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Short and Medium-Run Local Effects of Fixed Speed Enforcement Cameras on Accidents : Evidence from the French Case

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Short and Medium-Run Local Effects of Fixed Speed Enforcement Cameras on Accidents: Evidence from the French Case

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Abstract

This article seeks to assess the effect on road accidents of fixed speed enforcement cameras (SECs) set up in France since 2003. Accidents are measured on a monthly or quarterly basis in every town in France from 1998 to 2007. The set-up of cameras in towns strongly depends on the past history of accidents. To deal with potential endogeneity biases, two complementary approaches are used. The former relies on a very flexible semi-parametric model of the occurrence of accidents and the impact SECs could have on them, assuming that unobserved town-specific heterogeneity is fixed with respect to road accidents. The parameters that account for SEC effect are estimated by using non-linear GMM. The second approach is non parametric and directly measures the effect of SECs comparing towns where an SEC has been just installed to those where an SEC will be installed several months later. These approaches are complementary. The former is more precise and the latter is more robust with respect to endogeneity and mis-specification issues. We show that the effect of SECs is stronger during the first months after their installation, but decreases afterwards. This result is confirmed in both approaches. To our knowledge, the fact that the impact of SECs decrease over time is seldom stated in the international literature, which focuses mostly on short-term outcomes. The most plausible interpretation is that drivers get used to the exact location of SECs as time goes by and spatial halo effects decrease over time. Moreover, the two methods deliver quite similar magnitudes of the short-

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term impact of SECs. It shows that a classical Difference in Difference method is unable to capture the dynamics of accident towns and give a false description of the desired real impact.

Keywords: road accidents, speed enforcement cameras, panel data models, semi-parametric models, caliper matching
JEL Codes : C14, C33, R41

1 Introduction

Until the mid-2000s, France was among the worst European countries with regard to road safety. In 2003, the French government decided to install speed enforcement cameras (SECs) to improve road safety. These cameras were installed in fixed sites that were signaled to drivers by specific signaling. Simultaneously, a new procedure was implemented to automatically detect and punish speed violators, unlike the previous speed controls that were nearly all mobile. This rolling out of SECs was accompanied by a massive advertisement campaign aimed at informing people of the change in the legislation. Accordingly, the drop in the total number of accidents in France recorded in October 2003 has often been attributed to the introduction of this specific policy (see Graph 1). However, as far as we are aware, no rigorous evaluation is yet available on French micro data.

Speed Enforcement Cameras can have local and overall effects. They first oblige drivers to limit their speed at the exact place where they are located. Since many accidents are related to high speed (Ashenfelter and Greenstone, 2004), we may expect the local effects of SECs to lower the number of accidents at a local level. However, some papers argue that speed variance matters more than the absolute speed level (Lave, 1985 and Lave and Elias, 1994). Hence, SECs may induce some drivers to suddenly reduce their speed and this may cause accidents. Thus, local effects may have an ambiguous effect on the number of accidents.

The literature also reports "spatial halo effects" (see Hess, 2003). The presence of an SEC in a given place may have an influence on areas located around the site itself because drivers may continue to behave more cautiously. The installation of an SEC may also change driver behavior: some may find SECs so stringent that they choose to avoid them, preferring uncontrolled roads. Doing so, they might use less safe roads, and this might increase the total number of accidents in places around SECs.

Finally, on a very general level, SECs were not the only tool to be implemented by the French government in the period under review. Many advertising campaigns were conducted to change driver behavior, improvements in cars were implemented to enhance safety, a high number of speed humps and traffic circles were also built and the above factors are also considered as possible explanations for the strong decrease in the number of accidents.

In this article, we use a database that contains all the accidents involving at least one injured person in France from 1998 to 2008. Such accidents are called "casualty" accidents. Using this data, we can build for each town in France the quarterly number of accidents. Since we also know the localization of each SEC, as well as its installation date, we can examine for each town whether the setting up a new SEC coincided with a drop in the number of accidents. Although very simple, this approach raises several issues. First, the dependent variable to be examined is the number of accidents for every quarter between 1998 and 2008 and every town in France. This is a count data. For many observations, the number of observed accidents is zero. Accounting for this will require more sophisticated econometric approaches. Second, towns are very heterogeneous: some, like Marseille or Paris, are very large and record more than 100 accidents per quarter. In other far more numerous smaller towns, only one accident was observed over the whole period. Estimations need to properly account for this heterogeneity. Third, the heterogeneity between towns may make the SEC effect very specification-dependent: in small towns where there is only one road, an SEC will not affect the occurrence of accidents in the same way as in a large town where there are many roads and accidentogenous sites, like road crossing for instance. We refer to this last effect as a dilution effect.

To meet these issues, we first present a very simple micro-founded model of accidents in which we try to characterize the heterogeneity between towns and the possible dilution effect of SECs in large towns. This heterogeneity might be related to differences between towns in traffic or in danger associated to accidentogenous sites. This model leads us to propose different specifications of the relationship between the number of accidents and SECs, in particular related to the town's size.

To show the interest of this approach, we compare the findings derived from our micro-founded approach with estimations obtained by using standard specifications. We show that these results might be misleading with respect to the heterogeneity of accidents between towns. In particular, simply regressing the difference in the number of accidents before and after the set up of an SEC results in a seemingly substantial decreasing effect of SECs on accidents. We show that the specification on which this estimation relies is strongly contradicted by the data and we therefore propose other specifications. Most of these specifications rely on a non-linear function of a town-specific factor combined with an SEC effect, when it is located in the town. This non-linear function is derived from the structural model. We estimate the models using a general moment estimator on a quasi-differentiated representation to cancel town-specific fixed effects (Ahn et al., 2001). Note that these estimates only rely on first-moment assumptions and do not assume any parametric count-data distribution.

However, this method has two drawbacks. First, estimates depend on particular specifications, which cannot be tested and can only be claimed. The second drawback relates to endogeneity. The choice of towns to be equipped and the future trend of accidents may be caused by unobservable confounding factors. To check and solve these two problems, we use in a second part of the paper a matching estimation procedure based on the comparison of accident trends between towns equipped at date T with towns that will be equipped at $T + x$. When x is not very large, we consider that both groups can be compared, i.e. that the discrepancy of dates between control towns and treated towns is orthogonal to the outcomes of interest (here the quarterly number of accidents in the town).

The outline of the text follows. The next section briefly summarizes the main results of the literature on SEC evaluation and details the main methodological problems raised by the evaluation. Section 3 presents the micro-founded approach used to model accidents at town level and the estimation method derived from this approach. It also discusses the different endogeneity issues. The non-parametric approach and evaluation method are presented in section 4. Data and descriptive elements on recent figures covering road accidents in France are presented in section 5. In section 6, results derived using the two approaches are analyzed and compared. Section 7 concludes.

2 Position of the problem

2.1 What does the literature tell us?

Many studies try to evaluate the impact of speed enforcement cameras on road accidents and injuries. These articles use mostly English, Australian or American data, because these countries were the first to use such automatic devices to control driving speeds. Even if they use unequally rigorous methodologies, most of them conclude to significant impacts on accidents and injuries. Hence, a recent review reports (Wilson et al., 2006) a local impact ranging between a -6 and -35% decrease in accidents. All papers reported in this review are based on a difference-in-difference methodology (double comparison between before and after and between treated sites and control group of sites). The large range of magnitudes is caused by the heterogeneity of roads features, of speed limits and of the definition of the neighborhood: i.e. the exact area where outcomes are measured [1].

The question of heterogeneity has been amply debated in the literature seeking to estimate the effects of such devices on the number of accidents. The empirical Bayes approach has been developed to solve the selection problem, also named the Return To the Mean problem. The idea is that the sites where SECs are installed might also be the ones where there were randomly more accidents than should have occurred. Accordingly, if the accident-generating process is stationary, the number of accidents in such sites should be lower than in the other sites. This effect may over-estimate the impact of cameras. The empirical Bayes approach was introduced by Hauer, 1980 (see Hauer, 1997 for an extensive presentation of this method). It consists in assuming that the underlying heterogeneity of sites with respect to the number of accidents follows a Gamma distribution. Hence, the recorded number of accidents gives some information about this true heterogeneity parameter (denoted as the site-specific expected number of accidents) that the parametric specification helps to retrieve easily. Many transportation researchers use this method to assess the effect of SECs (see Mountain et al., 2005). In a recent paper, Elvik, 2008 questions the empirical validity of the empirical Bayes approach. He concludes that "it has to be preferred to alternative methods" but it "is not always accurate".

A few studies also tackle the question of halo effects. Hence, an SEC may have an impact on sites

[1] Hess, 2003 shows that impact are -46% on the immediate vicinity (250m radius) and -21% over a 2km radius

outside the immediate zone of effective control. Such effects are generally reported to be negative on accidents and injuries. This would indicate that drivers behave more carefully in large neighborhoods around controlled sites. These externalities tend to increase the benefits of SECs. But such studies are too few and these halo effects deserve more attention. In fact, other phenomena could lead to inverse externalities that are almost never put to the fore. For instance, the controlled zones could be avoided by some drivers, which could cause a traffic diversion. This might not be negligible, as many professional drivers (trucks, taxis) have GPS camera detectors or camera locations are public knowledge. This sort of diversion may have important consequences. Part of the decrease in crashes - as measured in the controlled zone - would then be due to a decrease in traffic. Mountain et al., 2004, drawing on abundant data (including traffic flows measurement) have recently showed that fixed-speed cameras installed on a 30 mph limited road network have reduced personal injury accidents by 24% and that only 19% were explained by a direct effect of SECs (and 5% due to traffic reduction). But, in that case, shifting traffic to other routes might generate more accidents.

3 Micro-founded Approach

The dependent variable that we use corresponds to the quarterly (or monthly) number of crashes in a given town. There are many towns in France (more than 36,000). Hence, in many towns the number of crashes is very low and is often equal to zero. In some towns, it can exceed one hundred per quarter.

The use of linear econometrics may be misleading, mainly because count-data have many zeros. For this reason, many papers use count-data econometrics to model these phenomena. But count-data models rely on very specific distributional assumptions: in most cases a Poisson process or a negative binomial distribution. The inclusion of explanatory variables within these models is supposed to be multiplicative. But we need a rationale to adopt this model. This rationale will be developed hereafter based on an illustrative model of the crashes.

The specification issue will also affect the way the effects of SECs are estimated: do they lower the number of crashes additively or multiplicatively ? This specification choice may lead to very different results, as we will see in section 6. The illustrative model is very helpful in understanding the implicit assumptions that drive us to adopt an additive or multiplicative specification.

3.1 The model

Consider a potential accidentogenous site j, in a town i. During a quarter t, the number of accidents y_{jt} is generated according a counting data generating process (e.g. a Poisson process or binomial negative) parameterized by λ_{jt} such that

$$E[y_{jt}] = \lambda_{jt} \tag{1}$$

The parameter λ_{jt} is affected by the installation of an SEC in site j at time t through three channels:

- the installation of the SEC at site j itself
- the installation of the SEC in the other sites in the town i that are close to site j.
- the installation of the SEC in accidentogenous sites in towns i' close to the place j, but farther away than in the previous case.

As argued above, installing an SEC in a given place may have ambiguous effects on the number of accidents, although we expect it to be negative. One difficulty, related to the data (but very often encountered in other contexts), is that we do not observe accidents at the accidentogenous site, should it be properly defined, but at the whole town level instead. Here we have an aggregation problem: large towns may have many accidentogenous sites. Hence, the higher the number of these sites, the more diluted the SEC effect will be. More detailed information can be recovered from the data on the kind of sites where SECs are installed and where accidents occur, but this information is not precise: it simply consists in a rough description of the street type.

To be able to identify SEC effects, we choose to impose some structure on the λ_{jt} parameters. We assume that they can be broken down into a time-dependent parameter λ_t and a site-specific parameter T_j such that

$$\lambda_{jt} = \lambda_t T_j \tag{2}$$

Here, λ_t will depend on the national road-safety policy: police action, national road safety campaigns, recent and general improvements in vehicles (more efficient brake system, etc.). We may be somewhat more general by making λ_t depend on some characteristics of the sites where the accidents happen: for instance the road type, the urban area type,... T_j corresponds here to the fixed characteristics of site j, which makes it particularly accidentogenous. It can be interpreted as site being a crossing road, or reflecting the average traffic that transits through this site. This interpretation holds if the variability of traffic between different sites is greater than between different time periods. At this stage, we are going to assume that T_j is the same for all accidentogenous sites in a town i, $T_j = T_i$, $\forall j \in i$, i-e the traffic is relatively homogenous in a given town and main differences in accidents across towns are related to differences in traffic level.

Each town is composed of a finite and fixed number of accidentogenous places : $N_i^1, ..., N_i^M$ places respectively of kind $1, ..., M$. The numbers $N_i^1, ..., N_i^M$ are constant over time. We assume that the accidents at a site j depends only on the road type m, the town i and time t. Hence $\lambda_{jt} = \lambda_{it}^m = \lambda_t^m T_i$. Summing up all accidentogenous sites of type m in the town i we can deduce the expected number of accidents that occur in such sites in town i at time t:

$$E[y_{it}^m] = N_i^m T_i \lambda_t^m \tag{3}$$

The direct local effect of a SEC

In this framework, the installation of an SEC should have a direct impact on the number of accidents in a given location. If this behavioral interpretation holds, this effect should be proportional to the observed traffic on site j. Let α be this so-called direct effect: it impacts the number of accidents on site j,

$$E\left(y_{jt}\right) = \left(1 - \alpha R_{jt}\right) T_i \lambda_t^m \tag{4}$$

where R_{jt} indicates whether an SEC is active in site j at time t. If we allow this effect to be dependent on the road type m, the expected sum of accidents at m-type sites in town i at time t is:

$$E[y_{it}^m] = \lambda_t^m T_i (N_i^m - \alpha_m R_{it}^m) \tag{5}$$

where R_{it}^m is the number of SECs located at m-type places in activity in the town i at time t.

Hence, the heterogeneity between towns is considered through two effects. The first one corresponds to the number of accidentogenous sites in the town (N_i^m), which can vary significantly from one town to the other. This effect is additive. The other one corresponds to the traffic that goes through town (T_i), this is multiplicative.

These two types of heterogeneity make us consider two different specifications in assessing the effect of SECs. Rural and small towns may not be very different with respect to the number of accidentogenous sites (most often only one), but they may be very different with respect to traffic flows. In this case, SECs will multiplicatively affect the number of accidents observed in the town. On the contrary, medium-sized or large towns may be very different with respect to the number of accidentogenous sites, as traffic is more similar. This setting clearly applies in large towns that suffer from road congestion, the variability of which between sites may be reduced with respect to the number of accidentogenous sites. In this setting, the effect is diluted when SECs are installed in only one site, leaving other sites unaffected.

We can then consider two models:

- The multiplicative model, such that $N_i^m = N^m$ (most often equals one), which will apply to small towns where the effect of SECs is multiplicative:

$$E\left(y_{it}\right) = \lambda_t^m T_i exp\left(\alpha_m' R_{it}^m\right) \tag{6}$$

  that is strictly equivalent to the specification above when R_{it}^m is a dummy since $\exp\left(\alpha_m'\right) = 1 - \alpha_m$ in this case.

- The "mixed" model, such that $T_i = T$ is the same across towns.

$$E\left(y_{it}\right) = \mu_t^m \left(N_i^m - \alpha_m R_{it}^m\right) \tag{7}$$

The spatial halo effect. Neglecting spillover effects inter-towns

Actually, the real impact of an SEC installed in site j may affect the number of accidents in site j' in the same town or in nearby towns. This diffusion phenomenon is denoted a "spatial halo" effect. Specifically, drivers' behavior may change exactly where enforcement is applied or where an enforcement symbol is observed. Most papers report a small halo effect in the proximity of signaled areas (speed reduction stays effective up to 1 kilometer, but rarely beyond). The "halo" distance may depend strongly on the driver's knowledge of the area. If the driver is aware of the exact SEC location, the halo effect may be very low. Consequently, the magnitude of the halo effect may be strongly related to the date on which SECs were installed. Indeed it takes more or less time for drivers (for example commuters) to learn the exact location of SECs. The halo effect may decrease over time and so would the global effect of SECs.

To focus on the direct effect of SECs, we choose to restrict our paper on the intra-town effect and to neglect potential effects of an SEC in a town i on the observed accidents in a town j. This choice may lead us to underestimate the real effects, since part of non-equipped towns may be in truth positively impacted by such a treatment. However, we believe that the small number of equipped towns (1,000 at the end of 2007) and consequently of their neighboring towns potentially impacted by the halo effect do not significantly alter the validity of direct comparison between towns that are equipped and those that are not.

3.2 Endogeneity issues

We describe here the main features of the administrative process to designate the locations of SECs.

The selection of sites equipped with SECs Like most of the studies about SEC evaluation, we would like to estimate the local effects of SECs, i.e. how they affect drivers' behavior to diminish the number of accidents. Unlike most of them, we can use the exhaustive aspect of our data to estimate the total number of prevented crashes resulting from this policy.

The installation of Speed Enforcement SECs was organized in the following way. [2]

1. The Internal Affairs Ministry allocates to each French "département" (area similar to a U.S county) the number of SECs to install. The criteria included the past number of crashes (average in 2001 and 2002) and the average speed.

2. Within each *"département"*, State representatives organized meetings with local road infrastructure

[2] All the procedures are summarized by the government notice NOR/INT/K/04/C of the 3rd February 2004.

offices (highways, high-traffic and medium-traffic roads, and town roads) to propose sites to install the SECs, to maximize their impact on the number of accidents and their seriousness.

These sites were chosen on the basis of indicators that characterize local accidentogenous conditions (on the roads that were eligible to be equipped):

(a) mean number of accidents per km and per year (computed over the five past years 1998-2002 or 1999- 2003)

(b) mean number of people killed or seriously injured per km and per year (computed over the five past years 1998-2002 or 1999-2003)

The 700 first sites were selected in 2003 and early 2004 for both 2004 and 2005. Authorities' efforts to formalize and make these choices transparent were remarkable. They wished to minimize the risk of legal challenges from drivers' associations, which were often consulted [3]

This process raises several concerns about endogeneity issues :

- **Issue 1: Heterogeneity in level.** SECs are installed at the very sites where the number of accidents is highest. It is crucial to understand how this heterogeneity affects changes in the number of accidents (or at least to control for it), to build a convenient counterfactual of treated towns.

- **Issue 2: Specific dynamics.** SECs might be installed in sites where accidents have recently occurred. Using a Difference-In-Difference identification of the impact without any precaution may lead to an overestimation of the SEC effect, since the SEC may have been installed because of this accident (see Heckman and Smith, 1999 for a seminal presentation of the problem).

- **Issue 3: Long-term trends.** SECs might be installed in sites where accidents are expected to increase more (or decrease less) than on average in the long run. For example, for a given level of accidents at date t, towns undergoing expansion (or with younger than average inhabitants) may be characterized by such phenomena. In this case, controlling only for fixed heterogeneity (in level) would underestimate the SEC effect.

- **Issue 4: Simultaneous policies or phenomena.** Installation of SECs may be accompanied by other changes that might affect drivers' behavior. One example is the presence of police. As police

[3] see Hamelin, 2008 for monographies on some départements.

control becomes less useful in sites equipped with SECs, authorities could reinforce police control in other accidentogenous sites. That could lead to an underestimation of the real impact.

Experimental designs, by affecting SECs to randomly chosen towns (treatment group) and comparing them to non-equipped towns (control group), can overcome most endogeneity biases described above. Unfortunately, no such design has been organized yet, either in France, or in other countries.

However, even in the absence of experimental design, let us examine how these biases might be overcome, by adopting various estimation strategies.

The first issue (endogeneity due to the heterogeneity in level) can be dealt with by using panel data, which allow the time variability of the number of accidents at a given site to be used. This is the main objective reached by the previous "micro-founded" models, which try to control for this heterogeneity, as it explains most of the differences in accident numbers between towns. In particular, we will see that assuming an additive separability for the temporal term and the individual effect is not a convenient way to model the phenomenon.

The second endogeneity issue concerns short-term dynamics before the SECs were installed. Actually, based on the institutional context, this source of endogeneity does not seem to be really plausible. Dramatic one-off accidents cannot have any consequence on the installation of an SEC at the place of the accident. The choice of towns to be equipped is based on permanent indicators (over 5 years between 1999 and 2003) [4]. Anyway, a possible way to deal with this endogeneity issue would be to assume weak exogeneity in the "micro-founded" models developed in the previous section (see the estimation section below)

Issues 3 and 4 may cause the estimation of the real causal impact of SEC to be biased. To account for long-term trends or for other simultaneous policies or phenomena, we develop in the last part of this paper an alternative approach that relies on the "natural experiment" aspect of setting up SECs in the different sites. Specifically, we split our sample considering sites where SECs have just been installed and compare them with sites where SECs are about to be installed (see section 4 for an application of this

[4] It may however remain a doubt on the effective temporality of equipment of the chosen towns, but this source would be probably negligible.

method).

Exogeneity assumptions In all previous models (additive, mixed or multiplicative) we assume $E[\epsilon_{it}] = 0, \forall t$, where $\epsilon_{it} = y_{it} - f(\alpha, R_{it}^m)$is the residual difference between the observed number of accidents y_{it} and the predicted number of accidents $f(\alpha, R_{it}^m)$ which corresponds to the relevant specification (6) for the multiplicative model and (7) for the mixed model. It is also necessary to have very clear assumptions regarding the relationships between the residual term and the treatment variable (here the dummy R_{it}: being equipped by a SEC or not at the date t). These assumptions induce the estimation method.

- **Strict exogeneity** $E[\epsilon_{it}|R_{i1},...,R_{iT}] = 0 \forall t$. In this assumption, for a given town, residual ϵ is fully independent from the decision to install a SEC, and even its possible anticipation. This is a strong assumption, although it seems to be supported by the description of the administrative process (see the previous paragraph). Actually, the decision to install an SEC is adopted long before it is effectively installed.

- **Weak exogeneity** $E[\epsilon_{it}|R_{i1},...,R_{it}] = 0$. We allow here residuals at date t to be correlated with the future R_{is},$s > t$. ϵ_{it} is a shock that is independent from the past, but might affect the probability of the installation of an SEC. This assumption encompasses the case where local authorities react to specific shocks in specific sites by setting up SECs there (i.e. second endogeneity issue). The comparison between this estimate and the one based on strict exogeneity (see above) might be considered as a test of the way the installation of SECs was completed, since the administrative process was designed to rule out such phenomena.

These two alternative assumptions do not lead to same optimal GMM estimators (see Wooldridge, 1997 for a more detailed presentation in the case of a multiplicative fixed effect model).

3.3 Estimation of the micro-founded model

In the literature about accidents, count-data model are often used. We do not use these models and rely instead only on first-moment conditions. First, count-data models often rely on parametric assumptions that are required to use maximum likelihood methods. Second, in our estimates, we are going to loosen some exogeneity assumptions (for instance estimate the model under only weak exogeneity assumptions).

It is not possible to do so with maximum likelihood. The drawback of using only first-moment conditions is that we may have less precise results, although they will likely be more reliable.

The mixed and multiplicative models are characterized by the presence of temporal effects multiplicatively separated from individual fixed effects (called "interactive fixed effects" (see Bai, 2009)). The multiplicative model is made even more complex by the introduction of $exp(\alpha R_{it})$ to account for the SEC effect. To estimate this model, the literature proposes two different strategies: one relies on the concentrated least squares (CLS) introduced by Kiefer, 1980, developed by Ahn et al., 2001 and generalized by Bai, 2009 for multi-factor models. In this paper, we rather use the second strategy (detailed by the article of Ahn et al., 2001). It uses the General Method of Moments applied to a quasi-differentiated model. More precisely, we adapt the first GMM-estimator proposed by Ahn et al., 2001 to a within quasi-differentiation.

Estimation under strict exogeneity We assume here that strict exogeneity holds. To solve the incidental parameter problem, we use the classical within estimator in an additive fixed effect model. As the fixed effects are multiplied by a temporal term, we quasi-differentiate the model to remove individual fixed effects.

The difference with Ahn and alii's paper is that they use a quasi-differentiation between the current period and the first one. For robustness reasons[5], we prefer to differentiate the current period with the mean over the whole estimation period.

Mixed Model : $y_{it} = \lambda_t \alpha R_{it} + \lambda_t \mu_i + \epsilon_{it}$ The strict exogeneity for the SEC installation, means that for each i

$$E[\epsilon_{it}] = 0;\ E[\epsilon_{is}|R_{it}] = 0,\ \forall s, t$$

Then, noting $z_{it} = y_{it} - \lambda_t \alpha R_{it}$, we quasi-differentiate the model to get rid of the fixed effects in the following way :

$$u_{it} = z_{it} - \lambda_t \frac{\sum_{u=1}^{T} z_{iu}}{\sum_{u=1}^{T} \lambda_u} \tag{8}$$

[5] if the fixed effect is not really fixed (for example affected by a slight drift) it may be more accurate to quasi-differentiate using the mean than the first period as an anchor.

Hence, $u_{it} = \epsilon_{it} - \lambda_t \frac{\sum_{u=1}^{T} \epsilon_u}{\sum_{u=1}^{T} \lambda_u}$. The exogeneity conditions become then $E[u_{it}] = 0$; $E[R_{is} u_{it}] = 0$ since the term $\mu_i$vanishes. Several GMM-estimators are proposed by Ahn and alii.. We use only the first one they propose, based on the contemporaneous exogeneity conditions only $E[u_{it}] = 0$; $E[R_{it} u_{it}] = 0$

The multiplicative model is estimated by using a similar procedure. The coefficient incorporates also $exp(\alpha R_{it})$

Multiplicative Model : $y_{it} = \lambda_t \mu_i exp(\alpha R_{it}) + \epsilon_{it}$ The strict exogeneity assumption means that for each i,

$$E[\epsilon_{it}] = 0; E[\epsilon_{is} | R_{it}] = 0 \forall s, t$$

Noting

$$u_{it} = y_{it} - \lambda_t exp(\alpha R_{it}) \frac{\sum_{u=1}^{T} y_{iu}}{\sum_{u=1}^{T} \lambda_u exp(\alpha R_{iu})} = \epsilon_{it} - \lambda_t exp(\alpha R_{it}) \frac{\sum_{u=1}^{T} \epsilon_{iu}}{\sum_{u=1}^{T} \lambda_u exp(\alpha R_{iu})} \quad (9)$$

As in the mixed model, the contemporaneous orthogonality conditions are : $E[u_{it}] = 0$; $E[R_{it} u_{it}] = 0$. We derive then the standard GMM-estimator.

Estimation under weak exogeneity The principle is identical. The only difference is that the current period is quasi-differentiated with the average over the period forward. Hence, for the mixed model, we define

$$u_{it} = z_{it} - \lambda_t \frac{\sum_{u=t}^{T} z_{iu}}{\sum_{u=t}^{T} \lambda_u} \quad (10)$$

and for the multiplicative model,

$$u_{it} = y_{it} - \lambda_t exp(\alpha R_{it}) \frac{\sum_{u=t}^{T} y_{iu}}{\sum_{u=t}^{T} \lambda_u exp(\alpha R_{iu})} \quad (11)$$

We apply exactly the same logic as in the previous section.

4 A non-parametric approach

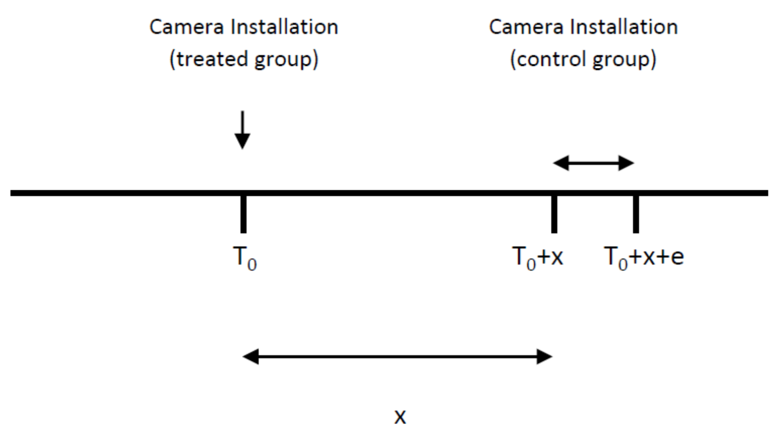
The main drawback of the structural approach is that the estimation may depend drastically on the chosen specification. We try to address this critic partially by introducing two elements: on the one hand, we estimate the model on groups of towns where trends in crashes are homogenous (see section 5.1) and, on the other hand, we expect fixed town effects to introduce significant flexibility. There is also the risk of results being biased by unobservable confounding factors. The previous approach implicitly compares treated towns (i.e. equipped with SECs) with towns that will never be treated or were not yet treated in 2007. Actually, a potential source of bias is that treated towns may feature accident characteristics that are different from those of the implicit control group (even in the short run).

Let us introduce some notations. $y_{it}(1)$ is the monthly number of accidents in a town i at date t if it is equipped with an SEC (treated road) (and $y_{it}(0)$ if it is not equipped). We note also W_i the dummy "being treated". The objective is to estimate the impact of the SEC on the treated towns $E[y_{it}(1)-y_{it}(0)|W_i = 1]$, in other words, to what extent would its monthly number of accidents have differed, had it been equipped with an SEC

The identification strategy developed in this section is based on the comparison between treated towns (i.e. where an SEC has been set up in the period 2003-2007) and a control group of towns that is as comparable as possible. To solve the potential endogeneity of the choice of treated towns with the outcome, the idea is to compare by a matching procedure a town i where a SEC is set up at date T_i with towns that will be treated a few months afterwards $T_i + x$ (in fact in the interval $[T_i + x, T_i + x + e]$) (see figure 1). The validity of this identification strategy is based on the following consideration: If x is not too large, the towns treated at T and towns treated at $T + x$ are chosen quasi-simultaneously and the temporality of effective set-up can be considered random, i.e. independent of the evolution of the number of accidents, had towns remained untreated. In particular, we can rule out the risk of occurrences of any phenomena similar to an "Ashenfelter's earning dip", which might happen in cases when the probability of being equipped depends on recent past and transitory shocks. In such a case, comparing two towns equipped only a few months one after another with the installation of the SEC could lead to severely biased estimations. This does not happen here, since sites were preselected a few years before. 700 sites (out of the 1,000 present in late 2007) were chosen in 2003 and in early 2004). Choice procedures were highly formalized (see section 3.2 for more details) and based on very objective and permanent measures, which exclude reactions to specific recent events.

We introduce a supplementary parameter, e, such that towns that have installed an SEC between $T_i + x$ and $T_i + x + e$ belong to the control group. This corresponds to the following trade-off. On one hand, a very low value of e may restrict the control group to very few towns, and this might result in highly imprecise and not very robust estimates. On the other hand, a high value of e may introduce towns that would be less comparable with the treated group, since they installed SECs far later. We present the estimates for a wide range of values for e to deal with this issue.

Figure 1: Definition of treated group and control group



As the number of treated towns is rather limited, we are obliged to limit our scope to groups 0, 1 and 2 and pool them in order to reach sufficient statistical power.

We focus on the outcome $y = \overline{A}_{it}$, computed as the monthly average over the period $[T_i, T_i + x]$. We use a caliper matching strategy (see Caliendo and Kopeinig, 2008). This matching method imposes a tolerance level on the distance between treated units and control units (caliper). This helps to bring down the number of bad matches. The distance between a treated town i and a control one is defined as the difference between the monthly average of accidents before the SEC was set up for each town. It is denoted P_i in the following formula and equal to the average of accidents over the period $[1, T_i - 1]$). To avoid any seasonality issues, we use a period equal to an integer number of years. We define the control group $\Omega_{\delta,e}(i)$ depending on the two parameters δ (caliper parameter) and e, as the set of towns j that answer to the following conditions :

- are equipped between $[T_i + x, T_i + x + e]$
- $|P_i(j) - P_i(i)| \leq \delta$

Before considering a comparison of outcomes, we check that before the installation of SECs (before T_i), treated group and control group present the same average accidents. In fact, given the chosen caliper parameter ($\delta = 0.001$), the average number of accidents before outcomes in treated towns and control groups are similar (in terms of P_j indicator). We then compare the outcomes $\overline{y}$ on the period $[T_i, T_i + x]$ between the two groups. Specifically we compute :

$$\widehat{\alpha_{\delta,e}} = \frac{1}{N_T} \sum_{i=1}^{N_T} \left(y_i - \frac{1}{\#\Omega_{\delta,e}(i)} \sum_{j \in \Omega_{\delta,e}(i)} y_j \right) \tag{12}$$

As control groups and treated groups are comparable before the treatment, we compare the average number of accidents after the treatment. When doing so, we do not need to change our difference estimation strategy. Standard errors are computed by bootstrap procedure at the town level.

5 Data

There are 36,700 towns in France. The data contain for every town in France and quarter between the first quarter of 1998 and the fourth quarter of 2007 the number of accidents on different kinds of road: "autoroutes" (highways with speed limited to 130 km/h), *"routes nationales"* (trunk roads, on which speed is usually limited between 90 and 110 km/h), "*routes départementales*" (medium-traffic roads mainly in rural areas, speed usually limited to 90km/h), "*routes communales*" (urban areas, speed limited to 50 km/h). Graph 3 presents changes in the total number of accidents and on each different type of road. Note that the sum of casualty accidents on each road is lower than the total number of accidents. Actually, the exact type of road was not always precisely reported in 3% to 8% of accidents. This number decreases with time: in the first quarter of 1998, 7.7% of the accidents were not reported in one of the type of road above, with this number dropping to 3.7% in the first quarter of 2007.

The total number of accidents decreased significantly between 1998 and 2007 (the annual number decreased by nearly 30% over the period), with the largest decrease recorded between 2001 and 2004. During the fourth quarter of 2003, 50 automatic speed enforcement cameras were installed in different sites in France. During the fourth quarter of 2007, 1,015 radars were operational throughout France, 18 in urban areas, 490 on medium-traffic roads, 332 on high- traffic roads and 175 on highways. In Graph 3, we can see that the number of accidents began to decline before the installation of the first SEC. The huge controversy about the real impact of SECs stems from this fact. On the one hand, many traffic experts are convinced of the efficiency of the massive introduction of SECs. On the other hand, some driver associations point out this absence of simultaneity between the installation of SECS and the beginning of the decline in accidents. They point out that although there was a huge increase in the number of SECs during 2005, the number of accidents seemed to decrease very slowly during the same year (see Graph 3). In particular, they are calling for lower fines for drivers who are caught driving above the speed threshold or for a smaller number of SECs to be installed in the future.

The drop in the number of accidents is mostly visible on high- and medium-traffic roads. It has been far smaller on highways and urban roads. In any event, a very low proportion of accidents occurs on highways and vehicles are moving under very safe conditions. Therefore, achieving marginal progress is not easy. Accidents inside urban territories might be less related to speed causes, especially when they involve pedestrians. Accident reduction varies as a function of town size (graph 8). Over the 1998-2007 period, accidents decreased more in rural and small towns than in large towns. In Paris, the reduction was even negligible (despite a significant fall between 2003 and 2005, but it was offset subsequently because of an increase in motorists' accidents).

Graph 6 shows the localization of SECs as a function of the risk of accidents. As expected, the probability that a town is equipped with an SEC increases gradually with the number of accidents per inhabitant. Between 5% and 6% of the more accidentogenous towns are equipped (6 highest deciles). Conversely, 0% to 1.5% of the less accidentogenous towns are equipped (7 highest deciles).

Heterogeneity of the town-specific accidents pattern Between the first quarter of 1998 and the fourth quarter of 2007, no casualty accident was reported in 5,116 towns out of 36,700, and only one in 4,429 towns. On the other hand, 1,060 towns reported more than 100 accidents over the period, 4 towns reported more than 10,000 accidents over the period. Graph 4 presents the distribution of the number of accidents per town (log of accidents in X-axis and log of the number of towns whose number of accidents is greater than x in Y-axis) at two dates (Q1 of 1998 and Q1 of 2007). It shows high heterogeneity between towns. The graph is close to a straight line. So the distribution of accidents can be roughly modeled by a Pareto distribution, with fatter tails than a Gaussian distribution. At a given moment, there are no accidents in more than 25,000 towns, i.e. in more than 70% of the towns. This issue has important consequences on the specification of statistical models. Because of this huge mass of zeros, linear models cannot mimic the distribution of accidents adequately.

5.1 Consistency of model with data

One of the main issues raised by the estimation method concerns the breaking down of the number of accidents between town and time dimension.

Building clusters with homogenous patterns for λ_t The mixed and multiplicative models assume that the change in the number of accidents can be summarized in one or two temporal trends once the town-specific coefficient is accounted for. This means that accident patterns between two towns can be related with each other by a simple linear relationship, whatever the size and the location of the town. This is a strong assumption that does not seem to be accepted, considering for instance the differences in accident trends between the Greater Paris Area and rural towns. Hence, we build sub-samples of towns for which changes in the number of accidents are close enough for this homogeneity assumption to be accepted.

We use four variables that describe the main features of towns. They include population density, log of the population and log of the surface area. The four first powers of these variables are included in the model. Lastly, the model includes the urban category.

The mixed or multiplicative models are based on each group of town being affected by the same common trend λ_t. The objective here is to build subsamples of towns where the estimated λ_t have the same within group temporal profile. The following procedure is used:

- **Step A** : we perform a regression of the sum of the accidents over the whole period (1998-2007) N_i on the variables described above. We get the predicted propensity score $\hat{p}_i$ from this regression.

- **Step B** : The towns are initially clustered into 19 groups. The thresholds that define theses groups are based on the ninth N_i first deciles 10 to 90 and the next percentiles 91 to 99. For each town, the statistic $\zeta_{it} = \frac{y_{it}}{y_{i.}}$ is computed. According to the mixed or multiplicative model, without the influence of SEC, we have :

$$\zeta_{it} = \lambda_t + \frac{\epsilon_{it}}{\theta_i} \tag{13}$$

  We apply then the following iterative procedure

- For each pair of consecutive clusters k and $k+1$, we estimate the model on the towns i that belong to these clusters and introduce as supplementary explanatory variables time dummies interacted with towns belonging to cluster l and not k.

$$\zeta_{it} = \lambda_t^0 + \lambda_t^1 1_{i \in k+1} + \nu_{it} \tag{14}$$

  If the trends were homogenous between the two clusters, the coefficients $\hat{\lambda}_t^1$ should be equal to zero. This is what we test here. The Wald statistic can be computed. Under the null, it follows a Chi-square with 40 degrees of freedom (number of time dummies). When the test is accepted (i.e. the p-value is greater than 0.05), clusters are grouped. However, comparing clusters $k-1$, k, and $k+1$, the test may be accepted grouping $k-1$ and k or k and $k+1$. In this case, we group as a priority the clusters for which the test's p-value is the highest. The remaining clusters will be considered at a further stage of the procedure. The procedure is iterative: after this stage, it comes back to the beginning of step B with the new definition of the clusters, which results from the grouping described above.

- At the end of Step B, we check whether estimates of λ_t are homogenous within each cluster. To do so, we slice each cluster into two samples (above and below the median). We also test whether the two series of coefficients are identical.

We have chosen to consider the number of accidents N_i to group towns with each other. Although this choice seems to be natural, it could be debatable, especially since the profile of accidents is not homogenous over the whole sample. To check this, we have run a principal component analysis using the series of all accidents over the period as dependent variables. It shows that variable N_i explains 97% of the variance of z_{it} over the whole sample and 33 % when we restrict the analysis to the towns with fewer than 100 accidents in 1998-2007.

Applying this procedure, we end up with 7 clusters for the whole sample and 6 if we exclude highways (table 1). Table 2 presents the distribution of the population of towns contained in each cluster.

Checking the validity of multiplicative fixed effects Without SECs, the two models (mixed and multiplicative) can be written in a very simple way.

$$E(y_{it}^m) = \lambda_t^m \theta_i^m$$

Fixed effects affect the expected number of accidents in a multiplicative way, which is not the usual way to consider heterogeneity. As for reduced form models, most common evaluations prefer to estimate the usual linear model with additive fixed effects

$$E(y_{it}^m) = \lambda_t^m + \theta_i^m$$

for which usual estimation methods can apply. Most often, it is natural to transform the data to find a linear breakdown, for instance by taking the logs of the dependent variable. We cannot apply such a transformation here because the number of accidents is often equal to zero, especially in small towns [6]

If the time dimension were not too long, the linear approximation would give similar results to the

[6] A transformation $\log(y_{it}^m + k)$ could also be used, that treats the zero issue. The problem here stands with the choice of k, which may deeply affect the results. Moreover, we could not find a structural representation of the mechanism that was consistent with this representation.

multiplicative breakdown, if μ_t^m does not vary excessively. This approximation may not be reliable when the time dimension becomes too important.

To test whether the multiplicative representation fits the data well, we estimate a model with two trends: one is multiplicative and the other is additive.

$$E(y_{it}^m) = \lambda_t^m \theta_i^m + \mu_t^m$$

Two identification conditions are necessary to estimate the model: $\overline{\lambda_t} = 1$ and $\overline{\mu_t} = 0$. The estimation method is the same as the one used for the structural model. The additive model will be tested using the Wald statistics of the hypothesis $\lambda_t \equiv 1$. This model is estimated for each group (7 groups for the whole sample). λ_t^m and μ_t^m are first estimated by the method explained in section 3.3. Then θ_i^m is estimated through a regression of y_{it}^m on λ_t^m and μ_t^m for each town i.

Results are summarized in table 3. The temporal dummies crossed with individual fixed effects explain a substantial part of the variance of the endogenous variable. In group 0, the explained variance represents 15% of the total variance. the explained variance increases with the town size. For group 3 and 4, the explained variance amounts to 50% and 90% for group 6. We also compute the Wald statistics. The equality to 1 of λ_t (t=12,...,40) is strongly rejected. The Wald statistics follow a $\chi^2(40)$ (40 degrees of freedom). The 95% percentile of the $\chi^2(40)$ is 55.75.

6 Results

6.1 Semi-parametric models

Models with no temporal effects Both multiplicative and mixed models are estimated on each group as described in the previous section. Table 4 presents the results for all roads and table 5 for all roads except highways. Because of the imprecision of codification of the localization of road accidents, it is unfortunately impossible to estimate the models at a more detailed level. We added in each Table the results of a pure additive model estimated by the standard within difference method. This one corresponds to the difference in difference estimation strategy, commonly applied in evaluation literature. The second part of the Tables presents the effects on the treated towns in terms of the number of accidents per quarter and per SEC. We focus on these indicators, because they can be compared across the different groups, even if traffic density varies within the groups. The proportion of obviated accidents depends in particular on the size of towns and cannot be interpreted easily. It is more convenient to focus on the level of effects (number of obviated accidents per quarter and per SEC).

First, all three models give negative and significant effects for SEC parameters. At least, even though estimates of the impact differ significantly, they confirm that SECs did have a negative effect on accidents. We do not find any significant results for groups beyond group 3. A surprisingly strong positive effect appears in the multiplicative model for pooled groups 3 to 6. This is of course a simple statistical artifact.

For small towns (group 0 and 1), DID estimations give very different results from mixed and multiplicative estimations. DID effects are substantial and not credible. For example, for group 0 corresponding to all roads, the apparent effect would be -0.143 by quarter and per SEC, i.e. circa 45% of the quarterly average level in 2003 (resp. 31% for group 1). In fact, this model assumes that trends in accidents are parallel in the different towns (they differ only by an additive constant term). As section 5.1 shows, this model is strongly false : the real trends are far better described by a common trend multiplied by a constant idiosyncratic term. As we use long panel data, the error in this assumption leads to results that are noticeably different from actual ones. That result confirms that such additive models - very widely used in evaluation literature - must be used carefully, particularly when panels are long and when an individual intercept is clearly irrelevant.

For small towns, additive and mixed models give similar results for the impact on the treated towns. They differ only for group 2. The multiplicative model gives more obviated accidents than the mixed one (-0.14 versus -0.09 for the estimation including all roads; -0.13 versus -0.10 for estimation without highways). At this stage, it is not really possible to discriminate between the two models (even though the rationale developed previously is rather in favor of the mixed model for medium and large towns). We hope to be able to do that by comparing it with a non-parametric evaluation (section 4). At least, results for group 0 and 1 provide a common estimate of the impact for small towns.

Models with temporal effects The same models are estimated but we introduce temporal flexibility in the SEC impact. We enable the impact of SECs to be potentially different at different ranges after their installation. Hence, the term αR_{it} is replaced by $\alpha_1 R^1_{it} + \alpha_2 R^2_{it}$. R^1_{it} is the number of SECs that were installed less than 6 months before. R^2_{it} is the number of SECs installed earlier than 6 months before t. Tables 6 and 7 report the set of these two parameters respectively for the whole sample and the whole sample without highways. As previously, the upper part of the tables reports the SEC parameter estimates and the lower part the impact on the treated towns.

In these Tables, we did not report the estimates given by the additive model, for this model is clearly rejected. The same pattern appears: mixed and additive model give similar results for the first groups whereas they diverge for group 2 (relatively bigger towns). For the latter group, the multiplicative model leads to larger SEC impacts (in the near or medium term) than the additive one.

The main result is that, for groups 0 to 2, the effect of SECs decreases over time. Of course, the short-term estimates are stronger than the previously obtained estimates. Hence, for group 0 (all roads), the short-term effect is between -0.087 and -0.097 (to be compared with -0.057 when only a fixed parameter is introduced). For group 1, the short-term effect is between -0.126 and -0.142 (compared with -0.06 obtained previously). The effect over the later period (from the 3rd quarter) remains significant but its level is often lower than or close to half of the short term effect. This phenomena appears systematically for each group and for each sample (with or without highways).

The test of over-identifying restrictions (Hansen's Test) is performed for each estimation. The hypothesis of joint nullity of moments is accepted most of the time. The estimations corresponding to a weak exogeneity assumption were achieved, but we did not get any significant estimates (results not reported here). Standard errors rose to a very high level and made any interpretation of results impossible.

To our knowledge, this paper is the first to examine the effect of SECs in the medium or long run. We show that the impact of SECs on accidents decreases over time, being roughly halved six months after the installation. Such a result has never been addressed by the literature, since the duration of observation was most often shorter. Several explanations might account for this decrease. First, drivers may over-react just after the SECs are installed, and be far more cautious in sites close to the SEC location, reducing their speed on a longer distance than necessary. With time, drivers may gain assurance and their level of road attention might decline, and this would also reduce the SEC effect. Another interpretation would be that drivers may learn rapidly where SECs have been installed, but it could take more time to find other routes to reach their destinations. We cannot disentangle these two explanations, because we lack traffic data.

Whatever the reason, this result (which is also confirmed by the non-parametric estimation) indicates that fixed SECs are not an adequate instrument for controlling the behavior of drivers in the long run. New instruments, such as mobile SECs or speed displaying devices, etc. might be more effective, because it would be more difficult for drivers to adjust their behavior to them. Another way would be not to signal SECs or introduce surveillance devices over large areas, so that drivers would be obliged to behave in a safe manner persistently.

Estimations restricted to equipped towns In the previous estimations, we attribute a set λ_t common to all towns, whether equipped or not. Let us consider for example that the specification chosen in the mixed or multiplicative model is true but that the two groups of towns (treated and non-treated) have different long-run accident trends patterns: in that case, the sets λ_t are not identical for treated units and for non-treated units. If we estimate these two models only on treated towns (i.e. the ones that will get an SEC during the observation period), it should be possible to control for the trend specific to the treated towns and measure the SEC impact.

Table 8 presents the results of the estimations restricted to the towns that were equipped between 2003 and 2007. The SEC effects appear to be smaller than the previous analog estimates on the whole sample. For example, the quarterly impact ranges from -0.149 to -0.113 (multiplicative model) or from -0.130 to -0.105. Graph 9 verifies that the hypothesis of common λ_t for treated and non-treated towns is fragile. The estimates achieved for treated towns are far outside the confidence interval of estimates on the whole sample. The trend for the former is more often above the latter. When the model estimation imposes a common trend, it biases the SEC estimates.

6.2 Non-parametric evaluation

The table 9 presents the results obtained by the non-parametric evaluation. For each combination of parameters (x, e), the Table gives the results for the comparisons of accident averages before the installation of SECs and the comparison of outcomes over x quarters thereafter. In this Table, the caliper parameter δ (see section 4) is chosen equal to 0.001 (see next paragraph about the sensitivity test to the choice of δ). The three first lines report the results for gathered classes 0, 1 and 2 corresponding to all roads. The three last lines correspond to the same definitions but we eliminate highways. Unfortunately, the same method does not work well when applied to other groups, due to the small number of treated towns.

When $\delta = 0.001$, accidents patterns before the installation of SECs are never significantly different between the treated group and the control group. We thus consider that treated towns and control towns are similar and we can compare outcomes after the SECs were installed without any additional assumptions.

We find significant impacts on the treated towns (all roads) for $x = 6$ months. Over the two first quarters, the impact is between -0.070 and -0.080. For $x = 12$ months, estimates decrease (between -0.052 and -0.065) and become non significant for $x = 18$ months. The results are similar when highways are removed from the sample. On this sub-sample (highways being removed), the quarterly impact is significant in the short run (but only at 10%): between -0.061 and -0.074 within 2 quarters after the SECs were installed. The impact becomes non-significant when it is computed over longer periods (over 1-year). However, the results obtained for x=4, 6 and 8 quarters do not prove that the long-run effects are negligible. What we prove here is that the apparent local impact of SEC decreases over time, but not down to zero. Actually, standard errors represent between 5% and 10% of the average number of accidents and it is impossible to obtain very precise estimates of the impact with this method, particularly for medium-term impacts (the control group size decreases).

Sensitivity to parameter choices Note that the results are very sensitive to the choice of e. For weaker values of e, we get smaller impact estimates. This raises some questions about the robustness of the method. Moreover, for $x = 3$ months, impacts seem to be small and non-significant. But that phenomenon is probably due to the lack of precision of the measurement of outcomes over a short period (3 months). The empirical average of accidents is more variable over 3 months than over 6 or 12 months. Accordingly, the number of treated towns is not high enough to measure the SEC impact appropriately in the very short run.

Table 10 and 11 study the sensitivity to the choice of the caliper parameter δ. Impact estimates remain remarkably few sensitive to the choice of δ. The larger δ is, the larger the number of treated towns included in the matching estimation (i.e. for which control towns can be found), and the larger the distance between treated towns and control ones (noted Δ_{before} average number of accidents before the installation of SECs). For $\delta > 0.007$, Δ_{before} becomes significantly positive, although it remains very low (and in particular negligible with respect to $\widehat{\alpha}$. The estimator $\widehat{\alpha}$ decreases (treated towns are more and more structurally - albeit slightly - accidentogenous than control towns).

Comparison of the micro-founded and non-parametric approaches The non-parametric method used here makes it possible to overcome the two potential endogeneity and of mis-specification issues inherent in the previously used semi-parametric method. However, its essential drawback consists in the lack of statistical power, due to the small number of treated and control units. The main result is that the two methods converge to show the decrease of the impact over time. It is a first strong result, rarely put to the fore.

We compare the results for the impacts over the first six months obtained through the two approaches (via the micro-founded model and via the non-parametric one). The magnitudes of non-parametric effects (reported in Table 12) remain significantly lower than the structural ones. The difference between the two methods appears particularly significant with respect to the sub-sample without highways: from -0.061 to -0.074 on the one hand and from -0.130 to -0.149 on the other. The effects suggested by the semi-parametric micro-founded approach appear to be twice as significant as those derived using the non-parametric approach. However, the two approaches bring more similar results when semi-parametric estimations are performed only on treated towns, i-e towns that are equiped with an SEC during the observation period. The change is particularly obvious for the set of roads without highways. The discrepancy between the two is reduced by nearly 50%. The two estimates, obtained by the two approaches become not significantly different. The use of the two approaches confirm the magnitude of the real impact of SECs.

7 Conclusion

One of the strongest conclusions of our paper is that, in France, between 2003 and 2007, speed enforcement cameras did have a significant effect on the number of accidents at sites where they are located. As we measure outcomes at the town level and therefore on very heterogeneous areas, it is impossible to define and measure a structural parameter that could summarize the impact of the SECs. However, in the smallest towns (group 0 and 1), the number of obviated accidents in the 6 first months after SECs were set up can be quantified as comprised between 15% and 25% of the total number of accidents in a year in towns where SECs were installed.

Our contribution presents an important methodological aspect. We have used two radically different evaluation techniques: the first is based on a micro-founded model estimated using a semi-parametric approach and the second is based on a non-parametric identification and estimation. They are thought to be complementary. The non-parametric one suffers from a lack of statistical power but tackles the potential endogeneity or mispecification issues that might not be summarized by the fixed effects included in the first model. The two methods deliver quite similar results. They also converge to show that a strategy based on a simple Difference in Difference estimation is significantly false.

Another important result is that the two methods clearly show that the average effect of SECs on equipped towns decreases over time. It is no longer significant after 12 or 18 months. As far as we are aware, this effect has not been reported in the specialized literature, which often focuses on short-run effects. In our opinion, a possible explanation of the decrease in the SEC effect is that the "spatial halo" effect of SECs fades over time. After a learning period following the setting up of SECs, i.e. when they know where the SECs are located, most drivers apparently adopt strategies that dampen the SEC effect, for instance lower speed but increasingly restricted to the immediate vicinity of the SEC or the use of alternative routes.

In fact, fixed SECs are very precisely signaled to drivers. In all likelihood, this fact makes them inefficient in terms of reducing accidents in the long term. France recorded between 2002 and 2007 one of the sharpest decreases in the number of accidents ever but this decrease was probably not due to the direct effects of SECs. It may have been caused by several simultaneous policies (introduction of a driving license penalty points system, more aggressive information campaign, increase in human speed control and field police presence, development of traffic circles). The announcement of a big development of SECs may have played a part in modifying driver behavior but its mechanical and direct effect is unlikely to explain even a small part of this decrease.

The French government has already decided to develop new speed control techniques. Hence, new generations of SECs are being tested, including devices for controlling the average speed over longer stretches or areas or speed SECs embedded in mobile vehicles or placed in variable and non-signaled locations.

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Table 1: Clusters built for each type of roads

	number of clusters	retained quantiles
Accidents	7	60 76 93 97 98 99
Highway accidents	4	10 40 70
other roads accidents	6	36 73 93 96 98

Figure 2: Killed persons in road accidents in Europe

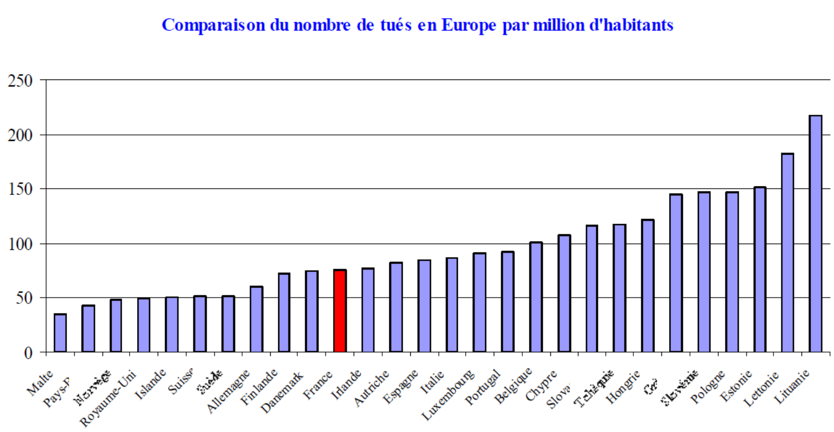

Figure 3: Number of accidents and active SEC per quarter

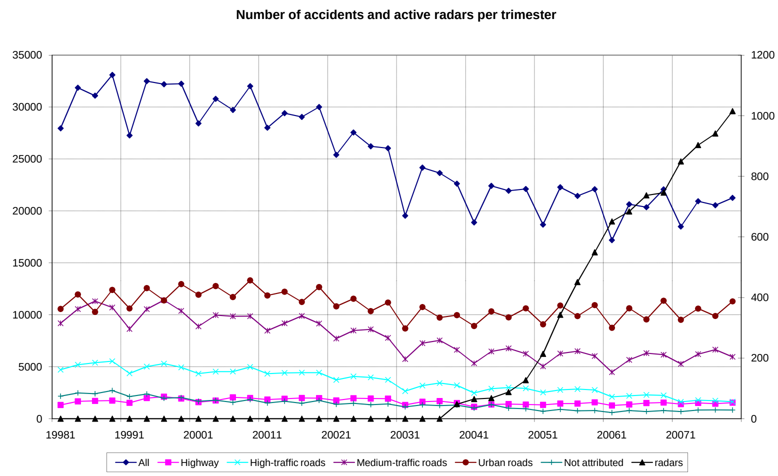

Figure 4: Heterogeneity of the number of accidents per town

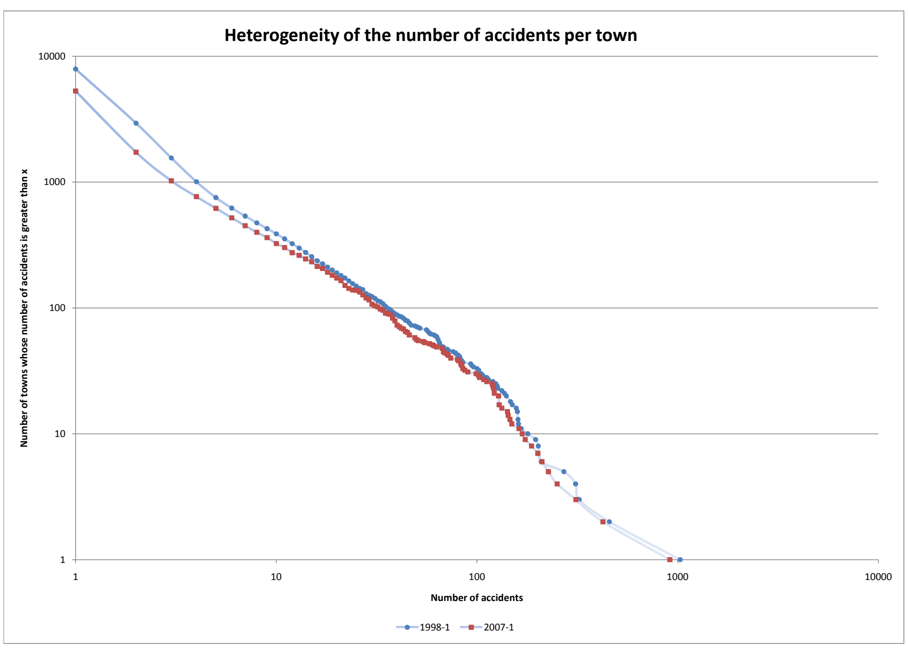

Figure 5: SEC's set up timing

Figure 6: Fixed SEC and previous accidents

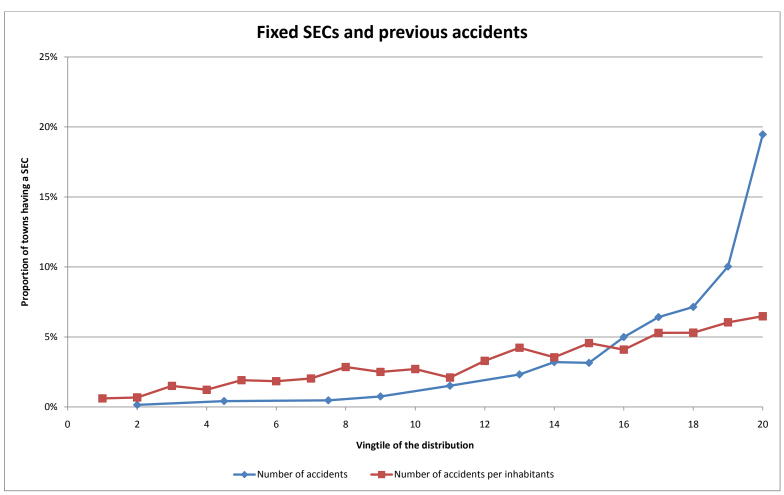

Figure 7: Number of accidents per quarter and urban area type

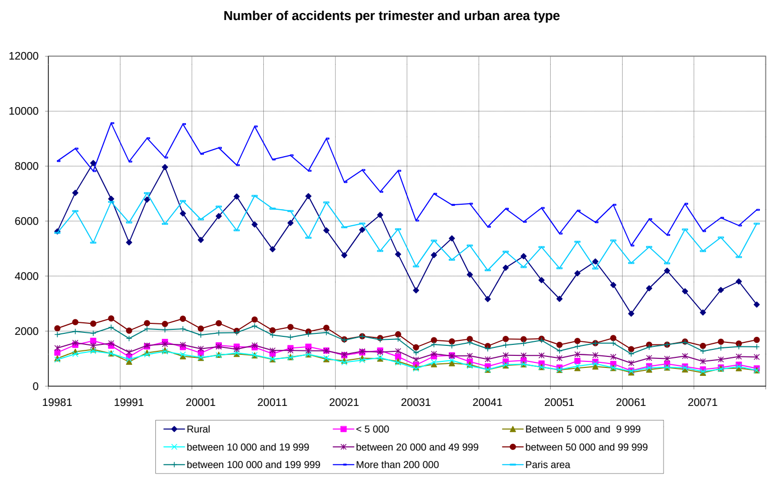

Figure 8: Evolution of accidents per urban area type

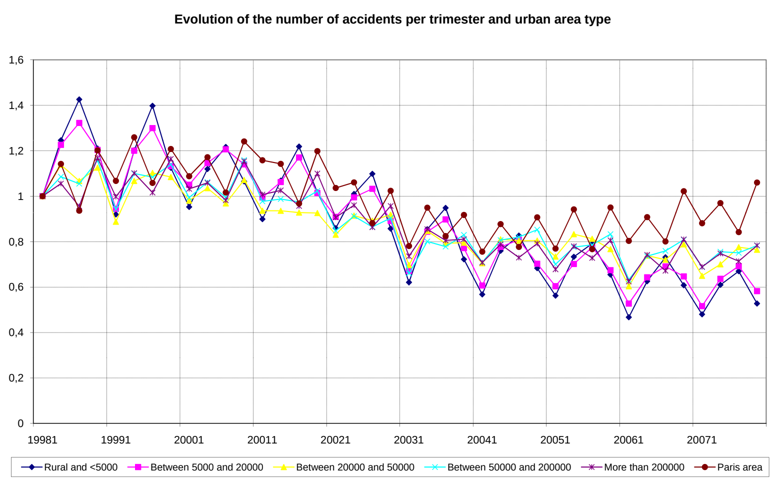

Figure 9: Comparison of temporal trends - estimates on the whole sample and on the treated towns

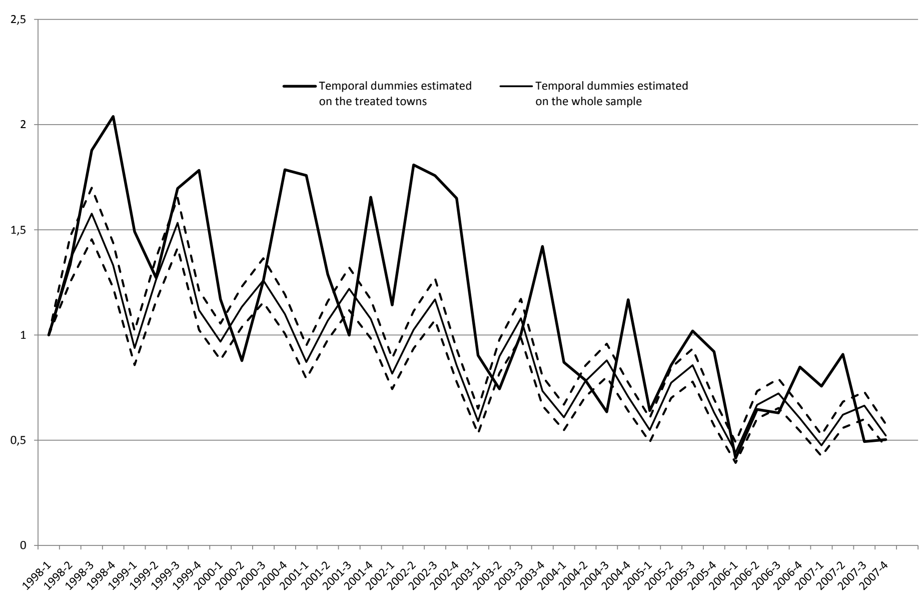

Table 2: Description of clusters

	Number of towns	Town Population Quantiles 10	25	50	75	90
		All roads				
0	20239	88	143	239	370	488
1	5396	604	662	764	898	1005
2	5734	1142	1319	1721	2405	3105
3	1349	3955	4478	5392	6619	7805
4	337	7907	9079	10250	11496	12712
5	337	12779	14592	17059	20090	23017
6	337	25213	29634	39722	59189	132844
		All roads (except highways)				
0	12118	71	109	163	219	260
1	12451	317	373	497	661	808
2	6730	1013	1170	1550	2249	3021
3	1009	3859	4275	4921	5733	6418
4	673	6518	7351	8621	10420	11952
5	673	14164	17059	24067	39722	72412
		Highways				
0	395	110	167	247	421	916
1	1183	153	235	382	643	1198
2	1183	514	750	1144	1788	3502
3	1182	2217	3434	7087	19660	43663

Source : National Casualty Accidents Database 1998-2007 - Observatoire National interministériel de la Sécurité Routière

Table 3: Multiplicative or additive model ? (all types of roads)

group	0	1	2	3	4	5	6
% of variance explained by $\lambda_t \mu_i$	0.15	0.18	0.35	0.47	0.53	0.71	0.97
Wald test (joint test $\lambda_t \equiv 1$)	9535	3298	4492	1050	281	572	2533

Source : National Casualty Accidents Database 1998-2007 - Observatoire National interministériel de la Sécurité Routière

Table 4: Effects of SECs on accidents - all roads
Strict Exogeneity

	Group 0	Group 1	Group 2	Group 0-1-2	Group 3-4-5-6
SEC Parameter estimate					
Additive model (Difference-in-difference)					
	-0.143**	-0.147**	-0.231**	-0.235**	-7.11**
	(0.01)	(0.019)	(0.021)	(0.008)	(0.106)
Mixed model					
	-0.093**	-0.104**	-0.147**	-0.127**	-0.818
	(0.020)	(0.035)	(0.037)	(0.030)	(1.008)
P-value (Hansen's Test)	0.45	0.91	0.75	0.55	0.02
Multiplicative model					
	-0.224**	-0.164**	-0.133**	-0.147**	0.011**
	(0.044)	(0.052)	(0.029)	(0.024)	(0.004)
P-value (Hansen's Test)	0.57	0.91	0.76	0.40	0.03
Impact on the treated towns					
Additive model	-0.143**	-0.147**	-0.231**	-0.235**	-7.11**
(Difference-in-difference)	(0.01)	(0.019)	(0.021)	(0.008)	(0.106)
Mixed model	-0.057**	-0.063**	-0.094**	-0.079**	-0.637
	(0.012)	(0.021)	(0.024)	(0.019)	(0.795)
Multiplicative model	-0.058**	-0.056**	-0.143**	-0.095**	0.743**
	(0.015)	(0.022)	(0.036)	(0.018)	(0.233)
Number of towns	18951	5054	5369	29377	2210
Quarterly Accidents average in treated towns (2003)	0.32	0.47	0.91	0.61	24.49

GMM estimation : explained variable = number of accidents. In the upper part of the table, each case reports coefficients of cameras from the three different models and for different clusters or set of clusters.
In the bottom part, each case reports the impact of cameras on the level of accidents in equipped towns.
P-value refers to the Hansen's test of GMM over-identifying restrictions.
Source : National Casualty Accidents Database 1998-2007 - Observatoire National interministériel de la Sécurité Routière.

Table 5: Effects of SECs on accidents - All roads (excepted highways)
Strict Exogeneity

	Group 0	Group 1	Group 2	Group 0-1-2	Group 3-4-5-6
SEC Parameter estimate					
Additive model (Difference-in-difference)					
	-0.094**	-0.147**	-0.196**	-0.212**	-4.06**
	(0.013)	(0.014)	(0.019)	(0.008)	(0.14)
Mixed model					
	-0.052**	-0.111**	-0.159**	-0.110**	0.017
	(0.025)	(0.029)	(0.034)	(0.022)	(0.811)
P-value (Hansen's Test)	0.79	0.12	0.67	0.17	0.66
Multiplicative model					
	-0.176*	-0.235**	-0.155**	-0.153**	0.011
	(0.092)	(0.054)	(0.031)	(0.028)	(0.017)
P-value (Hansen's Test)	0.78	0.11	0.67	0.18	0.66
Impact on the treated towns					
Additive model	-0.094**	-0.147**	-0.196**	-0.212**	-4.06**
(Difference-in-difference)	(0.013)	(0.014)	(0.019)	(0.008)	(0.14)
Mixed model	-0.032**	-0.065**	-0.098**	-0.067**	0.012
	(0.016)	(0.017)	(0.021)	(0.013)	(0.604)
Multiplicative model	-0.033	-0.061**	-0.131**	-0.079**	0.348
	(0.021)	(0.018)	(0.031)	(0.017)	(0.524)
Number of towns	11320	11632	6288	29240	2200
Quarterly Accidents average in treated towns (2003)	0.20	0.40	0.77	0.55	19.89

GMM estimation : explained variable = number of accidents. In the upper part of the table, each case reports coefficients of cameras from the three different models and for different clusters or set of clusters.
In the bottom part, each case reports the impact of cameras on the level of accidents in equipped towns.
P-value refers to the Hansen's test of GMM over-identifying restrictions.
Source : National Casualty Accidents Database 1998-2007 - Observatoire National interministériel de la Sécurité Routière.

Table 6: Effects of SECs on accidents - all roads - Temporal effects - Strict Exogeneity

	Group 0	Group 1	Group 2	Group 0-1-2	Group 3-4-5-6
SEC Parameter estimates					
Mixed model					
six first months	-0.152**	-0.226**	-0.262**	-0.177**	0.420
	(0.030)	(0.052)	(0.046)	(0.029)	(1.112)
after six months	-0.087**	-0.092**	-0.133**	-0.094**	0.191
	(0.018)	(0.033)	(0.040)	(0.023)	(1.185)
P-value (Hansen's Test)	0.47	0.15	0.70	0.62	0.67
Multiplicative model					
six first months	-0.332**	-0.311**	-0.223**	-0.217**	-0.057
	(0.074)	(0.082)	(0.038)	(0.036)	(0.037)
after six months	-0.187**	-0.147**	-0.113**	-0.119**	-0.053
	(0.048)	(0.050)	(0.033)	(0.027)	(0.042)
P-value (Hansen's Test)	0.43	0.13	0.62	0.51	0.22
Impact on the treated towns					
Mixed model					
six first months	-0.097**	-0.142**	-0.173**	-0.114**	0.329
	(0.019)	(0.033)	(0.031)	(0.019)	(0.869)
after six months	-0.053**	-0.055**	-0.084**	-0.058**	0.152
	(0.011)	(0.020)	(0.026)	(0.014)	(0.942)
Multiplicative model					
six first months	-0.087**	-0.126**	-0.219**	-0.126**	-0.121
	(0.034)	(0.044)	(0.054)	(0.029)	(0.099)
after six months	-0.048**	-0.045**	-0.109**	-0.069**	-0.132
	(0.015)	(0.018)	(0.035)	(0.018)	(0.110)
Number of towns	18951	5054	5369	29377	2210
Quarterly Accidents average in treated towns (2003)	0.32	0.47	0.91	0.61	24.49

GMM estimation : explained variable = number of accidents. In the upper part of the table, each case reports coefficients of cameras from the three different models and for different clusters or set of clusters.
In the bottom part, each case reports the impact of cameras on the level of accidents in equipped towns.
P-value refers to the Hansen's test of GMM over-identifying restrictions.
Source : National Casualty Accidents Database 1998-2007 - Observatoire National interministériel de la Sécurité Routière.

Table 7: Effects on the treated towns - all roads (except highways) - Temporal effects
Strict Exogeneity

	Group 0	Group 1	Group 2	Group 0-1-2	Group 3-4-5-6
SEC Parameter estimates					
Mixed model					
six first months	-0.171**	-0.185**	-0.245**	-0.206**	-0.456
	(0.023)	(0.038)	(0.042)	(0.034)	(0.437)
after six months	-0.062**	-0.083**	-0.116**	-0.101**	0.127
	(0.026)	(0.031)	(0.039)	(0.028)	(0.667)
P-value (Hansen's Test)	0.43	0.29	0.79	0.73	0.33
Multiplicative model					
six first months	-0.456**	-0.331**	-0.238**	-0.255**	-0.025
	(0.086)	(0.074)	(0.046)	(0.041)	(0.020)
after six months	-0.163*	-0.169**	-0.115**	-0.130**	0.015
	(0.106)	(0.060)	(0.037)	(0.033)	(0.021)
P-value (Hansen's Test)	0.31	0.29	0.74	0.77	0.33
Impact on the treated towns					
Mixed model					
six first months	-0.114**	-0.113**	-0.157**	-0.130**	-0.339
	(0.015)	(0.024)	(0.027)	(0.021)	(0.327)
after six months	-0.038**	-0.048**	-0.070**	-0.060**	0.096
	(0.016)	(0.018)	(0.024)	(0.017)	(0.500)
Multiplicative model					
six first months	-0.123**	-0.090**	-0.196**	-0.149**	-0.717
	(0.037)	(0.033)	(0.049)	(0.034)	(0.633)
after six months	-0.025	-0.040**	-0.079**	-0.069**	0.461
	(0.019)	(0.017)	(0.029)	(0.020)	(0.613)
Number of towns	11320	11632	6288	29240	2200
Quarterly Accidents average in treated towns (2003)	0.20	0.40	0.77	0.55	19.89

GMM estimation : explained variable = number of accidents. In the upper part of the table, each case reports coefficients of cameras from the three different models and for different clusters or set of clusters.
In the bottom part, each case reports the impact of cameras on the level of accidents in equipped towns.
P-value refers to the Hansen's test of GMM over-identifying restrictions.
Source : National Casualty Accidents Database 1998-2007 - Observatoire National interministériel de la Sécurité Routière.

Table 8: Impact on the treated towns
(estimation limited to equipped towns between 2002 and 2007)

	Group 0	Group 1	Group 2	Group 0-1-2
All treated roads				
Mixed model				
six first months	-0.094**	-0.119**	-0.138**	-0.103**
	(0.020)	(0.035)	(0.036)	(0.021)
after six months	-0.043**	-0.038	-0.038	-0.040
	(0.020)	(0.032)	(0.041)	(0.023)
Multiplicative model				
six first months	-0.113**	-0.129**	-0.166**	-0.123**
	(0.040)	(0.049)	(0.055)	(0.031)
after six months	-0.046**	-0.028	-0.042	-0.050*
	(0.022)	(0.025)	(0.048)	(0.026)
Number of observations	214	125	260	599
All treated roads (without highways)				
Mixed model				
six first months	-0.090**	-0.098**	-0.129**	-0.105**
	(0.018)	(0.027)	(0.031)	(0.023)
after six months	-0.041	-0.028	-0.017	-0.036
	(0.027)	(0.030)	(0.035)	(0.024)
Multiplicative model				
six first months	-0.108**	-0.108**	-0.168**	-0.113**
	(0.044)	(0.042)	(0.048)	(0.030)
after six months	-0.009	-0.028	-0.020	-0.036
	(0.023)	(0.024)	(0.039)	(0.024)
Number of observations	84	184	252	520

GMM estimation : explained variable = number of accidents. Each case reports the impact of cameras on the level of accidents in equipped towns.
Source : National Casualty Accidents Database 1998-2007 - Observatoire National interministériel de la Sécurité Routière.

Table 9: Non parametric approach
caliper matching

x	3			6			12			18		
e	3	6	12	3	6	12	3	6	12	3	6	12
All roads - Groups 0- 1 -2												
$\widehat{\alpha}$	.013	-.035	-.066	-.043	-.070**	-.080**	-.065*	-.049	-.052**	.021	-.023	-.033
	(.054)	(.042)	(.042)	(.037)	(.035)	(.032)	(.035)	(.030)	(.023)	(.035)	(.021)	(.022)
NT	325	407	454	318	377	425	249	332	364	198	285	313
All roads without highways - Groups 0- 1 -2												
$\widehat{\alpha}$	-.007	-.042	-.045	-.037	-.074*	-.061*	-.033	-.021	-.037	.024	-.019	-.032
	(.060)	(.054)	(.051)	(.041)	(.043)	(.035)	(.043)	(.040)	(.037)	(.041)	(.039)	(.032)
NT	291	348	388	279	331	377	215	290	327	171	258	282

NT : number of treated towns used in the estimator $\widehat{\alpha}$
** : significant at 10% level ; ** : significant at 5% level*
δ *(caliper parameter)=0.001 - standard-errors estimated by bootstrap*
Source : National Casualty Accidents Database 1998-2007 - Observatoire National interministériel de la Sécurité Routière

Table 10: Sensitivity to the caliper parameter
All roads - x=6 months ; e=12 months

δ (caliper)	Δ_{before}	$\widehat{\alpha}$	NT
0.001	0.0001	-0.080	425
	(0.0005)	(0.032)	
0.003	0.0001	-0.081	426
	(0.0004)	(0.032)	
0.005	0.0003	-0.079	433
	(0.0005)	(0.032)	
0.007	0.0018	-0.073	457
	(0.0007)	(0.031)	
0.010	0.0017	-0.077	464
	(0.0007)	(0.031)	
0.015	0.0027	-0.075	476
	(0.0008)	(0.032)	
0.020	0.0031	-0.070	482
	(0.0008)	(0.030)	

Source : National Casualty Accidents Database 1998-2007 - Observatoire National interministériel de la Sécurité Routière

Table 11: Sensitivity to the caliper parameter
All roads without highways - x=6 months ; e=12 months

δ (caliper)	Δ_{before}	$\widehat{\alpha}$	NT
0.001	-.0006	-0.061	377
	(0.0004)	(0.035)	
0.003	-.0006	-0.062	377
	(0.0004)	(0.035)	
0.005	-.0006	-0.061	383
	(0.0005)	(0.034)	
0.007	0.001	-0.061	405
	(0.005)	(0.033)	
0.010	0.001	-0.063	412
	(0.0006)	(0.033)	
0.015	0.002	-0.054	421
	(0.0006)	(0.031)	
0.020	0.003	-0.064	427
	(0.0007)	(0.031)	

Source : National Casualty Accidents Database 1998-2007 - Observatoire National interministériel de la Sécurité Routière

Table 12: Impact on the treated towns in the first six months (per quarter and per SEC) : Comparison of the two methods - groups 0-1-2

	Structural approach		Non-parametric approach	
	Mixed model	Mult. model	e =6 months	e =12 months
All roads				
Estimation on the whole sample	-0.114	-0.126		
	(0.019)	(0.029)	-0.070	-0.080
Estimation on the sub-sample of equipped towns	-0.103	-0.123	(0.035)	(0.032)
	(0.021)	(0.031)		
All roads (without highways)				
Estimation on the whole sample	-0.130	-0.149		
	(0.021)	(0.034)	-0.074	-0.061
Estimation on the sub-sample of equipped towns	-0.105	-0.113	(0.043)	(0.035)
	(0.023)	(0.030)		

Source : National Casualty Accidents Database 1998-2007 - Observatoire National interministériel de la Sécurité Routière