Does Going Slower Really Make Us Safer? A Difference-in-Differences Approach on the Effect of Speed Limit Reductions on the Collision Rate in Wales.

Cristian Dinuta, Kajsa Falsen & Fabian Steinmetz
Minerva University
14 Mint Plaza, Suite 300, San Francisco, CA 94103

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Abstract

The relationship between vehicle speed and accident severity is well established, and speed limits are a widely used policy instrument to improve road safety. However, their effectiveness is uncertain, as it depends on driver compliance and enforcement, and poorly accepted or weakly enforced limits may undermine policy legitimacy and reduce their impact. This paper evaluates the effect of Wales' 2023 reduction in default speed limits on restricted roads from 30 to 20mp/h on accident rates. Using a difference-in-differences approach, we first assess the validity of the parallel trends assumption and then estimate policy effects using a two-way fixed effects model. Our results indicate a substantial reduction in accident rates following the policy intervention, with estimated effects ranging between 18% and 20%. However, the statistical significance of these estimates varies across time periods, although most post-treatment periods are significant at the 95% level. Consequently, while the results suggest a potentially meaningful reduction in accidents, the overall evidence remains inconclusive.

1 Introduction

Road transportation is the most dangerous activity most people engage in daily. While its ubiquity has made this risk nearly invisible, collisions, in fact, claim approximately 1.2 million lives globally each year while injuring between 20 and 50 million more. As the leading cause of death for individuals aged 5–29 years worldwide (WHO, 2024), they also impose costs that extend far beyond immediate human suffering: healthcare expenditures and foregone productivity associated with road crashes consume approximately 3% of GDP in most countries (World Health Organization, 2023). Despite marginal improvements in recent years, this burden persists largely unchanged, suggesting that conventional approaches to road safety may be reaching their limits of effectiveness. The relationship between vehicle speed and collision severity is well-established in the scientific literature. UK Highway Code (2025) data reveal that a vehicle traveling at 30mp/h covers the same distance as a 20mp/h vehicle's full stopping distance while still moving at 24mp/h, which corresponds to a 40% reduction in fatal crashes (Yannis & Michelaraki, 2024) and attracts positive externalities including reduced fuel consumption and increased active travel. Similarly, Transport for London's (2025) empirical experience shows that the introduction of 20mp/h limits within the central Congestion Charging Zone yielded a 25% decrease in serious collisions, compared to 10% across London overall.

1.1 The Welsh context

Importantly, collision risk is not uniformly distributed. In Wales, 51% of collisions occur on 30 mp/h so called "restricted" roads, which are located in residential areas with high pedestrian activity (gov.wales, 2022). In September 2023, Wales became the first UK nation to lower the default speed limit from 30 to 20 mp/h on all restricted roads, following successful trials across eight locations between June 2021 and May 2022 [1]. This policy falls under the Well-Being of Future Generations Act and has raised questions about its effectiveness in media and politics, especially due to the £34M price only for introduction costs (Murray, 2025).
Preliminary data from mid-2024 show reduced casualties on affected roads, though officials acknowledge that multi-quarter observation periods are necessary for definitive conclusions (Department of Transport, 2023). This paper employs a difference-in-differences with two-way fixed effects approach to estimate the causal effect of Wales' default 20 mp/h policy on road collisions, using the implementation constrained to Wales to distinguish treatment effects from confounding factors and provide empirical evidence on the effectiveness of this policy.

2 Literature Review

There is generally a large body of research attempting to determine the causes of traffic accidents. Establishing the causes behind accidents is often challenging, as traffic accidents are relatively rare events, involve a substantial element of randomness, and typically result from multiple interacting factors, including driver behaviour, road conditions, vehicle characteristics, and environmental factors. Elvik et al. (2019) synthesise evidence on the relationship between speed and road safety outcomes and find a strong, non-linear association between average speed and both fatal and injury accidents, with some evidence that this relationship has strengthened since the early 2000s. At the same time, some studies find that speed affects accident severity more than accident frequency, underscoring the importance of context and road type (Wilmot, 1999). Speed regulation has long been used as a policy tool to influence driving behaviour for several purposes. Beyond road safety, speed limits are implemented to reduce fuel consumption and manage air quality,

[1] Because these districts were assigned the treatment for a short period of time, these were excluded from the treatment group, leaving $n = 16$ unique ONS districts in Wales to be part of the data.

mitigate traffic noise, and improve overall traffic flow. Their effectiveness, however, depends on the extent to which posted speed limits translate into actual driving speeds, which in turn often requires complementary enforcement or traffic-calming measures (Magkafas et al. 2025). In 1986 the US increased speed limits on specific highways. Ten years later, Lave et al (1996) found that increasing speed limits actually lowered fatality by 3-5 percent. They propose that this could be because enforcement resources was more effectively used elsewhere, and also argue that too low speed limits decrease the public's perception of their legitimacy. They also highlight that increasing the speed limit led to lower speed dispersion, which is another important factor in causing accidents. However, these findings are context-specific, derived from high-speed highways in the United States, where driving culture, enforcement practices, and road design differ substantially from those in the United Kingdom. In contrast, this study focuses on lower-speed roads, where interactions with pedestrians, cyclists, and local traffic are more frequent, and where reductions in speed are more likely to affect both crash probability and severity. As such, existing evidence from high-speed motorway settings may not generalize to the policy context examined here. Overall, the literature suggests that while speed is a central component of road safety, the effects of speed limit changes depend on road type, enforcement, behavioral responses, and baseline driving conditions. This heterogenity makes it interesting to study the effect of the changes in Wales' speed limits.

2.1 Causal dynamic

The unit of observation in this study is geographic areas within the United Kingdom. In 2019, Wales implemented a reduction in the speed limit on all restricted roads. The policy applied uniformly to all eligible roads in Wales and to none in England, Scotland, where the old baseline speed limit was kept in place. This setting lends itself to a difference-in-differences design, comparing changes in accident rates in Wales to contemporaneous changes elsewhere in the UK. As we will be using difference-in-differences, we need to think about confounders a bit differently than we might be used to from simpler methods. For DiD, we are comparing developments, which means the base level difference between Wales and England and Scotland does not matter. However, we are assuming that in the absence of the policy change, the development in accident rates would have been the same in Wales as in the rest of the UK. This means that if some areas make other changes, say they update their road infrastructure, we might wrongly attribute the improvement in accident rates to the policy change even if it is not. Ultimately, we need to identify variables that affect accident rates and change differently across groups over time. To help us better understand the causal dynamic, we constructed a Directed Acyclic Graph, based on the variables identified as potentially important in the literature review, most importantly: road conditions, weather and traffic density. From these, we zoomed out once more, to find the variables they were highly correlated to, to make sure we understood the full causal dynamic. All links are displayed in our DAG (Fig. 1).

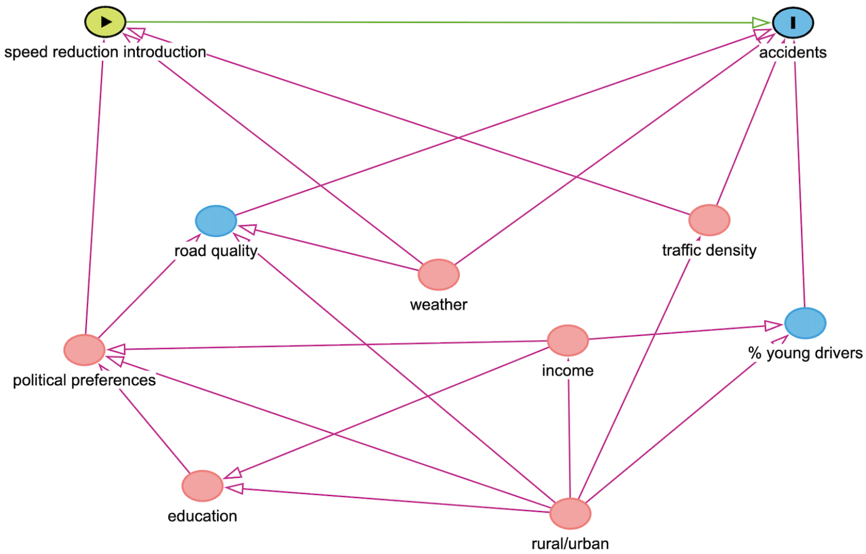

Fig. 1. Directed Acyclic Graph.

2.2 Covariates and confounders

Weather affects accident risk both directly through visibility and road surface conditions, and indirectly, by influencing road degradation and travel behaviour (Theofilatos, 2014; Cools et al., 2010). Traffic density captures exposure to risk and changes in congestion and mode composition (Albate, 2021). Socio-economic characteristics, such as income and education, may influence driving behaviour, vehicle quality, and support for traffic regulation, and may evolve differently across regions over time. Based on the DAG, we identified 31 backdoor paths (see Appendix B). To close all of these and control for all confounders, we identify the following minimal adjustment sets:

$$S_1 = \{\text{young drivers}, \text{road quality}, \text{traffic density}, \text{weather}\},$$
$$S_2 = \{\text{income}, \text{road quality}, \text{rural/urban}, \text{traffic density}, \text{weather}\},$$
$$S_3 = \{\text{political preferences}, \text{traffic density}, \text{weather}\}.$$

Based on the availability of data, S_2 was chosen as our set of confounding variables. We control for unit-level, time-varying measures of weather, traffic density, and area-level socio-economic characteristics, while relying on unit fixed effects to absorb time-invariant confounders such as urban classification, and time fixed effects to capture common shocks (like COVID-19). This combination allows us to isolate changes in accident risk conditional on traffic exposure and environmental conditions.

3 Data

Data was primarily sourced from the official British government website, or the National Office for Statistics[2].

3.1 Collision Rate - Accidents per 10,000 inhabitants

Data on road collisions is collected by the British police and reported yearly in the STATS19 dataset. Hence, this only includes police-reported and attended collisions and excludes cases where the police was not contacted, e.g. for very minor accidents. While this limits the generalizability of this study as it only captures specific, police-recorded types of collisions, it is reliable and provides very large sample sizes. To obtain pre- and post-treatment data, the "Last 5 Year report", which records every single reported case by date and locality, was aggregated to a quarterly level. As 2025 data has not been released as part of the official publications yet, the provisional estimates for the first two quarters were matched on the historic data using ONS codes. Finally, the accidents were normalized over the corresponding ONS code's population, which is recorded by the Office of National Statistics on a yearly basis (ONS, 2025). While there might be some spillover effects because collisions happen outside of the casualties' place of residence, this normalization is used for consistency.

Table 1. Descriptive Statistics by Treatment Status and Period

	Pre		**Post**	
	Control	Treatment	Control	Treatment
Collisions (mean)	3.720	2.658	3.583	2.095
Collisions (std)	2.070	1.341	1.828	1.279
Traffic density (mean)	9014.129	5255.050	8516.882	4295.595
Traffic density (std)	8451.463	7691.247	8194.003	4685.256
Income (mean)	6962.642	6173.417	8074.236	7179.821
Income (std)	1108.791	489.761	1170.028	559.778
Rainfall (mean)	210.042	356.818	242.879	417.702
Rainfall (std)	110.155	178.662	107.968	171.602

3.2 Precipitation

To construct the rainfall control variable, we rely on data from Met Office Climate Data Portal, which provides weather information on 1×1 km spatial grids covering

[2] The dataset can be accessed through this link.

the entire UK. Rainfall is measured in millimeters and compiled at a monthly frequency. We downloaded data covering 2020–2024 and the first three months of 2025 from the Met Office's website. Grid-level rainfall measures were spatially matched to 2023 local area boundaries to be able to see the change over time per unit. The final rainfall measure is obtained by aggregating grid-level observations within each local area.

3.3 Income

Data on gross annual income was obtained from the UK ONS on gov.uk. The data is published annually in batches and was extracted from Table 8.7.a, which reports gross annual income in British pounds by local authority using ONS area codes. To obtain income into a quarterly panel aligned with the policy introduction, I matched observations across datasets using ONS area codes and modified annual income figures into quarterly averages by dividing annual values by four. This transformation assumes income is evenly distributed across quarters, is required due to unavailability. Due to local authority mergers implemented around 2022, some areas could not be consistently matched from that year onward, resulting in missing observations for those units in the post-merging period. Data for Northern Ireland was excluded from the study because it was only available in aggregate form, not by area code.

3.4 Traffic Density

We extracted granular government data on traffic volume (number of vehicles using a road in a year), and aggregated by ONS-code and quarter. To create a comparable estimate of the traffic density in each local authority, we divided the corresponding yearly traffic volume by the length of the road network which was available on the British Transport Department.

4 Methodology

Since the policy was introduced in Wales in September 2023, it generated a clearly defined intervention occurring at a specific point in time and affecting a well-defined set of units (ONS local authorities in Wales), while comparable units in England and Scotland were not exposed to the policy and create a natural control group. Since treatment assignment is not randomized, observable characteristics such as traffic density, road quality, and weather conditions differ systematically across areas, implying that a simple cross-sectional regression of accident outcomes on treatment status would be vulnerable to confounding bias. While including observed confounders can mitigate some of this bias, such a specification would still rely on the strong assumption that treated and control units are otherwise comparable. This assumption is unlikely to hold in the presence of unobserved, time-invariant differences across areas, such as road topology, enforcement intensity, or local driving culture, that are correlated with both treatment exposure and accident risk.

A difference-in-differences (DiD) approach is therefore more appropriate, as it exploits variation over time to compare changes in outcomes in treated and untreated areas before and after the policy implementation, rather than relying on potentially misleading cross-sectional comparisons.

4.1 Two-way fixed effects model

More specifically, we implemented a two-way fixed effects DiD at the ONS area and quarter level, controlling for both area fixed effects (α_i for each area i) and time fixed effects (λ_t for each time period t) . Area fixed effects refer to aspects that do not vary with time, such as road incline or urbanization level, while time fixed effects tackle aspects affecting all or most regions, like rising gas prices. The treatment variable indicates whether an observation is an area in Wales in the post-policy period, while period-specific interactions allow us to analyze the evolution of treatment effects over time. In our model, we control for observed, time and unit varying covariates that might affect collisions differently across districts and time, including average income, log traffic density, and rainfall. We selected the study period from 2020 to 2025 to include a sufficiently long pre-treatment phase of 14 quarters, ensuring the parallel trends assumption holds prior to the start of the treatment in September 2023. This yields the following regression equation:

$$\begin{aligned}\log(\text{Collisions}_{it}) =& \beta_1(\text{Treatment}_i \times \text{Post}_t) + \beta_2 \log(\text{Traffic}_{it}) + \beta_3 \text{Weather}_{it} \\ &+ \beta_4(\text{Income}_{it}) + \alpha_i + \lambda_t + \epsilon_{it}\end{aligned}$$

4.2 Parallel trends assumption

The DiD method assumes that the treatment group would follow the same trend as the control group, had treatment not been administered. In the context of this study, this means that the collision rate would continue to move in parallel without the Welsh government's intervention. However, as we cannot observe potential outcomes of Wales under no treatment, we used several pre-treatment periods to assess whether treatment and control groups follow similar trends, cycles, or seasonal fluctuations up until the time of treatment. While there is no formal way to assess the exact extent to which this parallel trends assumption is met, judgment by eye (Fig. 2) tells us that the number of collisions in Wales and Scotland/England follow similar trends, despite the groups having slightly different orders of magnitude and deviations in the magnitude of the fluctuations.

5 Analysis

The first significant treatment effect at the 95% confidence level happens immediately after the policy comes into place in the fourth quarter of 2023. This increases our confidence level that the treatment effect actually represents a causal effect. The way to interpret the interaction coefficients is that the treatment group had a $(1 - e^{\text{coefficient}})$% lower number of collisions compared to the control group, *and*

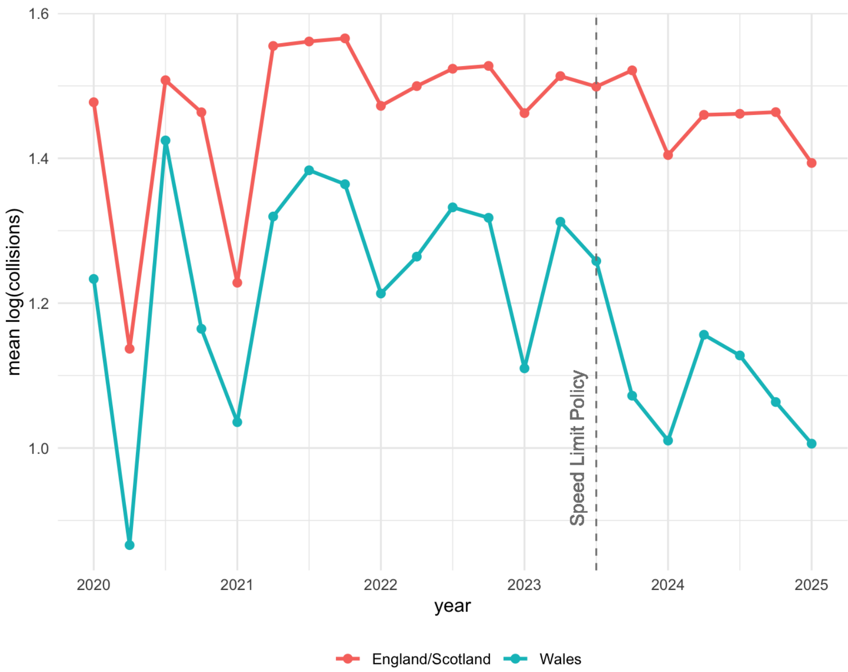

Fig. 2. Plotting the parallel trends assumption. While some of the dips appear larger in the treatment group, both groups follow the same fluctuations up until the treatment.

in relation to the treatment quarter (Q3–2023). So we are calculating how the number of collisions changed over time for each of the post treatment quarters (this is the first difference), and comparing these changes between treatment and control groups (this is the second difference). In the first post-treatment quarter (2023-Q4), the interaction term coefficient of approximately 0.2 tells us that imposing the speed limit led to a 22.5% reduction in the number of collisions in Wales. This effect is highly statistically significant with a p-value of 0.003, which means that this effect is very likely not due to chance. Figure 3 shows the coefficient of each interaction term alongside the 95% confidence interval which corresponds to the coefficient $\pm\, 1.96 \cdot s_e$.

A potential reason why the effect is the most pronounced in the first quarter, could be that awareness about the new policy heightened drivers' attention, reducing the number of collisions. It is also possible that enforcement was increased in an attempt to establish new a culture of adherence to these lowered speed limits. While our regression controls for unit-specific characteristics, common time shocks, weather, income, rural/urban divides and traffic density, we do not observe this granular level data of enforcement intensity directly and therefore cannot control for this.

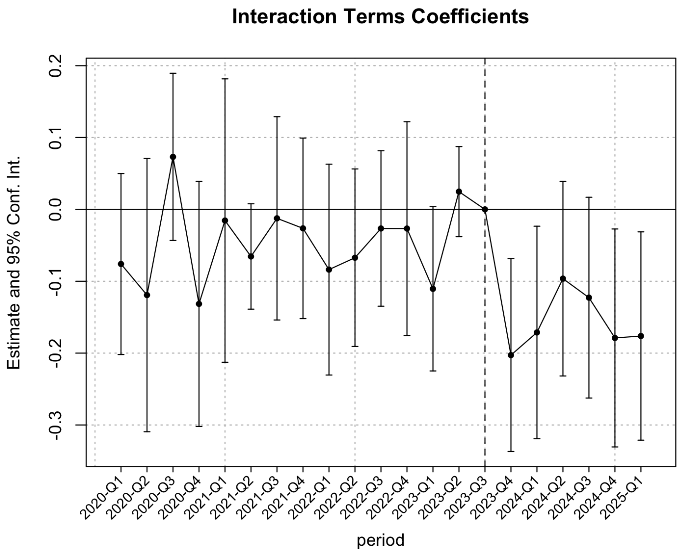

Fig. 3. Coefficients and their 95% confidence interval. Estimates are statistically significant under this confidence level if the error bars do not cross the horizontal line $y = 0$.

Table 2. Estimated Treatment Effects by Period

Variable	**Estimate**	**Std. Error**	**t value**	**p-value**
period::2023-Q4 × treatment	-0.20289	0.06797	-2.985	0.003^{***}
period::2024-Q1 × treatment	-0.17149	0.07452	-2.301	0.023^{**}
period::2024-Q2 × treatment	-0.09626	0.06859	-1.403	0.162
period::2024-Q3 × treatment	-0.12281	0.07058	-1.740	$0.084^{\dagger}$
period::2024-Q4 × treatment	-0.17913	0.07663	-2.337	0.021^{**}
period::2025-Q1 × treatment	-0.17626	0.07301	-2.414	0.017^{**}
log_traffic_density	-0.00469	0.00227	-2.067	0.040^{**}
income	0.00001	0.00001	0.722	0.471
rainfall	-0.00008	0.00008	-1.065	0.288

Notes: $^{\dagger}p < 0.10$, $^{*}p < 0.05$, $^{**}p < 0.025$, $^{***}p < 0.01$.

Another important observation is the absence of significant treatment effects for all pre-treatment quarters (Table 3). This further supports the parallel trend assumption as there is no credible evidence that there is a meaningful difference in collision trends before treatment started. Finally, while we obtain statistically significant results for most of the post-treatment period, the estimates for mid-2024 (Q2 and Q3) are statistically insignificant. This weakens the overall strength of our results and may reflect short-term adjustment effects, increased variability in outcomes, or limited statistical power due to the short post-treatment period.

Table 3. Pre-Treatment Effects by Period (2020–2023)

Variable	**Estimate**	**Std. Error**	**t value**	**p-value**
period::2020-Q1 × treatment	-0.07733	0.06390	-1.210	0.228
period::2020-Q2 × treatment	-0.12023	0.09658	-1.245	0.215
period::2020-Q3 × treatment	0.07196	0.05894	1.221	0.224
period::2020-Q4 × treatment	-0.13284	0.08660	-1.534	0.127
period::2021-Q1 × treatment	-0.01599	0.10025	-0.160	0.873
period::2021-Q2 × treatment	-0.06584	0.03715	-1.772	$0.078^{\dagger}$
period::2021-Q3 × treatment	-0.01266	0.07198	-0.176	0.861
period::2021-Q4 × treatment	-0.02681	0.06398	-0.419	0.676
period::2022-Q1 × treatment	-0.08570	0.07445	-1.151	0.251
period::2022-Q2 × treatment	-0.06900	0.06246	-1.105	0.271
period::2022-Q3 × treatment	-0.02825	0.05476	-0.516	0.607
period::2022-Q4 × treatment	-0.02872	0.07565	-0.380	0.705
period::2023-Q1 × treatment	-0.11060	0.05789	-1.911	$0.058^{\dagger}$
period::2023-Q2 × treatment	0.02501	0.03175	0.788	0.432

Notes: ${}^{\dagger}p < 0.10$, ${}^{*}p < 0.05$, ${}^{**}p < 0.025$, ${}^{***}p < 0.01$.

5.1 Placebo Tests

The robustness of the calculated effects was strengthened through several placebo tests. For this, the regression is run with a deliberately misplaced treatment quarter to assert that no significant treatment effects follow. Placebo tests were run for 2022-Q3 and 2021-Q4, one and two years before the treatment actually happened, and the coefficients and their significance level were reported (Appendix A). In all placebo test cases, significant effects only appear after the true policy implementation in the fourth quarter of 2023, if they appear at all. This is the expected outcome because no treatment was actually administered at these quarters, which should hence not produce any treatment effect.

6 Limitations

6.1 Data limitations

The main limitation of our data is the difference in time availability across variables. Income data was only available on an annual basis, so we could not assess variation throughout the year or capture any seasonality. In contrast, traffic volume and traffic density were measured quarterly, and weather data was available on a monthly basis. To address this, we compiled all variables at the ONS area level and adjusted them to a quarterly frequency in order to have a uniform analysis. This adjustment comes at the cost of slightly reduced precision, as some variation is smoothed out, limiting our ability to observe short-term dynamics and seasonal effects.

Additionally, due to data availability constraints, we excluded Northern Ireland from the study, as comparable area-level data could not be found and it was only available in aggregated form. Another limitation is that we analyze the total number of traffic accidents, without distinguishing between accident severity or fatalities, which limits the interpretation of the results in terms of the magnitude.

6.2 Design limitations

In terms of timing, the policy was implemented in September 2023, which corresponds to the last week of the third quarter of the year. This way, when using 2023-Q3 as the treatment quarter, we might underestimate the consequent treatment effects as in 2023-Q3, Wales was exposed to 1 week of treatment already.

The post-treatment period is also relatively short, as the policy was only recently implemented. As a result, future replications of this study with more comprehensive data and a longer post-treatment window could provide more accurate estimates and allow for an evaluation of longer-term effects.

Lastly, our design only looks at the rate of the number of accidents, and neglects any measure of severity, which limits our ability to really understand the full impact of the policy implementation.

7 Conclusion

We find strong effect of the speed limit reduction, ranging between a 18 and 22% decrease of accidents in Wales after the implementation of the new default limit. However, the significance of these effects is highly variable. The strong, significant effect immediately after the implementation might be attributed to increased awareness or policing in the immediate aftermath, but this would not explain why the effect is visible again a year after the implementation. This might be due to key limitations of our study design based on current data availability. Future studies should include more post-implementation periods, as these become available, extending on the possible trends and patterns uncovered in this study.

Additionally, given that reductions in accident frequency appear robust, future studies should extend the analysis to include measures of accident severity. Incorporating information on injury severity and fatalities would allow for a more comprehensive assessment of the policy's welfare benefits and enable comparison with the relatively high costs associated with implementation. Such extensions would provide a clearer basis for evaluating the overall effectiveness and cost-efficiency of speed limit reductions.

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9 Appendix

9.1 Appendix A: Data & Code Documentation

The DiD analysis and plots were created using R. The data can be accessed through this link, and the R code is available here.

9.2 Appendix B: Backdoor Paths

Backdoor Paths:

1. speed reduction introduction ← traffic density → accidents
2. speed reduction introduction ← weather → accidents
3. speed reduction introduction ← weather → road quality → accidents
4. speed reduction introduction ← political preferences → road quality → accidents
5. speed reduction introduction ← traffic density ← rural/urban → road quality → accidents
6. speed reduction introduction ← traffic density ← rural/urban → % young drivers → accidents
7. speed reduction introduction ← traffic density ← rural/urban → income → % young drivers → accidents
8. speed reduction introduction ← traffic density ← rural/urban → political preferences → road quality → accidents
9. speed reduction introduction ← traffic density ← rural/urban → education → political preferences → road quality → accidents
10. speed reduction introduction ← traffic density ← rural/urban ← income → % young drivers → accidents
11. speed reduction introduction ← traffic density ← rural/urban ← income → education → political preferences → road quality → accidents
12. speed reduction introduction ← traffic density ← rural/urban ← income → political preferences → road quality → accidents
13. speed reduction introduction ← traffic density ← rural/urban ← education → political preferences → road quality → accidents
14. speed reduction introduction ← political preferences ← rural/urban → road quality → accidents
15. speed reduction introduction ← political preferences ← rural/urban → % young drivers → accidents
16. speed reduction introduction ← political preferences ← rural/urban → income → % young drivers → accidents
17. speed reduction introduction ← political preferences ← rural/urban → traffic density → accidents
18. speed reduction introduction ← political preferences ← education → income → % young drivers → accidents
19. speed reduction introduction ← political preferences ← income → % young drivers → accidents
20. speed reduction introduction ← political preferences ← income → education ← rural/urban → road quality → accidents
21. speed reduction introduction ← political preferences ← income ← rural/urban → road quality → accidents
22. speed reduction introduction ← political preferences ← income ← rural/urban → % young drivers → accidents
23. speed reduction introduction ← political preferences ← income ← rural/urban → traffic density → accidents

24. speed reduction introduction ← political preferences ← education ← rural/urban → road quality → accidents
25. speed reduction introduction ← political preferences ← education ← rural/urban → % young drivers → accidents
26. speed reduction introduction ← political preferences ← education ← rural/urban → income → % young drivers → accidents
27. speed reduction introduction ← political preferences ← education ← rural/urban → traffic density → accidents
28. speed reduction introduction ← weather → road quality ← rural/urban → traffic density → accidents
29. speed reduction introduction ← weather → road quality ← rural/urban → % young drivers → accidents
30. speed reduction introduction ← weather → road quality ← rural/urban → income → % young drivers → accidents
31. speed reduction introduction ← weather → road quality ← political preferences → speed reduction introduction (creates a loop, invalid)

9.3 Appendix C: Placebo Tests

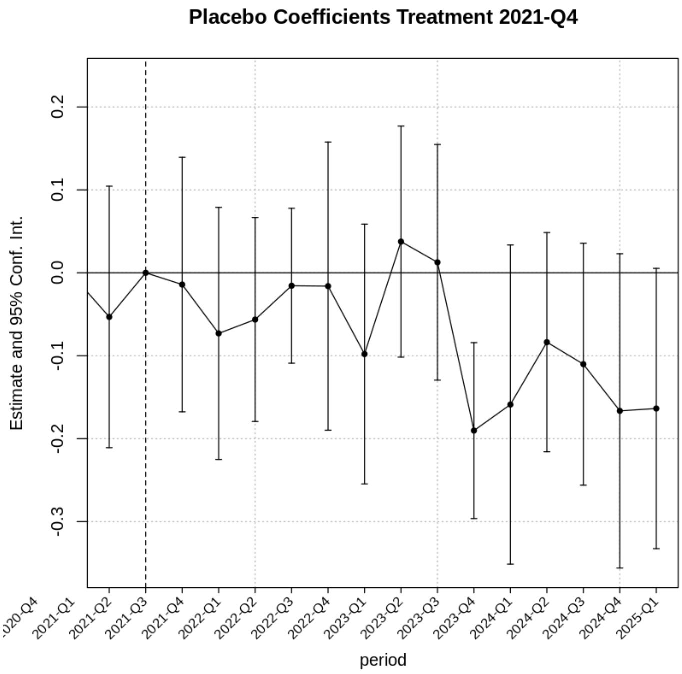

Fig. 4. Placebo 2021-Q4. Coefficients and 95% confidence intervals are plotted.

Table 4. Event-Study and Covariate Regression Results

	Estimate	Std. Error	t-value	p-value
period::2021-Q4 × Treatment	-0.014	0.078	-0.182	0.856
period::2022-Q1 × Treatment	-0.073	0.077	-0.949	0.344
period::2022-Q2 × Treatment	-0.056	0.062	-0.904	0.367
period::2022-Q3 × Treatment	-0.016	0.047	-0.329	0.742
period::2022-Q4 × Treatment	-0.016	0.088	-0.182	0.855
period::2023-Q1 × Treatment	-0.098	0.079	-1.234	0.219
period::2023-Q2 × Treatment	0.038	0.071	0.534	0.594
period::2023-Q3 × Treatment	0.013	0.072	0.176	0.861
period::2023-Q4 × Treatment	-0.190	0.054	-3.540	0.001***
period::2024-Q1 × Treatment	-0.159	0.097	-1.629	0.105
period::2024-Q2 × Treatment	-0.084	0.067	-1.249	0.213
period::2024-Q3 × Treatment	-0.110	0.074	-1.491	0.138
period::2024-Q4 × Treatment	-0.166	0.096	-1.734	0.085*
period::2025-Q1 × Treatment	-0.164	0.086	-1.911	0.058*
Log Traffic Density	-0.0047	0.0023	-2.067	0.040**
Income	0.00001	0.00001	0.722	0.471
Rainfall	-0.00008	0.00008	-1.065	0.288

Notes: Robust standard errors reported. $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.10$.

Table 5. Placebo Treatment Effects (Treatment Assigned in 2022-Q2)

Variable	**Estimate**	**Std. Error**	**t value**	**p-value**
period::2022-Q3 × treatment	0.04075	0.05464	0.746	0.457
period::2022-Q4 × treatment	0.04028	0.08209	0.491	0.624
period::2023-Q1 × treatment	-0.04161	0.06478	-0.642	0.522
period::2023-Q2 × treatment	0.09400	0.06282	1.496	0.136
period::2023-Q3 × treatment	0.06900	0.06246	1.105	0.271
period::2023-Q4 × treatment	-0.13389	0.07651	-1.750	$0.082^{\dagger}$
period::2024-Q1 × treatment	-0.10249	0.08532	-1.201	0.231
period::2024-Q2 × treatment	-0.02726	0.06276	-0.434	0.665
period::2024-Q3 × treatment	-0.05381	0.07161	-0.751	0.453
period::2024-Q4 × treatment	-0.11013	0.08885	-1.240	0.217
period::2025-Q1 × treatment	-0.10727	0.08100	-1.324	0.187
log_traffic_density	-0.00469	0.00227	-2.067	0.040^{*}
income	0.00001	0.00001	0.722	0.471
rainfall	-0.00008	0.00008	-1.065	0.288

Notes: This table reports placebo difference-in-differences estimates where treatment is artificially assigned to 2022-Q3. $^{\dagger}p < 0.10$, $^{*}p < 0.05$.

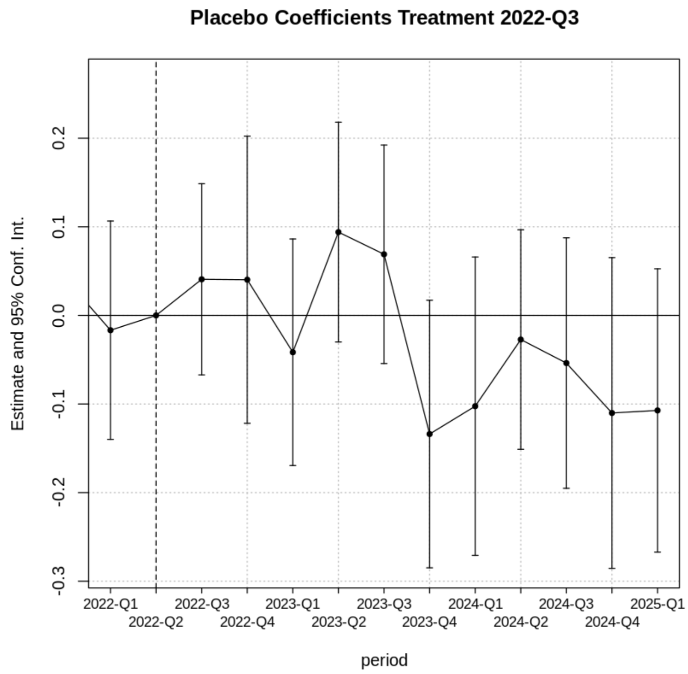

Fig. 5. Placebo 2022-Q3. Coefficients and 95% confidence intervals are plotted.