

Pay-as-you-speed

Two Field Experiments on Controlling Adverse Selection and Moral Hazard in Traffic Insurance

Gunnar Lindberg
Lars Hultkrantz
Jan-Eric Nilsson
Fridtjof Thomas¹

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Abstract: Around one million people are killed world wide every year in road-traffic accidents. The risks and consequences of accidents increase progressively with speed, which ultimately is determined by the individual driver. The behaviour of the motorist thus affects both her own and other peoples safety. Internalisation of external costs of road transport has hitherto been focused on distance-based taxes or insurance premiums. While these means, as they are designed today, may affect driven distance, they have no influence on driving behaviour. This paper argues that by linking on-board positioning systems to insurance premiums it is possible to reward careful driving and get drivers to self select into different risk categories depending on their compliance to speed limits. We report two economic field experiments that have tested ways to induce car-owners to have technical platforms installed in their vehicle in order to affect the extent of speeding. It is demonstrated that a bonus to remunerate those that have the device installed, tantamount to a lower insurance premium, increases drivers' propensity to accept the technical devices. In a second experiment the size of the bonus is made dependent on the actual frequency of speeding. We find that this is a second way to discipline users to drive at legal speeds.

Key-words: Traffic safety, impure public goods, moral hazard, adverse selection, self selection.

¹ All authors work at the Swedish National Road and Transport research Institute, and can be reached by e-mail Full.Name@vti.se. Hultkrantz is also at Örebro University.

1. Introduction

More than 20 million people are severely injured or killed on the world's roads each year, and the burden falls most heavily on developing countries, where it will grow heavier still because of the rapid increase in the number of vehicles (WHO 2004). A recent estimate of benefits from safety enhancing electronic equipment in motor vehicles is based on that accident costs may add up to about 2 percent of GDP in many countries (OECD 2003).

Accidents appear both because of chance and of illicit behaviour. Moreover, their consequences fall both on those behaving improperly and on the non culpable. In view of the enormous costs, it is no wonder that internalisation of external cost of transport since long is high on the transport policy agenda. Surcharges on fuel are used to internalise otherwise external costs, and much effort has been directed to calculate the (external) cost to society generated by additional distances driven; except for accidents this includes congestion, environmental costs and road wear and tear (cf. Nash & Matthews forthcoming). The rationale for using fuel as a basis for a Pigou tax for internalisation of accident costs is that the more a vehicle is driven, the more fuel is used and the larger is the risk that it ends up in an accident.

But parameters other than distance may be equally or more important for understanding variations in accident risks and in external accident costs. In this paper we are concerned with speed as an important explanation of accident risks; the higher the speed, the higher is the risk for getting involved in, and the more severe is the consequence of, an accident. In addition, the higher the speed of a particular vehicle, the higher is the accident risk for meeting or bypassed vehicles. The choice of speed, or the frequency of speeding, may also be collinear with other behavioural driving patterns related to accident risks, such as dangerous overtaking. Fuel charges are a much too blunt an instrument for handling this dimension of accident externalities.

Traffic safety is a public good in that one person's safe driving will benefit all vehicles in a traffic system. The individual will however choose speed by balancing his or her own safety against the private costs of arriving sooner or later to a destination, without necessarily taking the benefits to others of safe driving into account. As we will show below, the driver takes part in a prisoners-dilemma form of social interaction with other road users in their choice of

speed. Traffic safety will therefore be underprovided compared to a socially efficient outcome.

To discipline drivers into handling the externality in speed choice, most countries rely on combinations of regulations (speed limits), enforcements (speeding tickets) and vehicle insurance schemes (bonus/malus constructions). Except for road-side policing, these instruments have a common shortcoming in their limited possibility to base rewards or punishments on actual driving behaviour. In particular, insurance schemes mainly reflect the cost of material (vehicle) damages caused by accidents, but not the cost of fatalities and injuries which usually is the major component of the external cost. Moreover, insurance premiums are differentiated between drivers only by broad characteristics, such as age, that are only weakly related to actual driving behaviour.²

However, new technologies, including positioning systems (such as the Global Positioning System GPS), mobile communications, and improvements of the information infrastructure (digital maps etc.) – often collectively referred to as Intelligent Transport Systems (ITS) – now make observation of driving performance feasible. Several of these are already on the market, for instance to serve as navigation aids.

The idea behind this paper is that, successfully engineered, ITS gadgets can be used for more than individual convenience. In particular, ITS can be made into an important tool for reducing accident risks. Using economic incentives in the form of diligently designed insurance schemes, it is furthermore feasible to create a market-led introduction of these technologies for safety purposes based on the standard Pareto principle that the free choice of individuals to purchase an item improves their welfare.

The purpose of the paper is to report about two economic field experiments, based on the driver being remunerated for having and using the new equipment, in this way internalising

² Parry (2004) compare alternative policies to reduce traffic accidents in the U.S. context, based on estimates of marginal external accident costs for different driver/vehicle categories. Among four compared policies he finds the most efficient being a differentiated mileage tax where each driver/vehicle category is charged a tax equal to its per-mile external cost. An insurance reform, changing premiums that are currently perceived on a lump-sum annual basis to a per-mile basis, is found to be inferior to this. Insurance premiums were assumed to vary in proportion to vehicle price and between three driver-age categories (below 25, 25-70, and above 70). The resulting premiums were found to be only loosely connected to external costs.

accident externalities relative to speed. Using the classification of Harrison & List (2004), we characterise our experiment as a framed field experiment.

The first experiment has tested a policy where drivers have volunteered to install the equipment in return for a lump-sum (bonus) payment. With the equipment, information about going speed limits and about speeding is continuously provided to the driver, and this information may induce him or her to drive more carefully. The experiment shows that the offer of a bonus has a significant impact on the propensity to have the equipment installed. We argue that making this offer is a means to overcome an adverse selection problem; careful drivers opt for the equipment and get an insurance bonus while more frequent speeders don't, and consequently have to pay more for their policy.

The second experiment extends the use of the equipment, by using recorded data about actual driving and speeding as a basis for calculating the size of a bonus received for participating in the experiment: Volunteers get a (monthly) lump sum for participation, but the payment is reduced if the vehicle has been driven too fast. We show that linking payment to speeding has a significant effect on driving behaviour and argue that this is a means to deal with moral hazard aspects of driving. A real-world implementation would link the size of an insurance premium to the extent of speeding.

After a brief review of some related literature in the next section, we start the presentation in section 3 by discussing the public good properties of speed choice. Section 4 describes the adverse selection, and section 5 the moral hazard experiment. Section 6 concludes.

2. Previous literature

Intelligent Speed Adaptation (ISA) is the generic name for systems in which the driver of a vehicle gets feedback on whether speed limits are exceeded; this is but one example of an Intelligent Transport System appliance. Carsten and Tate (2005) categorise ISA systems as Advisory, Voluntary ("Driver Select") and Mandatory Systems, depending on how intervening it is.

Research projects and trials with ISA have been conducted in some European countries. The largest of these has been made in Sweden with several thousand ISA-equipped vehicles on the road, most with advisory systems that inform the driver about the going speed limit and provides a warning in case of speeding (Vägverket 2001). Based on UK trials, Carsten and Tate (2005) predict that a mandatory ISA system, preventing motorists from driving faster than speed limits and introduced in all road vehicles, would save 20 percent of injury accidents and 37 percent of fatal accidents in the United Kingdom. They also estimate a social benefit-cost ratio of such a program at between 8 and 15, depending on various circumstances.

A mandatory system would be difficult to implement for numerous reasons. It requires political support at the national level and co-ordination at the international. Also, even if participation is mandatory, the willingness of individual drivers to comply is likely to be an important issue as there may be several more or less innovative ways of disconnecting or shutting down on-board ISA devices.

In contrast, the present study focuses a voluntary ISA system, to which incentives for enrolment would be provided by a traffic insurance scheme that encourages instalment of such a system and/or compliance to the recommendations given by it. Our study is therefore related to the vast theoretical and empirical literature on optimal insurance schemes in an asymmetric information setting; see Dionne et al. (2000) for a generic model of adverse selection in insurance markets and Winter (2000) for a summary of the generic moral hazard model.

There are a few studies within this literature that use experimental methods to study how the design of insurance schemes affects enrolment (adverse selection) and behaviour (hidden action). For instance, using a (hypothetical) choice questionnaire Royalty and Hagens (2005) investigate the effect of the out-of-pocket premium on the decision of employees in the U.S. to enrol in employer health insurance and other benefits plans.

There is also a large literature within labour and public economics on the effects of social and private welfare programs on hidden action, such as the intensity of job search. Several studies take advantage of natural “quasi-experiment” design features of such programs. There are also

some field experiments based on classical experimental design. For instance, in the Maryland experiment (Klepinger et al. 2002) claimants to an unemployment insurance program were randomly assigned to one of four treatment groups, each representing a different work-search policy, or two control groups.

Our paper makes use of field experiments as a wind tunnel for testing new ideas: Based on a crude theoretical basis, two simple mechanisms are tested in a real application. The results are interpreted as supportive of the possibility to use more targeted insurance schemes to deal with an important social dilemma, i.e. to induce people to take the full consequences of their behaviour into account. We are therefore now trying the more difficult task to approach governments and insurance companies with this new option.

3. Under-provision of safety and the equipment as an impure public good

Consider the utility function U_i of driver i . The travel time (t) for any given trip is a function of speed (s), and the associated utility $U_i(t(s))$ is *c.p.* increasing in speed, i.e.

$\frac{\partial U_i}{\partial t} \frac{\partial t}{\partial s} > 0$ for all i . On the other hand, speed has an adverse effect on traffic safety (a_i) for the driver due to an increased accident risk³, as well as potentially more severe accident consequences, of higher speed. This is formalized as $U_i(a_i(s))$, with $\frac{\partial U_i}{\partial a_i} \frac{\partial a_i}{\partial s} < 0$ for all i .

Note that the details of $a_i(s)$ may or may not vary with i , meaning that for any given speed s' , $a_i(s')$ may or may not differ from $a_j(s')$ for $i \neq j$; we only require that *c.p.* changes of driver i 's speed affect driver i 's utility in the way stated above. It may or may not therefore be “objectively true” that some drivers can drive faster than others and still have an accident risk that is lower than the “typical” driver’s accident risk (i.e. are “exceptionally good drivers”).

The travel time saved by higher speed is a private good and the thereby increased accident risk is a private bad, consequently leaving the sign of the derivate of $U_i(t(s), a_i(s))$ with

³ Based on a review of several studies, Finch et al. (1994) estimate that accident risk is reduced by 3 percent for each 1 km/h reduction in average speed.

respect to speed undetermined, i.e. $\frac{\partial U_i}{\partial s} > 0$. This also means that the “equilibrium speed”

(s^*) – the speed that a driver chooses – may differ across drivers; possibly

$$s_i^*(t, a_i, U_i) \neq s_j^*(t, a_j, U_j).$$

But accident risks and/or accident consequences are also a function of the speed of all the other vehicles (S_{-i}) in the traffic environment; $U_i(t(s_i), a_i(s_i, S_{-i}))$. We have $\frac{\partial U_i}{\partial a_i} \frac{\partial a_i}{\partial S_{-i}} < 0$, meaning that for all drivers i , a *c.p.* speed increase by any other driver decreases their utility.

Suppose that an individual driver can purchase a technical device d that assists in choosing a better informed travel speed for a cost of p_d and let y_i represent the monetary value of driver i 's accumulated remaining consumption. Individual i will purchase the device voluntarily only if

$$U_i(t(s^*), a_i(s^*, S_{-i}), y_i - p_d | d = 1) > U_i(t(s^{**}), a_i(s^{**}, S_{-i}), y_i | d = 0),$$

where $d = 1$ ($d = 0$) indicates that the device is (is not) purchased. s^* and s^{**} may differ, i.e. the “equilibrium speed” may be different if the equipment is or is not installed. Note that the subjective experience of infringement due to surveillance of the driving behaviour is captured by the conditioning statement in the utility expression above, and can be seen as being an inherent part of any driver's utility function.

The device is only purchased by those who consider the increased utility derived from it to be higher than the decreased utility due to non-consumption of other commodities of equal monetary value, i.e. p_d . These drivers can generally be characterized by having stronger preferences for traffic safety and/or lower utility associated to additional consumption of other commodities. The latter might be true for e.g. high-income individuals, but since this group can be expected to have a high value for travel time savings $t(s)$ as well, it is by no means clear that the demand for the equipment will increase with income.

The aspect we focus on here is that individuals will not take the consequences of their choice of speed on others' utility into account. Thus there is an obvious risk for under-provision of this impure public good.

4. Field Experiment I: Luring Young Drivers into using In-Vehicle Electronic Equipment

Over the 1999 – 2002 period, the owners of about 250 private cars and 150 commercial vehicles in Borlänge, a town in mid-Sweden, were part of a field test where they had their vehicles equipped with a small computer including a digital map, a GPS and a mobile communication facility. The digital map comprised all roads in the area, both the municipality's and the state roads. All existing speed-signs were meticulously registered and changes in speed regulations were updated with regular time intervals. An in-vehicle display continuously informed the driver about the going speed limit, and an acoustic signal sounded if the vehicle was driven above the speed limit. The equipment was therefore basically a means for providing drivers with detailed information about whether or not they drove too fast.

As part of the project, driving behaviour – i.e. the speed relative to the going speed limit – was being recorded every tenth second, and even more frequently if the vehicle was speeding. An ex post assessment of the Borlänge test and of similar tests in three other Swedish cities, undertaken by the funding agency, the Swedish National Road Administration, indicated that the equipment had resulted in a 7 percent reduction of average speed in the fleet of vehicles (Vägverket 2002). It is reason to question this number, primarily since there is poor data to contrast the trial period against. For the purpose of this paper, we will, however take as a datum that improved information about the vehicle's speed relative to going speed limits, has a positive impact on driving behaviour.

In this section, we describe a field experiment that sought to attract additional vehicle owners to the test crew. The means for doing so was to offer a sample of the target population an economic incentive for having the equipment installed. Section 4.1 motivates the interest in young drivers as the particular new test crew and presents the frame used for identifying the target population. Section 4.2 details the offer made, section 4.3 reports the result of our questionnaire and section 4.4 concludes.

4.1 Attracting New Users

In 2002, several vehicles in the original test fleet had been sold or equipment had for other reasons been removed. The test fleet was considered too small, and since about 130 electronic platforms were available, a process to recruit new test drivers was initiated. In this, a point of departure was that the existing test crew, all of which had volunteered to participate back in 1998, did not represent an average of vehicle owners. In particular, younger drivers were under represented, with no car-owner under the age of 30 in the group.

Young drivers are of interest also for another reason. It appears to be a generally accepted proposition that young drivers are over represented in accident data in comparison to the size of the group.⁴ This may be explained by sheer inexperience, but there is also reason to believe that youngsters drive faster than the speed limit more often than the population at large. For these and for other reasons, it is of interest to examine the possibility to affect the behaviour of this particular group of drivers.

It was therefore decided that the *target population* for the present study was young drivers owning a car and living in the municipality of Borlänge. Our *frame* to obtain observational access was obtained from the Swedish Central Vehicle Registry that records all cars and their owners in the country. The addresses are updated regularly by retrieving information from the Swedish Register of the Total Population. Changes in car ownership are registered with a few days delay only.

In August 2003, a list was compiled from the Vehicle Registry containing the names and addresses of all individuals of age 18 to 28 (inclusively⁵) that had their main address in the municipality of Borlänge and was reported to own at least one car that was in active use in traffic. This frame is not perfect. First, some young drivers living in Borlänge and owning a car are not in the frame population. Most notably, temporarily visiting guest students from foreign countries, as well as Swedish students spending most of their time in Borlänge but still being registered under an address outside the municipality of Borlänge, are not contacted

⁴We have failed to find formal empirical proof of this proposition.

⁵ The exact criterion was that the individuals must turn/have turned 18 at the latest during the year 2003 and must not turn/have turned 29 during that same year or any previous year.

in this way. We acknowledge this fact but are not very concerned about this under-coverage of the frame.

Second, and of greater concern to us, is a possible over-coverage of the frame. Young drivers that have moved to one of the country's larger cities may have an economic incentive to hold on to their Borlänge address. This is so since Borlänge may qualify them for lower insurance premiums than in many other cities, most notably for vehicle damage insurances. Young individuals that grew up, and still have their parents living in the municipality, but are e.g. studying in another city, may use this possibility. We expect these individuals to have their mail forwarded to their address of permanent residence. Many of them may view themselves as not belonging to our group of interest anymore and consequently ignore our invitation in a sort of "self-deselection". Most typically this will not come to our attention, but will manifest itself as a non-response.

Despite the fact that we can only observe "non-response" in general, it is important to distinguish between non-response due to the absence of interest in the speed-tracking device as such, and non-response due to "self-deselection" of an individual caused by discrepancy of official from real permanent residence. This is especially true when estimating the proportion of individuals prepared to consider an installation of the device. As we will see, ambiguity in the interpretation of the non-response can be an important factor when interpreting the result of our experiment.

4.2 Proposal letter and selection of sub-groups

The frame population contained 1,271 individuals and a letter of invitation was sent out to each of the associated addresses. Since the experiment sought to recruit only 130 individuals, and in order to avoid too many "yes" answers, only half the population were actually offered the equipment for installation. The other half of the target population received an offer that was identical in all respects except that the proposed question was only hypothetical. We will focus here on the group that received the real offer.

The respondents receiving the factual offer were randomly selected from the individuals in the frame and allocated into two sub-groups before the contact letter was mailed. One of the

groups was asked to participate against receiving a monthly remuneration of SEK 150 (USD 20) for the full year that the experiment would last. They would therefore earn SEK 1800, net of taxes (which the project would pay), with the only provision that the equipment was functional during the whole period. Payments were to be made monthly after checking for that driving behaviour had been logged and successfully transferred into the data base. The other group was invited to participate without any remuneration being paid.

Table 1: Number of invitation letters. In parentheses the number sent out including those returned to sender due to unrecognized addressee.

	Offering device
With remuneration	212 (213)
Without remuneration	421 (423)
Sum	633 (636)

The project was described on a single page. Except for one of the four questions described above, the letter also contained a four-page questionnaire regarding attitudes towards safety aspects of travelling. This questionnaire is not of immediate interest to us, but would allow for refined ex post analyses of driver behaviour during the test period in later stages of the experiment.

4.3 The response

The 633 letters produced 264 answers, i.e. a response rate of 41.7 percent, see Table 2.

Table 2: Results from the questionnaire.

	With remuneration	Without remuneration	Sum
Obtained yes	25 (11.8 percent)	19 (4.5 percent)	44 (6.9 percent)
Obtained no	64 (30.2 percent)	156 (37.1 percent)	220 (34.8 percent)
No response	123 (58.0 percent)	246 (58.4 percent)	369 (58.3 percent)
Sum	212 (100 percent)	421 (100 percent)	633 (100 percent)

Table 2 shows that 11.5 percent in the group with remuneration⁶ returned a yes-answer as opposed to only 4.5 percent in the group not offered remuneration. Viewing these proportions as two independent realisations in the two groups, one can apply a simple significance test for comparing two proportions (see e.g. Moore and McCabe 2003, p. 592). Testing the hypothesis of equality of proportions, against the alternative that the proportion of positive answers with remuneration is larger than without remuneration, gives a *P*-value as small as .0003. This strongly indicates that it is not reasonable to assume that the observed difference is explained by chance alone.⁷ The remuneration can therefore be assumed to have had a substantial effect on the positive response of the young drivers, albeit many could still not be attracted by that particular remuneration.

The simple significance test for comparing two proportions overlook a lot of structure in the problem, and our data contains much more information about the willingness of young drivers to have the device installed. First, non-response is generated by two circumstances, either by the failure to get in contact with a selected individual, or by a contacted individual's informed decision not to respond to the request. Second, it seems reasonable to assume that an individual that is willing to participate *without* monetary compensation also would be willing to participate *with* monetary compensation. Therefore, the obtained yes-answers from the group *without* remuneration do contain information that is relevant for the interpretation of the answers obtained from the group *with* remuneration. Likewise, it seems reasonable to assume that an individual which returns a negative answer when *offered* the monetary compensation would not have returned a positive answer when *not offered* the monetary compensation.

This inherent structure can be directly modelled using contemporary statistical methodology. A separate paper by Thomas (2005) provides this analysis and shows the following (accounting for the structure in the problem as well as the ambiguity in the interpretation of non-response): between 6 and 12 percent of the population would be attracted to install the technical device in the absence of the remuneration whereas this proportion is between 10 and 17 percent when the remuneration in fact is offered. The size of the group that can be *induced*

⁶ As it turned out, we were grossly mistaken in our estimate of the propensity to participate since the 25 aye's was way off the target 130 participants. This did, however, not become a problem since the equipment started to malfunction at installation, forcing us to close down the whole project. The volunteered were paid off with a nominal amount of money and an excuse referring to first-generation techniques.

⁷ The validity of this statement depends only on the exercised control over the experiment and does not depend on assumptions regarding to what extent the participating group of young drivers is representative for 'all young drivers'.

to accept the device is about five percent of the population of young drivers. This group would not accept the device without remuneration but does so with the remuneration and is therefore of central interest for policy making as its behaviour can in fact be changed by the remuneration.

4.4 Discussion

The reported field experiment demonstrates that economic incentives matter: An offer of a monthly remuneration of SEK 150 for having an information-producing device installed in their vehicles, affects the number of young drivers that accept the offer. An informed judgement is that the proportion of young drivers in a municipality like Borlänge that can be induced to install the device in the absence of remuneration is close to 9 percent. Since around 14 percent seem to be willing to take measures to qualify if the potential reward is in parity with our compensation, we expect that the effect of the scheme is to increase the participation with around 5 percent of the relevant population.

For several reasons, subsidy schemes may be difficult to use in real-world applications. The experience from the present field experience can however readily be transferred into current practices in the insurance industry. Let us first note that vehicle insurance policies typically are differentiated across customer groups. In particular, people with new driving licenses, and those that have recently been involved in accidents, typically pay a higher premium than experienced drivers with no accident record. This is a way for companies to handle adverse selection by proxy. Rather than calculating charges that immediately link to observed behaviour – which at present is difficult or at least costly – the premium differentiation seeks to pinpoint insurance holders that are on average believed to be a higher risk.

The novel ITS technology however provides a means for identifying more or less careful drivers. A small group of vehicle users are technique nerds or are very keen on safety aspects of their driving and would anyway buy the equipment. An economic reward can lure an additional number of vehicle owners into having the device installed. Evidence from the Swedish experiments moreover indicates that the equipment per se may induce the owner to drive more carefully. If the community of equipment users drive more carefully than the average drivers do, they will by definition be better customers for the insurance company. The

device can therefore serve as a proxy for careful drivers, and these can be remunerated by a lower insurance premium. It is reason to believe that women and the elderly will be over represented in this group.

The others, i.e. vehicle owners that don't have the device installed, will by definition include a higher share of high-risk drivers, which will press the premiums upwards in this sub-group. The combination of whip (a higher premium for some) and carrot (a lower for others), and the possibility to self select into the low-premium scheme, may work to separate the market.

Insurance companies may be reluctant to start using these sophisticated premium schemes. Reasons include substantial administration costs and that insurance cost differences across sub-categories of drivers may be too small to induce entry into the subsidy scheme. It should be noted that the incentive used in the above experiment after all was fairly high and still attracted a fairly small share of the population.

The analysis in section 2 however pointed to the prisoners' dilemma aspects of the choice of speed, i.e. that drivers don't account of the risk to others of speeding. This implies a motive for government intervention. One way of doing is to link the use of ITS equipment and a differentiated insurance policy, to a complementary differentiation of the annual vehicle tax. Vehicle owners that (presumably) are more careful drivers than others would not only have to pay a lower insurance premium, but also have their vehicle tax reduced. For a balanced budget, the tax for other vehicles would have to be raised. Insurance companies can in this way be made agents for the government, and the industry could possibly also be allowed to use the annual vehicle inspection to ascertain that the equipment that has been installed also is functioning.

5. Field Experiment II: Intelligent Economic Speed Adaptation⁸

One shortcoming of the above experiment, and of the real-world application that has been drafted, is that the mere presence of the equipment in cars does not guarantee that drivers actually drive more cautiously. A second field experiment has therefore studied the behavioural consequences of directly linking the size of a payment to experimental subjects to

⁸ This section is based on Hultkrantz & Lindberg (2004).

their actual behaviour, i.e. to the extent of speeding.⁹ We will argue that this is an alternative, or possibly complementary, way to use economic incentives. This time, it is not a certain *type* of vehicle owner, but the actual *behaviour* that is remunerated i.e. the moral hazard dimension of the insurer-ensured relationship is in focus. The experimental design of our second field experiment is presented in section 5.1., results for two sub-groups of the population in sections 5.2 and 5.3 and section 5.4 sums up.

5.1 Experimental design

In May 2002, 114 car owners that remained from the original field test were invited to participate in a complementary economic experiment during September and October that year.¹⁰ The experiment budget would pay a monthly lump-sum bonus for participation. The bonus would, however, be reduced for each minute a vehicle drove faster than the speed limit. The purpose of the field experiment was to assess the consequences of this “speeding penalty” for actual driving behaviour.

A majority of the remaining test crew (95 persons out of 114) accepted to participate, while 9 rejected and 10 drivers did not respond. An ex post comparison of participants and non-participants shows that there is a self-selection bias in the recruitment to our experiment, in addition to the original bias in recruiting participants. Our test crew is on average older than the non-participants and have less severe speed violations.

An important pre-condition for the result analysis is the seasonal variation in driving behaviour due to climate variations. Under winter conditions, which usually begin in late October or in November and last until March or April, drivers typically reduce their speed. Our experiment, however, benefited from the observations of driving behaviour that had been collected during previous years. The impact of speed violation charges is therefore evaluated by comparing behaviour of each driver when economic incentives are in force with the same driver’s behaviour during the same month one year earlier.

⁹ It should be noted that this experiment was performed one year before the experiment presented above, but is presented here for the sake of clarity in exposition.

¹⁰ The group of drivers filtered out in this process are in no way a representative sample of car owners. The test group is likely to be highly motivated because of an interest in the technology, a preference for safe driving, desire to assist research, or whatever. The question for our experiment, hence, is how the driving behaviour of such highly motivated individuals changes when economic incentives are added.

Economic parameter values were systematically varied in order to assess their relative importance. First, participants were randomly assigned to a low or high bonus group (250 or 500 SEK/month).¹¹ Second, since the accident risk increases progressively with the speed of the car, the bonus reduction was made progressive. The cost for driving up to 10 percent above the speed limit for the low-bonus groups was 0.10 SEK/minute (TYPE I speeding), 0.25 SEK/minute for driving 11-20 percent above speed limits (TYPE II) and 1.00 SEK/minute for speed offences higher than 20 percent (TYPE III). Third, a double speeding penalty, i.e. 0.20, 0.50 and 2.00 SEK/minute in the respective groups, was also enacted. Fourth, a control group was created that were paid the respective bonus but faced no deductions for speeding. Table 3 summarises the six treatments.

Table 3: Treatment groups i-vi (no. of individuals in the group).

	Deduction when speeding		
Base bonus	0	Low	High
250 SEK	i (15)	iii (16)	v (16)
500 SEK	ii (16)	iv (16)	vi (16)

In addition to this premeditated setting of treatments, the refusal of 14 individuals to participate provides us with an extra reference group. Since the non-participants still were part of the overall test and therefore had their driving behaviour monitored, it was ex post feasible to contrast the behaviour of those that have and have not joined forces with the economic experiment.

Figure 1 and Table 4 describes the nature of the information used for assessing the consequences of the economic incentives. Figure 1 presents the speed of car number 58, a driver with low bonus/low deduction, using roads with speed limit 50 km/h in September 2001 and September 2002. A corresponding summary was made for all classes of speed limits. Frequency here refers to the share of total time driven at this speed. Eyeballing the figure, it is obvious that the incentive scheme has shifted the driving behaviour to the left, i.e. the proportion of time driven above the speed limit has been reduced. At the end of each

¹¹ After having randomised participants it was checked that the observed driving behaviour in September 2001 did not differ systematically across treatment groups.

month, participants received information about their speed behaviour, the sum of charges and remaining bonus in the way illustrated by Table 4.

Figure 1 Speed profile of car #58 on roads with speed limit 50 km/h September 2001 (no charges) and September 2002 (with charges).

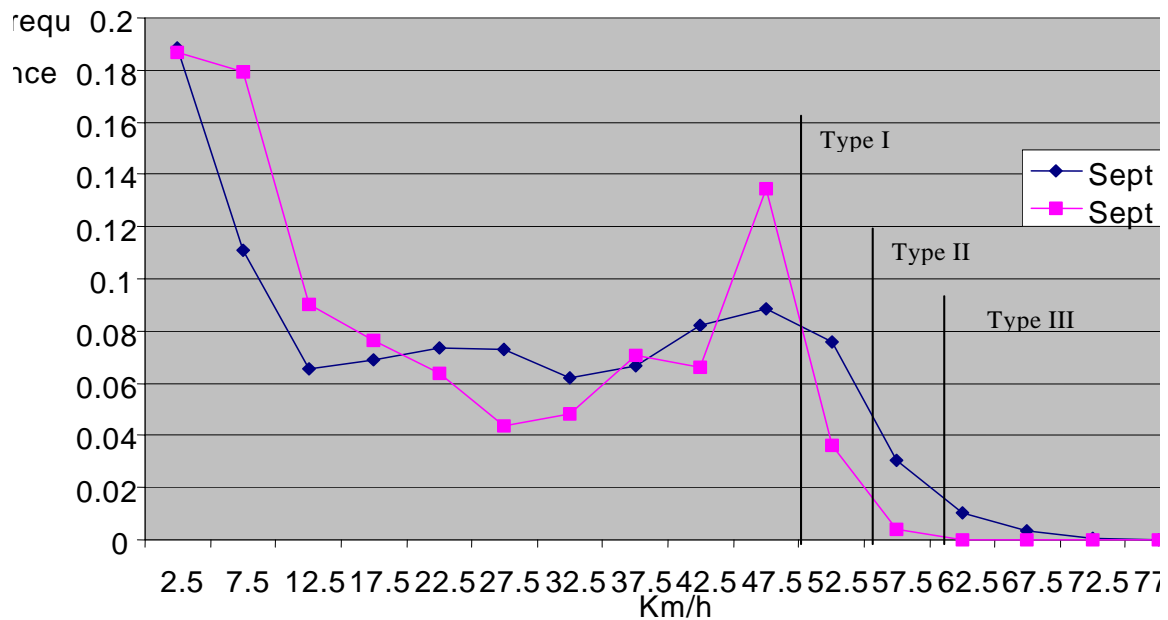


Table 4: Information provided to the owner of car # 58, September 2002 about no. of minutes speeding in each class of speed violations and consequent speed charges.

Speed violations	Speed limit					Price/minute	Total deduction
	30 km/h	50 km/h	70 km/h	90 km/h	110 km/h		
0percent to 10percent above speed limit	0	3	0	0	0	0,20 kr	,60 kr
11percent to 20percent above speed limit	0	0	0	0	0	1,00 kr	,00 kr
Above 21percent above speed limit	0	0	0	0	0	2,00 kr	,00 kr
Total	0 min	3 min	0 min	0 min	0 min		,60 kr

Fixed monthly payment for the period:

250 SEK

Deduction (rounded):

1 SEK

Payment (net before tax):

249 SEK

This payment will be transferred to your bank account. In addition, VTI has paid preliminary income tax directly to the Tax authority on your account with:

107 SEK

A relative measure of speed violation (PVM – Proportion Violation Minutes) is calculated for each individual (j) as the time the driver violates the speed limit for each type (severity class) of violation (i) (VIOL), relative to the total travel time during the month (M).

$$PVM_{ji} = \frac{VIOL_{ji}}{M_j} \quad (1)$$

i = Type I, Type II or Type III

j= individual 1...114.

PVM will differ across individuals due to non-observable individual characteristics, but the focus here is on the difference in behaviour over time on an individual level. The *absolute adaptation* is the difference in Proportional Violation Minutes prior to, and as a result of the experiment for each violation type (equation 2).

$$\Delta PVM_{ji} = PVM_{ji}^{2002} - PVM_{ji}^{2001} \quad (2)$$

5.2 Results: Adaptation of participants compared to non-participants

A first step in the analysis of how economic incentives affect behaviour is to compare the 95 individuals that opted to participate with the 14 persons that did not. The dataset is restricted to individuals where we have observations from both 2001 and 2002. Figure 2 shows that non-participants drive too fast about 17 percent of the time in 2001 and tend to have the same driving pattern in 2002. Participants drive too fast about 14 percent of the time in 2001, but the extent of speeding drops to about 8 percent in 2002. Obviously, the participation in the experiment has reduced the proportion of speed violations. Table 5 demonstrates that the difference in absolute adaptation between participants and non-participants is significant for all violation types except for the severest violations in October.

Figure 2: PVM, all violations for participants (diamonds) and non-participants (quadrates).

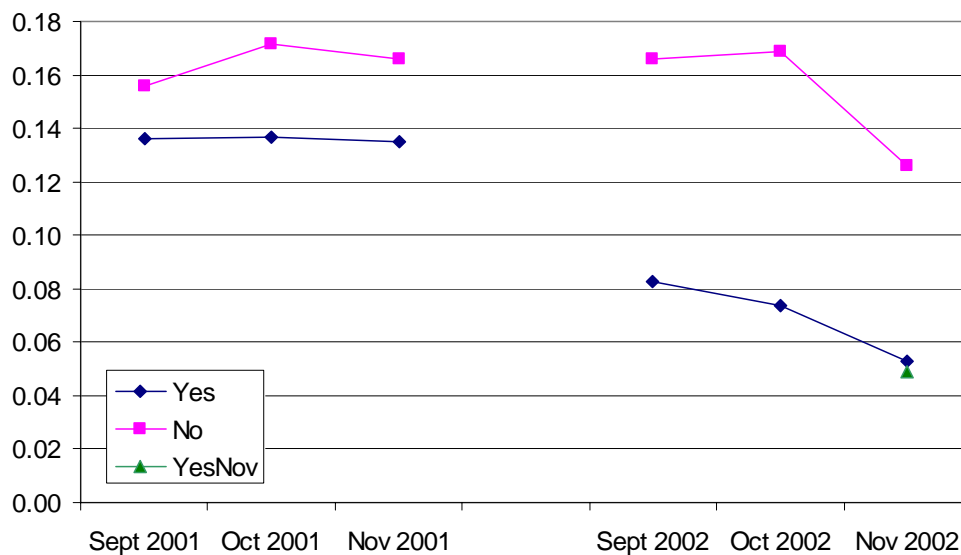


Table 5: Absolute speed adaptation between 2002 and 2001.

GROUP	Participants			Non-participants			Difference		
	Mean	Std dev	Obs	Mean	Stddev	Obs	Mean	Std dev	t-value
September – September									
Δ PVM All	-0.05	0.07	72	0.01	0.04	10	0.06	0.07	2.712**
Δ PVM I	-0.03	0.06	72	0.01	0.04	10	0.04	0.06	2.111**
Δ PVM II	-0.01	0.02	72	0.00	0.01	10	0.01	0.02	2.230**
Δ PVM III	-0.01	0.01	72	0.00	0.01	10	0.01	0.01	2.816**
October - October									
Δ PVM All	-0.06	0.06	44	0.00	0.04	6	0.06	0.06	2.448**
Δ PVM I	-0.04	0.04	44	-0.01	0.02	6	0.03	0.04	2.098**
Δ PVM II	-0.01	0.02	44	0.00	0.01	6	0.02	0.02	2.246**
Δ PVM III	-0.01	0.02	44	0.00	0.02	6	0.01	0.02	1.324

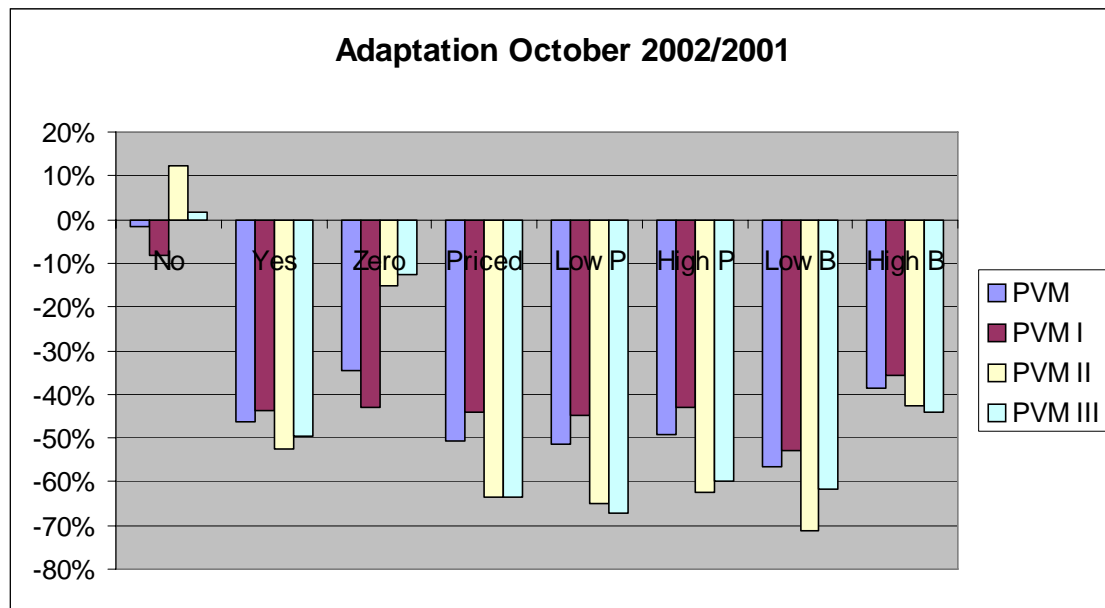
*) Significant on 90percent level, **) Significant on 95percent level.

5.3 Results: Adaptation within the group of participants

Figure 3 provides an overview over the observations for October, both for the 14 non-participants and for the 95 volunteers. Four treatment groups (64 drivers) had a speed related penalty (see table 3 above) while two groups (31 drivers) had no penalty but did receive a lump-sum for participating (zero-price). As indicated by Table 6 (below) we have fewer observations than so, due to malfunctioning equipment. The number of observations in some groups provides one reason for the problem to obtain significant differences between groups.

Both the groups with and without penalties reduced the proportion of speed violations during the experiment months compared to the same months previous year. The four treatment groups with speeding penalties violated the speed limit 15 percent of their driving time prior to the experiment and 7 to 9 percent during the experiment. The zero-priced group had a violation of 11 to 12 percent prior to, and 7 to 8 percent during the experiment.

Figure 3: Relative adaptation October 2002 compared with October 2001. No= non-participants. Zero – groups i+ii; priced – groups iii-vi; low B – groups iii+iv; high B – groups v+vi; Low P – groups iii+v; High P – groups iv+vi.



The formal test of significant differences in Table 6, indicates that there is a (weak) difference in adaptation during October but not in September. This can probably be explained by a learning effect. During the first month of the experiment, several no-penalty drivers contacted the project secretariat and asked if the zero price really was correct. This group may therefore have changed behaviour as a result of receiving a lump-sum bonus, but after the feed-back month they realised that the non-penalty was accurate and behaved accordingly, i.e. did not let the economic experiment affect their behaviour.

The table also indicates that it is only the difference in adaptation for the more severe violations (PVM 2 and PVM 3) that are significant. As expected, we find the strongest reaction in the more severe violations with the highest price per minute. But a reduction in severe violations means that the whole driving pattern is “pushed downwards”. Type I violations may therefore remain un-changed relative to the comparison period, simply because type II and type III speeding has been transferred and become (the less serious) type I violations.

Table 6: Speed Adaptation between 2002 and 2001. Comparison of participants with and without penalty.

GROUP	Penalty			No penalty			Difference		
	Mean	Std dev	Obs	Mean	Stddev	Obs	Mean	Std dev	t-value
September – September									
Δ PVM All	-0.06	0.08	49	-0.04	0.06	23	0.02	0.07	0.851
Δ PVM I	-0.04	0.06	49	-0.03	0.04	23	0.01	0.06	0.687
Δ PVM II	-0.01	0.02	49	-0.01	0.02	23	0.00	0.02	0.905
Δ PVM III	-0.01	0.01	49	-0.01	0.01	23	0.00	0.01	0.537
October - October									
Δ PVM All	-0.07	0.05	30	-0.04	0.06	14	0.03	0.06	1.816 [*]
Δ PVM I	-0.04	0.04	30	-0.04	0.03	14	0.01	0.04	0.592
Δ PVM II	-0.02	0.02	30	0.00	0.02	14	0.02	0.02	2.681 ^{**}
Δ PVM III	-0.01	0.02	30	0.00	0.02	14	0.01	0.02	1.991 [*]

^{*}) Significant at 90percent level, ^{**}) Significant at 95percent level.

5.4 Summary

Penalties that reduce the size of a bonus payment when vehicles are speeding have a significant effect on driving behaviour. We have shown this by comparing individuals that have participated in the experiment with those that opted out. Participants have significantly reduced their frequency of speeding when risking lower bonus payments. A similar, while not equally significant difference can be seen when we compare different ways to treat participants. Across bonus levels (high and low), and across different levels of penalty deductions, drivers that receive no penalty change their behaviour less than those that face speeding penalties. Moreover, penalties bite in particular for drivers that receive a low bonus for participating, presumably since the penalty then is a larger share of the base-line bonus payment.

We have demonstrated that the 14 non-participants, i.e. those that did not want to participate in the economic experiment, had a manifest behaviour different than the 95 participants; they drive faster. This provides an indication of the importance of self-selection bias, i.e. that it is

no chance that drivers differ in their propensity to accept offers of participation bonuses that on the face of it should be considered as beneficial; legal driving is remunerated with 250 or 500 SEK per month.

It should also be reiterated that our test crew is a far way from an average driver. First, a (random) sample of Borlänge citizen were invited to have the ITS equipment installed back in 1998. Secondly, those that joined in are presumably more interested in issues related to accident risks or whatever than the average. It is also reasonable to expect that the volunteers are more careful drivers than overall, since they would otherwise have had to listen to the annoying alarm pretty often. And third, the volunteers to our complementary moral hazard experiment represent a group of more careful drivers than the average. Our results indicate that we *still* are able to discern a significant effect of economic incentive on an already careful driving pattern.

6. Discussion

The technical development in the telecom-, computer and vehicle industries has progressed at impressing speed over the last decade. But to the surprise of many, the ITS technology has not been implemented in the transport sector to the extent expected. While electronic in-vehicle maps have been available for many years, the demand for such services is still low. The cost of the equipment, and the bonus services it provides, seems to be high relative to the price of a traditional map stored in the glove compartment.

The present paper has reported about two field experiments indicating that electronic in-vehicle equipment may have a potential use as a means for controlling one of the main determinants of motor vehicle accidents, i.e. the frequency of speeding. The possibility to track and register behaviour in a way which has not previously been feasible, together with elaborate economic incentive schemes, may open up for new ways to increase traffic safety. Since the type of ITS-based equipment that we have in mind is an (impure) public good that may otherwise be under-supplied, government intervention is required.

Like many new techniques, our suggestions for using the equipment to handle adverse selection and/or moral hazard types of problems, is fraught with problems. One is that it is

merely a first shot at an institutional setting. A second is that the interest from the insurance industry most probably is restricted by the propensity of individuals to respond to incentives. If only few drivers use the new equipment and benefit from lower insurance premiums, the administrative costs may be too high to warrant the additional complication that the scheme would imply. It is obvious that close cooperation between insurance companies and government representatives in order to capture the public-good qualities of the problems, is a prerequisite for further success. Also car-makers and the telecom industry have an obvious stake in the matter.

A third issue, and presumably the most contentious, is the big-brother aspect of the technique. The sort of equipment we have in mind will make it feasible to monitor the behaviour of individual vehicles, thereby obviously intruding on privacy. A first answer to this problem is that most parts of the technique are already in use in many taxis and heavy vehicles and that other, more sophisticated versions of it are currently being tested.¹² A second repost is the self-selection trick that is suggested: All insurance holders are invited to participate. But it is only those that volunteer that run the risk of being recorded.¹³ And volunteers are by their nature less concerned with the big-brother aspects of the problem.

We have also anticipated that volunteers are more careful drivers than average; since they don't really have to change their behaviour so much, it is easy for them to acquire the equipment and save on the insurance premium. In one way, this group is of secondary interest with respect to safety since they already are (relatively) law abiding and therefore less accident-prone. But the self-selection into the scheme will push up insurance charges for non-participants, meaning a better fit of accident risks and -costs to actual (dangerous) behaviour.

The ITS technique most probably also holds promise for other applications in the road sector. One obvious example is the growing interest in congestion pricing. The London version of levying fees is still a crude manual scheme. The potential for elaborate and detailed traffic control by way of in-vehicle devices of the sort tested here, providing extensive social

¹² Indeed, some insurance companies have started to explore the possibility to have an 'usage-based insurance rating system'. Progressive, in USA, has tried with a system called Autograph, which bases the insurance rates on how much, when and where the vehicle is driven' (www.progressive.com) and a similar pay-as-you-drive system is being tested in Britain (www.norwichunion.com/pay-as-you-drive/index.htm).

¹³ An open legal issue is whether records could be used as evidence in a prosecution of the driver after an accident. In the field trials reported here, a contract was signed that declared that the individual records

benefits, may therefore be considerable (Hultkrantz 2004). This is, however, nothing that can be left to the industry but a development that calls for elaborate governmental policy making.

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ultimately belonged to the driver. Fortunately, whether this actually would protect the driver in a court case has not come to a test.

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