The impact of driver monitoring with vehicle data recorders on accident occurrence

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R-97-8

Funded by:

VERBOND VAN VERZEKERAARS

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Methodology and results of a field trial in Belgium and The Netherlands

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Leidschendam, 1997
SWOV Institute for Road Safety Research, The Netherlands

Report documentation

Number:	R-97-8
Title:	The impact of driver monitoring with vehicle data recorders on accident occurrence
Subtitle:	Methodology and results of a field trial in Belgium and The Netherlands
Author(s):	P.I.J. Wouters & J.M.J. Bos
Research manager:	F.C.M. Wegman
Project number SWOV:	74.124
Client:	This research was funded by the Association of Dutch Insurers.
Keywords:	Behaviour, attitude (psychol), driver, safety, accident rate, data acquisition, modification, vehicle, test method, severity (accid, injury), analysis (math), statistics.
Contents of the project:	It is well established that the observation of a person's behaviour is likely to affect that behaviour. Thus, monitoring driving behaviour offers a possibility for influencing it, particularly when the driver is confronted with his own behaviour. The objective of the present study is to investigate the opportunities for improving traffic safety by confronting the driver with his monitored and recorded behaviour, making use of in-car electronic recording devices.
Number of pages:	56 pp. + 8 pp.
Price:	Dfl. 25,-
Published by:	SWOV, Leidschendam, 1997

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Summary

Human behaviour is a determining factor in road safety. For this reason, it is of crucial importance to encourage people to behave safely in traffic. It is known that people aware of being observed tend to modify their behaviour. By observing and recording the behaviours of drivers, it might then be possible to confront them with their behaviour. This could mean that drivers who realise that this can happen will adjust their behaviour ahead of time. They can also react this way as a result of an actual confrontation. For this form of behaviour influence to prove effective, it would ultimately have to result in fewer road traffic accidents.

Within this context, then, the goal of the study was to investigate if road safety could actually be increased by creating the possibility of confronting drivers when necessary with objective data about their own driving behaviour being recorded by telematic monitoring devices mounted inside their vehicles.
For this purpose, a study would monitor whether using this feedback mechanism would result in fewer and/or less severe road traffic accidents in actual everyday experience.

The first phase of this study was carried out within the framework of SAMOVAR, a project within the European Union Commission's research programme known as 'DRIVE 2'. Implementing the follow-up phase was made possible by the cooperation of the Association of Dutch Insurers and some of its members.

To be able to establish the effect on the number and severity of road traffic accidents, it was decided to implement a quasi-experimental field trial, the general design of which was a pre-test and post-test applied to both the experimental and control groups.

The design's specific implementation construction formed an independent subject within the study. Partially due to the time period over which a study of this nature had to extend, one of the assessments done beforehand was the number of vehicles that would be fitted with monitoring devices as well as the number of other vehicles.

The theoretical design was then modified to fit the actual research conditions, because these were ultimately determined partly by the fact that various fleet owners were included in the study (on a volunteer basis and at their own expense). As a result, it turned out that the vehicles available for the study displayed a great degree of variety in character and use. This necessitated a careful matching procedure when selecting a control group to compare with the vehicles equipped with monitoring devices.

Ultimately, 840 vehicles were involved in the study, 270 of which were fitted with monitoring devices already available on the market, the majority being 'accident reconstruction recorders', whilst some could generally be described as 'trip recorders' or 'journey recorders'.

These numbers were fewer than the sample size which had been assessed beforehand for indicating an effect within close margins of error. The numbers of vehicles also involved a diversity of fleets and for this reason created a non-homogeneous sample. Seven experimental groups of vehicles were equipped with recorders, for which twelve matched control groups could be selected. The advantage of this diversity, however, was that some insight could be gained into the distribution among such fleets as to the effect on accident occurrence.

The accident records of the vehicles involved in the study were recorded for a period of at least one year previous to the date on which the recorder was built into the vehicle as well as during at least one year following installation. Also recorded for these time periods were use, exposure and accident damage, with a separate data collection format being developed for this objective. The reliable gathering of all these pieces of information implied a considerable logistical effort due to factors such as the geographical spread of the fleets involved in the study.

This study established a statistically significant reduction in the number of accidents for several fleets in which the behaviour of the drivers was monitored in such a way that the drivers could also be confronted with their behaviour. As yet, these positive results can be given only within rather wide confidence intervals, this being due chiefly to the small sample size. When viewing the total group of fleets involved in the study, it is possible to estimate an accident reduction of some 20%.
In the case of the only fleet for which the costs of its own accident damage were known, there was also a favourable development in terms of accident reduction. In this respect, accident damage can also be considered a measure for the severity of the accidents outcomes.

It can be concluded that the methodology developed in the study is more generally applicable, especially when investigating the safety effects of virtually any in-car system that may influence driving behaviour.

In view of the results obtained, applying behaviour influence by driver monitoring is recommended. Further research is worthwhile, in particular in order to optimise its effects. This research might focus on subjects like the implementation of the feedback and its most effective use, improved equipment, and ways to sustain lasting effectiveness of the measure.

Contents

1. General introduction

1.1. Motivation, objective and scope of the study

Human behaviour is a factor of major importance in traffic safety.
A leading motive of this study is to examine how application of new technology can improve traffic safety by improving driver behaviour related to safety.

If drivers were able to behave according to the necessities of the actual traffic conditions and if they would in fact do so, no doubt road safety would be a less serious problem. Of course, this does not mean that all traffic accidents would then be avoidable, nor that it would be possible to perform a proper way of behaving in all circumstances. It is therefore also of importance to improve the traffic environment in order to make the driving task less complex and more natural, and driving less risky for the drivers themselves as well as for the other road users.

Driver attitudes and behaviour are nevertheless, as generally agreed, a foremost starting point for improving traffic safety. Accordingly, drivers have to be influenced, for instance by training, by publicity campaigns, or by police enforcement (though not saying that all behaviour in accordance with law is also safe behaviour too).
It is in general not easy, however, to address drivers individually and to find instruments for feedback and support.

It is well established that the observation of a person's behaviour is likely to affect that behaviour. Thus, monitoring driving behaviour offers a possibility for influencing it, particularly when the driver is confronted with his own behaviour.
Making use of this feedback mechanism is in fact at the basis of the study.

Information about vehicle movements and incidents and accidents in traffic can be gathered by in-car recording devices, or 'black boxes'.
Tachographs are a practical example of monitoring by device. They are in operation for over twenty years in trucks and buses for imposing the periods of rests and driving hours.
Tachographs, however, are unwieldy and inconvenient for use as a day-to-day system. The content of the data collection is limited and can be distorted without too much difficulty.
The new generation of electronic data-loggers or on-board computers is much more user-friendly and often offers a multi-purpose system as well.

The information recorded by such type of devices might be applied to check the observance by the driver of, for instance, the legal rules and regulations or a specific company's policy. To some extent, the recorded information is also of value for pointing out safety aspects to the drivers. By confronting drivers in such a way with evidence of their own behaviour, it is possible to influence their future behaviour.
Opportunities of the kind are especially valuable to fleet owners and insurance companies in developing and maintaining a safety policy.

At the same time, the recorded information can be utilised for other purposes as well.
Depending on the recorder type and/or its facilities, fleet operators can make use of in-vehicle data recorders as an operational management and logistical tool, for instance, for administering working hours and courses of time for loading, delivery and the trip-schedule, for supervising fuel consumption, vehicle maintenance and inspection, etc. And insurance companies might be interested in objective evidence concerning the circumstances of a traffic accident or even in the reconstruction of such an event in full detail.

The fundamental assumption in this study is that drivers, being aware of the fact that their behaviour is recorded - particularly just before and after an accident - will act in a safer way, for instance by adapting driving speed to traffic circumstances and will be acted upon, if necessary, by or on behalf of a higher authority, e.g. an employer.

The objective of the present study is to investigate the opportunities for improving traffic safety by confronting the driver with his monitored and recorded behaviour, making use of such in-car electronic recording devices.

There is some evidence in support of this assumption, and substantial effects in terms of fewer accidents and/or less severe accidents have been claimed as well. In a specific case in Germany, for instance, installing so-called 'accident reconstruction recorders' into a vehicle-fleet was claimed to have resulted into 30% fewer accidents. In addition, it is reported that a British insurance company offers fleet owners a premium reduction up to 15%, on the condition that a certain make of 'trip recorder' is installed in their vehicles.

However, so far effects have not been stated formally. Furthermore, it is not yet known exactly which are the factors that produce these effects, whether they could possibly be enhanced, or even if there will be positive effects in all circumstances. Nevertheless, it seams clear that drivers change their behaviour in such a way that they seem to be involved - on the average - less often, at least, in incidents.

1.2. Safety assessment of feedback on driver monitoring

In driving a vehicle, the driver operates - in permanent interaction with the outside world - on distinct 'levels' of driving behaviour. Those levels are mostly referred to as the 'strategic', the 'tactical' and the 'operational' level (e.g. Michon, 1979; OECD, 1984).
On the strategic level the driver is dealing with choices and decisions regarding the destination, the travel mode, the route, the time-table, etc.
On the tactical level he is taking action with respect to the road environment, including for instance the actual traffic, the traffic rules and regulations, the weather, the light conditions, and so forth.
The operational level concerns the factual execution of traffic tasks like orienting oneself, steering and course holding, accelerating, decelerating and braking, indicating one's direction, and so on.

Therefore, driving behaviour is related to manifold tasks, pertinent to each of those levels. Of course, influencing and modifying this behaviour is related to these levels as well.

In principle, influencing and modifying driving behaviour can be studied either by taking the 'results' - i.e. the effects - of changed behaviour as the subject of research, or the 'changes' in behaviour. In other words, research might focus on the *products* or the *processes* of an action of the kind.

Studying the processes is of interest, in particular in view of understanding driving behaviour and the opportunities it offers for optimising behaviour. As a rule, however, it is the most complicated way as well.
Not knowing the potential amount of an effect - this being the case in the present study - is a valid argument for examining that firstly.
For the same reason, priority has often to be given to appraise the effects of changed driver performances in cases of applying other types of newly developed, so-called Advanced Transport Telematics (ATT) systems.

Considering this, it was decided to start with assessing *the effects on vehicle accident rates and/or the accident severity of applying the feedback process.*
It is important to note however, that the feedback process itself is not the subject of this study and neither are the recording devices used.

In this context, a quasi-experimental field trial was proposed being an adequate research method for verifying the hypothesis: "drivers will act in a safer way if they are aware that their behaviour is being recorded and will be responded to."

1.3. The structure of the accident occurrence study

Obviously, the actual execution of the field trial for assessing the effect of driver monitoring on accident rates was to be embedded in a project in which several stages can be distinguished. They are in logical order:

- The real-world field trial we aimed at would require installing recorders in several fleets of commercial vehicles on a reasonably large scale and collecting data on accidents and on exposure of these vehicles over longer periods of time, as well as of at least as much non-equipped vehicles. And, most important of all, it would ask for the voluntary involvement and cooperation of many partners. The *feasibility* of such a study should thus be established in advance, being the first stage of the project.
  In that, topics like the cooperation with fleet-owners and insurance companies, the suitability of different types of recorders and the accessibility of the necessary accident and exposure were to be examined.

- Taking into account the outcome of the feasibility study, an appropriate *methodology* for studying effects on accident occurrence could be appraised next: the second stage in the project.
  Besides statistical and procedural requirements, it regarded in particular the issues of the design of the full scale quasi-experimental field trial, the required sample size and the analysis procedures to be employed.

- *The realisation and execution* of the field trial itself, the third stage of the project, could effectively start with the installation of data recorders in vehicles of the cooperating fleets.
  Creating adequate experimental and control groups, finalising the actual format of accident and exposure data and arranging the flow of research data were then among the main tasks to be carried out.

- The final stage of the project concerns the *analyses* of the research data and *reporting* the results.

1.4. The history and organisation of the project

Initially, the field trial formed part of the so-called 'SAMOVAR: DRIVE Project V2007'. This project beared on one of the 56 studies within the framework of the DRIVE II research programme 1992-1995 of the Commission of the European Communities on research and technology development in advanced road transport telematics.

SAMOVAR stands for 'Safety Assessment Monitoring On-vehicle with Automatic Recording'. Its studies were carried out by an international consortium, consisting of the 'Queen Mary and Westfield College' (QMW) of the University of London, the 'Motor Industry Research Laboratory' (MIRA), the 'Transport Research Laboratory' (TRL) and 'Royal Mail', all established in Great Britain, the Greek bureau 'IMPETUS Consultants', and the 'SWOV Institute for Road Safety Research' in The Netherlands.
Each of the partners had its specific interest in the SAMOVAR-project as a whole and took responsibility for carrying out its own share.

TRL and SWOV were both interested in the traffic safety aspect of preventing accidents. In the context of SAMOVAR, they cooperated in carrying out a field trial of the kind on accident occurrence rates and the related feasibility studies.
In that, SWOV was responsible for the methodology applied in such a field trial. TRL was responsible for accomplishing a field trial in Great Britain regarding vehicles of Royal Mail. And SWOV for one concerning several fleets of vehicles in Belgium and The Netherlands: *the 'Low Countries field trial'*.
Additionally, TRL spent some attention to driver behaviour related data, in particular changes in speeding and harsh braking over a period of time.

The required sample size of vehicles equipped with data recorders had been estimated during the study on the methodology. As reported in the feasibility study, getting these devices installed in reality had to entail seeking the definitive support and cooperation of fleet owners, data recorder manufacturers and/or importers, insurance companies, and regional and national authorities. For it was a matter of fact that the SAMOVAR field trial in the Low Countries could only be set up on the basis of voluntary collaboration between such partners, each funding its own share in the experiment.
The trial was planned to start in January 1994 and this was actually achieved with the installation of one fourth of the required number of data recorders in two fleets. Arrangements with other fleets were made at a later stage. The moment their cooperation actually began varied widely in time.

In some cases the installation programme itself took a considerable time. It resulted in different starting points of the trial. Moreover, and of greater importance, it resulted into shorter periods during which accident data could be collected within the time constraints of the DRIVE II research programme.

In order to cope with this unsatisfactory situation, arrangements were made for enlarging the period of data collection beyond the planned completion date for the SAMOVAR study (May 1995). This resulted into an '*additional phase*', in which the Low Countries field trial was completed along the lines of its original design. For this purpose separate reporting was planned, in addition to the 'intermediate' reporting at the end of SAMOVAR being part of the DRIVE 2 research programme.
Owing to the support of a union of Dutch insurance companies and some of its member-insurance companies, as well as of some of the cooperating fleets, SWOV was enabled to perform this additional phase.

This report is to be considered as the final account of the field trial carried out in the Low Countries.
It offers an overview of the applied methodology and the design of the trial and it covers all results, incorporating those when the trial was carried out as part of the SAMOVAR-project.
For comprehensive accounts of the stages of the SAMOVAR part of the study as mentioned above, we refer to the related reports of the consortium, in particular the 'Deliverables D2, D3, D6, D10 and D11'.

2. Methodology of the accident occurrence study

2.1. Design of the field trial

2.1.1. *Basic approach*

As pointed out in the preceding chapter, it is the fundamental notion of this study that the potential influence of a driver behaviour monitoring device on accident rates emanates from the driver's response to the possibility of being confronted with his recorded behaviour by, for instance, the responsible management. In other words, the *effect of the feedback* is the prime subject of study.
By consequence, the objective of the field trial is to assess *the change in accident occurrence* in case drivers can be confronted with their driving behaviour as monitored and recorded by an in-car data recorder.

In order to assess the size of such an effect on the amount (and/or severity) of accidents, the field setting of the trial requires a relatively uncomplicated, yet unambiguous type of design, avoiding practical constraints as much as possible.
In view of this, the preferred design - which will be elaborated hereafter - is one in which the accident occurrence of an experimental group and a control group is compared during a pre-test and a post-test phase: a so-called *'untreated control group design with pretest and post-test'*.
In case of a positive effect, the number of accidents among the experimental group will then be diminished after the intervention - i.e. the installation of the data recorders - in comparison with the number of accidents among the control group.

To be able to draw valid conclusions from such comparisons, it is a prerequisite that vehicles with and without a data recorder are driven on average in situations with about the same risks, and that differences in the number of kilometres can be counterbalanced.
Otherwise one cannot simply distinguish between the effect being investigated - the use of in-vehicle data recorders on traffic safety - and other influences like sample differences. For instance, there might be under-representation in one of the groups concerning the more dangerous trips within built-up areas, or over-representation of the generally safer passenger cars (perhaps as a result of the observation period, since heavy vehicles are less used during the weekend). Not being aware of an unbalanced data structure, a positive result in this hypothetical instance could erroneously be attributed to the recorders, whereas it is actually caused by the fact that the vehicles with a recorder were at lesser risk.

A way of dealing with such problems is to determine the most important safety factors, and to enhance homogeneity for the groups to be compared by selecting them along these factors. Strict homogeneity is not always needed. Groups might show heterogeneity, if of the same kind and to the same extend.

Apart from the data recorder of course, homogeneity may for instance be better in a before and after design, because drivers and vehicles stay the same even if traffic does not and time goes on. Furthermore, it is sometimes possible to adjust for improper effects, as through the use of accident rates in the case of different mileage, through the re-weighing of sample parts, or even through the re-calculation of results on the basis of known safety figures.

2.1.2. *Design conditions*

Some properties that are important with regard to the applied design will now be discussed in greater detail:

- the experimental and the control group have to be *comparable* with respect to their relevant characteristics;
- the *exposure* should not alter during the trial (unless changes and its influences are sufficiently known);
- the *registration* of accident information should be performed in an uniform way during the entire trial;
- the *intervention* or stimulus should not affect the control group; and
- the *feedback* to the driver should effectively be applied.

Comparability can be achieved by choosing the subjects of the trial on the basis of either arbitrariness or similarity: 'selection at random' versus 'matching'.
In the situation that fleets of different nature were willing to cooperate in the trial, matching of the experimental and the control fleet was the obvious way.

From a practical point of view, selection on the basis of similarity is also of importance in restricting the number of variables of the trial: the same kind of vehicles, of transport, of traffic circumstances (e.g. time, type of roads, inside or outside built-up areas, traffic density and mix, geographical features) and so on.
At the same time, these aspects are strongly related to exposure to dangers in traffic, which preferably should remain constant during the trial.

In our case, the subjects of the trial are in fact vehicles, whether fitted with data recorders or not. However, it is the drivers who ultimately cause an effect. Hence, if the selected vehicle is not always driven by the same driver(s), the driver has to be allotted to the vehicle, thus avoiding his preferences for driving a recorder-equipped or non-equipped vehicle.

The definition of a 'traffic accident' as applied here, is:
"A traffic accident is an event on public roads, which is related to traffic participation, in which at least one moving vehicle is involved and in which, as a result, one or more road users have been fatally injured, and/or injured, and/or in which material damage has been caused."

The necessity of having accident data available concerning the pre- and post-test period of the trial, will in practice result in making use of the administration of the cooperating company and/or the related insurance company. The last possibility is to be preferred, for instance because one can rely on the availability and accessibility of all files. Moreover and of greater importance, an insurance company might be helpful in informing us whether we are dealing with a 'normal/ordinary' fleet or not. Possibly it may be willing to supply data on other comparable (anonymous) fleets.

Whatever the level of quality might be, the way in which accidents are registered has to be the very same all over the period of investigation. This condition might be problematic. A 'cooperative' company, for example, is likely to enhance the scrutiny of accidents recording: they might want 'to make the best' of the experiment.

The possibilities in defining an 'accident/incident' and in classifying the severity of these events are dependent on the way they are recorded, and on homogeneity and reliability. From the feasibility study we learned that the 'cost of an accident as claimed by the insurance company' hardly presents a solution for these difficulties.

Feedback is an essential element in this study. Given the nature of different types of data recorders there is a diversity of use and users, and of applications and feedback-mechanisms. Hence, focusing the use explicitly on traffic safety and creating a distinct feedback are highly desirable. It also restricts the number of variables. Merely installing data recorders in the vehicles cannot be regarded as an appropriate intervention or stimulus in the field trial.

A trial involving a reasonably large number of vehicles is necessarily dependent on a supply of data recorders which are obtainable 'off-the-shelf'. No truly general purpose vehicle data recorder was known at the start of and during the study.

The in-vehicle data recorders which are currently commercially available fall into two categories as defined by their designated usages. One of these is the Accident Data Recorder (ADR), which, as the name suggests, has the specific task of collecting data which is of use to aid the reconstruction of a particular traffic accident. The other type, called the Journey Data Recorder (JDR), is a device with the primary function of providing data which can be used to improve the management of a fleet of vehicles.

In case of the ADR, the data contain facts on the causes of an accident. Information on 'who is to blame' can be counted in favour or against the driver. It was unlikely that within the field study facts indicating guilt would be used in a juridical context. However, it was undesirable that recorded facts are applied only to clear the driver, for that might result in differences in his attitudes.
In order to avoid this, drivers should be informed beforehand that the data of every recorded accident will be examined by the responsible management of the company and that internal action might be taken if they are to blame. Obviously, the fleet owner has to be committed to this course of action.

The JDR comprises information on the entire time of the trip, as well as on the final one and a half minute or so in greater detail; this last being the period before a collision, or a police check.

Analogous to the utilisation of an ADR, drivers should be told that those collision related and other data might be applied against them.
The trip records will provide at least an overall picture of the speed behaviour, since recorders of different manufacturers can be programmed for those aims. For instance, speed limit violations and their durations can be traced in such a way and the distribution of driving speed can be compared with the trip and its driving conditions. By telling the drivers that information of the kind will be used to monitor the driving behaviour during the trip and that the company will not accept illegal violations, the eventual effect in accident reduction will probably be the maximised.

The two different types of recorders - both offering an opportunity for feedback to the driver - could, in principle, be applied for the purposes of the experiment.
The information during trips, as collected by the JDR-type of device, might be used by the responsible management for *feedback on a regular base*.
The ADR-type of device, mainly intended for the reconstruction of the events during an accident and/or incident, solely records facts on driver and vehicle during the period just before and after an accident and/or incident, albeit in much greater detail. Thus, *feedback* can only take place *in an occasional way*.
It would be of interest to reveal potential differences of the impact on accident rates of monitoring driver behaviour using the two types of recorder.

Anyhow, if fitting of recorders is meant for intervention in the trial, the fitting has to be linked with an unambiguous statement on its objective: promoting traffic safety by evoking responsible behaviour from the driver, as well as on the utilisation of the information from the device by the company's management. This remark is pertinent to both types of recorders: ADRs and JDRs.
A written instruction seems to be the most suitable for this purpose. It can be drafted by the researchers in consultation with the management of the involved company.

Another problem to be tackled is the risk that the intervention influences the control group. Obviously, one cannot prevent informal communication. This is particularly relevant for smaller companies. In that case a solution might be to supply all vehicles of that fleet with a data recorder and to try and find accident data of another, comparable fleet where none of the vehicles has been equipped. As mentioned already, an insurance company could perhaps be able to achieve that.
The problem will be less critical in bigger companies, of course - and for that reason their participation is more desirable - but even then measures need to be taken like ensuring adequate geographical spreading and controlling the flow of information.

A company might participate because it is safety-minded. This could have resulted already in a safer behaviour of their employees. The smaller than average accident rates offer fewer possibilities for further improvements.
In the opposite case of a bad accident record being the motivation for taking part in the experiment, not only the proposed use of recorders might be

'helpful'. In fact, any measure will have an effect. The only remedy for this problem is to obtain some insight into their 'safety history'.

2.2. Sample size

2.2.1. *Statistical nature of accident data*

Besides the above mentioned methodological design problems, some basic statistical questions have to be considered.

The criterion variable in our trial is pertinent to the accident involvement of vehicles. Accidents are counted and these counts are applied for comparing the two experimental situations: vehicles with and without a recorder.
The counts are fundamentally statistical in nature, for accidents happen by chance to some extent and thus have to be understood as the outcome of a statistical process. In fact, counts can vary within the same setting without any apparent reason. This has to be considered when looking for the effect of the intended use of data recorders.
Obviously, the bigger the difference in the number of accidents in applying recorders or not, the smaller the likelihood that it is merely caused by chance based variation. And also, the greater the number of accidents, the less the relative fluctuation. For example, instead of two accidents, one or three could just as easily be observed; however, 50 or 150 accidents instead of 100 is far less likely.

The trial involves only a limited sample and so there is a chance of differences between experimental results and reality. Conclusions correctly drawn from the experimental data could deviate from the actual situation, thus being factually wrong and making reality unknown.

Because the statistical origin of the criterion variable might jeopardise conclusions, the following possible outcomes of the trial - depicted in *Diagram 1* - have to be considered beforehand in relation to the hypothesis of a positive recorder effect on traffic safety.

recorder effect according to experiment	conclusion from experiment if recorder effect in reality is:	
	positive	none
positive	true	false *type I error*
none	false *type II error*	true

Diagram 1. *Truth table for the recorder effect*

Obviously, data have to be analysed in a way that there is little chance of erroneously concluding a positive recorder effect. The main key to deal with the kind of problem lies in choosing an appropriate sample size for the trial, starting from a good design as discussed earlier.

2.2.2. *Traffic safety effect of the use of in-vehicle data recorders*

In the selected design the accident involvement of vehicles with and without a data recorder is compared during a pre-test and a post-test. As indicated before, minor differences in accident involvement could be due simply to statistical variation. An appropriate sample size of the experiment does not, however, depend solely on the number of accidents (numbers which have less relative dispersion the larger they are) upon which the involvement rate is going to be based. The size of the effect of the recorder is of importance as well.

Assuming a distinct in-vehicle data recorder effect and calculating the involvement rate then to be found, the number of accidents can be estimated beforehand, and also the number of vehicles required in the trial. This procedure diminishes the risk of erroneous conclusions.

For the purpose of estimating the necessary vehicle sample size, the model of analysis can be simplified by supposing that the fleets with and without a recorder are equally large and that vehicles in both fleets will be operated for about the same mileage and under similar safety conditions. Then, one might expect fewer accidents among vehicles of the fleet with a recorder in case of a positive recorder effect.
This type of analysis can be modified, if necessary, to more complex situations.

Fluctuations in the number of accidents are derived from the concept of accidents, as being rare events that happen by chance. It introduces a probability model for these numbers as described by the 'Poisson Distribution', or, for large numbers, by its 'Normal Approximation'. Conversely, the model provides test statistics for the difference between the numbers of accidents among vehicles with and without a recorder.
A recorder effect can then be stated within a certain reliability.

So, the test statistics can be used for estimating a sample size appropriate for stating real effects of the intended use of in-vehicle data recorders.
The computation is based on a configuration of numbers as if the trial had already been done. However, the proper configuration is the outcome of the trial which still has to be performed. Being aware of this, the statistical procedure will therefore make use of the 'Binomial Distribution'.

Some indications of a recorder effect are known. For instance, reductions in accidents of 30% are claimed by some manufacturers of data recorders. Data from Royal Mail in the United Kingdom seem to show a 17% reduction. It concerns an estimate for a few groups of vehicles with 500 recorders in use (by letter of Royal Mail; 19-7-1993). And there is a British insurance company offering up to 15% premium reduction if a recorder is installed.

Therefore, a preliminary estimate of 20% reduction in accidents seems to be a realistic starting point.

2.2.3. *Accident involvement rate of vehicles*

In a sample of the Birmingham Post Office fleet in Great Britain, annually 40% of the vehicles were at least once involved in an accident (by letter of TRL; 9-10-1992).

During a pilot study in the feasibility phase of SAMOVAR, a sample of Dutch fleets showed large differences in accident involvement among fleets. The involvement rates varied mainly from about 0.2 to 0.5, but even 1.0 and in the extreme 2.4 accidents on average per year per vehicle were reported, as presented in *Table 1.*
Considering these data, it is obvious that the differences are real and cannot be a result of chance alone. They cannot be explained either by differences in annual mileage, which vary from 30,000 to 120,000 km. Hence, they might have been generated, for instance, by the kind of transport, traffic and driving conditions, the safety 'climate' in a company, etc. This does not necessarily imply that safety effects of the recorders will show similar differences.
Although there are clear differences within the fleets themselves, stating incorrectly a recorder use effect may be avoided by properly selecting vehicles with and without a recorder within the fleets.

Type of vehicle	Number of veh. in fleet	Annual km per veh.	Number*) of accidents	Annual involvement	Risk per mill. veh.km
> 7.5 ton.	56	120,000	50	.5	5
	72	100,000	17	.4	4
	37	90,000	123	2.4	27
	90	85,000	215	.5	6
	36	60,000	7	.2	3
VAN etc.	33	120,000	53	.2	2
	17	40,000	30	1.0	24
	240	30,000	73	.3	10
	76	30,000	22	.3	10
car	55	30,000	19	.4	11

*) The numbers concern different lengths of observation time

Table 1. *Results from a pilot study among Dutch vehicle fleets.*

A dilemma has to be solved in selecting the kind of 'events' to be considered. As the pilot study in The Netherlands showed, fleet owners often give not only an account of the 'real' traffic accidents, but also of cases of 'financial loss' related to traffic participation, like parking damage. Events of the kind are not directly connected with traffic hazards. Nonetheless, it its their feeling and perhaps their experience as well, that such events also express the driver's attitude towards careful and safe driving. For this reason, not only the formal definition of a traffic accident, as given in § 2.1.2, is applied in this study.

On this point, it is noticed that in many instances drivers form a fixed combination with their vehicles, so in the trial drivers and vehicles may generally be looked at as referring to one another.

Dutch governmental accident and vehicle registration systems offer a further approach for estimating accident involvement rates.
Based upon the annual number of vehicles involved in traffic accidents and the total size of Dutch vehicle population, an annual involvement rate of between 0.01 and 0.02 might be concluded, as shown in *Table 2*.

It should be noted that the mean annual mileage estimated from official figures is far below the result in the pilot study for each of the categories of vehicles. Obviously, the vehicles in the pilot fleets are somewhat special. In itself this is not a problem for the project.
Moreover, official registration of accidents is far from complete in particular regarding the low-speed traffic, and the category of 'material damage only accidents' must also be added. In that way, the number of traffic accidents in The Netherlands in 1991 can be estimated as being 300,000 in total (in stead of 40,703 accidents in the official statistics, which only record fatalities, hospitalised injuries and slightly injured persons).
Correspondingly, involvement rates are likely to be ten or more times as large, and thus seem to match the situation of the pilot study (Harris, 1990).

Type of vehicles	Number 1) of vehicles (in thous.)	Annual 2) km per vehicle	Number 3) of vehicles in accidents	Annual involvement	Risk per million vehicle kms
> 7.5 ton	125	48.000	2.301	.02	.4
VAN etc.	418	19.000	3.812	.01	.5
car	5.509	15.000	39.027	.01	.5

1) CBS Statistiek van de motorvoertuigen 1-8-1990
2) CBS Statistiek van de wegen 1-1-1992, concerning 1990
3) CBS Statistiek van de verkeersongevallen op de openbare weg 1991, these 1991 numbers exclusive of accidents with material damage only

Table 2. *Figures from Dutch governmental accident and vehicle registrations*.

Summarising, figures seem to show that on average one should assume an involvement rate of at least 0 3 per annum.

2.2.4. *Required number of accidents in the trial*

Within the simplified model described before, the traffic safety effect of the use of data recorders will be derived by comparing the number of accidents observed among the vehicles with and without a recorder. In the trial these numbers can by chance take on a range of values around the proper averages for the given traffic safety conditions. Larger deviations might be increasingly less likely. Yet, there is still a risk of finding an exceptional difference and, being not aware of that, an incorrect conclusion can be drawn regarding the recorder effect.

The extent of the risk of such an error can be limited by choosing an appropriate trial size. As already explained in § 1.2.1, two kinds of errors have to be considered: the type I or α-error of finding a positive recorder effect where there is none in reality, and the type II or ß-error of not finding a positive recorder effect when it actually exists.

Although both errors have to be avoided, an error of the first kind is particularly serious, because it leads to ineffective action and inefficient expenditure. Therefore, the upper value of the one-sided confidence limit for the test statistic has to be set high. Then, the probability of mistakenly rejecting the null-hypothesis is less than a small 'α-amount'. It is conventional to choose a value of 5% for α. It implies that 'a positive recorder effect while there is none' would be accepted in less than 1 out of 20 experiments.

Further limiting the critical range of the test statistic will result in enlarging the minimum number of accidents required in the trial. Consequently, in order to make the trial feasible, one has to face a certain amount of risk. To cope with this, a higher chance ß of making an error of the second kind has to be accepted. It is convenient to choose a value of 20% for ß. The power (1-ß) of the test, as a measure of the capability in detecting a real difference out of the data, has then been set at the level of 80%, starting from the hypothesis of a given value for the actual recorder effect.

Within these settings, the expected annual number of accidents of the minimum number of vehicles for the trial has been determined by means of the simplified model, as well as the sensitivity for deviating parameter values. Before presenting these results, the calculating procedure will first be discussed.

The sample size for the trial can be calculated in a manner pointed out by Schneiderman (see: Lothar Sachs, Statistische Auswertungsmethoden, Springer Verlag 1968, p. 215). As described before, the procedure is based on the simplified analysis model. In particular, the groups of vehicles to be compared in the trial are supposed to be of the same size and have the same annual mileage. It is also assumed that the data can be gathered over a period of one year of recorder use.

The mathematical expression for the minimum numbers of vehicles needed in the trial is given below. The expression contains the following abbreviations:

'N': the required minimum number of non-recorder vehicles in the trial;
'inv': the annual accident involvement rate of these vehicles;
'eff': the recorder effect, so that the involvement rate with recorder is a factor (1-eff) times the involvement rate without.

Further, let the chances of errors of types I and II be α and ß, so that the related normalised one-sided confidence limits become z_α and z_β, for which the following values are relevant here:

- if α is taken to the size of 5% then z_α has the value of 1.645;
- if α is 10% then z_α is 1.282; and
- if α is 20% then z_α is 0.842, values being similar for ß.

The minimum number of vehicles is given by next expression:

$$N \geq (z_\alpha \sqrt{a} + z_\beta \sqrt{b})^2 / (eff * inv)^2 + 2 / (eff * inv)$$

with

$$a = ½ inv * (2\text{-}eff) * (2 - inv*(2\text{-}eff))$$

$$b = inv * ((1\text{-}inv) + (1\text{-}eff) * (1 - inv*(1\text{-}eff)))$$

(the term 2 / (eff * inv) being the correction for continuity).

The expected annual number of accidents without recorder is then
n = inv * N.

The results derived from this expression are summarised in *Table 3*. In it, one may observe that the expected number of accidents not only decreases with increasing sizes of the accepted error amounts α and ß, but also with the supposed size of the recorder effect. The expected number also decreases with an increasing annual accident involvement rate of the vehicles, thus accounting for smaller relative fluctuations in case of larger numbers of accidents.

Annual accident involvement	Error type %		Recorder effect			
	I	II	-10%	-15%	-20%	-25%
.2	5	10	1338	587	326	206
	10	10	1031	454	252	160
	5	20	972	428	238	151
	10	20	714	315	176	112
.3			1183	522	291	184
			913	403	225	143
			860	380	213	135
			633	281	158	101
.5			874	391	221	142
			675	303	172	111
			636	286	163	105
			470	212	121	79

Table 3. *Expected values of the annual numbers of accidents related to the minimum numbers of vehicles without recorder in the trial*.

2.2.5. *Sample size of the trial*

Taking the next step to calculate the minimum numbers of vehicles needed in the trial, the reciprocal accident involvement rate can be applied to the minimum number of accidents calculated in *Table 3*. This results in the values presented in *Table 4*.

In view of general safety developments, it may be desirable to correct for some downward safety trend in accidents in the final analysis. Vehicles are equipped with a recorder later over a period, thus a time gap between the use of vehicles without and with a recorder will be introduced.

In principle, two methods are available. In the first method, the official government statistics are used for calculating a trend correction factor. As discussed already, general data are not specific enough with respect to the vehicles of the co-operating fleets. So, this method is not appropriate for our purpose.

In the second method, one has to make use of the data concerning the control group of vehicles without a recorder. That group has been selected to correspond with the experimental group in the most important respects. It implies that the design of the trial will become less simple, but apart from this, it introduces additional variation. Of course, the question of a trend itself already causes more uncertainty. Nevertheless, the fact that trend information must be obtained by sampling methods means that the minimum number of vehicles in the experimental group has also to be increased.

Annual accident involvement	Error type %		Recorder effect			
	I	II	-10%	-15%	-20%	-25%
.2	5	10	6700	2940	1630	1030
	10	10	5160	2270	1260	800
	5	20	4860	2140	1190	750
	10	20	3570	1580	880	560
.3			3950	1740	970	620
			3040	1350	750	480
			2870	1270	710	450
			2110	940	530	340
.5			1750	780	440	290
			1350	610	350	220
			1270	570	330	210
			940	430	240	160

Table 4. *Minimum numbers of vehicles without recorder in the trial.*

In reality, it will be difficult to find enough vehicles in the co-operating fleets to be equipped with a recorder. Therefore, it could be necessary to make use of the fact that the minimum number of vehicles with a recorder may be diminished somewhat, if the number without a recorder is raised. In the case of the chosen parameter setting, the mentioned relation between the sample sizes of the experimental and control group is depicted in *Figure 1.*

By the same token, the accident involvement rate with and without recorder has to be compared, instead of the number of accidents. However, this is just a technical detail. It may be seen for the moment as a minor important aspect for the purpose of our computations, the more so because of the shown sensitivity of the minimum numbers for even small changes in the parameter settings.

Not all vehicles within a fleet will be involved in accidents to the same extent. There is even at least the possibility that the mean involvement rate is excessively contributed to a few 'accident prone' drivers. A recorder use effect may then be principally assigned to them.

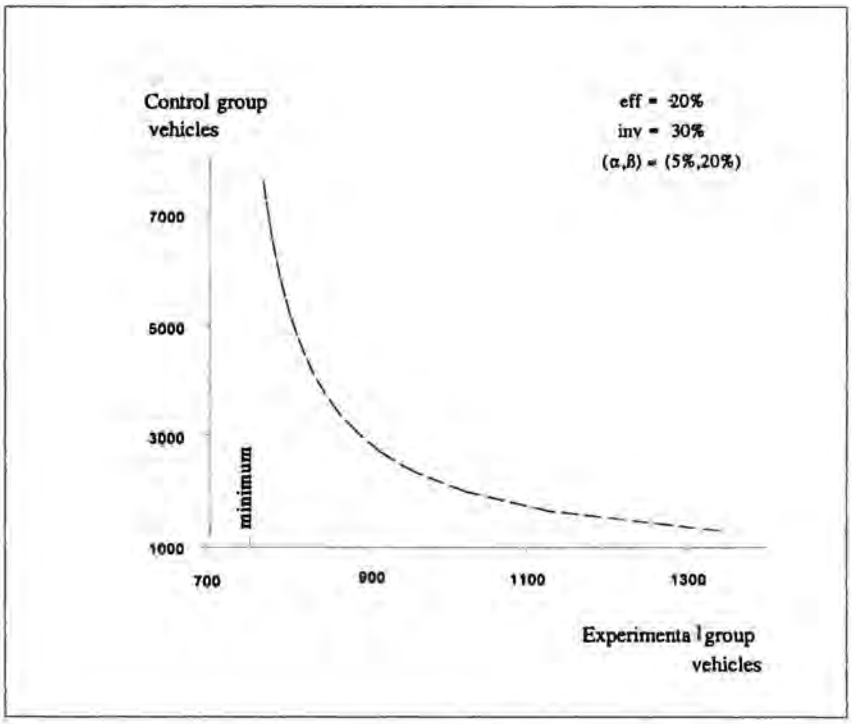

Figure 1. *Relation between sample sizes of experimental and control group.*

It raises the question of the need for a general safety approach of all drivers. Besides, the involvement rate would not be derived from sufficiently many independent events for meeting the statistical conditions of the applied sample size calculation method. It cannot easily be detected whether this is the case, or not. Nor every driver who has more accidents than others is thus 'accident prone' - if such a quality really exists - instead of having simply met with misfortune.

As discussed already, the calculated minimum number of vehicles that have to be brought into the trial concerns each comparison in which the recorder effect has to be calculated for. Nevertheless, there is no larger total minimum number of vehicles needed in the trial in order to find the size of an overall recorder effect for the total of all co-operating fleets, based on different comparisons of mutually homogeneous groups.

The main results of *Table 4* are now summarised in *Table 5* below. It is confined to the most relevant setting of critical one-sided limits, for α being the value of 5% and for ß the value of 20%.

Annual accident involvement	Recorder effect			
	-10%	-15%	-20%	-25%
.2	4860	2140	1190	750
.3	2870	1270	710	450
.5	1270	570	330	210

Table 5. *Minimum number of vehicles without recorder in the trial, given* $\alpha = 5\%$ *and* $\beta = 20\%$

Table 5 shows that some 500 to 700 vehicles without a recorder have to be involved in the trial and an equal number of vehicles with a recorder during a period of one year. Under these conditions, there will be a reasonable chance of finding positive recorder effects of 20% or more, if the mean annual accident involvement rate among the vehicles of the cooperating fleets is about 0.3.

2.3. Operationalisation of the trial design

2.3.1. *Research requirements in practice*

In order to perform the field trial in a proper way, the following conditions have to be met:

1. Enough vehicles installed with a data recorder have to be available.
2. At least as many comparable vehicles have to be available in the non-treated control group.
3. Accident and other data from these vehicles have to be gathered over at least a one year 'before' and a one year 'after' period.
4. When a vehicle equipped with a recorder is involved in a traffic accident, the driver should be confronted - if necessary - with his driving behaviour by or on behalf of the responsible fleet management, which has at disposal now a new source of information.

With regard to the conditions 1 and 2, the required number of vehicles in the experimental and control group each amounts at least 500, as discussed in § 2.2. Where such a number exceeds the size of modal fleets, it will be necessary to build up these groups by taking together vehicles from several fleets of a smaller size.

With regard to the fourth condition, it is essential that the driver is aware of the fact that his driving behaviour will be recorded and that he will be confronted with it, if necessary, by the responsible management. The way the latter will be done is up to the fleet management, but it is imperative that it is done.
Arrangements on applying some procedure regarding this essential feedback were explicitly made with the management of each fleet in which

recorders were installed. The procedure itself was left to the fleet management involved. Only suggestions were made how to deal with it, in particular with respect to the fact that the records are not necessarily to be solely used in a restrictive sense, but can also be applied as an aid in giving incentives in case of proper behaviour.
The feedback actually provided by the management and how this feedback is experienced by the drivers has not been studied. Lack of supplementary funding impeded the execution of a survey on self-reported behaviour among the managements and drivers.

The categories of the vehicles have to be known, as well as their utilisation, the traffic and other conditions in which they are operated, and the number and the cost of accidents in which they are involved.
All data have to be available from - preferably - one and a half year before the moment of installing the recorders, till one year after that moment.
The data will be used on the one hand to establish criteria for selecting the non-treated control group, and on the other hand, to assess afterwards the safety effect of installing recorders in terms of cost of accident damage.
The required information will be elaborated in the next section.

2.3.2. *A format for information gathering*

The information on vehicles and their use and on traffic accidents and exposure to traffic hazard - as generally described in § 2.1.2 - has to be defined and operationalised in order to be practically viable. For that purpose, a set of eight forms was developed in order to gather the operational information in a structured way from the different participating organisations. These included fleet owners or managers as well as insurance companies. There is also a need to take into account the divergent legal systems of the countries involved: Belgium and The Netherlands.

The forms ultimately define *the format for information gathering*. They are given, in their English version, in the *Appendix*.

The first two forms regard the experimental and the control *fleet*.
Information is requested on, respectively:
- the type of vehicle, distinguished into passenger car, van (up to 3½ t), light truck (3½ - 7½ t), heavy truck (over 7½ t), truck with full trailer/ semi-trailer, and bus/coach;
- the number of vehicles;
- whether vehicles have a permanent driver or not;
- in use for international transport or not;
- mostly used within urban areas or not;
- only used on working days or not;
- only used during day time or not.

The forms have to be completed once by the cooperating fleets and/or insurance companies, preferably at the beginning of their being involved in the trial.

The third and fourth form regard information on the individual *vehicles* of the experimental and the control fleet. The requested data concern:

- the number plate of the vehicle;
- the average annual mileage of the vehicle;
- the date of putting the vehicle into use;
- mileage 1½ years before installation of the recorder;
- date of installation of the recorder;
- mileage at the moment of installation of the recorder;
- date of putting the vehicle out of use;
- mileage when putting the vehicle out of use
- mileage 1 year after installation of the recorder.

These forms too have to be completed, as far as possible, firstly at the start of the trial and then as the subsequent relevant information becomes available.

Form 5 and 6 concern information on *traffic accidents*:
- number plate of the vehicle;
- date of the accident;
- time of the accident;
- within the urban area or not;
- mileage at the time of the accident;
- were other moving vehicles, bicyclists or pedestrians involved or not;
- did injuries occur or not;
- cost of own damage;
- is the damage cost recoverable or not;
- cost of damage to the opposite side.

The forms too have to be filled in at the start of the trial over the pre-test period and then, as soon as possible after an accident. In consultation with the cooperating partners, arrangements were sought for delivering the information during the post-test period on a regular basis, preferably at intervals of one or three months.
Unfortunately, the subject of actual feedback on monitored behaviour given to the driver could not be addressed, for mostly the forms ought to be filled in by insurance companies, and not by the responsible management.

Finally, a couple of forms is focused on cases resulting into 'non-traffic related damage', like parking damages and damages caused by incidents on business/industry sites. Information of the kind might give some insight on the impact of the use of in-vehicle data recorders on driver behaviour in outside-traffic situations. Besides that, collecting such data promotes proper use by our partners of the definition of a traffic accident. The required data concern:
- number plate of the vehicle;
- date of case with damage;
- mileage at the time of the case;
- cost of own damage;
- is the damage cost recoverable or not;
- cost of damage to the opposite side.

The forms too have to be completed at the start of the trial over the pre-test period and then, as soon as possible after a non-traffic related damage case.

2.3.3. *Methods of data analysis*

Although the sample size calculations were based on a specific data analysis model, the statistical technique to be applied in the factual data analyses serves another purpose and may surely start from more specific models. The experiences of field data gathering may show in due time that some distinctions are necessary or that information becomes available which can be employed in further analysis and by other methods.

The basic model already described makes use of accident counts that are classified within a two by two data matrix, distinguishing between the periods before and after the vehicles in the experimental group have been equipped with a recorder, and at the same time between the experimental and the non-treated control group. Because of differences in accident exposure, the data of the counts in each of the four cells have to be weighted with the corresponding factor of their mileage.
Diagram 2 shows the basic data table for the trial. Standard statistical procedures are available to decide upon the recorder effect for this case.

Numbers		Period before vehicles are equipped with a recorder	Period after vehicles are equipped with a recorder
Treated group vehicles	Traffic accidents	${}_tN_B$	${}_tN_A$
	Total fleet mileage	${}_tV_B$	${}_tV_A$
Control group vehicles		${}_cN_B$	${}_cN_A$
		${}_cV_B$	${}_cV_A$

Diagram 2. *Principal data layout of the recorder trial.*

From the pilot study during the feasibility phase of SAMOVAR, it seemed that the analysis of data should be done separately for each cooperating vehicle fleet. This is because experimental and control groups from different fleets will be unbalanced in terms of size, safety policy, vehicle utilisation, etc., as discussed in § 2.2.3. Moreover, some other classes may have to be distinguished simultaneously, such as vehicle categories or the traffic conditions vehicles have met during operation.
In this situation, separate results - by themselves too few for allowing a relevant decision on a recorder effect - have to be combined.

Two main approaches exist for handling this.

- In the first, the question is whether the recorders have a positive safety effect at all, or not. In principle, it can be judged by non-parametric methods like a sign-test applied on the total set of all partial outcomes.
- In the second, the question is to reach a kind of overall estimate of the recorder effect for the total of cooperating fleets. Differences in fleet sizes would result into unbalanced and non-optimal outcomes, the so-called 'Simpson's Paradox'. In order to avoid this, a kind of weighted

sum of the partial results can be estimated. For that purpose in particular, each control group is assumed to produce its own estimate of the overall safety development for which the development in the matched experimental group has to be compensated.

In the case of the study, the sample of participating fleets is far from representative for the population of all fleets. By consequence, the results of the latter procedure cannot be fully generalised. Therefore, an overall effect for combined groups will be calculated here in such a way that the result has the smallest confidence interval. Then, each estimate contributes to the combined overall effect in proportion to its reliability. The reliability is the inverse variance of the estimate and can be computed under the well-known assumption that the numbers of accidents on which the estimate is based, will follow the so-called Poisson distribution.

In the study, this method will be applied in all calculations in which several group results have to be combined. In general, it firstly concerns the combination of control group results within a cluster around an experimental group, and secondly the combination of the eventually reached, individual cluster results.
The overall result constitutes a kind of best estimate of the safety effect in the fleets sample of the study, given the fleet sizes and other particular characteristics of the sample.

Furthermore and apart from this overall result, it is relevant to know what maximum safety effect under normal conditions can be obtained by means of using a recorder, and in which circumstances the recorder does not work or works poorly. Presumably, it will not be easy to answer such questions, because of the partitioning of data and a sort of 'reversed regression-to-the-mean' problem, caused by ordering the outcomes afterwards.

Another approach for the analysis consists of making use of point data, instead of count data. Each accident is then an observation in its own right. It is characterised by its scores on a chosen set of relevant variables, in stead of being classified according to these scores and being counted in the indicated class.

Other statistical methods for analysis exist as well. One may think of multilevel multiple regression models. Or, if the appropriate information is available, of the survival analysis technique. In the latter case, the lengths of time or the kilometres driven up to a next accident are the criterion variable, being aware of the fact that data are being censored to the left and right. These techniques will not be elaborated at this stage.

3. Realisation and execution of the trial

3.1. Introduction

The methodology of the accident occurrence study, as described in Chapter 2, was developed taking into account the outcome of the previous feasibility study. As such, it comprised the theoretically most favourable design at the time.
Moreover, it had to serve as a guideline in setting-up the actual experiment. This entailed seeking the definitive support and cooperation of fleet owners, data recorder manufacturers and/or importers, insurance companies, and regional and national authorities. It was a matter of fact that the eventual field trial in the Low Countries could only be set up on the basis of voluntary collaboration between such partners, each funding its own share in the experiment and each having its own interest in the success of the project. Consequently, designing the trial was constrained in several respects and the trial itself had to be adapted to situations in practice.

The adaptations of the methodology and its consequences will be reviewed firstly, followed by a description of the fleets and the vehicles ultimately involved in the trial.

3.2. Adaptations to situations in practice

Perhaps the key feature specific to this particular trial was that the trial eventually included a relatively large number of small, quite different commercial fleets.
The fleets varied widely in kind and characteristics and , for instance, ranged from:
- passenger cars to coaches and lorries;
- the transportation of people or goods;
- journeys of a local, national and/or international nature.

3.2.1. *Design conditions*

As emphasised in § 2.1, the field setting of a trial requires, in general, a relatively uncomplicated, yet unambiguous type of design. In view of this, an 'untreated control group design with pre-test and post-test' was decided upon.
Application of a design of this sort requires as components:
- The experimental group. Here the accident history of vehicles of a particular fleet is essentially studied by comparing relevant accident data over:
  - the pre-test period which is before intervention (the moment that data recorders were installed); and
  - the post-test period before which is after intervention.
- The control group. Over the same period, data were also gathered with respect to vehicles of a second fleet to enable adjustments of the effects of environmental variables on the outcome of the comparison.

It was required that the experimental and the control groups were comparable. In particular, vehicles in both groups:

- preferably needed to be driven, on average, in situations with about the same risks.
- had to be counterbalanced for differences in the number of kilometres driven, this being a measure of exposure to traffic hazard.

In view of this, pairs of fleets were matched on a set of relevant aspects, determined in advance during the methodology study. As reported, the selected aspects considered to be of particular relevance were:

- the vehicle type;
- the utilisation of the vehicles; and
- the conditions of their use.

In a field trial of the kind, dealing with one or a small number of large fleets is advantageous by far for purposes of analysing data, in particular because of the homogeneity of the subjects studied. We had to deal with a larger number of fleets of more different kinds and mostly of a smaller size than expected before. An aspect that, of course, complicated the logistics of the experiment as well. Consequently, the possibilities for conclusions concerning the individual fleets are much more restricted by now.
Moreover, differences in effects between the fleets will show a larger diversity. A diversity, however, which delivers some insight into how the recorder effect might be dispersed in practice among commercial fleets

In the present trial, finding suitable pairs of matched experimental and control groups was quite a substantial task. In some cases, an external control group (a separate fleet) has been found. Besides, an internal control group (consisting of vehicles from the same fleet) could mostly be created. When arrangements with an external control group could not be made because of, say, insufficient time, and when an appropriate internal control group was not achievable, a second-best type of solution was tried by establishing a 'semi-control' group. In two cases the possibility arose to create such a semi-control group. The vehicles of both groups belonged to the same fleet as those of the experimental group, but differences existed in either the type of vehicles or their use. Although they were not fully comparable, there were certainly meaningful common characteristics.
For instance, internal labour conditions, policies on safety, servicing, fuel consumption and traffic conditions were probably very similar. Information on such a semi-control group was at least useful as a check on changes in the general accident proneness and in the vehicle use of a fleet. In a single case, we had the opportunity of defining a semi-control group within the external control group. Its data were useful for the same purposes.

Arrangements for applying some procedure regarding the essential feedback to drivers were explicitly made with the managements of each fleet in which recorders were installed. The actual procedure itself was left to the fleet management involved. Suggestions were made how to administer this feedback, in particular with respect to the fact that the records were not necessarily to be solely used in a restrictive sense, but could also be applied as an aid in giving incentives to promote proper and beneficial behaviour.

As discussed in § 2.1.2, it would have been interesting to reveal potential differences of the impact on accident rates by means of feedback on a regular basis (making use of a journey data recorder) or by means of feedback in a occasional way (applying an accident reconstruction recorder). Unfortunately, the vast majority of the boxes in our trial concerned only one type, notably the accident reconstruction recorder.

The favoured 'before' and 'after' periods were chosen to last at least one year before the intervention and one year afterwards. One of the reasons for this choice was to allow for seasonal fluctuations in vehicle use and accident and/or incident occurrence. Another rationality lay in the statistical relation between the required amount of accident data, the sample size and the confidence level of results.

3.2.2. *Sample size and data gathering*

Besides the design of the trial and its execution in practice, the sample size was also a methodological aspect of importance.
In § 2.2.3, an annual accident involvement rate of vehicles in commercial fleets was estimated and based on the best available data, stemming from various sources of different countries and concerning diverse types of vehicles and vehicle use. On average, an involvement rate of about 0.3 accidents per vehicle per annum was assumed.

Of course, the potential impact of the use of data recorders on accident occurrence was unknown. However, for the sake of calculating a sample size large enough for assessing an effect of some kind, it was assumed that the number of accidents would diminish with an average value of 20%. Starting from these assumptions, the minimum numbers of vehicles in the experimental and control groups were calculated in order to be statistically able to assess a recorder effect on accident rates within certain limits of confidence.

In order to achieve a reasonable possibility of stating such a positive recorder effect within a 90% confidence level (α = 5% two-sided), this procedure resulted in the conclusion that:
- 500 or more vehicles equipped with a recorder would have to be involved; and
- at least, an equal number of vehicles without a recorder.

Moreover:
- the trial should be of at least one year duration before intervention, and
- one year after intervention.

It was appreciated that a smaller experimental group would have been sufficient if:
- the actual accident involvement rate was higher than estimated; and/or
- the control group was enlarged; and/or
- the influence of the recorder was more pronounced than assumed, and/or
- a lower level of confidence was accepted.

A smaller experimental group will be sufficient if:
- the actual accident involvement rate is higher than estimated, and/or
- the control group is enlarged; and/or

- the influence of the recorder is more pronounced than assumed; and/or
- a lower level of confidence is accepted.

As will be described in the next chapter, arrangements with 11 different fleets - involving circa 840 vehicles of which 270 equipped with recorders - were made for taking part in the Low Countries trial.

In assessing the impact of feedback on traffic accidents occurrence through the use of recorders in commercial fleets, obviously the attention was primarily focused on changes in accident rates over the field trial duration in the pre-test and post-test periods. For this purpose, information on the date of each accident in the fleet, and information on the mileages of each vehicle in the fleet were needed.
The use of recorders might partly result into accidents of lesser severity. Such a phenomenon could obscure the impact in terms of 'the number of accidents' only, but an effect like this would also be beneficial. In view of this, information directly or indirectly related to the severity of an accident, was of importance. For the latter purpose, accident details were to be collected with regard to traffic participants involved in the accident, the cost of the damage, and time and location of the accident.

Fleet managers and insurance companies are also interested in the damage cost from accidents and incidents not directly related to traffic participation. For instance, vehicles often sustain damage when manoeuvring on parking lots. For this purpose, data on such incidents are really necessary to give the full picture. Although this particular aspect of costs is out of the direct scope of the present study, the information offers, nevertheless, some insight into whether recorder usage can lead more generally to careful, and therefore safer, driver behaviour. Collecting such data promotes the proper use by our partners of the formal definition in The Netherlands of a 'traffic accident', as mentioned in § 2.1.2, and other damage.

During the actual information gathering, it turned out that for most fleets the total mileage of vehicles over the pre-test period could not be determined reliably. Among the experimental groups within the eleven fleets in this report, only one large fleet appeared to be an exception in this respect. In general, it meant not only that the requested information could but partly be gathered. Another consequence was that not all gathered information could be used. In particular, methods of more detailed analyses as mentioned in § 2.3.3 could not be applied here, because of the shortage of data on the precise dates of accidents and of the mileages of the vehicles at specific dates.

In case the vehicles in a fleet were of the articulated type or concerned lorries and trailers, there might be separate insurance policies applicable to the individual parts of the combination. Replacements for vehicles in a fleet appeared to be another source of confusion. Sometimes, the 'new' vehicle was consecutively added on the insurance policy of the former one resulting in the loss of specific detail relating to the operational history of a vehicle. Moreover, systematic registering of mileage data and/or keeping that information during some length of time appeared to be an uncommon practice. Other potential sources of information like for instance service station accounts were not available in this study.

Related to the topic of the replacement of vehicles in a fleet is the fact that the number of vehicles can vary over the time window investigated. In one case, the fleet was enlarged substantially by merging with another fleet.

In order to deal with this problem, time in terms of 'the number of months of vehicle-use' was chosen as an alternative measure of exposure. This measure was applied to the accident analyses of all fleets considered in this trial and reported below. Separately, the planned measure of exposure: 'distance driven', was also used in the analyses of the mentioned fleets for which such information was available.

The handling of data on damage cost was rather complicated. Damage cost often remained unknown when, in particular, a third party caused the accident or when repairs took place as a 'unbooked' management decision. Moreover, costs had to be weighted in relation to the nature of the accident. The majority of damages were of a limited cost. Some, however, required an extremely large amount of money, which disturbed the reliability of the portrayal. In order to deal with this, the cost trends over the time of only one fleet are covered in this report.

3.3. Characteristics of the fleets and vehicles in the trial

Eleven fleets took part in the present study. In total, more than 840 different vehicles were involved, of which 270 vehicles were equipped with a recorder. The equipped vehicles constituted seven experimental groups, each belonging to an individual fleet. The experimental groups have been matched with control groups. Four of these are external control groups, five are internal control groups and two are internal semi-control groups. There is also one external semi-control group.
An overview of the combinations of fleets, their size, the dates of installing the recorders and the period over which data are available, is given in *Table 6.*

The main characteristics of these fleets are described hereafter, each *clustered around the experimental group.*

Fleet A1
The fleet consists of about 160 vehicles of the types: heavy trucks and trucks with full- or semi-trailer. The fleet is owned by an international transport company located in Belgium, which operates nationally but the vehicles are mostly utilised for international transport. They are driven by permanent drivers during the working days of the week only, on roads inside and outside urban areas, and during daytime and nighttime.
Within the fleet, *an experimental group of 110 vehicles* could be selected at random. They have been equipped with Accident Data Recorders since January 1994.

Fleet A2
The remaining vehicles of the last-mentioned fleet were chosen for making up *an internal control group of about 50 non-equipped vehicles* of the same type and the same use as the experimental group.

Fleet A3
The fleet is the property of an international transport company, located in the Netherlands. It comprises a total number of 105 vehicles with basically the same characteristics as those within the fleet A1. *These 105 vehicles were chosen to act as an external control group.*

Cluster	Fleet	Fleet size	Installation date	Pre-test period	Post-test period
A	experim.	110	January '94	10/92-1/94	1/94-12/95
	int. ctrl.	50	-	id.	id.
	ext. ctrl.	105	-	10/92-1/94	1/94-10/94
B	experim.	25	July '94	1/89-7/94	7/94-7/96
	int. ctrl.	28	-	id.	id.
	Int. semi-ctrl.	160	-	1/91-7/94	id.
	ext. ctrl.	53	-	id.	id.
	ext. semi-ctrl.	23	-	id.	id.
C	experim.	25	February '95	7/93-2/95	2/95-2/96
	ext. ctrl. I	25	-	id.	id.
	ext. ctrl. II	25	-	id.	id.
D	experim.	54	March '95	8/93-3/95	3/95-3/96
	int. semi-ctrl.	66	-	id.	id.
E	experim.	23	December '94	1/94-12/94	12/94-1/96
	int. ctrl.	21	-	id.	id.
F	experim.	23	January '94	1/93-1/94	1/94-7/95
	int. ctrl.	9	-	id.	id.
G	experim.	10	April '94	11/93-5/94	5/94-4/95
	int. ctrl.	5	-	id.	id.

NOTE : The number of vehicles cannot always be mentioned exactly, for the size of fleets might change in time. It holds in particular for those fleets participating in the project over longer periods. The numbers of vehicle months however, are counted per vehicle over the period the vehicle was in use.

Table 6. *The experimental and control groups included in the study*

Fleet B1
The fleet is owned by a regional distributor of cattle food, located in the Netherlands. Cooperation with this company was obtained by mediation of an insurance company.
The segment of the fleet involved is composed of 53 heavy trucks.
The vehicles are not permanently driven by the same driver, but they are randomly allotted to drivers by the management. The trucks are used for regional transport in the Netherlands, mostly outside urban areas, during the regular working days of the week from 07.00 until 23.00 hours.
Within the segment, 25 trucks have been equipped with Accident Data Recorders since July 1994. *This group of 25 vehicles constitutes the experimental group.*

Fleet B2
The remaining part of the same segment of this fleet, *the other 28 non-equipped trucks, makes up the internal control group.*

Fleet B3
A separate segment of the latter fleet concerns a group of on average 160 vehicles of different types and seizes, mostly medium weight and heavy trucks. They are in use for distribution purposes, as well as for irregular deliveries of goods in general. *These 160 vehicles are considered as an (internal) semi-control group.*

Fleet B4
Another distributor of cattle food also collaborated in the study. A segment of their fleet is comparable with the B1/ B2 fleet with regard to vehicle type and use. It consists of *53 vehicles, to be considered as an external control group.*

Fleet B5
A separate segment of the B4-fleet is composed of vehicles of different medium weight lorries for general transport of goods. It consists of *23 vehicles, used here as an external semi-control group.*

Fleet C1
A Dutch touring operator collaborating in the trial by mediation of an insurance company, is the owner of several 'independent' fleets of coaches, each located in the Netherlands. The vehicles are employed by permanent drivers, for national, but mostly international travel and touring purposes. They are in use on all days of the week, during day-time and night-time conditions, on roads inside and outside urban areas.
In one of the fleets, 25 randomly selected coaches have been equipped with Journey Data Recorders since February 1995. *These 25 vehicles make up the experimental group.*

Fleet C2
In a second separate fleet of the touring company, 25 coaches of comparable use were selected of which the drivers received special instructions with regard to safety regulations, working hours, speed, etc. *These 25 vehicles act as a special external control group.*

Fleet C3
In a third fleet owned by the same tour operator, *25 vehicles were chosen to act as an external control group* on the basis of comparability in utilisation with the coaches of the former experimental and control groups.

Fleet D1
One of the partners in the trial concerns a company specialised in the private transport of single or small groups of persons, located in The Netherlands. The cooperation was subsidised by a regional authority.
A section of its fleet consists of 54 so-called street taxis in total, equipped with Accident Data Recorders since March 1995. All vehicles are passenger cars, employed locally inside a medium sized town, during 24 hours of all

days of the week. The vehicles are allotted more or less at random to the drivers. *These 54 vehicles act as the experimental group.*

Fleet D2
Another section of the latter fleet consists of 66 vans. They are utilised for the transport of small groups of persons in general, and in particular for the transport of handicapped and elderly people and for additional local ambulance services. *These 66 vehicles are considered as an internal semi-control group.*

Fleet E1
A Dutch insurance company participating in the study was willing to deliver accident data and exposure data of the company's passenger cars in use by its staff. The vehicles are employed by permanent drivers, mostly for business purposes during the working days and hours of the week, but also for private use. *The 23 cars equipped with Accident Reconstruction Recorders since January 1995, act as the experimental group.*

Fleet E2
The same insurance company provided also the data with respect to 21 other company's cars in use by its staff under comparable conditions. *These 21 non-equipped passenger cars make up an internal control group.*

Fleet F1
The fleet is owned by an international travel and touring company, located in Belgium. Its coaches, each driven by a permanent (couple of) driver(s), are used for national and international transport of passengers, during 24 hours of all days of the week., on roads inside and outside urban areas. *The 23 coaches equipped with Accident Data Recorders since January 1994 are considered as an experimental group.*

Fleet F2
The same travel and touring company cooperated in providing the relevant data with regard to nine other coaches of its fleet. *These nine coaches act as an internal control group.*

Fleet G1
The fleet regards street taxis, operating in one of the bigger cities of Belgium. Since April 1994, ten passenger car were equipped with an Accident Reconstruction Recorder. The cars are used more or less continuously by different drivers. *These ten cars constitute the experimental group.*

Fleet G2
From the same fleet of Belgian street taxis *five non-equipped cars act as an internal control group.*

4. Data analyses and results

4.1. General approach

In assessing the effect of the use of data recorders on accident occurrence, the following procedures were executed, step by step, in order to analyse the accident data.

In the *first step*, the experimental group of vehicles in which recorders were installed was studied by comparing the numbers of accidents during the pre-test and the post-test periods. For this purpose, the numbers were calibrated for differences in exposure to traffic hazard.
In that, two different measures were applied. With respect to all fleets, information on the number of months during which their vehicles were in use was at our disposal. For a single fleet, mileages were also available. The latter measure is more specific for traffic participation and therefore of greater significance here.
The results of the comparison constitute an estimate of the feedback effect, which was improved in the following step.

The *second* step of the analysis was required to produce an adjusted estimate of the feedback effect, which resulted in the most reliable statistical estimate for this study. During the pre-test and post-test periods, the overall accident proneness (tendency) might have independently developed from effects other than those directly attributable to recorder utilisation. For instance, a likely influence might have been caused by economical factors and their influence on traffic. Effects of such environmental developments were controlled by applying data from comparable groups of vehicles not affected by recorder use. For this purpose, the safety developments in the control group were effectively subtracted from those in the experimental group.

In principle, the feedback effect would have become well defined in cases where we were able to compare the safety situation of equipped vehicles with the situation where these vehicles were non-equipped. This being impossible, we tried to create the most analogous situation. For this, several assumptions were of importance. With the exception of the feedback effect itself, it was supposed that no other changes affecting the safety of the experimental group had taken place other than could be compensated by the control group data. Thus, also registering and gathering of safety data had to be pursued on the same level of reliability for both groups over the period of study. The data were considered to make up a non-truncated and non-selective set. Moreover, our starting point was that the experimental and control groups of vehicles had been allocated at random, as agreed with our partners in the trial.

In the following sections of this chapter, we focus firstly on the data analysis of the fleet for which mileage could be applied as a measure of exposure. Then, the results with time being the measure will be presented for all seven experimental groups, each summarised separately. Reporting of the results will be continued hereafter.

So far, traffic accidents and incidents have been chosen as the criterion for traffic safety. Developments in damage cost offer some additional insight into the severity of accidents. A separate section in this chapter is devoted to that topic.

4.2. Accident analyses per cluster

4.2.1. *Cluster A*

4.2.1.1. *Accident analyses of cluster A with mileage as measure of exposure*

In *Table 7a* below, the number of traffic accidents is given with respect to vehicles in the experimental and the internal control group of cluster A, as well as the related mileage data (in kilometres).
The pre-test period considered here lasted from the first of January 1993 until the end of December 1993 and the post-test period from the beginning of January 1994 until the end of that year. The data related to 104 different vehicles, each equipped with a black box on 1 January 1994. They were in use during both periods, albeit some of the vehicles not for the whole length of time. The internal control group consisted of 26 vehicles, for which the same kind of remark must be made.

Cluster A; mileage		Pre-test period	Post-test period
Experimental group	accidents	137	124
	mileage	12,864 * 103 km	14,158 * 103 km
	accident rate	10.65	8.76
Internal control group	accidents	25	34
	mileage	2,999 * 103 km	2,925 * 103 km
	accident rate	8.34	11 62

Table 7a. *Accident and mileage data of vehicles in the experimental and internal control groups of cluster A.*

As shown in the table above, the accident rate of the experimental group dropped after the intervention from 10.65 to 8.76 accidents per million kilometres, a reduction of about 18%.
At the same time, this rate increased in the control group from 8.34 to 11.62, or by 39%. Thus, in case of non-intervention the best estimate of the experimental group's accident rate in the post-test period would be 14.85 accidents per million kilometres.

Combining the results, the estimate of the intervention effect is calculated to be a reduction in accident proneness by 41%.

Applying standard statistical procedures - in this case the so-called Weighted Poisson Model - it has been calculated that the reduction by 41% is significant at the confidence level of better than 90%. The confidence interval around the reduction value of 41% amounts to plus or minus (±) 28%. That means that the reduction will be in reality at least 13%, as shown in *Table 7b*.

Cluster A; mileage	Adjusted risk level factor	Confidence interval
Result	0.59	0.31 - 0.87

Table 7b. *Adjusted intervention effect and its 90% confidence interval for cluster A, using mileage data with fully overlapping experimental and control periods.*

In addition to these results, it was noted that the mileage of the experimental group has increased by some 10% after the intervention. The submitted data were reliable, being read-outs of the mileage recorder of the vehicles. However, a growth of this size might have influenced the working conditions of the drivers.
Another relevant aspect might be the lower accident rate of the internal control group in the pre-test period. The value, however, is not beyond normal statistical dispersion.

Furthermore, it was observed that the actual yearly accident involvement rate amounted to 1.2. The value of 0.3 previously used to calculate the required sample size was based upon accident statistics, whereas we are now able to apply incident figures as well. Together with an effect in accident reduction two times larger than assumed before, the fourfold involvement rate explains why a significant result is already found for this smaller sample.

4.2.1.2. *Accident analyses of cluster A with vehicle-months as measure of exposure*

In *Table 8a* below, the number of traffic accidents of cluster A are given with respect to vehicles in the experimental group and the internal and external control groups, as well as the related numbers of vehicle months. The pre-test period lasted from 1-10-1992 until 31-12-1993. The post-test period here lasted for the experimental group and the internal control group from 1-1-1994 until 30-11-1995, and for the external control group from 1-1-1994 until 30-9-1994.
The data related to - on average - about 110 vehicles equipped with a recorder, 50 vehicles in the internal control group and 105 in the external one.

Table 8a also gives the selected data for the two cases in which the post-test periods of the experimental and control groups fully overlap.

Cluster A; vehicle months		Pre-test period	Post-test period		
			total	full time overlap	
				I	II
Experimental group	accidents	183	256	256	205
	veh. months	1,660	2,440	2,440	1,810
	risk factor	.110	.105	.105	.113
Internal control group	accidents	43	104	104	78
	veh. months	670	1,170	1,170	900
	risk factor	.064	.089	.089	.087
External control group	accidents	186	93	..	93
	veh. months	1,570	940	..	940
	risk factor	.118	099	..	.099

Table 8a. *Accident and vehicle months data of vehicles in the experimental group and the control groups of cluster A.*

As the data shows:

- The risk factor of the experimental group decreased after the intervention from 0.110 to 0.105 accidents per vehicle month for the case of all data, i.e. a reduction by circa 5%.
- Parallel to this, the risk factor decreased for the combined control groups by circa 9%, and for the external control group by 16%, whereas this factor increased for the internal control group by 39%.

With regard to the 9% decrease in risk for the combined control groups, we have to refer to the background of its calculation, given in § 2.3 3.
As argued there, an overall effect for the combined control groups cannot be calculated by summing-up the groups or by averaging their individual results in a straightforward way. Differences in fleet sizes have to be taken into account. For that purpose, each control group is assumed to produce an estimate of the overall safety developments for which the developments in the experimental group has to be compensated. Each estimate contributes in proportion to the estimate's reliability.
This method will be applied in all further calculations in which several control groups have to be combined.

When we adjusted for the safety development in the aggregated control groups, a negative intervention effect of 4% resulted, as shown in *Table 8b*. A rise in accident proneness of this size is not statistically significant. Its confidence interval at the 90% level amounted to ± 25%.

Adjusting for the safety developments in the external control group resulted in a negative effect of 14%. This incline in accident proneness is not statistically significant. Its confidence interval amounted to ± 30%.
It indicates a reduction in accidents of some 16% at its maximum.

When we adjusted for the safety development in the internal control group, a positive intervention effect of 31% was calculated. This reduction in accident proneness was statistically significant at the 90% confidence level, since its confidence interval amounts to ± 23%. It means an significant minimum effect of some 8% reduction in accidents.

From a statistical point of view, it can be noted that the confidence intervals of the control group results are showing overlap. Nevertheless, differences in between these control groups might be supposed. We are not able to define them in this study, although discrepancies in the observation period might play a role in this. For that reason, data with regard to the two cases of full time overlap of experimental and control periods were given in *Table 8a* as well.
As a general remark, we note that the greater precision in data goes together with an increased confidence interval, caused by the smaller amount of data.

In the case of '*full time overlap I*', see *Table 8b*, all data on the experimental group were considered. Therefore, the data on the external control group had to be omitted.
The results show a significant positive intervention effect of about 31%, mainly caused by a strongly increased risk in the internal control group.

Generally speaking, greater value has to be attached to the independent data of an external control group. In the case of '*full time overlap II*', all data of the external control group were considered. Therefore, data on the experimental group and on the internal control group were omitted in so far they concerned the post-test period longer than nine months. The results, see *Table 8b*, show a non-significant negative intervention safety effect of about 13%.

Cluster A; vehicle months	Adjusted risk level factor	Confidence interval
Total	1.04	0.79 - 1.28
Full time overlap I	0.69	0.45 - 0.92
Full time overlap II	1.13	0.85 - 1.40

Table 8b. *Adjusted intervention effect and its 90% confidence interval for cluster A in case of using all data, and in the two cases of using only the data from fully overlapping experimental and control periods.*

4.2.1.3. *Mileages and vehicle months as measure of exposure in cluster A*

In most traffic safety research, 'mileages' is applied as measure of exposure to traffic hazard. Such data were, however, only partly available in our study. So, 'vehicle months' was chosen as an alternative measure.
The question might arise in how far these two measures of exposure are related.

For that purpose, the correlation coefficient was calculated with regard to the 130 vehicles in cluster A of which both types of data were at disposal for a period of two years.
The coefficient turned out to be 0.93. Thus, the measures were highly exchangeable, provided the fleets' vehicles are in continuous use as was the case here.

Although a result like this is by far no proof for general validity, it offers at least evidence for a strong association between both variables.

4.2.2. *Cluster B*

The data on cluster B are summarised in *Table 9a* below.

Cluster B; vehicle months		Pre-test period		Post-test period
		total	full time overlap	
Experimental group	accidents	54	29	8
	veh. months	1,650	1,350	580
	risk factor	.033	.021	.014
Internal control group	accidents	47	36	17
	veh. months	1,850	1,510	690
	risk factor	.025	.024	.025
Internal semi-control group	accidents	121	121	53
	veh. months	6,930	6,930	3,640
	risk factor	.017	.017	.015
External control group	accidents	49	49	17
	veh. months	2,230	2,230	1,290
	risk factor	.022	.022	.013
External semi-control group	accidents	15	15	7
	veh. months	960	950	520
	risk factor	.016	.016	013

Table 9a. *Accident and vehicle months data of vehicles in the experimental group and the four different control groups of cluster B.*

The pre-test period started at 1-1-1989 for the experimental and the internal control groups and at 1-1-1991 for the internal semi-control, the external control and the external semi-control groups. The intervention date was 1-7-1994. The post-test period considered ended at 30-6-1996.
The data related to 25 vehicles equipped with a black box, 28 vehicles in the internal control group, on average about 160 in the internal semi-control group, 53 in the external control group and 23 in the external semi-control group.
Table 9a also gives the selected data for the two cases in which the pre-test periods of the experimental and control groups fully overlap.

As *Table 9a* shows:
- The risk factor of the experimental group decreased after the intervention from 0.033 to 0.014 accidents per vehicle month, a reduction by circa 58%. Obviously, this result is based upon a relatively small number of accidents in the post-test period.

- Parallel to this, the risk factor for the combined control groups decreased by circa 22%.
- For the combined external control groups this factor decreased from 0.020 to 0.013; a reduction by circa 36%.
- For the combined internal control groups the reduction amounted to circa 14%, as the risk factor decreased from 0.019 to 0.016.

The results for the aggregated control groups were based upon 232 accidents over 11,970 vehicle months during the pre-test period, resulting into an overall risk factor of 0.019, and upon 94 accidents over 6,140 vehicle months during the post-test period which gives an overall risk factor of 0.015.

Adjusting for the overall developments in the totalised control groups a positive intervention effect of 46% was stated. The reduction in accidents is statistically significant at the 90% level, as its confidence interval of ± 36% indicates. It means that an accident reduction of at least some 10% can be assigned to the feedback system, as *Table 9b* shows.

Cluster B, vehicle months	Adjusted risk level factor	Confidence interval
Total	0.54	0.18 - 0.90
Full time overlap	0.83	0.26 - 1.40

Table 9b. *Adjusted intervention effect and its 90% confidence interval for cluster B using all data, and using data from fully overlapping experimental and control periods.*

Adjusting for the safety developments in the combined external control groups only, does show a positive intervention effect of 34%. This effect is not significant, given a confidence interval of ± 49%.
Adjusting for the developments in the combined internal control groups shows a significant positive safety effect of 51% with a confidence interval of ± 33%.

In case of selection of data in which not only the experimental and control group's post-test periods fully overlap but also their pre-test periods, a risk level reduction was found of some 17% after the intervention.
The reduction is mainly caused by the decrease of risk in the experimental group. With its confidence interval of ± 57% the positive safety effect is however by far not significant. The same remark can be made about the effects resulting from adjustments for the safety developments in the combined external and in the combined internal control groups.

4.2.3. *Cluster C*

In *Table 10a*, below, which relates to cluster C, the pre-test period of all groups started at 16-7-1993. The intervention took place at 1-2-1995.
The post-test period considered ended at 31-1-1996. The data concerned 25 vehicles in each of the three groups.

Obviously, the numbers of accidents are rather small for calculating reliable statistical results. Therefore, the data of both external control groups were added up here. Addition is statistically permitted, for the groups are strongly similar and are balanced in sizes and levels of traffic accident risks.

As the data shows:

- The risk factor for the experimental group decreased between pre- and post-test periods from 0.046 to 0.023 accidents per vehicle month, it is by 50%.
- At the same time, the factor for the combined control groups decreased by 12%, from 0.023 to 0.020 on average.

Cluster C; vehicle months		Pre-test period	Post-test period
Experimental group	accidents	21	7
	veh. months	460	300
	risk factor	.046	.023
External control group with instruction	accidents	13	4
	veh. months	460	300
	risk factor	.028	.013
External control group	accidents	8	8
	veh. months	460	300
	risk factor	.017	.026

Table 10a. *Accident and vehicle months data of vehicles in the experimental group and the two external control groups of cluster C.*

If the effect in the experimental group is adjusted for the safety developments in the control groups, a positive intervention effect of circa 42% remains, as shown in *Table 10b*. The effect is not statistically significant, in particular because of the relatively small number of accidents in the experimental group during the post-test period. The confidence interval is ± 55%.

Cluster C; vehicle months	Adjusted risk level factor	Confidence interval
Total	0.58	0.04 - 1.13

Table 10b. *Adjusted intervention effect and its 90% confidence interval for cluster C.*

4.2.4. *Cluster D*

The data to be discussed now concerns cluster D, summarised in *Table 11a*. The pre-test period here began at 1-8-1993. The intervention date was

1-3-1995. The post-test period ended at 29-2-1996. The experimental group consisted of 54 vehicles, whereas 66 vehicles took part in the internal semi-control group.

Cluster D; vehicle months		Pre-test period	Post-test period
Experimental group	accidents	153	127
	veh. months	1,030	650
	risk factor	.149	.195
Internal semi-control group	accidents	61	33
	veh. months	1,250	790
	risk factor	.049	.042

Table 11a. *Accident and vehicle months data of vehicles in the experimental group and the internal semi-control group of cluster D.*

As the data shows:
- The risk factor of the experimental group increased from 0.149 to 0.195, it is by 31%.
- In the internal semi-control group this factor declined from 0.049 to 0.042, or by 14%.

Cluster D; vehicle months	Adjusted risk level factor	Confidence interval
Total	1.54	0.91 - 2.16

Table 11b. *Adjusted intervention effect and its 90% confidence interval for cluster D.*

Combining these figures, a nett result was calculated of an increase in risk by 54%. As its confidence interval is ± 76%, so the increase is not significant at the 90% level of statistical confidence; see *Table 11b*.

Importantly, a remark has to be made here on the safety records of this fleet. Compared with the other fleets in the study, the risk level of the experimental group: 0.15 à 0.20 accidents per vehicle month, is 'substantial'. In that, one has to realise that the group concerns a fleet of street taxi's, intensively used, mostly within built-up areas, and thus highly exposed to accident risk.
However, compared with fleets of the same kind, viz. street taxi's, its safety level can be considered as 'good' (or even 'very good'), as evidenced in for instance the low insurance premiums of the fleet. Obviously, this is a result of the company's safety policy, which among others involves reasonable work schedules taking into account driver's age, regular breaks per day as well as over the weeks, and incentives for safe and comfortable driving. In fact, applying data recorders in this fleet, can be considered as a further element in the company's safety policy. It goes without saying that higher safety levels are the more difficult to improve.

4.2.5. *Cluster E*

The data of *Table 12a* concerns the smaller cluster E, for which the pre-test period began at 1-1-1994. Black boxes were installed in the vehicles of the experimental group at 16-12-1994. The post-test period started then and continued until 31-12-1995.
The data related to 23 vehicles in the experimental group and 21 in the external control group.

Cluster E; vehicle months		Pre-test period	Post-test period
Experimental group	accidents	14	14
	veh. months	250	280
	risk factor	.056	.050
External control group	accidents	12	12
	veh. months	220	240
	risk factor	.055	.050

Table 12a. *Accident and vehicle months data of vehicles in the experimental group and the external control group of cluster E.*

As the data shows:
- The risk factor decreased after intervention from 0.056 to 0.050; it is by some 11% for the experimental group.
- For the control group it decreased from 0.055 to 0.050, or circa 8%.

Adjusting for the safety developments in the control group a positive black box effect of about 3% is calculated. The effect, however, is by far not statistically significant since the confidence interval amounts to over 100%; see *Table 12b*.

Cluster E; vehicle months	Adjusted risk level factor	Confidence interval
Total	0.97	0.08 - 1.87

Table 12b. *Adjusted intervention effect and its 90% confidence interval for cluster E.*

4.2.6. *Cluster F*

The pre-test period of the rather small cluster F, see *Table 13a*, lasted from 1-1-1993 until 16-1-1994, when the black boxes were installed in the experimental group. The post-test period ended at 30-6-1995.
The experimental group consisted of 23 vehicles and the internal control group of nine vehicles.

Cluster F; vehicle months		Pre-test period	Post-test period
Experimental group	accidents	19	21
	veh. months	280	400
	risk factor	.068	052
Internal control group	accidents	2	8
	veh. months	110	160
	risk factor	.018	.050

Table 13a. *Accident and vehicle months data of vehicles in the experimental group and the internal control group of cluster F.*

From a statistical point of view, the reliability of the results for this single cluster are limited due to the rather small numbers of accidents and vehicle months, in particular the numbers involved in the control group.

The data in *Table 13a* shows that the risk factor decreased between the pre- and post-test periods from 0.068 to 0.052 accidents per vehicle month for the experimental group. This factor increased dramatically from 0.018 to 0.050 for the control group. The risk factor for the control group in the pre-test period seems to be underestimated by far, obviously for no other reason than a statistical one.

Cluster F; vehicle months	Adjusted risk level factor	Confidence interval
Total	0.28	0 - 0.68

Table 13b. *Adjusted intervention effect and its 90% confidence interval for cluster F.*

Therefore, too much value may not be attributed to the calculated adjusted positive safety effect of circa 70% with a confidence interval of about ± 50%; see *Table 13b*.

4.2.7. *Cluster G*

Finally, the data regarding cluster G has to be discussed, see *Table 14*. The pre-test period lasted here from 1-11-1993 until 31-3-1994. In the experimental group the black boxes were installed at 16-4-1994. The post-test period started then and ended at 31-3-1995. The experimental group consisted of ten vehicles, the internal control group of five vehicles.

Cluster G; vehicle months		Pre-test period	Post-test period
Experimental group	accidents	11	31
	veh. months	50	120
	risk factor	.220	.258
Internal control group	accidents	0	4
	veh. months	25	60
	risk factor	< .020	.067

Table 14. *Accident and vehicle months data of vehicles in the experimental group and the internal control group of cluster G.*

From a statistical point of view, the numbers of accidents and vehicle months, especially for the control group, were too small to get reliable results from this single cluster. Because of zero accidents, it was in fact not well possible to perform appropriate statistical calculations.

4.3. Severity of accidents

As discussed before, it is not only the number of the accidents which are important to traffic safety, but also their severity. Accidents might be classified in a severity scale, ranging from light incidents to calamities. The operationalisation of the scale is complicated. Besides, a major problem involves getting the detailed information on accidents required for classifying them. Anyhow, given the relatively small numbers of accidents reported so far, we are not able to analyse accidents in terms of a formal classification structure.

Considering those vehicles in cluster A of which mileage data are available, we found that 74 accidents out of a total of 320 did not occur on public roads, but on parking lots, private or industrial areas and the like. In 106 cases it was unknown whether they should have been considered as a traffic accident in the proper sense. This alone already meant a problem in classification.
Influences of the accident severity on the distribution of accidents over built-up and non-built-up areas were not found, albeit that the distinct vehicle mileages were not known.

Instead of classifying accidents, a way to deal with accident severity is weighting the accidents by their cost.
With respect to the just mentioned vehicles of cluster A, *Table 15* shows the total amount of damage cost of the accidents in which vehicles of the experimental and the internal control groups were involved during the mentioned pre-test and post-test periods.
Here also, completeness was a point of concern. Fleet owners and insurance companies were primarily interested in their contribution to the cost.
The third party costs generally remained unknown.

Five accidents were not included in this table because their cost were exceptionally high, each amounting to more than 0 5 million Bfrs., whereas the average cost turned out to be about 0.065 million Bfrs. per accident.

Besides, 97 accidents were not included, because their damage costs remained unknown. An explanation might be that the damage was to a third party who was held responsible for the accident.

Damage costs in Bfrs. Cluster A; mileage		Pre-test period (1 year)	Post-test period (1 year)
Experimental group	total costs	$7.02 * 10^6$	$5.08 * 10^6$
	cost per accident	$073 * 10^6$	$.056 * 10^6$
	cost per kilometre	.55	.36
Internal control group	total costs	$.88 * 10^6$	$1.55 * 10^6$
	cost per accident	$.073 * 10^6$	$.062 * 10^6$
	cost per kilometre	.29	.53

Table 15. *Damage cost of accidents concerning those vehicles in fleet A1 and A2 of which mileage data are available.*

As shown in *Table 15*, the average cost per accident is 73,000 Bfrs.
It decreased in the experimental group to the level of 56,000 Bfrs and in the control group to the level of 62,000 Bfrs. This resulted in an adjusted reduction in average cost per accident of 10% in the experimental group.

As found in § 4.2.1.1, the number of accidents in the experimental group fell after the intervention. Combined with the reduction in average cost per accident, the accident cost per vehicle kilometre decrease from 0.55 Bfrs. to 0.36 Bfrs. By the same token, the cost in the control group increase from 0.29 Bfrs. to 0.53 Bfrs.
Thus, adjusted for the developments in the control group, a reduction in cost per kilometre by 40% has been eventually established in the experimental group of this fleet.

5. Discussion of the results

To start with, it seems worthwhile to recapitulate shortly the line of research in this study.

As an operationalisation of the general objective of the study, the research effort was directed upon assessing *the effect on accident rates and/or the accident severity* of applying feedback on drivers on the basis of their driving behaviour monitored by in-vehicle data recorders (§ 1.2).
Within the context of the preferred methodology and design of such accident occurrence study (§ 2.1), a distinct sample of vehicles in the experimental and control groups was required in order to meet, among others, methodological and statistical criteria (§ 2.2).
Some research requirements (§ 2.3) could not be realised in practice. This regarded particularly the sample size and availability of external control groups.
In view of this, the design of the trial was adapted (§ 3.2).
Eventually, the trial involved seven clusters of fleets, widely varying in kind and characteristics (§ 3.3).
The effects of the intended recorder use on the accident rate of each of the seven clusters were extensively analysed (§ 4.2). In the case of only one cluster, the effect on accident severity could also be studied (§ 4.3).

The first conclusion from the analyses per cluster is of course that *a statistically significant accident reduction could already be stated for some of the clusters*. This result is the more remarkable for we were dealing with very small sample sizes in comparison with the required ones.

At the same time, the results of the analyses showed *a wide divergence in feedback effects among the clusters*, ranging from a reduction in accident proneness to even an increase, albeit not-significant. They also showed *large intervals of confidence*. These outcomes are summarised in *Table 16*, next page.

So far, the different results have been presented separately and independently. Indications of maximal individual effects remain worthwhile, for they offer some insight into the potency of applying the feedback mechanism.
At this point, however, the question arises whether *an overall result* could be determined *for this particular set of seven clusters as a whole*.
The best estimate of such an overall result ought to show *the smallest possible interval of confidence*.
The rest of this chapter is devoted to that subject.

Related to a discussion in § 4.2.1.2, an overall effect for the set of clusters cannot be calculated here by averaging individual results, for the number of data have to be balanced in order to avoid influences of differences in size. Moreover, the set-up in pairs of experimental and control groups would be overlooked by then.
In the applied statistical method, each cluster pair is assumed to produce an independent estimate of a common effect on accident occurrence. Each estimate contributes to this common effect with a value that is in proportion to the estimate's reliability. This reliability is the inverse variance of the

estimate as calculated on the basis of the assumed Poisson distribution of the numbers of accidents. The calculated values are included in *Table 16*.

Summary of result data; vehicle months	Adjusted risk level factor	Confidence interval	Contribution to overall result
Cluster A total	1.04	0.79 - 1.28	57%
full time overlap I	0 69	0 45 - 0 92	[53%]
full time overlap II	1.13	0.85 - 1 40	[56%]
Cluster B	0.54	0.18 - 0.90	14%
full time overlap I=II	0 83	0.26 - 1.40	[11%]
Cluster C	0.58	0.04 - 1.13	6%
Cluster D	1 54	0.91 - 2.16	13%
Cluster E	0.97	0.08 - 1.87	4%
Cluster F	0.28	0 - 0.68	6%
Cluster G	..	..	0%

NOTE The contribution percentages for the cases of full overlap are only partly given and placed within brackets [ ].
Cluster A with time overlap I means without the external control group data.
Cluster A with time overlap II means without 14 months post-test data of the experimental and of the internal control groups.
Cluster B with time overlap I=II means, in both cases of overlap in cluster A, without two early years pre-test data of the experimental and of the internal control group.

Table 16. *Estimated effects per cluster, confidence intervals of these effects at the 90 % confidence level and contributions per cluster to the overall result.*

As shown in *Table 16*, cluster A is contributing to the overall result by more than 50%. In such a case of an overall result being that much dependent on a single result, gaining insight into the relative influence of the remaining clusters is advisable. For this purpose, an overall sub-result for the aggregated clusters B up to and including F was computed, as given in *Table 17*, next page.

The sub-results for clusters B up to and including F shows an accident reduction of 39% in case all data were used. The reduction is calculated to be 31% in case only those data of cluster B are involved which have full time overlap for the experimental and control periods for this cluster. Both results are significant at the 90% level of confidence, as can be seen from *Table 17*.

Overall result data; vehicle months	Mean effect on risk	Confidence interval		
Cluster A total	+ 4%	- 21%	to	+ 28%
full time overlap I	- 31%	- 55%	to	- 8%
full time overlap II	+ 13%	- 15%	to	+ 40%
Clusters B to F	- 39%	- 60%	to	- 17%
full time overlap I=II	- 31%	-56%	to	- 7%
Overall result	- 20%	- 36%	to	- 4%
full time overlap I	- 31%	- 48%	to	- 14%
full time overlap II	- 12%	- 30%	to	+ 7%
NOTE : - Cluster G does not contribute to the overall effect as may be seen from Table 10 - Time overlaps are defined as above.				

Table 17. *Overall effects for cluster A, for the combination of clusters B to F, and for the combination of all clusters, with their 90%'s level of confidence intervals.*

The sub-results for clusters B up to and including F contrast with the results for cluster A, in particular in the case where the external control group is taken into account. However, as already pointed out with reference to *Table 8a*, the positive safety effect of cluster A in case the external control group was neglected for reasons of time overlap, is mainly due to a high increase in risk in the internal control group, and not so much to the smaller risk decrease in the experimental group.
Thus, examining both sub-results, there seems to exist a difference in between them, albeit that much variation exists among all fleets.

The overall results in *Table 17* are visualised in *Figure 2*.

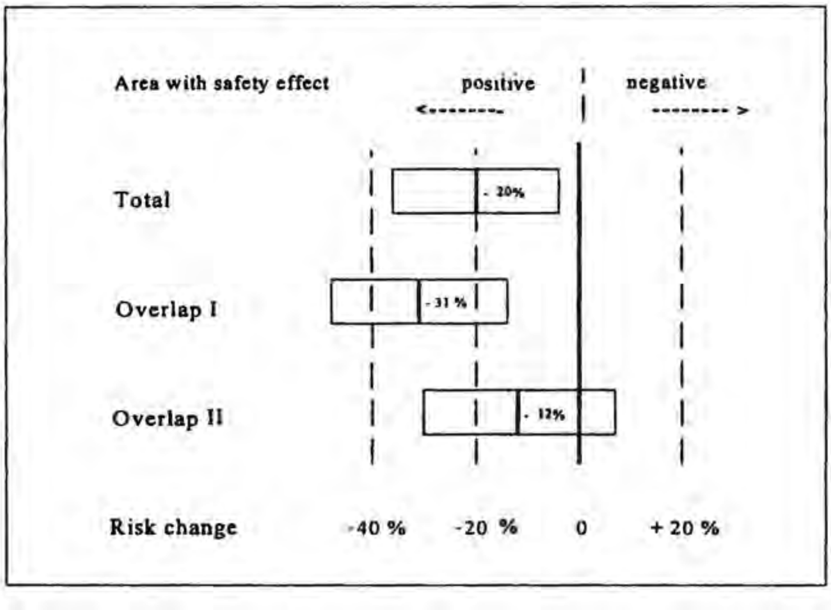

Figure 2. *Overall results concerning the fleets in the trial.*

As can be seen:

- The overall accident rate in the experimental groups, adjusted for the safety developments in the non-intervention control groups, is reduced with 20% after the intervention. This decline is significant at the 90% level of confidence.
- An overall result more reliable in methodological sense, ought to be based on data with full overlap of the pre-test and post-test periods for the experimental and their matching control groups. In that case the accident data of the external control fleet of cluster A have to be omitted. The estimated adjusted intervention effect amounts now to a significant reduction in accident level of 31% .
- If, alternatively, the post-test period of the experimental and internal control groups of cluster A was shortened for the non-overlapping 14 months, the accident reduction amounts to a non-significant 12%.

Additionally a remark on the last two cases: the non-overlapping 24 months of the pre-test period of the experimental and the internal control groups of cluster B were omitted here as well for obvious reasons.

6. Conclusions and recommendations

A leading motive of this research project was to improve traffic safety by improving safety related behaviour of the drivers.

Monitoring behaviour offers a possibility for influencing it.
It was the basic assumption in the project that the potential influence on an employee's driver behaviour by a monitoring device emanates from the driver's response to the possibility of being confronted with his recorded behaviour by others, including possibly his employer, and that this mechanism could be used for evoking and enhancing safer driving behaviour in traffic.
So, the response to this feedback was the main subject of interest in the project. Its existence must eventually reduce the accident rates and/or the severity of accidents in a fleet fitted with such monitors or vehicle data recorders.
In this context, the prime objective of the field trial was to appraise the *effect* of the driver's response to the feedback *on accident occurrence*.
In that, it should be noted that the feedback actually provided by the responsible management has not been studied, nor how this feedback was experienced by the driver. Furthermore, one has to realise that demonstrating an effect on the accident records of some particular fleets in this study does not comprise sufficient information for an overall assessment of the potential accident reduction. For, the factual levels of accident proneness among fleets, as well as the risk levels in fleets of different branches of transport, diverge too much for such a purpose.

The trial in the Low Countries was designed along the lines of an untreated control group design with pre-test and post-test phases. Its sample size and other requirements were determined during the study of the preferred methodology. The field trial comprised a major logistic operation.
Besides that, the voluntary cooperation of different partners had to be sought, as well as the greater part of the trial's funding. In order to meet scientific standards of validity and reliability of the trial and its data gathering, not only a strict data gathering format was necessary, but also many direct contacts with the participating organisations. In that, their geographical spread was a considerable handicap.
Eventually, some methodological requirements could not be met at full.
It regards especially a much smaller sample size than previously assessed.

Some 840 vehicles took part in the present study, of which 270 were equipped with a vehicle data recorder. The latter vehicles constituted seven experimental groups, for which twelve control groups could be matched.
It meant that a relatively large number of small fleets and parts of fleets were involved in the trial. These seven clusters of fleets differed widely among each other in character and vehicle use.

A statistically significant accident reduction by monitoring driving behaviour with the result of it the driver might be confronted, could be concluded for some particular fleets.

These results could only be given *within rather wide intervals of confidence.*
Obviously, this divergence stems partly from a pure statistical source and is caused by the small sample sizes. Influences like the fleet's accident record and the way feedback was applied might play a role too.

As could be asserted for the only fleet of which the relevant data were available, the result was coupled with *a beneficial development in damage cost.*

This being the case, it is worthwhile the more to continue the study on the larger scale originally aimed at.
In that way the outcome can be determined more precisely by confining the confidence intervals.
Apart from this, large variations in effects apparently occur among the different fleets. Thus, in further research, the insight into the variations of the effects among the fleets will be enlarged as well. It will offer opportunities for optimising a monitoring effect.
In this respect it would be of particular interest to examine influences of the implemented feedback. In practice, they might have originated for instance from applying incentive schemes by the fleet management, or from regular or occasional feedback in accordance with the type of the installed recorder. In that, it is recommended to study the feedback actually provided by the management and how this feedback is experienced by the drivers.
The way feedback is implemented is also of importance for the long-term effectiveness of the measure. One of the methods for diminishing possible introduction effects and to make safety an habitational aspect of driving might be the use of regular feedback. In that respect the journey data recorder seems to be preferable, because it gives beforehand cause to regular monitoring.

As already pointed out, the individual outcomes in the study cannot be utilised for a kind of meta-analysis to provide an estimate of a general recorder effect on accident occurrence.
Instead, we attempted to assess *an overall result for the particular set of the seven clusters of cooperating fleets.*
The use of data recorders in these fleets resulted *in an estimated accident reduction of some 20%.*
The margin in this value of about ± 15% shows that bigger as well as smaller results can be reached, partly dependent on the prior level of the fleet's safety record.

Finally, it is emphasised that the methodology applied here proved to be appropriate to demonstrate effects on accident occurrence.
More generally, *it might be concluded that the methodology - being one of the results of the study - is applicable for stating safety effects of the use of virtually any in-car system that influences driving behaviour.*

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Appendix

SAMOVAR 1A fleet NAME OF THE EXPERIMENTAL FLEET:

type of vehicle in the experimental group [1]	number of vehicles	have vehicles a permanent driver ?	are vehicles mostly in use for international transport ?	if not, are they mostly used within urban areas ?	are they only used on working-days ?	are they only used during day-time ?	remarks

SAMOVAR 1B fleet NAME OF THE CONTROL FLEET:

type of vehicle in the experimental group [1]	number of vehicles	have vehicles a permanent driver ?	are vehicles mostly in use for international transport ?	if not, are they mostly used within urban areas ?	are they only used on working-days ?	are they only used during day-time ?	remarks

[1] passenger car code = 1
van (up to 3½ t) code = 2
light truck (3½ - 7½ t) code = 3
heavy truck (over 7½ t) code = 4
truck with full trailer/semi-trailer code = 5
bus/coach code = 6

SAMOVAR 2A vehicles NAME OF THE EXPERIMENTAL FLEET: Starting date [1]: Closing date [2]:

number plate vehicle in experiment al group	average annual mileage	date of putting vehicle into use	mileage when putting vehicle into use	mileage 1½ years before installation of Black Box	date of installation of Black Box	mileage at moment of installation of Black Box	date of putting vehicle out of use	mileage when putting vehicle out of use	mileage 1 year after installation of Black Box	remarks

[1] 1½ years before installation of Black Box

[2] 1 year after installation of Black Box

SAMOVAR 2B vehicles NAME OF THE CONTROL FLEET: Starting date [1]: Closing date [2]:

number plate vehicle in control group	average annual mileage	date of putting vehicle into use	mileage when putting vehicle into use	mileage 1½ years before installation [3] Black Boxes	date of putting vehicle out of use	mileage when putting vehicle out of use	mileage 1 year after installation of Black Box	remarks

[1] 1½ years before, and: [2] 1 year after installation of Black Boxes, i.e.: [3] in the experimental group

SAMOVAR 3A traffic accidents NAME OF THE EXPERIMENTAL FLEET:

number plate of the vehicle	date of accident	time of accident	did the accident happen within the urban area ?	mileage at the time of the accident	were other moving vehicles, bicyclists or pedestrians involved ?	did injuries occur ?	cost of own damage	is the damage cost recoverable ?	cost of damage to opposite side	remarks

SAMOVAR 3B traffic accidents NAME OF THE CONTROL FLEET:

number plate of the vehicle	date of accident	time of accident	did the accident happen within the urban area?	mileage at the time of the accident	were other moving vehicles, bicyclists or pedestrians involved ?	did injuries occur ?	cost of own damage	is the damage cost recoverable ?	cost of damage to opposite side	remarks

SAMOVAR 4A other cases with damage NAME OF THE EXPERIMENTAL FLEET:

number plate of the vehicle	date of case with damage	mileage at time of case	cost of own damage	is the damage cost recoverable?	cost of damage to opposite side	remarks

PLEASE NOTE. For a traffic accident it is necessary that it happens on a public road and that at least one of the vehicles involved was taking part in traffic (parking damages and cases on business/industry sites are generally no traffic accidents).

SAMOVAR 4B other cases with damage NAME OF THE CONTROL FLEET:

number plate of the vehicle	date of case with damage	mileage at time of case	cost of own damage	is the damage cost recoverable?	cost of damage to opposite side	remarks

PLEASE NOTE. For a traffic accident it is necessary that it happens on a public road and that at least one of the vehicles involved was taking part in traffic (parking damages and cases on business/industry sites are generally no traffic accidents).