Registered Report

Does the Fear-Victimisation Paradox Apply to the Context of Road Violence?

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Abstract

This registered report investigates whether involvement in road traffic collisions meaningfully shapes pedestrians' and cyclists' perceptions of safety. Drawing on criminological insights - particularly the fear-victimisation paradox - we test whether previous literature showing no association between involvement in collisions and perceptions of road safety among cyclists could be explained by confounding bias. Using ESRA2 survey data from 24 European countries, the analysis aims to approximate average treatment effects of collision experience on perceived safety, accounting for vulnerability (age, gender) and exposure (active travel frequency). The study employs equivalence testing to assess whether any difference in victims' perceptions is substantively meaningful. If our hypotheses are corroborated, this study will: i) clarify a current paradox from the road violence and active travel literature; ii) demonstrate shared mechanisms in perceptions of safety across different contexts; and iii) open the possibility of designing more effective road safety and active travel strategies by drawing lessons from the fear of crime literature and crime prevention strategies seeking to improve perceptions of safety.

Keywords: Collisions; Road safety; Active travel; Fear of crime; Victimisation

Research Rationale

There is a growing recognition that road collisions represent the tip of an iceberg of harm imposed by car dependency.[1] However, transitioning from a car-centric transport model to one where active travel plays a substantial role can only be achieved by ensuring that vulnerable road users feel safe doing so (Campos, 2022). Following that logic, the latest UK Road Safety Strategy has set out the ambitious goal of 65% reduction in the number of people killed or seriously injured on roads in Great Britain by 2035 (Department for Transport, 2026).[2] The Road Safety Strategy further assumes that such reduction in collisions alone will improve perceptions of safety and ultimately encourage active travel: *"[…] improved road safety has benefits for everyone. Everyone wants to live in a place where parents can get their children to school by walking or wheeling, where active travel feels like a safe choice,"* (p.10).

It is tempting to view perceptions of safety as entirely determined by the risk of collision to which individuals are exposed, but that supposes perfect rational thinking and neglects how risk is moderated by one's perceived vulnerability. The field of criminology offers ample evidence of the discrepancy between objective risk of victimisation and subjective perceptions of safety. In the first iteration of the British Crime Survey, Hough and Mayhew (1983) demonstrated how those most at risk of victimisation - younger men - reported the lowest levels of fear, while those least at risk - older women - reported the highest.

This 'fear-victimisation paradox' has since been replicated across multiple national contexts and methodological approaches, showing that neighbourhood cues, perceived incivilities, media exposure, and, crucially, demographic characteristics associated with perceived physical vulnerability such as age and sex, can be stronger predictors of fear of crime than official crime statistics (Lorenc et al., 2012; Köber & Oberwittler, 2019). These findings suggest that perceptions of risk are shaped not only by exposure to harm, but also by broader cognitive and affective processes, including perceived vulnerability, the salience of rare events, and limited control over one's environment (Slovic, 1987; Ferraro, 1995).

Building on this insight, we consider whether similar processes may operate in the context of road traffic. Collisions, like crimes, are relatively rare given the frequency of everyday interactions with traffic, yet they are often sudden, difficult to anticipate, and associated with potentially severe consequences. Research in risk perception suggests that such characteristics tend to amplify subjective perceptions of risk and shape behavioural responses (Kahneman, 2011). From this perspective, we draw on the fear-victimisation paradox as a framework for understanding how perceived safety - conceived here as a broader evaluation of subjective risk rather than a direct equivalent of fear of crime- may be shaped by factors such as vulnerability, exposure, and perceived control.

Intentionality is perhaps the clearest feature distinguishing violent crime from most road collisions, both in legal and moral terms. Yet, we do not think that will affect the analogy that we are suggesting here since we expect the role of criminal intent in shaping perceived safety to be limited. For individuals navigating public space, both risks - crime victimisation and road collision - may be experienced as externally imposed and difficult to control, activating similar responses in terms of perceived vulnerability and cautious behaviour. This aligns with broader evidence showing that perceptions of safety are often more strongly influenced by feelings of exposure and lack of control than by the objective nature or origin of the risk itself (Jackson, 2009; Loader et al., 2025).

Beyond perceived vulnerability and the lack of control over the situation, the built environment represents an important contextual factor that can amplify both mechanisms. Environmental characteristics can heighten individuals' perceptions of vulnerability while simultaneously reducing their sense of control over their surroundings. In the context of fear of crime, cues such as inadequate street lighting and poor neighbourhood maintenance have been consistently associated with an increased perceived risk of victimisation (Kuen et al., 2022; Burt et al., 2026). Similar environmental characteristics have also been

[1] To put it in perspective, leaving aside any forms of social, economic and environmental harm (Anciaes et al., 2022; Miner et al., 2024), and focusing just on its public health impact: approximately 170,000 cardiovascular deaths are recorded in the UK per year (British Heart Foundation, 2025), air pollution is estimated to lead to 32,000 deaths per year (COMEAP, 2018), while the road death toll in 2024 was 1,602 (Department for Transport, 2025). The indirect death toll from car dependency therefore offsets its direct impact if only as few as 10% of cardiovascular deaths were prevented by substituting motor for active travel.

[2] 70% reduction for the case of children.

linked to pedestrians' perceptions of road safety, with poorly maintained infrastructure and inadequate lighting contributing to lower perceived safety in traffic environments (Ihssian & Ismail, 2023; Yao, Wang, & Wu, 2025).

The road harm literature is not overly informative in this respect. Government reports such as the European Safety Performance Indicator report often point at higher perceptions of safety in countries and regions with lower collision rates (see also ESRA, 2024), but those findings are not corroborated at the individual level (European Commission, 2024). In the only study - of which we are aware - exploring the association between collisions and perceptions of safety at the individual level, Sanders (2015) found that, for the most part, cyclists involved in collisions do not report lower perceptions of safety, but the opposite is true for 'near misses' (i.e., road traffic incident in which a collision was narrowly avoided). Such findings highlight the complexities behind the shaping of subjective beliefs, and the need for further research. Importantly, Sanders (2015) did not consider the potential bias stemming from confounders of both collisions and perceptions of safety. We, therefore, posit the following research questions:

Do pedestrians and cyclists involved in motor vehicle collisions report meaningfully lower perceptions of road safety than those who have not been involved in a collision? And does this difference increase after adjusting for levels of exposure and perceived vulnerability?

Our first proposition (P1) is that pedestrian and cyclist victims of road collisions will report perceptions of road safety that are not meaningfully different from those who have not been victimised. Informed by the fear of crime literature - see Figure 1 and its discussion in the analytical strategy section - our second proposition (P2) states that once we adjust for differences in vulnerability (age and gender) and exposure to traffic (active travel), the relationship between suffering a collision and perceived safety becomes relevant - if it was not already - and stronger.

Methodology

To minimise researcher bias our hypotheses and analytical strategy have been designed before any of the authors accessed any data from the E-Survey of Road Users' Attitudes (ESRA). This claim will be corroborated by providing a dated data sharing agreement with ESRA coordination team (ESRA@vias.be), which could be checked against date of the registered report in-principle acceptance.

Analytical Sample

We will use the ESRA2 survey (ESRA, 2022a), the latest version of the survey available to external researchers. This is an online panel survey conducted across 48 countries on the topic of road safety. The survey was coordinated by the VIAS institute and carried out by five market research agencies (INFAS, Ipsos, Punto de Fuga, Dynata and TNS Ilres), taking place in December 2018 for 36 of the participating countries and November 2019 for the remaining 16 countries that joined as a part of a second wave. The sample size in each country is 1,000 participants, selected using quota sampling, although it appears that only age and gender population distributions were precisely matched, with geographical distribution only approximated, defined as a 'soft quota' (ESRA, 2022b). We note that such sampling method will therefore be more likely to capture participants who spend more time online, which is likely to be associated with key demographic characteristics like income, education, age and ethnicity (Baker et al., 2010).

For our analysis we will focus on the 24 European countries where the survey was conducted.[3] This is to define a clear target population and because Europe is the only world region that is comprehensively covered in the survey. This provides a sample of 24,000 participants, which is sufficient to detect even relatively small effects with enough precision, as will be shown in subsection 'Analytical Strategy' below.

Theoretical Estimand

Our objective is to define and approximate a causal effect of collision involvement on perceived safety. However, given the cross-sectional and observational nature of the data, identification of this effect relies on strong and untestable assumptions, including conditional ignorability and correct model specification.

[3] These are: Austria, Belgium, Bulgaria, Czech Republic, Denmark, Finland, France, Germany, Greece, Hungary, Iceland, Ireland, Italy, Luxembourg, Netherlands, Norway, Poland, Portugal, Serbia, Slovenia, Spain, Sweden, Switzerland, United Kingdom.

In the absence of a design that approximates random assignment, these assumptions are unlikely to hold fully.

Following the framework proposed by Lundberg et al. (2021), we define our theoretical estimand as the average treatment effect (ATE), that is, the average causal effect of involvement in a traffic collision on perceived safety among active road users (pedestrians and cyclists) in Europe. Formally, this would be defined as the average difference, for each individual, between the level of perceived safety that they would report if involved in a traffic collision and the level they would report if not involved:

$$\text{ATE } = E[Y_i^1 - Y_i^0]$$

where Y_i^1 denotes the potential outcome for individual i under collision involvement, and Y_i^0 denotes the potential outcome for that same individual under no collision involvement. Thus, the estimand would represent the average, across all individuals in the population, of the difference between these two potential outcomes.

While this estimand is causal in nature, it cannot be directly identified in our setting. Accordingly, the quantities we estimate should be interpreted as observed associational difference that approximate to this effect, whose causal interpretation depends on strong and untestable assumptions. Formally this would be:

$$\text{Observed Difference } (\beta_1) = E[Y|A = 1 - Y|A = 0]$$

In the empirical analysis, we approximate this estimand using two specifications. First, we estimate an unadjusted association between collision involvement and perceived safety, which we interpret as a naïve approximation to the ATE. Second, we estimate an adjusted association that conditions on observed characteristics capturing vulnerability and exposure, which we interpret as a confounder-adjusted approximation to the ATE. Comparing these two quantities allows us to assess whether the observed association is consistent with suppression-type confounding, as predicted by the fear-victimisation paradox.

Hypotheses

To empirically test our propositions, we formulate two hypotheses, H1 and H2. The former follows a one-sided equivalence testing logic, while the latter assesses whether the observed association between collision involvement and perceived safety changes after adjustment for vulnerability and exposure.

$H1_0$: Pedestrians and cyclists involved in motor vehicle collisions report meaningfully lower perceptions of road safety than those who have not been involved in a collision, where meaningfully lower is defined as a mean difference of half a point on an 11-point scale.

$$H1_0\text{: Naïve Observed Difference} < -0.5$$

$H1_1$: Pedestrians and cyclists involved in motor vehicle collisions do not report meaningfully lower perceptions of road safety than those who have not been involved in a collision.

$$H1_1\text{: Naïve Observed Difference } \geq -0.5$$

In the absence of prior studies establishing a minimum important difference for road safety perceptions following a collision, the half-point threshold approximates Cohen's (2013) definition of a small effect (0.2 standard deviations). This is based on the assumption that the standard deviation for perceptions of safety is 2.5, the midpoint of the range of plausible standard deviations for a typical 10-point scale Likert, reasonably transferable to our 11-point scale.

$H2_0$: The association between collision involvement and perceived safety does not become stronger in magnitude after adjusting for individual vulnerability and exposure.

$$H2_0\text{: |Adjusted Observed Difference| } \leq \text{|Naïve Observed Differenced|}$$

$H2_1$: The association between collision involvement and perceived safety becomes stronger in magnitude after adjusting for individual vulnerability and exposure.

$$H2_1\text{: |Adjusted Observed Difference|} > \text{|Naïve Observed Differenced)|}$$

Analytical Strategy

For our analysis we will use R (v. 4.5.3). Our outcome variable is perceptions of safety, which we derive from Q16, worded as: *"How safe or unsafe do you feel when using the following transport modes in [country]?"*.[4] As noted, this is measured as an 11-point scale (ranging from 0 *"very unsafe"*, to 10 *"very safe"*). Responses referring to *"walk minimum 100m"* will be used to refer to pedestrians, and those referring to either *"cycle (non-electric)",* or *"cycle on an electric bicycle/e-bike/pedelec", will be used to refer to cyclists*. For respondents indicating involvement with both types of cycling, their perceptions of safety will be calculated as the average across these two items. Although perceived safety is measured on an ordinal scale, we treat it as a continuous variable in our analysis. Given the relatively fine-grained 11-point scale, this approximation is unlikely to materially affect the results. This approach assumes that differences between adjacent points on the scale can be meaningfully interpreted as approximately equal increments, and that intermediate (non-integer) values resulting from model-based predictions are substantively interpretable.

The main predictor of interest is being involved in a road collision, a binary variable that we derive from Q23_1a ("*In the past 12 months, how many times have you personally been involved in road crashes in which you or somebody else had to be taken to the hospital?"*), and from Q23_2a and Q23_3a, which are worded similarly only referring to injury that did not need hospitalisation, and to collisions without injury, respectively. As for the case of the outcome variable, we only consider responses to items referring to pedestrians or cyclists, separately. That information can be retrieved from Q23_1b *("Please indicate the transport modes you were using at the time of these crashes"*).

H1 and H2 will be tested four times each, distinguishing between perceptions from cyclists and pedestrians, and between collisions leading to injury and all types of collisions. This ($2 \cdot 2$) combination of samples referring to different mode of active travel and type of collision is conceived to allow for contextual variability in our conclusion.

H1 will be tested using one-sided equivalence tests (Lakens et al., 2018; Rainey, 2014; Wellek, 2010). To do so we will first estimate our quantity of interest, which for this case will be the difference in perceived safety between those involved and not involved in collisions. To get this quantity we will specify a naïve linear regression model with our outcome and our binary predictor. In a second step, we pre-define our substantive significance threshold as half-point difference in the scale of perceived safety. Third, we pre-define our sampling-adjusted decision bound as -0.6. This is the lower bound of a 95% confidence interval, with standard error for the difference of means equal to 0.05; derived from our sample of 24,000 individuals, after assuming a 10% victimisation rate (similar to the 11% rate detected by Ghalichi et al. (2024) for the same recall period and approximate country coverage), and a 2.5 standard deviation for perceptions of safety (as described in subsection 'Hypotheses', above) for both victims and non-victims. For the analysis of cyclists' perceptions of safety, the sampling adjusted bound becomes -0.7. This reflects an expected smaller sample of 6,480 rather 24,000 individuals, based on the 27% of ESRA2 participants that report cycling at least once a month (ESRA, 2022a). We then proceed to test whether the lower bound of the 95% confidence interval around the observed difference exceeds the sampling-adjusted decision bound. For this analysis we will use *marginaleffects* (Arel-Bundock, Greifer, & Heiss, 2024) and TOSTER (Caldwell. 2022).

To test H2, we estimate the mean difference based on confounded adjusted model. Specifically, we will control for age(Q3a), gender (Q2), and frequency of walking or cycling (Q10: *"During the past 12 months, how often did you use each of the following transport modes in [country]? How often did you…? -Walk minimum 100m (pedestrian; including jogging; inline skate; skateboard, …); -Cycle (non-electric); -Cycle on an electric bicycle/e-bike/pedelec"*), depending on whether the sample under analysis refers to pedestrians or cyclists. Frequency of cycling will be derived from adding responses to both types of categories.

Age is measured in years and will be introduced as a quadratic term to better reflect the expected non-linear effect of age on perceived safety. Gender will be introduced in its original categorical form, with *"female"* used as the reference category, and separate regression coefficients for *"male"* and *"other"*. Frequency of walking and cycling will be introduced as two distinct categorical variables, with never used as the reference

[4] The questionnaire for ESRA2 is available from p.29 to p.35 of the ESRA2 Methodology report: https://www.onisr.securite-routiere.gouv.fr/sites/default/files/2021-11/esra2-methodology-report-updatewave2-def.pdf

category in both, and separate regression coefficients for *"a few days a year"*, *"a few days a month"*, *"1 to 3 days a week"*, and *"at least 4 days a week"*.

To formally test whether the regression coefficient for collision becomes larger in magnitude after controlling for vulnerability and exposure to road traffic, we will use Bayesian statistics. Specifically, we will compare the posterior distributions for the estimates from linear models employing uninformative priors. If the posterior distribution from the model including controls is lower (i.e. victims of collision reporting lower perceptions of safety) than the posterior from the simple model, and the overlap between them is less than 5%, we will reject the null hypothesis and conclude that vulnerability and exposure are consistent with acting as confounders of the observed association between collisions and perceptions of safety. For this analysis we will use the R package *rjags* (Plummer, 2025).

Given the clustered structure of the data, with respondents nested within countries, we account for dependence in observations when estimating regression models. For H2, we use cluster-robust standard errors based on the sandwich estimator, clustering at the country level, as implemented in the R package *sandwich* (Zeileis et al., 2020), specifically using the *vcovCL* function. For the equivalence tests in H1, confidence intervals are derived from analytical standard errors assuming independent observations. While this assumption may lead to slight over-precision, we do not expect clustering to materially affect the results. Evidence from comparable cross-national surveys such as the European Social Survey (2024) suggests that between-country variation in attitudinal measures is typically small relative to individual-level variation. Therefore, any downward bias in standard errors due to clustering is likely to be limited. As shown in Figure 1, we expect demographic factors capturing subjective perceptions of road vulnerability such as age and gender to be associated with both the probability of being involved in a collision and with their own perceptions of safety, and for their respective associations to have the same sign. For example, male cyclists and pedestrians may be more likely to engage in riskier behaviours in the presence of road traffic (Tom & Granié, 2011; Prati et al., 2019) while simultaneously reporting higher levels of perceived safety (Prati et al., 2019). Similarly, older individuals may be less likely to engage in risky behaviours, but also report lower levels of perceived safety (LaGrange & Ferraro, 1989). Regardless of the different signs of these associations (positive for gender and negative for age), the fact that their direction is consistent with both our focal and outcome variables (collision and perception of safety) allows us to anticipate the direction of their confounding bias. Specifically, the expected negative effect of collision on perceptions of safety will be attenuated - potentially flipped into a positive effect - if age and gender are not controlled for.

We also consider the role of exposure to risk, captured through the frequency of active travel. Individuals who travel more frequently are, by definition, more exposed to traffic and therefore more likely to experience a collision. At the same time, repeated exposure may shape perceptions of safety, either by normalising risk or by reinforcing sensitivity to it. These factors create additional pathways through which the observed association between collisions and perceived safety may be attenuated or distorted.

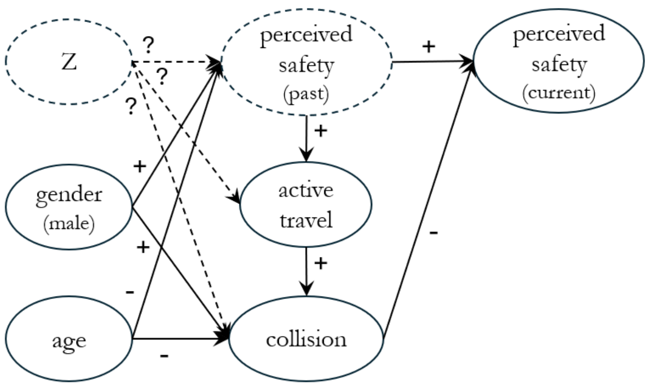

Figure 1. Causal diagram representing the effect of collisions on perceived safety and the confounding paths that could be biasing such effect. As we fail to observe confounding variables such as active travel infrastructure, roads width, volume and speed of traffic, and many more, represented by the node Z and a dashed ellipsis, we cannot identify the causal effect of collision on perceived safety. However, by controlling for active travel and vulnerability factors like age and gender, we can reduce part of the confounding bias affecting naïve associations between collision and current perceptions of safety.

Figure 1 summarises these relationships in a simplified causal diagram. While this representation makes explicit our assumptions about the direction of relationships, it also highlights important limitations. First, the diagram omits unobserved common causes of both collision involvement and perceived safety, which may bias the associations we estimate. These are denoted by the node Z in Figure 1 and could include built environment or traffic features such as access to active travel infrastructure, the width and quality of roads, or speed and volume of traffic. Second, the temporal ordering of key variables cannot be established with the available data, as both collision experience and active travel refer to the same reference period. This opens the possibility of reverse causality, whereby lower perceived safety reduces engagement in active travel and, consequently, the probability of experiencing a collision. Accordingly, our empirical analysis does not aim to identify a causal effect of collision involvement on perceived safety. Instead, we assess whether the observed associations are consistent with the mechanism implied by the fear-victimisation paradox. In particular, we examine whether the association between collision involvement and perceived safety becomes stronger in magnitude after adjusting for vulnerability and exposure, as would be expected if these factors act as suppressors of the relationship.

To complement this analysis, we conduct a sensitivity analysis based on robustness values (Cinelli & Hazlett, 2020). These values quantify the strength that an unobserved confounder would need to have, in terms of its associations with both collision involvement and perceived safety, to explain away the adjusted association observed in H2. Rather than serving as a formal test of our hypotheses, this analysis provides an interpretation aid, allowing readers to assess under what conditions the adjusted association could be plausibly interpreted as reflecting a causal effect. Importantly, this sensitivity analysis also speaks to concerns about reverse causality. Since unobserved factors affecting both active travel and perceived safety may induce bidirectional relationships between these variables and collision involvement, the robustness values can be interpreted as indicating how strong such unobserved influences would need to be to overturn our findings. In this way, the analysis does not resolve issues of identification, but it makes explicit the assumptions that would be required to sustain a causal interpretation of the adjusted association.

The last part of our analysis is entirely exploratory, hence why no further hypotheses are stated. We can anticipate the following additional analyses: i) expand our model using additional variables that we suspect to be acting as predictors of perceptions of safety, such as whether driving a vehicle is required for work reasons (Q5b), or the interviewee's level of education (Q4_1); ii) introduce number of collisions as a continuous variable and a quadratic term to explore its likely non-linear effect; and iii) introduce a random slopes term to explore the level of between-country variability in the relationship between collision and perceptions of safety.

Contribution

This study represents the first analysis of the association of perceptions of safety and road victimisation among pedestrians and, for any form of active travel, the first attempt to approximate the causal effect of collisions on perceptions of road safety. The only evidence derived from individual level data shows that cyclists involved in road collisions with motor vehicles do not report different perceptions of safety than those who have not been involved in collisions (Sanders, 2015). We believe this counterintuitive finding is due to the fear of victimisation paradox, which at its core posits that objective and subjective risk are not necessarily aligned.

Here, we are able to place the focus on two types of factors shown to confound the relationship between objective and subjective risk in the criminology literature: i) vulnerability, measured using individual's age and gender; and ii) exposure, adapted to the road violence context using active travel frequency as a proxy. We posit that after accounting for those factors we will better approximate the effect of road collision on perceptions of safety.

If the expected suppressor effect of vulnerability and exposure is confirmed, this would suggest that previous null findings may be driven by confounding bias rather than by the absence of an underlying relationship between collision involvement and perceived safety. More broadly, such a finding would point to a general mechanism shaping subjective perceptions of risk across different contexts, including both crime and road safety. This, in turn, would open a cross-disciplinary channel of knowledge exchange between research and policy areas that have largely developed in parallel: crime prevention and road safety.

In particular, it would create scope for drawing on insights from the fear of crime literature, as well as from interventions that explicitly target perceptions of safety, such as the UK Reassurance Policing Programme or the current Safe Streets strategy.

From a policy perspective, our findings would highlight the need to move beyond the assumption that improvements in objective safety will automatically translate into increased perceptions of safety and, ultimately, higher levels of active travel. Instead, they would support a more comprehensive approach combining infrastructure-based interventions - such as segregated cycling lanes and safer pedestrian crossings (Kim et al., 2024) - with measures targeting environmental cues associated with perceived insecurity, including visible signs of disorder (Sampson & Raudenbush, 1999). Crucially, such strategies should prioritise groups characterised by higher levels of perceived vulnerability, as improvements in their sense of safety are likely to have the greatest impact on participation in active travel. In this way, the study contributes to reframing road safety policy as not only a matter of reducing collisions, but also of actively shaping perceptions of safety.

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Appendix. Study Design

Question	Hypothesis	Sampling plan	Analysis Plan	Rationale for deciding the sensitivity of the test for confirming or disconfirming the hypothesis	Interpretation given different outcomes	Theory that could be shown wrong by the outcomes
Do pedestrians and cyclists involved in motor vehicle collisions report meaningfully lower perceptions of road safety than those who have not been involved in a collision?	Pedestrians and cyclists involved in motor vehicle collisions do not report meaningfully lower perceptions of road safety than those who have not been involved in a collision.	We will use the ESRA2 survey. This is an online panel survey conducted across 48 countries. The sample size in each country is 1,000 participants, selected using quota sampling, although it appears that only age and gender population distributions were precisely matched, with geographical distribution only approximated, defined as a 'soft quota' (ESRA, 2022b). We will focus on the 24 European countries covered in the survey, which gives us a sample of 24,000 individuals. To explore responses from cyclists we will derive a separate subsample, which will amount to approximately 6,480 individuals.	Difference of means (perceptions of safety for victims minus non-victims) and one-sided equivalence tests (using unadjusted associations). The tests will be repeated four times, distinguishing between perceptions from cyclists and pedestrians, and between collisions leading to injury and all types of collisions. The substantive threshold is established at -0.6 for the two tests based on the whole sample (i.e. testing pedestrians perceptions of safety), and -0.7 when focusing on cyclists. These thresholds were derived after establishing a minimum important difference at -0.5 (see next column) and calculating the standard errors for the difference of means for each of our two samples.	In the absence of prior studies establishing a minimum important difference for road safety perceptions following a collision, the half-point threshold approximates Cohen's (2013) definition of a small effect (0.2 standard deviations). This is based on the assumption that the standard deviation for perceptions of safety is 2.5, the midpoint of the range of plausible standard deviations for a typical 10-point scale Likert, reasonably transferable to our 11-point scale.	i) If the mean difference in perceptions of safety between pedestrians involved in a collision and those who have not been involved is not more negative than -0.6 points (-0.7 for cyclists), we will conclude equivalence between groups. ii) If the mean difference lies between -0.5 and -0.6 (or -0.5 and -0.7 for cyclists), we will conclude that our findings are inconclusive. iii) If the mean difference is more negative than -0.6 (or -0.7 for cyclists), we will accept the alternative hypothesis and conclude that collision involvement is associated with lower perceptions of safety. This association may reflect an underlying causal effect, but it may also be influenced by confounding factors not fully accounted for in the analysis.	If equivalence across groups is confirmed (i), we will have uncovered the first piece of evidence establishing how the fear of victimisation paradox applies to road violence. This opens a cross-disciplinary channel of knowledge exchange between policy and academic disciplines that rarely speak to each other: crime prevention and road safety. If the alternative hypothesis is accepted (iii), we could interpret active travellers reporting substantially lower perceptions of safety after having experienced a collision, as a result of: i) an underlying negative relationship between collision involvement and perceptions of safety being stronger than anticipated; or ii) expected confounders like vulnerability and exposure not exerting the anticipated bias.

Question	Hypothesis	Sampling plan	Analysis Plan	Rationale for deciding the sensitivity of the test for confirming or disconfirming the hypothesis	Interpretation given different outcomes	Theory that could be shown wrong by the outcomes
Does the association between collision involvement and perceived safety become stronger after adjusting for levels of exposure and vulnerability?	The association between collision involvement and perceived safety becomes stronger in magnitude after adjusting for individual vulnerability and exposure.	As above.	Comparison of posterior distributions from two Bayesian linear models using uninformative priors, with perceived safety as outcome variable, whether a victim of a collision as focal variable. One of the models will also control for the individual's age and gender, and frequency of cycling / walking. The model comparison will be carried out four times to distinguish between perceptions from cyclists and pedestrians, and between collisions leading to injury and all types of collisions.	To formally test whether the regression coefficient for collision becomes stronger after controlling for vulnerability and exposure to road traffic, we will use Bayesian statistics. Specifically, we will compare the posterior distributions for the estimates from linear models employing uninformative priors. If the posterior distribution from the model including controls is lower (i.e. victims of collision reporting lower perceptions of safety) than the posterior from the simple model, and the overlap between them is less than 5%, we will reject the null hypothesis and conclude that vulnerability and exposure are consistent with acting as confounders of the observed association between collisions and perceptions of safety.	i) If the posterior distribution for collision coefficient is lower in the model including vulnerability and exposure, and the two posterior distributions overlap by less than 5%, we reject the null hypothesis and interpret this as evidence consistent with suppression-type confounding. Specifically, the comparison of posterior distributions suggests that accounting for vulnerability and exposure strengthens the negative association between collision involvement and perceived safety. This pattern is consistent with the mechanism implied by the fear–victimisation paradox and may help explain the weak or non-existent associations reported in previous studies. ii) If the posterior distributions overlap by more than 5%, we conclude that we cannot establish whether vulnerability and exposure meaningfully affect the association between collisions and perceived safety. iii) If the posterior distributions overlap by less than 5%, but the posterior for the model including vulnerability and exposure is higher than that of the unadjusted model, we conclude that vulnerability and exposure affect the association between collisions and perceived safety, but in a direction opposite to that predicted. This would suggest that the mechanism implied by the fear–victimisation paradox does not hold in this context.	If we reject the null hypothesis, and the posterior is lower after including vulnerability and exposure (i), we will further suggest that the fear-victimisation paradox applies to the context of road safety. Specifically, we will show how failing to control for known predictors of perceptions of safety partially explain the paradoxical small or non-existent association between collision and perceptions of safety. From a policy perspective this will highlight the need to promote factors improving perceptions of safety as an active travel strategy (e.g. tackling car obesity), beyond simply focusing on collisions. Under the opposite scenario (iii) we will conclude that known confounders behind the fear-victimisation paradox, do not seem to apply to the road safety context.