

Why the method of Elvik et al 2003 is unscientific; its findings unreliable; and its cost-benefit calculation baseless

A critical review of Elvik R, Christensen P, Olsen SF 2003 'Daytime running lights – A systematic review of effects on road safety'

I. The basis of Elvik et al 2003's estimates of the effect of motorcar and motorcycle daytime running lights is the metaphysical and unscientific proposition: 'If three non-specific tests for the direction and magnitude of an effect yield the same result, the lack of specificity of the tests can be ignored' — or in lay terms: 'Three wrongs can make a right'

Elvik et al 2003 apply three tests, or 'estimators', of the effect of daytime lights upon accidents in their main analysis, or 'meta-analysis', namely (p3):

- The accident rate ratio
- The odds ratio
- The ratio of odds ratios.

Elvik et al admit that none of the tests are specific for the effect of daytime lights — ie that the tests respond to other factors that influence the incidence of accidents besides daytime lights (p78):

- '1. None of the three estimators for the effects of DRL on accidents can be trusted to always provide unbiased estimates of these effects. On the contrary, all three estimators may give misleading estimates of the effects of DRL.
2. It is not possible to control for all potentially confounding factors by means of the definition of an estimator of safety effects.
3. A tendency has been found for all three estimators to give inflated estimates of the effects of DRL on accidents.'

Yet they propose, without any basis—or attempt to argue a basis—for the proposition in science, that because all three tests yield an estimate of the effect of daytime lights that is in the same direction—as it happens, in the direction of a reduction of accidents from daytime lights—, and of the same order of magnitude, the estimates of the three tests are therefore valid (*ibid*):

- '4. There is no strong basis for recommending one of the three estimators. One should therefore base statements about the effects on accidents of DRL on whether or not estimates provided by the three estimators are consistent in direction and magnitude.

2. Elvik et al 2003 appraise the scientific quality of the motorcar and motorcycle daytime running light studies that they analyse by an unscientific process, namely they conduct a global assessment of the quality of the studies according to four incongruous criteria, two of which are not even quality criteria

Elvik et al 2003 assess the quality of the daytime running light studies for the purpose of:

- Determining 'whether the studies are in general of sufficiently high quality that one may trust their findings' (p79).
- Conducting a 'sensitivity analysis with respect to study quality' (p71) of the 'robustness of the summary estimates of effect' (p7) of daytime running lights from their 'meta-analysis' (*ibid*).

They do so, to re-order Elvik et al's list more conveniently, by scoring studies according to four criteria (p4):

- 1) 'The extent to which a study controls for confounding factors that may distort estimates of the effects of DRL.'
ie: Can Elvik et al rely upon the specificity of the response of the data of the study to the effect of daytime running lights for the purposes of their main analysis?
- 2) 'Whether or not a study provides information on the use of DRL in the country or region in which the study was conducted.'
ie: Does the data of the study also permit Elvik et al to test the robustness of the estimates of the main analysis by conducting a supporting analysis of the 'dose-response relationship' of the frequency of use of daytime running lights to accidents?
- 3) 'The extent to which a study specifies various types of accident that may be affected by DRL.'
ie: Does the data of the study also permit Elvik et al to extend the scope of their meta-analysis to encompass the possible side-effects of motorcar daytime running lights upon motorcycle, pedal cycle, and pedestrian accidents — or motorcar rear-end, as opposed to front or side, collisions?
- 4) 'The extent to which a study specifies accident severity.'
ie: Does the data of the study also permit Elvik et al to make a meaningful cost-benefit calculation for daytime running lights according to their separate effect upon accidents of different degrees of severity (and so cost)?

As can be seen all four criteria relate to distinct and separate study purposes — ie they are 'incongruous'.

Equally only the first criterion—or arguably also the second criterion—is a quality criterion.

3. Elvik et al 2003 fail in their supporting analyses:

- To demonstrate a dose-response relationship for daytime running lights
 - To establish how far the effect of daytime running lights may be expected to endure over time
 - To confirm that the effect of daytime running lights is greater in winter than in summer; or in countries of high latitude than in countries of low latitude.
- Given the controversial nature also of the method of Elvik et al 2003's main analysis, it must therefore be

concluded:— 'If daytime running lights were regulated by the medical authorities, on the evidence of Elvik et al's analyses, they would not receive clinical approval'

Elvik et al 2003 report their failure to demonstrate a dose-response relationship of the frequency of the use of daytime running lights, and accidents, as follows (p84):

'As can be seen from Figure 7, there is no apparent dose-response relationship between the variables. It does not seem to be the case that the greater the increase in the use of DRL, the larger the effect on accidents.'

They report their failure to establish how far the effect of daytime running lights may diminish with time (p4):

'There are indications that the effects of laws mandating the use of DRL tend to diminish over time. However, not all studies that have evaluated the effects of DRL-laws have found such a tendency. It is therefore concluded that further study is needed in order to determine more precisely the duration and stability over time of the effects of DRL-laws.'

They report the failure of the 'plausibility' tests by which they intended to verify that the effect of daytime running lights increases, as between countries, with degree of latitude (p86):

'Figure 8 shows the results of the analysis. The outlying estimate of effect for Israel (25% increase of accidents) has been omitted. There is hardly any relationship between latitude and effects of DRL.'

or as between the seasons, from summer to winter (p88):

'Very few studies have investigated variations in the effect of DRL with respect to season. The only studies that may shed some light on the question are the evaluations of DRL-laws in the Scandinavian countries.

...

Andersson and Nilsson (1981) did not find a larger effect of DRL during winter than during summer in Sweden, nor did Elvik (1993) in Norway.'

In addition, to repeat from Section 1., scientifically the method of Elvik et al's main analysis of the effect of daytime running lights is highly controversial.

And to repeat from Section 2., scientifically the criteria that they employed to assess quality of the studies that they analysed is also controversial.

It is understood, from medical sources on the internet, that the decision whether or not a drug shall receive clinical approval will usually turn upon an overall appraisal of its performance under a number of tests of its safety and efficacy. A failure by the drug to pass all of the tests will not necessarily lead to a decision to refuse to grant it approval.

But should an application for the clinical approval of a drug be:

- Supported only by the findings of a controversial analysis
 - Contradicted by the failure of the drug to pass three tests intended to confirm its efficacy
 - Contradicted by the failure of the drug to pass a test intended to demonstrate its enduring efficacy,
- it does seem reasonable to assert that the application must invariably be rejected.

4. Elvik et al 2003 wrongly claim in the Summary and Conclusions of their paper, in contradiction of what they say in the Main Body and Executive Summary, that they have made 'very robust' estimates of the effect of motorcar and motorcycle daytime running lights

Elvik et al 2003 claim in their Summary and Conclusions that they have made 'very robust' estimates of the effect of daytime running lights (pp104-105):

'6. The robustness of the summary estimates of effect given above have been tested with respect to some potential sources of error in meta-analyses, including:

- (a) Publication bias,
- (b) Varying quality of the studies included,
- (c) The statistical weights assigned to each estimate of effect and
- (d) The contribution of a single study to the overall estimate of effect.

In general, the summary estimates of effect were very robust. It is therefore concluded that the estimates of effect based on the meta-analysis are the best current estimates of the effects of DRL, given the evidence provided by the evaluation studies.'

This limited statistical claim appears nowhere else in such strong terms in the paper.

It does not appear at all when Elvik et al present and discuss the results of their analyses in the Main Body of their paper (pp55-89).

Likewise when Elvik et al present the same statistical findings, at greater length, in the conclusions of the Executive Summary, they merely say that their summary estimates of effect, namely the findings of their main analysis, passed all four tests — and then move on to conduct a final discussion of the quality of the studies (pp7-8).

To examine therefore what exactly Elvik et al do say in the rest of the paper, Appendix 1 lists:

- The questions that Elvik et al list in the paper (pp 2-3 & 9-10) as the question that they seek to answer by their analyses
- A selection of some of the 'less than confident' answers that Elvik et al gave to the questions in the Main Body or Executive Summary.

To refer to Appendix 1., in total contrast to the buoyant terms of the Summary and Conclusions:

- Elvik et al pass adverse comment on the quality of the individual studies whose data they analyse
- They report the lack of specificity for the effect of daytime running lights of the tests that they use in the main analysis
- And they report the failure of most of the analyses that were intended to corroborate and extend the findings of the main analysis to make any findings at all.

It is busy laymen, such as politicians and officials, rather than scientists, who are likely to be the main readers of the Summary and Conclusions.

Correspondingly, not only is 'very robust' a wrong description of the estimates of the effect of daytime running lights that Elvik et al made from their main analysis — also the Summary and Conclusions is totally the wrong place to use confusing statistical terminology of the sort.

5. Elvik et al 2003 base their cost-benefit calculation for motorcar daytime running lights largely upon unproven assumptions — or in lay terms 'guesswork'

Elvik et al 2003 report the basic assumptions of the cost-benefit calculation that they make for motorcar daytime running lights as follows (p93):

'Based on the systematic review of evidence presented in this report, it is assumed that mandatory use of DRL will be associated with a:

- Reduction of 15% in MD-accidents leading to fatal injury.
- Reduction of 10% in MD-accidents leading to serious injury.
- Reduction of 5% in MD-accidents leading to slight injury.
- No reduction in MD-accidents leading to property damage only.

These reductions are assumed to be the result of an increase in DRL-use from 10% to 90%. ...'

In fact the assumptions in question are largely unproven.

Thus Elvik et al fail in their analyses of the effect of daytime running lights to establish a dose-response relationship (p84):

'As can be seen from Figure 7, there is no apparent dose-response relationship between the variables. It does not seem to be the case that the greater the increase in the use of DRL, the larger the effect on accidents.'

so cannot predict the effect upon accidents of an increase in the frequency of daytime running light use.

And they find little evidence to assist them in establishing the relative effect of motorcar daytime running lights upon accidents of different severity (p87):

'The relationship between accident severity and the effects of DRL was discussed in Chapter 3. There is not very much evidence regarding this relationship.'

Indeed—pace a precise progression from no reduction of property damage only accidents, to a 15% reduction of fatal injury accidents—they conclude their discussion of the subject by openly guessing at a higher reduction of fatal accidents than injury accidents from motorcar daytime running lights (*ibid*):

'This means that available evidence is too unreliable to predict the effect of DRL on fatal accidents with much confidence. It is likely that the effect of DRL on fatal accidents is larger than the effect on other injury accidents, but it is currently not possible to quantify the effect on fatal accidents with very great precision.'

Or there is no motorcar daytime running lights study that breaks down injury accidents into serious and slight injury accidents — so no information of how daytime running lights separately affect serious and slight injury accidents in Elvik et al's paper at all.

Elvik et al report that they put the following valuations upon a road accident injury at Euro 2000 prices (p94):

Fatal	1,265,000
Serious	125,000
Slight	2,720

Given the very large differences between the respective figures of the valuations in question, for Elvik et al to base a cost-benefit calculation in the above way upon:

- A largely guessed at figure of the reduction of fatal injuries from daytime running lights
 - A wholly guessed at split of the reduction of injury accidents from daytime running lights between serious injury accidents, and slight injury accidents
- is a travesty of how the cost-benefit calculation of a road safety measure should be conducted.

6. Elvik et al 2003 omit to present the data of the motorcycle daytime running light studies that they analyse

Elvik et al 2003 supply a table headed 'Data extracted from each study' in Appendix 1 of their paper in which they list the data from the motorcar daytime running light studies that they analyse in their main analysis and other analyses (pp114–117).

But they do not include in the table also the data from the motorcycle daytime running light studies that they analyse.

The omission unhelpfully renders it impossible to criticise satisfactorily Elvik et al's motorcycle findings.

Or in Popper's terms, the omission wrongly renders the findings 'unfalsifiable'.

7. Elvik et al 2003 omit to include Lund 1979 amongst the motorcycle daytime running light studies whose data they analyse

Elvik et al 2003 omit to include in the motorcycle daytime running light studies whose data they analyse Lund 1979's study of the effect of the law that Denmark implemented in 1977 requiring all motorcycles to display daytime running lights.

Lund 1979 is a Working paper of Rådet for Trafiksikkerhedsforskning (Danish Council of Road Safety Research).

A Danish Government official supplied the present author with a Danish language version of the paper about the second half of the 1980s. Lund himself later supplied the author with an English language version.

Lund 1979 is included in the list of references of conference papers, Prower 1990 (No2) and Perlot & Prower 2002.

It is not known, therefore, how Elvik et al came to overlook Lund 1979.

In fact, Lund 1979 found a slight increase in motorcycle daytime multi-party accidents, as estimated by Elvik et al's Ratio of odds ratios test, following the implementation of the 1977 Danish law.

8. Elvik et al 2003, when analysing the data of the motorcar daytime running light studies by accident severity, incorrectly identify the data of Andersson et al 1976 as data of 'injury accidents'; whereas in fact the data of Andersson et al was, almost certainly, data of 'all police-reported accidents'

Elvik et al 2003, to repeat, supply a table headed 'Data extracted from each study' in Appendix 1 of their paper.

In the table Elvik et al identify the data that was reported by the motorcar daytime running light study Andersson et al 1976 as data of 'injury' accidents (p114). It can be supposed that in turn they analyse the data as such in their analysis of the data of the motorcar studies by accident severity.

Andersson et al 1976 do not in fact in their paper supply a description of the accidents that they analyse,

However, as reconstructed in Appendix 2 of the present paper with the help of the national Finnish accident data for 1975 that is reported by Nordic Road Safety Council 1976 Rapport 17, it is almost certain that the accidents were not 'injury' accidents, but 'all police-reported' accidents

9. Elvik et al 2003 do not explain how, in their analyses of the data of the motorcar daytime running light studies, they treat the duplicate entries of data that they include in their table of 'Data extracted from each study'

Elvik et al 2003, to repeat, supply a table headed 'Data extracted from each study' in Appendix 1 of their paper.

In the table Elvik et al list pairs of entries for:

- Andersson et al 1976 that repeat the same 'before' data, but differ in their 'after' data
- Schützenhöfer et al 1990 that repeat the same 'after' data, but differ in their 'before' data
- Elvik 1993 that repeat the same 'before' data, but differ in their 'after' data
- NHTSA 2000 that repeat the same 'multi-vehicle' data, but differ in their 'single-vehicle' data
- NHTSA 2000 that repeat the same 'after' data, but differ in their 'before' data.

To examine just the pairs of entries for Andersson et al 1976, in their study Andersson et al supply data for the following three consecutive two-year periods in Finland:

- The period of two years before the Finnish Government officially recommended the use of daytime running lights in 1970 ('Pre-recommendation period')
- The period of two years after the Finnish Government officially recommended the use of daytime running lights in 1970, but before the Finnish Government made it compulsory to use daytime running lights in 1972 ('Post-recommendation period')
- The period of two years after the Finnish Government made it compulsory to use daytime running lights in 1972 ('Post-law' period).

Elvik et al's pairs of entries list for the 'before' period, in each case, the figures of Andersson et al's 'Pre-recommendation period' data, and for the 'after' period, respectively the figures of Andersson et al's 'Post-recommendation period' and the 'Post-law period' data.

It is still not obvious why Elvik et al 2003 elect to list Andersson et al 1976's Finnish data twice over in this way.

And it equally not obvious how in turn Elvik et al have treated Andersson et al's data in their analyses.

10. Elvik et al 2003 do not explain how they come to report the same Odds ratio, and Ratio of odds ratios, values for the motorcar daytime running light studies:

- Kfv 1993(3 out of 6 entries)
- Sparks et al 1993
- Holló 1998 (2 out of 3 entries)
- Lassarre 2002

Elvik et al 2003 describe in their paper how their Odds ratio and Ratio of odds ratio test values are computed. It will be observed from the description that, for the same set of data that is entered in each line, or entry, of their table of 'Data extracted from each study', the two tests cannot yield the same value (pp10-11).

Nevertheless when Elvik et al come to report the Odds ratio and Ratio of odds ratios test values in the table of 'Data extracted from each study' for the following motorcar daytime running light studies, without explanation, they report identical values for each test (pp115-117):

- Kuratorium für Verkehrssicherheit 1993 (3 out of 6 entries)
- Sparks et al 1993
- Holló 1998 (2 out of 3 entries)
- Lassarre 2002.

11. Elvik et al 2003 fail to supply in full the data that they use to calculate the Odds ratio and Ratio of odds values for the motorcar daytime running light studies:

- Allen & Clark 1964
- Stein 1985
- Sparks et al 1989
- Kfv 1993(3 out of 6 entries)
- Sparks et al 1993
- Holló 1998 (2 out of 3 entries)
- Lassarre 2002

To repeat, Elvik et al 2003 describe in their paper how their Odds ratio and Ratio of odds ratio test values are computed. It will be observed from the description again that, in order to compute the value of each test for a set of data, each entry in their table of 'Data extracted from each study' must supply eight separate items of data.

Nevertheless, although Elvik et al report Odds ratio and Ratio of odds ratios test values for the following motorcar daytime running light studies, studies in the table of 'Data extracted from each study', without explanation, they fail to report at the same time the full set of eight items of data from which the test values are computed:

- Allen & Clark 1964
- Stein 1985
- Sparks et al 1989
- Kuratorium für Verkehrssicherheit 1993 (3 out of 6 entries)
- Sparks et al 1993
- Holló 1998 (2 out of 3 entries)
- Lassarre 2002.

Conclusion: Elvik et al 2003 purport to conduct a definitive analysis of the combined data of all of the motorcar and motorcycle daytime running light studies that have been conducted in the world to date, and to find both that daytime running lights reduce accidents, and are a cost-effective means of doing so.

However Elvik et al's method at two critical steps in the analysis is not scientific; their findings are not robust; and their cost-benefit calculation is based upon guesswork.

If daytime running lights were a drug, on Elvik et al's evidence they would be refused clinical approval.

The European Commission, it is understood, asked Elvik et al to conduct their study because the Commission has in mind to propose either that the use of daytime running lights by motorcar driver and motorcyclists shall be made compulsory in Europe, or that all new motorcars and motorcycles shall be compulsorily fitted with daytime running lights.

In the event Elvik et al 2003's study is deficient and worthless, and supplies no evidence in support of the European Commission's proposal — or indeed in support of the use of motorcar or motorcycle daytime running lights at all.

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References:

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[Tr: 'The effect upon traffic accidents in Finland of the recommended and compulsory use of warning lights']

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Koornstra M Bijleveld F Hagenzieker M 1997 'The safety effects of daytime running lights' Stichting Wetenschappelijk Onderzoek Verkeersveiligheid (SWOV) R-97-36

Lund HV 1979 'Effekt af motorcyklisters brug af varsellys på uheldsdata i perioden 1. maj 1977 - 30. juni 1978' Rådet for Trafiksikkerhedsforskning (RfT) (in Danish)

Lund HV 1979 'Effect of motor cyclists' use of running lights based on accident data during the period of 1st May, 1977 . 30th June, 1978' Danish Council of Road Safety Research

Nordisk Trafiksikkerhedsråd (NTR) 1976 Rapport 17: 'Varselljus – bilbelysning under dagtid'

[Tr: Report 17: 'Warning lights – motorcar lighting in daytime']

Perlot A Prower SM 2002 'Implications of daytime lights for the traffic situation of motorcycles and motorcars' International Motorcycle Conference 2002

Perlot A Prower SM 2003 'Review of the evidence for motorcycle and motorcar daytime lights'

Prower SM 1990 (No2) 'Why motorcyclists using daytime lights will still have accidents' International Motorcycle Safety Conference 1990

Appendix I: Adverse findings by Elvik et al 2003 that belie the statement in their Summary and Conclusions that: 'In general, the summary estimates of effect were very robust'

The findings are listed by the questions that Elvik et al 2003, at the behest of the European Commission, set themselves to answer in the Main Body (MB) or Executive Summary (ES) of their paper (p9; p2–3):

MB1. *How can one best define and measure the effect of daytime running lights on the number of accidents?*

ES1. *What is the best estimator of the effects on accidents of daytime running lights?*

p78 Based on this rather simple simulation, the following conclusions are drawn:

- 1. None of the three estimators for the effects of DRL on accidents can be trusted to always provide unbiased estimates of these effects. On the contrary, all three estimators may give misleading estimates of the effects of DRL.**
- 2. It is not possible to control for all potentially confounding factors by means of the definition of an estimator of safety effects.**
- 3. A tendency has been found for all three estimators to give inflated estimates of the effects of DRL on accidents.**

p78 **It is nevertheless disturbing that all three estimators of effect are found to generate quite large spurious estimates of effect. Moreover, the large majority of these estimates indicate a reduction of the number of accidents.**

p3 A simple simulation was conducted in order to test the performance of the three estimators in the presence of various confounding effects that could generate biased estimates of the effects of DRL. **It was found that neither of the three estimators adequately control for confounding, and that all of them can give biased estimates of the effects of DRL.**

MB2. *What is the methodological quality of available evaluation studies? Can findings be trusted?*

ES2. *What is the quality of the evidence provided by studies that have evaluated the safety effects of daytime running lights? Can these studies be trusted?*

p80 **The overall impression is that studies are of moderate quality.** Research on daytime running lights made a promising start, by conducting a number of experiments. Surprisingly, more recent studies are of poorer quality. **This means that one cannot rule out the possibility that the findings**

of many studies are influenced by uncontrolled confounding factors.

p80 **It is concluded that studies that have evaluated the effects on accidents of DRL are of highly varying quality. Few studies have employed a study design that controls for all relevant confounding factors.**

p4 **In general, studies do not have an ideal quality and it cannot be ruled out that some estimates of the effects of DRL are influenced by confounding factors.**

MB3. *Are the effects attributed to daytime running lights novelty effects that are likely to erode over time?*

ES3. *Are the effects of daytime running lights novelty effects that are likely to erode and possibly disappear completely over time?*

p82 **The evidence regarding novelty effects of DRL is inconclusive.** While there are indications that the effects on accidents of DRL-laws may be eroding over time, this evidence for this is not consistent.

p4 There are indications that the effects of laws mandating the use of DRL tend to diminish over time. However, not all studies that have evaluated the effects of DRL-laws have found such a tendency. **It is therefore concluded that further study is needed in order to determine more precisely the duration and stability over time of the effects of DRL-laws.**

MB4. *What is the relationship between the usage rate for daytime running lights and the effects on road safety (dose-response relationship)?*

p84 **As can be seen from Figure 7, there is no apparent dose-response relationship between the variables. It does not seem to be the case that the greater the increase in the use of DRL, the larger the effect on accidents.**

ES4. *What is the relationship between the effects of daytime running lights for each vehicle using it (intrinsic effects) and the effects of laws or campaigns that lead to an increased use of daytime running lights in a country or part of a country (aggregate effects)?*

p13 Koornstra et al (1997) have tried to model statistically the relationship between the intrinsic and aggregate effects of DRL. The objective of developing statistical models of this relationship, is to make all estimates of the safety effects of DRL comparable, by converting aggregate effects to intrinsic effects.

Stone (1999) has criticised the models developed by Koornstra et al (1997) and

suggested that these models rely on implausible assumptions and may give biased estimates of the effects of DRL. This report will not try to resolve this debate ...

p82 As noted in chapter 1, the relationship between the intrinsic effects of DRL – the effects on the individual accident rate of each road users of that road user using DRL – and the aggregate effects – the effects on the total number of accidents in a country associated with an increase in the use of DRL – has been the subject of research and controversy (Koornstra et al 1997, Stone 1999). **This report will not attempt to resolve the discussion surrounding the functions for relating aggregate and intrinsic effects of DRL proposed by Koornstra et al (1997). ...**

MB5. *Do the effects of daytime running lights vary systematically depending on geographical latitude?*

ES5. *Do the effects of daytime running lights vary depending on geographical latitude?*

p86 Figure 8 shows the results of the analysis. The outlying estimate of effect for Israel (25% increase of accidents) has been omitted. **There is hardly any relationship between latitude and effects of DRL.**

MB6. *Do the effects of daytime running lights vary systematically with respect to accident severity?*

ES6. *Do the effects of daytime running lights vary with respect to accident severity?*

p87 The relationship between accident severity and the effects of DRL was discussed in Chapter 3. **There is not very much evidence regarding this relationship.**

MB7. *Do the effects of daytime running lights vary systematically with respect to season (winter/summer)?*

ES7. *Do the effects of daytime running light depend on season (winter/summer)?*

p88 Very few studies have investigated variations in the effect of DRL with respect to season. The only studies that may shed some light on the question are the evaluations of DRL-laws in the Scandinavian countries.
...
Andersson and Nilsson (1981) did not find a larger effect of DRL during winter than during summer in Sweden, nor did Elvik (1993) in Norway.

MB8. *What are the effects on accidents involving motorcyclists of requiring daytime running lights for cars?*

p65 Table 4 shows summary estimates of the effects on accidents of DRL-campaigns or DRL-laws for cars.

... Most of the summary estimates of effect are broadly consistent across the three estimators of effect. Estimates referring to motorcycle accidents and to accidents of unspecified type are the exceptions. **Estimates of effect in these two categories are, however, based on small samples and are very far from statistical significance at conventional levels.**

... A weak tendency is found for a reduction of motorcycle accidents.

p89 Estimates of the effect for motorcycle accidents of the use of DRL by cars are available for aggregate effects only. These estimates indicate very small changes in the number of accidents, but most summary estimates point in the direction of accident reduction.

p6 Based on the meta-analysis, it is concluded that the DRL is unlikely to have any adverse effects on accidents involving ... motorcyclists.

MB9. *What are the effects on accidents involving pedestrians or cyclists of requiring daytime running lights for cars?*

p65 Table 4 shows summary estimates of the effects on accidents of DRL-campaigns or DRL-laws for cars.

...

... Pedestrian accidents and cycle accidents are reduced.

p88 There are five estimates of the intrinsic effects of DRL for cars on pedestrian accidents. All these estimates, as well as the summary estimates, indicates a reduction of pedestrian accidents. ...

There are nine estimates of the aggregate effects of DRL for cars on pedestrian accidents. **Five of these estimates show an increase of the number of accidents, four show a reduction. ...**

p88 No estimates of the intrinsic effects of DRL for cars on accidents involving cyclists have been found. There are just three estimates of the aggregate effects, all of them showing a reduction of accidents involving cyclists.

... It is concluded that requiring cars to use DRL is more likely to reduce the number of accidents involving pedestrians or cyclists than to increase the number of such accidents.

p6 Based on the meta-analysis, it is concluded that the DRL is unlikely to have any adverse effects on accidents involving pedestrians, cyclists Some estimates indicate an adverse effect of DRL-laws for pedestrian accidents, but the summary estimate of effect, taking all individual estimates into account, indicates a reduction in pedestrian accidents.

MB10. Are there adverse effects of daytime running lights on cars for other types of accident, in particular rear-end collisions?

p65-66 Table 4 shows summary estimates of the effects on accidents of DRL-campaigns or DRL-laws for cars.

...
... There is a tendency for all types of accidents to be reduced, except for rear-end collisions. For all multi-party daytime accidents, a reduction of 3-12% is found. Pedestrian accidents and cycle accidents are reduced. A weak tendency is found for a reduction of motorcycle accidents. The main pattern of these results is consistent with that found for the intrinsic effects of DRL for cars. **The effects found for rear-end collisions appear to be an exception. For these accidents, the intrinsic effect was a reduction, whereas the aggregate effect appears to be an increase. This is somewhat surprising**, although mechanisms can be imagined that could generate such an effect.

p89 Seven estimates of the intrinsic effects of DRL for cars on rear-end collisions have been found. Six of these estimates indicate a reduction of the number of accidents, one indicates an increase. The summary estimates of effect indicate a reduction of rear-end collisions. **Eight estimates of the aggregate effects of DRL for cars on rear-end collisions are available. Six of the estimates indicate an increase of the number of accidents, two indicate a reduction.** The summary estimates of effect indicate an increase of rear-end collisions. Thus, summary estimates of intrinsic and aggregate effects are inconsistent as far as rear-end collisions are concerned.

p6 **It is likely that a DRL-law could have an adverse effect on rear-end collisions.** Again, however, the evidence is somewhat mixed and alternative interpretations cannot be ruled out.

ES8. Does daytime running lights have adverse effects on accidents involving pedestrians, cyclists or motorcyclists or on rear-end collisions'?

[See MB 8.-10. above]

ES9. What are the costs and benefits of requiring all motor vehicles in the European Union to use daytime running lights?

p100 There are many sources of uncertainty in any cost-benefit analysis. Two of the most important sources of uncertainty concern how well known the effects of a measure are, and how reliable the monetary valuation of these effects are. **Ideally speaking, the total contribution of all sources of uncertainty should be summarised** in the form of a probability distribution for the results of a cost benefit analysis. However, in order to correctly estimate such a probability distribution, all sources of uncertainty must be known, the probability distribution of each variable entering the cost-benefit analysis must be known, and the covariance of the sources of uncertainty must be known. If these assumptions are met, computer programmes can be used to estimate the probability distribution for the results of a cost-benefit analysis.

In the case of daytime running lights, **it is unfortunately not the case that all sources of uncertainty are as well known as they ought to be for estimating the probability distribution of the results. Hence, a simpler assessment of uncertainty has to be made.** ...

p101 **It is obvious that the results of the cost-benefit analysis are highly uncertain.** The possibility cannot be ruled out that benefits are smaller than costs. How likely is such an outcome? Is it possible to estimate the probability that benefits are greater than costs?

As indicated above, the answer to this question is, strictly speaking, no. The reason is that not all sources of uncertainty are known and can be meaningfully quantified. **A crude estimate is, however, possible by making some assumptions with respect to the contribution of uncertain estimates of safety effects** to the uncertainty in the results of the cost benefit analysis.

Appendix 2: Correct description of Andersson et al 1976's Finnish data

1. Andersson et al 1976 only describe their data of accidents in Finland as relating to 'accidents'.

Nevertheless, Koornstra et al 1997, in their Finnish analyses, treated the data as 'casualty data' (p90 *et seq*), ie as data of personal injury accidents.

And Elvik et al 2003 have followed suit.

2. To attempt to supply the definition of 'accidents' that Andersson et al 1976 omitted, Nordic Road Safety Council 1976 Rapport 17 (p34) records figures of '*Polisrapporterade vägtrafikolyckor med personskada i städer och köpingar samt i landskommuner*' [Police-reported road traffic accidents with personal injury in cities and towns together with rural districts] in Finland in 1975 of:

Cities and towns	6487
Rural districts	3850
Total	10337 accidents.

By contrast, in accordance with Andersson et al 1976's definition of 'accidents', the '*Antal flerpartsolyckor på allmänna vägar utanför tätort i Finland*' [Number of multi-party accidents on public roads outside built-up areas in Finland] in 1974, by calculation from the data tables of Annexes 6–9, was 9982 accidents.

It may not be exact to equate NRSC 1976's 'Rural districts' with Andersson et al 1976's 'Non-built up areas'.

Nevertheless the disparity between them respectively of 3850 accidents in 1975, and 9982 accidents in 1974 can only be accounted for by the likelihood that, by contrast with the description of the 3850 accidents as 'Police-reported road traffic accidents with personal injury', the description of the 9982 accidents is 'All police-reported road traffic accidents'.

3. In accordance with this reconstruction from the comparative data of Andersson et al 1976 and NRSC 1976, Koornstra et al 1997 were wrong, and in turn Elvik et al 2003 are now also wrong, to describe the data of Andersson et al 1976 as data of personal injury accidents.

Rather the data of Andersson et al 1976 is almost certainly data of all police-reported accidents.