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Traffic Accidents and the London Congestion Charge

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Abstract

In a rare effort to internalize congestion costs, London recently instituted charges for traveling by car to the central city during peak hours. Although the theoretical influence on the number and severity of traffic accidents is ambiguous, we show that the policy generated a substantial reduction in both the number of accidents and in the accident rate. At the same time, the spatial, temporal and vehicle specific nature of the charge may cause unintended substitutions as traffic and accidents shift to other proximate areas, times and to uncharged vehicles. We demonstrate that, to the contrary, the congestion charge reduced accidents and the accident rate in adjacent areas, times and for uncharged vehicles. These results are consistent with the government's objective to use the congestion charge to more broadly promote public transport and change driving habits.

JEL Codes: I18, R48, H27

Keywords: Traffic; Congestion externalities; Pricing; Vehicle Accidents

This study analysed existing Road Accident Data and traffic counts data that are publicly available from the UK Data Archive and the Department for Transport, respectively. Further documentation about the data that were used in this paper is available from the Lancaster University data archive at http://dx.doi.org/10.17635/lancaster/researchdata/44."

1. Introduction

Early in 2003 London imposed a daily charge for driving on public roads within its central district. Economists hailed the charge as "a triumph of economics," a recognition by policy makers that congestion is a costly externality and that road pricing is an appropriate response (Leape 2006). While the charge remains flat and so does not vary with distance or time of day, it has been credited with substantial reductions in congestion and increases in travel speed. Less examined is the influence on traffic accidents. While reduced traffic accidents were touted as an additional social benefit, the policy created a series of offsetting behavioral incentives that leave the overall influence on traffic accidents in doubt. Examining this influence requires suitable counterfactuals as the number of London traffic accidents had been trending down prior to the congestion charge.

This paper examines monthly traffic accident counts in central London before and after the congestion charge compared to several sensible controls. We confirm a substantial and robust decline in accidents associated with the advent of the congestion charge. This represents an important public health and social policy finding as resources and lives were saved by diverting travel to safer transport modes and by reducing the aggregate amount of travel. Equally important, we demonstrate that accident rates, the number of accidents per million miles driven, also decline with the advent of the congestion charge. Reduced traffic congestion ameliorated an accident externality (Edlin and Karaca-Mandic 2006) as the congestion charge went beyond simply reducing miles driven and so accidents. It reduced the probability of drivers being in an accident for a given trip to central London.

As the charge is limited to a specific zone, for specific vehicles and for specific hours of the week, we test for substitution effects. These measure the extent to which the charge may increase accidents in areas outside the zone, the vehicle type or the hours to which it applies. Such increases might be anticipated if travelers continue to travel to Central London but substitute uncharged trips for charged trips. Thus, we examine whether or not traffic accidents increase on weekends and evenings (times not subject to the charge). We examine whether or not accidents increase for motorbikes, bicycles or taxis which are all exempt. Finally, we investigate whether accidents increase in areas immediately adjacent to the charge zone as previous through drivers skirt the charge zone or as drivers travel up to the zone and then cross onto public transport. We find no evidence of long-term accident increases in any of these three dimensions. Indeed, traffic accidents and accident rates decline in adjacent areas, out of charged times and for uncharged vehicles relative to controls. This contradicts earlier evaluations that fail to use suitable controls and examine only a shorter window for policy influences.

We also confirm that the decline in total accidents and accident rates in the charged zone is matched by declines associated with serious accidents and with fatalities. These declines also persist in proximate regions and uncharged times. In sum, the evidence suggests that the congestion charge helps in accomplishing the government objective of fundamentally changing behavior regarding the frequency and mode of transit into Central London with beneficial and general reductions in the number of traffic accidents and in accident rates, a point not previously made.

2. Background

Central London has long held a reputation as among the most congested of major Western cities. Over the second half of the twentieth century, traffic speeds decreased and vehicle counts increased. Just prior to imposing the charge, all-day average network travel speeds averaged a sluggish 8.6 mph and more than 1/3 of all travel time was spent simply not moving (Transport for London 2003). When compared to an uncongested speed of around 20 miles per hour, this represented 3.7 minutes per mile of lost time. Multiplied by the huge number of trips and the value of time, the waste was obviously enormous. Fully ninety percent of all London residents (not just those of Central London) agreed in polls that "there is too much traffic in London" and identified congestion as the "most important problem requiring action" (see survey description and references in Leape 2006, p. 157).

At least since Pigou (1924), economists have advocated governmental taxes and charges to bring the actual prices that consumers face into alignment with full social costs. The application of this notion to congested roads dates back to at least Walters (1961) and Vickrey (1963) who emphasize that consumers should pay directly for the costs they impose on other travelers as an incentive to use road resources efficiently. If road space is unpriced, traffic volumes will increase until congestion limits further growth with a resulting waste in travel time and reduction in travel reliability. Additional costs associated with congestion include increased air pollution and increased energy dependence (see Parry et al. 2007).

Despite the advantages of taxing congestion, there exists a long history of public and political opposition that has meant there have been relatively few examples (Harsman and Quigley 2010). In 2007 Stockholm introduced a tax deductible charge to enter the central city with the proceeds used for road construction. In 2013, following a series of temporary charges and lawsuits, Milan introduced a permanent congestion charge with much of the emphasis being on reducing pollution. A charge to enter lower Manhattan in New York City generated a decade of active debate but no action. Voters soundly defeated proposed congestion charges in

Manchester and Edinburgh. The political resistance often coalesces around opposition to a fee seen as largely unrelated to infrastructure cost recovery.[1]

In addition to political resistance, network issues make proper pricing inherently difficult. While pricing a single road between two destinations may be easy, properly pricing a complicated road network like Central London was thought unworkable. Each intersection, road and specific set of combinations contributes to congestion. Moreover, each of these contributes in differing degrees at different times of the day, week or year. Thus, while optimal charges vary by road, intersection and time of day, the creation and enforcement of such charges is likely intractable or infeasible (Newberry 1990; Shepherd and Sumalee 2004). Moreover, the proper pricing may interact in complicated ways with the extent and pricing of parking (Fosgerau and de Palma 2013) and the endogenous choice of speed by drivers (Verhoef and Rouwendal 2004). Thus, the London congestion charge emerged as a rather blunt instrument. It followed the basic approach "to make private transport relatively less attractive and public transport more attractive." (Newberry 1990 p. 35) It combined a flat charge for private and commercial vehicles entering the congestion zone, with the revenues from the charge earmarked for reinvestment in London's public transport.

London imposed an initial daily charge in February 2003 of £5 for driving on roads within the congestion zone between 7:00 am and 6:30 pm on weekdays.[2] The congestion zone is pictured in Figure 1. The original fee has since been increased to £8 in July of 2005, to £10 in 2011 and to £11.50 in 2014. Passes are typically purchased on-line and enforcement relies on a series of video cameras at every entry point to the zone and on mobile units within the zone. A

[1] Adding to confusion, polices are often misleadingly named. Vancouver voted in April 2015 on a "congestion tax" that was merely a general sales tax dedicated to public transit (Sinoski 2015).

[2] Beginning in February 2007, the end of the charge time was moved from 6:30 pm to 6:00 pm, a move we account for explicitly in identifying accidents in the treatment.

license plate recognition system matches against daily purchases and violators are sent penalty notices for escalating fines that average 20 to 30 times the daily charge. The day pass allows travel in and around the congestion zone of Central London. This eight square mile zone includes tourist sites, the City (London's financial district), Parliament, major government offices and prime business locations.

INSERT FIGURE 1

The charge applies to private and commercial vehicles entering the congestion zone during the charging hours. Importantly, motorcycles, bicycles, buses and taxis are exempt. Also exempt, are vehicles belonging to those who live within the zone but keep their vehicles off the street during the charging hours. When these residents do travel during the charging hours, they pay a highly discounted charge of only 10 percent of the full charge.

The revenue raised from the charging program has been substantial but so have the administrative costs (Leape 2006). The net revenue from charges was £97 million in 2004-5 and was supplemented by £70 million in penalties that same year. Such revenues have been largely spent on mass transit improvements with smaller expenditures on road safety and biking/walking initiatives. The earmarking of revenues for such alternative transport is anticipated to continue until at least 2023.

Early indications showed meaningful reductions in distances traveled within the zone. These comparisons of the year immediately before and after the charge showed, for example, that the total distance driven by cars was reduced by an enormous 34 percent (Leape 2006). At the same time, the distances driven by bikes, motorcycles, taxis and buses all increased resulting in a more modest overall decline in vehicle distances of 12 percent. Nonetheless, this was sufficient to reduce the time lost to congestion by nearly 30 percent (Transport for London 2005). Thus, the early indication was clear that the charge reduced congestion during the times it was applied, in the zone to which it applied, and for the vehicles to which it applied. This generates substantial social benefits as the values placed by individuals on reduced travel time and improved reliability are typically large (Small, Winston, and Yan, 2005).

In addition to reducing congestion and so saving time, a critical by-product of the charge was thought to be reduced traffic accidents. While clearly identified as "an additional social benefit" by Transport for London (2005), the logic implying an overall reduction in accidents and its interpretation seems in doubt. First, Shefer and Rietveld (1997) argue that there should be an inverse relationship between traffic congestion and accidents. The increase in speeds allowed by reduced congestion may increase the number and severity of accidents. Certainly, this balancing of time savings and the increased chance of traffic fatalities is at the heart of setting speed limits (Ashenfelter and Greenstone 2004). While the evidence seems to depend on the exact circumstances and perhaps even the type of roads being examined (Wang et al. 2009), the possibility exists that the congestion charge increased vehicle speed and at the same time increased the number of bikes and pedestrians with an uncertain net influence on the number and severity of accidents.

Second, even if the congestion charge reduced the number of accidents by reducing the trips by those charged, there are important avenues of substitution. In the empirical estimation we focus on three forms of substitution. Most fundamentally, those who would otherwise be charged may substitute the nearest uncharged route. As Parry and Bento (2002) emphasize, charging on one route or in one area may simply add to congestion elsewhere and in a complex network it may not be possible to monitor and charge all of these spillovers. Thus, cross traffic that might have gone directly through Central London can be expected to avoid the charge zone but increase congestion in adjacent areas. Commuters might be anticipated to drive up to the charge zone and search for parking before crossing into the zone without their vehicle. Parry and Bento (2002) argue that the increased congestion in alternative areas will increase traffic accidents in these uncharged adjacent areas. Second, those who would otherwise be charged may substitute to uncharged vehicles. As mentioned, buses, bikes, motorbikes and taxis are exempt.[3] Third, those who would otherwise be charged may substitute out of the weekday charge time by rearranging trips to the evenings or weekends. While not every trip might be easily shifted, it seems sensible for a variety of shopping, entertainment and social trips. Thus, in addition to examining the pattern of accidents in the charge zone during the charge time and for the charged vehicles, we will test for the extent of substitution on these three important margins.

Third, even if the congestion charge successfully reduced the number of accidents, the economic lesson remains in doubt. The reduction in accident costs and lost lives might be deemed socially beneficial from a public health perspective but it need not have ameliorated an accident externality associated with congestion. An accident externality exists when a driver recognizes his own risk when taking to the road but does not consider the risk he imposes on other drivers. Borrowing from Edlin and Karaca-Mandic (2006 p. 932), when congestion generates this externality, a one percent reduction in driving should decrease accidents by more than one percent. Thus, we join those studying the traffic externality by examining accident rates, accidents per million miles driven, to determine the influence of the congestion charge.[4] If

[3] Indeed, motorbike dealers ran advertising campaigns encouraging commuters to purchase their product with the slogan "make Mayor Livingston see red," as motorbikes would not be charged and so not contribute to the profitability of the congestion charge.

[4] Those testing for the presence and size of this externality include Edlin and Karaca-Mandic (2006), Saito et al. (2010) and Huang et al. (2013).

the charge simply reduces traffic miles and accidents proportionally, this rate should not change and there would be little evidence in favor of ameliorating an externality.

Others have been concerned with the influence of the congestion charge on traffic accidents. Early comparisons simply examined numbers of accidents in the charging zone before and after the charge (Leape 2006; Quddus 2008). As suggested, this may be problematic both because of substitution out of the zone and also because the trend was of decreasing accidents within the zone prior to introducing the charge. Li et al. (2012) examine a particularly short time frame and show a decrease in car casualties within the zone relative to those happening in the English city of Leeds. For motorcycle casualties they find an *increase* in London compared to Birmingham and, similarly, for bicycle casualties they find an *increase* in London compared to Manchester. None of these authors consider accident rates.

We provide a comprehensive examination of the influence of the congestion charge that examines all accidents as well as serious and fatal accidents. We explore how robust the results are to choice of the control, empirical specification and to varying the time frame. We examine the influence of the congestion charge on charged vehicle accidents within the charge zone and hours. We then investigate the influence of the congestion charge on adjacent regions, times and on uncharged vehicles. To the extent the data allow we also examine this rich set of issues not with accident counts but with the accident rate. In this fashion we shed light both on the public health issue of whether life and limb were saved and also on the economic issue of whether an externality was ameliorated.

3. Data and Methodology

We use road accident data from the Department of the Environment, Transport and the Regions (DETR) that contain all motor vehicle accidents reported to the police from 2000 to 2009 for all 416 local jurisdictions in Britain.[5] We know the type of accident (whether it caused either serious injury or death), the date and time of the accident, location of the accident and the age of the driver of any vehicle involved in the accident. This, when combined with GIS mapping of the congestion zone, allows us to accurately assign accidents to the congestion charge zone in the pre and post policy periods. In addition this allows us to assign accidents to areas that are adjacent to but outside the congestion charge zone (CCZ).

INSERT FIGURE 2

Figure 2 provides initial evidence of the congestion charge effect on traffic accidents in the CCZ. We use as an initial comparison the average monthly accidents per city for the 20 most populous cities in Great Britain (excluding London). The figure shows the accidents in charged times for charged vehicles for both the CCZ and the control group. It demonstrates a declining trend in accidents over the period and shows evidence of seasonality, well known features of traffic accidents in Great Britain which will be controlled for in our estimations. Otherwise the comparison series appears reasonably stable before and after the congestion charge. In contrast, the monthly accidents in the congestion charge zone drop markedly after the congestion charge. Initially accidents in the CCZ are approximately 40 higher per month than the comparison group. This difference essentially disappears after the introduction of the charge.

INSERT FIGURE 3

[5] Available from the UK data archive.

Figure 3 brings these points into sharper relief. It provides linear spline estimates of traffic accidents before and after the introduction of the congestion charge for the CCZ and the comparison group. There is a large reduction in accidents in the CCZ that is coincident with the introduction of charging with reasonably similar trends either side of the change. For the control group there is no evidence of a level change at the discontinuity nor is there a clear change in the trend.

The initial specification estimates the number of accidents per jurisdiction and month in a difference in difference formulation:

$$Acc_{it} = \phi + \delta CCZ_i + \alpha Policy_t + \beta(CCZ_i * Policy_t) + \gamma X_{it} + \tau T_t + \varepsilon_{it} \quad (1)$$

In this specification *Acc* is the number of accidents in the month and area (there are 21 areas, the twenty largest cities and the CCZ), *CCZ* indicates that the accident was within the congestion charge zone, *Policy* indicates that the accident happens after the date of the congestion charge policy, *T* is a linear time trend, *X* a vector of controls. The key parameter of interest is β which provides the difference in difference estimate of the effect of the congestion charge on accidents.

Subsequent estimates modify eq. (1) by adopting an alternative dependent variable, the accident rate. The rate measures the accident count in the relevant jurisdiction and time period divided by the miles driven (in millions) in the jurisdiction and time period. The miles driven (or traffic flows) come from critical nodes or segments of roadways monitored by the Department of Transport with the number of vehicles passing through each segment and the length of each segment used to generate the reported miles per jurisdiction.[6] We follow this same methodology to develop a measure specifically for the London congestion zone. The data on flows identifies

[6] The miles driven in Birmingham, for instance, are built up from 161 monitored segments.

the type of vehicle (automobiles, bikes, etc.) but is available only on an annual basis. Rather than divide the number of accidents in each month of the year by a constant annual estimate, we simply aggregate the rates measure within each jurisdiction: the annual number of accidents divided by the annual miles driven. While this dramatically reduces the sample size, it more accurately reflects the underlying variation in the data.

Several empirical challenges exist when identifying the effect of the congestion charge on accidents and accident rates. It is well known that during our period of analysis, traffic accidents and fatalities have generally been declining in England and in central London in particular (Department of Transport, 2013). We will explore the underlying parallel trends assumption in a flexible version of (1) where time trends are allowed to vary between the treatment and control. We include controls for jurisdiction area and annual measures for jurisdiction population (Green et al. 2014). We begin by contrasting the congestion charge area, time and vehicles to the controls of the 20 largest cities in Britain for the charge time and vehicles. We cluster standard errors at the jurisdiction level but ultimately experiment with this as well suggesting that the pattern we identify is robust.

We then move beyond this to allow the data to determine a synthetic control that optimally weights the various 20 cities to match the underlying characteristics of the treated CCZ (Abadie and Gardeazabal 2003 and Abadie et al. 2010). The matching process minimises the mean squared prediction error (the average number of accidents per jurisdiction in the CCZ minus that in the synthetic control) for the pre-policy periods. The resulting control exhibits the most similar traffic accident pattern to that observed in the CCZ before the passage of the congestion charge and is then compared to the CCZ in a straightforward difference in difference.

The likelihood that the congestion charge will have influenced traffic patterns in neighboring areas leads us to remove all other areas of London from our control group from the start. In subsequent analysis we explicitly seek to examine these geographic spillovers among other types of spillovers. We will also separately focus on serious and fatal accidents and explicitly consider other related policy changes during our data window.

4. Empirical Results on Accident Counts

The first column of Table 1 provides a difference in difference estimate of the policy effect following the specification in (1). This specification includes both a trend variable revealing the downward trend in accidents and its interaction. It also includes monthly dummies to capture the evident cyclicality seen in the raw data. In a pattern, often noted in the British data, the fourth quarter is found to have the highest number of accidents (the last three months of each calendar year).[7] Area and population controls behave as anticipated from earlier studies with jurisdictions with greater area having fewer accidents and those with greater population having more accidents. The critical policy estimate reveals that the congestion charge is associated with approximately 40 fewer accidents per month in the CCZ when compared to the other 20 cities. As the pre-policy monthly average in the CCZ was 111 this represents roughly a 35 percent decline.

INSERT TABLE 1

The underlying accidents within a given area can be viewed as generating a count variable. This may have implications for both our point estimates and their precision. The next

[7] This pattern is also obvious in an otherwise identical specification that replaces the monthly dummies with quarterly dummies. The estimated policy influence is not materially changed in such a specification.

two columns of Table 1 examine this by estimating both Poisson and negative binomial models of accidents. We note that there is no concern with zero inflation as none of the jurisdictions have a single period with zero accidents. The results mirror those already presented as they show statistically significant and large reductions in the number of accidents associated with the introduction of charging. These reductions of 32-36% correspond closely with the magnitude from the OLS estimates. The null of no over-dispersion of the dependent variable is rejected at the 1 per cent level. Thus, the model is more correctly estimated via negative binomial than Poisson. In an effort to determine whether we should continue to use the negative binomial, we calculated the mean squared residuals for both the negative binomial and the original linear specification in column (1). They were very similar but that for the linear specification did slightly better (1154.4 vs. 1155.9). Critically, we found no specification in which the linear estimate returned a significant policy reduction but the negative binomial did not. Thus, in estimates in subsequent tables we focus on OLS estimation but will also typically provide the percentage measure from the negative binomial for ease of comparison.[8]

A careful examination of Figure 2 suggests that the cyclical pattern evident in the CCZ may differ materially from that in the control. It appears the peak of the cycle for the CCZ is earlier in the year raising concern that this out of phase cyclicality may play a role in our estimates. To examine this concern we interact every monthly dummy with the CCZ to allow for separate cycles between treatment and control. While cyclical differences were confirmed with five of interactions proving significant, the differences proved of no consequence to the policy estimate. This estimate is shown in the fourth column of Table 1 and continues to indicate a highly significant decline of approximately 40 accidents per month.

[8] We also estimated both a simple logistic estimate and a linear estimate that controlled for jurisdiction fixed effects with no meaningful change in either significance or magnitude.

One feature of the data illustrated in Figures 2 and 3 is the large differences in average accident levels between the CCZ and the average of other cities in Great Britain. This reflects the unique position of central London in terms of activity and traffic density. This might cause concern regarding the suitability of our control group. To address this we adopt the synthetic panel approach as set out by Abadie and Gardeazabal (2003). This involves optimally weighting the comparison group to match the pre-treatment accident data for the CCZ. As a result of this weighting, the mean squared prediction error between the CCZ and the control was reduced from over 1000 using the 20 largest cities to only 20.4 with the optimal weighting of those cities. All cities took a positive weight in the optimal match although many received only a couple of percentage points. The largest weights were given to Birmingham, Leeds, Manchester and Liverpool. These four major cities took the large majority of the weight.[9]

INSERT FIGURE 4

The result of the matching is demonstrated in Figure 4. This shows a very close match between the pre-accident levels and trends for the CCZ and the synthetic control group, followed by a marked reduction in accidents post charge introduction. The corresponding point estimates from the difference-in-difference are reported in the final column of Table 1. These suggest that congestion charging reduced the number of accidents by more than 28 per month. Thus, there are differences between a not weighted and optimally weighted control but the basic result of a large decline remains apparent. Critically, the fact that the optimal weighting scheme includes all cities indicates that it is superior to simply using a single alternative jurisdiction as the control (as done in Li et al. 2012).

[9] It is important to note that the match is between only a part of London, the congestion zone, and an already selected set of the nation's largest cities. Those cities enter in their entirety.

As a further examination, we conducted the series of placebo tests suggested in Abadie et al (2010) by iteratively applying the synthetic control method used above to estimate the effect of the congestion charge in every other city in the control group. Successive iterations reassign the Congestion Charge intervention to one of the 20 largest cities and move the Congestion Charge Zone to the control group. Thus, we proceed sequentially through imagining each city in the control group passed the Congestion Charge in 2003 instead of London. We then compute the estimated effect associated with each placebo run. This iterative procedure provides a distribution of estimated policy effects for the cities where no intervention took place. The CCZ reduction of 28 accidents per month takes by far the largest difference-in-difference coefficient among the 21 estimated. The next largest coefficient shows a decline less than one-third that size and most coefficients from the placebos are essentially zero. The related ratio of the mean squared prediction error after the policy to that before the policy is also by far the largest for the CCZ.[10] Thus, the iterative procedure suggests that if one were to take a placebo test at random, there is little chance of finding results the size of that for the CCZ.

An additional concern may be that identification of the key parameters in Table 1 come from a change in policy by a small number of groups (one single local authority) in a relatively small number of overall groups. Clustering at the local authority level in this case can cause the reported standard errors to be misleadingly small. In response we return to the estimates in Table 1 and implement the Wild bootstrap procedure from Cameron et al. (2008). This dramatically reduces the high type I error rates common in the presence of clustering on a small number of groups. The procedure replicates the within group correlation in errors when generating new

[10]Birmingham has the largest mean squared prediction error for the entire study window. Indeed, it is so large (more than 3 times larger than the CCZ and 10 times larger in pre-policy period) that it might sensibly be excluded from the analysis as there is no combination of other jurisdictions that match its time series (Abadie et al. 2010 p. 502). Nonetheless, Birmingham returns an insignificant difference-in-difference coefficient.

estimates. Under the null hypothesis of no difference in difference effect, the Wild bootstrap p-values clustered at a local authority level with 10,000 replications are presented in the Appendix Table for the three linear specifications. All three p-values suggest statistical significance at common thresholds when using the preferred Rademacher weights.[11] Moreover, the supportive evidence from the series of placebo tests provides an alternative inference procedure recognized as appropriate in the face of a small number of clusters (Cameron and Miller 2015 p. 349). Thus, on balance, we believe the CCZ is associated with a large and meaningful decline in the number of traffic accidents.

The Spillover Effects of Congestion Charging

The prior analysis indicates that the congestion charge reduced accidents involving treated vehicles within the congestion zone and time. Yet, these estimates may dramatically differ from the full influence of the charge. The estimates may overstate the full influence if traffic moves into uncharged times, regions or vehicles. In the extreme, one might fear that accidents are simply displaced and not truly reduced rendering the previous estimates largely meaningless. The alternative is that the policy influence identified earlier spillovers over actually reducing accidents in adjacent regions and times. This seems at least plausible as the charged zone is at the center of a hub and spokes. It thus eliminates vehicle trips that would have come into the central district only after crossing many of the adjacent areas. Moreover, an explicit objective of the congestion charge zone policy was to encourage broader use of mass transit and this increased use could carry over to times outside the charged hours and areas.[12] Thus, we test for the broader influence of the congestion charge by measuring the substitution effects, the

[11] We recognize that with only a single treatment and control the wild bootstrap may not generate appropriate standard errors for the synthetic control estimate.
[12] Recall that the net revenue from congestion charge is earmarked to improve mass transit.

extent to which the charge influences accidents in areas outside the zone, the vehicle type or the hours to which it applies.

INSERT TABLE 2

Table 2 estimates variants of model (1) from Table 1 (i.e. difference in difference allowing for differential trends) for potential margins of substitution. First, we use GIS to identify all accidents outside the CCZ but within 2 kilometers of the CCZ boundary. We identify this as spillover region 1. We then identify all accidents outside the CCZ and outside spillover region1 but between 2 kilometers of the CCZ and 4 kilometers from the boundary of the CCZ and identify this as spillover region 2. The monthly accidents within each of these spillover zones is then used in place of those in the CCZ in a model that otherwise replicates Table 1 by comparing them to the accidents in the 20 largest English cities during the congestion charge times. As the first two columns of Table 2 show, there is no evidence of substitution. Not only does the number of accidents in these two regions fail to increase as a result of the congestion charge, but they significantly decrease. These effects are sizeable suggesting between 10 and 12 percent less accidents per month in each of these spillover areas. Thus, the response to congestion charge appears to be a reduced number of journeys through these areas into central London or an increase in the number of people who travel through these areas by mass transit. In either case, the reduction in accidents within the CCZ is clearly an underestimate of the full number of accident reductions.

We next examine what happened to the number of accidents occurring outside of the business hours, five days a week, in which the charge is levied. Again, trips that might have happened at these peak times (for shopping for example) may simply be postponed till later in the evening or the weekend. This would also cause a displacement in accidents rather than a reduction. Column 3 compares the accidents in the CCZ but out of charged hours to the accidents in the 20 largest cities out of charged hours. There is no evidence of displacement and, indeed, the out of hours accidents in the CCZ actually decline significantly relative to the control. This may reflect a general change in behavior and preferences that simply reduced the likelihood of driving into the CCZ. Moreover, the funds raised by the charge improved transit options and some of these improvements likely remain for out-of-charge times. Finally, some of the discouraged trips might be one way during the charge time and other way outside the charge time and so might otherwise have been partially associated with out-of-charge time accidents. The point remains that the congestion charge is associated with fewer accidents not only in the charged zone and time but outside the charged zone (but nearby) and outside of the charged time.

Finally, we examine the accidents in the CCZ and charged hours that involve at least one uncharged vehicle. Again, commuters can substitute away from charged automobiles to these taxis, buses, motorcycles and bicycles. Indeed, the traffic flows indicate that the miles in uncharged vehicles increases from 91 million miles before the charge to 119 million miles after the charge. Thus, one might anticipate an increase in accidents among these vehicles. We compare their accidents to those that involve at least one uncharged vehicle during the charged hours in the 20 largest cities. The estimates find a marked reduction in accidents involving these vehicles of around 12 percent. This may reflect fewer charged automobiles on the road and that this decreases the odds of the uncharged vehicles being in an accident even as the miles of uncharged vehicles actually increased.

We emphasize that while the estimates in Table 2 simply retain the 20 largest cities as the control, the results are robust to the matching procedure. In estimates available from the authors, we created a new synthetic control for each spillover examination (two on area and one each on time and vehicle type). The estimated coefficients on the difference-in-difference are very similarly sized to those in Table 2 and indicate in each case a significant reduction in the number of accidents in targeted spillover relative to the relevant synthetic control.

The critical point is that we have found no evidence of substitution in which uncharged adjacent areas, hours or vehicles have increased accidents as a result of the congestion charge. Instead, the influence of the congestion charge appears substantially larger than would be indicated by limiting the analysis to the zone, time and vehicles directly charged. Indeed, the reduction in accidents in the charged zone, time and vehicles is actually smaller than the sum of reductions in other areas, times and vehicles. Thus, there seems to have been a more general and fundamental change in the number of trips and/or mode of transportation.

INSERT TABLE 3

Serious Injuries and Fatalities

While the reduction in accidents is large and widespread, this need not translate into a lower incidence of accidents involving severe injury or death. As discussed, the higher road speeds associated with the congestion charge may increase the severity of the accidents that do occur. Minor accidents at a slow speed can involve serious injuries or death at a higher speed. Moreover, the potential substitution towards vehicles with a greater inherent danger of serious injury, such as more accidents involving automobiles and bicycles, also suggests that even though there may be fewer accidents there may be more accidents with serious consequences. In addition to this ambiguity, examining accidents that involve hospitalization and death are critical for at least two reasons. First, such accidents likely constitute the bulk of the social costs associated with traffic accidents and so are of strong policy interest. Second, such accidents are subject to less measurement error as they are much more likely to be reported and recorded in the administrative statistics.

Table 3 examines the influence of the congestion charge by re-estimating our main model for all serious and fatal accidents and then for only fatal accidents. The estimates are limited to the CCZ and for accidents involving a charged vehicle in charged times. They are, of course, smaller in absolute terms as serious and fatal accidents happen less frequently than do all accidents. Yet, they still remain negative and statistically significant. Moreover, in percentage terms these emerge as very large effects (25% and 35%). The estimates indicate that the congestion charge reduced the number of serious and fatal accidents in the congestion zone by 43 a year and reduced the number of fatalities by 4.3 a year.

The monetary savings associated with the congestion charge and these reductions in accidents can be roughly calculated from the estimated value of the direct and indirect costs of avoided accidents from the UK Department for Transport (DOT, 2013 p. 39). These estimates provide "valuation of both fatal and non-fatal casualties that has been based on a consistent willingness to pay (WTP) approach. This approach encompasses all aspects of the valuation of casualties, including the human costs, which reflect pain, grief, suffering; the direct economic costs of lost output and the medical costs associated with road accident injuries" (UK Department for Transport 2013 p.11). The costs are £1,914,229 for an avoided fatal road accident, £281,109 for an avoided serious accident and £22,773 for avoiding a neither fatal nor serious accident (slight). Our estimates indicate reductions within the CCZ for charged times and vehicles of 4.3 fatal accidents, 38.7 serious injury accidents and 427.68 accidents that are neither fatal nor serious. These aggregate to £28,849,659 (2012 UK Pounds) in avoided costs per annum.

While modest relative to the total of all charges, this benefit does not include the reduction in accidents in uncharged times, locations and vehicles, the saved time due to reduced congestion or the value of the charges reinvested in the transport system.[13]

As this point makes clear, the issue of spillovers and substitution can be critical in assessing the full influence of the congestion charge. We reproduce the estimates in the two adjacent uncharged regions, for the uncharged hours and for uncharged vehicles within charged hours. In each case the number of serious and fatal accidents falls relative to similarly constructed controls. The declines remain large and significant with percentage declines ranging from 11 to 23 percent. As in the case of all accidents, failure to recognize the reductions in adjacent areas, times and uncharged vehicles would grossly underestimate the true influence of the congestion charge on serious and fatal accidents.

5. Empirical Results on Accident Rates

The estimates of the previous section confirm a large decline in the accident count in the CCZ that is not offset by spillovers into uncharged times, vehicles or adjacent regions. This is an important policy finding as it indicates that lives were saved and costs avoided by the congestion charge. Yet, it does not necessarily suggest that reducing congestion ameliorated an accident externality. If the flow of traffic fell by 10 percent and the accident count fell by 10 percent, the odds of having an accident remain unchanged and there would be no reason to suspect that additional drivers both assume the accident risk and increase it for others. To examine the possibility that a congestion externality caused accidents and was ameliorated by the charge we now turn to estimating the accident rate.

[13] In addition, alternative estimates of the value of a statistical life in academic studies are often higher than those used by the UK Department for Transport. See for example Bellavance et al. (2009) and Viscusi and Aldy (2003).

The estimation strategy remains broadly similar but the dependent variable is measured as the number of accidents in the jurisdiction divided by the number of miles (measured in millions) driven in the jurisdiction. The availability of the traffic flow data forces us to move to annual data and raises the issue of how to deal with data from 2003 as the policy began in February of that year. As a conservative approach, we simply drop the year from the analysis although if we include it in the treatment period, the results are very similar. The pre-policy period saw average annual driving of 582 million miles in the congestion zone and this fell in the post-policy period to 500 million miles. At issue is whether or not the number of accidents declined sufficiently relative to this drop in miles that, as a consequence, the accident rate in the CCZ fell relative to the control.

Columns 1 and 2 of Table 4 present the estimates of the influence of the charge in the CCZ against the 20 largest cities. The first estimate mimics our original estimate for the accident counts by allowing for differential trends and using area and population as controls. The annual data necessarily eliminates the cyclical controls. The point estimate indicates that the congestion charge is associated with almost exactly 1 fewer accident per million miles driven. The average accident rate in the pre-policy period in the congestion zone is 4.51 accidents per million miles suggesting the *rate* fell approximately 22 percent following the policy. This percentage decline is obviously smaller than that in the accident count but it remains highly significant. Column 2 reproduces the estimate using the synthetic cohort approach. It returns an estimate that places the greatest weight on Liverpool, the sixth largest of the 20 largest cities but with weight also on Birmingham, Leicester and Kingston-on-Hull. The estimate is broadly similar indicating 1.2 fewer accidents per million miles in the CCZ following the policy change.

INSERT TABLE 4

If miles driven were reduced and accidents simply fell proportionally, there should be a coefficient of zero on the difference-in difference in the first two columns of Table 4. Instead, our data show that while the policy caused a substantial reduction in traffic miles, the decline in accidents was even larger causing the accident rate to fall. Thus, the odds of being in an accident fell for those continuing to drive into Central London after the policy change. The congestion charge improved their safety, an indication that the reduction in congestion caused by the charge helped ameliorate a traffic externality.

The estimates provide a simple way to decompose the reduction in accidents into those directly associated with reduced traffic flows holding the accident probability constant and those indirectly associated with reduced traffic flows through the reduced accident probability holding miles constant. The pre-policy accident rate was 4.51 accidents per million miles and the policy was associated with a reduction of 82 million miles driven. This implies a direct reduction of 369.8 accidents per year (4.51x82). The number of miles driven after the policy change remained 500 million miles but the accident rate fell 1.00 per million miles for an indirect reduction of 500.0 accidents per year. Thus, the reduction in the probability of an accident appears to be more important than the simple reduction in miles driven. This comparison helps spotlight the importance of the charge in ameliorating the traffic congestion externality.

It might be argued that the rate reduction could reflect a changing composition of drivers. In this view the charge deters the inherently more accident prone from driving rather than reducing a congestion externality. Although the exact path of such causation seems unclear, we use the measures above to identify the size of the required compositional change. The drivers of the 82 million fewer miles driven as a result of the charge would need a large enough accident probability that when deterred by the charge they cause the rate to fall by 1.00 accident per million miles. This would require those drivers to have an accident rate of over 10.6 accidents per million miles or three times that of the drivers remaining after the charge.[14] This large degree of sorting seems unlikely to us and even whether charge deters the inherently accident seems debatable but we admit we have no direct evidence on sorting.[15] Nonetheless, if the policy does deter the most inherently accident prone who are most likely to impose costs on others, it may still improve welfare if these costs are not otherwise fully internalized.

Again, concern about the number of clusters causes us to estimate the clustered standard errors using the wild bootstrap. As shown in the Appendix Table, for both estimates in Table 4 and for both weighting schemes, the results remain highly statistically significant.

The third and fourth columns of Table 4 examine the influence of the congestion charge in the adjacent areas. We again examine both a zone 2 kilometers outside the CCZ and from two to four kilometers outside the CCZ using the 20 largest cities as controls. In both cases the policy generates significant declines in the accident rate. There are .74 fewer accidents per million miles in the smaller zone and .75 fewer accidents per million miles in the larger zone. While smaller declines than in the CCZ, they argue that the pattern observed earlier with the number of accidents is large enough that the rate declines making travel through these adjacent areas safer for those that continue to do so.

In Table 5 we examine the rates of accident that result in serious injuries or fatalities. In the first column, we return to the CCZ and limit the rate measure to the number of accidents that result in serious injuries or deaths. Thus, the number of such accidents in each jurisdiction is divided by the miles driven in each jurisdiction. The evidence indicates a decline in the rate of

[14] 3.51(500/582) + X(82/582) = 4.51 implies that X = 10.61.
[15] We note that the charge is not associated with dramatic changes in the age of the drivers actually involved in accidents. The young and old are recognized to be at higher risk of traffic fatalities yet the average age of fatalities is 39.3 both before and after the charge and the standard deviation actually increased. For accidents overall the average age increases from 37.4 to 39.0 but the standard deviation again increased.

such accidents by almost .16 of an accident per million miles. This highly significant decline is matched by a decline in the rate of fatal accidents alone as shown in the second column. Here the estimate shows a decline of roughly .02 of an accident per million miles.

INSERT TABLE 5

In the third and fourth columns of Table 5 we examine the rates of accidents that result in serious and fatal injuries in the two adjacent regions. They show a now familiar pattern. The estimate for the two kilometer zone is a highly significant decline of .22 accidents per million miles. In the four kilometer zone the estimate is a still significant decline of .14 accidents per million miles. Thus, just as for the accident rates in general, the accident rates for serious and fatal accidents decline not only in the CCZ but in adjacent areas as well. In total, the evidence on the rates is consistent across the estimates. The declines we identified in accident counts in the previous section are sufficiently large that even given the decline in miles driven, they reduce the probability of an accident.[16] Thus, the congestion charge has made the roads safer for those that continue to drive on them.

6. Additional Robustness Checks and Policy Variations

A particular concern of policy makers has been the hazard faced by bicycle riders. Indeed, Li et al. (2012) suggest that the congestion charge led to an increase in accidents and serious injuries by those on bikes. We return to this using our preferred specification and limiting our dependent variable to accidents involving bikes. These results are reported in Table 6 where we provide estimates for all accidents and for serious and fatal accidents. Critically, we initially

[16] Indeed, the accident and KSI rates for uncharged vehicles within the CCZ also decline significantly and the associated estimates are available upon request.

show our longer evaluation window rather than the short window ending with 2005 as done in the previous study. Contrary to that previous evidence, we find a reduction in bike accidents that fits with the evidence for other types of spillovers.

INSERT TABLE 6

In an attempt to reconcile our results with this previous evidence we limit our estimation window to successively smaller post-policy periods. Reducing our period to the end of 2006 substantially reduces the size of the policy effect, and further trimming the period to the end of 2005 recovers the congestion charge increasing bike accidents, both overall and for serious and fatal accidents. Thus, an appropriate summary would be that there existed a short-term increase in bike accidents that dissipated and reversed. This fits with new inexperienced bicycle commuters initially flooding the congestion zone. Yet, this eventually became dominated by the underlying lower probability of traffic accidents as the riders either gained experience and ability as commuters or found alternative modes of transport.

In Table 7 we return to measuring accident rates. We create a bike accident rate by dividing the number of accidents involving bicycles in each jurisdiction by the total miles driven by bicycles in the jurisdiction. These rates become the dependent variable with evaluation windows of differing lengths. When examining all accidents in the first three columns the shortest window yields the smallest reduction in the accident rate but it is still a reduction. When examining the serious and fatal accident rates, the window of 2000 to 2005 window shows no improvement in the accident rate. Thus, we find no evidence even in the short term that biking became more dangerous. Instead, there was a 66 percent increase in the flow of bike miles in the post policy congestion zone and the short term increase in bike accidents largely reflects this flood of new bikers.[17] The overall rate of accidents declines even in the shortest policy window indicating that the zone was safer for bikers and highlighting the importance of examining both accident levels and rates.

INSERT TABLE 7

The Effect of Later Policy Changes

Our last step is to examine two additional sources of variation in the original congestion charge policy intervention. In the first source of variation we recognize that the original congestion charge was set at £5 but that this was subsequently increased to £8 in 2005.[18] We use this, in combination with variation in the consumer price index, to generate an annual real congestion zone charge in 2003 pounds. In the first column of Table 8 we replace the dummy variable for the policy with this real congestion zone charge as a measure of policy intensity. The resulting estimate indicates that each real pound in the charge causes a reduction of 5.2 accidents per month in the charged area. When examining the accident rate, each pound in charge causes a decline of a highly significant .001 accidents per million miles.[19]

INSERT TABLE 8

The second major source of variation was a temporary extension of the original congestion charge zone to incorporate more western areas (the so-called western extension). This extension occurred on February 17, 2007 but charging for the extension was removed on December 24, 2010. Mayor Boris Johnson was quoted shortly after the removal saying that the

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[17] This compares with only a 2 percent increase in the flow of bike miles in the control jurisdictions.

[18] Additional charge increases in 2011 and 2014 are outside our evaluation window.

[19] An alternative test might imagine separate policy variables for the initial CCZ and for the subsequent price increase. While both variables take negative coefficients, this estimate indicates the overwhelming majority of the influence is associated with the initial CCZ.

removal did not substantially increase congestion and “there has been no significant downside in removing the western extension zone (London24).” In part this may reflect that the extension always included “free through routes” that were never charged. Also, in part, our previous analysis of spatial spillovers suggests that adjacent areas, including the western extension, were, in effect, already partially treated. The traffic through this area was reduced by the initial congestion zone charge.

INSERT TABLE 9

We test the consequences of the Western Extension in two related fashions. For each test we extend our original model to incorporate the Western Extension as a separate treated jurisdiction (mapped by GIS and matched to traffic accidents). In the first test we include both the CCZ and the Western Extension but use a single treatment indicator that turns on for the CCZ in February 2003 and on for the Western Extension in February 2007. In the second test we again consider the two treated areas, the CCZ and the Western Extension, but estimate two separate difference-in difference estimates. To allow for the complicated dynamics associated with the likely spillovers, we include trends for the control (the other English cities), the CCZ and the Western Extension for both the pre-treatment and treatment period.

As shown in the first column of Table 9, the first test suggests that the treatment on the two combined regions is associated with a highly significant reduction of 39.6 accidents per month. The second test in column 2 suggests that the implementation of the CCZ is associated with a highly significant decline of 42 accidents within the CCZ and that the implementation of the Western Extension is associated with a significant but more modest decline of 6.8 accidents per period within the Extension. This pattern carries over to the estimates with rates. The third column shows that the treatment (differently timed for the two regions) is associated with a significant reduction of .79 fewer accidents per one million miles. The separate estimates in the fourth column show two significant influences. The original CCZ is associated with a reduction of .83 accidents per million miles and the extension brought an additional but smaller reduction of .50 accidents per million miles. The critical point from our perspective is that there is no evidence to suggest that the implementation of the Western Extension is somehow inappropriately generating the fundamental results we have been showing for the CCZ. The policy appears to have reduced both the number of accidents and the accident rate.

5. Conclusion

In what has been hailed as a triumph of economics, London has since 2003 charged drivers to enter the central congestion zone. While other cities, including Singapore, Milan and Manchester, have either implemented or considered such congestion charges there has not yet been a huge movement to mimic London. The advantages of reduced congestion include improved travel times and reliability, reduced air pollution from vehicles stuck in traffic and, potentially fewer traffic accidents and lost lives. Theoretically the increased speed may work to mitigate reduced congestion by increasing accidents and their severity and substitution away from the charged zone, hours and vehicles may also reduce or eliminate any net reduction in accidents.

We have undertaken a comprehensive examination of the consequences of the London congestion charge on vehicle accidents and accident rates. We find a substantial and significant reduction in the number of accidents in the charged zone for charged vehicles and times relative to sensible controls. This persists for serious and for fatal accidents. Critically, there is no evidence that the congestion charge resulted in a permanent increase in accidents for uncharged times, adjacent geographic regions or uncharged vehicles. Indeed, we find evidence of reductions and these results also persist for serious and for fatal accidents.

These findings argue that the congestion charge saved accident costs and lives but, in themselves, do not demonstrate that the charge helped solve an externality in which one driver imposes expected accident costs on others. This demonstration is provided by our finding that accident rates also dramatically declined. The probability of having an accident in Central London fell as a result of reducing traffic congestion. Indeed, our back of the envelope calculation suggests that this decline in the probability of an accident was more important in saving accidents and lives than was the reduction in total miles driven. Thus, by reducing congestion, the charge both saved lives by moving people out of automobiles but also by making the commute safer for those that continued to drive automobiles.

The importance of examining both accident levels and rates reappeared in our bicycle estimates. In a very short window, we confirmed earlier studies by showing that the number of bike accidents increased. Yet, this increase reflected only a flood of new bikers and bike miles as the accident rate for bikers actually decreased as a result of the congestion charge. Bike riding became safer after the policy.

We view the sum of our evidence as broadly consistent with the intention of the government to use the congestion charge as a mechanism to move travelers out of automobiles and into public or alternative transit. The charge discouraged the use of automobiles and the funds raised by the charge were spent on improving public transit, largely bus lines, which saw a large increase in bus ridership (Transport for London 2004). While we have focused on only one benefit from the charge, accident reduction, other benefits include increased speed, travel reliability and reduced air pollution. Obviously, important distributional aspects have also not been examined. Customers may have moved away from central city shops and entertainment as an example. Nonetheless, we provide the most comprehensive examination of the influence of the charge on traffic accidents and rates and find important reductions in lost money and lives as well as safer roads for those that continue to drive.

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Figure 1: The original London Congestion Charge Zone

Source: Transport for London (2004 p.8)

Figure 2: Accidents involving charged vehicles in charged times, CCZ vs the 20 largest cities in Great Britain

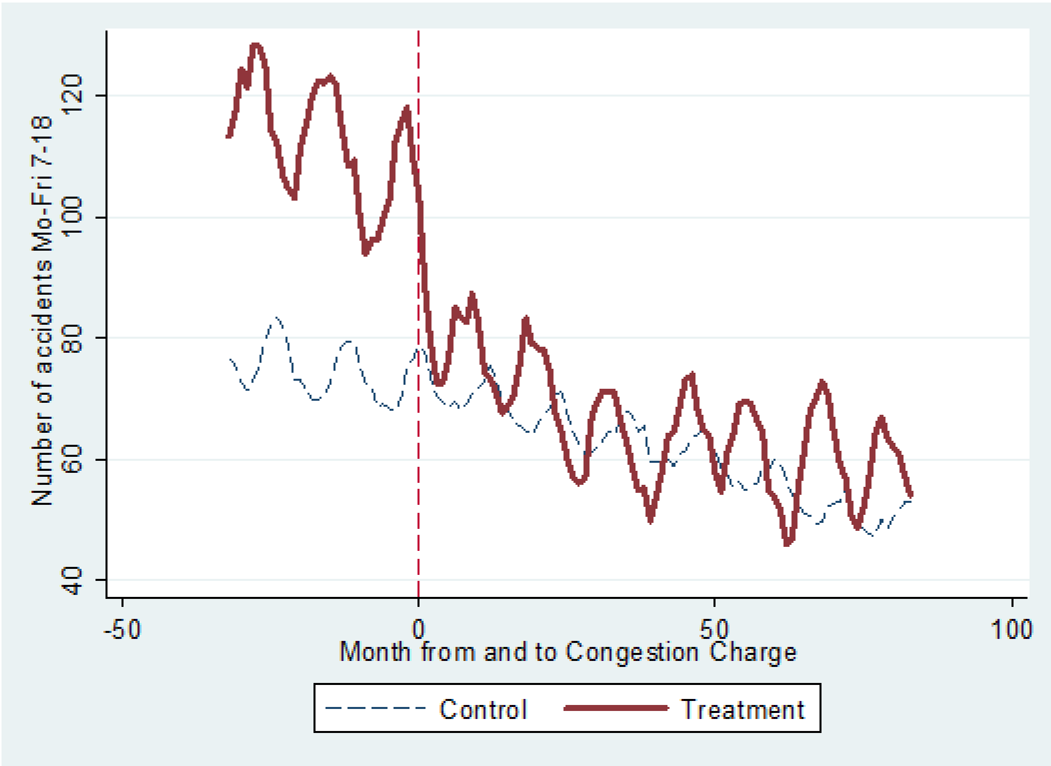
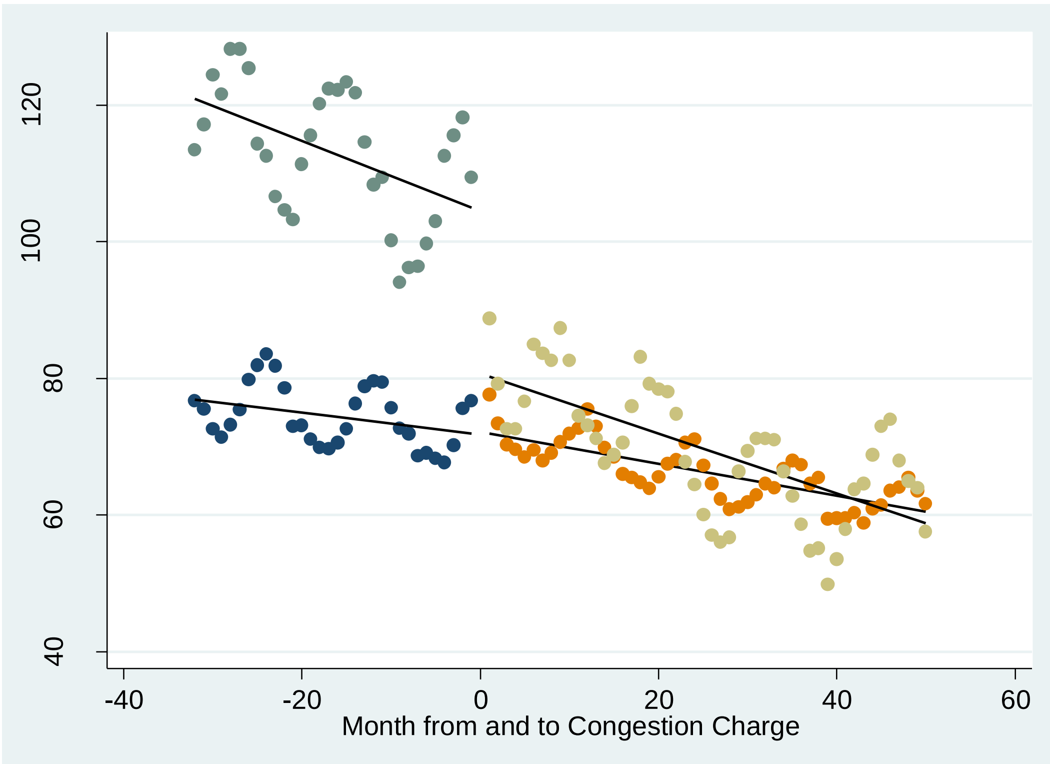


Figure 3: Spline Regression for Number of Accidents: Charged vehicles, charged hours in CCZ vs the 20 largest cities in Great Britain



Note: The discontinous upper lines are the number of accidents per month in the CCZ while the much more continues lower lines are the average number of accidents per month in the other 20 largest cites.

Figure 4: Synthetic Cohort Estimates of Congestion Charge Effect on Traffic Accidents

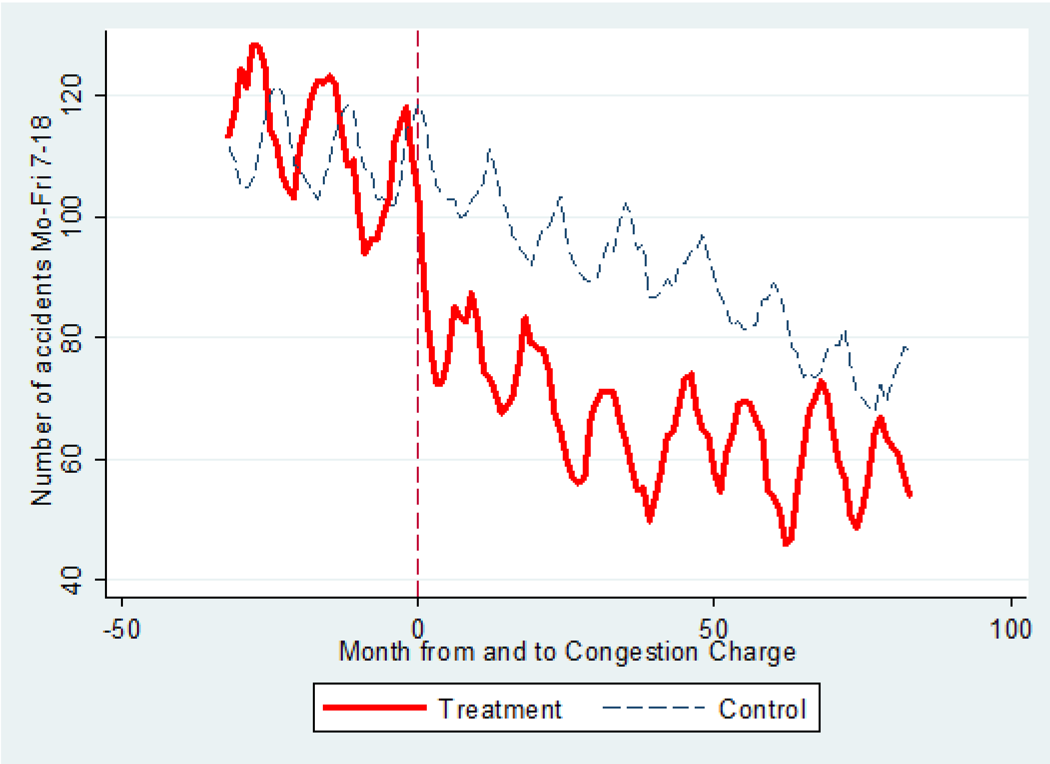


TABLE 1: Effect of Congestion Charges on Monthly Accidents for Charged Vehicles in Charged Times in the CCZ vs Charged Vehicles and Times in the 20 Largest British Cities, 2000-2009

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
	OLS	Poisson	Neg. Bin	OLS	Synthetic Control
Mean traffic accs CCZ (pre-policy)	111.027				
CCZ*Policy	-39.240	-0.389	-0.443	-40.847	-28.311
	(1.153)***	(0.017)***	(0.024)***	(1.193)***	(5.451)***
		[0.322]	[0.358]		
Policy	3.727	0.073	0.079	3.804	0.840
	(1.011)***	(0.016)***	(0.017)***	(1.013)***	(3.279)
CCZ	-147.745	-1.413	-1.554	-168.642	21.321
	(14.452)***	(0.143)***	(0.275)***	(14.590)***	(27.825)
Month Trend	-0.331	-0.005	-0.005	-0.331	-0.382
	(0.046)***	(0.000)***	(0.000)***	(0.047)***	(0.039)***
Month Trend * CCZ	-0.431	-0.005	-0.004	-0.418	0.143
	(0.041)***	(0.000)***	(0.001)***	(0.044)***	(0.083)*
Area	-395.651	0.140	-1.054	-396.064	0.000
	(186.575)**	(2.607)	(3.419)	(186.943)**	(0.000)
Population	184.589	1.905	2.055	184.632	-23.153
	(11.698)***	(0.124)***	(0.248)***	(11.712)***	(27.730)
Month dummies	Yes	Yes	Yes	Yes	Yes
Month dummies*CCZ				Yes	
Observations	2520	2520	2520	2520	240
R-squared	0.83			0.84	0.80

*Robust standard errors clustered at a local authority level in parentheses. The terms in square brackets, [ ], are the computed percentage declines for the respective estimates. All estimates include a constant. ***,**, * indicate statistical significance at the 1%, 5% and 10% level, respectively.*

TABLE 2: Spillover Effects of the Congestion Charge on Accidents vs. 20 Largest British Cities

	(1)	(2)	(3)	(4)
	Spillover Region 1	Spillover Region 2	Not Charged Time	Not Charged Vehicles
Mean traffic accs CCZ (pre-policy)	118.054	155.351	85.865	127.324
CCZ*Policy	-14.379	-15.983	-12.151	-14.018
	(0.954)***	(0.954)***	(0.786)***	(0.623)***
	[0.129]	[0.126]	[0.291]	[0.117]
Policy	2.663	2.681	-0.210	1.005
	(0.942)**	(0.941)***	(0.786)	(0.632)
CCZ	45.645	84.535	29.857	113.882
	(9.705)***	(9.705)***	(7.905)***	(2.404)***
Month Trend	-0.285	-0.285	-0.167	-0.075
	(0.039)***	(0.039)***	(0.028)***	(0.011)***
Month Trend * CCZ	-0.133	-0.217	-0.085	-0.507
	(0.036)***	(0.035)***	(0.026)***	(0.011)***
Month dummies	Yes	Yes	Yes	Yes
Observations	2520	2520	2520	2520
R-squared	0.11	0.19	0.07	0.70

*Robust standard errors clustered at a local authority level in parentheses. The estimates inside the square brackets, [ ], are the percentage declines computed from the otherwise identical Negative Binomial. All estimates include a constant. ***,**, * indicate statistical significance at the 1%, 5% and 10% level, respectively.*

TABLE 3: Serious and Fatal Injuries and Congestion Charging
Spillovers in Serious and Fatal Accidents

	KSI	Fatalities	(1) Spillover Region 1	(2) Spillover Region 2	(3) Not Charged Time	(4) Not Charged Vehicles
Mean traffic accs CCZ (pre-policy)	14.459	1.040	15.973	20.973	12.757	17.243
CCZ*Policy	-3.600	-0.359	-3.735	-2.265	-1.986	-2.049
	(0.241)***	(0.073)***	(0.241)***	(0.241)***	(0.319)***	(0.142)***
	[0.235]	[0.567]	[0.220]	[0.084]	[0.235]	[0.109]
Policy	-0.153	-0.073	-0.150	-0.158	-0.189	0.082
	(0.238)	(0.060)	(0.238)	(0.238)	(0.320)	(0.146)
CCZ	7.349	0.294	8.723	14.448	4.632	15.190
	(1.014)***	(0.057)***	(1.014)***	(1.014)***	(1.178)***	(0.343)***
Month Trend	-0.021	-0.000	-0.021	-0.021	-0.025	-0.008
	(0.004)***	(0.001)	(0.004)***	(0.004)***	(0.006)***	(0.002)***
Month Trend * CCZ	-0.016	0.001	-0.009	-0.047	-0.005	-0.056
	(0.003)***	(0.001)	(0.003)**	(0.003)***	(0.006)	(0.002)***
Month dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2515	1407	2515	2515	2515	2434
R-squared	0.09	0.02	0.12	0.23	0.06	0.51

*Robust standard errors clustered at a local authority level in parentheses. The estimates inside the square brackets, [ ], are the percentage declines computed from the otherwise identical Negative Binomial. All estimates include a constant. ***,**, * indicate statistical significance at the 1%, 5% and 10% level, respectively.*

Table 4: The Congestion Charge and Accident Rates, 2000-2009

	(1)	**(2)** Synthetic Control	Spillovers Region1	Spillovers Region2
Mean accs rates CCZ (pre-policy)	4.512	4.512	4.522	4.623
CCZ*Policy	-1.001	-1.203	-0.738	-0.745
	(0.063)***	(0.245)***	(0.058)***	(0.058)***
Policy	-0.060	0.153	-0.069	-0.069
	(0.058)	(0.164)	(0.058)	(0.058)
CCZ	0.070	-1.453	1.775	1.924
	(0.787)	(0.586)**	(0.249)***	(0.249)***
Year Trend	-0.097	-0.312	-0.093	-0.093
	(0.017)***	(0.025)***	(0.017)***	(0.017)***
Year Trend * CCZ	0.022	0.261	0.054	0.030
	(0.027)	(0.048)***	(0.017)***	(0.017)*
Area	-40.725	0.000		
	(14.698)**	(0.000)		
Population	1.582	0.910		
	(0.760)*	(0.623)		
Observations	189	18	189	189
R-squared	0.43	0.98	0.25	0.26

*Robust standard errors clustered at a local authority level in parentheses. All estimates include a constant. ***,**, * indicate statistical significance at the 1%, 5% and 10% level, respectively.*

Table 5: Congestion Charge, Spatial Spillovers and Accident Rates

	KSI	Fatalities	Spillovers KSI	
			Region 1	Region 2
Mean accs rates CCZ (pre-policy)	0.622	0.026	0.672	0.711
CCZ*Policy	-0.158	-0.021	-0.217	-0.136
	(0.011)***	(0.002)***	(0.011)***	(0.011)***
Policy	-0.025	0.004	-0.025	-0.025
	(0.011)**	(0.002)*	(0.011)**	(0.011)**
CCZ	0.284	-0.001	0.327	0.416
	(0.030)***	(0.002)	(0.030)***	(0.030)***
Year Trend	-0.008	-0.001	-0.008	-0.008
	(0.002)***	(0.000)***	(0.002)***	(0.002)***
Year Trend * CCZ	0.016	0.003	0.020	-0.006
	(0.002)***	(0.000)***	(0.002)***	(0.002)***
Observations	189	189	189	189
R-squared	0.28	0.13	0.31	0.32

*Robust standard errors clustered at a local authority level in parentheses. All estimates include a constant. ***,**, * indicate statistical significance at the 1%, 5% and 10% level, respectively.*

Table 6: Congestion Charge and Bike Accident Counts

	All Accidents			Serious and Fatal Accidents		
	2000-2009	**2000-2006**	**2000-2005**	**2000-2009**	**2000-2006**	**2000-2005**
Mean traffic accs CCZ (pre-policy)	31.703	31.703	31.703	3.946	3.946	3.946
CCZ*Policy	-2.853	-1.435	1.538	-0.604	0.207	0.993
	(0.263)***	(0.300)***	(0.295)***	(0.083)***	(0.107)*	(0.095)***
Policy	-0.688	-0.356	-0.057	0.058	-0.106	-0.002
	(0.253)**	(0.279)	(0.286)	(0.085)	(0.112)	(0.102)
CCZ	24.712	25.488	27.530	2.626	3.079	3.650
	(0.722)***	(0.719)***	(0.744)***	(0.077)***	(0.079)***	(0.087)***
Month Trend	0.003	-0.005	-0.016	0.002	0.006	0.003
	(0.004)	(0.007)	(0.008)**	(0.001)	(0.003)**	(0.003)
Month Trend * CCZ	0.017	-0.024	-0.131	0.012	-0.012	-0.042
	(0.004)***	(0.007)***	(0.007)***	(0.001)***	(0.002)***	(0.002)***
Month dummies	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2513	1757	1505	1775	1208	1044
R-squared	0.58	0.58	0.58	0.33	0.30	0.31

Robust standard errors clustered at a local authority level in parentheses. All estimates include a constant. ****,**, * indicate statistical significance at the 1%, 5% and 10% level, respectively.*

Table 7: Congestion Charge and Bike Accident Rates

	All Accidents			Serious and Fatal Accidents		
	2000-2009	**2000-2006**	**2000-2005**	**2000-2009**	**2000-2006**	**2000-2005**
Mean accs rates CCZ (pre-policy)	22.723	22.723	22.723	2.908	2.908	2.908
CCZ*Policy	-18.173	-20.404	-11.665	-2.010	-1.826	-0.058
	(3.013)***	(3.405)***	(2.939)***	(0.793)**	(1.020)*	(0.935)
Policy	8.731	15.214	8.366	0.768	1.659	0.611
	(3.013)***	(3.405)***	(2.939)***	(0.793)	(1.020)	(0.935)
CCZ	-32.197	-34.240	-27.686	-3.941	-3.914	-2.588
	(2.860)***	(3.452)***	(2.649)***	(0.663)***	(0.788)***	(0.775)***
Month Trend	-1.762	-4.000	-1.432	0.011	-0.297	0.096
	(0.465)***	(0.876)***	(0.890)	(0.124)	(0.235)	(0.228)
Month Trend * CCZ	1.888	2.909	-0.368	0.064	0.050	-0.613
	(0.465)***	(0.876)***	(0.890)	(0.124)	(0.235)	(0.228)**
Observations	189	126	105	189	126	105
R-squared	0.21	0.23	0.21	0.12	0.11	0.12

Robust standard errors clustered at a local authority level in parentheses. ****,**, * indicate statistical significance at the 1%, 5% and 10% level, respectively.*

Table 8: Prices, Congestion Charges and Traffic Accidents

	Accidents	Accident rates
Mean dependent variable CCZ (pre-policy)	111.027	4.512
CCZ*Price (£)	-5.227	-0.001
	(0.177)***	(0.000)***
Price (£)	0.482	-0.000
	(0.177)**	(0.000)
CCZ	32.923	1.803
	(9.686)***	(0.250)***
Observations	2520	189
R-squared	0.10	0.24

*All models include time trends, time trend interacted with CCZ and monthly dummies in column 1. Robust standard errors clustered at a local authority level in parentheses. ***,**, * indicate statistical significance at the 1%, 5% and 10% level, respectively.*

Table 9: The Impact of the Western Extension on Traffic Accidents

	Traffic accidents		Accident rates	
	(I)	**(II)**	**(I)**	**(II)**
Mean CCZ (pre-policy 03)	111.027	111.027	4.456	4.456
Mean WE (pre-policy 07)	40.106	40.106	2.597	2.597
Treatment*Policy	-39.601***		-0.793***	
	(4.230)		(0.117)	
	[0.357]			
CCZ*Policy 2003		-42.265***		-0.832***
		(1.984)		(0.097)
		[0.364]		
Western Extension*Policy 2007		-6.831*		-0.497**
		(3.548)		(0.188)
		[0.252]		
Policy 2003	2.012	2.133	-0.124	-0.122
	(2.744)	(2.709)	(0.156)	(0.156)
Policy 2007	4.653	3.163	0.061	0.047
	(4.414)	(4.232)	(0.246)	(0.250)
CCZ	43.782***	44.972***	1.559***	1.576***
	(9.254)	(9.042)	(0.270)	(0.269)
Western Extension	-30.184***	-30.679***	0.426	0.421
	(9.483)	(9.505)	(0.260)	(0.261)
Month Trend	-0.190***	-0.188***	-0.125***	-0.124***
	(0.057)	(0.057)	(0.037)	(0.037)
Month Trend * CCZ	-0.419***	-0.468***	0.128***	0.121***
	(0.094)	(0.060)	(0.040)	(0.037)
Month Trend * Western Extension	0.015	0.024	-0.061***	-0.060***
	(0.034)	(0.035)	(0.017)	(0.017)
Month Dummies	Yes	Yes		
Observations	2640	2640	198	198
R-squared	0.122	0.122	0.242	0.242

*Robust standard errors clustered at a local authority level in parentheses. The estimates inside the square brackets, [ ], are the percentage declines computed from the otherwise identical Negative Binomial. All estimates include a constant.***,**, * indicate statistical significance at the 1%, 5% and 10% level, respectively. Treatment*Policy is a dummy variable which takes value 1 for the initial CCZ from the 17th of February 2003 and for the Western Extension from the 19th of February 2007 onwards and 0 otherwise. CCZ corresponds to the initial congestion charge zone and Western Extension corresponds to the extended area. Policy 2003 takes value 1 from the 17th of February 2003 and 0 otherwise. Policy 2007 takes value 1 from February 2007 and 0 otherwise.*

APPENDIX

Table A.1. Alternative Approaches to Cluster Inference, Accident Levels and Rates.

	Accidents			**Accident rates**	
	Table1 Col. 1	**Table1 Col. 4**	**Table1 Col.5**	**Table4 Col.1**	**Table4 Col.2**
Clustering	0.0000	0.0000	0.0043	0.0001	0.0060
Wild cluster bootstrap					
Rademacher weights	0.0164	0.0364	0.0000	0.0000	0.0000
Mammen weights	0.5486	0.5388	0.0000	0.0000	0.0000

Note: p-values for the preferred specifications, Synthetic control approach (with population and area). The first row shows p-values based on standard errors clustered at a local authority level (city level). The second and third rows show p-values based on wild cluster bootstrap procedures (10,000 replications estimated under the null hypothesis of no Congestion Charge effect on accidents) using the Rademacher and Mammen weights, respectively.