The Safety Effects of Daytime Running Lights

Matthijs Koornstra, Frits Bijleveld & Marjan Hagenzieker

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A perspective on Daytime Running Lights (DRL) in the EU: the statistical re-analysis and a meta-analysis of 24 independent DRL-evaluations as well as an investigation of possible policies on a DRL-regulation in the EU

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Author(s):	Matthijs Koornstra, Frits Bijleveld & Marjan Hagenzieker
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SWOV Institute for Road Safety Research
P.O. Box 190
2260 BB Leidschendam
The Netherlands
Telephone 31703209323
Telefax 31703201261

Summary, conclusions and recommendations

History
The original reason for the use of Daytime Running Lights (DRL) was not the improvement of vehicle visibility. The use of DRL seems to have originated in 1961 as a campaign to operate motor vehicles with headlights on in daytime as a signal of the intention to comply with a Texas governor's request to drive safely. Also at that time, to quote a remark made by investigators of a DRL effect in the early sixties: `It seems that no one can conceive of an automobile or a Greyhound Bus being invisible on a bright clear day'. This view is applicable to most road users even today.

Research methods and conclusions
In this study the role of perception in accidents and the effects of the introduction of DRL have been reviewed together with all 24 existing evaluations of DRL. Additional statistical analysis and new techniques have been employed to produce the best estimates possible of the full effects of the introduction of DRL in the EU in terms of the saving of lives and reducing the costs of the road transport system.

DRL as a road safety measure is often difficult to understand for the road user because he or she `knows' that with sufficient attention every road user can be seen in daylight. Nevertheless, the research reviewed shows that visual perception in daytime traffic is far from perfect and it is worse in conditions of low ambient illumination. In a striking example 8% of cars in an open field in broad daylight were not visible from relevant distances without the use of DRL. On shady roads or those with backgrounds which mask objects in the foreground the visibility and contrast of cars in popular colours is greatly reduced.

It is known from in-depth accident studies that failing to see another road user in time (or at all) is a contributing factor in 50% of all daytime accidents and for daytime intersection accidents this increases to as much as 80%.

The psychological research reviewed shows that DRL does not only improve the visibility of motor vehicles in daytime, but also influences the timely peripheral perception of vehicles making conflicting movements. Moreover, cars with DRL are better identified as cars and their distances are estimated more safely compared to cars without DRL. All this contributes to the expectation that DRL has positive safety effects, especially in conditions of low ambient illumination. However, until recently, even road safety scientists debated the validity of DRL effects in other conditions than in Nordic winter daylight.

The scientific evidence for the safety effects of DRL in latitudes to the south of the northern Scandinavian countries has only become available recently

(Denmark, Hungary, Canada). Older DRL-evaluations for southern regions mainly concerned DRL for company fleets in the USA, but results, though positive, were not statistically significant. New meta-analysis of the earlier and more recently available DRL-studies, taken together, have now shown that DRL-effects on the same latitudes as those applicable to Europe are statistically significant.

This study investigated for the first time the differences between national and company fleet DRL-effects as well as the DRL-effects on accidents and on casualties. Both are found to be statistically significant.

In this study all existing (24) independent DRL-evaluations have been reviewed and/or re-analysed in order to obtain unbiased, and comparably defined, intrinsic DRL-safety-effects while estimating statistical uncertainties in an optimal way. Intrinsic DRL-safety-effects are defined as the effects of a change from 0% to 100% use of DRL by motor vehicles. The observed effects of DRL will differ, therefore, from the intrinsic effect when DRL usage is not zero at the start and/or not 100% at the end of observations.

The intrinsic DRL-effects calculated in this study cover 9 countries and are combined into 12 national intrinsic DRL effects, 5 on multiple (multi-vehicle) daytime accidents and 7 on casualties in multiple daytime accidents. The result of this analysis is the establishment of statistically significant curvilinear relationships between latitude and national DRL-effects with respect to both accidents and casualties. From the difference between these two relationships an estimate has been made for the relationship between latitude and DRL-effects on fatalities in multiple daytime accidents. *Figure 1* displays these relationships and the 12 national intrinsic DRL effects.

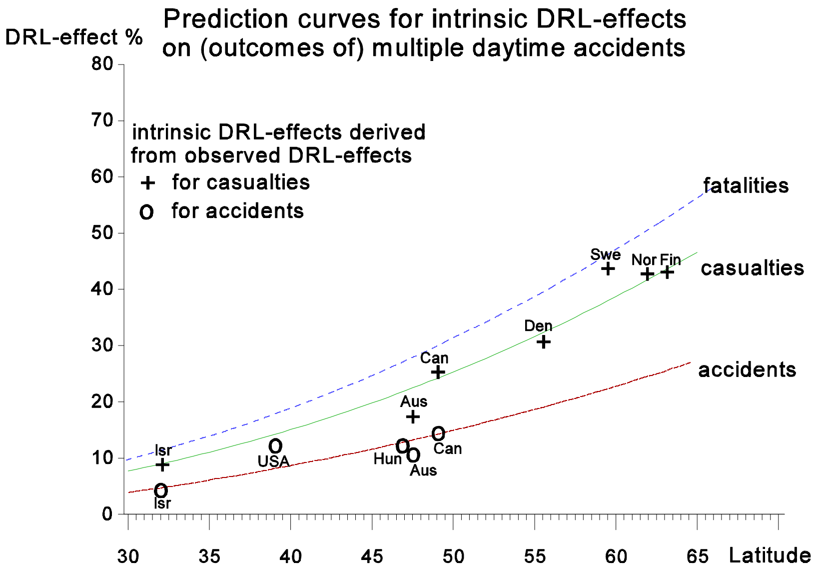

Figure 1. *Prediction curves for intrinsic DRL-effects on (outcomes of) multiple daytime accidents.*

The curvilinear natures of these relationships and the differences between them is explained by the lower ambient illumination levels at higher latitudes and the lower collision speeds in accidents with one or more DRL-users.

Savings and costs associated with DRL

Based on the intrinsic DRL-effects related to latitudes, estimates have been made for all the countries in the EU. The best estimation is that full DRL in the EU, corrected for the existing DRL usage (mainly in Finland, Sweden and Denmark), would prevent:

- 24.6% of fatalities in multiple daytime accidents;
- 20.0% of casualties in multiple daytime accidents;
- 12.4% multiple daytime accidents.

Since only about 50% of all reported accidents in the EU occur when DRL-effects apply, savings must be factored accordingly. Full application of DRL across all EU countries would, therefore, yield the annual prevention of :

- 5,500 fatalities;
- 155,000 registered injured persons;
- 740,000 registered accidents;
- 1.9 million accidents involving insurance claims.

This relatively simple approach to the calculation of savings is possible because it is shown that there are no adverse effects of DRL on road users not directly involved in the change. Pedestrians benefit in the same way as car occupants and there is no change in the risk to motorcyclists (already using DRL).

The financial basis for calculating savings is taken from the recently adopted EU road safety programme which is based on an overall calculated saving of 1 million ECU per fatality saved. However, accidents which can be prevented by DRL are relatively severe and simply using the average overall cost per fatality would exaggerate savings by about 13%. When corrected for this the 1 million ECU per fatality prevented becomes 0.87 million ECU when applied to DRL.

The total annual saving, therefore, is 0.87 million x 5,500 = 4.78 billion ECU.

The annual economic costs of automatic in-vehicle DRL have also been researched and the additional annual costs are:

Fuel costs	1.13 billion ECU
Car costs	0.08 billion ECU
Bulb costs	1.26 billion ECU
Environmental costs	0.18 billion ECU

Annual economic costs	2.65 billion ECU

Using these figures the benefit/cost ratio for full DRL in the EU is:

4,78 billion ECU benefits
-------------------------------- = 1.80
2.65 billion ECU costs

Recommendations for action

1. Both the scale of potential saving of lives and the benefit/cost-ratio demonstrated in this study indicate that the introduction of DRL across the whole EU is desirable and urgent.

2. On technical, practical and legal grounds it is recommended that compulsory DRL, when implemented in the EU, should be an automatic in-vehicle system that uses the existing low beam headlights (or special DR-lamps in the long run). Introduction in this form is expected to be more readily accepted than a DRL-obligation requiring behavioural changes by motorists (see remarks on perception, above, and sections 6.2 and 6.3).

3. The environmental costs, due to emissions of the 0.9% additional fuel needed for the light energy of DRL, are of importance. Environmental organisations have been against the introduction of the DRL-obligation in Denmark and have influenced political decisions on DRL-obligations in The Netherlands and Austria. In its conservative approach to benefit/cost calculations this study has identified a simple basis for the cost of environmental damage while ignoring the benefits provided by the savings. Past experience suggests that it would be wise to identify these benefits so that environmental arguments can be countered and the correct net effect of the introduction of DRL identified.

4. While it is very important that DRL safety effects are understood by policy makers, politicians and others with a professional interest, it is likely that public acceptance of compulsory DRL will require some form of social marketing of the policy in order to raise general awareness of the benefits of DRL. This should be a part of an implementation strategy to be developed. There will be additional costs associated with this recommendation but they will be `start-up' costs which can be set against the benefits over a period of time.

ERRATUM

Revision of additional costs and benefits for full DRL in the EU

In our report "The Safety Effects of Daytime Running Lights" (SWOV, R-97-36) annual costs and benefits of automatic in-vehicle DRL are estimated (pp.163-165). However, there is a mistake in the estimate for the additional fuel costs of DRL, and a probably too high estimate of the share of fatalities in multiple daytime accidents.

Revised costs for DRL
On page 162 the additional fuel consumption is estimated to be .17 to .15 litre per 100 km for cars with a fuel consumption of 10 litre per 100 km. Since 55% of the kilometres are daytime kilometres (incl. half the dawn and dusk periods) the additional fuel use is estimated to be 0.9%. So far the calculation is acceptable.

However, on page 163 the annual 2850 billion kilometres of motor vehicles in the EU are first reduced to the relevant 2690 billion for the EU (without Sweden, Denmark and Finland with compulsory DRL) and then again reduced to 55% for daytime kilometres as 1480 billion. The latter figure is divided by 10 (for 10 litres per 100 km) and is then incorrectly multiplied by 0.009 for the calculation of the additional fuel use by DRL as 1330 million litre fuel. In this way, the reduction for the 55% daytime kilometres is applied twice, which error is brought to our attention by colleagues from Germany (Bast).

The correct calculation is: (2690 billion/10) * 0.009 = 2420 million litres of additional fuel use by DRL as low beam headlights. For an average of 0.85 ECU per litre it means annual costs of 2.06 billion ECU for additional fuel and not the 1.13 billion ECU that is reported. We apologise for the error in the calculation. The revised total of additional costs for DRL are 3.58 billion ECU, instead of the reported 2.65 billion ECU.

Revised share of DRL-relevant fatalities
The additional benefits of full DRL use in the EU are based on the 1 million ECU per fatality, adopted by DG-VII. It is assumed that about 50% of the total fatalities occur in multiple daytime accidents, based on the countries where the statistics contain the differentiation of fatalities in single daytime, multiple daytime, single nighttime and multiple nighttime. After the publication of our report we obtained from German and French colleagues additional information.

For Germany in 1995, there were 9454 fatalities, but in multiple daytime accidents there were 3453 fatalities and in multiple accidents in dawn and dusk periods there were 307 fatalities (information from Bast). Taking half of the latter figure as relevant for DRL, this means that the DRL-relevant share of the fatalities is not 50% in Germany, but 38%. For injured persons, the DRL-relevant share in Germany is about 58%, which is higher than the assumed 50%.

In France the exact percentages are not known to us, but the daytime injured are 67% of all injured in road accidents and those injured in multiple accidents have a share of 79%. Thus, also for France the share of the injured in multiple daytime accidents is probably not lower than 50% (since .67*.79=.529). However, in France the fatalities in daytime accidents have a share of 53% and the share of fatalities in multiple accidents is 64%. So also for France the DRL-relevant share of the fatalities (their percentage in multiple daytime accidents) probably is less than 50%, and may even be close to about one third (since .53*.64=.34).

Also for the four most southern EU-countries we do not have the precise shares of DRL-relevant fatalities, but for other countries with known shares of DRL-relevant fatalities that average share is about 50% (incl. Great Britain). Since France and Germany account for 40% of all EU-fatalities, the share of the DRL-relevant fatalities in the EU (excl. Sweden, Finland and Denmark, which already use DRL) probably is less than the 50% that was assumed in our report.

Revised benefits from DRL

The DRL-relevant share of the EU-fatalities (multiple accidents during daytime and half the dawn and dusk periods in the EU) is better estimated as 45%, or conservatively estimated as 40%. The DRL-relevant share of 50% of the casualties still seems not to be overestimated, but for accidents the 50% may be an underestimate. If we take 40% as the DRL-relevant share of fatalities, then not the reported 5.500 fatalities would be additionally prevented by full DRL in the EU, but about 4.430 fatalities [deaths within 30 days * DRL-relevant share * additional DRL-effect = 45.000 * .40 * .246 = 4.428]. The additional numbers saved by full DRL in the EU would then be 4.430 fatalities, 155.000 injured and more than 740.000 accidents. Their ratios with respect to fatalities are no longer different from the ratios that underlie the EU-estimate of one million ECU costs per fatality. The one million ECU per fatality (including costs for the concurrently occurring injuries and damage-only accidents per fatality) then needs no correction to 0.87 million ECU per fatality, used in our report. Therefore, and in view of the revised DRL-relevant share of fata1ities, the revised benefits from full DRL in the EU are 4.43 billion ECU.

Revised benefit/cost ratio

The revised benefit/cost ratio for automatic in-vehicle DRL with low beam headlights is 1.24 (=4.43/3.58), instead of the reported ratio of 1.80.

That lower ratio, mainly due to the corrected additiona1 fuel costs, may indicate that one better introduces automatic in-vehicle DRL with special DRL lamps (21 W. with centre beams of no more than 800 cd.). These special DRL lamps use about 45% of additional fuel for low beam headlights, which also decreases the additiona1 pollution costs of DRL. It also asks less costs for bulb replacements, but causes extra costs for the car manufacturer. Thus higher prices for cars with special DRL lamps and lower other costs. The estimated annual costs for automatic in-vehicle DRL with special DRL lamps are:

fuel costs	1.18 billion ECU
car costs	0.70 billion ECU
bulb replacement costs	0.55 billion ECU
environmental costs	0.08 billion ECU

Tota1 additional costs	2.51 billion ECU

The benefit/cost ratio for automatic in-vehicle DRL with special DRL lamps becomes then 4.43/2.51=1.76. A behavioural obligation for DRL with low beam headlights has no additional car costs for automatic DRL (0.08 billion ECU) and its benefit/cost ratio is 4.43/3.50= 1.27. If behavioural DRL is combined with automatic in-vehicle DRL for specia1 DRL lamps in new cars then the benefit/cost ratio increases from 1.27 to 1.76 over the years until all motor vehicles are equipped with automatic DRL lamps.

M.J. Koornstra, director SWOV, 27 April 1998.

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1. Visual perception, road user behaviour, and DRL

In this chapter the research literature on why and how daytime running lights (DRL) could affect road user perception and behaviour is discussed: how does DRL influence visual perception, attention, and behaviour of road users, on which mechanisms is an effect of DRL based?

The greatest problem when determining the effect of any measure on visual performance or assessments (in terms of detection, visibility, conspicuity, etc.) is that the relationship between such indirect measures and behaviour and accidents has not been sufficiently documented. An improvement in `visibility' does not necessarily mean that driver behaviour will change. Nevertheless, it is worthwhile investigating these 'perceptual aspects'. Effects in terms of accidents can be better understood if the preceding processes are also considered. Insight into the underlying factors that could explain the effect of DRL also allows the assessment of specific hypotheses in future accident studies. When one considers the various stages of information-processing, i.e. perception - evaluation - decision-making - action, it will be clear that if something goes wrong at an early stage (e.g. perception), subsequent steps will be affected.

It hardly needs saying that the information relevant to those participating in traffic is predominantly visual in nature (Sivak, 1996). `Not seeing' a certain object is of crucial importance, as a mistake at this early stage will handicap each subsequent process - such as recognition, decision-making and action. Lighting on vehicles play a twofold role with regard to perception: it is important for `seeing' and `being seen'. In general, vehicle lighting is related to both how the vehicle is seen by others and how the vehicle illuminates its surroundings. One characteristic function of DRL is not so much to light its surroundings (as lights at night), allowing the driver of the vehicle to `see' properly, but to allow the vehicle to be `better seen' by others (compared to the vehicles not using lights). DRL will therefore be used mainly to make the vehicle more `visible' to others.

What could DRL add to the visual information that is already reaching us in traffic? Arguments relating to 'conspicuity' and 'detectability' etc. are often put forward. DRL could help to make vehicles more conspicuous, they could be detected sooner, they would be recognised sooner and/or better, the distance to other vehicles would be more accurately estimated, etc. The likely influence DRL would have on visibility, detection, conspicuity, recognition, and identification and judgments of e.g. distance will be discussed in this chapter. In addition, possible adverse effects of DRL will be discussed: For example, lighting in the daytime or during twilight may cause glare; road users without lights (e.g. cyclists and pedestrians) might become less conspicuous as a result of DRL; or brake-lights of cars using DRL could be masked.

First the concept of visual perception and terms such as conspicuity, detection, and glare will be discussed, and hypotheses that are commonly put forward for the use of DRL and which are related to perceptual (and, - consequently - behavioural) issues will be listed, as well as a number of hypotheses pointing at possible adverse effects or effects reducing the assumed benefits of DRL. Then some research findings will be presented related to these assumptions. Finally, some conclusions will be drawn.

1.1. Visual perception

Visual perception is a concept which refers to all perceptual processes and results imaginable. As a result of its generalised nature, the literature often distinguishes between the various aspects of perception. Concepts such as detection, conspicuity and visibility are often mentioned in the 'perception literature'. For the purposes of clarification, therefore, some of these concepts will now briefly be discussed.

Visibility and detection
The concepts of 'visibility' and 'detectability' are often interchanged. Visibility can be defined as a 50% probability of detection (threshold of visibility). If an object becomes 'more visible', it is generally implied that its detection 'improves' in one way or another, so that the probability of detection becomes increasingly greater; this implies that, in general, an object can be detected at a greater distance, or that observers need less time to decide whether or not an object is present (reaction time).

Visibility is subject to a human assessment component, as there is no equipment that can directly measure 'visibility': human intervention is always necessary to determine this parameter. Often, such factors are studied with the aid of detection experiments. One important factor that determines whether an object is detected is the contrast between object and background. Although contrast is related to visibility, it is not the same thing. Di Laura (1978, quoted in Sanders & McCormick, 1987), offers a simple example of this phenomenon. Take an object that contrasts 50% with the background on a large stage in a theatre and illuminate it with a pocket flashlight: it will hardly be visible. That same object, lit by a large floodlight measuring 10,000 times the luminous intensity of the flashlight. The contrast remains the same, but the 'visibility' differs markedly. Both luminance and contrast are important for visibility. Another factor is the size of objects; large objects are more visible than small ones. The degree to which the visual system is sensitive to contrast is therefore not the same under all circumstances. Blackwell (1946; 1968) has probably conducted the most extensive research into the sensitivity of the visual system. For example, the lower the luminance level, the greater the contrast between an object and its background should be in order to ensure the same probability of detection. But given a particular luminance, the detectability of an object will improve if the contrast with the background is enhanced or if the object is larger, for example.

Visibility and conspicuity
Sometimes visibility means more than simply 'detecting something'. One can detect 'something' amongst other elements; in that case, one can speak of 'conspicuity'. Conspicuity implies that a particular object must 'compete' with other objects in order to 'attract attention', while visibility implies the detection of the presence of a particular object against an 'empty' background. Visibility does not necessarily imply conspicuity; a particular object may also be visible between similar objects (i.e. be detectable), but may not necessarily be conspicuous.

There are many definitions that describe the term conspicuity. Wertheim (1986) and Theeuwes (1989) have offered an overview of these definitions. The measurement and definition of conspicuity is performed in so many different ways that it is in fact impossible to speak of *the* conspicuity of an object. However, all definitions of conspicuity do share a reference to ‘attention’: a conspicuous object draws attention to itself (Theeuwes, 1989, p. 14). All definitions also state that external, physical factors determine the conspicuity of an object.

According to Engel (1976, p. 87), visual conspicuity is defined as the ‘object factor, or more precisely, as the set of object factors (physical properties) determining the probability that a visible object will be noticed against its background’. Eccentricity, i.e. the angle between the object and the direction of view, is an important factor in conspicuity (Cole & Hughes, 1984; Engel, 1976). The contrast between object and background and the complexity of that background is also important.

Nevertheless, factors other than external ones can influence conspicuity. Engel (1976) makes a specific distinction between visual conspicuity (bottom-up) and cognitive conspicuity (top-down). In more or less the same manner, Hughes & Cole (1984) have pointed out that conspicuity cannot only be regarded as characteristic of an object, precisely because it has to do with attracting attention. Whether an object will attract the attention of an observer is largely determined by that observer. Hughes & Cole therefore distinguish between two types of conspicuity: ‘attention conspicuity’ and ‘search conspicuity’. The first type refers to the possibility that an object will attract the attention of an observer who is not specifically looking for such an object. The second type, ‘search conspicuity’ is defined as the characteristics of an object that allow it to be easily and quickly localised if the observer is looking for it.

Hughes & Cole (1984) summarise a number of factors that also determine whether an object will be conspicuous or not:

- physical properties of the object and its background;
- the information that is supplied, including information concerning the unusual or unexpected nature of the object;
- the observer’s need for information (is the observer looking for a particular object? etc.);
- the perceptual strategy of the observer (road user), which is also determined by the information in his environment and his need for information.

Detection, conspicuity and DRL

In general, the greater the contrast between the vehicle and its background, the greater the probability that it will be detected. For light coloured cars (paint), the contrast is generally greater than for dark coloured cars (Allen & Clark, 1964; Dahlstedt & Rumar, 1976). But the contrast of a light coloured car against the background does not alter if the ambient illumination changes. Because the visual system’s sensitivity to contrast diminishes with decreasing illuminance, the probability of detection will grow smaller as the ambient illumination drops.

Even on sunny days, the ambient illumination can vary considerably.
The driver is not only confronted by a diversity of background luminances

caused by the background itself, but also by more marked changes as the background alternates between shade and full sun. As a result, a vehicle that should be clearly visible in direct sunlight becomes relatively difficult to see in dark shade. The luminance of a light source, on the other hand, is constant - if the source is bright enough, its luminance will be greater than that of unlit objects in the surroundings. As the ambient illumination decreases, the contrast between the light source and its background will actually increase. Therefore, if a vehicle cannot be properly detected for one reason or another, it is always ‘advantageous’ for that vehicle to use lighting. This is particularly true during twilight, poor weather conditions - e.g. during rain, fog and snow, and when the sun is very low on the horizon - e.g. sunrise and sunset. Even on very sunny days, a car without lighting can easily ‘disappear’ into the background, e.g. in the shade of buildings or trees. The use of lighting can ensure that - thanks to the heightened contrast - a vehicle can still be easily detected under such conditions (Helmers, 1988).

Recognition, identification and the role of expectations

The most elementary form of perception is detecting whether ‘something is there’. It becomes more complicated when someone must also indicate the category of object that ‘something’ belongs to: the recognition or identification of objects. The terms recognition and identification are often interchanged, and imply that an object is given the right label by an observer (‘this is a car’). Some authors (Haber & Hershenson, 1980) have noted that with recognition, one is only stating that the object concerned has been ‘seen before’, while identification implies more than that: the recognised object is identified as belonging to a particular category, e.g. a car. In recognition and identification, experience and memory play a role. It is of course essential that road users ‘see’ relevant objects (in this case implying detection). But the detection of ‘something’ is generally insufficient to allow adequate decisions with regard to behaviour in traffic. This is why it is important that the correct interpretation is given to that which has been detected; the correct meaning or identification must be associated with the visual impression.

An event or action can be generated by ‘the surroundings’, or by the observer who is actively looking for a particular part of the surroundings, or else by an interaction between these two processes. The distinction between the processing and perception of ‘physical characteristics’ and the observer’s influence on this process of perception is also indicated by the terms for ‘bottom-up’ versus ‘top-down’ processes (Anderson, 1983).

Various researchers (Hughes & Cole, 1984; 1990) have shown that the observer himself exerts significant influence on whether a particular object is noticed. An observer who *expects* to encounter objects with certain physical characteristics, will more readily ‘see’ them than when he does not expect them. Hills (1980, p. 190-193) emphasises the role of expectations in traffic: ‘Another important factor affecting a driver’s detection and perception of a potential hazard is his perceptual ‘set’ or his expectancies. These are formed both from long-term experience and by the short-term experience of the previous few minutes driving. These can profoundly affect the driver’s interpretation of various visual features and signals in a scene and also the various visual judgments he has to make.

The incorrect selection of information from the surroundings (e.g at the wrong time, wrong information) can lead to accidents. The selection can occur both via top-down or bottom-up processes. Here we may use DRL to illustrate these processes. The lighting sec could ensure that the observer will 'automatically' look in that direction (bottom-up; attention conspicuity), in fact without his being conscious of the fact; it is also possible that - as the observer knows that all cars will always use lighting - he will be actively looking for such cues (top-down; search conspicuity). These processes can also be operating at the same time.

Expectations, systematic coding, and DRL

During dusk and dawn periods the road user has to contend with various cues to detecting other vehicles: there is a considerable mixture of vehicles with and without lights during these periods (Williams, 1989; Lindeijer & Bijleveld, 1991). This contrasts with the daylight situation where (apart from motorcycles) vehicles are predominantly unlit and at night when all vehicles are lit. The cues during dusk are also complicated by a variety of 'parking lights' which can range from virtually useless to very conspicuous. This variation can have serious implications towards the end of dusk when unlit vehicles are not only harder to detect, but also less 'expected'. In this case a motorist may have become accustomed to expecting that all other vehicles will have headlights on as this may have been the case for the last 10 minutes or so. An occasional unlit vehicle may not then be detected as readily and could thus be at some degree of extra risk.

The argument of 'homogeneity' has often been used with respect to road safety (Schreuder & Lindeijer, 1987). A disorganised multitude of (visual) elements in the field of vision can be dangerous, as it is then difficult to offer predictions about how the visual environment will look in the near future. The *systematic coding* of cars by means of lighting[1], for example, can ensure that road users learn to expect that motor vehicles participating in traffic have their (head) lights switched on. In this way, they can be more immediately recognised as being relevant objects to take into account, implying consequences for behaviour. Reversing this reasoning, this means that vehicles not using lights will no longer be expected, and therefore recognition will probably be delayed. The latter is only relevant with partial DRL use over a large percentage of users. Furthermore, homogeneity in the use of DRL - in any case under those circumstance where it is really necessary (i.e. during fog, rain, twilight) - means that everyone will at least be visible to the same extent.

Finally, it can be noted that detection, conspicuity, and recognition are all gradual matters and that 'visibility' (i.e. 'seeing' something) is in practice the outcome of all three factors. In practice, people are more or less satisfied with a degree of certainty; that they have seen something, have something localised, or know what that 'something' actually is.

[1] All cars painted in the same (light) colour would also offer an efficient coding system in this context, provided that the 'colour' coding does not indicate whether the car is actually participating in traffic, for example: a parked car will generally not use lighting and can therefore be recognised as 'not participating in traffic' at that moment; a 'red' or 'white' car will always be that colour, also when it is parked.

Glare and masking
In general, glare may be understood to be caused by luminance in the visual field which is considerably greater than the luminance to which the eyes are adapted, and therefore results in discomfort, hinder, irritation, or loss of visual performance and visibility. The sensitivity of the visual system adapts to the luminance of the surroundings. In simple terms, this means that the eye becomes desensitised (to light) as the adaptation luminance increases. When objects appear in the field of view, their luminance differing greatly from one another, the eye must constantly adapt as it looks from one to the other. This is called ‘transient adaptation’ and temporarily reduces the ability to ‘see’, until the eye has again adapted to its ‘new’ luminance. Aside from transient adaptation, the literature also distinguishes between:

- ‘*discomfort glare*’, also known in the Netherlands as ‘psychological blinding’ (German: ‘psychologische Blendung’; Arendt & Fisher, 1956, quoted in De Boer, 1967);
- *‘disability glare’*, also known in the Netherlands as ‘physiological blinding’ (German: ‘physiologische Blendung’);
- ‘*blinding glare’*, which can be regarded as ‘absolute blinding’. Blinding glare is of such an intensity that for a considerable period of time, nothing can be seen, and people are literally blinded (Kaufman & Christenson, 1972). Blinding glare requires such high luminance levels, however, that this form of glare is hardly experienced in practice.
  The following paragraphs, therefore, discusses discomfort and disability glare.

Discomfort glare (‘psychological blinding’)
Discomfort glare is the feeling of irritation or annoyance caused by high or irregular distributions of luminance in the field of view. The underlying processes causing discomfort glare are insufficiently documented. Much research has been conducted into the experiences of glare. As irritation or ‘discomfort’ is a subjective experience, it must be established by asking people to indicate the level of glare when exposed to a glare source (e.g. by giving it a particular score).

There are various ways to determine discomfort glare (Boyce, 1981; De Boer, 1967; Sivak & Olson, 1988). All methods demonstrate a marked similarity, and the results obtained through the various methods therefore correlate quite well. However, it is still not known what exactly constitutes this discomfort glare and what causes it.

Disability glare (‘physiological blinding’)
Glare which interferes with visual performance and visibility is called disability glare. Light entering the eye is scattered in the eyeball due to irregularities of the lens and the liquid in the eyeball. This scattered light creates a veiling luminance on the retina and reduces the contrast of the target viewed, making it less ‘visible’. This ‘glare effect’ is also called ‘*masking*’. In the last decades, an enormous amount of research has been conducted into this type of glare (Schreuder & Lindeijer, 1987).

Glare and DRL
The national and international standards for lighting on motor vehicles take into account disability glare. For example, the European standard states that the so-called glare intensity of low beam headlights in the direction of

oncoming traffic must not exceed 250 cd. Discomfort glare is not referred to in any of the standards however; disability glare - which affects visual performance - is considered more important than discomfort glare. With sufficient intensity under low ambient illumination, DRL lamps may cause masking of unequipped oncoming vehicles or may mask turn signals if these and the DRL lamps are mounted close together. With the question of whether glare will result when using lighting under high ambient illumination, the principal question in fact relates to whether - under particular conditions, e.g. twilight - discomfort glare would be an issue. In general, luminance levels in the daytime will be so great - and, as a result, the difference in luminance between a headlight and the background will be so small - that there can be no question of disability glare.

Behavioural adaptation and novelty effect
Behavioural adaptations can be defined as non-intended behaviours which may occur following the introduction of changes to the road-vehicle-user system and which are additional to the intended behavioural change; behavioural adaptations occur as road users respond to changes in the road transport system such that together with the induced change also their personal needs are achieved as a result, they create a continuum of additional effects ranging from an increase to a decrease in safety (Evans, 1985; OECD, 1990). For behavioural adaptation to occur it must be assumed that there is feedback to road users, that they can perceive the feedback (but not necessarily consciously), that road users have the ability and the motivation to change their behaviour. Feedback refers to knowledge and information received from the system (road-vehicle-road user) which results from changes in road users’ behaviour. Feedback, in this sense, is a major component of a number of driver behaviour models (Wilde, 1982; Fuller, 1984; Koornstra, 1990b).

Feedback occurs at a number of different levels. There is immediate feedback which, for example, would involve the perception of a newly installed traffic sign. Next, there is feedback from the system components, the vehicle, the road, the driver, and other road users. This feedback provides drivers with information about how their responses to the initial change is affecting vehicle performance and the behaviour of other road users, as well as how the initial change is affecting personal goals.
In addition, there is a more subtle feedback which results from observing the road system over time, and detecting changes in other drivers’ behaviours and the occurrence of incidents in the transport system such as accidents and near-collisions. This latter feedback probably cannot be verbalized by the road user, and must be inferred from long-term changes in behaviour. This raises the issue of whether drivers must be aware of feedback for it to affect their behaviour. In other areas of psychology, it has been argued that people need not be aware of stimuli in order for them to have an effect on behaviour; so it is likely that the driver does not need to be aware of the subtle feedback, which may occur over a long period of time, for it affects behaviour.

When a change is made to one component of the road-vehicle-user system, road users may be required or may be expected to respond to the change in some way which is consistent with a goal of greater safety. Thus, a safety measure may elicit an initial response from the road user. The initial response may be predictable and lead to some safety benefit (this is called the ‘novelty effect’). Behavioural adaptation occurs after the initial response and is a process during which road users incorporate the change into their normal behaviour, modifying the initial response on the basis of their perceptions of the vehicle, the road, other road users, and their personal goals of safety and mobility.

Behavioural adaptation and DRL
For some safety measures behavioural adaptation has been demonstrated (Rumar et al., 1976; Shinar et al., 1980; Smith & Lovegrove, 1983). Behavioural adaptation need not eliminate all safety benefit resulting from a change. Rumar et al. (1976) note that, although drivers drove faster with studded tires, there remained a net safety gain. It has been suggested that drivers may increase their risk-taking (e.g. driving at higher speeds) in response to perceived safety benefits of their car and other cars having DRL (Elvik, 1993; Perel, 1991; Williams & Lancaster, 1995).

1.2. Hypotheses regarding effects of DRL

The hypothesized effects of DRL, related to perceptual and behavioural processes as described in the previous sections, can be summarized under the following main headings:

Positive effects of DRL

a1. DRL increase visual contrast between vehicles and their background, and therefore increase conspicuity/visibility.

a2. DRL result in an increase in detection distance (and therefore allows drivers greater margins of safety in overtaking and turning interactions with DRL-equipped vehicles).

a3. DRL result in more accurate or ‘safer’ judgments of speed and distance.

a4. DRL as a consistent feature for identification can have influence on the ‘perceptual set’ of road users, facilitating identification and recognition of DRL-equipped vehicles.

Adverse (side) effects of DRL

b1. DRL can cause glare in dawn and dusk periods.

b2. Vehicles without DRL could be masked when they are ‘surrounded’ by vehicles with DRL.

b3. The relative conspicuity of bicyclists and pedestrians could decrease with DRL.

b4. Signal lights such as direction indicators and brake-lights (if rear position lights go on at the same time) could be masked by DRL.

b5. In countries where DRL for motorcycles was required prior to mandating DRL in cars, motorcyclists might lose their conspicuity advantage.

b6. Drivers may increase their risk-taking, driving at higher speeds, for example, in response to perceived safety benefits of their car and other cars having DRL (behavioural adaptation).

b7. Initial positive effects of DRL could diminish once people get used to vehicles with DRL, and with time their noticeability could be reduced, or drivers could come to ignore the extra information (novelty effect).

Research findings
In this and subsequent paragraphs, a number of studies are presented which relate to the question of when and how (visual) performance improves when vehicles use DRL, in comparison to the situation when they do not use their lights. The research findings are arranged according to the hypotheses and assumptions as presented in the previous section.

Ad a1. Assessments of (central) visibility/conspicuity
Hörberg & Rumar (1975; 1979) assessed the relative visibility of vehicles by means of 'paired comparisons'; test subjects had to indicate which of two vehicles was 'more visible'. One of the cars always used lighting (50, 150 or 400 cd), while the other did not. The ambient illumination was about 2,500 - 5,000 lux. The results showed that subjects even think that a car with a 50 cd lamp was more visible than a car without lights; better visibility however only became clearly apparent at 400 cd.

Allen & Clark (1964) established visibility with the aid of a 'visibility metre'. They noted that a lamp of 21 cd mounted to the front of a car at an illumination of 2,000 ft cd (= 21.529 lux) was just as visible as a black car. At 750 ft cd (= 8.074 lux), the 21 cd lamp was just as visible as a white car; at 250 ft cd (= 2.691 lux), the 21 cd lamp was better visible than cars without light. The article by Allen & Clark does not clarify exactly how this visibility meter worked.

Ad a2. Detection experiments
It has been argued that central visibility studies (Allen & Clark, 1964; Hörberg & Rumar, 1975) are of limited value, for example due to the very long DRL detection distances typically found for direct viewing with DRL-equipped vehicles. However, some difficult ambient conditions will exist in which improved central detection due to DRL is relevant (driving under extreme glare conditions or in demanding task situations). Peripheral detection studies are considered to be a better technique for relating DRL conspicuity (i.e. initial noticeability of the other vehicle) and detection of other (unexpected) approaching vehicles (i.e. at places that are not looked at centrally) to ambient illumination and DRL intensity (Rumar, 1980; Ziedman et al., 1990). A number of studies on peripheral detection of DRL lamps will be summarized in this section.

Hörberg & Rumar (1975; 1979) conducted a number of experiments to examine the effect of luminous intensity, size and colour of headlights on the detection distance of approaching vehicles by an observer at various angles of view (30° and 60°). The experiment was conducted on the runway of a military air base. The ambient illumination varied from 3,000 to 6,000 lux. The researchers used lamps of 50, 150, 400, and 60,000 cd (high beam headlights) and compared the detection distance with that obtained when lighting was not used. The results showed that headlights must be brighter to detect vehicles at a visual angle of 60°, compared with detection at a 30° angle of view over the same distances. At 60° peripheral perception, a considerably greater luminous intensity (>400 cd) is necessary to improve the detection distance at ambient illuminations between 3,000 and 6,000 lux (early twilight). At a 30° visual angle, a luminous intensity of 400 cd causes the detection distance of a vehicle to almost double, when compared with the same vehicle without lighting (see *Figure 2*, from Rumar, 1981).

Figure 2. *Mean vehicle detection distance (m) in daylight for two peripheral angles as a function of running light intensity (cd) and surface area (cm^2). (From: Rumar, 1981).*

Hörberg (1977; Hörberg & Rumar, 1979) used a similar experiment to study detection distances of vehicles at an angle of 20° for a number of different ambient illuminations, varying from 125 to 1750 lux. Lamps of 100, 200 and 300 cd were used. The results showed that the detection distance became greater as the luminous intensity of the lamps increased, up to a daylight illumination of about 1,000 lux. At ambient illuminations measuring over 1,000 lux, no improvement in detection distance was noted (none of the three light intensities).

Kirkpatrick et al. (1987) conducted a similar experiment. The detection distance of a vehicle that approached an observer at an angle of 15° was established under various daylight conditions. Lamps with a luminous intensity of 250, 500, 1,000, and 2,000 cd were used at ambient illuminations of 20,000 and 70,000 lux (bright daylight conditions). The results showed that the detection distance increased as the luminous intensity of the lamps increased. The average improvement in detection distance was about 24 m when the results for a 2,000 cd lamp were compared to unlit conditions. At an ambient illumination of 20,000 lux, an improvement in the detection distance was noted from light intensities of 1,000 cd; at a greater ambient illumination of 70,000 lux, improvement was only noted after 2,000 cd.

Attwood (1975; 1981) performed a similar study, but at a much larger range of ambient illuminations. *Figure 3* represents the result. Vehicles are detected sooner when the (low-beam) headlights were on than when they were off. It is assumed that the lamps have a luminous intensity of 600 cd (based on SAE standards). The detection distances were more or less constant over the entire range of ambient illumination when the vehicles used lighting. If they did not, the detection distance diminished as the ambient illumination decreased. At values of background luminance over about 100 cd/m^2, no further improvement was shown in detection distance if results were compared between the use (yes or no) of DRL; at lower values, the detection of a vehicle with lighting improved as the background luminance declined.

Figure 3. *Detection distance versus background luminance with and without DRL (From: Attwood, 1981).*

Perel (1991; Ziedman et al., 1990) also evaluated subjects' ability to detect a car approaching in their peripheral visual field (20°). Experiments were conducted on an airport runway in California. The detection of cars was a secondary task; the first task was a central vision task, namely a flash-count task. Under 'high' ambient light conditions (4,000 foot candles or more), DRL with an intensity of 1,600 cd provided a statistically significant improvement in peripheral detection distance. 800 cd lights showed a trend toward increased detection distances under lower ambient light conditions, but was not statistically significant. There was no improvement from 800 cd lights under high ambient conditions.

Ad a3. Experiments on distance and speed estimation, and gap acceptance
One of the hypotheses is that DRL would result in more accurate or ‘safer’ judgments of e.g. speed and distance. A number of experiments have been conducted to investigate these issues.

Hörberg (1977) studied the effects of the luminous intensity of headlights on the estimation of distances. Test persons had to compare the distance to two parked cars standing on different carriageways at a distance of between 250 and 550 m from the observer. One of the cars did not have its lights on, the other did (luminous intensity of 300 or 900 cd). The distance between the vehicles was 0, 15, 30, or 60 m and the test subject had to decide within several seconds which of the two cars was closest. The ambient illumination was 4,000-5,000 lux. Apparently, as the luminous intensity of the headlights increased, the estimated distance to that vehicle became smaller. In other words: if both vehicles were at the same distance from the observer, the vehicle with lights on was estimated to be closer than the unlit vehicle. It can be assumed that estimating a vehicle to be closer is ‘safer’, as a driver will respond more rapidly.

Attwood (1976; 1981) studied whether lighting on vehicles at various background luminances exerted an influence on ‘gap acceptance’. Test subjects had to decide in a simulated overtaking task when they could just overtake with safety, while a car (with or without lights) was approaching. The minimal accepted gaps varied, both depending on the intensity of the headlamp and the background luminance. The estimated luminous intensity of the low-beam lamp that was used is 600 cd (based on the SAE standard), that of the reduced low-beam lamp is estimated at 200 cd. At a background luminance of 343 cd/m^2, the low-beam lamp resulted in a considerably larger gap (70 m) acceptance when compared with the situation without light, or with the reduced low-beam lamp (20-25 m). At a very low background luminance (4.6 cd/m^2) the gaps had to be far greater before they were accepted as ‘just safe’, both with the low-beam and with the reduced low-beam lamp (120 to 50 m respectively). The acceptance of a larger gap can be interpreted as a ‘safer’ performance with respect to the situation without lighting.

Attwood (1981) discusses other studies concerned with gap acceptance in situations other than overtaking. Two studies (Olsen, Halstead-Nussloch & Sivak, 1979; Radideau, 1979) examined the gap accepted by still-standing drivers at intersections when another vehicle approached the intersection. It was hypothesized that the accepted gap for a clearly visible vehicle (i.e. with running lights) would be larger; that is, the driver would reject gaps that would otherwise have been accepted when the approaching vehicles were less conspicuous. The results of both studies are equivocal: for cars with DRL relative to cars without, a higher percentage of shorter gaps were both accepted and *rejected.* Theeuwes and Riemersma (1990) remark that although the results regarding gap-acceptance at stop-controlled intersections were not clear-cut, it is generally claimed that DRL-usage leads to larger safety margins, a finding which seems undoubtedly related to traffic safety. Although it is claimed that this increase in safety margins should be attributed to an underestimation of distances with DRL relatively to cars without DRL, one can also argue that larger safety margins are applied because DRL tends to reduce the distance estimation accuracy.

With regard to speed estimation, no research seems to be conducted specifically related to DRL-equipped cars. Two studies are referred to in the literature that investigated speed estimation for *motorcycles* with and without DRL (Shew et al., 1977 cited in e.g. Prower, 1990; Howells et al., 1980 cited in FORS, 1990). Shew et al. found that with headlights off, the speed of a motorcycle was estimated to be higher than when headlights were on, which implies that DRL on motorcycles will lead to speed judgments than can be regarded as more unsafe when compared to the situation without DRL. However, no other studies indicate such an adverse effect, and is not clear to what extent these results could be applied to cars. Howells et al. (1980) also compared speed estimates for motorcycles both with and without headlights on in daylight. They found no significant difference in speed estimates for motorcycles when headlights were on or off; in both situation speeds were underestimated: At 68 km/h, estimates with lights on and off were 64.04 and 63.51 km/h respectively, while at 88.5 km/h, the mean estimates were 74.50 and 74.51 km/h respectively. Also Nagayama et al. (1980) found that speeds of motorcyclist are more underestimated than for cars. From more fundamental research it is known that speed estimation - as distance estimations - are inferred from image size: if the image is growing larger we know the object is coming closer (Olson, 1993). The rate of change of image size should be a cue to the speed of approach. People are not very good at making distance or speed judgments (Boyce, 1981). This is particularly so when the judgment has to be made at a long distance and the approaching vehicle is moving directly towards the driver (change in size and perceived relative motion are small). Likely two DR-lamps on cars is advantageous for speed and distance estimations when compared to one lamp of motorcycles.

Ad a4. Recognition and identification
In addition to DRL as a consistent search feature that will lead to earlier detection or faster locating of a target, it can be assumed that DRL as a consistent identification feature can have influence on the ‘perceptual set’, facilitating identification, and recognition (Theeuwes & Riemersma, 1990). The previously described detection experiments generally required the test subjects to detect one vehicle in an otherwise empty traffic area. In addition, the test subjects always knew what they were supposed to see: a car. The experiments in fact only apply to road users who are alert, look in the right place at the right time, and know which object type they can expect.

In reality, all types of lit and unlit vehicles and road users (and other objects, lit or otherwise) will be found on the road; whether the results of detection experiments are relevant to these situations is not certain. Experiments in which test subjects not only detect road users - not necessarily cars alone - but also *identify* or *recognise* them as pedestrians, cars, bicycles etc. are needed to investigate this issue. The ‘correct recognition’ can then be demonstrated by the correct naming of the object, or from the ‘correct’ (traffic) manoeuvre the test subjects are expected to carry out. Such an experiment could assess whether cars with DRL are also better recognised as such; the lighting can then be regarded as extra coding, and foster a certain expectation of a vehicle ‘participating in traffic’, in contrast to a parked car, for example without lighting). As to date, little attention has been paid to such cognitive processes in studies on DRL. It can be assumed that recognition and identification performance also improves as the luminous intensity of lamps increases. An example of a study that investigated the identification of vehicles is presented below (Brendicke et al., 1994; Donne, 1990; Hole & Tyrrell, 1995 in section b5).

Cobb (1992) describes an experiment concerned with front lights on cars (so-called conspicuity lights to distinguish them from dipped head lights). Members of the public (55 different people, of various ages) drove their own cars round a research track, while making assessments of cars with various front lights fitted. Subjects had to identify the presence or absence of three vehicles (car, motorcycle, bicycle). They were obliged to concentrate also on the driving task, by the fact that other vehicles were on the track in the same time. A range of suitable intensities was sought with a minimum necessary to achieve sufficient conspicuity of the fitted car, and a maximum limit to avoid the resulting glare from masking the view of other road users such as motorcycles and bicycles. It was found that errors in observation occurred only in cloudy conditions, with completely correct assessments being made in both clear (sunny) and raining conditions.

Figure 4. *Rate multipliers for the identification of cars, motorcycles and bicycles for various intensities of car front lights (cd). (From: Cobb, 1992).*

Figure 4 shows a summary of the results of Cobb's study. The rate multiplier shows how the error rate varies with intensity of car lights.
The actual rate is divided by the average rate for that vehicle, meaning that values greater than 1.0 represent conditions where that vehicle is missed more often than average. The overall rates of drivers saying a vehicle was not there when it was actually present were 0.88% (30 out of 3,394) for cars, 7.8% (95 out of 1,222) for motorcycles (without lights), and 5.0% for bicycles (without lights; 70 out of 1,409). From the figure it can be deduced that the identification of cars increases from no lights, through light level A (13 cd lamp) to a maximum at light level B (165 cd), and then staying flat through levels C (1,250 cd) and D (25,000 cd). Motorcycles and bicycles are similar to each other, identification of these vehicle also increases (slightly) up to level B, flat to level C, but falls quite sharply to level D (due

to masking by the car lamps). When the car is ‘absent’ bicycles are identified much more often. Cobb concludes from the results that ‘Daytime Conspicuity Lights’ are needed in cloudy ‘daytime’ conditions and are not needed in clear conditions. The results of this experiment suggest that in such conditions DRL improves identification of cars, without masking bicycles and motorcycles (even improving their identification) when not high intensity lamps are used.

Ad b1. Assessments of (discomfort) glare
Kirkpatrick & Marshall (1989) studied the extent to which headlights (at various light intensities) caused discomfort glare at an average ambient illumination of about 1,900 lux, when observers see the lights of an approaching car in their rear-view mirror. Light intensities of 500, 1,000, 2,000, 4,000, and 7,000 cd were used. The subject had to indicate on a 9-point scale (De Boer scale) how annoying they felt this glare to be.
The results showed that the 2,000 cd lamp was considered by 80% of test subjects to be ‘just admissible’, while lamps of over 2,000 cd were regarded as unacceptable or disturbing; the 1,000 lamp was considered ‘satisfactory’. In a previous experiment, Kirkpatrick et al. (1987) studied discomfort glare via rear view mirrors as well; in this case, the average ambient illumination was about 700 lux. Lamps of 500, 1,000, and 2,000 cd were used, and test persons considered the 1000 lamp to be just admissible. Kirkpatrick & Marshall (1989) suggest that the difference in findings can probably be attributed to the so-called range effect. In the second experiment the light intensities varied from 500 to 7,000 cd, and in the previous experiment from 500 to 2,000 cd. Therefore, subjects could base their assessment on the relative discomfort they encountered, taking into account the range of light intensities to which they were subjected. Also Sivak et al. (1989) pointed out that previous exposure or ‘experience’ (albeit of an entirely different order of magnitude) also plays a role in the discomfort glare experienced. In one experiment, Americans and a group of Germans who had just arrived in the United States were asked to assess headlights on the degree of discomfort glare. The luminous intensity of European low-beam headlights is less than that of the American lights. The results showed that the Germans experienced significantly more discomfort from (American) headlights than did the American test subjects; glare therefore seems to be associated with previous experience.

In past years, the SAE has conducted a broad series of DRL tests (CIE, 1990; SAE, 1990). For example, during a DRL test in Florida observers assessed whether DRL of various light intensities under various ambient illuminations and various visual angles ‘could be seen’, and to what degree. The following scale for DRL was used:
- 0: not noticeable;
- 1: slightly noticeable;
- 2: noticeable;
- 3: very noticeable;
- 4: too bright.

Assessments in category 0 or 4 were regarded as unacceptable. The general conclusions of the test were that at small observation angles, assessors noticed the lamps more rapidly than at more peripheral angles of view. They noted the lamps more readily at short, rather than long distances, while lamps with a luminous intensity of 5,000 cd were considered by many observers to be ‘too bright’ under all test conditions. At ambient illumination of 90,000 lux, the lamps with a luminous intensity of 600 cd (angle of view 8°; distance 152 m) were hardly noticed. The 1,500 cd lamp was more noticeable and that of 5,000 cd even more so and not yet considered ‘too bright’. At a much lower level of ambient illumination (approx. 8,000 lux), the 600 cd lamp was also clearly distinguished. In a similar test conducted in Washington, DC, observers assessed whether lamps of respectively 200, 400, 500, 1,000, 2,000, 2,400, and 7,000 cd were considered to be ‘lighted’ (yes or no) and whether they were regarded as glaring (yes or no) at two levels of ambient illumination: approx. 40,000 and 800 lux. The results showed that all lamps, both during daylight and twilight, were regarded by over 80% of test subjects as being ‘lighted’. With the daylight test (40,000 lux), it was also shown that from about 2,400 cd, lamps were considered to be glaring by over 20% of assessors; during twilight this percentage was already seen with lamps from 1,000 cd.

Summary of the results: a conceptual framework

To date, test results in the field of DRL and visual perception for various types of study have been conducted or reported more or less separately. Such studies relate to the question of when an ‘improvement’ (e.g. in terms of detection) would occur as a result of DRL, or when ‘negative’ side effects (glare) could be anticipated as a result of DRL. Hagenzieker (1990) summarized and integrated the research findings from various types of DRL experiments into one conceptual framework. This conceptual framework, attempts to relate various types of study directly to each other, in order to obtain greater insight into the question of when positive or negative effects can be expected from DRL.

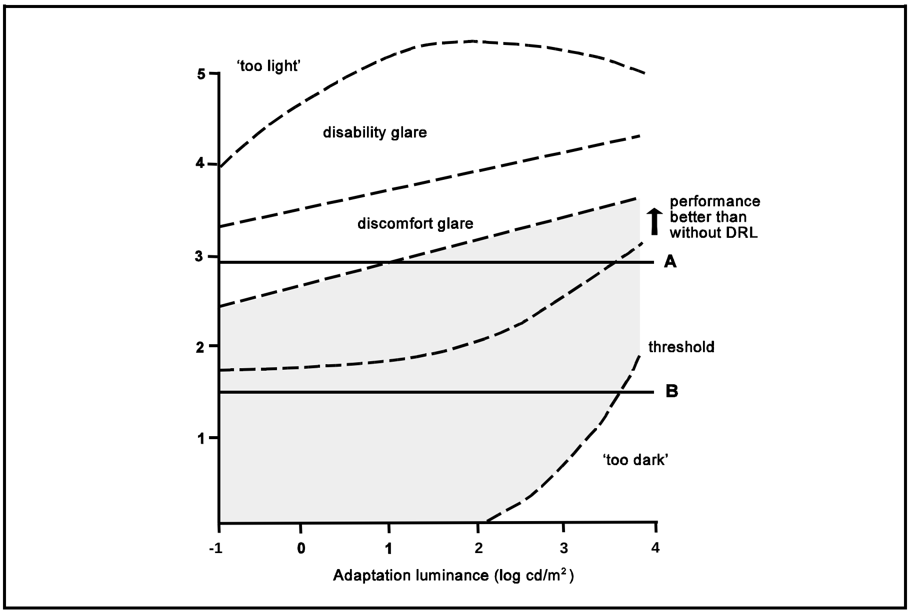

Figure 5. *Schematic representation of the conceptual framework.*

The horizontal axis of *Figure 5* shows the adaptation luminance, which is dependent on the ambient illumination level; the vertical axis shows the luminous intensity of the lamps. The area demarcated by curves in the above left and bottom right hand corner indicates the entire area within which perception (i.e. both detection, recognition etc.) is possible. Stimuli too dim to observe are situated in the area at bottom right; stimuli that are literally blinding, thus making perception impossible, are situated in the area above left. In the area where perception is possible, various sub-categories can be distinguished. The lower area represents the threshold level for the detection of points of light, given certain adaptation luminances; above lies an area where discrimination is possible - allowing recognition and identification - without negative side effects (the shaded area); above that is the area in which ‘good’ perception is still possible, but where a form of discomfort glare becomes apparent; the area above that indicates that disability glare will occur if lamps of this intensity enter the field of view of an observer. Although detection is still quite possible, the ‘details’ are hard to observe due to disability glare.

The horizontal lines in *Figure 5* indicate the luminous intensities of headlights. The graph illustrates that a headlight with luminous intensity A can be glaring at very low levels of adaptation luminance, although within a large intermediate area of adaptation luminance, it falls into the ‘well visible’ area; this headlamp is never found in the ‘too dim’ detection area. A headlight with luminous intensity B is shown not to cause glare under any circumstances, but at relatively high adaptation luminances it falls into the ‘too dim’ category, so that it no longer contributes to visibility.
The lines that show the boundaries above which some form of improvement in visual performance or assessment occurs when compared with the situation without lighting, and the boundary above which discomfort glare occurs are chosen in such a way that they agree with the findings of the experiments on detection and glare as summarized in earlier sections of this chapter. The area between these lines offers a indication of the desirable luminous intensity of vehicle lighting.

Figure 5 shows that the higher the adaptation luminance, the greater the luminous intensity must be to still effect an ‘improvement’ with respect to a situation without lighting, and the greater the luminous intensity can be before any form of glare becomes apparent. It follows that whatever the luminous intensity eventually chosen, it will always be difficult to strike a balance between ‘desirable improvement’ and ‘undesirable glare’. When the results of various DRL experiments on detection and glare are summarized in terms of the conceptual framework, it appears that under daylight conditions (> 100 - 200 cd/m^2) at a luminous intensity of about 1,000 cd, there will virtually never be any form of glare, while an improvement of visual performance can be anticipated. However, during the twilight period (with background luminances between 1 and 100-200 cd/m^2), glare can be experienced, even at a luminous intensity of about 1,000 cd. If, for this reason, a lower luminous intensity is selected, for example 400 cd, this will not offer any ‘improvement’ with respect to the situation without lighting under bright daylight conditions (1000 cd/m^2 or greater).

Padmos (1988) also points out this trade-off between the required luminous intensity on the one hand and the current illumination level on the other. He associates the luminous intensity of the headlights with the percentage of time with daylight (average per year, at average latitudes, e.g. as found in the Netherlands) when the lamp still contributes to the conspicuity - in terms of detection distances - of a car. He concludes that, if from the point of view of limiting glare it is desirable to restrict the luminous intensity to 250, 1,000, or 2,000 cd, the percentage of daytime in which DRL light will enhance conspicuity will be 8%, 46%, or 76%, respectively. The ambient illumination in the daytime (horizontal) on which Padmos (1988) bases his calculations is measured in the ‘open field’, i.e. without ‘obstacles’ or other surrounding background objects being present. For this reason, the percentages quoted at 8%, 46%, and 76% respectively will be on the low, cautious side; a road user generally will not be travelling through the open field, but on roads with constantly varying luminances (generally lower than those measured in the open field).

Other studies
Not all types of study can be summarized in terms of the conceptual framework as depicted in *Figure 5*. This relates to studies where the effect of vehicles using light is studied with regard to the detection of vehicles not using lights. Some studies have also looked into the possible masking effect DRL would have on brake-lights and indicators.

Ad b2. Masking of vehicles without DRL
The masking of vehicles without DRL may occur particularly at twilight periods. Attwood (1977; 1979) examined the extent to which vehicle lighting affects the detection of an oncoming unlit vehicle under dawn/dusk illumination levels. The results showed that if an unlit car must be detected between two cars with low-beam headlights, it would be more difficult to detect than if all cars used the same lighting (or were all unlit). This effect increased as the ambient illumination decreased or the luminous intensity of the lamps increased; the effect was therefore greatest during periods of (low) twilight. This masking effect led Attwood (1981) to recommend a DRL intensity level no greater than 2,000 cd.

Ad b3. The relative conspicuity of bicyclists and pedestrians from DRL
An associated question is whether the introduction of DRL will make slow traffic - such as pedestrians and cyclists - relatively less visible or conspicuous. Riemersma et al. (1987) studied changes in the conspicuity of bicyclists (without lights) in the vicinity of a car using lights.
The conspicuity was measured with a special ‘conspicuity meter’ (Wertheim, 1986; Wertheim & Tenkink, 1987), whereby the conspicuity was determined by establishing to what extent contrast could be reduced, until the object to be measured fell just below the borderline of visibility. In addition, eye movements were recorded, and test subjects underwent a naming experiment in which they had to relate what they saw at various scenes. With each of these three experiments, results showed that the lighting increased the conspicuity of the vehicle, without adversely affecting the conspicuity of the cyclist. These results are in line with those of Cobb (1992; see section Ad a4), which suggest that daytime conspicuity lights improve the identification of cars, without masking bicycles and motorcycles (without lights) when no high intensity lamps are used. Whether lit vehicles can actually cause unlit road users to be less easy to detect or less conspicuous is therefore impossible to conclude on the basis of the available studies, as the results of Cobb (1992) and Riemersma et al. (1987) do not seem to agree with the result of Attwood (1981).

Ad b4. Masking of signal lights such as direction indicators and brake-lights
An argument against the use of low-beam headlights as DRL might be the possible masking of both brake- and turn-signals, if the standard low-beam headlight is used as a running light and consequently also the rear lights are lit during daytime.

Färber et al. (1976; cited in CIE, 1990) and Attwood (1981) did not find any evidence for the masking of rear signal lights such as brake-lights. Based on the available studies, it would be fair to conclude that DRL will not cause a masking of brake-lights or indicators (Rumar, 1981). However, Akerboom et al. (1990) infer from a number of reaction-time experiments into various rear light configurations of cars, that the presence of (burning) rear lights leads to a slower reaction to brake-lights. Since these experiments were conducted in a laboratory setting, not using 'real' lights but 'simulated lights' on a vector-scan display, it is uncertain if generalisations of these results to real-life settings are valid.

Masking of front direction indicators might be a problem with very high (≥ 5,000 cd) DRL intensities (SAE 1990). At longer distances these problems may appear already at a DRL-intensity of 1,000 cd if they are mounted too close to the direction indicators. Kirkpatrick et al. (1987) report a study in which the masking effects of DRL were assessed in relation to indicators. They found that at a luminous intensity of 250 cd for the indicator and a range of 500 to 2,000 cd for DRL, the viewing distance affected the masking effect as did the lamp surface, although no major effect of DRL luminous intensity was found.

Ad b5. Motorcyclists might lose their conspicuity advantage
It is also possible that in countries where DRL for motorcycles is required prior to mandating DRL in passenger cars, motorcyclists might lose not only their relative, but also their absolute conspicuity advantage (see, e.g., Brendicke et al., 1994). It has also been argued, however, that the conspicuity of motorcycles will not be reduced if all vehicles have DRL and that the risk of motorcycle collisions may be reduced by making it easier for motorcyclists to see other vehicles. The CIE committee on DRL (1990) recognizes that motorcycles could be more conspicuous and more easily identified if they were illuminated in a manner distinct to other motor vehicles. One solution could be for motorcycles to use standard low beam plus two daytime running lights, giving a triangular appearance. That would give a distinct pattern and thereby facilitate identification and also improve the possibility to estimate their speed and distance.

An overview of experiments on improving motorcycle conspicuity, the effect of headlights on speed/distance perception of motorcycles, and the danger of being back lit by the sun with lights-on are given by Wulf et al. (1989) and FORS (1990). Some studies on this topic are described below.

Donne (1990) designed a field experiment to assess the relative conspicuity of various headlamps and fluorescent clothing for motorcycles/ motor-cyclists. Subjects, seated in the driving position of a car, were allowed brief glimpses through a shutter of the road ahead and were asked to identify the nearest approaching vehicle. The road sometimes contained no traffic, at other times only vehicles which were incidentally present, and sometimes the test motorcycle displaying one of the conspicuity aids or in the control condition. The experimental measure was based on the frequency with which the motorcycle was seen and *identified* (see also section Ad a4). Three different sites were used, all in urban, single-carriageway roads with traffic travelling in both directions. The subject faced oncoming traffic but the view was obscured by a screen which contained a shutter and a rear-view mirror. Subjects were told that the primary task was to monitor traffic from the rear as viewed in the mirror and to distinguish between private cars and commercial vehicles. These were to be counted. Occasionally they would be interrupted by the sounding of a buzzer which indicated that the shutter was about to open. They should then look through it and report what they could about the nearest approaching vehicle. On only about one in fifteen occasions was this vehicle the test motorcycle. The investigation was based on the premise that drivers sometimes fail to see motorcyclists who are using no conspicuity aids. Hence, shutter opening-times were adjusted for each subject, during a practice session, to set a glimpse time such that he or she sometimes overlooked vehicles (cars and/or motorcycles) visible through the shutter. Glimpse times for the 197 subjects ranged from 50 to 400 milliseconds. Analysis of subjects' responses showed that in the best case detectability could be improved from 53.6% to 64.4% (for a 40w, 180 mm diameter headlamp); the fluorescent jacket resulted in a detectability of 60.0%; a pair of daytime running lamps 62.2%.

Brendicke et al. (1994) investigated the effects of the use of a general daytime running light for motorcycles and cars. Subjects looked at slides showing motorcycle/car traffic situations with or without daytime running lights for cars, counting the number of vehicles (cars, motorcycles, and other vehicles) as well as the number of vehicles using daytime running lights. They also indicated the importance of contour, colour and lighting of all vehicles regarding the perception of these vehicles. The results showed that in some situations the absolute number of detected motorcycles *decreased* when the cars used DRL, namely at intersections outside built-up areas. In these situations the absolute number of detected cars also decreased somewhat when they used DRL. On the other hand, in built-up areas and in situations without intersections outside built-up areas no such decrease in detection was found, but instead a slight increase in detection when motorcycles used DRL. The results of the subjective evaluations of various aspects indicated different rankings for motorcycles and cars. For motorcycles contours and lighting of the vehicle were equally important, followed by colour whereas for cars contours and colours of the vehicle are rated as decisive with 'lighting' ranked third.

The results of an experiment by Hole & Tyrrell (1995) appear to offer some experimental support for speculation by Thomson (1980) that if the majority of motorcyclists used their headlights in daytime, the minority who did not use lights might be placed at greater risk of not being seen. Other motorists might develop a strategy of looking for lights rather than for motorcycles as such. Subjects had to decide as rapidly as possible whether or not a motorcyclist was present in each of a series of slides depicting traffic. The results of a first experiment showed that headlight-using motorcyclists were more quickly detected than unlit motorcyclists, especially when they were far away. However, repeated exposure to headlight-using motorcyclists significantly delayed detection of an *unlit* motorcyclist. A second experiment showed that this delayed-detection effect already occurred when only a majority of the motorcyclists (60%) shown were using their headlight.

Under laboratory conditions, at least, subjects readily appear to develop a 'set' for responding on the basis of headlight-use, even when this is an unreliable guide to the motorcyclists' presence.

With regard to 'back-lighting' the concern is that a headlight may make riders less visible when the sun is behind them as they approach a driver. A field trial by the FORS (Australia) in 1990 showed riders with lights on were easier to see, even when they were back-lit by sun (though extreme back-lighting does pose a hazard, with or without lights). With regard to speed and distance perception it has been claimed that daytime lights (for motorcycles) could lead other road users to make dangerous misjudgment of speed or distance of an approaching motorcycle. This claim is based on a single study which is already described in section Ad a3 (Shew et al., 1977; Prower, 1990). It was found that the speed of a motorcycle with unlit headlights is estimated higher than when headlights are on.

Ad b6. Behavioural adaptation
It has been suggested that drivers may increase their risk-taking, driving at higher speeds, for example, in response to perceived safety benefits of their car and other cars having DRL (Elvik, 1993; Perel, 1991; see also Williams & Lancaster, 1995). To date, no research exists concerning effects of DRL on driver behaviour, and possible behavioural adaptation effects. The reviewed DRL-studies do not indicate that, if a risk compensation occurs, it does not overshadow the positive effects of DRL on risk.

Ad b7. Novelty effect
The so-called novelty effect can be considered as one form of behavioural adaptation. It has been suggested that when DRL is first introduced into some part of the vehicle population, positive effects will be found only because DRL is new and unique, and the vehicles that have them stand out from those that do not. Once people get used to seeing vehicles with DRL, their effects will diminish, and, if all vehicles have them, their noticeability will be reduced or drivers will come to ignore the extra information. Experiments investigating this effect on a perceptual/ behavioural level have not been conducted. It can be argued that DRL improves a basic visual function - peripheral as well as central detection - and that has little to do with how many vehicles have them.

Prower (1990) infers from various experiments by Donne & Fulton (1980; 1985) that a novelty effect might exists. Two experiments, conducted by these authors several years apart - when only a few motorcycles used DRL and some years later when DRL was used more frequently - showed that in the latter experiment a smaller percentage of motorcycles with 'twin DRLs' was recalled by pedestrians, whereas this was not the case in the 'control condition' (without lights). However, this 'evidence' must be viewed with great caution, since the experiments were not set up to investigate the novelty effect and it is difficult to compare them to this end. Moreover, when comparing the results of two other conditions, also involving types of lights on motorcycles, similar results were found.

1.3. **Conclusions**

1.3.1. *General conclusion on positive effects of DRL*

The results of the reviewed experiments generally indicate a 'positive' effect of DRL on 'visual perception', particularly peripheral perception as well as perception under low levels of (daytime) ambient illumination and when not too high intensity lamps are used (to avoid glare effects).

In general, a disadvantage of the studies discussed is that test subjects most of the time knew exactly what to expect; this is not very realistic with respect to traffic situations. Nevertheless, taking results of various studies together into one 'framework', these improvements in performance and visibility should already occur with lamps from 100 cd at low adaptation luminances up to about 50 cd/m^2 (twilight); for higher adaptation luminances, higher luminous intensities are required, e.g. lamps of at least 300 - 400 cd at 1,000 cd/m^2 and at least 2,000 cd at adaptation luminances of about 5,000 - 6,000 cd/m^2. Special DR-lamps with light intensities that are electronic controlled by feedback from the ambient illumination level could meet all these requirements.

Conclusion A1:
Vehicles with DRL are more visible than vehicles without DRL.

With regard to the hypothesis that DRL increases visual contrast between vehicles and their background, and therefore increase conspicuity/ visibility, subjective assessments have shown that, in general - both depending on the level of ambient illumination and DRL intensity - vehicles with DRL are more visible that vehicles without DRL.

Conclusion A2:
DRL result in increased detection distance and angle.

The detection experiments have shown that detection distances are greater for vehicles with DRL when compared to unlit vehicles (under relatively low ambient illumination levels). Moreover, vehicles in the periphery of the visual field are earlier detected with DRL than without.

Conclusion A3:
DRL probably results in some 'safer' judgements.

Regarding the hypothesis that DRL results in more accurate or 'safer' judgments, it has been shown that vehicles with lights on are estimated to be closer than unlit vehicles; and in overtaking situations the minimal gap acceptance is greater when DRL is used than when lighting is not used. The acceptance of a larger gap and estimating a vehicle to be 'closer' can be interpreted as a 'safer' performance with respect to the situation without lighting. However, the results of studies concerned with gap acceptance in situations other than overtaking are less clear-cut. With regard to speed estimation, one available study investigated speed estimation for *motorcycles* with and without DRL. It was found that with headlights off, the speed of a motorcycle was estimated to be higher than when headlights were on, which would imply that DRL on motorcycles may lead to speed judgments than can be regarded as more unsafe as compared to the situation without DRL. However, no other studies indicating such an effect seem to be available, and is not clear to what extent these results could be applied to cars with and without DRL. Probably two lamps of DRL on cars is advantageous for speed- and distance-estimations as compared to one lamp of motorcycles.

Conclusion A4:
DRL probably improves identification of cars.

The effect of DRL on the recognition or identification of vehicles has hardly been studied; remarkably little is known about its effect on (other) cognitive processes, decision-making and (traffic) behaviour under dynamic conditions. It is recommended that these aspects be the subject of future study. The results of a study that explicitly looked at 'identification performance' suggest the following: particularly in cloudy weather - in a variety of 'daytime' conditions (ambient illumination levels), lighting improves identification of cars, without masking bicycles and motorcycles when not too high intensity lamps are used.

1.3.2. *General conclusion on adverse side-effects of DRL*

The results indicate that the detection of cars and motorcycles without DRL may relatively decrease somewhat when they are surrounded by motor vehicles with DRL. Brake-lights and indicators are not masked by DRL. DRL can cause glare when the light intensity is relatively high and the ambient illumination relatively low. No research concerning effects of DRL on possible behavioural risk adaptation effects and on a possible novelty effect is available.

Conclusion B1:
DRL can cause glare, depending on DRL-intensity (relatively high) and luminance level (relatively low).

There is a grey area between the wish to avoid signs of glare on the one hand and the wish to improve visual performance on the other. For example, a lamp of 1000 cd could result in symptoms of (discomfort) glare at an adaptation luminance below about 50 - 100 cd/m^2 (similar to twilight conditions). If there is question of glare due to DRL, this will be of particular relevance during twilight hours (also depending on the luminous intensity selected, of course). This applies especially when special DRL-lamps (i.e. no low-beam head-lights) are used; when lowbeam headlights are used as DRL the glare problem during twilight conditions is irrelevant in the way that such glare is not a specific 'DRL-problem' then.

Conclusion B2:
A car without DRL may be masked by surrounding cars with DRL.

Conclusion B3:
The conspicuity of bicyclists (and probably motorcyclists) next to a car with DRL is not reduced when not too high intensity lamps are used.

The results available with regard to the question of whether lighted vehicles hamper perception of unlit road users have led to conflicting findings for different vehicle/road user-types. On the one hand, it has been shown that an unlit car between two cars with DRL is more difficult to detect than if all cars used the same lighting (or were all unlit) - especially as the ambient illumination decreased or the luminous intensity of the lamps increased. On the other hand, other experiments have indicated that DRL on cars increased the conspicuity of that vehicle without adversely affecting the conspicuity of cyclists and (unlit) motorcyclists.

Conclusion B4:
DRL will not mask brake-lights or indicators.

Based on the available studies, it can be concluded that DRL will not cause the masking of brake-lights or indicators (although one - simulation - study has suggested that the presence of (burning) rear lights will lead to a slower reaction to brake-lights). Masking of front direction indicators might be a problem with very high ($\geq$ 5000 cd) DRL intensities.

Conclusion B5:
The existing conspicuity of motorcycles using DRL already, might be relatively somewhat reduced by a full use of DRL on all motor vehicles.

Experiments on motorcycle conspicuity suggest that DRL for motorcycles increases their detectability as compared to motorcycles without DRL. However, other conspicuity aids such as fluorescent jackets appear to result in similar improvements in detectability. Other studies showed that when a majority of motorcycles use DRL, motorcycles without DRL are detected later, and that in some complex situations motorcycle detection decreases when *cars* use DRL (these findings are in line with conclusion B2). The latter result offers some support for the hypothesis that in countries where DRL for motorcycles were required prior to mandating DRL in passenger cars, the conspicuity motorcyclists might be relatively reduced. One solution to this problem as suggested by the CIE committee on DRL, is that motorcycles could be more conspicuous and more easily identified if they were illuminated in a manner distinct to other motor vehicles, for example to use standard low beam plus twin DRLs, giving a triangular appearance. It also is possible to use a special colour for the motorcycle DRL. According to the CIE (1990), however, this does not seem to be an appealing solution considering that colour of light does not substantially improve conspicuity or accuracy of distance and speed judgment and that colour is already used for other purposes.

Conclusion B6/7:
No research is available concerning effects of DRL on possible behavioural risk adaptation effects and on possible novelty effects.

It has been suggested that drivers may increase their risk-taking, driving at higher speeds, for example, in response to perceived safety benefits of their car and other cars having DRL (behavioural adaptation). To date, no research exists concerning effects of DRL on driver behaviour, and possible behavioural adaptation effects. The so-called novelty effect can be considered as one form of behavioural adaptation. It has been argued that once people get used to seeing vehicles with DRL, their effects will diminish, and, if all vehicles have them, their noticeability will be reduced or drivers will come to ignore the extra information. Experiments investigating this effect on a perceptual/behavioural level have not been conducted.

These findings of the visual perception and behavioural effects of DRL are partially used in the next chapter, where hypothesis on the reduction of multiple daytime accidents for different types of accidents, due to an increased level of DRL-usage of motor vehicles, are formulated.

2. Comparability of DRL-effects

Daytime running lights clearly only can affect the prevention of actual accidents in encounters between road users during daylight. The evaluation of observed DRL-effects, therefore, concerns the reduction of multiple daytime accidents (MD-accidents). The main hypothesis, supported by the perceptual implications of DRL, is that an increase in DRL-usage results in a decrease of MD-accidents. By an analysis of all empirical evidence one, therefore, expects to reject the next basic hypothesis:

Basic Hypothesis 1:
A marked increase in DRL-usage causes no decrease of multiple daytime accidents.

Apart from methodological and statistical problems in the DRL-evaluation studies, addressed in chapter 3, there also are questions about the comparability of empirical DRL-effects in different studies with respect to:

1. different types of studies;
2. different changes in level of DRL-usage;
3. daytime accidents between multiple motor vehicles and between motor vehicle and non-motorised road users;
4. accident data under different ambient illumination conditions related to DRL-usage and DRL-effects;
5. different types of accidents, including crossing, frontal and rear-end accidents, severity types and motorcycle accidents.

In this chapter the influences on the expected safety effect of DRL for these differences are investigated. On the basis of that investigation and the perceptual and behavioural effects of DRL discussed in chapter 1, eight auxiliary conjectures, six corollaries and three additional basic hypothesis are formulated and discussed here and, where possible, tested in chapter 4 and 5.

The eight auxiliary conjectures on the equivalence or difference of DRL-effects on (subgroups of) MD-accidents with respect to the above mentioned differences are formulated here and investigated in chapter 4, which may contribute to a convincing interpretation of a genuine DRL-effect.

The six corollaries are formulated with respect to different levels of DRL-usage in order to specify corrections for the estimated DRL-effects (on MD-accidents from studies with differently changed levels of DRL-usage) in order to obtain comparable intrinsic DRL-effects (proportional DRL-effect on MD-accidents from a change of 0% to 100% DRL-usage).

The second basic hypothesis on the equivalence of intrinsic DRL-effects under equal average levels of ambient illumination is crucial for a valid comparison of the independent study results.

The third basic hypothesis concerns the dependence of intrinsic DRL-effects on the average level of ambient illumination and is tested in chapter 5 for the DRL-results of all studies.

The fourth basic hypothesis is crucial for the validity of the statistical analysis of the data in each independent study, reviewed in chapter 4.

2.1. Fleet-owner versus national DRL-studies

The oldest evaluations of a DRL-effect are studies on the effect of DRL-usage in large fleets of companies from the USA and Canada in the mid sixties. Such studies have been performed now and then since and most recently for four different large fleets in Austria. The first study on the DRL-effect of a national increase in the level of DRL-usage is from Finland in the mid seventies, followed in the eighties by Sweden and Norway. Recently the effect of a DRL-vehicle standard for new cars in Canada and of the behavioural DRL-obligations in Hungary (rural roads and motorways) are studied, while the effect of DRL in Israel (only wintertime) also is reported in this report (references are specified in chapter 4, where an annotated review of the DRL-studies and the re-analyses of data are presented).

Fleet-owner studies concern the reduction of MD-accidents due to introduction of DRL for motor vehicles of the fleet. These accidents are accidents between one motor vehicle with DRL (of the fleet) and a motor vehicle without DRL or a non-motorised road user who can not have DRL. It concerns a change in DRL-usage from 0% to approximately 100% for motor vehicles of the fleet. The proportional reduction of MD-accidents from a change of 0% to 100% DRL-usage is called an intrinsic DRL-effect. If the DRL-usage in the country of the fleet is virtually zero the proportional DRL-effect on MD-accidents, estimated from observed accident data in a fleet owner study, is an intrinsic DRL-effect.

In national DRL-studies, the proportional DRL-effects on MD-accidents, estimated from observed accident data differ from intrinsic DRL-effects for two related reasons. Firstly, because in national DRL-studies a change in DRL-usage never is from 0% to 100% and secondly, because of the first reason, the raw DRL-effects do not concern only encounters with one DRL-using motor vehicle. The proportional DRL-effect on MD-accidents, estimated from observed accident data in studies where the increase of DRL-usage is not from 0% to 100%, is denoted as a raw DRL-effect. If raw DRL-effects are corrected for a DRL-usage from 0% to 100% they become comparable intrinsic DRL-effects.

Apart from this difference between fleet owner and national DRL-studies, which is further analysed in the next paragraph, it may be questioned whether the possible safety effects in fleet-owner studies is only due to the introduction of DRL. It may be argued that fleet owners, that have introduced DRL, do have a before-period of relative high accidents. Moreover, the company may also influence the safe driving of its drivers in other ways and when the comparison is made with an other non-DRL using fleet, it also may be that the safety awareness in that company is different. On the one hand the DRL-effects from fleet-owner studies, therefore, may be biased by an overestimation of the actual DRL-effect. On the other hand, if a company that introduces DRL in its fleet has already a relatively high safety awareness among its drivers before the introduction of DRL, it may mean that the DRL-effect that can be expected is biased by an underestimation of the actual DRL-effect that a national DRL-obligation would have. A meta-analysis of different DRL-studies Elvik (1996) concludes that no bias is present in the DRL-effect of fleet-owner studies compared to national studies.

For the comparison of DRL-effects between fleet-owner and national studies it is assumed that:

Auxiliary Conjecture 1:
No bias of DRL-effects in fleet-owner studies is present.

In chapter 5 this is tested by the comparative analysis of DRL-effects.

2.2. DRL-effects from partial increases of DRL-usage

As stated, in national studies the DRL-usage does not change from 0% to 100%. The proportion of DRL-using motor vehicles before the national DRL-obligation is introduced (or before a change in DRL-level from a national information campaign for the promotion of DRL) varies between nations. It is called the *before DRL-level* and denoted by **ß**. The proportion of DRL-using motor vehicles after the intervention that increased the level of DRL-usage is called the *after DRL-level*, denoted by α. Here one must distinguish between five combinations of DRL using and non-DRL using road users in MD-accidents with possible different levels of DRL-usage for each period before and after the DRL-change (denoted as before period and after period). The proportions of DRL-usage for the respective combinations in MD-accidents are given in *Table 1*.

Combinations	Before	After
Two DRL motor vehicles	ß x ß	α x α
One DRL motor vehicle X Non-DRL motor vehicle	2 x (1-ß) x ß	2 x (1-α) x α
Non-DRL motor vehicle X Non-DRL motor vehicle	(1-ß) x (1-ß)	(1-α) x (1-α)
One DRL motor vehicle X Non-motorised road user	ß	α
Non-DRL motor vehicle X Non-motorised road user	1-ß	1-α

Table 1. *Proportions of non-DRL and DRL-users in MD-accidents*.

Table 1 illustrates that different increases from different before DRL-levels mean that the same intrinsic DRL-effect (proportional reduction effect on MD-accidents from 0% to 100% DRL-usage) will result in different raw DRL-effects on MD-accidents, estimated from the observed accident data.

In order to be able to compare the DRL-effects of:

- fleet owner studies with DRL-effects of national studies;
- different national studies with each other;
- MD-accidents between motor vehicles with the DRL-effects of pedestrians and cyclists,

one has to correct the raw DRL-effects to intrinsic DRL-effects. The raw DRL-effect is the estimated proportional reduction effect of DRL on MD-accidents, where that estimated proportional reduction relates an expected number of MD-accidents that would have been observed without a change in DRL-usage to the actual observed MD-accidents in the period after a partial increase of DRL-usage. In DRL-studies one derives that expected number of MD-accidents by some statistical analysis from the observed MD-accidents in the before-period and generally also from other accident data in the before- as well as after-period (the latter non-MD-accidents are assumed not to be influenced by DRL). The intrinsic DRL-effect corrects the estimated proportional reduction on MD-accidents (due to the increase of DRL-usage from before level ß to after level α) to an expected proportional reduction for a changed DRL-usage from 0% to 100%.

Intrinsic DRL-effects for pedestrians and cyclists

Equation 1 shows that correction from a raw DRL-effect to an intrinsic DRL-effect must be different for non-motorised road users and drivers of motor vehicles. If this raw DRL-effect for MD-accident of non-motorised road users is denoted by $\mathbf{E_1}$ then their intrinsic DRL-effect for non-motorised road users, denoted by I_1, obtained from a DRL-usage increase with a before DRL-level ß and an after DRL-level α follows by the next expression of *intrinsic DRL-effects on MD-accidents of motor vehicles with pedestrians and cyclists*:

Intrinsic DRL-effect $\mathbf{I_1 = E_1 / [\alpha - ß \cdot (1 - E_1)]}$ (1)

Since this intrinsic DRL-effect also is the probability that an individual cyclist or pedestrian avoids an accident in case there is an encounter with a DRL-using motor vehicle, this expression can be derived from:

- the number of MD-accidents without any DRL-usage, denoted as MD,
- the number of MD-accidents in the before period, denoted as MD_b,
- the before DRL-level **ß**,

where MD_b writes as:

$$MD_b = MD \cdot (1 - I_1 \cdot ß)$$

as well as the corresponding expression for the number of MD-accidents in the after period with after DRL-level α, denoted by MD_a, written as:

$$MD_a = MD \cdot (1 - I_1 \cdot \alpha)$$

and the definition of the raw DRL-effect as:

$$E_1 = 1 - MD_a/MD_b$$

Often one finds in national DRL-studies no correction for raw DRL-effects to intrinsic DRL-effects, although comparisons with other studies are sometimes made. Without corrections to intrinsic DRL-effects no valid comparison is possible, but where a correction is made in these studies the correction is solely based on expression (1). This is incorrect, since most MD-accidents occur between motor vehicles for which expression (1) does not hold. Therefore, the following corollary is explicitly formulated.

Corollary 1:
Only the intrinsic DRL-effect on MD-accidents between motor vehicles and non-motorised road users is obtained by expression (1).

Intrinsic DRL-effect for motor-vehicles
For DRL-effects on MD-accidents between motor-vehicle drivers there is no simple transformation of the raw DRL-effect into the intrinsic DRL-effect, unless some conjecture is made. The conjecture concerns a difference or equivalence of intrinsic DRL-effects on MD-accidents between motor vehicles with only one DRL-using motor vehicle and between two DRL-using motor vehicles. The equivalence conjecture assumes that preventive effect on MD-accidents due to DRL in encounters of two DRL-using motor vehicles is equal to that preventive effect in encounters of two motor vehicles with only one DRL-user. This simplifying conjecture is plausible, because only one of the drivers needs to avoid an accident by the perception of DRL in encounters between two motor vehicles irrespective of one or two DRL-using motor vehicles. One then assumes that accident avoidance behaviour of both drivers in encounters of two DRL-using motor vehicles does not contribute more to the avoidance of an accident than the accident avoidance behaviour of one of the drivers in such encounters. The possible DRL-effect on effective accident avoidance in case of encounters between two DRL-using motor vehicles then will be equal to the DRL-effect on effective accident avoidance in case of encounters between motor vehicles with only one DRL-user (in both cases with respect to avoidance behaviours without any DRL-usage).

So it is conjectured that the intrinsic DRL-effect from the first and second combination in Equation 1 are equal, whereby the i*ntrinsic DRL-effect on MD-accidents between motor-vehicles* becomes as follows:

Intrinsic DRL-effect $\mathbf{I_2 = E_2/[(2 \cdot \alpha - \alpha^2) - (2 \cdot ß - ß^2) \cdot (1 - E_2)]}$ (2)

Here this raw DRL-effect is denoted by $\mathbf{E_2}$ and its intrinsic DRL-effect by $\mathbf{I_2}$, that here also is the probability that one of the drivers avoids an accident in case the other uses DRL or both the other and the driver use DRL. Therefore one can derive this expression from the number of MD-accidents without any DRL-usage, denoted as MD, the before DRL-level **ß** and the here relevant number of MD-accidents in the before-period, denoted as MD_b, that then writes under equivalence assumptions as:

$$MD_b = MD \cdot [1 - I_2 \cdot 2(1 - ß) \cdot ß - I_2 \cdot ß^2]$$

as well as the corresponding expression for the number of MD-accidents in the after-period, denoted by MD_a, that then is written as:

$$MD_a = MD \cdot [1 - I_2 \cdot 2(1 - \alpha) \cdot \alpha - I_2 \cdot \alpha^2]$$

and the definition of its raw DRL-effect as:

$$E_2 = 1 - MD_a/MD_b$$

The alternative conjecture is that the chance of an effective accident avoidance for drivers in encounters of two DRL-users will be higher compared to that chance for drivers in multiple motor-vehicle encounters with only one DRL-using motor vehicle. If one assumes independence of driver behaviour, while the chance of an additional effective accident avoidance by one DRL-using driver in encounters where that driver perceives the DRL of the other still is the intrinsic DRL-effect I_2, then the chance that the accident is avoided in case of two DRL-using motor vehicles becomes:

$$2 \cdot (1 - I_2) \cdot I_2 + I_2 \cdot I_2 = I_2 + I_2 \cdot (1 - I_2)$$

However, the assumption of independence of driver behaviour seems not justified. For example: if one driver stops in time at a crossing or does not start an overtake manoeuvre due to the DRL of the second driver, that second driver generally will continue without danger. But, if the first driver had not acted earlier, the second would have reacted with the nearly the same probability of effective accident avoidance on the DRL of the first driver. It means that only a fraction of the additional effectiveness of accident avoidance by action of both drivers can be assumed. That fraction is unknown, but will be very close to zero due to the behavioural dependence between parties in a traffic encounter, otherwise there would not be so seldom an accident as outcome of encounters wherein only one road user is reacting.

Denoting that fraction by p one has to change by this reasoning the above expressions for MD_b and MD_a as follows:

$$MD_b = MD \cdot [1 - I_2 \cdot (2 \cdot ß - ß^2 + p \cdot (1 - I_2).ß^2)]$$

$$MD_a = MD \cdot [1 - I_2 \cdot (2 \cdot \alpha - \alpha^2 + p \cdot (1 - I_2).\alpha^2)]$$

The dominator of the expression for the corresponding intrinsic DRL-effect would contain, additional to the one in expression (2), the term:

$$p \cdot (1 - I_2) \cdot [-\alpha^2 + (1 - E_2) \cdot ß^2]$$

Even is p is so extremely high that the proportion in accident avoidance of both drivers is the square of the proportion of each separate driver (thus $[2\sqrt{p}+p].I_2=I_2$) then $p = 0.1716.I_2$ and always $p \cdot (1 - I_2) < 0.044$. Because in the studies always $\alpha^2 > .95$, $ß^2 < .25$ and $(1 - E_2) < 1$, it follows from these inequalities that $[-\alpha^2 + (1 - E_2) \cdot ß^2] < -.70$. So the dominator of expression (2) never becomes less then .031 smaller. Taking the same minima and maximum in expression (2) it self, it follows that expression (2) one only can underestimate the intrinsic DRL-effect by a factor > .88 of the actual effect. Since p will be much smaller, a different assumption than the equivalence assumption for expression (2) only can lead to a marginally higher intrinsic DRL-effect. It demonstrates that the intrinsic DRL-effect derived by expression is virtually equal to the unknown exact intrinsic DRL-effect or underestimates the exact effect by a negligible amount. Thus the following conjecture and corollary are formulated:

Auxiliary Conjecture 2:
The probabilities of accident avoidance due to DRL in encounters between motor vehicles with only one and with two DRL-using motor vehicle(s) can be taken to be equal.

Corollary 2:
If conjecture 2 is incorrect the exact intrinsic DRL-effect on MD-accidents between motor vehicles can not be derived, but that exact effect can only be virtually equal to (negligible higher than) the intrinsic DRL-effect obtained by expression (2). Therefore the intrinsic DRL-effect on MD-accidents between motor vehicles ought to be obtained by expression (2).

It will be noted that if the level of DRL-usage **ß** in a country where a fleet-owner study is performed is non-zero, then the raw DRL-effect is not the same as the intrinsic DRL-effect. Also where the national study concerns the DRL-effect on the MD-accidents of the motor vehicles that are equipped with a compulsory DRL vehicle standard from a particular year onward, while there is an otherwise existing non-zero level of voluntary DRL-usage, as in the national DRL-evaluation for Canada, the comparison is between before and after fleets and the intrinsic DRL-effect not correctly obtained by expression (2). In both cases there also may be a before level of DRL-usage $ß_f > 0$ for the fleet and there is a α_f proportion of fleet DRL-usage in the after period wherein the non-fleet DRL-usage remain the same level $ß > 0$. In both cases, according to conjecture 2, the relevant change is the proportional increase of encounters between motor vehicles with two or one DRL-user, thus from proportion $1 - (1-ß)(1-ß_f)$ to 1 or $1 - (1-ß)(1-\alpha_f)$. This leads to the *intrinsic DRL-effect on MD-accidents between motor vehicles for motor vehicles of fleets with DRL and an otherwise existing non-zero level of DRL-usage* as:

Intrinsic DRL-effect I_f for a fleet than becomes:

$$\mathbf{I_f = E_f/[(ß + \alpha_f - ß \cdot \alpha_f - (ß + ß_f - ß \cdot ß_f) \cdot (1 - E_f)]} \qquad (3)$$

where the raw DRL-effect for the DRL-equipped fleet-owner motor vehicles or for the registered motor vehicles with a DRL vehicle standard is denoted by $\mathbf{E_f}$, its intrinsic DRL-effect by $\mathbf{I_f}$ and the before level of DRL-usage in the fleet $ß_f$ otherwise existing level of DRL-usage by **ß**. If $ß_f = 0$ and $\alpha_f = 1$ it reduces to the same expression as expression (1) and incase $ß = ß_f$ and $\alpha_f = 1$ it is the same as expression (2) for $\alpha = 1$, because in the after period it then only concerns encounters between motor vehicles involving fleet vehicles with DRL. This is formulated as a corollary by:

Corollary 3:
The intrinsic DRL-effect on MD-accidents between motor vehicles involving motor vehicles of fleets that become equipped with DRL, where there is already an existing non-zero level of DRL-usage, is obtained by expression (3).

2.3. Motor vehicles and non-motorised road users in multiple daytime accidents

Apart from different DRL-levels in the before- and after-periods in different studies, it must be considered that the intrinsic DRL-effects for non-motorised road user may be different from intrinsic DRL-effects for motor vehicle drivers.

Equivalences or differences of intrinsic DRL-effects
Koornstra (1993) has argued that the DRL-effect on MD-accidents with non-motorised road users may be larger, because of their lower speed and higher manoeuvrability compared to motor vehicles. Especially in built-up areas, where sight distances can be rather short, the speed of motor vehicles may be too high and their manoeuvre space too small for an avoidance of a multiple motor-vehicle accident. This even if DRL caused an otherwise non-occurred avoidance reaction. Pedestrians and cyclist can stop or quickly change direction in short distances, once a motor vehicle is detected late. On the one hand, therefore, an effective avoidance of DRL-using motor vehicles by non-motorised road users can be assumed to occur more often than by motor-vehicle drivers. On the other hand, unions for cyclists and pedestrians (SWOV, 1991) have argued that DRL-using drivers may adapt to the increased accident avoiding behaviour of non-motorised road users. This may lead to more careless behaviour of DRL-using drivers with regard to cyclists and pedestrians and thus to more MD-accidents with non-motorised road users due to the DRL-usage.

The opposite effects of these two arguments may cancel each other out and the correctness of both arguments together may never be resolved. Moreover, the arguments for conjecture 2 (intrinsic DRL-effects on encounters of two DRL-using road users are virtually equal to the DRL-effects on encounters of one DRL-using road user and one without DRL) and the assumption that capabilities for accident avoidance behaviour of motorised and non-motorised road users are equal, also leads to the conjecture that intrinsic DRL-effects for pedestrians and cyclists are virtually equal to intrinsic DRL-effects for motor vehicle drivers. Because the DRL-effect on MD-accidents with non-motorised road users is controversial and may be different from the DRL-effect on multiple motor vehicle accidents if only one of the two above mentioned arguments is true, one should separate these two categories of MD-accidents in DRL-evaluations. Such will be done in the annotated review of the DRL-studies in chapter 4 in case separate data are available in these studies. If available, there also the following conjectures will be tested:

Auxiliary Conjecture 3:
The intrinsic DRL-effect on MD-accidents of motor vehicles with non-motorised road users is virtually equal to the intrinsic DRL-effect on MD-accidents between motor vehicles.

Clearly conjecture 3 can only hold if the respective raw DRL-effects from national DRL-studies are different, due to corollaries 1 and 2. If separate data on multiple daytime motor-vehicle accidents and MD-accidents with non-motorised road users are available for a rather long period after the change in the level of DRL-usage, it is possible to test the hypothesis of a risk compensation by DRL-users as a diminishing DRL-effect over time for all road users or only for non-motorised road users.

Auxiliary Conjecture 4a:
The intrinsic DRL-effect on MD-accidents is not diminishing in the long run after the introduction of a DRL-obligation.

Auxiliary Conjecture 4b:
The intrinsic DRL-effect on MD-accidents with non-motorised road users is not diminishing in the long term after the change in the level of DRL-usage.

Auxiliary conjectures 2, 3 and 4, given also that conjecture 1 holds, define that the intrinsic DRL-effects $\mathbf{I}_1$, $\mathbf{I}_2$ and $\mathbf{I}_3$ for a country (all intrinsic DRL-effects on MD-accidents of fleets and of motor vehicles or non-motorised road users) are equal. Since an improved perception by DRL depends on the light and contrast conditions during daylight it is only assumed that intrinsic DRL-effects on MD-accidents in different countries are equal if their average levels of ambient illumination are equal. This leads to the second basic hypothesis that includes the auxiliary conjectures 1, 2 and 3:

Basic Hypothesis 2:
The intrinsic DRL-effect from different fleet studies and different national studies (irrespective of different shares of MD-accidents between motor vehicles and MD-accidents with pedestrians and cyclists) are equal, provided that the average level of ambient illumination in daytime of the countries are equal.

If this hypothesis is rejected, then the national safety effect of DRL will depend on the respective shares of these accidents in the national total accidents, which for example in the USA compared to Denmark and the Netherlands are quite different. Also the DRL-effects from fleet-owner studies would not predict national DRL-effects for nations with relatively high shares of MD-accidents with pedestrian and cyclists.

Level of before DRL-usage inside and outside built-up areas
A study on the level of voluntary DRL-usage of the officially propagated use of DRL in the Netherlands (Lindeyer & Bijleveld, 1991) has revealed that the level of DRL-usage in built-up areas is lower than on rural roads and much lower than on motorways. For the average level of voluntary DRL-usage in the Netherlands of about 30% the usage in built-up areas is 22% and for rural roads and motorfreeways 31% and 37%, but varying with varying daylight conditions. In the DRL-study of Andersson and Nilsson (1981) for Sweden, similar variations of DRL-usage in the before period are also reported with less difference between inside and outside built-up areas, but based on a small sample of observation locations. Also Hocherman & Hakkert (1991) report in their study on a DRL-campaign in Israel that DRL-usage rates in urban areas were much lower than in rural areas, in similar weather and ambient illumination conditions.

Encounters between motor vehicles and non-motorised road users mainly occur in built-up areas, while their accidents are about 70% of the total in built-up areas in the Netherlands as well as in Sweden. The level of DRL-usage in encounters with non-motorised road users thus can be estimated to be approximately a factor 0.85 lower than the average level of voluntary DRL-usage. Since this more or less applies to all levels of voluntary DRL-usage in before-periods of countries where an obligation of DRL has been introduced, one has to correct the average before-level of DRL-usage in the intrinsic DRL-effect expression for MD-accidents with non-motorised road users. If not one may overestimate the that particular intrinsic DRL-effect from national studies. The following corollary formulates this explicitly:

Corollary 4:
Level of DRL-usage in built-up areas for countries that have introduced a DRL-obligation have been lower in the before-period than the average before level by a factor of 0.85 of the voluntary overall DRL-usage in the before-period.

If separate data for accidents of motor vehicles with pedestrians and cyclists are available in the national DRL-studies, reviewed in chapter 4, then a revised estimation of the intrinsic DRL-effect for pedestrians and cyclists in the relevant countries, based on their larger increase in the level of DRL-usage derived from corollary 4, also will be performed in order to avoid an overestimation of their intrinsic DRL-effect I_1.

2.4. Influence of ambient illumination on DRL-usage and -effects

DRL-usage and twilight
In twilight periods of dawn and dusk the voluntary level of light-using vehicles is the higher the more the twilight condition approach the night time. The proportion of light using motor vehicles in twilight may be increased by an increased level of DRL-usage during daytime. The effect of a reduced proportion of vehicles without burning lights in twilight even can have relatively large effects, because the needed contrast for the perception of motor vehicles in twilight conditions is markedly improved by burning lights. However, generally the proportion of vehicles without lights in twilight before the introduction of DRL is not known and also will be changing during dawn and dusk. Therefore, twilight accidents are better excluded from the estimation of DRL-effects on the reduction of multiple accidents. It only results in an underestimation of the DRL-effect.

DRL-usage in summer and winter
It is also known from the already referred research in the Netherlands that the level of voluntary DRL-usage (Lindeyer & Bijleveld, 1991) is markedly higher during daytime in low ambient illumination conditions (winter 40%, clouds, rain or snow up to 80%) than in high ambient illumination conditions (summer 20%, bright sunshine down to 12%). Similar differences are reported for the before-period in Sweden (Andersson & Nilsson, 1981), where in the summer the before DRL-level was about 25% to 35% and in the winter about 65% to 75%. Hocherman and Hakkert (1991) report that DRL-usage, after a winter DRL-campaign in Israel for the usage of DRL in impaired visibility conditions, ranged from an average 4% in clear and 20% in heavily clouded weather to 45%-65% in rain or fog. It is estimated that the average levels of DRL-usage over the year are decreased in the summer and increased in the winter half year by approximate factors of 0.7 and 1.35. One may assume that a similar difference for the before DRL-level in winter and summer holds for countries that have introduced a DRL-obligation, indicating that a correction of a raw DRL-effect by an increase of DRL-usage from the same average before DRL-level may be misleading. The raw DRL-effects in winter and summer will be quite different for equal intrinsic DRL-effects, due to different changes in DRL-usage. Therefore, the next corollary is formulated:

Corollary 5:
The before DRL-level in countries that have introduced a DRL-obligation have been lower in summer time and higher in winter time than the average before DRL-level by a factor of 0.7 and 1.35 of the average level of voluntary DRL-usage in the before period.

If the average before DRL-level is already rather high in a country, one might even expect from corollary 5 that a raw DRL-effect of a DRL-obligation on MD-accidents between motor vehicles in the winter hardly can be significant. By formula (2) the percentage of daytime encounters between motor vehicles where one or both are using DRL in winter before-periods then will be already close to the after DRL-level (e.g. Sweden: before $2 \cdot 0.75 - 0.75^2 = .9375$ and after $2 \cdot 0.95 - 0.95^2 = 0.9975$ thus an increase of only 6%). Without a sufficient change in DRL-usage in the winter, no significant raw DRL-effect for motor vehicles from a DRL-obligation can be found for the winter. Therefore, if separate accident data for the summer and the winter are available in the national DRL-studies which, as reviewed in chapter 4, do have sometimes relatively high average before DRL-levels then a revised estimation of the intrinsic DRL-effect from the DRL-obligation for these countries, based on the accidents in the summer and the winter separately and the larger DRL-level increase in the summer than in the winter as derived from corollary 5, will be performed. Moreover, combining corollaries 4 and 5, if separate accident data for MD-accidents with non-motorised road users for the summer also are available in these DRL-studies, then also a revised estimation of the intrinsic DRL-effect for pedestrians and cyclists of these countries, based on their summer accidents and a twice corrected before DRL-level in the summer derived from corollaries 5 and 6, will be performed. Clearly from the implied higher partial increases of the relevant DRL-usage in the summer one may expect that the respectively obtained raw DRL-effects will be higher and more significant in the summer than for the respective MD-accidents in the winter or winter and summer, but the respectively revised estimations of the intrinsic DRL-effects may be lower for the summer than for the winter with a less average visibility.

Intrinsic DRL-effects in summer and winter
Because a DRL-effect is in chapter 1 largely attributed to increased contrast for an enhanced focal and peripheral in-time perception of motor vehicles with DRL (and the cognitive and reactive implications for accident avoidance in conflicting manoeuvres), it also must be acknowledged that there may be a higher intrinsic DRL-effect for countries with a lower average level of ambient illumination (Nordic weather and daylight). The higher the latitude of the traffic, the lower the average level of ambient illumination for the traffic will be, due to a lower average angle of the sunlight at higher latitudes. Therefore the next hypothesis is to be rejected:

Basic Hypothesis 3:
The intrinsic DRL-effects on MD-accidents and the average angle of sunlight for daytime traffic in a country are not correlated or mean latitudes of national traffic (summer -11½ and winter + 11½ degrees) are not positively correlated with national DRL-effects.

This hypothesis will be tested in the meta-analysis of the reviewed DRL-results from different countries in chapter 5. If a relation between DRL-effect and the average annual angle of sunlight can be firmly established, then one also has a method for the prediction of the DRL-effect from a DRL-obligation in countries that not yet have such an obligation. Summer and winter half years correspond to ambient illumination levels of latitudes that are 11.5 degrees higher or lower than the average latitude for the traffic, because above the northern tropic the average angle of the sunlight is 11.5 degrees lower in the winter and in the summer 11.5 degrees higher, resulting additionally in different probabilities of cloudy weather conditions. It follows that the intrinsic DRL-effects in summer half years are lower than in winter half years in country. Consequently, the next conjecture is to be rejected by the empirical data.

Auxiliary Conjecture 5:
The intrinsic DRL-effect on MD-accidents in the summer of a country is not lower than this effect is in the winter of that country.

2.5. DRL-effects on different accident types

Single and night accidents
It is assumed that DRL has no influence on night and single accidents, but Hauer (1995) has hypothesised that single daytime accidents (SD-accidents) may increase by DRL, because more evasive actions in daytime conflicts due to DRL may result in more SD-accidents. Hauer also argues that more multiple night accidents (MN-accidents) may occur, because more often burned out light bulbs due to DRL-usage in daytime. Since single night accidents (SN-accidents) are not influenced by DRL Hauer’s hypotheses imply that the partial odds-ratio

$$R^* = MN \cdot (SD/SN)$$

increases by DRL. The hypotheses of Hauer are crucial, because in many DRL studies the raw DRL-effect is derived by so-called odds-ratios (discussed further in chapter 3). The odds ratio is defined by

$$R = (MD/SD)/(MN/SN)$$

It represents the interaction effect on multiple versus single accidents in daylight versus darkness conditions, where DRL supposedly decreases R. If the odds ratio and partial odds ratio in the before period are denoted as

R_b and R_b* then

$$MD_b = R_b \cdot R_b^*$$

where MD_b are the MD-accidents in the before-period and with index a for the after-period

$$MD_a = R_a . R_a^*$$

Also one has:

$$\text{Raw DRL-effect} = 1 - R_a/R_b$$

Here the raw DRL-effect is not the simple after reduction proportion of observed MD-accidents as 1 - MD_a/MD_b, but corrects for changes that may have occurred without a change of DRL. Under the assumption that the accidents in the partial odds ratios R* are not affected by DRL, a change in R* reflects the change in the MD-accidents in the after period that would have occurred without an increased DRL-usage. For example, changing ratios of day/night and/or multiple/single accidents may occur due to changing mobility patterns. It would change MD and would change R* in the same way, but R is not affected by such changing ratios. However, if the partial odds ratio R* increases due to DRL, as Hauer assumes to be the case ($R_a^* > R_b^*$), than the odds-ratio method for the estimation of the raw DRL-effect overestimates the raw DRL-effect on MD-accidents and neglects the possible adverse DRL-effect on SD- and MN-accidents. This would give overestimated safety effects of DRL on the total national accidents. In order to test Hauer's hypothesis the following hypothesis is formulated:

Basic Hypothesis 4:
The occurrence of single accidents and night accidents are not influenced by changes in DRL-usage.

Elvik (1996) in his meta-analysis of DRL-studies found no indications that support Hauer's hypothesis. If hypothesis 4 is not rejected than the changes in R* may indeed reflect expected changes of MD that are not due to DRL. In order to test the hypothesis one needs to have more observations than for two years before and after the DRL-change in order to inspect how R* and MD-accidents change in the before- and after-period and notably from the before period to the after-period in the different studies. Although an other statistically optimal estimation procedure based on hypothesis 4 will be used, comparable to the odds ratio method used in most studies, this will be done for those studies that contain the relevant separate data over more years in the annotated review of chapter 4. Changes in R* over time may complicate the interpretation of a genuine DRL effect by any estimation procedure of the raw DRL-effect based on hypothesis 4. Clearly the ideal situation is:

- relatively constant levels of R and R* within before- and after-periods, or a gradual change over time for these levels within these periods;
- a sudden change in the level of R between these periods without such a sudden change in R*, because then that sudden change can only be attributed to a change in MD-accidents caused by the changed level of DRL-usage.

Crossing, frontal and rear-end accidents
As discussed in the review of the visual perception effects of DRL in chapter 1, peripheral vision seems to be most enhanced by DRL. Therefore it might be that the DRL-effect on accidents in crossing manoeuvres of motor vehicles is larger than on frontal accidents. The perception of DRL seems least relevant for the prevention of rear-end accidents and other accidents between road users with coincident directions. Therefore, although the perception of brake-lights is not be masked by burning rear-lights of DRL-users (conclusion B4, which thus causes no relative increase of rear-end accidents) the DRL-effect on rear-end accidents may be the smallest or nearly absent. This is the reason why in some DRL-studies the rear-end accidents are left out in the DRL-evaluation. Therefore, one might question the next formulated conjecture.

Auxiliary Conjecture 6:
Intrinsic DRL-effects on MD-accidents between motor vehicles are for crossing accidents not higher and for rear-end accidents not lower than for frontal accidents.

If this hypothesis is rejected by the data of studies that contain the relevant data, it contributes to the interpretation of a genuine DRL-effect.

Fatal, injury and damage-only MD-accidents
If DRL is assumed only to reduce the probability of accident avoidance and not to influence the exposure to types of encounters, it does not matter whether the considered accident figures are fatalities or casualties or accidents or fatal or casualty accidents. However, if DRL does influence the braking behaviour of drivers in order to avoid accidents, one may expect in just not avoided accidents (thus where the driver has partially reacted on a DRL-using motor vehicle), that the collision speed also is lower. Therefore, possibly the following conjecture may be rejected:

Auxiliary Conjecture 7:
The intrinsic DRL-effect on all accidents, on casualties (or casualty accidents) and on fatalities (or fatal accidents) are equal.

Motorcycle MD-accidents
The last conjecture that is formulated concerns the influence on the MD-accidents of motorcyclists that used already DRL to an extent that approximates the after-level from an obligation of DRL for all motor vehicles. From chapter 1, on the one hand it may be hypothesised that DRL-using other motor vehicles mask the perception of motorcycles with DRL, which may result in more MD-accidents of motorcyclist. On the other hand the perception of cars and freight vehicles by motorcyclists may be improved by an increase of DRL-using other motor vehicles, which may reduce the number of MD-accidents of motorcyclists. However, by conjecture 2, where an equal accident prevention effect for encounters between motor vehicles with one and two DRL-using drives is assumed, no changed DRL-effect for motorcyclist is implied. Having a before proportion β_m of DRL-usage for motorcyclists and one for other motor vehicles of β with respectively α_m and α as their after DRL-levels, the proportional increase of relevant DRL-usage in encounters of motorcyclists and motor vehicles is a change from $(\beta_m + \beta - \beta_m \cdot \beta)$ to $(\alpha_m + \alpha - \alpha_m \cdot \alpha)$. If $\beta_m = \alpha_m = \alpha$ and $\alpha > \beta$ then the raw DRL-effect for motorcyclists must be smaller than for other motor vehicles, but their intrinsic DRL-effects may be equal. If masking of DRL-using motorcyclists increases due to a higher DRL-usage by other motor vehicles, the intrinsic DRL-effect for motorcyclists may diminish, which violates the last conjecture.

Auxiliary Conjecture 8:
The intrinsic DRL-effect for motorcyclists already using DRL is not affected by an increase of DRL for other (four-wheeled) motor vehicles.

In this case where it is assumed that approximately $\beta_m = \alpha_m = \alpha = 1$ (before and after DRL-level of motorcyclist approximates the full DRL-usage of other motor vehicles in the after period) the expression for an *additional intrinsic DRL-effect of motorcycles already using DRL on their accidents with other motor vehicles* can only depend on the increase of encounters with two DRL-using vehicles. Although this also contradicts conjecture 2, an additional non-zero DRL-effect for motorcyclists with an existing level of 100% DRL, from a proportional increase of their encounters with two DRL-users (from **ß** to **α** = 1) is identical to expression (1) and here is written as:

Additional DRL-effect $\mathbf{I_m = E_m/[1 - ß \cdot (1 - E_m)]}$ (4)

Here E_m is the additional raw DRL-effect for motorcyclists from the increase of DRL-usage for other motor vehicles and I_m its additional intrinsic DRL-effect. If significantly $I_m < 0$, it expresses an additional adverse DRL-effect for motorcyclists due to more DRL-using other motor vehicles. If significantly $I_m > 0$ it violates conjecture 2 of equal DRL-effects for encounters with one or two DRL-using motor vehicles. If no significant I_m is obtained, either there is no masking and conjecture 2 is acceptable or both masking and higher safety from two DRL-users occur, but compensate each other. In order to test this the last corollary is:

Corollary 6:
For motorcyclist which fully use DRL, the expression for a possibly additional DRL-effect on motorcyclists from an increased DRL-level for other motor vehicles is obtained by expression (4).

3. Methodological aspects of DRL-evaluations

Valid inferences on effects of accident reduction measures ask for a design of evaluation studies that makes such possible. It is not sufficient to compare, for example, the observed number of accidents before and after a measure is introduced, because over the time other things in traffic (such as traffic volumes, changes of extremely bad and mild winters etc.) can have influenced the observed number of accidents. Also care must be taken that measures are not selectively introduced after a sudden increase of accidents, because then also the number of accidents without an effect of a measure generally will decrease again to the average (so called regression to the mean), a flaw that may apply to fleet-owner studies.

What is needed for the period wherein a measure or induced change applies (in the here relevant case: an increased level of DRL-usage), is a sound estimation of the number of accident that would have occurred if only the evaluated measure or induced change had not taken place. Such a sound estimation is supposed to take all effects of other possible changes into account or is based on an experimental design wherein other changes are ruled out. Then a valid inference on the safety effect, as the reduction of observed accidents in the after-period compared to the estimated accidents for that after-period without the influence of the measure or induced change, is possible. Still then one also needs a statistical estimation of the certainty of the safety effect, which is based on some estimation of the random variance (and of systematic changes) in the accident data at hand.

The applicability of designs for the estimation of the expected accident number without the influence of the measure and the statistical analysis for the estimation of the effect and its certainty for the evaluation of DRL-effects as well as the flaws of some used designs and estimation procedures are discussed in this chapter. For the comparison of DRL-effects it is preferable that a statistical measurement of the random variation around the estimated DRL-effects is obtained as well as that there is no bias in the estimated level of DRL-effects due to different designs and estimation methods. In this chapter an optimal methodology and analysis for evaluation of DRL-effects is described, which then in the next chapter is applied (if needed and possible) for a re-analysis of the accident data from several DRL-studies in order to arrive at optimal and comparable estimates of the DRL-effects in each DRL-evaluation.

3.1. Different designs for the evaluation of DRL-effects

'Before/after' design
From one observed number of MD-accidents before the introduction of DRL and one observed number of MD-accident after, hardly any valid inference on the effect of DRL can be made. Even when the periods before and after are equal and circumstances in these periods are assumed to be comparable. No estimation of the random variation in the observed numbers can be made and, when conditions before and after are not controlled to be equal, other developments over time (not being the change in DRL-usage) also may have influenced the difference in observed MD-accidents numbers. The oldest fleet-owner study (Allen & Clark, 1964) uses this design by

comparing the MD-accidents before and after the DRL is introduced in the fleet.

Since other changing conditions in real traffic are not under control and their possible influence is not represented in the estimation for this design, its estimated MD-accidents without influence of DRL from the data in the before period need not be a valid estimation, while without empirical information on the error variance no significance of the estimated DRL-effect can be determined. A theoretical expected error distribution (derived from the Poisson distribution for before and after MD-accidents) is not sufficient, since the amount of overdispersion that generally is to be expected in data from the mixed Poisson processes in real traffic is unknown. The only thing that can be said from the significance by the theoretical error variance is that a theoretical insignificance of a DRL-effect is never actually significant, but a theoretical significance can be obtained from actually insignificant DRL-effects.

‘Single time-series with intervention’ design
The weakness of the before/after design can be tried to be remedied by the single time-series with intervention design. This design for the evaluation of a DRL-effect is based on independent observations of MD-accidents over more comparable (in length and circumstances) succeeding periods before and after the change in DRL-usage. Firstly, one can then check by the levels of MD-accidents separately in the before and after period whether circumstances have been systematically changing. If so, it may be that no trend or a similar trend in each period is present, which allows a comparison of expected and observed change in MD-accidents between periods. Secondly, the variation of MD-accidents around levels or trends in the separate periods can be used for the estimation of the error variance and thus for the establishment of the statistical significance of a possible decrease of the observed MD-accidents due to DRL between the periods before and after the increase of DRL-usage.

One needs at least three comparable before-observations, but preferably more when a trend is to be estimated, and at least one after-observation, but preferable more than two comparable after-observations for the verification of the assumed trend extrapolation (after interruption by DRL). The type of relevant analysis is a so-called time-series analysis with intervention (e.g Harvey & Durbin, 1986, applied to seat belt effect). Generally the parameter estimation of a gradually moving trend (trend with a moving average estimation) over the years ask for more comparable annual data than available. A specification by a few parameter of an almost monotonic trend that may be visible, can be used. A problem then is the type of trend over time that is assumed. Generally a linear approximation is assumed to be valid for not too long a time series (if seasonal data are used, also with a seasonal pattern), but there is no reason why trends in accidents over time should be linear (a linear decreasing extrapolation even predicts impossible negative accident numbers after many years).

A model with a log-linear trend and intervention for the MD-accidents is used for the estimation of the DRL-effect in a national study for Denmark (Hansen, 1994), where annual MD-accidents in the before-period seem to decrease. Although no a-priori reasons define accidents over time to change by an exponential function with a linear time dependence, it at least can not predict negative numbers and thus in principle is a better model for a monotonic accident trend than a simple linear model. Exponential decreasing functions of time have been successfully used as a model for the development of accident rates in many countries (see, Oppe, 1991). If this model would be used on estimated accidents rates (MD-accidents divided by annual kilometrage) the estimation of the Danish DRL-effect would have been higher. The weakness of the model with a log-linear trend and discrete interventions is that the method specifies an unknown trend and does not include any comparison for the intervention effect with a kind of control group that is not affected by DRL.

Experimental design
The design of several fleet-owner studies are based on the experimental design of experimental and control group comparison. Ideally either randomly chosen motor vehicles out of the total fleet are than equipped with DRL and their accidents are than compared to the accidents of the motor vehicles in the fleet which were not equipped with DRL or the accidents of a fleet equipped with DRL are compared to the accidents in another random chosen fleet out of a sample of fairly comparable fleets. Influences from other changes than DRL are supposed to be ruled out by the comparison of the number of accidents in the so-called control group without DRL with the ones in the so-called experimental group with DRL.
The validity of the inference on a DRL-effect depends on the actual randomness and balance of the selection within the fleet or the actual comparability with the other fleet. The DRL-effect is estimated by the proportional factor of the difference in MD-accidents between the experimental and control groups. The significance of the DRL-effect must be estimated from the variation of the numbers of DRL-relevant MD-accidents in comparable periods or of numbers for single and night accidents without DRL-relevance within each group. No estimate of error variance and thus no confidence intervals for the significance of the DRL-effect is possible from a design with a single number of accidents in each group, but sometimes for more experimental and/or control groups, or for single and night accidents the numbers of accidents are available.

Often the selected groups with and without DRL are not fully random or balanced selections or strictly comparable fleets. A way to correct for unbalanced selections which improves the validity of the DRL-effect estimation is to compare the rates of the MD-accidents. If the accident rate of the control group is denoted by **r**, the kilometrage of the experimental group by **V** then the expected MD-accidents for the experimental group without the influence of DRL, denoted by MD_e, becomes $MD_e = \mathbf{r} \cdot \mathbf{V}$ and the DRL-effect = 1 - MD_a/MD_e, where MD_a is the actual number of MD-accidents in the experimental group with the influence of DRL. It rules out differences in number of motor vehicles in each group and differences in distances driven, but other differences such as, for example, different driving environments with different exposures to risk and different risk taking characteristics of drivers in each group are still possible if no fully random selection can be guaranteed.

The main problem, even in studies with an excellent randomised group selection, is that the number of accidents generally are too small to find any significant effect. However, the combined evidence from these type of studies for DRL-effects without individually significant DRL-effects can give a significant DRL-effect.

Combined 'before/after' and experimental design
In this combined design that is more optimal than the separate designs, the possible non-randomness of the selection for the experimental group (motor vehicles with no DRL in the before- and DRL in the after-period) and control group (motor vehicles without DRL in before and after-period) not so crucial for inferences on DRL-effects. By a comparison between experimental and control group of changes of MD-accidents between before and after-period, any difference between the groups that does not have an interaction with the before/after condition is ruled out in the estimation of the DRL-effect. The proportional factor of the difference between the before/after changes of the two groups then is the estimated DRL-effect.

It might be, however, that there are again interaction factors between the two groups and the before/after-periods, which may invalidate the inference on a DRL-effect, especially is the control group is not fairly comparable to the experimental group. For example, if the experimental group comes from a selected fleet owner with a large national operating fleet that is equipped with DRL in the after-period and the control group consists of all the other national motor vehicles (as it is for an Austrian fleet-owner study on DRL, see KfV, 1993.) the main effects on the MD-accidents of the clearly present difference in groups are ruled out. It is however quite possible that the motor vehicles of the selected national fleet-owner are driving about the same amount of kilometres in the before and after-period, whereas the fleet of the other motor vehicles in the nation is increased in the after-period and then drive more kilometres than before. This particular interaction effect can be taken out if the comparison is between changes of accident rates in both groups, but other confounding effects with the DRL-intervention (interaction effects between groups and before/after) may be present.

Only if the control group in this design is a truly comparable or equivalent fleet or a random selection of motor vehicles without DRL in the after-period taken from within the fleet (and preferably with randomly changed drivers over DRL-selected and other vehicles in bot before- and after-period), such interactions are ruled out almost certainly. The same may be the case for designs with one, but preferably more than one, almost equivalent control group. In case of more control groups one may be able to check whether there are indeed interactions present or not, because different DRL-effects for different control groups are attributed to such interactions. If absent then only a common higher order interaction might invalidate the DRL-effect estimation. This is less likely the case when there are smaller differences between control groups than between experimental and control groups.

'Experimental time-series with intervention' design
If more equivalent successive observation periods for the MD-accidents of both the experimental and the control groups within the before- and after-period can be obtained, then this design also may allow for a trend with intervention analysis. The trend is assumed to be common for both groups, but the intervention effect from an increased DRL-usage only applies to a MD-accident difference between the before- and after-period in the experimental group.

Again the length of the available time-series of MD-accidents which is valid for the inference on the DRL-effect, based on the single intervention of a sudden change in DRL-usage, generally can not be long. Otherwise it can not be warranted that no other intervening factors have changed the difference between experimental and control group during longer time series. Moreover, any specification of the trend over time with a few parameters, needed for shorter time series, can not be justified. What seems more acceptable is the elimination of changes over time by main effects for each successive period within before and after-periods (and if the design allows it also the elimination of DRL-irrelevant interaction effects between these succeeding periods). The estimation and elimination of main and first order interaction effects for successive periods has the advantage that no specification of a trend is needed. Only the differences per successive period between experimental and control group then are relevant for the estimation of the DRL-effect and its significance. The variance of these differences within the before-period and within the after-periods after elimination of an average difference between the before and after differences, determines the significance of the DRL-effect that is measured by that average difference between the before and after differences. If evaluation periods cover different seasons one may or need to distinguish between DRL-effects for different seasons, due to opposing dependencies of raw DRL-effects on seasonal levels of ambient illumination and on seasonally different increases in DRL-levels.

'Before/after quasi-experimental' or 'odds ratio' design
The above example of a comparison between the before/after changes in MD-accidents of a large national fleet and the fleet of all other vehicles in the nation is more or less a kind of before/after quasi-experimental design (with a quasi-control group). This type of design and analysis is the most commonly used one for national DRL-evaluations. The so-called odds-ratio method for the DRL-effect in national studies is based on this before/after quasi-experimental design. For the national DRL-effect estimation from Finland and Sweden the experimental group data are formed by the MD-accidents (MD) of motor vehicles with a lower DRL-usage in the before-period than in the after-period (because of an intervention either by the DRL-obligation) and the quasi-control group data by the single daytime (SD), single night-time (SN) and multiple night accidents (MN).

For the analysis by the odds-ratio methodology (discussed in paragraph 2.5 and more fully in the next paragraph) one assumes on the basis of hypothesis 4 of chapter 2 and no DRL-change over time that some constant factor R times the partial odds ratio $R^* = MN \cdot (SD/SN)$ is an estimate of the MD-accidents, as $MD_e = R \cdot R^*$. When the indices a and b signify the after and before-periods then factor R becomes the odds ratio for the before-period as $R_b = MD_b/R_b^* = (MD_b/SD_b)/(MN_b/SN_b)$ and with R_a^* calculated

from the corresponding DRL-irrelevant data in the after-period one estimates by $MD_e = R_b \cdot R_a*$ as the expected MD-accidents for the after-period that would be expected without a change of DRL-usage. A higher after- than before-level than should then be reflected in a decrease of MD_a/MD_e and in positive DRL-effect = 1 - MD_a/MD_e. Generally one uses directly the odds ratios in before- and after-periods for the estimation of the DRL-effect. The corresponding odds ratio for the after-period is:
$R_a = (MD_a/SD_a)/(MN_a/SN_a)$ and since by definition R_a/R_b equals MD_a/MD_e the DRL-effect also equals 1 - R_a/R_b.

The theoretical error variance of the ratio R_a/R_b is established from the inverse numbers of the MD-, SD-, MN-, and SN-accidents in the before-period and after-period as in the Mantel-Haenszel test of homogeneity of ratios. However, the significance of a DRL-effect based on that theoretical error variance can only be useful in gauging its precision and should not be used for the testing of statistical significance (Fleiss, 1981). Nevertheless this is done in some DRL-studies. Only such a theoretically derived insignificance means an actual insignificance, but actually insignificant DRL-effects can be seemingly significant by this improper way of confidence estimation. Actually no empirical estimate for the error variance of the DRL-effect can be established by the odds ratio method, because the estimated DRL-effect corresponds to the highest interaction effect of a saturated parameter model for the data analysis (see next paragraph). Moreover, the odds-ratio estimation for the DRL-effect is a biased estimation procedure in case there is a non-saturated parameter model possible.

By the so-called log-linear model analysis (Knokke & Burke, 1980) for this design one estimates the same DRL-effect as the parameter for the higher order interaction between single/multiple, day/night and before/after, as is shown in the next paragraph in a more detailed way. This estimation procedure gives a statistically optimal and unbiased estimation of the DRL-effect, if one of the lower order interaction effects or main effects should be absent. It than can give a somewhat different DRL-effect for the same design and data and may also yield a smaller estimated error variance for the DRL-effect. When the data design contains the same data differentiation for at least one more before- or after-period, the DRL-effect (corresponding to the odds ratio) and its error variance always can be estimated by the log-linear model analysis in an unbiased and statistically optimal way. Since more data periods within the after- and before-periods leads to the ‘quasi-experimental time series with intervention’ design of the next section below that design and the estimation by log-linear model are further discussed under that heading.

The validity of the inference of a DRL-effect in this before/after quasi-experimental design is not only troublesome because of the unknown actual significance, also the interpretation of the highest order intervention effect as a DRL-effect is questionable. It has been argued (Theeuwes & Riemersma, 1995) that the estimated 11% DRL-effect in Sweden from the odds-ratio analysis (Anderson & Nillson, 1981) is due to peculiar changes in SD- and SN-accidents, because the DRL-irrelevant sum of SD-, MN-, and SD-accidents also reduced from before to after-period by 7.7%.
The arguments of Theeuwes & Riemersma (1995) boils down to the opinion that an expected MD-accidents without DRL in the after-period is better

estimated by $MD_e = Q_b \cdot [MN_a + SN_a + SD_a]$ with the factor Q_b as the before-period ratio $Q_b = MD_b/[MN_b + SN_b + SD_b]$ (assumed not to change with respect to the expected Q-factor without the influence of a DRL-change) than by $MD_e = R_b \cdot R_a*$, as it is for the odds-ratio analysis. On the one hand the factor Q_b may contain an irrelevant day/night effect that may change and which interaction between day/night and before/after is eliminated in the odds ratio estimation. On the other hand DRL-irrelevant changes in the ratio SD/SN or MN may have the effect that the estimated DRL-effect is due to changes in R_a*/R_b* that would not correspond to true changes in MD_e/MD_b, because by the odds ratio methodology one estimates a DRL-effect as:

$$1 - R_a/R_b = 1 - (MD_a/R_a*)/(MD_b/R_b*).$$

Also the saturated log-linear model analysis never can tell whether changes in MN-, SN-, or SD-accidents make the interpretation of a second order interaction effect between day/night, multiple/single, and before/after as a DRL-effect on MD-accidents invalid, because of the linear dependence of the dummy variables for different second order interaction effects in that saturated log-linear model.

‘Quasi-experimental time-series with intervention’ design
If one has the four time-series of observed MD-, SD-, MN-, and SN-accidents observed over several successive periods with a somewhere in between localised increase of DRL-usage, one has a ‘quasi-experimental time series with intervention’ design for the evaluation of the DRL-effect. As earlier stated, the relevant time series suitable for a valid DRL-effect estimation generally can not be very long, but may contain accident data for several years before and after the DRL-intervention without being disturbed by other interventions or developments than by DRL. Again one could calculate the odds ratio for each succeeding period and see whether there is a significant change in the development of the odds ratio at the intervention of the DRL-increase. The advantage compared to the above before/after quasi-experimental or simple odds ratio design is that a possible existing change trend in the successive odds ratios and the amount of change in the odds ratio due to the DRL-intervention can be distinguished and, therefore, can give a more valid estimation of the DRL-effect. Moreover, the error variance of the odds ratio based DRL-effect could be determined. However, the estimation procedure by the odds ratio methodology is statistically biased and not optimal with respect to significance estimations, where a log-linear model analysis yields an unbiased estimation of the DRL-effect and an optimal estimation of the error standard deviation.

In the log-linear model analysis one can, in principle, estimate parameters for differently specified functions of time for the description of the trend over the successive periods in the total accident data per period as well as for such trends in day/night and multiple/single effect parameters. Such specified trends generally will be linear with time (or auto-regressive) in the exponential function, as trend approximation for the relatively short time-series. The DRL-irrelevant effects of such time dependent changes, than are separated from the estimation of the DRL-effect. On the one hand the implicit correction for non-DRL relevant changes over time may yield a more valid estimation of the DRL-effect. On the other hand different function of time for the DRL-irrelevant trends gives different DRL-effects and no one can tell which function of time is correct.

The alternative analysis is not to fit specified trends over time, but to eliminate all DRL-irrelevant effects in the successive periods. It asks a model analysis that estimates the main effects of successive periods, multiple/single and day/night and all first order interaction effects of multiple/single, day/night and successive periods as well as the estimation of the DRL-effect as the interaction effect between before/after, multiple/ single and day/night. For n periods with 4 observed numbers MD-, SD-, MN-, and SN-accidents one needs the estimation of $(3 \cdot n + 1)$ DRL-irrelevant effects and one DRL-effect, leaving at least n-2 degrees of freedom for the error variance if all effects are significant and more if some effects are insignificant and left out in the estimation. In the next paragraph this further discussed in more detail.

The validity of the inference of a DRL-effect as the interaction effect between before/after, multiple/single and day/night depends on the question whether that interaction is due to a real underlying change in the MD-accidents between the before- and after-period or due to some underlying change in the other accidents such that R* is changed between the before- and after-period. One must notice that R=MD/R* per successive period is not influenced by the elimination of all first-order interaction effects, but that elimination makes that the residuals for MD and for R* per period can not be estimated separately. One actually should be concerned whether there is a real change in these residual values of MD or in the residual values of R*. Since the only change that is known to be present is the increase in DRL-usage one might hypothesize (see hypothesis 3) that it only can be a change in MD-accidents. But it is a quasi-experimental exploration of data in real traffic and there always may be other changes present. For example, increased alcohol enforcement structurally may change R* and less the MD-accidents. If such other non-random changes in residual R* occur somewhere during the before- or after-period, even gradually or most worse between the before- and after-period, than the DRL-effect can not or not fully be attributed to the change in DRL-usage and its effect on MD-accidents. It may over- or under-estimate the real DRL-effect.

Whether systematic DRL-irrelevant changes in residual R*-values are present between before- and after-period can be inferred from the interaction effects between multiple/single, day/night, and years as continuous time (if for example R* is log-linear changing over time) or from other interaction effects between multiple/single, day/night, and before/after than for the interaction between MD-elements and before/after. Such other interactions can not be estimated if the first order interaction effects between multiple/ single and successive periods and between day/night and successive periods are eliminated, due to linear dependence with the interaction between MD-elements and before/after. If one or both first order effects are insignificant, their elimination can be left out. Whether there are disturbing changes in the residual R*-values then can be tested by the significance of interaction effects between SD- or SN- or MN-elements and before/after.

Alternatively, if the first order interactions of successive periods with multiple/single and/or with day/night are replaced by linear with time changing multiple/night and/or day/night several interactions of MD-and/or SD- and/or SN- and/or MN-elements with before/after still can be estimated separately. One may investigate these alternative also if the first order interactions of successive periods with multiple/single and/or with day/night and the DRL-effect are significant. If interactions of SD- and/or SN- and/or MN-elements with before/after are insignificant and thus no significant before/after effect in the residual R*-values per period is present, while the interaction effect between MD-elements and before/after is significant, then it must be concluded that the latter interaction effect is a genuine DRL-effect.

The analysis may be separately performed for time-series of MD-accidents of motor vehicles with non-motorised road users and for time-series of MD-accidents between motor vehicles, where for the former the quasi-control time-series are the corresponding MN-accidents and the SD- and SN-accidents of motor vehicles. These separation of MD- and MN-accidents are necessary because of the difference in relevant effective change in DRL-usage for encounters between motor vehicles (one and two DRL-users) and encounters between motor vehicles and non-motorised road users (only one DRL-user), which difference causes different raw DRL-effects.

The analysis preferably should also be separately performed for winter and summer accidents in order to take in to account the effects of different levels of DRL-usage in before-periods and the different effectiveness of DRL in the summer and in the winter. Moreover, due to the possible variations in mild and inclement winters with variations in slippery and snow covered roads the single and specially the SN-accidents may be more varying with respect to MD- and MN-accidents in the winter than in the summer, which may cause a higher error variance for the analysis of the winter data.
A simultaneous analysis with a differentiation of summer and winter MD-accidents and/or non-motorised and motorised MD-accidents is possible by including a summer/winter effect and/or non-motorised/motorised effect and corresponding separate DRL-effects in the model. This should only be recommended if the error variances in summer and winter are not significantly different.

These kinds of checks on the DRL-effect as a genuine interaction effect on the residual MD-values, the possibility of analysis of effects over more periods, the reduced chance of wrong inferences from random effects in the last before- and the first after-period, the unbiased estimation and the statistically optimal testing, makes the analysis by the data design of a quasi-experimental time-series with intervention a much stronger analysis than the odds ratio analysis for the before/after quasi-experimental design.

‘Quasi-experimental time-series with multiple interventions’ design
The longer the time-series in the before and after-periods are the more likely that there has been some other intervention (or DRL-irrelevant and unknown factors) that may have influenced the relative share of MD-accidents. If more known and in time separable interventions are present the effects of such interventions and the DRL-intervention can be simultaneously fitted by the log-linear model analysis. The length of time-series for a valid and more convincing estimation of the DRL-effect by the log-linear model can than be enlarged.

Also multiple in time separated DRL-interventions, e.g. first a DRL-campaign and then a behavioural DRL-obligation possibly later followed by full DRL vehicle standard, can be taken into account in the analysis of longer time-series of MD-accidents with quasi-control time-series for SD-, MN, and SN-accidents. However, in the latter example it might be more appropriate to introduce a continuous design variable and model the DRL-effect as a function of that variable.

The flexibility of the log linear model analysis allows in principle also that the analysis incorporates multiples of continuous effects and in time separable discrete effects. Depending on the relevance of the modelling for the data at hand this will be done in some additional re-analyses of data from DRL-studies as presented in chapter 4.

3.2. Optimal estimation of DRL-effects

Data design in DRL-studies
The data design for the optimal estimation of DRL-effects for national DRL-evaluations is, as discussed in the sections above, the quasi-experimental time-series with intervention. Its structure is as follows:

	day	night
multiple	MD_{b-t}	MN_{b-t}
single	SD_{b-t}	SN_{b-t}

year t before DRL

for years t= ..,-3,-2 before DRL and for

	day	night
multiple	MD_{b-1}	MN_{b-1}
single	SD_{b-1}	SN_{b-1}

last year (t=-1) before DRL

Introduction of DRL

	day	night
multiple	MD_{a1}	MN_{a1}
single	SD_{a1}	SN_{a1}

first year (t=0) after DRL

and for years t=1,2,... after DRL as

	day	night
multiple	MD_{at}	MN_{at}
single	SD_{at}	SN_{at}

year t after DRL

The log-linear model for statistical analysis in DRL-evaluations
The basic formula for the log linear model in national DRL-evaluations is:

Estimated MD-accidents = $\exp(\mu_t + \tau + \pi + x_t + z_t + ß_0 + + ß_1 \cdot t + \delta)$

where there are 4 independent numbers of accidents per year t for n (before + after) years and thus the total number of available data is $4 \cdot n$.

The elements in the exponent are the parameters of the log-linear model analysis with the following meaning:

Parameters	number
main effects:	
year-effect = μ_t	n
(e.g.: absolute levels per year may differ)	
day = τ night = 0	1
(e.g.: overall level at day higher)	
multiple = π single = 0	1
(e.g.: overall level multiples higher)	
first order interactions:	
year x (mult./single) = x_t	(n-1)
(e.g.: with years less single accidents due to enhanced infrastructure?)	
year x (day/night) = z_t	(n-1)
(e.g.: day traffic growth more than night traffic?	
(mult./single) x (day/night) = $ß_0$	1
(e.g.: multiple day accidents relatively larger than MN (SD/SN)?)	
second order interactions:	
time x (mult./single) x (day/night) = $ß_1$	1
(e.g.: linear trend in odds ratio generally omitted, because absent)	
DRL effect = δ	1
= (before/after) x (mult./single) x (day/night)	
(in after-period the multiple day accidents are hypothesized to be relatively lower)	
Total number of parameters:	3.n + 3
Degrees of freedom for error variance:	n-3

If $n < 4$ the analysis becomes saturated and no testing of DRL is possible without leaving out possible insignificant effects {e.g.: time x (mult./ single) x (day/night) and or year x (mult./single) or year x (day/night)}.
If $n=2$ the year categories equal the before and after categories and no estimation of ß and of error variance can be obtained, if no other effects are insignificant {if time x (mult./single) x (day/night) is omitted and either (before/after) x (mult./single) or (before/after) x (day/night) are insignificant, as tested by the Chi-square-fit (Chi²-fit) of the model}.

The saturated log-linear model and odds-ratio analysis
Given one year before and one year after or, as it often is for the odds-ratio analysis, a summation of the accidents in the respective years of the before- and after-period, the analysis by the log-linear model simplifies to:

$$
\begin{aligned}
MD_b &= \exp(\mu_{-1} + 0 + \tau + \pi + 0 + 0 + 0 + 0)\\
MN_b &= \exp(\mu_{-1} + 0 + 0 + \pi + 0 + 0 + 0 + 0)\\
SD_b &= \exp(\mu_{-1} + 0 + \tau + 0 + 0 + 0 + 0 + 0)\\
SN_b &= \exp(\mu_{-1} + 0 + 0 + 0 + 0 + 0 + ß + 0)
\end{aligned}
$$

$$
\begin{aligned}
MD_a &= \exp(0 + \mu_1 + \tau + \pi + 0 + z + 0 + \delta)\\
MN_a &= \exp(0 + \mu_1 + 0 + \pi + 0 + 0 + 0 + 0)\\
SD_a &= \exp(0 + \mu_1 + \tau + 0 + x + z + 0 + 0)\\
SN_a &= \exp(0 + \mu_1 + 0 + 0 + x + 0 + ß + 0)
\end{aligned}
$$

Odds-ratios in terms of the log-linear model for the before-period is:

$$R_b = \frac{MD_b/SD_b}{MN_b/SN_b} = \frac{\exp(\pi)}{\exp(\pi - ß)} = \exp(ß)$$

and for the after-period:

$$R_a = \frac{MD_a/SD_a}{MN_a/SN_a} = \frac{\exp(\pi - x + \delta)}{\exp(\pi - x - ß)} = \exp(\delta + ß)$$

and thus their ratio in the DRL-effect becomes:

$$\text{DRL-effect} = 1 - R_a/R_b = 1 - \exp(\delta + ß)/\exp(ß) = 1 - \exp(\delta)$$

The ratio of the before and after odds-ratios estimates a DRL-effect, defined as $1 - R_a/R_b$, is identical to the DRL-effect from the log-linear model where it is defined as $1 - \exp(\delta)$. However, the odds ratio procedure based on observed sums of the data in the years of the before- and after-periods does **not** give an optimal estimation of the DRL-parameter δ. If the model analysis is not saturated (more data as parameters) then the separate data sets of all the years should be analysed simultaneously by a log-linear model that contains the DRL-effect as one of the parameters, which then can be derived from the estimated (instead of observed) data.

Log-linear analysis for estimation of a DRL-effect
The log-linear model solution for more successive sets of data follows from a vector with logarithmic data, a design matrix of dummy variables, and a parameter vector.

$$
\begin{bmatrix}
\ln(MD_{b-t}) \\ \ln(MN_{b-t}) \\ \ln(SD_{b-t}) \\ \ln(SN_{b-t}) \\ \ldots \\ \ln(MD_{b-1}) \\ \ln(MN_{b-1}) \\ \ln(SD_{b-1}) \\ \ln(SN_{b-1}) \\ \ln(MD_{a0}) \\ \ln(MN_{a0}) \\ \ln(SD_{a0}) \\ \ln(SN_{a0}) \\ \ldots \\ \ln(MD_{at}) \\ \ln(MN_{at}) \\ \ln(SD_{at}) \\ \ln(SN_{at})
\end{bmatrix}
=
\begin{bmatrix}
1.00.0 & 1 & 1 & 0.00. & 1.00. & 0 & -t & 0 \\
1.00.0 & 0 & 1 & 0.00. & 0.00. & 0 & 0 & 0 \\
1.00.0 & 1 & 0 & 1.00. & 1.00. & 0 & 0 & 0 \\
1.00.0 & 0 & 0 & 1.00. & 0.00. & 1 & 0 & 0 \\
\ldots & & & \ldots & \ldots & & & \\
0.10.0 & 1 & 1 & 0.00. & 0.10. & 0 & -1 & 0 \\
0.10.0 & 0 & 1 & 0.00. & 0.00. & 0 & 0 & 0 \\
0.10.0 & 1 & 0 & 0.10. & 0.10. & 0 & 0 & 0 \\
0.10.0 & 0 & 0 & 0.10. & 0.00. & 1 & 0 & 0 \\
0.01.0 & 1 & 1 & 0.00. & 0.01. & 0 & 0 & 1 \\
0.01.0 & 0 & 1 & 0.00. & 0.00. & 0 & 0 & 0 \\
0.01.0 & 1 & 0 & 0.01. & 0.01. & 0 & 0 & 0 \\
0.01.0 & 0 & 0 & 0.01. & 0.00. & 1 & 0 & 0 \\
\ldots & & & \ldots & \ldots & & & \\
0.00.1 & 1 & 1 & 0.00. & 0.00. & 0 & t & 1 \\
0.00.1 & 0 & 1 & 0.00. & 0.00. & 0 & 0 & 0 \\
0.00.1 & 1 & 0 & 0.00. & 0.00. & 0 & 0 & 0 \\
0.00.1 & 0 & 0 & 0.00. & 0.00. & 1 & 0 & 0
\end{bmatrix}
\begin{bmatrix}
\mu_{-t} \\ . \\ \mu_{-1} \\ \mu_0 \\ . \\ \mu_t \\ \tau \\ \pi \\ x_{-t} \\ . \\ x_{-1} \\ x_0 \\ . \\ z_{-t} \\ . \\ z_{-1} \\ z_0 \\ . \\ ß_0 \\ ß_1 \\ \delta
\end{bmatrix}
+ \text{error dev. terms}
$$

In the design the first set of dummy variables times the n parameters μ describes a main effect per year and for the last two dummy variables times the parameter τ and π give the main effects for differences between multiple versus single and between day versus night accidents. The second set of dummy variables minus the last one in this set times the n-1 parameters x and the n-1 parameters z describe the first order interaction effects between years x multiple/single and between years x day/night, while the last dummy variable in this set times the parameter $ß_0$ gives the first order interaction effect of multiple/single with day/night. The last two variables in the design matrix represent second order interactions. It can not contain the set of dummy variables for the interactions between categories of years x multiple/night x day/night, because it would exhaust the degrees of freedom and absorb the DRL-effect. Instead the continuous regression variable (0,t) for the interaction between years as:
time x multiple/single x day/night is included, which times the parameter $ß_1$ would represent a log-linear trend that may exist in the development of the odds ratio per year. The very last dummy variable times the δ parameter gives the DRL-effect on accidents from the second order interaction between multiple/single, day/night and before/after.

In matrix notation one writes this as:

$$M = D \cdot P + E$$

The least squares solution (for elements of E) of vector P with the parameters as elements is written as:

$$P = (D^T \cdot D)^{-1} \cdot D^T \cdot M$$

An iterative weighted least squares solution minimises the Chi² of the elements of E if a diagonal matrix W consists of elements W_t with:

$$W_t = \{[M_t - N_t)]/\sqrt{N_t})\}/\ln[M_t/N_t)]$$

where M_t are the observed data elements MD_{b-t}, MN_{b-t}, ... to SD_{at}, SN_{at} and N_t their respective estimated values. Solving with the first W = I

$$P = (D^T \cdot W^2 \cdot D)^{-1} \cdot D^T \cdot W^2 \cdot M$$

and repeatedly replacing W as defined above for $N = D \cdot P$ until convergence, gives the desired minimum (Chi)²-solution. Since the elements of W are dependent on the estimated numbers they must be iteratively determined. The test statistic for the difference of the parameters in P with hypothesized P_o is based on the normal distribution of P with variances S^2 (Dobson, 1990), where:

$$S^2 = \mathrm{diag}\{(D^{T} \cdot W^2 \cdot D)^{-1} \cdot (N-M)^{T} \cdot (N-M)\}$$

For m parameters the square root m-th diagonal element of S^2 is the standard deviation of error σ_m of the m-th parameter δ.
Hence with $\sigma_m = \sigma(\delta)$ the difference $\delta_o - \delta$ can be tested by the t-test with df=n-m.

The optimal estimate of the raw DRL-effect follows as:

$$\text{raw DRL-effect} = 1 - \exp(\delta)$$

with confidence intervals $1 - \exp[\delta \pm t_p \cdot \sigma(\delta)]$, where t_p is the test value with probability p for n-m degrees of freedom.

Its significance also can be derived from the F-test as ratio of (Chi)²-values from the estimated data with and without the inclusion of the parameter δ. The fit of the model is expressed by (Chi)² of the deviations between observed and predicted accidents. Generally the mean (Chi)² as (Chi)²/df will show that the hypothesized Poison variance has an overdispersion. Models that have a significantly higher (Chi)² than the expected (Chi)² at p = .05 (factor of overdispersion >1.5 if df=25, or >2 if df=7, or >3 if df=2 or >3.8 if df=1) generally are unacceptable and also often do not show significance for relevant parameters. Since there generally is some over-dispersion a test of δ-parameter against the theoretical expected (Chi)² for df=n-m generally gives an over-estimation of its significance. This is also the case for the so-called Mantel-Haenszel test of homogeneity for the ratio $r = R_a/R_b$ in the design of the sum of the data in the years of the before- and after-periods for the biased estimate of $\delta \approx r$, where the Mantel-Haenszel test of homogeneity is given as the (Chi)²-test for df=1 of:

$$(Chi)^2 = \ln(R_a/R_s)/\{\sigma[\ln(R_a)]\}$$

with

$$\sigma[\ln(R_a)] = \sqrt{[1/MD_a + 1/MN_a + 1/SD_a + 1/SN_a]}$$

and

$$R_s = (MD_{a+b}/MN_{a+b})/(SD_{a+b}/SN_{a+b})$$

This test is based on the theoretically expected (Chi)² with df=1 for:

$$[MD_a - MD_e]^2/MD_e \quad \text{with } MD_e = R_b.[MN_a.(SD_a/SN_a)\}$$

and therefore also overestimates the significance of the parameter δ when there is an overdispersion in the assumed Poison variance of the data, which generally will be the case for annual data wherein the Poisson parameter will not be constant (due to traffic growth and other influences such as annually differences in mild and inclement weather conditions).

An unbiased solution procedure, as alternative to the iterative weighted least squares solution for the log-linear model, can be obtained by a so-called maximum likelihood procedure for generalized linear models. It gives a log-linear model solution that is identical to the iteratively weighted least squares solution wherein the weights minimise the Chi-square of the deviations from the data (Dobson, 1990; Knoke & Burke, 1980). These maximum likelihood solutions for the log-linear model are contained in the program sets of GLIM (McCullagh & Nelder, 1986) and SAS with the specification ‘genmod = log’ (SAS, 1993). These programs also give the information on model fit, t-test significance of parameters by also given standard deviations of error (with and without overdispersion) and corresponding significance by F-tests for relevant $(Chi)^2$-ratios.

Investigations of estimated DRL-effects by alternative models
Referring to the design matrix with dummy (0,1) elements, one also can measure the DRL-effect of the second order interaction (multiple/single) x (day/night) x (before/after) by changing in the last dummy variable the unit values into zero values for MD-elements and change the zero values into unit values for either SD- or MN- or SN-elements and find that interaction as a before/after effect parameter on the SD- or MN- or SN-accidents, without the interpretation as a DRL-effect. The simultaneous estimation of several of these second order interaction parameters is not possible due to the linear dependence with the dummy variables for the interaction between years and multiple/single and/or between years and day/night. However, by leaving out the dummy variables for possible insignificant interactions between years and multiple/single and/or between years and day/night or by replacing these dummy variables by continuous regression variables as functions of time for $f(t)$ x multiple/ single and/or $f(t)$ x day/night, while adding one or two of the dummy variables that correspond to SN-, MN-, or SD-elements in the design, it becomes possible to investigate other second order interactions.

An additional analysis for the alterative design with $f(t)$ as regression variables for the first order interaction dummy variables with years in the design matrix may be recommended if this replacement in the model is not increasing the residual $(Chi)^2$ to a significantly unacceptable level. It may make it possible to investigate the influence of other second order interactions on the inference with respect to a DRL-effect. It can give insight whether the original DRL-effect (estimated from the design with first order interactions between years and multiple/single and between years and day/night) can be partly attributed to effects of other significant second order interactions with before/after. If that is not the case than the DRL-effect is a genuine DRL-effect, provided it is significant.

Other discrete or continuous interventions may complicate the inference on a DRL-effect in the analysis. For example: the share of day accidents may increase due to changed alcohol laws or enforcement. It may decrease the night accidents, but also perhaps relatively more the SN-accidents than the MN-accidents. Such an effect on all night accidents is absorbed in the main and first order interaction effects and do not affect the odds ratio, but a differentially decreased SN-effect would increase R*=MN.(SD/SN) and thus disturb the inference on the DRL-effect. Such an intervention can be eliminated by its inclusion in the design matrix as a dummy variable, when discrete and separated in time from the DRL-intervention, or as a regression variable, when continuous, and their relevant interactions in the design matrix also by their corresponding dummy or regression variables, provided that the additional parameters are not exhausting the degrees of freedoms. Given a measure for a changed alcohol enforcement per year, its effects can be additionally introduced as discrete or continuous main, first and second order interaction effects that are log-linear related to the measure. If especially its second order effect counteracts the DRL-effect (DRL does not affect R* and decreases shares of MD-accidents; increased alcohol enforcement increases R* and shares of MD-accidents) then a simultaneously estimated DRL-effect will be larger than the analysis without the estimation of the alcohol enforcement effects. If due to such gradual changes the R*-values and/or the odds ratios within the before- and after-period are changing in a log-linear or log-polynomial way with time, the inclusion of the already proposed third order interaction between $f(t)$ x (multiple/single) x (day/night) (as a [0,t] or [0,t] and [0,t²] regression variable(s) with parameter $ß_1$ or $ß_1$ and $ß_2$) could be included in the design of the log-linear model.

If the evaluation of DRL applies to a successive increasing DRL-usage over the years, one can not use a single intervention effect of DRL. If the DRL-usage increases with several marked shifts a cumulation of several in time separate interventions may be represented. This would ask for more parameters and less degrees of freedom for the error variance. Its estimated separate DRL-effects can be combined into a combined DRL-effect by a weighted average, depending on their variances, but its correction to an intrinsic DRL-effect must then be based on the same weighted average of relevant proportions of DRL-usage in the respective successive ‘after’-periods. In case the DRL-level increases gradually over the years, one may model a DRL-effect as:

$$\text{DRL-effect in year t} = 1 - \exp(\delta_i \cdot v_t)$$

where v_t is an appropriate continuous variable that replaces the (0,1) dummy variables for the DRL-effects in successive periods of several separable increases of DRL-usage. The intrinsic DRL-effect then needs no correction and is directly estimated by:

$$\text{Intrinsic DRL-effect} = 1 - \exp(\delta_i)$$

The determination of the regression variable v_t is problematic, because it asks that one knows the intrinsic DRL-effect already. If MD_o is the reference number of MD-accidents without DRL, than in year t

$$MD_t = MD_o\,[1 - \{1 - \exp(\delta_i)\} \cdot p_t]$$

where p_t is the relevant DRL-usage in year t ($p_t = \alpha_t$ for MD-accidents with one DRL-user or $p_t = 2 \cdot \alpha_t - \alpha_t \cdot \alpha_t$ for MD-accidents with two DRL-users and α_t as the proportion of DRL-usage in year t), while also

$$MD_t = MD_o \cdot \exp(\delta_i \cdot v_t)$$

so that

$$v_t = (1/\delta_i)\cdot\ln[1 - p_t.\{1 - \exp(\delta_i)\}] \quad (5)$$

where v_t and δ_i can be solved simultaneously by an iterative procedure.

The estimation of DRL-effects in fleet-owner studies
The most used design in fleet-owner studies is an experimental design. It data structure is as follows:

	DRL-relevant accidents types: yes	no
experimental group	E_1	E_2
control group	C_1	C_2

The number of accidents E_1 are MD-accidents between motor vehicles with DRL of the fleet with other motor vehicles without DRL or with all types of road users without DRL. The number of accidents E_2 are either the same accidents in a before-period without DRL or the sum of night and single accidents in the same period. The numbers of accidents C_1 and C_2 are the respective same accidents for the random selected motor vehicles in the fleet without DRL or in a comparable fleet without DRL.

If experimental and control group are balanced and randomly selected then one expects that E_1 is smaller than C_1 and that $E_2 = C_2$. The DRL-effect is often estimated as $1 - E_1/C_1$, but this is a biased estimate. The unbiased estimate is:

$$\text{DRL-effect} = 1 - E_1/E_e$$

where E_e is the expected number without influence of DRL as:

$$E_e = N\cdot[1 - (E_2 + C_2)]\cdot[1 - E_1/(E_1 + C_1)]$$

with

$$N = E_1 + C_1 + E_2 + C_2$$

Its significance can be tested by a (Chi)²-test with df=1 for deviations from
estimated values: $est(E_1) = E_e$
as well as: $est(C_1) = N\cdot[1 - (E_2 + C_2)]\cdot[1 - C_1/(E_1 + C_1)]$
and: $est(E_2) = est(C_2) = (E_2 + C_2)/2$

If the experimental and control are not randomly selected or comparable and/or not balanced groups then the DRL-effect often is derived from the odds ratio as DRL-effect = $1 - E_1/[E_2.(C_1/C_2)]$, but for this estimate there is no significance test possible, except the theoretical one for odds ratios that only can determine a too small range for its insignificance.

For the last case the expressions for the data in a log-linear model are:

$$\ln(E_1) = \mu + \tau + \pi + \delta$$
$$\ln(E_2) = \mu + \tau + 0 + 0$$
$$\ln(C_1) = \mu + 0 + \pi + 0$$
$$\ln(C_2) = \mu + 0 + 0 + 0$$

The parameter μ is the estimated main factor, the parameter δ gives the effect for experimental versus control group from the unbalanced or biased selection and the parameter π represents the effect for before versus after or for MD versus sum of SD, MN**,** and SN. Here one sees that:

$$\text{DRL-effect} = 1 - E_1/[C_1.(E_2/C_2)] = 1 - \exp(\delta)$$

where for a non-existing level of prior DRL-usage and fleet change from 0% to 100% DRL δ is equal to the δ_i for the intrinsic DRL-effect.

Since four parameter are estimated no degrees of freedom are left, unless the groups are assumed to be random selected and balanced. One then can omit parameter π, which leaves one degree of freedom, produces error deviations from the observed values and makes significance testing possible. The latter is also the case when more selective or unbalanced control and/or experimental groups are added. One has to add per additional group one parameter for their biased or unbalanced selection, but one also gets per group two data elements more and thus one degree of freedom is added per additional group.

In the combined before/after and experimental design also two such control groups are additionally present as biased before groups (because of before/after effect). However, the accidents without DRL-relevance have to be other ones than MD-accidents and that may cause an interaction effect between before/after and MD/non-MD, which asks for one additional parameter that is equal for that first order interaction within control and within experimental group, leaving one degree of freedom for error variance and significance testing in that design.

If the data for the accident type that has no relevance for DRL are not MD-accidents from the before-period, but separately available numbers for SD-, MN-, and SN-accidents in the same period, then the control group acts as a before-period in the odds ratio analysis for the design of a national DRL-evaluation, but with effects for control versus experimental group instead of main and interaction effects for before versus after. In the application of that analysis one may correctly assume that first order interaction effects that involve the control versus experimental group are absent, if no bias in the selection of the control group from the same fleet is present or if the other fleet without DRL is fully comparable. If also effects of unbalanced groups are absent one also may correctly omit the main effect of control versus experimental groups. Its second order interaction is the DRL-effect and thus always must be kept in the design analysis. Such an analysis then will give two (no first order interaction of control/experimental with day/night and with multiple/single) or three (also no main effect for control/experimental) parameters less than in a national DRL-evaluation with only one set of date before and after.

By applying that analysis for an unbiased or an unbiased and balanced control group one thus obtains also a possibility for significance testing and this is preferred above the analysis for the sum of accidents types without DRL-relevance in case these separate accident types are available. Similar things hold for the combined before/after and control experimental group with separate available MD-, SD-, MN-, and SN-accidents, where both before groups and the control group act as before groups without DRL. Here one has, however, full main and first order interaction effects for before/after. But there are n=4 sets of 4 numbers of accidents thus leaving n-2 or n-3 degrees of freedom. A similar log-linear analysis as in the national DRL-evaluation design applies here. The only difference in this design is that there are 3 sets that act as before-periods in the national design, where only the experimental group in the after-period acts as an after-period in the national design.

3.3. Combining estimates from independent analyses

One can combine the independent DRL-parameter estimates into an optimally estimated common parameter by a weighted average. If the independent estimates are results for the same population, the individual parameters are weighted by their proportion of inverse variance in the sum of inverse variances (Hedges & Olkin, 1985, p. 110-113). So for k independent studies from the same country with for the each study j with δ_{ij} as individual parameter and $[\sigma(\delta_{ij})]^2$ as its variance, one has an optimally combined DRL-parameter δ_{ic} for that country as:

$$\delta_{ic} = \Sigma w_j \cdot \delta_{ij} \qquad (6a)$$

where Σ is the sum over j=1 to k with proportional weights w_j, defined as:

$$w_j = [1/\{\sigma(\delta_{ij})\}^2]/\Sigma[1/\{\sigma(\delta_{ij})\}^2] \qquad (6b)$$

The variance of δ_{ic} for so far as caused by the individual parameter variances, as weighted within variance, becomes:

$$\{\sigma_w(\delta_{ic})\}^2 = [\Sigma w_j \cdot \{\sigma(\delta_{ij})\}^2]/k = 1/[\Sigma 1/\{\sigma(\delta_{ij})\}^2] \qquad (6c)$$

If, apart from weights determined by the inverse variances, additional weights q_j are applied (for example based on the differences in methodological validity of the study) then the revised proportional weights simply become:

$$w_j = [q_j/\{\sigma(\delta_{ij})\}^2]/[\Sigma q_j/\{\sigma(\delta_{ij})\}^2] \qquad (6d)$$

and with additional validity weights the pooled within variance of δ_{ic} becomes:

$$\{\sigma_w(\delta_{ic})\}^2 = [\Sigma(w_j \cdot \{\sigma(\delta_{ij})\}^2]/k = (\Sigma q_j/k)/[\Sigma q_j/\{\sigma(\delta_{ij})\}^2] \qquad (6e)$$

Additional to the pooled within variance the total variance of δ_{ic} depends on the weighted between variance of the individual δ_{ij} values around δ_{ic}, which latter variance becomes expressed as:

$$\{\sigma_b(\delta_{ic})\}^2 = [\Sigma w_j \cdot (\delta_{ij}-\delta_{ic})^2]/[(k-1) \qquad (6f)$$

Instead of iteratively replacing δ_{ic} for δ_{ij} in the calculation of $\{\sigma(\delta_{ij})\}^2$ for the estimation of the total variance of δ_{ic}, as Hedges & Olkin (1985) propose, the total variance of δ_{ic} is directly obtained by the sum of pooled within and between variances and thus:

$$\sigma(\delta_{ic}) = \sqrt{\{[\Sigma w_j \cdot (\delta_{ij}-\delta_{ic})^2]/(k-1) + [\Sigma w_j \cdot \{\sigma(\delta_{ij})\}^2]/k\}} \qquad (6g1)$$

or

$$\sigma(\delta_{ic}) = \sqrt{\{[\Sigma w_j \cdot (\delta_{ij}-\delta_{ic})^2]/(k-1) + (\Sigma q_j/k)/[\Sigma q_j/\{\sigma(\delta_{ij})\}^2]\}} \qquad (6g2)$$

The number of degrees of freedom (df) for the combined estimate δ_{ic} follows from the individual df of each study as:

$$df(ic) = \Sigma[df(ij) + 1] - 1\} \qquad (6h)$$

If the estimates δ_{ij} for the j-th study are not results from the same population (e.g. a summer- and a winter-period estimate or from different countries), but apart from the relevant differences between estimates one wants to estimate the weighted average (e.g. the whole parameter or the whole region parameter), clearly the between variance is of no concern and only the pooled within variance of the average is taken into account, hence then with a-priori weights q_j (for example $q_j = .5$ for summer and winter or weights q_j as the proportional share of the MD-accidents in the region of the total MD-accidents in the whole area):

$$\delta_{ic} = \Sigma q_j \cdot \delta_{ij} \qquad (7a)$$

$$\sigma(\delta_{ic}) = \sqrt{\Sigma[q_j \cdot \{\sigma(\delta_{ij})\}^2]} \qquad (7b)$$

with

$$df(ic) = \Sigma[df(ij)]\} \qquad (7c)$$

The differences between the individual parameters, then may be explained by a correlation with an other measure that is relevant for each individual DRL-parameter (e.g.: average ambient illumination level for the region of DRL-usage, measured by the average latitude or average sunlight angle).

The significance of each $\delta_i > 0$ or $\delta_{ic} > 0$ is established by the one-sided t-test value of $\delta_i/\sigma(\delta_i)$ or $\delta_{ic}/\sigma(\delta_{ic})$ against the expected value for the one-sided tail of the Student-distribution at $p = .05$ for df(i) or df(ic).

An other way to establish the significance of a non-zero δ-parameter from the individual evaluations can be obtained by the ‘inverse Chi-square method’ (Hedges & Olkin, 1985; Fisher 1932). In the additionally weighted version of this method:

$$-2 \cdot k \cdot [\Sigma q_j \cdot \ln(p_j)]/\Sigma q_j \qquad (8)$$

has a Chi-square distribution with df=k for the p-values of j=1 to k individual studies. However, this significance test applies to replications of experiments and not necessarily for a mixture of quasi-experimental before/after studies.

3.4. Conclusions on DRL-evaluation designs and analyses

For evaluations of DRL-effects the optimal designs are
for national evaluations:
- quasi-experimental time-series with intervention;

for fleet-owner evaluations:
- experimental design with one experimental and one unbiased and balanced control group;
- experimental design with more experimental and/or control groups that are unbalanced and/or selective groups;
- combined before/after and experimental design.

It is preferred (in case of the optimal designs for fleet-owner evaluations) or necessary (in case of national evaluations) to have separate data for MD-, SD-, MN-, and SN-accidents.

An unbiased estimation and statistically optimal testing of the DRL-effect can only be obtained by a non-saturated log-linear model analysis of these designs, which yields an optimally estimated standard deviation of the unbiased estimate of the DRL-effect parameter. Moreover, it gives information on the fit of the formulated model by the residual Chi-square value for the analysis. The analysis also allows an investigation of the possibility of other explanations of the assumed DRL-effect, by adding dummy-variable or regression variables for these other explaining factors in the model design. It may lead to the rejection of other explanations and thereby contribute to the establishment of a genuine DRL-effect.

The resulting DRL-parameters and standard deviations for these parameter of several independent studies can be optimally combined in to one unbiased parameter and one (smaller) optimally estimated standard deviation for the relevant results of samples from one population or as estimated average DRL-parameter for possible different parameters of different populations. The differences between DRL-parameters may be (cor)related with an independently measured variable that his hypothesized to contribute to these differences.

4. Annotated review and re-analyses of DRL-evaluations

In this chapter the results of published DRL-studies are reviewed and annotated in order to explore the meaning of the result for the hypothesis formulated in chapter 2. In most studies the analysis gives no unbiased estimation (in the statistical sense) of the DRL-effects and/or there is no or no optimal estimation of standard deviations for a significance testing of the DRL-effects. Where the published data allow such, additional analyses are performed in order to obtain statistically unbiased and optimal estimates. Generally only a few hypothesis of chapter 2 can be tested with the data of each study. When a possible test in the publication is absent, also additional test analyses are performed. In some of the older (especially fleet-owner) studies no data are presented but DRL-reduction percentages are presented, while not always is specified to what type(s) of accidents (daylight or multiple daylight or all accidents). In these cases the uncertainty is expressed as variance around the most probable DRL-effect.

For many other DRL-evaluations almost completely new analyses of the data are given, because of statistical shortcomings or less extensive usage of the possible statistical explorations of the presented data by the modern log-linear analysis techniques, described in chapter 3. A full report of the additional and new analyses as well as a discussion of the results in relation to the earlier findings are given in this chapter. Furthermore in a few cases additional data for longer before and after-periods and/or for other groups of accidents than used in the published study are obtained from the authors. In some cases where no re-analysis as such is necessary the error variance of the DRL-effect is not given and in order to estimate that variance the analysis is repeated. In the end for nearly all cases a new re-analysis is presented and its results are discussed.

This chapter presents the DRL-results and the relevant additional analyses of DRL-effects in paragraphs per country, where in case of more studies per country also an additional estimate of the average intrinsic DRL-effect per country is presented. If relevant evidence is present the indications for possible different DRL-effects for subgroups of accidents or road users, corresponding to the hypothesis in chapter 2, are explored per country.

4.1. DRL-evaluations in the USA

4.1.1. *DRL-campaign evaluation reported by Allen & Clark (1964)*

This oldest DRL-study reports the DRL-effect from a DRL-campaign that caused a DRL-usage of 80% during the holidays of the labour day period in 1961 in the state of Oklahoma. The evaluation is a *single time-series with intervention design* by the comparison to the accidents for same holidays periods of 1956 to 1960. The accident data are reported for 1956 to 1962 included. The report only states that for the comparable labour day periods the average total accidents (not only MD-accidents) before 1961 had decreased by 10% in 1961 and that the injuries reduced by nearly 25% and the fatalities by 75%. For the years 1962 and 1963 comparable holidays campaigns only led to a 12% DRL-usage, while again an increase of accidents is observed. The report also shows reductions in arrests (6% less) and courtesy warnings (37% less) for the same comparison. It seems as if the campaign 'Headlights for Safety' has influenced the safe driving as well, apart from the possible DRL-effect.

In the report no statistical analysis or significance test of these results are presented, nor is there any correction for increased mileage or for with time reducing accident rates. Since the number of assists in the periods are reported, while the assists correlate well with the number of accidents, the number of assists could serve as a measure of exposure. The rates of accidents to assists showed hardly a declining pattern over time, but the ratios of fatalities to assists showed a similar declining as for the fatality rates in USA observed from 1956 to 1960 included, as it might be expected for the difference between accidents and fatalities. It supports a log-linear analysis by the *quasi-experimental time-series design* of the accidents with numbers of assists as control series. This reanalysis is presented here.

The design model includes main effects for year as category and accidents versus assists, the first order interaction between years as continuous time variable and accidents versus assists and a DRL-effect for the last year. The same analysis for injuries and fatalities instead of accidents is also performed. The estimated raw DRL-effect on accidents is 2% and on injuries and fatalities 15%, but both cases not significant. These results relate to an increased DRL-level from 0% to 80% and to all accidents and all injuries fatalities, while MD-accidents or MD-fatalities have a share of about 55%. Using corrections based on 55% MD-accidents and on the transformation by expression (2) it follows that the intrinsic DRL-effects on MD-accidents is 3.2% and on injuries and fatalities 24.0%. From these analyses and the expression for the intrinsic DRL-effect as 1 - exp(δ_i) the mean DRL-parameter and its obtained standard deviation for MD-accidents are:

$$\boldsymbol{\delta_i = -0.032} \quad \text{and} \quad \boldsymbol{\sigma(\delta_i) = 0.227}$$

The positive DRL-effect not really supports a rejection of *hypothesis 1* and from the seemingly higher effects on more severe accidents also a rejection of *conjecture 7*. Clearly, in no way the results violate significantly hypothesis 1 (df=3 and one-sided t-test .032/.227 = .141 gives p = .4). No evidence for other hypotheses and conjectures can be obtained.

4.1.2. *Fleet-owner evaluation(s) reported by Allen & Clark (1964)*

The above mentioned article also reports the DRL-effect for the Greyhound buses from a simple *before/after design* in the first years of the sixties. The system-wide improvement form DRL is reported to be 11%. No absolute numbers or definition of accidents are given, but this 11% seems to relate to all MD-accidents. Fortunately independent results for Greyhound Lines divisions in the USA (and Canada, see next paragraph) are given as:
- Western division (15.7%): two year comparison daytime-accidents;
- Eastern division (7.0%): one year comparison daytime-accidents;
- Southern division (15.0%): no specification (as for 11% system-wide).

MD-accidents of buses likely are about 84% of all daytime accidents. Correcting the respective percentages for their shares of MD-accidents exists, one obtains:

- Western division: 18.6%;
- Eastern division: 8.3%;
- Southern division: 17.8%;
- System-wide: 13.1%.

No consistency with the overall 13.1% exists, unless the Eastern division is about twice as large as the other divisions. But since the western division comparison period is also twice as long as for the other divisions and thus its DRL-effect as reliable as for the Eastern division, the best overall DRL-effect probably is obtained by a weighted average, wherein the DRL-effect for the Southern division is weighted half the other DRL-effects. According to this weighting the weighted average δ_i from the intrinsic DRL-effect = 1 - exp(δ_i) and the standard deviation of the average δ_i, denoted by $\sigma(\delta_i)$, derived from the three separate and independent division results, are obtained as:

δ_i = -0.156 and **$\sigma(\delta_i)$ = 0.056**

Consequently the one-sided t-test value .156/.056 = 2.78 would yield for df=2 a significance level of p =. 06. Thus the intrinsic DRL-effect could be regarded as insignificantly different from zero. Also due to the uncertain weighting assumptions it does not violate *hypothesis 1* (no DRL-effect), but it may contribute weakly to its rejection. The report also indicates that crossing accidents are most reduced, supporting the expected rejection of *conjecture 6*. Southern versus northern results are indecisive for *conjecture 5* (differences of DRL-effect on different mean latitudes). No further information is relevant for other conjectures or hypotheses.

Fleet-owners results reported by Allen (1965)
The study contains the results of a questionnaire to fleet owners (n=181), who use DRL and who do not use DRL. The maximal DRL-effect, from an answer wherein the comparison concerns an experimental and control group with a 18 million mileage, was 38.7%. The distribution of the DRL-effect answers nor the accident data for these answers are known and no exact average DRL-effect estimation or estimation of standard deviations by weighting for different shares of fleets in such a questionnaire can be derived. However, the intrinsic DRL-parameter as well as its standard error could be obtained by the rough estimation of these values from the range of answers. Assuming a normal distribution of the DRL-parameter δ_i for the percentage answers ranging from an equally probable 0% to 39% gives a mean estimated DRL-effect of δ_i = -.247 or 22%. However, under a skewed distributions the estimation would give a higher or lower value. A distribution of DRL-parameters with equal probabilities for 0% and 39% DRL-effect that is skewed to the 0% by a rather acceptable factor of a half would yield a mean estimate of 11.6%. One may express the uncertainty by the rather questionable standard deviation for the mean estimates of 22% and 11.6%. It yields:

δ_i = -0.185 and **$\sigma(\delta_i)$ = 0.062**

It gives the rather unsound, but not necessarily biased, estimate of the intrinsic DRL-effect of 16.9% with a questionable (due to the unknown 181 observations) estimated confidence interval. It would mean that the effect is not significant (df=1, one sided t-test: -.185 + 6.31·.062 = .206 at the 5% level and its t-test value of .185/.062 = 2.98 and df=1 gives a significance level of p = .11) and thus the evidence in this report only indicates the rejection of hypothesis 1 (no DRL-effect). In the report no other accident information is relevant for other hypotheses or conjectures.

4.1.3. *Fleet-owner evaluation(s) of Cantilli (1965, 1970)*

Both studies concern the motor vehicles from the same fleet in New York randomly selected to be gradually equipped with DRL in an *experimental design*. The other vehicles form the control group for an analysis of MD-accidents during daytime including dawn and dusk. The first study of 1965 concerns a one year DRL-evaluation of 38 passenger cars with DRL and 200 control passenger cars. The DRL-effect on MD-accidents for the experimental fleet is reported to be a reduction of 44% compared to the control group (the 1970 report states that their corresponding accident rate per million miles are 10.22 and 19.20, a reduction of 53%). After these encouraging results more passenger cars and trucks of the same fleet owner were randomly selected to become equipped with DRL. After the summer of 1967 a randomly selected experimental fleet of 200 motor vehicles with DRL were for one year compared to the control group of 400 motor vehicles without DRL. The twice as large control group also drove about twice the miles of the experimental group and had 51 accidents, while the experimental group had 21 accidents. Thus 18% less accidents and also a 18% lower accident rate than the control group.

Although it is said that the results are significant on the basis of consistency of differences in cumulative accident rates for different types of accidents, the methodological shortcoming of cumulative comparisons does not allow this assertion due to the low absolute numbers of accidents. Since it concerns the same fleet owner in the same traffic environment and no indications of a marked difference of DRL for trucks and passenger cars are present, the DRL-effects of .44 and .18 of each study can be combined by the weighted sum of the two δ_i parameters, (derived from the intrinsic DRL-effect = 1 - exp(δ_i), with weights for respective shares of accidents. Also its standard deviation can be estimated by the weighted procedure. The results are:

$$\boldsymbol{\delta_i = -0.262} \qquad \text{and} \qquad \boldsymbol{\sigma(\delta_i) = 0.142}$$

This δ_i-value means an intrinsic DRL-effect of 23%, but the one-sided t-test (.262/.142 = 1.85 with df=1) yields p=.19. Thus no significant effect.

The results of Cantilli (1970) also indicates that:
1. severity of accidents reduce more than their occurrence;
2. rear-end accidents reduce more than crossing accidents;
3. MD-accidents are reduced most for black and not for yellow cars.

The last observation supports the interpretation of genuine DRL-effect, because yellow cars already have a higher conspicuity (see chapter 1).

The first observation supports the rejection of *conjecture 7*, while the second one indicates that *conjecture 6* is correct. Apart from these indications and an insignificant contribution to the rejection of *hypothesis 1*, the study contains no further information for other conjectures or hypotheses.

4.1.4. *Fleet-owner evaluation of Allen (1979)*

The study contains a comparison of the DRL-equipped fleet of the Checkers Cab Company with the Yellow Cab Company in a *experimental design* without splitting the accidents in DRL-relevant and DRL-irrelevant types. The reported DRL-effect is .072 or a DRL-effect parameter of

$$\boldsymbol{\delta_i = -0.075}$$

No absolute numbers of accidents and no standard deviation of the effect is given or can be obtained from two (unknown) accident numbers. Moreover the experiment concerns yellow cars that have already a high conspicuity. Therefore, also its results do not represent an expected DRL-effect for normal cars. Although, the DRL-effect indicates a rejection of main *hypothesis 1* (no DRL-effect), the absence of an estimated standard deviation and the selectivity of yellow cars makes that the result hardly can be used for the overall estimation of an intrinsic DRL-effect in the USA (to be obtained from all studies in the USA).

4.1.5. *Fleet owner evaluation reported by Attwood (1981)*

The article contains the evidence of DRL-usage from the Long Lines Division of ATT in one region of the USA from a two year period comparison as a *before/after design* and of eight regions from a half year comparison also in a before/after design. The evaluations on the totals of accidents give DRL-effects of 33% and 32%. No numbers of accidents are presented in the article. However, for the first investigation a reduction percentage for two-car crashes of 46% is reported and for all crashes during good visibility a reduction of 45%.

One could assume safely that the share of SN-accidents for the after-period are not changed, while the SD-, SN-, and MN-accidents each have a share of about 16% in all accidents of the before-period (acceptable shares for the USA). It would yield the next table for MD-, SD, MN-, and SN-accidents in the after-period as percentages of an unknown total from the before-period (bold figures reported and oblique figure and before percentages assumed, determining the other ones):

	Day	Night	Total	Change
Multiple	22.1%	13.6%	36.7%	**-46.0%**
Single	14.3%	*16.0%*	30.3%	+ 1.0%
Total	37.4%	29.6%	**67.0%**	**-33.0%**
Change	**-45.0%**	1.3%	**-33.0%**	

Table 2. *MD-, SD-, MN- and SN-accidents in the after-period as percentages of an unknown total from the before-period.*

It gives an odds ratio of 1.82. The odds ratio for typical USA shares of MD-, SD-, MN-, and SD accidents assumed for the before-period is 3.25.

In this way the evaluation upgrades to a *before/after quasi-experimental design* with a quasi-control group, showing a reduction of 33% for all accidents and hardly any change for DRL-irrelevant accidents. Its most probable effect is 44%, derived from 100·(1-1.82/3.25). Due to the lack of accident data the error in the odds ratio is unknown, but it easily may have a confidence interval that includes the difference between the assumed before and obtained after odds ratios. It follows that the DRL-effect on MD-accidents can be zero as well. The second evaluation with an DRL-effect of .32 on all accidents would give by the same procedure DRL-effect of 43%, but it could give quite different values if additional effects on single and night accidents were known. The standard deviation can not be derived, but the average intrinsic DRL-effect parameter of both studies could be estimated as:

$$\boldsymbol{\delta_i = -0.571} \qquad \text{and} \qquad \boldsymbol{\sigma(\delta_i) = \text{unknown}}$$

This high parameter with an intrinsic DRL-effect of 43.5% seems definitely different from zero, but its indication for the rejection of *hypothesis 1* (no DRL-effect) is rather questionable due to the assumptions made. No other evidence for this fleet is related to other hypothesis or conjectures.

4.1.6. *Fleet-owners evaluation of Stein (1984, 1985)*

The study of 1985 includes the data of the 1984 report and combines the data from four different fleets (Southern New England Telephone in Connecticut, South West Oil Company in southwestern USA, and nation-wide Dow Chemical and Merill Dow Pharmaceuticals. The total of the fleets is 3.313 motor vehicles (1.511 vans and pick-ups and 2.802 cars) operating somewhere in 1983 and 1984. The evaluation uses the *experimental design* with a random and nearly balanced selection of DRL-equipped vehicles and control vehicles for a comparison of DRL-relevant accidents (MD-accidents between motor vehicles) to DRL-irrelevant accidents (multiple night accidents and daytime accidents with parked cars). The report presents the absolute accident data separately according to the 2 x 2 x 2 design of experimental/control, passenger cars/ other vehicles and DRL-relevant/ DRL-irrelevant (ranging from 86 MD-accidents for cars with DRL to 24 DRL-irrelevant accidents of other vehicles without DRL) as well as the numbers for DRL-relevant accidents of experimental/control and cars/other vehicles in adverse weather conditions. A statistical analysis of the DRL-effect is not presented, although for adverse weather conditions the $(Chi)^2$-value with respect to all accidents was shown to be significant indicating an significant interaction that not necessarily tests the DRL-effect. The percentage of difference between the percentages of accidents of experimental and control vehicles are reported to be 7% for all vehicles, 5% for cars and for other vehicles 8% (n.b these are not reduction percentages of DRL-effect on MD-accidents). These differences are reported not to be statistical significant. The relevant odds ratio for an estimation of the DRL-effect on all MD-accidents gives a 15% reduction effect from DRL (for cars 12.5% and for other vehicles 14%).

An unbiased estimation of the DRL-effect and an optimal estimation of its standard deviation can be obtained by the log-linear model analysis.
The relevant design does not contain a difference between experimental and control group (because of balanced randomisation), except for the inter-action between types of accidents (MD-accidents versus DRL-irrelevant accidents) as the DRL-effect that is to be estimated (assuming equal DRL-effects for passenger cars and other vehicles). Main effects for passenger car versus other vehicles and for MD-accidents versus DRL-irrelevant accidents are included, but their interaction was excluded and when included it is insignificant as expected from the balanced randomisation. This analysis leaves 4 degrees of freedom for the error variances and fitted with an overdispersion factor of 1.214. The estimated intrinsic DRL-parameter and its standard deviation are:

$\boldsymbol{\delta_i = -0.163}$ and $\boldsymbol{\sigma(\delta_i) = 0.153}$

The corresponding intrinsic DRL-effect is 15%, but the result clearly does not significantly violate hypothesis 1 of no DRL-effect. The one-sided t-test .163/.153 = 1.06 yields for df=4 a significance level of p = .20.

From a methodological point of view this is the only fleet study in the USA that used a proper design. The rather weak statistical analysis of the data in the study is repaired by the additional log-linear analysis with optimal estimates, but leading to no different conclusion. The positive, but insignificant DRL-effect only contributes slightly to a possible rejection of *hypothesis 1* (no DRL-effect). The DRL-effect is reported to be larger in adverse weather, supporting the interpretation of genuine DRL-effect, although not significant. The DRL-irrelevant accidents (multiple night accidents and daytime accidents with parked cars) are exactly the same for the control and the experimental group, which indicates that *hypothesis 4* (no DRL-effect on SD- and MN-accidents) may be correct. No other hypothesis or conjecture can be highlighted by this study.

4.1.7. *Combined evidence and estimates for a DRL-effect in the USA*

Above there are seven independent estimates of the intrinsic DRL-effect parameter δ_i in the USA. One estimate concerns a fleet study with a hardly comparable estimate for yellow cabs. Only four estimates are fully comparable as results from empirical fleet owner evaluations, although two estimates (Greyhound and New York Port Authority fleets) are derived from proportional effects and by assumptions instead of from the data. One other estimate is empirically obtained from the data, but concerns the effect of a regional DRL-increase. The estimate based on asserted effects on fleets is the result of a questionnaire is hardly comparable to estimates with some structured data design.

The independent parameter estimates are optimally combined into one common parameter by a weighted average, when weighted by their proportion of inverse variance in the sum of inverse variances, as described section 3 of chapter 2. For only five individual parameter estimates the standard deviation is obtained. Only four estimated standard deviations are comparable and empirically obtained, while the other estimated standard deviation is questionable (the questionnaire result) due to its dependence on additional assumptions. Within the four empirical standard deviations two estimates are from the weak before/after design and/or from the analysis of replicated proportional effects without known data, whereas the other two contain estimates from the analysis of observed data in a stronger quasi-experimental design. Therefore, it seems justified to double the weights for the results of the latter two methodologically more valid evaluations, being evaluations 1) and 7) with relatively low parameters and high variances.

The four empirical evaluations, being the evaluations 1), 2), 4) and 7), then become weighted by weights that reflect the certainty and validity of their results. By expression (6a) of chapter 3, the weighted average DRL-parameter for mainly fleets in the USA and, by expression (6g) of chapter 3, its standard deviation are obtained as:

$\delta_{if} = -0.158$ and $\sigma(\delta_{if}) = 0.062$

Here, according to expression (6h) of chapter 3, df=13 and by the one sided t-test value .158/.062 = 2.54 one obtains significance level p = .015. Thus from these four studies with an empirical error estimate one concludes that the DRL-effect for mainly USA-fleets of 14.6% is very significant and that its 90% confidence interval ranges from 4.7% to 23.5%, although none of the four individual results is significant. By the alternative 'inverse Chi-square method' as expression (8) of chapter 3, the studies 1) and 7) with twice the validity weights of studies 2) and 4), it yields a Chi-square value for df=4 with a much higher significance level of $p < .001$. However, this latter significance test applies to replications of experiments and not a mixture of quasi-experimental studies.

In order not fully to omit the less valid evidence from the other questionable evaluations (the questionnaire study, as well as the ATT-fleet study and the hardly comparable yellow cab study), it seems reasonable to assign also mixed certainty and validity weights to their results. By halving the variance based weight for the questionnaire and by taking for the yellow cab and for the ATT fleet with unknown variances half the weight of the lowest other weight, one weights the results of all evaluations by a balanced mixture of their certainty and validity. Otherwise the most questionable and less valid evaluations would contribute as much to the combined USA-estimate as the more valid evaluations. The table below summarizes the terms needed for the argued combined estimate for the USA.

Study year	DRL-effect parameter	Inverse variance	Validity factor	Obtained weight	Prop. weight	Degrees freedom
1964	-.032	19.4	2	38.8	.060	3
1964	-.156	318.9	1	318.9	.497	2
1965	-.185	260.1	½	130.0	.203	1
1970	-.262	49.5	1	49.5	.077	1
1979	-.075	19.4	½	9.7	.015	0
1981	-.571	19.4	½	9.7	.015	0
1985	-.163	42.7	2	85.4	.132	4

Table 3. *Summary of δ_i-parameters and weights for studies in the USA.*

From the summary it can be observed that there is no effect of the year of the evaluation apart from the outlier for the ATT-fleet study reported in 1981. The seven estimates, combined by the adapted weights, give a weighted average estimate of the DRL-parameter for the USA based on all (mainly fleet) studies. This fleet DRL-parameter and its standard deviation as obtained by the same expressions (6a) to (6h) of chapter 3 are:

$\boldsymbol{\delta_{if} = -0.169}$ and $\boldsymbol{\sigma(\delta_{if}) = 0.049}$ with **df=17**

For all studies the combined absolute parameter is somewhat higher and its standard deviation lower than for the former four studies, which also by the higher number of degrees of freedom increases the significance. Since df=17, the significance level of this DRL-effect for USA-fleets by the one sided t-test value of .169/.049 = 3.45 becomes p = .002. Consequently the combined intrinsic DRL-effect for all USA-studies of 15.5% has a higher 90% confidence interval with a smaller range from 8.0% to 22.5%. However, if the fleet-owner studies are biased in comparison to national DRL-effect evaluations (if conjecture 1 of no biased fleet results is rejected by an up-ward effect for fleet-owner results) than it is not the expected national DRL-effect from a DRL-obligation in the USA.

4.2. DRL-evaluations in Canada

4.2.1. *Fleet-owner evaluation reported Allen & Clark (1964)*

In this already under the USA cited report also contains an unspecified DRL-effect of 24% for the total accidents of the Greyhound division in Canada obtained by a comparison in a *before/after design*. Again correcting for the 84% share of MD-accidents of all bus accidents its DRL-effect on MD-accidents becomes 28.6%. Assuming that the variance of this DRL-effect for Canada is proportionally the same as for the USA Greyhound fleets the effect parameter and its assumed standard deviation are:

$\boldsymbol{\delta_i = -0.336}$ and $\boldsymbol{\sigma(\delta_i) = .121}$

Its one sided t-test value .336/.121 = 2.77 with df=1 gives a significance level of p = .15 and thus by this questionable standard deviation the effect is insignificant, but it contributes weakly to a rejection of *hypothesis 1*.

4.2.2. *Fleet-owner evaluation of Attwood (1981)*

This already referred report also contains a DRL-effect evaluation for a fleet of 350 non-military vehicles selected from six military basis in Canada. The study has an *experimental design*, wherein one vehicle of vehicle pairs (matched by year, model, colour, and employment) is randomly selected for DRL-equipment. The control group without DRL had 20 MD-accidents in 21 month in 1975 to 1977, where the experimental group with DRL had 16 accidents. Although far from significant (too few accidents) it amounts to a 20% DRL-effect or a DRL-parameter δ_i of:

$\boldsymbol{\delta_i = -0.223}$ and $\boldsymbol{\sigma(\delta_i) = 0.399}$

No actual standard deviation can be obtained, but if the Poisson distribution is assumed it gives the presented standard deviation, showing it insignificantly contributes somewhat to a possible rejection of hypothesis 1 (no DRL-effect). The injuries and costs of accidents indicate that more severe accidents occur in the control group, which contributes to a rejection of *conjecture 7* (no DRL-effect difference in severity of accidents).

4.2.3. *Fleet-owner evaluation of Sparks et al. (1989, 1993)*

Both reports are based on the same data in a design of a *quasi-experimental time-series with intervention*. All MD-accidents and some sub-classes of fleet accidents in 4 years plus 4 months before and 4 months plus 3 years after the installation period of DRL in the fleet of the Central Vehicle Agency of the Government of Saskatchewan. The installation period was in the summer months of 1986, whereby the periods before and after are not balanced in seasonal effects. The comparison is made with respect to a quasi-control group, that consists of a randomly selected 8.3% of the same types of accidents from the police database for the same periods. The authors conclude that the DRL-relevant MD-accidents (excluding same direction accidents), here called selected MD-accidents, are significantly reduced by a DRL-effect of 28%, excluding twilight accidents the DRL-effect is 22%. Including passing MD-accidents the DRL-effect becomes 23% and including also rear-end accidents and accidents with parked vehicles, assuming no DRL-effects for these types, at least 15%. Moreover, it is reported that the DRL-effect on injury/fatal MD-accidents is 49% and on damage-only MD-accidents 22%, which difference is reported to be significant. Comparing DRL-effect on the casualties in the selected MD-accidents to the DRL-effect on all selected MD-accidents, the former DRL-effect is increased by a factor .49/.28 = 1.74.

Analysing only the data for the full years before and after 1986 would have balanced the seasonal effects. Moreover, the authors do not give the standard deviations for the DRL-parameters from their log-linear analysis. Based on the log-linear analysis of the data without the months in 1986 (main effects for years without 1986, for control/experimental and DRL-relevant/ DRL-irrelevant accidents and their first order interactions) the DRL-effect (second order interaction effect with before/ after) and its standard deviation are obtained for the *selected DRL-relevant MD-accidents* as:

$$\delta_i = -0.324 \qquad \text{and} \qquad \sigma(\delta_i) = 0.082$$

which means an intrinsic DRL-effect on the selected MD-accidents of 27.7% as already obtained by Sparks et al, by their analysis. The other results for other included sets of accidents, therefore, probably are also unbiased estimates. Since adding the passing accidents reduces the DRL-effect on the selected MD-accidents by a factor .23/.28=.82 and adding also rear-end MD-accidents and daytime accidents with parked cars by a factor higher than .15/.28=.54, one may assume that adding only passing and rear-end accidents reduces the DRL-effect on the selected MD-accidents by a factor higher than (.54/2 + .82)/1.5 =.73. Since the passing and rear-end MD-accidents have a share in all MD-accidents of about 46%, according to the national DRL-study discussed in the sequel, the DRL-effect for same direction accidents probably is about half the DRL-effect on other MD-accidents and for rear-end accidents about a quarter (not confirming conjecture 6). Thus the *comparable intrinsic DRL-effect on all MD-accidents* and its standard deviation are estimated as:

$$\boldsymbol{\delta_i = -0.240} \qquad \text{and} \qquad \boldsymbol{\sigma(\delta_i) = 0.061}$$

It gives a DRL-effect of 21.3 % on all MD-accidents. The one-sided t-test .240/.061 = 3.93 with df=18 has a significance level of $p < .001$.

Apart form the significant rejection of hypothesis 1 (no DRL-effect), the analysis by Sparks et al. show a higher DRL-effect on twilight and in rural MD-accidents as well as a lower DRL-effect on same direction MD-accidents. It supports the interpretation of genuine DRL-effect, while the latter may contribute to the rejection of *conjecture 6* (equal DRL-effects on MD-accidents types). Moreover, that casualties are significantly more reduced than damage-only accidents rejects *conjecture 7* (DRL-effect on casualties 1.74 higher than on MD-accidents). Last, it is remarked that the odds ratio of experimental/control and DRL-relevant/DRL-irrelevant accidents per year after 1986 do not show any decline, which may indicate that *conjecture 4a* is to be rejected since no risk adaptation from DRL occurs. No other facts are relevant for other hypotheses or conjectures.

4.2.4. *National evaluation of DRL-vehicle standard of Arora et al. (1994)*

The evaluation concerns the DRL-effect on selected MD-accidents between motor vehicles (MD-accidents without same direction accidents) during daylight hours in the year 1991 for whole Canada. The fitment of DRL became an obliged vehicle standard after 1989. Except the vehicles of model year 1990 that were sold in 1989, all vehicles of model years 1990 and 1991 are equipped with automatic DRL. The level of voluntary DRL-usage of older model years was 29.1% in 1991. In a replicated *before/after quasi-experimental design* the selected MD-accidents in year 1991 for vehicles of model year 1990 are compared with the selected MD-accidents in year 1991 for model year 1989. The comparison is also made with the selected MD-accidents in 1991 for several model years older than 1989 as well as for model year 1991, although the frequency of the 1991 vehicles over the year 1991 is different. As quasi-control groups serve the corresponding SD-, MN-, and SN-accidents, where for the MN-accidents the same direction (rear-end and passing) MN-accidents also are excluded.

The analysis in the report is based on the comparison of the respective odds-ratio of the MD- ,SD- ,MN-, and SN-accidents of different model years in 1991. The tested expectation is that the ratio of odds ratios is unity for ratios of model years with same DRL-use, but that the odds ratio for model years 1991 or 1990 and the odds ratio for model year 1989 or older model years have ratios that are smaller than unity. The before/after applies here not to years of accidents, but to model years of vehicles. Its design excludes irrelevant changes in odds ratios over time, but may contain DRL-irrelevant effects on odds ratios that dependent model years.

The report systematically shows that the mean raw DRL-effect from ratios for model year 1990 with older model years is significantly lower than unity. The estimated difference from unity for the most comparable model years of 1990 and 1989 was 8.3%. The authors tested the significance of that 8.3% by the theoretical error variance of the ratio of odds ratios, although not fully valid the result also would have been significant if tested by the standard deviations of the observed DRL-irrelevant ratio of odds ratios (s.d < 1.8%). Moreover, an additional 5.5 % higher ratio of odds ratio than unity is found for the model years ‘91/’90 with DRL. About the same mean of 5.5% higher ratio than unity is found for ratios of model years t and t-1 as well as t-1 and t-2 in years before ‘90. The authors leave it open whether this should lead to a correction of the DRL-effect, but state that the corrected the raw DRL-effect becomes 13.8%.

The answer to the question of a correction or not, depends on the way one determines the expected MD-accidents for model years 1991 and 1990 in case there had not been an obliged DRL-fitment for these models. Methodologically one ought to estimate the DRL-effect independently from other differences for model years and should apply the independently estimated DRL-effect on the MD-accidents of the model year 1990 and 1991 to their otherwise expected MD-accidents that includes such other predicted effects between model years. Anyhow, when the odds ratio decreases with the model age (which can be expected if newer cars are more used for daytime business trips in urban areas or if older cars are driven more by youngsters with more MN-accidents) it asks that the model-age dependence of the odds ratio is separated from the DRL-effect.

The authors do not correctly analyse the available data. The odds ratios computed are based on unbalanced (with respect to model age) partial totals of MD- and MN-accidents of a triangular matrix with pairs of multiple accidents between vehicles of different model years. Only the column totals of the upper triangular matrix of MD- and MN-accidents between vehicles of model-age pairs are used instead of the totals of the rectangular symmetric matrix. If the ratios for MD- and MN-accidents between the three age groups of vehicles for model years ‘before 1987', ‘1987 to 1989', and ‘after 1989' are divided by the ratio of the SD- and SN-accidents which correspond to model age of the columns (accidents with unknown model years are excluded and accidents with vehicles of pre-sold models 1992 in year 1991 are added to the accidents for vehicles of model year 1991), one obtains the following unbalanced odds ratio’s:

Odds ratio	Model years	Model years		
		Before ‘87	‘87-‘89	‘90-‘91
R_{b1}	<‘87	**3.533**	3.336	3.080
R_{b2}	‘87-‘89	3.894	**3.795**	3.379
R_a	>’89	3.621	3.403	**3.250**
R	all	3.690	3.457	3.185

Table 4. *Unbalanced odds ratios for the three age groups of vehicles for model years ‘before 1987', ‘1987 to 1989', and ‘after 1989'.*

The so defined odds ratios show a decrease for each row. One could argue that the decrease for models ‘90-‘91 is just the result of the extrapolated difference between the models before ‘87 and ‘87-‘89 and that no DRL-effect is present. However this would be a wrong conclusion, because the only valid comparison ought to be based on the comparison of the bold typed odds ratios on the diagonal, which show an increase before DRL for the model years is made compulsory and a decrease thereafter.

The MD- and MN-accidents in the ratios below and above the diagonal are exactly the same and thus their odds ratio difference is due to different SD/SN ratios of the columns, which then must decrease with the model age. That these SD/SN ratios are decreasing with the model years is clear from the observed SD/SN ratios for the model years, which are:

Model years	<‘87	‘87	‘88	‘89	‘90	‘91
Ratio SD/SN	1.10	1.26	1.28	1.30	1.33	1.24

Table 5. *SD/SN ratios for the model years.*

Again the lower SD/SN ratio for ‘91 is due to a relative higher darkness exposure of the models ‘91 and ‘92 sold in 1991 (their exposure over the year is skewed toward the end of 1991).

The odds ratio as the MD/MN-ratio divided by the SD/SN-ratio is only well determined for the bold typed odds ratios of the diagonal in the above matrix of odds ratios. Since the off-diagonal odds ratios contain multiple accidents for combinations of vehicles with a different model age, the single accidents of the row and column model age are both equally relevant. However, in the above table of odds ratios only single accidents for the column are used, as the authors have done in their analysis of odds ratio for model years ‘90 or ‘91 compared with preceding model years. Moreover, the authors used in their odds ratio analysis the selective totals of the accidents that belong to the columns of the upper diagonal matrix and thus also used multiple accidents which selectively and progressively excludes multiple accidents with newer vehicles for their odds ratios of older model years. So the odds ratio comparison of the authors is two-fold incorrect.

Firstly, the selective MD/MN-ratios are model age dependent. Secondly, their odds ratios are even more dependent on the model age, because of the unbalanced division of their selective MD/MN-ratios by the column SD/SN-ratios only, in stead of by the (geometric) mean of column and row ratios of SD/SN.

An appropriate analysis of pairs of MD- and MN-accidents between vehicles of different model years, could be based on their MN/MD ratios that are corrected for the SD/SN ratios of both model years. Such symmetric balanced odds ratios for pairs of model years x and y could be defined as:

$$R_{xy} = [MD_{xy}/MN_{xy}]/[(\sqrt{SD_x} \cdot \sqrt{SD_y})/(\sqrt{SN_x} \cdot \sqrt{SN_y})]$$

These balanced odds ratios of pairs for the former table become:

Model years	Model years		
	Before ‘87 b1	‘87-‘89 b2	‘90-‘91 a
<‘87	**3.533**	3.604	3.340
‘87-‘89	3.604	**3.795**	3.391
>’89	3.340	3.391	**3.250**

Table 6. *Balanced odds ratios for pairs of model years.*

By comparing the diagonal elements of the odds ratios in the latter or former presented matrix, one could estimate the raw DRL-effect on MD-accidents of ‘90-‘91 (with about 94% encounters between two-DRL-using vehicles) with respect to the MD-accidents of ‘87-‘89 or before ‘87 (with encounters between 8.5% two DRL-using vehicles and 41.2% vehicles with one DRL-using vehicle). Since 80% of all multiple accidents between the same group of model years from before ‘90 are between models from before ‘87, a proper estimate of the raw DRL-effect could be obtained by a weighted combination of the before-period odds ratios of the diagonal as:

$$1 - R_{a,a}/[.8 \cdot R_{b1,b1} + .2 \cdot R_{b2,b2}] = .094$$

It would yield a proper raw DRL-effect of 9.4% for Canada if the odds ratio without DRL is not dependent on the model years involved. However, in the latter table there are marked increases on the diagonal, as well as per column and row, between the odds ratios of <‘87 to ‘87-‘89. That increasing trend in odds-ratios for the later model years is not taken into account in the above estimate of 9.3%. Since the comparison of odds ratios from the most adjacent periods is least affected by that trend, while

$$1 - R_{a,a}/R_{b2,b2} = .144$$

one as well could argue that this particular raw DRL-effect is at least higher than 14.4%, if the trend is extrapolated. Other proper raw DRL-effects could be derived from the odds ratio comparison within rows or columns and extrapolations of row or column trends, but these raw DRL-effects would also correspond to other changes in levels of relevant DRL-usage (percentages for one and two DRL-users) and thus may still lead after their different corrections to similar intrinsic DRL-effects.

The authors corrected their improper raw DRL-effects to an intrinsic DRL-effect. It leads to their reported 11.3% intrinsic DRL-effect (or to their reported 15.5% where the full 5.5% overinvolvement is taken in account). But their correction is based on the incorrect expression (1) for the existing 29.1% encounters between motor vehicles with only one DRL-using motor vehicle, instead on the expression (2) or (3) for the existing 49.7% of vehicle encounters between two and one DRL-user(s).

Not all 1990 vehicles are equipped with DRL, because the vehicles of the model year 1990 are for a small percentage already sold in 1989 and not equipped with the DRL standard that became obligatory in december 1989. Also 1992 models were already sold in 1991 and the authors presented also the accident data of these 1992-models in 1991. Their daytime accidents are 9.1% of the daytime accidents of the 1990-models. So it may be assumed that at least about 7.5% of 1990-models in 1991 are not equipped with DRL and have the same voluntary DRL-usage as the DRL-usage of the older models, that is 29.1%. The DRL-usage for the vehicles of model years 1990 and 1991 together becomes about 97%. The correct transformation is obtained by expression (3) for the relevant proportions to which the DRL-effects apply and which are:

DRL	< ‘90 x < ‘90	< ‘90 x > ‘89	> ‘89 x ‘89	> ‘89 x all
no x yes	.412	.695	.062	.537
yes x yes	.085	.282	.937	.445
one + two	.497	.977	.999	.982

Table 7. *Relevant proportions of one and two DRL-users.*

The relevant proportions for the raw DRL-effect of a 14.4% raw DRL are .999 and .497 (or for $\boldsymbol{\alpha_f}$ = .97 and **ß** = $\mathbf{ß_f}$ = .291 in expression 3), which would give an intrinsic DRL-effect on selected MD-accidents of 24.4%. This intrinsic DRL-effect for Canada would apply to the selected MD-accidents, excluding same direction MD-accidents with a share of 46%. However, here the biased odds ratio methodology is used and no error standard deviation of the DRL-parameter can not be obtained for properly derived DRL-effects by that methodology (thus also no standard deviation is given by the authors).

Fortunately, as already used for the revision of the odds ratios, the report contains the data for SD- and SN-accidents in 1991 for the model years before 1987, 1987, 1988, 1990, 1991, and 1992 as well as the MD- and MN-accidents in 1991 between all pairs of these model years. So an estimation of the DRL effects is possible by the analysis of an appropriate log-linear model for these accidents data, wherein it even is possible to test whether there is an additional DRL-effect for two DRL-using vehicles or not (see conjecture 2). It asks for the following model, wherein for:

$k = j = t_k = t_j$ = 0 for model year 1991
= 1 for model year 1990
= 2 for model year 1989
= 3 for model year 1988
= 4 for model year 1987
= 9 for older model years (for mean model age 1982)

$Est(SN_k) = \exp[2 \cdot \mu_k]$

$Est(SD_k) = \exp[2 \cdot \mu_k + 2 \cdot \tau_k]$

$Est(MN_{kj}) = \exp[\mu_k + \mu_j + \pi_{kj}]$

$Est(MD_{kj}) = \exp[\mu_k + \mu_j + \pi_{kj} + \tau_k + \tau_j + ß + \delta_1 + \delta_2]$

Here:

μ_k = exposure to risk at night for model k

τ_k = additional exposure to daytime risk for model k
(τ_k may be replaced by $\tau_o + \tau_1 \cdot t_k$)
π_{kj} = additional exposure to multiple risk for model combinations k,l
(π_{kj} may be replaced by $\pi_k + \pi_j + \sigma + \pi_1 \cdot (t_k + t_j)$)
ß = interaction (day/night x multiple/single) for all model year combinations
(ß may be replaced $ß_o + ß_1 \cdot (t_k + t_j)$)
δ_1 = DRL-effect(1) for model combinations k,j
with k > 2 and j < 3
(one vehicle > '89, one vehicle <'90)
δ_2 = DRL-effect(2) for model combinations
with k > 2 and j > 2 (both vehicles > '89)
(it may be that $\delta_2 = \delta_1$)

If π_{kj} is replaced by $\pi_k + \pi_j + \sigma$, where for k=j σ=0 else σ=ln(2), while also a log-linear age effect for parameter $ß_1$ is included (the trend parameters for t_k and t_j must be identical because of the symmetric identity of $MD_{kj} = MD_{jk}$ and $MN_{kj} = MN_{jk}$), and denoting $t_k + t_j$ by t_{kj} the expression for $Est(MD_{kj})$, becomes:

$$\exp[\mu_k + \mu_j + \tau_k + \tau_j + \pi_k + \pi_j + \sigma + ß_o + ß_1 \cdot t_{kj} + \delta_1 + \delta_2]$$

or in its form with all possible trends one obtains:

$$\exp[\mu_k + \mu_j + 2 \cdot \tau_0 + \tau \cdot t_{kj} + \pi_k + \pi_j + \sigma + \pi_1 \cdot t_{kj} + ß_o + ß_1 \cdot t_{kj} + \delta_1 + \delta_2]$$

Here the possible log-linear effects of model age on the MD-involvement is represented by $ß_1 \cdot t_{kj}$ for model years combinations k and j. If the first order interaction effects of log-linear model age x multiple/single, expressed by in $\pi_k + \pi_j + \sigma + \pi_1 \cdot t_{kj}$, are used (both in MN- and MD-accidents of combinations k,j) then these log-linear effects and even log-polynomial ones and

σ are absorbed by π_{kj} in the model. In the same way the trend terms $2 \cdot \tau_o + \tau_1 \cdot t_{kj}$ and also log-polynomial trends are absorbed by the original parameters $\tau_k + \tau_j$. In view of the uncertainty of log-linearity of trends, the most appropriate model seems:

$$Est(SN_k) = \exp[2 \cdot \mu_k]$$

$$Est(SD_k) = \exp[2 \cdot \mu_k + 2 \cdot \tau_k]$$

$$Est(MN_{kj}) = \exp[\mu_k + \mu_j + \pi_{kj}]$$

$$Est(MD_{kj}) = \exp[\mu_k + \mu_j + \pi_{kj} + \tau_k + \tau_j + ß_o + ß_1 \cdot t_{kj} + \delta_1 + \delta_2]$$

It will be noted that in this and all other formulated models:

$$[Est(MD_{kj})/Est(MN_{kj})]/\{[Est(\surd SD_k).Est(\surd SD_j)]/[Est(\surd SN_k).Est(\surd SN_j)]\}$$

$$= \exp[ß_o + ß_1 \cdot t_{kj} + \delta_1 + \delta_2]$$

where the dummy variables for δ_1 and δ_2 are zero for $k > 1$ and $j > 1$. The terms $ß_o$ and $ß_1$ are not DRL-relevant and raw DRL-effect (1) = 1 - $\exp(\delta_1)$ as well as raw DRL-effect(2) = 1 - $\exp(\delta_2)$.

The latter log-linear model contains main effects for:
- model years or combinations of model years;
- and implicitly for - day/night and for - multiple/single and first order interaction effects for;
- combinations of model years with multiple/single;
- model years with day/night;
- multiple/single with day/night (=constant relative MD-involvement) as well as second order interactions as;
- log-linear trend effect in MD-involvement with age of model years;
- DRL-effect(1) for vehicles of model years ‘90 and ‘91 in MD-accidents; with vehicles older than model year ‘90;
- DRL-effect(2) for vehicles of model years ‘90 and ‘91 in MD-accidents with vehicles of the same model years ‘90 or ‘91.

The Canadian data make it possible to test conjecture 2 (virtually equal DRL-effects for MD-accidents with one or two DRL-using vehicles). It is assumed that the latter raw DRL-effect(2) is only marginally different from raw DRL-effect(1) and that their intrinsic DRL-effects are equal. If different it must be an intrinsic DRL-effect(2) that reduces MD-accidents additionally above the raw DRL-effect(1) by a higher intrinsic DRL-effect on encounters with two DRL-users than with one DRL-user.

The analysis fits not well for the model with for the parameters μ_k, μ_j, τ_k, τ_j, π_k, π_j, σ, $ß_o$ without trend parameters. With trend parameters π_1 and $ß_1$ the model also fits not well. Without trend parameter $ß_1$ for $Est(MD_k)$ and with π_{kj} the fit is rather good fit (overdispersion = 1.363). In that model without the $ß_1$ parameter, the δ estimates and their standard deviations are:

$\delta_1 = -.069$ and $\sigma(\delta_1) = .022$

$\delta_2 = -.095$ and $\sigma(\delta_2) = .058$

The model with the $ß_1$ parameter for a trend in the MD-involvement showed a negative $ß_1$ parameter, as it expected for an MD-overinvolvement for later model years observed in the odds ratio analysis of model pairs. Although its reduction effect is only a half percent per added model year, it is a just significant effect (one-sided significance p = .036). The δ parameter values and their standard deviation then become:

$\delta_1 = -.100$ and $\sigma(\delta_1) = .027$

$\delta_2 = -.157$ and $\sigma(\delta_2) = .064$

The value of δ_2 decreased when the mean age of model older than '87 was increased, which change did hardly effect the significance and values of δ_1 and the ß-parameter for the trend of MD-involvement. The optimal fit for the total design is found for t=9 for model years older than '87 and, therefore, that solution of parameters is taken. Parameter δ_2 is insignificantly different from δ_1, although in the expected direction of a larger DRL-effect(2). It may indicate that the change from 28.2.4% to 93.7% two DRL-using vehicles in the MD-accidents has a higher raw DRL-effect, since raw DRL-effect(1) = 9.5% versus raw DRL-effect(2) = 14.1% (notice that these estimates are similar to the estimates from the proper odds ratio comparison). Since there also is a small increase in one plus two DRL-using vehicles the actual intrinsic difference in the DRL-effects will be more insignificant. The intrinsic DRL-effects for δ_1 and δ_2 by expression (3) are 17.5% and 24.5% and have the following intrinsic parameter values and standard deviations:

$\delta_{i1} = -.192$ and $\sigma(\delta_{i1}) = .052$

$\delta_{i2} = -.281$ and $\sigma(\delta_{i2}) = .116$

The difference between the intrinsic DRL-parameters indeed is even more insignificant. So the crucial conjecture 2 for the applied corrections to intrinsic DRL-effects is confirmed or at least not rejected by the Canadian data. Since there is no significant difference between δ_{i2} and δ_{i1} the two parameters can be merged into one δ parameter (by weights corresponding to the inverse error variances). It yields:

$\delta_i = -0.207$ and $\sigma(\delta_i) = 0.058$ with df=19

This intrinsic DRL-parameter applies to the selected MD-accidents, without the same direction MD-accidents. Same direction MD-accidents have a share of 46% in all MD-accidents and applying half the DRL-effect for these same directions MD-accidents, obtained from Sparks et al. (1993), one estimates by the factor .77 on the DRL-effect for selected MD-accidents the *comparable national intrinsic DRL-effect on all MD-accidents* as:

$\delta_i = -0.155$ and **$\sigma(\delta_i) = 0.045$** with **df=18**

The intrinsic DRL-effect on all MD-accidents (thus inclusive the same direction accidents) for this study is 14.3% and with 18 degrees of freedom the one-sided t-test value .155/.045 = 3.44 gives a significance level of p = .002. The 90% confidence interval ranges from 7.4% to 20.8%.

So also these data from Canada violates *hypothesis 1* (no DRL-effect). Further more the design of the Canadian data has given the unique opportunity for a test of conjecture 2. Moreover, *conjecture 2* (equivalence of DRL-effect in encounters with one and with two DRL-users) is not falsified. Nothing else is relevant for other conjectures or hypotheses.

4.2.5. *Combined evidence and estimates for a DRL-effect in Canada*

As for the USA one now can combine the estimates of the DRL-effect in Canada from all four studies into one weighted estimate, assuming that fleet studies have no bias (hypothesising the acceptance of conjecture 1). The next table summarizes the results and weights used for the combined estimate, where the two first studies are additionally weighted by a factor of a half for their questionable validity (due to weak designs and/or an estimation of the standard deviation that is based on assumptions).

Study year	DRL-effect parameter	Inverse variance	Validity factor	Obtained weight	Prop. weight	Degrees freedom
1964	.336	68.3	½	34.3	.065	0
1981	.223	8.7	½	4.3	.008	0
1993	.240	268.7	1	268.7	.511	18
1994	.155	218.9	1	218.9	.416	18

Table 8. *Summary of results and weights for the DRL-effect in Canada*.

The combined estimate, its standard deviation and its df, according to expressions (6a) to (6h) of chapter 3, become

$\delta_i = -0.211$ and $\sigma(\delta_i) = 0.048$ with df=39

With df=39 its one-sided t-test .211/.048 = 4.40 gives a significance level of $p < .001$. The intrinsic DRL-effect on all MD-accidents in Canada, assuming no bias in the fleet results, is 19.0%. Its 90% confidence interval ranges from 12.3% to 25.3%. However, because the national DRL-evaluation gives the lowest separate DRL-effect, it may be that there is a bias in the fleet DRL-effects. By the same method the DRL-effects from the fleets in Canada can be combined to one estimate. It yields the following *fleet DRL-parameter on all MD-accidents*, its standard deviation and its df by the same expressions of (6a) to (6h) of chapter 3 as:

$\boldsymbol{\delta_{if} = -0.251}$ and $\boldsymbol{\sigma(\delta_{if}) = 0.051}$ with **df=19**

With df=19 its one-sided t-test .251/.051 = 4.92 gives a significance level of $p < .001$. The intrinsic DRL-effect on fleet MD-accidents in Canada is 22.2%. Its 90% confidence interval ranges from 15.0% to 28.8%.
The difference of this fleet DRL-parameter and the national DRL-parameter for Canada gives a t-test value of $.096/\sqrt{(.045^2+.051^2)} = 1.41$ and thus has a

just not significance level of p = 0.08. Nonetheless, it indicates that the national intrinsic DRL-effect of 14.3% may be 8.5% lower than the fleet DRL-effect (a DRL-parameter that is 0.096 lower than the fleet DRL-parameter or a factor .644 lower DRL-effect than the fleet DRL-effect).

If one applies the factor 1.74 for the significant higher DRL-effect on casualties in fleet MD-accidents (Sparks et al., 1993), to the above estimate it gives a Canadian *fleet DRL-parameter on casualties in MD-accidents* as:

$\boldsymbol{\delta_{if} = -0.488}$ and $\boldsymbol{\sigma(\delta_{if}) = 0.099}$ with **df=18**

It means a reduction of 38.6% for casualties in fleet MD-accidents by DRL. The same correction applied to the national Canadian DRL-effect on all MD-accidents of 14.3%, yields a national *intrinsic DRL-parameter on casualties in MD-accidents* of:

$\boldsymbol{\delta_{in} = -0.287}$ and $\boldsymbol{\sigma(\delta_{in}) = 0.083}$ with **df=18**

In conclusion the DRL-studies in Canada show a high significance and thus violate *hypothesis 1* (no DRL-effect). Also the indication that the DRL-effect on rear-end MD-accidents may be a quarter of the DRL-effect on all other MD-accidents, contributes to the rejection of *conjecture 6*. Moreover, the nearly significant difference between the national and fleet-owner DRL-effect is a firm indication for the rejection of *conjecture 1* (no fleet bias), while the very insignificant difference between DRL-effects on encounters with one and with two DRL-users may lead to the acceptance of *conjecture 2* (negligible different DRL-effects). Furthermore the significant difference between the DRL-effect on casualties in MD-accidents and on all MD-accidents contributes to the rejection of *conjecture 7*. Nothing else of the Canadian studies contribute to other conjectures or hypotheses.

4.3. DRL-evaluation in Finland

Finland is the country with the first national obligation for DRL-use. It was introduced as an obligation for wintertime on roads outside built-up areas in november 1972. The DRL-effect has been evaluated by Andersson et al. (1976) by a before/after quasi-experimental design with the casualties in MD-accidents as experimental group data and the casualties in SD-, MN- and SN-accidents as quasi control group data all for roads outside built-up areas in the winter period from november to march inclusive. The casualty data are assembled for the years 1968 to 1974, subdivided in three groups of 1968/1969 plus 1969/1970, 1970/1971 plus 1971/1972, and 1972/1973 plus 1973/1974. In the first two winters there was no recommendation or obligation to use DRL (before-periods), in the second two winters the use of DRL was recommended (recommendation periods) and in the third two winters DRL-usage was compulsory (obligation periods), all with respect to roads outside built-up areas. In the before-periods the DRL-usage was about 65%, in the recommendation periods about 87% and in the obligation period about 97%. The casualties MD-accidents from before to obligation periods decreased by 27%, while the casualties in MN-, SN- and SD-accidents increased by respectively 17%, 12% and 6%. When the casualties in MD-accidents between motor vehicles are compared with pedestrian casualties in MD-accidents it shows a larger reduction for pedestrians. The authors used the odds ratio method and concluded that the raw DRL-effect is 15.5%

between before and recommendation periods and 6.3% between recommendation and obligation periods or 20.8% between before and obligation periods.

The raw DRL-effect on casualties in MD-accidents between motor vehicles only for the before and obligation periods is 11.4%. The ratio of MD/MN-casualties of pedestrians divided by the same ratio for all other casualties decreased by 18%. Although the authors state, based on the 18% decrease, that the DRL-effect for pedestrians is virtually the same as the overall DRL-effect, it actually means that the pedestrian casualties in MD-accidents reduced 18% more than for other casualties in MD-accidents. The odds ratio for pedestrians decreased by 34%, which should lead to the conclusion that the raw DRL-effect for pedestrians is about 34%. An even larger raw DRL-effect is found for casualties in MD-accidents with other non-motorised road users (mainly elks on Finnish rural roads in the winter). The separation into rear-end, crossing, and head-on accidents of the motor vehicle accidents indicates that casualties in rear-end accidents increased by 9% (but for the same years in summer time by 40%), those in crossing accidents decreased by 17% and in head-on accidents also by 28%. The authors do not correct the raw DRL-effect to an intrinsic DRL-effect, however by expression (1) the intrinsic DRL-effect for pedestrian casualties in MD-accidents is 62% (raw effect 34%) and by expression (2) for casualties in MD-accidents between motor vehicles it becomes 51% (raw effect 11.4%).

Due to the odds ratio method the authors can not give explicit estimates of standard deviations of DRL-effects, nor estimates of the error variance, but the Chi-square test showed that the DRL-effect may be significant. The report gives the separate data per winter in each period and separated for accidents between motor vehicles and pedestrians and accidents between motor vehicles and other non-motorised road users. This makes it possible to analyse the casualty data by the log-linear model, where in this case one does not have a single DRL intervention. The joint analysis of all three periods must be based on either two (0,1)-dummy variable for the DRL-effects of before versus recommended and of before versus obligation periods or on regression variable(s) v_t for the DRL-parameter. The regression variable v_t then represents the level of relevant DRL-usage according to expression (5). The latter model is preferred because it leaves more degrees of freedom and gives directly the intrinsic DRL-effect(s).

The first applied re-analysis uses the total casualties for each of the two winters within the periods as a re-analysis comparable to the odds ratio method used by the authors (main effects for winters, multiple/single, day/night, their first order interactions and the second order interaction with before-recommendation periods and before-obligation periods as two DRL-effects). The interaction between winters and day/night is not significant and is omitted from the design. This analysis with two winters within before- and after-periods and no interaction between winters and day/night leaves more degrees of freedom for statistical testing. In this model the intrinsic DRL-effect parameters and their standard deviations are:

before versus recommendation periods:

$\delta = -.143$ and $\sigma(\delta) = 0.041$

before versus obligation periods:

$\delta = -.295$ and $\sigma(\delta) = 0.043$

The corresponding raw DRL-effects are 13.3% and 25.5%. With df=8 their one-sided t-test values of .143/.041 = 3.48 and .295./.043 = 6.86 are significant with levels of p = .005 and p < .0001. These DRL-effect percentages differ from the ones obtained by the authors from the ratio of odds ratios for the combined winters per period, being 15.5% and 20.8%. If the (insignificant) interaction between winters and day/night is included the raw DRL-effects are similar to the ones found by the authors, but due to the loss of degrees of freedom to df=3, they are respectively not and just significant. For this mixture of total MD-accidents no exact correction to an intrinsic DRL-effect exists, but using the mean of the result from expression (1) and (2), it gives intrinsic DRL-effects of 43.3% and 63.1%.

The second re-analysis uses simultaneously the separate data for casualties in daytime accidents between motor vehicles, in daytime accidents with pedestrians and in daytime accidents with other non-motorised road users for each of the two winters in the before, recommendation, and obligation periods. Without the again insignificant interaction between winters and day/night it has the advantage that more degrees of freedom are left for a more optimal estimation of the error variance and thus can reliably estimate the possible different DRL-effects on MD-accidents between motor vehicles, on MD-accidents with pedestrians and MD-accidents with other non-motorised road users. The DRL-effect results are for:

a. *Casualties in daytime accidents between motor vehicles*

before versus recommendation periods:
$\delta = -.101$ and $\sigma(\delta) = 0.059$

before versus obligation periods:
$\delta = -.183$ and $\sigma(\delta) = 0.061$

The raw DRL-effect on MD-accidents between motor vehicles is insignificant between the before and recommendation periods, but between the before and obligation periods with the larger increase in DRL-level it is significant. Corrected by the relevant average proportions of DRL-usage for the recommendation and obligation periods in expression (2) the two intrinsic DRL-effect parameters are -.705 and -1.047, but their difference is not significant with their respective standard deviations of .412 and .349. Combining these intrinsic DRL-effect to a weighted mean intrinsic DRL-effect on MD-accidents between motor vehicles (weights are inverse variances) and its standard deviation (estimated by their weighted error variances with 50% covariance and the deviations of the two DRL-effects) are:

$\delta_i = -0.903$ and $\sigma(\delta_i) = 0.339$

The one-sided t-test value .903/.339 = 2.66 with df=13 (twice df=8 minus df=4 for shared before-periods plus df=1 for one instead of two DRL-parameters) gives a significance level of $p = .01$ for the intrinsic DRL-effect of 59.6%. Its 90% confidence interval ranges from 26.1% to 77.8%.

b. Pedestrian casualties in MD-accidents

before versus recommendation periods:
$\delta = -.415$ and $\sigma(\delta) = 0.086$

before versus obligation periods:
$\delta = -.473$ and $\sigma(\delta) = 0.088$

The DRL-effect for each comparison period is significant. Corrected for their respective intrinsic DRL-effects by expression (1) the intrinsic DRL-effect parameters become -1.473 and -1.097, but again their difference is not significant with standard deviations of .305 and .204. The combined weighted mean intrinsic DRL-effect for pedestrian casualties in MD-accidents and its standard deviation (estimated in the same way) are:

$\delta_i = -1.212$ and $\sigma(\delta_i) = 0.345$

The one-sided t-test value 1.212/.345 = 3.51 with df=13 yields a significance level of $p < .005$ for the intrinsic DRL-effect of 70.2%. Its 90% confidence interval ranges from 45.2% to 83.8%

c. Casualties in MD-accidents with other non-motorised road users

before versus recommendation periods:
$\delta = -.199$ and $\sigma(\delta) = 0.150$

before versus obligation periods:
$\delta = -.817$ and $\sigma(\delta) = 0.174$

Here the DRL-effect for the first comparison period is not significant, but the DRL-effect for the second comparison is. When their respective raw DRL-effects are corrected by expression (1) the intrinsic DRL-effects become -.763 and -1.701, but with their standard deviations of respectively .575 and .362 the difference is not significant. The weighted mean intrinsic DRL-effect on casualties in MD-accidents with other non-motorised road users and its tentatively estimated standard deviation are:

$\delta_i = -1.435$ and $\sigma(\delta_i) = 0.827$

The one-sided t-test value 1.435/.827 = 1.73 with df=13 would just be not significant, since the significance level is $p = .055$. The intrinsic DRL-effect of 76.2% has a 90% confidence interval of -2.9% to 94.5%

From the confidence intervals it follows that the three intrinsic DRL-effects on casualties in MD-accidents are not significantly different. The intrinsic DRL-effect on casualties in MD-accidents with other non-motorised road users are mainly casualties in MD-accidents caused by animals such as elks. Since in Nordic parts of Scandinavia it is a substantial cause of road casualties the preventive effect of DRL on such accidents, is an important safety aspect in these Nordic regions. However, it contributes not to a valid generalisation of DRL-effects for more southern countries, although it is not significantly lower than for pedestrians. The high intrinsic DRL-effect in Finland for pedestrian casualties means that without DRL the number of pedestrian casualties in MD-accidents will be more than 3 times higher. Instead of the observed 273 pedestrian casualties there would be about 900 of such casualties without any use of DRL in the obligation period. This may seem suspect, but with 65% DRL-usage in the before-period the pedestrian casualties in MD-accidents amount to 401 and with 97% DRL-usage in the obligation period it reduced to 273, while the pedestrian casualties in MN-accidents increased from 518 to 565.

The preferred re-analysis is based on main effects for each successive winter, for multiple vehicle/multiple pedestrian/ multiple other versus single, for day versus night and on their first order interactions as well as on the DRL-effects as the third order interaction of multiple vehicle/ multiple pedestrian/ multiple other versus single x day versus night x relevant regression variable v_t. It gives separate intrinsic DRL-effects for motor vehicle drivers, for pedestrians and for other motorised road users by the model fit of three DRL-parameters each with their corresponding regression variables v_t for the relevant DRL-usage proportions.

For the computation of the regression variables v_t the relevant proportions (p_t) of DRL-usage in the successive periods for accidents of motor vehicles with pedestrians and with other road users are $p_1 = p_2 = .65$, $p_3 = p_4 = .87$ and $p_5 = p_6 = .97$ for the winters in the before, recommendation and obligation periods, while for multiple accidents between motor vehicles the relevant proportions for their encounters with one and two DRL-users become $p_1 = p_2 = .88$, $p_3 = p_4 = .97$ and $p_5 = p_6 = 1.0$. Furthermore, the variable v_t depends also on the intrinsic DRL-effect(s) and thus must be iteratively obtained. The values for the three variables v_t converged sufficiently by iteratively changing the v_t values for solved δ_i values from the log-linear analyses with successively revised v_t values.

It yields the next v_t values:
- motor vehicles $v_1=v_2 = .797$, $v_3=v_4 = .969$, $v_5=v_6 = .998$
- pedestrians $v_1=v_2 = .520$, $v_3=v_4 = .792$, $v_5=v_6 = .948$
- other non-motor. road users $v_1=v_2 = .469$, $v_3=v_4 = .751$, $v_5=v_6 = .934$

The δ_i parameters and their estimated standard deviations are for:

casualties in MD-accidents between motor vehicles

$\delta_i = -1.142$ and $\sigma(\delta_i) = 0.645$

pedestrian casualties in MD-accidents

$\delta_i = -1.070$ and $\sigma(\delta_i) = 0.531$

casualties in MD-accidents with other non-motorised road users

$\delta_i = -1.499$ and $\sigma(\delta_i) = 0.448$

Since here df=12, the 90% confidence interval is obtained from the one-sided t-test value at p =.05 by adding 1.78 times the standard deviation to the parameter value. Thus all three intrinsic DRL-effects (respectively 68.1%, 65.7% and 77.7%) are significant, but their differences not. For thus justified solution with one identical DRL-parameter for each of the casualties in MD-accident types the v_t values become:

- motor vehicles $v_1=v_2 = .782$, $v_3=v_4 = .966$, $v_5=v_6 = .998$
- pedestrians $v_1=v_2 = .492$, $v_3=v_4 = .771$, $v_5=v_6 = .941$
- other non-motor. road users $v_1=v_2 = .462$, $v_3=v_4 = .771$, $v_5=v_6 = .941$

The results for that one δ_i parameter and its standard deviation are:

$\delta_i = -1.304$ and $\sigma(\delta_i) = 0.387$

The fit of the models with v_t for simultaneous analysed injury data of each type of multiple accidents is rather unacceptable (overdispersion 3.6). However, the single parameter solution for the analysis of the sum of all types of multiple accidents with a weighted mean of the above v_t values gives an excellent model fit with about the same solution for δ_i. This better model fit can only mean that the error variances of different accident types are negatively correlated. This indeed may be the case if in mild winters the exposure of non-motorised road users is relatively high and the casualty risk for motor vehicle drivers relatively low and if the reverse holds for inclement winters.

Therefore, it seems more justified to analyse the intrinsic DRL-effects by the model for pedestrians and motor vehicles separately with different v_t values for pedestrians and for motor vehicles. These model separate analyses indeed yield acceptable levels of overdispersion. For accidents with pedestrians the fit of the model shows a residual $(Chi)^2$-value of 3.28 for df=4.
Its converged v_t values are $v_1=v_2 = .525$, $v_3=v_4 = .795$, $v_5=v_6 = .949$ and the *DRL-effect on the pedestrian casualties in MD-accidents* and its standard deviation are:

$\boldsymbol{\delta_{ip} = -1.034}$ and $\boldsymbol{\sigma(\delta_{ip}) = 0.252}$

Thus the intrinsic DRL-effect for pedestrians is 64.4% with df=4 and a 90% confidence interval ranging from 39.2% to 79.2%.

For casualties in MD-accidents between motor vehicles the fit of the model also is excellent (overdispersion = 1.056 with df=4). Its converged values for v_t are $v_1=v_2 = .800$, $v_3=v_4 = .983$, $v_5=v_6 = .999$ and the national *DRL-effect on the casualties in MD-accidents between motor vehicles* and its standard deviation are:

$\boldsymbol{\delta_{in} = -1.100}$ and $\boldsymbol{\sigma(\delta_{in}) = 0.358}$ with **df=4**

It means an intrinsic DRL-effect of 66.7% and its 90% confidence interval ranges between 28.6% to 84.4%.

In conclusion the evaluation of casualties on roads outside built-up areas in the winters of Finland very significantly rejects *hypothesis 1* (no DRL-effect). The effect for pedestrian casualties MD-accidents and the effect on casualties in MD-accidents between motor vehicles are almost identical. It leads to the acceptance of *conjecture 3* (equal DRL-effects).

4.4. DRL-evaluation in Sweden

The study of the national effect of the DRL-obligation per oct. 1977 in Sweden by Anderson & Nilsson (1981) is the most disputed study of all DRL-effect studies (Koornstra, 1993, Theeuwes & Riemersma, 1995, Williams & Farmer, 1996). The study concerns a two year (Oct. to Sept.) before an after comparison in a *before/after quasi experimental design*, where the authors compare several subsets of casualties in MD- and MN-accidents with the same total casualties in SD- and SN-accidents by the multiplicative effect specification for the odds ratios. In the analysis uses the ratio of the odds ratios of the first before year t = -2 (not using the data of the last before year t = -1) and the first after year t = 0 (not using the last after year t = 1). The casualties in dusk and dawn are included in the night data, which probably influences the DRL-effect compared to the case where they were omitted. The significance of the δ parameter, derived from Ra/Rb for a = (t=0) and b = (t=-2) as second order interaction between before/after x multiple/night x day/night, is tested by the (Chi)²-value with df=1 for the observed accidents and the expected accidents predicted by main effects for year (t=-2 and t=0), day/night and multiple/single as well as first order interaction effects for year x single/multiple, year x day/night and multiple/single x day/night and thus predicted without DRL-effect δ. Since with that effect the predictions are equal to the observations, it will be clear that this test of the δ parameter is with respect to the theoretically expected (Chi)²-value from a Poisson distribution of the data, while an empirical error variance generally will show an overdispersion. Thus this test has the same shortcoming as the Mantel-Haenszel test of odds ratios.

The next raw DRL-effects on casualties in MD-accidents are reported:

Subset	Summer		Winter		Sum of DRL-effect
	Built-up		Built-up		
	inside	outside	inside	outside	
Opposing	13%	8%	8%	11%*	10%
Crossing	12%	-25%	13%	15%	9%
Coincident	2%	-4%	-6%	16%	2%
Cyclist	25%*	19%	10%	18%	21%
Pedestrians	27%*	-7%	7%	9%	17%
All	15%	5%	7%	11%	11%
Negative figures are adverse DRL-effects and figures with * are (Chi)²-significant at $p < .05$.					

Table 9. *Raw DRL-effects on casualties in MD-accidents.*

Although the total DRL-effect of 11% is not significant, the authors conclude that the pattern of effects is according to their expectations. The increase in DRL-usage is larger in summer and built-up areas and thus there the largest raw DRL-effects ought to be observed, while also larger DRL-effects are expected for casualties in crossing and opposing MD-accidents. The authors do not correct to intrinsic DRL-effects, but if correctly performed for the relevant increases in DRL-usage it would mean a correction for the average before DRL-level of 50% (for the summer lower and for the winter higher and lower percentages in built-up and higher percentages outside built-up areas, see corollary 3). The relevant change of DRL-usage in the winter for encounters between motor vehicles with one and two DRL-using motor vehicles is from 91% to virtually 100%, which increase of 9% can hardly cause a large raw DRL-effect for casualties in MD-accidents between motor vehicles. In the summer this relevant increase for motor vehicles is 51%. For the non-motorised road users the increase of DRL-usage is from 70% to about 98% in the winter (an increase of 28%), while the relevant increase in DRL-usage for their MD-accidents in the summer is from 30% to about 97% (an increase of 67%). The relevant increase in DRL-usage for pedestrians and cyclists in built-up areas in the summer will be the largest off all subgroups, probably form 26% to 97%, thus about 71% increase. If there exists a DRL-effect than it should be the highest for pedestrians and cyclist in built-up areas during the summer, which actually is the case. So indeed, the pattern of DRL-effects indicate a genuine DRL-effect.

One may take the mean raw DRL-effect for pedestrians and cyclist in built-up areas as the DRL-effect in the Swedish summer, because it can hardly be reliably observed in other subdivisions of accidents or the total accidents. This raw DRL-effect of 26% corrected for its relevant before DRL-level of 26% (as relevant before DRL-level in built-up areas during the summer) and after DRL-level of 97%, gives an intrinsic DRL-effect of 32.2% for the summer of Sweden. The comparable mean raw DRL-effect on casualties in crossing and opposite MD-accidents between motor vehicles on roads in built-up areas during the summer is about 12.5% and its correction to an intrinsic DRL-effect (by 45% and virtually 100% as the relevant before and after DRL-level for one and two DRL-using motor vehicles in built-up areas during the summer) gives 20.6%. It suggests that non-motorised road users may benefit more from DRL than drivers.

The authors also did not utilize all the available data of separate before and after years. In their additional analysis for the sum of injury accidents in before and after years by the odds ratio leads to an insignificant 8% raw DRL-effect. However, in a log-linear analysis of Theeuwes & Riemersma (1995) for the accident data per year in each of the two years in the before and after-periods with the usual design (main effects, first order interactions and the DRL-effect as second order interaction between multiple/single, day/night and before/after) also yields no significance for the DRL-effect ($p = .25$). The same analysis repeated reveals that the fit of the model is unacceptably bad (overdispersion by a factor of 4.3), which indicate that there are quite large unexplained annual variations. Already Theeuwes and Riemersma noted that the casualties in SD- and SN-accidents fluctuate per year within the before and after-period with respect to the casualties in multiple accidents, while the night casualties in the after-period are as much reduced as the casualties in MD-accidents by the estimated DRL-effect.

Probably the difference between mild and inclement winters (winters without or with much snow and ice on the roads) cause a rather high error variance for the model analysis, which might be absent for a model analysis of summer casualties only. Moreover, it is incorrect to analyse the casualty data without differentiation between seasons and/or road user types, because due to their different increases in DRL-usage there is no constant DRL-effect in the aggregated data per year.

Fortunately the authors present the casualties in MD- and MN-accidents between motor vehicles and their SD- and SN-accidents for each month in the two before and after years. This makes it possible to analyse these monthly data in a more sensitive way per seasonal grouping of month, whereby also the standard deviation of the DRL-parameters per seasonal period can be obtained. However, the monthly pedestrian and cyclist casualties in MD- and MN-accidents are not given and thus for non-motorised road users such a more sensitive reanalysis is not possible.

The before DRL-usage in the months April to September or may to august (the months around the longest daylight day) can be regarded as equally low at a level of 35% to 25% (30% to 20% in bright summer weather is reported) as well as equally high at a level of about 70% in the months November to February (the months around the shortest day, for which it is reported that the DRL-level is 60% to 70% in bright winter weather).
For the intermediate months of April, or March and April, as well as October, or September and October, the before DRL-level will be varying around the 50% to 55%, since the mean before DRL-level is reported as 50% to 55%. It also can be expected that the casualty data fluctuate more in intermediate period and the winter than in the summer, due to more varying weather conditions (especially with varying impacts on casualties from single accidents in the winter month) and more varying DRL-usage in intermediate periods (especially with fluctuating impacts on the DRL-usage and on the casualties in MD-accidents in the intermediate and winter months). It therefore is preferred to reanalyse the Swedish data by a log-linear analysis of the monthly data for each of three groups of months, where the months that are included in the summer period are determined by the grouping of summer months that gives the smallest residual variance. For a fixed winter period of November to february, the months for an intermediate period are determined by the grouping of the summer months. If also no significant DRL-effect for that summer period (high increase in DRL-usage and the smallest error) is found, then the Swedish data indeed indicate that there is no DRL-effect.

Analysing the monthly data per summer, intermediate and winter period one has m times 4 data (m months and 4 years) for 4 casualty types, giving $16 \cdot m$ observations. In the design with main effects for all separate months, multiple/single and day/night and their first order interactions as well as the second order interaction of (before/after) x (multiple/single) x (day/night) as the DRL-effect for the seasonal period, one has $4 \cdot m + 2$ main effect parameters, $2 \cdot (4 \cdot m - 1) + 1 = 8 \cdot m - 1$ first order interaction parameters and 1 DRL-effect parameter, leaving $4.m - 2$ degrees of freedom for the error variance. Given that m is 3 to 5 it means relative large numbers of degrees of freedom for the error variance for which it is expected that the summer DRL-effect may be significant, because the error variance for the selected summer months is expected to be low, while the relevant before DRL-level in the summer is 57% to 44% (for encounters between one and two DRL-using motor vehicles) with a corresponding after DRL-level of almost 100%. The winter DRL-effect is expected to be less or not significant, because the relevant before DRL-level is 91% and the error variance is expected to be high (due to mild and harsh winters with more varying numbers for single accidents). The intermediate DRL-effect probably also is less or not significant, because here the relevant before DRL-level is 77%, while its error variance is expected to the highest due to varying weather conditions in these months.

As expected the fit of the model gives an insignificant residual $(Chi)^2$-value for the different summer periods (thus an acceptable low error variance) and significant residual $(Chi)^2$-values for correspondingly differing intermediate periods and the fixed winter period, where the fit for the differing intermediate periods was worse than for the fixed winter period. The best and nearly equally well fitting summer periods are the ones from May to August inclusive (with residual $(Chi)^2=15.4$ for df=14) and from May to September inclusive (with residual $(Chi)^2=18.9$ for df=18). Somewhat less and nearly equally fitting are the summer periods from April to August inclusive or to September inclusive (respective residual $(Chi)^2$-values of 21.6 for df= 18 and 27.2 for df=22). Both summer periods that include September give the highest (and nearly equal) significance for the estimated DRL-effect. Since the overdispersion for the summer period is the lowest for May to September inclusive, it seems best to report the results for that summer period and the winter period of November to February inclusive [residual $(Chi)^2=42.3$ for df=14], without paying much attention to the rather unreliable results for the intermediate months of March, April, and October [residual $(Chi)^2=59.9$ for df=10].

The obtained raw DRL-parameters and their standard deviations for casualties in MD-accidents between motor vehicles by these log-linear analyses for the separate summer, winter and intermediate periods are:

summer (may - september)

$\delta = -.180$ and $\sigma(\delta) = 0.077$

winter (november - february)

$\delta = -.263$ and $\sigma(\delta) = 0.123$

intermediate (march, april, october)

$\delta = -.094$ and $\sigma(\delta) = .211$

It may be interesting to notice that $\delta = -.2154$ for May to July inclusive and that $\delta = -.204$ for October to March inclusive. Together with the above results it illustrates the counteracting effects from a large increase of DRL-usage with a low intrinsic DRL-effect in the mid-summer and from a small increase of DRL-usage with a high intrinsic DRL-effect in the mid-winter. One obtains by a before DRL-level in the summer (May-Sept.) of 32.5% and in the four month winter (Nov.-Feb.) of 70% as well as in the three intermediate months (March, April and October) of 52.5% in the correction expression (2) for raw DRL-effects the following the intrinsic DRL-effect parameters for the reduction of national casualties in MD-accidents and their standard deviations:

summer (May to September)

$\boldsymbol{\delta_{ins} = -0.360}$ and $\boldsymbol{\sigma(\delta_{ins}) = 0.154}$ with **df=18**

winter (November to February)

$\boldsymbol{\delta_{inw} = -1.464}$ and $\boldsymbol{\sigma(\delta_{inw}) = 0.685}$ with **df=14**

intermediate (March, April, October)

$\boldsymbol{\delta_{ini} = -0.304}$ and $\boldsymbol{\sigma(\delta_{ini}) = 0.682}$ with **df=10**

Although the model fit for the data of the four winter mouth is unacceptable, which results also in a relative high standard deviation for the DRL-parameter, its intrinsic DRL-effect parameter still is significant at $p = .025$ (one sided t-test value 1.464/.685 = 2.14 with df=14). It means an intrinsic DRL-effect of 76.9% for these four winter months with a 90% confidence interval of 22.8% to 93.1%, while the intrinsic DRL-effect on casualties in MD-accidents between vehicles in the five summer month of Sweden is 30.2%. Its one-sided t-test value .360/.154 = 2.34 with df=18 gives a significance of $p = .016$ and its 90% confidence interval ranges from 8.5% to 46.8%.

The difference between the intrinsic DRL-effects for these selected summer and winter periods is almost significant (one-sided t-test $p = .07$), while the difference between their variances is very significant (F-tests for their variances gives $p < .005$). The intrinsic DRL-effect for the intermediate months is close to the intrinsic DRL-effect for the summer month, but its standard deviation nearly equals the winter month and thus the intrinsic DRL-effect is not significant for the intermediate mouths. Combining 1/3 of the DRL-effect for the intermediate mouths with the DRL-effect for the selected summer period and 2/3 with the DRL-effect for the selected winter months, one obtains from these weighted effect percentages the DRL-parameters for the half year summer and winter as:

summer half year

$\delta_{ins} = -0.350$ and $\sigma(\delta_{ins}) = 0.223$

winter half year

$\delta_{inw} = -0.916$ and $\sigma(\delta_{inw}) = 0.734$

Clearly these intrinsic DRL-effects for the summer and winter half year are no longer significant, but most probable intrinsic DRL-effect for the winter half year becomes still 60%. This half year winter effect for Sweden is, as expected, close below the 68.1% that was found for the DRL-effect on casualties in MD-accidents between vehicles in the winter of Finland. The summer half year effect of 29.5% is not significantly lower than the winter half year effect (one-sided t-test $p = .15$), but again the variances of the effect in the winter half year is significantly higher than the variance of the summer half year effect (F-tests $p < .005$). Nevertheless the intrinsic DRL-effect for the whole year in Sweden can be estimated by the weighted average of estimated period DRL-effect percentages (weighted by before casualties in MD-accidents per season). It yields:

$$\delta_{in} = -0.569 \qquad \text{and} \qquad \sigma(\delta_{in}) = 0.523$$

Again no significant DRL-effect for the whole year is obtained. It clearly demonstrates that the insignificance of a whole year effect is mainly due to the high error variance for the intermediate months, the nearly significant differences between the intrinsic DRL-effects for the selected summer and winter periods and the different DRL-increases for these periods. The nearly significant difference between the intrinsic DRL-effects for the selected summer and winter periods and the different DRL-increases define two genuinely different raw DRL-effects, which should not be represented by one single raw DRL-effect for the whole year. The whole year intrinsic DRL-effect with nearly 30% summer-effect and 60% winter-effect actually is 43.4% in Sweden.

In conclusion the selected winter and summer periods prove that the DRL-effects for Sweden are significant, despite the critical comments by several authors who have concluded that a significant Swedish DRL-effect can not be demonstrated. It indeed can not be a significant effect from the annual data due to seasonally different DRL-usage levels and seasonally different intrinsic DRL-effects as well as due to annually varying weather conditions in the intermediate and mid-winter periods. The latter cause seasonally and annually DRL-irrelevant data fluctuations, especially for the winter casualties in SD- and SN-accidents and the former seasonal differences of DRL-reduced casualties in MD-accidents in the after-period. Therefore, models that estimate a fixed raw DRL-effect from totals per year give an unacceptable fit and thus a high error variance with no significance for a DRL-effect. However, for the five summer mouth the analysis gives a rather small error variance with a significant DRL-effect. Moreover, despite the high error variance for the four winter month, the higher intrinsic DRL-effect for these months is so much higher that it also is significant.

It is concluded that the data indeed significantly leads to rejection of *hypothesis 1* (no DRL-effect). The significant DRL-effect in the selected summer period also gives confidence with respect to the summer results in built-up areas that are reported for the non-motorised road users as 25% and 27% raw DRL-effects for cyclists and pedestrians respectively. Corrected by expression (1) it already is mentioned that intrinsic DRL-effect non-motorised road users in built-up areas during the summer may be 32.2%. Although 2.6% higher than for motorised road users on all roads in the summer, it certainly is not a significant difference, which says that *conjecture 3* (equal intrinsic DRL-effects for drivers and non-motorised road users) is not violated. The reported DRL-effects do indicate that *conjecture 6* (equal intrinsic DRL-effects for different types of accidents) may be invalidated, at least by the probably lower positive intrinsic DRL-effect on casualties in rear-end accidents. The nearly significant difference between the selected summer and winter periods contributes to the expected rejection of *conjecture 5* (no summer and winter difference).

4.5. DRL-evaluations in Norway

In Norway the voluntary usage of DRL has been increasing from 1% or 2% in the early seventies to about 30% in 1979. Due to the one hour switch of summer time in 1980 the level of DRL-usage in 1980 reduced somewhat to about 27%, but increased again from 35% in 1981 to 60% in 1984 and in 1985 the DRL-level was 66% (Vaaje, 1986; Statens Vegesen, 1987). From 1985 onward new motor vehicles were obligatory equipped with DRL that automatically switched on by ignition, while after early 1988 DRL became compulsory for all motor vehicles (Elvik, 1993; for motorcycles it was already obligatory since 1978).

Assuming that the new vehicles from model years 1985 to 1987, equipped with automatic DRL, consists of an additional 9% of all motor vehicles (replacing for a share of 9% partial DRL-usage by full DRL-usage), one can by interpolation estimate the total DRL-usage level from before 1975 and from 1980 to 1990. By using corollary 5 for voluntary DRL-usage one also can estimate the summer and winter DRL-usage for each of these years by interpolated percentages between percentages of years that are cited from the reports. These percentages are given in *Table 10* (bold percentages are given and the other ones estimated).

DRL	<‘75	80	81	82	83	84	85	86	87	88	89	90
Voluntary	**2**	27	**35**	45	53	**60**	58	56	54	7	0	0
Obligatory	0	0	0	0	0	0	8	17	26	90	98	99
Total	**2**	27	**35**	45	53	**60**	**66**	73	80	97	98	99
Summer	1	19	25	32	37	42	49	57	64	95	98	99
Winter	3	36	47	61	72	81	86	92	99	99	99	99

Table 10. *The percentages of DRL-usage for the years 1980-1990.*

The above percentages are the estimated percentages of motor vehicles with DRL and thus apply to encounters with one DRL-user, but for encounters between vehicles the percentages for one and two DRL-users are to be used and these latter percentages are given in the table below.

DRL	<‘75	80	81	82	83	84	85	86	87	88	89	90
Total	4	47	58	70	78	84	88	93	96	100		
Summer	2	34	44	53	60	66	74	82	87	100		
Winter	6	59	72	85	92	96	98	99	100	100		

Table 11. *Relevant percentages of one and two DRL-users.*

Vaaje (1986) compared the casualties for the two year periods of ‘80/’81 and ‘84/’85. Vaaje concludes from the comparison that all casualties in MD-accidents increased by 3% and that the single and night casualties increased by 17%, while the pedestrian casualties in MD-accidents decreased by 17% without an increase for the pedestrian casualties in MN-accidents. Vaaje states that the reduction effects of 14% and 17% are significant, but judged them too high for a DRL-effect from an increase DRL-usage between ‘80/’81 and ‘84/’85 of about 30%, although no other explaining factors could be found. Vaaje did not use the odds ratio analysis, but the relevant injury data for an odds ratio analysis are given in the report. For casualties in MD-accidents between motor vehicles the odds ratios for ‘80/’81 and ‘84/’85 are respectively 2.619 and 2.165, which gives a raw DRL-effect of 17.3% or corrected by expression (2) with the average percentages for the respective years in the above table an intrinsic DRL-effect on casualties in MD-accidents between motor vehicles of 40.6%. For pedestrian casualties in MD-accidents the respective odds ratios are 1.517 and 1.208, which gives a raw DRL-effect for pedestrians of 20.4%. Its intrinsic DRL-effect, using expression (1) with the corresponding average percentages of the first table, becomes 53.2%. Although probably not significant, the difference between the two intrinsic DRL-effects indicate that pedestrians may benefit more from DRL than drivers. Since pedestrian casualties in MD-accidents are 25% of all casualties in MD-accidents in ‘80 to ‘85, the weighted intrinsic DRL-effect on all casualties in MD-accidents becomes 43.8%, based on odds ratio comparisons of ‘80/’81 and ‘84/’85.

In the additional comments of the Norwegian road authority (Statens Vegens, 1987) to the report of Vaaje the odds ratios for 1971-‘75 is given as 2.88 and for 1981-‘85 as 2.41. From which the Norwegian road authority concluded that the Norwegian data up to 1985 confirm the positive DRL-effect that was found in Sweden and Finland. Taking the ratio of the odds ratios for 1971-‘75 and 1984-‘85 the raw DRL-effect is 25.3%, which by expression (2) with 2% before and 63% after DRL-levels would give an intrinsic DRL-effect of 30.3% and by expression (1) would be 41.1%. For pedestrian casualties MD-accidents with a share of about 25% in the mid seventies to the mid eighties in Norway the intrinsic DRL-effect on all casualties in MD-accidents becomes about 33%, when based on the comparison between 1971-‘75 and 1984-‘85. However, the years 1971-‘75 have a summer time that is shifted by one hour with different daylight/darkness traffic exposure than for 1984-‘85. So the 43.8% obtained from the comparison of Vaaje’s data may be more valid.

Elvik (1993) analysed the DRL-effect for Norway by using the sum of injury data for three groupings of years in an odds ratio comparison.
The first group of years is his before-period and ranges between the years of Vaaje’s comparison periods. It starts with 1980, because it is the first year with the switched summer hours, and ends with 1984, because for 1985 the automatic DRL for new cars was introduced. For that reason the second group of years is the first after-period which starts with 1985 and ends with 1987, because after 1987 the use of DRL became obliged for all motor vehicles. The last group of years is the second after-period from 1988 to 1990 inclusive, wherein the full DRL-obligation holds.

Elvik reports from the two ratios of odds ratios for the totals of casualties in MD-, MN-, SD-, and SN-accidents during the before-period and each after-period (without twilight casualties and motorcycle casualties, because DRL was obliged for motorcycles after 1977), that these measures caused:

- no decline in casualties in MD-accidents between motor vehicles;
- no decline in pedestrian casualties in MD-accidents;

but also reports that,

- casualties in MD-accidents without pedestrian and rear-end accidents decreased by 10%;
- summer casualties in MD-accidents declined by 15%;
- casualties in daytime rear-end accidents increased by 20%.

Elvik examined the possibility of confounding factors for his results, which results contradict the earlier results of Vaaje, and noticed an unexplained increase of casualties in SN-accidents as well as an increase of casualties in daytime rear-end injury accidents. Both increases, if DRL-irrelevant, may mask the actual DRL-effect in Elviks' odds ratio analysis.

An increase of casualties in SN-accidents as such needs not to disturb the odds ratio analysis, but a relative decrease in the SD/SN ratio that is also present in the average annual partial odds ratio $R_p^* = MN \cdot (SD/SN)/n_p$, where n_p is the number of years per period, may do so. The three SD/SN ratios are:

SD/SN	Before	After 1	After 2	Ratio 1/before	Ratio 2/before
Summer	2.592	2.766	3.180	1.067	1.226
Winter	0.703	*0.653*	0.837	**0.903**	1.242
Total	1.430	1.475	1.581	1.031	1.106

Table 12. *SD/SN ratios and their after/before ratios.*

One directly sees from the increase in SD/SN per successive period indicates that the casualties in SN-accidents are not relatively more increased, except for the first winter after-period. Multiplied by the casualties in MN-accidents the corresponding partial odds ratios R_p^*, which are assumed to reflect also the trends for casualties in MD-accidents in case no increase of DRL-usage is present. These partial odds ratios become:

R_p^*	Before	After 1	After 2	Ratio 1/before	Ratio 2/before
Summer	481.1	570.7	598.9	1.186	1.245
Winter	618.5	*578.3*	670.2	**0.935**	1.084
Total	1523.5	1591.0	1563.6	1.044	1.026

Table 13. *Partial odds ratios and their after/before ratios.*

Apart from the first after winter period this indicates that the expected casualties in MD-accidents without an increasing DRL-usage should be increasing. This is what one also expects to happen in an eleven year period of traffic growth that is not fully compensated by a risk reduction. In no way a relative larger increase of casualties in SN-accidents can have masked the DRL-effect from the odds ratio analysis or it must be so for the first winter after-period. The actual odds ratios are identical to $R_p = (MD/n_p)/R_p^*$ and are for the same grouping of years as follows:

R_p	Before	After 1	After 2	Ratio 1/before	Ratio 2/before
Summer	3.931	3.431	3.249	0.872	0.826
Winter	2.063	*2.418*	1.944	**1.172**	0.942
Total	2.079	*2.110*	2.078	**1.015**	1.000

Table 14. *Odds ratios and their after/before ratios.*

Here the total odds ratios slightly differ from Elvik's total odds ratios, due to differences between the reported total casualties (used by Elvik) and the sum of the reported summer and winter casualties (used here for consistency reasons). Since the main trends in the odds ratios are decreasing and the main trends in the partial odds-ratios increasing, it must mean that the average annual casualties in MD-accidents per period (MD/n_p) are decreasing, instead of increasing as expected without an increase in DRL-usage. The difference in winter and summer may indicate that other DRL-irrelevant developments have influenced the first winter after-period the most.

Changing economic situations may influence the traffic growth in daytime and urban areas the most, and mild and harsh winters may cause DRL-irrelevant variations in the R_p^*- and R_p-values. This may have influenced the odds ratio in the period 1985-'87 (upsurge, extremely harsh winters for '84/85 and '85/'86) differently compared to the summer periods and the winter periods 1980-'84 and 1988-'90 (recessions). Such DRL-irrelevant changes over time may always occur if long time-series are analysed. Anyhow the similar summer and winter developments between the before-period and the second after-period seems to indicate a more valid comparison between these periods than between the before-period and first after-period or between the two after-periods. Moreover, the largest increase in DRL-usage occur between the before-period and the second after-period. Because of both reasons, the comparison between the before-period and second after-period will yield the most reliable DRL-effects.

An analysis of odds ratios for the sum of winter and summer casualties is sub-optimal, as for Sweden already is demonstrated. Here not only are the levels of DRL-usage in the winter and summer different, but in Norway the later performed analyses also show that there is a consistent interaction between the winter/summer factor and the developments in the MD/MN and SD/SN ratios for the summer and for the winter over the years. Probably due to that interaction the ratio of total odds ratios from the before and second after-period is unity, while each comparable ratio of odds ratios for the summer and for the winter is less than unity.

For the preferred comparison of the summer odds ratios the raw DRL-effect (S02) is 1 - 0.826 and thus is 18.4%. For that comparison of the winter odds ratios the raw DRL-effect (W02) is 1 - 0.942 or 5.8%. Probably due to their negative covariance the corresponding total raw DRL-effect is zero. If the less reliable odds ratio comparison for the before and first after-periods in the summer and winter are also taken into account, one obtains a raw summer DRL-effect (S01) of 12.8% and an adverse raw winter DRL-effect (W01) of -17.2%. Clearly the latter is an unexpected result. However, if the latter comparison is used, the comparison between both after-periods also should be taken into account. It yields a raw summer DRL-effect (S12) of 5.3% and a raw winter DRL-effect of 19.6%.

The three raw DRL-effects for the summer of 12.8% (S01) and 5.3% (S12) as well as 18.4% (S02) are incorrectly combined by Elvik, stating that the DRL-effect is 15%. The raw DRL-effect (S02) corresponds to a larger increase of DRL-usage than for the raw DRL-effects (S01) and (S12) and thus the raw effects first must be transformed to intrinsic DRL-effects before they can be combined. Their exact intrinsic DRL-effects depends on the mixture of casualties in MD-accidents between vehicles and between vehicles and non-motorised road users. For the shares of pedestrian casualties in MD-accidents in the respective periods the weighted average of relevant DRL-usage per period (obtained from the DRL-usage tables per year presented above) one can weight the corrections to intrinsic DRL-effects by expression (1), for pedestrians, and (2) for drivers. The resulting three values of the intrinsic summer DRL-effects are 36.8% (S01), 19.0% (S12) and 29.8% (S2). The average intrinsic DRL-effect on casualties in MD-accidents in the summer obtained from the odds ratio method thus should be 28.5%.

The raw DRL-effects (W01) and W(12) which both involve the first winter after-period may be questionable, due to the extreme winters in ‘84/’85 and ‘85/’86 as discussed before. It is unlikely that an increase in DRL-usage can cause an increase of casualties in MD-accidents, but a positive DRL-effect (W01) for the first winter after-period obviously is absent. Combining the winter DRL-effects (W01), (W12) and (W02) of respectively -17.2%, 19.6% and 5.6%, while the DRL-increase for (W01) is larger then for (W12), gives a positive intrinsic DRL-effect. Estimated by the same weighted averaging procedure for the winter as for the summer periods, the average intrinsic winter DRL-effect becomes 20.3%. It demonstrates that the analysis of the sum of summer and winter casualties is misleading, because with a positive DRL-effect in the winter and the summer the total DRL-effect also must be positive. So when the estimation is based on the odds ratio comparison between the periods and the averaging of intrinsic DRL-effects, it could mean that the actual intrinsic DRL-effect on casualties in MD-accidents in Norway must be 20.3% for the winter and 28.5% for the summer. But a lower intrinsic DRL-effect for the winter than the summer, due to the adverse DRL-effect (W01), is unexpected.

The casualties rear-end MD-accidents are about 60% higher in 1990 than in 1987/88. Its increase after 1987 in Norway can not be attributed to DRL, since the relevant DRL-usage in encounters with one and two DRL-using vehicles was 100% after 1987. The increase of casualties in rear-end MD-accidents occurred to a lesser extend already from 1980 onward (63% more in ‘86/’87 than in ‘80/’81). In Norway the casualties in rear-end MD-accidents increased by a factor 2.96 from 266 in 1980 to 730 in 1990, the casualties in rear-end MN-accidents by 2.03 from 60 in 1980 to 122 in 1990. Such large increases of casualties in rear-end accidents has also occurred since 1980 in several European countries without DRL (Van Kampen, 1993; Wilding, 1995). In The Netherlands these casualties increased from 1983 to 1992 by a factor 2.06, while all other casualties decreased with a factor .91, so a relative increase of 226%. But in the late eighties the voluntary DRL-usage also increased from nearly nil to about 20% in The Netherlands. In the UK, where no DRL-usage is present, the casualties in rear-end accidents increased by a factor 3.52 from 1980 to 1990, while other injury accidents increased by 1.10 (Wilding, 1995).

If one can compare the development of casualties in rear-end accidents of Norway and the UK, one even can argue that the increase in DRL-usage in Norway could have prevented maximally 30% {1 - 2.46/3.52} of the casualties in rear-end MD-accidents that otherwise would have occurred. The everywhere in Europe observed similar increase of casualties in rear-end accidents probably is due to the growth of traffic intensities (especially in and around rush hours) and perhaps in the late eighties and early nineties also partially due to the increasing, but less than 50%, share of vehicles with ABS. Such a DRL-irrelevant increase in rear-end injury MD-accidents may indeed mask the DRL-effect on all casualties in MD-accidents. Because the share of casualties in rear-end MD-accidents increases between 1980 and 1990, the correction of the odds ratios for this DRL-irrelevant increase of casualties in rear-end MD-accidents also becomes larger for the successive periods.

The averaged summer and winter intrinsic DRL-effects are virtually equal to the intrinsic DRL-effects obtained from the comparison between before and second after-periods. Thus if the odds ratios for the second after-period are adjusted for the DRL-irrelevant increase of casualties in rear-end accidents and these adjusted odds ratios are used for the estimation of the intrinsic DRL-effects, then intrinsic DRL-effects without the influence of this DRL-irrelevant increase are obtained. Since the share of the casualties in MD-accidents other than in rear-end MD-accidents decreases from 90.7% to 80.2% from the before-period to the second after-period, the correction factor would become 0.802/0.907 = 0.884.

The comparison of Norway and the UK sustains that one has to correct the odds ratios by a factor for a DRL-irrelevant increase of casualties in rear-end accidents. If the correction factor for rear-end accidents is equally applied to the DRL-effect of the summer and the winter, it would mean that the intrinsic DRL-effects without the influence of the DRL-irrelevant increase of casualties in rear-end accidents becomes for the winter close to the 42.3% of the summer. It might give an explanation for the originally derived lower effect for the winter than for the summer. However, rear-end MD-accidents in Norway probably occur relatively more in the winter months. Therefore an appropriate correction for the winter could increase the winter DRL-effect and decrease the summer DRL-effect. It could bring the winter results more in line with the winter results of Sweden and Finland. This appropriate correction can not be applied, because the casualties in rear-end accidents are not separately given for the summer and winter. However, that additional difference will not much affect the average effect of the summer and winter. Anyhow the mean intrinsic summer and winter DRL-effect adjusted by a rear-end correction factor can be very well in line with the result of Vaaje (intrinsic DRL-effect on all casualties in MD-accidents from the comparison between 1980/81 and 1984/85 of 43.8%), as well as the Swedish annual DRL-effect of 43.4%.

Elvik reports a raw DRL-effect on the total casualties in MD-accidents without pedestrians and rear-end accidents of 10%, but Elvik seems to have averaged raw DRL-effects of different period comparisons. The actual raw DRL-effect for the second after-period compared to the before-period is 13.1%. When corrected to an intrinsic DRL-effect it amounts to an intrinsic DRL-effect of 31.6%, where the disregarded difference in summer and winter DRL-usage may have deflated this estimated intrinsic DRL-effect on all casualties in MD-accidents between motor vehicles without pedestrian and rear-end accidents. Besides that deflation effect, this intrinsic DRL-effect on casualties in MD-accidents without pedestrian and rear-end accidents of 31.6% remains lower than the above corrected intrinsic DRL-effect on casualties in MD-accidents of 43.8%, which includes pedestrian casualties. In contrast to Elvik's conclusion, it may mean that pedestrians benefit more form DRL than drivers as also Vaaje found by comparing 1980/'81 and 1984/'85.

The reported and unadjusted results by Elvik (1993) contradict the earlier results of Vaaje (1986) for Norway. Elvik explicitly notices that the DRL results of his study differ from most previous studies and remarks that this may be due to methodological flaws in some of the previous studies. However, besides the incorrect averaging of raw DRL-effects, also several other methodological flaws affect Elvik's own study in so far as it concerns the estimation of the effect from an increase in DRL-usage and not the dichotomous effects of changes in DRL-laws.

Firstly, the analysis of period totals by the odds ratio is rather sub-optimal, because it hides the changes in DRL-usage during the years and also the difference in DRL-usage between the summers and winters before 1988. In fact the increase of relevant DRL-usage for encounters between motor vehicles within Elvik's before-period is at least twice the increases of DRL-usage (winter as well as summer) between the before-period and the first as well as the second after-period.

Secondly, the increase in relevant DRL-usage from the first after-period to the second after-period is from average 73% to 98% (one DRL-user) or from average 92% to 100% (one and two DRL-users). The latter marginal increase of relevant DRL-usage for motor vehicles hardly can produce any large raw DRL-effect between the first and last after-period of Elvik. However, Elvik seems to have averaged the raw DRL effects for each comparison of periods and applies no correction to intrinsic DRL-effects.

Thirdly, Elvik uses the biased odds ratio method of totals per period and tests the DRL-effect by the summed (Chi)²-values of the Mantel-Haenszel test as:

$$(Chi)^2\text{-DRL-effect} = \Sigma \ln(R_i/R_s)/\{s[\ln(R_i)]\} \text{ with df=2}$$

where R_i is the odds ratio for the before (i=1), the first after (i=2) and second after (i=3) periods with R_s as the odds ratio for totals of casualties from the sum of the three periods. Since the raw DRL-effect between the before and first after-period and between the first and second after-period only can be smaller than between the before and second after-period, the above summed (Chi)²-test used by Elvik not only deflates the real DRL-effect, but also and even more its significance.

Lastly Elvik's analysis of the DRL-effect by the odds ratio method certainly are influenced by a DRL-irrelevant increase of the casualties in rear-end MD-accidents. This influence increases the odds ratio for the later periods, while the DRL-effect is assumed to decrease these odds ratios. Although Elvik also analysed the casualties without pedestrian and rear-end casualties, the analysis of the sum of winter and summer casualties can be misleading due to winter/summer interactions that counteracts the DRL-effect. The separately analysed summer and winter effects by Elvik are influenced by the increase of rear-end casualties, while Elvik incorrectly attributed this increase partially to the effect of DRL.

Because Elvik and Vaaje presented nearly all relevant data per year, it is possible to analyse the data for each year by an appropriate log-linear model that accounts for the annually increasing levels of DRL-usage by using the v_t values from expression (5) of chapter 3. Since the casualty data are confounded by a DRL-irrelevant increase in rear-end casualties, it firstly is necessary to exclude the rear-end casualties from the analysis. Secondly, the summer and winter data must be analysed separately in order to estimate the DRL-effects for different increases of DRL-usage in summer and winter and to avoid the interactions between years and summer/winter with a diminishing influence on the DRL-effect. Thirdly, the analyses of pedestrian casualties and of casualties in accidents between vehicles must be separated, because of differences in relevant DRL-levels.

The rear-end casualty data as well as the pedestrian casualty data, however, are only given for their annual totals in daylight and darkness. In order to be able to analyse summer and winter data separately without their rear-end and pedestrian casualties the annual numbers for rear-end and pedestrian MD- and MN-accidents must be divided over the summer and winter. One alternative is to divide the respective annual casualties in rear-end and pedestrian MD- and MN-accidents according to the annual shares of the summer and winter casualties in MD- and MN-accidents. But the casualties in rear-end accidents will be relatively higher in the winter than in the summer (due to less friction on winter roads) and pedestrian casualties will be relatively higher in the summer than in the winter (due to higher summer exposure of pedestrians). So this alternative gives an one sided limit for the above subdivision of pedestrian and rear-end casualties over the summer and winter. In an other alternative with an opposite one sided limit, the summer proportion of rear-end casualties p_{sr} per year becomes a-priori estimated as p_{sr} = (s-r)/(s+w-r), here s and w are the annual summer and winter casualties in MD- or MN-accidents and r are the annual casualties in rear-end MD- or MN-accidents. By definition the alternative winter proportion limit becomes maximal $p_{wr} = 1 - p_{sr}$. This alternative attributes a maximal proportion of casualties in rear-end MD- and MN-accidents to the winters per year, expressing that rear-end casualties occur relatively more often on winter roads in Nordic countries. For a similar other limiting alternative the numbers of pedestrian casualties in MD- and MN-accidents are subdivided according to such proportions p_{sp} and p_{wp} that a maximum of pedestrian casualties are allocated in the summer and a minimum in the winter, expressing that pedestrian exposure is higher in the summer. Here, the a-priori minimum winter proportion is $p_{wp} = (w-p)/(w+s-p)$, where p is the annual number of pedestrian casualties and the maximum summer proportion is $p_{sp} = 1 - p_{wp}$. It results in a maximal share of pedestrian casualties in summer MD-accidents.

If the annual summer and winter casualties in MD- and MN-accidents are subdivided by these summer and winter proportions for each alternative it yields lower and upper limits for DRL-effects on summer and winter casualties in MD-accidents without rear-end and pedestrian accidents. The mean of the lower and upper limit can be regarded as the most probable summer and winter corrections for the rear-end and pedestrian casualties in MD- and MN accidents. Without these adjustments one only can analyse the annual sums of summer and winter casualties without those in rear-end and pedestrian accidents, which is a sub-optimal analysis in view of the different increases of DRL-usage in the summer and the winter.

Several re-analyses of the data from 1980 to 1990 are performed by the log-linear model in order to estimate the unbiased intrinsic DRL-effect for annually increasing percentages of DRL-usage and to obtain optimal estimates of the standard deviations. The model contains main effects for years, multiple/single and day/night and first order interactions between years x multiple/single, years x day/night and multiple/single x day/night and is here adapted for the second order interaction as continuous DRL-effect by the term $\delta_{i.}v_t$ (with iteratively determined values v_t as regression variable for the MD-elements in the design matrix).

For the re-analysis of the annual totals of casualty data of 1980-‘90 without the rear-end and pedestrian accidents the above model fits with residual $(Chi)^2$-value of 15.15 for df=9, which means a just not significant overdispersion of 1.68. But the interaction of years with day/night is not significant. In order to obtain more degrees of freedom and a smaller overdispersion of the model fit, the analysis without that interaction is performed and its nearly identical DRL-effect parameter and its smaller standard deviation are:

$\delta_i = -0.277$ and $\sigma(\delta_i) = 0.112$

Since without the interaction of years with day/night df=19, it is a significant intrinsic DRL-effect (one-sided t-test .277/.112 = 2.47 with df=19 gives p = .012). So in contrast to the 10% raw DRL-effect of the sub-optimal analysis of Elvik, the Norwegian intrinsic DRL-effect on casualties in MD-accidents between motor vehicles without those in rear-end and pedestrian accidents over the years 1980 to 1990 turns out to be at least 24.2%. However, this analysis of the totals of annual casualties still is not optimal, due to different DRL-usage percentages in winter and summer periods. The use of average annual percentages for v_t introduces a high error variance) and the hypothesised first (and higher) order interactions between summer/winter and years, if negatively correlated with the v_t, can reduce the absolute level of the DRL-parameter.

For the summer and winter data of 1980-‘90 adjusted by the second alternative for subtractions of pedestrian casualties (subdivided into maximal summer and minimal winter numbers) as well as of rear-end casualties (subdivided into minimal summer and maximal winter numbers) the model with additional main and first and higher order interactions effects for winter/summer is analysed for a separate summer and a winter DRL-effect by different parameters δ_{si} and δ_{wi} for the different summer and winter regression variables v_{st} and v_{wt} (each based on different DRL-usage percentages). This model fit gives a residual (Chi)²-value of 54.85 with df=18 (significant overdispersion = 3.05). Here the first order interaction of years x (day/night) is again very insignificant as also is the second order interaction of years x (day/night) x (summer/winter) and these two interaction effects are thus omitted in order to obtain a higher number of degrees of freedom and a better model fit.

In this model the first order interactions of years x (summer/winter) is significant and very much more so the first order interactions between (summer/winter) x (day/night) and (summer/winter) x (multiple/single) as well as the second order interaction of (summer/winter) x years x (multiple/single) and (summer/winter) x (day/night) x (multiple/single). All these interactions are DRL-irrelevant and can not be estimated in the analysis of annual totals, but notably the last interaction will affect the estimation of the DRL-effect very much. Its summer and winter DRL-parameters and standard deviations are:

$\delta_{si} = -0.940$ and $\sigma(\delta_{si}) = 0.242$

and

$\delta_{wi} = -0.480$ and $\sigma(\delta_{wi}) = 0.176$

Although the fit of the model is not satisfactory (overdispersion = 2.66 with df=38) these DRL-effects, that are assumed to be the minimum summer and maximum winter effects for the adjusted summer and winter data of the second alternative, are both very significant. The significant interactions summer/winter are responsible for the originally obtained lower DRL-effect from the annual data.

The same analysis (again without insignificant interactions) of the summer and winter data, adjusted by the first alternative for the subtractions of pedestrian (minimum for summer and maximum for winter) and rear-end (maximum for summer and minimum for winter) casualties, yields:

δ_{si} = -0.961 and $\sigma(\delta_{si})$ = 0.251

and

δ_{wi} = -0.403 and $\sigma(\delta_{wi})$ = 0.178

In this case the DRL-effects are expected maximum summer and minimum winter effects (summer and winter data adjusted by the first alternative). Again the fit of the model is not satisfactory (overdispersion = 2.65 with df=38 for the same significant interaction effects).

The minimum and maximum DRL-effects for the summer and for the winter are not very different. Therefore, the most probable summer and winter effects is the average of the above two estimates. The average for the summer is:

DRL-effect on casualties in summer MD-accidents between vehicles

δ_{is} = -0.951 and $\sigma(\delta_{is})$ = 0.247

It means that the intrinsic summer DRL-effect is 61.3% in Norway. It is a very significant effect, because with df=38 its 90% interval ranges from 41.5.7% to 74.5%.

The most probable winter effect, obtained as the average of the above estimates, results in:

DRL-effect on casualties in winter MD-accidents between vehicles

δ_{iw} = -0.442 and $\sigma(\delta_{iw})$ = 0.181

This most probable winter DRL-effect of 35.7% is significant (one-sided t-test p = .01), but in contrast to the expectation it is just significantly lower DRL-effect than the summer DRL-effect (one-sided t-test, p=.05) and also its parameter variance is significantly smaller (F-test, p=.03). It may be due to overestimated differences in summer and winter DRL-usage which are assumed to be given by corollary 5, since for Norway no report contains observed differences. If the summer-winter differences in DRL-usage are overestimated the effect of such an overestimation will be cancelled out in the average whole year estimate. Because of the significant differences between the summer and winter DRL-effects and their variances, one must not estimate a DRL-effect by an analysis of annual totals, but by the average of the summer and winter DRL-effects. This average DRL-effect percentages yields a whole year DRL-effect on casualties in MD-accidents between vehicles without rear-end accidents of:

δ_i = -0.664 and $\sigma(\delta_i)$ = 0.153 with df=76

It amounts to a whole year DRL-effect on casualties in MD-accidents between vehicles without rear-end accidents of 48.5% for Norway. Its very high significance of $p < .001$ (one-sided t-test) also gives a 90% confidence interval that ranges from 33.5% to 60.1%%. Due to the above optimal designed analyses of summer and winter data, that has taken care of the interactions with the summer/winter effects, this whole year DRL-effect is higher than for the sub-optimal analysis of the annual data.

The whole year intrinsic DRL-effect obtained from the average of the separated summer and winter DRL-effects over the period 1980 to 1990 is 8% higher then the whole year effect obtained by Vaaje's odds ratios comparison of 1980/'81 and 1984/'85. The comparison with Vaaje's DRL-effect, which includes rear-end casualties as well as the comparison between the rear-end casualty increases in the UK and in Norway (which latter comparison indicates a range from zero to maximal 30% DRL-effect on rear-end casualties) also demonstrates that casualties in rear-end MD-accidents in Norway probably are about a quarter influenced by DRL, compared to the DRL influence on other casualties in MD-accidents, because the average Norwegian share of casualties in rear-end MD-accidents is 15%. So from the Norwegian data without the DRL-irrelevant increase of casualties in rear-end MD-accidents, the same factor of a quarter as for the Canadian data of Sparks et al. (1993) is found. By the average Norwegian share of casualties in rear-end MD-accidents of 15%, it follows that the DRL-effect on casualties in all MD-accidents indeed has to be a factor .88 lower than estimated above. One obtains by this correction the whole year national *DRL-effect on casualties in all MD-accidents* between vehicles as:

$\boldsymbol{\delta_{in} = -0.557}$ and $\boldsymbol{\sigma(\delta_{in}) = 0.128}$ with **df=75**

It amounts to a DRL-effect on casualties in all MD-accidents between motor vehicles of 42.7%. Its significance test gives $p < .001$ and its 90% confidence interval ranges from 29.1% to 53.7%.

Next the two summer and winter DRL-effects on pedestrian casualties need to analysed for the pedestrian DRL-effect. The annual totals of pedestrian casualties in MD-accidents again may show no DRL-effect, due to an underlying hidden interactions with the summer/winter effect in the whole year pedestrian casualties in MD- and MN-accidents in relation to the motor vehicle casualties in SD- and SN-accidents. The average results from the two alternative summer and winter DRL-effects on pedestrian casualties (with hardly differing results) are:

DRL-effect on pedestrian casualties in summer MD-accidents

$\delta_{is} = -3.000$ and $\sigma(\delta_{isp}) = 1.838$

DRL-effect on pedestrian casualties in winter MD-accidents

$\delta_{iw} = -0.256$ and $\sigma(\delta_{iwp}) = 0.414$

Here again the magnitude of effects on pedestrians is reversed with respect to the expected summer and winter effect, but both effects are not significant. Their average DRL-effect percentages gives the *whole year DRL-effect on pedestrian casualties in MD-accidents*

$\boldsymbol{\delta_{ip} = -0.886}$ and $\boldsymbol{\sigma(\delta_{ip}) = 0.971}$

The DRL-effect for pedestrians is not significant, due to the relative low numbers of pedestrians casualties. Still its level may indicate that the most probable DRL-effect for pedestrians is larger than for motor vehicle drivers. Because its difference with the DRL-effect between motor vehicles is also insignificant, it only is a weak indication.

If there is in the long run an adaptation effect to DRL, the intrinsic DRL-effect for 1980-‘84 should be higher than for 1980-‘90. The comparison of the whole year DRL-effect over 1980 to 1990 with Vaaje’s DRL-effect over 1980/’81 to 1984/’85 does not indicate that DRL-adaptation over the years is present for the DRL-effect on casualties in MD-accidents between motor vehicles. Therefore, the data from ‘80 to ‘84 are also analysed and the results compared to the results for ‘80 to ‘90 in order to validate statistically the absence or presence of an adaptive effect. Especially with respect to pedestrian casualties in MD-accidents this possibility of a DRL-adaptation should be investigated with respect to the formulated conjectures 4a and 4b, but in view of the lack of significance of a DRL-effect on pedestrians it is of no use to do so for the Norwegian time-series (it can not give any statistical significance for an adaptation effects.

The summer and winter DRL-effects for the years 1980 to 1984 for the average effects in the two alternative data adjustments are:

summer DRL-effect on casualties in MD-accidents between vehicles 80-84

$\delta_{is} = -0.405$ and $\sigma(\delta_{is}) = 0.222$

winter DRL-effect on casualties in MD-accidents between vehicles 80-84

$\delta_{iw} = -0.725$ and $\sigma(\delta_{iw}) = 0.154$

The first four years compared to the summer and winter results for the period 1980-‘90, show a reversal of magnitudes with a higher intrinsic DRL-effect for the winter than for the summer, as it also ought to be according to the expected perception effects of DRL. Since the whole year effect from the average DRL-effect percentages for ‘80-‘84 ($\delta_i = -0.552$) is virtually equal to the whole year effect for ‘80-‘90 (with $\delta_i = -.557$), there clearly is no overall adaptation to DRL.

The results from the analysis over 1980-‘84 compared with the results from the analysis of 1980-‘90, imply an increased summer and a reduced winter effect after 1984 for different relevant increases in DRL-usage in the summer (from 66% in 1984 to 100% after 1987) and the winter (from 96% in 1984 to 100% after 1986). Clearly the increase in relevant DRL-usage for encounters between motor vehicles in the winter after 1984 is too small to expect any winter DRL-effect after 1984, but this does not explain why for 1980-‘90 compared to 1980-‘84 the estimated winter effect for 1980-‘90 for the regression variable v_t decreased, nor why its summer effect for 1980-‘90 compared to 1980-‘84 increased. If there would exist influences on odds ratios after 1984, which partially are independent from the DRL-increase over the years, it could explain these differences in results. The irregular level of the odds ratios in successive winters after 1984 (with decreases in the first winters after ‘84/’85), can not be attributed to the small further increase of DRL-usage in the successive winters after 1984. Probably the harsh winters of the first years after 1984 had DRL-irrelevant effects on the odds ratios. The regular further decrease of the odds ratios in the summers after 1984 very well can be attributed to the further increase of DRL-usage in the summers after 1984, but the lesser increase of relevant DRL-usage and higher increase of odds ratio increase after 1984 than before remains unexplained, or there must have been more bright summers after 1984. Since the estimated intrinsic DRL-effects on casualties in MD-accidents between motor vehicles for the summer and winter in Norway are markedly different and reversed in magnitude for 1980-‘84 and 1980-‘90, while the whole year average remain rather stable and seems hardly affected by partially DRL-irrelevant influences on the odds ratios (which indeed possibly may exist), it seems justified to use only the whole year average effect over 1980-‘90 of $\delta_i = -0.557$ as the most reliable intrinsic DRL-effect on casualties in all MD-accidents between vehicles in Norway.

The last analysis for Norway concerns the separate available time series of motorcycle casualties for Norway. It makes it possible to investigate the existence of an adverse effect on motorcyclist with DRL due to an increased DRL-usage of other motor vehicles, because it has been hypothesised that a full DRL-usage for cars may mask the perception of motorcyclist with DRL. With reference to the log-linear model for the Canadian data, it must be reminded for the modelling of a DRL-effect on motorcycle casualties in MD-accidents that not only the model inclusion of motorcycle casualties in SD-and SN-accidents is relevant, but also the inclusion of casualties in SD- and SN-accidents of other motor vehicles. The relevant odds ratio comparison over years t writes as

$$R_{42t} = (MD_{42t}/MN_{42t})/[(\sqrt{SD_{2t}}/\sqrt{SN_{2t}}).(\sqrt{SD_{4t}}/\sqrt{SN_{4t}})]$$

where the index 2 refers to the two-wheel motorcycles and the index 4 to the other motor vehicles with 4 or more wheels. The corresponding log-linear model that should be analysed for a DRL-effect on motorcycle casualties in MD-accidents, due to the successive increases of DRL-usage by other motor vehicles, reads as:

$$Est(SN_{2t}) = 2.\mu_{2t}$$

$$Est(SD_{2t}) = 2.\mu_{2t} + 2.\tau_{2t}$$

$$Est(SN_{4t}) = 2.\mu_{4t}$$

$$Est(SD_{4t}) = 2.\mu_{4t} + 2.\tau_{4t}$$

$$Est(MN_{42t}) = \mu_{2t} + \mu_{4t} + \pi_t$$

$$Est(MD_{42t}) = \mu_{2t} + \tau_{2t} + \mu_{4t} + \tau_{4t} + \pi_t + ß_m + \delta_m \cdot v_{mt}$$

Here v_{mt} are iteratively determined by expression (5) of chapter 3 for the relevant annual increasing DRL-levels of two DRL-users in encounters between motorcycles and other motor vehicles. Assuming 100% DRL-usage for motorcycles between 1980 and 1990 inclusive, the relevant percentages of two DRL-users in encounters between motorcycles an other motor vehicles, as needed for expression (4) in chapter 2, are equal to the total percentages of DRL-usage for the motor vehicles in the first table presented in the introduction of this paragraph on Norway. According to conjecture 2 (equal effects for encounters with one and with two DRL-using motor vehicles) and conjecture 8 (no masking of DRL-using motorcycles due to an increased DRL-usage by other motor vehicles) the δ_m for motorcycles should be (insignificantly different from) zero. The above log-linear model analysis fits with an insignificant overdispersion (1.74 with df=9) and yields the δ_m parameter and its standard deviation as:

$$\delta_m = 0.014 \quad \text{and} \quad \sigma(\delta_m) = 0.183$$

Thus a nearly zero or an insignificantly small adverse DRL-effect exists for motorcyclists with DRL as the result of the increased DRL-usage by other motor vehicles. Thus due to increased DRL-usage of other motor vehicles, motorcyclist do not have hardly more, but also not less casualties in MD-accidents with other motor vehicles.

It does not indicate that motorcyclists, who used already DRL to a full extent, are masked by an increased DRL-usage of other motor vehicles. The insignificant positive DRL-parameter for motorcyclists anyhow indicates that there also is no higher DRL-effect on encounters with two DRL-users than on encounters with only one DRL-user. In short the nearly zero value of δ_m confirms conjectures 2 and 3, although it may be each opposite effect is present to some extent, but that both compensate each other into a nearly zero net effect (on the one hand more avoidance of accidents by motorcyclists, due to more DRL-usage of other motor vehicles, and on the other hand less avoidance of accidents with motorcyclists by other motor vehicle drivers, due to increased masking of motorcyclist in traffic with increased DRL-usage).

From the reanalyses of the Norwegian data it is concluded that there is a high and significant whole year DRL-effect in Norway with rather unstable estimated summer and winter DRL-effects, possibly due to different DRL-irrelevant first and second order interactions with summer/winter, (especially before and after 1984/’85). As such it rejects *hypothesis 1* (no DRL-effect), but it does not give a clear answer to the confirmation or rejection of *conjecture 5* (higher and lower intrinsic DRL-effect in the winter than in the summer). The insignificantly higher DRL-effect for pedestrians in Norway indicates no lower DRL-effect for pedestrians and at least contributes to the acceptance of *conjecture 3* (equal intrinsic DRL-effects for motorvehicle drivers and pedestrians). The insignificant and nearly zero additional DRL-effect for motorcyclists contributes to the acceptance of *conjecture 2* (equal intrinsic DRL-effects for encounters with one and with two DRL-users) and leads also to the acceptance of *conjecture 8* (no relatively negative effect for motorcyclists). No other information is relevant for other hypothesis or conjectures.

4.6. DRL-evaluation in Denmark

In Denmark the use of DRL became compulsory in october 1990. In the period before october 1990 the voluntary use of DRL was already about 20%, while the DRL-level increased to above 95% in the after-period. Hansen (1993) analysed the DRL-effect by casualty data for five quarters after the DRL-obligation and again evaluated the DRL-effect in Denmark with similar results for an after-period of eleven quarters (Hansen, 1994).

The model used by Hansen is a log-linear single time-series analysis with seasonal quarter effects, a time trend effect and a DRL-intervention or before/after effect for the casualties in MD-accidents in 15 successive quarters of the before-period and in 5 or 11 quarters of the after-period. Thus the model writes:

$$Est(MD_{qt}) = \exp(\mu_q + \alpha \cdot t_q + \delta.x)$$

Here μ_q is the relevant quarter effect, t_q the time regression variable for the annual quarters with values increasing by 0.25 for each successive quarter, while x=0 for the before-period and x=1 for the after-period. Using this model Hansen found a significant 6% to 7% raw DRL-effect on casualties in MD-accidents between vehicles, but also differentiated these casualties into different types of conflict manoeuvres. For casualties from left turns in conflict with oncoming vehicles a significant raw DRL-effect of 34% is found, for casualties from crossing vehicles the insignificant raw DRL-effect is 11%, for casualties in rear-end accidents the insignificant raw DRL-effect is 4%, while for casualties in other MD-accidents insignificant adverse DRL-effects are obtained (8% and 9% increases for frontal and overtaking accidents). For cyclist casualties an insignificant 4% DRL-effect for their MD-accidents is found, while it is reported that the cyclist casualties in MN-accidents increased by 10%. However, the pedestrian casualties in MD-accidents showed an insignificant increase of 16%, while their casualties in MN-accidents only increased by 9%. For motorcyclists, already using DRL, no change in casualties occurred, but Hansen remarks their casualties in MN-, SD-, and SN-accidents decreased (insignificantly) by 11%, 7%, and 45% respectively.

Because no control group or quasi-control groups are used, Hansen discussed the possibility of other changes that might have affected the development of casualties in MD-accidents. He reports that the only confounding factor may be an increased police enforcement of drunken driving. For this reason Hansen excluded the casualties in accidents with drunken drivers as registered by the police. Although Hansen often refers to developments of casualties in MN- or SD- and SN-accidents when he discusses the results for casualties in other MD-accidents than between motor vehicles (without motorcyclists), he states that the use of the odds ratio methodology seems not suitable for the analysis of the Danish data due to changes in the odds ratio before the introduction of DRL and because it does not take into account that trends over time are present in the data.

The weakness of Hansen's analysis is the absence of (quasi-)control groups, which may invalidate his results by other effects or developments that coincide with or overlap the introduction of the DRL-obligation. Clearly the objection of Hansen against the odds ratio analysis with the quasi-control groups (because of time dependent trends in the data), simply is taken away by a log-linear model that estimates main and first order interaction effects for years and simultaneously estimates the same DRL-effect as the odds-ratio method, but in an optimal and unbiased way by the second order interaction effect between before/after, day/night and multiple/single. This model has the advantage above Hansen's analysis that no specified time trend has to be assumed (all differences between annual totals, multiples versus singles per year and day versus night casualties per year are eliminated). However, the objection that the odds ratio changes before the introduction of the DRL-obligation can be a serious objection for the just formulated log-linear model analysis, but also for Hansen's log-linear trend model with intervention itself. A closer inspection reveals that the odds ratio seem to increase somewhat with time, but also markedly together with the increase of alcohol enforcement in the year just before the introduction of the DRL-obligation. It might very well be that increased alcohol enforcement over time influences the successive casualties in the registered non-alcohol MD-accidents as well as the first order interaction between multiple/single and day/night over time in such a way that the odds ratio increases with time. The former by incomplete detection of alcohol accidents by the police and the latter for example also by increasing the ratio MD/MN more than the ratio SD/SN. An objective measure of the changing alcohol enforcement could be obtained from the ratio of casualties in all non-alcohol to all alcohol accidents, which measure than could be included in some way as a regression variable in the log-linear model in order to separate this effect from the in time overlapping DRL-effect.

Hansen kindly provided for this study the relevant Danish injury data in year-periods from October to September for 86/'87 to '92/'93, that is for four 12-month periods before and three 12-month periods after the introduction of the DRL-obligation. This enables a reanalysis of the data by the log-linear model and adjustments of that model for an alcohol enforcement and/or time trend effects. It is, however, useful to inspect first the development of the odds ratio of the non-alcohol as well as of the total (incl. non-alcohol) casualties. The next table shows these odds ratios for casualties in all accidents, except motorcycle accidents.

Odds ratio	Before 12-month periods				After 12-month periods		
	86/87	87/88	88/89	89/90	90/91	91/92	92/93
Non-alc.	2.73	2.92	3.15	3.58	2.67	2.85	2.53
Total	2.96	2.81	3.04	3.19	2.72	2.68	**2.59**

Table 15. *Odds ratios for casualties in all accidents, except motorcycle accidents.*

The odds ratio development show an increasing time-trend in the before-period and a fluctuating or decreasing level in the after-period.
The fluctuating or decreasing odds ratios in the after-period may indicate at least that there is no adaptation to DRL with a diminishing effect.
The increasing time-trend in the before-period rather implies that the odds ratios in the after-period without the influence of the DRL-obligation would have been higher than in the before-period and probably increasing.
Therefore, and because the ratio of the average after- to average before-odds ratios yields 0.867 (non-alc) or 0.887 (total), the actual raw DRL-effect is at least higher than 13.3% or 11.3%.

It also is instructive to see how the ratio of casualties in non-alcohol to alcohol accidents (which hardly will be influenced by a DRL-effect) develops. This ratio (and its index used later) is shown in the next table.

Ratio	Before 12-month periods				After 12-month periods		
	86/87	87/88	88/89	89/90	90/91	91/92	92/93
Non-alc./alc.	3.88	3.96	3.71	4.16	4.62	4.61	4.66
Index a_t	1.00	1.02	0.98	1.07	1.19	1.19	1.20

Table 16. *The development of the ratio of casualties in non-alcohol to alcohol accidents.*

The variations of the odds ratios, and especially the higher one for '89/'90 just before the introduction of the DRL-obligation, coincides with the variation and increase of alcohol ratio in the before-period. Since the influence of the DRL-irrelevant confounding factors may disturb the DRL-effect estimation, there may be serious doubts on the validity of the trend with intervention analysis of Hansen, especially if the casualties in non-alcohol MD-accidents in the before-period are influenced by a safer driving in reaction to increased enforcement or are contaminated partially by casualties in alcohol accidents that are not as such detected by the police (which is the case in most other European countries).

If the non-alcohol/alcohol ratio contributes to the prediction of the odds ratio, than the higher and nearly constant level of the alcohol ratio in the after-period would imply that the odds ratio in the after-period without an increase in DRL-usage is expected to be much higher than the odds ratio for '89/'90. This in turn also would imply a much higher raw DRL-effect than 13.3% or 11.3%, obtained from the ratio of the average before and after odds ratios. Moreover, exclusion of casualties in alcohol accidents also gives a deformation of the Danish DRL-effect on all casualties in MD-accidents that is comparable to other countries. Therefore, a reanalysis of the Danish annual casualties is performed by the earlier used log-linear model without and with adjustment of confounding factors for trend and/or alcohol enforcement index as additional regression variables t (=years) and a_t [or $\ln(a_t)$] in the log-linear expression for $Est(MD_t)$. The log-linear models solved for the Danish data, are then written as:

$Est(SN_t) = \exp[\mu_t]$

$Est(SD_t) = \exp[\mu_t + \tau_t]$ all models

$Est(MN_t) = \exp[\mu_t + \pi_t]$

$Est(MD_t) = \exp[\mu_t + \pi_t + \tau_t + ß + \delta \cdot x_t]$ model 1

$Est(MD_t) = \exp[\mu_t + \pi_t + \tau_t + ß_o + ß_1 \cdot t + \delta \cdot x_t]$ model 2

$Est(MD_t) = \exp[\mu_t + \pi_t + \tau_t + ß_o + \alpha \cdot a_t + \delta \cdot x_t]$ model 3a

$Est(MD_t) = \exp[\mu_t + \pi_t + \tau_t + ß_o + \alpha \cdot \ln(a_t) + \delta \cdot x_t]$
or model 3b
$Est(MD_t) = \exp[\mu_t + \pi_t + \tau_t + ß_o + \delta \cdot x_t] \cdot [a_t]^{\alpha}$

where x_t=0 if t is in the before-period and x_t=1 for t in the after-period.

Analysing the Danish totals for the DRL-effect on casualties in MD-accidents between motor vehicles without accidents with motorcyclists (and thus also without cyclist or pedestrian casualties) by the usual log-linear model of Model 1 (main and first order interaction effects for year, multiple/single and day/night with the DRL-effect as second order interaction effect of before/after x day/night x multiple/single) for the four years in the before-period ‘86/’87 to ‘89/’90 and three years in the after-period ‘90/’91 to ‘92/’93, it yields for the data sets of:

all casualties excl. motorcyclist	$\delta = -.130$	and	$\sigma(\delta) = 0.051$
idem excl. alcohol accidents	$\delta = -.132$	and	$\sigma(\delta) = 0.076$
idem excl. coincident directions	$\delta = -.177$	and	$\sigma(\delta) = 0.057$
idem. excl. alc. and coinc. acc.	$\delta = -.186$	and	$\sigma(\delta) = 0.070$

The results for the analysis by model 1 (with acceptable overdispersion for all four data sets) show that it does not matter much for the raw DRL-effect when casualties in accidents with alcohol are excluded or not, but exclusion of casualties in alcohol accidents increases the standard deviation of the DRL-parameter. There thus is no need to exclude casualties in alcohol accidents from the analysis in this design with quasi-control group casualties in four years before and three years after the DRL-obligation. The exclusion of casualties in coincident direction accidents increases the raw DRL-effect by an additional 4% to 5% and has no much effect on the error variance. Since casualties in coincident direction accidents are about 30% of all casualties in MD-accidents between motor vehicles in Denmark the raw DRL-effect on casualties in MD-accidents between motor vehicles with coincident direction accidents seems almost absent. However, casualties in rear-end MD-accidents are increasing in countries without DRL in Europe and it very well may be that the absence of that increase in Denmark, also after 1990, partially is due to DRL.

Model 2 only gives a significant time-trend for the casualties in MD-accidents in the annual data without alcohol accidents (trend for residuals after main and first order interactions between years, day/night and multiple/single: $\beta_1 = 0.059$ with significance $p = 0.044$ and $\delta = -0.3432$ with $p = 0.005$), while that trend is insignificant in the analysis of the annual casualties in all accidents. The exclusion of alcohol accidents, therefore, seems to cause a time-trend in the most crucial interaction that is expected to be changed by DRL. It once more stresses that the analysis of the DRL-effect by Hansen, who analysed only the casualties in non-alcohol MD-accidents (by a log-linear trend model with intervention for the single time-series of casualties in non-alcohol MD-accidents), is questionable.

Models 3a and 3b give similar DRL-effect as Model 2, but with slightly more significant DRL-parameter and better fit for Model 3b. As is to be expected the only nearly significant α-parameter for alcohol enforcement is found for the first data set of casualties in all accidents (excl. motorcycles) reported above without the simultaneous adjustment. Its adjusted results as well as for the data set without casualties in coincident direction accidents from model 3b are:

all casualties excl. motorcycles	δ = -0.363	and	$\sigma(\delta)$	= 0.129
	α = 1.44	and	$\sigma(\alpha)$	= 0.75
idem excl. coincident directions	δ = -0.388	and	$\sigma(\delta)$	= 0.166
	α = 1.30	and	$\sigma(\alpha)$	= 0.97

This model fits simultaneously the effect of the increased alcohol enforcement (increasing from 1988/'89 to 1991/'92) and the DRL-obligation (between 1989/'90 and 1990/'91). Only for the set of casualty data that includes the alcohol accidents the alcohol enforcement effect is nearly significant (one sided t-test 1.44/0.75 = 1.92 with df=5 gives p = 0.06). Clearly the alcohol enforcement effect, independently measured by the ratio of casualties in non-alcohol and alcohol accidents, and the DRL-effect are partially compensating. Without the simultaneously and separately estimated effect of the alcohol enforcement the DRL-effect becomes underestimated, as the comparison of the δ-parameter in the two solutions show. For this adjusted model the alcohol enforcement effect for casualties without those in coincident manoeuvre accidents is not significant (p = 0.18), but its estimated DRL-effect becomes not much different from the DRL-effect on all casualties in MD-accidents. The parameter differences may indicate that the DRL-effect on casualties in coincident direction accidents, if corrected for increased alcohol enforcement, only is slightly less than for casualties in other MD-accidents. This only can imply that casualties in alcohol MD-accidents between vehicles with coinciding directions are somewhat less prevented by DRL.

Since the before DRL-level is 20% the correction by expression (2) of chapter 2 gives for the intrinsic DRL-parameter for all casualties in MD-accidents with adjustment for the alcohol enforcement effect:

$$\delta_i = -0.519 \qquad \text{and} \qquad \sigma(\delta_i) = 0.184$$

or an adjusted intrinsic DRL-effect of 40.5% with a 90% confidence interval that ranges from 13.6% to 59.0%. Since this estimate heavily is dependent on the effect of the alcohol enforcement that is mainly estimated from the effect on the last year before the DRL-obligation (no earlier increase of alcohol enforcement and also not after 1990/'91), it gives a high covariance between the alcohol enforcement regression variable and the dummy variable for before/after. The above DRL-effect, therefore, is rather uncertain, despite the just significant alcohol enforcement effect. It thus may be wise to see what the probably much lower intrinsic DRL-effect for the unadjusted model is. It gives:

$$\delta_i = -0.196 \qquad \text{and} \qquad \sigma(\delta_i) = 0.077$$

or an unadjusted intrinsic DRL-effect of 17.8% with a 90% confidence interval that ranges from 4.5% to 29.2%. However, one optimally takes the simple average of both parameter estimates and the average parameter variance as its variance, since each model may estimate the true intrinsic

DRL-effect. Taking it as the values with the highest likelihood the national intrinsic DRL-parameter and its standard deviation become:

DRL-effect on casualties in all MD-accidents between motor vehicles

$\boldsymbol{\delta_{in} = -0.358}$ and $\boldsymbol{\sigma(\delta_{in}) = 0.141}$ with **df=5**

or an intrinsic DRL-effect of 30.1% with a 90% confidence interval that ranges from 7.1% to 47.4%. Hansen (personal communication) has objected that such a high reduction percentage (raw reduction of about 20%) for the casualties in MD-accidents seems very improbable in view of the linear decreasing trend of casualties in MD-accidents before the DRL-obligation. However, without the estimated effect increased alcohol enforcement the casualties in MD-accidents would not have been decreasing in the last before year (as well as even more so for casualties in the other night accidents). Also the marked decreases of the casualties in SD-, MN-, and SN-accidents in the after years only can be due to increased alcohol enforcement. Without the decrease of casualties in MD-accidents in the last year before the DRL-obligation, there hardly is a decreasing trend for the casualties in MD-accidents in the before period. Apart from objections against linearity of trends in casualties, the raw DRL-effect of about 7% found by Hansen very well could be 13% higher, if the trend in the before years would not have been influenced by increased alcohol enforcement in the last before year.

Without casualties in coincident direction accidents the same average leads to the intrinsic DRL-parameter of $\delta_i = 0.283$, hence a reduction by a factor of .82 on the intrinsic DRL-effect percentage on casualties in all MD-accidents. Since the share of casualties from coincident direction accidents is about 30%, the intrinsic DRL-effect on casualties in coincident direction injury MD-accidents is a factor .6 of the intrinsic DRL-effect on casualties in all MD-accidents. Casualties from rear-end MD-accidents are less than half the casualties of all coincident direction MD-accidents, while the DRL-effect on casualties in other coincident direction MD-accidents hardly is reduced. It reconfirms the earlier estimations from Canada and Norway for the factor of about a quarter for the DRL-effect on (casualties in) rear-end accidents.

The log-linear analysis for cyclist and pedestrian casualties in MD- and MN-accidents as two types of MD- and MN-casualties together with casualties in SD- and SN-accidents of motor vehicles (without motorcycles) yields no significant time-trend, nor a significant alcohol enforcement effect. Using the unadjusted model with the second order interaction effect between before/after, day/night and pedestrian/ cyclist/single as two DRL-effects (one for pedestrians and one for cyclists), the results transformed to the intrinsic DRL-effects (raw effects corrected for 20% before and 97% after DRL-level) are:

DRL-effect on pedestrian casualties in MD-accidents

$\boldsymbol{\delta_{ip} = -0.177}$ and $\boldsymbol{\sigma(\delta_{ip}) = 0.070}$

DRL-effect on cyclist casualties in MD-accidents

$\boldsymbol{\delta_{ic} = -0.191}$ and $\boldsymbol{\sigma(\delta_{ic}) = 0.076}$

Clearly these intrinsic DRL-effects of 16.2% for pedestrians and 17.4% for cyclists are significant, but not different from each other. The somewhat lower intrinsic DRL-effects also do not differ significantly from the intrinsic DRL-effect between motor vehicles (without motorcycles).

For Denmark one also can, as for Norway, test whether there is a negative or positive DRL-effect for motorcyclist, already using DRL fully since 1978, from the DRL-obligation for other motor vehicles. The same model as for Norway, but here with a dichotomous dummy variable for the increase of DRL-usage from the before to the after-period, gives a non significant residual (Chi)²-value of 8.29 with df=5 and yields:

$\delta_m = 0.012$ and $\sigma(\delta_m) = 0.213$

So also here a nearly zero or insignificantly small adverse DRL-effect exists for motorcyclists with DRL as the result of the increased DRL-usage by other motor vehicles. It again indicates that motorcyclists who use already DRL to a full extent are not masked by an increased DRL-usage of other motor vehicles as well as that there is no higher DRL-effect on encounters with two DRL-users than on encounters with only one DRL-user (or both opposite effects compensate each other).

In conclusion: the significant intrinsic DRL-effects for Denmark violates *hypothesis 1* (no DRL-effect). The insignificantly lower DRL-effect for pedestrians/cyclists questionably sustains an acceptance of *conjecture 3* (equal intrinsic DRL-effects). This insignificant difference and the insignificant effect for motorcyclists from the DRL-obligation for other motor vehicles contributes to the acceptance of *conjecture 2* (equal intrinsic DRL-effects for encounters with one and with two DRL-users), while the latter also contributes to the acceptance and *conjecture 8* (no relatively negative effect for motorcyclists). The clear absence for adaptation to DRL (the DRL-effect rather seems to increase) contributes to the acceptance of *conjecture 4a* and *4b*. The reconfirmed indication for a lower DRL-effect on casualties in rear-end accidents makes a rejection of *conjecture 6* more likely. No much further information form the Danish data analysis is relevant for other conjectures of hypotheses.

4.7. DRL-evaluations in Austria

From 1987 onward in Austria a week in the late autumn is a special Road Safety Week, during which drivers are recommended by publicity campaigns to show their commitment to road safety by using low beam headlights. The first time in 1987 the actual DRL-usage in that week was 14.4%, but some DRL-usage continued using it for 3% to 8% afterwards although before the autumn of 1987 the DRL-usage was virtually absent. In these road safety weeks of 1988 and 1989 the DRL-usage increased to over 25% in bright sunlight conditions and was much higher in cloudy or rainy weather conditions. After these weeks the DRL-usage stayed for some period at an average level of 22% to 27%, but also later decreased to a much lower DRL-level of about 8% in sunny weather conditions just before the

DRL-campaigns in the Road Safety Week of later years. Using corollary 5 the average overall existing DRL-usage from autumn 1987 to autumn 1989 can be estimated to be about 7% and after 1990 probably about 10%.

In the context of officially recommended DRL-usage, four different fleets of national of local authorities were urged to use DRL for longer periods in order to evaluate the safety effect of DRL in these fleets. Three studies on DRL-evaluations for these four fleets are published and reviewed here.

4.7.1. *Army fleet evaluation (KfV, 1989)*

The first study concerns the vehicles of the Austrian army (KfV, 1989), where 1 oct. ‘86 to 30 sept. 1987 is used as the before-period for the after-period of 1 oct. ‘87 to 30 sept. ‘88 with DRL. The report states that the MD-accidents decreased by 12.2% from 956 to 839, which is judged to be significant (theoretical $p < 0.01$ for observed (Chi)²-value of 7.6 with df=1, but no test by empirical error variance is possible). However, it also is stated that the sum of SD-, SN-, and MN-accidents decreased by 10.5% from 247 to 221. Since the separate data are given, the odds ratio values $R_b = 2.182$, $R_a = 1.542$ and DRL-effect = $1 - R_a/R_b = .293$ are easily obtained, which implies that

$\boldsymbol{\delta_i = -0.347}$ and $\boldsymbol{\sigma(\delta_i) = 0.379}$ (theoretical)

The MD-, and MN-accidents are also differentiated in types of accidents, which reveals for this military vehicles are extreme large share of accidents with stopping and parking vehicles. Excluding these daytime accidents or adding these daytime accidents to the SD-accidents also give by the odds ratios similar DRL-effects.

The rather high DRL-effect is anyhow far from significant and thus this study only slightly contributes to a possible rejection of *hypothesis 1* (no DRL-effect). From the data differentiation it also may be worthwhile to notice that the MD-accidents with pedestrians decreased from 16 to 5. It indicates an insignificantly larger DRL-effect for pedestrians, which can contribute hardly to a rejection of *conjecture 3*.

4.7.2. *Evaluation of Graz authority fleet (Shützenhofer et al, 1990)*

The second study (Schützenhofer et al., 1990) analysed the DRL-effect from one period with DRL-usage for the vehicle fleet of the Graz town authority by comparing the MD- and MN-accidents for one before (1.5.87 to 31.12.87) and one after-period (1.5.89 to 31.12.89) with the inbetween period (1.5.88 to 31.12.88) of DRL-usage, while both control periods are without DRL-usage. The authors stated that the decrease in daytime accident of 21.6% from 236 in the before-period to 185 in the inbetween period with DRL is significant (theoretically expected p=.02 for observed (Chi)²-value of 6.2 with df=1), but that the increase of 15.1% form 185 in the period with DRL to 213 in the after-period without DRL is not. For the same three successive periods the nighttime accidents are given as 50, 48, and 64, while also the separate accident types, containing single and multiple daytime accidents, are reported.

The reanalysis by the log-linear model with main effects for periods (3 categories) and for MD/SD/night-accidents (3 categories) as well as the interaction effect between MD/SD/night and control/experimental periods as the DRL-effect on MD-accidents for the before and after-periods without DRL versus the in-between period with DRL, yields a residual (Chi)²-value of 5.6 with df=3 (p > .10) and:

$\delta = -0.175$ and $\sigma(\delta) = 0.214$

However, here the main effect for successive periods is insignificant. The reanalysis without the main effect for periods yields a model fit with a residual (Chi)²-value of 6.51 and d=5 (p=0.25) and after correction for an otherwise existing DRL-level of 7% the following intrinsic DRL-effect and its standard deviation:

$\boldsymbol{\delta_i = -0.299}$ and $\boldsymbol{\sigma(\delta_i) = 0.119}$

or a DRL-effect of 25.9% that by the one-sided test .299/0.119 = 2.51 with d=5 is significant at p = 0.025.

This significant result violates *hypothesis 1* (no DRL-effect). Also the data for injury accidents within these MD- and MN-accidents are given, but the log-linear analysis revealed a nearly zero DRL-effect that, due to the relatively low numbers, is not significantly different from the above parameter. Nonetheless it is an indication that DRL may not affect the severity accidents and thus contribute slightly to the acceptance of *conjecture 7*. From the differentiation of MD-accident types it also can be observed that the MD-accidents with pedestrians for the three successive periods decreased from 22 to 15 and from 15 to 9, here indicating no DRL-effect on MD-accidents with pedestrians. Its lower DRL-effect for pedestrians contributes hardly to the rejection of *conjecture 3*, because these accidents differences (low numbers) have no statistical significance.

4.7.3. *Evaluation of Austrian railway and post fleets (KfV, 1990, 1993)*

The third study (KfV, 1990, 1993) concerns the introduction of DRL in two different fleets, one small fleet consisting of buses used by the Austrian State Railways and the other large fleets consisting of vehicles (buses, cars and freight vehicles) used by the Austrian Post. The first study (KfV, 1990) compares the accidents of one before and of one year after the introduction of DRL for both fleets, but the analysis did not show any DRL-effect. The second study (KfV) concerns the comparison of the accidents in two before years of 1.4.1987 to 31.3.1988 and 1.4.1988 to 31.3.1989, whereafter the usage of DRL was urged, with those in two after years of 1.4.1989 to 31.3.1990 and 1.4.1990 to 31.3.1991 and showed a positive DRL-effect for both of the fleets. The overall DRL-usage in Austria for all vehicles in the before-years is only about 7% and in the after-years about 10%, while the DRL-usage in the fleets was about zero in the before-years and ranged in the after-years from 67% to 77% (average 72%) for the buses of the railway and from 38% to 81% (average 60%) for the post vehicles. Apart from the MD-accidents comparison with the DRL-irrelevant MN-SD- and SN-accidents also a comparison for the injury accidents of the fleets with all Austrian injury accidents is made. The authors do not use a statistically optimal methodology for the analysis, but concluded from several comparisons that the introduction of DRL decreased the MD-accidents, especially for the second after-year and more evidently for the injury accidents (injury MD-accidents reduced by 28.7% compared to all Austrian injury MD-accidents which reduced by 2.6%), This DRL-effect is established, despite the fact that the post and railway vehicles already have a high conspicuity due to bright colours.

a. Reanalysis of State Railway fleet accidents
The log-linear model analysis of the MD-, MN-, SD-, and SN-accidents of two separate before- and two separate after-years for the railway fleet yields a model fit wherein the first order interactions with years are insignificant and without these interactions the residual (Chi)²-value of 3.8 with df=8 gives still a very good model fit and yields for the raw DRL-effect:

$\delta = -0.041$ and $\sigma(\delta) = 0.052$

When the same direction accidents are left out it gives $\delta = -0.058$, which indicates that the rear-end accidents are more affected by DRL. However, it must be remembered that none of the effects are statistically significant. Since the average after DRL-level is 72% and the otherwise overall existing DRL-levels of 7% the before-period and 10% in the after-period, the correction to an intrinsic DRL-effect as discussed in chapter 2 yields:

$\boldsymbol{\delta_i = -0.059}$ and $\sigma(\delta_i) = 0.075$

which means an insignificant intrinsic DRL-effect of 5.7%. Nonetheless it indicates slightly that *hypothesis 1* (no DRL-effect) may be incorrect.

b. Reanalysis of Post fleet accidents
The same analysis for accidents of the post vehicles gives significant first order interactions with residual (Chi)²-value = 0.85 with df=2 and yields:

$\delta = -0.064$ and $\sigma(\delta) = 0.070$

When the same direction accidents are left out it gives $\delta = -0.054$, which indicates that the rear-end accidents here are less affected by DRL (no statistically significant effect). The average after DRL-level for the post fleet is 60% and the existing DRL-level is 7% in the before-period and 10% in the after-period. The correction to an intrinsic DRL-effect yields:

$\boldsymbol{\delta_i = -0.108}$ and $\boldsymbol{\sigma(\delta_i) = 0.118}$

It means an insignificant intrinsic DRL-effect of 10.2.% that only slightly contributes to the possible rejection of *hypothesis 1* (no DRL-effect).
The MD-accidents with pedestrians in the railway and post fleets reduced from an average annual number of 176 in the before-years to the average annual number of 156.5 in the after-years or by 11%. It indicates that the DRL-effect on MD-accidents with pedestrians is somewhat higher than on MD-accidents between motor vehicles. Although also here the difference is not significant it again questions the acceptance of *conjecture 3*.

c. Reanalysis of casualties in Railway and Post fleet accidents
The single day and night casualties of both fleets are for several years zero and therefore the same analysis for casualties can not be performed, but the casualties in the MD- and MN-accidents for both fleets together are large enough for a log-linear model analysis that compares these casualties with casualties in all MD- and MN-accidents for Austria as quasi-control groups. The log-linear model with main effects for years, day/ night and control/experimental group as well as all their first order interactions and a DRL-effect (as second order interaction between before/after x day/night and control/experimental group) yields an excellent model fit with d=2 for the residual (Chi)²-value of 1.92 and a raw DRL-effect of:

$\delta = -0.096$ and $\sigma(\delta) = 0.232$

It indicates that the casualties in MD-accidents here are more affected by DRL. However, also here no effect is statistically significant and surely not the difference with the DRL-effect on all MD-accidents of both fleets. The average after DRL-level for the post vehicles and railway buses is 65% and correction to an *intrinsic DRL-effect on casualties in fleet MD-accidents* yields:

$\delta_i = -0.154$ and $\sigma(\delta_i) = 0.372$

This insignificant, but relatively higher DRL-effect on casualties in fleet MD-accidents of 14.3.% is a factor 1.64 higher than the weighted average of 8.7% for the DRL-effect on all post and railway MD-accidents (post twice the weight of railway: post has four times more casualties, but also a nearly twice as high error variance), which is close to the factor 1.74 found for the Canadian fleet study of Sparks et al. (1993). At least it is an indication that *conjecture 7* (no severity differences) may be incorrect.

4.7.4. *Combined estimate for a DRL-effect in Austria*

The four fleet results for Austria can be combined in the same way as for Canada or the USA into a DRL-effect for the country by the weights of results summarised in the next table, where the validity of the results for the fleet of the army with its theoretical error variance and for the bright coloured vehicle fleets of the railway and post are taken to be half the validity of the results for the vehicle fleet of the Graz town authority.

Study fleet	Intrinsic parameter	Inverse variance	Validity factor	Obtained weight	Prop. weight
1. Army	-.347	7.0	½	3.0	.018
2. Graz	-.299	70.6	1	70.6	.355
3. Railway	-.059	177.8	½	88.9	.447
4. Post	-.108	71.8	½	35.9	.180

Table 17. *Summary of results and weights for the DRL-effect in Austria*.

The combined estimates of the *intrinsic DRL-parameter on fleet MD-accidents* and its standard deviation, obtained by (6a) to (6h) of chapter 3, become

$$\boldsymbol{\delta_{if} = -0.158} \qquad \text{and} \qquad \boldsymbol{\sigma(\delta_{if}) = 0.086} \qquad \text{with } \mathbf{df=18}$$

Its one-sided t-test .158/.086 = 1.84 with df=18 shows a significance level of p = .04. The intrinsic DRL-effect on fleet MD-accidents in Austria becomes 14.6% and its 90% confidence interval ranges from 0.1% to 26.4%.

Applying the factor 1.64 for a higher DRL-effect on casualties in fleet MD-accidents, found for the post and railway fleet, to the above estimate one obtains the *intrinsic DRL-effect on casualties in fleet MD-accidents* as:

$$\boldsymbol{\delta_{if} = -0.274} \qquad \text{and} \qquad \boldsymbol{\sigma(\delta_{if}) = 0.149} \qquad \text{with } \mathbf{df=17}$$

However, because it are DRL-effects from fleet studies, it needs not to imply that the same DRL-effects in Austria will be observed after 100% DRL-usage from a national DRL-obligation. On the one hand fleet studies may be up-ward biased in their DRL-effect results, but on the other hand the two above DRL-effects are more than half determined by the individual DRL-results for fleets with bright coloured vehicles for which the DRL-effects are hypothesized to be lower.

In conclusion, although not very significant, the joint evidence from the Austrian fleets leads to the rejection of *hypothesis 1* (no DRL-effect). Although no where significant, the indications in these Austrian evaluations with respect to the difference between the DRL-effects on MD-accidents with pedestrians and on MD-accidents between motor vehicles contradict each other, which also leads to the conclusion that the joint evidence contributes to the confirmation of *conjecture 3* and *2* (equal DRL-effects for motor vehicle drivers and non-motorised road users or for encounters with one and two DRL-users). Also not significantly or univocally the data suggests that DRL-effect on rear-end accidents is also not much less than on other MD-accidents, which may contribute to the acceptance of *conjecture 6* (no different DRL-effects for types of MD-accidents between vehicles). Finally the data indicate also (not significantly) that casualties in MD-accidents are more prevented by DRL compared to all MD-accidents, which may contribute somewhat to the rejection of *conjecture 7* (no severity difference). No further information in the Austrian studies is relevant for other conjectures or hypotheses.

4.8. DRL-evaluations in Israel

4.8.1. *Evaluation of military heavy vehicle fleet (Hakkert, 1990)*

For experimental periods of compulsory use of DRL on all military heavy vehicles in the winter months Dec. 1986 and Jan. 1987 as well as from Nov. 1987 to Feb. 1988 the military heavy vehicle accidents are compared with the accidents of DRL-unaffected military cars and with single accidents in a classified study by Hakkert from which a research note is publicly available (Hakkert, 1990). The research note does not contain the absolute numbers of

the relevant accident data, but the proportional DRL-effects are reported. Casualties in MD-accidents with military heavy vehicles reduced in the DRL-periods during daytime and twilight by 43%, while the casualties in MD-accidents with the military cars without DRL during the same periods decreased by only 4%. Also no or only a negligible reduction in SD-accidents occurred. The damage-only accidents with military heavy vehicles fell by 27% compared to 14% reduction for these accidents of the military cars without DRL. Hakkert concludes that the DRL-effect is at least 10% to 15%, but also that this result has no statistical significance, due to the relatively low numbers of accidents. Nonetheless, taking the share of injury accidents to be 15%, the ratio
[0.73 x 0.85 + 0.57 x 0.15] /[0.86 x 0.85 +.96 x 0.15] = 0.706/0.875 = .806
gives the intrinsic DRL-effect on all MD-accidents in the Israeli mid-winter for this study, which estimates:

$\delta_{iw} = -.215$ and $\sigma(\delta_{iw})$ = unknown

It would mean a mid-winter DRL-effect of 19.4% on all MD-accidents in Israel. The DRL-effect on casualties in MD-accidents is estimated by the ratio of .57/.96=.593 or an effect of 40.7%, which would imply that the DRL-effect on casualties in MD-accidents is .407/.194 = 2.10 higher than on all MD-accidents. Although the DRL-effect certainly is not a significant effect, it contributes slightly to the rejection of *hypothesis 1* (no DRL-effect) and *conjecture 7* (on severity difference)

4.8.2. *Evaluation of a winter DRL-campaign (Hocherman & Hakkert, 1990)*

The effects of a DRL-campaign during the winter months of 1989/'90 in Israel are evaluated by Hocherman and Hakkert (1990). The DRL-level for the winter of 1988/'89, thus the winter before the campaign in the next winter, was 20% to 45% (average 32.5%) during inclement weather conditions and about 5% in bright weather. During the campaign the DRL-level was 45% to 65% (average 50%) in inclement weather and about 10% in bright weather. From these figures one may assume that the average before DRL-level in the winter was about 9% and the average DRL-level in the campaign winter about 16%.

Hocherman and Hakkert (1990) compared the MD-accidents to the MN-accidents between motor vehicles in two winters before the winter DRL-campaign with the same accidents during the winter DRL-campaign, but did not find a positive DRL-effect. Also these MD-accidents are differentiated for rural and urban roads, for frontal and right-angle accidents, for rainy and dry weather and for MD-accidents with pedestrians, but none of these sub-classes seemed to reveal a positive DRL-effect. The authors contribute the absence of a DRL-effect to the limited nature of the experiment (a three month DRL-campaign that concentrated on the DRL-usage during bad weather conditions, while the increase in DRL-usage was rather small, actually 7%).Indeed of the before/after ratios of the ratios of MD- and MN-accidents for identical defined subsets of day and night accidents show higher ratios than unity, indicating adverse DRL-effects.

It is very unlikely that DRL has an adverse effect on MD-accidents and most probably the daytime kilometrage in Israel has increased over the relevant years more than the nighttime kilometrage. The average total kilometrage of 1987/'88 and 1988/'89 increased by 10% to the kilometrage of 1989/'90. If the nighttime kilometrage is about 25% and increased also by 25% of the daytime kilometrage then the expected MD/MN ratio of the DRL-campaign winter would have increased (without a change in DRL-usage) by a factor of 1.125/1.025 = 1.098.

If one computes the alternative odds ratio of multiple accidents without the accidents with pedestrians in the Israeli winter as:

$$\frac{(\text{after MD})/(\text{after MN})}{(\text{average before MD})/(\text{average before MN})} = \frac{2068.0/1174.0}{1803.5/1124.5} = 1.098$$

Correcting this ratio for the expected increase from the above assumption of differential growth of day- and nighttime kilometrage gives an exactly zero DRL-effect. It would mean that an intrinsic DRL-effect on winter MD-accident between motor vehicles is not adverse, but absent.

That the intrinsic DRL-effect could be positive as well is sustained by the before/after ratio of the ratio of rainy weather MD-accidents and MN-accidents (not excluding the accidents with pedestrians, because rainy MD-accidents with pedestrians are not given) as:

$$\frac{(\text{after rainy MD})/(\text{after MN})}{(\text{mean before rainy MD})/(\text{mean before MN})} = \frac{362.0/1473}{375.5/1517} = 0.993$$

Adjusting this odds ratio also for the differential kilometrage increase of 1.098, the raw DRL-effect on all winter MD-accidents in rainy weather would become 0.993/1.098 = .904 or 9.6%. Correcting this raw DRL-effect for rainy weather conditions by the before and after DRL-levels of 32.5% and 50% in rainy weather conditions gives an intrinsic DRL-effect of:

$\delta_{iwr} = -0.370$ and $\sigma(\delta_{iwr})$ = unknown

which would mean that the actual intrinsic DRL-effect in bad winter weather conditions even could be as large as 31.6%. Since the inclement weather conditions only are present for about 15% in the Israeli winter, while the DRL-effect in good illumination conditions is taken to be zero (there hardly can be any DRL-effect from the negligible increase of DRL-usage in these conditions), it would mean that the intrinsic DRL-effect on winter MD-accidents could equal:

$\delta_{iw} = -0.048$ and $\sigma(\delta_{iw})$ = unknown

Since these estimates are highly dependent on the assumption of the speculative, although reasonable, assumption of a differential day versus night growth of kilometrage, it remains rather doubtful that there actually is a positive DRL-effect. However, the assumed minimum of $\delta_{iw} = 0$ and the derived maximum of $\delta_{iw} = -0.048$ give some confidence that actual intrinsic

DRL-effect on winter MD-accidents between vehicles from this DRL-campaign is close to the average of these estimates, thus as:

δ_{iw} = -0.024 and $\sigma(\delta_{iw})$ = unknown

Since the odds ratio for accidents with pedestrians (after/before ratio of the ratio of MD-and MN-accidents with pedestrians) is 1.298, even the correction for a differential day versus night growth of kilometrage does not lead to a positive DRL-effect for pedestrians in this case.

In conclusion one may say that this study does not contribute to the rejection of *hypothesis 1* (no DRL-effect) and that it contains a slight indication that the DRL-effect for pedestrians may be adverse, which may possibly indicate that *conjecture 3* (equal intrinsic DRL-effect for pedestrians) is questionable as well as *hypothesis 2* (all intrinsic DRL-effects equal).

4.8.3. *Combined evidence from the DRL-evaluations in Israel*

The two above estimates of an intrinsic DRL-effect in the Israeli winters are equally weak indication of some positive DRL-effect. The best one probably can do is to take again the average of the army result and the adjusted result of the DRL-campaign. Taking this average as the best possible DRL-estimate for Israel and taking the deviations of each estimate as the source for its standard deviation, it yields:

δ_{iw} = -0.120 and **$\sigma(\delta_{iw})$ = 0.135** with **df=1**

It means an insignificant intrinsic DRL-effect of 11.3% on MD-accidents between motor vehicles in the Israeli winter, obtained from a *mix of fleet and national data*. Because df=1 its 90% confidence interval ranges from very large adverse to very large positive DRL-effects. It expresses the relatively high uncertainty of the DRL-effect for Israel. If the factor 2.10 from the fleet study for the higher DRL-effect on casualties in MD-accidents is applied to the above estimate, the Israeli *intrinsic DRL-effect on casualties in MD-accidents from equally weighted fleet and national evaluations* would become:

δ_{iw} = -0.272 and **$\sigma(\delta_{iw})$ = 0.295** with **df=1**

Although these DRL-effects for Israel is in no way significant, these intrinsic DRL-effect still may contribute slightly to a rejection of *hypothesis 1* (no DRL-effect). The seemingly higher DRL-effect for casualties in MD-accidents may indicate that *conjecture 7* is to be rejected (equal DRL-effects for different accident severity). Also the absence of a positive DRL-effect on pedestrians from the DRL-campaign may indicate slightly that *conjecture 3* is not valid. No additional information in these studies contributes to other conjectures or hypotheses.

4.9. **DRL-evaluation in Hungary**

In Hungary the DRL-obligation was introduced per 1.3.1993 on main rural roads and semi-motorways. Hollo (1994) evaluated the DRL-effect by comparing the accidents in one before-year and one after-year. The before

DRL-level on the relevant roads is reported (Hollo, 1993) to range between 23% and 35% (average 30%) and the after DRL-level between 68% to 92% (average 82%). Because of the positive safety results of the DRL-obligation on the partial road network outside built-up areas, compared to remaining road rural network without a DRL-obligation and the increased DRL-usage that was observed without enforcement, DRL became compulsory on all roads outside built-up areas in Hungary from the data of 1.6.94 onward. Hollo (1995) again investigated the DRL-effect in the nine month from 1.6.94 to 28.2.95 with an overall DRL-level of 85.7% on all roads outside built-up areas, by comparing its accidents with the accidents in the same nine month of 1.6.92 to 28.2.93 as before-period without a DRL-obligation, but with a voluntary DRL-usage of 30%. In the second study (Hollo, 1995) the usual odds ratio method for the DRL-evaluation in both comparisons is used. Accidents with pedestrians were excluded and accidents with cyclists are not separated, but accidents with cyclists on Hungarian roads outside built-up areas constitute a very minor proportion. In the first investigation (Hollo, 1994) it was found that DRL does not or affect or affects less the rear-end accidents, the reason why Hollo (1995) analysed the odds ratio for all accidents without rear-end accidents for both evaluations. Hollo corrected the raw DRL-effect to an intrinsic DRL-effect, but used 42% and 90% or 85.7% as before and after DRL-levels in correction expression (1) for encounters with one DRL-user only. Instead the reported percentages of 30% for the before DRL-level and of 82% and 85.7% for the after DRL-levels should be used for a correction in expression (2) for encounters with one and two DRL-users, as discussed in chapter 2. For the corrected raw DRL-effects by that questionable former way, Hollo (1995) reports that the two evaluations yield 14.3% and 17.7% as intrinsic DRL-effects.

Hollo's first evaluation, using the before and after odds ratio comparison for the first partial DRL-obligation (only on semi-motorways and main rural roads), yields a raw DRL-effect of 7.3% on MD-accidents without rear-end accidents in all weather conditions. Transformed to an intrinsic DRL-effect (by the correct expression (2) with 30% before and 82% after DRL-usage) it gives 14.7% (instead of 14.3%) as the intrinsic DRL-effect on MD-accidents without rear-end accidents. So the DRL-parameter estimate for this first evaluation becomes δ_i = -0.159. The second evaluation concerns the effect of the full DRL-obligation (on all roads outside built-up areas) on the MD-accidents without rear-end accidents analysed by the odds ratio comparison of the 9 months before March 1993 (without any DRL-obligation) and the 9 month before March 1995 (with full DRL-obligation). This second evaluation yields a raw DRL-effect of 8.4% and corrected to an intrinsic DRL-effect, using 30% before and 85.7% after DRL-level it results in 17.0% (instead of 17.7%) as the intrinsic DRL-effect. So this second evaluation for the accidents from July to March, yields an intrinsic DRL-parameter of δ_i = -0.186.

Since the odds ratio methodology is used no error standard deviation is obtained. However, by using the data of both evaluations in the usual log-linear model analysis with two before-periods (overlapping partially in time and area) and two independent after-periods, the intrinsic DRL-effect and its standard deviation can be estimated. The model then asks for a regression variable v_t, based on the level of DRL-usage for each period, instead of two dummy variables for each DRL-effect. This analysis yields:

$\delta_i = -0.176$ and $\sigma(\delta_i) = 0.032$ with df=2

This DRL-parameter is about the same as the mean of the two correctly obtained intrinsic DRL-parameters from Hollo's odds ratio analysis. Since the data of the two before periods are partially dependent, it may be that the log-linear analysis with the low residual (Chi)²-value of .069 underestimates the standard deviation of the intrinsic DRL-parameter. However, the log-linear analyses without the one or the other before period give similar DRL-effects and even lower standard deviations with df=1. So the dependence of the two before periods does not influence the estimation very much, but the too good model fit remains somewhat suspect. Nonetheless the intrinsic DRL-effect of 16.1% on the MD-accidents without rear-end accidents on Hungarian roads outside built-up areas seems rather significant (one-sided t-test .176/.032 = 5.5 gives p=.02 for df=2). Its 90% confidence interval ranges from 7.9% to 23.6%.

The previous analysis of Hollo (1993, 1994) indicates that conjecture 6 (no differences of DRL-effects for different MD-accident types) may be rejected for the Hungarian data, because no DRL-effect seemed present for rear-end accidents. Since rear-end MD-accidents tend to increase independently of DRL every where in Europe, once again it could be assumed that the DRL-effect on rear-end accidents is not absent, but perhaps lower. If one assumes that the intrinsic DRL-effect on rear-end MD-accidents is a quarter of the effect on the other MD-accidents, as it was established to be for some other countries, then given that rear-end accident on the relevant roads are about 32% of all MD-accidents in Hungary, it follows that under this assumption that the national *DRL-effect all on MD-accidents* becomes:

$\boldsymbol{\delta_{in} = -0.131}$ and $\boldsymbol{\sigma(\delta_{in}) = 0.024}$ with **df=2**

which then would mean a significant intrinsic DRL-effect of 12.2% on all MD-accidents on all roads outside built-up areas in Hungary.

In conclusion also the results for Hungary violate *hypothesis 1* (no DRL-effect). There are indications that *conjecture 6* (equal DRL-effect for types of MD-accidents) may have to be rejected, since the rear-end accidents in Hungary seem hardly to be prevented by DRL. No other aspects in the data of Hungary are relevant for other conjectures or hypotheses.

5. Prediction of the different national DRL-effects

In the previous chapter 24 independent empirical studies on the safety effect of increased DRL-usage or of the obligation of DRL are reviewed. 8 studies concern national studies for the effect of a DRL-obligation in 6 countries (2 studies for both Norway and Hungary). One national and one regional study concern the safety effect of a DRL-recommendation. The other 14 studies evaluate the safety effect from the use of DRL in fleets. In order obtain statistically unbiased and comparable results with minimal effects of different analysis method, most studies are re-analysed by the same optimal analysis method (where possible) and the results expressed by intrinsic DRL-effects on MD-accidents and/or on their casualties.

None of all these evaluations or re-analysed data of these studies shows an adverse DRL-effect, or it must be the Israeli study on the recommendation of DRL-usage in inclement winter weather (hardly increasing the actual DRL-usage). Only 8 DRL-evaluations show an intrinsic DRL-effect that is smaller than its also estimated standard deviation. However, in 3 of these evaluations the standard deviation is so high that the intrinsic DRL-effect still is higher than 20%. Of the remaining 5 evaluations the DRL-effect is less than 10%, but 2 of them concern the effect of DRL-recommendations in southern regions (Texas and Israel) and the other 3 evaluations concern DRL-effects on brightly coloured vehicle fleets (yellow cabs in New York and Austrian railway and post fleets). For 9 countries on the northern hemisphere the intrinsic DRL-effects are combined in national estimates. For 8 countries it yields a statistically significant DRL-effect and only for Israel an insignificant positive DRL-effect. It proves the very *significant rejection of hypothesis 1* (no DRL-effect). Thus one concludes:

Conclusion 1:
There undoubtedly is a positive road safety effect from the use of DRL (or increased DRL-usage percentages).

However, in view of the established DRL-effects, the intrinsic DRL-effects are not the same for countries and possibly also not for specific types of accidents and for fleet or national DRL-evaluations.

5.1. DRL-effect differences between countries, types and severity of accidents

In a meta-analysis of some DRL-studies that are also included in the previous chapter, but by taking most results as published, Koornstra (1989) demonstrated that the differences in DRL-effects per country are related with the latitude of the country. Later Koornstra (1993) re-analysed some results and adjusted the results correctly to intrinsic DRL-effects and showed that there exists a curvilinear relation between the intrinsic DRL-effect and the latitude of the country. Elvik (1996) in his meta-analysis also found indications for a weak relation between DRL-effect and latitude. Elvik questioned the fit and the mathematical function, used by Koornstra, for that relationship, but Elvik used three differently defined DRL-effects for each country (all three not unbiased DRL-effect estimates).

A relationship with latitude is mainly explained by the lower visibility for more northern countries due to the lower average angle of the sunlight. Therefore, one also has to differentiate between DRL-effects derived from accident data for the whole year and derived from the winter (or summer) accident data. This is not acknowledged in the meta-analyses of Koornstra (1993) as well as Elvik (1996). Moreover, both meta-analyses do not differentiate between DRL-effects on casualties in MD-accidents and on all MD-accidents and also not between DRL-effects on all MD-accidents and on MD-accidents without same direction accidents or without rear-end accidents. If the DRL-effects are equal for these different accidents as formulated respectively in conjecture 7 (equal DRL-effects for different severity classes of accidents) and conjecture 6 (equal DRL-effects for different types of accidents between motor vehicles), these differentiations do not influence the relationship between latitude and DRL-effects. Here it is argued that one first must test whether these conjectures are acceptable and if not the relationship may be different for DRL-effects of differently defined accidents outcomes.

From the 24 DRL-evaluations reviewed or re-analysed in the previous chapter only 8 contain some information on the comparison of the DRL-effects on same direction or rear-end accidents compared to other accidents between motor vehicles. Only one study (Cantilli, 1970) indicates a higher DRL-effect for rear-end accidents than on crossing accidents, all other studies give indications for the reverse, although none established the significance of the difference for a seemingly adverse (Elvik (1993), probably caused by other DRL-irrelevant developments) or a lower effect on rear-end accidents. The distribution of one positive difference and 7 negative differences gives, according to the sign test, a probability of less than $p = 0.05$ for no difference. The joint available evidence, therefore, shows a *significant rejection of conjecture 6*, stating equal DRL-effects on different manoeuvre types of MD-accidents between motor vehicles. Thus one arrives at:

Conclusion 2:
The intrinsic DRL-effect on rear-end MD-accidents is statistically significant lower than on other MD-accidents.

In the reviews and re-analyses of data for the Canadian study of Sparks et al. (1993), the Norwegian study of Elvik (1993) and the Danish study of Hansen (1993, 1994), the proportionally lower DRL-effects on (casualties in) same direction and on (casualties in) rear-end MD accidents are estimated. There the DRL-effect on rear-end accidents is estimated consistently to be a factor .25 of the DRL-effect on other MD-accidents between motor vehicles and the DRL-effect on all same direction MD-accidents (also including passing accidents) is estimated to be a factor .5 of the DRL-effect on other MD-accidents between motor vehicles.
The originally derived intrinsic DRL-effects for Canada, Norway and Hungary, which are based on MD-accidents without coincident direction accidents or without rear-end accidents, are adjusted to intrinsic DRL-effects on all MD-accidents between motor vehicles by these estimated factors and the respective shares of the same direction or rear-end accidents

in all MD-accidents between motor vehicles. Since these adjustments depend on different factors for different shares of these rear-end or same direction MD-accidents these corrections are unique of each country.

For the establishment of a relationship between intrinsic DRL-effect and latitude (or adjusted latitude as average angle of the sunlight in the seasonal evaluation period of a country), one has to have identically defined DRL-effects. Therefore, in the further predictive analyses these uniquely adjusted intrinsic DRL-effects on all MD-accidents or on casualties in MD-accidents for Canada, Norway and Hungary are used in combination with the directly analysed intrinsic DRL-effects on all MD-accidents or on casualties in MD-accident for the other countries.

From all the 24 individual DRL-evaluations reviewed only 7 contain some information on the comparison of the DRL-effects on casualties in MD-accidents and on all MD-accidents. Only for one study a significant higher DRL-effect on casualties than on accidents (Sparks et al, 1993) is established, but also only for one a slight indication for a lower DRL-effect on casualties than on all accidents (Schützenhofer et al. 1990) is observed. All other five studies revealed an insignificant or unspecified lower DRL-effect for all accidents than for casualties. The sign-test of the distribution of 6 positive and one negative differences from 7 independent studies gives a probability of less than $p = 0.05$ for no difference. This joint evidence of a difference and the one significant higher DRL-effect on casualties than on accidents, leads to the *significant rejection of conjecture 7* that formulates the equality of DRL-effects on MD-accidents categories with different severity outcomes.

Conclusion 3a:
The intrinsic DRL-effect on casualties in MD-accidents is statistically significant higher than on MD-accidents.

Only for three studies the factor by which the intrinsic DRL-effect on casualties in MD-accidents increases compared to the intrinsic DRL-effect on MD-accidents could be estimated relatively reliable, because the data for both severity types are analysed (Sparks et al. 1993; KfV, 1993; Hakkert, 1990). In all three cases it concerns a factor derived from fleet DRL-evaluations with factors of 1.74 (Canada), 1.64 (Austria) and 2.10 (Israel). For Canada this factor is taken to upgrade the national DRL-effect on all MD-accidents to an estimate of national DRL-effect on casualties in MD-accidents in Canada, for Austria to upgrade the combined DRL-effect on fleet MD-accidents to a combined estimate of the DRL-effect on casualties in fleet MD-accidents in Austria and for Israel to upgrade the combined DRL-effect on MD-accidents (mixed fleet and national data) to an estimate of the DRL-effect on casualties in MD-accidents in Israel.

In order not to loose these quantitative information from within these studies these additional upgraded DRL-effects on casualties in MD-accidents for these three countries are also used together with the directly derived DRL-effects on casualties in MD-accidents for Nordic countries in the predictive analysis of the relationship between DRL-effects and latitude of the countries. The joint estimation of a quantitative factor for the proportional difference between intrinsic DRL-effects on casualties in MD-accidents and on MD-accidents can be obtained from a parameter for a dummy variable (all=0, casualties=1) in the model for that relationship derived from all available DRL-effects (both on MD-casualties and on MD-accidents) for different countries and seasonal periods.

Next one has to consider the possibility of biased DRL-effects from DRL-evaluations of fleets that have introduced DRL, because the introduction of DRL in fleets may be accompanied by a higher safety awareness of its drivers. Only for Canada there exist a possibility for an estimation of such possible fleet bias (here an up-ward effect), because it is the only country with independent and reliable estimates of DRL-effects from fleet studies and from the evaluation of the DRL-obligation. The proportional difference factor for the DRL-effect on all MD-accidents in Canada is 1.59. Although this indicates that there is an up-ward fleet bias in the DRL-effects, it is insufficient to estimate reliably that fleet bias only from data for one country. However, if there is a relationship between DRL-effects and latitudes (or adjusted latitudes by average deviation of the seasonal sunlight angle for countries with seasonal evaluation periods) a fleet bias can be estimated more reliable and tested for its significance by a parameter for a dummy variable (fleet=0 and national=1) in a model for that relationship derived from all fleet and national DRL-effects available for different countries and seasonal periods.

In the next table all the available DRL-effects for the establishment of a possible relation between intrinsic DRL-effects and latitudes (plus or minus seasonal adjustment) are summarised (in rankorder of DRL-effects).

Country	x_1	x_2	δ_i	$\sigma(\delta_i)$	Period	Latitude +/- season	%DRL
Swe.(1)	0	0	1.464	0.685	nov.-feb.	59.5 + 16.0 = 75.5	76.9
Fin.	0	0	1.100	0.358	nov.-march	63.0 + 13.5 = 76.5	66.7
Nor.*	0	0	0.557	0.128	year	62.0 = 62.0	42.7
Can.*[u]	1	0	0.488	0.099	year	49.0 = 49.0	38.6
Swe.(2)	0	0	0.360	0.158	may-sept.	59.5 - 13.5 = 46.0	30.2
Den.	0	0	0.358	0.141	year	55.5 = 55.5	30.1
Swe.(3)	0	0	0.304	0.682	mar/apr./oct.	59.5 + 1.0 = 60.5	26.1
Can.*[u]	0	0	0.287	0.082	year	49.0 = 49.0	24.9
Aus.[u]	1	0	0.274	0.149	year	47.5 = 47.5	24.0
Isr.[u]	x_1	0	0.272	0.292	mid winter	32.0 + 14.0 = 46.0	23.8
Can.*	1	1	0.259	0.052	year	49.0 = 49.0	22.8
USA	x_1	1	0.169	0.049	year	39.0 = 39.0	15.5
Aus.	1	1	0.158	0.086	year	47.5 = 47.5	14.6
Can.*	0	1	0.155	0.045	year	49.0 = 49.0	14.3
Hun.*	0	1	0.131	0.024	year	47.0 = 47.0	12.2
Isr.	x_1	1	0.120	0.135	mid winter	32.0 + 14.0 = 46.0	11.3

* adjusted effects by factors for shares of excluded same direction or rear-end accidents
[u] upgraded to effects on casualties by the national factors for the effects on MD-accidents.
For the USA x_1=0.915; Israel x_1=0.5, because of the mixed fleet/regional proportions

Table 18. *Summary of all the available DRL-effects for the establishment of a possible relation between intrinsic DRL-effects and latitudes (plus or minus seasonal adjustment).*

In order to have more variation in adjusted latitudes the three independent estimates for Sweden with different seasonal periods and the upgraded DRL-effects on casualties in MD-accidents for Canada, Austria and Israel, are included. Together there are 16 intrinsic DRL-effects for nine countries. Only the four upgraded DRL-effects are transformations of other combined DRL-effects by three proportional factors that are independently derived from evaluations of DRL-effect differences between casualties and MD-accidents in these countries. From the four Canadian estimates only two are directly derived from a national and combined fleet evaluations.

The first part of the table contains nine DRL-effects on national casualties in MD-accidents, denoted by 0 in the second dummy variable x_2. The second part shows six DRL-effects on all MD-accidents, denoted by x_2=1. The first dummy variable x_1 has 0-values if the DRL-effect is derived fully from a national evaluation of the DRL-obligation, while for x_1=1 it is a DRL-effect that is derived fully from fleet DRL-evaluations. Since the combined DRL-effect for the USA concerns mainly fleet DRL-effects, but because it is for a proportion of 8.5% determined by a regional DRL-campaign effect the value of x_1 is set to x_1 = 0.915. The value of x_1 = 0.5 for Israel reflects that the combined DRL-effect is equally derived from a fleet (army) and a national (DRL-campaign) evaluation. The values of the latitudes are the average latitudes of the traffic in a country (for example for Canada much closer to the USA-border than the average latitude of Canadian territory). In case the evaluation concerns seasonally different periods and not a full year, these latitudes are adjusted to lower or higher values by deviations of the sunlight angle from the average angle for latitude (for example: November to February a lower sunlight angle of in average 16 degrees is added.

Clearly there is a relation between (adjusted) latitude and level of DRL-effect. DRL-effects on all MD-accidents are only obtained for (adjusted) latitudes below 50 degrees; however, of nine DRL-effects on casualties in MD-accidents four are at (adjusted) latitudes below 50 degrees.

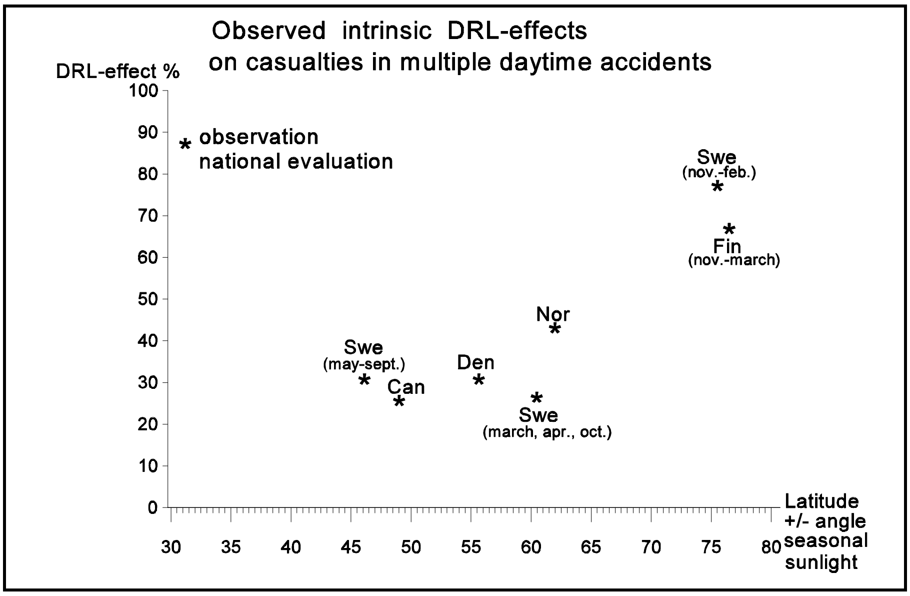

Figure 5. *The first seven national DRL-effects on casualties in MD-accidents plotted against the (adjusted) latitude.*

In order to inspect the nature of the possible relationship between DRL-effects and adjusted latitudes without disturbance of effects from the differences described by the dummy variables x_1 and x_2, the first seven national DRL-effects on casualties in MD-accidents are plotted against the (adjusted) latitude in the *Figure 5.*

The evident relation between DRL-effects on casualties in MD-accidents and the (adjusted) latitude can be seen to be linear as well as curvilinear, but a linear relation would predict adverse DRL-effects somewhere below the latitude of 35 degrees. Because the DRL-effect for the Swedish intermediate months (March, April and October), that deviates the most from a curvilinear relation, has a four times higher standard deviation (together with the Swedish mid-winter effect) than the next high one (Finland) and because DRL is supposed to have no adverse effects below a certain latitude, a curvilinear relation between DRL-effects and latitudes is hypothesised. In order to estimate simultaneously the significant effect of accident severity and the possible fleet bias, two models with proportional effects for the two dummy variables on a curvilinear function between DRL-effect percentages and (adjusted) latitudes are formulated.

Model 1: DRL % = $\{\exp(a_1.x_1 + a_2.x_2)\}.\{a_0 + b.(\text{degrees})^c\}$

Model 2: DRL % = $\{\exp(a_1.x_1 + a_2.x_2)\}.\{a_0 + b.[\tan(\text{degrees})]^c\}$

Apart from the modification by the parameters for the dummy variables, model 2 is the model that Koornstra (1993) found to be the optimal model for his meta-analysis without fleet and severity effects. Here a_1, a_2, a_0, b and c are the five parameters to be estimated. These model parameters are fitted to the 15 observed DRL-effects (one upgraded Canadian effect is left out, because fully dependent on the other three Canadian effects) by weighted least squares. The weights (for fitted δ_i-parameters in the transformed function) are the inverse variances $(1/\{\sigma(\delta_i)^2\}$. These weights are needed for an efficient estimation and an optimal significance test.

It turned out that a_0=0.034 in both models, but that this parameter is not significantly different from zero. In both models parameter a_1 is significantly positive and parameter a_2 significantly negative. It means that there is a significant proportional up-ward fleet bias in the fleet DRL-effects and that there also is a significant proportional down-ward effect for DRL-effects on all MD-accidents compared to DRL-effects on casualties in MD-accidents. Also parameter b is significant and parameter c significantly different from unity. Therefore, the models with a_0=0 are further considered. It simplifies the models, because the models only have a multiplicative factor that depends on the dummy variables and a curvilinearity parameter.

Model 2 gives c=0.811 and thus reduces the curvature of the tangent function, this in contrast to the result in Koornstra (1993) where the curvature is increased for c=1.54. Clearly his incorrect assumption that DRL-effects on casualties in MD-accidents are not higher than on MD-accidents, while the former are mainly to be found on relatively high latitudes, has caused an increased curvature. Nonetheless, in the models fitted the curvilinearity of the relation is significant. Model 2 does fit slightly less well (not significantly less) than Model 1. Although Model 2 is theoretically more justified than Model 1, because the tangent measures the average length of shadows and the travel length of the sunlight trough the atmosphere (determining contrasts and ambient illumination), the somewhat simpler and better fitting Model 1 is used. For Model 1 the parameter c = 2.329 and parameter b = .00279, while $\exp(a_1) = 1.418$ and $\exp(a_2) = 0.596$. Thus the up-ward fleet bias raises the DRL-effect by 41.8% of the national DRL-effect and the DRL-effect on all MD-accidents is 41.4% lower than the DRL-effect on casualties in MD-accidents. It explains 97.5 % of the variance in the DRL-effects by these four model parameters. The multi-plicative parameters $\exp(a_1.x_1).\exp(a_2.x_2).b$ define four multiplicative parameters b_{00}, b_{10}, b_{01}, and b_{11} with respect to the four prediction curves.

The two curves for DRL-effects on casualties in MD-accidents are described by:

$$100[1-\exp(\delta_i|\text{nation,casualty})] = b_{00}(\text{degrees})^c = 0.00279(\text{degrees})^{2.329}$$

$$100[1-\exp(\delta_i|\text{fleet,casualty})] = b_{10}(\text{degrees})^c = 0.00396(\text{degrees})^{2.329}$$

These curves together with their correspondingly defined DRL-effect observations are plotted in the next figure.

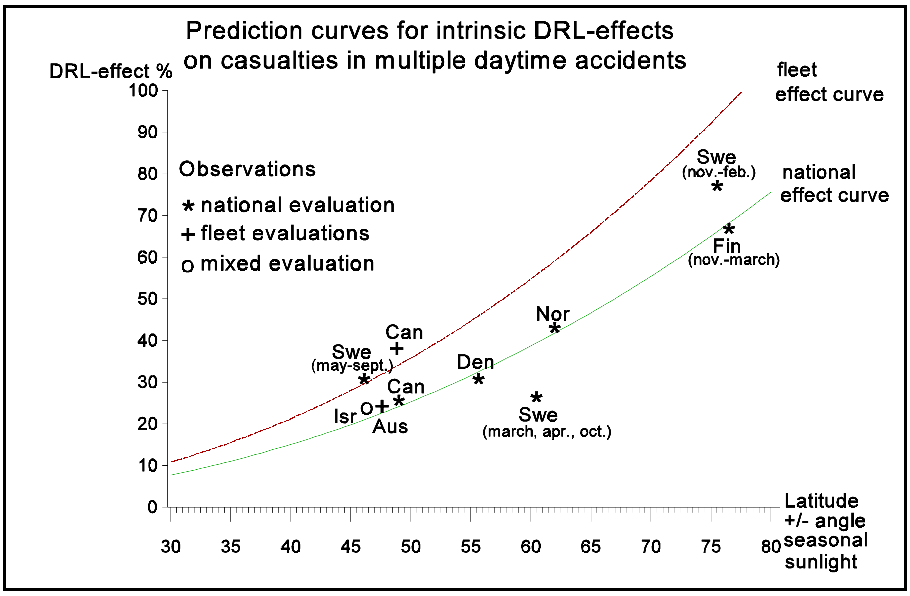

Figure 6. *Prediction curves for intrinsic DRL-effects on casualties in multiple daytime accidents, with their corresponding defined DRL-effect observations.*

The two fitted curves for DRL-effects on all MD-accidents are described by:

$$100[1-\exp(\delta_i|\text{nation,all MD})] = b_{01}(\text{degrees})^c = 0.00166(\text{degrees})^{2.329}$$

$$100[1-\exp(\delta_i|\text{fleet,all MD})] = b_{11}(\text{degrees})^c = 0.00236(\text{degrees})^{2.329}$$

and these curves also are displayed in the *Figure 7* together with their related observations.

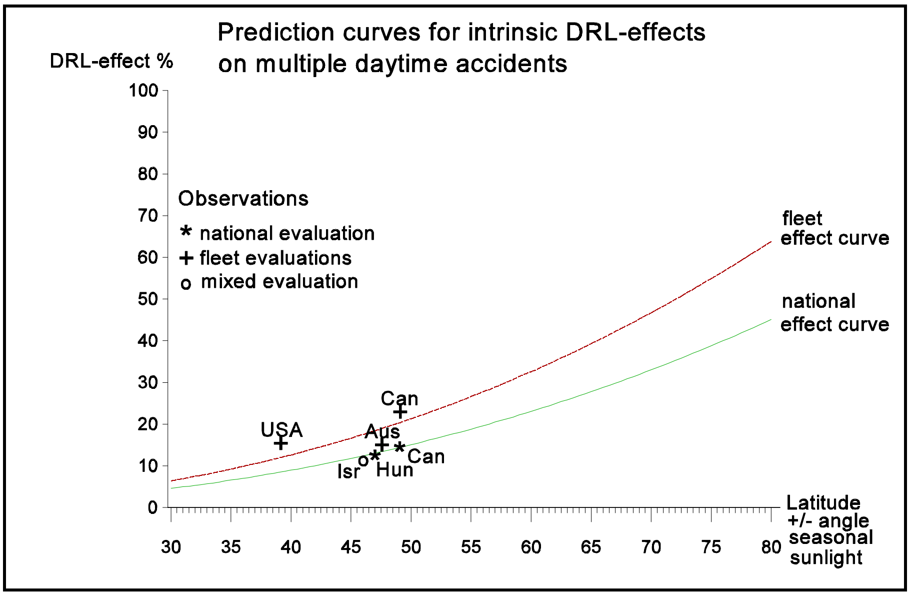

Figure 7. *Prediction curves for intrinsic DRL-effects on casualties in multiple daytime accidents, together with their related observations.*

Large deviations from these curves with respect to the standard deviations of the observed effects are for both fleet DRL-effects in Austria and for the Swedish mid-summer DRL-effect from the national effect curve for casualties in MD-accidents. Because the fleet DRL-effects in Austria are for 62.7% determined by the DRL-effects for the brightly coloured railway and post fleets the proportional difference between the observed and predicted DRL-effects of 0.75 is largely explained if bright colours of vehicles reduce the DRL-effect by 40%. Also the yellow cab study in the USA shows about a 40% lower DRL-effect than the expected fleet DRL-effect for the USA. By this meta-analysis of the DRL-effects several hypothesis and conjectures are tested, while by this analysis also quantitative specifications for rejected hypotheses and conjectures are obtained.

Firstly, conclusion 3a can be more precisely specified.

Conclusion 3b:
The intrinsic DRL-effect on casualties in MD-accidents is statistically significant higher than on MD-accidents by a multiplicative factor of 1.678.

Secondly, there is a quantified upward fleet bias that is statistically significant. Therefore, *conjecture 1 is to be rejected significantly*. This quantified fleet bias effect is formulated in the next conclusion.

Conclusion 4:
The intrinsic DRL-effects obtained from fleet evaluations is upwardly biased compared to DRL-effects obtained from national evaluations by a statistically significant multiplicative factor of 1.418

An additional conclusion, not related to a formulated conjecture, is in line with expectations from the perceptual explanation of a DRL and reads as:

Conclusion 5:
The intrinsic DRL-effect for bright coloured (yellow, orange-red) vehicles probably is about half the intrinsic DRL-effect for the national vehicle fleet.

The most important conclusion, however, is that there is significantly curvilinear relation between DRL-effects and the (adjusted) latitude. It leads to the *significant rejection of hypothesis 3* (no correlation). The almost significant DRL-effect difference between the Swedish mid-winter and mid summer together with the nearly identical results for Models 1 and 2, only can mean that the relation with (adjusted) latitude is to be explained by the reduced contrasts and visibility in countries and seasons with an average lower sunlight angle. The significant curvilinear relation also leads to the *significant rejection of conjecture 5* (no summer and winter difference). The main conclusion from the meta-analysis is:

Conclusion 6:
The intrinsic DRL-effects and the deviation from 90 degrees of the average sunlight angle for daytime traffic (measured by the latitude of the traffic adjusted for seasonal angle differences) have a significantly positive and significantly curvilinear relation, which allows the fairly accurate prediction of the national different DRL-effects on MD-accidents and on casualties in MD-accidents from the average latitude of the traffic in a country.

In the 24 DRL-evaluations, 8 evaluations contain 10 quantitative assessments on both DRL-effects for non-motorised road users and motor vehicles. All 10 DRL-results for non-motorised road users obtained from the 8 relevant evaluations, showed no significant difference between intrinsic) DRL-effects for motorised and non-motorised road users. Moreover, out of the 10 results for non-motorised road users 4 are higher and 6 are lower than for drivers. It indicates hardly any difference. Only for three countries (Finland, Norway and Denmark) the standard deviations of the differences between the two intrinsic DRL-effects could be estimated by the re-analyses of the data. The average difference and its standard deviation from the pooled variances are $\delta_{i,diff}$=-0.017 and $\sigma(\delta_{i,diff})$=0.687. Because the nordic intrinsic DRL-effects for motor vehicles are high it means that the DRL-effects for non-motorised road users are about 1% lower, but also that this difference has no statistical significance. Therefore, the *statistical acceptance of conjecture 3* on the equivalence of these two intrinsic DRL-effects is guaranteed. This is stated as the next conclusion.

Conclusion 6:
The intrinsic DRL-effects for motor vehicles and non-motorised road users are not significant different.

Only from the analysis of the Norwegian and the Danish data for the evaluation of the DRL-obligation, it has been possible to test the presence of a possible adverse effect of the increased DRL-usage for motorcyclists who already fully use DRL. Both analyses showed a adverse effect of 1%. Because both almost negligible adverse DRL-effects are in no way significantly different from zero, the *statistical acceptance of conjecture 8* (absence of an additional effect) is formulated as the following conclusion.

Conclusion 7:
The intrinsic DRL-effect for motorcyclists already using DRL is not significantly changed by an increase of DRL for other (four-wheeled) motor vehicles.

This conclusion and conclusion 6 imply that DRL-effects on encounters with only one DRL-user are not lower than DRL-effects on encounters with two DRL- users. Together with the insignificant difference between intrinsic DRL-effects on MD-accidents with one and with two DRL-users from the re-analysis of the national dat for Canada (the only data that allows a quantitative test of that difference), it guarantees the *statistical acceptance of conjecture 2*. This is formulated in the next conclusion.

Conclusion 8:
The probabilities of accident avoidance due to DRL in encounters between motor vehicles with only one and with two DRL-using motor vehicles are virtually not different.

Only because of conclusion 3b (or 3b) on the significant up-ward fleet bias of intrinsic DRL-effects from fleet studies, *hypothesis 2 is significantly rejected* (stating that intrinsic DRL-effects under equal levels of ambient illumination are assumed to be equal for fleet and national studies as well as for motorised and non-motorised road users), despite conclusion 8 and conclusion 6. This significant rejection invalidates partially the results of the meta-analysis of Koornstra (1993) and of Elvik (1996), because both meta-analyses are based on hypothesis 2.

The remaining conjectures that to be discussed concern the possibilities of a novelty effect or risk adaptation to DRL, which may lead to a diminishing DRL-effect in the longer run. Conjecture 4a states the absence of such an adaptation and conjecture 4b specifies that absence for DRL-effects on non-motorised road users. The latter specific conjecture hardly is highlighted by the re-analyses of relevant data in the previous chapter, because of the relatively high standard deviation of error for DRL-effects for non-motorised road users. However, three studies with a longer time-series after the full use of DRL shows for all three time-series that there is no significant adaptation to DRL for drivers. These studies for the Canadian fleet study of Sparks et al.(1993) and the national Norwegian and Danish studies contain the only available time-series whereby the presence of adaptation can be tested. Thus the *statistical acceptance of conjecture 4a* as formulated in the next conclusion is assured.

Conclusion 9:
The effect of DRL is significantly not diminishing in the long run after the introduction of a DRL-obligation.

One hypothesis that is crucial for the validity of the DRL-effects from the re-analysis by the log-linear model and also from the odds ratio methodology is not actually tested by the reviewed and re-analysed studies in previous chapter. It concerns the plausible hypothesis 4 for the absence of influences on single accidents and multiple night accidents from increased DRL-usage. This hypothesis can only be tested in the design of a controlled DRL-experiment and not by quasi-experimental designs for DRL-evaluations. The only studies with a controlled experimental design are the study of Stein (1985) in the USA and to some extent the study of Sparks et al. (1993) in Canada, but these studies contains the data differentiation between DRL-relevant and DRL-irrelevant accidents and not between single/multiple and day/night accidents.

Hauer (1995) has conjectured that more evasion manoeuvres due to DRL and more burned out car lamps due to their daytime use for DRL cause more single daytime and multiple night accidents. The only relevant study of Stein (1985), that included multiple night accidents in the DRL-irrelevant accidents, indicates that the multiple night accidents of the control (without DRL) and experimental group (with DRL) are unaffected, but the best one can say is that evidence for any substantial influence of DRL-usage on single and night accidents is mainly lacking. It leads to the next and last conclusion.

Conclusion 10:
The evidence of any substantial DRL-influence on single and night accidents is mainly absent, while the scarce evidence and the plausibility of its absence makes it very likely that there is virtually no influence of DRL-usage on single and night accidents.

This last conclusion hypothesis 4 is hardly based on an empirical confirmation and thus obtains the status of an axiomatic corollary that is assumed on plausibility grounds and scarce empirical evidence. This conclusion, however, determines the validity of the log-linear analysis of the quasi-experimental time-series with intervention for most of the statistical evaluation of DRL-effects in chapter 4.

5.2. Prediction of the DRL-effect for countries in the EU

The conclusions from the meta-analysis in the previous section and its fitted prediction formulas allows the transformation of seasonal DRL-effects and of fleet DRL-effects to whole year national DRL-effects. Firstly, the ratio of the expected effect for the seasonally adjusted and the actual average traffic latitude of a country transforms the seasonal DRL-effect to a whole year effect for the country and secondly, the division of the fleet DRL-effect of a country by the fleet bias of 1.418 transforms the fleet DRL-effect to a national DRL-effect. For the countries 9 countries one obtains in this way 7 national DRL-effects on casualties in MD-accidents (4 directly observed for Norway, Sweden and Denmark and, empirically up-graded, for Canada; the other 3 indirectly derived). For the 5 national DRL-effects on MD-accidents only two 2 are directly observed (Hungary and Canada) and 4 are indirectly derived. The next table summarizes the directly observed or indirectly observed DRL-effects and for the latter one the relevant transformation factors.

Country	Obs. DRL %		Correction			Nat. DRL %	
	all	injury	lat.	fleet	ratio	all	injury
Finland	-	66.7	-13.5	no	0.636[1]	-	42.8
Norway	-	42.7	-	no	-	-	42.7
Sweden	-	43.4	-	no	-	-	43.4
Denmark	-	30.1	-	no	-	-	30.1
Canada	-	24.9*	-	no	-	-	24.9
Canada	14.3	-	-	no	-	14.3	-
Austria	-	24.0*	-	1.0	0.705[2]	-	16.9
Austria	14.6	-	-	1.0	0.705[2]	10.3	-
Hungary	12.2	-	-	no	-	12.2	-
USA	15.5	-	-	.915	0.790[3]	12.2	-
Israel	-	23.8*	-14.0	.50	0.353[4]	-	8.4
Israel	11.3	-	-14.0	.50	0.353[4]	4.0	-

* up-graded by the empirical ratio for injury/all from that country
1 down-graded by ratio of predicted DRL-effects at latitudes 63 and 76.5 as 0.636
2 down-graded by fleet bias of 1/1.418
3 down-graded for 91.5% by fleet bias of 1/1.418
4 down graded by square root of fleet bias = 0.840 times the ratio of predicted DRL-effects at latitudes 32 and 46 as 0.420

Table 19. *Summary of the directly observed or indirectly observed DRL-effects and for the latter one the relevant transformation factors.*

For Canada, Austria and Israel there are independently established empirical differences between the DRL-effects on all MD-accidents and casualties in MD-accidents, whereon their upgraded combined DRL-effects are based. Consequently only for these three countries there are two directly or indirectly derived DRL-effects, one on casualties in MD-accidents and one on all MD-accidents. In other countries either the DRL-effects on casualties in MD-accidents or on all MD-accidents are not researched. One could estimate these missing DRL-effects by transforming the given DRL-effect for the expected difference between the two, but such effects hardly can be regarded as derived from observed national DRL-effects. All twelve national intrinsic DRL-effects in the above table are in some way derived from observed raw DRL-effects within the country itself.

The next two figures display the twelve national DRL-effects together with their minimum prediction curves and their p=.05 confidence curves.

The minimum prediction curves are the lower 90% confidence interval curves derived from the standard deviation of the parameters that describe the optimal prediction curve. It means that all actual DRL-effects for any country will be found by 95% chance above that minimum prediction curve for the respective types of accident outcomes.

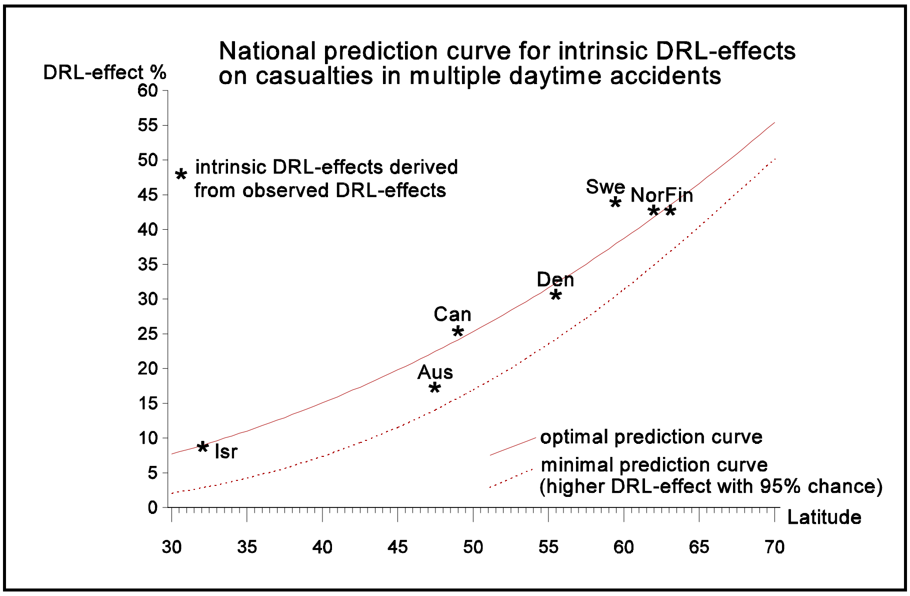

Figure 8. *National prediction curve for intrinsic DRL-effects on casualties in multiple daytime accidents (with their minimum prediction curves and their p=.05 confidence curves).*

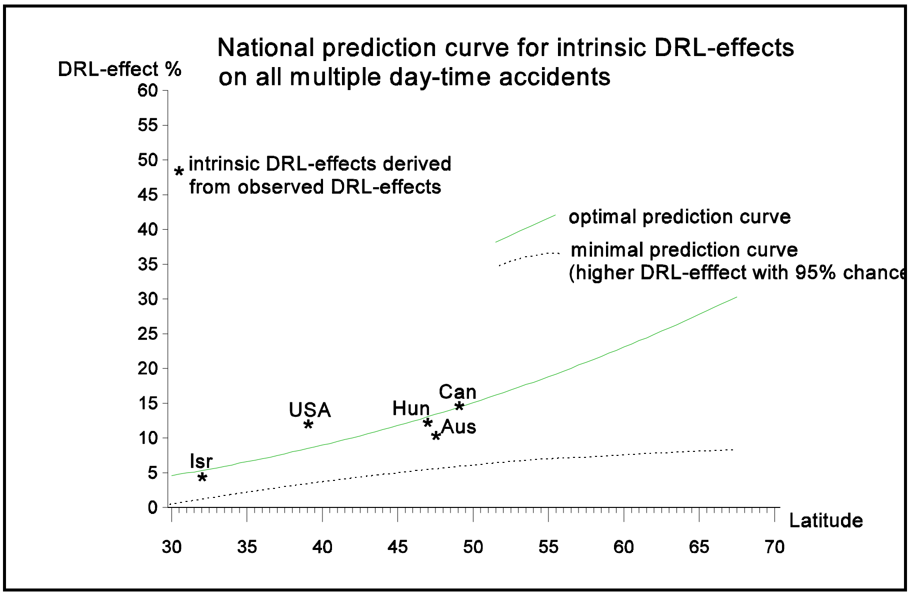

Figure 9. *National prediction curve for intrinsic DRL-effects on all multiple daytime accidents (with their minimum prediction curves and their p=.05 confidence curves).*

There are no observed DRL-effects on MD-accidents above the latitude of 49 degrees. As a consequence of the relative high uncertainty for these DRL-effects above of 49 degrees, the minimum prediction curve for MD-accidents deviates above 49 degrees more from its optimal prediction than the minimum prediction curve for casualties in MD-accidents deviates from its optimal curve. However, if it is assumed that the DRL-effects on all MD-accidents differ by a constant factor from the DRL-effects on casualties in MD-accidents, then the uncertainty of the prediction for all MD-accidents is the somewhat proportionally increased uncertainty of the prediction for casualties in MD-accidents. The relative increase of the uncertainty is due to the additional uncertainty of that constant factor for the transformation into DRL-effects on MD-accidents, which latter uncertainty is theoretically not dependent on the latitude. It would define a similar shape for both minimum prediction curves without increasing deviations of the minimum prediction curve for DRL-effects on MD-accidents above 49 degrees.

The fact that DRL-effects on casualties in accidents are higher than those on MD-accidents only can be explained by the lower collision speeds in just not avoided accidents with DRL-using motor vehicles. The effects of DRL are mainly explained by the improvement of the perception of other road users with DRL. Still sometimes there is a too late perception for the full avoidance of an accident, but the enhanced perception by DRL then will shorten the reaction time too and, if not by steering, braking then will reduce the severity of accidents (if not fully be avoided). Without DRL-usage more accidents will have a higher collision speed, because then there often will be no perception of the other road user at all. It is known from many in-depth accident studies (e.g. Nagayama, 1978; Cairney & Catchpole, 1990) that not or too late 'seeing' the other is a causal factor in about in 50% of the accidents. For intersection accidents this even amounts to 80% (Carney & Catchpole, 1990). Therefore, if DRL does not prevent the accident, it still increases the share of (too late) braking manoeuvres in non-avoided accidents. One may expect that the relatively lower collision speeds in fatal accidents with DRL also have a higher DRL-effect on fatalities than on casualties, because lower speeds prevents relatively more fatalities than casualties and more casualty accidents than accidents (Nilsson, 1982; Evans, 1991). The relation between multi plicative reduction factors for prevented accident outcomes and multiplicative reduction factors for decreased average speed (or decreased average collision speed) is given by power functions. Its powers are empirically (Nilsson, 1982) and theoretically derived (Koornstra, 1996). They are 2 for accidents, 3 for casualties, and 4 for fatalities. The quadratic effect of collision speed on fatalities with respect to accidents, theoretically is explained by the kinetic energy that is absorbed in a fatal accident, because the kinetic energy ($\frac{1}{2}m.v^2$) depends on the square of the collision speed. Multiplying the expected δ_i-values in $\exp(\delta_i)$ from the DRL-prediction curve on MD-accidents by a somewhat higher factor than 1.5 (power ratio 3 : 2), it yields DRL-effects that marginally differ from the DRL-effects of the prediction curve on casualties in MD-accidents. Because of this result, one not only can be sure that indeed DRL reduces the collision speed, but also that the average of expected δ_i-values for the DRL-prediction curve on MD-accidents and on their casualties, when respectively multiplied by factors 2 (power ratio 4 : 2) and 1.333 (power ratio 4 : 3), approximate or underestimate the δ_i values for a DRL-prediction curve on fatalities in MD-accidents. In terms of the original reduction percentages of the DRL-prediction curves the three DRL-prediction curves (for fatalities approximated by the best fitting multiplicative factor) are described by :

$$100[1-\exp(\delta_i|_{\text{nation,fatality}})] = 0.00331(\text{degrees})^{2.329}$$

$$100[1-\exp(\delta_i|_{\text{nation,casualty}})] = 0.00279(\text{degrees})^{2.329}$$

$$100[1-\exp(\delta_i|_{\text{nation,all MD}})] = 0.00166(\text{degrees})^{2.329}$$

In view of the excellent fit of the observed DRL-effects to the two optimal prediction curves and the empirically validated powers for the theoretical power relations between multiplicative reduction factors for decreased collision speeds and for prevented types of accident outcomes, there is a sound basis for the prediction of three DRL-effects in any country by the average latitude of the traffic in the country. In the next table the predicted whole year and winter DRL-effects on all MD-accidents and on casualties in MD-accidents for the fifteen countries of the European Union are presented as well as their road traffic fatalities (death within 30 days after accident) in 1995. For predictions of prevented numbers of accidents, casualties and fatalities by DRL one has to keep in mind that MD-accidents have an average share in all accidents for the EU-countries of about 50% and in the winter of about 23%. Based on the intrinsic DRL-effects on fatalities in MD-accidents (without having fitted actually observed DRL-effects on fatalities) and these shares, also the expected numbers of fatalities that would be saved by full DRL in the EU are given for the whole year and for the winter (1 October to 31 March) of 1995. These latter saved numbers are for Austria and the Netherlands corrected for the levels of existing DRL-usage (in winter 1.35 higher) and for respectively 30% and 40% non-motorised traffic fatalities.

Country	Latitude	Exist. DRL	Pred. year DRL-effect %		Pred. winter DRL-effect %		1995 fatalities	Expect. saved fatalities	
			Casualt.	Accid.	Casualty	Accident		year	winter
Fin.	63.0	obl.	43.4	28.8	64.1	38.2	441	0	0
Swe.	59.5	obl.	38.0	22.2	57.3	34.1	572	0	0
Den.	55.5	obl.	32.3	19.2	50.0	29.8	582	0	0
Irl.	53.5	abs.	29.6	17.7	46.6	27.8	437	77	56
UK.	53.0	abs.	29.0	17.3	45.8	27.3	3765	648	470
Neth.	52.0	22%	27.7	16.5	44.2	26.3	1334	153	101
Bel.	51.5	abs.	27.1	16.2	43.4	25.8	1449	233	171
Ger.	51.5	abs	27.1	16.2	43.4	25.8	9454	1571	1118
Lux.	49.5	abs.	24.7	14.7	40.2	24.0	68	10	7
Aus.	47.5	10%	22.5	13.4	37.2	22.2	1210	132	99
Fr.	47.0	abs.	21.9	13.1	36.5	21.7	8891	1176	885
It.	42.5	abs.	17.3	10.3	30.3	18.0	7003	721	578
Sp.	40.0	abs.	15.5	9.2	27.1	16.2	5751	512	425
Port.	39.5	abs.	14.6	8.7	26.5	15.8	2710	234	196
Gr.	39.0	abs.	14.2	8.5	25.9	15.4	2240*	188	158
EU	47.0	4%	**21.9**	**13.1**	**36.5**	**21.7**	45907	5646	4264
* Estimated; in 1990 and 1994 there were 1998 and 2195 fatalities in Greece									

Table 19. *Summary of the predicted whole year and winter DRL-effects on all MD-accidents and on casualties in MD-accidents for the fifteen countries of the European Union, as well as their road traffic fatalities (death within 30 days after accident) in 1995.*

From the total fatalities of 45,907 (day and night) in the EU 5,646 fatalities would be additionally saved by a DRL-obligation for the EU in daytime and by a winter DRL-obligation 4264 fatalities or respectively 12.3% and 9.3% of the annual road fatalities (day and night) in the EU. The DRL-effect percentages for casualties, corrected for its shares of 50% and 23% in respectively the annual and winter casualties and corrected for existing DRL-use, amount to nearly 10.1% (full DRL) and nearly 7.7% (winter-DRL). Applied to the 45,000 fatalities and about 350,000 serious injured persons as well as nearly 1.2 million slightly injured persons (1.6 million casualties) nowadays approximately registered annually within the EU (ETSC, 1997), while the predicted fatalities are subtracted from the predicted casualties in order to obtain predicted injured persons, it predicts that 155.000 or 10% injured persons would be registered less annually if DRL should have been used fully in the EU of today. For a winter DRL-obligation it means just less than 120,000 or 7.6% injured persons less. However, probably about 30% of the accidents with serious injured persons and about 60% of the accidents with slightly injured persons are not registered (OECD-IRTAD, 1994). Also these non-registered ones would be prevented with the same 10% (full DRL) and 7.6% (winter DRL).

For the expected 10.1% of the registered casualties that would be prevented by DRL, one has to keep in mind that the additional use of DRL by a EU-wide DRL-obligation affects only the 50% share of daytime casualties (single and night accidents with casualties constitute the other half of the total). Moreover, a EU-wide DRL-obligation also only can have preventive effects for so as DRL is not used already and it is almost fully used by the DRL-obligation in Finland, Sweden and Denmark and by the voluntary use in the Netherlands and Austria partially for respectively 22% and 10%. This explains why the intrinsic prevention effect of 21.9% of DRL for the multiple daytime accidents with casualties in the EU, becomes 10.1% of all casualties in the EU. The intrinsic prevention percentage for casualties in the winter from a winter DRL-obligation in the EU is 36.5%, but the winter casualties constitute only 23% of the annual total casualties in the EU. As a consequence of this, and the already existing DRL-use that reduces the additional DRL-effect, the 36.5% of the intrinsic winter DRL-effect becomes reduced to 7.6% of the annually registered total casualties in the EU.

In the EU a total of nearly 12 million road accidents are nowadays registered and probably about 30 million are estimated to be claimed on insurance companies. The actual total of annual accidents in the EU is estimated to be probably about 60 million of which 48 million are not reported to the police and thus not registered (figures for 1995 from FERSI (1996) with a small addition for the somewhat increasing number of registered damage-only accidents). The whole year and the winter DRL-effect percentages for all MD-accidents are a fraction of .597 from the respective DRL-effects on casualties in MD-accidents. Because the saving percentages from DRL for all MD-accidents are thus nearly 40% lower than for casualties in MD-accidents, it follows that a full DRL-obligation in the EU would reduce about 6.2% of all accidents and for a winter DRL-obligation in the EU the reduction is about 4.7% of all annual accidents. This amounts annually to about 740,000 registered accidents less due to a full DRL-obligation in the EU and due to a winter DRL-obligation in the EU about 560,000 accidents less would be registered annually. Also these percentages of 6.2% (full DRL) and 4.7% (winter DRL) are the additional prevention effects of DRL with respect to the annual total of registered accidents in the EU and relate to intrinsic DRL-effects on MD-accidents of 13.1% (of annual total) and 21.7% (of winter total) without correction for existing DRL-usage. The prevented MD-accidents must be multiplied by a factor of 2.5 in order to obtain an estimate of the claimed accidents, which thus amounts to the prevention of almost 1.9 million annually claimed MD-accidents or 1.4 million claimed MD-accidents in the winter.

The next table gives a summary of the respective DRL-effect percentages and the numbers of fatalities, injured persons and accidents in rounded-off figures prevented nowadays by a full and winter use of DRL in the EU.

Registered	Year DRL		Winter DRL	
	Saved of total		Saved of total	
	%	number x 1000	%	number x 1000
Fatalities	12.3	5.5	9.3	4.2
Injured pers.	10.0	155	7.6	115
Accidents	6.2	740	4.7	560

Table 20. *Summary of the respective DRL-effect percentages and the numbers of fatalities, injured persons and accidents in rounded-off figures prevented nowadays by a full and winter use of DRL in the EU.*

For countries in the EU below the latitude of 55 degrees, which excludes the countries with a DRL-obligation, the winter DRL-effect is 1.56 (at 54 degrees) to 1.82 (at 39 degrees) higher than the whole year DRL-effect.
In average 23% of all MD-accidents occur in the winter and 27% in the summer (the other percentage is the 50% DRL-irrelevant single and night accidents). It explains why DRL reduces in the summer to 24%; in the winter it is 76% of the annual numbers.

One may perhaps better expresses the actual DRL-effects in the EU by prevention percentages of the corresponding multiple daytime totals that occur annually or in the winter for the whole EU. The differences of these percentages with the intrinsic DRL-effects then are only due to the already existing use of DRL in some countries of the EU. *Table 21* presents the intrinsic DRL-effects and the actual DRL-effects for the EU for the whole year and the winter, as percentages of the annual or winter totals of fatalities in MD-accidents, injured persons in MD-accidents, and their total as casualties in MD-accidents, as well as all MD-accidents and their prevented numbers in rounded-off figures.

Registered	Whole year				Winter		
	Intr. %	EU MD-accidents x 1000	Prevention		EU MD-accidents x 1000	Prevention	
			%	Number x 1000		%	Number x 1000
Fatalities	26.0	22.5	24.6	5.5	10.4	40.4	4.2
Injured pers.	21.8	775	20.0	155	355	32.9	115
Casualties	21.9	800	20.1	160	365	33.1	120
Accidents	13.1	6,000	12.4	740	2,750	20.4	560

Table 21. *Intrinsic DRL-effects and the actual DRL-effects for the EU for the whole year and the winter, as percentages of the annual or winter totals of fatalities in MD-accidents, injured persons in MD-accidents, and their total as casualties in MD-accidents, as well as all MD-accidents and their prevented numbers in rounded-off figures.*

The statistical uncertainty of predicted DRL-effects on fatalities is unknown, but that uncertainty of predicted DRL-effects on casualties and on accidents is known from the analyses of the observed DRL-effects, as lastly illustrated by the minimum prediction curves. It follows that the DRL-effect of 20.1% on casualties in the EU will with 95% chance be higher than 12.7% (lower than 28.0%). For the DRL-effect on MD-accidents of 12.4% the uncertainty is relatively much higher, but with 95% chance it will be higher than 3.4% (lower than 20.6%). For the winter DRL-effect on winter casualties of 33.1% is with 95% chance higher than 25.7% (lower than 40.3%), while the DRL-effect on winter MD-accidents of 20.4% is with 95% chance higher than 5.9 % (lower than 32.7).

Before concluding this chapter some remarks are made. The prevention figures per country are only predicted for numbers of fatalities, because they are identically defined (death within 30 days after the accident) and almost completely registered. Serious and slightly injured persons have nationally different definitions and are differently under reported (OECD-IRTAD, 1994), the latter much more for damage-only accidents. Still the predicted intrinsic DRL-effects per country are probably valid, because the proportional DRL-effects probably do not very much depend on the differences in definition and reporting. However, it would make no sense to present the prevented serious and slightly injured persons or total accidents per country due to their incomparability. Nonetheless the total and prevented numbers for non-fatal outcomes of accidents in the EU are estimates of the actually registered numbers or claimed accidents in the EU and their definitions and under reporting are just the aggregates of the ones for the nations in the EU.

5.3. Conclusions on effects of a DRL-obligation for the EU

Conclusion I:
A DRL-obligation for the EU most likely will prevent 24.6% of the fatalities and 20.0% injured persons from multiple daytime accidents within the EU. Nowadays this would prevent 5,500 fatalities and about 155,000 injured persons annually registered on EU roads.

Because the estimates are based on statistical analyses of observed DRL-effects on casualties and an expected DRL-effect on fatalities, the estimate for registered casualties have a known statistical uncertainty, but the for fatalities is unknown. This allows the following auxiliary remark.

Auxiliary Remark I:
With 95% chance the 20.1% of prevented casualties from multiple daytime accidents within the EU by a DRL-obligation for the EU will be higher than 12.7% or lower than 28.%. Nowadays this would almost undoubtedly prevent more than 100,000 or less than 215,000 casualties annually registered on EU roads.

Furthermore with respect to DRL-effects on all multiple daytime accidents the following conclusions and remarks are formulated.

Conclusion II:
A DRL-obligation for the EU most likely will prevent 12.4% multiple daytime accidents within the EU. Presently this would amount to an annual prevention of about 740,000 registered accidents on EU roads or almost 1.9 million accidents with insurance claims in the EU.

This estimate is based on statistical analyses of a relatively smaller number of observed DRL-effects than for DRL-effects on casualties. Therefore, this estimate has a relatively higher known statistical uncertainty, from which the following auxiliary remark is derived.

Auxiliary Remark II:
With 95% chance the 12.4% of prevented multiple daytime accidents within the EU by a DRL-obligation for the EU will be higher than 3.4% or lower than 20.6%, whereby it follows that nowadays this would almost certainly prevent per year more than 205,000 or less than 1.25 million registered accidents on EU roads or more than 0.5 million or less than 3 million claimed accidents in the EU.

If not a full DRL-obligation is considered, but in view of the higher winter DRL-effects only a DRL-obligation from begin October to end March in the EU, then the following conclusion with respect to the prevented fatalities and injured persons is formulated:

Conclusion III:
A winter DRL-obligation for the EU most likely will prevent 40.4% of the fatalities and 32.9% injured persons from multiple daytime accidents in the winter within the EU. Nowadays this would amount to a prevention of 4,200 fatalities and about 115,000 injured persons per year registered on EU roads.

Also for these estimates only the statistical uncertainty for the sum of fatalities and injured persons is known, from which the next auxiliary remark follows.

Auxiliary Remark III:
With 95% chance the 33.1% of prevented casualties in winter within the EU by a winter DRL-obligation will be higher than 25.7% or lower than 40.3%. Nowadays this would almost certainly prevent more than 93,000 or less than 150,000 casualties per year registered on EU roads.

With respect to a winter DRL-obligation for the EU and its effect on all multiple daytime accidents in the winter, the last conclusion is formulated.

Conclusion IV:
A winter DRL-obligation for the EU most likely prevents 20.4% multiple daytime accidents in the winter within the EU. Presently it would amount to an annual prevention of about 560,000 registered accidents on EU roads or about 1.4 million claimed accidents in the EU.

Once again due to relatively few observed DRL-effects on all multiple daytime accidents these estimates have a relatively high known statistical uncertainty, from which the last auxiliary remark is deduced.

Auxiliary Remark IV:
With 95% chance the 20.4% prevented multiple daytime accidents in the winter within the EU by a winter DRL-obligation for the EU will be higher than 5.9% or lower than 32.7%. It follows that nowadays this would almost certainly prevent annually more than 160,000 or less than 900,000 registered accidents on EU roads or more than 400,000 or less than 2.25 million claimed accidents in the EU.

6. Policy perspectives on DRL in the EU

In view of the conclusions of the previous chapter, summarised by

number of fatalities	**5.500**
number of registered injured persons	**155.000**
number of registered accidents	**740.000**
number of accidents with insurance claims	**1.900.000**

that would most likely have been prevented by a present full DRL-use in the EU, it seems evident that a kind of compulsory DRL for all the countries of the EU should not be postponed any longer. For the four most southern countries of the EU a winter DRL-obligation may be considered, since there about 80% of the prevented numbers by full DRL occur in the winter. In the sequel it is shown that the ratio of benefits to costs also contributes to the necessity of a compulsory DRL. One may wonder why this evident road safety measure has not been considered earlier.

6.1. Incomprehensibility and lack of evidence for road users

Road-user incomprehensibility of DRL

The original reason for the use of DRL also has not been the increased vehicle visibility. Daytime running lights seems to have originated as a campaign to operate motor vehicles with headlights on in daytime as a signal of intention to comply with a Texas governor's request in the early sixties to drive safely. To quote a remark made in 1964: - "It seems that no one can conceive of an automobile or a Greyhound Bus being invisible on a bright clear day" (Allen & Clarke, 1964). Still most road users can not today.

DRL as a road safety measure is uncomprehensible for the road user, because one 'knows' that with sufficient attention every vehicle is seen on the road in daylight. Moreover, experienced drivers judge themselves as rather good experts in safe road behaviour and think that scientific research can not tell them things they do not already know, which surely does not imply that they always behave on the road according to what they 'know' to be safe. However, the research reviewed in chapter 1 shows that visual perception in traffic is far from perfect. Perhaps most strikingly, 8% of cars with usual colours in the open field during daylight (excluding dawn and dusk) are not visible without DRL from certain not too far distances, compared to cars with DRL (Padmos, 1988). On usual roads in shades and/or with masking backgrounds the visibility and contrast of cars with usual colours is far more reduced. It is known from many in-depth accident studies (e.g.: Nagayama, 1978; Cairney & Catchpole, 1990) that not or too late 'seeing' an other road user is a causal factor in about in 50% of the total of daytime accidents and for intersection accidents this becomes the case in 80% of the daytime accidents (Carney & Catchpole, 1990). The safety effect of DRL is now shown to be significant in usual European daylight conditions, while it increases in worsening daylight conditions, and is higher for crossing and frontal daytime accidents than for other multiple daytime accidents. It shows that DRL indeed is one of the effective means to assist road users in their visual perception tasks on the roads and their mastering of safe road behaviour. But until recently even road safety scientists have debated the validity of DRL-effects in usual European daylight. Consequently road users have not been informed of its positive effects on their visual perception and safe road behaviour. Road users can not be aware of what they have not seen or are not informed of otherwise.

Emerging scientific evidence
The scientific evidence for the effectiveness of DRL for less Nordic countries than the Scandinavian countries is rather recent. The two Danish DRL-evaluations are from 1993 and 1994 and as shown here have, due to a sub-optimal methodology and confounding developments, under estimated the DRL-effect for Denmark. The large fleet evaluation of DRL in Canada and the national Canadian DRL-evaluation are respectively published in 1993 and 1994. The national Hungarian DRL-evaluation also is from 1994 and also the publication year of the major Austrian fleet evaluations of DRL is 1993, while many older individual fleet DRL-studies in somewhat southern regions did only show statistically insignificant positive results (though some high ones). Only recent metaanalyses of Koornstra (1993) and Elvik (1996) of all the then available studies on DRL have shown that DRL-effects on latitudes of Europe will be significant and range from 10% to 15% (Elvik, 1996) or from 8% to 22% (Koornstra, 1993) reduction of multiple daytime accidents. Moreover, earlier studies did not and more recent ones wrongly correct the DRL-effect for prior DRL-usage levels, which have deflated the DRL-effects. In Koornstra (1993) is for the first time shown that there may exist a strong relation between latitudes and DRL-effects with diminishing effects for more southern latitudes, but Elvik (1996) only found a weak relation. Both meta-analyses assumed that fleet DRL-effects and national DRL-effects, when corrected for prior DRL-levels, are comparable and both also assumed the DRL-effects on fatalities and on injured persons in MD-accidents as well as on all MD-accidents are the same.

It is in this report that for the first time their possible differences are researched and found to be statistically significant. Also for the first time are the data from the DRL-studies re-analysed (where possible) with the same optimal methodology, which has eliminated possible biased DRL-effects as well as differences due to different (sub-optimal) methods of analysis. Especially the much higher DRL-effects on injured persons (and fatalities) are surprising, but the differentiated DRL-effects and the re-analyses also enabled the determination of a statistically significant different curvilinear relations between latitudes and the DRL-effects on fatalities and injured persons from MD-accidents and on all MD-accidents. Until now estimations for DRL-effects for the EU-countries (Koornstra, 1993; ITS, 1997) have ranged above and below the 6.2%, here obtained for the DRL-effect on all accidents (day and night) in the EU. On the basis of their statistically significant relations with latitude the higher than expected DRL-effects for fatalities and injuries also are obtained here for the first time.

In summarising this section, the perceptual and new statistical evidence for the high DRL-effects on fatalities and injured persons has to made know to and understood by policy makers, members of parliament and road users. Without that knowledge and understanding the political feasibility of a compulsory DRL not emerge and the incomprehensibility for road users of DRL as a road safety measure will last for too long.

6.2. Attitude and misbelief towards DRL

These two reasons explain why DRL has not been introduced earlier as a national obligation, apart from Nordic countries with impaired visibility during daytime especially in the winter when the ambient illumination level is low due to a relatively small angle of the sunlight. No wonder that the first DRL-obligation is introduced in Finland for the winter in 1972/’73. But it only became an obligation after campaigns for the recommendation of DRL during the winter in two previous years increased the use of DRL to above 50%. The evidence for the safety effects in the winter has upgraded the winter DRL-obligation to a whole year obligation.

The Finnish experience has stimulated the whole year DRL-obligation for Sweden introduced in October 1977, where the voluntary DRL-use in average had already increased to 50%. In Norway the voluntary use of DRL by official DRL-recommendation campaigns increased from nearly zero in the mid seventies to 60% in 1985, whereafter DRL became an automatic vehicle standard followed in 1987/’88 by a DRL-obligation for drivers of motor vehicles of older model years. What is apparent is that a DRL-law even in these Nordic countries only passed their parliaments after the drivers, motivated by information campaigns for DRL, already used DRL voluntary for 50% or more.

In Denmark, where DRL is compulsory since October 1990, the prior use of DRL was about 20%, but there the majority of drivers were after lengthy discussions in the public news media no longer against a DRL-obligation (Lindeyer, 1993b). The public discussion in Denmark showed a strong opposition to DRL from the organisations for pedestrian safety and cyclists, while also the European motorcyclist organisation showed concerns about negative effects on their safety from DRL for cars. Nonetheless the DRL-law passed the Danish parliament without much opposition.

In The Netherlands the minister of transport proposed a DRL-obligation after October 1990, simultaneous with the Danish DRL-obligation, but this was postponed by pressure of parliament. Nonetheless, in The Netherlands a majority of license holders as well as the ANWB, the Dutch member of the AIT, and the Dutch federation of insurance companies were not against the DRL-obligation after pro-and-cons were discussed in the news papers (Lindeyer, 1991). However, at that time there still existed scientific scepticism with regard to the DRL-effect for The Netherlands (see discussions of Dutch road safety scientists: Riemersma & Theeuwes, 1990a, 1990b; Koornstra, 1990a). This and the anti-DRL lobby of the relative strong Dutch pedestrian and cyclists organisations and the ‘green’ organisations and party in the Netherlands, convinced the majority of the Dutch parliament members that a DRL-obligation for The Netherlands was premature. In the early nineties DRL has been recommended in The Netherlands by a national publicity campaign. This raised the support for and use of DRL in The

Netherlands (Lindeijer, 1993a; Lindeijer & Bijleveld, 1991) to an average level of 22% and 60% to 80% in daytime weather conditions with an impaired visibility, but up to now no new initiative for a DRL-obligation is taken. Also a try of an experimental DRL-obligation for the Benelux countries failed, notwithstanding a positive advice from an international steering committee of experts from most EU-countries for such a DRL-experiment in a region of North-West Europe (SWOV, 1991).

Canada introduced compulsory DRL as an automatic vehicle standard for vehicles from model year 1990 onward. The DRL-law passed parliament after the voluntary use of DRL raised to nearly 30% and was sustained by the Canadian provinces after the positive experiences with DRL for government fleets in the province of Saskatchewan (Sparks et al., 1989). Despite the majority of the traffic and inhabitants of Canada are much more southern located than in the North-West European countries, the DRL vehicle standard in Canada has evoked less opposition than the behavioural DRL-obligation in some North-West European countries.

Also in Austria that is located on latitudes that overlap with southern Canada and where also positive DRL-effects for government fleets are obtained, the behavioural DRL-obligation proposed last year by the Austrian ministry of transport has not yet been accepted due to public and parliament opposition. Probably DRL as compulsory vehicle standard is more accommodating than a behavioural DRL-obligation.

The only exception of a behavioural DRL-obligation that is accepted in a country on an average latitude below 50 degrees is Hungary, because the one remaining country with a behavioural DRL-obligation is Poland that introduced a winter DRL-obligation in 1993/’94. It must be noted that Hungary introduced stepwise a partial DRL-obligation, first in 1993 on its semi-motorways and main rural roads and then in 1994 for all roads outside built-up areas. Also in Hungary the prior DRL-usage of more than 30% on roads outside built-up areas was already relatively high, while the partial DRL-obligation was part of a series of road safety measures (including a chance from 60 km/h to 50 km/h in built-up areas) in an amendment of the Hungarian highway code. Both circumstances might have contributed to the acceptance of the partial DRL-obligation by the relatively unexperienced democratic parliament of Hungary.

Moreover, Hungary, as well as Poland, introduced the DRL-obligation after the sudden increase in road fatalities and injuries after 1989, due to the relative exploding increase in motorised traffic after the fall of the communist regimes. So also in view of the needed road safety action the acceptance of the partial DRL-obligations in Hungary (only outside built-up areas) and in Poland (only in the winter) might be understood. Not much is known of the public and political acceptance of the Polish winter DRL-obligation, except that a DRL-obligation has been recommended to the Polish government by a EU/World Bank organised committee for a Polish road safety policy. There also is no scientific evaluation of the DRL-effect on the winter daytime accidents in Poland.

The earliest use of DRL as safety device is observed for the company fleets in the USA. Although Allen and Clark (1964) report at that early time of 1964 - "finally, running lights on the front of motor vehicles is becoming common place at least in the Midwest, and may become law" - no US-state has passed a DRL-law for all motor vehicles up to now. Nonetheless, nowadays compulsory DRL is again discussed (Williams & Lancaster, 1995) in the USA and General Motors promotes the installation of automatic DRL on their new cars in the USA. Also Saab and Volvo promote DRL as a vehicle standard in Europe and the USA.

Summarising the policy relevant aspects of this overview on introduced DRL-obligations in the world it is noted that:

- full behavioural DRL-obligations in less northern countries than Canada and Denmark probably will meet serious opposition;
- full behavioural DRL-obligations have been accepted in Nordic countries after the voluntary DRL-use was raised to 50% or more by the promotion of DRL in the news media and/or in publicity campaigns of the national authorities;
- partial behavioural DRL-obligations (only in winter time or only outside built-up areas) may be more readily accepted than a full behavioural DRL-obligation;
- full DRL-obligations are probably more acceptable if they are stepwise introduced by enlarging the road network and/or time periods of initial DRL-obligations for parts of the road network and/or time periods;
- gradually introduced compulsory DRL by a vehicle standard for DRL on new vehicles from a year in the near future onward has not met opposition even in regions on the same latitudes as France or Austria;
- drivers of motor vehicles are more easily convinced of the safety benefits of DRL for them than pedestrians and cyclists, while drivers are, when informed on the positive DRL-effects, in majority not against a DRL-obligation;
- the European organisations of pedestrians, cyclists and motorcyclist as well as most of their national member organisations are against an obligation of DRL for cars, notwithstanding that the additional costs for DRL are paid by the car drivers;
- national organisations and political parties with a strong concerns on environmental protection and air pollution by traffic are against any DRL-obligation.
- car and insurance companies and national touring clubs in Europe probably are the players in the field from which support for a DRL-obligation may be feasible.

The opposition of pedestrian, cyclist and motorcyclist organisations are in view of the equal positive DRL-effects for them as for car drivers partially based on misbelief. The amount of the additional fuel use for the light energy of DRL and thus the negative influence of DRL on environmental quality will be discussed in the sequel.

6.3. Kinds of DRL and possible ways of implementing DRL

DRL generally is voluntary or compulsory, but compulsory DRL as an automatic in-vehicle standard for new vehicles from a particular model year onward can be combined with voluntary DRL on vehicles of older model years. Voluntary DRL can be behavioural by switching on the low beam headlights in daytime or can be non-behavioural by the voluntary retro-fit (or already optional installed) automatic in-vehicle DRL.

Compulsory DRL can be a behavioural DRL-obligation or a vehicle regulation for automatic in-vehicle DRL. Compulsory DRL can be partial with respect to national fleets as an automatic in-vehicle DRL standard for new vehicles, where the part of vehicles not equipped with in-vehicle DRL diminishes with time to almost nil. DRL as a behavioural obligation can be partial with respect to periods of the year, such as winters, or with respect to parts of the road network, such as roads outside built-up areas.

Partial DRL-obligations by definition are not as effective as compulsory full DRL, but compulsory winter DRL for the EU has about 75% effectiveness, provided that its winter use is rather complete. For the most southern countries in the EU a winter DRL-obligation even has an effectiveness of about 80%. There is no evidence that DRL on rural roads is more effective than on roads inside built-up areas. Moreover, the Hungarian data show that a partially applied behavioural DRL-obligation has the disadvantage that its compliance will not be complete and than ought to be enforced. Partial DRL-obligations may have the advantage of a higher public and political acceptance than a full DRL-obligation. Full DRL-obligations have an almost complete compliance without enforcement as the relatively recent Danish DRL-obligation has shown.

From the point of view of legal competence with respect to DRL-regulations, a behavioural DRL-obligation will be the only possibility of nations in the EU that are willing to implement a partial of full DRL-regulation. It also is clear that the EU has the legal competence for an automatic in-vehicle DRL-regulation. Its stepwise technical introduction may have the advantage of an easier acceptance by the road users in the countries of the EU as well as the CEC and the EU-parliament, provided that the safety benefits are well marketed prior to such an in-vehicle DRL-regulation.

Intensive European DRL-campaigns, initiated by the CEC, for the voluntary use of DRL (perhaps only for the winter in southern EU-countries), may be part of such a prior marketing of DRL. Cooperation from national touring clubs, car industry and related sails and repair organisations as well as national road safety organisations and insurance companies in these DRL-campaign seems feasible. This could be investigated with the ERSF as representative of these organisations (such as AIT, ACEA and PRI) and with the European federation of insurance companies.

These campaigns also should address not only motor vehicle drivers, but also especially pedestrians and cyclists in order to convince them of the safety benefit for themselves, which in the previous two chapters herein is proven to be statistically significant as large for them as for motorvehicle drivers. Otherwise, pedestrian and cyclist organisations may become part of the lobby against DRL. Moreover, these campaigns should address motorcyclists in order to try to convince them that their assertion of adverse effects for them from DRL for cars is wrong, because unjustified as the analyses for Norway, Denmark have shown.

It is to be expected that, sustained by EU-plans for an automatic in-vehicle DRL-regulation, some nations in the EU will additionally take a behavioural DRL-obligation (probably The Netherlands and Austria) in order to have realised sooner the safety effects of a full DRL-use.

Summarising the above sections it is recommended that:
- intensive DRL-campaigns and social marketing of DRL are organised in the EU by the CEC in order to raise awareness of the safety effects and benefits from DRL;
- plans are made for a EU-regulation on automatic in-vehicle DRL for new motor vehicles from a particular year onward. The year to be chosen preferably is decided after the political and public acceptance of the DRL-regulation has become clear.

6.4. Technical and environmental aspects of DRL

The majority of the information presented in the sequel of this section is based on the conclusions of Dutch governmental working party on DRL and on a report of Schoon (1991) on the technical aspects of DRL and its automatic devices. Where necessary the facts are updated to 1997.

Types of vehicles
Compulsory DRL as an automatic in-vehicle DRL, when implemented from a particular year onward, should apply to new:
- mopeds with a maximum speed above 25 km/h;
- agricultural motor vehicles that are also using public roads;
- other motor vehicles that are allowed to (or actually can) drive faster than 20 km/h;
- trams.

After some years (for example 7 or 10) these vehicles of older model years than the first implementation year, should be compulsory retro-fitted with an automatic in-vehicle DRL, already available on the market. An exception could be made for historical motor vehicles that can have technical installation problems or the exception could apply to those vehicles that are older than say 30 years.

Lighting aspects
DRL as an automatic in-vehicle standard can be realised by standard low beam headlights that are switched on automatic after the vehicle has been started and are switch off automatic when the motor is stopped, which will be called automatic standard DRL. Alternatively DRL can be realised by special lamps that are switch on automatic after the starting of the motor and switched off after stopping the motor. The latter will be called automatic DR-Lamps. The same two lighting possibilities hold for behavioural DRL-obligations, which as switched-on standard low beam headlights by the driver is called standard DRL, and in case of special lamps that are switched on by the driver, the DRL is called DR-Lamps.

Standard DRL has the advantage that it needs no additional equipment and can be obliged or used voluntary at any time. In countries of the EU where DRL is obliged, nearly all new vehicles are already equipped with automatic standard DRL. Standard and automatic standard DRL has the technical disadvantage that the light is not optimally used, because only the light beam above the cut off line of the low beam headlights is perceivable and thus uses more energy than is needed for their purpose in daytime. An other disadvantage is that the additional use of the low beam headlights causes a shorter life time of its bulbs. It can cause that inattentive drivers will drive their vehicle more often with only one burning light also in the night. These disadvantages can be avoided in two ways: either by additional DR-lamps or by applying an automatic device that reduces the voltage of the low beam headlights (or high beam, headlights discussed later). Low beam headlights with automatic reduced voltage, however, also produce less light and thus are reduces the conspicuity of the vehicle with such a kind of DRL. Although it also reduces the possibility of clare, DRL from low beam headlights with reduced voltage is not be recommended due to the decreased conspicuity of its vehicle. In North-America (notably for the Canadian in-vehicle DRL), 20% voltage reduction on low beam headlights are often used, but the cut off line of low beam headlights for American cars is less sharp than for European cars. It restores again the conspicuity of the American vehicles with DRL from low beam headlights with reduced voltage to the conspicuity level of European cars with normal (automatic) standard DRL.

DR-Lamps asks for additional lamps placed in or below the front bumpers, but according to the ECE-rules also a higher montage is allowed. The ECE-regulation nr. 87 gives the specifications of the lamps and ECE-regulation nr. 48 the installation requirements. DR-Lamps are an optional ECE-regulation. ECE-regulation nr. 87 has not been agreed on by the EU-nations of Austria, Denmark, France, Italy and Spain. These countries need not to accept vehicles on their roads with DR-Lamps. Also DR-Lamps are two front lamps and their light intensity is in the centre of the beam 400 - 800 cd. from lights of 21 W., instead of 55 W. for low beam headlights. When DR-Lamps are switched on then also rear lights are switch-on and DR-Lamps are automatic switched off when normal low beam headlights are switch on.

As a kind of DR-Lamps also can be consider DRL from high beam headlights with an automatic light reduction of 50% or more, which devices are available on the market. This type of DR-Lamps has two advantages. Firstly, no other lamps than existing lamps, that are seldom used and only partially used for DRL, are needed. It does not shorten the life time of the bulbs and thus do not require more bulb replacements. Secondly, the light reduction is achieved by reducing the electric charge, in stead of dimming the light by electric resistance, which asks for less energy than low beam headlights. However, its disadvantages do not make it the preferred kind of DRL. High beam headlight give a very concentrated light beam. When reduced to 1000 cd or below, which is necessary to avoid glare, gives hardly visible light beams in the horizontal zone outside angles of about 5 degrees of the central light direction. Thus this kind of DRL does not enhance peripheral conspicuity. Moreover, the variation in maximum cd of the high beam headlights allowed in Europe (and actually present) is so large that no uniform reduction of voltage can be determined, although needed in order to avoid glare from this kind of DRL.

In the short run this leaves automatic standard DRL as the preferred DRL for an automatic in-vehicle DRL-regulation. The advantage compared to standard DRL is that one does not forget to switch off the low beam headlights, which may cause empty batteries. Retro-fit devices for automatic standard DRL which also switch on and off the rear lights are available on the market and costs about 25 to 50 ECU excluding the installation costs of about a half hour. Also automatic signal devices for standard DRL, preventing that the driver forget to switch on or off the low beam headlights are available on the market. Signal devices that auditively warns the driver for burning low beam headlights when leaving the car are nowadays already standard for most vehicles. Those that function auditively after the motor is started or stopped can be retro-fitted and cost about 10 ECU and 15 minutes for installation. In the long run automatic DR-Lamps with 21 W., instead of the 55 W. of the low beam headlights, may be preferred for two reasons. Firstly, it asks less additional use of fuel and secondly it hardly can cause glare in dawn and dusk hours.

Automatic standard DRL (or automatic DR-Lamps) also switches on and off the rear lights, which has advantages in case one would forget to use lights in impaired visibility conditions and during dawn and dusk hours. The possible disadvantage of masking the brake-lights, that may relatively increase rear-end accidents (not proven to be so), is already in the EU overcome by the EU-regulation for the third high mounted brake-light on new vehicles from 1 October 1998 onward.

Additional fuel consumption and environmental aspects
DRL gets its electric energy from the battery and thus asks for additional fuel use for this energy. The additional fuel use for (automatic) standard DRL depends on the type and mass of the vehicle. For a truck with 30 litre fuel per 100 km its additional fuel use by DRL is far less than 1%. However, for a small efficient car with 5 litre per 100 km its additional fuel use by DRL can reach the 3%. Three studies are available that have measured the fuel use of low beam headlights for cars (Lawson, 1986; BASt, 1989; Schoon, 1991). The German and Dutch studies conclude respectively that 0.17 and 0.15 litre per 100 km is needed for low beam headlights of cars with 10 litre per 100 km. The older Canadian study gives nearly the double amount based on the fuel needed for electric energy supply and the amount of electricity needed for low beam headlights, but the older American cars produced electricity by far less efficiency than European cars. Because about 55% of the annual kilometres of motor vehicles in the EU are driven in the DRL-relevant daytime (45% in the night and half the dusk and dawn hours) the additional fuel use for standard DRL is estimated as about 0.9% in Europe. For DR-Lamps of 21 W. the additional fuel use is about 0.4%.

Consequently the emissions also are increased by 0.9% for full use of standard DRL. The annual increase of kilometrage (in average 2% in the past and also predicted for the next decennium) and the rapidly decreasing emissions of road traffic that are polluting (except CO_2) the last 15 years (which will continue to do so, see Koornstra, 1997), makes the constant 0.9% additional fuel use for DRL with decreasing pollution rates a very minor contribution factor to air pollution, compared to other cumulative factors. But these rational arguments will not convince environmentalists that the large safety benefits of a DRL-regulation are by far outweighing its small pollution disadvantage, since most are anyhow against increased energy use.

In concluding it is recommended on technical and practical grounds that compulsory DRL, when implemented in the EU, should be an automatic in-vehicle DRL that uses low beam headlights or special DR-Lamps.

6.5. Economic costs and benefits of DRL in the EU

Additional fuel costs
Above it is calculated that 0.9 litre fuel per 1000 km is needed for standard DRL in 1997. In the EU of 1997 the kilometrage of the DRL-relevant motor vehicles is estimated to be 2850 billion kilometres (2400 bill. km in 1990 and 2580 bill. km in 1995 for cars, freight vehicles and motorcycles (see, FERSI, 1996) extrapolated to 1997 and 8% added for mopeds and other vehicles). Excluding Denmark, Sweden and Finland, where compulsory DRL is already implemented, and the 45% of the kilometres driven at night and in half the dusk and dawn hours, it are about 1480 billion km that are relevant for DRL in the EU which ask for 1330 million litre additional fuel by DRL. In the EU, where in average the price of one litre fuel is below 0.85 ECU, the costs for the additional fuel needed by standard DRL are maximum 1.13 billion ECU.

Additional car costs for automatic DRL
The mentioned costs for retro-fit of automatic devices for standard DRL are not comparable to the in-production costs of automatic in-vehicle DRL. Most modern cars have already all the needed electronics and wiring for automatic light switches (auditive signalling for lights that burn when leaving the car or automatic switching off these lights). In that case automatic in-vehicle DRL only asks some additional circuit connections in the mass production of cars. This can not costs more than 1.0 ECU per vehicle (a fair maximum).

For small cars and other DRL-relevant vehicles (e.g. mopeds) without the needed wiring for automatic light switches, the additional costs for the massive in-production installation of automatic DRL can be estimated to be less than 15 ECU per vehicle (deduced from the average of the 25 ECU to 50 ECU for separate retro-fit devices, reduced by 50% for the efficiency of mass in-vehicle production of the installation and some absent resale and distribution costs).

The additional costs for automatic in-vehicle DRL are implicitly contained in the price of these new vehicles and for 20% of the vehicles without already existing wire connections for automatic light switches, it follows that in average maximum 3.8 ECU costs per vehicle are involved. Taking all DRL-relevant motor vehicles in the EU, it concerns the costs for less than 210 million relevant vehicles (derived from FERSI (1996) and 10% added for other DRL-relevant vehicles than cars, freight vehicles and motorcycles). So a maximum of 800 million ECU additional costs for all new vehicles with automatic in-vehicle DRL, if the whole DRL-relevant EU vehicle fleet would be renewed in one year. The new vehicle sales per year has never been larger than 10% of the existing fleet after the mid eighties. Thus the maximum of annual additional costs for automatic in-vehicle DRL on all relevant vehicles in the EU are about 0.08 billion.

Additional replacement costs of bulbs
Taking the 55% of the kilometres that apply to additional use of low beam headlights and rear lights by automatic standard DRL (excluding the night and half the dusk and dawn kilometres), it follows that their using time is increased by a factor of 2.22. Given the reduced wear from switching on and off the lights, their replacements rates are increased by DRL with a factor 2. These additional bulb replacements costs per year per vehicle can be estimated to be nearly 6 ECU (Schoon, 1990). Hence the total additional bulb replacements for the 210 million DRL-relevant vehicles in the EU caused by automatic in-vehicle DRL are maximum 1.26 billion ECU per year when all vehicles would be renewed in 1977.

Economic environment costs
The cost of pollution and some other environmental costs by road transport in the EU are estimated in the Green paper on ‘Fair and efficient pricing in Transport’ (CEC, 1995). Excluding the proportional part for Denmark, Sweden and Finland and the DRL-irrelevant costs of noise and disposal waste of vehicles, the total economic environment costs by road transport for the relevant part of the ECU is estimated as 20 billion ECU for emissions of fuel. Given the 0.9% additional fuel use it means additionally .18 billion ECU economic environment per year costs due to automatic in-vehicle DRL that would in the end apply to all EU-vehicles.

Total annual cost of automatic standard DRL
No other additional costs for automatic standard DRL can thought of than those mentioned above. So the maximum additional costs per year are:

fuel costs	1.13 billion ECU
car costs	0.08 billion ECU
bulb costs	1.26 billion ECU
environment costs	0.18 billion ECU
	====
Maximum costs	2.65 billion ECU

The fuel and environment costs for DR-Lamps are less than half the fuel and environment costs for DRL from low beam headlights, but the additional car installation costs for (automatic) DR-Lamps are increased by almost the same amount as their savings on fuel and environment costs.

Economic ratio of total benefits and costs
The economic benefits of DRL relate tot the prevented fatalities, injured persons and damage-only accidents by DRL. In the recently adopted road safety plan the so-called "one-million ECU test" takes one million ECU for the hard economic benefits of one fatality and the usual concurrently occurring injured persons and accidents that are prevented by a safety measure. This, however, applies to the economic benefits of prevented numbers of registered fatalities, injured persons and accidents. If the non-registered one would have been included the actual economic benefits are 2 million ECU per prevented fatality. Given the predicted 5.5 thousand fatalities that would be prevented by full DRL in the EU, the total economic benefits of full DRL would amount annually to 11 billion ECU nowadays. It would yield a benefit/cost ratio for DRL of

11 billion ECU benefits
-- = 4.15
maximum 2.65 billion ECU costs

Nevertheless, the official one million ECU per prevented fatality could be taken as the basis for the benefits. The ratio of benefits to costs for DRL then would become:

5.5 billion ECU benefits
-- = 2.08
maximum 2.65 billion ECU costs

However, the one million ECU per fatality assumes that per fatality the usually occurring 35 injured persons and 260 damage-only accidents per fatality are also prevented. But DRL prevents relatively more fatalities than injured persons and relatively more injured persons than damage-only accidents. The ratios 1 : 34 : 230 for fatalities to registered injured persons to registered damage-only accidents become for DRL 1 : 28 : 135. Based on the economic costs of a fatality, an injured person and a damage-only accident (ETSC, 1997) the one million ECU per fatality becomes for DRL prevented fatalities 0.87 ECU. Correcting the benefits for this circumstance of DRL-effects (similar would apply to all speed related safety effects) the bare minimum benefit/cost ratio becomes:

minimum 4.78 billion ECU benefits
-- = 1.80
maximum 2.65 billion ECU costs

So certainly a return of 1.8 ECU on an invested 1 ECU for DRL, because corrected for the under-reported casualty and damage-only accidents the actual benefit/cost ratio is nearly 3.6.

Only in the summer of Portugal, Spain, Italy and Greece are the maximum costs of DRL higher than its minimum benefits. The ratio of minimum benefits and maximum costs of a behavioural winter DRL-obligation (45% of the above costs and 75% of the benefits) is about 3. Nonetheless DRL for the summer in the countries of the EU with exclusion for the four most southern countries means 32% of the above costs and 18% of the benefits and thus still gives a benefit/cost ratio of just above 1. Moreover, DRL for the winter must be a partial and behavioural obligation and probably will not get a full compliance, which reduces the benefits. Therefore, a full year DRL as a gradually introduced DRL from an automatic in-vehicle DRL-regulation for the whole of the EU from a particular year onwards remains the recommended kind of DRL for the EU.

Relevant also may be that DRL is one of the few safety measures for which the investments do not come from the public authorities, but are paid by the road users themselves, who in the end will also get the benefits form less increased or lower insurance costs. In particular the investments are mainly made by motor vehicle owners, while the non-financial safety benefits for non-motorised road users are the same as for drivers.

6.6. **Conclusion**

It is recommended on technical, practical and legal grounds that compulsory DRL, when implemented in the EU, should be an automatic in-vehicle DRL that uses either low beam headlights of special DR-lamps. Because of the large safety effects from full DRL in the EU with a benefit/cost ratio of at least 1.8, it is recommended to make plans for a EU-regulation on automatic in-vehicle DRL for new motor vehicles from a particular year onward. The year to be chosen preferably is decided after the political and public acceptance of the DRL-regulation has become clear. However, any acceptance of a DRL-regulation probably will not emerge, unless prior to a DRL-regulation intensive DRL-campaigns and social marketing of DRL in the EU, initiated by the CEC, has raised the political and public awareness of the safety effects and benefits from DRL in all the countries of the EU.

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