Safety effects of permanent running lights for bicycles

A controlled experiment

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Published in:
Accident Analysis & Prevention

DOI (link to publication from Publisher):
10.1016/j.aap.2012.07.006

Publication date:
2013

Document Version
Accepted author manuscript, peer reviewed version

Elsevier Editorial System(tm) for Accident Analysis & Prevention
Manuscript Draft

Manuscript Number:

Title: Safety effects of permanent running lights for bicycles: A controlled experiment

Article Type: Full Length Paper

Keywords: Bicycle running lights, cyclists, safety evaluation, controlled experiment

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Safety effects of permanent running lights for bicycles: A controlled experiment

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Abstract
Making the use of daytime running lights mandatory for motor vehicles is generally documented to have had a positive impact upon traffic safety. Improving traffic safety for bicyclists is a focal point in the road traffic safety work in Denmark. In 2004 and 2005 a controlled experiment including 3.845 cyclists was carried out in Odense, Denmark in order to examine, if permanent running lights mounted to bicycles would improve traffic safety for cyclists. The permanent running lights were mounted to 1.845 bicycles and the accident rate was recorded through 12 months for this treatment group and 2.000 other bicyclists, the latter serving as a control group without bicycle running lights. The safety effect of the running lights is analyzed by comparing incidence rates – number of bicycle accidents recorded per man-month – for the treatment group and the control group. The incidence rate, including all recorded bicycle accidents with personal injury to the participating cyclist, is 19% lower for cyclists with permanent running lights mounted; indicating that the permanent bicycle running light significantly improves traffic safety for cyclists. The study shows that use of permanent bicycle running lights reduces the occurrence of multiparty accidents involving cyclists significantly. In the study the bicycle accidents were recorded trough self-reporting on the Internet. Possible shortcomings and problems related to this accident recording are discussed and analyzed.

Key words: Bicycle running lights, cyclists, safety evaluation, controlled experiment

1. Background

By October 1st 1990 it was made mandatory for car users to use daytime running lights in Denmark. The safety effects of this legislation were documented through simple before-after studies by Hansen (1993; 1995). It was concluded that the introduction of daytime running lights had reduced the number of accidents – especially accidents involving more than one party. In 1996, Elvik (1996) conducted a meta-analysis estimating the mean effect of introducing daytime running lights to motorized vehicles. The mean effect was estimated to a 3-12% reduction in the occurrence of multiparty daytime accidents. In the "Handbook of traffic safety measures" (Elvik et al., 2009), the effect of making daytime running lights mandatory for motorized vehicles is estimated to 5-10% reduction in daytime multi-party accidents, and it is documented that the effect varies between different types of accidents.

The Danish Road Safety Commission formulates objectives and strategies for the Danish road safety work. For several years, the improvement of road safety for cyclists has been singled out as an area of special priority by the commission (Danish Road Safety Commission, 2001). Bicyclists have been declared a high risk group. In the beginning of the last decade, when this project was initiated, the number of cyclists killed in road traffic in Denmark amounted to 50-60 persons per year; the total number of injured cyclists recorded by the police amounted to 1.500 – 1.750 persons per year (Danmarks Statistik, 2003). In terms of accident risks the number of cyclists killed and seriously injured per kilometre travelled is more than 9 times higher than the number of car-users killed or seriously injured per travelled kilometre (Brems and Munch, 2008). More over, accidents involving cyclists are in general more severe than accidents not involving cyclists (Madsen, 2005). The safety problems related to cyclists are unfortunately even bigger than the official statistics based on police recordings indicate. This is due to a severe underreporting of accidents involving cyclists. Comparisons between police recordings and hospital recordings of people injured in road traffic accidents shows that only 15% of the total road traffic injuries are in fact recorded by the police. When it comes to injured cyclists only 6% of the injuries are recorded by the police (Danmarks Statistik, 2009). Consequently, the number of injured cyclists equals the number of injured car-users on a yearly basis in Denmark even though the total number of passenger-kilometres for cars is more than 20 times higher than for bicycles (Statistics Denmark, 2009).

Faced with climatic changes and in order to improve public health it has been and still is a national Danish priority to change modal-split and specifically move trips from car to bike. Given the higher risks related to transport by bike, the task of identifying measures that can effectively improve traffic safety for bicyclists is of special interest in Danish road

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safety research. One theory as to why the accident risk is higher for bicyclists is that cyclists are less visible in traffic. In that context studies by Williams and Hoffmann (1979), Thomson (1980) and Wulf et al. (1994) indicate that poor visibility may explain why the accident rates are especially high for moped users and motorcyclists. In-depth studies of accidents involving cyclists made by the Danish Road Traffic Accident Investigation Board indicate that this too may be the case for cyclists (Havarikommission for Vejtrafikulykker, 2008).

1.1 Running lights for bicycles; hypothesis of effect

In order to improve the visibility of mopeds and motorcycles, the use of daytime running lights had already been made mandatory in Denmark several years before daytime running lights was made mandatory for cars. According to Elvik et al. (2009), the average effect of introducing mandatory use of daytime running lights for mopeds and motorcycles is a 7% reduction in multiparty daytime accidents involving motorcycles and mopeds.

In the wake of the positive effects of introducing daytime running lights for motor vehicles, the possibility of introducing a running light for cyclists was discussed. The idea was that positive safety effects were likely for two reasons; I) A permanent running light for bicycles would improve the visibility of bicyclists during daytime, where cyclists normally do not use their conventional bicycle lights. II) With a bicycle running light permanently fixed to the bike, the problem of cyclists forgetting their conventional bicycle light when it is dark or in the twilight period would be eliminated. Consequently introducing a permanent bicycle running light should in general lead to an improvement of the visibility and hence the safety of bicyclists during daytime, twilight and night time hours. As visibility should be improved, the key hypothesis is that the use of bicycle running lights will reduce the occurrence of multiparty accidents involving cyclists.

1.2 Bicycle running lights technology

The idea of introducing a permanent bicycle lights for cyclists was for several years hampered by the lack of a convincing technical solution. However, in 2002 the Traffic Research Group at Aalborg University and the Municipality of Odense; the latter in the capacity of being the national cycling city of Denmark, were introduced to a new type of bicycle light; manufactured by the Danish company Reelight, which had the potential to serve as a permanent running light for bicycles. The light is based upon the electro-dynamic induction principle. Two magnets are fixed to the each of the spokes of both wheels and the lights are mounted to the front and the rear wheel fork. When the magnets passes the light an electric current is induced, which makes the lights flash, when the wheels are rolling. As opposed to the classical dynamo set; the new magnet lights are silent and more or less free of friction. In comparison to battery lights; the magnet light does not require any batteries and the light is on the moment, the wheels are rolling.

As the magnet lights are fixed permanently to the bike, are battery-free and requires little maintenance; problems of forgetting the lights and/or batteries being flat should be eliminated. Since the lights are on, when the wheels are rolling, the hypothesis is that such permanent driving light will improve the visibility of cyclists under all circumstances thus reducing their accident risk. However there were downsides to the design of the running lights. First of all, they only flash, when the magnets pass the light; consequently they would not flash, when the bike was not moving e.g. at intersections. Secondly the lights were placed at low positions on the forks; depending on the size of the bike up to 0.4 metres above the ground, thus reducing the visibility of the lights in comparison to most traditional battery operated bicycle lights, where the front light is typically mounted on the handle bar.

2. Experimental design

At the time of introduction, the magnetic lights were in fact illegal as the use of flashing front lights was prohibited in Denmark. In combination with the downsides related to the design; Danish authorities would not approve the running lights until the safety effects of the lights had been examined. The Municipality of Odense and the Traffic Research Group were therefore granted permission to perform a test of the bicycle running lights in Odense with the aim of documenting positive as well as negative safety effects related to the bicycle running lights.

Principally the safety effects of the running lights can be documented in two ways: I) As an observational before-after study based upon an examination of accident rates amongst cyclists before and after the running lights were mounted to their bikes. II) As a controlled experiment, i.e. comparing accident rates among a group cyclists with running lights mounted to their bikes (the treatment group) and a comparable group of cyclists without running lights (the control group).

Optimally the effect of a given countermeasure is given by the ratio between the expected number of accident in time T with the countermeasure implemented and the expected number of accidents in the same timeframe T as it would have been had the countermeasure not been in place. Many road safety evaluation studies are carried out as observational before-after studies, where the accident counts after are used as a proxy for the expected number of accidents with the treatment in place, while the accident count before is used to predict what the expected accident count would have been had the treatment not been in place, see e.g. Hauer (1997). The challenge in the before-after study is that one has to be able to isolate the changes in the accident count from before to after that are due to; general accident trends, random variation/regression-to-the-mean, changes in exposure etc. from the parts of the change that is due to the treatment under evaluation.

Many earlier studies of the safety effects of daytime running lights for motor vehicles were performed as observational before-after studies and the mean effects of daytime running lights are generally estimated from observational before-after studies. However, Elvik (1993; 1996) states that observational before-after studies may not provide sufficient control for confounding factors that may have affected the outcome of the evaluation. In comparison controlled experiments are deemed to provide a better control for confounding factors in studies of this type, see e.g. Hauer (1997). Consequently, such study design was adopted in the evaluation of the safety effects of bicycle running lights.

2.1 The controlled experiment

The basic concept of the controlled experiment is to create a minimum of two experimental groups; one group that receives treatment (the treatment group) and one group that does not receive treatment (control group). Ideally the two groups must be identical with respect to extraneous factors influencing the outcome of interest, so that if none of the groups were treated the outcome recorded in time T for both groups would be the same. Consequently the effect of the treatment can be found by comparing the outcome of interest in time T for the treatment group and the control group. In order to obtain the desired control for confounding factors, the experimental units must however be allocated to the treatment and the control group at random; i.e. through randomization. Applying such randomization to the experimental design ensures that any variation in the outcome not due to the treatment under evaluation can justifiably be labelled as chance variation, i.e. random variation. Hence any systematic variation found in the outcome of interest must be attributable to treatment effect (Rothman et al., 2008).

Within medicine controlled experiments find widespread use when it comes to documenting the effects of new types of medicine and treatment. As the experimental groups are normally patients; that is persons with a given diagnosis, these studies are performed as clinical studies. In such studies the aim is not to prevent the occurrence but to control the consequences of the occurrence. However, when it comes to the bicycle running lights the aim is to reduce the occurrence of accidents – hence the controlled experiment has to be performed as a field trial. Field trials are studies that include free-living subjects. The challenge in field trials is that large experimental groups are generally required, especially when the outcome of interest is rare. Furthermore one needs to make sure that all outcomes are recorded for both the treatment and the control group.

In the given case the outcome of interest is accidents by bike for both the treatment and the control group. Ideally the police should record all accidents by bike, but as described above only 5-10 percent of all injured cyclists are recorded by the police. Hence, if the study was to be based upon police recordings of bicycle accidents involving persons from the treatment and the control group, both experimental groups would have to be very large in order to safely evaluate the safety effects of the bicycle running lights. Consequently, it was decided that the study should be based upon self-reporting of bicycle accidents on the Internet for the participants in the treatment and the control group.

2.2 Self-reporting of accidents

To that end a web-based survey was developed. The web-survey was designed as a questionnaire developed from the Danish police' accident recording scheme, thus obtaining the same key information from each self-reported accident as is normally obtainable from the police recordings. During the project each of the participants received a personal e-mail every second month reminding them to report any accidents that they may had been involved in as cyclists for the last two months. The e-mails as well as the web-survey included an accident definition according to which an accident was defined as; "an incident where you, riding your bicycle, have unwilling been forced off your bike and/or crashed either by yourself or due to collision or near-collision with others; cars, trucks, vans, mopeds, other cyclists, pedestrians etc.". The information recorded included; time of accident (month, day, hour), accident type, counterpart(s), road conditions, light conditions, weather conditions, accident description, injuries, severity of injuries sustained, hospital treatment of injuries, reporting of accident to police and insurance company. All persons in the treatment and control group were asked to answer the survey every second month. Participants who had not been involved in an accident should answer accordingly. Participants who had not replied within 14 days received a reminder by e-mail.

2.3 Recruitment of participants

In order to investigate if the safety effect of the bicycle running lights varies over the year, it was decided that both the treatment and the control group should report their bicycle accidents through a whole year. Based upon the experiences from an earlier bike-and-bus promoting project that had deployed a self reporting of bicycle accidents (Lohmann-Hansen et al., 2001), it was estimated that both the treatment and the control group should consist of approximately 2.000 persons in order to obtain valid estimates of the safety effects of the permanent running light for bicycles. Having presented the project in the media, more than 18.000 persons from a total population of 180.000 persons in the Municipality of Odense expressed interest in the project and a total of 11.800 persons volunteered for the project. The persons to be included in the treatment group were offered a free set of bicycle running lights that would be mounted to their bike free-of-charge prior to the start of the project November 1st 2004. The persons selected for the control group would, as an incentive to participate, receive a free set of bicycle running lights after the projected was concluded by October 31st 2005.

The optimum randomization is achieved if each individual in the treatment and in the control group is selected individually; that is selected at random from the pool of volunteers and then randomly assigned to either the treatment group or the control group. However in this case, it was a specific objective that children were represented in the study. In order to secure their participation, it was decided to draw households instead of individuals at random and then randomly assign these to the treatment and control group. It was however only the household members that had volunteered that would in fact participate. This randomization procedure is known as cluster randomization. The problem of cluster randomization is that it threatens to undermine the control for confounders, if the clusters are large in comparison to the total study size (Rothman et al., 2008). In this case the average cluster size is 1.7, hence the appliance of cluster randomization is not considered problematic.

Ideally the experiment should have been performed as a doubled blinded experiment, where neither the participants nor the researchers knows specifically who is assigned to the treatment group and the control group respectively. However, for obvious reasons such design could not be adopted in this study.

2.4 Participants

At the start of the project by November 1st 2004 a total of 1.845 persons had the bicycle running lights fitted to their bicycle and 2.000 persons had accepted to participate in the control group. When a randomization is performed, it is implicitly assumed that for each of the possible confounding variables the mean value is the same for both the treatment and the control group. In reality some variations in the values of the variables should be present, but the randomization process should secure that the mean values for each experimental group are close to each other. However in order to able to assess, if the treatment and the control group are in fact probabilistically equivalent, a web-questionnaire was conducted as part of the recruitment. Following the cluster randomization it was from the individual answers to this questionnaire and through the appliance of chi^2-tests tested if the groups were equivalent in terms of e.g.; gender, age, car-ownership, use of bicycle summer and winter, see table 1. The statistical analysis revealed no significant differences on possible confounders between the treatment and the control group, nor do the participants diverge from the total pool of volunteers. This suggests that the cluster randomization provides the required control for confounding factors.

Throughout the test year the persons in the treatment and the control group were asked to use their bicycles as they normally would and report back every second month, if they had been involved in an accident as a cyclist. In general the participation was high throughout the project as reflected in table 2. A total of 1.592 persons (86.3%) from the treatment group reported back on all 6 questionnaires, as did a total of 1.714 persons from the control group (85.7%), see table 3. In that respect the drop-out rate over the whole year is almost identical for both groups.

2.5 Accident data

From November 1st 2004 to October 31st 2005 a total of 277 bicycle accidents were reported by the participants. 109 accidents were reported by the treatment group and 168 accidents were reported by the control group. 1.738 persons from the treatment group were not involved in an accident as a cyclist as is the case for 1.846 persons from the control group, see table 4. In the evaluation of the safety effects of the bicycle running lights it was, see below, decided only to include data from the participants, who had reported accidents throughout the whole research period. The number of accidents reported from participants with complete accident reporting amounts to a total of 255 bicycle accidents. The characteristics of these accidents are summarized in table 5.

3. Data analysis

Within medicine and clinical epidemiology the effects of given treatments are often documented by estimating incidence rate ratios, as it is considered a simple, easily understood measure of effect in clinical and field trials (Greenberg et al., 2001). The basic concept of incidence rate ratio analysis is to compare the incidence rate of the outcome of interest for the treatment group to the incidence rate of the same outcome for the control group. The incidence rate, IR, for a given experimental group, IR_g, is defined in the following way (Fletcher and Fletcher, 2005; Rothman et al., 2008):

$$IR_g = X_g / \sum_{i=1}^{I} t_{g,i} \quad (1)$$

X is the reported number of outcomes of interest during time of observation for group g and $t_{g,i}$ is the time in which the event was a possibility and hence would have been recorded for person i belonging to experimental group g.

3.1 The incidence rate ratio

Incidence rate ratios are in general applied in cases where the outcome of interest may not occur during the observation period (Rothman et al., 2008). Consequently, this approach lends it self well to the evaluation of the safety effects of bicycle running lights, as only a relatively small proportion of the participants have in fact been involved in an accident as a cyclist through the year of observation.

Accordingly the safety effects of the bicycle running lights are estimated in terms of the incidence rate ratio given by (Rothman, 2002):

$$IRR_j = IR_{j,T} / IR_{j,C} \quad (2)$$

IRR_j denotes the incidence rate ratio for accidents of type j. $IR_{j,T}$ is the incidence rate for accident type j for the treatment group and $IR_{j,C}$ is the incidence rate for accident type j for the control group. The incidence rates for the experimental groups are given by:

$$IR_{j,T} = \frac{X_{j,T}}{\text{Total number of man month in treatment group}} \quad (3)$$

$$IR_{j,C} = \frac{X_{j,C}}{\text{Total number of man month in control group}} \quad (4)$$

$X_{j,T}$ is the recorded number of bicycle accidents of type j in the treatment group and $X_{j,C}$ is the recorded number of accidents of type j in the control group.

The incidence rate ratio may assume values in the interval [0; ∞[. An estimate around 1.0 would indicate that the incidence rates are identical, i.e. the bicycle running lights does not improve nor does it impair traffic safety for cyclists. An incidence rate ratio larger than 1.0, would indicate that the bicycle running lights impair safety, whereas an incidence rate ratio less than 1.0, would indicate a safety improvement. The incidence rate ratio is skewed toward the right, as any positive effect of the bicycle running light is compressed into the interval ranging from 0 to 1.0. In comparison any negative safety effects are reflected in incidence rate ratios within the interval from 1.0 to infinity.

3.2. Confidence interval for IRR

In the study randomization is applied to the selection of the experimental groups in order to control for confounding factors that may affect both the results and the conclusions. The comparison of gender, age, car ownership, use of bike etc. suggests that on the average there are no significant differences between the two groups on possibly confounding variables. Hence any differences in the incidence rates should be down to the safety effects of the bicycle running lights or down to random variation in accident occurrence between the treatment group and the control group.

In order to determine, if found variations in the incidence rates are likely to be a result of the bicycle running lights 95% confidence intervals are estimated for the incidence rate ratios. If the 95% CI does not include the non-effect value 1.0 the difference in the incidence rates is significant; i.e. the bicycle running lights are most likely to have affected the occurrence of bicycle accidents amongst the members of the treatment group. On the other hand, should the 95% CI include the non-effect value 1.0 the changes to safety suggested by the IRR may be purely or primarily random.

In estimating the 95% CI for the incidence rate ratios it is common to apply a statistical model, which allows the outcome of interest; here the number of bicycle accidents, to vary randomly without any upper limit, specifically in terms of the Poisson model (Rothman, 2002). Consequently, the standard error of the estimated incidence rate for accident type j, for each experimental group g, SE ($IR_{j,g}$), is given by:

$$SE(IR_{j,g}) = \sqrt{X_{j,g} / \left(\sum_{i=1}^{I} t_{g,i}\right)^2} \quad (5)$$

In order to account for the skewness of the incidence rate ratios, it is customary to estimate the 95% CI through a logarithmic transformation (Greenberg et al., 2001; Rothman, 2002). Consequently the 95% CI for the IRR_j is given by (Juul, 2005):

$$95\%\ CI(IRR_j) = EXP\left\lfloor \ln IRR_j \pm 1.96 * SE(\ln IRR_j) \right\rfloor \quad (6)$$

$$\ln IRR_j = \ln IR_{j,T} - \ln IR_{j,C} \quad (7)$$

$$SE(\ln IRR_j) = \sqrt{1/X_{j,T} + 1/X_{j,C}} \quad (8)$$

The 90% CI (IRR_j) is also estimated in order to identify tendencies towards positive or negative safety effects and is given by:

$$90\%\ CI(IRR_j) = EXP\left\lfloor \ln IRR_j \pm 1.645 * SE(\ln IRR_j) \right\rfloor (9)$$

The formulas presented here only provide approximate results, as formula (8) represents a simplified way to estimate the standard error of the natural logarithm of the incidence rate ratio. This approximate method will provide results close to the results obtained by the much more complicated exact methods, if the study sample is large. In terms of defining how large the study sample must be in order to provide near-exact estimates of the confidence intervals, the literature is not very specific. However, Rothman (2002) states that even for studies with modest samples, the interpretation of the study based on approximate results is rarely different from the interpretation offered, when the exact methods are used for estimating the 95% CI.

3.3 Missing values

One important aspect in the data analysis is the handling of missing data. A total of 86.0% of the participants reported back on all six questionnaires, while e.g. 2.1% of the participants only reported back 4 out of 6 times. Consequently for 14.0% of the participants, the accident report is incomplete. The key question is; how do we deal with these missing data? Does missing data contain accidents or not? One way of dealing with the missing data, is to include the data as they are; that is a person with two missing answers will only contribute with 8 man months and the accident recordings within these eight months are included in the estimate of the incidence rate.

One could argue that a missing data are likely not to cover accidents; especially if the missing data lies in between answers/accident recordings; hence one would be inclined to treat the missing data as a zero accident recording, thus imputing the observation “0” to the missing accident recordings. However, one cannot rule out the possibility that some of the missing data may in fact include an accident and if this is the case, the zero-imputation would be wrong. Substantive literature concerning the dealing with missing data is available as are a number of methods for dealing with missing data, see e.g. Allison (2001).

In this study it was decided to perform a complete-subject analyses; thus excluding the data from all participants with one or more missing accident recordings. Such approach to dealing with missing values is valid, if the participants have been selected at random as the missing data should then too completely missing at random (Little and Rubin, 2002). The downside to this approach is of course that the data that has been recorded from the persons with missing data will not be included in the analysis. In this case the appliance of a complete-subject analysis has the consequence that 11 accidents recorded by the treatment group and 11 accidents recorded by the control group are omitted from the analysis. The total incidence rate (accidents per man month) estimated from the group of persons omitted due to missing recordings amounts to 0.0065, whereas the total incidence rata for the complete-subject group is 0.0064. Moreover, further statistical analysis shows that the group of persons with incomplete accident recordings does not significantly diverge from the participants with complete accident records; hence the omission does not effect the randomization.

3.3 Incidence rates versus accident risks
When it comes to interpreting the incidence rates, it is important to stress that they reflect an incidence rate in terms of the recorded number of accidents per man month. As such the incidence rates can be interpreted as an accident rate. These accident rates cannot be regarded as a measure of accident risk, because the recorded accidents are related to the size of each experimental group and the duration of the experiment, and are not directly linked to a measure of exposure such as; e.g. the number of trips made by bike, total number of kilometres travelled by bike or total duration of trips made by bike. As an example, tables 6 and 7 reflect that the accident rates for the cyclists are higher under daylight conditions than under night time conditions. From this it cannot automatically be concluded that the accident risk is higher for cyclists during daytime hours than during night time hours, as the number of trips made by bike is significantly higher during daytime.

In terms of evaluating the safety effects of bicycle running lights, the incidence rate ratios still serves the purpose very well, as the random selection of participants for the treatment and the control group should ensure that the exposure rate under daylight, twilight and night time conditions are of equal magnitude for the two experimental groups. As the exposure rates can be regarded as identical found variations in the estimated incidence rates should reflect differences in the accident risk between the two groups during daytime, twilight and night time conditions. Consequently, the incidence rate ratios should reflect the safety effects of the bicycle running lights.

4. Results
The initial results shows that there is a significant difference in the accident rates both in terms of all recorded bicycle accidents and the recorded bicycle accidents with personal injury to the members of the experimental groups. Specifically, the accident rate is significantly lower for the treatment group with bicycle running lights than for the control group. As such the initial results reflect a significant, non-random positive safety effect of the bicycle running lights, see tables 6 and 7. In terms of all reported accidents the accident rate is 33% lower for the cyclists with bicycle running lights than for those with out. The accident rates for bicycle accidents resulting in personal injuries suggests an ever better safety improvement, as the rate of bicycle accidents resulting is personal injury is 41% lower for the treatment group.

This general improvement to traffic safety is down to significantly lower accident rates for the treatment group when it comes to accidents in daylight and a tendency towards lower accident rates for accidents during the twilight period at morning and in the evening. On the other hand there is no significant difference in the accident rates, when it comes to accidents occurring, when it is dark; i.e. night time accidents. This suggests that the bicycle running lights especially improves the visibility of cyclists in daylight and partly in the twilight periods. This is consistent with the fact that cyclists, as is the case for the control group, do not use their conventional bicycle lights during daytime, and furthermore it is far from all cyclists that use their conventional bicycle lights in the twilight periods.

The fact that there is no significant difference in the accident rates for accidents occurring, when it is dark, may reflect that the persons in the control group are good at remembering to put on their conventional bicycle lights, when it is dark. This is very likely, as the participation in the project may have emphasised the importance of using bicycle lights during the dark hours for the persons in the control group. Alternatively the members of the control group are good at compensating for the factual higher accident risk, should they forget to put on their conventional bicycle lights when it is dark, e.g. by cycling at lower speeds, choosing routes with low traffic volumes, by generally giving yield to other road users. If this is in fact the case, the results will tend to underestimate the safety effect of the bicycle running lights, when it comes to bicycle accidents in the twilight periods and in the dark hours.

As such the initial results only give support to the hypothesis that bicycle running lights improve bicycle safety through an increased visibility of the cyclists especially during daylight hours.

5. Discussion
In comparison to the effects of daytime running lights for motor vehicles, motorcycles and mopeds the found positive effects of the bicycle running lights are considerable larger. Part of the explanation may very well be that the safety improvements offered by permanent bicycle running lights are larger than for motor vehicles, motorcycles and mopeds as cyclists are much less visible than these road users. However, the magnitude of the found effects is likely also to reflect a weakness in the experimental set up.

5.1 Sources of error
The members of both the treatment and the control group were selected by randomization from a total pool of volunteers of 11.800 persons. As they have signed up voluntarily, self-selection is a possible source of error in the experi-

ment. In that respect it could be argued that the persons who have volunteered for the project are persons who are more cautious in traffic than normal. Consequently, this should result in accident risks that are lower for the experimental groups than for the population as a whole. On the other hand, the participants in both the treatment group and the control group as well as the total pool of volunteers, tend to cycle more frequently than the average Dane, meaning that their level of exposure to bicycle accidents may in fact be higher than normal. The prior suggests lower than normal accident rates; the latter higher than normal accident rates, as the accident rate is determined by both the accident risk and the level of exposure.

In terms of evaluating the safety effects of the bicycle running lights, self-selection; and hence lower than average accident risk but higher than normal exposure to bicycle accidents, is due to the randomization process equally likely for both the treatment group and the control group. As a result, self-selection should not in principle represent a source of error in the study, as the safety effect is estimated in terms of the incidence rate ratios; the ratio between the accident rate of the treatment group and the accident rate of the control group.

The self reporting of accidents is on the other hand somewhat problematic. Prior to the study, it was expected that the bicycle running lights would reduce the occurrence of multiparty accidents involving cyclists. The initial results suggest that this is a very likely outcome, as the accident rate is 45% lower for the treatment group than for the control group, when all reported accidents are taken into account, and 61% lower when only accidents with personal injury is taken into account. The bicycle running lights were, however, not expected to affect the occurrence of solo accidents, but the initial results show that the accident rates for solo accidents are 24% (all accidents) and 27% (person injury accidents) lower for the treatment group than for the control group; the effects close to being significant.

It is likely that this apparent effect of the bicycle running lights is actually a result of a systematic under-reporting of accidents in the treatment group due to an inherent bias in favour of the bicycle running lights amongst the members of the treatment group. During the project, additional questionnaires were carried out in order to evaluate the design and functionality of the bicycle running lights. From the data gathered here, it is evident that the members of the treatment group were very fond of the running light as they found the bicycle running lights very convenient, e.g. they did not have to buy batteries any more, they did not have to fear being stopped by the police for having forgotten their bicycle lights, they felt very safe with the bicycle running lights etc. As a consequence it is likely that the treatment group has been somewhat strategic in their reporting of accidents by omitting some of the minor bicycle accidents. The effect for solo accidents is almost the same for relevant subgroups of accidents, see table 8, which suggests that the underreporting is general and not associated with certain accident types.

5.2 Corrected estimates of safety effect

An additional analysis was performed in order to control for this apparent underreporting of accidents by the treatment group. This additional analysis was performed on the basis of the reported cyclist accidents with personal injury to the participants. The control for the underreporting of accidents was performed by using the estimated incidence rate ratio for solo accidents with personal injuries as a general correctional factor, C_{corr} = 0.73, as the level of underreporting appears to be of the same magnitude for the analyzed accident types. This correctional factor was multiplied to the reported number of accidents in the control group; thus reducing the accident count in the control group in accordance with the likely underreporting found in the reference group. Consequently corrected estimates of the incidence rates for the control group were estimated as follows:

$$IR_{j,C,corr} = \frac{X_{j,C} * C_{corr}}{\text{Total number of man month in control group}} \quad (10)$$

From the corrected estimates of the incidence rates for the control group, the incidence rate ratios and the corresponding 95% CI and 90% CI were re-estimated, see table 9. The corrected incidence rate ratios indicate that the accident rate for bicycle accidents with personal injury is 19% lower for the treatment group than for the control group. Furthermore the corrected estimates suggest that the safety improvements are related to a reduced accident rate under daylights and twilight conditions. The use of bicycle running lights does not seem to reduce accident rates, when it is dark. As stated above, this may be down to the fact that the members of the control group are good at remembering their conventional bicycle lights or good at risk compensating, when it is dark. The downscaling in the number of accidents reported by the control group, which is a result of the applied correction for likely underreporting by the treatment group, however has the effect that none of the estimated differences in the incidence rates are significant; the incidence rates for multiparty accidents being the only, but very important exception.

For multiparty accidents with personal injury to the participating cyclist, the corrected incidence rate ratio is estimated to 0.53; thus suggesting that the accident rate is 47% lower for the users of the bicycle running lights. This result is significant at the 2% level. The fact that there is a significant difference in the accident rates in terms of the multiparty accidents is in accordance with the key hypothesis of this study; the use of bicycle running lights reduces the occurrence of multiparty accidents (with personal injury) involving cyclists. In comparison to the obtained effects of making day-time running lights mandatory for cars, motorcycles and mopeds, the corrected estimated effect to multiparty accidents is despite the correction still considerable. Again this suggests that the improvement of the visibility due to the use of permanent running lights is more substantial for cyclists than for other road users.

In order to gain further insight as to how the bicycle running lights affect the occurrence of multiparty accidents separate analysis of the multiparty accidents with personal injury were performed applying the general correctional for the underreporting of accidents, see table 10. From this it follows that the lower accident rate for multiparty accidents for the treatment group is down to a significantly lower accident rate for multiparty bicycle accidents in daylight, thereby indicating that the use of bicycle running lights will primarily reduce the occurrence of daytime multiparty bicycle accidents with personal injury. In terms of the effect on multiparty bicycle accidents during twilight and in the dark hours; the recorded number of multiparty bicycle accidents is too small for any conclusions to be drawn with certainty as reflected in the (very) wide 95% CI's for the incidence rate ratios.

In terms of the counterparts in the multi-party bicycle accidents, the results indicate that the bicycle running lights reduces the occurrence of bicycle accidents with personal injury, where the bicyclist collide with other cyclists and pedestrians as well as accidents, where the counterparts are motor vehicles; cars, vans, trucks/busses, motorcyclists and mopeds. The rate of bicycle accidents involving other cyclists and pedestrians as the counterpart is 45% lower for the treatment group than the control group and the difference in the accident rate is nearly significant. The rate of multiparty accidents with personal injury involving motor vehicles as counterpart is 49% lower for the treatment group. However, in terms of the latter the found difference in the accident rates is significant only at the 10% level; i.e. when applying a 90% CI for the IRR.

5.3 Aftermath

Based upon the results from the controlled experiment performed in Odense, the Danish Road Safety and Transport Agency decided to legalise the bicycle running lights. Prior to the experiment there was some concern regarding the fact that the bicycle running lights did not flash, when the cyclists were at halt. Since then, bicycle running lights have undergone further technical improvements. Latest editions of the bicycle running lights continue to be on, even when the wheels are no longer rotating; they stop flashing 2 to 4 minutes after the bicycle is brought to a halt. Newer models even provide a steady, continuous light in stead of a flashing light. Accordingly, it would be relevant to test, if flashing bicycle running lights provide better visibility than a steady continuous bicycle running light; this should be an object of future research activities. Furthermore it should be examined, if the safety effects could be enhanced, if the bicycle running lights were attached to the bike at a higher position.

In 2004 the Danish Cyclists Federation conducted a national registration of cyclists without cycling lights within lightning hours. According to the registration 29% of all the recorded cyclists did not use a cycling light although it is mandatory within the lightning hours. In 2009 the proportion of cyclists without cycling lights had fallen to 16%. According to both the Danish Cyclists Federation and Danish Police this is due to the availability and subsequent widespread – although not mandatory – use of the bicycle running lights (Fausbøll, 2010).

6. Conclusions

The controlled experiment gives evidence that the use of permanent bicycle running lights will significantly improve traffic safety for cyclists due to the improvement of visibility. The results of the project indicate that the occurrence of bicycle accidents with personal injury to the cyclist is 19% lower for persons using a permanent bicycle running light than for persons not using a permanent bicycle running light. Significant effects are documented for multiparty bicycle accidents with personal injury, where the accident rate is 47% lower for persons using a bicycle running light. The results indicate that the bicycle running lights may reduce the occurrence of multiparty bicycle accidents involving motor vehicles as the counterpart as well as the occurrence of multiparty accidents involving other cyclists and pedestrians as the counterpart.

The safety effects are especially related to daytime multiparty bicycle accidents. In daylight most cyclists do not use their conventional cycle lights, but the results reflect that the use of bicycle lights during daylight could significantly reduce the occurrence of multiparty accidents involving cyclists. Cycling without bicycle lights is generally considered most problematic during the twilight and the dark hours. In that respect the introduction of permanent bicycle running lights should effectively solve the problem of cyclists forgetting their bicycle lights; thus leading to a reduction in the occurrence of multiparty bicycle accidents during the twilight and the dark hours. However, the performed experiment only suggests a possible reduction in the occurrence of multiparty bicycle accidents during twilight hours. Not being able to document significant effects to the occurrence of bicycle accidents during twilight and dark hours may be down to the size of the project and the fact that, as a result of self-selection, the members of the control group are generally good at remembering their conventional bicycle lights, when it is dark.

Methodologically the evaluation of the bicycle running lights was performed by analyzing and comparing incidence rates; i.e. bicycle accidents recorded in a treatment group and a control group. In order to control for confounding the participants in the experiment were selected at random and significant effects were documented through statistical analysis of incidence rate ratios. As official accident data bases suffers from a severe underreporting of bicycle accidents, the evaluation of the safety effects was based upon a self-reporting of accidents from the participants. The initial analysis of the reported accidents reveals that the use of self-reporting schemes is problematic, if the participants in the experiment are biased in favour of the treatment under evaluation. In the given study it is likely that the members of the treatment group have been biased in favour of the bicycle running lights, which has resulted in a likely under-reporting of bicycle accidents from the treatment group. A control for this likely underreporting has been performed by using the difference in the reporting of bicycle solo accidents by the treatment group and the control group as a proxy for the likely underreporting.

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Table(s)

Tables – Safety effects of bicycle running lights

Table 1: Participants characteristics; treatment group and control group. Only participants with full accident reporting are included.

Characteristic		Treatment group	Control group
Gender	Female	53.7%	53.6%
	Male	46.3%	46.4%
Age	Mean	31.0 years	31.4 years
Car ownership	0 cars in household	24.1%	24.4%
	1 car in household	65.2%	64.9%
	2 cars in household	10.0%	10.0%
	> 2 cars in household	0.7%	0.7%
Use of bike – winter	Daily	69.3%	69.9%
	3-4 times per week	21.0%	20.1%
	1-2 times per week	8.8%	8.7%
	Once every 14 days	0.7%	1.1%
	Monthly	0.2%	0.2%
Use of bike – summer	Daily	79.7%	80.9%
	2-4 times per week	17.1%	15.8%
	Weekly	2.9%	2.9%
	Once every 14 days	0.1%	0.2%
	Monthly	0.1%	0.1%
Participants per household	Mean cluster size	1.74	1.71

Table 2: Participants dropout rate – illustrated by the number of participants responding to the bimonthly reporting of accidents.

Reporting no.	Treatment group			Control group		
	Answers	Participants	Answers %	Answers	Participants	Answers %
1	1.774	1.845	96.2%	1.918	2.000	95.9%
2	1.767	1.845	95.8%	1.903	2.000	95.2%
3	1.760	1.845	95.4%	1.862	2.000	93.1%
4	1.719	1.845	93.2%	1.828	2.000	91.4%
5	1.704	1.845	92.4%	1.812	2.000	90.6%
6	1.679	1.845	91.0%	1.802	2.000	90.1%

Table 3: Proportions of participants in the treatment and control group with full accidents recording, 10 months accident recording, 8 month accidents recording etc. Participants with 0 months accident recordings have not answered any of the questionnaires.

Accident recording Months	Treatment group		Control group	
	Persons	Percentage of group	Persons	Percentage of group
0	34	1.8%	44	2.2%
2	16	0.9%	33	1.7%
4	15	0.8%	38	1.9%
6	51	2.8%	44	2.2%
8	39	2.1%	42	2.1%
10	98	5.3%	85	4.3%
12 (full record)	1.592	86.3%	1.714	85.7%
Sum	1.845	100.0%	2.000	100.0%

Table 4: Accidents recorded by treatment group and control group

Group	Number of bicycle accidents reported				No. of Participants
	0	1	2	3	
Treatment	1.738	105	2	-	1.845
Control	1.846	141	12	1	2.000
Total	3.584	246	14	1	3.845

Table 5: Accident characteristics.

Accident characteristics	Treatment group		Control group	
	All	Personal injury	All	Personal injury
Accidents	98	69	157	125
Winter period	60	38	87	70
Summer period	38	31	70	55
Daylight accidents	57	45	101	81
Accidents during twilight hours	13	5	24	15
Accidents during dark hours	27	19	31	28
Solo accidents	64	51	91	75
Multiparty accidents	34	18	66	50
Accidents reported to police	1	-	4	4
Accidents reported to insurance company	10	9	18	17
Injuries treated at hospital/emergency rooms	10	10	23	23
Injuries treated by general practitioner only	1	1	7	7

Table 6: Incidence rates, incidence rate ratios, 95% and 90% confidence intervals for incidence rate ratios – initial analysis. All recorded bicycle accidents involving members from the treatment and control group (November 1st 2004 – October 31st 2005).

Accident type	Data	Treatment group	Control group	Total
All Accidents	Recorded accidents	98	157	255
	Man months	19.104	20.568	39.672
	Incidence rate – IR * 10^3	5.13	7.63	6.43
	Incidence rate ratio – IRR	0.67		-
	95% CI (IRR)	[0.52 ; 0.86]		-
	90% CI (IRR)	[0.54 ; 0.83]		-
Winter period (November 1st 2004 – April 30th 2005)	Recorded accidents	60	87	147
	Man months	9.552	10.284	19.836
	Incidence rate – IR * 10^3	6.28	8.46	7.41
	Incidence rate ratio – IRR	0.74		-
	95% CI (IRR)	[0.53 ; 1.03]		-
	90% CI (IRR)	[0.56 ; 0.98]		-
Summer period (May 1st 2005 – October 31st 2005)	Recorded accidents	38	70	108
	Man months	9.552	10.284	19.836
	Incidence rate – IR * 10^3	3.98	6.81	5.44
	Incidence rate ratio – IRR	0.58		-
	95% CI (IRR)	[0.39 ; 0.87]		-
	90% CI (IRR)	[0.42 ; 0.81]		-
Multiparty accidents	Recorded accidents	34	66	100
	Man months	19.104	20.568	39.672
	Incidence rate – IR * 10^3	1.78	3.21	2.52
	Incidence rate ratio – IRR	0.55		-
	95% CI (IRR)	[0.37 ; 0.84]		-
	90% CI (IRR)	[0.39 ; 0.78]		-
Solo accidents	Recorded accidents	64	91	155
	Man months	19.104	20.568	39.672
	Incidence rate – IR * 10^3	3.35	4.24	3.91
	Incidence rate ratio – IRR	0.76		-
	95% CI (IRR)	[0.55 ; 1.04]		-
	90% CI (IRR)	[0.58 ; 0.99]		-
Daylight accidents	Recorded accidents	57	101	158
	Man months	19.104	20.568	39.672
	Incidence rate – IR * 10^3	2.98	4.91	3.98
	Incidence rate ratio – IRR	0.61		-
	95% CI (IRR)	[0.44 ; 0.84]		-
	90% CI (IRR)	[0.46 ; 0.80]		-
Twilight accidents	Recorded accidents	13	24	37
	Man months	19.104	20.568	39.672
	Incidence rate – IR * 10^3	0.68	1.17	0.93
	Incidence rate ratio – IRR	0.58		-
	95% CI (IRR)	[0.30 ; 1.15]		-
	90% CI (IRR)	[0.33; 1.03]		-
Night time accidents	Recorded accidents	27	31	58
	Man months	19.104	20.568	39.672
	Incidence rate – IR * 10^3	1.41	1.51	1.46
	Incidence rate ratio – IRR	0.94		-
	95% CI (IRR)	[0.56 ; 1.57]		-
	90% CI (IRR)	[0.61 ; 1.45]		-

Table 7: Incidence rates, incidence rate ratios, 95% and 90% confidence intervals for incidence rate ratios – initial analysis. All recorded bicycle accidents resulting in personal injury to members of the treatment and control group (November 1st 2004 – October 31st 2005).

Accident type	Data	Treatment group	Control group	Total
Accidents (personal injury)	Recorded accidents	69	125	194
	Man months	19.104	20.568	39.672
	Incidence rate – IR * 10^3	3.61	6.08	4.89
	Incidence rate ratio – IRR	0.59		-
	95% CI (IRR)	[0.44 ; 0.80]		-
	90% CI (IRR)	[0.46 ; 0.76]		-
Winter period (November 1st 2004 – April 30th 2005)	Recorded accidents	38	70	108
	Man months	9.552	10.284	19.836
	Incidence rate – IR * 10^3	3.98	6.81	5.44
	Incidence rate ratio – IRR	0.58		-
	95% CI (IRR)	[0.39 ; 0.87]		-
	90% CI (IRR)	[0.42 ; 0.81]		-
Summer period (May 1st 2005 – October 31st 2005)	Recorded accidents	31	55	86
	Man months	9.552	10.284	19.836
	Incidence rate – IR * 10^3	3.25	5.35	4.33
	Incidence rate ratio – IRR	0.61		-
	95% CI (IRR)	[0.39 ; 0.94]		-
	90% CI (IRR)	[0.42 ; 0.88]		-
Multiparty accidents	Recorded accidents	18	50	68
	Man months	19.104	20.568	39.672
	Incidence rate – IR * 10^3	0.94	2.43	1.71
	Incidence rate ratio – IRR	0.39		-
	95% CI (IRR)	[0.23 ; 0.66]		-
	90% CI (IRR)	[0.25 ; 0.61]		-
Solo accidents	Recorded accidents	51	75	126
	Man months	19.104	20.568	39.672
	Incidence rate – IR * 10^3	2.67	3.65	3.18
	Incidence rate ratio – IRR	0.73		-
	95% CI (IRR)	[0.51 ; 1.05]		-
	90% CI (IRR)	[0.54 ; 0.99]		-
Daylight accidents	Recorded accidents	45	81	126
	Man months	19.104	20.568	39.672
	Incidence rate – IR * 10^3	2.36	3.94	3.18
	Incidence rate ratio – IRR	0.60		-
	95% CI (IRR)	[0.42 ; 0.86]		-
	90% CI (IRR)	[0.44 ; 0.81]		-
Twilight accidents	Recorded accidents	5	15	20
	Man months	19.104	20.568	39.672
	Incidence rate – IR * 10^3	0.26	0.73	0.50
	Incidence rate ratio – IRR	0.36		-
	95% CI (IRR)	[0.13 ; 0.99]		-
	90% CI (IRR)	[0.15 ; 0.84]		-
Night time accidents	Recorded accidents	19	28	47
	Man months	19.104	20.568	39.672
	Incidence rate – IR * 10^3	0.99	1.36	1.18
	Incidence rate ratio – IRR	0.73		-
	95% CI (IRR)	[0.45 ; 1.19]		-
	90% CI (IRR)	[;]		

Table 8: Incidence rate ratios for solo accidents reflecting the likely systematic underreporting of accidents in the treatment group.

All solo accidents				
Solo accidents	Incidence rates * 10^3		IRR	95% CI (IRR)
	Treatment group	Control group		
All	3.35	4.42	0.76	[0.55 ; 1.05]
Winter	4.40	5.54	0.79	[0.53 ; 1.19]
Summer	2.30	3.31	0.70	[0.41 ; 1.19]
Daylight	1.94	2.43	0.80	[0.52 ; 1.22]
Twilight	0.26	0.68	0.39	[0.14 ; 1.07]
Night time	1.10	1.26	0.87	[0.49 ; 1.55]
Solo accidents with personal injury				
Solo accidents	Incidence rates * 10^3		IRR	95% CI (IRR)
	Treatment group	Control group		
All	2.67	3.65	0.73	[0.51 ; 1.04]
Winter	3.25	4.38	0.74	[0.47 ; 1.17]
Summer	2.09	2.92	0.72	[0.41 ; 1.26]
Daylight	1.62	1.94	0.83	[0.52 ; 1.33]
Twilight	0.16	0.53	0.29	[0.09 ; 1.05]
Night time	0.89	1.12	0.80	[0.43 ; 1.49]

Table 9: Corrected Incidence rates, incidence rate ratios, 95% and 90% confidence intervals for incidence rate ratios – correction made in order to control for the apparent underreporting of bicycle accidents in the treatment group. All recorded bicycle accidents resulting in personal injury to members of the treatment and control group (November 1st 2004 – October 31st 2005).

Accident type	Data	Treatment group	Control group	Total
All Accidents	Recorded accidents	69	125	194
	Corrected number of accidents	69	91.5	160.5
	Man months	19.104	20.568	39.672
	$IR_{Corr} * 10^3$	3.61	4.45	4.05
	IRR_{Corr}	0.81		-
	95% CI (IRR_{Corr})	[0.61 ; 1.09]		-
	90% CI (IRR_{Corr})	[0.63 ; 1.05]		-
Winter period (November 1st 2004 – April 30th 2005)	Recorded accidents	38	70	108
	Corrected number of accidents	38	51.2	89.2
	Man months	9.552	10.284	19.836
	$IR_{Corr} * 10^3$	3.98	4.98	4.50
	IRR_{Corr}	0.80		-
	95% CI (IRR_{Corr})	[0.54 ; 1.18]		-
	90% CI (IRR_{Corr})	[0.57 ; 1.11]		-
Summer period (May 1st 2005 – October 31st 2005)	Recorded accidents	31	55	86
	Corrected number of accidents	31	40.3	71.3
	Man months	9.552	10.284	19.836
	$IR_{Corr} * 10^3$	3.25	3.91	3.59
	IRR_{Corr}	0.83		-
	95% CI (IRR_{Corr})	[0.53 ; 1.29]		-
	90% CI (IRR_{Corr})	[0.57 ; 1.20]		-
Multiparty accidents	Recorded accidents	18	50	68
	Corrected number of accidents	18	36,6	54.6
	Man months	19.104	20.568	39.672
	$IR_{Corr} * 10^3$	0.94	1.78	1.38
	IRR_{Corr}	0.53		-
	95% CI (IRR_{Corr})	[0.31 ; 0.91]		-
	90% CI (IRR_{Corr})	[0.34 ; 0.83]		-
Solo accidents	Recorded accidents	51	75	126
	Corrected number of accidents	51	54.9	105.9
	Man months	19.104	20.568	39.672
	$IR_{Corr} * 10^3$	2.67	2.67	2.67
	IRR_{Corr}	1.00		-
	95% CI (IRR_{Corr})	[0.70 ; 1.43]		-
	90% CI (IRR_{Corr})	[0.74 ; 1.35]		-
Daylight accidents	Recorded accidents	45	81	126
	Corrected number of accidents	45	59.3	104.3
	Man months	19.104	20.568	39.672
	$IR_{Corr} * 10^3$	2.36	2.88	2.63
	IRR_{Corr}	0.82		-
	95% CI (IRR_{Corr})	[0.57 ; 1.18]		-
	90% CI (IRR_{Corr})	[0.60 ; 1.11]		-
Twilight accidents	Recorded accidents	5	15	20
	Corrected number of accidents	5	11.0	16.0
	Man months	19.104	20.568	39.672
	$IR_{Corr} * 10^3$	0.26	0.53	0.40
	IRR_{Corr}	0.49		-
	95% CI (IRR_{Corr})	[0.18 ; 1.35]		-
	90% CI (IRR_{Corr})	[0.21 ; 1.15]		-
Night time accidents	Recorded accidents	19	28	47
	Corrected number of accidents	19	20.5	39.5

	Man months	19.104	20.568	39.672
	$IR_{Corr} * 10^3$	0.99	1.00	1.00
	IRR_{Corr}	1.00		-
	95% CI (IRR_{Corr})	[0.56 ; 1.79]		-
	90% CI (IRR_{Corr})	[0.61 ; 1.63]		-

*Table 10: Corrected incidence rates, incidence rate ratios and 95% confidence intervals for incidence rate ratios – multiparty accidents with personal injury only. Correction made in order to control for the apparent underreporting of bicycle accidents in the treatment group. * Significant at 10% level (90% confidence interval for IRR).*

Multiparty accidents with personal injury				
Multiparty accidents	Incidence rates * 10^3		IRR	95% CI (IRR)
	Treatment group	Control group – corrected		
All	0.94	1.78	0.53	[0.31 ; 0.91]
Winter	0.73	1.78	0.41	[0.18 ; 0.95]
Summer	1.15	1.78	0.65	[0.32 ; 1.31]
Daylight	0.73	1.46	0.50	[0.27 ; 0.92]
Twilight	0.10	0.14	0.74	[0.13 ; 4.01]
Night time	0.10	0.18	0.84	[0.11 ; 3.03]
Counterpart: truck/bus, van, car, MC, moped	0.42	0.82	0.51	[0.23 ; 1.14]*
Counterpart: cyclist, pedestrian	0.52	0.96	0.55	[0.30 ; 1.00]