Randomized trials and self-reported accidents as a method to study safety-enhancing measures for cyclists-two case studies

Lahrmann, Harry; Madsen, Tanja Kidholm Osmann; Olesen, Anne Vingaard

Published in:
Accident Analysis and Prevention

DOI (link to publication from Publisher):
10.1016/j.aap.2017.07.019

Publication date:
2018

Document Version
Publisher's PDF, also known as Version of record

Accident Analysis and Prevention 114 (2018) 17–24

Contents lists available at ScienceDirect

Accident Analysis and Prevention

journal homepage: www.elsevier.com/locate/aap

Randomized trials and self-reported accidents as a method to study safety-enhancing measures for cyclists—two case studies

Harry Lahrmann*, Tanja Kidholm Osmann Madsen, Anne Vingaard Olesen

Department of Civil Engineering, Aalborg University, Thomas Manns Vej 23, DK-9220 Aalborg OE, Denmark

A R T I C L E I N F O

Keywords:
Cyclist traffic safety
Randomised controlled trial
Self-reported accident
Permanent running lights
Yellow bike jacket

A B S T R A C T

A large number of studies show that high visibility in traffic is important in the struggle of getting the attention from other road users and thus an important safety factor. Cyclists have a much higher risk of being killed or injured in a traffic accident than car drivers so for them high visibility is particularly important. A number of studies have examined the effect of high visibility, such as reflective clothing, but most studies have been primitive, the data limited and the results very uncertain.

In this paper we describe the safety impact of increased visibility of cyclists through two randomised controlled trials: permanent running lights on bicycles and a yellow bicycle jacket, respectively.

The effect of running lights was studied through a trial where the lights were mounted to 1,845 bicycles and 2,000 others comprised a control group. The bicycle accidents were recorded every two month in a year through self-reporting on the Internet. Participants were asked to report all cycling accidents independently of severity to avoid differences between participants as regards to which accidents were reported. They reported a total of 255 accidents i.e. 7 accidents per 100 cyclists. The results showed that the incidence rate for multiparty bicycle accidents with personal injury was 47% lower for cyclists with permanent running light. The difference is statistically significant at the 5% level.

The effect of a yellow bicycle jacket was examined through a trial with 6,800 volunteer cyclists. The half of the group received a bicycle jacket and the other half comprised a control group. Both groups reported every month all their bicycle accidents independently of severity on the Internet. They reported a total of 694 accidents i.e. 10 accidents per 100 cyclists. The treatment group was asked each month if they carried the jacket on their last cycling trip. The results showed that on a random day the treatment group carried the jacket or other fluorescent cycling garment on 77% of their cycle trips. The incidence rate for multiparty accidents with personal injury was 38% lower than the control group. The difference is statistically significant at the 5% level.

The trials were not blind and it seems that the lack of blinding has influenced the level of the groups accident reporting. To address this bias we used a correction factor formed by the difference in the number of single accidents of the two groups.

The experiences with self-reporting of accidents via a web based questionnaire sent by e-mail with one respective two month intervals were very good; in both trials more than 80% answered all questionnaires whereas less than 2% did not answer, and the quality of the self-reported accident was considered high.

1. Introduction

Cycling is healthy, and a large Danish population study has shown that the mortality rate is 28% lower for cyclists compared to the part of the population using passive transport (Andersen et al., 2000). Furthermore, cycling provide a substantial contribution to reduce congestion in cities. Cycling is therefore a central instrument in many plans for a sustainable transport system in cities; see e.g. the EU white paper on transport (European Commission, 2011) and the Danish national bicycle strategy (Ministry of Transportation 2014). On the other hand, cyclists are also an exposed road user group. In 2010, nearly 2,000 cyclists were killed in traffic, corresponding to 7% of all traffic fatalities in the EU (Candappa et al., 2012). The risk of being killed or injured in a traffic accident is significantly higher for cyclists than for car drivers (Hansen and Jensen, 2012), and the risk is actually far greater than reflected by the official accident statistics. In 2014, 830 personal injuries involving cyclists were reported to the official Danish accident statistics, but if we also counts the numbers from the emergency rooms

* Corresponding author.
E-mail addresses: hsl@civil.aau.dk (H. Lahrmann), tkom@civil.aau.dk (T.K.O. Madsen), avo@civil.aau.dk (A.V. Olesen).

http://dx.doi.org/10.1016/j.aap.2017.07.019
Received 17 August 2016; Received in revised form 21 June 2017; Accepted 16 July 2017
Available online 01 August 2017

and hospitals 16,481 cyclists were injured in Denmark in 2014. Of these numbers, 4,611 of the injured parties were attributed to multi-party accidents (Statistics Denmark, 2014). This means that the official accident statistics registers only 5% of bicycle accidents treated in Danish hospitals. However, not even the figure from emergency rooms and hospitals intercepts all bicycle accidents. Some injuries are treated by general practitioners only and finally there are also self-treated injuries. Several studies have tried to quantify the total volume of cycle accidents through self-reporting from a group of cyclists in a period of time. A Belgian study had a sample of 1,087 regular cyclists. Over a year, 62 of them were involved in 70 bicycle accidents. Police, hospital/emergency room or insurance companies were involved in only 7%, 10% and 30% of the cases, respectively (Geus et al., 2012). An Australian study examined self-reported accident data from 2,038 cyclists. During 25,971 days of cycling, 198 crashes were reported. Of those, 101 resulted in an injury which was either self-treated (85), was treated by a general practitioner (12) or in a hospital without an overnight stay (4). There were no crashes with a hospital overnight stay. Seven crashes were reported to the police. (Poulos et al., 2015). A British study collected data on cycling accident-related injury in the last five years from 4,961 cyclists using an online questionnaire. The cyclists were recruited from large British cycling organisation networks. 54% of the sample reported a cycling injury accident. (Hollingworth et al., 2015). Thus, it is important to investigate how the number of bicycle accidents can be reduced.

In the early 1990s it was made mandatory for car users to use daytime running lights in some countries. 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 daytime multi-party 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. It is therefore reasonable to assume that permanent running lights on bicycles will also reduce the number of cycling accidents.

A 2004 *meta*-analysis did not find any studies directly measuring the safety effect of increased visibility, but it reports of 42 projects that studied the effect of visibility aids. The analysis concludes that visibility aids have the potential of making the motorists aware of the cyclists sooner (Kwan and Mapstone, 2004). In a New Zealand study, 2,500 cyclists were asked about their bicycle accidents for the past 12 months, and the study showed that the number of self-reported accidents was lower among cyclists who stated that they always wore garments in fluorescent colours (Thornley et al., 2008). In an Australian study, 185 cyclists involved in accidents were interviewed, and only two of them stated their own lack of visibility as a factor in the accident while 61% stated driver inattention as a factor (Lacherez et al., 2013). The study concluded that cyclists involved in accidents underestimate the importance of their own visibility. Another Australian study shows that cyclists overestimate their own visibility at night (Wood et al., 2013). A number of the vehicle-bicycle accidents are characterized as "looked-but-failed-to-see" accidents where the motorist did not acknowledge the presence of the cyclist in time, even though the motorist explains that he actually did look to the side from where the cyclist came. The assumption is that the number of these situations can be reduced by increasing the cyclists' visibility; a visibility that can be important to whether or not the situation results in an accident (Herslund and Jørgensen, 2003). This is supported by a Finnish in-depth study of vehicle-bicycle accidents concluding that motorists notice the cyclist too late in accidents (Räsänen and Summala, 1998).

The evidence on the use of visibility aids for cyclists is thus dominated by two directions. Firstly, a large *meta*-analysis that shows no effect. Secondly, several "into the substance" studies which suggest that cyclists could benefit from more visibility, for example in the form of running lights and fluorescent garments − and more awareness.

In this paper we will report the results from two Danish studies with the goal to improve cyclists' visibility in order to investigate the safety effect of different types of visibility measures. In one study it was tested whether permanent running lights on bicycles improve cyclist safety (Madsen et al., 2013). In the second study, the safety effect of a yellow bicycle jacket was studied (Lahrmann et al., 2015). Finally, we will discuss how the results can be used in the future work with cyclist safety.

2. Methodology

2.1. Randomised controlled trial

Many road safety evaluation studies are carried out as observational before–after studies. This is generally also the case for earlier studies of the safety effects of daytime running lights for motor vehicles. 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. By comparison, randomised controlled trials (RCT) 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 both studies. The basic concept of a RCT is to create two groups; one group that receives treatment (i.e. the treatment group) and one group that does not receive treatment (i.e. the 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 with the control group. In order to obtain the desired control for confounding factors, the trial units must however be allocated to the treatment and the control group randomly; i.e. through randomization (Rothman et al., 2008).

2.2. Self-reporting of accidents

In the two studies the outcome of interest is cycling accidents for both the treatment and the control group. Ideally, the police should record all cycling accidents, but as described above the police only record few of the bicycle accidents and it would have required both a very large number of participants and a long trial period if police recorded accident should have been used. Therefore it was decided to use self-reporting of accidents in the studies. The question is however if we can trust on self-reported accident. Oblivion and/or memory loss may influence the correctness of recall (Lajunen and Öakan, 2011) as well as social desirability effects (Wåhlberg et al., 2010; Wåhlberg, 2010). Self-reports are also suspected to suffer from statistical bias due to under-reporting by those with many crashes and possible over-reporting by some subgroups (Tivesten and Wiberg, 2013; Wåhlberg 2009). The agreement between self-reported accident data and other data sources are sometimes low, which has been mentioned as a problem with the self-reported data (Wåhlberg, 2009). However, low agreement with other data sources does not in itself diminish the validity of self-reported data; it depends on the validity of the data source to which the self-reported data are compared. Other studies find high level of accuracy in drivers self-report and police recorded crashes (Boufous et al., 2010).

2.3. Trial setup

Both trials lasted for one year. In the *permanent running lights trial* (PRL), the light was mounted on the bicycles of the treatment group before start and the control group was promised to get the light after the trial had finished. In the *yellow bicycle jacket trial* (YBJ), the treatment group got the bicycle jacket before start and promised to use the jacket every time they cycled during the year whereas the control group received the jacket after the year. In both projects each of the participants received a personal e-mail at regular intervals asking if they had been involved in an accident as cyclists during the time since they were asked last time. In the PRL trial the period was two month, in the YBJ trial one month. From the e-mail the participants was directed to a web-based survey in which they were asked to report any bicycle accidents they had since the last survey independently of the severity of the accident. The web-survey was designed as a questionnaire and the first question was: *Have you been involved in an accident on a bicycle since the previous questionnaire?* All who answered ‘no’ to the question should answer only this one question. Respondents who answered ‘yes’ were directed to a questionnaire developed based on the Danish police’ accident recording scheme, thus obtaining the same key information from each self-reported accident as is normally obtained from the police recordings. The recorded information 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. In the YBJ trial the participants were also asked to locate the accident and give a prosaic description of the accident. Participants who had not replied within 14 days received a reminder by e-mail. To increase the response rate, a monthly/bimonthly lottery with small prizes (approx. 50 €) was made among the participants who had answered the questionnaire.

In order to investigate if the safety effect of the two treatments varied over the year, it was decided that both the treatment and the control group should report their bicycle accidents through a whole year.

The permanent running lights were fixed on the bicycle and always turned on when the bicycle was in motion. With the yellow bicycle jacket, we could not be sure that the participants in the treatment group wore the bicycle jacket when riding the bicycle. Therefore, on a random day each month we sent the treatment group another web-based questionnaire in which they were asked whether they had worn the bicycle jacket the last time they rode their bicycle.

2.4. Recruiting project participants

In both trials the participants inherently had to be volunteers, which means that the participants are likely to believe in the efficacy of the two measures. This provides a number of advantages for the trials, but also a number of disadvantages. The advantages and disadvantages will be discussed later in this paper. Based upon the experiences from an earlier bike-and-bus promoting project that had also used self-reporting of bicycle accidents (Lohmann-Hansen et al., 2001), it was estimated that total group should consist of approximately 4,000 participants in the PRL trial and 8,000 in the YBJ trial in order to obtain valid estimates of the safety effects of the treatment.

Participants were recruited through advertising, press coverage in the local media and social media where volunteers were invited to sign up through a webpage. After a selection process, a total of 3,845 cyclists entered the PRL trial with 1,845 in the treatment group and 2,000 in the control group. In the YBL trial, a total of 6,793 participants started, with 3,402 in treatment group and 3,391 in the control group. Upon sign up, the participants gave their informed consent to use the collected data for research purposes and use them in combination with other registers. In Madsen et al. (2013) and Lahrmann et al. (2015) the recruitment and selection is described in detail.

3. Results

3.1. Participants

In the PRL trial, 54% of the participants were men and 46% women. The average age was 31 years, about ¾ used their bicycle daily and about ¾ had access to a car.

In the YBJ trial, 57% of the participants were men and 43% women. They were on average 46 years old, they used their bicycle almost every day both summer and winter and their typical destination was work/education. Although they frequently rode their bicycle, 80% of the households had at least one car.

3.2. Participants' involvement in projects

Both trials had a very high response rate of the accidents questionnaires. In the PRL trial 86% answered all accidents questionnaires. In the YBJ trial 80% answered all accidents questionnaires and only 2% did not respond to any of the questionnaires.

In the YBJ trial, on average 90% answered the monthly questionnaires about their use of the jacket or other fluorescent cycling garment. The average usage rate over the year was 77%, but with great variations during the 12 months of the project. The highest usage rate was in the first month of the project − November − and the lowest was in July.

3.3. Accident data

In PRL trial 255 bicycle accidents were reported by the participants (7 accidents per 100 cyclists on the average); 98 accidents (5 per 100 cyclists) by the treatment group and 157 accidents (8 per 100 cyclists) by the control group. In the YBJ trial 694 accidents were reported (10 accidents per 100 cyclists on the average); 274 accidents (8 per 100 cyclists) by the treatment group and 420 (12 per 100 riders) by the control group. Based on the participants’ accident descriptions it was assessed whether the accidents were single accidents or multi-party accidents. Accidents with a counterpart directly or indirectly involved in the emergence of the accident were classified as multi-party accidents, while accidents with no other road users involved were classified as single accidents. Furthermore, the severity of the accidents has been assessed from the questionnaires. Tables 1 and 2 show the characteristics for the participants’ reported accidents for all accidents and personal injury accidents only, respectively. The column “Personal injury” only includes the more severe personal injuries where the injury consists of more than just bruises.

3.4. Analysis of accident data

The effect of each of the two measures is evaluated by comparing the incidence rates between the treatment and control groups and between the different sub-groupings of the accident data. The incidence rate is estimated based on the number of reported accidents and is for a given accident type/grouping stated by:

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

X_g is the number of reported accidents for participants belonging to group g.

$t_{g,i}$ is the number of months where the individual participants has been active in group g.

The incidence rate describes the number of accidents in group g per month.

The effect of the two measures is given with the incidence rate ratio, which is defined as the relation between the incidence rate for the treatment group and the incidence rate for the control group:

$$\mathrm{IRR_j} = \mathrm{IR_{j,T}} / \mathrm{IR_{j,C}}$$

Regarding the assessment of whether or not the bicycle jacket has any significant safety effect, the 95% confidence interval for the incidence rate ratio is estimated.

Table 1
Accident characteristics for the permanent running lights trial.

Accident characteristics	Treatment group		Control group	
	All accidents	Personal injury accidents	All accidents	Personal injury accidents
Accidents in total	98	69	157	125
Type				
Single accident	64	51	91	75
Multi-party accident	34	18	66	50
Season				
Winter	60	38	87	70
Summer	38	31	70	55
Lighting conditions				
Daylight	57	45	101	81
Twilight	13	5	24	15
Dark	27	19	31	28
Contact with police, emergency room and insurance				
Accidents reported by police	1	–	4	4
Accidents reported to insurance companies	10	9	18	17
Treatment at hospital/ emergency rooms	10	10	23	23
Treatment by general practitioner only	1	1	7	7

Looking at figures in Tables 1 and 2 it is noted that the control group in both trials has experienced significantly more single accidents than the treatment group ($p < 0.05$). This is surprising since the higher visibility measurements could hardly have had any influence on the number of single accidents. An explanation to the fewer single accidents in the treatment group could be that the two trials was not blind and that the participants were volunteers who believe in the effect of the jacket and the PRL and thus have been affected by their belief to report accidents in such a manner that the treatment group reported fewer accidents than they objectively should have while the control group most likely has reported a bit more. Thus, the number of multi-party accidents in the control group in both trials was reduced using a correction factor equal to the apparent effect of the bicycle jacket/PRL on single accidents.

Table 3
Permanent running lights trial: Corrected incidence rate, incidence rate ratios and 95% confidence intervals for incidence rate ratios − multi-party accidents with personal injury more severe than bruises. Correction made in order to control for the apparent underreporting of bicycle accidents in the treatment group.

Accident type	Incidence rates * 10^3		IRR	95% CI (IRR)
	Treatment group	Control group (adjusted)		
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]

3.5. Safety effect of the two measures

Table 3 shows the safety effect of the permanent running lights based on the registered multi-party accidents in the PRL trial and with adjustment for the presumed underreporting in the treatment group. Effects for a number of sub-groups for the reported accidents have been stated. The registered personal injury accidents were chosen as the basis

Table 2
Accident characteristics for the yellow bicycle jacket trial.

Accident characteristics	Treatment group		Control group	
	All accidents	Personal injury accidents	All accidents	Personal injury accidents
Accidents in total	274	123	420	179
Type				
Single accident	150	80	199	96
Multi-party accident	124	43	221	83
Season				
Winter	174	66	257	101
Summer	100	57	163	78
Lighting conditions				
Daylight	169	77	273	122
Twilight	41	15	63	21
Dark	63	30	84	36
Contact with police, emergency room and insurance				
Accidents reported by police	8	7	15	8
Accidents reported to insurance companies	34	26	62	41
Treatment at emergency room/hospital	45	38	54	46
Treatment only by own doctor/doctor from the emergency service	7	3	14	9
Usage rate of bicycle jacket				
Low use of bicycle jacket	141	63	–	–
High use of bicycle jacket	133	60	–	–
No bicycle jacket	–	–	420	179
Usage of bicycle jacket when in accident				
Wore bicycle jacket or some fluorescent garment	209	87	–	–
Did not wear the bicycle jacket	65	36	–	–

Table 4
Yellow bicycle jacket trial: Corrected incidence rates, incidence rate ratios and 95% confidence intervals for incidence rate ratios − multi-party accidents with personal injury more severe than bruises. Correction made in order to control for the apparent underreporting of bicycle accidents in the treatment group.

Accident type	Incidence rates * 10^3		IRR	95% CI (IRR)
	Treatment group	Control group (adjusted)		
All	1.15	1.84	0.62	[0.39; 1.00]
Winter	0.89	1.73	0.52	[0.27; 0.98]
Summer	1.40	1.96	0.72	[0.41; 1.26]
Daylight	0.85	1.49	0.57	[0.34; 0.96]
Twilight	0.13	0.13	1.00	[0.29; 3.40]
Night time	0.13	0.22	0.60	[0.20; 1.83]
Counterpart: truck/bus, van, car, MC, moped	0.53	1.02	0.52	[0.29; 0.95]
Counterpart: cyclist, pedestrian	0.61	0.82	0.75	[0.41; 1.36]
Low jacket use	1.45	1.84	0.79	[0.46; 1.34]
High jacket use	0.86	1.84	0.47	[0.26; 0.86]

of the analysis because this ensures the most uniform accident definition for both the treatment group and the control group.

It should be noted that the IRR for all subgroups is less than 1. This means that permanent running lights on bicycles have a positive safety impact on all of these subgroups. Furthermore, it is noted that the effects are larger in winter than in summer and during the day compared to night. This suggests that the bicycle running lights especially improves the visibility of cyclists in daylight. This is consistent with the fact that cyclists, as is the case for the control group, usually do not use conventional bicycle lights during daytime. Besides, far from all cyclists use conventional bicycle lights in twilight periods, which may explain the higher effect in twilight compared to during night time. It is also noted that the confidence intervals for some subgroups include the value 1.00, which means that those effects are not statistically significant ($p < 0.05$).

Table 4 shows the safety effect of the bicycle jacket based on the registered multi-party accidents in the YBJ trial after adjustment for the presumed underreporting in the treatment group. The effects for a number of sub-groups for the reported multi-party accidents have also been stated. Note that unlike Table 3 the figures in Table 4 are calculated only based on multi-party accidents. Thus, the incidence rates are considerable lower than in Table 3. Similar to the PRL trial, the registered personal injury accidents were chosen as the basis of the analysis in the YBJ trial.

It should be noted that all IRR, except for accidents that occurred in twilight, are less than 1.0. This implicates that there is a positive effect for all sub-groups. However, it should also be noted that some confidence intervals are higher than 1.0, which means that these effects are not statistically significant.

A main result from Table 4 is that there were 38% fewer multi-party personal injury accidents in the treatment group compared to the control group, and that the difference is statistically significant ($p < 0.05$).

The difference between the groups are greater during winter than during summer and greater in daylight than in dark hours, which indicates that the greatest safety effects are connected to using the bicycle jacket in the daytime and in the winter period; a difference that could be connected with the fact that the daylight is often weak in daytime hours during winter, that many cyclists do not have permanent running lights and that the jacket usage rate during winter was higher than in the summer.

It was also studied whether the safety effect of the bicycle jacket is highest for the half of the treatment group that frequently used the jacket, compared to the half of the treatment group that used the jacket less frequently. In Table 4 it is noted that the group with high jacket usage had 53% fewer accidents than the control group compared to only 21% fewer accidents in the group with low jacket usage, where only the prior is statistically significant. Hence, the study shows − not surprisingly − that the safety effect of the bicycle jacket varies with the usage.

4. Discussion

4.1. Study design

According to our knowledge, these are the first two trials, which have used the gold standard for effect studies − the RCT concept − to elucidate the safety effect by increasing the cyclist's visibility. Other studies have mainly elucidated how increased visibility can improve distance and probability of detection. Still, others have simply through questionnaires combined use of visibility aids and the number of self-reported accidents. (Kwan and Mapstone, 2004; Thornley et al., 2008)

For obvious reasons we could not make the two trials double-blind RCT's and it seems that the lack of blinding has influenced the level of the accident reporting in the two groups. We have addressed this bias by using a correction factor formed by the difference in the number of single accidents in the two groups. This correction means that there is reason to believe that the results are not compromised crucially of the lack blinding. Likewise, the results are so convincing that even if part of the effect can be attributed to the lack of blinding, there will still be a positive effect left which can be assumed to be a result of the increased visibility.

Generally, there has not been a tradition of using RCT in effect studies of traffic safety measures. With-without studies are often carried out on measures where the decision on measures/no measures in no way have been random. Although measures are grouped by a number of variables, which are assumed to have an impact on road safety, there is no guarantee that all variables with impact on the number of accidents are captured. Thus, the results of with-without studies can be compromised by high uncertainty. If one will avoid this, the only way is the RCT concept. We hope that these two studies can inspire others to do more RCT's in effect studies on road safety.

4.2. Participants

In the ideal RCT a randomly selected group is drawn from an entire population and this group provides an offer to participate, if they met the trial conditions. Afterwards a dropout analysis is done to check for any imbalances in the group participating in the trial compared to the entire population. In this trial we did not have this opportunity, but only the second best option, i.e. to advertise for volunteers. This was on the other hand an advantage, because since the participants actively signed up for the trial they were very interested in the project and continued participating actively in the project until it finished. As shown in 3.2 the average answer ratio of the questionnaires was in both trials very high and much higher than normally seen in such studies. The risk from of a large involvement in a randomised trial, that is not conducted blindly, is that the two groups do not report in a similar manner – the participants believe in the effect of the initiative which influences their reports. It is also obvious that since the participants are volunteers, they cannot be expected to be representatives of Danish cyclists neither in attitude or behaviour.

4.3. *Self-reported accidents as effect goals*

The experiences with self-reporting of accidents via a web-based questionnaire sent by e-mail with one respective two month's intervals were very good; we received both high answer ratios and high quality of answers in terms of very specific accident descriptions in both trials, and by asking for accidents frequently, there is higher probability that the participants don't forget any accidents. In this way we eliminate the criticism that has often been against self-reported accident, i.e. that participants forget accidents.

The quality of the answers can be described from this story: In the YBJ trial – the second trial – we asked the participants to locate their accident on a map. This was done by asking the participants to go on www.maps.google.com, zoom in on the location of the accident on the map, mark the accident location and copy the coordinates from the accident location into the questionnaire. If the respondent could not use this location method, they could describe in words where the accident had taken place. After this, a project employee called the participants and together they could find the coordinates to the accident. The result was surprisingly good; all reported accidents were located and less than 50 of the people being involved in an accident needed help to locate the accident.

From these experiences, it cannot be concluded that the self-reporting of accidents will always lead to good results, but the two projects show that there seems to be a potential for using self-reporting in the accident preventing work. In particular, the two projects show that the concept of an e-mail every month gives very high response rates. At the same time the answers show that the participants were very careful with the answers. The vast majority of the reported accidents had long prose descriptions to supplement the structured questions in the questionnaire. This shows that the participants were very interested in providing details about their accident. We do not think this interest is limited to our participants, but believe that this is a general phenomenon: road users that have been involved in traffic accidents want to share information about their accident if they believe it can make a difference. Therefore it seems to be an obvious opportunity to use self-reports to get details about the many accidents involving cyclists that we know nothing about, because of the large dark figure in the official accident statistics. However, it must be added that the two projects do not clarify how large a bias will occur when only one part describes the accidents and as such may have a tendency to embellish their own behaviour prior to the accident (Fig. 1).

4.4. *Safety effect of improving cyclists' visibility*

4.4.1. *Results*

The results of the two trials showed 38% and 48% fewer multi-party personal injury accidents, respectively. These positive effects are noteworthy high. There are good reasons to believe that part of the explanation is that the safety improvements offered by permanent bicycle running lights and a yellow bicycle jacket is obtained because cyclists are usually much less visible than other road users.

However, the magnitude of the found effects is likely to reflect a weakness in the trial set up. The members of both the treatment and the control group in both trials were selected by randomization from a total pool of volunteers. As they have signed up voluntarily, self-selection is a possible source of error in the trial. 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 two groups than for the population as a whole. In terms of evaluating the safety effects of two measures, self-selection; and hence lower than average accident risk, 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.

As discussed in Section 3.4 it is likely that the apparent effect on single accidents in both trials actually reflect a systematic under-reporting of accidents in the treatment group due to an inherent bias in favour of the measure amongst the members of the treatment group. 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; this is reflected by the apparent under-reporting of single accidents in the treatment group. A hypothesis could be that the reporting of single accidents in the treatment groups would be lower in the group of accidents reported to the police and insurance companies as well as accidents requiring medical help are. However, the number of accidents is too small for such an analysis, see Tables 1 and 2. Although the differences in the number of accidents reported to police/insurance company and/or requiring medical care support the conclusion that the lower accident rates for the treatment group in both trials is not only a result of systematic under-reporting of accidents; they do in fact reflect a positive safety effect of permanent running lights and yellow bicycle jackets.

As the average jacket usage was only 77% in the YBJ trial, the risk reduction of the individual cyclist who wears the jacket at all times is even greater. This is also emphasised by the fact that the effect was largest during the winter when the usage rate was on its highest and largest among those who stated a large jacket usage in the questionnaire.

The mileage driven can affect the number of accidents. In general, the higher the mileage, the higher number of accidents. A difference in

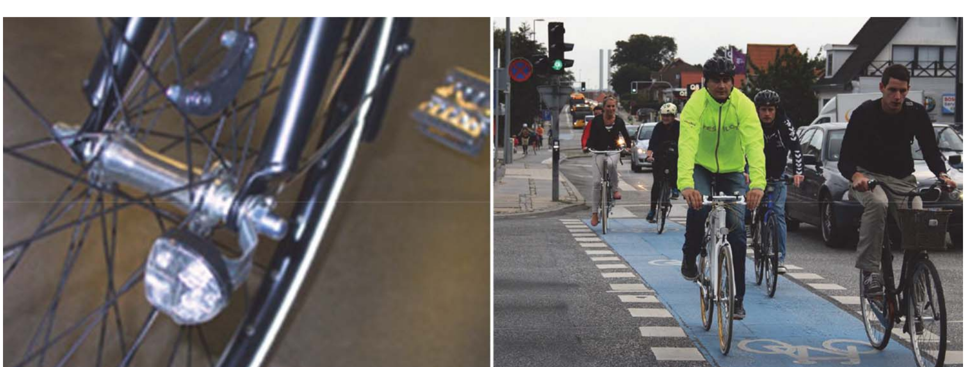

Fig. 1. Permanent running lights (left) and the yellow bicycle jacket (right). (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.).

the exposure between the control and treatment groups can thus result in a bias. However, due to the RCT design, the participants in the two groups are per definition similar which reduces the influence of a possible difference in mileage driven on the safety effect.

4.4.2. Reliability and validity of results

The two trials were conducted as RCT's with nearly 4,000 participants in the PRL trial and nearly 7,000 in the YBJ trial. Furthermore, as previously described there was a very high involvement from the participants. Therefore, the reliability of the trials is assessed to be very high.

The internal validity of the trials is affected by the fact that the studies are not blind. As discussed previously the participants seems to be influenced by a wish of a positive outcome. This bias, however, were compensated for by scaling down the effect via the ratio between the numbers of single accidents in the treatment groups and the numbers of single accidents in the control groups.

What is the impact on the external validity of the fact that the two groups are not representative of cyclists in Denmark? The groups consisted of volunteer cyclists. We therefore assume that they are likely to be more safety-conscious than the average cyclist in Denmark. But how will it affect the effect of the two safety measures? It is likely that the effect of the permanent running light or the yellow bicycle jacket is different for a more risk-seeking cyclist than our participants. A risk-willing cyclist will presumably be involved in more situations which result in accidents compared to the safety-conscious cyclists, because the latter may tend to give way to a counterpart to a higher degree than the risk-willing cyclist, even in cases where the counterpart has to give way. As an effect of the visibility measure, it is likely that the average cyclist will experience a higher reduction in the number of accidents than found in the two trials.

The external validity of the trial in relation to the individual cyclist risk is challenged by the fact that it must be assumed that the effect will change if the environment changes − for example, it must be assumed that the effect will drop if more and more cyclists start using a permanent running light and a yellow bicycle jacket. From the individual cyclist's perspective his safety depends on by which certainty other road users recognize him. This depends on how visible he is in comparison with other road users and the environment. For the individual cyclist using a yellow bicycle jacket or permanent running lights, this phenomenon implicates that his risk would increase if all other cyclists also use this equipment. But the total number of accidents involving cyclists will still decrease if everyone uses these measures; the effect is just smaller than for the first cyclist who takes on these visibility tools. In conclusion, we can say that the benefit of visibility aids is about the struggle for other road users' awareness in traffic. If cyclists want to improve their own safety, they must play their part of the game exactly as motorists did when they got daytime running lights.

The external validity is also influenced by the fact that it is assumed that other road users' risk will increase when attention is directed to cyclists, including other cyclists with low visibility. The above mentioned considerations are not specific to these two safety measures but apply generally if visibilities of elements in road traffic image are changed. Overall, the assessment of the external validity is that the effect on the individual cyclists' safety will certainly decrease as the number of users of the two visibility aids increase, but not to a degree that it may compromise the overall effect.

4.5. Policy recommendation

Both trials clearly document the fact that higher visibility of cyclists would significantly reduce the number of personal injury accidents with cyclists. The study has as such shown that there are large and cost-efficient safety improvements connected to initiatives that can increase the cyclists' visibility in traffic. Therefore it could be recommended to prioritize such initiatives in the traffic safety work.

When permanent running light on bicycles has such a positive effect on bicycle accidents, it is reasonable to suggest other types of attention-increasing equipment on the bicycle. Mopeds, for instance, have both turn signals and brake lights − so why not develop such systems for bicycles? With modern LED light technology it should be possible. It is however probably harder to get cyclists to voluntarily select cycling clothing with high visibility. But the clothing companies could be encouraged to design cycling clothing that is both smart and has a high visibility. Another way to go would be to develop wearable safety equipment that is easy to put on and off − perhaps it could even be mounted on the bicycle when not in use.

Experience from the implementation of the two projects in Denmark, however, also shows that initiatives that strengthen the rules for bicycle equipment and is focusing on cyclists' use of safety equipment meets strong resistance from cyclist NGO's. The argument is that if the authorities make more rules regarding the use of the bicycle, and if the focus is on the many bicycle accidents and how these can be prevented − for example by high visibility − it will provoke cyclists to choose the car instead and this will counteract the overall goal of a higher proportion bicycle rides for the benefit of public health and sustainability of transport. The authors of this is paper do not agree with these arguments. We do not find it reasonable to downgrade cyclist safety for better health and sustainability. It should be possible to focus both on getting more people to choose the bicycle as well as to improve their safety.

References

Andersen, L.B., Schnohr, P., Schroll, M., Hein, H.O., 2000. All-cause mortality associated with physical activity during leisure time, work, sports, and cycling to work. Arch. Intern. Med. 160.

Boufous, S., Ivers, R., Senserrick, T., Stevenson, M., Norton, R., Williamson, A., 2010. Accuracy of self-report of on-road crashes and traffic offences in a cohort of young drivers: the DRIVE study. Inj. Prev. 16 (4), 275–277.

Candappa N. et al. (2012). Basic fact sheet "cyclists", deliverable D3.9 of the EC FP7 project aCoTa.

Elvik, R., Høye, A., Vaa, T., Sørensen, M., 2009. Handbook of Road Safety Measures, second ed. Emerald Group Publishing Limited.

Elvik, R., 1993. The effects on accidents of compulsory use of daytime running lights for cars in Norway. Accid. Anal. Prev. 25, 383–398.

Elvik, R., 1996. A meta-analysis of studies concerning the safety effects of daytime running lights on cars. Accid. Anal. Prev. 28, 685–694.

European Commission (2011) WHITE PAPER Roadmap to a Single European Transport Area – Towards a competitive and resource efficient transport system.

Geus, B., Vandenbulcke, G., Panis, L.I., Thomas, I., Degraeuwe, B., Cumps, E., Aertsens, J., Torfs, R., Meeusen, R., 2012. A prospective cohort study on minor accidents involving commuter cyclists in Belgium. Accid. Anal. Prev. 45, 683–693.

Hansen, A.S., Jensen, C., 2012. Risiko itrafikken 2007–2010 [Risk in traffic 2007–2010]. DTU Transp (In Danish).

Hauer, E., 1997. Observational before—after studies in road safety—estimating the effect of highway and traffic engineering measures on road safety. Pergamon.

Herslund, M., Jørgensen, N.O., 2003. Looked-but-failed-to-see-errors in traffic. Accid. Anal. Prev. 35 (6), 885–891.

Hollingworth, M.A., Harper, A.J.L., Hamer, M., 2015. Risk factors for cycling accident related injury: the UK cycling for health survey. J. Transp. Health 2, 189–194.

Kwan, I., Mapstone, J., 2004. Visibility aids for pedestrians and cyclists: a systematic review of randomised controlled trials. Accid. Anal. Prev. 36 (3), 305–312.

Lacherez, P., Wood, J.M., Marszalek, R.P., King, M.J., 2013. Visibility-related characteristics of crashes involving bicyclists and motor vehicles − responses from an online questionnaire study. Transp. Res. Part F: Traffic Psychol. Behav. 20, 52–58.

Lahrmann, H., Madsen, T.K.O., Olesen, A.V., Madsen, J.C.O., 2015. The safety impact of a yellow bicycle jacket. Submitted J.

Lajunen, T., Öakan, T., 2011. Self-Report Instruments and Methods. Handbook of Traffic Psychology. Elsevier Inc.pp. 43–59.

Lohmann-Hansen, A., Lahrmann, H., Madsen, J.C.O., 2001. CykelBus'terprojektet I Århus: Fra bil til cykel eller bus med positive virkemidler −Projektevaluering. Transportrådet (In Danish).

Madsen, J.C.O., Andersen, T., Lahrmann, H.S., 2013. Safety effects of permanent running lights for bicycles: a controlled experiment. Accid. Anal. Prev. 50, 820–829.

Poulos, R.G., Hatfield, J., Rissel, C., FlackLK, Murphy S, Grzebieta, R., McIntosh, A.S., 2015. An exposure based study of crash and injury rates in a cohort of transport and recreational cyclists in New South Wales, Australia. Accid. Anal. Prev. 78, 29–38.

Räsänen, M., Summala, H., 1998. Attention and expectation problems in bicycle-car collisions: an in-depth study. Accid. Anal. Prev. 30 (5), 657–666.

Rothman, K.J., Greenland, S., Lash, T.L., 2008. Modern Epidemiology. Wolters Kluwer Health. Williams and Wilkins, Lippincott.

Statistics Denmark (2014). Injured in road traffic accidents reported by the police,

casualty wards and hospitals by reporter, accident situation, transport unit, sex, age and type of injury. Retrieved from http://statbank.dk/moerke.
Thornley, S.J., Woodward, A., Langley, J.D., Ameratunga, S.N., Rodgers, A., 2008. Conspicuity and bicycle crashes: preliminay findings of the taupo bicycle study. Inj. Prev. 14 (1), 11–18.
Tivesten, E., Wiberg, H., 2013. What can the drivers' own description from combined sources provide in an analysis of driver distraction and low vigilance in accident situations? Accid. Anal. Prev. 52, 51–63.
Wåhlberg, A.E.A., Dorn, L., Kline, T., 2010. The effect of social desirability on self reported and recorded road traffic accidents. Transp. Res. Part F: Traffic Psychol. Behav. 13 (2), 106–114.
Wåhlberg, A.E.A., 2009. Driver behaviour and accident research methodology: unresolved problems. (Human factors in road and rail transport). Farnham, England. Ashgate.
Wåhlberg, A.E.A., 2010. Social desirability effects in driver behavior inventories. J. Saf. Res. 41, 99–106.
Wood, J.M., Tyrrel, R.A., Marszalek, R., Lacherez, P., Carberry, T., 2013. Bicyclists overestimate their own night-time conspicuity and underestimate the benefits of retroreflective markers on moveable joints. Accid. Anal. Prev. 55, 48–53.