

A New Experimental Method for Estimating Demand for Non-market Goods

With an Application to the Value of a Statistical Life*

Grady Killeen

University of California, Berkeley

January 21, 2026

Abstract

Economists often study non-market goods such as health and air quality. This paper introduces a new method to estimate demand for such amenities and applies it to measure the value of a statistical life (VSL) in Kenya. My approach is to update beliefs about the life-saving efficacy of a product (a motorcycle helmet) and elicit product choice. This generates instruments allowing one to use subjective beliefs to estimate demand, rather than assuming rational expectations. This method does not require beliefs to be reported error-free but does require classical mismeasurement. I validate this assumption using features of the experimental design. The estimated VSL is \$224, near the left tail of Kenyan estimates. Standard methods for estimating VSL produce skewed results, driven by severe violations of rational expectations. These findings help explain low observed demand for many health products and suggest that directing more development aid towards consumption may increase welfare.

Keywords: Non-market amenities, value of a statistical life, rational expectations

*I thank Edward Miguel and Supreet Kaur for excellent feedback and advising. This project benefited from suggestions by Michael Greenstone, Michael Kremer, Christopher Snyder, Paul Niehaus, Patrick Kline, Kurt Lavetti, and Kelsey Jack. I am grateful to the UC Berkeley development and industrial organization communities and to seminar and conference participants for valuable feedback. I thank William Jack, Whitney Tate, Nyambaga Muyesu, Josephine Okello and the Georgetown University Initiative on Innovation, Development, and Evaluation for support with implementation. I gratefully acknowledge funding from the Center for Effective Global Action. This material is based upon work supported by the National Science Foundation Graduate Research Fellowship under Grant No. DGE 2146752. This study received IRB approval from Amref Health Africa (protocol P1306-2022) and the University of California, Berkeley (protocol 2022-07-15518). AEA Trial Registry RCT ID: AEARCTR-0010101 (Killeen, 2025). Email: gkilleen@berkeley.edu

1 Introduction

Many goods that are important to welfare – such as clean air and water, health, and reputation – are not directly traded. Pricing these amenities is important to design public policies, but the absence of markets makes it difficult to identify consumer preferences. This paper introduces a new experimental method to estimate demand for non-market goods by revealed preference and applies it to estimate consumers’ willingness to pay to reduce mortality risk. My approach draws on the fact that even though these amenities are not traded, products often change exposure to or consumption of such goods (e.g. air purifiers).

Attempting to exogenously vary exposure to non-market goods is often infeasible. Rather, my approach shifts beliefs about the extent to which a given consumer product affects exposure to an amenity. Demand for the consumer product then enables an inference about how the amenity affects consumer utility. I estimate demand for the non-market good by examining how willingness to pay for the product changes with perceived exposure to the amenity, using the randomized provision of information to instrument for subjective beliefs. This method does not assume rational expectations, a common requirement of traditional approaches. It also does not require that agents fully update beliefs in response to information, or that individuals report their subjective beliefs without error. The primary identifying assumption is that measurement error in reported expectations (the gap between an agent’s measured and true subjective belief) is classical.

I design two tests to validate this assumption. One leverages multiple information treatments of varying intensity to verify that estimates scale with changes in beliefs, helping rule out features like experimenter demand effects. The second adapts insights from the local average treatment effects literature to construct estimates that weight observations differently as a function of priors. This identifies bias driven by common patterns of belief misreporting such as rounding or overstating low probabilities. These tests are useful in their own right since they generalize to other settings where researchers rely on data from elicited beliefs.

I apply this method to study demand for mortality risk reduction, the value of a statistical life (VSL), in Kenya. VSL is important for public policy but challenging to estimate. Mortality risk is a feature of economic decisions including healthcare, environmental regulation and transportation. Policymakers directly use VSL in benefit-cost analyses, and NGOs rely on the parameter to allocate

development aid.¹ In theory, VSL estimates allow practitioners to use consumers' own preferences for trading off mortality risk and consumption to guide policy, maximizing welfare and avoiding paternalism. But economists have raised concerns that methods typically used to estimate the parameter may be prone to bias, causing misallocation (Ashenfelter, 2006).

VSL is often estimated by taking decisions where mortality risk is salient, then estimating demand models where the level of mortality risk present in data proxies for beliefs. This approach is biased if agents have beliefs about risk that differ from statistical estimates (i.e. their beliefs violate rational expectations). Moreover, estimates may be prone to omitted variable bias if mortality risk is correlated with other determinants of utility (Ashenfelter and Greenstone, 2004). Many studies also estimate VSL over highly selected populations where there is quasi-random variation in risk. Consistent with potential bias, estimates of VSL vary by orders of magnitude: in Africa, they range from \$0-\$700,000 (Berry et al., 2020; León and Miguel, 2017).²

In this paper, I estimate the VSL of urban Kenyans by presenting motorcycle taxi passengers with randomly assigned information about the efficacy of helmets at preventing death. I then offer the choice between a helmet or cash. The approach does not rely on rational expectations, and the random assignment of information ensures that selection into risk does not bias estimates. Respondents were assigned to an information control or one of two treatments. One treatment arm was presented with estimates of unhelmeted mortality risk and the results of Liu et al. (2008), which estimates that helmets reduce one's risk of dying by 42%. The second received the same risk information, but a 70% helmet effectiveness estimate (Ouellet and Kasantikul, 2006), which differs because experts are unsure how well helmets work. I then elicited respondents' beliefs about the likelihood that a helmet would save their life and estimated helmet demand using a Becker et al. (BDM, 1964) mechanism, inverted to offer a free helmet or cash so that demand is not limited by cash on hand. I estimate VSL by fitting a regression of willingness to pay on the probability that a helmet will save the agent's life, using treatment assignment to instrument for beliefs.

I study the Kenyan motorcycle taxi market because helmet use is low, enabling VSL estimation over a minimally selected population. Traffic accidents are a leading cause of death in Kenya and

¹For instance, GiveWell uses “moral weights” when ranking charities, which are derived from VSL as discussed in section 5.1. GiveWell directed more than \$500 million to charities in 2021.

²Stated preference VSL elicitation does not assume rational expectations, but agents have no incentivize to answer truthfully and estimates may be prone to desirability bias, so I focus on revealed preference methods.

motorcycle taxi use is widespread. Yet only 3% of passengers wore helmets near the time of the study (Bachani et al., 2017). Since fatality risk rises with trip volume, the setting also facilitates comparison with traditional VSL estimation approaches.

In the first main finding of the study, I estimate that Kenyans have low demand for marginal reductions in mortality risk, with a VSL of PPP USD \$224 (5% of annual income).³ Receipt of the 42% helmet effectiveness estimate lowers average beliefs about the likelihood that a helmet will save one's life (a product of accident risk and helmet efficacy) from 2.22% to 1.41%, reducing demand for a helmet by \$3. This approach recovers informative bounds on VSL, rejecting values below \$34 and above \$429 with 95% confidence.

The low VSL estimates are consistent with economic theory. VSL is inversely proportional to the marginal utility of consumption and should rise with income as the opportunity cost of funds falls. The gap between this estimate and a \$700,000 VSL (León and Miguel, 2017), among a sample with incomes near \$75,000, can be fully explained by typical macroeconomic estimates of utility curvature with wealth (Havránek, 2015).⁴ This aligns with evidence that health spending, as a share of consumption, rises sharply with income (Hall and Jones, 2007). I also estimate heterogeneity that matches theory: VSL is \$500 higher among wealthier individuals in the sample, with an estimated income elasticity of 1.14, and higher among healthier respondents.

Is it plausible that the VSL in this sample is below annual income? This finding aligns with evidence of low demand for preventative health in LMICs. For instance, Cohen and Dupas (2010) find that only 40% of pregnant women purchase malaria nets at a price of \$0.6, implying a cost per life saved under \$200 (Pryce et al., 2018). Similarly, Banerjee et al. (2010) show that \$4 in-kind incentives substantially increase childhood vaccination. Low demand could reflect decision-making errors or binding constraints. But adoption has often remained low when such frictions are targeted (Dupas and Miguel, 2017). The low estimates may be driven in part by cultural factors. VSL is sensitive to religious views, with evidence showing the parameter is substantially lower in samples with more religious and fatalistic beliefs (Colmer, 2020; de Lima, 2020). Kenya is highly religious, with one of the highest rates of belief in an afterlife globally (Evans et al., 2025).

Although the VSL estimates in this paper are consistent with low demand for preventative

³I use the World Bank's 2021 PPP conversion factor to convert to USD (43.8).

⁴I selected this paper because it considers a population in Africa, making it comparable on dimensions other than income. VSL estimates from the United States are higher still.

health and theoretically admissible, it is possible that they understate demand for health. One possibility is that behavioral biases depress investment in preventative health, biasing revealed-preference VSL estimates. Another is that poor consumers may place relatively greater value on protection from non-fatal health shocks, given limited financial capacity to cope with illness or disability. VSL captures preferences over mortality risk only, so would not reflect this channel.

This paper is an important step forward even if such hypotheses hold. The results suggest that neither rational expectations violations nor omitted variable bias—two leading concerns with revealed-preference approaches—can explain the low VSLs observed in poor populations. And by validating an experimental method to estimate the parameter, this paper opens the door to design experiments to test such hypotheses, which is typically infeasible with observational data.

The second component of this study validates that beliefs are measured without non-classical error. Two leading threats are experimenter demand effects and systematic misreporting. Since the concern is a gap between reported and decision-relevant subjective beliefs (I allow for arbitrary deviations from the empirical truth), tools like bunching tests are uninformative. But two design features help test for bias. First, the two treatments provide signals of varying intensity. If beliefs have classical error, VSL estimates should be unchanged if one arm is excluded. Otherwise one would expect values to differ since posteriors are centered at different points across treatments. Using any combination of arms to instrument for beliefs yields similar estimates. Helmet demand also falls in response to the low signal, providing revealed preference evidence of belief updating.

Another design feature that allows for tests of misreported beliefs draws on the logic of local average treatment effects. Instrumenting for posteriors using treatment assignment weights observations linearly in priors, but one may construct instruments that weight observations by priors squared, yielding similar results. The stability of estimates suggests findings are not substantially affected by misreporting correlated with true beliefs, which includes most cases where agents systematically under or over-report probabilities (e.g. rounding or difficulty with small probabilities).

Additional checks further support the validity of the results. First, I proxy for perceived mortality risk using respondents' estimates of deaths per 10,000 motorcycle riders over a helmet's lifespan. This approximates the elicited probability used in the main estimates but avoids low probability reports. VSL estimates are similar, suggesting challenges articulating rare events do not drive results. VSL also does not vary significantly with education or digit span recall. Second,

helmet demand is similar between the control and a pure control not asked about risk, and instrumenting for beliefs using an observational shifter (knowing an accident victim) yields similar VSL estimates, inconsistent with Hawthorne effects. Finally, the BDM mechanism was implemented in a high-stakes, realistic setting where evidence supports its reliability (Berry et al., 2020).

The third part of the paper tests if rational expectations, a core assumption in most VSL estimation methods, holds in this context. The data suggest systematic departures from rational expectations because agents learn from their experiences and those of their social network, not from representative data. Those that know someone involved in a motorcycle accident perceive higher 5-year fatal accident risk, but those that ride motorcycles frequently, making them mechanically more likely to die on one, do not. Respondents also reported twice the risk if it rained the day of the survey, raising the salience of risk. Such deviations from rational expectations are likely to bias VSL estimates from typical approaches. Consistent with this prediction, two common methods produce results outside of confidence sets obtained from the new approach.

These results have important policy implications. The VSL estimates are similar to the lower range of estimates from LMICs (Kremer et al., 2011), but they are much smaller than the values used in 5 recent benefit-cost analyses of Kenyan programs. In 4 out of 5 cases, they cause benefit-cost ratios to fall below 1. In addition, an NGO that directs USD billions of aid allocates funds based on a VSL over 100 times as high. This suggests that practitioners may underweight recipient consumption, possibly leading to a misallocation of foreign aid.

The findings should not be interpreted as a call to eliminate health funding for LMICs. The low VSL highlights that the parameter only reflects preferences for marginal mortality risk reductions versus consumption. For many policies (e.g. WHO funding allocation), decisions are over whom to distribute resources to, and the consumption of recipients is unaffected. In these cases, consumers do not face health-consumption tradeoffs, so applying VSL would undervalue welfare gains of poor beneficiaries. VSL also should not be used to value medical treatments (e.g. antiretrovirals) that produce large reductions in mortality risk. The transfers needed to compensate individuals for large risks would lower their marginal utility of consumption, changing demand for safety when risks are non-marginal. Finally, many health interventions yield additional benefits, such as lower exposure to non-fatal morbidities or positive externalities, that are not captured by VSL.

This paper is among the first to estimate VSL using subjective beliefs and with methods that

are robust to omitted variable bias.⁵ This addresses two leading concerns with traditional methods. The results also provide one of the first tests of rational expectations in the VSL literature. This builds on Ashenfelter and Greenstone (2004) and Greenberg et al. (2021) which study endogeneity and heterogeneity in VSL. It also relates to Baylis et al. (2023) which study rational expectations in the setting of air quality. The VSL estimates are also independently important because knowledge of demand for safety in LMICs is limited (Greenstone and Jack, 2015).⁶ I produce one of the first urban estimates using a design that ensures results are not determined by cash on hand, which has caused some to question revealed preference estimates in poor settings (Redfern et al., 2019).

This method could be applied to value other non-market amenities. Goods such as time and environmental quality are economically important but difficult to value (Campbell and Brown, 2003; Greenstone and Jack, 2015). Policy decisions often rely on estimates that are not incentivized (e.g. contingent valuation surveys) when there is insufficient variation for revealed preference measures (Mendelsohn and Olmstead, 2009). Methods also tend to assume rational expectations, which contradicts recent evidence (Baylis et al., 2023). The framework I introduce could potentially estimate demand for such amenities using products such as ride sharing services or air purifiers.

This research also contributes to a literature on eliciting beliefs for use in economic models (e.g. Manski, 2004; Wiswall and Zafar, 2018). I show how one can design information treatments that produce multiple instruments for beliefs which mitigate measurement error and facilitate tests for bias. Practically, the survey instrument was also effective at measuring expectations. These tools are important for applied work since noisy elicitation has prevented the use of subjective belief data even when authors note concerns with assuming rational expectations (León and Miguel, 2017).

2 Study design and context

2.1 Motorcycle taxis and helmet use in Kenya

This study considers a sample of motorcycle taxi passengers in Nairobi, Kenya. This setting has three features which are ideal to study demand for mortality risk reduction. First, motorcycle taxis are near universally used and it is rare for passengers to wear helmets, so VSL may be estimated

⁵Shrestha (2020) attempts to estimate VSL using similar methods, but confidence sets are uninformative.

⁶Kremer et al. (2011) and Berry et al. (2020) study the VSL of rural children, and León and Miguel (2017) and Ito and Zhang (2020) examine relatively wealthy populations.

over an informative sample. Second, motorcycle helmets substantially reduce one's risk of death, allowing one to estimate demand for a substantive safety improvement. Third, empirical mortality risk varies across the sample since it is a function of ridership. This facilitates comparisons to typical methods for estimating VSL.

Motorcycle taxi use is widespread and growing in Africa, attributed to low costs. In Kenya, there are an estimated 2.4 million drivers providing taxi services, combining for 22 million trips per day.⁷ Transportation is dangerous. Data from the National Transport and Safety Authority (NTSA) reports 1,722 motorcycle deaths in 2021, up from 715 in 2017. Traffic accidents are the leading cause of death among boys 15-19 in Kenya, and a top-five cause of death for all Kenyans.⁸

Despite the high risks, helmet use among taxi passengers is rare. Bachani et al. (2017) measure passenger helmet use at 3%. Helmeted passengers typically borrowed one from the driver, which anecdotally became uncommon after the COVID-19 pandemic due to hygiene concerns, further reducing helmet use. Low helmet adoption suggests that demand may be low. However, safe helmets have only recently become available at an affordable price. The *FIA Foundation* began offering helmets in Kenya in 2021, about a year before the experiment, and a local producer began manufacturing helmets at the same time. Retail diffusion of both products was limited, and many consumers reported that they were unaware they could purchase a helmet without a motorcycle.

Do consumers with low safety demand select into riding motorcycles? This setting was chosen because most Kenyans use motorcycle taxis, so selection is limited at the extensive margin. They are used similarly to traditional taxis in high-income countries. It is common to take one on trips where public transit is unavailable or when running late, but less common for daily commutes, which are more often taken on buses in Nairobi. Transportation surveys tend to focus on commuting, and I am not aware of representative data capturing the share of consumers that use motorcycle taxis. The Kenyan field team estimated that at least 85% of urban Kenyan adults use motorcycle taxis. Back of the envelope calculations also suggest that ridership needs to be high among urban adults to rationalize ridership.⁹ Selection may be limited because alternative modes of public transportation are dangerous, crowded, and uncomfortable.

⁷Fred Matiang'i, "The urgency of bodaboda reforms", *Nation.Africa*, 2022.

⁸"New initiative to tackle road crash deaths in Kenya," *World Health Organization*.

⁹There were 22 million trips per day in 2022, and a population of 53 million. 40% of Kenyans are farmers and at least 10% are 5 or younger. Respondents report taking 7.5 trips/week on average. This suggests 80% of urban Kenyans adults ride motorcycle taxis ($22 \approx 53 \cdot 0.5 \cdot \frac{7.5}{7} \cdot 0.8$).

Ridership volume at the intensive margin may be more selected, for instance if richer consumers used motorcycles more often. A strength of this setting is that ridership volume is observed, allowing one to re-weight estimates to limit any effect of ridership on sample composition. Section 5.1 shows that doing so inconsequentially affects results, empirically suggesting that ridership selection does not meaningfully affect VSL estimates in this context.

2.2 Recruitment

This study recruited consumers from motorcycle taxi stands in Nairobi during two waves of data collection. Surveyors censused 188 taxi stands and conducted surveys at 97. The stands were selected for broad geographic and demographic coverage. However, areas with high crime were excluded for safety. Survey locations are plotted on a map of Nairobi in Appendix Figure A1.

The study leveraged arrival times of consumers to sample from the population of passengers at each location. Surveyors attempted to recruit the first individual to arrive at a stand after completing a survey. Those that did not report regular access to a motorcycle helmet were informed that they could choose a free helmet or a cash gift if they completed a 15-30 minute survey. The high value of the gifts (about \$15 on average) relative to survey time yielded a high response rate. Over 90% of passengers agreed to take part in the survey. The majority of those that did not lack time.¹⁰

Demographic information presented in Table 1 shows that the study reached a broad sample, suggesting that the results are informative when considering East African samples. Income aligns with representative samples of the population. The mean annual income is USD PPP \$6,730 with a median of \$4,762. The World Bank reported a GDP/capita of \$5,211 for Kenya in 2021, and the Kenyan National Bureau of Statistics reported gross per capita production of \$7,907 for Nairobi county in 2017.¹¹ However, the sample is not perfectly representative. More males (981) than females (444) were surveyed, likely because men spend more time away from home in urban Kenya. Education is relatively high, with an average of about 12 years of schooling completed.

¹⁰Surveyors reported recruitment rates and the share of those screened out due to helmet access to the field manager daily to verify that helmet access was rare. The field manager reported that under 10% of those screened out reported helmet access, less than 1% of approached passengers.

¹¹Source: 2019 Gross County Product Report and 2017 World Bank PPP conversion rate.

2.3 Information treatment: Motorcycle fatality risks and helmet effectiveness

This study implemented a randomized information treatment to produce variation in beliefs to estimate VSL. There are four arms: a pure control, control, and two treatments. The treatments were presented with information about the mortality risk of motorcycles and the life-saving effectiveness of helmets, while the control arms received no such information.

The pure control and control vary in the questions that they were asked. The pure control was not asked about motorcycle safety. In contrast, the control was asked questions about the risks of motorcycles and their beliefs about the effectiveness of helmets. The pure control was included to test if asking respondents these questions affected demand. These observations are excluded from VSL estimates since beliefs were not elicited. In practice, and reassuringly, there are no differences in demand between the pure control and control.

The treatments presented different studies of helmet effectiveness. Respondents first received an estimate of their unhelmeted mortality risk over the 5 year lifespan of a helmet.¹² They were then presented with one of two studies. Those in a “low treatment” arm were presented the results of Liu et al. (2008) which estimates, via a meta-analysis, that helmets reduce one’s risk of dying by 42%. Respondents assigned to a “high treatment” arm were presented with an estimate from Ouellet and Kasantikul (2006) that helmets reduce fatality risk by about 70% in Thailand. Surveyors followed standard scripts, so only the study and results vary across arms. The sources of information and the fact that the studies were conducted outside of Kenya were disclosed.

Both studies of helmet efficacy are from peer reviewed publications. There is a consensus that motorcycle helmets are effective, but there is uncertainty about exactly how well they work, particularly across contexts. The 42% figure presented in Liu et al. (2008) is often presented by the UN and NGOs operating in Kenya, motivating its use in this study. But their meta analysis primarily considers studies of high-income settings where driving speeds and helmet quality differ. Ouellet and Kasantikul (2006) present estimates from Thailand, which may be more similar in driving and helmet environment. It is unclear which estimate is more representative of the efficacy of helmets

¹²I calculated per trip risk for the average Kenyan from NTSA data, then estimated 5 year risks based on the respondent’s ridership. Mortality rates were obtained by dividing deaths the year before the study across Kenya by 22 million (trips per day in Kenya) times 365. I combined the per trip estimate with the respondent’s 5 year ridership, obtained by scaling trips/week.

in Kenya, meaning neither arm was given any information that I believed was inaccurate.¹³

2.4 Helmet valuations

The study measured demand for a helmet at the end of the survey using a Becker et al. (1964) willingness to pay mechanism. Respondents were asked the smallest cash payment that they would prefer to a free helmet. Surveyors then revealed a randomly selected payment amount between 5 and 600 Kenyan shillings (Ksh). If the payment amount was greater than or equal to the respondent's bid, then they received the cash. Otherwise they were given the free helmet. The study offered a choice over a helmet versus cash to ensure that cash on hand did not limit demand. The mechanism elicits willingness to pay rather than accept since respondents were not asked to give up a helmet they already owned. This aligns with the model and with standard VSL applications, which measure equivalent variation for proposed policies rather than compensating variation for losses. While preferences over existing health goods may differ due to endowment effects, most benefit-cost analyses use VSL in settings that closely match this experimental structure.

The maximum draw was based on assessments by an NGO and the helmet manufacturer that most valuations would fall below Ksh 600. Ethical concerns were also raised that if draws significantly exceeded market prices, consumers may infer that helmets were expensive which could harm their diffusion. The manufacturer sold the helmets used in this study at a price of Ksh 580, 15% of weekly median wages. A limitation of the willingness to pay mechanism is that one's true valuation is not the unique weakly dominant bid if an agent knows their valuation exceeds the maximum draw. To hedge against the risk of setting the maximum too low, and to avoid anchoring, the BDM script did not mention the upper bound or helmet value. In principle, one could infer the maximum draw from the consent form, which disclosed possible gifts but did not tie them to the game. The consent was presented well before the BDM mechanism, making it unlikely that information would be recalled. In piloting, surveyors verified that respondents could not state the value. The maximum was revealed when requested, which happened once.

In practice, helmet valuations often exceeded the wholesale price.¹⁴ Appendix Figure A2

¹³An ethics appendix (Appendix D) details steps taken by the study to ensure that no respondents were harmed.

¹⁴High demand is reconciled with low VSL because consumers helmets for reasons other than their life saving potential. Respondents frequently cited protection from non-fatal injuries, resulting in high hospital bills and missed wages, as a leading attribute during piloting.

demonstrates that there is no unusual behavior in bids near Ksh 600. In addition, agents in the low treatment arm are less likely to receive a helmet ($p < 0.1$, $p < .05$ if baseline risk is above 1 in 100,000), validating that valuations affect real outcomes. I also show that results are robust to using an IV probit estimator that is not influenced by variation in valuations above the maximum draw. Thus the mechanism appears to accurately capture willingness to pay.

2.5 Randomization

Respondents were assigned to the information arms using a random number drawn in SurveyCTO. In wave one, respondents were assigned to the pure control with probability 0.1 and each of the other groups with probability 0.3. In the second wave, the pure control was eliminated (since demand was not significantly different across the control and pure control in wave 1) and respondents were assigned to the remaining arms with equal likelihood.

3 Model and identification

This section presents a simple model of demand for helmets and illustrates the method used for estimating demand for non-market goods, showing how it identifies VSL. Furthermore, the model illustrates the assumptions needed to identify VSL under typical approaches assuming rational expectations and demonstrates that these estimates are unbiased only if all individuals have mortality beliefs equal to the econometrician's estimate of risk.

Consider a population of individuals indexed by $i \in \{1, \dots, \infty\}$. Individuals maximize expected utility and Bayesian update their beliefs about motorcycle risks and helmet effectiveness. Belief formation is modeled to illustrate how a common learning model can generate bias when beliefs are proxied by empirical risk. Identification requires a weaker condition that the information presented changes expectations, which I test empirically.

Each consumer has a prior about the probability of dying while wearing a helmet in a motorcycle accident that would be fatal without a helmet given by

$$Pr(D = 1 | \mathcal{I}_i) \sim Beta(a_{iH}, b_{iH})$$

where \mathcal{I}_i denotes the individual's information set and D is a Bernoulli random variable equal to 1 if the agent dies. Beliefs are over the unknown parameter of this distribution. There is true stochastic variation in whether agents in an accident die, but agents must learn the parameter governing this process. The Beta distribution is natural in this setting because it is a conjugate prior for Bernoulli trials. a_{iH} may be interpreted as the expected number of fatalities and b_{iH} the number of survivals out of $a_{iH} + b_{iH}$ accidents. Define

$$H_{i0} \equiv \mathbb{E}[Pr(D = 1|\mathcal{I}_i)] = \frac{a_{iH}}{a_{iH} + b_{iH}}$$

Suppose that the consumer receives a signal that the estimated efficacy of helmets is $\theta_H \sim Binomial(a_{EH} + b_{EH}, a_{EH}/(a_{EH} + b_{EH}))$. The Binomial likelihood captures the fact that studies of helmet efficacy report the number of deaths given a number of accidents. a_{EH} represents the number of fatalities and b_{EH} the number of survivals out of $a_{EH} + b_{EH}$ empirically recorded accidents. The agent's posteriors about the efficacy of helmets are

$$Pr(D = 1|\mathcal{I}_i, \theta_H) \sim Beta(a_{iH} + a_{EH}, b_{iH} + b_{EH})$$

with expected value

$$H_{i1} \equiv \mathbb{E}[Pr(D = 1|\mathcal{I}_i, \theta)] = \frac{a_{iH} + a_{EH}}{a_{iH} + a_{EH} + b_{iH} + b_{EH}}$$

If $\frac{a_{iH}}{a_{iH} + b_{iH}} \neq \frac{a_{EH}}{a_{EH} + b_{EH}}$, the empirical mortality rate differs from the agent's prior expectation, so the consumer's posterior mean will differ from their prior ($H_{i0} \neq H_{i1}$). The degree to which beliefs update depends on bias in priors and the precision of beliefs. If agents learn through their experiences or their social network then beliefs will likely deviate from empirical estimates, violating rational expectations. Learning from one's experiences is subject to survivor bias. Social networks information is prone to small sample biases. In addition, a higher likelihood of learning about fatal trips would cause agents to overestimate risk.

The likelihood that a helmet saves one's life is a product of the probability of an accident and the effectiveness of a helmet given an accident. Therefore, suppose that the agent has a prior about

the per trip risk of suffering an accident that would be fatal to an unhelmeted passenger given by

$$Pr(A = 1|\mathcal{I}_i) \sim Beta(a_{iA}, b_{iA})$$

where A is a Bernoulli random variable equal to 1 if a trip ends in an accident. Beliefs are again over the unknown parameter of this Bernoulli distribution.

Suppose the consumer completes n_i motorcycle rides over the lifespan of a helmet. An unhelmeted agent's expectation of getting into a fatal accident over these trips is given by

$$r_{iu} = 1 - \int_{Pr(A=1|\mathcal{I}_i)=0}^1 [1 - Pr(A = 1|\mathcal{I}_i)]^{n_i} dPr(A = 1|\mathcal{I}_i) \quad (1)$$

$[1 - Pr(A = 1|\mathcal{I}_i)]^{n_i}$ is the probability of surviving n_i trips given that the probability of suffering a fatal accident is $Pr(A = 1|\mathcal{I}_i)$ on any given trip. Since priors about $Pr(A = 1|\mathcal{I}_i)$ are non-degenerate, one must integrate over beliefs to obtain equation 1.

I assume that helmeted individuals involved in an accident that would otherwise be fatal are deterred from continuing to use motorcycles.¹⁵ Let z_i indicate whether the agent is exposed to the signal of helmet effectiveness θ . Their subjective probability of suffering a fatal motorcycle accident with a helmet is

$$r_{ih}(z_i) = \begin{cases} H_{i0} \cdot r_{iu}, & z_i = 0 \\ H_{i1} \cdot r_{iu}, & z_i = 1 \end{cases} \quad (2)$$

which is the unhelmeted risk times the perceived chance that a helmet fails to prevent death.¹⁶ The subscript u refers to the agent's perceived unhelmeted risk and h to their helmeted risk.

Let p_i be the price of a helmet. The present value of utility from being alive is given by $u_a(x_i)$ where x_i is a vector of characteristics, such as income, health, and age. Denote flow utility of consumption by $u(c_i; x_i)$ and the expected utility from not being alive (which may be positive, e.g. if one believes in an afterlife) by $u_d(x_i)$. The agent's expected utility from purchasing a helmet is

$$U_{ih}(z_i) = \zeta_h + [1 - r_{ih}(z_i)] \cdot u_a(x_i) - p_i \cdot u'(c_i; x_i) + r_{ih}(z_i) \cdot u_d(x_i) + \epsilon_{ih} \quad (3)$$

¹⁵Absent this assumption, $r_{ih0} = \frac{H_{i0} \cdot r_{iu}}{1 - H_{i0} \cdot r_{iu}}$. Results are similar since suffering two accidents is unlikely.

¹⁶Agents may increase ridership if they perceive a helmet is more effective. Empirically accounting for a ridership response has little effect on estimates, so I omit this from the model for simplicity.

ζ_h captures average utility from characteristics of helmets other than mortality risk reduction, such as injury protection, comfort, and cultural stigmas that may limit helmet usage in some settings. ϵ_{ih} denotes idiosyncratic variation in utility. Without purchasing a helmet, expected utility is

$$U_{iu} = (1 - r_{iu}) \cdot u_a(x_i) + r_{iu} \cdot u_d(x_i) + \epsilon_{iu}$$

Setting $\epsilon_i = \epsilon_{ih} - \epsilon_{iu}$ and $\Delta r_i(z_i) = r_{iu} - r_{ih}(z_i)$,

$$U_{i,h-u}(z_i) \equiv U_{ih}(z_i) - U_{iu} = \zeta_h + \Delta r_i(z_i) \cdot [u_a(x_i) - u_d(x_i)] - p_i \cdot u'(c_i; x_i) + \epsilon_i$$

In words, one's expected utility from a helmet is a function of non-safety preferences for helmets, the agent's *belief* about the probability that the helmet will save their life, $\Delta r_i(z_i)$, times differences in expected utility from surviving versus not, less the price of a helmet times the marginal utility of consumption. The goal is to identify VSL, the change in income needed to compensate an agent for a change in mortality risk. Totally differentiating,

$$dU_{i,h-u} = \frac{\partial U_{i,h-u}}{\partial \Delta r_i} d\Delta r_i - \frac{\partial U_{i,h-u}}{\partial p_i} dp_i$$

Setting $dU_{i,h-u} = 0$,

$$VSL_i \equiv \frac{dp_i}{d\Delta r_i} = \left(\frac{\partial U_{i,h-u}}{\partial \Delta r_i} \right) \Bigg/ \left(\frac{\partial U_{i,h-u}}{\partial p_i} \right) = \frac{u_a(x_i) - u_d(x_i)}{u'(c_i; x_i)} \quad (4)$$

This predicts that VSL will increase with income. Theory suggests that $u_a(x_i)$ will increase and $u'(c_i; x_i)$ will fall as agents become richer. $u'(c_i; x_i)$ is likely to be particularly important as estimates often indicate it varies non-linearly with income (Havr醕ek, 2015). This reflects a lower opportunity cost of funds, so VSL will be higher even if agents obtain no more utility from averting a death. Expected utility from not being alive may also vary substantially across contexts, especially with beliefs about an afterlife which may raise $u_d(x_i)$ substantially above 0.

3.1 Identification under rational expectations

For simplicity, suppose that $\beta = u_a(x_i) - u_d(x_i)$ and $\alpha = u'(c_i; x_i)$ are homogeneous across agents. Let Δr_i^* be a statistical estimate of the likelihood a helmet will save one's life. A common

set of identifying assumptions in the VSL literature (but not this paper) is

$$\text{Full Information Rational Expectations: } \Delta r_i = \Delta r_i^* \quad \forall i \quad (5a)$$

$$\text{Exogeneity and Logit Errors: } \epsilon_{ih}, \epsilon_{iu} \sim_{iid} EV1 \quad (5b)$$

Given data denoting if agents purchased helmets y_i , empirical risks Δr_i^* and prices p_i ,

$$Pr(y_i = 1) = \Lambda(\zeta_h + \beta \cdot \Delta r_i^* - \alpha \cdot p_i) \quad (6)$$

Λ is the Logistic CDF and so α, β and thus VSL, which is $\frac{\beta}{\alpha}$, are identified.¹⁷

Assumption 5a is violated if $Pr(\Delta r_i \neq \Delta r_i^*) > 0$, meaning agents hold beliefs about risk that diverge from statistical estimates, even if they match in expectation (mean-zero differences would produce attenuation bias). This is plausible if consumers have the same information as researchers, but fails if they possess private information or lack access to the data used by researchers. Rational expectations has been assumed despite these documented limitations because of a lack of an incentive-compatible alternative (prior to this paper) capable of producing informative estimates.¹⁸

Assumption 5b is violated if mortality risk is endogenously related to unobserved determinants of utility. For example, frequent motorcycle riders face higher risk but may also experience greater disutility from helmet discomfort. Some studies use instruments to relax this assumption but still require rational expectations (e.g. Ito and Zhang, 2020).

3.2 Identification using subjective belief data

This study estimates VSL without assuming rational expectations or exogenous mortality risk by using an instrument that shifts agents' subjective beliefs. The model shows that this is an appropriate way to identify VSL since presenting agents with information about helmet efficacy will change the expected utility they obtain from the life saving potential of a helmet (unless their priors exactly align with the signal or are degenerate). Updating beliefs about helmet efficacy produces

¹⁷In practice studies often estimate mixed logit models, but the assumptions are similar.

¹⁸For instance, León and Miguel (2017) note that “we follow the existing literature and utilize a standard expected utility individual choice framework, using accident risk from historical data, in part due to the absence of a well-articulated and widely accepted alternative analytical approach that incorporates these behavioral concerns and generates meaningful valuation estimates.”

the same identifying variation that exogenous differences in mortality risk would under rational expectations, but this approach is robust to biased beliefs and endogeneity. Formally, assume

$$\text{Exclusion Restriction: } \text{Cov}(z_i, \epsilon_i) = 0 \quad (7a)$$

$$\text{Instrument Relevance: } \text{Cov}(\Delta r_i, z_i) \neq 0 \quad (7b)$$

Letting v_i denote willingness to pay for a helmet, an agent's indifference point between paying v_i for a helmet versus not is given by

$$\begin{aligned} \epsilon_{iu} &= \zeta_h + \beta \cdot \Delta r_i(z_i) - \alpha \cdot v_i + \epsilon_{ih} \\ \alpha v_i &= \zeta_h + \beta \cdot \Delta r_i(z_i) + \epsilon_i \\ v_i &= \frac{1}{\alpha} \zeta_h + \frac{\beta}{\alpha} \cdot \Delta r_i(z_i) + \frac{1}{\alpha} \epsilon_i = \zeta'_h + VSL \cdot \Delta r_i(z_i) + \epsilon'_i \end{aligned} \quad (8)$$

VSL is identified from data on v_i and (potentially misreported) beliefs $\Delta r_i(z_i)$, using information exposure z_i to instrument for beliefs (ζ'_h is an intercept). When z_i is randomized, then denoting measurement error free beliefs by Δr_i^t , assumption 7a is equivalent to

$$\text{Classical Measurement Error: } \mathbb{E}[\Delta r_i - \Delta r_i^t] = 0 \quad (7a')$$

This holds since z_i only contains information about Δr_i and (by randomization) is independent of other drivers of v_i . So exclusion holds if misreporting is uncorrelated with z_i .

The model shows that relevance typically holds under Bayesian learning, but estimates are consistent as long as information changes beliefs (even if agents learn in non-standard ways). Relevance may be verified empirically by testing if $\text{Cov}(\Delta r_i, z_i) \neq 0$.

The linearity of VSL in mortality risk follows from considering marginal reductions in risk. For large changes, the relationship would likely be convex since the marginal utility of consumption would fall with transfers, so applying VSL may underestimate welfare gains.

This same framework may be used to estimate demand for other non-market goods by letting r_i be the probability of exposure to the good and z_i be an instrument shifting subjective beliefs about this probability. For instance, when studying demand for clean air, r_i may be the probability of exposure to unclean air mitigated by an air purifier. Or to study preferences for privacy, r_i may be the risk of a privacy breach reduced by security software.

4 Data and empirical specification

4.1 Data

I use data from 1,571 surveys collected in two waves. The first wave was conducted from October to December 2022, and the second between February and March 2023. The first wave included 921 surveys, counting pure control observations. The second included 650.

The survey collected detailed demographic data and information about the motorcycle taxi use of all respondents, and baseline and posterior beliefs about motorcycle taxi risks from those that were not assigned to the pure control group.

The methodology used to elicit beliefs was refined in piloting. Details of the final approach are presented in Appendix A and the full instrument is available on the AEA RCT Registry.¹⁹ The survey elicited priors about per trip mortality risk, the average number of deaths per 10,000 motorcycle taxi passengers over 1 and 5 year periods, and the respondent's risk of dying in a motorcycle accident over 5 years. Posteriors about the respondent's 5 year mortality risk without a helmet and the effectiveness of helmets at preventing death were collected. Distinct measures were obtained to validate the variables against each other.

Briefly, priors and posteriors about unhelmeted risk were elicited by first having respondents select what range of probabilities their beliefs fell into from a set of bands that span the unit interval (such as less than 1 in 10 million, 1 in 10 million to 1 in 1 million, up to greater than 1 in 10). Respondents were then asked to list a more precise belief within the interval. This two-step approach reduced rounding in piloting.²⁰

Belief elicitation was not incentivized because the object of interest is respondents' perceptions of their own mortality risk. Incentivizing accuracy would require benchmarking against some average risk, inducing misreporting if respondents believed their own risk differed from the mean or mistrusted the data (e.g. anecdotally many respondents had concerns politicians under-count deaths). However, helmet demand is increasing in beliefs about motorcycle risk and helmet effectiveness (Appendix Table A2), indicating that measures do align with real decisions. Furthermore, the study design includes multiple features to test for demand effects, poor comprehension of small

¹⁹The instrument is appended to the end of the PAP amendment.

²⁰There is likely still rounding, especially of high beliefs. Such rounding likely results in classical error, but I test for bias from non-classical error. Results are robust to including fixed effects for the interval selected.

probabilities, or other misreporting that could bias VSL estimates as detailed in Section 5.2.

Table 1 summarizes sample demographics and balance across treatments. Appendix Table A1 similarly shows motorcycle use and beliefs. Variables are generally balanced across arms, although there is some imbalance among the pure control (which had a smaller number of respondents).

4.2 Experimental VSL estimation

The primary estimate of VSL is obtained via the two-stage least squares model

$$\begin{aligned} v_i &= \zeta'_h + VSL\Delta r_i + X'_i\gamma_0 + \gamma_1 r_{0,i} + \epsilon'_i \\ \Delta r_i &= Z'_i\pi + X'_i\pi_c + \pi_r r_{0,i} + \nu_i \end{aligned} \tag{10}$$

where v_i is the respondent's willingness to pay for a helmet, Δr_i is their perceived reduction in mortality risk from a helmet, X_i denotes controls, and $r_{0,i}$ is the respondent's baseline belief about their unhelmeted mortality risk. I report results with two sets of instruments, Z_i . First, I consider an “interacted” set $Z_i = (T'_i, r_{0,i} \cdot T'_i)'$ where T_i is a vector of treatment assignment indicators. The second set consists of “treatment only,” $Z_i = T_i$.

The preferred estimate uses the interacted instruments because they absorb heterogeneity in priors, greatly improving power. Intuitively, if there is variation in the perceived riskiness of motorcycles, then Δr_i will vary both due to beliefs about risk and the efficacy of helmets. The interacted instruments capture this, so the first stage more accurately predicts beliefs. Abadie et al. (2023) shows that similar instruments improve asymptotic mean squared error.²¹

Controls are selected using single-post LASSO. Possible controls include demographic variables, motorcycle trip characteristics, and the information sources used to construct beliefs about mortality risk. Estimates also include surveyor fixed effects.²²

I follow the pre-analysis plan and report results over two samples. First, I use data from the control and both treatments. Second, I restrict the sample to treated respondents to help rule out confounds. The primary tables report homoskedastic standard errors and weak instrument robust

²¹I pre-specified two sets of IVs, but the original pre-analysis plan (PAP) registered a slightly different version of the interacted IV using n_i , motorcycle trips/week, instead of $r_{0,i}$. This inaccurately assumed n_i predicts $r_{0,i}$. An amendment before wave 2 specified $r_{0,i}$.

²²Due to an error, the initial PAP only listed the demographic variables. The PAP amendment specified the full set of potential controls and enumerator FEs. Results are similar if they are excluded.

confidence sets. Results are similar using GMM with heteroskedastic robust errors. Appendix B provides details, including justifications for these decisions.

4.3 Alternative VSL estimation procedures for comparison

I compare the experimental VSL estimates to two traditional approaches used to estimate the parameter. First, I estimate

$$v_i = \zeta_h + VSL\Delta r_i^* + X'_i\gamma_0 + \epsilon_i \quad (11)$$

where Δr_i^* is the empirical likelihood that a helmet will save the respondent's life, estimated from ridership and an estimate of helmet efficacy. This approach is similar to León and Miguel (2017) and Ito and Zhang (2020) which estimate random coefficient logit models under rational expectations. But the specification is linear since willingness to pay is observed. Following León and Miguel (2017), I do not instrument for Δr_i^* .

Second, I report the estimates

$$VSL_i = \frac{v_i}{\Delta r_i^*} \quad (12)$$

This assumes rational expectations and that agents do not receive utility from any characteristics of helmets other than safety. This specification follows Berry et al. (2020) and is similar to Kremer et al. (2011). The method is typically used with health products, where assumptions are plausible.

These approaches typically do not separate willingness to pay for non-fatal illness/injury prevention from mortality risk reduction. Therefore authors often interpret estimates as upper bounds on VSL (León and Miguel, 2017; Kremer et al., 2011).

5 Results

5.1 Estimates of the value of a statistical life

Elicited beliefs about the probability of a fatal motorcycle accident fall in plausible ranges and are consistent across measures, supporting their use in estimating VSL. The mean reported 5-year mortality risk is 0.034, with a median of 0.001.²³ For comparison, the estimated median empirical risk is about 1/4,000. Beliefs about deaths per 10,000 passengers and one's own risk are highly correlated ($R^2 > 0.1$), though agents perceive lower personal versus average mortality risk.

²³Results are robust to excluding implausible responses or winsorizing beliefs (Appendix Table A3).

Beliefs about 1-year and 5-year mortality risk are also consistent ($R^2 = 0.57$). Over 60% of respondents report 5-year risk estimates between 4 and 6 times their 1-year estimate, and less than 1% gave logically inconsistent answers. Respondents' 5-year risk beliefs are also aligned with per-trip fatality risk assessments ($R^2 = 0.2$).

First stage: Effect of information on beliefs

Table 2 demonstrates that randomized information exposure had a statistically significant effect on the agents' beliefs that a motorcycle helmet will save their life.

Respondents were first presented with estimates of their unhelmeted mortality risk constructed using data from the NTSA. Consistent with anecdotal reports of mistrust in the NTSA, respondents reported no change in beliefs given this information (columns 1 and 2). There is no significant difference between either treatment arm, which received the same information, versus the control. This suggests agents did not feel compelled by experimenter demand effects to misreport beliefs.

Respondents did update beliefs about the effectiveness of helmets in response to the information treatments (Table 2, columns 3-4), which provides the primary variation used by the interacted instrument. At baseline, agents *overestimated* the effectiveness of helmets: the mean control group belief was 79%, with many respondents reporting values above 90%. Exposure to the 70% ("high") and 42% ("low") effectiveness estimates led respondents to revise beliefs downward to 75% and 65%, respectively, with the difference between treatment arms significant at the 1% level.²⁴

The change in beliefs about helmet effectiveness was large enough to shift respondents' perceived probability that a helmet would save their life (the product of accident risk and helmet efficacy). The dependent variable in Columns 5-6 of Table 2 equals 10,000 times this perceived probability. Relative to the control group, the low-treatment group reports values that are about 80 units lower on average (36%, $p < 0.01$). The high-treatment group does not differ significantly from control (the signal was close to priors) though point estimates move in the predicted direction.

These results establish that information affected beliefs, confirming that treatment assignment is a relevant instrument for perceived reductions in mortality risk.

The value of a statistical life

Primary estimates of the value of a statistical life are reported in Table 3. The preferred estimate

²⁴Under Bayesian updating one would not expect a 30 p.p. gap in beliefs between the treatment arms since beliefs are a weighted average of priors and the signal. Estimates both imply a weight of about 60% on priors.

in column 1 is \$224, suggesting low demand for mortality risk reduction. A weak-instrument robust 95% confidence set excludes values below \$34 and above \$429. Estimates are consistent across specifications, both with and without controls, and across instrument sets.

The estimates pass a battery of robustness checks. Panel b shows that excluding control observations does not materially affect results, though estimates are somewhat larger. Despite reduced power, values above \$1,038 are still rejected at the 95% level. Panel c similarly shows that leaving any experimental arm out does not meaningfully alter estimates.

This method for estimating VSL does not require experimental variation if a valid observational instrument for subjective beliefs exists. Leveraging this, I instrument for risk using a plausibly exogenous shifter of beliefs in column 4: an indicator for knowing a motorcycle accident victim (conditioning on taxi terminal fixed effects). This instrument is speculative as victim exposure could be endogenous. However, the identifying logic is that accident realizations are driven largely by luck, given that respondents travel in similar areas. The aim of this exercise is not to replace information treatments. Rather, the value is that this variation cannot be driven by demand effects, helping check for this confound. Estimates are remarkably similar to those obtained from random variation, with the mean VSL estimate in column 4 at \$560. In fact, a J-test fails to reject the equality of estimates when both this and treatment assignment are used as instruments.

Appendix Table A3 shows that results hold if beliefs about helmet efficacy are winsorized. Appendix Table A4 shows that the results are robust to changes in planned future ridership (Panel a), subjective beliefs about helmet lifespan (Panel b), and are not driven by selection into high ridership (Panel c).²⁵ Appendix Table A5 shows that IV probit results regressing receipt of a helmet on price (BDM draw) and perceived risk reduction (instrumented by treatment) also yields similar results, indicating that results are not driven by valuations above the maximum BDM draw.

As detailed in section 5.2, the local average VSL identified weights individuals with higher priors about the riskiness of motorcycles more heavily. Appendix Table A6 examines whether demographic covariates are correlated with this variable to test if the experiment is identifying the parameter over a particularly selected population. The only covariate significantly correlated with priors is gender, which I show below is not a significant dimension of VSL heterogeneity.

²⁵Estimates exclude observations from the second day of data collection, as specified in the PAP amendment, because in some cases motorcycle drivers were pretending to be passengers to obtain a free helmet and submitting false surveys. Estimates are similar but less precise if they are included (95% CI -\$159 to 241).

Although demand for safety in this setting appears low, economic theory suggests these estimates are consistent with much higher VSLs in richer settings, where the opportunity cost of funds is far lower. Comparing results from this paper to estimates across contexts reveals that this study's estimates are of a similar order of magnitude to those from similar income levels. Some differences may be due to methodology, but this helps to confirm the importance of income.

Figure 1 plots estimates from all LMIC revealed preference estimates of VSL that I am aware of. An estimate from Greenberg et al. (2021) is also presented, which examines US soldiers using related methods, for comparison. Estimates from this paper are lower than most existing values, but similar to those from populations with comparable incomes. For instance, Kremer et al. (2011) estimate an upper bound of about USD \$1,000 to avert a child death in rural Kenya, and Berry et al. (2020) estimate median VSLs near \$0 and just over \$4,000 in Ghana (corresponding to two different treatment effect estimates). Ito and Zhang (2020) and León and Miguel (2017) find much higher VSLs among wealthier populations within LMICs.

As a thought experiment, one may assume that one's utility from being alive is constant across income levels and calculate what curvature of marginal utility of consumption would be required to rationalize differences in VSL. Under a CRRA utility function, the gap between this study's estimate and the VSL of \$700,000 in León and Miguel (2017) implies a coefficient of relative risk aversion of $\theta < 3$.²⁶ This falls solidly in the range of empirical estimates: Havránek (2015) reports a mean elasticity of inter-temporal substitution of 1/3 in a meta-analysis, equivalent to $\theta = 3$. While CRRA utility may not hold exactly, and methodological differences may drive gaps in results, this exercise demonstrates that large cross-country variation in VSL can readily be rationalized by differences in the marginal utility of consumption. This is consistent with Hall and Jones (2007), which finds that health investment rises sharply with income.

Welfare implications of the estimated VSL

The welfare implications of the VSL estimates presented depend on the values that are currently used in benefit-cost analysis. If different VSL values are considered, then using these estimates could yield welfare gains by aligning policy with preferences. A key assumption underlying this welfare exercise is that the VSL identified here is informative for the populations affected by the

²⁶ Assume $\beta_i = \beta, \alpha_i = Y_i^{-\theta}$ where Y_i is income. Then $\frac{VSL_1}{VSL_2} = \left(\frac{Y_1}{Y_2}\right)^\theta \Rightarrow \theta = \log\left(\frac{VSL_1}{VSL_2}\right) / \log\left(\frac{Y_1}{Y_2}\right)$. Thus $\theta \approx \frac{\log(700,000/224)}{\log(75,000/4,750)} = 2.91$

policies considered. In practice, benefit–cost analyses typically either adjust rich-country VSLs for income differences or apply developing-country estimates to populations with similar income levels. I take a more conservative approach by focusing on policies affecting Kenyans, minimizing (though not eliminating) concerns that population heterogeneity drives results.

I selected the five most recent benefit–cost analyses in Kenya from Google Scholar that use VSL to assess how these results affect policy conclusions.²⁷ I re-calculate benefit–cost ratios (BCRs) using the preferred VSL estimate and compare them to the original findings. Appendix Figure A3 shows that BCRs fall sharply: on average by over 99%, and in four cases the BCR falls from above to below 1, reversing the original policy conclusion.

The results also suggest that some international development assistance may be misallocated. GiveWell, an NGO that has matched over \$1 billion to charities, weights averting the death of someone aged 15–49 at 104 times doubling their consumption for a year,²⁸ directly affecting which charities are recommended. This paper suggests that these weights may under-value the benefits of consumption gains by orders of magnitude, suggesting that allocating more funding towards programs like cash transfers could enhance welfare.

Heterogeneity in the value of a statistical life

If VSL is heterogeneous, estimates based on a selected population may differ from the average VSL relevant for targeted policies. This paper focuses on urban commuters, a group of interest for transportation policy. To assess broader applicability, I examine VSL heterogeneity on observables in Table 4. Results align closely with theoretical predictions, but the degree of heterogeneity across individuals in the sample is modest, suggesting the conclusion (demand for safety is low) is unlikely to change across similar populations.

Column 1 shows that VSL is similar below and above the median age. This is consistent with Aldy and Viscusi (2007), which find that VSL peaks near age 40.

Columns 2 and 3 show a statistically significant positive relationship between VSL and income, matching theory. Column 2 reports heterogeneity with respect to $\text{asinh}(\text{wages})$, demeaned so the first coefficient captures average VSL.²⁹ Estimates indicate that a 1% increase in wages is

²⁷ Accessed June 2024, including Babagoli et al. (2022) (menstrual cups/sanitary pads), Mwai et al. (2023) (primary health), Hamze et al. (2017) (cleft lip/palate repair), and Oyugi et al. (2023) (maternity care).

²⁸ Based on 2023 update. Weights are influenced by a stated preference VSL (Redfern et al., 2019).

²⁹ I set the wages of the unemployed to 0 and use asinh since unemployment is often involuntary. Results are similar, but less precise, dropping the unemployed and using $\log(\text{wage})$.

associated with an increase in VSL near \$4. This implies a back of the envelope income-elasticity of 1.14. Column 3 shows agents with above median wages have a VSL that is higher by \$500.

Column 4 provides evidence that VSL is also increasing with health. There is not significant heterogeneity with respect to having children, digit span recall (a cognitive test of memory), years of education, or gender. This implies that the fact the study sampled more men than women is not likely to substantially influence estimates.

5.2 Are VSL estimates biased by non-classical measurement error?

The primary identifying assumption in this paper is that any gap between agents' true subjective beliefs and their reported beliefs reflects only classical measurement error. Research shows that agents decision-relevant beliefs can be shaped by heuristics like rounding (Lacetera et al., 2012). This renders common tests of belief accuracy (such as bunching analysis) uninformative in this context, since the method allows for arbitrary differences between subjective beliefs and the empirical truth. To address this, the experimental design incorporates features that facilitate direct tests of belief validity. The results support the view that expectations are elicited with primarily mean-zero noise, suggesting that the VSL estimates are consistent.

I included two treatments of different intensity so that VSL may be estimated at multiple points of the belief distribution. Estimates should be invariant to the arms included under classical error. However, if beliefs were misreported – unless misstatement was proportional across arms – estimates would differ. For example, if lower reported helmet efficacy in the low treatment arm reflected demand effects, estimates using the low treatment and control groups would be smaller (since the denominator would be over-estimated) than those using the high treatment and control. Estimates are similar across all combinations of arms (Table 3), helping rule out this concern.

The treatment only and interacted instruments facilitate a second test. In short, the interacted IV places greater weight on individuals with higher priors about motorcycle risk. If reported beliefs were subject to non-classical measurement error correlated with true beliefs, the IVs would yield different estimates. Appendix C presents a formal argument, based on the literature on local average treatment effects (e.g. Abadie et al., 2023; Mogstad et al., 2018), and shows that this analysis can test for common patterns of misreporting.

Intuitively, willingness to pay for a helmet is a function of an agent's true belief about motorcy-

cle risks, $r_{0,i}$, helmet effectiveness, and their VSL. Treatment only estimates are proportional to the covariance between willingness to pay and treatment assignment, so a weighted average of VSLs proportional to $r_{0,i}$ is identified. With the interacted instrument, first stage predictions also depend on elicited $r_{0,i}$. This leads to a weighted average of VSLs proportional to $r_{0,i}^2$ if errors are classical. If beliefs are systematically mismeasured, weights will be a function of the correlation between $r_{0,i}$ and error with this instrument, whereas treatment only weights will remain proportional to $r_{0,i}$. Thus, the IVs will produce different estimates, unless errors are orthogonal to true beliefs.

For instance, some agents report very high risks of riding motorcycles. Are they overstating their belief? This would result in the interacted IV producing a lower estimate, as long as those reporting very high beliefs have above average true expectations. The appendix argues that typical forms of rounding and demand effects would also cause the IVs to diverge.³⁰ Yet empirically, VSL estimates are similar across instrument sets, and a Hansen J-test fails to reject their equality ($p > 0.5$), suggesting that significant bias from misreported beliefs is unlikely.

Hawthorne effects are further ruled out by the reduction in willingness to pay in the low treatment arm, providing revealed preference evidence of belief updating. And helmet demand is similar in the pure control and control arms, despite the fact that the control was asked about the risks of motorcycles, which may plausibly generate demand effects. Lastly, as noted above, VSL estimates based on variation from accident victim exposure are statistically indistinguishable.

To further address concerns about bias from difficulty articulating small probabilities, I proxy for perceived helmet risk reduction by combining beliefs about the number of motorcycle deaths per 10,000 users over 5 years with reported helmet efficacy. This avoids relying on direct elicitation of small probabilities. I estimate a VSL of \$151, which is close to (and statistically indistinguishable from) the preferred estimate. The lack of VSL heterogeneity with respect to education or digit span recall is also inconsistent with this concern.

5.3 Testing rational expectations

The primary assumption relaxed by this paper is that agents' beliefs satisfy rational expectations. Results suggest that this condition is violated in this context and that VSL estimates obtained using typical methods are likely biased.

³⁰ Appendix Figure A4 illustrates this in the case of rounding.

This study captures both agents' subjective beliefs and their empirical mortality risk, allowing for tests of rational expectations. The two variables are uncorrelated ($\rho = -0.0008$) and respondents report average beliefs above empirical estimates, suggesting a violation of rational expectations. However, measurement error could obscure a relationship. I therefore instrument for final beliefs using another measure (deaths/10,000 users), which addresses bias from independent classical errors (Gillen et al., 2019). This yields a statistically significant positive relationship, but the coefficient is below 0.001. I reject equality to 1 (the rational expectations benchmark) with 99.9% confidence. Because this test may be sensitive to correlated errors across belief measures, I supplement the analysis with two additional data sources to examine how agents form expectations.

First, the survey asked respondents to list the information sources they used to form beliefs, plotted in Figure 2. The most common source is own experience (79%), followed by family members (48%) and social media (38%). Under a third of respondents reported consulting more objective media or government sources. These information sources are consistent with deviations from rational expectations since they depend on private information and are prone to bias.

Second, I regress beliefs on (1) variables that shift empirical risk and (2) variables that should influence beliefs (but not necessarily true risk) if reported information sources are accurate. I use trip volume and duration as empirical shifters, since more frequent motorcycle use mechanically increases risk, particularly conditional on taxi terminal fixed effects. In fact, a 1% increase in ridership is associated with a 0.13 percentage point increase in the likelihood of reporting a prior accident in the data ($p < .01$). To test if agents form beliefs based on personal or social network experiences, I consider indicators for having suffered a motorcycle accident or knowing a victim.

If rational expectations held, one would expect positive coefficients on the empirical shifters of risk. Table 5 demonstrates that this is not the case: beliefs about risk are unrelated to trip volume or the length of a typical trip with or without terminal fixed effects and covariates. In contrast, accident history – personal or in one's social network – is strongly predictive of beliefs. These patterns indicate a violation of rational expectations.

One remaining concern is that accident history may reflect risk heterogeneity observable to respondents but not to the econometrician, in which case rational expectations could hold under a more complete model of empirical risk. At a minimum, the results highlight the sensitivity of rational expectations to the researcher's data. To further probe expectation formation, I test whether

reported five-year risks vary with rainfall on the survey day. Weather should not affect long-run accident risk, but rain heightens the salience of motorcycle dangers. Respondents reported roughly twice the risk if it rained, indicating a departure from rational expectations.

I next examine if typical approaches to estimate VSL, which assume rational expectations, consistently recover the parameter in Table 6.³¹ Estimates from typical methods fall outside the experimental confidence sets, and different observational approaches yield starkly different results.

Columns 1 - 4 report estimates of VSL obtained from Equation 11. Column 1 omits covariates; columns 2–4 use double-post LASSO for covariate selection, with column 3 adding enumerator and column 4 adding taxi terminal fixed effects. Estimates are highly sensitive to covariates, and confidence intervals are wide. The point estimate exceeds \$15,000 without controls and is negative in column 3. Columns 2 and 4 yield point estimates closer to the experimental benchmark, around \$2,700 and \$1,500, respectively. While this offers some evidence that rational expectations-based methods can recover VSL, standard errors are large and these results may be driven by a weak correlation between willingness to pay and ridership. Estimates using Equation 12 are reported in column 5. The mean estimate exceeds \$380,000.

The experimental approach relaxes rational expectations and addresses endogeneity, raising the question of which drives differences between methods. Column 6 estimates VSL using subjective belief data, instrumenting for final beliefs with priors about the number of motorcycle deaths/10,000 passengers over 5 years. If measurement error is independent across measures, this corrects attenuation bias but not omitted variable bias. The resulting estimate, about \$110, is smaller but not statistically different from the experimental value. While suggestive, this indicates that rational expectations violations may be more important than endogeneity in this setting, consistent with beliefs being shaped by idiosyncratic experiences, such as accident realizations.

5.4 Other threats to identification

There are several threats to the experimental VSL estimates aside from misreported beliefs.

First, the treatments may have influenced beliefs about dimensions of helmets other than their life-saving potential, particularly injury prevention. To limit this, the information treatments ex-

³¹One must take a stance of the efficacy of helmets. Panel a reports results using Liu et al. (2008). Panel b reports estimates using Ouellet and Kasantikul (2006). Discussion focuses on panel a.

cluded injury content and no injury-related questions were asked. If injury beliefs did shift, the VSL estimates would be upper bounds so long as perceptions of helmets efficacy at preventing injury moved in the same direction as consumers' beliefs about their efficacy at preventing death, not affecting the conclusion that demand for reductions in mortality risk is low.

Second, VSL may be underestimated if agents did not plan to regularly use helmets. Piloting indicated that (consistent with the fact that helmets are costly) respondents believed that they would regularly use them, so such bias seems unlikely (although possible). If planned use were infrequent, measured helmet efficacy would overstate the true risk reduction, with the bias increasing in the level of reported effectiveness. VSL estimates would thus differ across arms, since the high treatment group would be more affected than the low treatment. The similarity of estimates excluding arms therefore provides support that infrequent helmet use is not driving the results.

Resale is also improbable because respondents all accepted a helmet with quality tags and the box removed, which undermines resale value but avoids hassle if worn. And even if respondents anticipated using a helmet 10% of the time, VSL estimates would remain well below those used in policy. However, some respondents did report a risk they would lose the helmet. Appendix Table A4 shows that estimates are similar when adjusting for anticipated duration of use.

Monotonicity violations could affect the interpretation of estimates as an average VSL. But only 3% of control respondents had priors below the 42% figure presented to the low treatment group, and estimates are similar when only control and low treatment observations are considered.

Could mismeasured helmet valuations bias results? Research shows that the BDM is accurate in similar stakes environments (Berry et al., 2020). Moreover, a level shift in valuations would not bias VSL estimates, which are identified from differences in willingness to pay across arms. The similarity of IV probit estimates also provides evidence against this confound.

Finally, agents could face capital constraints. Demand was elicited via a helmet-versus-cash choice to ensure valuations are not constrained by cash on hand. While temporary liquidity needs may exist, the estimated mean VSL is only \$500 among respondents with above median income and median willingness to pay is 1/4 weekly wages, suggesting substantial bias is unlikely. And estimates are about \$290 if the unemployed are excluded. Moreover, since recipients of aid programs likely face similar constraints, the VSL estimates should still accurately reflect the value of consumption gains, even if imperfect capital markets contribute to high consumption value.

6 Discussion

This section examines whether the VSL estimates in this paper are plausible and considers potential limitations of the estimates and with leveraging VSL to value health investments in poor settings. I also describe the conditions needed to apply the tools presented in this paper to other amenities.

6.1 Is an average VSL below annual income plausible?

The average VSL estimated in this study is a fraction of annual income. Is such low demand for safety plausible?

An important insight for interpreting this result is that VSL is not equivalent to the present value of lifetime consumption. Section 3 highlights that the parameter is a function of one's expected utility from being alive minus perceptions about utility from not being alive. As a result, VSL may be sensitive to cultural and religious beliefs (Colmer, 2020). For instance, those with strong beliefs in an afterlife are likely to have VSLs that fall well below the lifetime utility of consumption because their expected utility from not being alive is high. Empirical evidence validates this view, finding that VSL falls sharply with strength of religious (de Lima, 2020) and fatalistic views (León and Miguel, 2017). Fatalistic beliefs have been shown to limit HIV testing in Kenya (Oo et al., 2024), and the country has near universal belief in an afterlife (Evans et al., 2025).

Estimates also align with a consistent finding that demand for effective preventive health products is often low in LMICs (Dupas and Miguel, 2017). These investments offer a useful point of comparison since they involve similar tradeoffs between modest mortality risks and cost.

A number of studies illustrate low demand. Cohen and Dupas (2010) show that only 40% of pregnant women in Kenya pay \$0.60 for a bed net that all adopt for free. Malaria nets save an estimated 5.6 lives per 1,000 users annually, implying a cost per life saved below \$110 at \$0.60 (Pryce et al., 2018). Ashraf et al. (2010) find that demand for water treatment, which prevents a leading form of child mortality in diarrhea, drops 30 percentage points when price rise from \$0.10 to \$0.25. Uptake of deworming pills falls by 60 percentage points when costs rise by \$0.30 (Kremer and Miguel, 2007). And Banerjee et al. (2010) show that offering in-kind incentives worth \$4 to vaccinate children (at no cost) increases vaccination rates from 18% to 39%. Estimates suggest vaccines reduce child mortality by at least 0.5 percentage points in India (Kumar, 2024).

Moreover, Dupas and Miguel (2017) note that demand for such products typically remains limited when capital or psychological constraints are mitigated, consistent with a low VSL.

6.2 Could VSL understate demand for health in poor countries?

In rich countries, welfare gains from prospective regulations or health interventions are often driven primarily by the value of lives saved. Does a low VSL therefore imply that investments in safety or public health institutions are not welfare enhancing in developing countries?

This conclusion would follow if preferences for mortality risk reduction and protection from non-fatal morbidity scaled proportionally with income. However, theory suggests the opposite. The same concavity of consumption utility that lowers willingness to pay for marginal reductions in mortality risk at low incomes may increase the relative disutility of non-fatal health shocks. Although speculative, this raises concern about the common practice of valuing health interventions primarily through lives saved (with fixed adjustments for disability) in poor settings.

One can model health investment as a problem where agents receive concave utility from consumption each period and investments in safety increase survival odds (Hall and Jones, 2007). This framework implies that VSL rises nonlinearly with income: the marginal utility from higher consumption in a period falls as consumption rises, whereas expected utility is linear in survival probability. Non-fatal health shocks enter differently. Rather than increasing the probability that an agent survives, they reduce quality of life (consumption) conditional on survival. When consumption is low, such losses translate into large utility costs, implying that the relative disutility of morbidity to mortality risk should rise as income falls. Intuitively, the quality of life reduction from an illness or disability may be higher if one lacks resources to cope (Gertler and Gruber, 2002).

This argument raises the possibility that poor consumers may have much higher demand for health interventions than their VSL suggests, since most programs that lower mortality risk also reduce morbidity risk. This conjecture is consistent with the fact that helmet demand often exceeds production costs in this setting despite the low VSL. However, the design of the experiment only allows me to determine that the intercept of agents' utility function, which captures all determinants of utility other than helmets' lifesaving potential, is positive. Future research designed to estimate demand for protection from non-fatal health shocks may therefore be valuable to inform whether VSL captures the main sources of agents' utility of health investments in developing countries.

6.3 Could behavioral biases explain low VSL estimates?

This paper relaxes two strong assumptions made in revealed-preference VSL estimates (rational expectations and risk exogeneity) and, as argued above, the resulting estimates (while surprisingly low) are consistent with theoretical predictions and related empirical evidence. However, estimates of VSL could be affected by behavioral biases that this paper is not designed to test.

One such force is present bias. Preventative health investments require upfront costs to protect against uncertain future risks, which could depress VSL estimates if individuals exhibit non-standard discounting. Dupas and Miguel (2017) argue that evidence of demand for preventative health (especially in voucher experiments) is often inconsistent with present bias. However, direct evidence studying whether such behavioral biases affect VSL estimates would be valuable to determine whether agents in LMICs do in fact have weak preferences for mortality risk reductions.

An advantage of the method presented in this paper is that it provides an approach that could be readily extended to study such mechanisms. Identifying observational settings where the variation needed to test if behavioral biases affect VSL is difficult. By contrast, one could combine this paper’s framework with experimental tests for behavioral biases. This study therefore provides a first step towards rigorously testing further assumptions underpinning VSL estimates.

6.4 Do low LMIC VSLs imply they should receive fewer global health resources?

These results should not be interpreted as a call to cut health funding to LMICs because the use of the estimates is not theoretically supported to value all health programs. VSL is relevant for decisions where a population faces a mortality risk-consumption choice. This informs many important decisions such as those discussed earlier. It does not apply when funds are earmarked for health.

To illustrate this, suppose that there are otherwise homogeneous high and low-income consumers indexed by h, ℓ . As in section 3, let α denote the marginal utility of consumption and β expected utility from averting death. Suppose a high-income government distributes N vaccines to low-income consumers, each of which saves a life with probability p . The cost is $c \cdot N$ paid for by N high-income consumers. The global welfare gain is

$$W = N \cdot p \cdot \beta_\ell - N \cdot c \cdot \alpha_h$$

W is positive if $p \cdot VSL_\ell \cdot \frac{\alpha_\ell}{\alpha_h} > c$. Valuing lives saved instead at VSL_ℓ would understate welfare

gains by $\frac{\alpha\ell}{\alpha_n}$, which may be orders of magnitude. Intuitively, low-income consumers have low VSLs due to a high opportunity cost of funds. If high-income consumers pay costs, low-income consumers face no consumption tradeoff so their marginal utility of consumption should not be used to price costs. This distinction matters because applications of VSL often value global health investments using local VSLs, mechanically reaching the conclusion that fewer resources should flow to poor countries (Ahuja et al., 2021; Agrawal et al., 2023).

6.5 What conditions are needed to estimate demand for other amenities?

The method introduced in this paper – updating beliefs about the effect of a product on exposure to a non-market amenity and then eliciting demand for the product – may also be informative to study demand for other amenities. However, the applicability of this method may be constrained in some settings by the existence of a suitable product to evaluate.

Applying the method requires the existence of a product that clearly affects exposure to a non-market amenity, that the product has not been ubiquitously adopted, and that there is credible information about the relationship between the product and amenity that can be used to update beliefs. Such a set of conditions may be challenging to identify in high-income settings where product diffusion is rapid, although strong cultural forces may produce informative populations in some cases (e.g. bicycle helmet usage is rare in the Netherlands). In developing countries, the slow diffusion of products creates far more opportunities to apply such tools. For instance, the use of air purifiers and water purifiers is often limited in sub-Saharan Africa, creating a feasible environment to study preferences for clean air and water. This method may therefore be particularly valuable to value non-market amenities in developing countries.

7 Conclusion

This study introduces a framework for experimentally estimating demand for non-market amenities using subjective belief data and uses the approach to estimate the value of a statistical life. I leverage the fact that products exist which affect an agent's exposure to non-market goods. This paper demonstrates that researchers may cause agents to update their beliefs about how such a product affects the amenity of interest, then examine how product demand changes to uncover willingness to pay for the amenity. This method is tractable and low-cost in many settings where

exogenous variation in attribute exposure does not exist, and it avoids the need to assume that agents' beliefs satisfy rational expectations, which appears to be violated in this setting.

I estimate low demand for mortality risk reduction in Kenya. This is consistent with a common finding that uptake of preventative health products is low in LMICs. These results suggest scope to improve welfare by better aligning policy decisions with recipients' preferences. Highlighting potential misallocation, a major aid organization underweights consumption gains by orders of magnitude relative to the level implied by these estimates. This may have direct welfare consequences, as funds are allocated to preventative health programs at the expense of cash transfers.

This does not mean that health programs are not valuable. Rather, it suggests that recipients may value consumption so strongly that its benefits can exceed those of some health investments. Theory supports applying VSL only when a direct health-consumption tradeoff is faced, so these findings do not support reallocating health funds towards rich countries. Furthermore, VSL excludes welfare gains from protection from non-fatal health shocks, which may be particularly consequential in poor countries. Research studying the disutility from such shocks would be valuable to determine whether focusing welfare calculations on lives saved is appropriate in LMICs.

Finally, theory suggests that VSL is likely to vary substantially across populations due to factors like wealth and religious beliefs. However, methodological differences between estimates make it challenging to study VSL heterogeneity. It may therefore be valuable to apply the method in this paper in additional contexts to provide insight into the determinants of demand for mortality risk reduction. Similarly, introducing an experimental method for producing the variation needed to identify VSL raises the possibility of extending the method to test whether forces such as psychological biases contribute to low estimates.

References

- Abadie, A., J. Gu, and S. Shen (2023, 3). Instrumental variable estimation with first-stage heterogeneity. *Journal of Econometrics*, 105425.
- Agrawal, V., N. Sood, and C. M. Whaley (2023, 10). The Impact of the Global COVID-19 Vaccination Campaign on All-Cause Mortality. *NBER Working Paper*.
- Ahuja, A., S. Athey, A. Baker, E. Budish, J. C. Castillo, R. Glennerster, S. D. Kominers, M. Kremer, J. Lee, C. Prendergast, C. M. Snyder, A. Tabarrok, B. J. Tan, and W. Wiecek (2021). Preparing for a Pandemic: Accelerating Vaccine Availability. *AEA P&P* 111, 331–35.
- Aldy, J. E. and W. K. Viscusi (2007, 7). Age Differences in the Value of Statistical Life: Revealed Preference Evidence. <https://doi.org/10.1093/reep/rem014> 1(2), 241–260.
- Anderson, T. W. and H. Rubin (1949). Estimation of the Parameters of a Single Equation in a Complete System of Stochastic Equations. *Annals of Mathematical Statistics* 20(1).
- Ashenfelter, O. (2006, 3). Measuring the Value of a Statistical Life: Problems and Prospects*. *The Economic Journal* 116(510), C10–C23.
- Ashenfelter, O. and M. Greenstone (2004). Estimating the Value of a Statistical Life: The Importance of Omitted Variables and Publication Bias. *American Economic Review* 94(2).
- Ashraf, N., J. Berry, and J. M. Shapiro (2010). Can Higher Prices Stimulate Product Use? Evidence from a Field Experiment in Zambia. *American Economic Review* 100(5).
- Babagoli, M., A. Benshaul-Tolonen, G. Zulaika, E. Nyothach, C. Oduor, D. Obor, L. Mason, E. Kerubo, I. Ngere, K. Laserson, R. Edwards, and P. Phillips-Howard (2022, 9). Cost-Effectiveness and Cost-Benefit Analyses of Providing Menstrual Cups and Sanitary Pads to Schoolgirls in Rural Kenya. *Women's Health Reports* 3(1), 773.
- Bachani, A. M., Y. W. Hung, S. Mogere, D. Akunga, J. Nyamari, and A. A. Hyder (2017, 3). Helmet wearing in Kenya: prevalence, knowledge, attitude, practice and implications. *Public Health* 144, S23–S31.
- Banerjee, A. V., E. Duflo, R. Glennerster, and D. Kothari (2010, 6). Improving immunisation coverage in rural India: clustered randomised controlled evaluation of immunisation campaigns with and without incentives. *BMJ (Clinical research ed.)* 340(7759), 1291.
- Baylis, P., M. Greenstone, K. Lee, and H. Sahai (2023). Is the Demand for Clean Air Too Low? Experimental Evidence from Delhi.
- Becker, G. M., M. H. Degroot, and J. Marschak (1964). Measuring utility by a single-response sequential method. *Behavioral Science* 9(3), 226–232.
- Belloni, A., V. Chernozhukov, and C. Hansen (2014). Inference on Treatment Effects after Selection among High-Dimensional Controls. *The Review of Economic Studies* 81(2).

- Berry, J., G. Fischer, and R. Guiteras (2020). Eliciting and Utilizing Willingness to Pay: Evidence from Field Trials in Northern Ghana. <https://doi.org/10.1086/705374> 128(4).
- Campbell, H. F. and R. P. C. Brown (2003, 7). Valuation of Non-marketed Goods. *Benefit-Cost Analysis*, 261–287.
- Cohen, J. and P. Dupas (2010). Free distribution or cost-sharing? Evidence from a randomized malaria prevention experiment. *Quarterly Journal of Economics* 125(1), 1–45.
- Colmer, J. (2020, 9). What is the meaning of (statistical) life? Benefit–cost analysis in the time of COVID-19. *Oxford Review of Economic Policy* 36(Supplement_1), S56–S63.
- de Lima, M. (2020). The value of a statistical life in Mexico. *J Environ Econ Polic* 9(2).
- Dupas, P. and E. Miguel (2017, 1). Impacts and Determinants of Health Levels in Low-Income Countries. *Handbook of Economic Field Experiments* 2, 3–93.
- Evans, J., K. Lesage, W. Miner, K. J. Starr, and M. Corichi (2025, 5). Spiritual and Religious Beliefs and Practices Around the World — Pew Research Center.
- Gertler, P. and J. Gruber (2002, 3). Insuring Consumption Against Illness. *American Economic Review* 92(1), 51–70.
- Gillen, B., E. Snowberg, and L. Yariv (2019). Experimenting with measurement error: Techniques with applications to the caltech cohort study. *Journal of Political Economy* 127(4).
- Greenberg, K., M. Greenstone, S. P. Ryan, and M. Yankovich (2021). The Heterogeneous Value of a Statistical Life: Evidence from U.S. Army Reenlistment Decisions. *NBER Working Paper*.
- Greenstone, M. and B. K. Jack (2015, 3). Envirodevonomics: A Research Agenda for an Emerging Field. *Journal of Economic Literature* 53(1), 5–42.
- Hall, R. E. and C. I. Jones (2007, 2). The Value of Life and the Rise in Health Spending. *The Quarterly Journal of Economics* 122(1), 39–72.
- Hamze, H., A. Mengiste, and J. Carter (2017). The impact and cost-effectiveness of the Amref Health Africa-Smile Train Cleft Lip and Palate Surgical Repair Programme in Eastern and Central Africa. *The Pan African medical journal* 28, 35.
- Havránek, T. (2015). Measuring Intertemporal Substitution: The Importance of Method Choices and Selective Reporting. *Journal of the European Economic Association* 13(6).
- Ito, K. and S. Zhang (2020, 5). Willingness to pay for clean air: Evidence from air purifier markets in China. *Journal of Political Economy* 128(5), 1627–1672.
- Killeen, G. (2025, 4). Experimentally estimating the value of a statistical life.
- Kremer, M., J. Leino, E. Miguel, and A. P. Zwane (2011). Spring Cleaning: Rural Water Impacts, Valuation, and Property Rights Institutions*. *The Quarterly Journal of Economics* 126(1).

- Kremer, M. and E. Miguel (2007, 8). The Illusion of Sustainability. *The Quarterly Journal of Economics* 122(3), 1007–1065.
- Kumar, S. (2024). The Effects of Childhood Immunization Program on Health and Education: Micro-Evidence from India.
- Lacetera, N., D. G. Pope, and J. R. Sydnor (2012, 8). Heuristic Thinking and Limited Attention in the Car Market. *American Economic Review* 102(5), 2206–36.
- León, G. and E. Miguel (2017, 1). Risky Transportation Choices and the Value of a Statistical Life. *American Economic Journal: Applied Economics* 9(1), 202–28.
- Liu, B. C., R. Ivers, R. Norton, S. Boufous, S. Blows, and S. K. Lo (2008). Helmets for preventing injury in motorcycle riders. *The Cochrane database of systematic reviews* (1).
- Manski, C. F. (2004, 9). Measuring Expectations. *Econometrica* 72(5), 1329–1376.
- Mendelsohn, R. and S. Olmstead (2009). The economic valuation of environmental amenities and disamenities: Methods and applications. *Annu Rev Environ Resour* 34, 325–347.
- Mogstad, M., A. Santos, and A. Torgovitsky (2018). Using Instrumental Variables for Inference About Policy Relevant Treatment Parameters. *Econometrica* 86(5).
- Moreira, M. (2003). A Conditional Likelihood Ratio Test for Structural Models. *Econometrica*.
- Mwai, D., S. Hussein, A. Olago, M. Kimani, D. Njuguna, R. Njiraini, E. Wangia, E. Olwanda, L. Mwaura, and W. Rotich (2023, 3). Investment case for primary health care in low- and middle-income countries: A case study of Kenya. *PLOS ONE* 18(3), e0283156.
- Oo, T. A. A., M. L. Romo, G. George, E. Mwai, E. Nyaga, J. E. Mantell, J. O. Odhiambo, K. Govender, and E. A. Kelvin (2024, 12). Using the Health Belief Model to Understand Why Making Oral HIV Self-Testing Available to Truck Drivers in Kenya Had Little Impact on Six-Month Testing. *AIDS and Behavior* 28(12), 4118–4126.
- Ouellet, J. V. and V. Kasantikul (2006). Motorcycle Helmet Effect on a Per-Crash Basis in Thailand and the United States. <https://doi.org/10.1080/15389580500338652> 7(1).
- Oyugi, B., O. Nizalova, S. Kendall, and S. Peckham (2023, 2). Does a free maternity policy in Kenya work? Impact and cost-benefit consideration based on demographic health survey data. *The European Journal of Health Economics*.
- Pryce, J., M. Richardson, and C. Lengeler (2018, 11). Insecticide-treated nets for preventing malaria. *The Cochrane Database of Systematic Reviews* 2018(11), CD000363.
- Redfern, A., M. Gould, M. Chege, S. Li, F. A. Garay, and W. Slotznick (2019). Beneficiary preferences: Findings from Kenya and Ghana. Technical report, IDinsight.
- Shrestha, M. (2020, 2). Get Rich or Die Tryin': Perceived Earnings, Perceived Mortality Rates, and Migration Decisions of Potential Work Migrants from Nepal. *World Bank Economic Review*.
- Wiswall, M. and B. Zafar (2018, 2). Preference for the Workplace, Investment in Human Capital, and Gender. *The Quarterly Journal of Economics* 133(1), 457–507.

Figure 1: Revealed-preference VSL estimates across studies

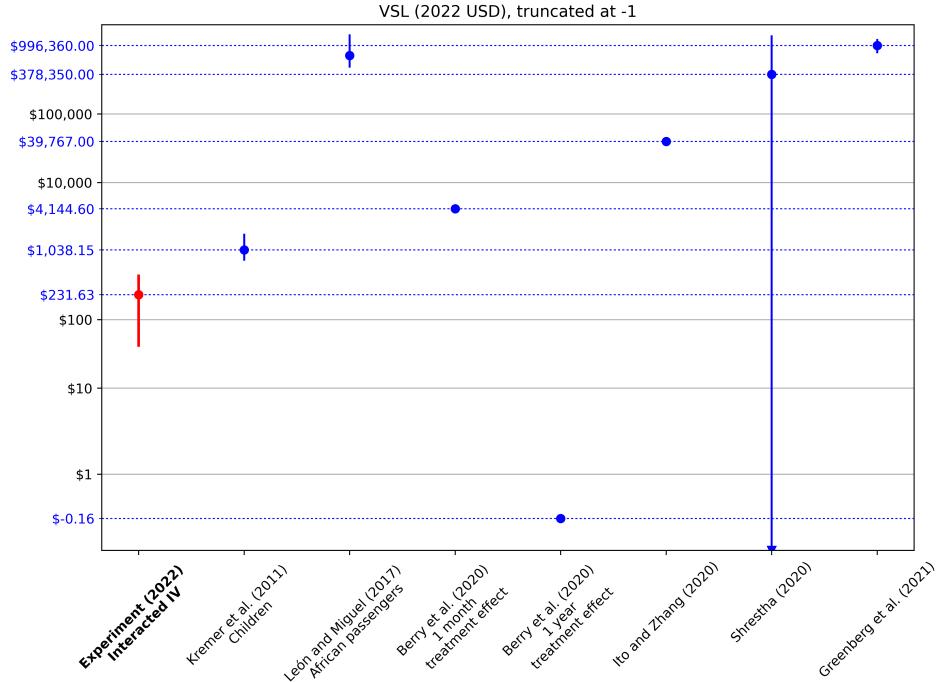


Figure 1 plots revealed-preference VSL estimates and (where available) 95% confidence intervals from this paper, Kremer et al. (2011, rural Kenya), León and Miguel (2017, wealthy international travelers in Sierra Leone), Berry et al. (2020, rural Ghana), Ito and Zhang (2020, urban China), Shrestha (2020, potential Nepalese migrants), and Greenberg et al. (2021, American soldiers). Greenberg et al. (2021) is included for comparison to a high-income setting. The other estimates are from low and middle income economies. All estimates are presented in 2022 USD calculated by inflating based on the paper's publication year using the CPI inflation calculator. The lower bound of the 95% confidence interval from Shrestha (2020), which is below -\$600,000, is truncated at -\$1. Ito and Zhang (2020) does not report a confidence interval.

Figure 2: Information sources used to form beliefs about motorcycle safety

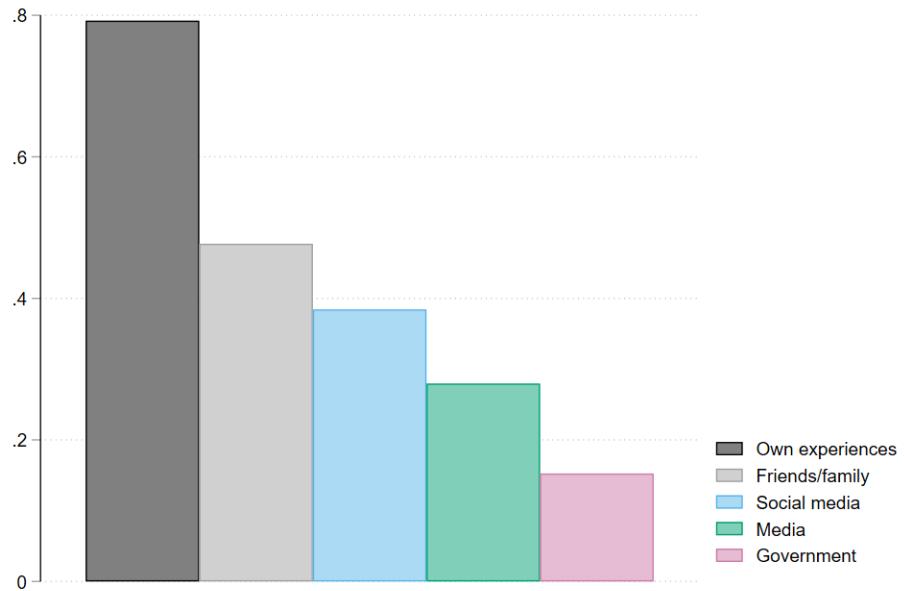


Figure 2 plots the information sources that respondents reported using to form beliefs about the mortality risk of motorcycles. Respondents were able to select multiple options, so the columns do not add to 1.

Table 1: Summary statistics and balance: Demographics

	(1) Control	(2) Pure control - control	(3) Low treatment - Control	(4) High treatment - Control	(5) High - low treatment
Age	32.735 [8.126]	0.105 (1.159)	0.282 (0.572)	0.659 (0.589)	0.377 (0.590)
Female	0.352 [0.478]	-0.039 (0.060)	-0.064** (0.030)	-0.050 (0.030)	0.014 (0.029)
Health (1-5)	3.403 [0.667]	-0.034 (0.086)	-0.056 (0.043)	-0.012 (0.044)	0.044 (0.043)
Life expectancy	81.438 [6.947]	-0.134 (0.840)	0.457 (0.427)	-0.119 (0.439)	-0.577 (0.413)
Employed	0.898 [0.303]	-0.051 (0.043)	-0.041* (0.022)	-0.047** (0.022)	-0.006 (0.022)
Income (\$1000s)	6.866 [8.427]	-0.150 (1.472)	0.635 (0.834)	1.017 (0.862)	0.384 (0.956)
$\mathbb{E}[\text{Wage in 5 years}]$ /wage today	6.166 [11.685]	-0.636 (1.761)	-0.925 (0.710)	-1.422* (0.734)	-0.491 (0.667)
1(children)	0.774 [0.418]	0.033 (0.053)	-0.002 (0.027)	-0.014 (0.028)	-0.012 (0.027)
Digit span recall	3.020 [1.391]	-0.156 (0.183)	-0.006 (0.089)	-0.010 (0.091)	-0.002 (0.088)
Years of education	12.111 [2.869]	-0.468 (0.371)	0.161 (0.184)	-0.025 (0.189)	-0.186 (0.181)
Primary school complete	0.964 [0.186]	-0.035 (0.026)	0.007 (0.013)	-0.022 (0.013)	-0.029** (0.013)
Secondary school complete	0.722 [0.449]	-0.047 (0.058)	0.024 (0.029)	-0.008 (0.029)	-0.031 (0.028)
College degree	0.241 [0.428]	-0.054 (0.055)	-0.003 (0.028)	0.014 (0.028)	0.018 (0.027)
N (exc. control in col. 2-4)	452	80	531	473	
Joint p-value		0.743	0.183	0.193	0.271

Standard deviations in brackets. Standard errors in parenthesis. * $p < .10$, ** $p < .05$, *** $p < .01$

Column (1) reports the mean and standard deviation of the indicated variable across the control group. Columns (2) - (5) report the difference in mean of each variable across treatment arms, where terms in parentheses are standard errors of this pairwise t-test. The joint p-value reported at the bottom of each table reflects an F-test of joint orthogonality estimated by regressing treatment status on each of the covariates, imputing missing values with the median to avoid dropping the observations. The observation counts in columns 2-4 reflect the size of the treatment group compared to the control. 35 observations for which surveyors reported motorcycle taxi drivers pretending to be passengers are excluded.

Table 2: Effect of information on beliefs

	(1) Posterior risk	(2) Posterior risk	(3) Helmet effectiveness	(4) Helmet effectiveness	(5) Risk reduction	(6) Risk reduction, winsorized
Low treatment	-25.90 (21.77)	-25.90 (21.77)	-14.08 (0.97)	-14.08 (0.97)	-80.66 (18.31)	-45.76 (14.00)
High treatment	6.77 (24.91)	6.77 (24.91)	-3.98 (0.88)	-3.98 (0.88)	-28.11 (19.55)	1.30 (15.92)
Control mean Pr(High treatment = low treatment)	330.97 0.08	330.97 0.08	78.68 0.00	78.68 0.00	221.79 0.00	228.62 0.00
Observations	1,425	1,425	1,427	1,427	1,425	1,425
Controls	BL Risk	LASSO	BL Risk	LASSO	LASSO	LASSO
Enumerator FE	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parenthesis.

Columns (1) - (2) report 10,000 times the respondent's stated posterior belief about the probability that they will suffer a fatal accident over the next 5 years, the lifespan of a helmet, if they do not purchase a helmet. Columns (3) - (4) report the respondent's stated belief about the effectiveness of motorcycle helmets, expressed as the percentage reduction in mortality risk. Columns (5) - (6) report 10,000 times the likelihood that a helmet will save the respondent's life over its 5 year lifespan based on their beliefs, the product of the prior two variables. The outcome in column (6) is winsorized at the 2nd and 98th percentiles. All estimates include wave fixed effects. Respondents assigned to the "Low treatment" group received the results of Liu et al. (2008) which estimates that helmets are 42% effective. Those assigned to "High treatment" were presented with the results of Ouellet and Kasantikul (2006) that helmets were 70% effective in a study in Thailand. Individuals in both treatment arms received empirical estimates of their risk of a fatal accident based on ridership. All models exclude 35 observations for which surveyors reported motorcycle taxi drivers pretending to be passengers.

Table 3: Value of a statistical life: Primary estimates

Panel A: Full sample		(1) Interacted	(2) Interacted	(3) Treatment only	(4) Know victim
VSL		223.90 (97.88)	215.23 (98.38)	347.26 (259.49)	559.99 (225.82)
Cragg-Donald F-statistic	F-	40.75	40.12	10.99	16.24
Weak IV Robust Confidence Set		[34.22, 428.34]	[24.10, 420.73]	[-160.17, 1,066.93]	[193.40, 1,230.60]
Inversion test		CLR	CLR	CLR	AR
Observations		1,425	1,425	1,425	1,425
Controls		LASSO	BL Risk	BL Risk	None
Fixed effects		Enumerator	Enumerator	Enumerator	Terminal

Panel B: Treated respondents only		(1) Interacted	(2) Interacted	(3) Treatment only
VSL		394.52 (257.04)	396.03 (253.28)	774.94 (478.06)
F-statistic		16.07	16.84	11.24
Confidence Set		[-88.12, 1,038.34]	[-80.04, 1,021.81]	[-39.13, 2,403.92]
Inversion test		CLR	CLR	AR
Observations		982	982	982
Controls		LASSO	BL Risk	BL Risk

Panel C: Interacted estimates leaving 1 arm out		(1) High treatment	(2) Low treatment	(3) Control
VSL		213.28 (87.39)	178.31 (174.54)	394.52 (257.04)
F-statistic		82.35	23.50	16.07
Confidence Set		[43.23, 390.86]	[-170.17, 559.50]	[-88.12, 1,038.34]
Observations		963	905	982

Standard errors in parenthesis.

Panel A reports regressions using observations in all experimental arms. Panel B excludes control observations. “Interacted” columns use a first stage consisting of treatment and treatment interacted with baseline beliefs as instruments. Columns labeled “Treatment only” use only treatment assignment. Panel C reports “interacted” estimates leaving one experimental arm out, listed in the column title. All estimates control for baseline risk beliefs. Column (4) reports estimates that instrument for beliefs using exposure to a motorcycle accident victim, with robust standard errors in parenthesis. This instrument is speculative as knowing a victim may be endogenous, but is included to show that plausibly exogenous observational instruments align well with experimental estimates. Weak instrument robust confidence sets are reported in brackets, using CLR inversion in Panel C and the indicated tests in Panels A and B. Panel C includes enumerator fixed effects and LASSO selected controls in each regression. Excludes 35 observations where surveyors reported motorcycle taxi drivers pretending to be passengers.

Table 4: Heterogeneity in the value of a statistical life

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1(Age > median)	asinh(Wage)	1(Wage > median)	1(health > median)	1(children)	1(digit recall score > median)	1(education > median)	1(female)
VSL	286.64 (148.10)	340.44 (155.09)	8.11 (186.16)	42.85 (169.85)	250.85 (123.04)	60.51 (197.61)	214.97 (181.00)	223.03 (119.15)
VSL x Interaction	4.39 (268.81) [0.987]	386.85 (127.27) [0.002]	501.51 (273.24) [0.066]	497.87 (254.27) [0.050]	-97.05 (247.69) [0.695]	297.54 (242.17) [0.219]	70.90 (232.93) [0.761]	260.45 (238.62) [0.275]
VSL elasticity			1.14 (0.49)					
Cragg-Donald F-stat	17.72	12.37	18.78	17.01	21.70	12.10	18.78	16.78
Sanderson-Windmeijer first stage F-stats								
Base	36.18	19.58	33.87	48.22	31.56	16.14	25.07	27.87
Interaction	25.10	22.48	26.66	28.34	42.31	22.06	30.73	36.53
Observations	1,423	1,408	1,408	1,425	1,425	1,425	1,417	1,425
Controls	LASSO	LASSO	LASSO	LASSO	LASSO	LASSO	LASSO	LASSO

Standard errors in parenthesis. P-values in brackets.

All columns report VSL estimates across the full sample and use a first stage consisting of treatment indicators and treatment indicators interacted with baseline motorcycle risk beliefs, and each of these values interacted with the demographic variable, as instruments for the mortality risk reduction of a helmet. The low treatment and control arms are pooled since posterior beliefs are similar among these groups, and once splitting on some dimensions of heterogeneity, the sample of respondents within certain groups otherwise becomes small since randomization was not stratified. In columns 1, 3-4, and 6-7, the demographic variable is converted to an indicator equal to 1 if the response was above the median. Unemployed individuals have wages coded to 0 in columns 2 and 3 since unemployment is typically involuntary in this sample. The estimates exclude 35 observations for which surveyors reported motorcycle taxi drivers pretending to be passengers.

Table 5: Correlates with beliefs

	(1) 10,000 × Risk	(2) 10,000 × Risk	(3) 10,000 × Risk	(4) 10,000 × Risk	(5) 10,000 × Risk
Panel A: No covariates					
Trips/week	-0.13 (3.71)				
Trip length		-1.01 (1.80)			
Previous accident			136.02 (47.02)		
Contact in accident				266.14 (37.21)	
Raining					301.83 (172.92)
Sample average	333.22	333.22	333.22	333.22	333.22
Observations	1,427	1,427	1,427	1,427	1,427
Panel B: Taxi terminal FEs and controls					
Trips/week	-0.27 (3.98)				
Trip length		0.77 (1.65)			
Previous accident			90.05 (49.74)		
Contact in accident				204.19 (58.20)	
Raining					325.00 (123.58)
Sample average	333.22	333.22	333.22	333.22	333.22
Observations	1,427	1,427	1,427	1,427	1,427

Robust standard errors in parenthesis. Standard errors in column 5 clustered by day.

Table 5 reports the correlation of demographic variables with prior beliefs about one's risk of dying in a motorcycle accident over a 5 year span. Estimates in Panel A do not include any controls or fixed effects. Estimates in Panel B include controls selected using double-post LASSO and taxi terminal/stand fixed effects (Belloni et al., 2014). All estimates include wave fixed effects and interpolate missing controls using the median of the variable. Rainfall is coded to 1 if Visual Crossing reported positive precipitation from a weather station in Nairobi on the day the survey was conducted.

Table 6: Value of a statistical life: Estimates using alternative methods

Panel A: 42% helmet effectiveness treated as truth						
	Rational expectations			Subjective beliefs		
	(1) OLS	(2) LASSO	(3) LASSO	(4) LASSO	(5) $\frac{W^T P_i}{\Delta r_i^*}$	(6) Beliefs
VSL	15,244.30 (10,900.69)	2,744.41 (10,630.65)	-6,167.09 (10,840.79)	1,490.08 (10,747.15)	381,326.62 (13,859.27)	110.96 (53.74)
95% confidence set	[-6,121.06, 36,609.66]	[-18091.67, 23,580.49]	[-27415.04, 15,080.86]	[-19574.34, 22,554.50]	[354,162.44, 408,490.79]	[5.63, 216.29]
F-statistic						
Observations	1,425	1,425	1,425	1,425	1,425	19.73
Controls	None	DPLASSO	DPLASSO	DPLASSO	None	1,425
Enumerator FE	No	No	Yes	No	No	None
Taxi Terminal FE	No	No	No	Yes	No	Yes

Panel B: 70% helmet effectiveness treated as truth						
	Rational expectations			Subjective beliefs		
	(1) OLS	(2) LASSO	(3) LASSO	(4) LASSO	(5) $\frac{W^T P_i}{\Delta r_i^*}$	(6) Beliefs
VSL	9,146.58 (6,540.42)	1,646.65 (6,378.39)	-3,700.25 (6,504.48)	894.05 (6,448.29)	228,795.97 (8,315.56)	110.96 (53.74)
95% confidence set	[-3,672.63, 21,965.79]	[-10855.00, 14,148.30]	[-16449.03, 9,048.52]	[-11744.61, 13,532.70]	[212,497.47, 245,094.48]	[5.63, 216.29]
F-statistic						
Observations	1,425	1,425	1,425	1,425	1,425	19.73
Controls	None	DPLASSO	DPLASSO	DPLASSO	None	1,425
Enumerator FE	No	No	Yes	No	No	LASSO
Taxi Terminal FE	No	No	No	Yes	No	Yes

Robust standard errors in parenthesis.

Columns (1) - (4) report VSL estimates obtained using equation 11. Column (5) estimates VSL using equation 12, reporting the mean. Panel A uses empirical helmet effectiveness from Liu et al. (2008).

Panel B uses Ouellet and Kasantikul (2006). Column (6) reports VSL estimates using beliefs about deaths/10,000 riders to instrument for agents' beliefs about the risk reduction of a helmet. 35 observations where motorcycle taxi drivers pretended to be passengers are excluded.

Appendix

A Belief elicitation

The survey consisted of five modules. First, we collected tracking data and demographic information about respondents. Second, we obtained information about their use of motorcycle taxis, including weekly ridership volume, trip length, trip types, and reasons for using motorcycle taxis. Third, surveyors elicited the respondents' beliefs about their likelihood of dying in a motorcycle taxi accident. The first part of this module was identical across the treatment and control arms. We refer to variables measured during this portion of the module as baseline beliefs. During the second component, surveyors presented empirical risk estimates to respondents in the treatments arms and elicited posterior beliefs. Fourth, surveyors presented individuals in the treatment arms with the results of the appropriate study about helmet efficacy and then measured posterior beliefs about the effectiveness of helmets across the control and treatment arms. Finally, respondents completed the BDM game and received a helmet or mobile money payment. The third and fourth modules were skipped for those in the pure control group.

Prior to the first survey wave, surveyors completed a one week pilot that was focused on identifying a reliable survey module to measure beliefs about mortality risk. The final set of questions begins by providing the passengers with reference points to help them express and contextualize rare events. We informed the respondents that Jamhuri (Independence) Day occurs one out of every 365.25 days and that a leap day occurs one out of 1,461 days.

Surveyors next asked respondents how many fatal accidents occur per 100,000 motorcycle taxi trips. In addition to measuring the respondent's views about per trip risks, this question was designed to help the passengers think carefully about mortality risks by walking them through first thinking about how dangerous each trip is, then about their volume of trips. We then asked the respondent how many deaths there are per 10,000 passengers over the course of 1 year and 5 years in Nairobi.

We next asked the respondents about their own risk of suffering a fatal accident over the following 5 years. We did this in two steps. First, we asked them to select which range of risks seemed most accurate, for instance less than 1 in 10,000,000, between 1 in 10,000,000 and 1 in 1,000,000, etc. After selecting a range, we asked the passengers to respond with their belief within the range. Piloting revealed that this two step approach helped respondents answer accurately.

The respondents were then asked which information sources they used to construct their beliefs and whether they had been in a previous accident. We then presented those in the treatment groups with empirical estimates of their 5-year fatal accident risk as a function of their ridership, then elicited posterior beliefs. Empirical estimates were constructed by normalizing the number of recorded deaths by the NTSA the year before the study across Kenya by the total estimated number of motorcycle trips to get a per trip risk estimate. Then we used the respondent's reports of their ridership to estimate risk over the 5 year helmet lifespan.

The motorcycle taxi context was chosen in part because empirical risks are high enough over the lifespan of a helmet to limit problems associated with understanding small probabilities. For a passenger that takes 6 trips per week, the median in this sample, we estimate that there is over a 1 in 5,000 chance that they will die in a motorcycle taxi accident in the next 5 years absent a helmet. Given limitations of the data used to construct this estimate, this may be a lower bound on the true risk. While this is still a relatively low probability, it is among the most probable causes of death for young adults and thus minimizes the cognitive burden of understanding small probabilities compared to other settings.

The efficacy of helmets are much easier to understand and communicate, so we follow a simpler survey procedure. We first present the low treatment with the Liu et al. (2008) estimate that helmets reduce one's likelihood of dying by 42% and the high treatment with the Ouellet and Kasantikul (2006) result that helmets reduce mortality risk by 70%. The control and treatment groups were then asked for their own beliefs about how effectively helmets prevent death, expressed as the number of people that they believe would survive if all passengers wore a helmet per 100 deaths if no one wore a helmet.

B VSL Inference

The primary estimates of VSL report homoskedastic standard errors and weak IV robust confidence sets. This is supported by the latent utility model presented in section 3. Recall that the two-stage least squares model which identifies VSL is given by

$$v_i = \zeta_h + VSL\Delta r_i + X'_i\gamma_0 + \gamma_1 r_{0,i} + \epsilon_i$$

$$\Delta r_i = Z'_i\pi + X'_i\pi_c + \pi_r r_{0,i} + \nu_i$$

where ϵ_i denotes determinants of an agent's utility from a helmet which are observed to the individual but not the econometrician. In the case where $Z_i = T_i$, by randomization we know immediately that $\mathbb{E}[\epsilon_i^2|Z_i] = \mathbb{E}[\epsilon_i^2]$.

If $Z_i = (T'_i, r_{0,i} \cdot T'_i)'$, then errors may be heteroskedastic with respect to $r_{0,i}$. However, controls for $r_{0,i}$ will ensure homoskedasticity because $r_{0,i} \cdot T_i$ adds no information about ϵ_i^2 after accounting for $r_{0,i}$, so homoskedastic standard errors about VSL will still be accurate.

Formally, fix $r_{0,i}$. If $r_{0,i} \neq 0$, then T_i is uniquely determined by $r_{0,i}$ and $T_i \cdot r_{0,i}$ so $\mathbb{E}[\epsilon_i^2|r_{0,i} \cdot T_i, r_{0,i}] = \mathbb{E}[\epsilon_i^2|T_i, r_{0,i}] = \mathbb{E}[\epsilon_i^2|r_{0,i}]$ by the independence of T_i .

If $r_{0,i} = 0$, then T_i is not restricted by $r_{0,i} \cdot T_i$ so immediately $\mathbb{E}[\epsilon_i^2|r_{0,i} \cdot T_i, r_{0,i}] = \mathbb{E}[\epsilon_i^2|r_{0,i}]$. Denote $\sigma^2(r_{0,i}) = \mathbb{E}[\epsilon_i^2|r_{0,i}]$.

Keeping $r_{0,i}$ fixed, from the asymptotic variance for two-stage least squares

$$\begin{aligned} Avar(\sqrt{N}(\widehat{VSL} - VSL|r_{0,i})) &= \mathbb{E}[(\Delta\hat{r}_i)^2]^{-1} plim \frac{\Delta r Z(Z'Z)^{-1}Z'\epsilon\epsilon'Z(Z'Z)^{-1}Z'\Delta r}{N} \mathbb{E}[(\Delta\hat{r}_i)^2]^{-1} \\ &= \mathbb{E}[(\Delta\hat{r}_i)^2]^{-1} \sigma^2(r_{0,i}) \mathbb{E}[(\Delta\hat{r}_i)^2] \mathbb{E}[(\Delta\hat{r}_i)^2]^{-1} \\ &= \epsilon_i^2(r_{0,i}) \mathbb{E}[(\Delta\hat{r}_i)^2]^{-1} := V(r_{0,i}) \end{aligned}$$

where $\Delta\hat{r}_i = P_Z \Delta r_i$. Hence, by the Law of Total Variance,

$$\begin{aligned} Avar(\sqrt{N}(\widehat{VSL} - VSL)) &= E[V(r_{0,i})] + Var(\mathbb{E}[\sqrt{N}(\widehat{VSL} - VSL)|r_{0,i}]) \\ &= E[V(r_{0,i})] = \mathbb{E}[\epsilon_i^2] \mathbb{E}[(\Delta\hat{r}_i)^2]^{-1} \end{aligned}$$

So errors are homoskedastic under homogeneous VSL . Although there is some evidence of heterogeneous VSL in this sample, estimates suggest it is small relative to other determinants of utility. The standard deviation of willingness to pay is about 41. Even if the standard deviation of VSL were as large as the mean, the contribution to ϵ_i would be just $.022 \cdot .223$ (10% of the standard deviation of WTP), while the difference in perceived risk reduction from a helmet is well under .022 across treatment arms. Hence, the data suggest that standard errors are approximately homoskedastic because the contribution of demand for safety is second order compared to demand for other characteristics of helmets, consistent with the large demand intercept and small coefficient on mortality risk reduction.

Leveraging this approximation allows for efficient VSL estimation using two-stage least squares and lends itself to a well-established literature on weak instrument robust inference un-

der homoskedasticity. I use the Stata package *weakiv* to construct the confidence sets and use CLR inversion for over-identified models (Moreira, 2003) and AR inversion for just identified models (Anderson and Rubin, 1949). These confidence sets were selected for efficiency in the respective cases.

For robustness, I also report estimates constructed using continuously updating GMM (CUE) with heterodekastic robust standard errors in Appendix Table A5. I use GMM since 2SLS is less efficient under heteroskedasticity and to show that results are similar under different weighting of instruments. Weak instrument robust confidence sets are reported based on inversion of a CLR test statistic. Standard errors increase, but the primary conclusions are unchanged. The upper bound of confidence sets is about \$700 with the interacted IV and \$1,000 with the treatment only instrument with or without covariates.

C Local average VSLs identified by interacted and non-interacted instruments

This section derives the weighted average VSLs identified by the treatment only and interacted vectors of instruments. I begin with the case where r_{i0} and Δr_i are measured without systematic error. Since agents do not update beliefs about the unhelmeted risk of motorcycles when presented with information, their posterior beliefs about the likelihood that a helmet will save their life is approximately given by

$$\Delta r_i = H_i(T_i)r_{i0} + \nu_i$$

where r_{i0} is their prior about the mortality risk of motorcycles and H_i is the posterior belief about the efficacy of a helmet. Letting L_i indicate assignment to the low treatment arm and H_i to the high treatment arm, the relationship is therefore approximated by

$$\Delta r_i = \pi_c r_{i0} + \pi_L L_i \cdot r_{i0} + \pi_H H_i \cdot r_{i0} + \nu_i$$

One may also estimate a treatment only first stage

$$\Delta r_i = \pi_1 L_i + \pi_2 H_i + \nu'_i$$

And an individual's helmet valuation can be written as

$$v_i = \alpha + VSL_i \Delta r_i + \epsilon_i$$

I apply the Frisch-Waugh-Lovell theorem to partial out the constant in the treatment only first stage and r_{i0} from the interacted, denoting by \tilde{x}_i the partialled out variables.

Beginning with the treatment only IV, substitution of the known form for Δr_i into the structural equation for v_i yields

$$\begin{aligned} V\hat{SL}_{2SLS}^{TO} &= \frac{Cov(v_i, \pi_1\tilde{L}_i + \pi_2\tilde{H}_i)}{Cov(\Delta r_i, \pi_1\tilde{L}_i + \pi_2\tilde{H}_i)} \\ &= \frac{Cov(VSL_i\pi_L L_i \cdot r_{i0} + VSL_i\pi_H H_i \cdot r_{i0}, \pi_1\tilde{L}_i + \pi_2\tilde{H}_i)}{\pi_L\pi_1V(L_i)\mathbb{E}[r_{i0}] + \pi_H\pi_2V(H_i)\mathbb{E}[r_{i0}]} \\ &= \mathbb{E}\left[VSL_i\frac{r_{i0}(\pi_1\pi_L V(L_i) + \pi_2\pi_H V(H_i))}{\mathbb{E}[r_{i0}](\pi_1\pi_L V(L_i) + \pi_2\pi_H V(H_i))}\right] \\ &= \mathbb{E}\left[VSL_i\frac{r_{i0}}{\mathbb{E}[r_{i0}]}\right] \end{aligned}$$

Showing individuals are weighted linearly in priors, r_{i0} . In the interacted case,

$$\begin{aligned} V\hat{SL}_{2SLS}^{INT} &= \frac{Cov(v_i, \pi_L r_{i0} \cdot \tilde{L}_i + \pi_H r_{i0} \cdot \tilde{H}_i)}{Cov(\Delta r_i, \pi_L r_{i0} \cdot \tilde{L}_i + \pi_H r_{i0} \cdot \tilde{H}_i)} \\ &= \frac{Cov(VSL_i\pi_L L_i \cdot r_{i0} + VSL_i\pi_H H_i \cdot r_{i0}, \pi_L r_{i0} \cdot \tilde{L}_i + \pi_H r_{i0} \cdot \tilde{H}_i)}{\pi_L^2V(L_i)\mathbb{E}[r_{i0}^2] + \pi_H^2V(H_i)\mathbb{E}[r_{i0}^2]} \\ &= \mathbb{E}\left[VSL_i\frac{r_{i0}^2(\pi_L^2V(L_i) + \pi_H^2V(H_i))}{\mathbb{E}[r_{i0}^2](\pi_L^2V(L_i) + \pi_H^2V(H_i))}\right] \\ &= \mathbb{E}\left[VSL_i\frac{r_{i0}^2}{\mathbb{E}[r_{i0}^2]}\right] \end{aligned}$$

Thus the interacted instruments weight observations proportionally to priors squared, r_{i0}^2 .

Experimenter demand effects:

Next consider a model of experimenter demand effects where agents report $\Delta r_i^* = \pi_c r_{i0} + \pi_L \zeta_L L_i \cdot r_{i0} + \pi_H \zeta_H H_i \cdot r_{i0} + \nu_i \neq \Delta r_i$. This covers a case where agents over or under-report true changes in beliefs in response to the intervention. Valuations are still a product of true beliefs, Δr_i . In this case,

$$\begin{aligned} V\hat{SL}_{2SLS}^{TO} &= \mathbb{E}\left[VSL_i\frac{r_{i0}}{\mathbb{E}[r_{i0}]}\right] \cdot \frac{V(L_i)\pi_1\pi_L\zeta_L + V(H_i)\pi_2\pi_H\zeta_H}{V(L_i)\pi_1\pi_L\zeta_L^2 + V(H_i)\pi_2\pi_H\zeta_H^2} \\ V\hat{SL}_{2SLS}^{INT} &= \mathbb{E}\left[VSL_i\frac{r_{i0}^2}{\mathbb{E}[r_{i0}^2]}\right] \cdot \frac{V(L_i)\pi_L^2\zeta_L + V(H_i)\pi_H^2\zeta_H}{V(L_i)\pi_L^2\zeta_L^2 + V(H_i)\pi_H^2\zeta_H^2} \end{aligned}$$

Hence, $V\hat{SL}_{2SLS}^{TO} \neq V\hat{SL}_{2SLS}^{INT}$ unless $\zeta_L = \zeta_H$ since I reject $\pi_L = \pi_H$ in the data.

Misreported mortality risk:

Suppose that agents report $r_{i0}\zeta_i$ for some $\zeta_i > 0$ due to systematic measurement error but the true beliefs guiding valuations are r_{i0} . This captures challenges reporting small probabilities. One can show that

$$\begin{aligned} V\hat{SL}_{2SLS}^{TO} &= \mathbb{E} \left[VSL_i \frac{r_{i0}}{\mathbb{E}[r_{i0}\zeta_i]} \right] \\ V\hat{SL}_{2SLS}^{INT} &= \mathbb{E} \left[VSL_i \frac{r_{i0}^2 \zeta_i}{\mathbb{E}[r_{i0}^2 \zeta_i^2]} \right] \end{aligned}$$

Therefore one typically has $V\hat{SL}_{2SLS}^{TO} \neq V\hat{SL}_{2SLS}^{INT}$ unless ζ_i and r_{i0} are independent, for instance if $\zeta_i = c$. If individuals with low beliefs round down and those with high beliefs round up, $\mathbb{E}[\zeta_i r_{i0}] > 0$. If agents always round small probabilities up or down, $\mathbb{E}[\zeta_i r_{i0}] \neq 0$.

D Ethics Appendix

This appendix describes ethical considerations taken to prevent harming respondents.

The motorcycle helmets used in the study were manufactured by Boda Plus, a local Kenyan producer. I was referred to this brand by a road safety NGO, which believed that they were high quality. I verified that the helmets adhered to the Kenyan Bureau of Standards safety regulations before the study. Independent journalists have also concluded the Boda Plus manufactures high quality helmets. For instance, Weronika Strzyzynska reports in *The Guardian* article “Africa sees sharp rise in road traffic deaths as motorbike taxis boom” (2023) that “Kenyan company Boda Plus’s helmets meet high safety standards but can’t compete on price with low-quality imported ones.”

The first result presented to respondents, that helmets reduce mortality risk by 42% as reported in Liu et al. (2008), was selected because it is often presented to consumers by the Kenyan Government, NGOs and the UN Road Safety Fund. To give one example, the FIA Foundation cites this figure in their post “Helmets testing and awareness needed to curb Kenyan motorcycle deaths, says report supported by FIA Foundation.”

The second result presented to respondents is from Ouellet and Kasantikul (2006) which finds that helmeted passengers have about a 70% lower mortality rate in Thailand. Although Liu et al. (2008) is often cited, the underlying studies used in the meta analysis are primarily from high-income settings. In such cases, motorcycles drive faster and helmets are higher quality, so there may be different effectiveness versus LMICs. I selected the Ouellet and Kasantikul (2006) based on this logic, which was the most relevant LMIC that I could find. I also considered the studies from developing countries analyzed in Liu et al. (2008), but they are older or do not report mortality rates. Liu et al. (2008) did not identify any RCT evidence about helmet efficacy, possibly due to ethical concerns with a control group, which is why randomized evidence is not used. Liu et al. (2008) main inclusion criteria is that studies have a clear control group: the authors note that most papers fail to account for selection, and a large share use data only on those that end up hospitalized, which fails to account for victims that never enter a hospital. This places Ouellet and Kasantikul (2006), which attempts to reach all accident scenes rather than leveraging data on those admitted to hospitals, among the higher quality group of studies, although it still includes minimal corrections

for selection into helmet usage (e.g. by safer drivers). In short, there are limitations to both Liu et al. (2008) and Ouellet and Kasantikul (2006) such that neither is clearly dominant, yet both are in line with the quality of evidence that practitioners have recently used in health campaigns.

Ouellet and Kasantikul (2006) leverages observational accident data. This study was selected because the authors also estimate that helmets are about 50% effective in Los Angeles, aligning well with the Liu et al. (2008) estimate from high-income settings. The specific estimate from Ouellet and Kasantikul (2006) presented is based on helmets that stayed on. The authors note that when helmets were ejected, the mortality rate is higher. But, as presented in the study, this typically reflects incorrect helmet use and not effectiveness. Specifically, the study reports that about 6.8% of the unhelmeted riders died, versus 1.9% of the helmet-retained riders, resulting in the estimated efficacy of about 70% presented.

Respondents were presented with one study since interpreting two requires more time, and the variation helps to understand demand for safety. This should be thought of as two different draws from the distribution of how well helmets work, not one true and one false draw. The script for both studies informed respondents that estimates were from studies outside of Kenya so may not reflect Kenyan helmet effectiveness and that other studies may produce different results. The study received IRB approval from the University of California Committee on the Protection of Human Subjects and the Amref Health Africa IRB, which is based in Kenya.

An anonymous referee expressed the thoughtful concern that the intervention may have inadvertently harmed treated respondents by reducing their willingness to pay for helmets. I appreciate the referee's careful attention to the ethical dimensions of research. However, I interpret the findings differently. The observed decline in willingness to pay followed exposure to a well-regarded study on helmet effectiveness, which corrected overly optimistic priors. From this perspective, the intervention enabled more informed decision-making, leaving participants better off. In my view, withholding evidence on helmet effectiveness in order to encourage product uptake would raise its own ethical concerns. Moreover, the sample only included those not using helmets, so it did not cause anyone that was using a helmet to stop using one.

E Appendix Figures

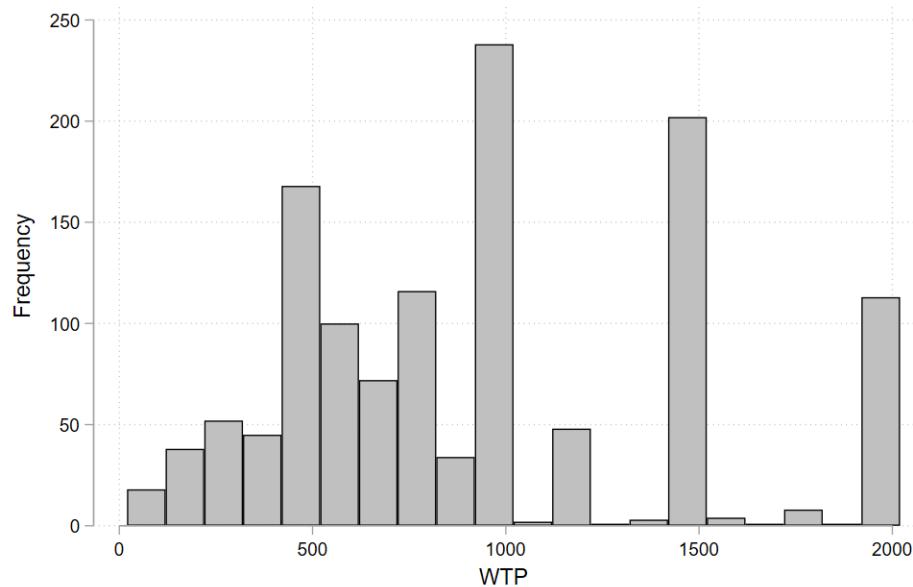
A1: Survey locations



Figure A1 plots survey locations over a map of Nairobi. Color denotes population, where no color denotes no residents. The 2019 census estimated a population of about 4.4 million within the city. Map data is from Open Street Map. Population data is from the LandScan Global 2021 data set produced by Oak Ridge National Laboratory.

A2: Distribution of helmet bids (Kenyan shillings)

(a) A. Histogram of bids, restricted axis



(b) B. Histogram of bids, full distribution

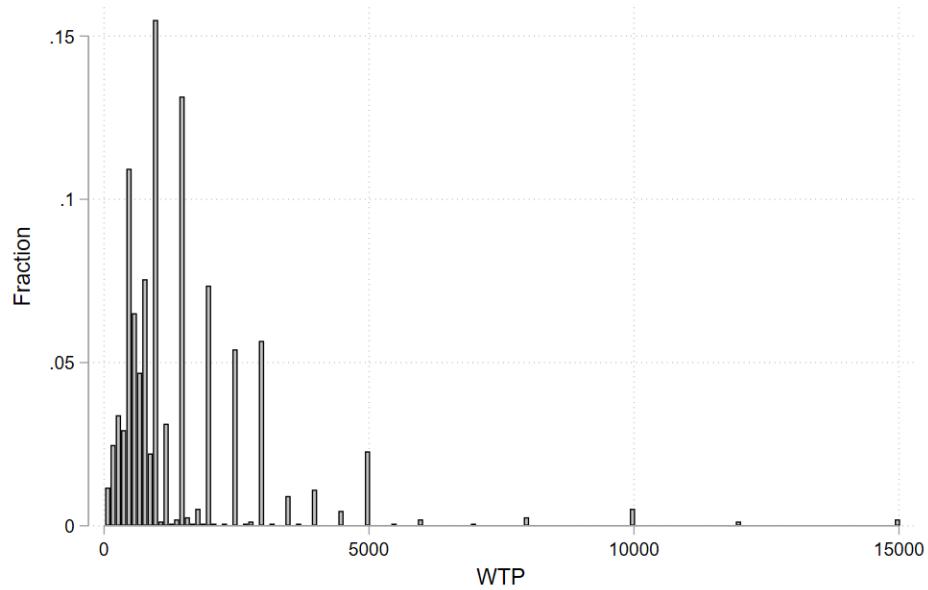


Figure A2 plots the distribution of willingness to pay for helmets in Kenyan shillings (Ksh). Panel A reports a histogram of bids, excluding outliers above Ksh 2,000 for clarity. Panel B reports the same histogram across the full distribution of bids. Figures present Ksh rather than USD to illustrate that respondents are more likely to select round numbers.

A3: Effect of study VSL on published benefit-cost ratios

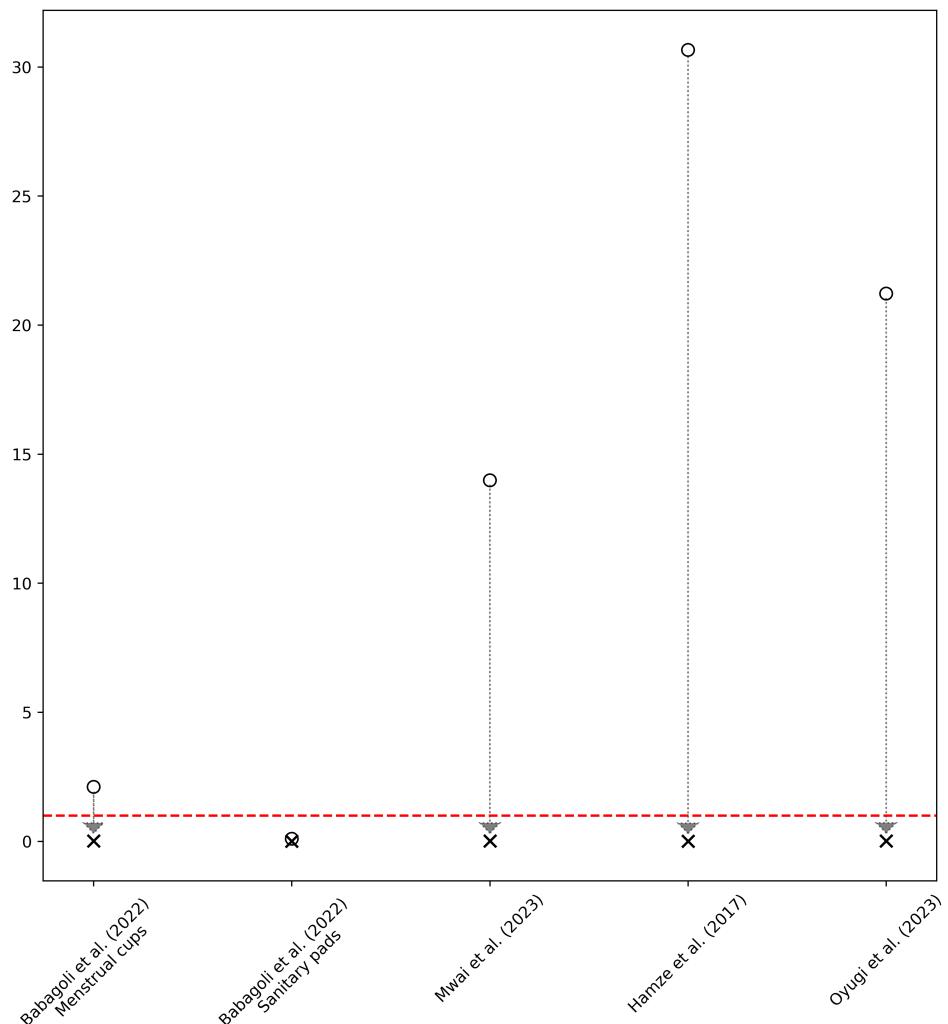
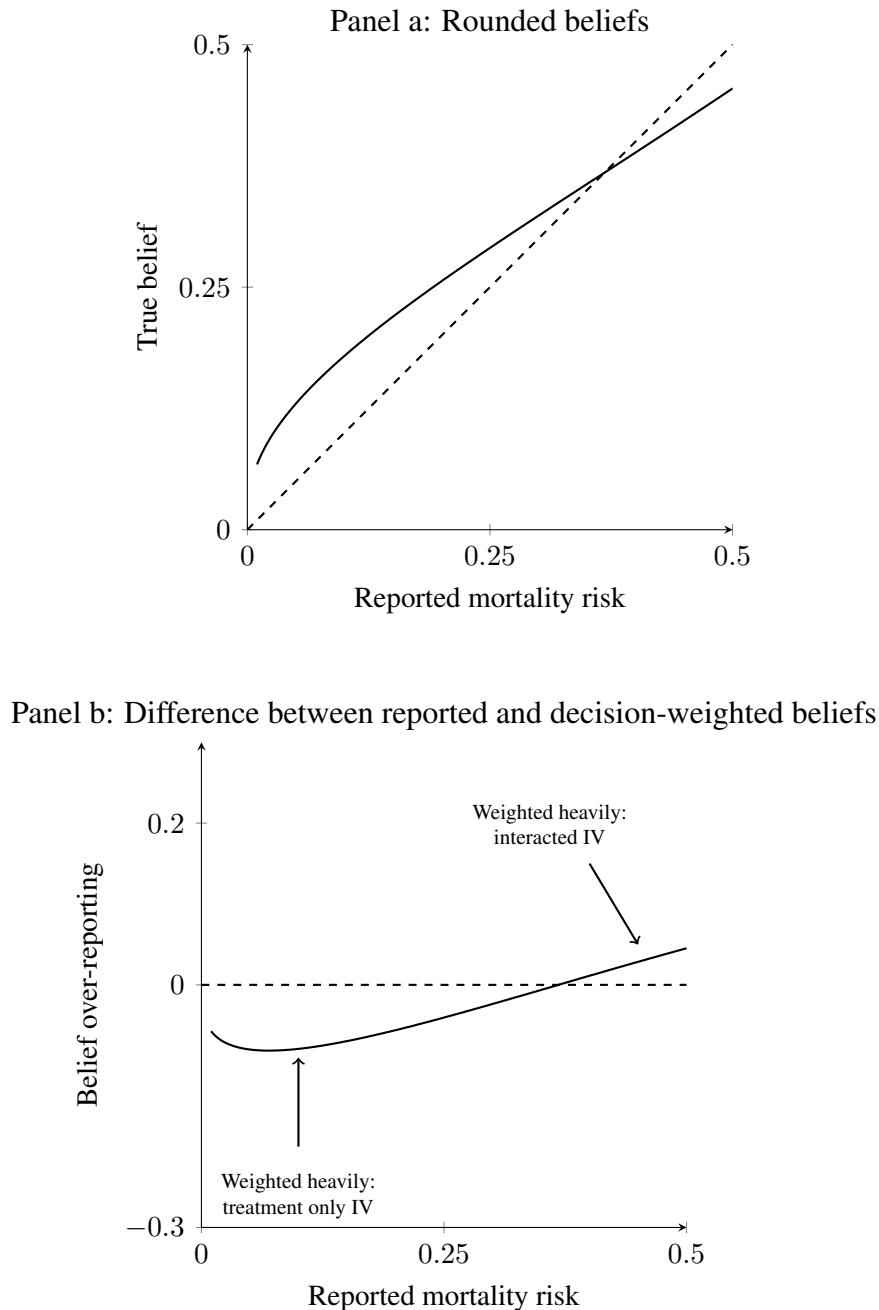


Figure A3 examines how the benefit-cost ratios (BCRs) of published benefit-cost analyses of Kenyan programs change when the study's original VSL or VSLY is replaced with the preferred estimates from this study. Hollow circles denote the original BCR estimate, and an x denotes the revised estimate. The first two estimates are from Babagoli et al. (2022). The third estimate is from Mwai et al. (2023), the fourth is from Hamze et al. (2017), and the final figure is from Oyugi et al. (2023). The horizontal red line is at $BCR = 1$, the threshold for benefits exceeding costs.

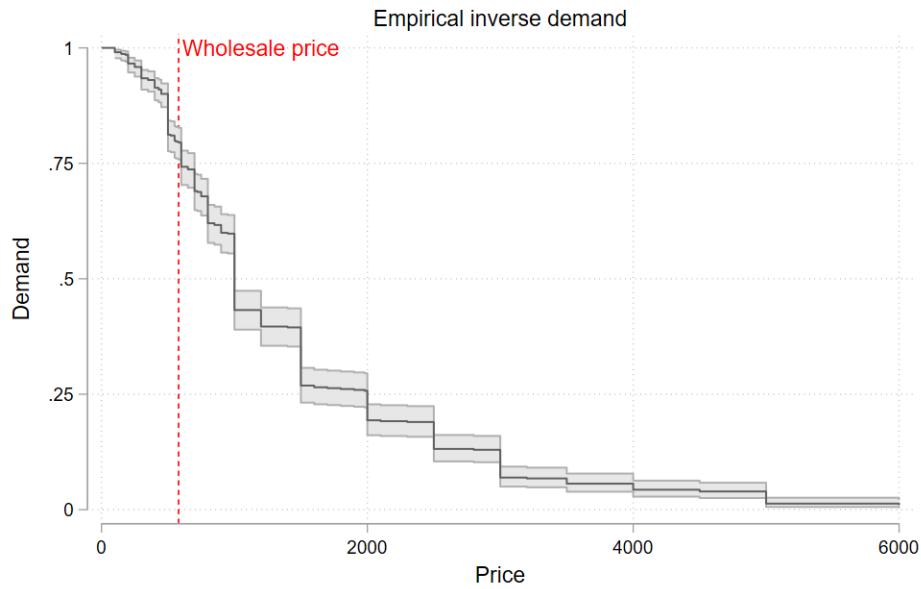
A4: Belief distortion under rounding



Appendix Figure A4 illustrates how agents with low beliefs rounding down and those with high beliefs rounding up would produce different estimates between the “treatment only” and “interacted instruments.” This figure plots a stylized case where agents that report they do not believe they face any risk of dying on a motorcycle act on a belief where they do face some risk, and those that report extremely high risks act on lower values. As shown in panel b, this would induce a positive correlation between misreporting and reported risk. The “interacted” instruments would therefore weight individuals with over-reported beliefs more heavily and overestimate VSL versus the “treatment only” instruments.

A5: Demand for helmets

(a) Inverse demand, control group



(b) Elasticity of demand, control group

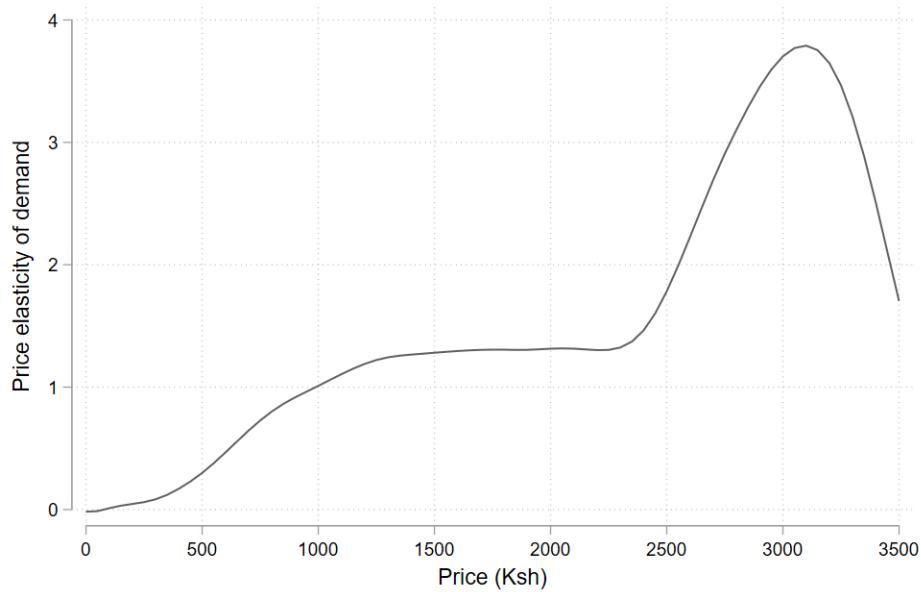


Figure A5 plots estimates of demand for helmets among observations in the control and pure control arms. The plot of demand includes a pointwise confidence interval. The plot of the elasticity of demand is based on a local polynomial estimation adapted from Berry et al. (2020). The vertical line denotes the wholesale price of helmets, which was Ksh 580 during the study. The figures exclude 35 observations for which surveyors reported motorcycle taxi drivers pretending to be passengers.

F Appendix Tables

A1: Summary statistics and balance: Motorcycle taxi use

	(1) Control Summary statistics	(2) Pure control - control	(3) Low treatment - Control	(4) High treatment - Control	(5) High - low treatment
Trips/week	7.531 [5.242]	1.178 (0.725)	0.196 (0.348)	-0.379 (0.358)	-0.593* (0.343)
Average trip length (minutes)	19.593 [12.567]	0.334 (1.538)	-0.704 (0.725)	-1.301* (0.745)	-0.583 (0.672)
$\mathbb{E}[\text{Deaths}/10,000 \text{ passengers}, 1 \text{ year}]$	371.584 [1,186.706]	NA	-46.930 (76.351)	-0.326 (78.569)	46.539 (76.478)
$\mathbb{E}[\text{Deaths}/10,000 \text{ passengers}, 5 \text{ years}]$	977.118 [2,479.942]	NA	-154.821 (192.351)	266.926 (197.928)	420.383** (205.063)
Confidence in beliefs	3.461 [0.699]	NA	0.034 (0.042)	0.011 (0.044)	-0.022 (0.041)
10000*Pr(Fatal accident, 5 years)	354.133 [973.710]	NA	-19.377 (56.011)	-40.568 (57.630)	-20.464 (52.741)
Previous accident	0.488 [0.500]	NA	-0.011 (0.032)	-0.030 (0.033)	-0.019 (0.032)
Know accident victim	0.947 [0.224]	NA	-0.022 (0.017)	-0.036** (0.017)	-0.014 (0.017)
Use motorcycle taxi: Commuting	0.781 [0.414]	0.027 (0.054)	-0.032 (0.027)	0.008 (0.028)	0.038 (0.027)
Shopping	0.420 [0.494]	-0.031 (0.064)	-0.033 (0.031)	-0.018 (0.032)	0.016 (0.031)
Leisure	0.261 [0.440]	0.057 (0.058)	0.074** (0.029)	0.036 (0.030)	-0.037 (0.029)
Deliveries	0.095 [0.294]	-0.012 (0.014)	0.015 (0.018)	-0.000 (0.018)	-0.015 (0.018)
Emergency/hospital transportation	0.095 [0.294]	-0.008 (0.014)	-0.009 (0.017)	-0.007 (0.017)	0.003 (0.017)
Reason for use: Speed	0.816 [0.388]	0.114** (0.045)	0.019 (0.024)	0.003 (0.025)	-0.017 (0.024)
Convenience	0.717 [0.451]	-0.005 (0.061)	0.005 (0.029)	-0.045 (0.030)	-0.049* (0.029)
Only option	0.168 [0.374]	0.007 (0.047)	-0.025 (0.023)	-0.010 (0.024)	0.015 (0.023)
Price	0.106 [0.308]	0.037 (0.038)	-0.008 (0.019)	-0.026 (0.019)	-0.018 (0.018)
Safety/Avoid dangerous areas	0.069 [0.253]	-0.017 (0.033)	-0.027* (0.015)	-0.010 (0.015)	0.017 (0.014)
Enjoyment	0.011 [0.105]	-0.012 (0.015)	-0.002 (0.007)	0.008 (0.007)	0.009 (0.007)
Risk information: Own experiences	0.819 [0.386]	NA	-0.036 (0.026)	-0.042 (0.027)	-0.007 (0.026)
Friends/family	0.454 [0.498]	NA	0.029 (0.032)	0.038 (0.033)	0.009 (0.032)
Social media	0.414 [0.493]	NA	-0.049 (0.031)	-0.034 (0.032)	0.016 (0.031)
Media	0.288 [0.453]	NA	0.014 (0.029)	-0.027 (0.029)	-0.042 (0.028)
Government	0.135 [0.342]	NA	0.015 (0.023)	0.026 (0.023)	0.011 (0.023)
N (exc. control in col. 2-4)	452	80	531	473	
p-value of joint orthogonality		0.008	0.136	0.221	0.221

Standard deviations in brackets. Standard errors in parenthesis.

Column (1) reports the mean and standard deviation of the indicated variable across the control group. Columns (2) - (5) report the difference in mean of each variable across treatment arms, where terms in parentheses are standard errors of this pairwise t-test. Missing values are imputed using the median across the full sample for the joint test.

A2: Elasticity of helmet demand with respect to subjective beliefs

	All observations		Information control	
	(1) Motorcycle risk	(2) Motorcycle risk	(3) Helmet effectiveness	(4) Helmet effectiveness
Elasticity	0.012 (0.007)	0.014 (0.007)	0.420 (0.161)	0.441 (0.159)
Controls	No	Yes	No	Yes
Observations	1,421	1,421	452	452

Robust standard errors in parenthesis.

Table A2 reports the elasticity of willingness to pay for a helmet with respect to priors about the likelihood of suffering a fatal accident in columns (1) - (2) and perceived efficacy of a helmet at preventing death among the information control arm in columns (3) - (4). Columns (2) and (4) include controls for the log of income, digit span recall, and log years of education. Estimates exclude 35 observations for which surveyors reported motorcycle taxi drivers pretending to be passengers.

A3: Value of a statistical life: Winsorized beliefs

Panel A: Full sample

	Raw WTP				Winsorized WTP	
	(1) Interacted	(2) Treatment only	(3) Interacted	(4) Treatment only	(5) Interacted	(6) Treatment only
VSL	534.88 (252.81)	671.97 (416.68)	601.70 (280.05)	653.89 (452.65)	282.14 (195.98)	436.77 (326.19)
Cragg-Donald F-stat	9.54	7.38	8.03	6.23	8.03	6.23
Weak IV Robust Confidence Set Inversion test	[64.21, 1,197.10] CLR	[-90.81, 2,026.93] CLR	[64.21, 1,197.10] CLR	[-187.78, 2,297.45] CLR	[-108.59, 772.92] CLR	[-194.93, 1,492.94] CLR
Observations Controls Enumerator FE	1,425 BL Risk Yes	1,425 BL Risk Yes	1,425 LASSO Yes	1,425 LASSO Yes	1,425 LASSO Yes	1,425 LASSO Yes

Panel B: Treated respondents only

	Raw WTP				Winsorized WTP	
	(1) Interacted	(2) Treatment only	(3) Interacted	(4) Treatment only	(5) Interacted	(6) Treatment only
VSL	676.59 (401.69)	969.44 (595.22)	690.84 (414.44)	1,022.45 (633.37)	323.45 (281.93)	535.40 (420.67)
Cragg-Donald F-stat	10.67	10.97	9.95	9.83	9.95	9.83
Weak IV Robust Confidence Set Inversion test	[-66.05, 1,785.13] CLR	[-49.48, 3,012.90] AR	[-72.23, 1,869.17] CLR	[-45.73, 3,365.04] AR	[-237.52, 1,067.29] CLR	[-239.47, 1,959.91] AR
Observations Controls Enumerator FE	982 BL Risk Yes	982 BL Risk Yes	982 LASSO Yes	982 LASSO Yes	982 LASSO Yes	982 LASSO Yes

Standard errors in parenthesis.

Panel A reports regressions using observations in the control group, low treatment group, and high treatment group. Panel B uses observations from the low and high treatment arms only. Columns labeled “Interacted” use a first stage consisting of treatment indicators and treatment indicators interacted with baseline motorcycle risk beliefs as instruments for the mortality risk reduction of a helmet. Columns labeled “Treatment only” use only treatment assignment as an instrument. All models control for baseline risk beliefs. Columns (5) - (6) report estimates where willingness to pay is winsorized at the 2nd and 98th percentiles. Over-identified models report weak instrument robust confidence sets constructed using inversion of a conditional likelihood ratio (Moreira, 2003). For just-identified models, weak instrument robust confidence sets are constructed by inverting an AR test (Anderson and Rubin, 1949). All models exclude 35 observations for which surveyors reported motorcycle taxi drivers pretending to be passengers. Beliefs about the mortality reduction from a helmet are winsorized at the 2nd and 98th percentiles.

A4: Robustness of VSL to alternative assumptions

Panel A: Change in planned future ridership

	Raw WTP				Winsorized WTP	
	(1) Interacted	(2) Treatment only	(3) Interacted	(4) Treatment only	(5) Interacted	(6) Treatment only
VSL	141.57 (64.91)	287.46 (187.29)	154.61 (65.85)	264.60 (195.50)	66.63 (48.09)	199.55 (144.70)
Cragg-Donald	45.37	10.37	44.74	9.52	44.75	9.52
F-stat						
Weak IV Robust	[13.66, 274.52]	[-82.08, 803.62]	[25.46, 290.05]	[-130.39, 811.47]	[-28.66, 164.14]	[-86.79, 578.53]
Confidence Set						
Inversion test	CLR	CLR	CLR	CLR	CLR	CLR
Observations	1,425	1,425	1,425	1,425	1,425	1,425
Controls	BL Risk	BL Risk	LASSO	LASSO	LASSO	LASSO

Panel B: Different beliefs about helmet lifespan

	Raw WTP				Winsorized WTP	
	(1) Interacted	(2) Treatment only	(3) Interacted	(4) Treatment only	(5) Interacted	(6) Treatment only
VSL	255.77 (89.62)	332.13 (245.11)	277.79 (89.07)	298.61 (246.66)	144.55 (64.47)	240.87 (182.05)
Cragg-Donald	41.49	10.38	42.24	10.06	42.24	10.06
F-stat						
Weak IV Robust	[83.72, 445.25]	[-151.15, 1,032.40]	[107.83, 466.78]	[-198.32, 988.80]	[19.06, 277.94]	[-118.93, 712.36]
Confidence Set						
Inversion test	CLR	CLR	CLR	CLR	CLR	CLR
Observations	1,425	1,425	1,425	1,425	1,425	1,425
Controls	BL Risk	BL Risk	LASSO	LASSO	LASSO	LASSO

Panel C: Weighted by 1/rides per week

	Raw WTP				Winsorized WTP	
	(1) Interacted	(2) Treatment only	(3) Interacted	(4) Treatment only	(5) Interacted	(6) Treatment only
VSL	272.75 (100.91)	96.56 (311.00)	286.78 (98.70)	86.40 (304.39)	145.99 (72.20)	110.97 (225.65)
Cragg-Donald	43.20	7.97	45.41	8.26	45.41	8.26
F-stat						
Weak IV Robust	[77.43, 482.10]	[-637.11, 865.77]	[96.84, 492.58]	[-632.16, 837.48]	[4.88, 293.38]	[-388.60, 671.41]
Confidence Set						
Inversion test	CLR	CLR	CLR	CLR	CLR	CLR
Observations	1,425	1,425	1,425	1,425	1,425	1,425
Controls	BL Risk	BL Risk	LASSO	LASSO	LASSO	LASSO

Standard errors in parenthesis.

Panel A accounts for increases in planned future ridership associated with receiving a helmet. For those that received a cash gift, this value is imputed by regressing planned future ridership on past ridership interacted with treatment assignment. Panel B uses respondents' stated beliefs about the lifespan of the helmet, rather than the manufacturer's suggestion. Panel C weights each observation by the inverse of motorcycle trips/week to account for selection into ridership. All models control for baseline beliefs and enumerate fixed effects. Excludes 35 observations where motorcycle taxi drivers pretended to be passengers.

A5: Value of a statistical life: GMM and IV probit estimates with robust standard errors

	GMM: Raw WTP		GMM: Winsorized WTP		IV probit	
	(1) Interacted	(2) Treatment only	(3) Interacted	(4) Treatment only	(5) Treatment only	(6) Treatment only
VSL	245.74 (141.87)	308.12 (238.51)	102.70 (76.48)	252.59 (193.40)	161.25 (118.35)	161.92 (118.42)
Price	NA	NA	NA	NA	-0.13 (0.03)	-0.13 (0.03)
Risk reduction	NA	NA	NA	NA	20.31 (10.17)	20.71 (10.40)
Cragg-Donald F-stat	8.16	10.96	8.16	10.96	NA	NA
Weak IV Robust Confidence Set	[-40.74, 689.51]	[-154.63, 978.63]	[-63.84, 323.75]	[-122.64, 750.35]	NA	NA
Inversion test	CLR	CLR	CLR	CLR	NA	NA
Observations	1,425	1,425	1,425	1,425	1,425	1,425
Controls	LASSO	LASSO	LASSO	LASSO	BL Risk	LASSO
Enumerator FE	Yes	Yes	Yes	Yes	Yes	Yes

Robust standard errors in parenthesis and weak instrument robust confidence sets in brackets.

Table A5 reports VSL estimates under heteroskedastic robust standard errors using continuous updating GMM (CUE) in columns (1)-(4) and IV probit, estimated via maximum likelihood, in columns (5)-(6). Weak instrument robust confidence sets are calculated using conditional likelihood ratio test inversion. Columns labeled “Interacted” use a first stage consisting of treatment indicators and treatment indicators interacted with baseline motorcycle risk beliefs as instruments for the mortality risk reduction of a helmet. Columns labeled “Treatment only” use only treatment assignment as an instrument. All models control for baseline risk beliefs. Columns (3) - (4) report estimates where willingness to pay is winsorized at the 2nd and 98th percentiles. IV probit estimates are estimated by fitting a regression of an indicator for receiving a helmet on the randomly drawn price (from the BDM game) and risk reduction, instrumented for with treatment assignment. Interacted instruments are not used for these estimates since the effective number of observations is smaller and the arguments supporting that instrument depend on linearity. Estimates exclude 35 observations for which surveyors reported motorcycle taxi drivers pretending to be passengers.

A6: Correlates between perceived mortality risk and demographics

	(1) Years of education	(2) Female	(3) log(Income)	(4) Age	(5) Above-median health	(6) 1(Children)
Coefficient	2.20 (8.15)	-95.56 (49.46)	43.83 (29.20)	-1.20 (2.37)	-57.59 (46.69)	53.09 (52.22)
Sample average	333.22	333.22	333.22	333.22	333.22	
Observations	1,419	1,427	1,221	1,425	1,427	1,427

Robust standard errors in parenthesis.

Table A6 reports coefficients of regressions of perceived baseline mortality risk on demographic covariates. As detailed in the results, the local average VSL identified by this study weights observations with higher perceived risk more heavily, so this table aims to understand whether the LATE is over a selected population. Column (1) considers years of education, (2) an indicator equal to 1 if the respondent is female, (3) the log of income, (4) respondent age, (5) an indicator for reporting above-median health, and (6) an indicator for having children. No controls are colluded since the aim is only to understand what covariates are correlated with perceived risk. Estimates exclude 35 observations for which surveyors reported motorcycle taxi drivers pretending to be passengers.