Searching for causal effects of road traffic safety interventions

Applications of the interrupted time series design

Carl Bonander

Faculty of Health, Science and Technology

Risk and Environmental Studies

LICENTIATE THESIS | Karlstad University Studies | 2015:22

Searching for causal effects of road traffic safety interventions

Applications of the interrupted time series design

Carl Bonander

LICENTIATE THESIS | Karlstad University Studies | 2015:22

Searching for causal effects of road traffic safety interventions - Applications of the interrupted time series design

Carl Bonander

LICENTIATE THESIS

Karlstad University Studies | 2015:22

urn:nbn:se:kau:diva-35781

ISSN 1403-8099

ISBN 978-91-7063-638-7

Distribution:
Karlstad University
Faculty of Health, Science and Technology
Department of Environmental and Life Sciences
SE-651 88 Karlstad, Sweden
+46 54 700 10 00

Print: Universitetstryckeriet, Karlstad 2015

WWW.KAU.SE

Abstract

Traffic-related injuries represent a global public health problem, and contribute largely to mortality and years lived with disability worldwide. Over the course of the last decades, improvements to road traffic safety and injury surveillance systems have resulted in a shift in focus from the prevention of motor vehicle accidents to the control of injury events involving vulnerable road users (VRUs), such as cyclists and moped riders. There have been calls for improvements to the evaluation of safety interventions due to methodological problems associated with the most commonly used study designs. The purpose of this licentiate thesis was to assess the strengths and limitations of the interrupted time series (ITS) design, which has gained some attention for its ability to provide valid effect estimates. Two national road safety interventions involving VRUs were selected as cases: the Swedish bicycle helmet law for children under the age 15, and the tightening of licensing rules for Class 1 mopeds. The empirical results suggest that both interventions were effective in improving the safety of VRUs. Unless other concurrent events affect the treatment population at the exact time of intervention, the effect estimates should be internally valid. One of the main limitations of the study design is the inability to identify why the interventions were successful, especially if they are complex and multifaceted. A lack of reliable exposure data can also pose a further threat to studies of interventions involving VRUs if the intervention can affect the exposure itself. It may also be difficult to generalize the exact effect estimates to other regions and populations. Future studies should consider the use of the ITS design to enhance the internal validity of before-after measurements.

Sammanfattning

Trafikrelaterade skador är ett globalt folkhälsoproblem som bidrar stort till skaderelaterad mortalitet och morbiditet världen runt. Under de senaste årtiondena har både förbättrad trafiksäkerhet och ett mer rättvisande skadeövervakningssystem medfört en förskjutning av fokus från prevention av trafikolyckor där motorfordon är involverade till förhindrandet av skador bland oskyddade trafikanter, såsom cyklister och mopedister. Samtidigt har metodologiska förbättringar vad gäller utvärdering av säkerhetsinsatser efterfrågats på grund av den låga nivån av intern validitet som associeras med de vanligast använda studiedesignerna inom forskningsfältet. Syftet med denna licentiatuppsats var därför att bedöma styrkor och begränsningar med en metod för att studera effekten av interventioner med hjälp av trendbrottsanalys (eng. *interrupted time series (ITS) design*), som har fått en del uppmärksamhet för sin förmåga generera giltiga effektuppskattningar. Den svenska cykelshjälmlagen för barn under 15 år och en skärpning av körkortskrav för klass 1-mopeder valdes ut som empiriska fall för tillämpning av metoden. Resultaten tyder på att båda interventionerna har haft en positiv säkerhetseffekt. Om inte andra samtida händelser har påverkat behandlingsgruppen precis vid interventionernas införande bör effektmåtten vara giltiga. En av de största begränsningarna med studiedesignen är att det är problematiskt att avgöra varför och hur en intervention fungerade, särskilt om den är komplex och mångfacetterad. En brist på tillförlitliga exponeringsdata utgör ytterligare ett hot mot kvalitén i studier som involverar oskyddade trafikanter, särskilt om interventionen kan påverka exponeringsmönster. Det kan också vara svårt att generalisera de exakta effektuppskattningarna till andra regioner och populationer. Framtida studier bör överväga användningen av ITS-designen för att stärka den interna validiteten i före-efter mätningar.

List of papers

I. Bonander, C., Nilson, F. & Andersson, R. (2014). The effect of the Swedish bicycle helmet law for children: An interrupted time series study. *Journal of Safety Research,* 51, 15-22.

II. Bonander, C., Andersson, R. & Nilson, F. (2015). The effect of stricter licensing on road traffic injury events involving 15 to 17-year-old moped drivers in Sweden: a time series intervention study. Submitted to *Accident Analysis & Prevention*.

The articles are reprinted with the prior permission of the publisher.

Author contributions

The papers included in this licentiate thesis are the result of collaborative efforts between the three authors. However, the majority of the work from study initiation, the formulation of research questions, data collection, statistical analysis, and writing of the initial manuscripts were carried out by the main author. Finn Nilson participated in the interpretation of the results, initiation of the papers and contributed to the discussion and conclusions. Ragnar Andersson participated in the planning process, interpretation of the results and writing of the final versions of the manuscripts.

Contents

1. Introduction

From a historical perspective, unintentional injuries have been viewed as random occurrences without any specific cause or hope for prevention. During the 20th century, scientists in biomechanics (DeHaven 1944; Stapp 1957) and epidemiology (Haddon Jr 1980) have successfully pointed out that this is not the case; the causes of injury are simple to define and they are almost never completely random. Focus has shifted from blaming individuals for erroneous actions to looking for latent and recurring errors at the system level (Robertson 2007; Reason 2000), and some agencies responsible for different systems have taken it upon themselves to create forgiving environments in which individuals should be allowed, and even assumed, to make mistakes without any dire consequences. Such injury prevention strategies are generally advocated as more effective than viewing individuals as the source of the error (Haddon Jr 1980; Reason 2000), and the approach has, for instance, been adopted by the Swedish Transport Administration regarding the safety aspects of the road traffic system.

Road traffic injuries are a leading cause of premature death globally (Peden et al. 2012). However, the incidence and risk of such fatalities tend to decrease over time as the state of the road traffic system improves, countries develop and society as a whole learns to cope with new technologies (Oppe 1987; Koornstra 1988). In conjunction with improvements to injury surveillance systems, the large decrease in the occurrence of motor vehicle fatalities has increasingly shifted focus toward vulnerable road users in high-income countries (Wegman et al. 2012). A modal shift from passive to active transportation, such as bicycling and walking, is also often advocated by public health professionals and environmentalists in order to improve the general health of the population and to reduce fossil fuel emissions (de Hartog et al. 2010; Pucher et al. 2011). In order for society to transition sustainably into a state in which we are less dependent on motor vehicles and more reliant on active modes of transportation, a holistic approach in which all relevant health and environmental aspects are taken into account should be promoted. If the amount of vulnerable road users in traffic is expected to grow, issues of safety should also be considered so that the number of injuries does not increase with the rise in exposure.

Additional improvements to increasingly available and refined injury surveillance systems (Horan & Mallonee 2003; Tingvall et al. 2013), along with enhanced knowledge of the effects of injury control measures, will most likely enhance the societal learning process with regards to the safety of vulnerable road users. However, there have been calls for further improvements to the quality of the scientific evidence of injury control measures given that most studies in injury epidemiology are observational and thus prone to many types of bias, limiting the ability to sufficiently estimate the causal effects of interventions (Biglan et al. 2000; Cox 2013). Given that the level of safety within the road traffic system appears to be a function of time (Oppe 1987); it is often necessary to separate the effects of the intervention from secular trends. The aim of this licentiate thesis is therefore to discuss the strengths and limitations of time series intervention analysis along with threats to the internal validity in observational studies in the presence of time trends. The interrupted time series design was applied in an attempt to estimate the causal effects of two national road safety regulations targeting the safety of vulnerable road users, using different empirical strategies to enhance the internal validity of the effect estimates.

2. Background

Injury is widely recognized as a major global health problem. It is associated with immense health care costs and contributions to all-cause mortality and disability worldwide. It is also the leading cause of death among teenagers and young adults (Peden et al., 2012). Globally, road traffic injuries are the fifth leading cause of death and the number one injury-related cause of death, resulting in approximately 4.8 million fatalities annually (Ärnlöv & Larsson 2014). The estimated worldwide economic losses generated by road traffic injuries were 168 billion US dollars in 2005 (Dalal et al. 2013). Although road traffic fatality rates are generally higher in developing nations compared to high-income countries (Ärnlöv & Larsson 2014), road traffic injuries still contribute greatly to injury-related disability-adjusted life years, a metric that measures the sum total of the years-of-life lost and years lived with disability due to injuries and diseases, ranking third behind self-harm and falls in Sweden (Murray et al. 2013). Furthermore, transport-related injuries accounted for 65% of all premature deaths among Swedish teenagers in the age group 15-19 years in 2013 (The National Board of Health and Welfare 2014a).

Sweden has a long-standing tradition of safety policy, especially regarding the reduction of deaths and severe injuries due to road traffic crashes. This is perhaps best illustrated by the Swedish Parliament's adoption of Vision Zero, a long-term political goal to reduce fatal and severe traffic-related injuries in Sweden toward zero (Belin et al., 2012). In recent years, the number of car occupants killed or hospitalized due to road traffic injuries has decreased considerably in relation to other modes of transportation (The National Board of Health and Welfare 2014a; 2014b), and vulnerable road users (such as cyclists and moped riders) are now increasingly being recognized as a high-risk group worthy of increased focus in injury research and control (Peden et al., 2004). While injuries to car occupants still dominate with regards to the highest absolute number of transport-related deaths per year, studies accounting for exposure per traffic volume show that vulnerable road users are at more than twice the risk of death per distance travelled compared to car occupants (Bjørnskau 2009).

Any road user that is not on the inside of a protected vehicle is often defined as a vulnerable road user (VRU). The term, therefore, includes, for example, cyclists, pedestrians, motorcyclists and moped users. VRUs are more susceptible to injuries in the event of a collision with a motor vehicle because the force is exerted directly to the human body instead of the motor vehicle, which can absorb a large amount of energy. Riders of powered two-wheelers can move at high speeds and are thus very vulnerable even in the event of a single vehicle crash. Even at relatively low speeds, cyclists and pedestrians can still be severely injured as a result of a fall in contact with hard surfaces, such as asphalt and concrete. This is because the amount of force exerted is also a function of the deceleration process (Stapp 1957). Rapid deceleration, which occurs if the contact surface is not pliant enough to soften the impact, can result in excessive kinetic forces being exerted back to the body and produce serious traumatic injuries, especially if vulnerable body areas, like the back, neck and head are affected (Rizzi et al. 2013; Holtslag et al. 2007; Langlois et al. 2006).

2.1 Understanding secular trends

Oppe (1989) theorized that the number of road traffic fatalities at any given year is highly dependent on the state of the transport system in a country at that time. A system can be defined as a set of interacting or interdependent components forming an integrated whole. Every system is dependent on inputs and outputs; something can be put into the system which, as a consequence, produces certain outputs or results. The transport system can be viewed as a classic production system, in which the traffic volume is the production unit, and the fatality rate is an estimate of the probability of failure per unit of production. This means that the total number of vehicle kilometers travelled in a country a year (V_t) is the system output that year, and that the fatality rate (R_t) can be defined as the ratio of fatalities in traffic (F_t) to traffic volume at year t (F_t/V_t), and that the total loss on safety is equal to $F_t = V_t \cdot \left(\frac{F_t}{V_t}\right)$. He noted that although the total number of fatalities will be monitored and trigger political action, safety control will not be connected to the total safety loss, but rather to the amount of safety loss per production unit (Oppe 1991).

Koornstra (1988) draws an interesting parallel between Darwin's theory of biological adaptation and the emergence of traffic safety. The growth of traffic is analogous to the growth of the population of a new species, which is dependent on a process of selection and reproduction that ensures that only those members of a species that survive the premature period will produce offspring. This selection process leads to a growing birth rate as well as to a reduction of the probability of non-survival before the mature reproductive life period. The number of mature survivors, i.e. the size of the population, follows an S-shaped curve from the beginning of the process up to the carrying capacity of the environment for that certain species. In the case of the transport system, the number of mature survivors are analogous to the traffic volume (or the output), and the saturation level for traffic volume is analogous to the carrying capacity of the environment. Due to a steadily decreasing probability of death before mature age, the number of premature non-survivors of a new species will follow a bell-shaped curve over time. It was proposed that the theory of adaptive self-organizing systems can be applied almost directly to the transport system, except unlike biological self-organizing systems, adaptation is governed by the decisions of decision-making bodies and individuals instead of blind mutation. The theory suggests that the development of the number of fatalities in traffic will be related to the change in traffic volume over time, and that the development of the number of fatalities in traffic will follow a bell-shaped curve as time progresses and society adapts to the changes induced by industrialization (Koornstra 1988).

Returning to the terminology used by Oppe (1991), time plays an important role in a production process. In the beginning, demand will grow rapidly for a successful new product, but that this growth will slow and subsequently halt when the supply reaches a certain saturation level. The saturation level is determined by a combination of factors. The number of drivers is limited by the size of the population and by the time available for travelling, and the total length of the road network is limited by not only economic factors, but physical space (Koornstra 1988). This indicates that the increase in traffic volume in a country will follow an S-shaped curve over time, like the growth of a population. Oppe (1991) proposed that the following simple logistic function can be used to predict traffic volume:

$$V_t = \frac{V_{max}}{1 + e^{-(at+B)}},$$

where $V_{\max}$ is the maximum traffic volume attainable in a country, or the saturation level, and a and B are scale parameters that, along with $V_{\max}$, must be estimated from empirical data. The maximum traffic volume, growth rate and time at which the saturation level is predicted to be reached all vary by country.

The development of traffic safety can be regarded as a societal learning process, in which communities adapt to the large changes induced by the introduction of motorized traffic over time (Oppe 1991). By applying a simple model from mathematical learning theory to the safety problem, it was proposed that the number of contributions to safety, made by the society as a whole at time t, is proportional to the amount of situations left to improve. He therefore assumed that a negative exponential learning model can be used to predict how fatality rates develop over time:

$$R_t = e^{\alpha t+\beta} \qquad \alpha < 0,$$

where α and β are scale parameters that vary by country and must be estimated from empirical data. The negative relationship with time was hypothesized to be the result of combination of efforts made to improve the traffic system, such as improvements to vehicle design, injury control measures, changes in legislation, educational efforts, individual learning, and improvements to the road system itself. It was also theorized that traffic density may have a direct effect on safety.

When applying these models to data from the U.S., the Netherlands, West Germany and Great Britain, Oppe (1989) showed that the model for fatality rates explained, on average, 96% of the variance over time, and that the model for traffic volume explained 99% of the variance in traffic volume. By combining the information obtained from these models, it was estimated that there would be 30133 fatalities in the U.S. in 2010. The actual number was 32999, which is not far off. At the time when the theory was derived, there was no empirical evidence to indicate that the growth rate in traffic volume would level off. Re-estimation of the models using newer data shows that the validity of the adaptive self-organizing system theory is strong with regard to accuracy and hypothesized shape of the curves (Figure 1).

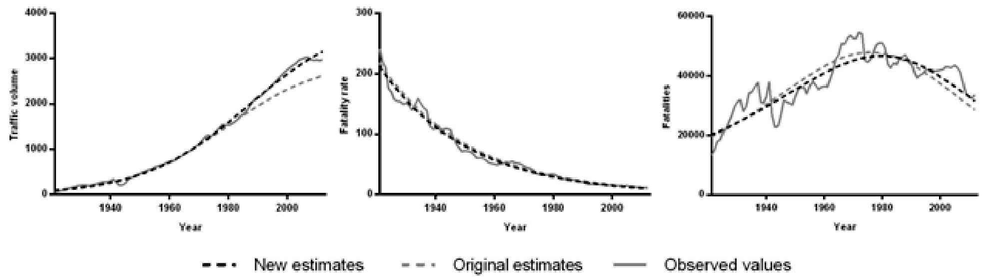

Figure 1. *Traffic volume (in billion vehicle miles), traffic-related fatalities and fatality rate (per billion vehicle miles) in the U.S. from 1921 to 2012. The source of the empirical data is the U.S.-based Fatality Analysis Reporting System (FARS). Predictions are based on the theory proposed by Koornstra (1988) and Oppe (1989). Original estimates are from Oppe (1989), which were derived using empirical data from 1933-1985. New estimates were derived using empirical data from 1921 to 2012.*

The empirical curve for traffic volume over time, while not a perfect fit for the logistic curve proposed toward the end of the period, confirms the presence of a saturation level. The negative exponential learning curve for fatality rates is highly accurate, and the bell-shaped curve for the absolute number of fatalities is also present. Empirical evidence of similar accuracy has been found using data from other industrialized countries as well (Oppe, 1989; 1991). There is thus quite strong empirical evidence that the transport system can be viewed as an adaptive self-organizing system. However, this model can only explain how traffic safety develops over time, not what the disaggregated direct causes of this development are. In order to understand to which extent safety interventions directly affect these curves, we must be able to disentangle the effects of injury control measures from other secular processes.

Koornstra (1988) noted that objective evaluation of the effects of injury control measures and the prioritization and selection of the most effective measures is analogous to selection in biological systems. For this selection process to be effective, society requires accurate and valid knowledge about the effects of specific injury control measures. Some of this knowledge can easily be derived from epidemiological theory (Haddon Jr 1980), but the actual results cannot be fully known until there is empirical evidence of the causal effects of an intervention. For instance, the use of air bags in cars has resulted in largely positive effects on the safety of car occupants in the event of a crash. However, the risk of injury to children in passenger-side child safety seats actually increased, which illustrates the need for continued evaluation of the actual effects, and subsequent improvement, of safety measures. Furthermore, if an intervention is dependent on behavior change, the population level effects may be much harder to predict using theory alone (Gielen & Sleet 2003).

The systematic approach to injury control and prevention is often considered a continuous process, usually presented in the form of a circular quality assurance-model that starts with the epidemiological study of injury risks in a population in order to identify vulnerable groups or factors that increase the risk of injury. This is then followed by a process of risk assessment, the purpose of which is to prioritize the greatest risks from a chosen set of criteria, such as the injuries that produce the highest costs to society in the form of lives lost, years lived with medical disability or monetary burden on the healthcare system, or the ones occurring during activities where an individual's risk of obtaining an injury is relatively high in comparison to others. An intervention can then be devised to reduce either the probability of an injury occurring due to a certain external cause, to reduce the severity of such injuries, or to reduce the absolute number of injuries in a population by eliminating or decreasing exposure to injury events using the knowledge obtained in etiologic study of these injuries. The circular process ends, and starts again, with an updated study of the injury risks to evaluate whether or not the intervention was successful in protecting the population against the risk of being injured (Andersson 2012).

2.3 Evaluating interventions

Policymakers and public health risk managers require concise and conclusive evidence in order to make sound and appropriate decisions about which tactics and changes to employ in order to effectively decrease injury risks in the population. Evidence-based policy and practice is often advocated in the fields of medicine and public health (Roberts & Yeager 2006), since subjective opinions, even from experts, can be severely biased. In fact, studies in human psychology suggests that people, including expert professionals and scientists, are prone to misattribution of causes, selective interpretation and overconfidence in their own assessments (Fugelsang et al. 2004; Littell 2008; Breakwell 2007). To avoid this type of subjectivity, pharmaceuticals are always tested using the most stringent quantitative, objective study designs in order to estimate the causal effects of a new drug on the outcomes which it is meant to affect. The best, and probably most common, strategy in medicine to assert such causal claims is to conduct a randomized controlled trial (RCT). The theory behind the RCT design is quite simple, but prior to detailing this, the reason why the design is preferred will briefly be explained.

The problem that must be dealt with in all studies that concern the causal effects of an intervention or exposure to a risk factor is the issue of confounding. A confounder is an observable or unobservable variable that somehow influences the statistical relationship between the exposure variable and the outcome of interest without being a part of the causal chain between them (Bonita et al. 2006). In other words, a confounder is correlated with the exposure variable while it independently affects the outcome of interest. The influence can be huge, and potentially even explain the entire observed association. A common example of this is the observed statistical correlation between ice cream sales and drownings. Clearly ice cream sales do not cause the observed increase, but ice cream sales and the exposure to the risk of drowning are both higher on sunny days when temperatures are hot. It is thus very unlikely that banning the sale of ice cream will affect the rate of drownings. The confounder's influence can also be small and explain only parts of the variation, but still effectively bias the magnitude of the estimated effect size.

Policymakers and risk management professionals are not only interested in whether an intervention works; they also need to know how large the effect is in order to compare its effectiveness to other alternative interventions (Grandelius 2014; Cox 2013). If the effect size is biased, the wrong interventions might be prioritized.

In order to gain insight into the causal effects of interventions, we must attempt to quantify what is per definition an unobservable state. The outcome had an individual not worn a seat belt or a cyclist not worn a helmet during an accident can never be truly observed, since we cannot go back in time and change an individual's treatment exposure status at that exact event. The true causal effect on outcome Y of exposure to an intervention or risk factor, D, for individual i[1] is given by the difference in outcome between the observable state and the unobservable state $(Yi|D = 1 - Yi|D = 0)$ (Angrist & Pischke 2008).

Statistics provides a solution that allows for the estimation of this so called counterfactual state by measuring averages across intervention and treatment populations to estimate the population average outcomes for D = 1 and D = 0, from which an average causal effect can be estimated. However, observational research is often prone to several potential sources of bias in the estimation of causal effects. For instance, people may self-select to treatment, or treatment may be provided to those who need it the most. This selection bias can confound the observed average difference between those exposed to treatment and those who are not exposed to treatment because they may differ systematically (Angrist & Pischke 2008). Without taking this into account, any difference observed will most likely be the causal effect plus the influence of selection bias.

For an effect estimate to be internally valid, i.e. close or equal to the "true effect" for the population, jurisdiction or location under study, the influence of all observable and unobservable factors other than the intervention must thus be eliminated. Multiple regression analysis is often used to adjust an effect estimate for observed confounders at

[1] Can be substituted by the level at which the intervention is implemented; such as school, community, state, country etc.

the analysis stage in observational studies (Robertson 2007). However, since all potentially relevant confounders cannot be observed with absolute certainty, we can never conclude that the relationship is causal using these standard measures of association (Angrist & Pischke 2008). In RCTs, the issue of unobserved confounding is effectively dealt with in the study design phase by exploiting a simple statistical theory that states that if the study group is randomized into treatment and control groups, and if the sample is large enough, all potential differences between the treatment and control subjects will be randomly distributed between these two groups. The only average difference between the groups will thus be that one received the treatment while the other did not. Other practical issues may of course arise that can bias the estimate even in RCTs, but detailing the potential problems of this study design is not the focus of this thesis. Rather, the issue at hand is achieving comparable levels of internal validity in observational data in order to estimate the effects of policy changes that involve no element of randomization, which often requires more careful considerations in the analysis stage of the study (Bonita et al. 2006).

2.4 Causality in injury control research

There have been calls for methodological improvements regarding the empirical study of the effect of injury control measures (Biglan et al. 2000; Nilsen 2006; Cox 2013). Often, observational study designs are employed such as the classic epidemiologic case-control design (Thompson et al. 1999), or poorly constructed quasi-experiments are attempted in which the internal validity is too low for causal inference to be considered, such as simple non-randomized before-after studies (Macpherson & Spinks 2008), which are prone to a number of potential biases. Because only two time points are used, a potentially large bias can be the presence of secular trends (Cook et al. 1979). Even if a concurrent control group is included in the study, an assumption of equal trend between the case and control site or community must be met for causal inference, and this assumption cannot be validated without a longer time series of data. If the number of pre-intervention time points available for analysis is too low, there is simply not enough information to rule out diverging time trends, meaning that an effect estimate based on such counterfactuals may be biased (Morgan & Winship 2014; Angrist & Pischke 2008). Usually, researchers are forced to conclude that the effects under study may be simple correlations in which the degree of confounding is unknown (Grimshaw et al. 2000). In fact, even the sign of the effect (positive or negative) might be unknown (Cox 2013). If the injury-reducing effect of the intervention is studied at all, that is. Some studies focus on intermediary functions, such as improved skills or knowledge (Richmond et al. 2014; Ian & Irene 2001), safety equipment use (Owen et al. 2011; Macpherson & Spinks 2008) or other effects of other outcomes, such as visibility (Kwan & Mapstone 2009), instead of focusing on what is actually important from a public health perspective, i.e. the reduction in injuries. Of course, understanding the causal pathways from intervention to injury reduction through these intermediary functions is interesting for the purpose of replicating complex interventions since some subsets of intermediary functions may be effective while others are not. In the best case scenario, injury control research should assess the effects of an intervention on injuries along with the causal mechanisms behind the effect, or absence thereof, through process evaluation (Nilsen 2006). However, by only studying the process, or merely using these intermediary functions as outcome measures, researchers cannot draw any conclusions about the effects on the risk of injuries associated with the intervention unless there are some other studies that have been able to provide sufficient, *unconfounded* evidence of a causal relationship between the intermediary function and the risk of injury. Such evidence is rare in injury epidemiology due to the extensive use of observational study designs (Robertson, 2007). Even the efficacy of an injury control measure that is theoretically sound and grounded in the laws of physics, such as the bicycle helmet, can be put in question due to the absence of randomization in observational effect studies (Thompson et al. 1999; Curnow 2005; Olivier et al. 2014). As such, studies of bicycle helmet laws that only measure the effects on the prevalence of helmet use have been opposed by some researchers as evidence of an effect of such interventions on population-level risk of head injuries to cyclists (Robinson 2006).

In the academic field of injury control and prevention, experimental studies are extremely rare considering the ethical aspects of testing safety equipment on live persons. Many injury control measures are also introduced by governmental bodies, such as changes in policy or regulation, usually with no element of randomization (Robertson 2007). As such, there is limited knowledge of the causal effects of interventions, given that the best way of eliminating the influence of confounding elements relies on distributing these factors randomly by random selection of individuals into treatment and control groups at the design stage (Bonita et al. 2006). Any other study design is most likely prone to selection bias, meaning that those who are treated, or those who use the proposed safety equipment, the road section that is re-built etc., may be different from those that are not treated. It is, however, not entirely impossible to estimate causal effects using observational data. With the addition of a certain set of identifying assumptions, e.g. assumptions that must be met for a causal effect to be identified, quasi-experimental designs and econometric methods can be used to estimate causal effects using counterfactual analysis (Morgan & Winship 2014; Angrist & Pischke 2008). Some of these methods involve repeated observations, often using aggregated state or country-level data, and can be used to estimate the causal effects of interventions at the societal level at which they are implemented. By observing an outcome over a longer time period, statistical models can be estimated to test for the occurrence and magnitude of changes at the exact time an intervention starts while accounting for secular trends. These changes may be interpreted as causal if all other rival plausible hypothesis of the change, other than the intervention itself, can be sufficiently ruled out (Cox 2013; Morgan & Winship 2014; Glass 1997).

2.5 The interrupted time series design

In the presence of secular trends in non-randomized quasi-experimental studies, simple before and after analysis of a difference in means (by for instance, a t-test) will likely result in a biased estimate of the intervention effect. For instance, if there is a downward trend in injuries due to car accidents over the course of a ten-year period in which an intervention was introduced half-way through, a t-test would most likely show a large negative effect resulting in the conclusion that the intervention was successful. Such an estimate is guaranteed to be biased with regards to effect size and any inference based on the study would thus be erroneous. The internal validity would be low as parts, or perhaps all, of the estimated effect could be explained by variation extraneous to the intervention, caused by some unobserved time-varying factor such as economic development or general improvements in safety. Using an interrupted time series (ITS) design, these unobserved factors can easily be modelled. In its simplest form, the ITS model can be expressed as:

$$Y_t = \alpha + \beta_1 f(T) + \beta_2 D_t + e_t, \qquad (1)$$

where Y, the outcome of interest, is some undefined function of time (i.e. linear, quadratic, cubic or any other higher order polynomial), expressed as $f(T)$ on the right-hand side of the equation; D_t is a dummy variable used to indicate the period in which the intervention is active; and e_t is a time varying error term that captures the residual variation not explained by the estimated time function, β_1, which is the estimated effect of the unobserved time-varying factors or the estimated intervention effect, β_2. Since it is a time series model, the issue of time-correlated errors (or residual autocorrelation) is often present and must be dealt with to minimize the risk of falsely rejecting the null hypothesis due to inflated standard errors of the parameter estimates (Morgan & Winship 2014).

Before analyzing the effects of an intervention, the functional form of the effect must be considered. Glass (1997) lists several different types of intervention effects which all relate to different functional forms, and argues that prior knowledge of how an intervention should affect the outcome can enhance the analysis. For instance, an effect can be abrupt and permanent, gradually increasing or delayed due to some practical circumstances surrounding the intervention. See Figure 2 for a conceptual sketch of the measurement of abrupt and gradual effects in a time series.

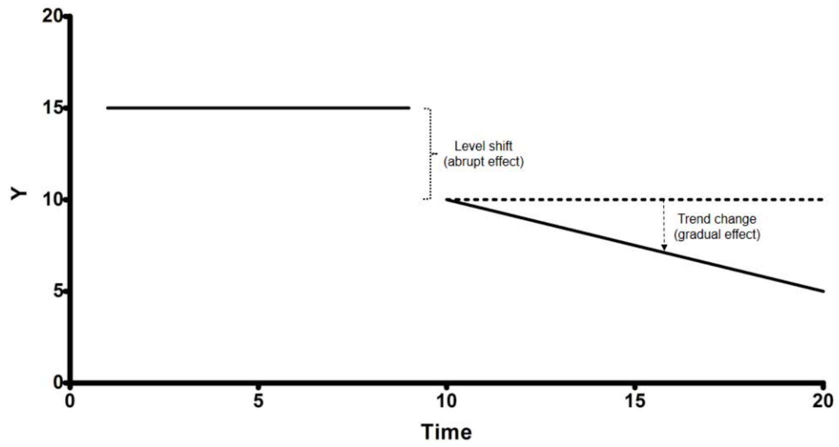

Figure 2. *Example of a time series plot from a fictional interrupted time series study where both abrupt and gradual intervention effects are present. The abrupt effect is the change in level (or intercept) of the regression line, and is constant across the post-intervention period. The gradual effect is a change in slope between the pre-intervention and post-intervention segments.*

The identifying assumption of an ITS analysis is that in the absence of the intervention, the trajectory of *Y* would have been the same after the intervention as before. Since this assumption is untestable as there is no way of observing this counterfactual state, the method is strongly dependent on a researcher's willingness to extrapolate the pre-intervention trend onto a post-intervention period in which the intervention did not take place. Furthermore, the functional form of the time trend must be identified. Usually, a linear trend is assumed in a segmented regression model, where the trends in the pre and post segments are both assumed to be linear, but allowed to vary in slope (Wagner et al. 2002). Another approach is to use the observed data to estimate the functional form non-parametrically using semi-parametric generalized additive models instead of fully parametric regression models (Tobías & Sáez Zafra 2004). They hold an advantage over regular regression models as they are able to provide greater flexibility in modelling complex nonlinear trends (Sullivan et al. 2015), and do not require a selective input of the researcher since the approach is data-driven and can be chosen automatically using computer algorithms (Rigby & Stasinopoulos 2013). The non-parametric trend estimates, however, often provide little in terms of interpretational value, and gradual effects (such as a change in linear trend between periods as detailed in Wagner et al. 2002) are harder to study. However, as Glass (1997) notes, the greater the temporal distance between the intervention and the hypothesized effect, the weaker the argument for a casual effect becomes. Gradual effects must thus often be argued for more comprehensively, and if there is no reason to believe a gradual effect might exist, the change in slope between periods might just be due to other extraneous factors.

The ITS design requires evenly spaced time series data, such as monthly or yearly injury incidence rates, which is often collected as a part of routine injury surveillance systems (Holder 2001). The method has previously been used to assess the impacts of large scale regional and national road safety policy interventions, such as stricter alcohol policies (Asbridge et al. 2004; Asbridge et al. 2009; Mann et al. 2002; Macdonald et al. 2013; Pridemore et al. 2013), helmet laws for bicyclists (Scuffham et al. 2000; Walter et al. 2011; Dennis et al. 2013), moped riders and motorcyclists (Ballart & Riba 1995), seat belt laws (Wagenaar & Margolis 1990; Wagenaar et al. 1988), penalty points systems (Castillo-Manzano et al. 2010), extended drinking hours (Vingilis et al. 2005) as well as the relaxing of licensing rules for motorcycles (Pérez et al. 2009). Despite this, the method appears under-used for smaller scale injury control measures, such as community-based interventions (Biglan et al. 2000), even though it in theory can be used to evaluate the effects of an intervention on a single individual (Sullivan et al. 2015). It also appears neglected in before-after studies that could have easily used segmented regression analysis to further strengthen the internal validity. Although perhaps not directly transferable to injury control research, Ramsay et al. (2003) reanalysed the data from 33 published studies on health care behaviour change strategies that had insufficiently analysed time series data using t-tests to measure mean differences before and after an intervention. They found that while all of the studies reported statistically significant interventions effects in the original papers, almost half of them returned insignificant results when secular trends were accounted for (i.e. the outcome of interest was already in the process of changing before the intervention took place).

2.5.1 Seasonality

Seasonality is a potential issue that must be considered in time series analyses of monthly data (Box & Jenkins 1976), and it is very common for injuries to follow seasonal patterns. This is likely due to within-year variations in exposure (Robertson 2007; Gill & Goldacre 2009) due to, for instance, weather-related factors (Brown & Baass 1997). Stolwijk et al (1999) suggested the use of a linear combination of the trigonometric sine and cosine functions to study the effects of seasonality on an outcome, and this method can be implemented in a regression framework as a means to adjust for seasonality if and when seasonal variation is present. Ignoring the presence of seasonality may lead to false inferences, and will almost definitely result in residual autocorrelation (which in it itself increases the risk of Type I error, see below). The simple ITS model (Equation 1) can be extended to incorporate seasonal variation in the outcome:

$$Y_t = \alpha + \beta_1 f(T) + \beta_2 D_t + \sum_{k=1}^{6} \left[\beta_{3k} \sin\left(\frac{2k\pi t}{S}\right)_t + \beta_{4k} \cos\left(\frac{2k\pi t}{S}\right)_t \right] + e_t, \qquad (2)$$

where k takes on a value between 1 to 6 depending on which types of seasonal patterns are to be modelled (1 for annual seasonality, 2 for six-monthly seasonality, etc.); S is the number of time points described by the sine and cosine functions (12 for monthly data, 4 for quarterly data, etc.) and t is a discrete variable used to describe time, counting from 1, 2, ..., T, which is the last observation. By utilizing this parameterization, the intervention effect estimate (β_2) can be adjusted for both trend and seasonality. The number of seasonal patterns can be chosen based on theoretical or data-driven approaches.

2.5.2 Difference in discontinuity between case and comparison series

In certain cases, an intervention might only apply to a subset of the population. For instance, the Swedish bicycle helmet law only applies to children under the age of 15 years. Recall that the identifying assumption of the ITS design is that nothing else happened at the same time that could explain the observed effect. One strategy to enhance the internal validity in such studies is to study the effect on a comparison series that should not be affected by the intervention (Morgan & Winship 2014). The comparison series is not required to be identical to the case series with regards to observed and unobserved characteristics, but should be similar enough that alternative hypotheses as to why an effect was observed can sufficiently be ruled out. The ITS analyses of the case and comparison series can be performed separately by estimating Equation 1 or 2, or incorporated into a single comparative interrupted time series (CITS) model to obtain a difference-in-discontinuity (Grembi et al. 2014; Somers et al. 2013) estimate:

$$Y_t = \alpha + \beta_1 f(T) + \beta_2 D_t + \beta_3 Tr + \beta_4 Tr \times f(T) + \beta_5 Tr \times D_t + e_t, \quad (3)$$

where Tr is a dummy variable used to indicate the treatment group, coded as 1 for the case series and 0 for the comparison series; $Tr \; x \; f(T)$ is an interaction term used to allow the time trend (and its functional form) to vary between group status, and $Tr \; x \; D_t$ is the interaction between group and intervention status. The parameter associated with the latter, β_5, measures the difference in discontinuity of the time trend between the case and control series when the intervention becomes active. The seasonality variables from Equation 2 can also be added to Equation 3, along with group interactions to allow for group-varying seasonality.

While the amount of studies that have assessed the internal validity of the CITS design appear limited, one study has provided evidence that it can produce internally valid effect estimates very close to those derived from a randomized experiment, independent of whether the comparison group is matched to be as equivalent as possible, as long as differences in pre-intervention trends are correctly modelled (Clair et al. 2014).

2.5.3 Autocorrelation

In addition to trend and seasonality, time series data often exhibit some form of time-dependence in the error process (e_t), which if present, will violate the independence assumption of standard regression models and lead to inflated standard errors (Morgan & Winship 2014). The risk of type I error, or over-rejection of the null hypothesis, is thus increased if this is not accounted for. Autocorrelation in a time series occurs when the residuals, i.e. the difference between the observed and predicted values ($Y_t - \widehat{Y_t}$) are correlated at some or several time lag(s) (t-1, t-2, ... etc). First-order autocorrelation, which indicates a dependence between the residuals of two adjacent time points, is fairly common, and so is twelfth-order autocorrelation, that is residual dependence between time points one year apart if monthly time series data is analysed, especially if seasonality is present (Box & Jenkins 1976).

There are several ways to test for autocorrelation, such as the Durbin-Watson test for first-order autocorrelation (Durbin & Watson 1950), the Ljung-Box Q-test for higher order autocorrelation (Ljung & Box 1978). There are also plot-based tests, in which the correlation structure can be studied visually (Bartlett 1948). The latter are very useful for deciding on which technique to apply when dealing with this issue, and to study the presence of seasonal effects (Box & Jenkins 1976). The Ljung-Box Q-test is preferable because it provides a test of significance, where p-values below 0.05 indicate that autocorrelation is present in the time series. However, a maximum lag up to which autocorrelation is tested for must be provided, and this subjective choice can influence the p-value associated with the test. In an effort to eliminate the subjective part of the analysis, Escanciano & Lobato (2009) proposed a version of the test in which the optimal maximum lag is selected by an algorithm using a data-driven approach.

There are also several different techniques that can be applied to deal with autocorrelation. One that is often applied in simple situations is to include a lagged dependent variable as a predictor in the model. More advanced techniques involve adding a set of standardized lagged residuals as a covariate (Schwartz et al. 1996), adjusting the covariance matrix to account for the autocorrelation structure in the error term (Andrews 1991) or to include the autocorrelation function as a latent stochastic process in the mean of the model (Davis et al. 2000). Sometimes, adjustments for trend and seasonality may be adequate to capture the time dependence in the outcome series. If significant autocorrelation was present according to the test proposed by Escanciano & Lobato (2009) even after these adjustments, the type of autocorrelation present in the series can be identified through an iterative process by studying graphs of the autocorrelation function (ACF) and partial autocorrelation function (PACF) (Box & Jenkins 1976). If the underlying process is a function of past observations ($Y_{t-1}, \ldots, Y_{t-p}$), an autoregressive (AR) model can be estimated, and if the process is a function of past shocks ($e_{t-1}, \ldots, e_{t-q}$), a moving average (MA) model might be more appropriate. The time series models are usually denoted by ARMA(p,q), where p is the AR degree and q is the MA degree.

2.5.4 Time series models

There are sophisticated time series modelling alternatives to standard regression techniques, such as ARIMA (Autoregressive Integrated

Moving Average) models (Box & Jenkins 1976). These models are able to deal with the many issues that may arise in time series data, such as autocorrelation and seasonality. The greatest difference between ARIMA models and other time series regression techniques is perhaps that the time trend is treated as a stochastic component by detrending the dependent variable through differencing, as opposed to including the trend as a covariate in the model. Generally, they are used for forecasting, but intervention analysis can also be performed (Wei 1994). These models, however, work best under the assumption that the dependent variable is normally distributed, which is rarely true in a time series of low counts/rare events such as monthly injury data in sparsely populated geographical areas. If the mean of the count variable is high (>15-20), log-transformation of the dependent variable may be adequate to achieve normality, but this has been advised against since better alternatives are available (O'Hara & Kotze 2010).

In injury control research, the outcome of interest in a time series is often in the form of rare non-negative integer-valued counts (injuries or accidents) or rates (injuries per some exposure variable or population). Given this, they will most likely follow a Poisson error distribution, and the assumptions of models that require a normal (Gaussian) error distribution, such as ordinary least squares and ARIMA, will thus likely be violated (Rivara et al. 2009). Poisson regression is likely the better choice in many, if not all, cases where count data is modelled because they are designed to model non-negative integer valued data. Rates per some exposure variable, such as kilometres driven or time spent in traffic, can also be modelled using the same models by including the natural log of the exposure as an offset term (Cameron & Trivedi 2013). The interpretation of the parameters in a Poisson regression model is also fairly straightforward and easy to generalize to other contexts since they can be expressed as relative risks or incidence rate ratios; two popular effect measures in epidemiology that can easily be converted to percentage change (Schmidt & Kohlmann 2008). However, these models come with their own assumptions and other issues may arise that must be dealt with accordingly.

In the theoretical Poisson distribution, the variance is equal to the expected mean; an assumption which is rarely true in real life data. Often, the variance exceeds the expected mean to a certain degree, resulting in so called overdispersion. This can lead to unrealistically narrow confidence intervals and biased p-values if left untreated. Ways to deal with overdispersion includes estimating a scale parameter by which to multiply the standard errors or to assume a negative binomial distribution instead (Ver Hoef & Boveng 2007).

Another issue that might arise is the presence of excess zeroes in the data, which may occur if the events are very rare or if the interval between time points is short (in days, weeks). This problem can be dealt with using zero-inflated Poisson (ZIP) or zero-inflated negative binomial regression (ZINB) models (Lambert 1992; Hall 2000).

In analyses of proportional data, which is bound between the interval of 0 and 1, the errors will likely follow a beta distribution (Ferrari & Cribari-Neto 2004), and will most likely be non-normal especially if the proportions are close to the upper or lower bounds of the interval. In the presence of exact zeroes and ones, which was the case in some months of the time series studied, the beta distribution can be substituted with a zero and one-inflated beta distribution proposed by Ospina & Ferrari (2012).

There are some alternatives to the above mentioned ARIMA models that can deal with many of the issues presented above, which also allow for generalizations to non-normal error distributions (McKenzie 2003; Brandt et al. 2000; Weiß 2008; Benjamin et al. 2003). For instance, generalized autoregressive moving average (GARMA) models (Benjamin et al. 2003) can be used to quantify the effect interventions in ITS analyses while dealing with non-normally distributed error distributions and incorporating time series dependence in the analysis. Since most tests for autocorrelation are valid only under the assumption that the errors are normally distributed, Benjamin et al (2003) recommend the use of a normalization procedure detailed by Dunn & Smyth (1996) to obtain a set of normalized randomized quantile residuals to use for these tests instead to ensure that the p-values associated with the autocorrelation tests are not biased.

2.5.5 Semi-parametric time series models

As noted in the beginning of Section 2.5, the outcome (Y) is preferably modelled non-parametrically as some unknown function of time, *f(T)*, to avoid making any prior assumptions about the functional form of the time trend. Stasinopoulos & Rigby (2007) proposed a class of semi-parametric generalized additive models for location, shape and scale (GAMLSS), in which both parametric and non-parametric terms can be modelled simultaneously. Non-parametric smoothers, such as cubic or penalized splines, can therefore be used to estimate *f(T)*. In their package for the statistical software R, GAMLSS, Rigby & Stasinopoulos (2013) implemented an algorithm for automatic smoothing parameter estimation in GAMLSS and GARMA models, eliminating the subjective choice of functional form, which must otherwise be specified by the researcher. The method is data-driven and the optimal choice of between linear, quadratic, cubic or any high-order polynomial trend is selected based on the best fit for the available data. This method is not only preferable because it is less subjective, but also because it has a good chance of capturing and correcting for non-linearity bias; that is, when non-linear trends are mistaken for structural breaks or discontinuities due to model miss-specification (Angrist & Pischke 2008; Sullivan et al. 2015). See Figure 3 below for a graphical example of this.

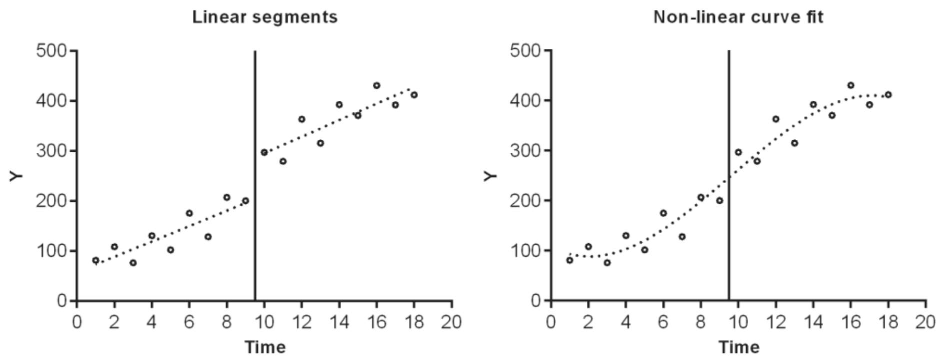

Figure 3. *Fictional example of when a non-linear time trend has been mistaken for a structural break in a time series. The results from a segmented linear regression model in the graph to the left indicate a significant intervention effect, while the non-linear curve in the graph to the right indicate no discontinuity at the time of intervention (which is indicated by the vertical line).*

3. Aims

The application of the ITS design may serve as a possible solution to many of the threats to internal validity in before-after studies of safety interventions. High quality time series data from hospital-based registers and traffic accident reporting systems are readily available in many countries, and recent developments in semi-parametric regression modelling has enabled the modelling of complex non-linear trends in time series of low counts, rates and proportions. An increase in the use of the ITS design may thus be warranted in order to enhance the internal validity in studies of intervention effects to build a better foundation for evidence-based policymaking and practice. The aim of this licentiate thesis is therefore to assess the strengths and limitations of the interrupted time series design to evaluate the impact of interventions in the road traffic environment, and the specific aims of the studies included are:

Study I: To assess and quantify the effect of the Swedish bicycle helmet law for children on the risk of head injuries among child cyclists.

Study II: To assess and quantify the effect of the introduction of the AM license for Class 1 mopeds on road traffic injury events involving teenage moped drivers.

4. Methods and materials

4.1 Description of the interventions

In **Study I**, the intervention in focus was a mandatory bicycle helmet law for children under the age of 15 years. It was enacted nationally in Sweden in January 2005, and is still in place at the time of writing. A key component to the understanding of this intervention lies within the broader context of the Swedish judicial system, namely that the age of criminal responsibility is 15 years – meaning that children who cycle without a helmet cannot be penalized by fines even in the presence of a helmet law. The case of the bicycle helmet intervention was partly chosen due to a debate within the scientific literature regarding the effect of bicycle helmet laws (Olivier et al. 2014), where opponents to mandatory helmet use often invoke the above mentioned potential sources of bias to invalidate studies of the effect of bicycle helmet legislation on the risk of head injuries (Robinson 2006; Clarke 2012; Robinson 2007). Furthermore, bicycle accidents result in a high number of severe injuries in road traffic with high risk of permanent disability per year according to recent measurements using insurance data, and head injuries contribute greatly to this risk (Rizzi et al. 2013; Niska et al. 2013; Malm et al. 2008).

In **Study II**, the studied intervention was a tightening of the licensing rules for Class 1 mopeds[2] associated with an EU directive (2006/126/EC) on driving licenses which requires all member states to introduce the AM license category for mopeds. Moped riders are approximately nine times as likely as a car occupant to be killed in an accident per kilometre travelled (Bjørnskau 2009). It is also a popular mode of transport among teenagers, especially 15-year-olds, where other options for motorized transportation are few. At age 15, moped crashes are the leading cause of road traffic injuries in Sweden (Strandroth 2007), and moped riders accounted for 44% of all road traffic mortality in this age group in the last 10 years, which is almost

[2] To be classified as a Class 1 moped, the moped must have a speed restriction of 45 km/h. This is the fastest type of moped in Sweden.

twice that of car occupants at the same age (Swedish Transport Agency 2011). The incidence of fatal and non-fatal injuries to moped riders remains relatively high until early adulthood (Strandroth 2007).

As a result of the new licensing rules, an AM license is now required to drive a Class 1 moped in Sweden since October 2009. However, this did not include previous holders of a conditional driving license for mopeds, administered before October 2009. Instead, they could be issued an AM license upon request without completing the new course (Swedish Transport Agency 2009). Moreover, holders of any other type of driving license can legally operate a moped.

Before October 2009, drivers of Class 1 mopeds were required to complete an 8-hour theoretical course, which usually involved some practical elements and skills training away from traffic, and to pass a theory test issued by an authorized examiner in order to obtain a conditional driving license. While the procedure to obtain a license still involves a mandatory theory test, there are some vital practical differences. The education now includes a minimum of four hours of practical training, including traffic-based driving practice, extending the length of the mandatory education to a total of 12 hours. Due to this, acquiring an AM license now involves additional fees that were not present prior to October 2009. Obtaining a conditional license used to cost approximately 2000 SEK (240 USD in February 2015) prior to the intervention. According to the Swedish Transport Agency, acquiring an AM license now costs approximately 5000 SEK (600 USD in February 2015), which means that the intervention may have served as an economic deterrent to moped ownership and, by extension, moped use by more than doubling the price of the driving education. Another important aspect of the new licensing rules is that the AM driving license can be recalled in the event of a severe traffic violation (such as drunk driving), which was not the case prior to October 2009.

4.2 Data collection

4.2.1 Study I

For **Study I**, data on the monthly number of bicycle-related hospital admissions (ICD-10 (International Classification of Diseases) external cause codes V10-19) during the period 1998–2012 were obtained from the Swedish National Patient Register (NPR). This database contains information on hospital admissions with complete national coverage of all hospitals in Sweden since 1987 and the data are considered valid and suitable for large-scale population-based studies (Ludvigsson et al. 2011). Head injuries were defined as a main diagnosis of intracranial injury (ICD-10 injury code S06), skull fracture (S02.0, S02.1, S02.9), scalp injury (S00.0, S01.0, S08.1), and miscellaneous head injury diagnoses (S07.1, S09.8), specifically excluding injuries to areas of the head that are not covered by a bicycle helmet (such as the face and neck). All other available injury codes were defined as non-head injuries. The injury data were stratified into two different age groups: ≤14 years (treatment group) and ≥15 years (comparison group), and the data was also stratified by sex to test for differential effects by gender.

The NPR was deemed the most reliable *long-term* data source for head injury data over the course of the study period (1998-2012). The pre-intervention trend component and counterfactual trajectory during the post-intervention period in the absence of the intervention is highly reliant on the ability to forecast based on information from prior time points, and the more time points available the better (Box & Jenkins 1976). The restriction to the period after 1998 was chosen due to a change in classification system from ICD-9 to ICD-10 in 1997 since there were some concerns regarding potential losses in data quality as a result of some large changes in external cause coding (Janssen & Kunst 2004). It was assumed that this could have affected the reliability of the bicycle-related injury data since it requires a link between injury diagnosis and external cause code. In a later study, Nilson et al (2014) confirmed that there was a large discontinuity in the proportion of injuries recorded in the NPR without an external cause code at the time of the coding change in 1997; and that the issues, while exponentially decaying, lingered for several years after the transition to ICD-10. The data from the 1998-2001 period analysed in **Study I** might thus be of lower quality than the data from the years post, but this should not affect the intervention effect estimate since it was implemented in 2005. Furthermore, the data quality issues should be sufficiently captured by the non-linear time trend adjustments. Although we were unaware of the issues at the time, an alternative would have been to include a variable measuring the proportion of patients admitted due to injuries without an external cause code as a covariate, or a set of year-dummies in the statistical analysis to directly adjust for the years of lower data quality. The full time series of hospital admissions data consisted of 84 pre-intervention time points and 96 post-intervention time points.

As an addition to this thesis, helmet use data was also collected from two separate sources. Data on observed helmet use among 6 to 12-year-old children cycling to and from primary schools was obtained from annual reports by the National Road and Transport Research Institute (VTI) (Larsson 2014). The observed helmet use data is available from 1998 and aggregated at the annual level. The number of pre-legislation time points in this series was 6, and the corresponding number for the post-intervention period was 9. Helmet use data from cyclists presenting at emergency departments was also collected from the Swedish Traffic Accident Data Acquisition (STRADA) register. This data was aggregated at the monthly level from January 2000 to December 2013, which resulted in a time series of 60 pre-intervention time points and 108 post-intervention time points.

4.2.2 Study II

Data on road traffic injury events involving Class 1 moped drivers in the age group 15-17 years reported by the police was extracted from the STRADA register for the study period 1st January 2007 to 31th December 2013. The register contains all road traffic injuries and deaths reported by every police district in Sweden (Swedish Transport Agency 2011). The data that the register contains is anonymized, and the police reports only contain information on the age and sex of the persons involved, along with detailed information on the crash event, such as time, location, vehicle type(s), crash type and a visual assessment of the severity of the injuries sustained. The police are required by law to report all road traffic injury events to which they become aware of to the register, which is done using a standardized form. The assessment of injury severity is made at the scene of the crash. A severely injured person is defined as having sustained a fracture, crush injury, laceration, severe cut, concussion or internal injuries. An injury that the officer suspects will result in hospital admission is also classified as severe. Other injuries are classified as slight (Swedish Transport Agency 2013). Information on vehicle type and driver age, time of the event (year and month), and the severity of the injuries sustained by the persons involved was used in order to identify relevant cases.

The number of Class 1 moped drivers fatally injured during the study period was deemed too low to analyze separately (n=17). Deaths were thus excluded from the statistical analysis in order to avoid making any statements regarding reductions in fatal injury events by aggregating them together with non-fatal injury events. Previous studies have shown that the police tend to overstate the severity of non-fatal injuries (Farmer 2003), and the severity coding was thus considered too unreliable to disaggregate severe and slight injury events in the main analyses.

Moped registration data was also retrieved from Statistics Sweden. This data consists of the overall number of registered Class 1 mopeds in traffic per month, and is available from January 1st 2007. Age-specific moped registration data only concerns the age of the owner, which is most commonly a person aged 41-50 years, suggesting that the user (which is the person of interest in this case) cannot be identified properly since these are most likely parents of the actual users.

The full time series included monthly aggregations of non-fatal injury data from 84 months (33 pre-legislation and 51 post-legislation). The main outcome measures were the number of non-fatal injury events involving Class 1 moped drivers between 15 to 17 years of age, and the number of non-fatal injuries (to drivers, passengers and counterparts) as a consequence of these events.

4.3 Study design and statistical analysis

In subsection, the empirical strategies employed in the attempt to identify the causal effects of the interventions, and strengthen the internal validity of the effect estimates, will be detailed.

In **Study I**, where we measured the effect of the Swedish bicycle helmet law on the risk of head injury among child cyclists, the proportion of patients hospitalized with a head injury as a result of a bicycle accident (of all injuries due to bicycle accidents) was used as the main outcome measure. This was chosen since it should eliminate the effect of potential changes in exposure as consequence of the law, and should be a viable indicator of the risk of obtaining a head injury in the event of a bicycle crash that is severe enough to warrant hospitalization. Unless the composition of injuries changed due to some other unobserved factor associated with the law other than an increase in helmet use observed at the same time, the effect estimate derived from this outcome measure should be internally valid even in the presence of exposure-related changes. To enhance the internal validity of the estimate further, a CITS design (Equation 3) was also employed using the proportion of head injuries among adult cyclists (15+ years) as a comparison series.

In **Study II**, the aim was to identify whether the introduction of the AM licence category for Class 1 mopeds resulted in a reduction in road traffic injury events involving Class 1 moped drivers. Instead of using a comparison series to enhance the internal validity of the ITS design, we exploited a characteristic of the intervention that meant that all those born prior to August 31 1994, e.g. those who were younger than 15 years at the time of intervention, could not obtain an AM license or operate a Class 1 moped by any other means than taking the educational course associated with the intervention. Recall that the intervention was devised so that previous holders of conditional moped licenses, which were used prior to the intervention, could submit a request to have their license changed to an AM license. Furthermore, holders of other types of driving licenses can still operate a Class 1 moped legally without an AM license. The birth cohort that was hypothesized to be most affected by the stricter licensing rules was thus followed through time by studying the instant effect of the intervention on 15-year-olds in October 2009 when the changes were enacted, and delayed effects among 16-year-olds in October 2010 and 17-year-olds in October 2011. Three separate ITS models (Equation 1) were therefore estimated in which the intervention dummy, D, was delayed by 12 and 24 time points for 16-year-olds and 17-year-olds, respectively.

Since no good arguments for a gradual effect in any of the interventions studied as a part of this thesis could be found, only abrupt and permanent effects were considered[3]. The identifying assumption of a causal effect in the ITS analyses in both studies thus becomes that no concurrent events affected the outcome at the exact time of intervention that can explain an abrupt and permanent effect in the post-intervention period.

4.3.1 Seasonality

The seasonal adjustments detailed in Equation 2 were included in all analyses, since monthly data was analyzed in both studies. A data-driven strategy was applied to find the optimal adjustments for seasonality. First, a model with only full year seasonality was estimated, followed by an iterative re-estimation process where more seasonal patterns were added ($k = 2$, $k = 3$, etc.) until seasonal effects were no longer visible in residual plots. In both studies, a combination of full-year and six-monthly seasonality best, and most parsimoniously, described the seasonal variation in the outcome data.

[3] If the reader is interested, Zhang et al (2009) present a method for dealing with cases in which both abrupt and gradual effects are present.

4.3.2 Model estimation

The equations 1-3 were estimated using semi-parametric generalized additive models (Stasinopoulos & Rigby 2007). In particular, time trend was estimated using automatic non-parametric smoothers (Rigby & Stasinopoulus 2013) to avoid making any prior assumptions of its functional form, and to decrease the risk of non-linearity bias. In cases where autocorrelation was present, generalized autoregressive moving average models were estimated instead (Benjamin et al. 2003). The dependent variable in **Study II** was in the form of aggregate counts. To avoid inflated standard errors due to overdispersion, a negative binomial distribution was assumed instead of a standard Poisson distribution. Zero-inflation was accounted for in some stratified exploratory time series of very low counts (these analyses are only presented in the appended article). In **Study I**, the main dependent variable was in the form of aggregate proportions, and a beta distribution was therefore assumed. In some cases, adjustments for zero and one-inflation were necessary.

A portmanteau test with automatic lag selection, proposed by Escanciano & Lobato (2009) was employed in both **Study I** and **II**. The method was preferred since it is data driven and thus eliminates the subjective choice of maximum lag order at which to test for autocorrelation. Since the dependent variable in both studies were assumed to be non-normal, the normalization procedure detailed by Dunn & Smyth (1996) was applied in **Study I** & **II** to check for residual autocorrelation before and after any adjustments were made, as recommended by Benjamin et al (2003). After the autocorrelation process was identified visually using ACF and PACF graphs, an iterative procedure of fitting the corresponding, and most parsimonious, models to the data was undertaken. The residuals from the final models were then tested again using the portmanteau test proposed by Escanciano & Lobato (2009) to confirm that no significant autocorrelation was present.

All statistical analyses were performed in R (R Core Team 2014) using the GAMLSS (Rigby & Stasinopoulus 2007) and vrtest (Kim 2010) packages. P-values below 0.05 were considered statistically significant.

4.4 Ethical considerations

The data collected for the purpose of this thesis involves information regarding the health of human beings, which according to the Personal Data Act (1998:204) is classified as sensitive information if it can be linked to a specific individual. To conduct research on such data, approval by an ethics board is required only if a dataset includes information so that an individual can be identified either directly (through a personal identification number, name, etc.), indirectly or through an identification key that can be used by the register holder to identify specific individuals, according to the Act concerning the Ethical Review of Research Involving Humans (SFS 2003:460). However, if the dataset available to the researcher does not contain personal information, submission of an ethics application for approval by an ethics review board is not required.

All data used in this thesis was collected by third parties. The Swedish NPR is hosted by the National Board of Health and Welfare, and the collection of the hospital discharge data included in this register is governed by the National Board of Health and Welfare policies SOSFS 2008:26, SOSFS 2009:26 and SOSFS 2011:4. The register aims to serve public health and research interests.

The police-reported data collected from Swedish Traffic Accident Data Acquisition (STRADA) is also collected routinely and governed by the Act concerning the investigation of accidents (SFS 1990:712), which states that the police are required to inform the relevant regulatory authority of any accidents that come to their knowledge. In the case of road traffic accidents, this authority is the host of the STRADA register, the Swedish Transport Agency.

The hosting authorities manage and determine the availability of the routinely collected data for research purposes. In general, the data from the NPR is made available to the public if the National Board of Health and Welfare determines that there is no risk of indirect identification of specific individuals. The Swedish Transport Agency allows researchers to access anonymized data from the STRADA register for research purposes.

The data obtained from the NPR is aggregated at the national level, i.e. it does not even contain individual level data. The data obtained from STRADA is anonymized, and cannot be linked to specific people. Nevertheless, some ethical considerations are still required in the presentation of such data in order to avoid any risk of indirect identification of individuals. The studies are only concerned with the population-level impacts of the two policy changes under study, and all data is presented in the form of aggregate numbers at the national level. Furthermore, they are purely observational with no influence on the decision to implement the studied interventions. It was the research team's collective understanding that there was no potential for harm to human subjects as a result of the analysis of data collected as part of routine injury surveillance systems, and that the possibility to identify specific individuals using the available data was extremely low even with the use of indirect identification through deduction. As a result, approval from an ethical review board to conduct these studies was not requested.

5. Results

5.1 Main results from Study I

Throughout the time period 2000 to 2013, the prevalence of helmet use among child cyclists under the age of 15 presenting at emergency departments was 47.75%. The corresponding prevalence for adult cyclists aged 15-28 years was 10.55%. In the pre-intervention period (2000-2004), helmet use prevalence was at 35.65% among children. The prevalence in the post-intervention period (2005-2013) was 54.47%, which is considerably higher than before the helmet law.

Table 1. *Age and sex-specific effect estimates of the Swedish bicycle helmet law on the prevalence of helmet use observed at primary schools (in observational studies conducted by VTI) and at emergency departments (STRADA). All estimates are adjusted for trend and seasonality.*

Group	Data	Effect estimate (percentage point change, 95% CI)	P-value
Male cyclists			
6-12 years	Observational	+12.84 (3.04, 22.65)	0.01
6-12 years	STRADA	+19.87 (15.56, 24.19)	<0.01
0-14 years	STRADA	+14.46 (11.12, 17.80)	<0.01
12-14 years	STRADA	+15.66 (11.03, 23.33)	<0.01
15-28 years	STRADA	-0.32 (-3.82, 3.17)	0.86
0-14 years (CITS)	STRADA	+12.14 (7.56, 16.71)	<0.01
Female cyclists			
6-12 years	Observational	+19.76 (6.56, 32.62)	<0.01
6-12 years	STRADA	+12.29 (7.13, 17.44)	<0.01
0-14 years	STRADA	+13.53 (9.36, 17.71)	<0.01
12-14 years	STRADA	+17.38 (11.43, 23.33)	<0.01
15-28 years	STRADA	+2.85 (-0.46, 6.15)	0.09
0-14 years (CITS)	STRADA	+9.63 (4.88, 14.39)	<0.01

P-values for residual autocorrelation according to a portmanteau test proposed by Escanciano & Lobato (2009) were above 0.05 and residual plots showed no evidence of autocorrelation in the final models. CITS estimates are the difference in discontinuity between 0 to 14-year-olds and 15 to 28-year-olds.

The results from the interrupted time series (ITS) analysis show statistically significant increases in helmet use among both male and female children under the age of 15 in 2005 when the helmet law was enacted after adjusting for trend and seasonality. As expected, no intervention effect was present among adults (Table 1).

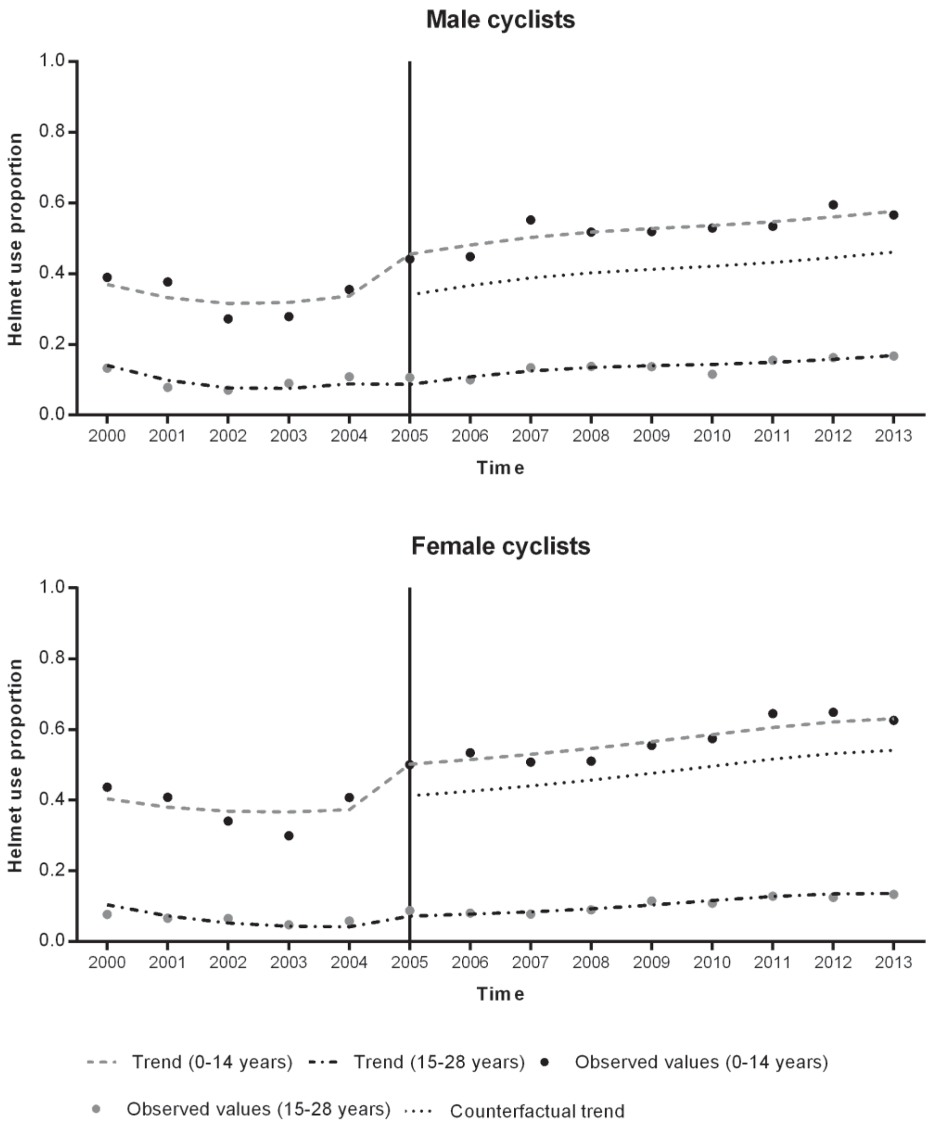

Figure 4. *Average annual prevalence of helmet use among cyclists presenting at emergency departments in Sweden from January 2000 to December 2013 stratified by age group and sex with observed values and model predictions. The vertical line indicates when the bicycle helmet law for children came into effect.*

Similarly, the results from the comparative interrupted time series (CITS) models indicate that the prevalence of helmet use increased by 12.14 percentage points among males and 9.63 percentage points among females in the treatment population (0-14 years). The estimated discontinuity at the time of intervention, along with counterfactual trends for the post-intervention period, can be seen in Figure 4.

From January 1998 to December 2013, a total of 56,477 cyclists were admitted to hospitals in Sweden. One quarter (27%) of the admitted cyclists was under the age of 15, a majority (68%) of which were male.

Table 2. *Results from the time series intervention analysis, displaying the estimated effect of the Swedish bicycle helmet law on head injury proportions among male and female children (0–14 years) hospitalized due to bicycle-related injuries. All estimates are adjusted for time trend and seasonality.*

Model type	**Effect estimate (Odds ratio, 95% CI)**	**Percentage point difference**	**P-value**
Males 0-14 years			
ITS[a]	0.59 (0.45-0.78)	-11.8	<0.001
CITS[b]	0.68 (0.50-0.94)	-7.8	0.02
Females 0-14 years			
ITS[a]	0.99 (0.75-1.31)	-0.1	0.94
CITS[b]	1.05 (0.77-1.44)	1.7	0.74

CI = confidence interval

a Estimate is based on time series intervention analysis without the adult control group.

b Estimate is based on the difference in discontinuity at the time of intervention between children (0-14 years) and adult controls (15+ years).

According to the results from the ITS model, the bicycle helmet law was associated with an 11.8 percentage point decrease in the proportion of head injuries among male children. When controlled against adult males in a CITS model, the effect estimate was reduced to a 7.8 percentage point difference between male children and male adults. However, regarding female children, there was no indication of an effect on the proportion of head injuries as a result of the intervention. The results did not change notably when controlled against adult females (Table 2 & Figure 5). This was an unexpected finding, and does not correspond with the helmet use analyses.

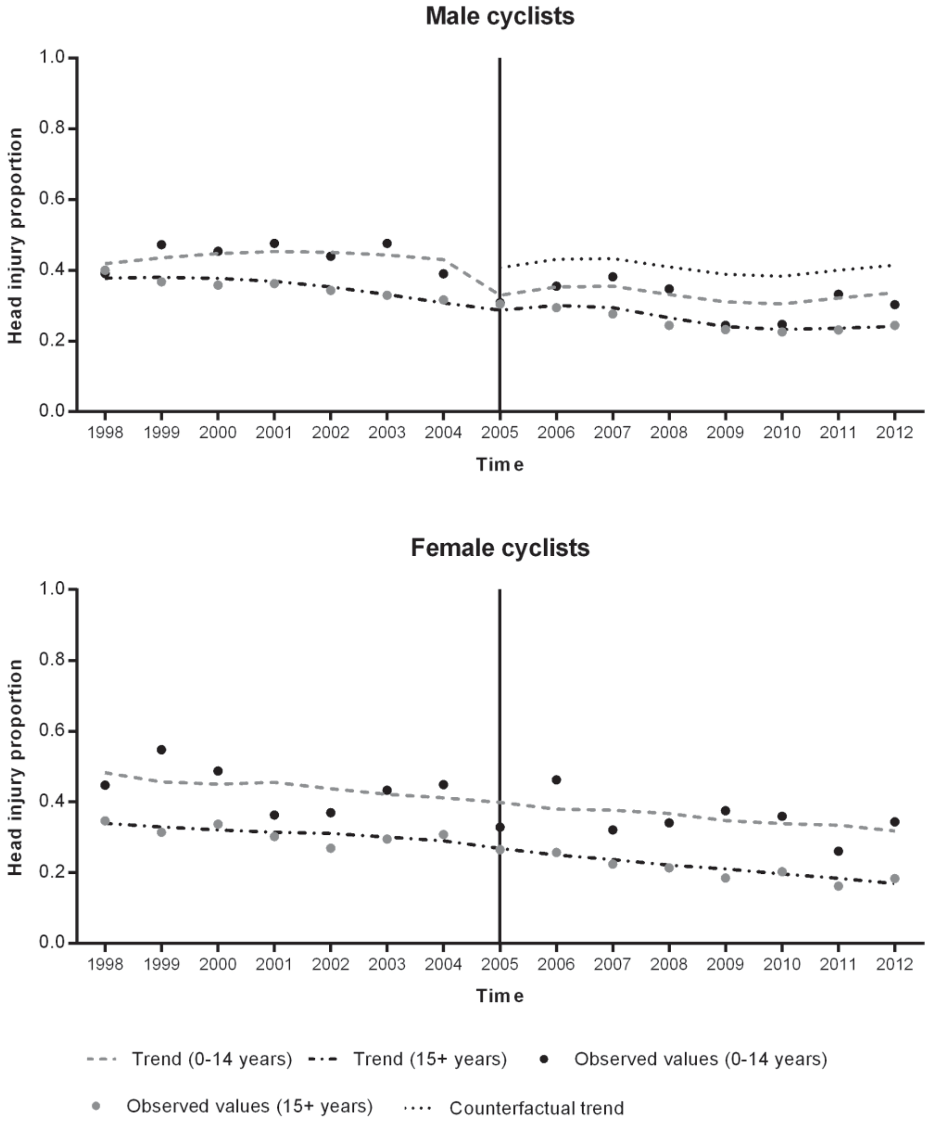

***Figure 5**. Average annual proportion of head injuries among cyclists admitted to hospitals in Sweden from January 1998 to December 2012 stratified by age group and sex with observed values and model predictions. The vertical line indicates when the bicycle helmet law for children came into effect.*

5.2 Main results from Study II

A total of 3876 road traffic injury events involving 15 to 17-year-old Class 1 moped drivers, wherein 5235 persons were non-fatally injured, were reported by the police during the study period (2007-2013). Of the injured, 4600 (88%) were classified as having been slightly injured and 635 (12%) as severely injured. The majority (72%) of the injured were moped drivers, followed by moped passengers (19%) and other road users in conflict or collision with a Class 1 moped driver (9%). Stratified by the age of the moped driver, 2336 (60%) of the studied injury events involved 15-year-old moped drivers, 1130 (29%) involved 16-year-old moped drivers and 463 (12%) involved 17-year-old moped drivers.

Table 3. *The estimated intervention effect (incidence rate ratio, IRR) of the introduction of the AM license category for Class 1 mopeds in Sweden on the number of non-fatal injury events involving 15 to 17-year-old Class 1 moped drivers, adjusted for secular trend and seasonality in using GAMLSS models assuming a negative binomial distribution. Bolded estimates are statistically significant ($p < 0.05$).*

Age of driver/Variable	Incidence model[a] (IRR, 95% CI)	Exposure model[b] (IRR, 95% CI)
15 years		
Intervention	**0.59 (0.48-0.72)**	**0.78 (0.64-0.96)**
16 years		
Intervention	0.94 (0.73-1.20)	1.34 (0.98-1.85)
Delayed by 1 year	**0.61 (0.48-0.79)**	**0.69 (0.50-0.95)**
17 years		
Intervention	0.83 (0.58-1.17)	0.88 (0.62-1.26)
Delayed by 1 year	1.17 (0.83-1.66)	1.23 (0.78-1.94)
Delayed by 2 years	**0.64 (0.46-0.90)**	**0.61 (0.41-0.89)**

IRR = Incidence rate ratio.

a The incidence model estimates are adjusted for trend and seasonality.

b The exposure model estimates are adjusted for trend, seasonality and the number of registered mopeds in traffic.

The results from ITS model using the monthly number of non-fatal injury events involving 15-year-old drivers showed a statistically significant intervention effect at October 2009 after adjusting for trend and seasonality, which suggests that the introduction of the AM license resulted in a 41% reduction in the number of injury events in this population. As expected, there was no significant effect on injury events involving 16 and 17-year-old drivers at this point in time. However, significant intervention effects were found by delaying the intervention variable by one year for 16-year-olds and two years for 17-year-olds, as the new licensing rules would then fully encompass drivers of these ages as well. The results would indicate that the intervention resulted in a delayed reduction of 39% and 36% in injury events involving Class 1 moped drivers of ages 16 and 17, respectively (Table 3).

The estimated break in secular trend is displayed graphically in Figure 6, along with predicted values (and 95% confidence intervals) had the AM license not been introduced based on the inverse of the intervention effect estimate.

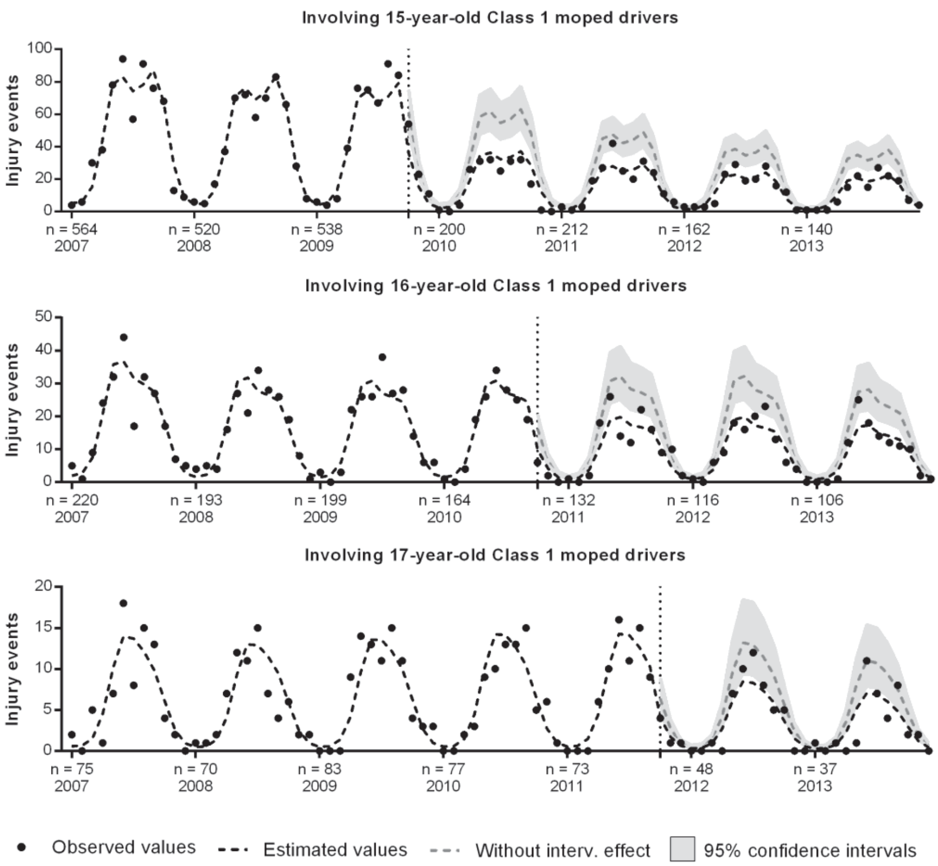

Figure 6. *Time series of the number of injury events involving teenage (15-17 years) Class 1 moped drivers reported by the Swedish police stratified by age of the driver, with estimates from the negative binomial GAMLSS models and predicted values (with 95% confidence intervals) had the intervention not taken place based on the reciprocal of the intervention effect (1/IRR) from the incidence model.*

As displayed in Figure 7, the intervention appears to have been accompanied by a decrease in the number of registered Class 1 mopeds in traffic. After adjusting for exposure by means of using this as a covariate in the model, the effect estimate was decreased to a reduction of 22% in non-fatal injury events involving 15-year-old drivers, indicating that roughly half of the effect could be explained by a decrease in the number of registered mopeds in traffic. However, the estimate remained statistically significant (p = 0.02), suggesting that the intervention effect was only partially mediated by changes in exposure in conjunction with the introduction of the AM license. The addition of the exposure covariate only marginally affected the intervention effect estimates regarding injury events involving 16 and 17-year-old drivers (Table 3).

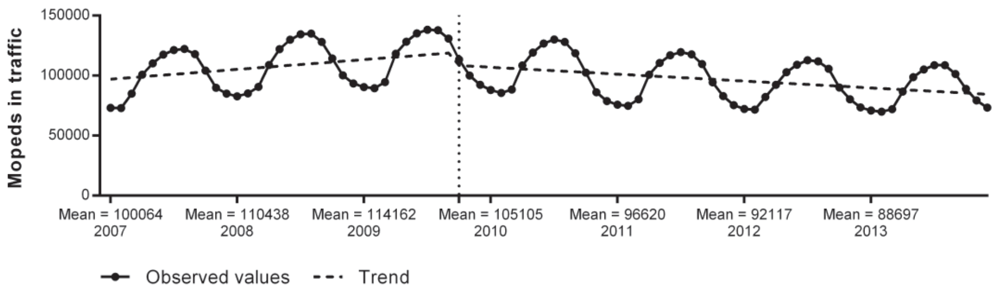

Figure 7. *Time series of the number of registered Class 1 mopeds in traffic in Sweden by month from 2007 to 2013, with estimated trends from a segmented linear regression model before and after the introduction of the AM license category in October 2009.*

6. Discussion

In the following section, the methodological strengths and limitations of the ITS design will be reflected upon with regards to internal and external validity, along with other limitations associated with the included studies. In light of these methodological considerations, the implications of the empirical studies will also be discussed.

6.1 Threats to internal validity

6.1.1 Historical bias and secular trends

As noted in Section 2, the presence of secular trends induced by other ecological factors that affect the outcome of interest can result in biased estimates in before-after measurements. These trends are explicitly modelled in the statistical analysis of the ITS design, and should thus be less of an issue than in simple before-after studies. Nevertheless, some consideration concerning the adequacy of the statistical adjustments is required. For instance, the functional form of the trend might be incorrectly specified, resulting in biased effect estimates. While the semi-parametric smoothing techniques used in the studies included in this thesis should reduce the risk of incorrect trend specification, the data-driven approach is still highly dependent on the length of the available time frame. If the study period is too short, secular trends might not be discernable. There are many different recommendations for the minimum amount of time points before and after an intervention required to perform an ITS analysis. The number of time points required varies depending on several factors. The time scale is an important factor in this consideration. If data is collected on a monthly basis and there is a seasonal component to the outcome (such as the number of injured bicyclists in the winter versus summer periods), more time points are required to ensure that the observed effect was not due to seasonal changes that are likely to occur every year. Some authors recommend 50 to 100 data points before and after an intervention to compute complex statistical models with seasonal effects and serial dependencies between adjacent time points (Hartmann et al. 1980), but others have shown that interrupted time series analysis can be performed with as few as 8 time points per period (Tryon 1982). The Cochrane Effective Practice and Organisation of Care Group explicitly states that there must be at least three time points before and three time points after an intervention for an ITS study to be included in their systematic reviews (Oxman 2011). Another important factor is the random variability of the data. If there is a great amount of random variability in a short time series, it may be impossible to determine whether an apparent effect was due to the intervention or randomness. In statistical terms, the number of time points is the sample size, and with large variation in a small sample, there may not be sufficient power to detect a smaller effect or to estimate the underlying trends correctly. On the other hand, if the random variability is small relative to the intervention effect, the study may not require as many time points.

In the studies included in this thesis, monthly data was used partly to increase the number of time points, which may increase the ability to model trends if seasonality is correctly adjusted for. Of course, this still does not enable the modelling of more long term trends and cyclic components that would require observations outside the study period to be fully understood.

6.1.2 Instrument bias

An effect estimate can also be biased if an intervention induces a change in the measurement of the dependent variable. Since the majority of the data used for both studies were collected as a part of routine injury surveillance systems, the risk for instrument bias should be minimal. However, the observational helmet use data could for instance be subject to some degree of such bias if the data collectors were influenced by the presence of a helmet law. The same issues may apply to the reporting of helmet use in the emergency department data. While it appears unlikely that such an issue would explain the entire observed effect from two separate sources, it could have biased the estimated effect size.

6.1.3 Regression-to-the-mean bias

A well-known statistical phenomenon is that a value closer to the mean of the dependent variable will follow an extreme value. This is a prominent bias in before-after studies. Since the ITS design models changes in the level of the mean associated with an intervention using more than one time point before the intervention, regression-to-the-mean bias should be of little issue if trends are correctly specified in the regression model (Barnett et al. 2005). However, an extreme outlier in the time point prior to an intervention can cause some issues. If it is large enough to, for instance, result in a significant upward pre-intervention trend estimate even in the absence of an actual trend, the intervention effect estimate will be biased (Wagner et al. 2002). While no indication of such outliers was found in the time series data used for the studies included in this thesis, the issue does pose a threat to the validity of the design unless controlled for by, for instance, using a dummy variable to indicate the wild data point in the regression model.

6.1.4 Concurrent events

The main weakness of the ITS design is most likely that it is next to impossible to separate concurrent confounding events from the intervention effect estimate (Morgan & Winship 2014). For instance, media campaigns associated with the intervention itself or other safety initiatives introduced at the same time could influence the size and direction of the change in injury events. In the case of **Study II**, the case for a causal effect is strengthened by the fact that the reductions appear to have followed the birth cohort that were most affected by the changes, i.e. those who were younger than 15 when the AM license category was introduced, over time. Other unobserved changes (such as media campaigns) that accompanied the intervention should only affect the first estimate for 15-year-olds in October 2009. It appears unlikely that the same unobserved factors would re-appear and bias the observed reduction in injury events one year later for 16-year-olds and two years later for 17-year-olds. However, it is still possible that some unobserved influential changes affected the safety of moped drivers in general at these time points. Furthermore, the possibility that this birth cohort is different from previous cohorts in ways that would influence the occurrence of moped-related injury events even in the absence of the intervention cannot be ruled out.

In **Study I**, adults were used as a concurrent control group in an attempt to rule out safety effects of other unobserved concurrent events, such as increased exposure to helmet use campaigns in media. However, a potential limitation to this approach is that some concurrent events may affect the treatment and control populations differently. The enhancements to the internal validity provided by including a control group is dependent on the degree to which the control group is affected by the events that need to be controlled for. Changes in data quality, hospital admissions policy and other road safety initiatives that are likely to affect the two groups equally should be accounted for, while school-based helmet promotion programs and other interventions initiated at the same time that specifically targeted the treatment population might not be. The author is unaware of any national initiatives that conform to the latter category, but the possibility of such bias cannot be fully discounted.

6.1.5 Spillover effect bias

If an intervention causes spillover effects into a control area or population, a CITS effect estimate will likely be negatively biased because the difference in discontinuity at the time of intervention will be smaller. It does not seem unlikely that a bicycle helmet law for children could influence adults to wear helmets, especially parents in the presence of their children. However, both helmet use and hospital admissions data would suggest that the average adult cyclist was not affected by the helmet law. Nevertheless, in theoretically ambiguous cases such as this it may be more conservative to assume that there might be some spillover effect bias present.

Study II focused only on road traffic injury events involving drivers aged 15-17 years, and it is possible that the impact of the intervention is not limited to this age group. There is also a possibility that the population that would have driven a moped in the absence of the intervention has shifted toward using other modes of transportation, perhaps increasing the incidence of injuries associated with these.

6.2 Potential measurement issues

Research that relies on administrative data sources is highly dependent on the availability and quality of relevant data. Both studies included in this thesis measured the effect of interventions retrospectively using previously collected time series data, and are thus restricted in terms of choice of definitions (by for instance, ICD codes), geographic and temporal coverage of the register, accuracy of the registered data, available covariates and the availability of exposure data (Sorensen et al. 1996). The latter will likely pose a threat to most studies that aim to quantify the effects of an intervention on injury risks per traffic volume among vulnerable road users using the ITS design, since reliable exposure data for these modes of transportation appear rare (especially in a continuous time series format). However, this is not a problem if the aim is to only quantify changes in the absolute number of injuries. The known measurement-related issues of the included studies are detailed and discussed below.

In **Study I**, there is a lack of true exposure data, such as trends in bicycle use. It has been suggested that mandatory bicycle helmet laws can deter people from cycling (Robinson 2007), and concerns have been raised that this can result in an overall negative health impact due to a reduction in active transportation, especially since the health benefits from cycling have been shown to outweigh the safety risks (de Hartog et al. 2010). Although a recent Canadian study showed no signs of reduced cycling as a result of mandatory helmet legislation (Dennis et al. 2010), and the modal share of cycling in Sweden seems to have increased from 10% in 2005/2006 (Swedish Institute for Transport and Communications Analysis 2007) to 17% in 2011 (Gallup 2011), the possibility of a deterring effect among children cannot be fully discounted as this data has not been collected continuously and does not display different age strata. In an attempt to overcome this lack of exposure data, the effect of the law was measured by analyzing head injuries as a proportion of the total bicycle-related admissions, thereby taking fluctuations in bicycle ridership into account by using non-head injuries as a proxy, as has been done in previous studies (Cook & Sheikh 2003; Dennis et al. 2013; Karkhaneh et al. 2013). The intervention analysis is thus based on the assumption that the exposure to head injury and non-head injury during injury events did not change in some other way than by increasing helmet use at the time of the intervention. However, this may not hold true if the pattern of the injury events changes, for example if collisions with motor vehicles become less prevalent by deterring a certain population more prone to these events. Furthermore, information on injury severity is not available in the Swedish National Patient Register (NPR) since the database uses the ICD coding system. The number of severe diagnoses such as skull fractures was also very low, meaning that any shifts in injury severity, which may also be a plausible effect of a helmet law, could not be studied. Finally, although the Swedish National Patient Register is considered reliable since 1987, with high data quality and low rates of registered admissions with missing external cause codes (Ludvigsson et al., 2011), the research team had no control over the injury registration process. Therefore, mistakes and misses in the data cannot be ruled out.

An alternative data source, which was considered initially, was the Swedish Traffic Accident Data Acquisition (STRADA) database. STRADA contains data on injury severity and helmet use, but lacks the same long-term stability in data quality during the study period. It was introduced in 2000, and by that time only a select few emergency departments had agreed to report traffic injury information to the register. Since helmet use varies greatly by region (Gustafsson 2013), it was expected that the longitudinal head injury and helmet data could be confounded by the addition of more emergency departments across Sweden. During the course of the study period, the coverage has gradually increased, and by 2013 the coverage is almost complete, with 99% of all emergency departments reporting detailed road traffic injury data to the register. Despite these potential limitations, helmet use data was still collected from the STRADA register because the

NPR does not include information on helmet use. As a precautionary measure, helmet use data was also collected from annually published observational helmet use studies in order to validate the helmet use data from STRADA. The trends and intervention effect estimates were similar, which indicates that the emergency department helmet use data may be representative. However, it cannot be concluded that cyclists who present at emergency departments do not differ systematically from cyclists in general until more in-depth validation studies are made.

There are also several measurement-related limitations in **Study II**. Firstly, there is the use of registered mopeds as a proxy for exposure. While the number of mopeds in traffic may be an indication of population-level exposure, it is inferior to actual measurements of exposure, such as distance travelled or time spent in traffic. Without actual exposure data it is impossible to fully discard the notion that the intervention had no effect on the risk of injury beyond a reduction in exposure. However, unfortunately, to the author's knowledge, no such data exists for mopeds in Sweden in a continuous time series format. The use of overall mopeds in traffic without being able to disaggregate by the age of the moped user also poses a further threat to the validity of the proxy measure.

Secondly, police-reported injury data may be subject to a larger degree of underreporting compared to hospital data, especially concerning vulnerable road users (Rosman & Knuiman 1994; Cercarelli et al. 1996), and the absolute number of injuries reported in this study is thus likely conservative. Furthermore, the reporting of injury severity by the police is not as reliable as severity scores coded by physicians at hospitals (Farmer 2003). The STRADA database also contains data from emergency departments, but coverage, while it has increased over the last decade, is still not complete. Police-reported injuries were thus deemed to provide the most consistent outcome measure over time during the study period. Using hospital admissions data from the NPR may have been preferable with regards to the reliability of injury reporting. However, due to the structure of the ICD-10 external cause codes, moped injuries belong to the same external cause code as motorcycle injuries, therefore eliminating this possibility. Furthermore, STRADA contains information on moped type, and distinguishing Class 1 mopeds from other models was necessary for the purpose of **Study II**.

6.3 Threats to external validity

Limitations that often plague many studies that aim for high internal validity are issues of generalizability (Rothwell 2005). The studies included in this thesis are no different. For instance, the effect estimates would likely be different if the Swedish bicycle helmet law was implemented at a different point in time (for instance, when acceptability was lower) or if the law had encompassed a different age group. The ability to generalize the exact effect estimates obtained in **Study I** to other jurisdictions therefore appears low, especially to countries that are very different from Sweden. The effect of the stricter licensing rules for Class 1 mopeds may also be unique to the circumstances in which the intervention was implemented. For instance, the effect may have been different if the price change associated with the enhanced driving education was smaller, or if mopeds were more or less popular among adolescents to begin with.

Despite this, two things should be noted. Firstly, if the internal validity of a study is low, it can be argued that the generalization of its results is fruitless since we do not even know if the results are correct. Secondly, even though the results of a study may not be applicable to all regions or populations of the world, they may serve as a part of a larger puzzle aimed to achieve a more complete picture of the effects of injury control measures. The results can be used for meta-analysis in systematic reviews to obtain summary effect measures (Elvik et al. 2009), or meta-regression studies to test which characteristics of an intervention affect the effect size (Baker et al. 2009). After enough studies accumulate, a meta-regression of, for instance, helmet law studies may help to answer questions such as which age groups to select for treatment in order to achieve greater increases in helmet use and the degree to which police enforcement affects the results.

6.4 Determining the causal mechanisms behind the effect

An issue which must be dealt with in studies measuring only the effect of an intervention is that questions such as 'how' and 'why' the intervention worked can rarely be answered (Nilsen 2006). This can be a severe limitation since it directly affects the ability to reproduce complex interventions. Instead, a direct study of the process must be replaced with an attempt to discuss the results with the use of theoretical arguments and empirical evidence from previous studies. The ability to identify the causal mechanisms behind the observed effects is therefore also prone to the same cognitive biases that we try to avoid by using empirical data and objective tests to quantify intervention effects.

The results from **Study I** indicate that the Swedish bicycle helmet law decreased the proportion of child cyclists hospitalized due to head injuries in conjunction with a simultaneous increase in helmet use. There is a plethora of case-control studies and meta-analyses that provide observational evidence of efficacy of bicycle helmets in reducing the risk of head injury (Attewell et al. 2001; Thompson et al. 1999), which would suggest that the changes are causally linked. However, no evidence of an effect on the proportion of head injuries among female child cyclists was found, even though there was a clear jump in helmet use in the same population. In adolescents, the presence of peers can exacerbate the willingness to perform risky behaviors beyond those that an individual actually intends to perform in order to gain social approval (Gardner & Steinberg 2005; Chein et al. 2011). A possible explanation for the lack of evidence of an effect on the risk of head injuries among female children may be that the subset of the female population that are subject to severe bicycle-related injuries that result in hospital admission are somehow different from the populations observed at cycling to and from schools, perhaps with regards to factors that affect risk-taking behavior. For instance, children cycling to and from friends or parties during evenings and nights may be less prone to wear a helmet. Yet, this does not provide a satisfactory explanation of why an increase in helmet use was observed in emergency department data as well. Furthermore, for the hypothesis to be true, it would require that female children are more affected by the perception of peers than males. While there is some evidence that female adolescents are more susceptible to peer effects concerning marijuana and cigarette use (Mason et al. 2014), no studies appear to have tested for such gender differences with regards to bicycle helmets. However, perhaps the simplest potential explanation is that the number of female cyclists in the treatment group hospitalized each month was too low for a 10-19 percentage point increase in helmet use to be discernable using hospital admissions data. The amount of female cyclists hospitalized during the study period in the treatment group was considerably lower than male cyclists. This may have induced higher variability in the time series of head injury proportion, which can limit the possibility to successfully discern discontinuities in a time series (Wagner et al. 2002). Nevertheless, this inconsistency in the evidence provided by the study can also be used to support rival anti-helmet legislation claims, such as notions that bicycle helmets do not decrease the risk of brain injury due to angular acceleration (Curnow 2005) or that mandatory helmet laws are not associated with simultaneous decreases in head injuries (Robinson 2007). However, the absence of a statistically significant effect should not be interpreted as evidence of no effect, which would require substantially smaller standard errors (Tarnow-Mordi & Healy 1999).

Whether or not increased helmet use caused the observed effect is perhaps not as perplexing to most as the question of what caused the increase in helmet use in the absence of classic deterrence components associated with criminal laws, such as an evaluation of the risk of being caught or the severity of punishment versus the perceived utility of non-compliance (Akers 1990). Recall that children under the age of 15 cannot be sanctioned by a police officer in the event of non-compliance with the mandatory helmet law since the age of criminal responsibility in Sweden is 15 years. Generally, legislative actions are more effective when enforced (Lund & Aarø 2004; Elvik et al. 2009), and the observed effect on helmet use in **Study I** is somewhat lower than the effects observed in countries where mandatory helmet laws pertain to the entire population and where legal sanctions for non-compliance are possible (Dennis et al. 2010; Macpherson & Spinks 2008; Karkhaneh et al. 2011; Scuffham et al.

2000). However, the evidence still suggests that helmet use increased as a result of the legislative action even in the absence of the ability to penalize child cyclists for cycling without a helmet. Studies have also shown there are many barriers to bicycle helmet use, including discomfort, social stigma and perceived ugliness (Villamor et al. 2008; Finnoff et al. 2001), and the evidence that voluntary helmet promotion campaigns actually increase the use of helmets is limited (Owen et al. 2011; Constant et al. 2012). In most theories of behavior change, social norms, and perceptions thereof, are assumed to contribute greatly to an individual's intention to perform certain behaviors (Gielen & Sleet 2003). One potential explanation for the observed effect on helmet use may thus be that the Swedish bicycle helmet law has imposed a new normative state regarding helmet use among child cyclists. In particular, parents may have been given a new set of tools to convince their children to wear helmets when cycling, simply because "that is the law". If this is true, the intervention may thus have operated mainly through parents or other influential people rather than through classic law enforcement. In fact, there is some, albeit conflicting, evidence that targeting parents in safety education campaigns may be more effective than targeting children themselves (Bass et al. 1993; Lund & Aarø 2004). This would probably require a high degree of general acceptance for the use of bicycle helmets (Robertson 2007), since it appears unlikely that parents (or other peers) would enforce or promote the use of helmets without a belief that the pros of helmet use outweigh the cons. Decades of helmet promotion campaigns (Nolén et al. 2005) may have enabled this aspect of the helmet law, and while probably impossible to test empirically, could serve as a possible explanation for its effect. The social context in which the law was implemented may thus be unique to Sweden, making generalization of the results questionable, and the inability to conclusively answer these questions regarding causal mechanisms illustrates a limitation with any study design that is only concerned with quantifying the effects of an intervention. The absence of process evaluation limits the potential to understand the mechanisms behind this observed increase in helmet use.

Furthermore, it can be difficult, and sometimes impossible, to disentangle the effects of different aspects of the intervention. In the case of the tightening of the licensing rules for Class 1 mopeds, there are several potential rivaling explanations for the observed effect on road traffic injury events. The empirical results indicate that the number of injury events involving teenage drivers of Class 1 mopeds has been reduced substantially as a result of the introduction of the AM license. In order to enhance the understanding of the intervention effect, exploratory analyses were conducted by means of stratifying the outcome variable into different categories of injury severity and by role of the road user/vehicle occupant (see Table 3 in the appended article). There appeared to be no coherent difference in relative effect size between the different outcomes studied, indicating that there was a general decrease in the occurrence of injury events involving 15 to 17-year-old moped drivers. Furthermore, there was no clear differential effect by type of crash event or role of the injured road user (driver, passenger or counterpart), indicating that the intervention has reduced the number of injury events involving teenage Class 1 moped drivers in general. Such a general decrease may be indicative of a decrease in exposure, unless the education targets and affects all types of events equally. However, the results suggest that the intervention effect could only partially be explained by changes in exposure, as measured by the number of registered mopeds in traffic. To the author's knowledge, no other studies have investigated an introduction of a similar change in licensing rules for moped drivers. In a similar study, Pérez et al. (2009) found that the number of injured motorcyclists increased after the licensing requirements for motorcycles in Spain were relaxed to allow individuals with any driving license to operate light motorcycles, but they were able to fully explain the increase by adjusting for the number of motorcycles in traffic. If the introduction of the AM license had no effect other than reducing the popularity of mopeds among teenagers, an opposite pattern should have been found in this study. Therefore, the results would indicate that there are perhaps other mechanisms to explain the reduction in injuries associated with the intervention.

Putting aside the potential bias regarding the use of mopeds in traffic as a proxy for exposure, it could be hypothesized that the remainder of the effect is due to the additional education received by new moped drivers. A process evaluation study found that the driving course may have led to an improved knowledge of traffic rules among the students, but the authors also concluded that there was a general lack of focus on improving their risk awareness (Stave 2012). In general, the evidence to support a causal link between information, attitude, behavior and road traffic injuries has been shown to be relatively weak in comparison to modification of the structural environment (Lund & Aarø 2004). In a systematic review of bicycle skills training interventions for children and adolescents, Richmond et al. (2014) found that while several studies have shown an increase in safety-related knowledge as a result of such education, no studies have been able to find statistically significant evidence of an effect on injuries. Even among young car drivers, the evidence of an effect of education on injuries is sparse according to a Cochrane review (Ian & Irene 2001). If anything, safety education seems to appeal more to low-risk drivers (Ulleberg 2001). This would indicate that those who are higher-at-risk of being involved in a road traffic injury event may not benefit from these aspects of the intervention. As noted by the authors of a study comparing information on bicycle helmets to economic incentives in their effect on helmet use (Constant et al. 2012); it appeared that providing information was futile as the effectiveness of bicycle helmets was already widely recognized by the participants. To educate a population with limited or no previous experience of operating a motorized vehicle in traffic on the rules of traffic appears more likely to have some effect, especially with regards to the additional traffic-based driving practice that was introduced along with the AM license. However, to the author's knowledge, no studies have investigated the link between education and injuries with regard to young moped drivers, and caution is therefore advised in the interpretation that the risk of injury may have been reduced as a result of the additional education received. If anything, skills training has generally been shown to be ineffective or even increase the risk of injury due to over-confidence (Lund & Aarø 2004). Furthermore, it would perhaps be expected that, if there was a reduction in injury events as a result of an increased knowledge of traffic rules, the effect size would have been larger with regards to events involving other road users compared to single-vehicle collisions. However, no found conclusive evidence to support this hypothesis was found.

Another factor that could potentially explain the reduction in injuries is the added (actual or perceived) risk of losing the AM license in the event of a traffic violation. This was not the case prior to the intervention, since the conditional moped license was not technically classified as a driving license and thus not subject to the same laws. Previous studies have shown that willingness to commit traffic violations increases the risk of being involved in a road traffic collision as rider of mopeds or other powered two-wheelers in general (Steg & van Brussel 2009; Vlahogianni et al. 2012). A decrease in the willingness to commit these violations induced either by education or the potential loss of the driving license could thus serve as a potential explanation for observed effect. For instance, a reduction in speeding violations should result in a reduction in injuries due to a lowering of the average energy exchange in the event of a collision. It could be argued that the design-related speed restriction on mopeds disallows the exceeding of speed limits on most urban roads and thus reducing the potential effect of a decline in speeding violations. However, the risk of being injured increases exponentially in proportion to vehicle speed, and even a small reduction in average speed could have a noticeable impact on injury incidence (Nilsson 2004). Prior to the intervention, an in-depth study of moped-related deaths found that a large portion of the fatally injured was riding mopeds which had been modified (unrestricted, or trimmed) to enable riders to exceed the construction standard speed limit of 45 km/h (28 mph) (Strandroth 2007). However, the proportion of self-reported use of trimmed mopeds (17%) did not change after the introduction of the AM license according to a before-after survey (Forward et al. 2012). Another serious traffic offence, which is also an important risk factor for collision involvement among moped riders, is driving while intoxicated (Moskal et al. 2012). If moped drivers are successfully deterred from doing this, this should counterfactually result in a decline in injury events involving moped drivers. Although this may be more relevant to drivers in older age groups than those included in this study as the legal drinking age is Sweden is 18 years, underage drinking is not entirely uncommon with a prevalence of 38% among 15 and 16-year-olds (Hibell et al. 2012). However, despite all these considerations, the change in licensing rules appeared to have had a minimal effect with regards to self-reported risk behavior, and the proportion of surveyed drivers (15-24 years) who reported that they feared they would get caught or be involved in a collision while intoxicated was actually significantly lower after the introduction of the AM license (Forward et al. 2012).

Another hypothesis is that the intervention may have reduced the risk of moped injury through a shift in the socioeconomic characteristics of moped drivers. One study found that Swedish adolescents from low-income families were significantly more likely to be injured while driving a moped compared to children from middle-to-high income families (Hasselberg et al. 2001), and being part of a family with lower socioeconomic status (SES) has previously been associated with a 52% risk increase of traffic injuries among Swedish 15 to 19-year-olds compared to being part of a family with high SES (Engström et al. 2002). It seems likely that the families with low SES could have been disproportionally affected by the price change induced by the intervention, which could serve as a potential explanation for some of the discrepancy between the reduction in injuries and the decline in registered mopeds in traffic.

7. Conclusions and implications

In conclusion, there are some weaknesses associated with the ITS design that limit the ability to fully understand key causal mechanisms behind the observable effects on an outcome of interest. This is a common trait in all studies of intervention effects. In some cases the problem can be minimized by the use of process evaluation. A more problematic issue that cannot be solved in the analysis stage of a study is the inability to fully disentangle the effects of subsets of complex interventions. Studies that rely on administrative data to retrospectively study interventions that have already been implemented may also be limited by data availability and quality.

These limitations aside, it is quite clear that the ITS design can provide strong quasi-experimental evidence regarding the observable effects of road traffic safety interventions, even in the presence of secular trends. The ability to adjust for such processes is especially relevant since the incidence of injuries due to most external causes change over time with economic development and societal adaptation to new technologies and risks (Moniruzzaman & Andersson 2008; Nilson 2014; Koornstra 1988). Since the internal validity of the design has previously been shown to rival that of randomized and cluster-randomized controlled trials (Fretheim et al. 2013; Clair et al. 2014; Fretheim et al. 2015), an extension of its use may be warranted. Administrative data on injuries and accidents is routinely collected in many countries (Ludvigsson et al. 2011; Robertson 2007; Andersson 2012), and due to the sheer availability of (in many cases) long time series data, the method appears both feasible and cost-effective.

A further use of methods for causal inference in observational studies will likely provide a stronger foundation of evidence regarding the effects of safety interventions. As noted above, the method appears to have been used mostly to evaluate the effects of national and regional traffic-related interventions. Future studies of small scale interventions, such as municipality-level changes in safety policy or the re-construction of road sections, should consider the use of the ITS method. While statistical power and high variability due to a low number of events per time point can be an issue in these cases, small studies can still provide material for synthesis and summary effect measures in meta-analyses (Bangdiwala et al. 2012). An extension to interventions for other causes of injury, such as falls, fires, drownings and suicide, may also be warranted in order to enhance the quality of the evidence in fields of injury prevention other than road traffic safety.

Acknowledgements

Without the help of a long list of people, this licentiate thesis would have never seen the light of day. The unwavering support of my supervisors **Ragnar Andersson** and **Finn Nilson** has been invaluable. How do I even begin to quantify the impact your guidance has had on my progression as an aspiring researcher? You have inspired me to work hard and learn more than I did during my entire time as an undergraduate and master's student combined. Thank you so much for your confidence in me and my work, and for always taking the time to answer my many questions.

I would also like to thank my colleague, fellow PhD student, lecturer, and ex-project leader, **Johanna Gustavsson**, for even considering hiring this clown as a research assistant in the first place. And of course for giving me rides into town after work. And maybe for the funny conversations and thoughtful discussions about our studies and teaching too.

To the three of you: thank you for seeing something in me that I did not see myself. I think it is safe to conclude that your intervention has had a significant impact on me, both professionally and personally.

To the extended family of colleagues at the departments of Risk Management, Public Health, Economics, and Environmental Science at Karlstad University that I have had the pleasure of getting to know over the past few years: thank you so much for sharing your wisdoms with me. Special thanks go to **Anders Jonsson**, **Carolina Jernbro**, **Linda Beckman**, **Syed Moniruzzaman**, **Malin Knutz** and **Niklas Jakobsson** for discussing the many various problems of methodology and epidemiology with me. I would also like to thank **Eva Svensson** & **Hilde Ibsen** for injecting my brain with challenging thoughts of the scientific method and the philosophy of science, even when I was a lowly and ungrateful student fresh out of high school who wanted nothing more than to go home and play video games. **The Swedish Civil Contingencies Agency** and **Länsförsäkringar** also have my sincerest gratitude for funding the research projects that enabled us to conduct the studies included in this work.

I should probably also give some long overdue thanks to my mother, **Birgit**, and father, **Roger**, for all your help, advice and support. Especially mom, for the much needed last minute proof-reading of pretty much every text I have ever written. Your ability to notice grammatical errors, or as you put it, “find improvements”, is awesome. Any linguistic errors that remain are guaranteed to be the result of last minute additions unseen by the eyes of the one true spell-checking machine. To my friends that still linger, and to the ones that had the sense to leave, thank you for making Karlstad such a great place to live in. A warm and special thank you also goes out to my lady, my queen and my goddess for giving me something far better to think about than this complicated mess.

References

Akers, R.L. (1990). Rational choice, deterrence, and social learning theory in criminology: The path not taken. *Journal of Criminal Law and Criminology,* 81 (3), 653-1067.

Andersson, R. (2012). *Personsäkerhet på tvären*. Available from: https://arbetsblogg.files.wordpress.com/2012/11/personsc3a4kerhet-pc3a5-tvc3a4ren.pdf [2015-04/07].

Andrews, D.W. (1991). Heteroskedasticity and autocorrelation consistent covariance matrix estimation. *Econometrica: Journal of the Econometric Society,* 59 (3), 817-858.

Angrist, J.D. & Pischke, J. (2008). *Mostly harmless econometrics: An empiricist's companion*. New Jersey: Princeton university press.

Ärnlöv, J. & Larsson, A. (2014). Global, regional, and national age-sex specific all-cause and cause-specific mortality for 240 causes of death, 1990-2013: a systematic analysis for the Global Burden of Disease Study 2013. *The Lancet,* 380 (9859), 2095-2128.

Asbridge, M., Mann, R.E., Flam-Zalcman, R. & Stoduto, G. (2004). The criminalization of impaired driving in Canada: Assessing the deterrent impact of Canada's first per se law. *Journal of studies on alcohol,* 65 (4), 450-459.

Asbridge, M., Mann, R.E., Smart, R.G., Stoduto, G., Beirness, D., Lamble, R. & Vingilis, E. (2009). The effects of Ontario's administrative driver's licence suspension law on total driver fatalities: A multiple time series analysis. *Drugs: Education, Prevention and Policy,* 16 (2), 140-151.

Attewell, R.G., Glase, K. & McFadden, M. (2001). Bicycle helmet efficacy: a meta-analysis. *Accident Analysis & Prevention,* 33 (3), 345-352.

Baker, W., Michael White, C., Cappelleri, J., Kluger, J. & Coleman, C. (2009). Understanding heterogeneity in meta-analysis: the role of meta-regression. *International journal of clinical practice,* 63 (10), 1426-1434.

Ballart, X. & Riba, C. (1995). Impact of legislation requiring moped and motorbike riders to wear helmets. *Evaluation and program planning,* 18 (4), 311-320.

Bangdiwala, S.I., Villaveces, A., Garrettson, M. & Ringwalt, C. (2012). Statistical methods for designing and assessing the effectiveness of community-based interventions with small numbers. *International journal of injury control and safety promotion,* 19 (3), 242-248.

Barnett, A.G., van der Pols, J.C. & Dobson, A.J. (2005). Regression to the mean: what it is and how to deal with it. *International journal of epidemiology,* 34 (1), 215-220.

Bartlett, M.S. (1948). Smoothing periodograms from time series with continuous spectra. *Nature,* 161 (4096), 686-687.

Bass, J.L., Christoffel, K.K., Widome, M., Boyle, W., Scheidt, P., Stanwick, R. & Roberts, K. (1993). Childhood injury prevention counseling in primary care settings: a critical review of the literature. *Pediatrics,* 92 (4), 544-550.

Benjamin, M.A., Rigby, R.A. & Stasinopoulos, D.M. (2003). Generalized autoregressive moving average models. *Journal of the American Statistical association,* 98 (461), 214-223.

Biglan, A., Ary, D. & Wagenaar, A.C. (2000). The value of interrupted time-series experiments for community intervention research. *Prevention Science,* 1 (1), 31-49.

Bjørnskau, T. (2009). *Road traffic exposure and risk among high risk groups in Norway.* TØI Report 1042/2009. Institute of Transport Economics.

Bonita, R., Beaglehole, R. & Kjellström, T. (2006). *Basic epidemiology.* Geneva: The World Health Organization.

Box, G.E. & Jenkins, G.M. (1976). *Time series analysis: forecasting and control, revised ed.* Holden-Day.

Brandt, P.T., Williams, J.T., Fordham, B.O. & Pollins, B. (2000). Dynamic modeling for persistent event-count time series. *American Journal of Political Science,* 44 (4), 823-843.

Breakwell, G. (2007). *The psychology of risk.* Cambridge: Cambridge University Press.

Brown, B. & Baass, K. (1997). Seasonal variation in frequencies and rates of highway accidents as function of severity. *Transportation Research Record: Journal of the Transportation Research Board,* 1581 (1), 59-65.

Cameron, A.C. & Trivedi, P.K. (2013). *Regression analysis of count data*. Cambridge: Cambridge university press.

Castillo-Manzano, J.I., Castro-Nuño, M. & Pedregal, D.J. (2010). An econometric analysis of the effects of the penalty points system driver's license in Spain. *Accident Analysis and Prevention,* 42 (4), 1310-1319.

Cercarelli, L.R., Rosman, D. & Ryan, G. (1996). Comparison of accident and emergency with police road injury data. *Journal of Trauma and Acute Care Surgery,* 40 (5), 805-809.

Chein, J., Albert, D., O'Brien, L., Uckert, K. & Steinberg, L. (2011). Peers increase adolescent risk taking by enhancing activity in the brain's reward circuitry. *Developmental science,* 14 (2), F1-F10.

Clair, T.S., Cook, T.D. & Hallberg, K. (2014). Examining the Internal Validity and Statistical Precision of the Comparative Interrupted Time Series Design by Comparison With a Randomized Experiment. *American Journal of Evaluation,* 35 (3), 311-327.

Clarke, C.F. (2012). Evaluation of New Zealand's bicycle helmet law. *New Zealand Medical Journal,* 125 (1349).

Constant, A., Messiah, A., Felonneau, M.-. & Lagarde, E. (2012). Investigating helmet promotion for cyclists: Results from a randomised study with observation of behaviour, using a semi-automatic video system. *PLoS ONE,* 7 (2).

Cook, T.D., Campbell, D.T. & Day, A. (1979). *Quasi-experimentation: Design & analysis issues for field settings.* Houghton Mifflin Boston.

Cook, A. & Sheikh, A. (2003). Trends in serious head injuries among English cyclists and pedestrians. *Injury Prevention,* 9 (3), 266-267.

Cox, L.A.T. (2013). Improving causal inferences in risk analysis. *Risk Analysis,* 33 (10), 1762-1771.

Curnow, W.J. (2005). The Cochrane Collaboration and bicycle helmets. *Accident Analysis and Prevention,* 37 (3), 569-573.

Dalal, K., Lin, Z., Gifford, M. & Svanström, L. (2013). Economics of global burden of road traffic injuries and their relationship with health system variables. *International journal of preventive medicine,* 4 (12), 1442.

Davis, R.A., Dunsmuir, W.T. & Wang, Y. (2000). On autocorrelation in a Poisson regression model. *Biometrika,* 87 (3), 491-505.

de Hartog, J.J., Boogaard, H., Nijland, H. & Hoek, G. (2010). Do the health benefits of cycling outweigh the risks? *Environmental health perspectives,* 118 (8), 1109-1116.

DeHaven, H. (1944). Mechanics of injury under force conditions. *Mech.Eng,* 66 (4), 264-268.

Dennis, J., Potter, B., Ramsay, T. & Zarychanski, R. (2010). The effects of provincial bicycle helmet legislation on helmet use and bicycle ridership in canada. *Injury Prevention,* 16 (4), 219-224.

Dennis, J., Ramsay, T., Turgeon, A.F. & Zarychanski, R. (2013). Helmet legislation and admissions to hospital for cycling related head injuries in Canadian provinces and territories: Interrupted time series analysis. *BMJ (Online),* 346 (7912).

Dunn, P.K. & Smyth, G.K. (1996). Randomized quantile residuals. *Journal of Computational and Graphical Statistics,* 5 (3), 236-244.

Durbin, J. & Watson, G.S. (1950). Testing for serial correlation in least squares regression: I. *Biometrika,* 37 (3/4), 409-428.

Elvik, R., Vaa, T., Erke, A. & Sorensen, M. (2009). *The handbook of road safety measures.* Emerald Group Publishing.

Engström, K., Diderichsen, F. & Laflamme, L. (2002). Socioeconomic differences in injury risks in childhood and adolescence: a nation-wide study of intentional and unintentional injuries in Sweden. *Injury Prevention,* 8 (2), 137-142.

Escanciano, J.C. & Lobato, I.N. (2009). An automatic Portmanteau test for serial correlation. *Journal of Econometrics,* 151 (2), 140-149.

Farmer, C.M. (2003). Reliability of police-reported information for determining crash and injury severity. *Traffic injury prevention*, 4 (1), 38-44.

Ferrari, S. & Cribari-Neto, F. (2004). Beta regression for modelling rates and proportions. *Journal of Applied Statistics,* 31 (7), 799-815.

Finnoff, J.T., Laskowski, E.R., Altman, K.L. & Diehl, N.N. (2001). Barriers to bicycle helmet use. *Pediatrics,* 108 (1), E4.

Forward, S., Henriksson, P., Nyberg, J. & Forsberg, I. (2012). *Evaluation of the effects of new rules for Class I moped license: a before and after study*. VTI Report 762. The Swedish National Road and Transport Research Institute, Linköping.

Fretheim, A., Soumerai, S.B., Zhang, F., Oxman, A.D. & Ross-Degnan, D. (2013). Interrupted time-series analysis yielded an effect estimate concordant with the cluster-randomized controlled trial result. *Journal of clinical epidemiology,* 66 (8), 883-887.

Fretheim, A., Zhang, F., Ross-Degnan, D., Oxman, A.D., Cheyne, H., Foy, R., Goodacre, S., Herrin, J., Kerse, N., McKinlay, R.J., Wright, A. & Soumerai, S.B. (2015). A reanalysis of cluster randomized trials showed interrupted time-series studies were valuable in health system evaluation. *Journal of clinical epidemiology,* 68 (3), 324-333.

Fugelsang, J.A., Stein, C.B., Green, A.E. & Dunbar, K.N. (2004). Theory and data interactions of the scientific mind: evidence from the molecular and the cognitive laboratory. *Canadian Journal of Experimental Psychology/Revue canadienne de psychologie expérimentale,* 58 (2), 86.

Gallup (2011). *Future of Transport*. Flash EB #312. European Commisson.

Gardner, M. & Steinberg, L. (2005). Peer influence on risk taking, risk preference, and risky decision making in adolescence and adulthood: an experimental study. *Developmental psychology,* 41 (4), 625.

Gielen, A.C. & Sleet, D. (2003). Application of behavior-change theories and methods to injury prevention. *Epidemiologic reviews,* 25 (1), 65-76.

Gill, M. & Goldacre, M.J. (2009). Seasonal variation in hospital admission for road traffic injuries in England: analysis of hospital statistics. *Injury prevention : journal of the International Society for Child and Adolescent Injury Prevention,* 15 (6), 374-378.

Glass, G.V. (1997). Interrupted Time Series Quasi-Experiments. In Jaeger, R.M. (ed.) *Complementary methods for research in education.* (2nd edn.). Washington D.C.: American Educational Research Association. 589.

Grandelius, K. (2014). *Enklare effektsamband för transportpolitisk måluppfyllelseanalys.* TRV 2014/6277. The Swedish Transport Administration.

Grembi, V., Nannicini, T. & Troiano, U. (2014). Policy responses to fiscal restraints: A difference-in-discontinuities design (Harvard Economics Department Working Paper).

Grimshaw, J., Campbell, M., Eccles, M. & Steen, N. (2000). Experimental and quasi-experimental designs for evaluating guideline implementation strategies. *Family practice,* 17 Suppl 1, S11-16.

Gustafsson, S. (2013). *Bicycle helmet use from a regional perspective: Focus group interviews and analysis of accident and injury data.* VTI Report 23-2013. The Swedish National Road and Transport Research Institute, Linköping.

Haddon Jr, W. (1980). Advances in the epidemiology of injuries as a basis for public policy. *Public health reports,* 95 (5), 411.

Hall, D.B. (2000). Zero-inflated Poisson and binomial regression with random effects: a case study. *Biometrics,* 56 (4), 1030-1039.

Hartmann, D.P., Gottman, J.M., Jones, R.R., Gardner, W., Kazdin, A.E. & Vaught, R.S. (1980). Interrupted time-series analysis and its application to behavioral data. *Journal of applied behavior analysis,* 13 (4), 543-559.

Hasselberg, M., Laflamme, L. & Ringbäck Weitoft, G. (2001). Socioeconomic differences in road traffic injuries during childhood and youth: A closer look at different kinds of road user. *Journal of epidemiology and community health,* 55 (12), 858-862.

Hibell, B., Guttormsson, U., Ahlström, S., Balakireva, O., Bjarnason, T., Kokkevi, A. & Kraus, L. (2012). *The 2011 ESPAD report: Substance Use Among Students in 36 European Countries.* Stockholm, Sweden: The Swedish Council for Information on Alcohol and Other Drugs (CAN).

Holder, Y. (2001). *Injury surveillance guidelines.* Geneva: The World Health Organisation.

Holtslag, H.R., van Beeck, E.F., Lindeman, E. & Leenen, L.P. (2007). Determinants of long-term functional consequences after major trauma. *The Journal of trauma,* 62 (4), 919-927.

Horan, J.M. & Mallonee, S. (2003). Injury surveillance. *Epidemiologic reviews,* 25 (1), 24-42.

Ian, R. & Irene, K. (2001). School based driver education for the prevention of traffic crashes. *Cochrane database of systematic reviews (Online),* (3).

Janssen, F. & Kunst, A.E. (2004). ICD coding changes and discontinuities in trends in cause-specific mortality in six European countries, 1950-99. *Bulletin of the World Health Organization,* 82 (12), 904-913.

Karkhaneh, M., Rowe, B.H., Duncan Saunders, L., Voaklander, D. & Hagel, B. (2011). Bicycle helmet use after the introduction of all ages helmet legislation in an Urban Community in Alberta, Canada. *Canadian Journal of Public Health,* 102 (2), 134-138.

Karkhaneh, M., Rowe, B.H., Saunders, L.D., Voaklander, D.C. & Hagel, B.E. (2013). Trends in head injuries associated with mandatory bicycle helmet legislation targeting children and adolescents. *Accident Analysis and Prevention,* 59, 206-212.

Kim, H.J. (2010). *vrtest: Variance Ratio tests and other tests for Martigale Difference Hypothesis.* (R package version 0.95 edn.).

Koornstra, M.J. (1988). *Development of Road Safety in Some European Countries and the USA.* SWOV Institute for Road Safety.

Kwan, I. & Mapstone, J. (2009). Interventions for increasing pedestrian and cyclist visibility for the prevention of death and injuries. *Cochrane Database of Systematic Reviews,* (4).

Lambert, D. (1992). Zero-inflated Poisson regression, with an application to defects in manufacturing. *Technometrics,* 34 (1), 1-14.

Langlois, J.A., Rutland-Brown, W. & Wald, M.M. (2006). The epidemiology and impact of traumatic brain injury: A brief overview. *Journal of Head Trauma Rehabilitation,* 21 (5), 375-378.

Larsson, J. (2014). *Cykelhjälmsanvändning i Sverige 1998–2013: resultat från VTI: s senaste observationsstudie. [In English: Bicycle helmet use in Sweden 1998-2013: results from VTI:s latest observational study].* VTI Note 8-2014. The Swedish National Road and Transport Research Institute, Linköping.

Littell, J.H. (2008). Evidence-based or biased? The quality of published reviews of evidence-based practices. *Children and Youth Services Review,* 30 (11), 1299-1317.

Ljung, G.M. & Box, G.E.P. (1978). On a measure of lack of fit in time series models. *Biometrika,* 65 (2), 297-303.

Ludvigsson, J.F., Andersson, E., Ekbom, A., Feychting, M., Kim, J-L., Reuterwall, C., Heurgren, M. & Otterblad Olausson, P. (2011). External review and validation of the Swedish national inpatient register. *BMC Public Health,* 11 (1), 450.

Lund, J. & Aarø, L.E. (2004). Accident prevention. Presentation of a model placing emphasis on human, structural and cultural factors. *Safety Science,* 42 (4), 271-324.

Macdonald, S., Zhao, J., Martin, G., Brubacher, J., Stockwell, T., Arason, N., Steinmetz, S. & Chan, H. (2013). The impact on alcohol-related collisions of the partial decriminalization of impaired driving in British Columbia, Canada. *Accident Analysis and Prevention,* 59, 200-205.

Macpherson, A. & Spinks, A. (2008). Bicycle helmet legislation for the uptake of helmet use and prevention of head injuries. *Cochrane Database of Systematic Reviews,* (3).

Malm, S., Krafft, M., Kullgren, A., Ydenius, A. & Tingvall, C. (2008). Risk of permanent medical impairment (RPMI) in road traffic accidents. In *Annals of Advances in Automotive Medicine - 52nd Annual Scientific Conference.* Vol. 52, p. 93. Association for the Advancement of Automotive Medicine.

Mann, R.E., Smart, R.G., Stoduto, G., Beirness, D., Lamble, R. & Vingilis, E. (2002). The early effects of Ontario's Administrative Driver's Licence Suspension Law on driver fatalities with a BAC > 80 mg%. *Canadian Journal of Public Health,* 93 (3), 176-180.

Mason, M.J., Mennis, J., Linker, J., Bares, C. & Zaharakis, N. (2014). Peer attitudes effects on adolescent substance use: The moderating role of race and gender. *Prevention science,* 15 (1), 56-64.

Mckenzie, E. (2003). Discrete variate time series. In Raoand, C. & Shanbhag, D. (eds.) *Handbook of Statistics*. Amsterdam: Elsevier Science, 573–606.

Moniruzzaman, S. & Andersson, R. (2008). Economic development as a determinant of injury mortality–a longitudinal approach. *Social science & medicine,* 66 (8), 1699-1708.

Morgan, S.L. & Winship, C. (2014). *Counterfactuals and causal inference*. Cambridge: Cambridge University Press.

Moskal, A., Martin, J.-. & Laumon, B. (2012). Risk factors for injury accidents among moped and motorcycle riders. *Accident Analysis and Prevention,* 49, 5-11.

Murray, C.J., Vos, T., Lozano, R., Naghavi, M., Flaxman, A.D., Michaud, C., Ezzati, M., Shibuya, K., Salomon, J.A. & Abdalla, S. (2013). Disability-adjusted life years (DALYs) for 291 diseases and injuries in 21 regions, 1990–2010: a systematic analysis for the Global Burden of Disease Study 2010. *The Lancet,* 380 (9859), 2197-2223.

Nilsen, P. (2006). *Opening the black box of community-based injury prevention programmes: towards improved understanding of factors that influence programme effectiveness*. PhD Thesis, Linköping University.

Nilson, F. (2014). *Fall-Related Injuries Amongst Elderly in Sweden: Still an Emerging Risk?* PhD Thesis, Karlstad University.

Nilson, F., Bonander, C. & Andersson, R. (2014). The effect of the transition from the ninth to the tenth revision of the International Classification of Diseases on external cause registration of injury morbidity in Sweden. *Injury prevention,* Epub ahead of print. Doi: 10.1136/injuryprev-2014-041337.

Nilsson, G. (2004). *Traffic safety dimensions and the power model to describe the effect of speed on safety*. PhD Thesis, Lund University.

Niska, A., Gustafsson, S., Nyberg, J. & Eriksson, J. (2013). *Single bicycle accidents. Analysis of hospital injury data and interviews.* VTI Report 779. The Swedish National Road and Transport Research Institute, Linköping.

Nolén, S., Ekman, R. & Lindqvist, K. (2005). Bicycle helmet use in Sweden during the 1990s and in the future. *Health promotion international,* 20 (1), 33-40.

O'Hara, R.B. & Kotze, D.J. (2010). Do not log-transform count data. *Methods in Ecology and Evolution,* 1 (2), 118-122.

Olivier, J., Wang, J.J., Walter, S. & Grzebieta, R. (2014). Anti-helmet arguments: lies, damned lies and flawed statistics. *the Australasian College of Road Safety,* 25 (4), 10.

Oppe, S. (1989). Macroscopic models for traffic and traffic safety. *Accident Analysis & Prevention,* 21 (3), 225-232.

Oppe, S. (1991). The development of traffic and traffic safety in six developed countries. *Accident Analysis & Prevention,* 23 (5), 401-412.

Ospina, R. & Ferrari, S.L. (2012). A general class of zero-or-one inflated beta regression models. *Computational Statistics & Data Analysis,* 56 (6), 1609-1623.

Owen, R., Kendrick, D., Mulvaney, C., Coleman, T. & Royal, S. (2011). Non-legislative interventions for the promotion of cycle helmet wearing by children. *Cochrane database of systematic reviews (Online),* (11).

Oxman, A. (2011). *What study designs should be included in an EPOC review and what should they be called?* The Cochrane Effective Practice and Organisation of Care Group. Available from: http://epoc.cochrane.org/sites/epoc.cochrane.org/files/uploads/EPOC%20Study%20Designs%20About.pdf [2015-04/07]

Pérez, K., Marí-Dell'Olmo, M., Borrell, C., Nebot, M., Villalbí, J.R., Santamariña, E. & Tobias, A. (2009). Road injuries and relaxed licensing requirements for driving light motorcycles in Spain: A time-series analysis. *Bulletin of the World Health Organization,* 87 (7), 497-504.

Pridemore, W.A., Chamlin, M.B., Kaylen, M.T. & Andreev, E. (2013). The impact of a national alcohol policy on deaths due to transport accidents in Russia. *Addiction*, 108 (12), 2112-2118.

Pucher, J., Buehler, R. & Seinen, M. (2011). Bicycling renaissance in North America? An update and re-appraisal of cycling trends and policies. Transport Research Part A: Policy and Practice, 45 (6), 451-475.

R Core Team (2014). *R: A language and environment for statistical computing*. Vienna, Austria: R Foundation for Statistical Computing.

Ramsay, C.R., Matowe, L., Grilli, R., Grimshaw, J.M. & Thomas, R.E. (2003). Interrupted time series designs in health technology assessment: lessons from two systematic reviews of behavior change strategies. *International Journal of Technology Assessment in Health Care,* 19 (04), 613-623.

Reason, J. (2000). Human error: models and management. *BMJ (Clinical research ed.),* 320 (7237), 768-770.

Richmond, S.A., Zhang, Y.J., Stover, A., Howard, A. & Macarthur, C. (2014). Prevention of bicycle-related injuries in children and youth: a systematic review of bicycle skills training interventions. *Injury Prevention,* 20 (3), 191-195.

Rigby, R.A. & Stasinopoulos, D.M. (2013). Automatic smoothing parameter selection in GAMLSS with an application to centile estimation. *Statistical methods in medical research,* 23(4), 318-332.

Rivara, F.P., Cummings, P., Koepsell, T.D., Grossman, D.C. & Maier, R.V. (2009). *Injury control: a guide to research and program evaluation*. Cambridge: Cambridge University Press.

Rizzi, M., Stigson, H. & Krafft, M. (2013). Cyclist injuries leading to permanent medical impairment in sweden and the effect of bicycle helmets. In *nternational Research Council on the Biomechanics of Injury Conference, IRCOBI 2013*. 412.

Robertson, L. (2007). *Injury Epidemiology: Research and Control Strategies: Research and Control Strategies.* (3rd edn.). New York: Oxford University Press.

Robinson, D.L. (2006). No clear evidence from countries that have enforced the wearing of helmets. *British medical journal,* 332 (7543), 722-725.

Robinson, D.L. (2007). Bicycle helmet legislation: Can we reach a consensus? *Accident Analysis and Prevention,* 39 (1), 86-93.

Rosman, D.L. & Knuiman, M.W. (1994). A comparison of hospital and police road injury data. *Accident Analysis & Prevention,* 26 (2), 215-222.

Rothwell, P.M. (2005). External validity of randomised controlled trials:"to whom do the results of this trial apply?". *The Lancet,* 365 (9453), 82-93.

Roberts, A. & Yeager, K. (2006). *Foundations of Evidence-Based Social Work Practice.* Oxford: Oxford University Press.

Schmidt, C.O. & Kohlmann, T. (2008). When to use the odds ratio or the relative risk? *International journal of public health,* 53 (3), 165-167.

Schwartz, J., Spix, C., Touloumi, G., Bacharova, L., Barumamdzadeh, T., le Tertre, A., Piekarksi, T., Ponce de Leon, A., Ponka, A., Rossi, G., Saez, M. & Schouten, J.P. (1996). Methodological issues in studies of air pollution and daily counts of deaths or hospital admissions. *Journal of epidemiology and community health,* 50 Suppl 1 , S3-11.

Scuffham, P., Alsop, J., Cryer, C. & Langley, J.D. (2000). Head injuries to bicyclists and the New Zealand bicycle helmet law. *Accident Analysis and Prevention,* 32 (4), 565-573.

Somers, M., Zhu, P., Jacob, R. & Bloom, H. (2013). The validity and precision of the comparative interrupted time series design and the difference-in-difference design in educational evaluation (MDRC working paper in research methodology). *MDRC: New York, NY.*

Sorensen, H.T., Sabroe, S. & Olsen, J. (1996). A framework for evaluation of secondary data sources for epidemiological research. *International journal of epidemiology,* 25 (2), 435-442.

Stapp, J.P. (1957). Human tolerance to deceleration. *The American Journal of Surgery,* 93 (4), 734-740.

Stasinopoulos, D.M. & Rigby, R.A. (2007). Generalized additive models for location scale and shape (GAMLSS) in R. *Journal of Statistical Software,* 23 (7), 1-46.

Stave, C. (2012). *Process evaluation of driving license for EU-moped.* VTI Report 756. The Swedish National Road and Transport Research Institute, Linköping.

Steg, L. & van Brussel, A. (2009). Accidents, aberrant behaviours, and speeding of young moped riders. *Transportation research part F: traffic psychology and behaviour,* 12 (6), 503-511.

Stolwijk, A.M., Straatman, H. & Zielhuis, G.A. (1999). Studying seasonality by using sine and cosine functions in regression analysis. *Journal of epidemiology and community health,* 53 (4), 235-238.

Strandroth, J. (2007). *Mopedolyckor efter EU-mopedens införande – Analys av mopedolyckor 2000-2006.* Swedish Road Administration. Available from: http://www.trafikverket.se/contentassets/562c141b30ca4ee4b7bdfb5e276435a9/mopedolyckor_efter_eu_mopedens_inforande.pdf [2015-04/07].

Sullivan, K.J., Shadish, W.R. & Steiner, P.M. (2015). An Introduction to Modeling Longitudinal Data With Generalized Additive Models: Applications to Single-Case Designs. *Psychological methods,* 20(1), 26-42.

Swedish Institute for Transport and Communications Analysis (2007). *RES 2005-2006 - The national travel survey.* SIKA Statistics 2007:19. SIKA, Stockholm.

Swedish Transport Agency. (2009). *Thinking of Driving a Moped?* [Online] Available from: http://www.korkortsportalen.se/upload/bibliotek/broschyrer/moped_andra_sprak/Thinking_of_driving_a_moped-New_rules_from_1_October_2009.pdf [2013-11/19].

Swedish Transport Agency. (2011). *STRADA - Swedish Traffic Accident Data Acquisition.* [Online] Available from: http://www.transportstyrelsen.se/en/road/STRADA/ [2013-11/19].

Swedish Transport Agency (2013). *[Road traffic accidents, Guidelines for reporting].* [Online] Available from: http://www.transportstyrelsen.se/globalassets/global/publikationer/vag/strada/pv09451_handledning_vagtrafikolyckor-2013-04-04.pdf [2015-04/07].

Tarnow-Mordi, W. O., & Healy, M. J. (1999). Distinguishing between "no evidence of effect" and "evidence of no effect" in randomised controlled trials and other comparisons. *Archives of disease in childhood,* 80 (3), 210-211.

The National Board of Health and Welfare. (2014a). *National Cause of Death register*. [Online] Available from: http://www.socialstyrelsen.se/statistics/statisticaldatabase/causeofdeath [2015-04/04].

The National Board of Health and Welfare. (2014b). *National Patient Register*. [Online] Available from: http://www.socialstyrelsen.se/statistics/statisticaldatabase/inpatientcarediagnoses [2015-04/04].

Thompson, D., Rivara, F. & Thompson, R. (1999). Helmets for preventing head and facial injuries in bicyclists. *Cochrane Database of Systematic Reviews,* 4.

Tingvall, C., Ifver, J., Krafft, M., Kullgren, A., Lie, A., Rizzi, M., Sternlund, S., Stigson, H. & Strandroth, J. (2013). The Consequences of Adopting a MAIS 3 Injury Target for Road Safety in the EU, a Comparison with Targets Based on Fatalities and Long-term Consequences. In *IRCOBI Conf. on the Biomechanics of Injury, Gothenburg, Sweden.*

Tobías, A. & Sáez Zafra, M. (2004). Time-series regression models to study the short-term effects of environmental factors on health (No. 11). Department of Economics, University of Girona.

Tryon, W.W. (1982). A simplified time-series analysis for evaluating treatment interventions. *Journal of applied behavior analysis,* 15 (3), 423-429.

Ulleberg, P. (2001). Personality subtypes of young drivers. Relationship to risk-taking preferences, accident involvement, and response to a traffic safety campaign. *Transportation Research Part F: Traffic Psychology and Behaviour,* 4 (4), 279-297.

Ver Hoef, J.M. & Boveng, P.L. (2007). Quasi-Poisson vs. negative binomial regression: how should we model overdispersed count data? *Ecology,* 88 (11), 2766-2772.

Villamor, E., Hammer, S. & Martinez-olaizola, A. (2008). Barriers to bicycle helmet use among Dutch paediatricians. *Child: Care, Health and Development,* 34 (6), 743-747.

Vingilis, E., McLeod, A.I., Seeley, J., Mann, R.E., Beirness, D. & Compton, C.P. (2005). Road safety impact of extended drinking hours in Ontario. *Accident Analysis and Prevention,* 37 (3), 549-556.

Vlahogianni, E.I., Yannis, G. & Golias, J.C. (2012). Overview of critical risk factors in Power-Two-Wheeler safety. *Accident Analysis & Prevention,* 49 , 12-22.

Wagenaar, A.C. & Margolis, L.H. (1990). Effects of a mandatory safety belt law on hospital admissions. *Accident Analysis and Prevention,* 22 (3), 253-261.

Wagenaar, A.C., Maybee, R.G. & Sullivan, K.P. (1988). Mandatory seat belt laws in eight states: a time-series evaluation. *Journal of Safety Research,* 19 (2), 51-70.

Wagner, A.K., Soumerai, S.B., Zhang, F. & Ross-Degnan, D. (2002). Segmented regression analysis of interrupted time series studies in medication use research. *Journal of clinical pharmacy and therapeutics,* 27 (4), 299-309.

Walter, S.R., Olivier, J., Churches, T. & Grzebieta, R. (2011). The impact of compulsory cycle helmet legislation on cyclist head injuries in New South Wales, Australia. *Accident Analysis and Prevention,* 43 (6), 2064-2071.

Wegman, F., Zhang, F. & Dijkstra, A. (2012). How to make more cycling good for road safety? *Accident Analysis and Prevention,* 44 (1), 19-29.

Wei, W.W. (1994). *Time series analysis: Univariate and Multivariate Methods.* Boston: Addison-Wesley publ.

Weiß, C.H. (2008). The combined INAR (p) models for time series of counts. *Statistics & Probability Letters,* 78 (13), 1817-1822.

Zhang, F., Wagner, A.K., Soumerai, S.B. & Ross-Degnan, D. (2009). Methods for estimating confidence intervals in interrupted time series analyses of health interventions. *Journal of clinical epidemiology*, 62 (2), 143-148.

Searching for causal effects of road traffic safety interventions

Traffic-related injuries represent a global public health problem, and contribute largely to mortality and years lived with disability. Over the course of the last decades, improvements to road traffic safety and injury surveillance systems have resulted in a shift in focus from motor vehicle accidents to injury events involving vulnerable road users (VRUs), such as cyclists and moped riders. There have been calls for improvements to the evaluation of safety interventions due to methodological problems associated with the most commonly used study designs. The purpose of this licentiate thesis was to assess the strengths and limitations of the interrupted time series (ITS) design, which has gained some attention for its ability to provide valid effect estimates while accounting for secular trends. Two national interventions involving VRUs were selected as cases: the Swedish bicycle helmet law for children under the age 15, and the tightening of licensing rules for Class 1 mopeds. The empirical results suggest that both interventions were effective. These results are discussed in the light of some methodological considerations regarding internal and external validity, data quality and the ability to fully understand key causal mechanisms behind complex interventions.

ISBN 978-91-7063-638-7

ISSN 1403-8099

LICENTIATE THESIS | Karlstad University Studies | 2015:22