An Analysis of Road Accidents in Great Britain

Submitted by

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Declaration of Authorship

I

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hereby declare that this thesis and the work presented in it is entirely my own. Where I have consulted the work of others, this is always clearly stated.

Signed: ______________________

Date: ________________________

Abstract

This thesis provides an analysis of road accidents in Great Britain in three parts. The first determines whether vehicle accident and casualty rates decrease during and after a recession. Using the total unemployment rate as a proxy for macroeconomic conditions, a fixed effects regression design on local authorities within Great Britain from 2004-2010 is analysed. The findings suggest that the rate of accidents that occur during non-working hours and over the weekend, as well as young male casualties are the most sensitive to relative changes in the unemployment rate even after controlling for traffic volume. The second investigates the impact of the Santander Cycle Hire Scheme on accidents and casualties. A difference in difference regression design on local authorities within Greater London from 2000-2017 is used. The results suggest the scheme benefits cyclists by decreasing the pedal cycle accident rate per million miles but does not benefit motorists and pedestrians, increasing the car and pedestrian accident rates respectively. This effect is via slight accident and casualty rates and remains robust to a spill-over effect. The third investigates whether terror incidents affect road accidents and casualties. A fixed effects regression design on police force areas within Great Britain from 2000-2017 is conducted where lagged variables are used to control for any spill-over effects. The impact of an incident is separated into the effect of an incident occurring and a measure of the intensity. After controlling for traffic volume, the results suggest that the effect is due to a change in the quantity and quality of driving, the latter effect being quite high. The effect of more than one incident occurring is positive and large most of which is due to a change in serious and slight accidents and casualties rather than fatal.

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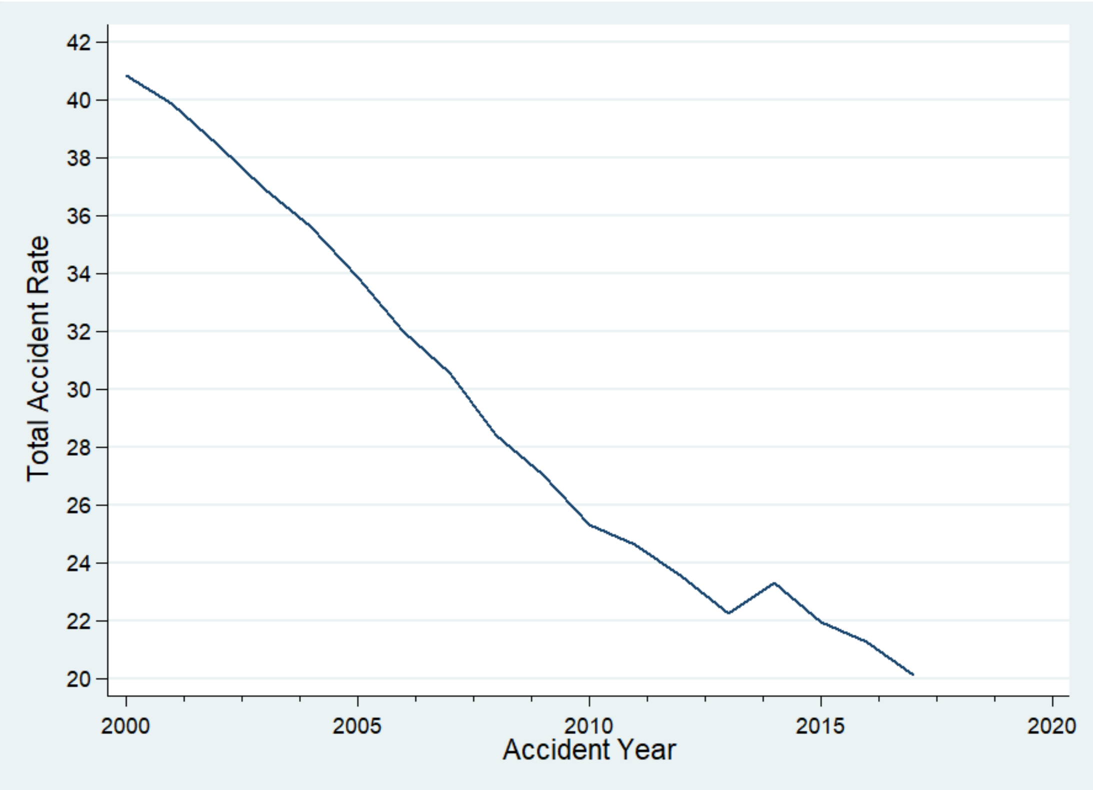
Chapter 1

Introduction

Road accidents contribute to a substantial share of fatalities and injuries within Great Britain. Figure 1.1 plots the total accident rate per 10 000 people for Great Britain from 2000 till 2017 and demonstrates that the number of accidents per 10 000 people is quite high in 2000. The total accident rate decreases steadily with a notable jump in 2014 however, this is short lived, and the rate continues to decrease till the figure is halved by 2017. Even so, the average cost of fatal road accidents in 2018 for Great Britain was approximately £2 000 000 (Statista Research Department, 2020). Therefore, research within this area is important in order to analyse the effects various events and policies have on road accidents.

Figure 1. 1 - Total Accident Rate per 10 000 people within Great Britain 2000 to 2017



Broughton, Lloyd and Wallbank (2015) examine plausible reasons for the steep reduction in fatal accidents between 2007 and 2010 within Great Britain performing a summary analysis using multiple data sources including the Department for Transport road accident data employed within this thesis. They find changes in the quantity of driving with a reduction in overall traffic, HGV traffic and young male drivers and an increase in pedal cycle traffic. Additionally, they find changes in the quality of driving with a reduction in speeding and drink driving during this period concluding that the fall in fatal accidents during the recession was through a change in behaviour.

Further research has been conducted on speed and drink driving. Rock (1995) attempts to determine the effects of an increase in the speed limit from 55mph to 65mph on rural highways in Illinois, USA. Using an ARIMA model on monthly time series data including five years before the change and four years after, the analysis concludes that the increase in the speed limit led to an increase of 300 accidents per month in rural Illinois coupled with an increase in fatalities and injuries. A study conducted by Rhum (1996) on 46 of the U.S. states from 1982 to 1988 examines various alcohol related policies, including a beer tax, on vehicle fatality rates. He finds that while most of the policies have little impact the beer tax is negatively related to fatal vehicle accidents. This is investigated further by Bielinska-Kwapisz and Young (2006) who conduct an analysis using panel data consisting of U.S. states from 1982 to 2000 and alcohol taxes as instrumental variables to determine whether a relationship exists between alcohol prices, consumption and road accident fatalities. They specify that alcohol taxes are not wholly suitable instruments however their findings suggest that a negative relationship exists between alcohol prices and fatalities while a positive relationship exists between alcohol consumption and fatalities.

Several studies have been conducted on the effect of sleep deprivation on road accidents. A detailed analysis on accidents using police record data in Southwest England (from 1987 to 1992) and surveys in the Midlands (in August 1991 and 1992, and April 1994) as well as interviews, conducted by Horne and Reyner (1995) find that sleep related vehicle accidents account for a large portion of all major road and motorway vehicle accidents (16% in Southwest England and 20% in the Midlands) and are chiefly dependent on the time of day with peaks at around 2am, 6am and 4pm. A study conducted on accidents taking place from 2002-2011 in the United States excluding Arizona and Indiana on the 2007 Daylight Savings Time (DST) Extension by Smith (2016), concludes that the transition into DST increases fatal accidents, the result due to sleep deprivation rather than changes in ambient light.

Broughton, Lloyd and Wallbank (2015), also examine whether developments in vehicle safety contributed to the decrease in road accidents. They conclude that, during the recession, the downward trend in accidents is not directly related to improvements in vehicle safety and determine this may be due to driver confidence stating that individuals in safer cars may begin to drive more recklessly. A vast amount of research has been conducted on the effect of larger vehicles such as sports utility vehicles and light trucks on accidents (Gayer, 2004; White, 2004; Li, 2012; Anderson and Auffhammer, 2014). Anderson (2008) examines the effect of larger vehicles in the United States from 1982 to 2004 on road accidents and finds that a one percentage point increase in the share of light trucks (including SUVs) increases annual fatalities due to road accidents by 143 per year, four-fifths of which are occupants of other vehicles and pedestrians.

Finally, regarding the effect of driver experience, Borowsky, Oron-Gilad and Shinar (2010) perform a study in Israel asking 21 young inexperienced, 19 experienced and 16 elderly drivers connected to an eye tracking system and shown six hazard perception movies, to identify hazardous situations. Their findings support previous literature on this topic (Chapman and Underwood, 1998; Underwood et al., 2005) stating that there is an improvement in recognition of possible hazards with driving experience.

This thesis attempts to contribute to the literature using accident, casualty and vehicle data from the Department for Transport, 'Road Accident Statistics Branch'.[1] It is a repeated cross-sectional register of road accidents compiled annually. The data

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[1] Formerly the Department of the Environment, Transport and the Regions (DETR).

is available from the UK Data Archive from 1985 till 2014. The road accident data covering the period from 2015 to 2017 is sourced separately for England, Scotland and Wales from the Department of Transport, Transport Scotland and Stats Wales respectively. Due to a lack of geographic data the local authority of London Airport, which falls within the Metropolitan police force area, is dropped from the sample.

Every road accident in which one or more vehicles (or vehicle and pedestrian) are involved and which include human injury or death taking place on the public highway (including footways) and notified to the police within 30 days of occurrence is reported by police officers in Great Britain using a 'STATS19' report form.[2] Accidents taking place on private roads or car parks and any accidents involving "confirmed suicides only" are not reported (Department for Transport, 2011).

Any person killed or injured in a road accident is reported, by police officers, as a casualty. As per the STATS19 form, a 'fatal casualty' is one whose injury causes death in less than 30 days as a result of the accident and does not include death from natural causes or suicide. Casualties who sustain serious injuries requiring medical treatment such as fracture, internal injury or severe cuts etc. and/or die 30 or more days after the accident from injuries sustained in that accident are reported as 'serious casualties' and 'slight casualties' are those who do not necessarily require medical treatment but sustain injuries such as sprains, whiplash, bruises or cuts. This includes shock requiring roadside attention however, those who are shaken but do not require roadside assistance are not included unless they require medical attention. Pedal cycles include pedal cycles, tandems and tricycles ridden in the carriageway, cycleway or pavement, toy cycles ridden in the carriageway and those that travel at a maximum speed of 15 mph with battery assistance. Cars include estate cars, three wheeled cars, four-wheel drive vehicles and family vans.

[2] The form defines a highway ("road" in Scotland) as a road with "unrestricted right of access for all or some classes of motor vehicles" and vehicles are defined according to "structural type" (Department for Transport, 2011).

It is illegal to drive without at least third party insurance in the UK which covers an accident causing damage or injury to any other person, vehicle, animal or property but does not cover one's own damage. The penalties for not having insurance are high including, a £100 fine, having your vehicle clamped, impounded or destroyed, court prosecution with a possible maximum fine of £1 000 and an uninsured driver could be disqualified from driving (UK Government, 2020).

Given the definitions of accidents and casualties specified above, the data does not include small collisions causing scratches or light dents to a vehicle. Furthermore, in order to sustain a cut or even whiplash, the collision would require a force great enough to cause significant damage to the vehicle. Therefore, the insured driver would be reporting the accident. Similarly, if an uninsured driver collided with a pedestrian or cyclist the latter would report the accident and if the collision occurred between the uninsured driver and property, again, even whiplash would require significant damage to the property, be it a wall etc., that the damage would be reported by the owner within 30 days. Therefore, given the motor insurance laws, possible penalties involved and accident and casualty definitions, it is assumed that all accidents are reported within this dataset.

Fatal, serious and slight accidents are defined by whether the casualties involved suffered fatal, serious or slight injuries. The most serious severity is chosen to define the accident in those instances involving more than one casualty with differing injury severities.

Figures 1.2, 1.3 and 1.4 depict the distribution of fatal, serious and slight accident rates per 10 000 population by country (England, Wales and Scotland) from 2000 to 2017. All figures depict an overall reduction in the accident rate for all severities however, England experienced a steady reduction over time compared to Wales and Scotland. Comparing the three figures, there are fewer fatal accidents per 10 000 people than serious, with considerably more slight accidents compared to both fatal and serious. The steepest decline in both the fatal and serious accident rates seems to be experienced by Scotland and the slight accident rate by England with the slight accident rate decreasing steadily over time for all countries.

Figure 1. 2 - Fatal Accident Rate per 10 000 people per country 2000 to 2017

Figure 1. 3 - Serious Accident Rate per 10 000 people per country 2000 to 2017

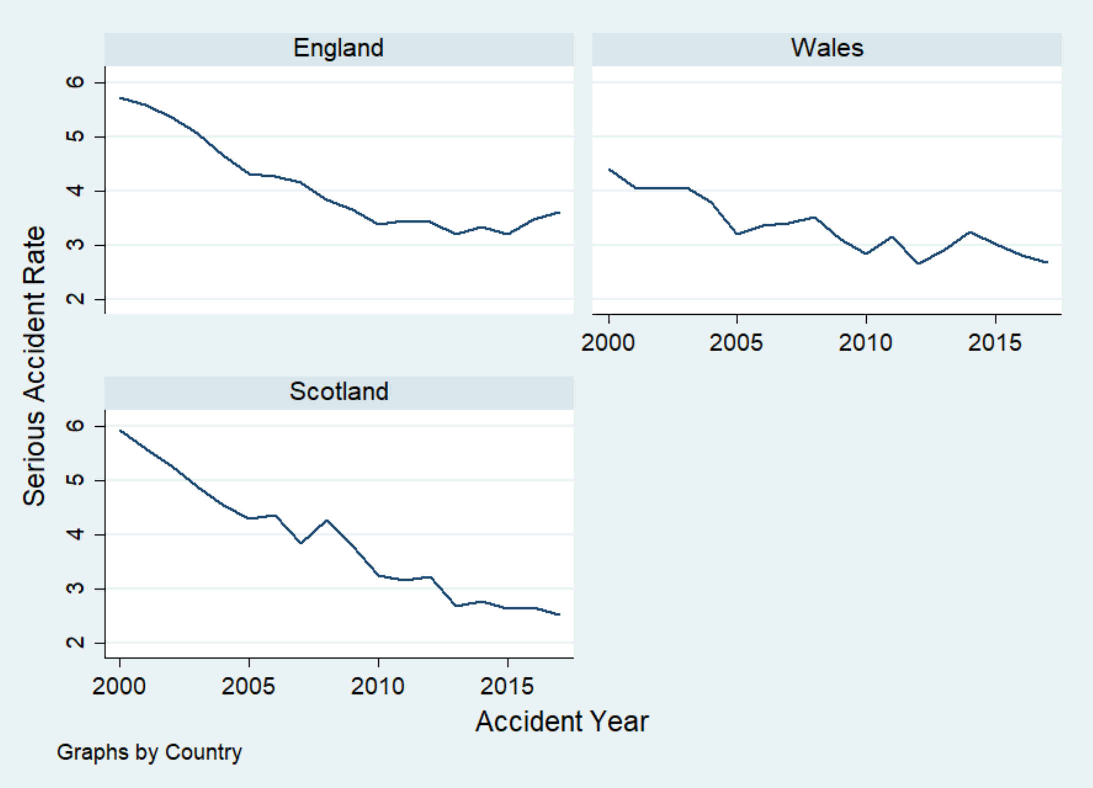
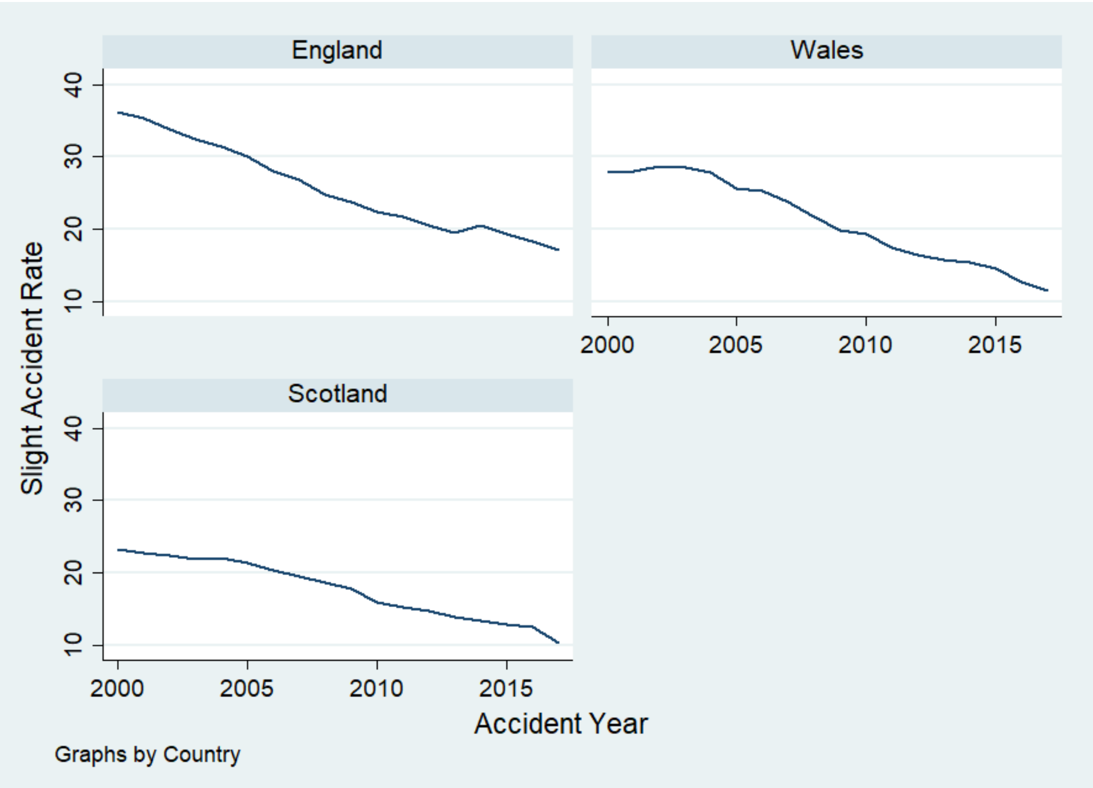


Figure 1. 4 - Slight Accident Rate per 10 000 people by country 2000 to 2017



The traffic volume data, sourced from the Department for Transport, is presented in units (thousand vehicle miles) and figures give the “total volume of traffic on the stretch of road” which is specified within the data, for each year and area. They are “calculated by multiplying the ‘Annual Average Daily Flow’ by the corresponding length of road and by the number of days in the year” (Department for Transport, 2017).

This thesis provides an analysis of road accidents in Great Britain in three chapters.

The first investigates whether vehicle accident and casualty rates decrease during and after a recession and hypothesises the decrease to be through the quantity and quality of driving. Considering the quantity of driving, traffic volume will either decrease due to a reduction in the work commute or individuals may wish to save money they would normally spend on fuel (Wagenaar, 1983; Leigh and Waldon, 1991; Cotti and Tefft, 2011). Alternatively, considering the quality of driving, an unemployed individual may have a lower opportunity cost of time subsequently improving driving actions. Furthermore, a reduction in average individual income may lead to improved quality of driving through a reduction in alcohol consumption (Leigh and Waldon, 1991; Traynor, 2008; Cotti and Tefft, 2011).

Using the total unemployment rate as a proxy for macroeconomic conditions, the results of a fixed effects regression design on local authorities within Great Britain from 2004-2010 suggest that there is a negative relationship between the unemployment rate and vehicle accident and casualty rates. The effect is analysed further by decomposing the vehicle and accident casualty rates into fatal, serious and slight accidents and casualties as well as accidents during various times of the day and week and dividing casualties by age group and gender to determine which of these dependent variables is most sensitive to relative changes in unemployment.

The findings suggest that the rate of accidents that occur during non-working hours and over the weekend, as well as young male casualties are the most sensitive to relative changes in the unemployment rate even after controlling for traffic volume.

A subsample using larger geographic areas to allow for changes in commuting patterns produces similar results and the local authority accident and casualty rates are positively related to the total employment rate validating the results.

Furthermore, an analysis on ‘peak hour’ accident rates indicates that the accident rate during the Winter morning peak hours are the most sensitive to relative changes in the unemployment rate with larger elasticities than those of the ‘non-working hours’ and ‘weekend’ accident rates, even after controlling for traffic volume. Finally, an analysis, utilizing job density, as an alternative to the unemployment rate, to account for commutes into the local authority, and controlling for traffic volume, finds a negative association where the rate of accidents that occur during working hours and workdays, as well as young male casualties are the most sensitive to relative changes in job density.

The second chapter investigates the impact of the Santander Cycle Hire Scheme on accidents and casualties and hypothesises that the scheme will increase the pedal cycle volume of traffic therefore increasing road accidents. A difference in difference regression design on local authorities within Greater London from 2000-2017 is used to determine the effect of the scheme on the number of accidents and casualties within the treatment group compared to the control group. The treatment group comprises local authorities covered by the scheme and the control group comprises all other local authorities of London and all scheme expansions are considered when conducting the analysis.

The effect is further decomposed into the impact on pedal cycle and car accidents in order to ascertain whether the scheme has a greater impact on cyclists or vehicle drivers. The accident and casualty severity are also analysed by decomposing the analysis to include fatal, serious and slight accidents and casualties to establish the severity of scheme effect. The effect of the scheme on pedestrians is also considered.

Traffic volume is controlled for by using the accident and casualty rates per million miles and a spill-over group, consisting of local authorities that are neighbours to the treated local authorities but which are not treated themselves, is also added to all specifications to determine whether the effect of the scheme spills over into other areas and, if so, to measure this spill-over effect.

The results suggest the scheme benefits cyclists by decreasing the pedal cycle accident rate per million miles but does not benefit motorists and pedestrians, increasing the car and pedestrian accident rates respectively. However, the scheme only significantly affects the slight accident and casualty rates therefore this adverse effect on motorists and pedestrians is only through slight accidents. Moreover, these results remain robust to a spill-over effect control, Cycle Superhighway, London Congestion Charge and London Summer Olympics controls.

The treated local authorities are then assigned to two groups, those with a high share of docking stations per square mile and those with a low share. Overall, the group of more intensely treated local authorities have a larger effect on the accident and casualty rates than those less intensely treated confirming that the results are due to the Santander Cycle Scheme.

The third chapter investigates whether terror incidents affect road accidents and casualties, hypothesising that this may be through a change in the quantity and quality of driving. Considering the quantity of driving, this may be due to a shift from public transport to driving given people's perception of the risk involved in taking public transport after an incident has occurred (Litman, 2005). Considering the quality of driving, perhaps the stress caused by an incident affects the way in which people are now driving (Gigerenzer, 2004; Stecklov and Goldstein, 2004; Gigerenzer, 2006). Alternatively, those who switched from public transport to driving may not be as experienced and therefore involved in more accidents.

A fixed effects regression design on police force areas within Great Britain from 2000-2017 is conducted to estimate this effect on the total number of accidents and casualties. Lagged variables are used to control for any spill-over effects and the analysis also controls for the total volume of traffic. The impact of a terror incident is separated into the effect of an incident occurring and a measure of the intensity of incidents. This effect is further decomposed into fatal, serious and slight accidents and casualties.

After controlling for total volume of traffic, the results confirm that the effect of either an incident or multiple incidents occurring in a given year and police force area on road accidents and casualties operates via a change in the quantity and quality of driving where the effect due to a change in the quality of driving is quite high. Furthermore, the effect of more than one incident occurring in a given year and police force area is positive and large compared to the effect of an incident occurring which is negative and small by comparison. However, most of the effect is due to a change in serious and slight accidents rather than fatal. This result also applies to casualties where most of the effect is experienced by serious and slight casualties.

Incidents are decomposed into attack type with the results suggesting that road accidents are more responsive to assassinations and unarmed assault. Various robustness checks are conducted including an analysis using high, medium and low media counts where incidents with a high media count have the largest effect on total accidents both one year and two years later. The Government MI5 threat level is used to measure the effect of a possible threat on road accidents and finds that a severe threat level will increase the total number of accidents. The fatalities and injuries caused by incidents are used as alternative measure of intensity where the negative effect of incident related fatalities is larger than the positive effect of incident related injuries in that it decreases accidents by more than the incident related injuries increase accidents. All these effects are through slight accidents and casualties.

Finally, a single event study using the 7th of July London incident which took place in 2005 is conducted to address time and spatial spill overs. The analysis finds that this incident decreases the total number of accidents per year locally compared to increasing them nationally and the effect is larger and lasts longer locally than nationally.

Chapter 2

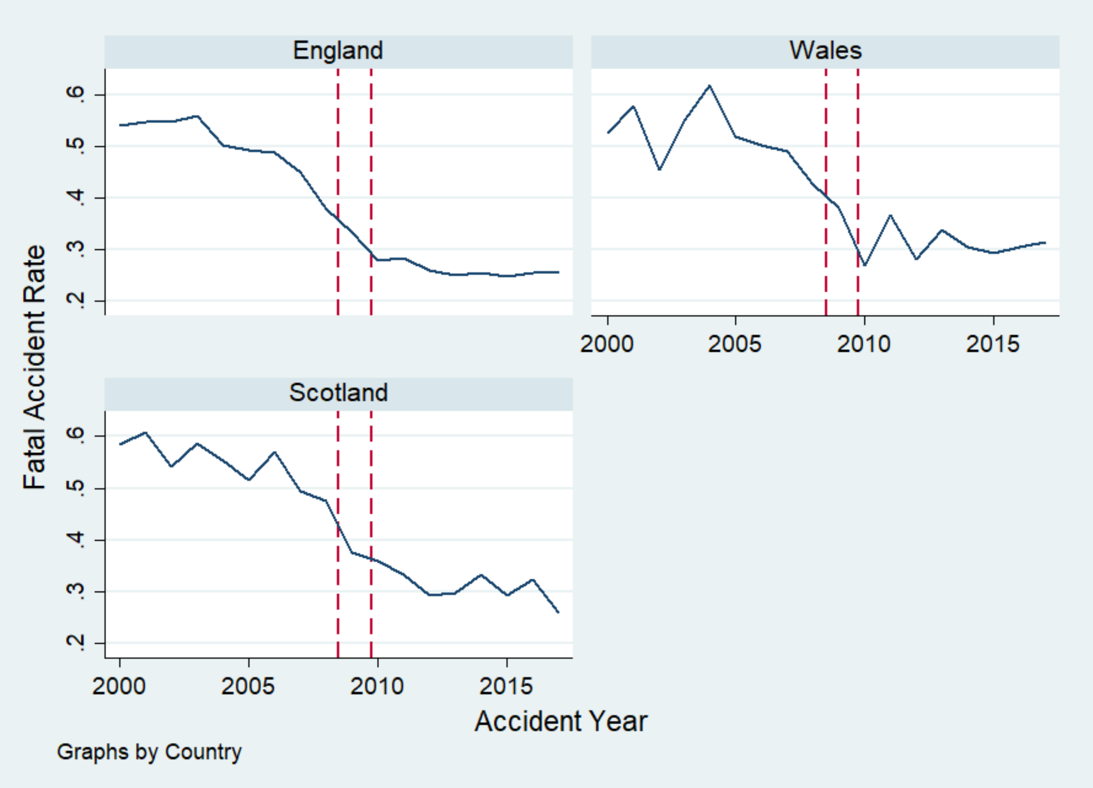
Road Accidents and Casualties in the Great Recession

2.1 Introduction

The recession period in the United Kingdom (measured by quarter-on-quarter changes of seasonally adjusted real GDP) took place from Q2 2008 till Q3 2009 (Office for National Statistics, 2013). While the recession and subsequent financial crises is said to have lasted a period of 18 months in the United Kingdom, the effects of this global recession were already felt towards the end of 2007 and was the deepest recession the country had suffered since the war. It affected many sectors with output declining by 7 percent towards the end of 2008. Annual real GDP declined by 1 percent in 2008 and 4 percent in 2009 before increasing by only 1.8 percent in 2010 (OECD Country Statistical Profiles, 2013). Furthermore, unemployment rose from 5.3 percent in 2007 to 5.9 percent in 2008 and continued to rise to a significant 7.9 percent in 2009 (Office for National Statistics, 2013).

Figures 2.1 depicts the distribution of fatal accident rate per 10 000 population by country (England, Wales and Scotland) from 2000 to 2017 including dashed red lines representing the recession. While the fatal accident rate begins to decrease in all countries prior to the recession and continues to decrease post the recession in Scotland, it follows a similar trend prior to and post the recession in all countries with a noticeable downward shift in this trend from Q2 2008 till Q3 2009.

Figure 2. 1 - Fatal Accident Rate per 10 000 people per country from 2000 to 2017 including recession period



Past research has found a negative association between the total unemployment rate, used as a proxy for changes in macroeconomic conditions, and fatal vehicle accidents (Wagenaar, 1984; Ruhm, 1995; Traynor, 2008; Cotti and Tefft, 2011).

Figures 2.2 and 2.3 display the 2004-2010 weighted average total unemployment and total fatal accident rates respectively by local authority within Great Britain. It is clear from the figures that higher levels of the average total unemployment rate are generally associated with lower levels of the average fatal accident rate within a given local authority.

Figure 2. 2 - Mean Unemployment Rates by Local Authority in Great Britain

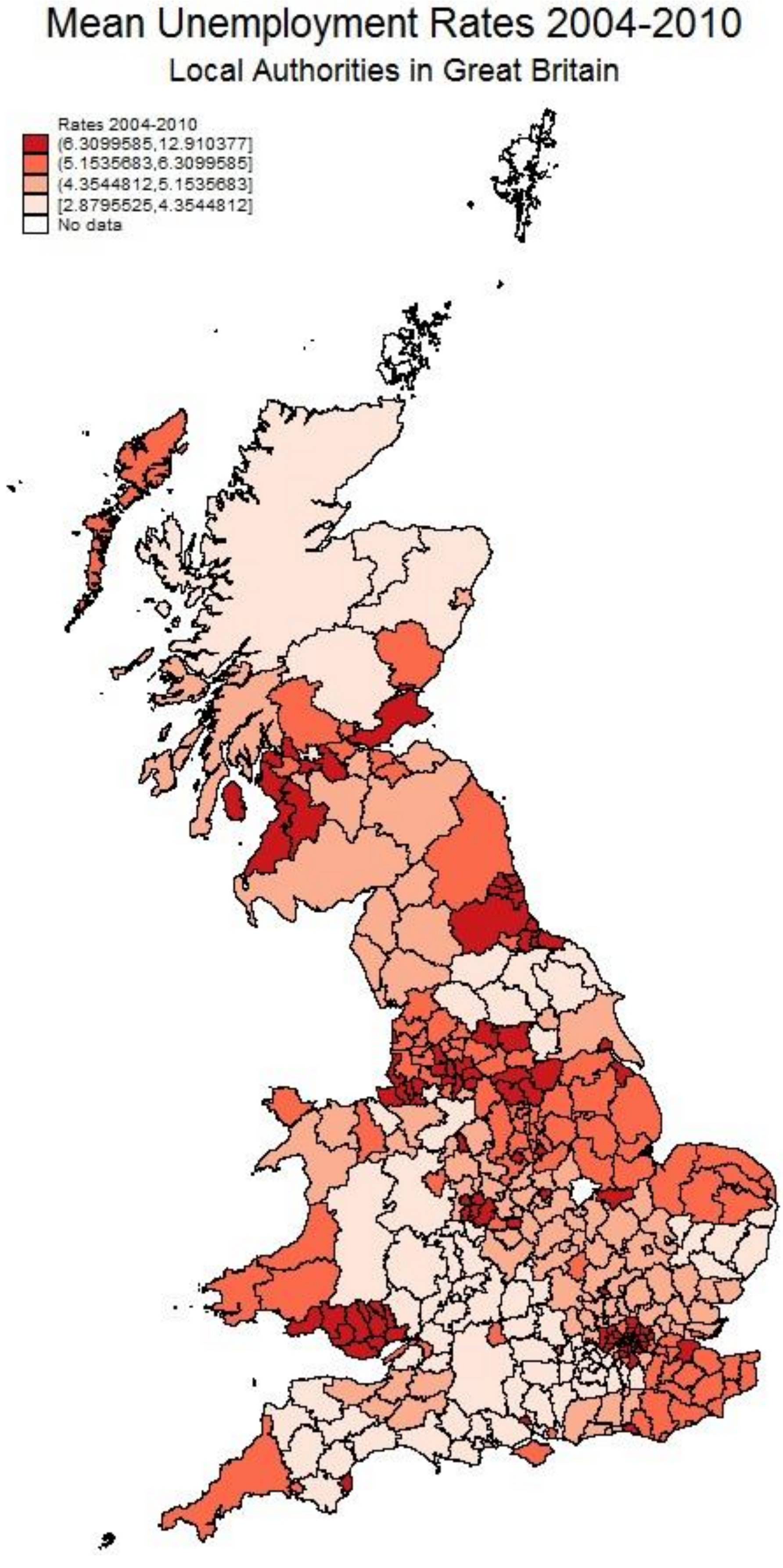
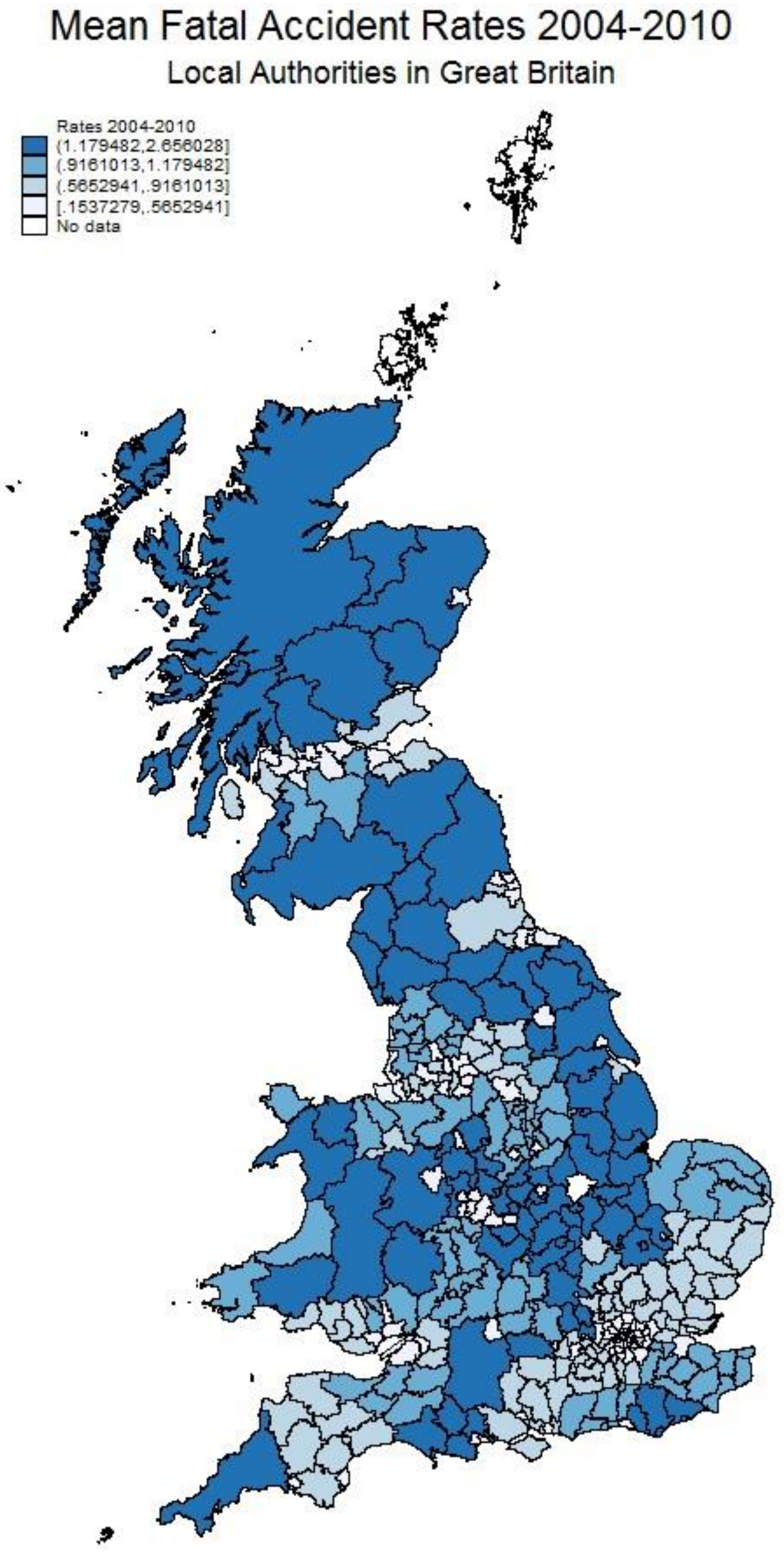


Figure 2. 3 - Mean Fatal Accident Rates by Local Authority in Great Britain



This paper therefore conjectures that, similar to its predecessors, an increase in the total unemployment rate will lead to a decrease in the fatal accident rate. Unlike past research, however, the paper will attempt to decompose this effect and determine from which vehicle accident and casualty subgroup most of this negative association is coming from.

It is hypothesised that an increase in the total unemployment rate may decrease the vehicle accident and casualty rates through a change in the quantity or quality of driving. Considering the quantity of driving, traffic volume will either decrease due to a reduction in the work commute or individuals may wish to save money they would normally spend on fuel (Wagenaar, 1984; Leigh and Waldon, 1991; Cotti and Tefft, 2011). Alternatively, considering the quality of driving, an unemployed individual may have a lower opportunity cost of time subsequently improving driving actions. For example, unemployed individuals may not feel the need to reduce travel times by increasing their speed or taking other risks associated with traffic accidents while driving (Traynor, 2008; Cotti and Tefft, 2011). Furthermore, a reduction in average individual income may lead to improved quality of driving through a reduction in alcohol consumption (Leigh and Waldon, 1991; Traynor, 2008; Cotti and Tefft, 2011).

A fixed effects analysis is used to determine whether there is a negative association between the total unemployment rate and vehicle accident/casualty rates and, if so, which of these dependent variables is most sensitive to relative changes in unemployment. The results confirm a negative association between the total unemployment rate and accident/casualty rates and suggest that this negative effect is chiefly from vehicle accidents that occur over the weekend and out of working hours as well as male casualties aged 20-39. Furthermore, the total unemployment rate findings are robust to a traffic volume control.

Further robustness checks are carried out to control for the effect of the employment rate, changes in commuting patterns, a further decomposition into the timing of accidents and the effect of job density.

These findings have significant policy implications. The UK government 'Making Roads Safer' policy, first published in October 2012, stipulates that by providing safety education and improving the skills of drivers/riders the personal cost (along with other costs) due to traffic accidents may be reduced. The policy aims to do this through various actions such as revising speed limits, becoming tougher on 'drink and drug' drivers, improved driver/rider training and road safety education for children to name only a few. This paper's findings that some vehicle accidents and subsequent casualties are more negatively affected than others, implies that, during (and after) a recession, more emphasis should be placed on certain policy actions than others. Furthermore, it should be kept in mind that, a decrease in vehicle accidents and casualties may not be due to policy measures already in place during a recession but may be due, in part, to the recession itself.

The next section reviews previous research related to this topic. The subsequent sections describe the data and estimation strategy used in the analysis, discuss the results and finally conclude.

2.2 Literature Review

Perhaps one of the first forays into this subject was conducted by Wagenaar (1984) who attempted to determine whether changes in macroeconomic conditions during the late 1970s and early 1980s recession in the US lead to a decrease in the number of traffic casualties involved in a motor vehicle accident where at least one injury occurred. As in subsequent research on the topic he used the unemployment rate as a proxy for macroeconomic conditions and controlled for vehicle miles travelled. Wagenaar hypothesised that an increase in the unemployment rate would either increase the frequency of casualties due to an increase in stress and risky behaviour as a consequence of unemployment or decrease the frequency of casualties through a decrease in the vehicle miles travelled. Using monthly data for the years 1972-1982 in Michigan and ARIMA and dynamic regression time-series modelling he found that there is a significant negative relationship between the unemployment rate and frequency of crashes, however it is small in magnitude. Also, and quite surprisingly, he finds that the vehicle miles travelled is not a significant intervening effect.

This result is in contrast to the paper by Leigh and Waldon (1991) who find that after controlling for both rural and urban miles driven the estimated coefficient on unemployment becomes positive and remains significant. However, these authors attempt to explain changes in highway fatalities rather than casualties using changes in the unemployment rate and use a similar hypothesis to that of Wagenaar (1984). They hypothesise that as unemployment rises aggregate driving decreases therefore reducing highway fatalities. Alternatively, unemployment may increase highway fatalities due to an increase in the aggregate level of stress caused by unemployment. In addition to this they further hypothesise that alcohol consumption may either increase (due to the stress of being unemployed) or decrease (due to lower incomes) with an increase in unemployment thereby increasing or decreasing, respectively, highway fatalities. Using a random effects research design and data from the 50 US states including the District of Columbia for the years 1976-1980 they find that, if the number of miles driven is controlled for an increase in the unemployment rate increases highway fatalities and conclude, as they hypothesised, that this is due to an increase in stress associated with unemployment. However, they stipulate that a rise in unemployment does lead to less driving and therefore an increase in the unemployment rate, on balance, leads to a decrease in highway fatalities. These results differ to the ones of this paper as, after controlling for traffic volume, we find that the estimated coefficients on unemployment become more negative.

Cotti and Tefft (2011) also use data from the 50 US states (for years 2003-2009) in attempting to determine the effect macroeconomic conditions associated with the great recession (from 2007 to 2008 in the US) had on fatal automobile accidents. However, unlike Leigh and Waldon (1991) they make use of a fixed effects research design. Using the state-quarter-year unemployment rate and real per capita personal income as proxies for macroeconomic conditions, as seen in most of the research on this topic, they hypothesise that an increase in the unemployment rate will reduce the fatal automobile accidents through a change in the quantity of driving and driving behaviour. Whereas other studies, including our paper, control for some form of traffic volume, this paper stands out due to the decomposition of the dependent variable (total fatal accident rate) into fatal accidents per mile and miles travelled per capita. Furthermore, the dependent variables are distinguished between fatal accidents that do and do not involve alcohol. They find that an increase in the unemployment rate is associated with fewer accidents per mile and alcohol related accidents are more responsive to changes in unemployment. This corresponds with our findings that, even after controlling for traffic volume, the unemployment rate still has a negative effect on the accident and casualty rates, probably through a change in driving behaviour. Another similarity between this and our analysis is that the authors find that fatal accidents involving those aged 60 and over are the least sensitive to changes in the unemployment rate.

Ruhm (1995) attempts to ascertain the relationship between macroeconomic conditions (again using the unemployment rate as a proxy) and alcohol related outcomes using vehicle fatalities as one of the proxies for alcohol related outcomes. So, while Leigh and Waldon (1991) control for alcohol consumption, Ruhm uses fatal motor vehicle accidents as an indicator of alcohol involved driving. The data covers 48 US states over 1975-1988 and, similar to Cotti and Tefft (2011) and our paper, uses a fixed effects research design. Ruhm (1995) finds a negative relationship between the macroeconomic conditions and total fatal accidents and doesn’t find any evidence that drinking or risky driving increases as unemployment rises. This finding is in line with our results given that, after controlling for traffic volume, we hypothesise that the negative effect unemployment has on the vehicle accident and casualty rates may be due to an improvement in the quality of driving. Also similar to our analysis, Ruhm finds that traffic fatalities of 21-24 year olds are more sensitive to changes in the unemployment rate than those of 15-20 year olds and his results are robust to changes in the macroeconomic condition proxy. By using the employment rate rather than the unemployment rate, he finds a positive relationship between the percentage of employed population and traffic fatalities.

Gerdtham and Ruhm (2006) attempt to determine the effect changes in macroeconomic conditions have on mortality rates as a whole using a fixed effects estimation strategy on 23 OECD countries (including the UK) over the years 1960-1997. They conclude that health decreases during an economic downturn but perhaps more significant to our analysis, by also determining the effect changes in macroeconomic conditions have on 'cause specific mortality', they find that fatal vehicle accidents are the most sensitive to changes in macroeconomic conditions (again proxied by the unemployment rate).

Scuffham (2003) examines the relationship between economic conditions and the 'trends and seasonal patterns' in fatal crashes using a time series analysis, similar to Wagenaar (1984), for New Zealand from 1970-1994. The dependent variable used is the quarterly number of fatal crashes and independent variables include, amongst others, the unemployment rate and distance travelled. Scuffham finds that an increase in unemployment and real GDP decrease fatal crashes and that the effect of real GDP is greater than that of unemployment.

While the majority of research on this topic finds a negative relationship between macroeconomic conditions and traffic accidents most of it focuses its attention on fatal vehicle accidents and or casualties while only some of it (e.g. Cotti and Tefft, 2011; Ruhm, 1995) separate the traffic fatalities into age groups within the analysis. This paper contributes to the literature by determining the effect on various traffic accident and casualty rates, decomposing them into fatal, serious and slight vehicle accidents and as well as vehicle accidents during various times of the day and dividing casualties by age group and gender. In doing so we are able to determine which group of vehicle accidents or casualties is most sensitive to changes in macroeconomic conditions thereby further aiding policy making decisions.

2.3 Data

This dataset ranges from 2004-2010 and therefore includes 4 years prior to the recession. It contains variables on accident, casualty, unemployment, employment, traffic volume rates and job density by local authority within Great Britain.[3]

A subsample, which ranges from 2004-2010 and contains variables on accident, casualty and unemployment rates by police force area within Great Britain is used to control for changes in commuting patterns. Due to its size the City of London Police is added to the Metropolitan Police within the data.

The dependent variables used within this analysis are configured from the accident and casualty data, (discussed in Chapter 1) then converted to rates using the mid-year (2010) Office for National Statistics Population Estimates where each rate is per 10 000 population. The accident rate variables consist of Fatal, Serious and Slight Accident Rates.

Accident Rate during Working Hours and Accident Rate during Non-Working Hours were generated using information on the hour of the day that the accident occurred where 'Working Hours' represents the rate of accidents from 07:00 – 18:00 and 'Non-Working Hours' represents the rate of accidents from 18:00-24:00 and 24:00-07:00. Workdays Accident Rate and Weekend Accident Rate were generated using information on the day of the week the accident occurred where 'Workdays' represents the accident rate from Monday-Friday and 'Weekend' Saturday and Sunday. As can be expected the variation in the accident rate during Working Hours is substantially larger than that of the accident rate during Non-Working Hours whereas, the Workdays Accident Rate variance is almost identical to that of the Weekend Accident Rate.

[3] The local authorities in the dataset are made of the Non-Metropolitan Counties, Unitary Authorities, London boroughs and Metropolitan Districts of England, unitary Council Areas of Scotland and unitary Principal Areas of Wales.

The timing of accidents is decomposed further by including Accident Rates during Peak Hours (peak travel times) expanding upon the Accident Rate during Working and Non-Working Hours. Morning Peak Hours represents the rate of accidents from 07:00 - 10:00 and Afternoon Peak Hours from 16:00 – 19:00. Furthermore, the Dark Morning and Dark Afternoon Peak Hours represent the rate of accidents during peak hours in Winter (December to February, which contain the lowest average number of daylight hours). These are included since more accidents are expected to occur when it is dark which is the case for part of the morning and afternoon peak hour commute in Winter. Interestingly the variation in the accident rate during the Afternoon Peak Hours, while small, is double that of the Morning Peak Hours. While both very small, the variation of the accident rate for Dark Afternoon Peak Hours, is slightly larger than that of the Dark Morning Peak Hours.

The variables Male and Female Casualty Rates are self-explanatory. While both quite large the variation in the male casualty rate is more than double that of the female casualty. There are four variables representing the casualty rates over various age groups (0-19, 20-39, 40-64, 65 and over) all of which were generated using information on the age of the casualty. The variation in the casualty rate of those 65 and over is much lower than the other age groups with the largest variation in the casualty rate of 20-39 year olds.

Data on both the unemployment and employment rates (per 100 population) are used as proxies for macroeconomic conditions.[4] For example, if a negative relationship exists between the unemployment rate and accident and casualty rates then this will be validated by a positive relationship between the employment rate and accident and casualty rates.[5] This labour market data covers the period from 2004-2010 and is taken from the Office for National Statistics Annual Population Survey. Figures are provided by local authority (including non-metropolitan counties

[4] The unemployment rate is used as a proxy extensively by past research on the topic (e.g., Wagenaar, 1984; Leigh and Waldon, 1991; Ruhm, 1995; Scuffham, 2003; Gerdtham and Ruhm, 2006; Cotti and Tefft, 2011).
[5] A similar robustness check is also carried out by Ruhm (1995).

rather than non-metropolitan districts for England) and police force area. The independent variable ‘Total Unemployment Rate’ was created using the unemployment rate data and represents the unemployment rate for ages 16-64. The independent variable ‘Total Employment Rate’ was created using the employment rate data and represents the employment rate for ages 16-64.

Given that it is hypothesised that an increase in the total unemployment rate may decrease the vehicle accident and casualty rates through a change in the quantity or quality of driving, data on traffic volume, taken from the Department for Transport and available from 2000-2012, is included in the dataset. Assuming that causality runs from unemployment to traffic volume (and not in the other direction), should the estimated coefficients on unemployment remain negative after controlling for traffic volume then it is reasonable to assume that this negative relationship is also through a change in the quality of driving. The traffic volume data is presented in units (thousand vehicle miles) and figures give the “total volume of traffic on the stretch of road” which is specified within the data, for each year and local authority.

The following variables were created using this data; ‘Motorcycles Traffic Volume Rate’, ‘Cars and Taxis Traffic Volume Rate’, ‘Buses and Coaches Traffic Volume Rate’, ‘Light Goods Vehicles Traffic Volume Rate’ and ‘All Heavy Good Vehicles Traffic Volume Rate’ all of which are used as dependent variables when trying to ascertain the relationship between the total unemployment rate and traffic volume rates. The variables were created by adding the total miles travelled for each type of vehicle within each local authority for each year then converting them to traffic volume per capita using the mid-year (2010) Office for National Statistics Population Estimates. The ‘Total Unemployment Rate’ has a nontrivial negative correlation with the ‘All Motor Vehicle Traffic Volume Rate’.

The unemployment rate is taken from a residence based survey therefore, a local authority with a high unemployment rate would account for fewer commutes out of the area thereby affecting the accident and casualty rates. However, commutes into the local authority have not been accounted for. Job Density replaces the unemployment rate in order to do so. Figures are provided by local authority for the period from 2004-2010 and are taken from the Office for National Statistics. This is a workplace-based measure and is composed of jobs done by residents and workers of any age who commute into the area (National Statistics, 2020). The job density variable is the total number of filled jobs divided by the 16-64 population for each local authority and year (National Statistics, 2020).

Years prior to 2004 were dropped due to the lack of labour market data available and the following local authorities were dropped; City of London was dropped due to missing unemployment and employment rate data, almost all the unemployment and employment rate data was missing for Rutland, Orkney Islands and Shetland Islands, Eilean Siar and Wandsworth. Isles of Scilly was dropped due to missing accident and casualty data.

The subsample therefore contains panel data from 2004-2010 by 199 local authorities within Great Britain for the accident and casualty rates, employment and unemployment rates, job density and traffic volume rates.

Descriptive statistics for the variables used can be found in Table 2.1.

Table 2. 1 - Descriptive Statistics

Variable	Obs	Sample mean	Standard deviation	Min	Max
Accident Year	1393	2007.026	2.000968	2004	2010
Local Authority	1393	1897.414	1470.576	101	5001
Outcome Variables					
Accident Rates					
Fatal Accident Rate	1393	0.8199743	0.5542483	0	4.709141
Serious Accident Rate	1393	5.893529	2.164557	1.261261	16.44592
Slight Accident Rate	1393	33.76013	8.064668	10.06711	74.12556
Accident Rate during Working Hours	1393	30.00374	6.870939	9.955257	60.04484
Accident Rate during Non-Work Hours	1393	10.46989	2.771436	3.061224	27.75785
Workdays Accident Rate	1393	5.972046	1.358373	1.879195	13.70404
Weekend Accident Rate	1393	5.306703	1.350164	1.118568	11.06679
Accident Rate during Morning Peak Hours	1,393	7.952567	1.987722	1.789709	15.47085
Accident Rate during Afternoon Peak Hours	1,393	11.64478	2.814927	4.198473	27.08585
Accident Rate during Dark Morning Peak Hours	1,393	1.999403	0.6113052	0.2906977	5.032022
Accident Rate during Dark Afternoon Peak Hours	1,393	2.688775	0.8206361	0.2491694	7.799274
Casualty Rates					
Casualty Rate of Ages 0-19	1393	19.33771	5.22473	5.643341	35.07246
Casualty Rate of Ages 20-39	1393	32.08604	8.610263	9.155766	62.2
Casualty Rate of Ages 40-64	1393	17.20511	4.404246	4.732824	47.90576
Casualty Rate of Ages 65+	1393	9.349051	2.406036	1.253919	27.45491
Male Casualty Rate	1393	47.86752	11.26548	14.25234	112.4664
Female Casualty Rate	1393	33.3346	7.870978	11.89931	67.82297
Explanatory Variables					
Total Unemployment Rate	1393	6.059079	2.461271	1.1	16.3
Total Employment Rate	1393	71.96774	5.22803	53.3	83.7
Job Density	1,393	0.7765993	0.2519057	0.37	4
Motorcycles Traffic Volume Rate	1393	0.0290401	0.0209755	0.0046291	0.1526144
Cars and Taxis Traffic Volume Rate	1393	2.846024	1.750804	0.5553401	11.67158
Buses & Coaches Traffic Volume Rate	1393	0.0279023	0.0118787	0.0089889	0.0935111
Light Goods Vehicles Traffic Volume Rate	1393	0.4412985	0.2577363	0.0772609	1.663648
All Heavy Goods Vehicles Traffic Volume Rate	1393	0.2739586	0.2101532	0.0202678	1.215688
All Motor Vehicles Traffic Volume Rate	1393	3.618226	2.202036	0.7288774	14.35767

Note: Summary statistics are weighted by the local authority total population.

2.4 Empirical Strategy

As already mentioned, past research has concentrated on the effect macroeconomic conditions have on fatal accidents (e.g., Leigh and Waldon, 1991; Ruhm, 1995; Gerdtham and Ruhm, 2006; Traynor, 2008; Cotti and Tefft, 2011). By contrast this paper attempts to break down this effect asking, does an increase in the unemployment rate decrease fatal accidents due to a large decrease in, for instance, the accident rate during working hours or non-working hours.

In order to do this the following equation is estimated using OLS and WLS -

$$Y_{it} = \beta_0 + \beta_1 X_{it} + \alpha_i + \tau_t + \varepsilon_{it} \qquad (1)$$

where the standard errors are corrected to allow for correlation within a local authority due to common unobserved group level factors (Solon et al., 2013). A fixed effects model is used to alleviate any potential omitted variable bias. By using local authority fixed effects any unobservable variables across groups, that are assumed to be time invariant, are controlled for and only the within group effect remains (Angrist and Pischke, 2008). Year fixed effects control for time aspects that affect the outcome variable across all local authorities over time (Cotti and Tefft, 2011). Given that there is sufficient variation in the total unemployment rate within each local authority the use of a fixed effects model is plausible in this case.

Y is a vector of the various casualty and accident rates in a given local authority and year. Although it is normally standard practice to use the natural logarithm of the outcome variable (e.g., Leigh and Waldon, 1991; Ruhm, 1995; Cotti and Tefft, 2011) some local authorities do not experience any fatal accidents or casualties in certain years hence both variables have a minimum value of 0. The above equation would, therefore, be undefined if the natural logarithm were used in this instance. X is vector of the macroeconomic conditions total unemployment rate, total employment rate and job density in a given local authority and year. The local authority fixed effects are defined as α and the time fixed effects as τ. The standard errors are clustered by local authority.

A vector Z, of the intervening variable 'All Motor Vehicle Traffic Volume' per year and local authority, is then added to equation (1) and estimated using WLS since it may be hypothesised that the total unemployment rate has an effect on the accident and casualty rates through this variable (Wagenaar, 1984; Leigh and Waldon, 1991).

Since each observation of the dependent and independent variables is a mean the specifications are weighted by the relevant dependent variable's local authority population correcting for possible heteroskedasticity in the local authority/year error term that may arise from the grouping (Solon et al., 2013). For example, when regressing the male casualty rate on total unemployment rate the local authority male population estimates are used as weights to account for the possibility that there will be smaller variation in the error term of an observation from a local authority with a larger male population than that of a local authority with a smaller male population.

2.5 Empirical Analysis

Multiple specifications of equation (1) using total unemployment rate as the independent variable are presented in Table 2.2. Each specification uses one of the accident rates or one of the casualty rates as the dependent variable. Columns (a) present OLS estimates and columns (b) WLS estimates.

Even though the OLS standard errors are smaller than the WLS standard errors for almost all the specifications the difference is slight therefore does not negate the precision of the WLS estimates. Furthermore, the OLS and WLS estimates are not dramatically different and maintain the same sign implying that model misspecification and endogenous sampling may not be a concern (Solon et al., 2013).

Since the dependent variables have varying means and the independent variables are kept in level format (rather than taking the natural logarithm) the elasticity is calculated and reported for each specification.[6] Given the difference in variation among the accident and casualty rates discussed in the data section of this paper it is advisable to interpret the elasticity for comparability.

[6] This is done throughout all the Tables presented in this paper.

Table 2. 2 - Fixed effect estimates of the relationship between total unemployment rates and accident or casualty rates

	Unemployment Rate			Unemployment Rate	
Regressand	a	b	Regressand	a	b
Fatal Accident Rate	-0.0080723	-0.0038504	Casualty Rate of Ages 0-19	-0.156442***	-0.1172686**
	(.0093221)	(.009834)		(.059143)	(.0585094)
	[-.0668529]	[-.0284520]		[-.0533943]	[-.0369070]
Serious Accident Rate	-0.0293203	-0.0432766	Casualty Rate of Ages 20-39	-0.2923138***	-0.3082503***
	(.0293179)	(.0297926)		(.0919072)	(.1017617)
	[-.0327496]	[-.0444922]		[-.0607660]	[-.0616833]
Slight Accident Rate	-0.2248642**	-0.1732118*	Casualty Rate of Ages 40-64	-0.0531015	-0.0259126
	(.089644)	(.1037531)		(.0488383)	(.0526483)
	[-.0440746]	[-.0310871]		[-.0203920]	[-.0090248]
Accident Rate during Working Hours	-0.157259**	-0.1364979	Casualty Rate of Ages 65+	0.0257799	0.0107166
	(.0780212)	(.0875127)		(.0459626)	(.0460902)
	[-.0346797]	[-.0275649]		[.0180771]	[.0067511]
Accident Rate during Non-Work Hours	-0.1049979***	-0.0838408**	Male Casualty Rate	-0.3945546***	-0.3300111**
	(.0332979)	(.0356203)		(.1252043)	(.1414142)
	[-.0663145]	[-.0485199]		[-.0545530]	[-.0418183]
Workdays Accident Rate	-0.0298649*	-0.0228154	Female Casualty Rate	-0.1349748	-0.1153984
	(.0152327)	(.0170097)		(.0920102)	(.0985952)
	[-.0331099]	[-.0231479]		[-.0267705]	[-.0209459]
Weekend Accident Rate	-0.0564661***	-0.0531308***			
	(.0174901)	(.0176283)			
	[-.0702327]	[-.0606636]			

Notes: Data from 2004-2010 for the 199 LAs are used. All WLS specifications are weighted by the respective LA populations. All specifications include LA and year dummy variables.

Column (a) represents OLS estimates and (b) WLS estimates. *, **, *** denotes P<0.1, P<0.05, P<0.01 respectively. Robust standard errors are reported in () and elasticities in []. N=1393.

The elasticities are all negative and fall within the same range namely from -0.02 to -0.06. The only significant results are those of the slight accident rate which has an estimated coefficient on unemployment that is significant at the 10 percent level, and, perhaps more telling since they have the highest (in absolute value terms)[7] elasticities, the accident rate during non-working hours and weekend accident rate which have significant estimated coefficients on unemployment at the 5 and 1 percent levels respectively. Therefore, should the total unemployment rate increase by one percentage point this would raise the rate of the total unemployment rate by 17 percent leading to a 1 percent decrease in the weekend accident rate.

The negative effect that an increase in the total unemployment rate has on the accident rate, therefore, seems to stem mostly from non-working hours whether that being after hours or over the weekend which suggests a possible income effect. However, it is not clear whether this effect is due to a change in the quantity and/or quality of driving.

A reduction in average individual income due to an increase in the unemployment rate may decrease accident rates through two effects, either traffic volume will decrease since individuals may wish to save money they would normally spend on fuel (a change in quantity of driving) or an unemployed individual may have a lower opportunity cost of time subsequently improving driving actions (change in quality of driving). For example, unemployed individuals may not feel the need to reduce travel times by increasing their speed or taking other risks associated with traffic accidents while driving (Leigh and Waldon, 1991; Traynor, 2008; Cotti and Tefft, 2011). Furthermore, a reduction in average individual income may lead to improved quality of driving through a reduction in alcohol consumption (Leigh and Waldon, 1991; Traynor, 2008; Cotti and Tefft, 2011).

[7] Unless the elasticities are positive, when comparing them the absolute value figure is assumed throughout the paper i.e. -0.05 is a ‘higher’ or ‘greater’ value than -0.02 in absolute value terms.

The casualty rate of those aged 40-64 have a low elasticity of -0.01 and the estimated coefficient on unemployment is not significant. Conversely, and perhaps unsurprising, the casualty rate of those aged 0-19 have a higher elasticity of -0.04 and those aged 20-39 an elasticity of -0.06. The estimated coefficients are also statistically significant. What is perhaps more interesting is that the casualty rate of those aged 65 and over have an elasticity of 0.01. Given that changes in the total unemployment rate are not likely to affect those aged 65 and over a small elasticity was expected, furthermore the estimated coefficient on unemployment is not statistically significant. While the female casualty rate has a lower elasticity of -0.02, the male casualty rate has an elasticity of -0.04 and the estimated coefficient is significant.

These results suggest that the negative effect the total unemployment rate has on casualty rates derives mostly from the casualty rates of younger males (specifically the age group from 20-39). This may be explained by a decrease in young male drivers during the recession (Broughton et al., 2015). In addition to this, males aged 16-24 are the group most affected by unemployment (Office for National Statistics APS, 2013).

Again, as with the accident rates, it is unclear whether the negative effect the total unemployment rate has on the casualty rates is due to a change in the quantity and/or quality of driving. Should the effect be through the quantity of driving it is hypothesised that an increase in the total unemployment rate will lead to a decrease in traffic volume due to either a reduction in the work commute or, as mentioned above, individuals may wish to save money they would normally spend on fuel therefore drive less (Leigh and Waldon, 1991; Cotti and Tefft, 2011). Alternatively, during a recession the quantity of driving may also increase. For example, both the unemployed and employed (whose earnings may decrease and/or propensity to save may increase during a recession) may choose to vacation closer to home and therefore drive rather than travel by air (Cotti and Tefft, 2011).

Traffic Volume Control

In order to investigate this further multiple specifications of equation (1) using the various traffic volume rates as dependent variables and the total unemployment rate as the independent variable are reported in Table 2.3.

Table 2. 3 - Fixed effect estimates of the relationship between total unemployment rates and traffic volume rates

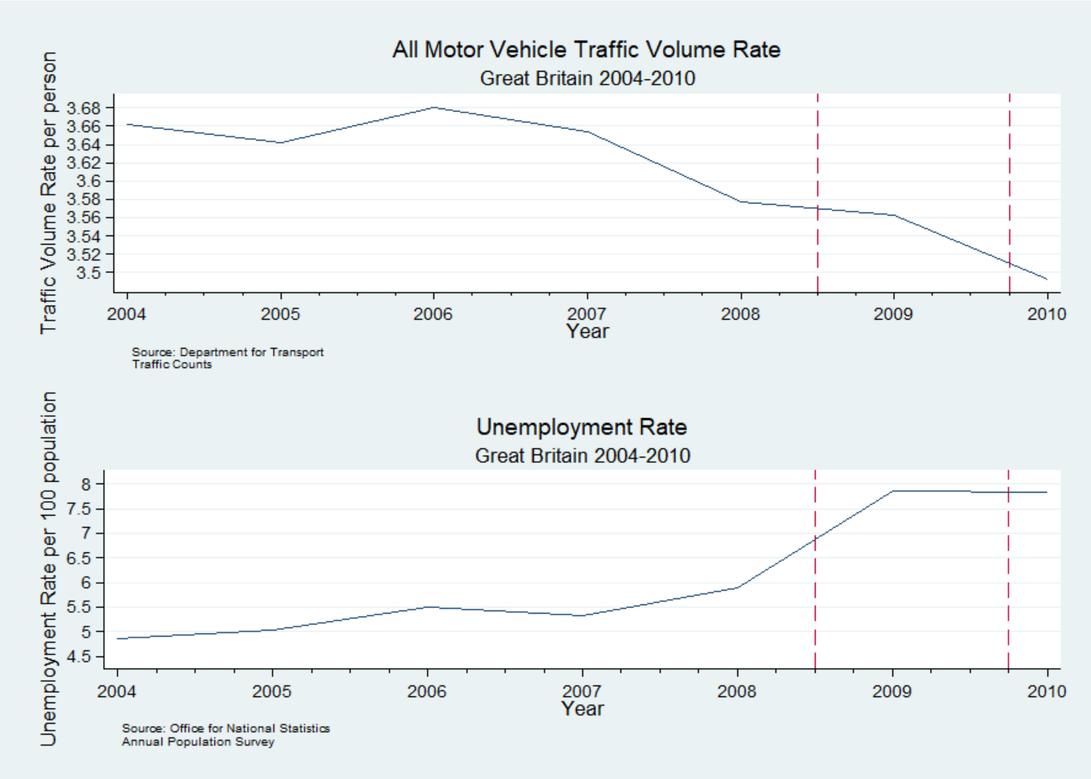
	Unemployment Rate
Regressand	
All Motor Vehicles Traffic Volume Rate	0.0140199***
	(.0041528)
	[.0234777]
Motorcycles Traffic Volume Rate	0.0002383*
	(.0001307)
	[.0497202]
Cars and Taxis Traffic Volume Rate	0.013153***
	(.003807)
	[.0280022]
Buses & Coaches Traffic Volume Rate	-0.0000464
	(.0000989)
	[-.0100759]
Light Goods Vehicles Traffic Volume Rate	0.0003388
	(.0007997)
	[.0046518]
All Heavy Goods Vehicles Traffic Volume Rate	0.0003361
	(.0007032)
	[.0074334]

Notes: Data from 2004-2010 for the 199 LAs are used.
All specifications are weighted by the LA total populations and include LA and year dummy variables. Robust standard errors are reported in () and elasticities in []. N=1393. *, **, *** denotes P<0.1, P<0.05, P<0.01 respectively.

The results suggest that an increase in the unemployment rate increases the 'all vehicle traffic volume rate'. This is in contrast to Figure 2.4 which displays the national average total unemployment rate and all motor vehicle traffic volume rate figures from 2004-2010. The two variables are negatively correlated which is evident from the figure since they tend to move in opposite directions during the recession (the dashed red lines depict the recession period within the United Kingdom).

However, this national trend may be explained by other factors such as changes in fuel prices during the recession.

Figure 2. 4 - All Motor Vehicle Traffic Volume and Total Unemployment Rates



The motorcycle traffic volume rate seems to be the most sensitive to relative changes in the total unemployment rate with an elasticity of 0.05. The 'light goods vehicles traffic volume rate' and 'all heavy goods vehicles traffic volume rate' have the lowest elasticity, while still positive, of 0.005 and 0.007 respectively indicating that they are the least sensitive to relative changes in the total unemployment rate. The 'all motor vehicle traffic volume rate' which is used as the control variable in Table 2.4 has an elasticity of 0.02 and the estimated coefficient is statistically significant.

In order to control for traffic volume, the 'all motor vehicle traffic volume rate' variable is added to equation (1) as an independent variable. Multiple specifications of equation (1) using the total unemployment rate and 'all motor vehicle traffic volume rate' as independent variables are presented in Table 2.4.

Table 2. 4 - Fixed effect estimates of the relationship between total unemployment, all motor vehicles traffic volume and accident or casualty rates

	Unemployment Rate	All Vehicles TV Rate		Unemployment Rate	All Vehicles TV Rate
Regressand			Regressand		
Fatal Accident Rate	-0.0062942	0.1743142*	Casualty Rate of Ages 0-19	-0.1223847**	0.3667599
	(.0098757)	(.1043081)		(.0581787)	(.6447531)
	[-.0465101]	[.7691804]		[-.0385172]	[.0684466]
Serious Accident Rate	-0.0372339	-0.4310048	Casualty Rate of Ages 20-39	-0.3397528***	2.436975*
	(.0297858)	(.4444972)		(.1009327)	(1.42912)
	[-.0382798]	[-.2646076]		['-.0679872]	[.2647449]
Slight Accident Rate	-0.2048403**	2.255969*	Casualty Rate of Ages 40-64	-0.0353484	0.6404982
	(.1013068)	(1.272737)		(.0533953)	(.7053729)
	[-.0367636]	[.2417824]		[-.0123112]	[.1398463]
Accident Rate during Working Hours	-0.1608596*	1.737648	Casualty Rate of Ages 65+	0.0052043	0.3748808
	(.0863686)	(1.08596)		(.0464218)	(.4104018)
	[-.0324847]	[.2095473]		[.0032785]	[.1511137]
Accident Rate during Non-Work Hours	-0.0875088**	0.2616301	Male Casualty Rate	-0.3521874**	1.580638
	(.0366719)	(.4669869)		(.1421211)	(1.775981)
	[-.0506426]	[.0904152]		[-.0446284]	[.1193636]
Workdays Accident Rate	-0.0266151	0.2710218	Female Casualty Rate	-0.1486544	2.372507*
	(.0168943)	(.1991143)		(.0973405)	(1.213934)
	[-.0270030]	[.1642014]		['-.0269822]	[.2576608]
Weekend Accident Rate	-0.0576464***	0.3220842			
	(.0178628)	(.2591894)			
	[-.0658194]	[.2196040]			

Notes: Data from 2004-2010 for the 199 LAs are used. All WLS specifications are weighted by the respective LA populations and include LA and year dummy variables.

*, **, *** denotes P<0.1, P<0.05, P<0.01 respectively. Robust standard errors are reported in () and elastcities in []. N=1393

Aside from the serious accident rate[8] the estimated coefficients on the 'all motor vehicle traffic volume rates' are all positive and the accident and casualty elasticities with respect to this variable are on average 0.2. Given these findings and the fact that the 'all motor vehicle traffic volume rate' is negatively correlated with the total unemployment rate the estimated coefficients on the total unemployment rate within Table 2.4 are expected to be on average smaller (more negative). In other words, before controlling for the all motor vehicle traffic volume rate the total unemployment rate was capturing the positive effect traffic volume has on the accident and casualty rates therefore, once this variable is controlled for the effect the total unemployment rate has on the accident and casualty rates should become more negative given that the positive effect is now being captured by the all motor vehicle traffic volume rate.

Considering first the severity of accident, before controlling for traffic volume the serious accident rate was the most sensitive to relative changes in the total unemployment rate. Now the fatal accident rate has the highest elasticity of -0.05 whereas the elasticity of the serious accident rate has remained the same.[9] After controlling for the traffic volume rate the elasticity of the accident rate during non-working hours remains the same at -0.05 and the estimated coefficient on the total unemployment rate is still significant. While the elasticity of the accident rate during working hours has also remained the same at -0.03, the estimated coefficient on unemployment is now statistically significant at the 10 percent level. The elasticities have increased for both the weekday and weekend accident rates however the weekend accident rate elasticity is still larger at -0.07, with a significant estimated coefficient on unemployment, than that of the workday accident rate which is -0.03.[10]

[8] The estimates of the association between the all motor vehicle traffic volume rates and this accident rate is not statistically significant.

[9] In cases where the elasticity is said to have 'remained the same', in actuality is has changed however this change is so slight that it is negligible.

[10] Again, this refers to an absolute value increase i.e. -0.02 increases to -0.05 in absolute value terms.

Even after controlling for the all vehicle traffic volume rate the negative effect that an increase in the total unemployment rate has on the accident rate still seems to come from non-working hours whether that being after work hours or over the weekend.

Considering the specifications using the casualty rates as dependent variables, after controlling for the traffic volume rate there is not much change in the casualty rates of the various age groups. The elasticity of the casualty rates of those aged 20-39 has increased to -0.07 and the estimated coefficient is still statistically significant. The elasticity of the casualty rate of those aged 65 and over is still positive and even though it decreases to 0.003 the change is negligible and, of all the casualty rates, it is the least sensitive to relative changes in the total unemployment rate which is not surprising as discussed earlier.

The elasticity of the female casualty rate increases from -0.02 to -0.03 however the estimated coefficient is still not significant and the male casualty rate is still more sensitive to relative changes in the total unemployment rate with an elasticity of -0.04 (which has not changed) with a statistically significant estimated coefficient.

After the all vehicle traffic volume rate has been controlled for the results still suggest that the negative effect the total unemployment rate has on casualty rates derives mostly from the casualty rates of young males.

Therefore, the negative association between the total unemployment rate and accident and casualty rates remains and this negative effect seems to be derived from the same accident and casualty rates even when controlling for traffic volume. This would suggest that, after controlling for traffic volume, the negative effect the total unemployment rate has on the accident and casualty rates may be through the quality of driving. Possibilities may include a reduction in speeding and or drink driving (Cotti and Tefft, 2011; Broughton et al., 2015).

Employment and Commuting Patterns

The findings are robust when using an alternative proxy for macroeconomic conditions. Multiple specifications of equation (1) using total employment rate as the independent variable are presented in Table 2.5 where columns (a) present OLS estimates and columns (b) WLS estimates. The negative association between the total unemployment rates and accident and casualty rates is confirmed by the positive association between these and the total employment rate. Considering the accident rates, the elasticity of the fatal accident rate is 0.31. The elasticity of the slight accident rate is 0.22 and the serious accident rate is the least sensitive to relative changes in the employment rate with an elasticity of 0.17. The elasticities of the accident rates during working hours and non-working hours are now similar with values of 0.21 and 0.23 respectively. The weekend accident rate is slightly more sensitive to relative changes in the employment rate, with an elasticity of 0.3, than the workday accident rate which has an elasticity of 0.2.

These findings are mostly consistent with those obtained when using the total unemployment rate as the independent variable. It seems that the positive effect that the employment rate has on accident rates is coming from fatal accidents and during the weekend. These findings are once again consistent with a change in the quality of driving since the consumption of alcohol is expected to increase over the weekend leading to an increase in accidents overall.

When considering the casualty rates, the effect on the age groups due to an increase in employment is similar to the effect due to an increase in unemployment. The casualty rate of those aged 20-39 are the most sensitive to relative changes in the employment rate with an elasticity of 0.38 and the estimated coefficient on employment is significant. Furthermore, the elasticity of the casualty rate of those aged 0-19 is 0.22. The casualty rate of those aged 40-64 has a lower elasticity of 0.05 and unsurprising, given the effect of the unemployment rate on this variable, the casualty rate of those aged 65 and over has a negative elasticity of -0.05 implying that a percentage point increase in the employment rate decreases the casualty rate of those aged 65 and over. Again, the estimated coefficient on employment is not significant which may be due to the fact that those aged 65 and over are relatively unaffected by changes in either the employment or unemployment rate.

Finally, when considering gender, when using the unemployment rate as the independent variable the male casualty rate was more sensitive to relative changes in the unemployment rate than the female now the two have similar elasticities with respect to the employment rate of 0.20 and 0.23.

It seems as if the positive effect the employment rate has on casualty rates comes mostly from the casualty rates of those aged 20-39. At that age the individual can drive, and one expects that age group to be the most responsive to an increase in income therefore affecting the quantity and quality of driving. Furthermore, this age group includes young and possibly inexperienced drivers who are less capable of recognising possible road hazards while driving (Chapman and Underwood, 1998; Underwood et al., 2005; Borowsky et al., 2010).

If an individual used to commute outside their area of residence for work before becoming unemployed then the accidents and casualties in more than one local authority will have been affected by the change. In order to allow for changes in commuting patterns due to unemployment a sample using larger geographic areas is utilized. Police force areas comprise several local authorities, the Metropolitan police force area, for example, comprises all local authorities of Greater London. This analysis is conducted using equation (1) where the local authority fixed effects have been replaced by police force area fixed effects and standard errors are now clustered by police force area. Furthermore, specifications are weighted by the relevant dependent variable's police force area population.

Table 2. 5 - Fixed effect estimates of the relationship between total employment rates and accident or casualty rates

	Employment Rate			Employment Rate	
Regressand	OLS	WLS	Regressand	OLS	WLS
Fatal Accident Rate	0.007435	0.0035093	Casualty Rate of Ages 0-19	0.0843998*	0.0590087
	(.0059384)	(.006134)		(.0450983)	(.0482046)
	[.6925516]	[.3080053]		[.3239901]	[.2192896]
Serious Accident Rate	0.0266639	0.0135711	Casualty Rate of Ages 20-39	0.1280084*	0.1665324**
	(.018574)	(.0176501)		(.0651044)	(.0733359)
	[.3349727]	[.1657210]		[.2992942]	[.3781939]
Slight Accident Rate	0.1031534	0.104755	Casualty Rate of Ages 40-64	0.0178879	0.0120262
	(.0667583)	(.0752219)		(.0346522)	(.0362895)
	[.2274049]	[.2233102]		[.0772610]	[.0509752]
Accident Rate during Working Hours	0.0969382*	0.0885852	Casualty Rate of Ages 65+	0.0094508	-0.0058219
	(.0545543)	(.0606859)		(.0329153)	(.0304469)
	[.2404384]	[.2124827]		[.0745356]	[-.0452336]
Accident Rate during Non-Work Hours	0.0403142	0.0332502	Male Casualty Rate	0.1580678*	0.1360089
	(.0284013)	(.0319721)		(.0919893)	(.1069841)
	[.2863746]	[.2285546]		[.2458122]	[.2043940]
Workdays Accident Rate	0.0182706	0.0162518	Female Casualty Rate	0.1132732*	0.1071514
	(.0111228)	(.0123136)		(.0684909)	(.072943)
	[.2278232]	[.1958467]		[.2526851]	[.2313541]
Weekend Accident Rate	0.0229498*	0.0202882			
	(.0126842)	(.0136308)			
	[.3210545]	[.2751418]			

Notes: Data from 2004-2010 for the 199 LAs are used. All WLS specifications are weighted by the respective LA populations and include LA and year dummy variables.

*, **, *** denotes P<0.1, P<0.05, P<0.01 respectively. Robust standard errors are reported in () and elasticities in []. N=1393.

The estimates and elasticities using this sample and the unemployment rate as the independent variable are presented in Table 2.6. Overall, all the significant estimates are negative confirming the negative relationship that exists between the unemployment rate and accident and casualty rates. The OLS and WLS estimates do not vary too greatly and comparing the WLS estimates to those of Table 2.2, all but those for the slight accident rate, casualty rate of ages 0 to 19 and male casualty rate remain significant when using a larger geographic area. Given the larger area one would expect the elasticities to be more negative, which they are for all the significant estimates.

Therefore, after allowing for changes in commuting patterns, the results remain robust and the rate of accidents that occur during non-working hours and over the weekend as well as casualties in their 20s and 30s remain the most sensitive to relative changes in the unemployment rate.

Table 2. 6 - Fixed effect estimates of the relationship between total unemployment rates and accident or casualty rates by police force area

	Unemployment Rate			Unemployment Rate	
Regressand	a	b	Regressand	a	b
Fatal Accident Rate	0.0104	-0.00130	Casualty Rate of Ages 0-19	-0.135	-0.200
	(0.0242)	(0.0296)		(0.207)	(0.230)
	[0.0586]	[-0.0096]		[-0.0373]	[-0.0627]
Serious Accident Rate	-0.0357	-0.0357	Casualty Rate of Ages 20-39	-0.488	-0.829*
	(0.138)	(0.0764)		(0.349)	(0.459)
	[-0.2011]	[-0.0367]		[-0.0835]	[-0.1599]
Slight Accident Rate	-0.352	-0.515	Casualty Rate of Ages 40-64	-0.0270	-0.0823
	(0.352)	(0.391)		(0.163)	(0.149)
	[-0.0539]	[-0.0924]		[-0.0090]	[-0.0292]
Accident Rate during Working Hours	-0.202	-0.347	Casualty Rate of Ages 65+	-0.0935	-0.0747
	(0.295)	(0.312)		(0.110)	(0.0895)
	[-0.0373]	[-0.0701]		[-0.0566]	[-0.0486]
Accident Rate during Non-Work Hours	-0.175	-0.205*	Male Casualty Rate	-0.383	-0.698
	(0.114)	(0.115)		(0.510)	(0.576)
	[-0.0949]	[-0.1185]		[-0.0448]	[-0.0882]
Workdays Accident Rate	-0.0375	-0.0574	Female Casualty Rate	-0.367	-0.415
	(0.0616)	(0.0600)		(0.317)	(0.305)
	[-0.0352]	[-0.0582]		[-0.0609]	[-0.0755]
Weekend Accident Rate	-0.0950*	-0.132**			
	(0.0503)	(0.0630)			
	[-0.0980]	[-0.1508]			

Notes: Data from 2004-2010 for the 50 PFAs are used. All WLS specifications are weighted by the respective PFA populations. All specifications include PFA and year dummy variables.
Column (a) represents OLS estimates and (b) WLS estimates. *, **, *** denotes $P<0.1$, $P<0.05$ and $P<0.01$ respectively. Robust standard errors are reported in () and elasticities in []. N=350.

Additional Robustness Checks

Given that the accident rate during non-working hours is more sensitive to relative changes in the unemployment rate a further analysis, decomposing 'non-working' and 'working hours' into 'peak times' is conducted to determine whether these will be more sensitive to relative changes in the unemployment rate. An increase in the unemployment rate is expected to have a larger effect on the 'peak hours' accident rates since these are the times when the employed would be commuting to and from work. In addition to this, an increase in the unemployment rate is anticipated to have an even larger effect on the 'Winter peak hours' accident rates since part of the morning and afternoon peak hour times are dark in Winter and more accidents are expected to occur in the dark.

Tables 2.7 (A, B and C) represent the estimates of equation (1) using various 'peak hour' accident rates as dependent variables[11]. Tables 2.7A and 2.7B present OLS and WLS estimates and Table 2.7C controls for the 'all vehicle traffic volume rate'.

The OLS standard errors are slightly larger than the WLS standard errors and the estimates are very similar for all specifications. The WLS estimates are therefore reported.

[11] All specifications going forward will once again include local authority fixed effects and standard errors clustered by local authority.

Table 2. 7 - Fixed effect estimates of the relationship between explanatory variables and accident rates during peak

Table A - Relationship between total unemployment rate and accident rates during peak hours

	OLS	WLS	OLS	WLS	OLS	WLS	OLS	WLS
VARIABLES	Morning	Morning	Afternoon	Afternoon	Dark Morning	Dark Morning	Dark Afternoon	Dark Afternoon
Unemployment Rate	-0.0374	-0.0277	-0.105***	-0.0827**	-0.0183	-0.0259**	-0.0313**	-0.0248*
	(0.0269)	(0.0290)	(0.0380)	(0.0406)	(0.0121)	(0.0113)	(0.0136)	(0.0135)
	[-0.0317]	[-0.0211]	[-0.0594]	[-0.0430]	[-0.0623]	[-0.0785]	[-0.0771]	[-0.0559]

Table B - Relationship between total employment rate and accident rates during peak hours

	OLS	WLS	OLS	WLS	OLS	WLS	OLS	WLS
VARIABLES	Morning	Morning	Afternoon	Afternoon	Dark Morning	Dark Morning	Dark Afternoon	Dark Afternoon
Employment Rate	0.0415**	0.0341*	0.0526*	0.0357	0.00762	0.00804	0.0175	0.00920
	(0.0192)	(0.0190)	(0.0303)	(0.0294)	(0.00753)	(0.00707)	(0.0107)	(0.0108)
	[0.3954]	[0.3086]	[0.3344]	[0.2206]	[0.2780]	[0.2894]	[0.4846]	[0.2462]

Table C - Relationship between the total unemployment rate,
all motor vehicles traffic volume rate and accident rates during peak hours

	WLS	WLS	WLS	WLS
VARIABLES	Morning	Afternoon	Dark Morning	Dark Afternoon
Unemployment Rate	-0.0377	-0.0922**	-0.0307***	-0.0280**
	(0.0281)	(0.0398)	(0.0110)	(0.0134)
	[-0.0287]	[-0.0480]	[-0.0930]	[-0.0631]
All Vehicles TV Rate	0.718**	0.677	0.345**	0.234
	(0.362)	(0.528)	(0.148)	(0.208)
	[0.3267]	[0.2104]	[0.6243]	[0.3149]

Notes: Data from 2004-2010 for the 199 LAs are used for all tables. All WLS specifications are weighted by the respective LA populations.
All specifications include year and LA dummy variables. *** p<0.01, ** p<0.05, * p<0.1
Robust standard errors are reported in () and elasticities in []. N = 1393.

Table 2.7A represents the effect the unemployment rate has on the ‘peak hour’ accident rates. All elasticities are negative and only the morning peak accident rate estimated coefficients on unemployment are not significant. The afternoon peak accident rate elasticity of -0.04 is lower than its dark counterpart that has an elasticity of -0.06 implying that the negative effect of an increase in the unemployment rate on afternoon peak accidents rates is larger in Winter when commuters are travelling in the dark. However, the dark morning peak accident rate is the most sensitive to relative changes in the total unemployment rate with an elasticity of -0.08, higher than both the elasticities of the accident rate during non-working hours of -0.05 and the weekend accident rate of -0.06 from Table 2.2.

Table 2.7C presents the estimated coefficients and elasticities after controlling for the all vehicles traffic volume rate. All elasticities are negative and, once again, only the morning peak accident rate estimated coefficients on unemployment are not significant. After controlling for traffic volume, the afternoon peak hour accident rate elasticity is slightly more negative at -0.05. The dark afternoon peak hour accident rate elasticity remains the same at -0.06 however the dark morning peak hour accident rate elasticity is also slightly more negative at -0.09. Therefore, even after controlling for quantity of driving the results remain robust since the dark morning peak hour accident rate is still the most sensitive to relative changes in the total unemployment rate. Similarly, considering the effect of the ‘all vehicles traffic volume rate’ on the peak hour accidents rates, the dark morning peak hour has an elasticity of 0.62, double the other elasticities.

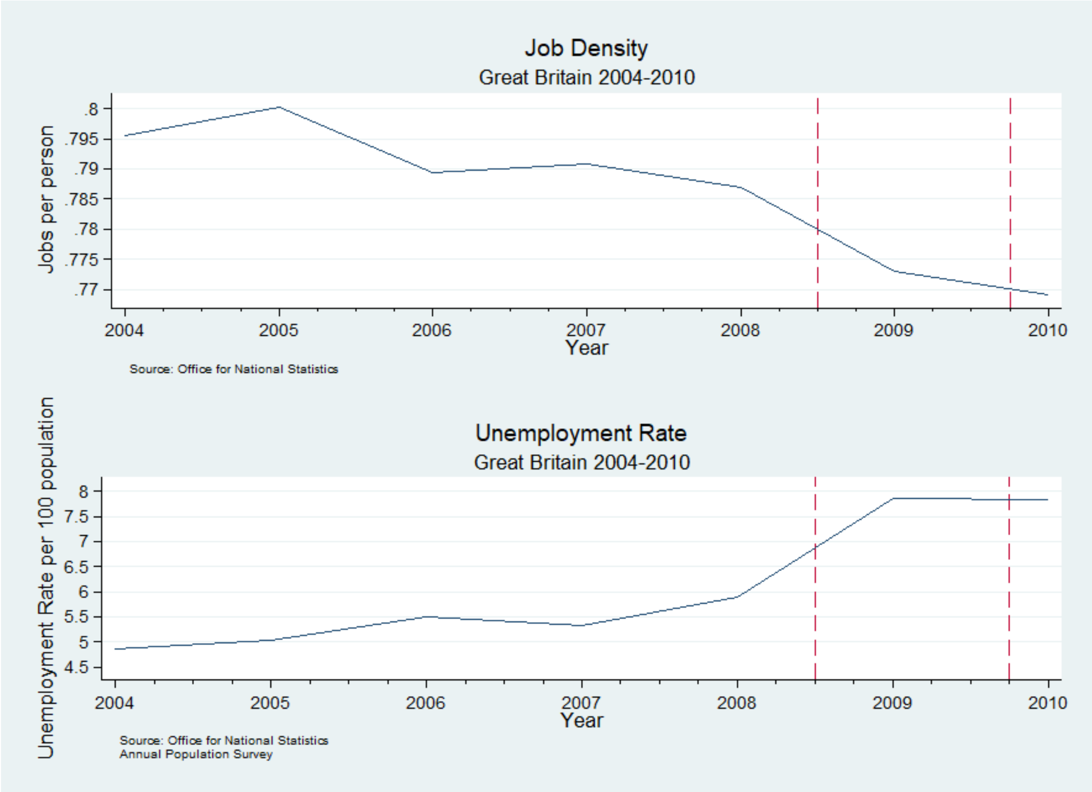
Table 2.7B represents the effect of an increase in the employment rate on the ‘peak hour’ accident rates. Only the morning peak hour accident rate estimates are significant with the largest elasticity of 0.31. While not all the estimated coefficients are significant, they are all positive therefore the negative association between the total unemployment rate and ‘peak hour’ accident rates is confirmed by the positive association between these and the total employment rate.

These results imply that the accident rate during the morning peak times in Winter are the most sensitive to relative changes in the unemployment rate, even after controlling for traffic volume. Furthermore, the ‘dark morning peak’ accident rate elasticities are larger than those of the ‘non-working hours’ and ‘weekend’ accident rates. This larger effect could be due to certain behavioural factors likely occurring during morning peak times. For example, people may be drowsy, or sleep deprived during the morning peak commute, which will only be exacerbated in Winter due to it being dark, leading to an increase in accidents (Horne and Reyner, 1995; Smith, 2016). Additionally, speeding may occur at this time which has been shown to increase accidents (Rock, 1995). Therefore, a lack of these factors owing to an increase in the unemployment rate will decrease the accident rates during the Winter peak hours by more than the ‘non-working hours’ and ‘weekend’ accident rates.

Since the unemployment rate is taken from a residence based labour market survey, a local authority with a high unemployment rate would account for fewer commutes out of the area thereby affecting the accident and casualty rates. However, commutes into the local authority have not been accounted for. Job density is therefore used as a proxy for macroeconomic conditions instead of the unemployment rate to account for this.

Figure 2.5 demonstrates that job density decreased steadily as the unemployment rate increased during the recession within Great Britain. The overall levels of job density are also much lower than those of the unemployment rate implying that more people are unemployed than there are jobs done by those who commute into the area.

Figure 2. 5 - Job Density and Total Unemployment Rate



Multiple specifications of equation (1) using job density as the independent variable and controlling for the all vehicles traffic volume rate are presented in Table 2.8 where only WLS estimates are reported. A negative association exists between the job density and accident and casualty rates and the elasticities are larger than those using the unemployment rate. Perhaps this negative association exists since, as demonstrated by Figure 2.5, there are fewer people commuting out of the area due to an increase in unemployment, and fewer people commuting into the area due to a decrease in job density, leading to less accidents overall. Unlike those using the unemployment rate, the Serious, Working Hours and Workdays accident rate estimated coefficients on job density are significant which is not surprising given that job density is a work-place based measure. There may also be fewer larger vehicles such as SUVs on the road during a recession given this vehicle's higher price, which may contribute to a reduction in fatal accidents providing a possible explanation as to why a negative relationship exists between the serious accident rate and job density (White, 2004; Anderson, 2008). The '0-19' and 'male' casualty rates estimated coefficients on job density are significant similar to the unemployment rate estimates.

Therefore, commutes into the local authority decrease the accident and casualty rates however the serious accident rate and accident rates that occur during working hours and workdays as well as young male casualties (below 20) are the most sensitive to relative changes in job density even after controlling for traffic volume. Using job density as an alternative proxy for macroeconomic conditions produces similar results to the unemployment rate during a recession especially with regards to young males.

Table 2. 8 - Fixed effect estimates of the relationship between job density, all motor vehicles traffic volume and accident or casualty rates

	Accident Rates						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Fatal	Serious	Slight	Working Hours	Non-Work Hours	Workdays	Weekend
Job Density	-0.681	-2.669*	-6.078	-7.590**	-1.839	-1.493**	-0.981
	(0.484)	(1.565)	(4.375)	(3.571)	(1.687)	(0.715)	(0.843)
	[-0.6450]	[-0.3517]	[-0.1398]	[-0.1965]	[-0.1364]	[-0.1941]	[-0.1436]
All Vehicles TV Rate	0.161	-0.501	1.927	1.461	0.126	0.224	0.234
	(0.103)	(0.438)	(1.256)	(1.064)	(0.455)	(0.194)	(0.253)
	[0.7104]	[-0.3076]	[0.2065]	[0.1762]	[0.0435]	[0.1357]	[0.1595]
	Casualty Rates						
	(1)	(2)	(3)	(4)	(5)	(6)	
	Age Group 0-19	Age Group 20-39	Age Group 40-64	Age Group 65+	Male	Female	
Job Density	-7.702***	-4.759	-3.337	-1.535	-11.02*	-8.109	
	(2.876)	(4.408)	(2.375)	(1.942)	(5.837)	(5.268)	
	[-0.3092]	[-0.1175]	[-0.1520]	[-0.1280]	[-0.1787]	[-0.1889]	
All Vehicles TV Rate	0.153	1.874	0.573	0.378	1.005	2.113*	
	(0.641)	(1.408)	(0.691)	(0.407)	(1.727)	(1.195)	
	[0.0286]	[0.2156]	[0.1216]	[0.1469]	[0.0759]	[0.2293]	

Notes: Data from 2004-2010 for the 199 LAs are used for all tables. All WLS specifications are weighted by the respective LA populations.
All specifications include year and LA dummy variables. *** $p<0.01$, ** $p<0.05$, * $p<0.1$
Robust standard errors are reported in () and elasticities in []. N = 1393.

2.6 Conclusion

This paper conjectures that vehicle accident and casualty rates decrease in a recession. Using the total unemployment rate as a proxy for macroeconomic conditions an increase in the total unemployment rate is expected to decrease the vehicle accident and casualty rates, furthermore, this decrease is assumed to be through the quantity and quality of driving. It is suggested that an increase in the unemployment rate will decrease traffic volume and therefore (through this effect) decrease the accident and casualty rates. Traffic volume is assumed to decrease with an increase in unemployment due to either a reduction in the work commute or individuals may wish to save money they would normally spend on fuel. Alternatively, traffic volume may increase since individuals may choose to vacation closer to home and therefore drive rather than travel by air. An increase in the unemployment rate may affect an individual's quality of driving through a lower opportunity cost of time or a reduction in average individual income (due to an increase in unemployment) may lead to a fall in alcohol consumption thereby also reducing risky driving associated with traffic accidents.

The results of a fixed effects regression design on local authorities within Great Britain from 2004-2010 suggest that there is a negative relationship between the unemployment rate and vehicle accident and casualty rates. This paper analyses the effect further by decomposing the vehicle and accident casualty rates into fatal, serious and slight vehicle accidents as well as vehicle accidents during various times of the day and dividing casualties by age group and gender. This indicates that the rate of accidents that occur during non-working hours and over the weekend, as well as young male casualties are the most sensitive to relative changes in the unemployment rate even after controlling for traffic volume. Furthermore, a subsample using larger geographic areas to allow for changes in commuting patterns produces similar results.

Moreover, after controlling for traffic volume the estimated coefficients on unemployment become more negative thereby implying that once traffic volume is controlled for the accident and casualty rates become more sensitive to changes in the total unemployment rate. It is therefore established that changes in the unemployment rate have a larger effect on the accident and casualty rates through changes in the quality of driving. This may be due to lower opportunity cost of time or a decrease in alcohol consumption. This analysis would therefore benefit from the inclusion of an alcohol consumption intervening variable.

Further analysis demonstrated that the accident and casualty rates are positively related to the total employment rate which validates the results.

A further decomposition into the timing of accidents indicates that the accident rate during the Winter morning peak hours are the most sensitive to relative changes in the unemployment rate with larger elasticities than those of the ‘non-working hours’ and ‘weekend’ accident rates, even after controlling for traffic volume.

Finally, an analysis, utilizing job density, as an alternative to the unemployment rate, to account for commutes into the local authority, and controlling for traffic volume finds a negative association between job density and the accident and casualty rates. The results also indicate that the rate of accidents that occur during working hours and workdays, as well as young male casualties are the most sensitive to relative changes in job density.

These findings suggest that during either an economic downturn or upturn more emphasis should be placed on certain policy actions, aimed at reducing traffic accidents, than others. Policy makers should also keep in mind that, during a recession, a decrease in vehicle accident and casualties may not be entirely due to the policy measures already in place. Furthermore, this paper is an addition to research stating that health (which can be proxied by vehicle accidents and casualties) is pro-cyclical.

Chapter 3

Road Accidents and the Santander Cycle Hire Scheme

3.1 Introduction

Cycling has always been a popular form of exercise however, in recent years its popularity as an alternative mode of transport has grown considerably and continues to do so. Since 2001, there has been an estimated 173 percent increase in the number of cycling trips on London's major roads alone with half a million trips taking place every day (Transport for London, 2015). This growth, in turn, has sparked the growth of cycle related policies such as the Santander Cycle Hire Scheme implemented in July 2010.

The scheme, previously sponsored by Barclays, is an affordable, easy and convenient way to travel within central London. It became popular very quickly, prompting approximately 70 percent of people to start cycling in London (Cycle Hire Casual Users Profile, TfL, 2013. Base 1,109 respondents).

Some literature has concentrated on the economic aspects of cycling, specifically the economic implications of the health benefits associated with this mode of transport. While other literature has focused on the impact of other cycle schemes such as the Cycle to Work Scheme (Swift et al., 2016). There is a body of work focusing on various bicycle sharing schemes, analysing the effect these schemes have had on congestion (Fishman et al., 2014; Ricci, 2015). Research on the Barclay/Santander Cycle Scheme tackles various aspects of the scheme itself such as the effect of the shift from membership only use to casual use (Lathia et al., 2012).

This paper investigates the impact the Santander Cycle Hire Scheme has had on road accidents. Other studies using road accident data have analysed the effects of policies such as the London Congestion Charge (Li et al., 2012; Green et al., 2016) however, none, to my knowledge, have conducted a study on the effect of the Santander Cycle Hire Scheme on road accidents.

It is hypothesised that the scheme will increase the pedal cycle volume of traffic therefore increasing road accidents. A difference in difference approach is taken to determine the effect of the scheme on the number of accidents and casualties (monthly counts) within the treatment group compared to the control group where the treatment group comprises the local authorities covered by the scheme and the control group comprises all other local authorities of London. Since the scheme has been expanded since its initial implementation in July 2010, all expansions are considered when conducting the analysis. This effect is further decomposed into the impact on pedal cycle and car accidents in order to ascertain whether the scheme has a greater impact on cyclists or vehicle drivers. The accident and casualty severity are also analysed by decomposing the analysis to include fatal, serious and slight accidents and casualties to establish the severity of scheme effect. Finally, the effect of the scheme on pedestrians is also considered.

Traffic volume is controlled for by using the accident and casualty rates which are calculated by dividing the number of accidents and casualties in a given year and local authority by the total traffic volume of that same year and local authority. The initial analysis uses monthly counts as dependent variables, however, due to a lack of monthly volume of traffic data the accident and casualty annual rates are subsequently used. Since some of the treated local authorities are only partly covered by the scheme, the intensity of the scheme is controlled for by dividing the number of docking stations in a given year and local authority by the area measured in square miles of that same local authority. The overlapping policies of cycle superhighways and the London Congestion Charge are also controlled for as well as the London Summer Olympics which took place in 2012. Three more years are then added to the analysis to incorporate another expansion of the scheme and determine whether the effects of the scheme changes with time. Finally, a spill-over group, consisting of local authorities that are neighbours to the treated local authorities but which are not treated themselves, is also added to all specifications to determine whether the effect of the scheme spills over into other areas and, if so, to measure this spill-over effect.

The subsequent section of this paper discusses the details and background of the scheme and provides an analysis on existing literature. This is followed by a description of the data and estimation strategy used in the analysis, discussion of the results and finally conclusion. The main results are presented using Tables 3.1 to 3.6 and robustness checks are presented using graphs (Figures 3.8 to 3.18).

3.2 The Scheme

The Santander Cycle Scheme is a public bicycle hiring scheme covering certain boroughs of Greater London and was based on the French Vélib' network in Paris. Initially sponsored by Barclays, the scheme has been sponsored by Santander since 2015 and the bicycles are therefore branded as Santander Cycles.

Taken from several policy documents, the overall purpose of the scheme is threefold; to reduce congestion and improve air quality in London by promoting cleaner forms of transport therefore reducing carbon emissions; promote a healthy lifestyle by encouraging people to walk and cycle; provide a low cost form of transport and improve the access and reliability of London’s public transport system (Transport for London, 2015).

Since the launch of the scheme there has been a shift in behaviour with more people using cycles in the city than before. As of 2018 there are approximately 800 docking stations comprising more than 11 000 bicycles and almost 73.5 million bicycle journeys have been made since 2010 (Transport for London, 2015).

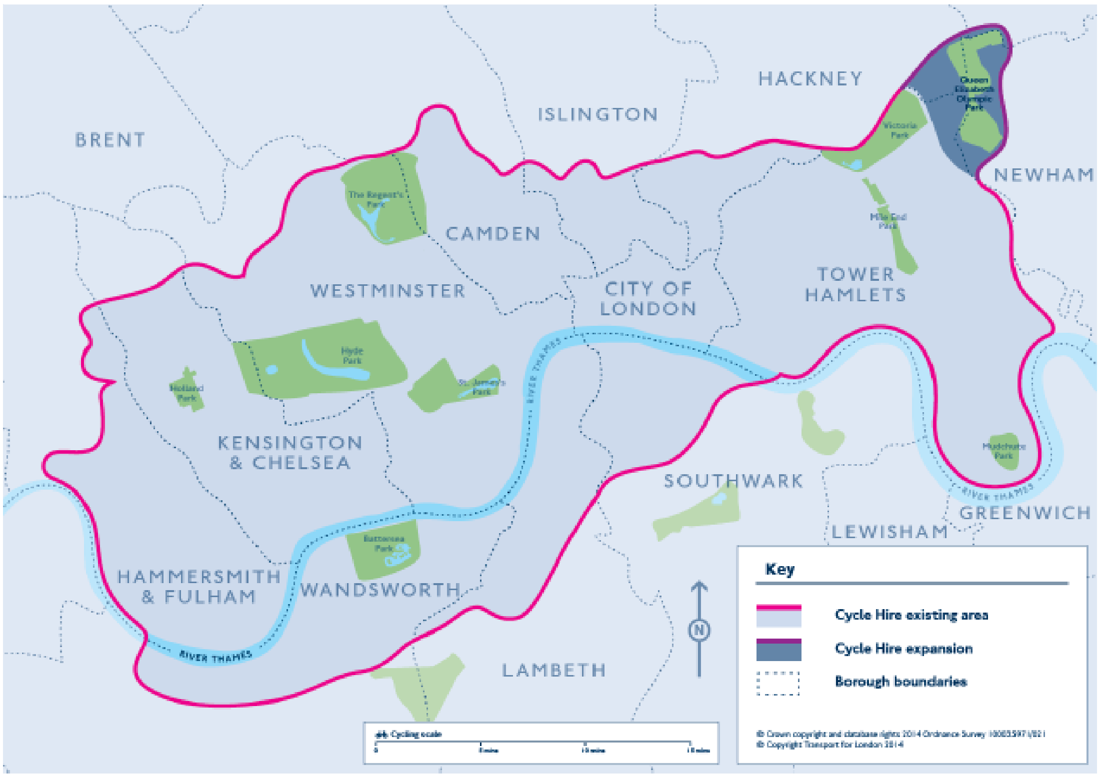
In a survey conducted by Transport for London in 2013 on users of the scheme (then known as the Barclays Cycle Hire), it was found that 41 percent of users previously using the London Underground were now using the scheme bicycles (Cycle Hire Casual Users Profile, TfL, 2013. Base 1,109 respondents).

In addition to this it reveals a high level of customer satisfaction where 72 percent of people surveyed gave the scheme an 8 out of 10 with most recommending it to a friend (Transport for London, 2015). The scheme encouraged approximately 70 percent of people to start cycling in London and most journeys have been under 30 minutes in duration (Transport for London, 2015).

The scheme was initially implemented in July 2010 either totally or partially covering the boroughs of Camden, Hackney, Islington, Kensington and Chelsea, Lambeth, Southwark, The City of London, Tower Hamlets and Westminster. Since then there have been 3 major expansions. The first took place in March 2012 adding the borough of Hammersmith and Fulham and increasing the number of docking stations in Hackney and Tower Hamlets, the second took place in October 2013 adding Wandsworth and increasing the number of docking stations in Hammersmith and Fulham, Kensington and Chelsea and Lambeth, the third took place in November 2015 adding Newham. The scheme is now the largest of its kind in Europe with further expansions planned.

Figure 3.1 demonstrates the existing scheme network as of April 2015 (Transport for London, 2015). The border outlined in this figure is for docking stations only where each consists of at least one terminal and should have a minimum of 27 docking points (Transport for London, 2015). The cycles themselves can be used anywhere.

Figure 3. 1 - Santander Cycle Hire Scheme Area as of October 2015



Source: Planning, Design and Access Statement, Transport for London (2015)

The established criteria used to select sites for docking stations include not disturbing established green areas such as parks, grassy areas or moving trees; not disturbing 'street furniture' including already existing cycle stands; the stations must not infringe on space used for pedestrian and vehicle paths or access; only be installed in secure, well-lit areas that include either CCTV or natural surveillance; be within close proximity to tourist destinations, leisure activities, places of work and personal residence; not have a detrimental impact on the townscapes or setting of heritage assets and must avoid areas of high pedestrian congestion or are unsuitable for cyclists (Transport for London, 2015). Not all these criteria will apply for every chosen location.

Given this list of criteria it is evident that road accidents have not influenced the choice of docking station placement. In fact, given the last criteria listed, quite the opposite, highly congested pedestrian areas or locations unsafe for cyclists are avoided. Furthermore, if the purpose of the scheme is to reduce congestion as mentioned above, then highly congested boroughs may have initially been chosen and not necessarily boroughs with a high accident rate per person.

The scheme is available 24 hours a day and a person can retrieve a cycle from any docking station returning it to another within 24 hours. £2 is paid to access the cycles for 24 hours. A user can make as many journeys as they like with unrestricted access to all boroughs, returning the cycle to any docking station. However, the cycles are intended for short journeys so, while the first 30 minutes is covered by the initial charge, an additional £2 is paid for every extra 30 minutes of use. Furthermore, if the bike is damaged or not returned, the user can be charged up to £300.

Transport for London has made finding a docking station easy providing a full list of stations and terminals on their website and via an app.

The scheme is easy to use, the user is required to insert a card (debit/credit) at the docking terminal which will then print a release code which is valid for 10 minutes and can only be used at that docking station. Four cycles can be hired at a time, each with its own release code. The user then chooses a bike, is asked to ensure the breaks, bell etc. are functioning, enters the release code, adjusts the seat and off they go. The cycle lights switch on automatically upon release and safety information is provided at every terminal. It is requested that faulty cycles are reported immediately at the docking terminal. In order to return the cycle, the user simply finds an empty docking station and locks it back in place. If the docking station is full, the terminal/website/app can be used to source an empty one and by selecting the correct option an extra free 15 minutes is added giving the user enough time to find a docking station with an empty space.

A membership option is also provided and costs £90 a year. Furthermore, users can also register for a key costing £3 to be used instead of a debit/credit card. The membership offers the user a monthly summary of activity including total distance covered and calories burnt.

3.3 Literature Review

There is quite a bit of literature on the effect of policy changes on road accidents specifically with respect to pricing policies such as fuel taxes and congestion charges (Parry et al., 2007). For the most part, literature on fuel taxes has found that an increase in taxes has led to a decrease in road accidents and studies on gasoline prices and motor vehicle fatalities confirm this negative relationship (Leigh and Wilkinson, 1991; Grabowski and Morrisey, 2004). A panel based fixed effects study by Grabowski and Morrisey (2006) using US data from 1982-2000 treats the price of fuel as an exogenous variable and found that an increase in state fuel taxes results in a decrease in traffic fatalities. This effect is through fewer vehicle miles travelled brought on by higher fuel prices. A similar study conducted on 144 countries from 1991-2010 found the same negative relationship. In their international study Burke and Nishitateno (2015) use a country's oil reserves and the annual international crude oil price as an instrument for that country's fuel price in order to deal with any endogeneity problems.

This association with traffic fatalities and volume of traffic is often addressed which is why this paper controls for traffic volume when examining the impact of the cycle scheme on road accidents. However, Grabowski and Morrisey (2006) imply that this effect may diminish over time when users substitute to more fuel-efficient modes of transport such as smaller vehicles, bicycles and motorcycles exposing them to more severe accidents. A vast amount of research has been conducted on the effect of larger vehicles such as sports utility vehicles and light trucks on accidents (Gayer, 2004; Anderson, 2008; Li, 2012; Anderson and Auffhammer, 2014). A paper, conducted by White (2004) using a sample of police reported motor accidents in the US from 1995 to 2001, found that an extra one million light trucks used instead of cars leads to between 34 and 93 additional fatalities amongst car occupants, pedestrians, cyclists and motorcyclists.

This need for an increase in cyclist safety has been addressed by many cities throughout the world either as its own initiative or via certain policies such as bicycle hire schemes. Making roads safer for cyclists becomes a government priority in order to encourage further use of these schemes that are often initially launched to promote the use of bicycles for various reasons. One of these reasons is due to the public health benefits of using bicycles (Ricci, 2015; Swift et al., 2016).

Furthermore, there are also economic benefits to society through lowered health expenditure and improvements to the environment (Swift et al., 2016). A study which critically reviews literature to determine the impact of the UK Cycle to Work Scheme conducted by Swift et al. (2016) found that while the impact of the scheme on the overall volume of cycling is inconclusive, a survey of 13 000 scheme users participating in the scheme administered by Cycle to Work Alliance in 2016, provides evidence that the scheme is associated with an increase of cycling to work among scheme users especially for those who did not cycle or only cycled occasionally before. Furthermore, they concluded that the amount of cycling with respect to miles travelled is rising. As mentioned by the authors, this review is not exhaustive and limited, being based on a survey conducted on scheme users only.

Another reason for the use of these policies is to reduce carbon emissions via a reduction in traffic congestion. Papers addressing the relationship between bicycle share schemes and congestion found that while schemes in other countries lower the motor vehicle traffic volume, they increase it in London. A study was conducted by Fishman et al. (2014) in the US, Australia and Great Britain in 2012 on survey and trip data from the bicycle share schemes in Melbourne, Brisbane, Minneapolis/St.Paul, Washington D.C. and London to ascertain the kilometre value car trips are substituted by ‘bicycle share’ trips. They include an examination on the motor vehicle support services that are used for scheme maintenance and to rebalance the docking stations as part of the analysis. There is a reduction in motor vehicle use measured in km travelled per annum in all cities but London. The authors conclude that this is due to a low car mode substitution rate and the substantial use of trucks to rebalance the docking stations.

The cost of rebalancing the docking stations is therefore not only a monetary one but also an environmental one so this issue, and how to ameliorate it, has been addressed by several studies (Rainer-Harbach et al., 2013; Raviv and Kolka, 2013; Chemla et al., 2013). A possible solution provided is to send control signals to customers so that they make slight changes to their journeys resulting in a balanced docking station (Aeschbach et al., 2015).

While there is extensive research on the effects of other policies, such as the London Congestion Charge, on road accidents, to the best of my knowledge, this research is lacking with respect to the Santander Cycle Hire Scheme.[12] This paper therefore attempts to fill a gap by analysing the effect the Cycle Hire Scheme has had on road accidents hypothesising that the scheme will lead to an increase in pedal cycle volume of traffic, as evidenced by past research, therefore increasing road accidents.

3.4 Data

A sample using the road accident, casualty and vehicle data discussed in Chapter 1 is created where the local authorities within the dataset are made up of London boroughs and include the City of London.

Since the hypothesis is that an increase in the use of pedal cycles due to the implementation of the scheme will increase the number of accidents the analysis estimates the effect of the scheme on total number of accidents but also decomposes this analysis to fatal, serious and slight accidents, fatal, serious and slight casualties and accidents involving pedal cycles, cars and pedestrians in order to ascertain the true effect of the scheme.

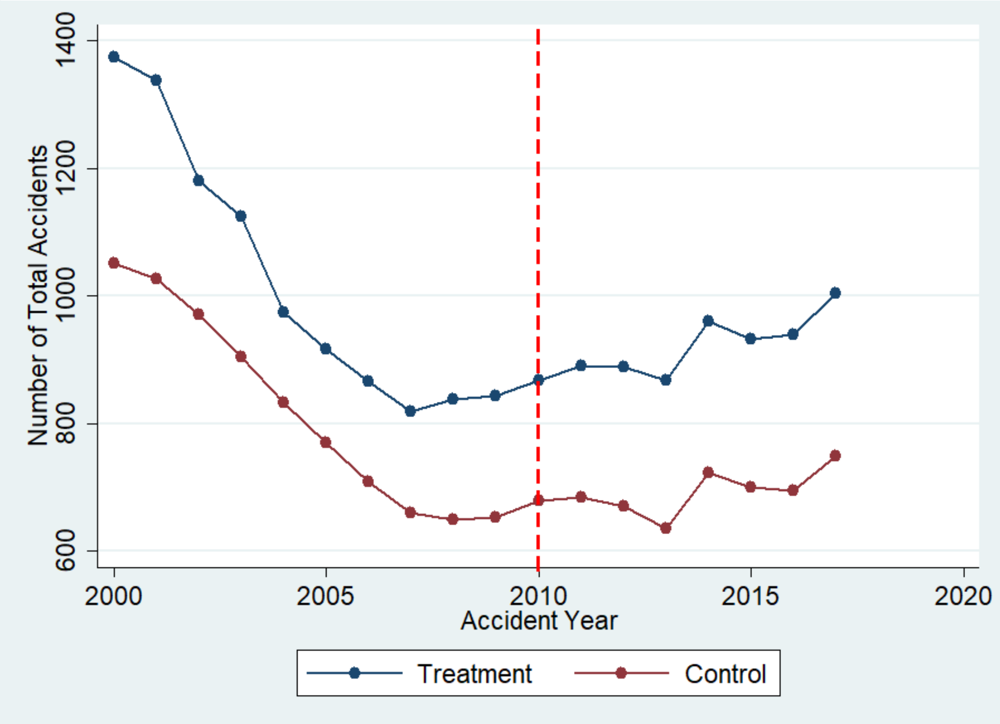
[12] Examples of this research which also use a difference in difference approach include studies by Li et al. (2012) and Green et al. (2016).

The Road Accident Data provides information on each accident including where the accident took place, the casualties, if any, involved, type of vehicle involved and more. Each accident is counted by month, year and local authority to produce a total count for each variable. These are ‘Total number of accidents’, ‘Fatal, Serious and Slight Accidents and Casualties’, Cycle and Car Accidents and Accidents involving Pedestrians’.

Since the scheme has four phases, the initial treatment and three extensions, ranging from 2000 till 2017, four treatment group variables are created each corresponding to a phase of treatment. ‘Treat0’, for instance, corresponds to the initial scheme implementation where a value of 1 is given to the local authorities treated within the first phase and 0 otherwise. The local authorities within these groups are collectively referred to as the treatment group. All other local authorities of Greater London unaffected by the scheme are assigned to a control group.

Figure 3.2 plots the average number of accidents for the treatment group corresponding to the initial scheme implementation vs the control group. Given that the data ranges from 2000 till 2017 a comparison in the average outcome between the treatment and control groups pre and post implementation of the scheme can be made. Phase 1 of the scheme was implemented in July 2010, indicated by the dashed red line, within Camden, Hackney, Islington, Kensington and Chelsea, Lambeth, Southwark, The City of London, Tower Hamlets and Westminster. It is clear from the figure that more accidents occur overall in the treatment group than the control group which is expected given that these include central boroughs of Greater London. While not entirely the same the treatment and control group do follow a similar path before treatment and continue to follow the same path soon after treatment (from 2012). Immediately after treatment, however, the number of accidents in the treatment group rises by more than that of the control group between 2010 and 2011 only to drop by less than the control group between 2011 and 2012. It is hypothesised that this immediate rise in accidents may be due to a substitution effect, replacing motor vehicles with pedal cycles leading to a rise in pedal cycle accidents rather than motor vehicle accidents. The overall trend seen in the data will be controlled for using year and month dummy variables.

Figure 3. 2 - Average number of Accidents in the Phase 1 Treatment group vs the Control Group in Greater London



As suggested by Green et al. (2016), an analysis of the accident and casualty rates is used to ascertain whether an accident externality exists when people start using pedal cycles since they take the possible risk to themselves into account but not that to others.

The annual total volume of traffic measured in 1000 vehicle miles is provided for each junction to junction link on the major road network per local authority. These are aggregated to form one volume of traffic figure per local authority and year. The total accident rate, fatal, serious, slight accident and casualty rates and pedestrian rates are calculated by aggregating the monthly counts of these variables to form annual figures per local authority and dividing them by the all motor vehicle volume of traffic for each corresponding year and local authority. The cycle and car accident counts are also aggregated to form the annual counts per local authority and are then divided by the pedal cycle volume of traffic and car and taxi volume of traffic respectively. All resulting rates are measured per million vehicle miles travelled.

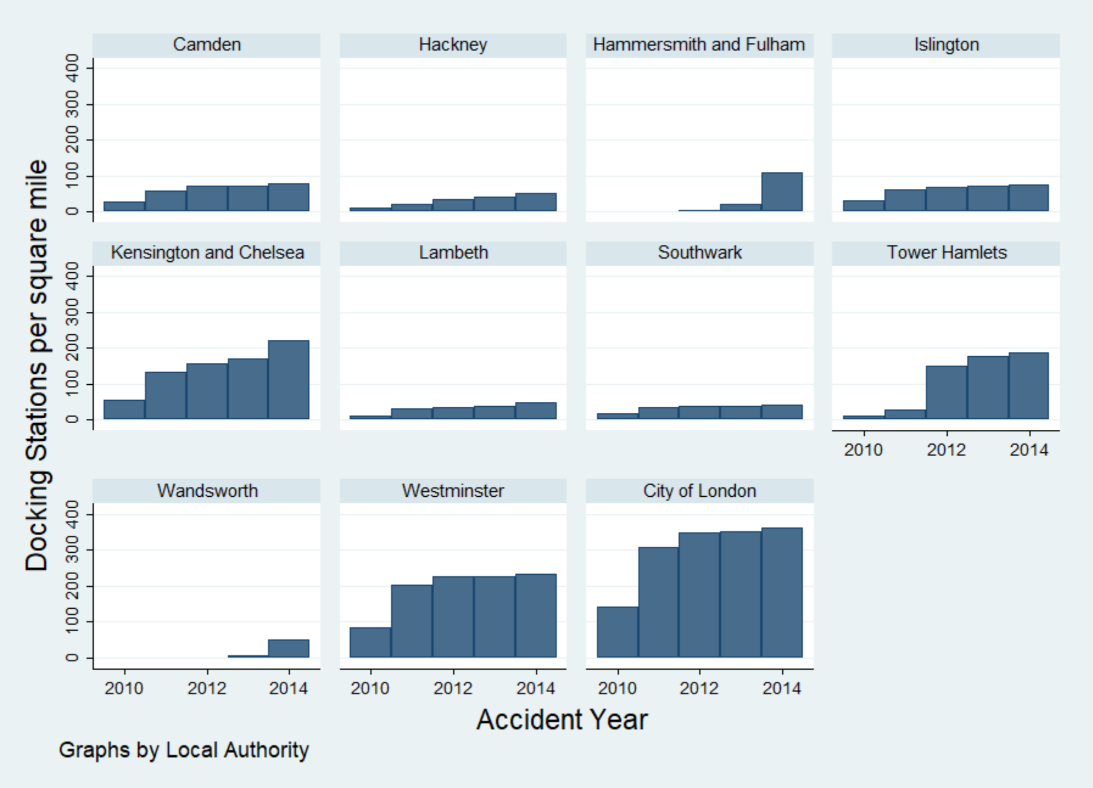
A further control is added to the analysis in the form of a spillover variable. Figure 3.1, which represents a map of the cycle scheme region, demonstrates that only parts of certain local authorities are covered by the scheme. This is not a definitive boundary since, while the docking stations lie within the boundary represented in the figure, the cycles can be used outside of this border and returned to any docking station. The scheme is therefore implemented in the entire local authority which is added to the treatment group. It is unlikely, however, that the cycles will be used in neighbouring local authorities since, while the cycles can be hired for a 24-hour period, the intention of the scheme was for them to be used primarily for short distance trips so the individual is charged £2 per any extra 30 minutes of use. While the cycles will most likely be ridden outside the 'docking station boundary line' it is unlikely that they will be ridden further than the local authority boundary often.

Nonetheless, to account for the possibility of a spill over into neighbouring local authorities the variable is added to the analysis as a robustness check. It is created by assigning a value of 1 to all the local authorities that neighbour the treated areas but are not treated themselves and 0 otherwise. One variable is created for all phases of the scheme so any local authority that moves from the 'spillover' group to the 'treated' group will be assigned a value of 0. For example, the scheme is only implemented in the borough of Newham during the third extension (November 2015). The borough, which neighbours the initial implementation (July 2010) treated areas of Hackney and Tower Hamlets but which was not treated itself, is given a value of 1 in the construction of the spillover variable and 0 in the treatment variable. It remains 1 until November 2015 when it is given a value of 0 and then remains 0 in the spillover variable and 1 in the treatment variable. The local authorities within this group are collectively referred to as the neighbouring or spillover group.

Even though the boundary represented in Figure 3.1 is not definitive as mentioned above, a further control accounting for the intensity of the scheme is implemented as a robustness check. Data, provided by Transport for London, on the number of docking stations per treated local authority for each year since implementation of the scheme is used, where the total docking stations per year and local authority are divided by the respective local authority area, measured in square miles, to create an intensity variable.

Figure 3.3 plots the distribution of this variable from 2010 (implementation of the scheme) to 2014 by local authority within the treatment group. The figure demonstrates that City of London and Westminster are the most intensely treated areas, which is expected since, they are either entirely or mostly covered by the scheme. These are followed by Kensington and Chelsea and Tower Hamlets. The outer local authorities such as Hackney, Lambeth and Southwark, which are only slightly covered by the scheme, also share a smaller portion of total docking stations per square mile. The expansions taking place in 2012 and 2013 are clearly visible in Hammersmith and Fulham and Wandsworth. This is also evident in Tower Hamlets which had an increase in the share of docking stations in 2012. It therefore stands to reason that City of London and Westminster should have a larger effect on the accident and casualty rates.

Figure 3. 3 - Distribution of the Intensity variable over time 2010 to 2014



In order to ascertain whether more intensely treated local authorities have a larger effect on the accident rate than those less intensely treated, the areas are assigned into two groups. Those with more than 100 docking stations per square mile namely, City of London, Kensington and Chelsea, Tower Hamlets and Westminster, are assigned to the high intensity group with the remaining areas, Camden, Hackney, Hammersmith and Fulham, Islington, Lambeth, Southwark and Wandsworth, assigned to the low intensity group.

The variable High Intensity (HIntensity) is then created by assigning a value of 1 to all treated local authorities in the high intensity group at the commencement of treatment of each phase and is 0 otherwise. Similarly, the variable Low Intensity (LIntensity) assigns a value of 1 to all treated local authorities in the low intensity group.

There are two resulting subsamples, the first contains monthly data on accident, casualty and vehicle counts from 2000 till 2014 by local authority within Greater

London. The second contains annual data on accident, casualty and vehicle counts and rates (calculated using volume of traffic measured in 1000 vehicle miles) from 2000 till 2017 by local authority within Greater London.

An analysis is initially conducted on monthly data from 2000 till 2014 however, since monthly volume of traffic data is unavailable, annual data is used when estimating the effect of the scheme on accident and casualty rates. Furthermore, since monthly accident and casualty data is unavailable for the years 2015 till 2017 an annual analysis is conducted using data from 2000 to 2017 to include another expansion of the scheme. The analysis using these additional years will not include accidents involving pedal cycles and cars as data is not available for these from 2015 to 2017.

3.5 Empirical Strategy

A count of the total number of accidents per local authority and month can be estimated using the standard difference in difference model –

$$Y_{it} = \alpha + \beta(Scheme_i * Policy_t) + \delta Scheme_i + \theta Policy_t + \gamma X_{it} + \varepsilon_{it} \qquad (1)$$

A sub analysis is conducted to ascertain whether the scheme has a different effect on various components of the dependent variable. Y is therefore a vector of the various accident and casualty variables; a count of the total number of accidents, the total number of fatal, serious and slight accidents, the total number of fatal, serious and slight casualties and the total number of accidents involving pedal cycles, cars and pedestrians for each local authority and month. $Scheme_i$ is a dummy variable given the value of 1 for areas treated by the scheme (local authorities in this case) and 0 otherwise, $Policy_t$ is a dummy variable given a value of 1 for treated times and 0 otherwise and X is a vector of controls.

This model can be generalised to a two-way fixed effects model by –

$$Y_{it} = \alpha + \beta(Scheme_i * Policy_t) + \gamma X_{it} + \delta_i + f(t) + \varepsilon_{it} \quad (2)$$

Where δ_i are area dummy variables, replacing $Scheme_i$, and t is a time variable where a value of 1 is assigned to local authority 1 in month 1 and year 1 etc., a value of 13 is assigned to local authority 1 in month 1 for year 2 etc., a value of 25 is assigned to local authority 1 in month 1 for year 3 and so on where the trend variable includes values 1 to 180 (15 years x 12 months) for each local authority. A fully flexible version of f(t) includes time dummy variables, replacing $Policy_t$.

Since the cycle scheme has more than one 'treatment phase' a single dummy variable is created to replace $Scheme_i*Policy_t$. $Treatment_{it}$ E {0,1}, indicates whether the scheme is present in area i at time t by becoming, and remaining, 1 for the treated local authorities at the commencement of treatment of each phase and is 0 otherwise.

The model now becomes –

$$Y_{it} = \alpha + \beta Treatment_{it} + \gamma X_{it} + \delta_i + f(t) + \varepsilon_{it} \quad (3)$$

With 15 years of monthly data, the fully flexible version of f(t) results in 108 dummies. A tidier specification includes year and month fixed effects, where 'year' represents the accident (calendar) year, accounts for a larger common annual trend within the data and month dummies, where 'month' represents the accident month and January is given a value of 1, February 2 and so on, account for seasonal variation. The local authority fixed effects control for all time invariant unobservable characteristics across groups including treatment status leaving only the within group effect and the time fixed effects control for time specific characteristics including post treatment status (Angrist and Pischke, 2008; Cotti and Tefft, 2011).

The model is therefore now -

$$Y_{it} = \alpha + \beta Treatment_{it} + \gamma X_{it} + \delta_i + \tau_0 year_t + \tau_1 month_t + \varepsilon_{it} \quad (4)$$

Where robust standard errors are reported to account for autocorrelation between pre and post treatment within the same local authority.

The main assumption of the difference in difference model, the Parallel Trend Assumption, requires that, in the absence of treatment, the treatment and control group follow an average outcome which is parallel over time (Abadie, 2005). Or, put differently, it requires that trends of the treatment and control group have the same slope in the absence of treatment.

When several pre-treatment periods are available and by using an extension of the Parallel Trend Assumption, The Common Trends Assumption, as defined by Blundell et al. (2004), researches add group specific polynomial time trends to relax the assumption so that trends may now diverge provided they are linear (Friedberg, 1998 and Wolfers, 2006).

To implement this the monthly trend variable f(t) is interacted with the treatment group dummy (Scheme_i) for each phase of the scheme creating three separate interaction variables for the monthly data till 2014 namely, trend*scheme0, trend*scheme1 and trend*scheme2. An annual trend is interacted with the treatment dummy variables creating four separate interaction variables for the annual data till 2017 namely trend*scheme0, trend*scheme1, trend*scheme2 and trend*scheme3.

The addition of these variables allows for differential trends between the treatment and control groups and therefore act as a robustness check (Mora and Reggio, 2012; Green et al., 2016). If their estimates are small and insignificant then there are no differential trends between treated and control groups and the Parallel Trend Assumption is not violated.

Finally, as an additional robustness check the equation is modified to use an alternative dependent variable, the accident and casualty rates. These, as mentioned, are calculated by dividing the total accident and casualty counts by the total volume of traffic measured in 1 000 miles for each local authority. The resulting rate is measured per million miles travelled. Rates are also calculated for pedal cycle accidents using the volume of pedal cycle traffic measured in 1000 miles and car accidents using the volume of car and taxi traffic measured in 1000 miles. Weighting is not required when using the accident and casualty rates since the variation in area size is captured by using the volume of traffic for each local authority to calculate the rate (Solon et al., 2013).

Since only annual volume of traffic data is available the specification changes from monthly to annual giving –

$$Y_{it} = \alpha + \beta Treatment_{it} + \gamma X_{it} + \delta_i + \tau year_t + \varepsilon_{it} \qquad (5)$$

Although it reduces the number of observations, this paper follows the same intuition as Green et al. (2016), since, if the monthly accident and casualty counts are divided by the annual total volume of traffic figures then there would be measurement error in the dependent variable albeit with seasonality. Rather, year and local authority fixed effects are used.

Other assumptions required for difference in difference estimation are that the intervention should be unrelated to the outcome in that the local authorities in which the scheme was implemented should not have been chosen due to that area's number of accidents. The composition of treated and control groups is stable and that there are no spill over effects; the control group should not receive treatment, for example, due to its proximity to the treated group.

Since none of the criteria used to select treated areas listed in the 'Background and Literature Review' section of this paper are related to traffic accidents within the area, the first assumption is not violated and the geographical composition of local authorities within Greater London have not changed from 2000 to 2017.

The ‘spill over’ assumption is mentioned in the ‘Data’ section of this paper however a spillover variable is also added to this model as a robustness check. If this variable’s estimate is small, then it confirms that the scheme did not spill over into neighbouring local authorities.

3.6 Empirical Results

The results in Tables 3.1, 3.2 and 3.3 were estimated using variations of specification (4) and include year, month and local authority dummy variables to account for an annual trend, seasonal variation and within area variation respectively.[13] Column 1 of Table 3.1 provides the difference in difference estimate of the scheme effect and is significant. It implies that the scheme is associated with 2.0 more total accidents per month in the treated local authorities vs the untreated local authorities where the neighbours to the treated group are added to the control group in this case. In an attempt to relax the parallel trends assumption, the polynomial time trends are added to the specification in the second column. trendScheme0 represents the local authorities in which the scheme was first implemented in July 2010, trendScheme1 those added via the first extension which took place in March 2012 and trendScheme2 those added via the second extension which took place in November 2013. While all the interaction terms are statistically significant, all estimates are small. These estimates suggest that there is a small differential trend between the three treated groups and untreated areas. After adding the polynomial time trends the coefficient of interest now indicates that the scheme is associated with 6.4 more total accidents per month in the treated group vs the control group. All specifications will include these interaction terms going forward unless otherwise indicated.

[13] The dummy variables are included in all specifications unless otherwise stated.

Table 3. 1 - Scheme effect on total, cycle and car accidents (2000-2014)

	(1) Total Accidents	(2) Total Accidents	(3) Total Accidents	(4) Cycle Accidents	(5) Cycle Accidents	(6) Car Accidents	(7) Car Accidents
Treatment	1.984***	6.365***	7.035***	2.884***	3.736***	3.369***	3.879***
	(0.760)	(1.162)	(1.216)	(0.480)	(0.488)	(1.302)	(1.400)
trendScheme0		-0.0590***	-0.0585***	0.0394***	0.0400***	-0.0117	-0.0113
		(0.0127)	(0.0127)	(0.00412)	(0.00412)	(0.0148)	(0.0148)
trendScheme1		0.0291*	0.0283*	-0.00101	-0.00193	0.110***	0.109***
		(0.0150)	(0.0149)	(0.00648)	(0.00647)	(0.0170)	(0.0170)
trendScheme2		0.0338*	0.0315*	0.0658***	0.0629***	0.0999***	0.0982***
		(0.0177)	(0.0176)	(0.00867)	(0.00857)	(0.0190)	(0.0190)
Spillover			1.287**		1.636***		0.980
			(0.653)		(0.215)		(0.937)
_cons	64.27***	63.45***	63.69***	1.070***	1.383***	92.45***	92.63***
	(1.156)	(1.148)	(1.163)	(0.292)	(0.289)	(1.614)	(1.637)
N	5940	5940	5940	5940	5940	5940	5940

Robust standard errors are reported in parentheses where * $p<0.10$, ** $p<0.05$ and *** $p<0.01$

All specifications include year, month and local authority dummy variables.

Each dependant variable is a count of the number of accidents in the respective category.

Columns 4 and 6 decompose this effect into pedal cycle accidents and car accidents. The pedal cycle estimate is significant and reveals that the scheme is associated with 2.9 more pedal cycle accidents per month in the treated area vs the control area. The car estimate is also significant and reveals that the scheme is associated with 3.4 more car accidents per month in the treated group compared with the control group. Once again, the polynomial time trends are mostly significant but small for both specifications.

These results suggest that the increase in the total accidents per month within the treated group compared to the control group is due mostly to an increase in car accidents per month rather than cycle accidents. It is hypothesised that an increase in cycle accidents may be due to the increase in cycle miles driven within the treated areas however, it is unclear whether this increase is proportional to the increase in miles or not. This will be addressed when the pedal cycle accident rate is estimated. However, a change, other than any annual or seasonal variation that would already be taking place, in the number of car miles driven in the treatment group vs the control group is not expected which would imply that the increase in number of car accidents in the treated areas is due to the greater quantity of cyclists on the road. This result may also diminish over time as motorists adapt to the increase in pedal cycles on the road. Unfortunately, this could not be explored further due to the lack of data but would be an area of interest for further analysis.

It seemed unlikely that there would be a large spill over of cyclists using the Santander Cycles into the treated area's neighbouring local authorities. However, a Spillover variable has been added to specification (4) as a robustness check. These results can be found in columns 3, 5 and 7. After the addition of this variable the scheme is associated with 7.0, rather than 6.4, more total accidents per month in the treated group compared to the neighbour and control groups. The neighbours to the treated group are now included in the specification and are therefore no longer included in the control group and the estimate is statistically significant. This is not a large increase.

The difference in difference estimate of the scheme effect on monthly pedal cycle accidents after controlling for spill-over effects is 3.7, rather than 2.9, a substantial increase. Furthermore, the Spillover estimate is significant and large. It implies that the scheme is associated with 1.6 more cycle accidents per month in the neighbouring local authorities vs the treated and control groups. In contrast, the estimate of the scheme effect on the monthly car accidents after controlling for the spill-over effect is 3.9, rather than 3.4, and is significant. Furthermore, the Spillover estimate is small and not significant. These results suggest that the effect of the scheme does spill over into the neighbouring untreated local authorities for pedal cycles but not cars. The polynomial time trends remain small for all the results discussed in columns 3, 5 and 7.

The influence of the scheme on the treated group vs the control group is further decomposed in Table 3.2 which estimates the effect on the fatal, serious and slight accidents per month. The difference in difference estimates of the scheme effect on monthly fatal and serious accidents are small and not significant. In contrast to this the estimate for slight accidents is significant and implies that the scheme is associated with 6.7 more slight accidents per month in the treated group vs the control group. Therefore, the bulk of the total accidents per month are slight rather than fatal or serious. After controlling for the spill-over effect, the scheme is associated with 7.5, rather than 6.7, more slight accidents per month in the treatment group compared to the neighbour and control groups. Furthermore, the Spillover estimate is significant and indicates that the scheme is associated with 1.6 more slight accidents per month in the neighbour group compared to the treatment and control groups. A spill-over effect therefore exists for slight accidents per month.

Table 3. 2 - Scheme effect on fatal, serious and slight accidents (2000-2014)

	(1) Fatal Accidents	(2) Fatal Accidents	(3) Serious Accidents	(4) Serious Accidents	(5) Slight Accidents	(6) Slight Accidents
Treatment	0.0612	0.0343	-0.353	-0.498	6.656***	7.498***
	(0.0610)	(0.0641)	(0.323)	(0.337)	(1.059)	(1.106)
trendScheme0	-0.0000207	-0.0000387	0.00146	0.00136	-0.0604***	-0.0598***
	(0.000636)	(0.000636)	(0.00352)	(0.00352)	(0.0113)	(0.0113)
trendScheme1	0.000564	0.000593	0.0173***	0.0175***	0.0111	0.0102
	(0.000905)	(0.000907)	(0.00541)	(0.00541)	(0.0133)	(0.0132)
trendScheme2	-0.000184	-0.0000915	0.00741	0.00791	0.0266	0.0237
	(0.000953)	(0.000959)	(0.00522)	(0.00524)	(0.0168)	(0.0167)
Spillover		-0.0516		-0.279		1.617***
		(0.0384)		(0.181)		(0.598)
_cons	0.583***	0.573***	9.046***	8.993***	53.82***	54.13***
	(0.0665)	(0.0672)	(0.323)	(0.324)	(1.032)	(1.046)
N	5940	5940	5940	5940	5940	5940

Robust standard errors are reported in parentheses where * $p<0.10$, ** $p<0.05$ and *** $p<0.01$

All specifications include year, month and local authority dummy variables.

Each dependant variable is a count of the number of accidents in the respective category.

Similar findings are observed in Table 3.3 which provides results for the scheme effect on fatal, serious and slight casualties as well as pedestrians. The estimates of the scheme effect on fatal and serious casualties are small and not significant however, using slight casualties per month as the dependent variable results in a significant estimate and implies that the scheme is associated with 5.8 more slight casualties per month in the treated group compared to the control group. When controlling for the spill-over effect, the estimate increases to 6.8, moreover the Spillover estimate is significant. Columns 7 and 8 estimate the effect of the scheme on pedestrian casualties. These results are significant and imply that the scheme is associated with 2.4 more pedestrian casualties per month in the treated group compared to the control group. This decreases to a significant 2.0 in column 8 after controlling for the spill-over effect however, the Spillover estimate is small.

The results of Tables 3.2 and 3.3, imply that while there is an increase in the total number of accidents and, specifically, car accidents per month in the treated group vs the control group, these accidents are only slight. Since a slight accident requires at least one person to be slightly injured where this, in turn, is defined as not requiring medical attention, it can be deduced that the scheme has not had a large adverse impact on people's health or well-being when considering traffic accidents. There is, however, a financial cost involved, no matter the severity of the accident. Furthermore, pedestrians within the treated area are adversely affected by the scheme. This increase in the number of slight accidents and casualties, and pedestrian casualties per month implies that the source of the accidents is most likely due, once again, to the increased volume in pedal cycle traffic.

In the instances where a spill-over effect occurs, this raises the impact of the scheme on the treated groups as well as the number of accidents per month in neighbouring authorities compared to the treatment and control groups. Therefore, the scheme has a larger effect than what was initially hypothesised. However, for the most part, the estimates have only changed slightly with the addition of the Spillover variable and are therefore robust to this check.

Table 3. 3 - Scheme effect on fatal, serious, slight and pedestrian casualties (2000-2014)

	(1) Fatal Accidents	(2) Fatal Accidents	(3) Serious Accidents	(4) Serious Accidents	(5) Slight Accidents	(6) Slight Accidents
Treatment	0.0612 (0.0610)	0.0343 (0.0641)	-0.353 (0.323)	-0.498 (0.337)	6.656*** (1.059)	7.498*** (1.106)
trendScheme0	-0.0000207 (0.000636)	-0.0000387 (0.000636)	0.00146 (0.00352)	0.00136 (0.00352)	-0.0604*** (0.0113)	-0.0598*** (0.0113)
trendScheme1	0.000564 (0.000905)	0.000593 (0.000907)	0.0173*** (0.00541)	0.0175*** (0.00541)	0.0111 (0.0133)	0.0102 (0.0132)
trendScheme2	-0.000184 (0.000953)	-0.0000915 (0.000959)	0.00741 (0.00522)	0.00791 (0.00524)	0.0266 (0.0168)	0.0237 (0.0167)
Spillover		-0.0516 (0.0384)		-0.279 (0.181)		1.617*** (0.598)
_cons	0.583*** (0.0665)	0.573*** (0.0672)	9.046*** (0.323)	8.993*** (0.324)	53.82*** (1.032)	54.13*** (1.046)
N	5940	5940	5940	5940	5940	5940

Robust standard errors are reported in parentheses where * p<0.10, ** p<0.05 and *** p<0.01

All specifications include year, month and local authority dummy variables.

Each dependant variable is a count of the number of accidents in the respective category.

Traffic Volume Control

A further robustness check controls for traffic volume. This is achieved by calculating the accident and casualty rates using volume of traffic measured in 1000 miles then using these as dependent variables. Due to the availability of the volume of traffic data the analysis becomes an annual one where the results in Tables 3.4, 3.5 and 3.6 now use variations of specification (5) so only year and local authority dummy variables are added. The Treatment and Spillover variables are also adjusted when shifting to annual data. Whereas before the Treatment variable, for example, was assigned a value of 1 on July 2010 for treated local authorities it is now given a value of 1 for all treated areas in 2010. Therefore, the variables now become 1 on the year each phase of treatment commences for the respective local authorities. A separate analysis was also conducted where the first year was dropped and the results remained similar.

If the scheme increases the volume of traffic and number of accidents proportionally, the accident rate should not change.[14] If, however, the scheme increases the volume of traffic by less than it increases the number of accidents then the rate will be positive. This can be seen in columns 1 and 2 of Table 3.4. Both these estimates are significant and including the polynomial annual time trends results in an increase of 0.7 total accidents per million miles in the treatment group compared to the control group. The coefficients of the annual trends themselves are small.

Columns 6 and 7 provide the estimates of the scheme effect on the car accident rate in the treated group compared to the control group and in the treated group compared to the group of untreated neighbours and control groups respectively. The estimate is positive and significant for both with a rise from 0.6 to 0.7 when controlling for the spill-over effect, furthermore the Spillover estimate in column 7 is significant. The scheme is therefore associated with 0.6 more car accidents per

[14] This intuition is borrowed from Green et al. (2016) and others studying the presence of traffic externalities (Edlin and Karac-Mandic, 2006; Saito et al., 2010; Huang et al., 2013).

million miles in the treatment group compared to the control group and 0.7 more car accidents per million miles in the treatment group compared to the spillover and control groups.

Figures 3.4 and 3.5 demonstrates that the average number of car miles driven within Greater London decreases over this time period and when decomposing this downward trend into the treated local authorities compared to the untreated local authorities one can see that, while fewer miles are driven overall in the treated group, the trend is mostly downward sloping and similar for both. Previous results suggest there are more car accidents per month in the treated group compared to the control group due to the scheme. Therefore, since the downward trend in car miles driven is similar in both groups and the car accident rate coefficient is positive and large the result remains robust even after controlling for car traffic volume suggesting once again that the increase in the car accident rate in the treated areas is due to the greater quantity of pedal cycles, rather than cars, on the road.[15]

[15] Figures 3.6 and 3.7 demonstrates that the average number of pedal cycle miles driven is increasing in the treatment group compared to the control group.

Figure 3. 4 - Average number of Car Miles driven in Greater London 2000 to 2014

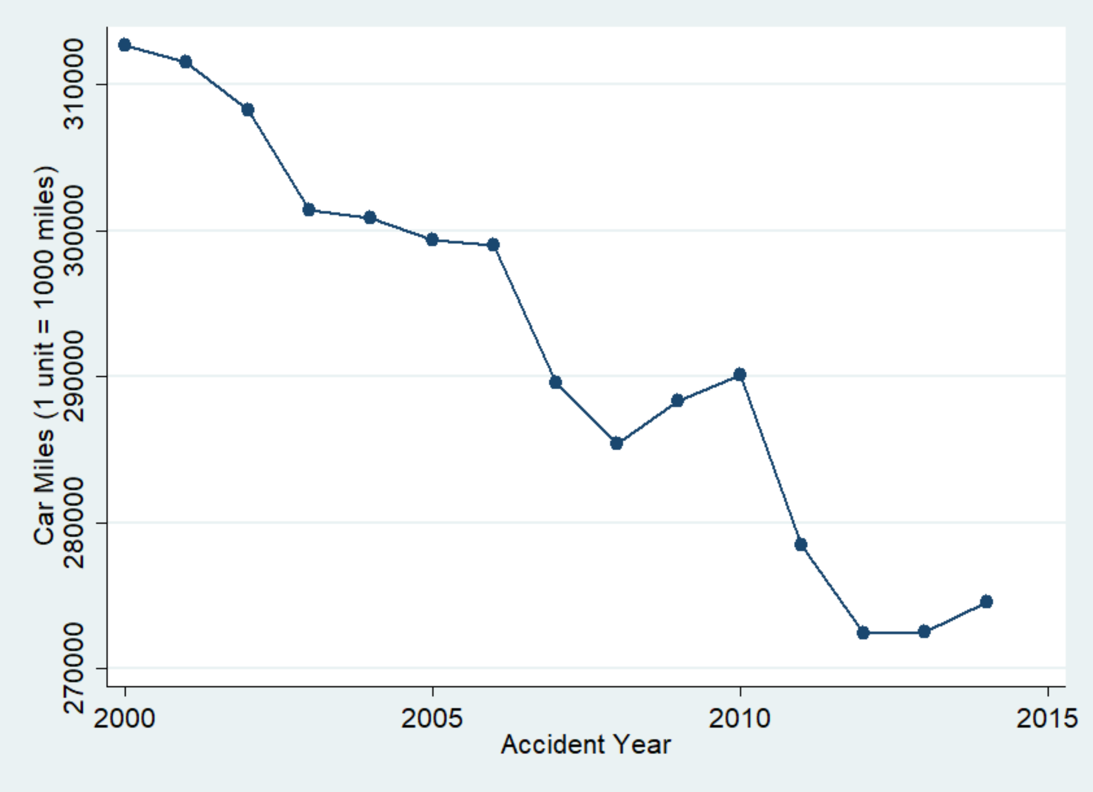


Figure 3. 5 - Average number of Car Miles driven in the Phase 1 Treatment group vs the Control Group in Greater London 2000 to 2014

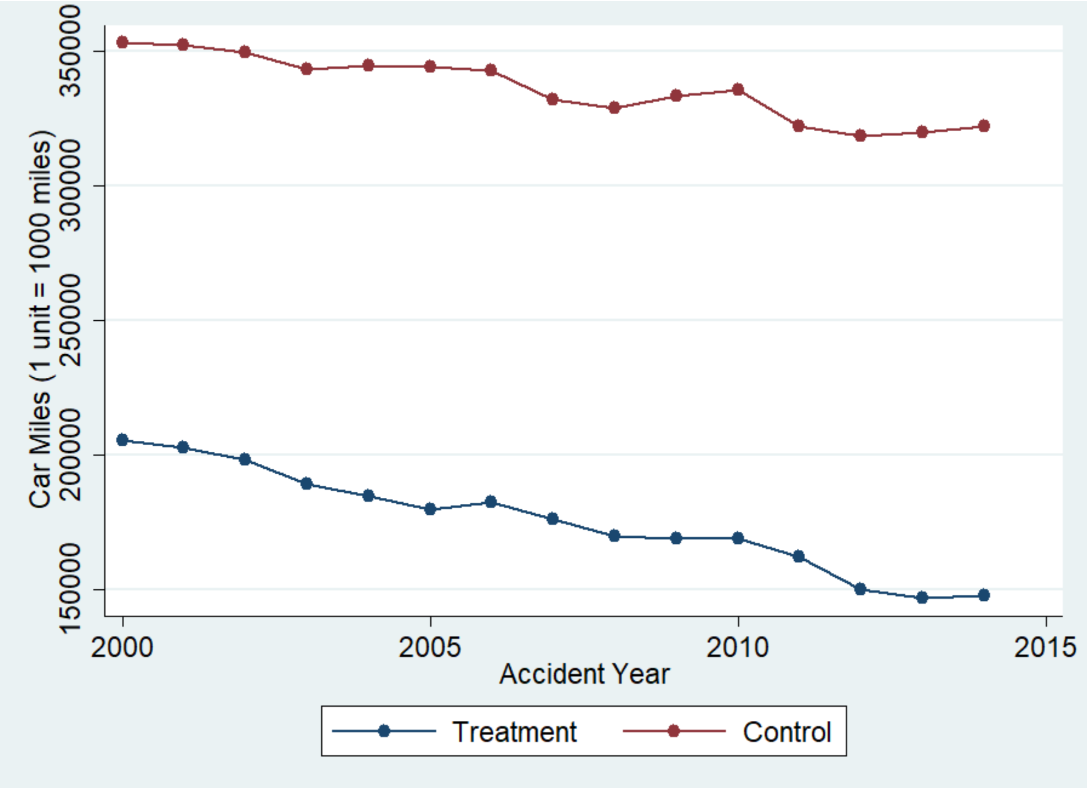
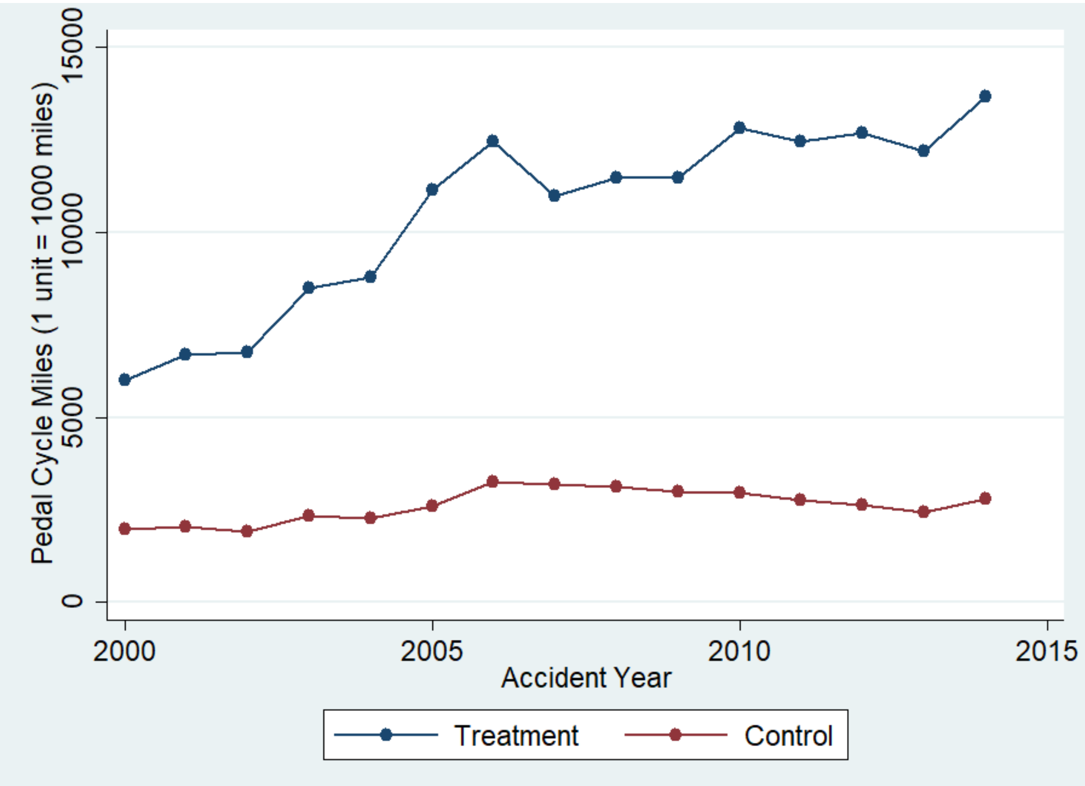


Figure 3. 6 - Average number of Pedal Cycle Miles driven in Greater London 2000 to 2014

Figure 3. 7 - Average number of Pedal Cycle Miles driven in the Phase 1 Treatment group vs the Control Group in Greater London 2000 to 2014



The estimate of the scheme effect on the treated group vs the control group for the cycle accident rate, provided in column 4, is -10.7 and significant. This implies that the scheme is associated with 10.7 fewer pedal cycle accidents per million miles travelled in the treated groups compared to the control group. Using the intuition above, if the scheme increases the volume of pedal cycle traffic by more than it increases the number of pedal cycle accidents then the pedal cycle rate estimate will be negative.

This estimate increases to -10.1 when controlling for the spill-over effect implying that the scheme is associated with 10.1 fewer accidents per million miles in the treatment group compared to the untreated neighbours and the control group. Therefore, when controlling for the spill-over effect the pedal cycle accident rate per million miles travelled falls by less in the treated group. The estimate of the Spillover variable, however, is not significant. Therefore, within the treated local authorities and controlling for spill-over regions, the volume of pedal cycle miles driven is far greater than the 3.7 increase in number of pedal cycle accidents.

The increase in volume of pedal cycle traffic in the treatment group vs the control group is most likely due to the scheme and, given the results of Table 3.4, the increase in pedal cycle accidents is less than the increase in pedal cycle miles driven in the treatment group vs the control group. This analysis does not distinguish between the users of Santander Cycles and other cyclists however, overall, there is a reduction in the pedal cycle accident rate per million miles within the treated group due to the scheme.

The scheme was implemented to promote a switch in mode of transport providing a cheaper alternative for shorter journeys therefore encouraging the use of bicycles and introducing novices to the benefits of using them compared to other modes of transport.

These results lead one to ask why this scheme would decrease the pedal cycle accident rate when it is assumed that those using the Santander Cycles are not wearing protective gear and likely comprise inexperienced as well as unlicensed users who are unfamiliar with the rules of the road. Perhaps drivers of motor vehicles are more careful within the treated area due to the increase of volume of pedal cycle traffic or it could be due to Transport for London's initiative to make certain areas safer for cyclists by implementing cycle superhighways for instance. Another possible reason could be due to an improvement in the traffic externality posed by the cyclists themselves. They may now be more aware of the risk they pose on others.

Table 3. 4 - Scheme effect on total, cycle and car accident rates (2000-2014)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Total	Total	Total	Cycle	Cycle	Car	Car
Treatment	0.382***	0.657***	0.748***	-10.73***	-10.14***	0.612***	0.729***
	(0.0737)	(0.124)	(0.127)	(1.879)	(2.628)	(0.160)	(0.170)
trendScheme0		-0.0413**	-0.0423***	0.660***	0.654***	-0.0358	-0.0370
		(0.0163)	(0.0162)	(0.244)	(0.244)	(0.0231)	(0.0230)
trendScheme1		-0.0231	-0.0253	0.171	0.157	0.0119	0.00918
		(0.0163)	(0.0156)	(0.264)	(0.267)	(0.0221)	(0.0214)
trendScheme2		0.0160	0.0130	0.712***	0.693***	0.0540***	0.0501***
		(0.0169)	(0.0159)	(0.233)	(0.236)	(0.0196)	(0.0188)
Spillover			0.140***		0.904		0.180**
			(0.0470)		(2.322)		(0.0760)
_cons	2.834***	2.781***	2.806***	73.82***	73.98***	4.848***	4.880***
	(0.0967)	(0.0944)	(0.0917)	(3.366)	(3.469)	(0.173)	(0.170)
N	495	495	495	495	495	495	495

Robust standard errors are reported in parentheses where * $p<0.10$, ** $p<0.05$ and *** $p<0.01$

All specifications include year and local authority dummy variables.

Each dependant variable is an accident rate per million vehicle miles for each respective category.

Table 3.5 further decomposes the accident rate into fatal, serious and slight. Similar results are obtained as in Table 3.2. The difference in difference estimates on fatal and serious accident rates are very small and either not significant or only significant at the 10 percent level, becoming insignificant when controlling for the spill-over effect. The estimate of scheme effect on the slight accident rate is significant and implies that the scheme is associated with 0.6 more slight accidents per million miles in the treated group vs the control group. After controlling for the spill-over effect, the scheme is associated with 0.7 more slight accidents per million miles in the treatment group compared to the neighbour and control groups. Furthermore, the Spillover estimate is significant and implies that the scheme is associated with 0.2 more slight accidents per million miles in the neighbour group compared to the treatment and control groups. Once again, the polynomial annual time trends are very small.

Similarly, the results of Table 3.6, which provide the scheme effect on fatal, serious and slight casualty and pedestrian rates per million miles, show significant results for the slight casualty and pedestrian rates only. The estimates of scheme effect on fatal and serious casualty rates are very small and either not significant or only significant at the 10 percent level, becoming insignificant when controlling for the spill-over effect. The slight casualty rate estimate is significant and implies that the scheme is associated with 0.7 more slight casualties per million miles in the treated group compared to the control group. When controlling for the spill-over effect, the estimate only increases to 0.8 and the Spillover estimate of 0.2 is significant. Columns 7 and 8 estimate the scheme effect on the pedestrian casualty rate measured in million miles. These results are significant and imply that the scheme is associated with 0.2 more pedestrian casualties per million miles in the treated group compared to the control group. This only decreases marginally in column 8 after controlling for the spill-over effect, moreover, the Spillover estimate is small and not significant. After controlling for traffic volume, the results remain similar to those from Tables 3.2 and 3.3.

Table 3. 5 - Scheme effect on fatal, serious and slight accident rates (2000-2014)

	(1) Fatal	(2) Fatal	(3) Serious	(4) Serious	(5) Slight	(6) Slight
Treatment	0.00236	0.000866	0.0534*	0.0458	0.601***	0.701***
	(0.00293)	(0.00310)	(0.0272)	(0.0286)	(0.109)	(0.111)
trendScheme0	-0.000146	-0.000131	-0.00546	-0.00538	-0.0357***	-0.0368***
	(0.000356)	(0.000358)	(0.00414)	(0.00416)	(0.0136)	(0.0135)
trendScheme1	-0.0000519	-0.0000168	-0.000709	-0.000530	-0.0224	-0.0247*
	(0.000419)	(0.000422)	(0.00327)	(0.00332)	(0.0142)	(0.0133)
trendScheme2	-0.0000124	0.0000370	0.00299	0.00324	0.0130	0.00968
	(0.000480)	(0.000494)	(0.00234)	(0.00239)	(0.0163)	(0.0152)
Spillover		-0.00229*		-0.0117		0.154***
		(0.00137)		(0.0108)		(0.0436)
_cons	0.0260***	0.0256***	0.367***	0.365***	2.388***	2.415***
	(0.00269)	(0.00275)	(0.0191)	(0.0192)	(0.0821)	(0.0786)
N	495	495	495	495	495	495

Robust standard errors are reported in parentheses where * $p<0.10$, ** $p<0.05$ and *** $p<0.01$

All specifications include year and local authority dummy variables.

Each dependant variable is an accident rate per million vehicle miles for each respective category.

Table 3. 6 - Scheme effect on fatal, serious, slight and pedestrian casualty rates (2000-2014)

	(1) Fatal	(2) Fatal	(3) Serious	(4) Serious	(5) Slight	(6) Slight	(7) Pedestrian	(8) Pedestrian
Treatment	0.00183	0.000200	0.0518*	0.0411	0.650***	0.768***	0.172***	0.167***
	(0.00308)	(0.00326)	(0.0285)	(0.0300)	(0.126)	(0.130)	(0.0376)	(0.0386)
trendScheme0	-0.000104	-0.0000873	-0.00526	-0.00515	-0.0352**	-0.0363**	-0.0251***	-0.0250***
	(0.000377)	(0.000379)	(0.00445)	(0.00446)	(0.0158)	(0.0157)	(0.00488)	(0.00490)
trendScheme1	0.0000392	0.0000777	-0.000561	-0.000308	-0.0154	-0.0181	-0.0130**	-0.0129**
	(0.000427)	(0.000430)	(0.00341)	(0.00347)	(0.0169)	(0.0158)	(0.00505)	(0.00509)
trendScheme2	0.0000217	0.0000758	0.00393	0.00429	0.0210	0.0171	-0.00640	-0.00625
	(0.000458)	(0.000473)	(0.00254)	(0.00261)	(0.0168)	(0.0156)	(0.00490)	(0.00498)
Spillover		-0.00251*		-0.0165		0.181***		-0.00699
		(0.00147)		(0.0119)		(0.0550)		(0.0126)
_cons	0.0276***	0.0271***	0.413***	0.410***	3.117***	3.149***	0.552***	0.550***
	(0.00300)	(0.00306)	(0.0214)	(0.0215)	(0.114)	(0.110)	(0.0263)	(0.0263)
N	495	495	495	495	495	495	495	495

Robust standard errors are reported in parentheses where * p<0.10, ** p<0.05 and *** p<0.01

All specifications include year and local authority dummy variables.

Each dependant variable is a casualty rate per million vehicle miles for each respective category.

Intensity of Treatment

Figures 3.8, 3.9 and 3.10 depict the results estimated using variations of specification (5) where the Treatment variable has been replaced by the Intensity variable and include year and local authority dummy variables. Given that these estimates are very small a 90% confidence interval is used to demonstrate their significance. However, going forward all other results will be presented using a 95% confidence interval. Since the Treatment and Intensity variables are of a different scale, they cannot be compared however, the results indicate that the estimates complement those of Tables 3.4, 3.5 and 3.6 since they all remain significant and retain their signs.[16] The estimates using the serious accident and casualty rates are now significant but remain positive. Furthermore, the three sets of analysis from Figure 3.8 have an R-squared of 0.9546, 0.8060 and 0.9354 for total, cycle and car accident rates (excluding the spillover variable) respectively, signifying that the model fits the data well. The results therefore remain robust when using the intensity of the Santander Cycle Scheme to measure the effect on accident and casualty rates.

[16] As the estimates are quite small it may not be clear from the graph however, the intensity effect on the cycle and car accident rates are significant at the 1% level. Table A1, confirming these results, can be found in the Appendix.

All specifications include year and local authority dummy variables. N = 495.

Figure 3. 8 - Intensity on Total, Cycle and Car Accident Rates per million vehicle miles

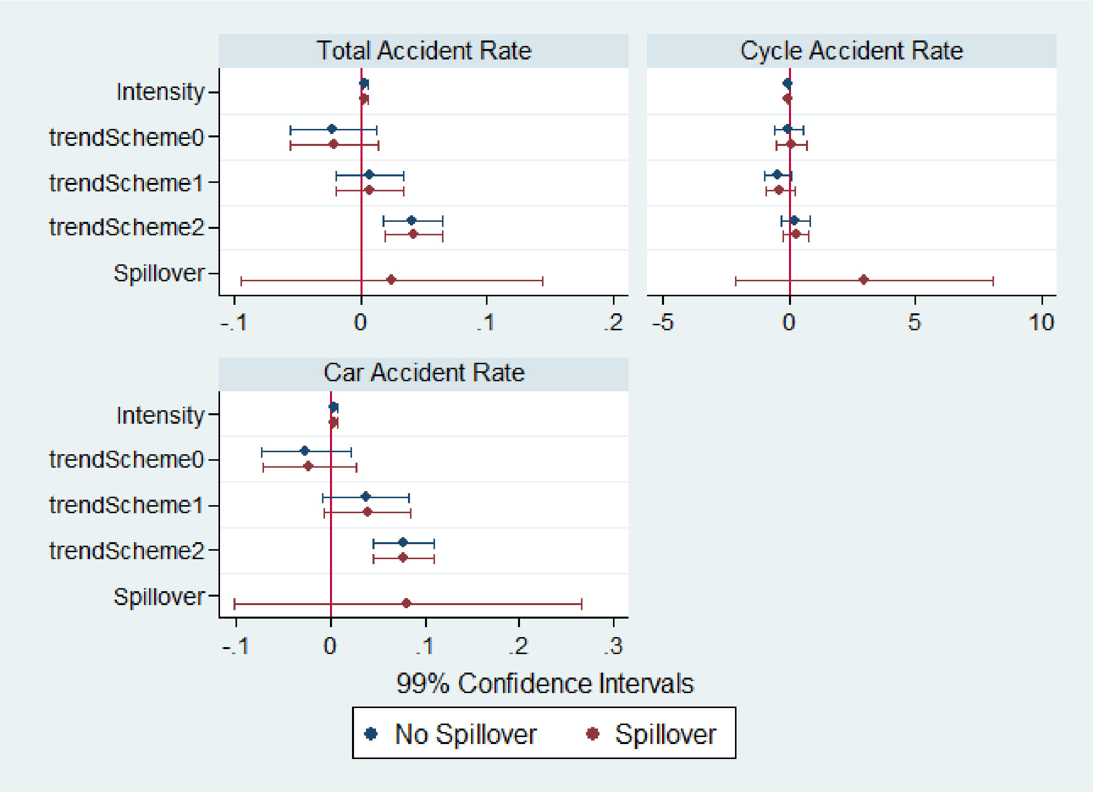


Figure 3. 9 - Intensity on Fatal, Serious and Slight Accident Rates per million vehicle miles

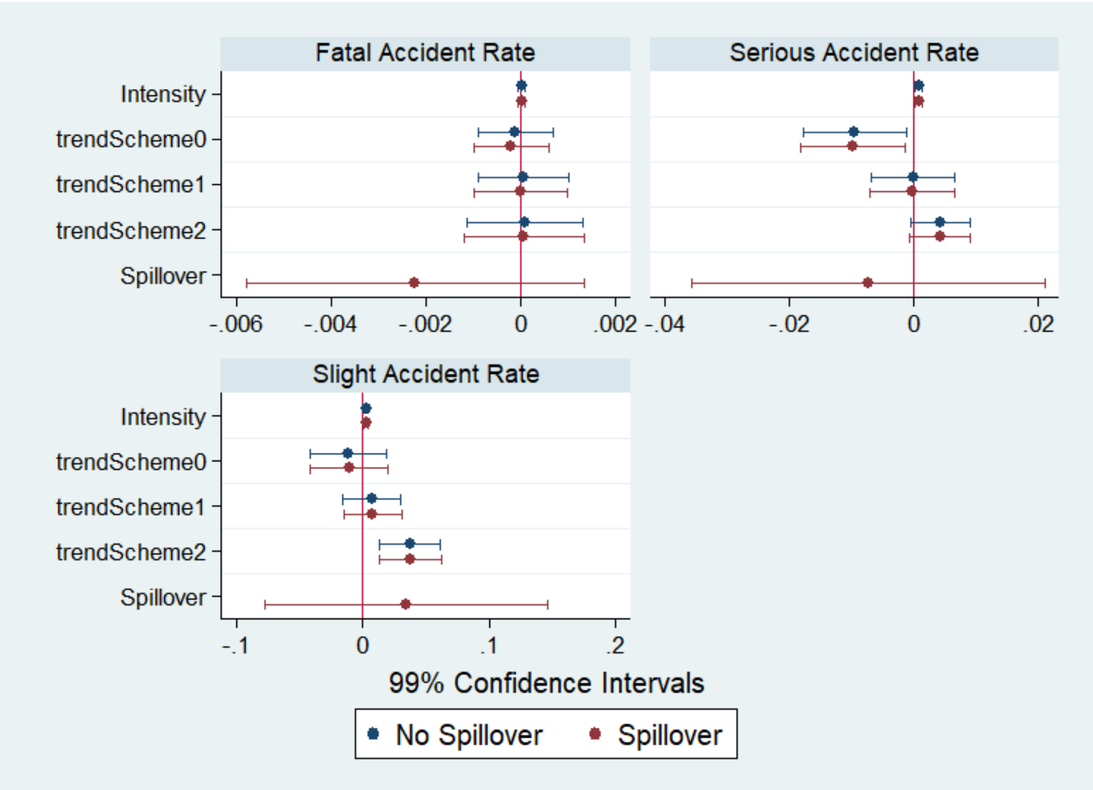
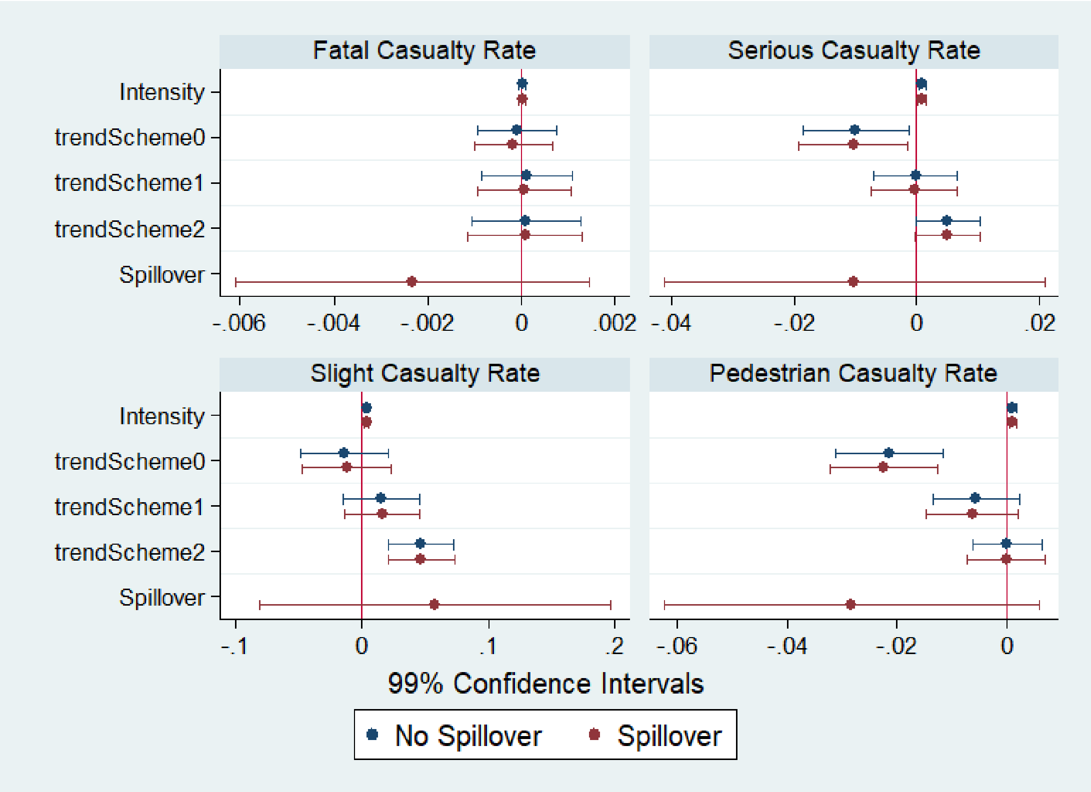


Figure 3. 10 - Intensity on Fatal, Serious, Slight and Pedestrian Casualty Rates per million vehicle miles



Figures 3.11, 3.12 and 3.13 depict the results estimated using variations of specification (5) however, now the Treatment variable has been replaced by the High and Low Intensity variables. Again, the results remain significant and retain their signs. In addition to this, the High Intensity estimate is larger than the Low Intensity estimate for all accident rates. Table 3.1 demonstrates that the scheme leads to an increase in the number of cycle accidents per month. Since, from Figure 3.11, the Low Intensity estimate is more negative than the High Intensity estimate, this implies that the volume of pedal cycle traffic is increased by more than the increase in the number of pedal cycle accidents in the low intensity group compared to the high. So, taking this into account, if the high intensity group has a larger volume of pedal cycle traffic these results imply that there are more pedal cycle accidents in the low intensity group compared to the high. Therefore, the 'negative', in that it increases accidents, effect of the scheme is larger in the high intensity group for the car accident rate and the 'positive', in that it decreases accidents, effect of the scheme is larger in the high intensity group for the cycle accident rate.

All specifications include year and local authority dummy variables. N = 495.

Figure 3. 11 - High and Low Intensity on Total, Cycle and Car Accident Rates per million vehicle miles

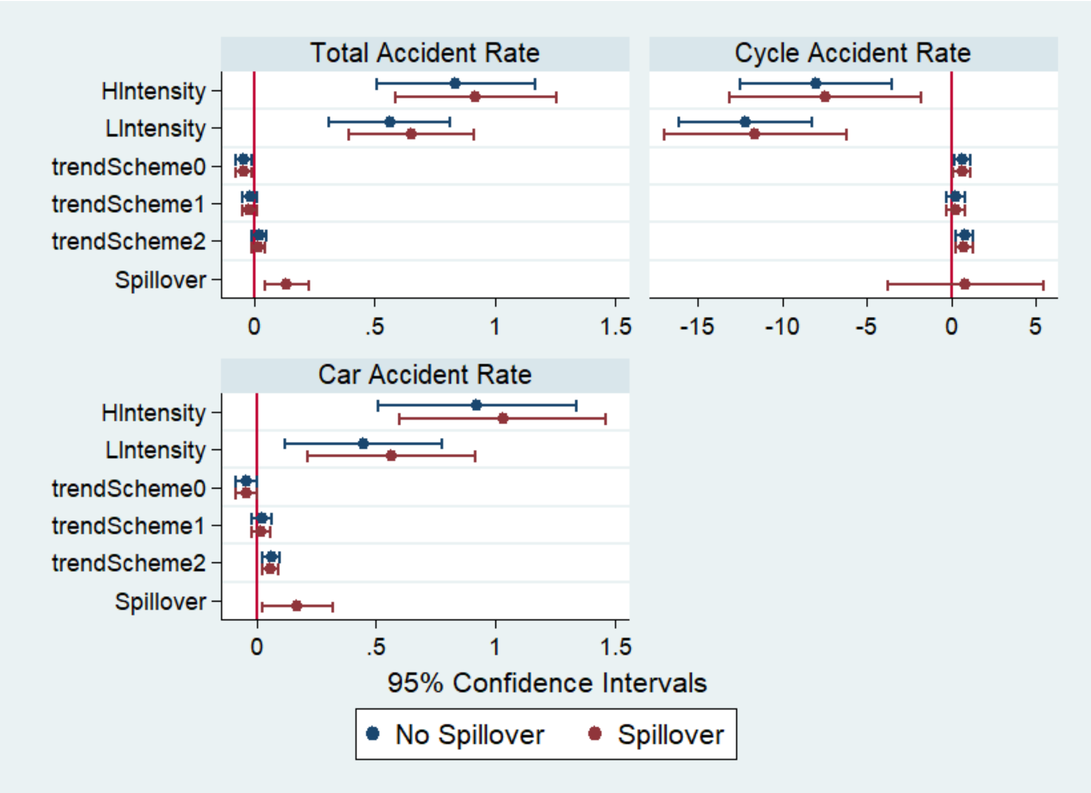
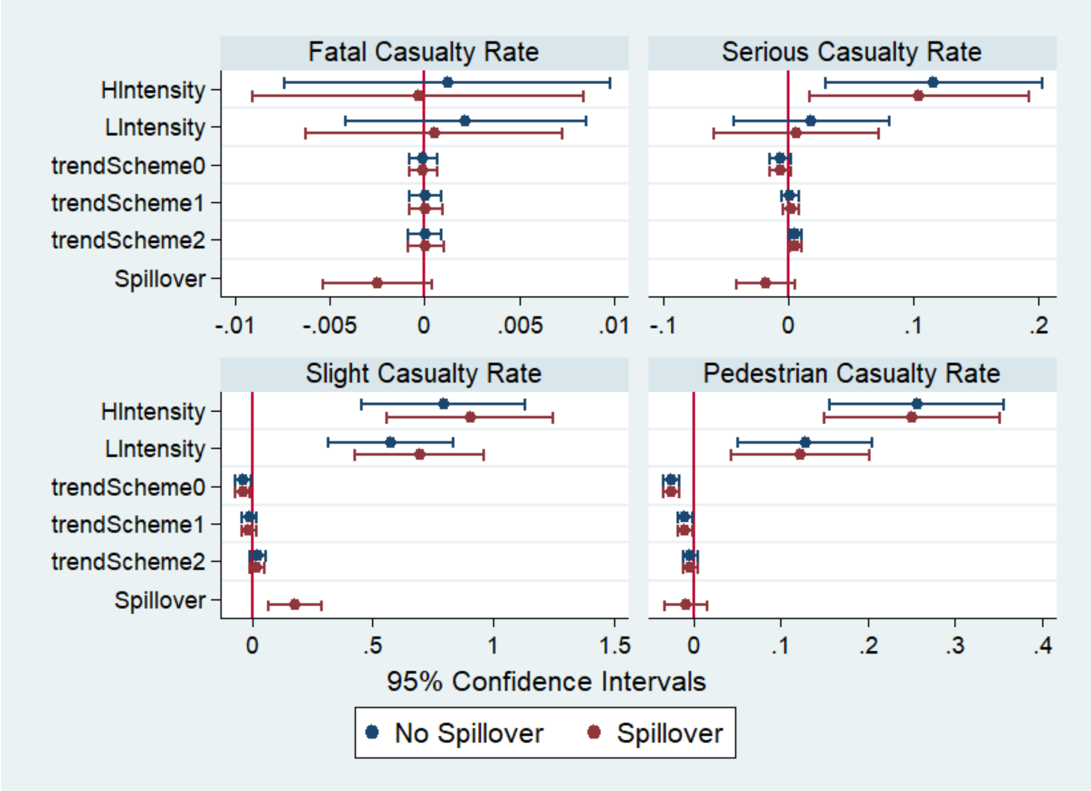


Figure 3. 12 - High and Low Intensity on Fatal, Serious and Slight Accident Rates per million vehicle miles

Figure 3. 13 - High and Low Intensity on Fatal, Serious, Slight and Pedestrian Casualty Rates per million vehicle miles



Comparing the estimates to those of Table 3.4, 3.5 and 3.6, the difference in difference estimate of the high intensity group are also larger than the estimates of the treatment group as a whole. Moreover, the Spillover estimates remain similar and all polynomial time trends are very small. Comparing the results to those of Tables 3.5 and 3.6, while the treatment group effect estimates on serious accident and casualty rates are only significant at the 10 percent level, becoming insignificant when controlling for the spill-over effect, now the high intensity group effect on these rates is significant and positive implying that the high intensity group has a significant effect on severe accident and casualty rates. This group has a larger share of docking stations per square mile therefore more cycles are available. This will lead to a greater use of the scheme in these areas and due to the larger intrinsic risk of serious injury with pedal cycles, will lead to more severe accidents[17].

[17] This intuition is borrowed from Green et al. (2016).

Overall, the group of treated local authorities with more docking stations per square mile have a larger effect on the accident and casualty rates than those areas with fewer docking stations per square mile confirming that the results are due to the Santander Cycle Scheme.

Additional Robustness Checks and Controls

The Cycle Superhighways, London Congestion Charge and London Summer Olympics may all affect road accidents and either took place or, are implemented, in parts, or all, of some areas treated by the Santander Cycle Scheme. It is therefore necessary to control for these to ensure the scheme results remain robust.

One policy is that of the cycle superhighways. There are a number of cycle routes within Greater London, identified by their respective signs. These cycleways were rebranded in 2019 and include the now defunct cycle superhighways and quietways. The quietways were announced in 2015 with the purpose of providing routes for less confident cyclists and those who want to travel at a calmer pace. The superhighways, on the other hand, were developed for commuters and experienced cyclists by providing more direct routes between outer and central London. There are a total of 7 routes, announced in 2008 with the first launched in 2010.

Since the quietways are out of the way pathways for a specific type of user there is no need to include them in the analysis. However, a Cycle Superhighway variable (CycleSuper) is created by assigning a value of 1 to treated local authorities at the year of route commencement and 0 otherwise for all 7 superhighways.

A second policy is the London Congestion Charge. Most motor vehicles driving through the Congestion Charge Zone from Monday to Friday between 7am and 6pm are charged a daily fee of £11.50. The charge does not operate on public holidays or from 25/12 to 01/01 (Christmas period). The policy came into effect on 17 February 2003 with only one extension, the Western Extension, launched in February 2007 and subsequently withdrawn in January 2011. The zone is clearly marked and monitored by cameras to ensure it is paid.

First, a variable is created to incorporate the charging days and times. $CCTime_{it}$ is a count of all car accidents that took place from Monday to Friday, 7am to 6pm within each year and local authority excluding public holidays and the Christmas period. Then, a variable is created to incorporate the Congestion Charge Zone.

Since it is being used as a control, $CCZone_{it}$ E {0, 0.25, 0.5, 0.75, 1} where, a local authority entirely covered by the charge (City of London only) is represented by 1, partly covered (Westminster) by 0.5, slightly covered (Camden, Islington, Lambeth and Southwark) by 0.25, and not covered by 0. Hackney and Tower Hamlets are excluded since only a tiny portion of each local authority is covered. The variable becomes active from 2003. In 2007, the Western Extension almost entirely covered Westminster which is re-assigned a value of 0.75 and partly covered Kensington and Chelsea which is assigned a value of 0.5. These become 0.5 and 0 respectively again from 2011 due to the withdrawal of the extension. Finally, the congestion charge variable, $CCControl_{it}$, is created by taking, $CCTime_{it}$*$CCZone_{it}$.

Large events such as the Summer Olympic Games, which took place in London from the 23^{rd} July till the 13^{th} August 2012, also need to be controlled for to ensure these results remain robust. While it may be argued that the event may have had an effect on all local authorities, assigning them all a value of 1 for 2012 will lead to multicollinearity due to the year fixed effects. Therefore, only the six host boroughs of Barking and Dagenham, Greenwich, Hackney, Newham, Tower Hamlets and Waltham Forest are used to represent the event. This is in line with the boroughs used by the Department for Transport when providing statistics on transport data relating to the Olympic Games (Department for Transport, 2012). An Olympics variable is therefore created by assigning a value of 1 to host local authorities in 2012 and 0 otherwise.

All specifications include year and local authority dummy variables. N = 495.

Figure 3. 14 - Total, Cycle and Car Accident Rates per million vehicle miles including controls

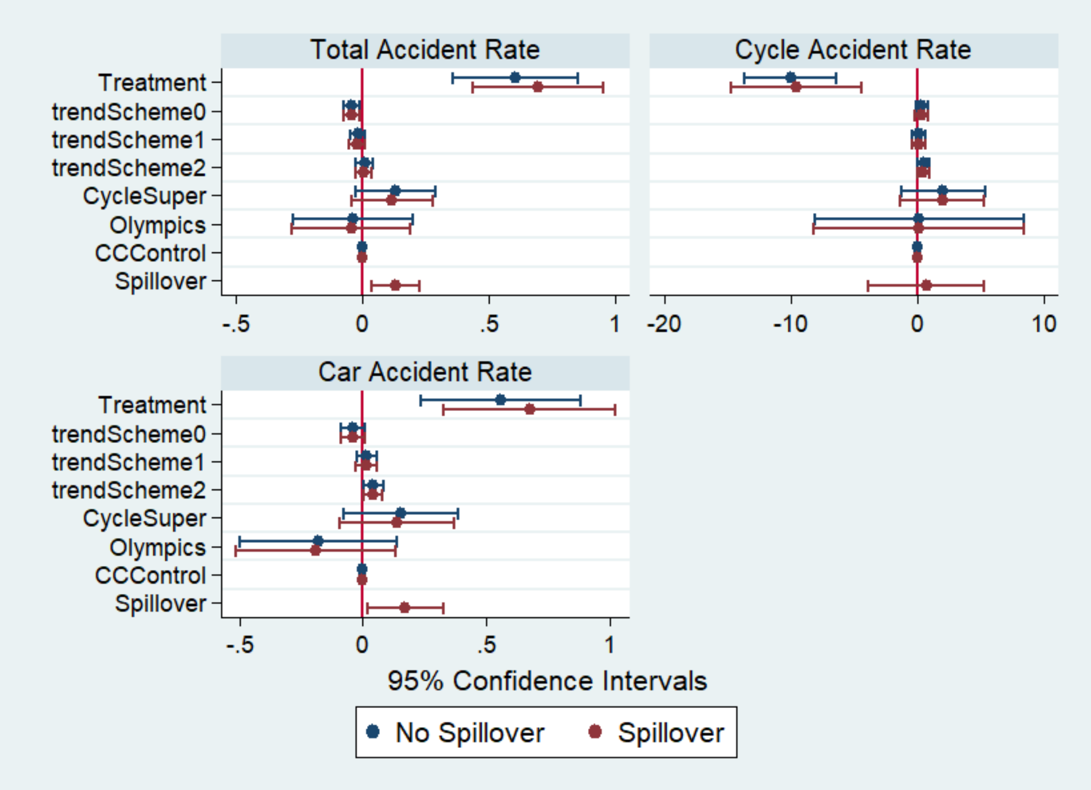
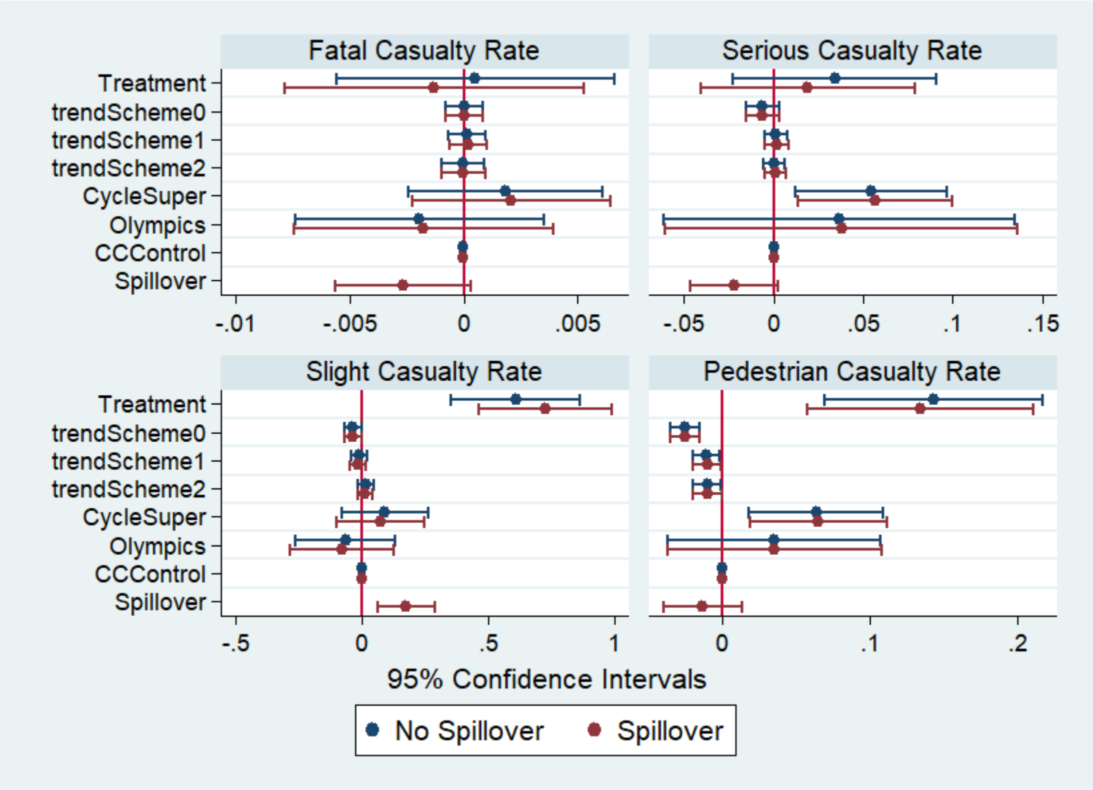


Figure 3. 15 - Fatal, Serious and Slight Accident Rates per million vehicle miles including controls

Figure 3. 16 - Fatal, Serious, Slight and Pedestrian Casualty Rates per million vehicle miles including controls



Figures 3.14, 3.15 and 3.16 depict the results estimated using variations of specification (5), including controls for the cycle superhighways, London Congestion Charge and London Summer Olympics and include year and local authority dummy variables. Comparing the results to those of Tables 3.4. 3.5 and 3.6, the estimates of the scheme effect do not lose significance and retain the same signs. All positive Treatment estimates decrease by a small amount and the negative estimates of the scheme effect on the cycle accident rate become slightly less negative. Moreover, the Spillover estimates remain similar and all polynomial time trends are very small.

The Cycle Superhighway estimates are all small and positive, implying that the cycle superhighway is associated with more accidents and casualties per million miles in the local authorities containing cycle superhighways compared to those that don't, however, none of the estimates are significant. The superhighway effect on the cycle accident rate stands out in that it is also positive. This could be due to the fact that, unlike the Santander Cycle Scheme, the cycle superhighways are limited to bicycles only, excluding other vehicles, therefore leading to a larger increase in the number of cycle accidents compared to the increase in cycle volume of traffic within the cycle routes. The estimates of the cycle superhighway effect on the severe accident and casualty and pedestrian casualty rates are significant which could be due, once again, to the larger intrinsic risk of serious injury with pedal cycles leading to more severe accidents. Estimations using specification (5) including the cycle superhighway control only (omitting the Olympics and Congestion Charge controls) produced very similar results.

The estimate of the Congestion Charge effect on the cycle accident rate is small, positive and significant. This implies that the Congestion Charge is associated with more cycle accidents per million miles in the local authorities covered by the charge compared to those that are not. This is in keeping with the literature which suggests that an increase in pedal cycle use due to the Congestion Charge will lead to an increase in cycle accidents (Li et al., 2012). All other Congestion Charge and Olympics estimates are small and not significant. The scheme effect results therefore remain robust to the addition of these three controls, all of which include areas that overlap with parts of the scheme treated areas.

In order to evaluate whether additional years have an impact on the effect of the scheme, annual data from 2000 till 2017 is analysed using specification (5). Once again, year and local authority dummy variables are added as well as the polynomial annual time trends. A third extension took place in November 2015 so, the interaction term trend*Scheme3 is added where Scheme3 is 1 for local authorities treated by the third extension of the scheme and 0 otherwise. Due to lack of pedal cycle and car accident data from 2015 to 2017 these variables are removed from the analysis going forward. All estimates reported here are rounded up. Figure 3.17 depicts the estimates using total, fatal, serious and slight accident rates as dependent variables and Figure 3.18 depicts the estimates using fatal, serious, slight and pedestrian casualty rates as dependent variables. The rates are calculated by dividing the annual number of accidents and casualties within each local authority by the total volume of traffic measured in 1000 miles for the same local authority.

All specifications include year and local authority dummy variables. N = 594.

Figure 3. 17 - Total, Fatal, Serious and Slight Accident Rates per million vehicle miles 2000 - 2017

Figure 3. 18 - Fatal, Serious, Slight and Pedestrian Casualty Rates per million vehicle miles 2000-2017

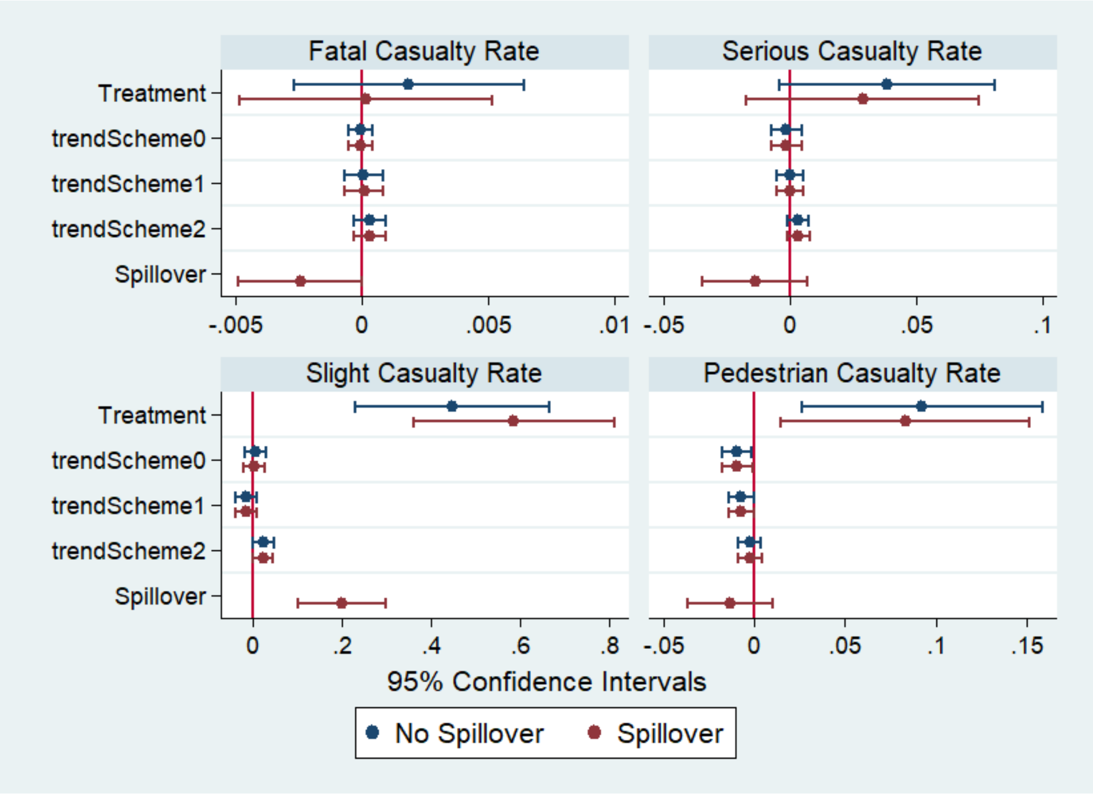


Figure 3.17 presents the estimate of scheme influence in the treated group compared to the control group which is significant and implies that the scheme is associated with 0.5 more total accident per million miles in the treatment group compared to the control group which is lower than the estimate produced using data till 2014 of 0.7. When controlling for the spill-over effect the estimate increases to 0.6 and is significant. Furthermore, the Spillover variable estimate is significant and implies that the scheme is associated with 0.2 more total accidents per million miles in the untreated neighbouring local authorities than the treated group and control group. These results demonstrate that after three years the effect of the scheme, including the third extension and therefore larger treatment group, has become smaller. Similar to the data till 2014, the fatal and serious accident rate estimates are not significant and very small. The estimate of the influence of the scheme on the slight accident rate is significant and implies that the scheme is associated with 0.5 more slight accidents per million miles in the treatment group compared to the control group. The estimate was 0.6 using data till 2014 and therefore, once again, the effect of the scheme has become smaller with the additional years and extension. After controlling for the spill-over effect the estimate becomes 0.6 and is significant. Furthermore, the Spillover variable estimate is significant and implies that the scheme is associated with 0.2 more slight accidents per million miles in the untreated neighbouring local authorities compared to the treatment and control groups. All of the polynomial annual time trends are small.

Similar to the data till 2014, the fatal and serious casualty rate estimates provided in Figure 3.18 are not significant. The estimate of scheme effect on the slight casualty rate is significant and implies that the scheme is associated with 0.5 more slight accidents per million vehicle miles in the treated group vs the control group. When controlling for the spill-over effect this estimate, which is significant, becomes 0.7. The Spillover variable estimate is also significant and implies that the scheme is associated with a 0.2 increase in slight accidents per million miles in the untreated neighbouring group vs the treatment and control groups. This is a large spill-over effect of slight accidents into the neighbouring local authorities. The difference in difference estimate of the scheme effect on the treatment groups vs the control group is 0.7 using data till 2014 so, once again, the effect of the scheme diminishes with the inclusion of additional years and a third extension. Finally, the pedestrian casualty rate estimate is significant and implies that the scheme is associated with 0.1 more pedestrian casualties per million miles in the treatment group vs the control group. Not only is the estimate of the Spillover variable not significant the difference in difference estimate of the influence of the scheme on the treatment group vs the control group changes very little when controlling for spill-over effects implying that there is no spill-over effect of pedestrian casualties into the neighbouring groups. The estimate is 0.2 using data till 2014 so the effect of the scheme has become smaller for pedestrian casualties too. The polynomial annual time trend estimates are very small for all specifications.

3.7 Conclusion

This study has been conducted to determine the effect the Santander Cycle Hire Scheme has had on road accidents and casualties using data from 2000 till 2014 and 2017. By decomposing the dependent variable, a count of the total number of accidents, into pedal cycle and car accidents, fatal, serious and slight accidents and casualties and pedestrians, it becomes possible to analyse how the scheme affects these differently. It is hypothesised that the scheme leads to an increase in the volume of pedal cycle traffic which, in turn, may lead to an adverse scheme effect for others.

The difference in difference estimates of scheme effect on the treatment group compared to the control group are positive and significant for all accident counts except, fatal and serious accidents and casualties where the estimates are not significant. Therefore, the number of accidents and casualties per month increased in the treatment group compared to the control group due to the scheme. This is more so for cars than pedal cycles, however, when controlling for the spill-over effect the estimate increased bringing the pedal cycle accident count and car accident count estimate closer in value. The estimates of scheme effect on slight accidents and casualties and pedestrians are also significant and positive implying that the scheme causes an increase in slight accident and casualties and pedestrian casualties per month in the treatment group compared to the control group. Therefore, initial results demonstrate an adverse effect on all road accidents and casualties per month in the treatment group compared to the control group in that the scheme raises the accident and casualty counts however, this effect is only via slight accidents and casualties.

Since it is hypothesised that this adverse effect may be due to an increase in the number of pedal cycles on the road it is necessary to control for traffic volume. This is done by using the annual accident and casualty rates per million miles as dependent variables. The car accident rate and pedestrian casualty rate remain significant and positive whereas the pedal cycle accident rate is now significant and negative.

Since the pedal cycle traffic volume increases and does so by more in the treatment group than the control group whereas the car volume of traffic decreases following the same trend in both the treatment and control group, it can be deduced that the scheme benefits cyclists by decreasing the pedal cycle accident rate per million miles but does not benefit motorists and pedestrians. However, the scheme only significantly affects the slight accident and casualty rates therefore this adverse effect on motorists and pedestrians is only through slight accidents. Moreover, these results remain robust to a spill-over effect control, London Congestion Charge and London Summer Olympics controls.

Given that some of the treated local authorities are only partly covered by the scheme, it is necessary to control for the intensity of the scheme. This is done by dividing the number of docking stations in a given year and local authority by the area measured in square miles of that same local authority. The resulting Intensity estimates complement those of the Treatment estimates. The treated local authorities are then assigned to two groups, high and low intensity, to ascertain whether more intensely treated areas have a larger effect than those less intensely treated. Overall, the group of treated local authorities with more docking stations per square mile have a larger effect on the accident and casualty rates than those areas with fewer docking stations per square mile confirming that the results are due to the Santander Cycle Scheme.

Adding three additional years to the analysis and taking the third extension into account, reveals that the scheme effect on the treatment group compared to the control group diminishes over time. Since the third extension only covers part of Newham but the entire local authority was added to the treatment group, these results are somewhat limited however, being a robustness check itself and due to a lack of pedal cycle and car accident data post 2014, a control for the intensity of the scheme is not conducted.

These results could be due to the cyclists' greater awareness of the risk they may pose on others or could also be due to a behavioural shift in motorists who may be taking extra care within the treated areas due to the increase of cyclists on the road. Due to a lack of pedal cycle and car accident data post 2014, this analysis is unable to explore whether these results may diminish over time as behaviour changes, limiting the study. It may also be due to Transport for London's initiative to make certain areas safer for cyclists by implementing cycle superhighways for example. Controlling for this policy produces similar estimates therefore confirming that the results are not due to the cycle superhighways but rather the Santander Cycle Scheme.

While the car and pedestrian accident and casualty rates have increased marginally, this is only through slight accidents. Furthermore, the Santander Cycle Scheme has led to a large decrease in the pedal cycle accident rate therefore, making roads safer for cyclists.

Chapter 4

The Impact of Terror Incidents on Road Accidents in Great Britain

4.1 Introduction

A total of 173 terror incidents have taken place in Great Britain from 2000 till 2017.[18] The largest of which being the 2005 London bombings taking place on July 7th. Four homemade bombs were detonated, three of which were on the London Underground system near Aldgate, Edgware Road and Russell Square and one on a bus in Tavistock Square. There were 56 fatalities and over 700 people were injured making it one of Britain's deadliest attacks. More recently, an incident took place in Westminster on the 22nd March 2017 followed closely by an incident in London Bridge on 3rd June 2017. On the 22nd of March, a car was driven into pedestrians on Westminster Bridge killing 6 people and injuring 50 people. The car was then driven through the fence of the Palace where the attacker stabbed and killed an unarmed police officer before being fatally shot by an armed police officer. On the 3rd of June, on London Bridge, a van was driven into pedestrians before crashing. The attackers then proceeded to stab people within the Borough Market area before being fatally shot by Metropolitan police officers. There were 11 deaths and 48 injuries, 21 of which were critical. On the 22nd of May 2017 a suicide bombing killed 23 people and injured over 100 at an Ariana concert held at the Manchester Arena.

Figure 4.1 demonstrates that there has been an upward trend in the number of incidents per year since 2010 with a maximum of 32 incidents in 2017. The large increase in the occurrence of incidents supports the need for this paper which attempts to determine the effect of incidents on road accidents in Great Britain over the period from 2000 to 2017 hypothesising that the effects will be through either a change in the quantity or quality of driving or both. Looking at Figure 4.2, the mean

[18] Terror incidents will be referred to simply as incidents or attacks for the remainder of the paper.

of total accidents follows a constant downward trend within this time period, with a solitary increase from 2013 to 2014.

Figure 4. 1 - Total Incidents 2000 to 2017

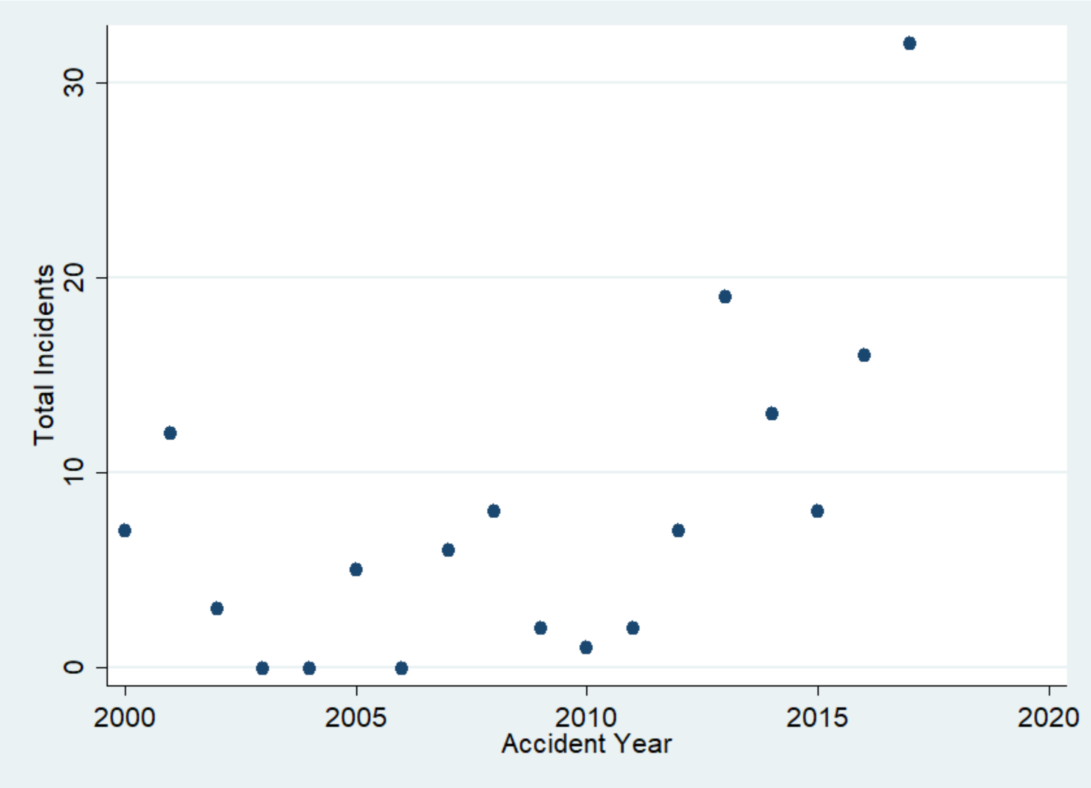
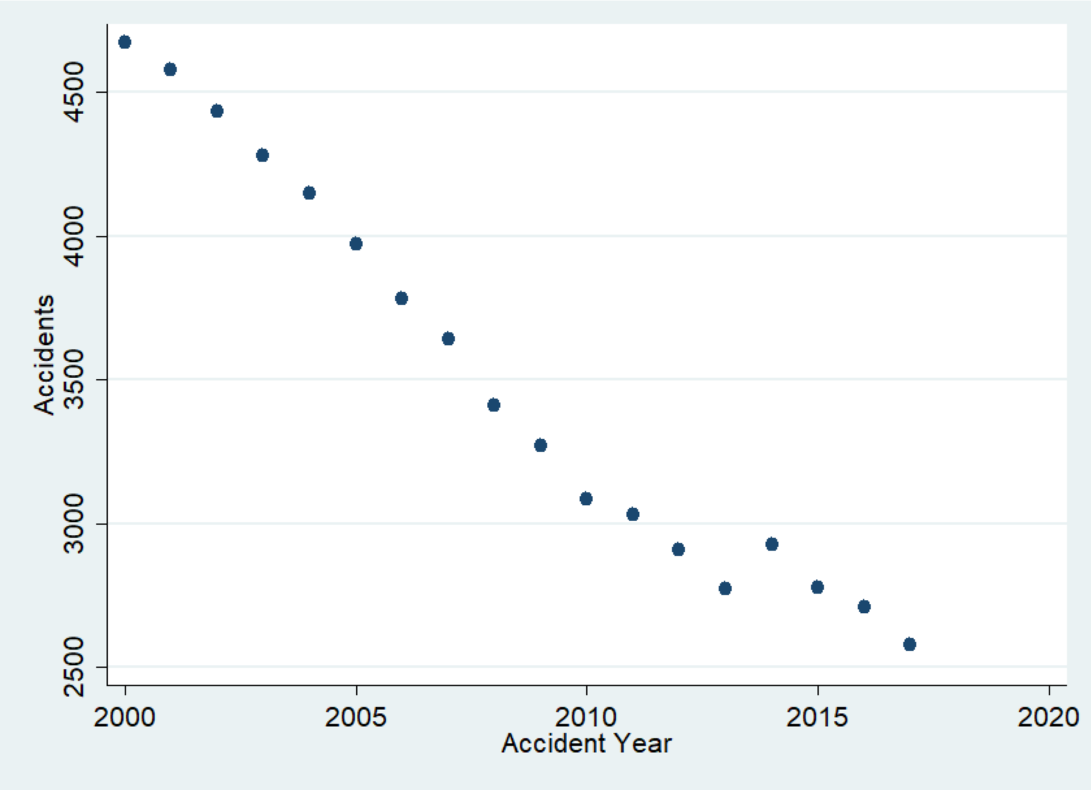


Figure 4. 2 - Average Number of Accidents 2000 to 2017



Most of the research conducted on acts of terror focuses on economic variables such as income, consumption, investment and GDP (Abadie and Gardeazabal, 2003; Eckstein and Tsiddon, 2004; Persitz, 2007; Abadie and Gardeazabal, 2008). Furthermore, research conducted on the effect of these acts on road accidents focuses on the New York 9/11 attacks (Gigerenzer, 2004; Sivak and Flannagan, 2004; Gigerenzer, 2006; Blalock et al., 2009). The literature suggests that a substitution from air travel to road travel takes place therefore leading to more accident fatalities through an increase in the quantity of driving. Other literature controls for the volume of traffic and finds that incidents increase traffic fatalities concluding it may be caused by stress (Stecklov and Goldstein, 2004).

A common thread within the literature is that people's behaviour changes with regards to modes of transport or the way in which they drive however, this literature does not investigate the behavioural change further.

To investigate whether incidents affect road accidents through a change in either or both the quantity and quality of driving, a fixed effects, similar to a study conducted by Cinar (2017), is used on a balanced panel dataset from 2000 to 2017 for Great Britain to estimate this effect on the total number of accidents for a given year and police force area. Similar to a study conducted by Stecklov and Goldstein (2004), lagged variables are used to control for any spill-over effects and the volume of traffic is also controlled for. To the best of my knowledge an analysis on the effect of incidents on road accidents in Great Britain has not been conducted and while some papers use a terror index, such as Eckstein and Tsiddon (2004), this paper separates the impact of a terror incident into the effect of a terror incident occurring and a measure of the intensity of the incidents. This effect is further decomposed into fatal, serious and slight accidents and casualties.

Further analysis is conducted to determine whether the various attack types have differing effects on road accidents and whether incidents with high media coverage have a larger effect than those with low media coverage. A single event study is conducted to control for possible spatial and geographic spill overs and an alternative measure of terrorist threat and intensity are also used.

The subsequent section of this paper critically evaluates literature on the topic. This is followed by a description of the data and estimation strategy used in the analysis, discussion of the results and finally conclusion.

4.2 Literature Review

Research conducted on terror incidents is usually focused on the effect these attacks have on the economy. Most of the literature focuses on the effect on income, consumption, investment, foreign investment, foreign exchange and GDP (Abadie and Gardeazabal, 2003; Eckstein and Tsiddon, 2004; Persitz, 2007; Abadie and Gardeazabal, 2008; Maitah et al., 2017). Focusing on the distinction between direct and indirect costs, Enders and Olson (2012) find that terrorism reduces a country's overall growth rate with the cost of terrorism concentrated on the transportation, tourism and financial market sectors as well affecting foreign direct investment. A panel study covering 115 countries from 2000 to 2015 conducted using fixed effects and random effects finds that terror incidents cause a negative impact on economic growth, particularly in low income countries (Cinar, 2017).

Research conducted on road accidents explores various topics such as the effect of changes in fuel prices and vehicle ownership rates (Leigh and Wilkinson, 1991; Grabowski and Morrisey, 2004; Wells, 2007). One of the areas devoted to the analysis of road accidents is that of terror incidents.

This paper hypothesises that incidents will affect road accidents through quantity and quality of driving. Therefore, the volume of traffic is used as a control when estimating the effect. This is similar to a large part of the literature which focuses on the 9/11 attacks and posits that the attack caused a substitution from air travel to road travel therefore leading to more accident fatalities through an increase in the quantity of driving (Gigerenzer, 2004; Sivak and Flannagan, 2004; Gigerenzer, 2006; Blalock et al., 2009).

As with this paper, Blalock et al. (2009) use year fixed effects and control for the cost of driving using the fuel price amongst other controls when determining the effect of the attack on road fatalities. Their analysis finds that the response to the attack lead to 344 road fatalities per month in late 2001. Using US data for the three months after the attack, Gigerenzer (2004) finds that the number of fatalities caused by road accidents after the attack is higher than the number of flight passengers killed during the attack and therefore concludes that it is necessary to educate the public on the risk of dread or fear.

This conclusion makes quite an impact however, a study conducted by Sivak and Flannagan (2004) re-evaluate the effect of the attack and their results contradict those of Gigerenzer (2004). The study, conducted using data on the trends in road traffic fatalities from January to August for 2000 to 2001, examines the effect of the attack on different road types rather than just rural interstate highways. The authors find that the effect of 9/11 had a much larger effect on road traffic fatalities than that estimated by Gigerenzer (2004) and furthermore, while Gigerenzer (2014) hypothesised that air travel is substituted by long distance travel between states on roads, by analysing the effect on various road types Sivak and Flannagan (2004) found that this is not the case, instead it is replaced by short distance local travel.

Although contradicting the findings of Gigerenzer (2014) the overall outcome remains, there is a risk associated with dread and fear after a terror incident occurs. A study by Litman (2005) examines this theory but with regards to public transport. The analysis includes, along with U.S. and Canadian, the London public transport system and after accounting for several terror incidents, including the July 07, 2005 attack, finds that public transport is a safer mode of travel than motor vehicles where total per passenger and mile fatality rates are one-tenth that of motor vehicles. The authors conclude that, given the media attention and resulting fear from a terror incident, attacks on public transport will lead to more fatalities if road travel substitutes public transit.

All this literature has one thing in common, terror incidents cause behavioural changes. Studies looking at the effect of these attacks on road accidents discuss changes in the quantity of driving. A study by Stecklov and Goldstein (2004) conducted on Israeli daily data from January 2001 to June 2002 finds a 35 percent increase in fatal accidents 3 days after an attack. The authors use lags within their model and control for day of the week, month, year and major holidays. First, using an OLS design they estimate the effect of terror incidents on daily volume of traffic then, using Poisson regression, estimate the effect on accident rates calculated using volume of traffic essentially controlling for volume of traffic. They conclude that the increase in traffic fatalities, some of which may be due to suicide brought on by the terror incident, reveals a delay in response to violence and that it may be caused by stress.

4.3 Data

The Road Accident Data, (discussed in Chapter 1) provides information on each accident including the date, where the accident took place, the casualties, if any, involved, police force area and more. Due to its size the City of London Police is added to the Metropolitan Police within the data. Each accident is counted by year and police force area to produce a total count for each variable. These are 'Total number of accidents' and 'Fatal, Serious and Slight Accidents and Casualties'.

Since a change in the volume of traffic may affect the number of accidents and casualties this is controlled for in two ways. First, the total volume of traffic is added as an independent variable to the specification, second, accident and casualty rates are used as dependent variables.

The annual total volume of traffic measured in 1000 vehicle miles is provided for each junction to junction link on the major road network per police force area. These are aggregated to form one volume of traffic figure per police force area and year. The total accident rate, fatal, serious, slight accident and casualty rates are calculated by dividing the annual counts per police force area and year by the all motor vehicle volume of traffic for each corresponding police force area and year. All resulting rates are measured per million vehicle miles travelled.

The incident data is sourced from The Global Terrorism Database (GTD) which is maintained by the 'National Consortium for the Study of Terrorism and Responses to Terrorism' (START) based at the University of Maryland (Global Terrorism Database Codebook, 2017). The data is available from 1970 to 2017 and lists incidents that have occurred throughout the world. Each incident is assigned an ID and includes details such as date, country, city and attack type to name a few. In order to ensure that the data comprises terror attacks only observations where 'there is no doubt as to whether the incident is an act of terrorism' (Global Terrorism Database Codebook, 2017) are kept. Furthermore, only incidents where 'The attack is not part of a multiple incident' (Global Terrorism Database Code, 2017) and where the 'event occurred in a city/village/town and the latitude/longitude is for that location' (Global Terrorism Database Codebook, 2017) are kept. By eliminating attacks that are not part of a multiple incident, the events which took place on the 7^{th} and 21^{st} of July 2005 in London are not included in the sample however, the 7^{th} of July incident is used as part of a single event study as a robustness check.

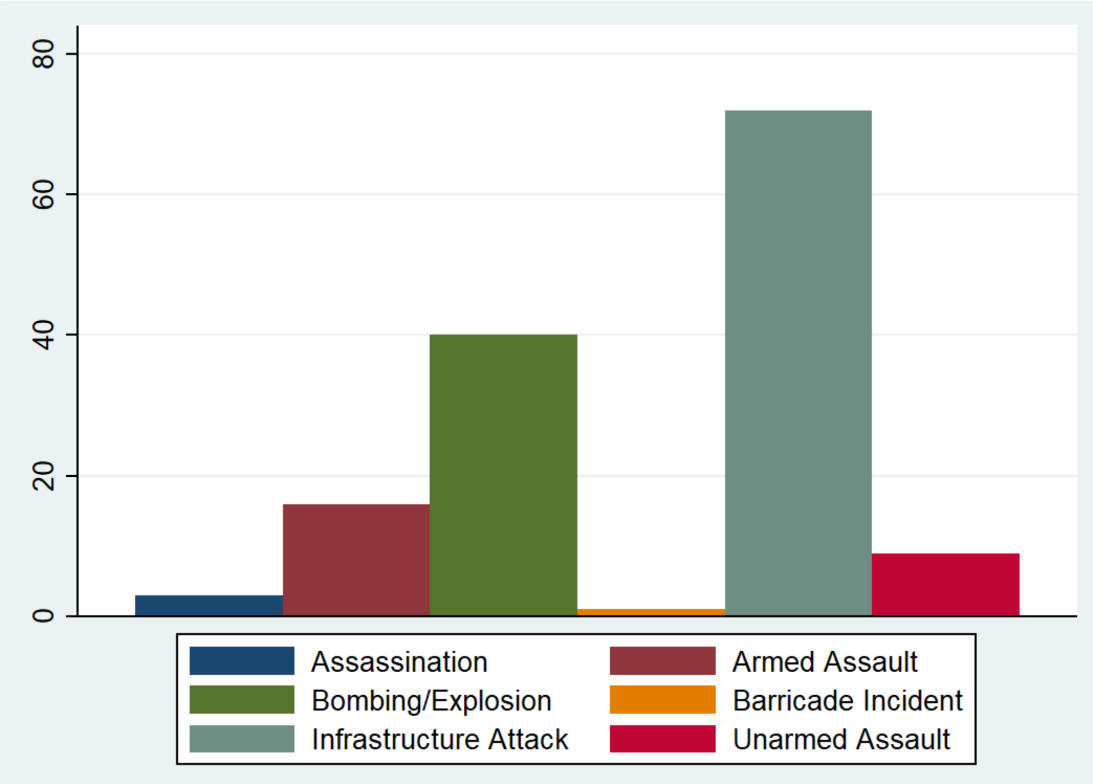
Each incident is counted by year and police force area to produce a total count variable, 'total incidents'. The corresponding dummy variable, 'incident', is given a value of 1 for all values where 'total incidents' is greater than 0 and a value of 0 otherwise. This dummy variable indicates whether an incident took place in a given year and police force area only and not how many took place. The variable 'multiple' is a dummy variable given a value of 1 for all values where 'total incidents' is greater than or equal to 2 and a value of 0 otherwise. This variable indicates whether more than one incident took place in a given year and police force area and measures the intensity of an incident. If more than one incident occurs within a given year and police force area, then both 'incident' and 'multiple' will have a value of 1 for that particular observation.

The type of attack is also recorded for each incident and lists the broad class of tactics used. The Global Terrorism Database variable is defined by nine categories and up to three types can be recorded for each incident if the attack comprises a sequence of events. The categories are as follows; assassination, hijacking, kidnapping, barricade incident, bombing/explosion, armed assault, unarmed assault, facility or infrastructure attack and unknown, where the attack type cannot be determined from the available information (Global Terrorism Database Codebook, 2017). The data therefore contains three attack type categorical variables for each incident.

Since the sample used in this analysis contains data from 2000 till 2017 for Great Britain only and excludes those that are part of a multiple incident, not all incidents fall within one of the nine categories listed above, rather, six categories now apply. The incidents within this sample are of the following attack type categories;

assassination, armed assault, bombing/explosion, barricade incident, facility or infrastructure attack and unarmed assault. Furthermore, none of the incidents comprised a sequence of events so only one ‘attack type’ variable is listed. The distribution of this variable can be found in Figure 4.3, plotting the total number of incidents per attack type within the sample. The figure demonstrates that facility or infrastructure attacks are the most common with 72 incidents followed by bombing/explosion attacks with 40, 16 armed assaults, 9 unarmed assaults, 3 assassinations and 1 barricade (hostage) incident.

Figure 4. 3 - Total number of incidents per attack type 2000 to 2017



This categorical variable is transformed into six dummy variables; ‘assassination’, ‘armed assault’, ‘bombing/explosion’, ‘barricade incident’, facility/infrastructure’ and ‘unarmed assault’ where a value of 1 indicates that an incident has occurred for that category within a given year and police force area and is 0 otherwise. There are occasions when more than one incident occurs in a given year and police force area therefore, a group of incidents in this case may be represented by more than one attack type dummy variable.

In order to ascertain whether a highly publicised incident has a larger effect on road accidents all incidents are classified into three categories, those with a high, medium and low media count. This was conducted by finding news on each incident within one national newspaper, The Guardian. All incidents that were reported on the front page one to two days after the event took place are classified as having high media coverage, all incidents that were reported anywhere else in the paper one to two days after the event took place are classified as having medium media coverage and any incidents that were either reported anywhere in the paper more than two days after the event or were not reported at all are classified has having low media coverage. Since 'The Guardian' newspaper is only archived till 2003, past issues of the online paper are used for all incidents from 2004. The online format has a Front Page, Top Stories and UK News section therefore any Front Page, Top Stories or Breaking News articles are treated as if they were on the front page of the paper and any stories within the UK News section are treated as being anywhere else in the paper. Of the 141 incidents in this sample, 18 are classified as having high media coverage, 41 as medium and 82 as low.

Three dummy variables are then created, High Media, Medium Media and Low Media, where the high media variable is 1 for all years and police force areas that experienced one or more incidents classified as having high media coverage and 0 otherwise. The medium media variable is 1 for all years and police force areas that experienced one or more incidents classified as having medium media coverage and the low media variable is 1 for all years and police force areas that experienced one or more incidents classified as having low media coverage. Therefore, more than one media coverage dummy variable can be 1 for a given year and police force area.

The Government MI5 threat level is used as an alternative measure to the incident and multiple incident dummy variables. Since the threat level was only implemented in 2006, a subsample of road accidents is used from 2006 till 2017.

While there are five threat levels; low, moderate, substantial, severe and critical, only the substantial, severe and critical levels have been used since 2006 for Great Britain. Furthermore, the Critical threat level is only activated for a few days at a time before being changed back to severe therefore, since this analysis uses annual data, the critical threat level is not accounted for but rather the year is classified as being severe. Two dummy variables are created, Substantial and Severe which are 1 for all years the threat level is either Substantial or Severe and 0 otherwise for both variables. When analysing the effect one variable is dropped to avoid issues of multicollinearity.

The number of fatalities and wounded are used as an alternative measure of intensity. Each incident within the GTD has the number of fatalities and wounded listed, if any. A count of the total number of fatalities and wounded from incidents for each police force area and year is therefore available. The number of fatalities are either 0 or 1 for all police force areas and years except Metropolitan and Manchester in 2017, that incurred 18 and 23 fatalities respectively, and the number of wounded are below 10 for all police force areas and years except Metropolitan and Manchester in 2017, that incurred 145 and 119 wounded respectively. Given the few instances of high fatalities and injuries two dummy variables are created, Fatalities and Wounded, to represent whether a given year and police force area incurred any incident related fatalities or injuries.

A single event study using the 7th of July London event which took place in 2005 is conducted to control for possible immediate time and spatial spill overs. This attack is chosen since it is the largest transport related incident to have occurred in Great Britain. Another large event took place on the 21st of this month in London with no fatalities or injuries and is also transport related. However, given that this is an annual analysis, the results may be due to the effect of both attacks.

To control for spatial spill overs two variables are created, Local, which is 1 for the Metropolitan police force area, the area in which the attack occurred, and year 2005, the year in which the attack occurred, and 0 otherwise. National, is 1 for the

Strathclyde police force area and year 2005 and 0 otherwise. This police force area is chosen as a comparison for the event study for several reasons, it is a large area, in another country within Great Britain, Scotland, and contains Scotland's largest city, Glasgow. Furthermore, no incidents occurred in this area in 2005. A group of police force areas that contain fast rail systems (such as the tube in London) are also used as a comparison. There are four rapid transit systems in the UK including the tube in London. The others are found in Glasgow (part of Strathclyde police force area), Tyne and Wear (part of Northumbria police force area) and Liverpool (part of Merseyside police force area). Therefore, the variable Transport National is created which is 1 for the police force areas of Strathclyde, Northumbria and Merseyside and the year 2005 and 0 otherwise. No incidents took place in Northumbria and only 1 incident, with 0 fatalities and 1 minor injury and which is not transport related took place in Merseyside in 2005. Furthermore, the event that took place in Merseyside did not take place in Liverpool.

To control for time spill overs two variables are created. These variables account for the effect of the incident for 12 months post the event. The first is an interaction between a dummy variable which is 1 for the Metropolitan police force area and year 2005 and the variable 'Year = 177/365' to account for 2005 from the 177th day of the year (representing the 7th of July). The second is an interaction between a dummy variable which is 1 for the Metropolitan police force area and year 2006 and the variable 'Next Year = 188/365' to account for 2006 from the first day of the year to the 188th day. Therefore, the two variables will measure the effect of the incident for a total of 12 months. The same is done for the National and Transport National variables.

The resulting sample used in this analysis, consists of 141 incidents and is a balanced panel dataset which contains annual observations from 2000 to 2017 for each police force area (50 in total) of Great Britain and contains the following variables; 'incident', a dummy variable representing whether or not an incident has taken place, 'multiple', a dummy variable representing whether more than one incident has taken place, a count of the total number of accidents, a count of the fatal, serious and slight accidents and casualties and the total accident rate. Furthermore, lagged variables are created in order to ascertain whether the effect of the incident leads to a time spill over (Stecklov and Goldstein, 2004). Two lags are taken of the dummy variables ‘incident’ and ‘multiple’ to account for the time spill over. The sample also contains all attack type, media count, ‘Fatalities’ and ‘Wounded’ dummy variables as well as the variables used for the single event study.

Figures 4.1 and 4.2 depict a scatter plot of the average number of accidents (dependent variable) over time and the total number of incidents (independent variable) over time. It is evident from the figure that, while the average number of accidents follow a linear downward trend, the total number of incidents do not, rather the plots are sporadic and form a U shape therefore suggesting the possible need for a quadratic trend within the analysis. These figures are expected as, due to the unpredictable nature of terror attacks, it would be unusual for the data to follow a linear trend.

Even though dummy variables are used within the analysis, descriptive statistics for count variables within the sample can be found in Table 4.1.

Table 4. 1 - Descriptive Statistics

Variable	Obs	Sample Mean	Standard Deviation	Min	Max
Accident Year	900	2008.5	5.191012	2000	2017
Police Force Area	900	25.5	14.43889	1	50
Total Incident	900	0.1566667	0.6891096	0	9
Outcome Variables					
Total Accidents	900	3499.093	3955.087	236	38018
Fatal Accidents	900	46.61111	32.24482	2	293
Serious Accidents	900	484.4567	495.7527	43	5392
Slight Accidents	900	2968.026	3455.832	182	32348
Fatal Casualties	900	50.44222	34.25459	2	299
Serious Casualties	900	551.6656	537.4466	52	5881
Slight Casualties	900	4133.317	4347.188	248	40249
Total Accident Rate	900	0.8070122	0.3802069	0.2043119	2.964386
Explanatory Variables					
Incident Dummy Variables					
Incident Dummy	900	0.0866667	0.2815024	0	1
Multiple Dummy	900	0.0288889	0.1675874	0	1
Attack Type Variables					
Assassination	900	0.0033333	0.0576708	0	1
Armed Assault	900	0.0177778	0.1818076	0	4
Bombing/Explosion	900	0.0444444	0.2987222	0	5
Barricade Incident	900	0.0011111	0.0333333	0	1
Facility/Infrastructure	900	0.08	0.4502379	0	6
Unarmed Assault	900	0.01	0.1371488	0	3
Media Count Variables					
High Media Count	900	0.02	0.2152165	0	4
Medium Media Count	900	0.0455556	0.2607686	0	3
Low Media Count	900	0.0911111	0.4481192	0	6
Threat Level Dummy Variables					
Substantial	900	0.2222222	0.4159709	0	1
Severe	900	0.4444444	0.4971803	0	1
Other					
Incident Fatalities	900	0.0522222	0.9761024	0	23
Incident Wounded	900	0.3388889	6.261243	0	145
Total Volume of Traffic	900	3999909	2313647	938986.7	1.28E+07
Total Population	900	1207622	1141461	147500	8824800

Note: Total Volume of Traffic (1 unit = 1000 miles)

4.4 Empirical Strategy

The effect of an incident on road accidents is estimated using a fixed effects model

$$Y_{it} = \alpha + \beta I_{it} + \delta M_{it} + f(t) + u_i + \varepsilon_{it} \qquad (1)$$

Since this analysis decomposes the effect into that on fatal, serious and slight accidents and casualties, Y is a vector of the various accident and casualty count variables including the total number of accidents for a given police force area and year. I_{it} is the dummy variable 'incident' which indicates whether an incident occurred in area i at time t. β provides the estimate of this effect on road accidents. M_{it} is the dummy variable 'multiple' which indicates whether more than one incident occurred in area i and time t. δ provides the estimate of this effect on road accidents.

Two versions of f(t) are used in the initial specification. One is a quadratic time trend to account for the non-linear trend in the incident data and the other is a fully flexible version including time fixed effects in the form of 18 year dummy variables. The year fixed effects control for time specific characteristics that affect the dependent variables across all police force areas over time (Cotti and Tefft, 2011). u is a vector of area fixed effects in the form of police force dummy variables (of which there are 50). By accepting the assumption that unobservable factors which might affect the number of accidents and number of incidents simultaneously are time-invariant, fixed effects may be used to remove any omitted variable bias where the police force area fixed effects control for all time invariant unobservable characteristics across groups leaving only the within group effect (Angrist and Pischke, 2008).

The concern of a time spill over needs to be accounted for. An incident that takes place in 2002, for example, may influence the behaviour of motorists for many years, especially if that incident is widely reported. Furthermore, given that the incident occurs on a specific day, but the number of accidents is measured throughout 2002, the incident, depending on whether it fell towards the beginning or end of the year, may not affect all the road accidents taking place in 2002 instead, this effect will most likely spill over into 2003. It is therefore necessary to analyse lagged versions of the independent variables and not to account for the immediate effect estimated using I_{it}. However, in doing so, only the short-term effect of incidents occurring in 2017 will be estimated which is a pity given that this is a high incidence year.

Taking lags up to two periods the specification now becomes

$$Y_{it} = \alpha + \beta_1 I_{it-1} + \beta_2 I_{it-2} + \delta_1 M_{it-1} + \delta_2 M_{it-2} + \gamma X_{it} f(t) + u_i + \varepsilon_{it} \quad (2)$$

The vector Y now includes the total accident rate. X is a vector of control variables that contains the 'total volume of traffic'. Both are used in separate versions of specification (2) to determine whether the results remain robust to a traffic volume control.

It can be also be assumed that the effect of an incident taking place in a local area may spill over into other areas given the propensity for larger events being reported in the news. However, the analysis uses larger areas (police force area) therefore, the geographical spill-over effect becomes less of a concern.

A variable 'national' is constructed to test this. I_t is a count variable and indicates the presence of an incident anywhere in the country at time t. It is defined by

$$I_t = \Sigma\{I_{it}\}$$

A variation of specification (2) that includes this variable, does not include the variable 'multiple' and uses lags of the count of total incidents, rather than the dummy variable, is estimated. The resulting estimates of the effect at the national level are small and not significant therefore confirming that a geographic spill-over effect is not a concern and does not need to be considered in this analysis. This and an immediate time spill-over effect are, however, addressed using the single event study.

4.5 Empirical Results

Table 4.2 reports results using variations of specification (2). Note that the number of observations has dropped from 900 to 800 due to the lags used, two per police force areas, and all specifications include police force area dummy variables. Columns 1, 3, 5 and 7 contain year fixed effects instead of a linear and quadratic trend. The two variations of f(t) are included as a robustness check and indeed all the estimates are very similar for both versions of f(t) with significant trend and quadratic trend estimates. Going forward, only the estimates where year fixed effects are used will be discussed.

Columns 1 and 2 report the estimates of an incident occurring and multiple incidents occurring, both with two lags. These two variables are used to compare the marginal effect of a first incident to the marginal effect of another additional incident. People are inclined to respond differently if a given police force area in a given year suffers from more than one incident. Any emotions a person may be feeling after an attack will only be exacerbated with each additional attack therefore, since variable 'multiple' indicates whether 2 or more incidents have occurred, it can be used as a measure of intensity of incidents.

Although only the estimates of the effect of multiple incidents on the total number of accidents two years later are significant a pattern emerges with these results. The marginal effect of a first incident is different to the marginal effect of another additional incident and this difference holds with each lag. The estimate on the effect of an incident occurring is much smaller and negative for both lags. By comparison, the estimates of the effect of multiple incidents are significant, large and positive. Looking at column 1, more than one incident occurring in a given year and police force area raises the total accident count by 600 accidents two years after the events. Since 'multiple' measures the intensity of incidents a large effect is expected.

Table 4. 2 - Incident effect on total accidents, accident rate per million miles and total volume of traffic (1 unit = 1000 miles)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total Accidents	Total Accidents	Total Accidents	Total Accidents	Total Accident Rate	Total Accident Rate	Total Volume of Traffic	Total Volume of Traffic
lagIncident1	-145.4	-154.8	-156.5	-167.0	0.00302	0.00190	53464.3***	45583.4**
	(106.9)	(104.6)	(107.5)	(104.8)	(0.0156)	(0.0153)	(18631.2)	(21239.8)
lagIncident2	-83.48	-95.37	-92.20	-103.8	-0.00231	-0.00354	42224.3**	31483.6
	(108.9)	(106.6)	(109.3)	(106.6)	(0.0159)	(0.0156)	(18991.7)	(21638.2)
lagMultiple1	263.2	284.9	269.1	290.5	0.00250	0.00574	-28690.6	-20867.0
	(186.2)	(183.5)	(186.3)	(183.4)	(0.0272)	(0.0269)	(32467.7)	(37262.3)
lagMultiple2	600.1***	628.6***	579.1***	597.9***	0.00925	0.0150	101947.7***	114953.4***
	(204.1)	(201.1)	(205.3)	(202.0)	(0.0298)	(0.0295)	(35585.0)	(40826.9)
trend		-248.0***		-242.6***		-0.0514***		-20484.2***
		(25.28)		(25.53)		(0.00370)		(5133.7)
trend2		5.813***		5.326***		0.000847***		1827.0***
		(1.188)		(1.232)		(0.000174)		(241.2)
Total Volume of Traffic			0.000206	0.000266				
			(0.000212)	(0.000180)				
_cons	26780.7***	27524.9***	24339.2***	24335.5***	2.416***	2.561***	11828802.6***	11973366.6***
	(213.5)	(228.2)	(2518.7)	(2172.2)	(0.0311)	(0.0334)	(37214.9)	(46329.7)
N	800	800	800	800	800	800	800	800

Standard errors are reported in parentheses where * p<0.10, ** p<0.05 and *** p<0.01
All specifications include police force area dummy variables.
Specifications 1, 3, 5 & 7 include year dummy variables.

The total volume of traffic is controlled for in two ways. The first ads the total volume of traffic variable, where 1 unit = 1000 miles, to the specification. These estimates are reported in columns 3 and 4. The second uses the accident rate, calculated by dividing the total number of accidents in a given year and police force area by the total volume of traffic within that year and police force area, instead of the total number of accidents as the dependent variable. These estimates are reported in columns 5 and 6 and none of them are significant. The estimates in columns 3 and 4 have the same signs as those in columns 1 and 2 and while the effect of an increase in the total volume of traffic on the total number of accidents in a given year and police force area is not significant and very small, it is positive, implying that an increase in the volume of traffic may lead to more accidents.

Furthermore, the estimates of the effect of multiple incidents remain significant and imply that the effect of more than one incident occurring in a given year and police force area increases the total number of accidents by 579 two years later after controlling for total volume of traffic. Now, once the volume of traffic is controlled for, the accident count increases by less than it did before adding the control. Therefore, changes in the volume of traffic accounts for some of the impact of multiple incidents on the total number of accidents.

Columns 7 and 8 report the estimates obtained when using the total volume of traffic as the dependent variable. Most of these estimates are significant and imply that, the effect of an incident occurring in a given year and police force area raises the total volume of traffic by more than 53 million miles one year later and 42 million miles two years later. Once again, since it measures the intensity of an incident occurring, the estimate on ‘multiple’ is much larger. The effect of more than one incident occurring in a given year and police force area increases the total volume of traffic by almost 102 million miles two years later. These results imply that the total volume of traffic increases due to an incident and that the increase is greater the greater the intensity of the incident. This may be due to a shift from public transport to driving given people’s perception of the risk involved in taking public transport after an incident has occurred (Litman, 2005).

Therefore, given the results provided in columns 3 to 8, the effect of a multiple incident increases the total number of accidents and volume of traffic two years later. However, the total number of accidents increases by less after controlling for the total volume of traffic confirming that the increase in the total volume of traffic, due to the effect of multiple incidents, increases the total number of accidents. So, the effect of a multiple incident in a given year and police force area affects the total number of accidents both through a change in the quantity and quality of driving. Furthermore, it seems that the effect due to a change in the quality of driving is quite high.

While an analysis of the change in behaviour is beyond the scope of this paper, perhaps, as suggested by the literature, the stress caused by an incident affects the way in which people are now driving (Gigerenzer, 2004; Stecklov and Goldstein, 2004; Gigerenzer, 2006). Stress may lead to an increase in road accidents and fatalities through both speeding or an increase in drink driving (Rock, 1995; Rhum, 1995; Bielinska-Kwapisz and Young, 2006). Also, perhaps, those who switched from public transport to driving may not be as experienced at recognising possible road hazards and therefore involved in more accidents (Chapman and Underwood, 1998; Underwood et al., 2005; Borowsky et al., 2010). The results also indicate a delay in the response to multiple incidents. This study is limited in that, by using the lagged variables only the short-term effect of incidents occurring in 2017 will be estimated which is a pity given that this is a high incidence year.

Table 4.3 reports results using variations of specification (2) where, once again, all specifications include police force area dummy variables and now also include year dummy variables. The effect estimated in Table 4.2 is now decomposed into fatal, serious and slight accidents where each dependent variable is still a count. The rates are no longer used however, columns 2, 3 and 6 control for the total volume of traffic. The same pattern in Table 4.2 emerges in Table 4.3 where the effect of an incident occurring is small and negative and the effect of multiple incidents occurring is large and positive.

Table 4. 3 - Incident effect on fatal, serious and slight accidents

	(1) Fatal Accidents	(2) Fatal Accidents	(3) Serious Accidents	(4) Serious Accidents	(5) Slight Accidents	(6) Slight Accidents
lagIncident1	-5.223***	-6.206***	-35.24	-54.46***	-105.0	-95.79
	(1.746)	(1.723)	(21.88)	(20.96)	(96.39)	(96.94)
lagIncident2	-0.998	-1.775	-32.20	-47.38**	-50.28	-43.04
	(1.780)	(1.752)	(22.31)	(21.32)	(98.25)	(98.60)
lagMultiple1	7.285**	7.812***	115.5***	125.8***	140.4	135.5
	(3.042)	(2.987)	(38.13)	(36.35)	(168.0)	(168.1)
lagMultiple2	4.314	2.440	185.1***	148.4***	410.7**	428.2**
	(3.334)	(3.290)	(41.79)	(40.04)	(184.1)	(185.1)
Total Volume of Traffic		0.0000184***		0.000360***		-0.000172
		(0.00000340)		(0.0000414)		(0.000191)
_cons	191.4***	-26.15	3104.4***	-1148.3**	23485.0***	25513.6***
	(3.487)	(40.37)	(43.71)	(491.3)	(192.5)	(2271.8)
N	800	800	800	800	800	800

Standard errors are reported in parentheses where * p<0.10, ** p<0.05 and *** p<0.01

All specifications include year and police force area dummy variables.

Each dependant variable is a count of the number of accidents in the respective category.

Columns 1 and 2 provide the estimates of the effect of an incident and multiple incidents occurring on fatal accidents. The significant estimates in column 1 imply that the effect of an incident occurring within a given year and police force area decreases the number of fatal accidents by 5.2 one year later and the effect of more than one incident occurring increases the number of fatal accidents by 7.3 one year later. The absolute value of these estimates decreases, while remaining the same sign, after two years however they are not significant. The results imply that the greater the intensity of incidents in a given year and police force area the larger the number of fatal accidents one year later.

This pattern does not change when controlling for total volume of traffic. The estimates provided in column 2 imply that, after controlling for total volume of traffic, the effect of an incident occurring within a given year and police force area decreases the number of fatal accidents by 6.2 one year later and the effect of more than one incident occurring increases fatal accidents by 7.8 one year later. The estimate of the effect of total volume of traffic is also significant and implies that an increase in the total volume of traffic by 1000 miles increases the number of fatal accidents by 0.02.

These results suggest that when one incident occurs, controlling for the total volume of traffic dampens the effect. Therefore, an increase in the total volume of traffic increases the number of fatal accidents so that the drop caused by the quality of driving decreases by less. When more than one incident occurs the change in effect after controlling for traffic volume is very small. This result may be explained by the result provided in Table 4.2 column 7, which suggests that the effect of a multiple incident is to decrease the total volume of traffic one year later however this estimate is not significant.

Columns 3 and 4 provide the estimates of the effect of an incident and multiple incidents occurring on serious accidents. All the estimates in column 4 are significant and imply that, after controlling for the total volume of traffic, the effect of an incident occurring in a given year and police force area decreases the number of serious accidents by 54.5 one year later and by 47.4 two years later. The effect of more than one incident occurring in a given year and police force area increases the number of serious accidents by 126 one year later and 148 two years later. Therefore, after controlling for the total volume of traffic, the effects of incidents have a much stronger impact on serious accidents than fatal accidents. Furthermore, the effect of an incident occurring on serious accidents dampens two years later suggesting a quicker response compared to the effect of multiple incidents occurring, where, the effect becomes larger two years later suggesting a delayed response to multiple incidents. The estimate for the total volume of traffic is significant and implies that an increase of 1000 miles in a given year and police force area increases the number of serious accidents by 0.36, which is larger than the effect on fatal accidents however still small.

The significant estimates in column 3 imply that, before controlling for the total volume of traffic, the effect of more than one incident occurring in a given year and police force area increases the number of serious accidents by 116 one year later and 185 two years later.

Once again, these results suggest that when more than one incident occurs the change in effect after controlling for traffic volume is very small one year later and is explained by the result in Table 4.2. Two years later, the effect of multiple incidents is dampened after controlling for the total volume of traffic.

Columns 5 and 6 provide the estimates of the effect of an incident and multiple incidents occurring on slight accidents. The significant estimates imply that the effect of more than one incident in a given year and police force area increases the number of slight accidents by 411 two years later and 428 two years later after controlling for the total volume of traffic. The estimate for the total volume of traffic is now negative but not significant. This may suggest why controlling for volume of traffic now heightens the effect of multiple incidents two years later. These results suggest that the effect of multiple incidents have a much stronger impact on slight accidents than fatal and serious accidents.

Table 4.4 reports results using variations of specification (2) and all specifications include police force area and year dummy variables. The effect estimated in Table 4.2 is now decomposed into fatal, serious and slight casualties where each dependent variable is still a count. Once again columns 2, 3 and 6 control for the total volume of traffic.

Columns 1 and 2 provide the estimates of the effect of an incident and multiple incidents occurring on fatal casualties. The effect on fatal casualties follows the same pattern as that seen on fatal accidents. The significant estimates in column 1 imply that the effect of an incident occurring in a given year and police force area decreases the number of fatal casualties by 6 one year later and the effect of more than one incident occurring increases fatal casualties by 8.1 one year later. Once again, the results imply that the greater the intensity of incidents in a given year and police force area the larger the number of fatal casualties one year later.

This pattern does not change when controlling for total volume of traffic. The estimates provided in column 2 imply that, after controlling for total volume of traffic, the effect of an incident occurring within a given year and police force area decreases the number of fatal casualties by 7.2 one year later and the effect of more than one incident occurring increases fatal casualties by 8.7 one year later. The estimate of the effect of total volume of traffic is also significant and implies that an increase of 1000 miles increases the number of fatal casualties by 0.02.

These results suggest again that when one incident occurs, controlling for the total volume of traffic dampens the effect and when more than one incident occurs the change in effect after controlling for traffic volume is very small which may be explained by the results provided in Table 4.2 column 7.

Columns 3 and 4 provide the estimates of the effect of an incident and multiple incidents occurring on serious casualties. The significant estimates imply that the effect of an incident in a given year and police force area decreases the number of serious casualties by 41 one year later and by 43.6 two years later. After controlling for the total volume of traffic the effect decreases the number of serious casualties by 63 one year later and by 61.1 two years later. These estimates imply three things, firstly, the effect on serious casualties, like serious accidents, is stronger than the effect of fatal casualties. Secondly, the total volume of traffic dampens the effect of an incident occurring on serious casualties, since, after controlling for it, the estimates become more negative. Thirdly, after controlling for the total volume of traffic, the effect is lower two years later but not by much.

The effect of more than one incident in a given year and police force area increases the number of serious casualties by 127 one year later and 207 two years later. After controlling for the total volume of traffic the effect increases the number of serious casualties by 139 one year later and 165 two years later. The effect of multiple incidents is therefore, once again, large and positive implying a substantial increase in the number of serious casualties. Furthermore, this effect becomes stronger after each year. The significant estimate of the total volume of traffic implies that an increase of 1000 miles increases the number of serious casualties by 0.4, a small but significant amount. The results therefore suggest that controlling for the total volume of traffic dampens the effect of a multiple incident two years later however, it is heightened one year later but not by much which may be explained by the results in Table 4.2 column 7.

Columns 5 and 6 provide the estimates of the effect of an incident and multiple incidents occurring on slight casualties. The significant estimates imply that the effect of more than one incident in a given year and police force area leads to an increase in the number of slight casualties by 533 two years later and by 559 two years later after controlling for the total volume of traffic. All other estimates are not significant. Once again, the effect of multiple incidents on slight casualties two years later is higher than both the effect of fatal and serious casualties. Controlling for the total volume of traffic heightens the effect which may be explained by the negative total volume of traffic estimate however, this is not significant.

Table 4. 4 - Incident effect on fatal, serious and slight casualties

	(1) Fatal Casualties	(2) Fatal Casualties	(3) Serious Casualties	(4) Serious Casualties	(5) Slight Casualties	(6) Slight Casualties
lagIncident1	-6.066***	-7.201***	-40.97*	-63.04***	-172.6	-158.8
	(1.918)	(1.889)	(24.55)	(23.46)	(129.6)	(130.3)
lagIncident2	-1.511	-2.408	-43.62*	-61.05**	-85.81	-74.94
	(1.956)	(1.921)	(25.03)	(23.87)	(132.1)	(132.5)
lagMultiple1	8.114**	8.724***	126.8***	138.7***	269.0	261.6
	(3.343)	(3.275)	(42.79)	(40.68)	(225.8)	(225.9)
lagMultiple2	5.211	3.045	206.6***	164.5***	532.6**	558.9**
	(3.664)	(3.608)	(46.90)	(44.82)	(247.5)	(248.9)
Total Volume of Traffic		0.0000212***		0.000413***		-0.000258
		(0.00000373)		(0.0000463)		(0.000257)
_cons	200.6***	-50.69	3353.2***	-1530.1***	28867.3***	31913.7***
	(3.832)	(44.27)	(49.04)	(549.9)	(258.8)	(3053.8)
N	800	800	800	800	800	800

Standard errors are reported in parentheses where * p<0.10, ** p<0.05 and *** p<0.01
All specifications include year and police force area dummy variables.
Each dependant variable is a count of the number of casualties in the respective category.

Table 4. 5 - Incident effect controlling for total population

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total Accidents	Total Accidents	Total Accidents	Total Accidents	Total Accident Rate	Total Accident Rate	Total Volume of Traffic	Total Volume of Traffic
lagIncident1	-122.7	-131.6	-72.16	-107.9	0.00209	0.000950	58849.9***	51166.1***
	(100.7)	(98.64)	(100.6)	(98.74)	(0.0155)	(0.0153)	(16572.5)	(19514.0)
lagIncident2	-40.71	-52.55	4.315	-33.24	-0.00406	-0.00530	52384.5***	41769.0**
	(102.8)	(100.6)	(102.5)	(100.5)	(0.0158)	(0.0156)	(16904.3)	(19893.3)
lagMultiple1	364.0**	381.2**	359.9**	382.3**	-0.00163	0.00179	-4747.8	2264.6
	(175.8)	(173.3)	(174.2)	(172.7)	(0.0271)	(0.0268)	(28923.2)	(34280.8)
lagMultiple1	472.0**	502.4***	533.5***	541.5***	0.0145	0.0202	71512.5**	84647.7**
	(192.8)	(190.0)	(191.7)	(190.0)	(0.0297)	(0.0294)	(31719.5)	(37586.5)
Total Population	-0.00295***	-0.00296***	-0.00356***	-0.00328***	0.000000121**	0.000000121**	-0.702***	-0.710***
	(0.000306)	(0.000304)	(0.000341)	(0.000331)	(4.72e-08)	(4.71e-08)	(0.0503)	(0.0602)
trend		-224.5***		-231.4***		-0.0524***		-14835.0***
		(23.96)		(24.03)		(0.00371)		(4739.5)
trend2		5.932***		6.790***		0.000842***		1855.6***
		(1.120)		(1.167)		(0.000173)		(221.6)
Total Volume of Traffic			-0.000860***	-0.000462**				
			(0.000223)	(0.000185)				
_cons	50294.6***	50982.8***	65264.4***	59123.7***	1.454***	1.599***	17414959.2***	17607680.5***
	(2443.6)	(2425.7)	(4574.1)	(4053.3)	(0.377)	(0.376)	(402012.3)	(479894.0)
N	800	800	800	800	800	800	800	800

Standard errors are reported in parentheses where * p<0.10, ** p<0.05 and *** p<0.01

All specifications include police force area dummy variables. Specifications 1, 3, 5 & 7 include year dummy variables.

An alternative version of (2) which adds the total population for a given year and police force area as a control to all columns of Table 4.2 is conducted as a robustness check with the results reported in Table 4.5. The main findings hold in that, for the most part, the signs do not change, and the statistically significant estimates remain significant.

Tables 4.6 and 4.7 report results using variations of specification (2) where lags up to two periods of the attack type dummy variables replace the incident and multiple incident dummy variables. All specifications control for the total volume of traffic.

All results are significant apart from the estimates of the barricade incident attack type. Surprisingly assassinations (of which there are three in the sample) have the largest effect on total accidents per year. The effect of this attack type per year and police force area decrease the total number of accidents by 1, 804 one year later. Both assassinations and infrastructure attacks decrease the total number of accidents however, the latter has the smallest effect where infrastructure attacks per year and police force area lead to a decrease in the total number of accidents of 351 one year later and 408 two years later. All other attack types increase the total number of accidents where the effect of unarmed assaults per year and police force area is the largest, increasing the total number of accidents by 733 one year later and 924 two years later. For the most part, these effects are through slight accidents and casualties. An analysis conducted using the total number of incidents per attack type instead of dummy variables produces similar results.

These results are surprising given that assassinations and infrastructure attacks, the first of which only occurred 3 times in the sample and the second of which 72 (the most of all attack types) have both the largest and smallest effect respectively. It is also interesting that these two attack types lead to a decrease in the number of accidents whereas bombing/explosions, armed and unarmed assaults lead to an increase in the number of accidents. It therefore seems that, with respect to road accidents, people are more responsive to assassinations and unarmed assault where the first may induce people to drive more carefully and the second to drive less carefully due to stress caused by the incident.

Table 4. 6 - Attack Type Effect on Total, Fatal, Serious and Slight Accidents

	(1) Total	(2) Fatal	(3) Serious	(4) Slight
lagAssassinations1	-1803.7***	-26.12***	-565.9***	-1211.6***
	(399.7)	(6.536)	(75.84)	(364.5)
lagAssassinations2	-596.1	-33.54***	-631.3***	68.74
	(494.4)	(8.086)	(93.82)	(450.9)
lagArmedAssault1	407.1	-3.286	51.84	358.6
	(283.6)	(4.639)	(53.83)	(258.7)
lagArmedAssault2	882.1***	7.457*	77.13	797.5***
	(267.4)	(4.374)	(50.75)	(243.9)
lagBombing1	386.7***	-1.802	29.45	359.0***
	(135.2)	(2.211)	(25.66)	(123.3)
lagBombing2	551.0***	4.376**	94.82***	451.8***
	(132.2)	(2.161)	(25.08)	(120.5)
lagBarricade1	92.40	2.897	-58.11	147.6
	(684.4)	(11.19)	(129.9)	(624.1)
lagBarricade2	-499.6	-15.31	-97.43	-386.8
	(678.0)	(11.09)	(128.7)	(618.3)
lagInfrastructure1	-351.2***	-5.547***	-52.23**	-293.4***
	(118.9)	(1.945)	(22.57)	(108.5)
lagInfrastructure2	-408.2***	-6.695***	-78.67***	-322.8***
	(136.4)	(2.231)	(25.89)	(124.4)
lagUnarmedAssault1	733.1**	7.289	268.7***	457.1
	(318.7)	(5.211)	(60.47)	(290.6)
lagUnarmedAssault2	923.7**	2.634	240.9***	680.1*
	(387.7)	(6.339)	(73.56)	(353.5)
Total Volume of Traffic	-0.000166	0.0000116***	0.000224***	-0.000401**
	(0.000212)	(0.00000346)	(0.0000402)	(0.000193)
N	800	800	800	800

Standard errors are reported in parentheses where * p<0.10, ** p<0.05 and *** p<0.01
All specifications include year and police force area dummy variables.
Each dependent variable is a count of the number of accidents in the respective category.

Table 4. 7 - Attack Type Effect on Fatal, Serious and Slight Casualties

	(1) Fatal	(2) Serious	(3) Slight
lagAssassinations1	-25.58***	-654.8***	-1988.8***
	(7.178)	(84.52)	(491.3)
lagAssassinations2	-36.95***	-744.6***	-280.0
	(8.880)	(104.6)	(607.8)
lagArmedAssault1	-4.525	38.60	390.7
	(5.094)	(59.99)	(348.7)
lagArmedAssault2	8.518*	88.10	977.5***
	(4.803)	(56.56)	(328.8)
lagBombing1	-2.216	32.34	489.9***
	(2.428)	(28.59)	(166.2)
lagBombing2	4.619*	92.61***	581.2***
	(2.373)	(27.95)	(162.5)
lagBarricade1	3.851	-58.28	373.6
	(12.29)	(144.7)	(841.3)
lagBarricade2	-16.62	-122.4	-353.5
	(12.18)	(143.4)	(833.5)
lagInfrastructure1	-6.340***	-61.91**	-363.0**
	(2.136)	(25.15)	(146.2)
lagInfrastructure2	-7.953***	-93.88***	-382.9**
	(2.450)	(28.85)	(167.7)
lagUnarmedAssault1	6.767	298.0***	275.1
	(5.723)	(67.39)	(391.7)
lagUnarmedAssault2	0.906	273.7***	664.7
	(6.962)	(81.98)	(476.5)
Total Volume of Traffic	0.0000141***	0.000257***	-0.000607**
	(0.00000380)	(0.0000448)	(0.000260)
N	800	800	800

Standard errors are reported in parentheses where * p<0.10, ** p<0.05 and *** p<0.01
All specifications include year and police force area dummy variables.
Each dependent variable is a count of the number of casualties
in the respective category.

Additional Robustness Checks

Table 4.8 reports results using variations of specification (2) where lags up to two periods of the media count dummy variables replace the incident and multiple incident dummy variables. All specifications control for the total volume of traffic.

Column 1 reports the estimates of the effect of incidents with high, medium and low media counts on total accidents. The significant estimates imply that incidents with a high media count have the largest effect on total accidents both one year and two years later with the effect getting larger, or more positive, two years later. The effect of incidents with a medium media count in a given year and police force area is also positive but smaller than the high and increases the number of total accidents by 574 two years later.

Conversely the effect of incidents with a low media count is to decrease the total number of accidents, more so two years later. These coincide with the results of Table 4.2 which imply that the effect of an incident occurring is negative while the effect of more than one incident occurring is positive. It seems as if the effect of one incident occurring and the effect of incidents with a low media count is to decrease the total number of accidents whereas the effect of more than one incident occurring and incidents with a high media count is to increase the total number of accidents.

This increase in total accidents due to multiple incidents and incidents that are widely reported could both be owing to a change in behaviour given that the total volume of traffic has been controlled for. Once again, the stress of a highly reported or multiple incidents occurring in a given year and police force area may affect the way in which people drive. It may lead to an increase in speeding and/or drink driving therefore increasing the number of accidents and fatalities (Rock, 1995; Rhum, 1995; Bielinska-Kwapisz and Young, 2006).

It is noticed that, within the sample, the large and therefore highly publicised incidents are often followed by much smaller yet still publicised incidents that are linked to the original event in the form of retaliation. Therefore, while the smaller incidents may not make front page news they do fall into the medium media count category, whereas similar incidents that are not linked to a larger event frequently fall into the low media count category as they are often not reported on at all. These smaller incidents could therefore, due to their link to the larger event and subsequent media coverage, lead to a larger behavioural effect than would otherwise have occurred. This could also explain the large and positive estimates of the effect of incidents with high and medium media counts.

Column 2, 3 and 4 report the estimates of the effect of incidents with high, medium and low media counts on the number of fatal, serious and slight accidents respectively. Overall, the significant estimates imply that incidents with a high media count have the largest effect with the effect getting larger, or more positive, two years later. Furthermore, incidents with a low media count have a negative effect, becoming more negative two years later. The number of slight accidents are, once again, the most responsive to incidents where incidents with a high media count in a given year and police force area increase slight accidents by 1,187 one year later and 1,585 two years later. A larger effect than that on either fatal or severe accidents. A similar pattern emerges for fatal, serious and slight casualties in columns 5, 6 and 7. Therefore, while effects of incidents with high, medium and low media counts on total accidents is large this effect is through slight accidents and casualties both one and two years later.

Table 4. 8 - Media Mention Effect on Total, Fatal, Serious and Slight Accidents and Casualties

	(1) Accidents	(2)	(3)	(4)	(5) Casualties	(6)	(7)
	Total	Fatal	Serious	Slight	Fatal	Serious	Slight
lagHighMedia1	1250.1***	-0.326	63.19	1187.3***	-0.764	68.31	1294.6***
	(235.3)	(4.042)	(49.35)	(212.0)	(4.444)	(55.34)	(288.6)
lagHighMedia2	1853.1***	16.78***	251.1***	1585.3***	16.84***	277.4***	1966.4***
	(242.7)	(4.169)	(50.89)	(218.6)	(4.583)	(57.07)	(297.6)
lagMediumMedia1	203.4	1.902	-15.46	217.0*	1.924	-12.56	362.5**
	(133.8)	(2.299)	(28.07)	(120.6)	(2.528)	(31.48)	(164.2)
lagMediumMedia2	574.1***	2.572	92.97***	478.6***	2.618	89.67***	657.9***
	(141.3)	(2.428)	(29.64)	(127.3)	(2.669)	(33.23)	(173.3)
lagLowMedia1	-310.3***	-4.580**	8.574	-314.3***	-5.342***	5.341	-432.7***
	(103.5)	(1.779)	(21.72)	(93.29)	(1.956)	(24.35)	(127.0)
lagLowMedia2	-388.2***	-4.734**	-54.20**	-329.3***	-5.463***	-65.12**	-449.0***
	(110.1)	(1.891)	(23.09)	(99.19)	(2.079)	(25.89)	(135.0)
Total Volume of Traffic	-0.0000209	0.0000165***	0.000333***	-0.000371**	0.0000194***	0.000384***	-0.000501**
	(0.000197)	(0.00000338)	(0.0000412)	(0.000177)	(0.00000371)	(0.0000462)	(0.000241)
N	800	800	800	800	800	800	800

Standard errors are reported in parentheses where * p<0.10, ** p<0.05 and *** p<0.01

All specifications include year and police force area dummy variables.

Each dependent variable is a count of the number of accidents and casualties in the respective category.

The Government MI5 threat level is used as a measure of the effect of a possible threat on road accidents. Table 4.9 reports results using variations of specification (1) where the current threat level dummy variable replaces the incident and multiple incident dummy variables. All specifications control for the total volume of traffic. Note that the number of observations has dropped to 600 due to the use of the smaller subsample. Since this analysis is attempting to determine the effect of the current threat level, lags are not implemented. Furthermore, since only two threat levels, substantial and severe, are implemented within the sample, the substantial dummy variable is chosen as the reference and omitted from the analysis.

The results indicate that a severe threat level will increase the total number of accidents per year by 1, 076 more than a substantial threat level. Furthermore, this large increase is through slight accidents and casualties. This is in line with the media count results implying that a more serious threat level has a larger effect on road accidents than one which is less serious. Given that all the estimates are significant, the results also imply that the possibility of a threat has an effect on road accidents where a more serious threat level increases the number of accidents. This is in line with previous literature on the topic implying that the risk of dread or fear may lead to additional accident fatalities (Gigerenzer, 2004). Therefore, the impending sense of danger that may be felt due to multiple incidents or incidents with a high media count in a given year and police force area may explain the increase in the number of accidents as appose to the decrease in accidents from one incident or incidents with a low media count.

Table 4. 9 - Threat Level Effect on Total, Fatal, Serious and Slight Accidents and Casualties

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Accidents				Casualties		
	Total	Fatal	Serious	Slight	Fatal	Serious	Slight
Severe	1075.8***	25.33***	88.38***	962.1***	27.96***	118.8***	1426.2***
	(82.65)	(1.693)	(18.66)	(80.54)	(1.848)	(20.47)	(108.4)
Total Volume of Traffic	-0.00101***	0.0000162***	0.000278***	-0.00130***	0.0000190***	0.000332***	-0.00155***
	(0.000179)	(0.00000366)	(0.0000404)	(0.000174)	(0.00000400)	(0.0000443)	(0.000234)
N	600	600	600	600	600	600	600

Standard errors are reported in parentheses where * p<0.10, ** p<0.05 and *** p<0.01

All specifications include year and police force area dummy variables.

Each dependent variable is a count of the number of accidents and casualties in the respective category.

Incident related fatalities and injuries are used as an alternative measure of intensity where the variables Fatalities and Wounded are dummy variables representing whether a given year and police force area incurred any incident related fatalities or injuries. Table 4.10 reports results using variations of specification (2) where lags up to two periods of these variables replace the incident and multiple incident dummy variables. All specifications control for the total volume of traffic.

The results indicate that the effect of incident related fatalities occurring in a given year and police force area is to decrease the total number of accidents by 1, 207 one year later and 686 two years later. The effect of incident related injuries occurring in a given year and police force area is to increase the total number of accidents by 380 one year later and 876 two years later. Therefore, the effect of incident related fatalities occurring is larger one year later (in that it decreases the total number of accidents by more) than two years later as appose to incident related injuries which increase the total number of accidents and have a larger long run effect. Furthermore, while it is negative, the effect of incident related fatalities is larger than the effect of incident related injuries in that it decreases accidents by more than the incident related injuries increase accidents. Once again, the significant estimates imply that the largest effect from both incident related fatalities and injuries is through slight accidents and casualties. The negative effect of incident related fatalities is surprising, however, there are fewer fatalities, with a sample mean of 0.05 and maximum of 23, than injuries, with a sample mean of 0.34 and maximum of 145, therefore, while an increase in incident related fatalities are expected to garner more attention, perhaps the sheer number of injuries, compared to fatalities, may be found to be more alarming. Furthermore, there are a greater number of incidents with reported injuries only within the sample than incidents reporting both fatalities and injuries.

Table 4. 10 - Effect of Incident Fatalities and Injuries on Total, Fatal, Serious and Slight Accidents and Casualties

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Accidents				Casualties		
	Total	Fatal	Serious	Slight	Fatal	Serious	Slight
lagFatalities1	-1207.2***	-10.57**	-271.2***	-925.4***	-12.51**	-299.6***	-1455.9***
	(297.5)	(4.834)	(58.23)	(268.2)	(5.308)	(65.22)	(360.8)
lagFatalities2	-685.6*	-11.57*	-400.6***	-273.4	-12.00*	-444.1***	-429.6
	(394.2)	(6.405)	(77.16)	(355.4)	(7.033)	(86.43)	(478.1)
lagWounded1	379.8**	-4.859	24.24	360.5**	-5.000	20.96	417.5*
	(184.7)	(3.001)	(36.16)	(166.5)	(3.295)	(40.50)	(224.0)
lagWounded2	876.2***	4.676	104.8***	766.8***	4.380	110.4***	919.2***
	(193.1)	(3.138)	(37.80)	(174.1)	(3.446)	(42.34)	(234.2)
Total Volume of Traffic	0.000245	0.0000166***	0.000348***	-0.000120	0.0000192***	0.000397***	-0.000206
	(0.000206)	(0.00000335)	(0.0000403)	(0.000186)	(0.00000368)	(0.0000452)	(0.000250)
N	800	800	800	800	800	800	800

Standard errors are reported in parentheses where * p<0.10, ** p<0.05 and *** p<0.01

All specifications include year and police force area dummy variables.

Each dependent variable is a count of the number of accidents and casualties in the respective category.

Single Event Study

Table 4.11 reports results of a single event study, the 7th of July London event, using variations of specification (1) and one dependant variable, total number of accidents, where the various spatial and time spill over variables replace the incident and multiple incident dummy variables. All specifications control for the total volume of traffic.

Column 1 reports the estimates of the effect of the event on the total number of accidents per year locally (Metropolitan police force area) and nationally (Strathclyde police force area) compared to all other areas. The estimate of the event effect on the annual number of accidents locally is not significant and negative. Although the national estimate is significant and positive, the effect is much smaller in that the event decreases the total number of annual accidents locally by more than it increases them nationally. These results imply that the event is associated with 733 more accidents in Strathclyde vs the Metropolitan police force area and other police force areas.

Column 2 reports the estimates of the effect of the event on the total number of accidents per year locally and nationally controlling for a time spill over. The estimates in column 1 measure the effect of the event in the year it took place, however, having occurred in July the event will not affect all road accidents taking place in 2005. Since this is a single event study, the immediate effect can now be accounted for using the 'time' interaction term 'Local_Year', which measures the local effect from the time of the event till the end of 2005. The time spill over is accounted for using 'Local_Next Year' which measures the local effect from the beginning of 2006 till the 6th of July. The two together thereby measure the local effect of the event for 12 months. The variables 'National_Year' and 'National_Next Year' measure the national effect in a similar fashion.

The results imply that the local effect after the event occurred in 2005 is larger, in that it decreases the total accidents by more, than the effect accounting for the entire year. However, this estimate is not significant. Furthermore, the spill-over effect into 2006 is both significant and quite large compared to that of 2005 implying that the event has a larger long run effect locally. Conversely, the event has a larger short run effect nationally with a large, positive and significant 'National_Year' estimate. The national effect is significant but smaller in 2006.

Column 3 reports the estimates of the effect of the event on the total number of accidents per year locally and nationally controlling for a spatial spill over. Before the National group only contained Strathclyde, a further robustness check is conducted by including all police force areas with fast rail systems to the group, namely, Strathclyde, Northumbria and Merseyside creating a new group Transport National. The results are similar to those of column 1 and therefore are robust to two types of national groups. Column 4 reports the estimates controlling for both time and spatial spill overs using the transport related national group. Once again, the results are similar to those of column 2.

When using the transport national group the estimates are all smaller (or less negative) and while the short run local effects are not significant, the long run local effects are significant and larger than the national effect controlling for both Strathclyde on its own and within a transport related national group. So, while the 7th July incident decreases the total number of accidents per year locally compared to increasing them nationally, the effect is larger and lasts longer locally than nationally.

Table 4. 11 - Effect of 07 July 2005 London Event on Total Accidents

	(1) Total	(2) Total	(3) Total	(4) Total
Local	-1346.0		-1323.1	
	(1174.9)		(1174.4)	
National	732.7**			
	(287.3)			
Total Volume of Traffic	0.00123	0.00138	0.00123	0.00138
	(0.000907)	(0.000912)	(0.000905)	(0.000910)
Local_Year		-3315.8		-3267.3
		(2510.6)		(2509.1)
Local_Next Year		-7189.7***		-7164.1***
		(2709.5)		(2706.6)
National_Year		1638.1***		
		(626.1)		
National_Next Year		995.3**		
		(435.6)		
Transport National			611.1***	
			(151.4)	
Transport National_Year				1330.2***
				(331.0)
Transport National_Next Year				737.8***
				(283.3)
N	900	900	900	900

Standard errors are reported in parentheses where * p<0.10, ** p<0.05 and *** p<0.01
All specifications include year and police force area dummy variables.
Each dependent variable is a count of the number of accidents in the respective category.

Table 4.12 reports results on fatal, serious and slight accidents including both the time and spatial spill overs. While the majority of estimates are not significant, the results confirm that the negative local effect of this event on total accidents is through a decrease in slight accidents and, while not significant, this event increases the number of fatal accidents locally, with a smaller short run effect but much larger long run effect locally compared to nationally.

Table 4. 12 - Effect of 07 July 2005 London Event on Fatal, Serious and Slight Accidents

	(1) Fatal	(2) Serious	(3) Slight
Local_Year	6.319	-647.1	-2626.5
	(24.52)	(503.7)	(2055.2)
Local_Next Year	28.93	-335.2	-6857.8***
	(25.88)	(529.8)	(2207.2)
NatT_Year	7.893	125.8	1196.6***
	(6.764)	(77.60)	(276.8)
Transport National_Year	9.514	102.0	626.2***
	(9.247)	(73.42)	(226.8)
Transport National_Next Year	0.0000189**	0.000507***	0.000851
	(0.00000930)	(0.000192)	(0.000731)
N	900	900	900

Standard errors are reported in parentheses where * p<0.10, ** p<0.05 and *** p<0.01
All specifications include year and police force area dummy variables.
Each dependent variable is a count of the number of accidents in the respective category.

4.6 Conclusion

This paper attempts to ascertain whether the occurrence of incidents affects road accidents and hypothesises that this effect would be via either a change in the quantity or quality of driving or both.

The results suggest that overall, the effect of more than one incident occurring in a given year and police force area is positive and large compared to the effect of an incident occurring which is negative and small by comparison. This is expected since, in the event of more than one incident occurring in a given year and police force area any stress caused by an incident will only be intensified by additional incidents. The results also demonstrate that often, this effect becomes larger two years later.

The results suggest that the total volume of traffic increases when an incident or multiple incidents occur. After controlling for it, in all specifications, the effect on accidents and casualties is mostly dampened. It can therefore be deduced that the effect of either an incident or multiple incidents occurring in a given year and police force area on road accidents and casualties operates via a change in the quantity and quality of driving where the effect due to a change in the quality of driving is quite high.

Although it is beyond the scope of this paper to determine why behaviour changes, it is assumed that, as suggested by the literature, the stress caused by an incident affects the way in which people are now driving. Alternatively, perhaps those who switched from public transport to driving may not be as experienced and therefore involved in more accidents.

Finally, the effect found on total accidents is decomposed into fatal, serious and slight accidents. These results suggest that most of the effect is due to a change in serious and slight accidents rather than fatal. The analysis also estimates the effect on casualties and finds that most of the effect is experienced by serious and slight casualties. In both cases the highest effect of multiple incidents occurring in a given year and police force area is experienced by slight accidents and casualties two years later. This implies that while the number of accidents increase, thankfully they are mostly slight.

When decomposing the incidents into attack type, the results suggest that road accidents are more responsive to assassinations and unarmed assault where the first may induce people to drive more carefully and the second to drive less carefully due to stress caused by the incident.

Various robustness checks are conducted including an analysis using high, medium and low media counts where incidents with a high media count have the largest effect on total accidents both one year and two years later, with the effect getting larger, or more positive, two years later, similar to the effect of multiple incidents occurring whereas the effect of incidents with a low media count is to decrease the total number of accidents, more so two years later, similar to the effect of an incident occurring in a given year and police force area.

The Government MI5 threat level is used as a measure of the effect of a possible threat on road accidents. The analysis, which does not include lags, implies that a severe threat level will increase the total number of accidents where this increase is through slight accidents and casualties. These results complement the main findings where, it may be that the greater threat imposed by multiple incidents or incidents with a high media count in a given year and police force area explains the increase in the number of accidents.

The fatalities and injuries caused by incidents are used as alternative measure of intensity where the effect of incident related fatalities is larger than the effect of incident related injuries in that it decreases accidents by more than the incident related injuries increase accidents and the largest effect from both incident related fatalities and injuries is through slight accidents and casualties.

Finally, a single event study using the 7th of July London incident which took place in 2005 is conducted to address immediate time and spatial spill overs. The analysis found that this incident decreases the total number of accidents per year locally, primarily through slight accidents, compared to increasing them nationally and the effect is larger and lasts longer locally than nationally.

This study is limited in that, by using the lagged variables only the short-term effect of incidents occurring in 2017 will be estimated which is a pity given that this is a high incidence year.

Overall, these results demonstrate that the cost of multiple terror incidents is higher than the casualties of the incidents themselves. They incur a greater number of casualties via road accidents and for years later. It is therefore necessary for policy makers to address the behavioural change experienced by people after multiple attacks.

Chapter 5

Conclusion

This thesis provides an analysis of road accidents in Great Britain in three chapters.

The first investigates whether vehicle accident and casualty rates decrease during and after a recession and hypothesises the decrease to be through the quantity and quality of driving. The findings suggest that the rate of accidents that occur during non-working hours and over the weekend, as well as young male casualties are the most sensitive to relative changes in the unemployment rate even after controlling for traffic volume. Furthermore, a subsample using larger geographic areas to allow for changes in commuting patterns produces similar results and the local authority accident and casualty rates are positively related to the total employment rate validating the main findings.

An analysis on 'peak hour' accident rates indicates that the accident rate during the Winter morning peak hours is the most sensitive to relative changes in the unemployment rate with larger elasticities than those of the 'non-working hours' and 'weekend' accident rates, even after controlling for traffic volume. Finally, an analysis, utilizing job density, as an alternative to the unemployment rate, to account for commutes into the local authority, and controlling for traffic volume, finds a negative association where the rate of accidents that occur during working hours and workdays, as well as young male casualties are the most sensitive to relative changes in job density.

The main findings suggest that, during a recession, road accidents decrease through both the quantity and quality of driving however changes in the unemployment rate have a larger effect on the accident and casualty rates through changes in the quality of driving. This may be due to lower opportunity cost of time or, given that the rate of accidents that occur during non-working hours and over the weekend, as well as young male casualties are the most sensitive to relative changes in the unemployment rate, a decrease in alcohol consumption. The analysis would, therefore, greatly benefit from the inclusion of alcohol related road accidents by year and local authority however, the data is unavailable. The addition of this intervening variable may shed further light on the behaviour of motorists during a recession and, consequently, the effect on road accidents.

Overall, more emphasis should be placed on certain policy actions, aimed at reducing traffic accidents during either an economic downturn or upturn. Policy makers should also keep in mind that, during a recession, a decrease in vehicle accident and casualties may not be entirely due to the policy measures already in place.

The second chapter investigates the impact of the Santander Cycle Hire Scheme on accidents and casualties and hypothesises that the scheme will increase the pedal cycle volume of traffic therefore increasing road accidents. After controlling for traffic volume, the results suggest the scheme benefits cyclists by decreasing the pedal cycle accident rate per million miles but does not benefit motorists and pedestrians, increasing the car and pedestrian accident rates respectively. However, the scheme only significantly affects the slight accident and casualty rates therefore this adverse effect on motorists and pedestrians is only through slight accidents. Moreover, these results remain robust to a spill-over effect control, Cycle Superhighway, London Congestion Charge and London Summer Olympics controls. A further analysis also finds that the group of more intensely treated local authorities have a larger effect on the accident and casualty rates than those less intensely treated confirming that the results are due to the Santander Cycle Scheme.

While the car and pedestrian accident and casualty rates have increased marginally, this is only through slight accidents. Furthermore, the Santander Cycle Scheme has led to a large decrease in the pedal cycle accident rate therefore, making roads safer for cyclists.

A pertinent topic discussed over the last year is whether number plates for cyclists should be introduced. An interesting and yet unintentional by-product of the Santander Cycle Hire Scheme is that, by either becoming a member of the scheme or using their debit/credit cards as casual users, the cyclists are, in fact becoming registered. While it is beyond the scope of this analysis to determine whether there is a need for bicycle registration the main findings, that cyclists are the only ones benefiting from the scheme, may provide the ground-work for further analysis regarding other bicycle policies such as possible registration.

The third chapter investigates whether terror incidents affect road accidents and casualties, hypothesising that this may be through a change in the quantity and quality of driving. After controlling for total volume of traffic, the results confirm that the effect of either an incident or multiple incidents occurring in a given year and police force area on road accidents and casualties operates via a change in the quantity and quality of driving where the effect due to a change in the quality of driving is quite high. The quality of driving may be influenced by factors such as increased stress or a greater awareness of one's surroundings. Furthermore, the effect of more than one incident occurring in a given year and police force area is positive and large compared to an incident occurring which is negative and small by comparison. However, most of this impact is due to a change in serious and slight accidents and casualties rather than fatal.

These findings are complemented by an analysis separating the incidents into those with high, medium and low media counts. The results imply that incidents with a high media count have the largest effect on road accidents and coincide with the main findings given that the effect of one incident occurring and the effect of incidents with a low media count is to decrease the total number of accidents whereas the effect of more than one incident occurring and incidents with a high media count is to increase the total number of accidents.

The findings are further complemented by an analysis investigating the effect of the prospect of an attack using the Government MI5 threat level. The results imply that the possibility of a threat has an effect on road accidents where a more serious threat level increases the number of accidents, therefore, the impending sense of danger that may be felt due to multiple incidents or incidents with a high media count may explain the change in quality of driving and subsequent increase in the number of accidents compared to the decrease in accidents from one incident or incidents with a low media count.

Further analysis conducted using attack types suggests that road accidents are more responsive to assassinations and unarmed assault. Fatalities and injuries caused by incidents, used as alternative measures of intensity, have opposite effects where the negative effect of incident related fatalities is larger than the positive effect of incident related injuries. Finally, a single event study using the 7th of July London incident which took place in 2005 finds that this incident decreases the total number of accidents per year locally compared to increasing them nationally and the effect is larger and lasts longer locally than nationally.

Overall, these results demonstrate that the cost of multiple terror incidents is higher than the casualties of the incidents themselves. They incur a greater number of casualties via road accidents and for years later. It is therefore necessary for policy makers to address the behavioural change experienced by people after multiple attacks.

Appendix

Table A 1 - Intensity Effect on Total, Cycle and Car Accident Rates (2000-2014)

	(1) Total	(2) Total	(3) Total	(4) Cycle	(5) Cycle	(6) Car	(7) Car
Intensity	0.00292***	0.00368***	0.00371***	-0.0275***	-0.0238***	0.00426***	0.00436***
	(0.000539)	(0.000712)	(0.000717)	(0.00870)	(0.00913)	(0.000940)	(0.000948)
trendScheme0		-0.0216	-0.0206	-0.00662	0.108	-0.0262	-0.0231
		(0.0132)	(0.0134)	(0.208)	(0.234)	(0.0186)	(0.0191)
trendScheme1		0.00692	0.00748	-0.428**	-0.362*	0.0372**	0.0390**
		(0.0103)	(0.0103)	(0.211)	(0.219)	(0.0179)	(0.0175)
trendScheme2		0.0414***	0.0416***	0.252	0.273	0.0765***	0.0771***
		(0.00882)	(0.00882)	(0.216)	(0.199)	(0.0125)	(0.0126)
Spillover			0.0247		2.976		0.0818
			(0.0463)		(1.966)		(0.0713)
_cons	2.825***	2.802***	2.807***	73.12***	73.79***	4.858***	4.877***
	(0.0957)	(0.0934)	(0.0930)	(3.436)	(3.601)	(0.172)	(0.170)
N	495	495	495	495	495	495	495

Robust standard errors are reported in parentheses where * $p<0.10$, ** $p<0.05$ and *** $p<0.01$

All specifications include year and local authority dummy variables.

Each dependant variable is an accident rate per million vehicle miles for each respective category.

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