the experiment"*. However, the next conclusion could be considered slightly misleading because it states that *"there is in these findings no support for the possibility that situations could exist in which a negative effect could occur"*.

It is acknowledged in the report that the contrast between object and background is critical to the detection and identification of the object. If the example photos accurately represent the range of situations studied then only situations where, from the point of view of the observer, there is background space separating the car and the other road user have been assessed. If this is the case then the report has not assessed the situation where the car forms the background for the other road user, particularly where both vehicles have lights in direct line with one another. This could potentially be a very important accident mechanism and if this was not evaluated then it is not possible to draw any firm conclusions about the effects from interim report 3. As such, the range of stimuli that seemed to have been used in the experiment were limited, and the conclusions drawn from them cannot easily be extrapolated to all situations- especially to some of the most common and severe accident types for motorcyclists (such as where a car turning 90 degrees from a side road onto a main road pulls out in front of the motorcyclist such that the motorcycle collides with the side of the car). Although any single experiment cannot cover all hazardous situations, it does mean that some caution should be applied to interpreting the obtained results.

5.2 Comparison with other DRL experiments

The experiments reported by Interim Report 3 (Brouwer *et al*, 2004) were compared with experiments carried out on the same subject by Cobb (1992) and those carried out by JARI in Japan and reported to GRE (GRE, 2003; GRE, 2004).

The three different experiments carried out all used substantially different methodologies. None was perfect and each has strengths and weaknesses. The experiments described in Interim Report 3 were laboratory tests simulating traffic scenes with photographs using tightly controlled conditions and objective measurements of participant responses but no secondary driving task. Those described for GRE (2003 & 2004) were based on the use of real vehicles in an open test environment. They used subjective evaluations rather than objective measurements, no secondary driving task and an uncontrolled viewing time. The experiment by Cobb (1992) used the most comprehensive and realistic methodology, using real vehicles on a test track designed to reflect urban road layouts with the field of view restricted by trees, controlled viewing opportunities, competition for resources in terms of driving the vehicle the participant was in and avoiding another participants vehicle taking part in the trial at the same time. The results were analysed using both objective measurements and subjective evaluation. However, the analyses were based in many cases on a relatively small number of recorded errors.

One key difference in the work was that Interim Report 3 did not assess the effects of differing intensities of head light and different ambient luminance and there was the possibility that the headlight to ambient light intensity ratios may not have been accurately portrayed by the photographic slide method used. This means that the conclusions of Interim report 3 are only valid for the relative light intensities actually studied. Both Cobb (1992) and GRE (2003 & 2004) did vary these factors.

This is reflected in the conclusions of the various reports where Interim Report 3 simply states that there is no adverse affect on motorcyclists whereas Cobb (1992) and GRE (2002) both find that there can be an adverse affect on motorcyclists but that it is possible to design dedicated DRL with an intensity that do have a positive effect on the conspicuity of the car without adversely affecting the conspicuity of a motorcycle with a DRL.

Although Interim Report 3 does not acknowledge the influence of relative light intensities, if it is assumed that the relative light intensities that were studied fell within the ranges defined as acceptable by Cobb (1992) and GRE (2003 & 2004), then there is actually consistency in the findings. Cobb recommended DRL intensities of between 150 and 600 Candelas (cd) while GRE (2003 & 2004) recommended intensities of less than 200cd. Provided that the lights evaluated by Interim report 3 (subject to any variation caused by the photography/projection processes) were less than 600cd, then there is agreement in the results of these studies. If all three studies are considered as a whole then DRL of an intensity of 200cd should provide a net safety benefit without compromising motorcyclist safety. However, this finding would suggest that the policy options presented in the EC final report that involved the use of standard passing beam headlights may potentially have an adverse affect on motorcyclist safety.

6 Sensitivity of the cost benefit analysis

One of the tasks within this review was to assess the robustness of the cost benefit analysis that was carried out, in particular, to carry out a sensitivity analysis on the input variables that were used.

6.1 Policy options for the use of DRL

Within Interim Report 2, five options relating to the proposed introduction & future use of DRL within Europe were proposed. These options, which all relate to the mandatory use of DRL within the EU, are:

- The use of DRL is required by all motor vehicles from a certain date. This is a simple behavioural measure, which does not include any new technical standards for vehicles. Drivers are simply required to turn on headlights at all times. This option will be referred to as the behavioural option.
- The use of DRL is required by all motor vehicles from a certain date. In addition, new motor vehicles sold after the same date will be required to have an automatic switching-on of low beam headlights. This option will be referred to as the behavioural plus low beam option.
- The use of DRL is required by all motor vehicles from a certain date. In addition, new cars sold after the same date will be required to have dedicated DRL that are switched on automatically. This option will be referred to as the behavioural plus dedicated DRL option.
- New cars sold after a certain date are required to have an automatic switching-on of low beam headlights. Cars that do not have automatic DRL will not be required to turn on low beam headlights. This policy option will be referred to as the technical low beam option.
- New cars sold after a certain date are required to have dedicated DRL that are turned on automatically. Cars that do not have dedicated DRL will not be required to turn on headlights. This policy option will be referred to as the technical dedicated DRL option.

6.2 The Cost Benefit model

Section 6.1 of Interim Report 2 lists eight steps in the analysis that was carried out. Section 6.2 presents the results and section 6.3 discusses them. Favourable benefit/cost ratios (i.e. values greater than one) were reported for each of the five options.

6.3 Description of the sensitivity testing and the results obtained

The cost benefit analysis model is contained in an Excel spreadsheet; a copy was made available to TRL by the Interim Report's main author. When the spreadsheet was investigated, it was discovered that it was not designed for sensitivity testing – the values of all factors are included explicitly (by their numerical values) at each point where they are used for a calculation. In order to facilitate sensitivity testing, the spreadsheet was changed into a more flexible format.

Sensitivity testing is normally carried out by varying the values of the key input variables by +/- 10%. This has been carried out on the following factors (variables in the revised spreadsheet):

- The numbers of cases affected.
- The proportions of affected cases which were prevented (separate proportions for fatal, serious & slight).
- The valuation of the prevention of each casualty.
- The annual discount rate.
- The three variables used to estimate the additional air pollution.
- The costs of bulbs, lamps and DRL installation.
- Fuel costs.
- Initial DRL take-up percentages (different values for cars & motorcycles).
- The cost of bulb replacements.

Table 12 contains the results of this sensitivity testing.

Table 12: Results of the initial cost benefit model sensitivity testing

Variable(s) tested (TRL test value(s) in brackets)	IR2 report value(s) of the variable(s)	Cost/benefit ratios for the five alternative policy options				
		(1) Behavioural measure	(2) Behavioural + low beam	(3) Behavioural + dedicated	(4) Automatic low beam only	(5)Automatic dedicated
IR2 report values -		**1.96**	**1.71**	**1.72**	**1.42**	**1.55**
Proportion of cases affected (0.44)	0.4	2.23	1.94	1.94	1.61	1.74
Proportion of cases affected (0.36)	0.4	1.69	1.47	1.51	1.22	1.36
Proportion of cases prevented (0.165, 0.11, 0.055)	(0.15, 0.10, 0.05)	2.23	1.94	1.94	1.61	1.74
Proportions of cases prevented (0.135, 0.09, 0.045)	(0.15, 0.10, 0.05)	1.69	1.47	1.51	1.22	1.36
Casualty valuations (110% of IR2 values)	(1,265,000€, 125,000€ & 2,720€)	2.23	1.94	1.94	1.61	1.74
Casualty valuations (90% of IR2 values)	(1,265,000€, 125,000€ & 2,720€)	1.69	1.47	1.51	1.22	1.36
Discount rate (5.5%)	5%	1.96	1.71	1.73	1.42	1.54
Discount rate (4.5%)	5%	1.96	1.70	1.72	1.42	1.56
Air pollution factors (110% of IR2 values)	(2636502 km, 1.35%, 0.04€ per km)	1.72	1.50	1.58	1.25	1.45
Air pollution factors (90% of IR2 values)	(2636502 km, 1.35%, 0.04€ per km)	2.16	1.88	1.85	1.56	1.63
Bulb, lamp & DRL installation costs (5.5€, 1.375€ & 27.5€)	(5€, 1.25€ & 25€)	1.96	1.69	1.67	1.40	1.50
Bulb, lamp & DRL installation costs (4.5€, 1.125€ & 22.5€)	(5€, 1.25€ & 25€)	1.96	1.73	1.78	1.44	1.60
Fuel cost (0.33€/ltr)	(0.30€/ltr)	1.86	1.63	1.67	1.36	1.51
Fuel cost (0.27€/ltr)	(0.30€/ltr)	2.07	1.79	1.78	1.48	1.59
Initial take-up (Cars 88%, m/c 55%)	(Cars 80%, m/c 50%)	1.87	1.64	1.66	1.42	1.55
Initial take-up (Cars 72%, m/c 45%)	(Cars 80%, m/c 50%)	2.06	1.78	1.79	1.42	1.55
Cost of bulb replacements (Cars 6.6€, m/c 2.2€)	(Cars 6€, m/c 2€)	1.87	1.64	1.66	1.36	1.49
Cost of bulb replacements (Cars 5.4€, m/c 1.8€)	(Cars 6€, m/c 2€)	2.06	1.78	1.79	1.48	1.62
Additional fuel consumption (petrol 0.0176, diesel 0.0077)	(petrol 0.016, diesel 0.007)	1.87	1.64	1.72	1.36	1.51
Additional fuel consumption (petrol 0.0144, diesel 0.0063)	(petrol 0.016, diesel 0.007)	2.07	1.79	1.72	1.48	1.59

Sensitivity testing has not been included in the table for the variable “accident population”. This is because a 10% change in the accident population would have the same impact as a 10% change in the proportion of cases affected. Each of the 10% changes (plus or minus) produced modest changes in the values of the B/C ratios. As a result of these changes, none of the B/C ratios became less than 1.

When considering all of the results of the cost benefit analysis and the subsequent sensitivity analyses, it should be noted that Interim Report calculated confidence intervals on the cost benefit ratios and all of these confidence intervals spanned a ratio of 1.0. This means that the cost benefit ratios are not statistically significantly different from 1.0 and should be treated with caution.

It can be seen from the results in the above table that the variables that have the largest effect on the cost benefit ratio for a given percentage change in the input assumptions are:

- The proportion of accidents affected
- The proportion of affected cases that would be prevented (including the split by accident severity)
- The financial value of casualties.

Where this review identifies large possible variations in these three parameters it will have a substantial influence on the cost benefit ratios derived.

7 Discussion and analysis

Overall, the research reported in the EC DRL reports represents a thorough and comprehensive analysis of the available data. Although it is possible to be critical of several specific aspects of the work very substantial evidence has been presented that the introduction of DRL would result in a net casualty reduction effect. However, there appears to be greater scientific uncertainty concerning the size of the expected effect. Some of the parameters in the statistical analysis were not found to be statistically significant and should, therefore, be treated with some caution. In particular, the evidence for assuming a 15% reduction in fatal accidents is weak and it was considered that it would be more technically defensible to assume that a mean effect of between 3.9% and 5,9% (depending on which biases and assumptions are considered) applied to accidents of all injury severities and that there would be no effect on damage only accidents.

The investigation of the effect of DRL for passenger cars on the conspicuity of vulnerable road users appeared, in general, to be a well controlled experiment. Again, it is possible to criticise several specific aspects of the research but, in most cases, it was not considered likely that these would have substantially affected the main results and conclusions of the work. However, a few more serious concerns with this work were identified:

- The conspicuity of motorcycles in the presence of differing intensities of DRL and different ambient lighting conditions was not investigated.
- There was some concern that the photographic methods used *may potentially* not have replicated the real world environment sufficiently realistically.
- The relative positions of cars and motorcycles that were evaluated by the work did not include situations at a junction where the motorcycle was approaching from the side and was positioned in front of a car equipped with DRL. All road scenes considered appeared to place the motorcycle to the side of the car such that daylight was visible between the two to physically separate them in the image.

However, when this research was compared with other experiments carried out in this area (Cobb, 1992; GRE, 2003, 2004) it was considered that if the limitations of scope of Interim Report 3 were accepted, then the three studies actually presented consistent conclusions. These were that DRL with high light intensities could impair the conspicuity of motorcyclists but it was possible to design DRL that could improve the conspicuity of cars in the dim ambient light conditions of most relevance without adversely affecting the conspicuity of motorcyclists. The exact recommendations varied but DRL of 200cd would have fallen within the recommended ranges of both Cobb (1992) and GRE (2003, 2004). This shows that it is very important that the technical details of the implementation of DRL are considered very carefully since it may be that a policy option which involved the use of existing passing beam headlights (or high intensity dedicated DRL) as DRL could have an adverse effect on motorcyclist conspicuity. Further research to assess the concerns identified above will be necessary to gain confidence that implementation of any particular DRL policy option would not have an adverse affect for motorcyclists.

There was very little evidence presented in the EC reports on the justification of the estimates of environmental dis-benefits in terms of increased fuel consumption and emissions. However, an independent assessment of those effects using a sophisticated computer modelling technique has suggested that the estimate of a 0.5% to 1.5% increase in fuel use and carbon dioxide emissions appears reasonably accurate and possibly even slightly higher than justified. The computer modelling undertaken as part of this review suggested increases of 0.28% for dedicated DRL (21 watts each) and 1.0% for passing beam headlights (55w each plus rear and interior lights).

The area where greatest scientific uncertainty was found was in the cost benefit analysis. It was noted that the cost benefit analysis presented by Elvik *et al* (2003) showed benefit/cost ratios considerably in excess of 1. However, the confidence limits calculated for these ratios were not statistically significantly different from a ratio of 1. It had been assumed that the accident reduction effect of DRL would be 15% of fatal accidents, 10% of serious accidents and 5 % of slight accidents. In fact this review has suggested that this relationship between the size of the effect and accident severity was very weakly supported by the statistical studies of accidents and it was considered more technically defensible to assume that the mean value of 5% (or perhaps 5.9% if being generous) applied to all injury accidents.

In addition, this review found that the statistical analysis also acknowledged a publication bias in the data and analysis has shown that this would in fact reduce the size of the mean benefit by 2% to 3.9%. One further issue that was considered to have an important effect on the benefit/cost ratios was the fuel cost assumed. The analysis has been carried out using a cost per litre for fuel that excludes tax. This means that the cost per litre of petrol in the UK was valued at €0.30 or approximately £0.20. The validity of this approach will depend largely on who is considered as the relevant parties that costs are attributed to. DRL will therefore cost the consumer in the UK considerably more than predicted in the cost benefit but Government tax revenue would actually increase such that net cost to society is as predicted in the analysis.

The benefit/cost ratios were re-calculated taking these findings into account and the results are shown in Table 13, below.

Table 13: Results of further cost benefit model sensitivity testing

Variable(s) tested [test value(s)]	**IR2 report value(s) of the variable(s)**	**Cost/benefit ratios for the five alternative policy options**				
		(1) Behavioural measure	**(2) Behavioural + low beam**	**(3) Behavioural + dedicated**	**(4) Automatic low beam only**	**(5) Automatic dedicated**
Proportion of cases prevented (5.9% for all severities)	(0.15, 0.10, 0.05)	0.61	0.53	0.63	0.44	0.61
Proportion of cases prevented (3.9% for all severities)	(0.15, 0.10, 0.05)	0.16	0.14	0.26	0.11	0.30
Combination case 1	[see note (1)]	3.75	3.22	2.96	2.56	2.57
Combination case 2	[see note (2)]	1.33	1.15	1.12	0.91	1.01
Combination case 3	[see note (3)]	0.66	0.57	0.61	0.45	0.58

Notes: (1) This case assumes that all of the variables in the cost benefit analysis spreadsheet that have potential ranges of values are set to their most beneficial case (i.e. those which increase the B/C ratios); (2) As case (1) but with the proportions of cases affected set at (0.059, 0.059, 0.059); (3) As case (2) but with the proportions of cases affected set at (0.039, 0.039, 0.039).

It can be seen that changing the assumptions from the very weakly supported estimate of effect in relation to accident severity (15%, 10%, 5%) to the more technically defensible 5.9% mean effect for all severities has a very large effect on the conclusions, changing the benefit/cost ratios from substantially greater than one to substantially less than one. Including the effect of publication bias reduces the ratios still further to a minimum of 0.11 for policy option 4.

It is interesting to consider the cost of fuel from a motorist’s perspective. The cost benefit analysis excludes the tax element of the fuel cost, which particularly in the UK is quite substantial. While this is technically correct for an analysis of the benefits and costs to “Europe plc” it does not reflect the increased costs to motorists. The additional cost to UK motorists associated with the use of DRL would be four times the fuel cost considered in the cost benefit analysis. If the cost benefit analysis was carried out in this way the benefit to cost ratio would reduce from 1.96 to 0.77 for option 1, although this would exclude an increase in Government tax revenue.

The size of effects estimates are therefore the most crucial factor in the cost benefit argument in favour of DRL. It seems clear, that in terms of the cost benefit analysis, considerable technical uncertainty remains. This uncertainty is reflected in Interim Report 2 where the cost benefit analysis is presented with a lower boundary, an upper boundary and a best estimate. For option 1 the lower boundary for the benefit to cost ratio was 0.35 and the upper was 3.57. In the final report only the best estimate value of 1.96 for option 1 was presented. Given the level of this uncertainty it does not seem reasonable to present a single value of benefit/cost ratio for each policy option. It would be far more technically defensible to present a range of predicted values within which it can be scientifically confident that the true answers lie. The analysis presented in Table 13 show that this range could be as wide as 0.11 to 3.75 and the range presented in Interim Report 2 was only slightly narrower than this. Although it is likely that further analysis could confidently reduce this range it also seems likely that the range will still span the “break-even” benefit to cost ratio of one.

8 Conclusions

1. There is substantial evidence that the mandatory use of DRL would provide a net accident reduction. However, the evidence concerning the magnitude of the effect and particularly the relationship with accident severity is considerably weaker.
2. The estimates of the fuel and emissions increases as a result of implementing DRL are reasonable and possibly slightly conservative (high).
3. The research into the potential of DRL on cars to impair the conspicuity of motorcyclists and other vulnerable road users was well controlled but limited in scope and did not consider some important variables. However, when compared with other similar studies some consistent conclusions could be drawn. These were that it should be possible to design dedicated DRL of low intensity that are beneficial to the conspicuity of cars without adversely affecting the conspicuity of motorcyclists. However, DRL of higher intensity (potentially including standard passing beam headlights) could have an adverse effect on motorcyclist conspicuity in some circumstances.
4. There is considerable scientific uncertainty inherent in the values of the benefit to cost ratios presented in the EC work. The key variable is the assumption that the accident benefits would be considerably greater for fatal accidents (15%) than for serious (10%) or slight (5%) accidents. This assumption was very weakly supported by the available data and changing it to a more technically defensible assumption that the mean effect of 5.9% remained the same for all accident severities reduced the benefit to cost ratios to much less than 1.
5. It was considered that it would be more technically valid to present a range of possible benefit to cost ratios within which there could be confidence that the true answer would lie, thus reflecting the technical uncertainty. The analysis showed that a ratio of 1 would fall within this range meaning that, although an accident reduction potential exists, it is not possible to say with certainty whether the benefits of implementing DRL would outweigh the costs.

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Appendix A. Estimated emissions derived from the PHEM model.

Table 14: Estimated emission and fuel consumption rates (g/km).

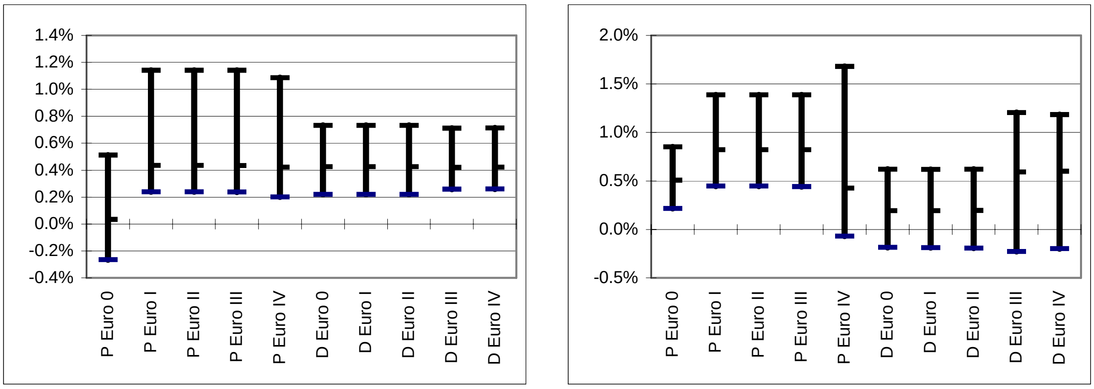
		FC	CO	HC	NOx	PM
Petrol Euro 0	Base	79.987	6.6609	0.9496	1.4576	
	+42W	80.154	6.6969	0.9516	1.4688	
	+160W	80.876	6.7924	0.9572	1.4992	
Petrol Euro I	Base	65.656	4.6574	0.1689	1.0034	
	+42W	65.997	4.6933	0.1701	1.0091	
	+160W	66.896	4.8094	0.1736	1.0250	
Petrol Euro II	Base	65.500	2.1352	0.0923	0.1160	
	+42W	65.840	2.1516	0.0930	0.1168	
	+160W	66.737	2.2048	0.0949	0.1187	
Petrol Euro III	Base	64.915	1.4494	0.0224	0.0500	
	+42W	65.251	1.4606	0.0225	0.0503	
	+160W	66.140	1.4967	0.0230	0.0511	
Petrol Euro IV	Base	60.578	0.4893	0.0073	0.0336	
	+42W	60.884	0.4915	0.0073	0.0337	
	+160W	61.684	0.4996	0.0074	0.0341	
Diesel Euro 0	Base	58.926	0.5888	0.1839	0.9996	0.2139
	+42W	59.195	0.5906	0.1841	1.0047	0.2143
	+160W	60.001	0.5957	0.1848	1.0197	0.2157
Diesel Euro I	Base	57.223	0.4004	0.0935	0.8626	0.1293
	+42W	57.484	0.4016	0.0936	0.8669	0.1295
	+160W	58.267	0.4050	0.0940	0.8799	0.1304
Diesel Euro II	Base	54.764	0.1702	0.0635	0.9530	0.0718
	+42W	55.014	0.1707	0.0636	0.9578	0.0719
	+160W	55.763	0.1721	0.0638	0.9722	0.0724
Diesel Euro III	Base	56.467	0.0503	0.0093	0.7479	0.0384
	+42W	56.735	0.0507	0.0093	0.7523	0.0386
	+160W	57.536	0.0521	0.0094	0.7652	0.0391
Diesel Euro IV	Base	56.450	0.0301	0.0061	0.3868	0.0161
	+42W	56.718	0.0304	0.0061	0.3891	0.0162
	+160W	57.520	0.0312	0.0062	0.3958	0.0164

Table 15: Range of relative changes in fuel consumption and emissions: +42W (showing 5th, 50th and 95th percentiles).

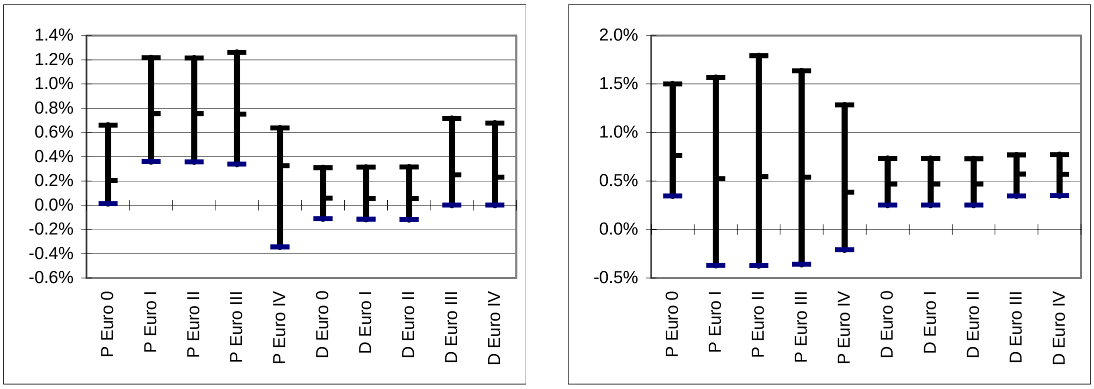
Vehicle Category	Percentile	FC	CO	HC	NOx	PM
Petrol Euro 0	**5th**	-0.26%	0.22%	0.01%	0.35%	
	50th	0.03%	0.51%	0.20%	0.76%	
	95th	0.51%	0.85%	0.66%	1.50%	
Petrol Euro I	**5th**	0.24%	0.44%	0.36%	-0.37%	
	50th	0.43%	0.82%	0.76%	0.52%	
	95th	1.14%	1.39%	1.22%	1.57%	
Petrol Euro II	**5th**	0.24%	0.44%	0.36%	-0.37%	
	50th	0.44%	0.82%	0.76%	0.54%	
	95th	1.14%	1.39%	1.21%	1.79%	
Petrol Euro III	**5th**	0.24%	0.44%	0.34%	-0.36%	
	50th	0.43%	0.82%	0.75%	0.54%	
	95th	1.14%	1.39%	1.26%	1.64%	
Petrol Euro IV	**5th**	0.20%	-0.07%	-0.35%	-0.21%	
	50th	0.42%	0.42%	0.33%	0.38%	
	95th	1.09%	1.68%	0.64%	1.28%	
Diesel Euro 0	**5th**	0.22%	-0.19%	-0.11%	0.25%	0.02%
	50th	0.43%	0.19%	0.06%	0.47%	0.14%
	95th	0.73%	0.62%	0.31%	0.73%	0.34%
Diesel Euro I	**5th**	0.22%	-0.19%	-0.12%	0.25%	0.02%
	50th	0.43%	0.19%	0.05%	0.47%	0.13%
	95th	0.73%	0.62%	0.31%	0.73%	0.34%
Diesel Euro II	**5th**	0.22%	-0.19%	-0.12%	0.25%	0.02%
	50th	0.43%	0.20%	0.05%	0.47%	0.14%
	95th	0.73%	0.62%	0.32%	0.73%	0.34%
Diesel Euro III	**5th**	0.26%	-0.23%	0.00%	0.35%	0.08%
	50th	0.42%	0.59%	0.25%	0.57%	0.33%
	95th	0.71%	1.21%	0.72%	0.77%	0.95%
Diesel Euro IV	**5th**	0.26%	-0.20%	0.00%	0.35%	0.07%
	50th	0.42%	0.60%	0.23%	0.57%	0.32%
	95th	0.71%	1.18%	0.68%	0.77%	0.97%
Diesel Euro IV	**5th**	0.26%	-0.20%	0.00%	0.35%	0.07%
	50th	0.42%	0.60%	0.23%	0.57%	0.32%
	95th	0.71%	1.18%	0.68%	0.77%	0.97%

Table 16: Range of relative changes in fuel consumption and emissions: +160W (showing 5th, 50th and 95th percentiles).

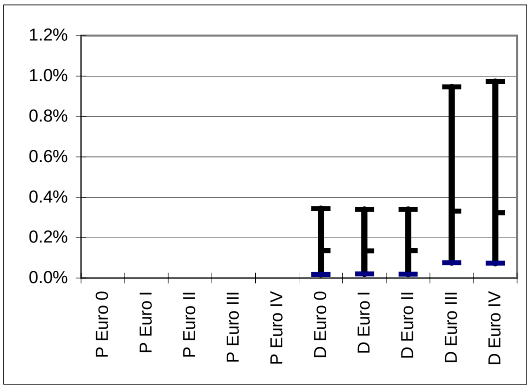
Vehicle Category	Percentile	FC	CO	HC	NOx	PM
Petrol Euro 0	**5th**	-2.09%	0.60%	0.18%	1.63%	
	50th	0.36%	1.93%	0.82%	2.91%	
	95th	2.08%	2.80%	1.87%	4.63%	
Petrol Euro I	**5th**	1.12%	1.60%	1.66%	-1.05%	
	50th	1.69%	3.25%	2.97%	2.16%	
	95th	3.07%	6.12%	4.77%	4.84%	
Petrol Euro II	**5th**	1.12%	1.60%	1.65%	-1.06%	
	50th	1.69%	3.25%	2.97%	2.30%	
	95th	3.07%	6.12%	4.77%	5.03%	
Petrol Euro III	**5th**	1.12%	1.56%	1.56%	-0.94%	
	50th	1.69%	3.27%	2.95%	2.14%	
	95th	3.07%	6.21%	4.85%	5.10%	
Petrol Euro IV	**5th**	0.92%	-0.62%	0.01%	-2.81%	
	50th	1.66%	1.72%	1.28%	1.46%	
	95th	2.89%	7.17%	2.08%	4.43%	
Diesel Euro 0	**5th**	0.83%	-0.74%	-0.67%	0.95%	0.10%
	50th	1.69%	0.74%	0.23%	1.85%	0.62%
	95th	2.94%	2.22%	1.39%	3.08%	2.03%
Diesel Euro I	**5th**	0.83%	-0.74%	-0.65%	0.95%	0.10%
	50th	1.69%	0.74%	0.23%	1.85%	0.62%
	95th	2.94%	2.22%	1.40%	3.08%	2.04%
Diesel Euro II	**5th**	0.83%	-0.73%	-0.67%	0.95%	0.11%
	50th	1.69%	0.74%	0.24%	1.85%	0.63%
	95th	2.94%	2.22%	1.39%	3.08%	2.04%
Diesel Euro III	**5th**	0.97%	-0.40%	0.00%	1.45%	0.36%
	50th	1.71%	1.98%	1.02%	2.23%	1.44%
	95th	2.86%	4.47%	2.81%	3.07%	3.77%
Diesel Euro IV	**5th**	0.97%	-0.39%	0.00%	1.45%	0.35%
	50th	1.71%	1.97%	1.04%	2.23%	1.47%
	95th	2.86%	4.48%	2.72%	3.07%	3.82%
Diesel Euro IV	**5th**	0.97%	-0.39%	0.00%	1.45%	0.35%
	50th	1.71%	1.97%	1.04%	2.23%	1.47%
	95th	2.86%	4.48%	2.72%	3.07%	3.82%



a. Fuel consumption **b. CO emissions**

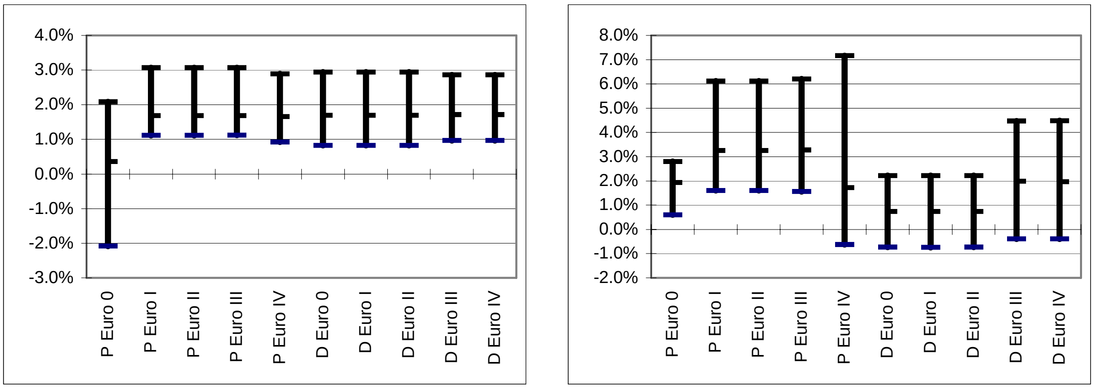


c. HC emissions **d. NO_x emissions**

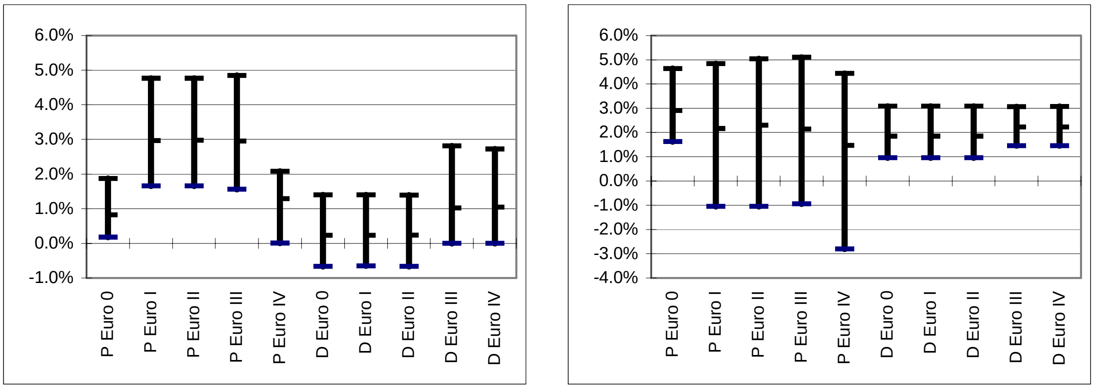


e. Particulate matter

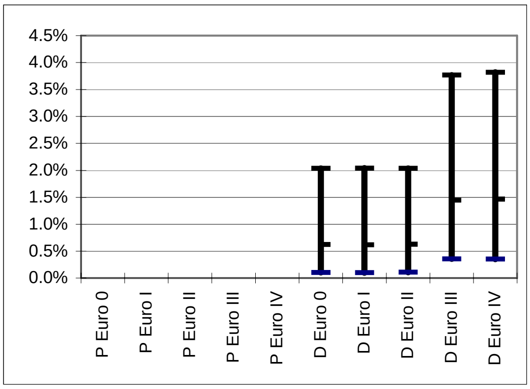
Figure 4: Range of relative changes in fuel consumption and emissions: +42W (showing 5th, 50th and 95th percentiles).



a. Fuel consumption **b. CO emissions**



c. HC emissions **d. NO_x emissions**

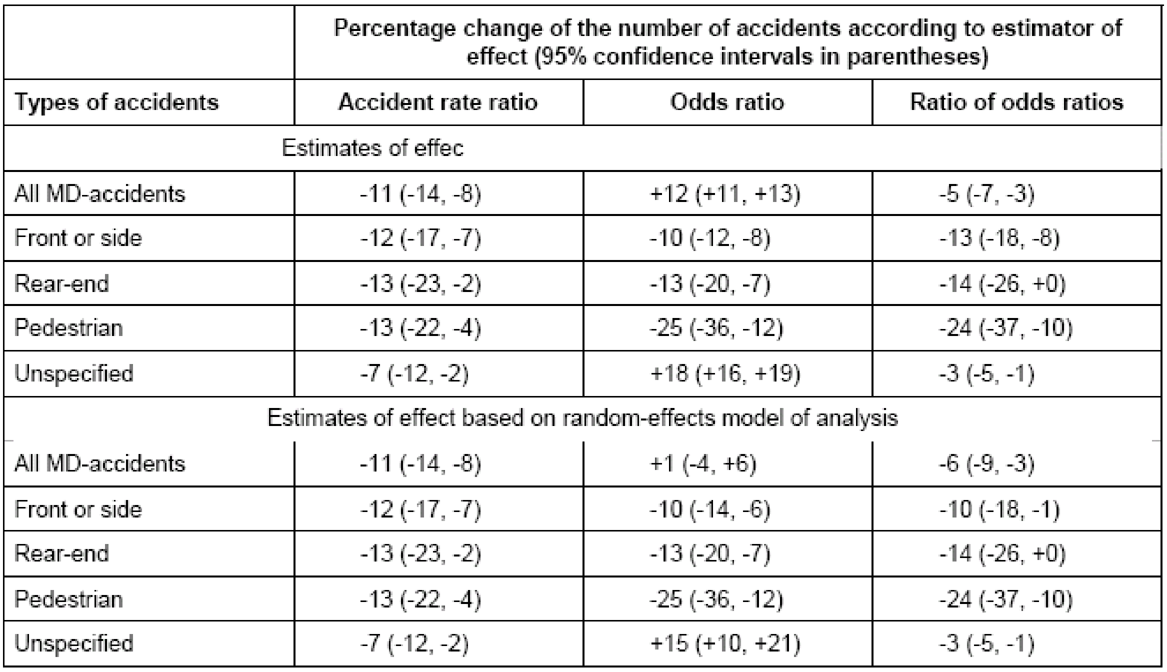


e. Particulate matter

Figure 5: Range of relative changes in fuel consumption and emissions: +160W (showing 5th, 50th and 95th percentiles).

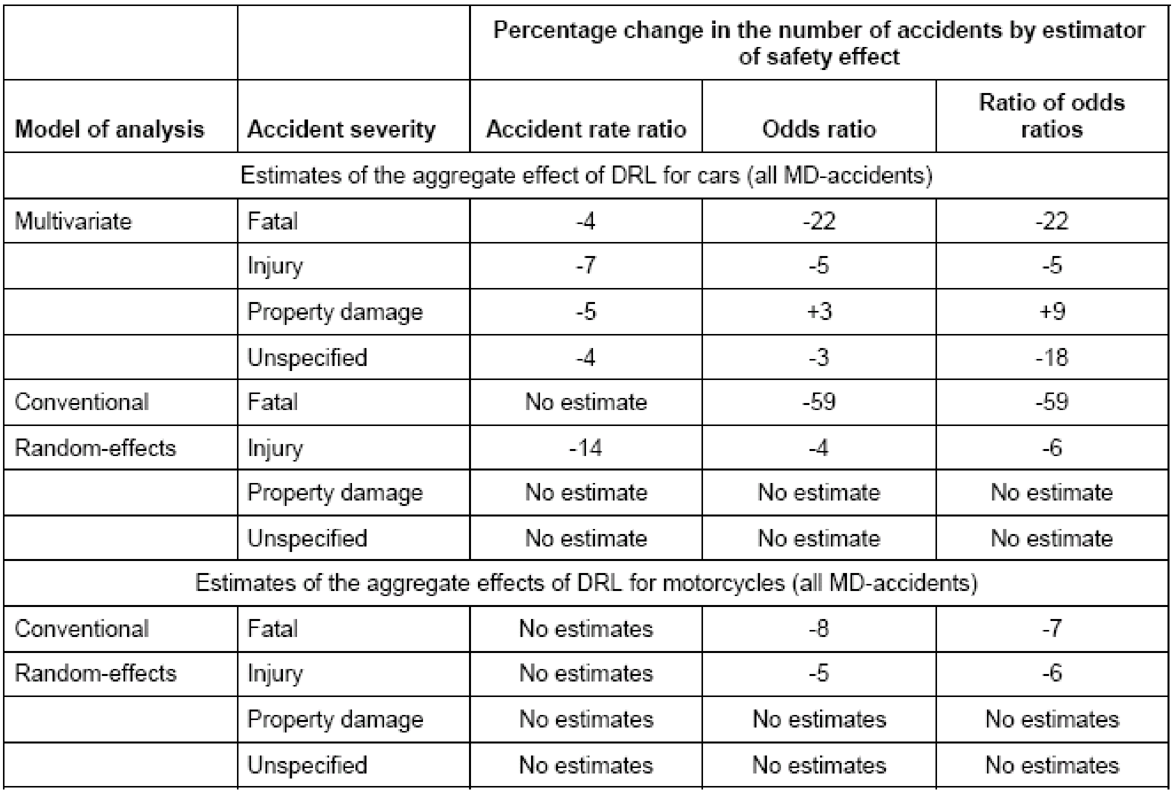
Appendix B. Tables from IR2 report

Table 3: Summary estimates of the intrinsic effects of DRL on cars. Based on meta-analysis.



	Percentage change of the number of accidents according to estimator of effect (95% confidence intervals in parentheses)		
Types of accidents	Accident rate ratio	Odds ratio	Ratio of odds ratios
Estimates of effec			
All MD-accidents	-11 (-14, -8)	+12 (+11, +13)	-5 (-7, -3)
Front or side	-12 (-17, -7)	-10 (-12, -8)	-13 (-18, -8)
Rear-end	-13 (-23, -2)	-13 (-20, -7)	-14 (-26, +0)
Pedestrian	-13 (-22, -4)	-25 (-36, -12)	-24 (-37, -10)
Unspecified	-7 (-12, -2)	+18 (+16, +19)	-3 (-5, -1)
Estimates of effect based on random-effects model of analysis			
All MD-accidents	-11 (-14, -8)	+1 (-4, +6)	-6 (-9, -3)
Front or side	-12 (-17, -7)	-10 (-14, -6)	-10 (-18, -1)
Rear-end	-13 (-23, -2)	-13 (-20, -7)	-14 (-26, +0)
Pedestrian	-13 (-22, -4)	-25 (-36, -12)	-24 (-37, -10)
Unspecified	-7 (-12, -2)	+15 (+10, +21)	-3 (-5, -1)

Table 5: Overview of main pattern in summary estimates of the aggregate effects on accidents of DRL for cars or motorcycles.



		Percentage change in the number of accidents by estimator of safety effect		
Model of analysis	Accident severity	Accident rate ratio	Odds ratio	Ratio of odds ratios
Estimates of the aggregate effect of DRL for cars (all MD-accidents)				
Multivariate	Fatal	-4	-22	-22
	Injury	-7	-5	-5
	Property damage	-5	+3	+9
	Unspecified	-4	-3	-18
Conventional	Fatal	No estimate	-59	-59
Random-effects	Injury	-14	-4	-6
	Property damage	No estimate	No estimate	No estimate
	Unspecified	No estimate	No estimate	No estimate
Estimates of the aggregate effects of DRL for motorcycles (all MD-accidents)				
Conventional	Fatal	No estimates	-8	-7
Random-effects	Injury	No estimates	-5	-6
	Property damage	No estimates	No estimates	No estimates
	Unspecified	No estimates	No estimates	No estimates