Methodologies for evaluating usage of vehicle data recorders

D3-Report prepared for DRIVE Project V 2007 Safety Assessment Monitoring On-vehicle with Automatic Recording (SAMOVAR)

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1. Introduction

Human behaviour is an important factor in traffic safety. If drivers were able to behave according to the necessities of the actual traffic conditions, no doubt road safety would be a less serious problem.
This does not mean that all traffic accidents would then be avoidable, nor that it would be possible to perform or even to discern a proper way of behaving in all circumstances. On the other hand, it is still 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.

Therefore, driver attitudes and behaviour are undoubtably a 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 safe also behaviour). It is, however, in general not easy to address drivers individually and to find instruments of feedback and support.

Monitoring behaviour also offers a possibility for influencing it, in particular when the driver is confronted with his own behaviour. Making use of this (feedback) mechanism is in fact at the basis of the SAMOVAR-project.

The latter opportunity is especially worthwhile to fleet owners and insurance companies in developing and maintaining a safety policy. Information about vehicle movements and traffic incidents can be gathered by in-car recording devices, or 'black boxes'. To some extent, it might be applied for pointing out safety aspects to their drivers.

The final objective of the SAMOVAR-project is to study the opportunities for improving traffic safety by confronting the driver with his monitored and recorded behaviour, making use of in-car electronic recording devices. The underlying assumption then is that drivers will act in a safer way, for instance by adapting driving speed to traffic circumstances, being aware of the fact that their behaviour is registered - also just before and after an accident - and will be acted upon if necessary, by or on behalf of the employer.

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 from what exactly they emanate, whether they could possibly be enhanced, or even if there will be positive effects in all circumstances. Nevertheless, it is obvious that drivers can change their behaviour in such a way that they seem to be involved - on the average - less often, at least, in incidents.

A quasi-experimental field trial in which such subjects are to be investigated, has been planned within the SAMOVAR-project. Its objective is to determine the influence of the above described utilization of black boxes on traffic safety. Directly, and of prime interest, safety improvement will be assessed in terms of fewer accidents and/or less severe accidents. Indirectly, changes in driver behaviour related to traffic safety will be measured, in particular with respect to driving speed.

The feasibility of such an experiment has already been established. As reported in the D2-document, the feasability study addressed topics such as the suitability of different types of recorders, the cooperation with fleet-owners and insurance companies, and the accessibility of the necessary accident and exposure data.

This D3-document deals with the design of the trial, its methodology and the way behavioural and accident data can be analysed.

2. The accident occurrence study

2.1. Design of the field trial

2.1.1. *Basic approach*

The objective of the field trial is to assess the change in accident occurrence when drivers are or might be confronted with their driving behaviour, as monitored and recorded by an in-car black box or vehicle data recorder (VDR).

In order to assess such an effect on the amount (and/or severity) of accidents, the field setting of the trial asks for 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 pretest and a posttest phase: a so called '*untreated control group design with pretest and posttest*'.
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 VDRs, 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 VDR 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 searched effect of the use of VDRs 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 effect of the VDRs, whereas it is actually caused by the fact that the vehicles with a VDR 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 inhomogeneity, if of the same kind and to the same extend.
Apart from the VDR 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 continuously during the entire trial; and
- the *intervention* or stimulus should not affect the control group.

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 case of our trial, the subjects are in fact vehicles, whether fitted with VDRs 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 VDR-equipped or non-equipped vehicle.

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.

The necessity of having accident data available concerning the pre- and the posttest period of the trial, will result in practice 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 is even 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 the 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' presents a practical solution for these difficulties.

It is a principle of the SAMOVAR-project that the potential influence of a driver monitoring device emanates from how the driver responds to being monitored and confronted with his driving behaviour. Thus, feedback is an essential element. Given the nature of different types of VDRs, there

exists 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 black boxes in the vehicles cannot be regarded as an appropriate intervention or stimulus in the field trial.

In the case where the VDR is an accident data recorder (ADR), its 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 is unlikely that within the field study facts indicating guild will be used in a juridical context. However, it is 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 to be informed beforehand that the data of every recorded accident will be examined by the responsible management of the company and that internal action will be taken if they are to blame. Obviously the fleet owner has to be committed to this course of action.

The journey data recorder (JDR) constitutes another type of VDR. It 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; the last being the period before a collision or a police check.
Analogous to the utilization 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. Speed limit violations and their durations can easily be traced since specific types of recorders can be programmed for those aims. By telling the drivers that information of the kind will be used to monitor their 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.

Summing up, if the fitting of VDRs 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 utilization of the information from the device by the company's management. This remark is of course 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 in particular relevant for smaller companies. In that case a solution might be to supply all vehicles of that fleet with a VDR 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.

In the present trial it is expected that several, smaller fleets will be involved. A company might participate because it is safety-minded, (resulting in a smaller reduction in accidents than average), or because it has to deal with a safety problem (any measure will be 'helpful'). The only remedy for the moment is to obtain some insight in 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 as well.

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 VDR. 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 judged in looking for the effect of the intended use of black boxes.
Obviously, the bigger the difference in the number of accidents in applying black boxes or not, the smaller the likelihood it is merely caused by statistical variation. And also, the more accidents the less the relative fluctuation. For example, instead of two accidents equally well one or three can be stated; evidently, 50 or 150 accidents instead of 100 is less likely.

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

For the statistical origin of the criterion variable might jeopardize 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 black box effect on traffic safety.

Neither of these verdicts can ever be proven exactly; they can only be highly probable at best. In practice, this will be sufficient.
Obviously, data have to be analysed in a way that there is little chance of erroneously concluding a positive black box effect. The main key to deal with the kind of problems lies in choosing an appropriate sample size for the trial, starting from a good design as discussed earlier.

Black box effect according to experiment	Conclusion from experiment if black box effect in reality is:	
	positive	none
positive	true	false *type I error*
none	false *type II error*	true

Diagram 1. *Truth table for the black box trial.*

2.2.2. *Traffic safety effect of black boxes*

In the selected design the accident involvement of vehicles with and without a VDR is compared during a pre- and a posttest. As indicated before, minor differences in accident involvement could only be due to statistical variation. Besides, they are not really relevant in practice.
An appropriate sample size of the experiment does, however, not solely depend 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 black box is of importance as well.

Assuming a distinct black box 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 too.

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 VDR 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 black box in case of a positive black box effect.
This way 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 black box. Within a certain reliability, a black box effect can be stated then.

So, the test statistics can be used for estimating a sample size appropriate for stating real effects of the black box. 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 black box effect are known. For instance, reductions in accidents of 30% are mentioned by some manufacturers of VDR's. 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 black box will be installed.
Therefore, a first guess of a 15% 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 was 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 2.5 accidents on average per year per vehicle were reported, as presented in Table 1.

Type of vehicle	Number of vehicles in fleet	Annual km per vehicle	Number of *) accidents	Annual involvement	Risk per million veh.km
> 7.5 ton	56	120,000	50	0.5	5
	72	100,000	17	0.4	4
	37	90,000	123	2.4	27
	90	85,000	215	0.5	6
	36	60,000	7	0.2	3
Van etc.	33	120,000	53	0.2	2
	17	40,000	30	1.0	24
	240	30,000	73	0.3	10
	76	30,000	22	0.3	10
Car	55	30,000	19	0.4	11

*) *The numbers concern different lengths of observation time, during which black boxes were installed in at least part of the fleet.*

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

Considering these data, it is obvious that the differences are real and cannot be a result of chance only. 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 by, for instance, the kind of transport, traffic and driving conditions, the safety 'climate' in a company, etc. This fact does not necessarily imply that in a future experiment safety effects of black boxes will differ likewise. Even in case of stated differences, the total result is still showing yet an overall black box effect for the fleets in the study.
And although there are surely differences within the fleets themselves, stating incorrectly a black box effect may be avoided, as mentioned earlier, by properly selecting vehicles with and without a black box within the 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.
On this point, it is noticed that many times 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.

Type of vehicle	Number[1)] of vehicles * 1000	Annual[2)] km per vehicle	Number[3)] of veh. in accidents	Annual involve-ment	Risk per million veh.km
> 7.5 ton	125	48,000	2,301	0.02	0.4
Van etc	418	19,000	3,812	0.01	0.5
Car	5,509	15,000	39,027	0.01	0.5

[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.*

It has to be notified 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. As such 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 acci-dents' have also to 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, only regarding fatalities, hospitalised injuries and slightly injured persons). Correspondingly, involvement rates are likely to be 10 or more times as large, and thus seem to match the situation of the pilot study (S. Harris, SWOV internal note, 1992).

Summarising, figures seem to show that on average one has to rely on an involvement rate of 0.2 at least, but by way of precaution a larger value in further calculations cannot be recommended.

2.2.4. *Required number of accidents in the trial*

Within the simplified model described before, the traffic safety effect of black boxes will be derived by comparing the number of accidents observed among the vehicles with and without a black box.
In the trial these numbers can accidentally take on a range of values around the proper averages for the given traffic safety conditions; averages, which are related to each other by the black box effect. 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 black box effect.

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

Although both errors have to be avoided, particularly an error of the first kind is 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 convenient to choose a value of 5% for α. It implies that 'a positive black box effect while there is none' is accepted in less than 1 out of 20 experiments.

Further limiting the critical range of the test statistic will result in enlarging the minimum in 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 β of making an error of the second kind has to be accepted. It is convenient to choose a value of 20% for β. The power $(1-\beta)$ 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 factual black box 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 be discussed firstly.

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. Especially, the groups of vehicles to be compared in the trial are supposed to have the same size and the same annual mileage. It is also assumed that the data can be gathered over a period of one year of black box use.

The mathematical expression for the minimum numbers of vehicles needed in the trial is given hereafter.

The expression contains the following abbreviations:
- 'N' is the minimum number of non-black box vehicles to be observed in the trial,
- 'inv' is the annual accident involvement rate of these vehicles, and
- 'eff' the black box effect, so that the involvement rate with black box is a factor (1-eff) times the involvement rate without.

Furthermore let the chances of errors of types I and II be α and β, 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 β.

The minimum number of vehicles is given now by next expression:
$N \geq (z_\alpha \sqrt{a} + z_\beta \sqrt{b})^2 / (eff * inv)^2 + 2 / (eff * inv)$
with
$a = ½ inv * (2-eff) * (2 - inv*(2-eff))$
$b = inv * ((1-inv) + (1-eff) * (1 - inv*(1-eff)))$
being the term $2 / (eff * inv)$ the correction for continuity.

The expected annual number of accidents without black box 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 does not only decrease with increasing sizes of the accepted error amounts α and β, but also with the supposed size of the black box effect. The expected number decreases with an increasing annual accident involvement rate of the vehicles too, thus accounting for smaller relative fluctuations in case of larger probabilities.

Annual accid. involv.	Err.type I II %	Black box effect -10 %	-15 %	-20 %	-25 %
0.2	5 10	1,338	587	326	206
	10 10	1,031	454	252	160
	5 20	972	428	238	151
	10 20	714	315	176	112
0.3		1,183	522	291	184
		913	403	225	143
		860	380	213	135
		633	281	158	101
0.5		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 black box in the trial.*

Annual accid. involv.	Err.type I II %	Black box effect -10 %	-15 %	-20 %	-25 %
0.2	5 10	6,700	2,940	1,630	1,030
	10 10	5,160	2,270	1,260	800
	5 20	4,860	2,140	1,190	750
	10 20	3,570	1,580	880	560
0.3		3,950	1,740	970	620
		3,040	1.350	750	480
		2,870	1,270	710	450
		2,110	940	530	340
0.5		1,750	780	440	290
		1,350	610	350	220
		1,270	570	330	210
		940	430	240	160

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

2.2.5. *Sample size of the trial*

Taking the step to the minimum numbers of vehicles needed in the trial, the reciprocal accident involvement rate might be applied to the minimum number of accidents calculated in Table 3. It 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 the final analysis. Vehicles are equipped with a black box later over a period, thus a time gap between the use of vehicles without and with a black box will be introduced.
In principle, two methods are available.

In the first method, the official governmental statistic is 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, one has to make use of the data concerning the control group of vehicles without a black box. 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, does mean that also the minimum number of vehicles in the experimental group has to be increased.

In reality, it will be difficult to find enough vehicles in the co-operating fleets to be equipped with a black box. Therefore, it could be necessary to make use of the fact that this increased minimum number of vehicles with a black box may be diminished somewhat, if the number without a black box is raised the more. 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 a black box has to be compared, instead of between 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 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 black box effect may then be principally assigned to them.
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. Not every driver who has more accidents than others is thus 'accident prone' - if such a quality really exists - instead of having met with misfortune.

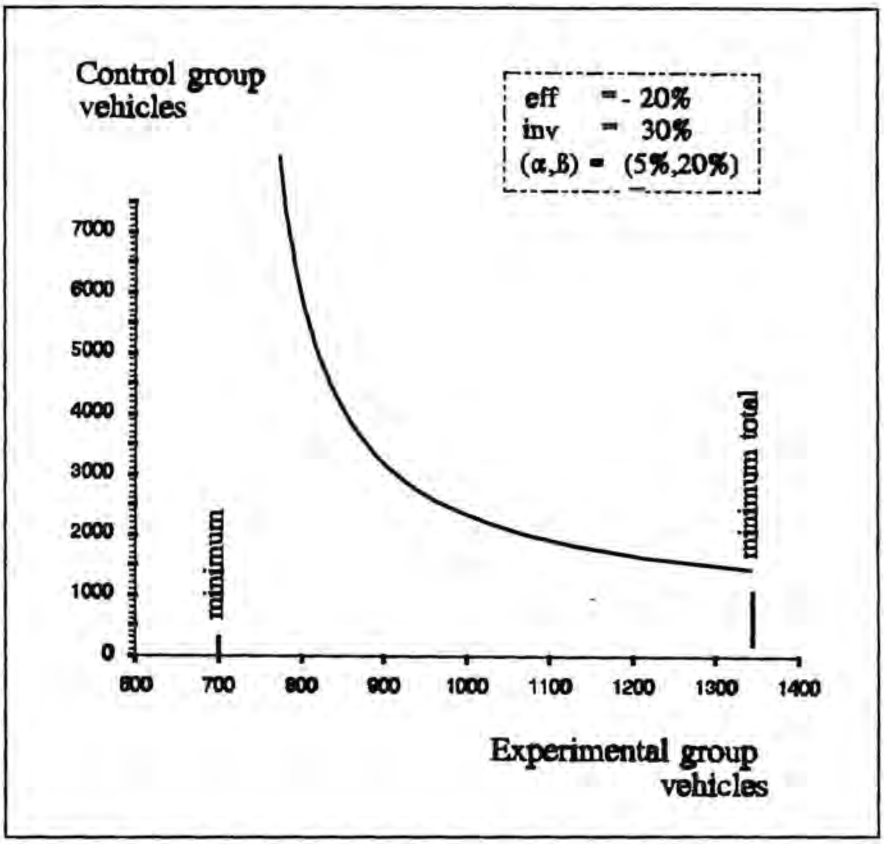

Figure 1. *Relation betweem sample sizes of experimental and control group.*

As discussed already, the calculated minimum number of vehicles that have to be brought into the trial concerns each comparison in which the black box 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 black box 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 summarized in Diagram 2. 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 involv.	Black box effect -10 %	-15 %	-20 %	-25 %
0.2	4,860	2,140	1,190	**750**
0.3	2,870	1,270	**710**	**450**
0.5	1,270	**570**	**330**	**210**

Diagram 2. *Minimum number of vehicles without black box in the trial, given α = 5 % and ß = 20%.*

This diagram shows that at least some 500 to 700 vehicles without black box have to be involved into the trial and an equal number of vehicles with a black box during a period of one year. Under these conditions, there will be a reasonable chance of finding positive black box effects of 20 % or more, if the mean annual accident involvement rate among the vehicles of the co-operating fleets is about 0.3.

2.3. 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 VDR 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.
4. In any case a vehicle is involved in a traffic accident, its driver has to be confronted - if necessary - with his driving behaviour by the responsible fleet management.

With regard to the conditions 1 and 2, the following statements can be made.
The number of vehicles to be involved in the experimental group, amounts from 600 to 700.
Whereas such a number exceeds the size of modal fleets, it will be necessary to build up the experimental group by taking together vehicles from several fleets of a smaller size.
The categories of the vehicles have to be known, as well as their utilization, 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 one and a half year before the moment of installing the VDR's, 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 black boxes in terms of cost of accident damage.

The list below gives an overview of these information in more detail:
Vehicle type
. van
. lorry up to 7.5 t.
. lorry over 7.5 t.
. articulated vehicle
. lorry with trailer
. bus / coach
. passenger car

Utilization
. national transport
. international transport
. kind of cargo
. in-company transport or transport by order
. private, professional or combined use of the passenger car
. fixed combination of driver and vehicle , or not
. kind of insurance

Conditions of use
. annual mileage
. type of roads
. light conditions
. daily or not
. 7 days a week or less
. only weekdays

In relation to the third condition, the following is of importance. In order to determine a diminishing amount of accident damage, information on damage cost is needed over the entire period of two and a halve years. The fleet management, or favourably its insurance company, will be requested to supply the kind of data listed below on both the experimental and the control group of vehicles:
- the damage cost per accident as reimbursed by the insurance company;
- whether only material damage is accounted for, or not;
- whether the accident occurred inside built-up areas, or not.

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."

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; imperative is that it is to be done.

2.4. Methods of 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 black box, 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 3 shows the basic data table for the trial. Standard statistical procedures are available to decide upon the black box effect for this case.

From the pilot study during the feasibility phase of SAMOVAR, it turned out that the analysis of data has anyhow to be done separately for each co-operating vehicle fleet. For, experimental and control groups from different fleets will be unbalanced in terms of size, safety policy, vehicle utilization, etc., as discussed in para 2.2.3. Moreover, some other classes

Numbers		Period before vehicles are equipped with a black box	Period after vehicles are equipped with a black box
Treated group vehicles	Traffic accidents	${}_{t}N_{B}$	${}_{t}N_{A}$
	Total fleet mileage	${}_{t}V_{B}$	${}_{t}V_{A}$
Control group vehicles		${}_{c}N_{B}$	${}_{c}N_{A}$
		${}_{c}V_{B}$	${}_{c}V_{A}$

Diagram 3. *Principal data layout of the black box trial.*

may have to be distinguished simultaneously, like 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 black box effect - have to be combined.

Two main approaches exist in handling this.
In the first, the question is whether the black boxes have a positive safety effect at all, or not. In principle that 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 black box effect for the total of all co-operating fleets. Then, a sort of weighed sum of all partial results can be estimated. This procedure is only satisfying when the black box effect for all distinguished classes would have approximately the same size.

Furthermore, it is relevant to know what maximum safety effect under normal conditions can be obtained by means of using a black box, and in which circumstances the black box does not work or works poorly. Presumably, it will not be easy to answer the latter 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 characterized by its scores on a chosen set of relevant variables, in place 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 multi-level 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 by now.

3. The behavioural study

3.1. Recorded behaviour

In this study, we have to deal with a lack of recorded behavioural data with respect to the pretest period. At the same time, behavioural data over a trip can only be obtained by means of JDR's, for ADR's only register these data if an accident takes place and over a limited period of time.

Making use of JDR's, a valuable experiment can be set up with the objective of assessing the influence of time on the speed behaviour by comparing the behavioural data of the very beginning of the trial with those at the end.
In this option, the 'speed behaviour' of several groups of driver is compared over the time. For that purpose a kind of operationalisation of this behaviour is required, for instance in terms of averages and variations in driving speed per period.

3.2. Self-reported behaviour; a research proposal

In addition to accident occurrence analyses elaborated in Chapter 2, the usage of black-boxes can be evaluated in a more 'subjective' or qualitative manner by means of a questionnaire / survey.
It is proposed to design two questionnaires, one addressed to the *managements* of the companies involved in the project, and another addressed to *individuals who actually have experience in driving vehicles equipped with black-boxes.*

The questionnaire will be relatively short, and will contain items on the following topics:
- *feedback* (what feedback is actually provided by the management, and how is this feedback experienced by the drivers concerned);
- *acceptance* (by both management and drivers);
- *appreciation* (or 'fear') of the equipment (drivers);
- *perceived cost-effectiveness* (management);
- *motives* (why are black-boxes appreciated or not, and have these motives changed as compared to the situation before black-boxes had been installed; by both management and drivers);
- *background variables* (e.g. type of company, age of driver, driving experience, vehicle type etc.)

It is expected that information on these topics can add to an understanding of the behavioral 'working mechanisms' of the equipment. In addition, the information obtained could provide valuable clues for possible future implementations.

The questionnaire will be distributed at the end of the experimental period: one has to have experience with the black-boxes (accident reconstruction apparatus or trip-recorders).
The project will be conducted on a relatively small-scale, about 200 - 300 questionnaires will be distributed among various companies and their employees (random or stratified sample).