



Do increased speeding fines save lives?

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ABSTRACT

On 24 April 2017, fines for the most serious speeding offences increased from 100% to 150% of relevant weekly income in England and Wales. In this paper we establish whether this policy saved lives. Using data on all road accidents reported to police we evaluate the effects of this increased financial penalty using a two-year bandwidth and applying Regression Discontinuity in Time and Difference-in-Difference methods. Overall, we find no evidence of reduced serious or fatal accidents following the policy, indicating most drivers are not significantly changing their risky behaviours when confronted with higher penalties. There is, however, some evidence of fewer accidents in more economically advantaged areas. This suggests that drivers are responding to the amount of the fine increase rather than the income share and wealthier drivers are therefore more affected.

1. Introduction & background

Road accidents are an important cause of preventable fatalities, and governments worldwide use financial incentives to encourage safe driving. However, despite a large literature, including papers in leading economics journals, there remains considerable uncertainty about the best size and structure of incentives needed to further reduce serious accidents and fatalities (see, for example, Bourgeon and Picard, 2007; De Paola, Scoppa and Falcone, 2013; Hansen, 2015; Kantowicz-Reznichenko, 2015; Montag, 2014). Some recent examples of the literature focus on the link between general enforcement (which increases expected penalties) and fatalities (DeAngelo and Hansen, 2014), traffic cameras and accidents (Gallagher and Fisher, 2020; Tang and Ahlfeldt, 2020), texting/mobile phone use bans and accidents (Abouk and Adams, 2013; Burger et al., 2014; Ferdinand et al., 2014; Rocco and Sampaio, 2016), drug driving laws and fatalities (Anderson and Rees, 2015), licence suspensions and speeding recidivism (Gehrsitz, 2017) and speeding and fines (Traxler et al., 2018). In this paper we investigate whether a substantive change in fines for speeding introduced in England and Wales in April 2017 reduced numbers of serious or fatal accidents using a complete register of all accidents reported to police.

Financial penalties are widely used as an instrument to improve road safety (see for example Elvik (2016) for a meta-analysis of studies that consider traffic violations and accidents). One key factor affecting road safety is speed. Across Britain in 2017, 7% of serious and 14% of fatal accidents were reported to involve speeding violations, at a cost of £721.6m (UK Department for Transport, 2018a). These costs include loss of output, human, medical and ambulance costs, which leave a large burden on individual health and the healthcare system that could be reduced by successful accident prevention efforts. Although one way of potentially reducing accident numbers is to control driver behaviour, setting speed limits does not guarantee compliance. A system of penalties is therefore required to change incentives and induce would-be offenders to obey the laws and to penalise violations. Different levels of speeding are associated with different levels of (expected) harm, according to the probability of an accident occurring and accident severity. Penalties should therefore increase with offence seriousness in order to deter individuals from committing more serious offences (Polinsky and Shavell, 2000).

On 24 April 2017, the Sentencing Guidelines Council increased fines for the most serious speeding offences from 100% to 150% of relevant weekly income for England and Wales (Sentencing Guidelines Council, 2017a).¹ Income based fines are designed to have an equal impact on the

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¹ Where an offender earns more than £120 per week after deductions for tax and national insurance (or equivalent if self-employed), relevant weekly income is actual income. However, if such actual income is less than £120 or if the offender's only source of income is state benefit, relevant weekly income is deemed to be £120. Where there is insufficient reliable information, the court can make its own determination about relevant weekly income. If there is no information, the court deems relevant weekly income to be £440. The court may also take into account broader financial circumstances (Sentencing Guidelines Council, 2017a).

offender regardless of his or her financial circumstances and they are designed to impose hardship but not to constrain reasonable living expenses. The new fines applied for vehicles travelling more than 20mph over a 20–30 mph limit, more than 26 mph over a 40–50 mph limit or more than 30mph over higher speed limits. For less serious speeding offences, the fines remained at 100% of relevant weekly income. The change was designed to ‘take into account the increase in potential harm that can result as speed above the limit increases’ and to ‘ensure that there is a clear increase in fine level as the seriousness of offending increases’ (Sentencing Guidelines Council, 2017b, p. 1). However, maximum fines remained at £2 500 for offences committed on motorways and £1 000 for those on non-motorways. These maxima ensure the fine is proportionate to the seriousness of the offence. Fig. 1 shows the increase in fines by income level. For individuals on the median full-time employee income, their fine increased from £427 to £641. For individuals working full time and earning income at the 90th percentile, their fine increased from £779 to either £1 169 if the offence occurred on a motorway or £1 000 if the offence occurred on a non-motorway.²

Key studies of speeding penalties have found mixed results. Traxler et al. (2018) investigated driver responses to increases in speeding penalties introduced in 2009 in Germany. The change involved an increase in a step-wise (or notched) penalty scheme. They used data on measured speed and speeding fines collected for the Autobahn and found a 25% reduction in the small share of speeding drivers driving 20 or more kilometres over the speed limit (for which there was a 40–60% increase in fines) and an increase in speeding tickets just below this cut-off where fines remained unchanged. Chen et al. (2020) analysed stated preference data for professional drivers in Hong Kong and found a 1 point increase in driving offence points led to a 4% increase in speed compliance but that a 10% increase in fines only led to a 1.7% increase in compliance. An Australian study by Watson et al. (2020) found vehicle impoundment was effective in reducing speeding offences. Of most relevance, US accident data over the period 2010–2014 showed a 1% increase in speeding fines reduced fatal, injury and property damage accidents by more than 10% (C. Dong et al., 2018). Montag (2014) found a 33.3% reduction in road deaths in the three months following penalty increases (including a near trebling of speeding fines) in the Czech Republic, although the effects beyond the first year post-reform were approximately zero. However, Elvik and Christensen (2007) found no significant effect of increased speeding fines in Norway and H. Dong et al. (2019) found 48% of Chinese drivers made the same risky driving violations after losing penalty points in 2017.

Responses to penalties for other offences may also be relevant. Individuals can disobey red lights for similar reasons to speeding. Bar-Ilan and Sacerdote (2004) found red-light running decreases with an increase in the fine, particularly for younger drivers and for those with older vehicles. Increase in fines and licence suspension for reckless driving in Portugal from January 1998 reduced monthly accidents by 0.5% but had no significant effect on fatalities (Tavares et al., 2008). In the US, higher penalties resulted in fewer accidents as follows (C. Dong et al., 2018): the introduction of a hand-held mobile phone ban reduced injury accidents by 24%, a text messaging ban reduced fatal and injury accidents by more than 20% and a \$1 increase in fines for drink driving reduced injury accidents by 19.8%. Brazilian, Spanish and Taiwanese data also show increased drink driving sanctions reduced numbers of fatal accidents (Chan et al., 2017; Guimarães and da Silva, 2019;

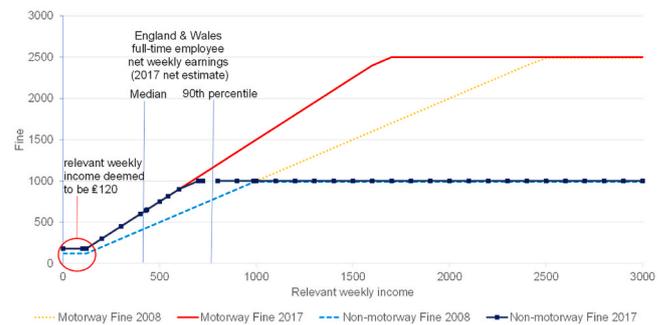


Fig. 1. Most serious speeding offence fine regimes, 2008 and 2017.

Rodríguez-López et al., 2019). However, Fry (in press) examined the effects of a doubling of penalties for mobile phone use in 2017 on serious and fatal accidents in Britain and found no significant effect.

In this paper we contribute to the literature linking speeding fines and road accidents. We analyse the effects on numbers of fatal or serious accidents of the increase in speeding fines in England and Wales using British Stats19 data on the universe of accidents. The fine is income contingent, which is a relatively rare form of sanction outside the UK and Europe (see Kantorowicz-Reznichenko (2018) for a summary) and of natural interest to economists. With unique data on area deprivation associated with each driver’s residence to proxy driver income, we can examine whether the effects of the fine increase differ by income. As caps on fines differ between motorways and non-motorways, also we look for differences by road type as the increase in fine rates may have different effects across the income distribution.

After accounting for longer term trends and seasonal/day of the week effects, we use Regression Discontinuity in Time (RDiT) methods to estimate the effect of the intervention and Difference-in-Difference (DiD) methods to identify ‘treatment’ effects. To our knowledge this is the first attempt to comprehensively examine the effects of this road safety intervention on numbers of serious or fatal accidents. These results are critical for policymakers to determine the effectiveness of specific interventions (do they save lives or just raise revenue?) and the likely success of future endeavours.

2. Data and summary statistics

Accident data comes from the British Stats19 database (UK Department for Transport, 2019a), which records details of every personal injury road accident involving at least one vehicle and reported to police (UK Department for Transport, 2016). Accident severity is identified by the most serious casualty in the accident. This is determined by police at the scene and may include measurement error as the extent of some injuries may not be apparent at the time (UK Department for Transport, 2013). We focus on serious or fatal accidents as they are a non-random subset of the total (Fry and Farrell, 2022) and represent more than three-quarters of all costs associated with accidents involving injury (UK Department for Transport, 2018a).

We analyse accidents by day for each Local Authority (LA) in England and Wales (and in some cases Scotland) for 365 days either side of the intervention. Accident numbers are low and there are no serious or fatal accidents on 85% of LA-days in England/Wales and on 89% of LA-

² These income figures are based on the UK Annual Survey of Hours and Earnings for 2017.

Table 1
Summary statistics on daily serious or fatal accidents at Local Authority level for England, Wales and Scotland by Government Office Region.^a

Country/GOR	Time	Obs	Mean	SE	Std. Dev.	Min	Max
England & Wales	1 Mar 2016–28 Feb 2017	127,020	0.172	0.001	0.434	0	6
	1 Mar 2017–23 Apr 2017	18,792	0.169	0.003	0.431	0	4
	24 Apr 2017–23 Apr 2018	127,368	0.177	0.001	0.444	0	6
East of England	1 Mar 2016–28 Feb 2017	17,155	0.156	0.003	0.402	0	4
	1 Mar 2017–23 Apr 2017	2538	0.142	0.008	0.385	0	3
	24 Apr 2017–23 Apr 2018	17,202	0.146	0.003	0.388	0	4
East Midlands	1 Mar 2016–28 Feb 2017	14,600	0.118	0.003	0.352	0	3
	1 Mar 2017–23 Apr 2017	2160	0.117	0.007	0.347	0	2
	24 Apr 2017–23 Apr 2018	14,640	0.119	0.003	0.351	0	4
London	1 Mar 2016–28 Feb 2017	12,410	0.208	0.004	0.470	0	3
	1 Mar 2017–23 Apr 2017	1836	0.300	0.013	0.564	0	3
	24 Apr 2017–23 Apr 2018	12,444	0.300	0.005	0.570	0	6
North East England	1 Mar 2016–28 Feb 2017	4380	0.196	0.007	0.464	0	6
	1 Mar 2017–23 Apr 2017	648	0.207	0.018	0.469	0	3
	24 Apr 2017–23 Apr 2018	4392	0.192	0.007	0.462	0	5
North West England	1 Mar 2016–28 Feb 2017	14,235	0.180	0.004	0.438	0	4
	1 Mar 2017–23 Apr 2017	2106	0.175	0.009	0.434	0	3
	24 Apr 2017–23 Apr 2018	14,274	0.176	0.004	0.432	0	4
South East England	1 Mar 2016–28 Feb 2017	24,455	0.173	0.003	0.422	0	4
	1 Mar 2017–23 Apr 2017	3618	0.143	0.006	0.387	0	4
	24 Apr 2017–23 Apr 2018	24,522	0.160	0.003	0.408	0	4
South West England	1 Mar 2016–28 Feb 2017	13,140	0.149	0.004	0.414	0	6
	1 Mar 2017–23 Apr 2017	1944	0.135	0.009	0.396	0	4
	24 Apr 2017–23 Apr 2018	13,176	0.149	0.004	0.410	0	5
West Midlands	1 Mar 2016–28 Feb 2017	10,950	0.194	0.005	0.498	0	5
	1 Mar 2017–23 Apr 2017	1620	0.158	0.011	0.433	0	4
	24 Apr 2017–23 Apr 2018	10,980	0.174	0.004	0.459	0	6
Yorkshire & the Humber	1 Mar 2016–28 Feb 2017	7665	0.286	0.006	0.565	0	4
	1 Mar 2017–23 Apr 2017	1134	0.288	0.017	0.561	0	3
	24 Apr 2017–23 Apr 2018	7686	0.327	0.007	0.629	0	6
Wales	1 Mar 2016–28 Feb 2017	8030	0.123	0.004	0.356	0	4
	1 Mar 2017–23 Apr 2017	1188	0.128	0.011	0.370	0	3
	24 Apr 2017–23 Apr 2018	8052	0.111	0.004	0.343	0	3
Scotland	1 Mar 2016–28 Feb 2017	11,680	0.135	0.004	0.386	0	5
	1 Mar 2017–23 Apr 2017	1728	0.122	0.009	0.357	0	3
	24 Apr 2017–23 Apr 2018	11,712	0.122	0.003	0.361	0	3

^a Samples are split between the year prior to the mobiles intervention, the time between the two interventions and the year after the speeding intervention.

days in Scotland. Daily precipitation, maximum and minimum temperature data from the UK Met Office Integrated Data Archive System (MIDAS) account for any unusual climatic events in an LA at the time of the intervention (UK Met Office, 2018).

Although our interest is in the effects of the increased speeding fines, we need to control for any effects of an increase in penalties for using a mobile phone that occurred during our analysis window. There are small variations in mean numbers of accidents at various times around the two interventions (Table 1).

Traffic volumes, environmental and vehicle safety features, driver behaviour and weather conditions can give rise to variations in accidents according to trends, seasons, weekdays, public holidays and weather. For illustrative purposes, we plot raw numbers of serious or fatal accidents at the aggregate level for England and Wales by month in Fig. 2. Predicted numbers of accidents based on trend, seasonal and public holiday effects are also shown (although for simplicity we abstract from weekday and weather effects). This chart shows numbers of accidents would be expected to increase significantly just after the intervention, based on these factors alone. It is therefore important to remove these effects prior to analysis of the intervention.

3. Models

Our treatment comprises the 50% increase in speeding fines in England and Wales from 24 April 2017. As the fine relates to the most serious cases of speeding, we focus on accidents in which someone is killed or seriously injured, KSI. In order to tightly identify the effects, we use a two-step process similar to that used by Castriota and Tonin (2019), De Paola et al. (2013) and Hausman and Rapson (2018), in which we assume the pattern in accidents would have followed the same

path in the absence of the intervention and correct for trend and other potentially confounding effects through time. This approach improves on the usual one-step approach (in which potentially confounding trend, seasonal, holiday, weather and treatment effects would be estimated from a single model), as the former accounts for longer term trend, seasonal/holiday and weather effects allowing for more precise estimates of these controls.

The first step involves estimating over a 10-year period a fixed effects model to remove long term effects of the potentially confounding factors from the raw data to create an adjusted series:

$$\begin{aligned}
 k_{sit} = & \beta_0 + \beta_{1,i}Trend_{it} + \sum_{k=2}^{12}\beta_{2,k}month_{it} + \sum_{l=2}^7\beta_{3,l}day_{it} \\
 & + \beta_4NY_t + \beta_5GF_t + \beta_6EM_t + \beta_7MH_t + \beta_8SBH_t + \beta_9ABH_t + \beta_{10}XD_t + \beta_{11}BD_t \\
 & + \beta_{12}RW_t + \beta_{13}DJ_t + \beta_{14}max_temp_{it} + \beta_{15}min_temp_{it} + \beta_{16}prcp_{it} + \alpha_i + \epsilon_{it}
 \end{aligned}
 \tag{1}$$

($i = 1, \dots, 380$; $t = -3035, \dots, 616$)

where k_{sit} is the number of KSI accidents in LA i on day t , $Trend_{it}$ is an LA-specific time trend and $month_{it}$ and day_{it} are month and day of the week dummies, respectively. Public holiday dummies are estimated for: NY New Year's day, GF Good Friday, EM Easter Monday, MH May bank holiday, SBH Spring bank holiday (in May), ABH August bank holiday, XD Christmas day, BD Boxing day, RW 2011 Royal Wedding and DJ 2012 Diamond Jubilee. max_temp_{it} and min_temp_{it} are daily maximum

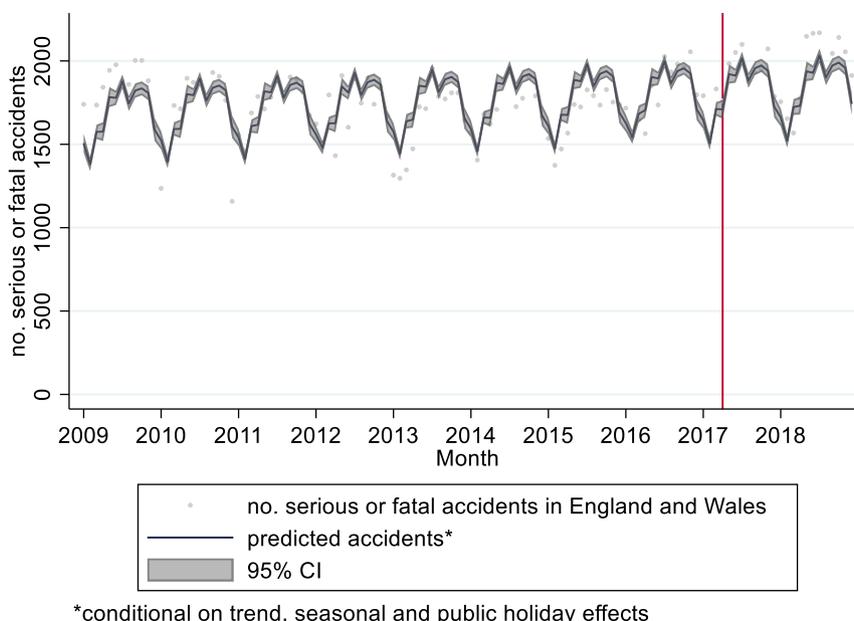


Fig. 2. Serious or fatal accidents and long term (national) trend, seasonal and public holiday effects, England and Wales^a
^aEffects are estimated from a regression model incorporating a national trend, seasonal and public holiday dummies over the period 1/1/2009 to 31/12/2018. The date of the intervention is indicated by the red line.

and minimum air temperatures in each LA, $prcp_{it}$ is daily precipitation in each LA and α_i are LA fixed effects.³ In some analyses we focus on 348 LAs in England and Wales. Typically, our estimation sample for this step runs from 1/1/2009 to 31/12/2018.⁴ Estimated standard errors are robust to heteroscedasticity. Having estimated equation (1), we then remove these effects from the raw data in preparation for step two: a Regression Discontinuity in Time (RDiT) or Difference-in Difference (DiD) analysis.⁵

RDiT is a variant of Regression Discontinuity Design, in which time is the running variable and treatment begins at a particular known point in time. We assume drivers are treated (subject to the higher penalty) if the accident occurs on or after 24 April 2017 and untreated if the accident occurs prior to this. To identify the effect of the fine increase, we model daily serious or fatal accidents for each LA over 365 days pre and post intervention. Our effect may be confounded by the effect of an increase in mobile phone penalties on 1 March 2017, so we specify our pooled RDiT model as:

$$\widehat{ks}_{it} = \delta_0 + \delta_1 Post_mobile_t + \delta_2 Post_speed_t + v_{it} \quad (2)$$

($i = 1, \dots, 348; t = -365, \dots, 365$)

where \widehat{ks}_{it} is the adjusted number of serious or fatal accidents in LA i on day t (i.e. the residuals from step one), $Post_mobile$ and $Post_speed$ are dummy variables identifying days before and after the mobile phone and speed interventions, respectively. δ_2 is the parameter of interest, showing the average effect over one year. We adopt a two-year window to allow for an adjustment process in which individuals take time to

³ Weather measurements relate to the station nearest the centroid of the LA area that was open on the day and had recorded measurements. The station was identified using Stata's `geonear` user written command. Average distances from centroids 7.14 miles to temperature stations and 3.15 miles to rainfall stations.

⁴ IMD data are only available from 1 Jan 2016, so this limits our sample period for step one when we investigate heterogeneity by driver SES.

⁵ These techniques are similar to the Interrupted Time Series approach, although ITS uses one step.

recognise the regime change and adapt their driving in response.⁶ This means we estimate a longer-term effect of the intervention. We also distinguish effects across the income distribution using Index of Multiple Deprivation (IMD) data that is recorded for each driver's area of residence.⁷ Whilst this does not directly measure a driver's SES, it does give some idea about relative disadvantage for the area in which the driver lives. For this analysis, we identify numbers of serious or fatal accidents involving at least one driver from the relevant IMD decile and estimate

⁶ This has the added benefit of more power of our estimates than if we used a shorter window.

⁷ A separate IMD measure is calculated for England (IMD), Wales (WIMD) and Scotland (SIMD). For England, the Index is calculated from a set of 37 indices which measure relative levels of deprivation across 7 domains in neighbourhoods, called Lower-layer Super Output Areas (LSOA) (UK Department for Communities and Local Government, 2015). Data for the indices come from administrative data and the Census. The Income Deprivation Domain and the Employment Deprivation Domain have the highest weight in the Index (some 45%), with Education, Skills and Training, Health/Disability, Crime, Barriers to Housing and Services and Living Environment also contributing. The 2015 Index is based on indicator data mostly from 2012/13 and is calculated at LSOA level. In using the Index, it should be borne in mind that the index is an average across individuals and some individuals in the most deprived areas may not be highly deprived (and there might be some highly deprived individuals in the least deprived areas). LSOAs are ranked by their IMD and ranks are split into deciles, to indicate cutpoints on a spectrum of disadvantage. Although the deciles split the areas evenly, their levels of disadvantage are not split evenly. The 2014 Welsh Index of Multiple Deprivation (WIMD) is calculated at LSOA level and comprises 8 domains similar to those of the English IMD, based on 33 indicators that use administrative and census data mostly over the period 2010–2014. Income and employment domains contribute 47% to the Index. The Scottish Index of Multiple Deprivation (SIMD) is calculated for some 6 976 data zones in Scotland and combines data on 38 indicators into 7 domains, which are then combined into the SIMD (UK Office of National Statistics, 2016). The methodology is similar to that of the English and Welsh IMDs and uses administrative data provided by a selection of organisations and 2011 Census information. The income and employment domains contribute about 56% to the overall SIMD.

Table 2
RDiT (pooled) modelling results, England and Wales.^a

Variables	Coefficient (SE)
Post_mobile	0.003 (0.003)
Post_speed	-0.000 (0.003)
Constant	0.003** (0.001)
Adjusted R-squared	0.000

^aThe RDiT sample covers 365 days either side of the speeding intervention (April 24, 2017) and includes an intercept shift from the date of the mobiles intervention (post_mobile = 1 from March 1, 2017). Post_speed is the variable of interest. Mean number of serious or fatal accidents over this sample period is 0.176 (SD 0.441). Dependent variable is the residuals from the first step model. Robust standard errors are shown in parentheses. n = 254,388. Step one modelling results are given in table A.1 in the appendix. ***p < 0.01, **p < 0.05, *p < 0.1.

equation (2) on that sample. Therefore there is necessarily some overlap in accidents compared to the total.

In step two we also use DiD to explore heterogeneity by road type (with different fine caps), using serious or fatal accidents on B and C roads with a cap of £1 000 as a control group relative to those on motorways (with a cap of £2 500) as the treatment group. This means that for wealthier drivers the penalty change has less (dis)incentive effect on B and C roads, which are more dangerous. We also explore the robustness of our findings from the RDiT analysis by comparing serious or fatal accidents in LAs in England and Wales (treatment) with those in Scotland (control). While conditions may differ in Scotland, what is important in our analysis is that leading up to the intervention the trends in accidents are parallel to those in England and Wales. Controlling for long term effects of trend, seasonal, holiday, weather and time invariant LA-specific factors in step one should aid with this. Thus our definitions of treatment and control groups (and our sample) differ according to the research question. However, the most general specification for our DiD model is as follows.

$$ksti_{it} = \xi_0 + \xi_1 T_t + \xi_2 G_i + \xi_3 T_t \bullet G_i + \xi_4 Post_mobile_{it} + \varepsilon_{it}$$

(i = 1, ..., 380; t = -365, ..., 365) (3)

where T_t is an indicator for before/after the intervention, G_i indicates treatment or control group and ξ_3 is the DiD (average) treatment effect. We use a +/-365-day window and include an intercept shift for the mobile intervention (Post_mobile).

4. Results

There is no significant effect of a 50% increase in speeding fines on serious or fatal accidents in England and Wales over the year post intervention (Table 2). This is likely due to no change in driver behaviour, which may result if individuals do not consider the increase in fine to be significant (low income elasticity), if they deem the probability of conviction to be low, or if they are largely unaware of the change. Interestingly, there is no significant effect of the mobiles intervention and this is consistent with the results in Fry (in press).

A downward sloping demand curve (consistent with the income effect) would imply an increase in the ‘price’ of offending reduces demand for offending and that this reduces numbers of consequent serious or fatal accidents. However, the magnitude of this effect may differ by income as (although fines are linked to wage income) individuals at different ends of the wealth distribution may have different financial resources available to ‘cushion’ the effect of the fine increase. For example, wealthier individuals might have savings to draw upon and poorer individuals might have benefits that are excluded from the

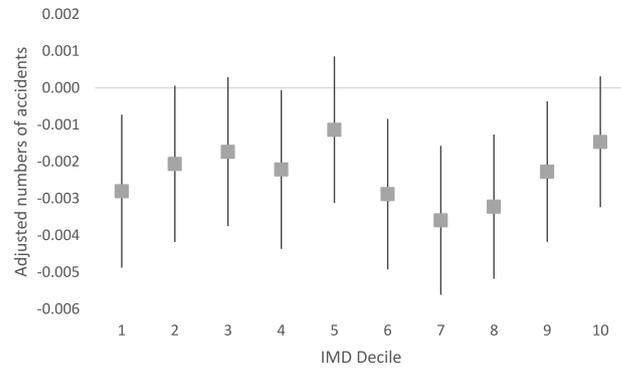


Fig. 3. RDiT (pooled) modelling results by IMD, England and Wales^a
^aThe RDiT sample covers 365 days either side of the speed intervention (April 24, 2017). Dependent variables are the residuals from the first step models. Coefficients and 90% confidence intervals shown, based on robust standard errors. n = 254,388. Step 1 modelling results are given in table A2 in the appendix. RDiT results are given in table A.3 in the appendix.

income assessment.

If individuals react to the level of the fine rather than the proportion of income (a cognitively simpler task, consistent with bounded rationality), we might expect wealthier individuals to respond more strongly to the change. There is some evidence of this as there are fewer accidents involving drivers from the sixth, seventh and eighth IMD deciles (and, to a lesser extent the ninth IMD decile) after the fine increase (Fig. 3). Interestingly, there is also a significant reduction in accidents involving a driver from the lowest IMD decile. This would be consistent with these individuals having no access to other forms of income to ‘cushion’ the impact of the increased fine.⁸

Although the speed intervention increases fines as a proportion of income, the maximum fine remains unchanged at £2500 for offences on motorways and at £1000 for non-motorways. Very wealthy individuals may pay less than 150% of their income under the new regime and the actual percentage will vary by road type (we compare motorways and B/C roads to maximise the difference). Comparing accidents on different road types, results indicate the small gap in numbers of serious or fatal accidents between motorways and B/C roads is narrowing significantly post-intervention (Table 3). This would indicate a larger decrease in accidents on B/C roads. Perhaps this is linked to average speeds travelled on these different roads and the ‘power curve’ linking speed to accident severity: for the same effect on speeding behaviour, the accident outcome (and therefore number of serious or fatal accidents) is likely to be worse on a motorway than a B or C road, closing the gap in numbers of such accidents. Individuals may also consider there is a relatively larger risk of detection on B/C roads and reduce speed accordingly.

Table 4 reports RDiT modelling results for different bandwidths, showing how effects vary with the analysis window. For each bandwidth the effect is not significantly different from zero and is small. This suggests there is little change in the effect throughout the year.

We explicitly allow for changes in the effects over time by including a

⁸ As the number of accidents in each IMD decile is based on accidents in which at least one driver is from that decile, there is necessarily some overlap in accidents. The relatively large standard error for the overall result (a coefficient of effectively zero in Table 2) and the small negative point estimates by IMD with quite large standard errors in Fig. 3 and table A.3 imply that the confidence intervals are likely to overlap. This would indicate an average effect across the deciles would overlap the total effect. As mentioned earlier, the sample period for step one is shorter when we include IMD data and this will have a small effect on the adjustment process compared to the full observation period.

Table 3
DiD (pooled) modelling results by road type, England and Wales.^a

Variables	Coefficient (SE)
Post_mobile	-0.000 (0.001)
T (1 = post_speed)	-0.002 (0.001)
G (1 = motorway)	-0.001 (0.001)
DiD	0.002** (0.001)
Constant	0.001** (0.001)

^a The DiD sample covers 365 days either side of the speed intervention (April 24, 2017). Dependent variable is the residuals from the first step models. Robust standard errors are shown in parentheses. Adjusted R-squared = 0.000. n = 390,354. Step 1 modelling results are given in table A.5 in the appendix. ***p < 0.01, **p < 0.05, *p < 0.1.

multiple step functions for different time periods (Fig. 4). For the quarterly results we see no significant effects. However, there are some effects by month. Although there are significant increases in serious or fatal accidents at 3 and 8 months relative to the intercept, the confidence intervals are only significantly different from the surrounding month effects for month 8 (although at a delay of 8 months, this (counterintuitive) positive effect is likely due to other factors).

So far we have seen a distinct lack of effect of the speeding intervention on numbers of serious or fatal accidents in England and Wales. However, it would be useful to compare this result to a control area (Scotland) that did not have the intervention using DiD analysis. The profile of accidents in Scotland varies from that of England and Wales. Across LAs, the mean number of accidents per day is lower in Scotland and less variable. Identifying the treatment effect relies on isolating the effect on the intervention from all other factors. By estimating and removing influences of many other factors in step one, we ensure pre-intervention trends are parallel. The remaining effect we estimate in step two's DiD should be the treatment effect. Table 5 reports the DiD estimates and shows no significant treatment effect.⁹ Fig. 5 displays the adjusted numbers of accidents and fitted values from the DiD estimation. For presentational purposes, we aggregate to weekly data. Two spikes in the fitted series at year end are due to additional days in the week to close out the year. The increase in fitted values prior to the speeding intervention are associated with the increase in mobile phone penalties in March but, as shown in Fry (in press), the effects for England and Wales and for Scotland are not statistically significant. The chart shows the effect of the speeding intervention coinciding with the vertical red line, which is not statistically significant.¹⁰

There are two other interventions that, although not directly coincident with the speed intervention, may have affected our results of the

⁹ There is considerable scope for heterogeneity in driver behaviour and accidents across all of England, Wales and Scotland. To remove some of that heterogeneity, a better comparison might be between areas close to the England/Scotland border. Repeating the DiD analysis including only LAs close to the border (see appendix table A.6), we still see no significant effect of the intervention, suggesting driver behaviour has not changed appreciably in response to the speeding intervention.

¹⁰ Heteroscedasticity robust standard errors are based on 348 treated groups (LAs). There are new developments that would deal with these groups as one treated group, given that the intervention is applied at the national level. However, Ferman and Pinto (2019) state "there is no heteroskedasticity robust inference method in DID when there is only one treated group" (p.466). Therefore, it appears there is currently no improvement available in standard software over what has been done. Moreover, the p value on the DiD term is far from significant at 0.176.

DiD effect. Firstly, on 5 December 2014, the legal Blood Alcohol Content (BAC) limit in Scotland was reduced. Secondly, on 1 May 2018, a minimum unit price on alcohol was introduced in Scotland. These two interventions could affect our DiD results. However, analysis of the effects of these interventions on accidents by Cooper et al. (2020) and Francesconi and James (2022) showed no significant effect of either intervention, suggesting there are no confounding influences on our results.

4.1. Reasons for lack of effect

There may be limited scope for the increase in fines to greatly affect numbers of serious or fatal accidents. In Britain, cars (including taxis) and light commercial vehicles (LCVs) make up more than 85% of traffic. Although data are not available for drivers targeted by the intervention, published information indicates less than 2% of cars and LCVs were detected travelling 20mph or more over the speed limit (UK Department for Transport, 2018b).¹¹ Although numbers of speeding offenders found guilty declined in 2017 by nearly 5%, the number increased 5.7% in 2018, suggesting a reversion to prior behaviour sometime after the intervention (UK Department for Transport, 2019b). The data for 2017 cannot distinguish changes before and after the intervention, however aggregate statistics indicate accidents involving speed (7.2%) remain in line with the 7.1% average since 2012 (UK Department for Transport, 2018c).

Enforcement activity can also affect the expected penalty that individuals respond to. While we are not able to measure numbers of police officers directly involved in enforcing speeding bans, numbers of constables give some insight. From 2016 to 2019 the trend in numbers of constables has been steady (UK Home Office, 2019). We might therefore expect drivers to consider enforcement activity to have remained fairly constant and this would lower the expected penalty (increasing incentives to offend) compared to a situation in which policing activity increased.

Of course, drivers will only respond to incentives if they are aware of those incentives. A small scale survey of UK drivers at the time of the change in speeding fines revealed that 58% of respondents were not aware of the change (Hudson, 2017). More broadly, a search of Newsbank newspapers from England, Scotland, Wales and the UK from 1 Jan 2017 to 30 April 2017 showed very few articles per day, although a peak of some 65 articles occurred on the day of the intervention coming into effect. Google trends data showed few searches relating to speed that peaked at the time of the intervention and about one month later. This suggests individuals might have been aware of the immediate changes associated with the intervention, but that recall might have been adversely affected.

Tied in with enforcement and awareness is the notion of deterrence. General deterrence is the effect of a punishment on potential offenders (the general public being deterred by the punishment experience of others — whether these others are acquainted or not), whereas specific deterrence is the effect on punished offenders (i.e. deterring repeat offending) (Stafford and Warr, 1993). Our data do not allow us to identify whether our lack of effect is due to a lack of general deterrence, specific deterrence or a combination of both. While there may exist data on one off versus repeat offences, publicly available data does not have this level of detail and is typically provided on an annual basis, which would preclude precise identification of whether our lack of treatment effect is due to the lack of one, the other or both types of deterrence. Based on this analysis, we would not expect significant changes in behaviour and therefore accidents.

¹¹ Traffic congestion may naturally limit speed, reducing the capacity for excessive speeding in the more populous areas. However, excluding the 20 most population dense LAs in London, we still find no significant effect of the intervention (results available on request).

Table 4
RDiT (pooled) modelling results by bandwidth, England and Wales.^a

Bandwidth	30 days	60 days	90 days	180 days	365 days	
Variable	Coefficient (SE)		Coefficient (SE)		Coefficient (SE)	
Post_mobile	n/a	-0.000	-0.004	-0.002	0.003	
	n/a	(0.010)	(0.005)	(0.004)	(0.003)	
Post_speed	-0.002	-0.001	0.004	-0.001	-0.000	
	(0.010)	(0.004)	(0.004)	(0.004)	(0.003)	
Constant	0.007	0.007	0.011***	0.009***	0.003**	
	(0.004)	(0.009)	(0.004)	(0.002)	(0.001)	
n	21,228	42,108	62,988	125,628	254,388	
Adjusted R-squared	0.000	0.000	0.000	0.000	0.000	

^a The RDiT sample covers up to 365 days either side of the speed intervention (April 24, 2017), as shown by the relevant bandwidth. Dependent variables are the residuals from the first step model. Robust standard errors are shown in parentheses. Step 1 modelling results are given in table A.1 in the appendix. ***p < 0.01, **p < 0.05, *p < 0.1.

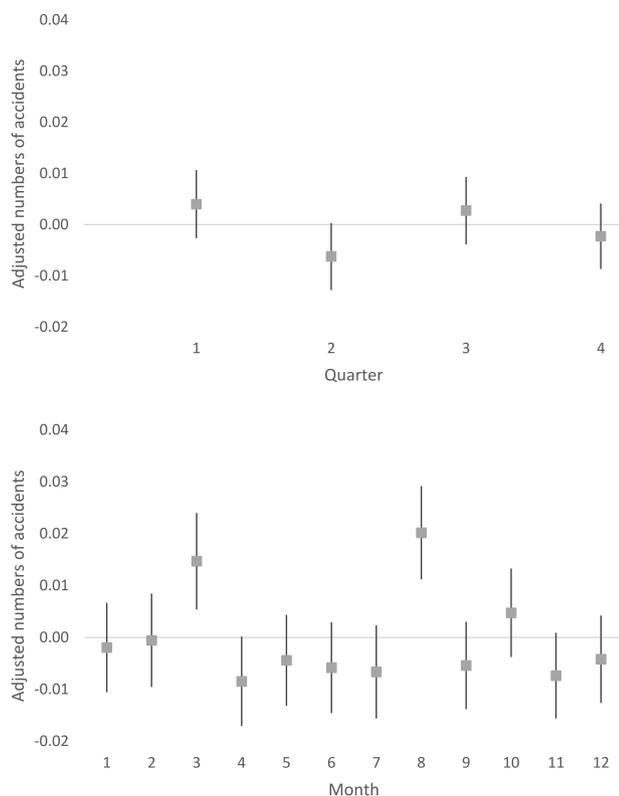


Fig. 4. RDiT (pooled) modelling results by quarter/month, England and Wales^a
^aThe RDiT sample covers 365 days either side of the speed intervention (April 24, 2017). Dependent variables are the residuals from the first step models. Coefficients and 90% confidence intervals shown, based on robust standard errors. Each month represents 30 days and each quarter 90 days. n = 254,388. Step 1 modelling results are given in table A.1 in the appendix. RDiT results are given in table A.4 in the appendix. ***p < 0.01, **p < 0.05, *p < 0.1.

5. Conclusions

Penalties are designed to modify behaviour towards the socially optimum outcome. One type of crime that can have significant health impacts via road accidents is behavioural driving offences, such as speeding. Using data on all injury accidents reported to police in Britain, we examined the effects of a fine increase on numbers of serious or fatal accidents. By analysing the link between speeding fines and serious or fatal accidents, our contribution is unique.

Table 5
DiD (pooled) modelling results, England/Wales and Scotland.^a

Variables	Coefficient (SE)
Post_mobile	0.004
	(0.003)
T (1 = post_speed)	-0.008
	(0.006)
G (1 = England/Wales)	0.001
	(0.004)
DiD	0.007
	(0.005)
Constant	0.002
	(0.004)

^aThe DiD sample covers 365 days either side of the speed intervention (April 24, 2017). Dependent variable is the residuals from the first step models. Robust standard errors are shown in parentheses. Adjusted R-squared = 0.000. n = 277,780. Step 1 modelling results are given in table A.5 in the appendix. ***p < 0.01, **p < 0.05, *p < 0.1.

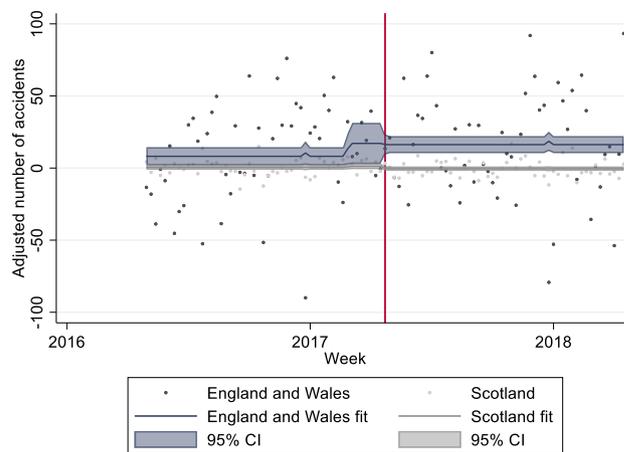


Fig. 5. Adjusted numbers of accidents per week and DiD fitted values, England/Wales and Scotland^a

^aThe DiD sample covers 365 days either side of the speed intervention (April 24, 2017). Dependent variables are the residuals from the first step models. For presentational purposes, figures are aggregated to weekly. Spikes in the fitted series at year end are due to additional days in the week to close out the year. The increase in fitted values prior to the speeding intervention are associated with an increase in mobile phone penalties in March 2017 but the effects are not statistically significant.

Overall, we find no evidence that drivers change their risky behaviour when confronted with higher penalties associated with the intervention and this result is robust to variations in the sample and allowing for non-linearities in effects. Although most of the literature suggests there should be a reduction in serious or fatal accidents, our result is consistent with the longer term lack of effect found by Montag (2014) and that of Elvik and Christensen (2007), who attribute their results to a lack of enforcement activity. Our lack of a large effect could be due to inadequate publicity about the change and/or insufficient enforcement activity. To be effective in reducing accidents and their severity, penalties need to be appropriate to the severity of the offence and the resources of the offender and to be well publicised and enforced in order to deter errant behaviour. It could also be that fines reduced speed but not sufficiently to significantly lower the probability of a fatality or serious injury.

There is some evidence that accidents decline in more economically advantaged areas and this would be consistent with wealthier drivers responding more to the level of the fine increase rather than the income share and lowering their speed and therefore accident risk accordingly. There are fewer accidents on motorways than B and C roads, but the gap narrows post-intervention and this would be consistent with a higher probability of speeding detection on the more minor roads. The small difference is likely due to the non-motorway cap only applying to the wealthiest drivers. Perhaps fines reduce speeding on low speed roads where fatality risk is lower and does not change much with large variations in speed.

Individuals respond to the expected penalty, which comprises the actual penalty and the probability of conviction (driven by enforcement activity). Therefore, road safety may be improved by further increasing fines beyond the 50% investigated here, increasing penalty points or by

changing licence suspension periods.

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Author CRediT statement

Jane M. Fry — Conceptualization; Formal analysis; Investigation; Methodology; Visualization; Writing - original draft; Writing - review & editing.

Declaration of competing interest

None.

Data availability

The authors do not have permission to share data.

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Appendix

Table A.1
Step 1 modelling results, main model^a

Variable	No. serious or fatal accidents
LA-specific time trends	✓
LA fixed effects	✓
February	0.000 (0.002)
March	-0.007*** (0.002)
April	-0.012*** (0.002)
May	-0.004** (0.002)
June	-0.007*** (0.002)
July	-0.009*** (0.002)
August	-0.017*** (0.002)
September	0.000 (0.002)
October	0.008*** (0.002)
November	0.023*** (0.002)
December	0.008*** (0.002)
Monday	0.007*** (0.001)
Tuesday	0.015*** (0.001)
Wednesday	0.014*** (0.001)
Thursday	0.017*** (0.001)
Friday	0.035***

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Table A.1 (continued)

Variable	No. serious or fatal accidents
	(0.001)
Saturday	0.017***
	(0.001)
New Year's Day	-0.008
	(0.007)
Good Friday	-0.033***
	(0.007)
Easter Monday	-0.010
	(0.007)
May Bank Holiday	-0.004
	(0.007)
Spring Bank Holiday	-0.028***
	(0.007)
August Bank Holiday	-0.006
	(0.007)
Christmas Day	-0.069***
	(0.005)
Boxing Day	-0.057***
	(0.006)
Royal Wedding Day	-0.007
	(0.022)
Diamond Jubilee Day	-0.003
	(0.024)
Maximum temperature	0.005***
	(0.000)
Minimum temperature	-0.002***
	(0.000)
Precipitation amount	-0.000
	(0.000)
Constant	0.097***
	(0.002)
Adjusted R-squared	0.073

^an = 1,270,896. Robust standard errors are shown in parentheses.
 ***p < 0.01, **p < 0.05, *p < 0.1.

Table A.2

Step 1 modelling results, Difference-in-Difference models by IMD decile, England and Wales serious or fatal accidents^a

VARIABLES	D1	D2	D3	D4	D5	D6	D7	D8	D9	D10
LA-specific time trends	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
LA fixed effects	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
February	0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.000 (0.001)	0.001 (0.001)	0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.001 (0.001)
March	-0.002 (0.001)	-0.001 (0.001)	-0.003*** (0.001)	-0.001 (0.001)	-0.002* (0.001)	-0.002* (0.001)	-0.004*** (0.001)	-0.003** (0.001)	-0.001 (0.001)	-0.003*** (0.001)
April	-0.002 (0.001)	-0.0010 (0.001)	-0.002* (0.001)	-0.003** (0.001)	-0.001 (0.001)	-0.003** (0.001)	-0.002 (0.001)	-0.003** (0.001)	-0.001 (0.001)	-0.003** (0.001)
May	-0.001 (0.002)	-0.002 (0.001)	-0.002 (0.002)	-0.003 (0.002)	0.000 (0.002)	-0.002 (0.002)	-0.001 (0.002)	-0.000 (0.001)	-0.000 (0.001)	-0.001 (0.001)
June	-0.002 (0.002)	-0.004** (0.002)	-0.005*** (0.002)	-0.004*** (0.002)	-0.002 (0.002)	-0.003 (0.002)	-0.002 (0.002)	0.001 (0.002)	-0.000 (0.002)	-0.002 (0.001)
July	-0.003 (0.002)	-0.004** (0.002)	-0.005*** (0.002)	-0.005** (0.002)	-0.003 (0.002)	-0.004** (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.002 (0.002)	-0.002* (0.002)
August	-0.004 (0.002)	-0.005*** (0.002)	-0.007*** (0.002)	-0.008*** (0.002)	-0.004** (0.002)	-0.006*** (0.002)	-0.004** (0.002)	-0.004** (0.002)	-0.003* (0.002)	-0.005*** (0.002)
September	-0.004* (0.002)	-0.003* (0.002)	-0.004*** (0.002)	-0.003* (0.002)	-0.002 (0.002)	-0.004** (0.002)	-0.003 (0.002)	-0.001 (0.002)	-0.001 (0.002)	-0.003* (0.001)
October	-0.002 (0.002)	-0.001 (0.002)	-0.002 (0.001)	-0.003* (0.002)	-0.001 (0.001)	-0.003** (0.001)	-0.002 (0.002)	-0.001 (0.001)	0.000 (0.001)	-0.001 (0.001)
November	0.002 (0.002)	0.001 (0.002)	-0.001 (0.002)	-0.000 (0.001)	0.002 (0.001)	0.001 (0.001)	-0.000 (0.001)	0.002 (0.001)	0.003** (0.001)	-0.002 (0.001)
December	-0.003* (0.002)	-0.001 (0.002)	-0.003 (0.002)	-0.001 (0.002)	-0.002 (0.001)	-0.001 (0.001)	-0.002 (0.001)	-0.001 (0.001)	-0.001 (0.001)	-0.002 (0.001)
Monday	0.000 (0.001)	0.001 (0.001)	0.003*** (0.001)	0.001 (0.001)	0.003*** (0.001)	0.002*** (0.001)	0.003*** (0.001)	0.003*** (0.001)	0.001 (0.001)	0.001 (0.001)
Tuesday	0.002** (0.001)	0.002** (0.001)	0.005*** (0.001)	0.005*** (0.001)	0.002* (0.001)	0.004*** (0.001)	0.004*** (0.001)	0.004*** (0.001)	0.002* (0.001)	0.002** (0.001)
Wednesday	0.002** (0.001)	0.003*** (0.001)	0.005*** (0.001)	0.004*** (0.001)	0.003*** (0.001)	0.003*** (0.001)	0.004*** (0.001)	0.004*** (0.001)	0.003*** (0.001)	0.003*** (0.001)
Thursday	0.002** (0.001)	0.002*** (0.001)	0.003*** (0.001)	0.004*** (0.001)	0.002** (0.001)	0.004*** (0.001)	0.005*** (0.001)	0.004*** (0.001)	0.004*** (0.001)	0.003*** (0.001)
Friday	0.005*** (0.001)	0.005*** (0.001)	0.007*** (0.001)	0.005*** (0.001)	0.005*** (0.001)	0.005*** (0.001)	0.007*** (0.001)	0.007*** (0.001)	0.005*** (0.001)	0.005*** (0.001)
Saturday	0.002* (0.001)	0.001 (0.001)	0.002** (0.001)	0.001 (0.001)	0.001 (0.001)	0.001 (0.001)	0.003*** (0.001)	0.001* (0.001)	0.001 (0.001)	0.001 (0.001)

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Table A.2 (continued)

VARIABLES	D1	D2	D3	D4	D5	D6	D7	D8	D9	D10
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
New Year's Day	-0.003 (0.004)	0.010* (0.006)	0.003 (0.005)	0.000 (0.004)	-0.006 (0.004)	-0.003 (0.004)	-0.006 (0.004)	-0.006 (0.004)	-0.002 (0.004)	0.004 (0.004)
Good Friday	-0.001 (0.005)	-0.005 (0.004)	-0.007* (0.004)	0.005 (0.005)	-0.009** (0.004)	-0.001 (0.005)	-0.007* (0.004)	-0.004 (0.004)	-0.009*** (0.004)	-0.008** (0.003)
Easter Monday	-0.003 (0.004)	-0.009** (0.004)	-0.006 (0.004)	-0.005 (0.004)	-0.006 (0.004)	-0.004 (0.004)	-0.007** (0.003)	-0.006* (0.003)	-0.003 (0.004)	-0.006* (0.003)
May Bank Holiday	0.004 (0.005)	0.003 (0.005)	0.003 (0.005)	-0.001 (0.005)	-0.005 (0.005)	0.003 (0.005)	-0.001 (0.005)	-0.001 (0.005)	-0.001 (0.005)	-0.001 (0.004)
Spring Bank Holiday	-0.001 (0.004)	0.002 (0.005)	-0.008* (0.005)	-0.007* (0.004)	-0.000 (0.005)	-0.008* (0.005)	-0.007* (0.004)	-0.008** (0.004)	-0.003 (0.004)	-0.007 (0.004)
August Bank Holiday	-0.001 (0.004)	0.007 (0.005)	0.003 (0.005)	0.010* (0.006)	0.010* (0.006)	-0.001 (0.005)	-0.004 (0.004)	0.001 (0.005)	-0.003 (0.004)	-0.003 (0.004)
Christmas Day	-0.003 (0.004)	-0.004 (0.004)	-0.010*** (0.004)	-0.014*** (0.003)	-0.011*** (0.003)	-0.003 (0.004)	-0.008** (0.003)	-0.012*** (0.003)	-0.009*** (0.003)	-0.011*** (0.002)
Boxing Day	-0.006 (0.004)	-0.009** (0.004)	-0.006 (0.004)	-0.010*** (0.004)	-0.004 (0.004)	-0.009** (0.004)	-0.002 (0.004)	-0.009*** (0.003)	-0.008** (0.003)	-0.006* (0.003)
Maximum temperature	0.001*** (0.000)									
Minimum temperature	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)
Precipitation amount	-0.000 (0.000)	0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	-0.000 (0.000)	0.000 (0.000)	0.000 (0.000)	-0.000* (0.000)	-0.000 (0.000)	-0.000 (0.000)
Constant	0.006 (0.012)	0.001 (0.012)	0.001 (0.011)	0.004 (0.008)	0.005 (0.007)	0.011** (0.005)	0.006 (0.005)	0.004 (0.004)	0.009** (0.004)	0.010 (b)
Observations	381,408	381,408	381,408	381,408	381,408	381,408	381,408	381,408	381,408	381,408
Adjusted R-squared	0.065	0.031	0.022	0.018	0.016	0.012	0.011	0.011	0.012	0.019

^aEach dependent variable represents the number of serious or fatal accidents involving at least one driver from the relevant IMD. IMD1 is the lowest decile and represents drivers from addresses in areas with the highest level of deprivation. Robust standard errors are shown in parentheses.

^bThe model is unable to estimate a standard error for the constant as there is one region with a collinear (i.e. constant) time trend (zero accidents involving an IMD10 driver throughout the sample). IMD data only available from 1 Jan 2016 to 31 Dec 2018. ***p < 0.01, **p < 0.05, *p < 0.1.

Table A.3

RDiT (pooled) modelling results by IMD, England and Wales^a

IMD decile	Post_mobile		Post_speed		Constant		Adjusted R-squared	Mean no. KSI accidents
	Coefficient	SE	Coefficient	SE	Coefficient	SE		
1	-0.000	(0.001)	-0.003**	(0.001)	0.001**	(0.001)	0.000	0.022
2	-0.001	(0.001)	-0.002	(0.001)	0.001***	(0.001)	0.000	0.023
3	-0.001	(0.001)	-0.002	(0.001)	0.001*	(0.000)	0.000	0.022
4	-0.001	(0.001)	-0.002*	(0.001)	0.001*	(0.001)	0.000	0.023
5	-0.003**	(0.001)	-0.001	(0.001)	0.001**	(0.001)	0.000	0.022
6	-0.001	(0.001)	-0.003**	(0.001)	0.001**	(0.000)	0.000	0.022
7	0.001	(0.001)	-0.004***	(0.001)	0.001**	(0.000)	0.000	0.021
8	-0.000	(0.001)	-0.003***	(0.001)	0.001***	(0.000)	0.000	0.020
9	-0.001	(0.001)	-0.002*	(0.001)	0.001**	(0.000)	0.000	0.020
10	-0.002	(0.001)	-0.001	(0.001)	0.001***	(0.000)	0.000	0.017

^aThe RDiT sample covers 365 days either side of the speed intervention (April 24, 2017). Dependent variables are the residuals from the first step models. Robust standard errors are shown in parentheses. n = 254,388. Step 1 modelling results are given in table A.2. ***p < 0.01, **p < 0.05, *p < 0.1.

Table A.4

RDiT (pooled) modelling results by month/quarter, England and Wales^a

Variable	Month	Quarter
	Coefficient (SE)	Coefficient (SE)
post_mobile	0.003 (0.003)	0.003 (0.003)
post_speed coefficients		
mth1	-0.002 (0.005)	
mth2	-0.001 (0.005)	
mth3/qtr1	0.015*** (0.006)	0.004 (0.004)
mth4	-0.008 (0.005)	
mth5	-0.004 (0.005)	

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Table A.4 (continued)

Variable	Month	Quarter
	Coefficient (SE)	Coefficient (SE)
mth6/qtr2	-0.006 (0.005)	-0.006 (0.004)
mth7	-0.007 (0.005)	
mth8	0.020*** (0.005)	
mth9/qtr3	-0.005 (0.005)	0.003 (0.004)
mth10	0.005 (0.005)	
mth11	-0.007 (0.005)	
mth12/qtr4	-0.004 (0.005)	-0.002 (0.004)
mth13/qtr5	0.025** (0.012)	0.025** (0.012)
Constant	0.003** (0.001)	0.003** (0.001)
Adjusted R-squared	0.000	0.000

^aThe RDIT sample covers 365 days either side of the speed intervention (April 24, 2017). Dependent variables are the residuals from the first step model. Each month represents 30 days and each quarter 90 days, with the final month and quarter covering 5 days. Robust standard errors are shown in parentheses. n = 254,388. Step 1 modelling results are given in [table A.1](#). ***p < 0.01, **p < 0.05, *p < 0.1.

Table A.5

Step 1 modelling results, Difference-in-Difference models, number of serious or fatal accidents^a

Variables	Motorways	B and C roads	All LAs		Scotland	Border LAs		Scotland
			England and Wales	England and Wales				
LA-specific time trends	✓	✓	✓	✓	✓	✓	✓	✓
LA fixed effects	✓	✓	✓	✓	✓	✓	✓	✓
February	0.000 (0.001)	-0.000 (0.001)	0.000 (0.002)	0.000 (0.002)	-0.003 (0.006)	-0.004 (0.021)	-0.004 (0.023)	-0.046* (0.024)
March	-0.001** (0.001)	-0.003*** (0.001)	-0.007*** (0.002)	-0.018*** (0.005)	0.025 (0.023)	0.025 (0.025)	0.025 (0.025)	-0.020 (0.025)
April	-0.001* (0.001)	-0.004*** (0.001)	-0.012*** (0.002)	-0.014** (0.006)	0.046* (0.025)	0.046* (0.026)	0.046* (0.026)	-0.036 (0.026)
May	-0.001 (0.001)	-0.003*** (0.001)	-0.004** (0.002)	0.000 (0.006)	0.046* (0.026)	0.046* (0.026)	0.046* (0.026)	-0.002 (0.028)
June	-0.000 (0.001)	-0.003*** (0.001)	-0.007*** (0.002)	0.010 (0.007)	0.025 (0.029)	0.025 (0.029)	0.025 (0.029)	0.078** (0.031)
July	0.001 (0.001)	-0.004*** (0.001)	-0.009*** (0.002)	-0.010 (0.007)	0.027 (0.030)	0.027 (0.030)	0.027 (0.030)	0.034 (0.034)
August	0.001 (0.001)	-0.006*** (0.001)	-0.017*** (0.002)	0.003 (0.007)	0.050* (0.029)	0.050* (0.029)	0.050* (0.029)	0.077** (0.032)
September	-0.000 (0.001)	-0.001 (0.001)	0.000 (0.002)	0.009 (0.007)	0.024 (0.028)	0.024 (0.028)	0.024 (0.028)	0.066** (0.031)
October	0.000 (0.001)	0.001 (0.001)	0.008*** (0.002)	-0.002 (0.006)	0.008 (0.025)	0.008 (0.025)	0.008 (0.025)	0.005 (0.027)
November	0.000 (0.001)	0.005*** (0.001)	0.023*** (0.002)	0.009 (0.006)	0.000 (0.022)	0.000 (0.022)	0.000 (0.022)	-0.002 (0.025)
December	0.001 (0.001)	0.001* (0.001)	0.008*** (0.002)	-0.003 (0.005)	-0.006 (0.021)	-0.006 (0.021)	-0.006 (0.021)	0.001 (0.025)
Monday	-0.000 (0.000)	0.001** (0.001)	0.007*** (0.001)	0.002 (0.004)	-0.094*** (0.018)	-0.094*** (0.018)	-0.094*** (0.018)	-0.030 (0.019)
Tuesday	-0.001*** (0.000)	0.003*** (0.001)	0.015*** (0.001)	0.004 (0.004)	-0.072*** (0.018)	-0.072*** (0.018)	-0.072*** (0.018)	-0.022 (0.019)
Wednesday	-0.001*** (0.000)	0.003*** (0.001)	0.014*** (0.001)	0.007* (0.004)	-0.079*** (0.018)	-0.079*** (0.018)	-0.079*** (0.018)	-0.008 (0.019)
Thursday	-0.001** (0.000)	0.003*** (0.001)	0.017*** (0.001)	0.011*** (0.004)	-0.068*** (0.018)	-0.068*** (0.018)	-0.068*** (0.018)	-0.010 (0.019)
Friday	0.001*** (0.000)	0.007*** (0.001)	0.035*** (0.001)	0.027*** (0.004)	-0.046** (0.018)	-0.046** (0.018)	-0.046** (0.018)	0.005 (0.019)
Saturday	-0.001 (0.000)	0.004*** (0.001)	0.017*** (0.001)	0.027*** (0.004)	-0.018 (0.019)	-0.018 (0.019)	-0.018 (0.019)	0.033* (0.019)
New Year's Day	0.000 (0.002)	-0.005 (0.003)	-0.008 (0.007)	-0.048*** (0.017)	0.012 (0.107)	0.012 (0.107)	0.012 (0.107)	-0.043 (0.102)
Good Friday	-0.002 (0.002)	-0.006* (0.003)	-0.033*** (0.007)	-0.039** (0.019)	-0.093 (0.087)	-0.093 (0.087)	-0.093 (0.087)	0.074 (0.087)
Easter Monday	0.003	0.002	-0.010	-0.004	0.110	0.110	0.110	-0.009

(continued on next page)

Table A.5 (continued)

Variables	Motorways	B and C roads	All LAs	Scotland	Border LAs	Scotland
			England and Wales		England and Wales	
May Bank Holiday	(0.002) 0.005*	(0.003) -0.000	(0.007) -0.004	(0.020) 0.022	(0.129) 0.047	(0.080) 0.219
Spring Bank Holiday	(0.003) -0.002	(0.003) -0.005	(0.007) -0.028***	(0.026) 0.008	(0.102) 0.079	(0.141) 0.125
August Bank Holiday	(0.002) 0.003	(0.003) -0.002	(0.007) -0.006	(0.023) -0.016	(0.113) 0.039	(0.100) -0.124
Christmas Day	(0.003) -0.001	(0.003) -0.014***	(0.007) -0.069***	(0.021) -0.033*	(0.101) 0.095	(0.083) -0.110
Boxing Day	(0.002) 0.001	(0.003) -0.011***	(0.006) -0.057***	(0.015) -0.058***	(0.118) 0.047	(0.067) -0.147**
Royal Wedding Day	(0.005) -0.005	(0.011) -0.003	(0.022) -0.007	(0.113) 0.131	(0.310) 0.684**	(0.663) 1.371**
Diamond Jubilee Day	(0.005) -0.003	(0.011) 0.004	(0.024) -0.003	(0.065) -0.013	(0.026) -0.432***	(0.055) -0.397***
Maximum temperature	0.000***	0.001***	0.005***	0.004***	0.007***	0.000
Minimum temperature	0.000	(0.000)	(0.000)	(0.000)	(0.002)	(0.002)
Precipitation amount	-0.000***	-0.001***	-0.003***	-0.002***	-0.006***	-0.001
Constant	0.000	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
	(0.000)***	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
	0.007***	0.028***	0.097***	0.130***	0.203***	0.318***
	(0.001)	(0.001)	(0.002)	(0.006)	(0.027)	(0.026)
n	679,272	1,270,896	1,270,896	116,864	12,613	12,012
Adjusted R-squared	0.007	0.021	0.073	0.084	0.052	0.032

^a Border LAs are: Allerdale, Barrow-in-Furness, Carlisle, Copeland, Eden, Northumberland, South Lakeland in England and City of Edinburgh, Dumfries and Galloway, East Lothian, Midlothian, Scottish Borders and West Lothian in Scotland. Robust standard errors are shown in parentheses. ***p < 0.01, **p < 0.05, *p < 0.1.

Table A.6
DiD (pooled) modelling results, England and Scotland border^a

Variables	Coefficient (SE)
Post_mobile	0.011 (0.034)
T (1 = post_speed)	-0.044 (0.038)
G (1 = England/Wales)	0.002 (0.024)
DiD	0.025 (0.033)
Constant	0.022 (0.018)

^aThe DiD sample covers 365 days either side of the speed intervention (April 24, 2017). Dependent variable is the residuals from the first step models. Only LAs near the Scottish border are included (Allerdale, Barrow-in-Furness, Carlisle, Copeland, Eden, Northumberland, South Lakeland in England and City of Edinburgh, Dumfries and Galloway, East Lothian, Midlothian, Scottish Borders and West Lothian in Scotland). Robust standard errors are shown in parentheses. Adjusted R-squared = -0.000. n = 4630. Step 1 modelling results are given in table A.4. ***p < 0.01, **p < 0.05, *p < 0.1.

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