A New Experimental Method for Estimating Demand for Non-market Goods With an Application to the Value of a Statistical Life*

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April 26, 2025

Abstract

Economists often study preferences for non-market goods such as health. 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 efficacy of a product that reduces mortality risk (a helmet) and elicit product choice. This generates instruments allowing one to use subjective beliefs to estimate demand, rather than assuming rational expectations. This procedure does not require beliefs to be reported error-free but does require classical mismeasurement. I validate this assumption using insights from the local average treatment effects literature. The method estimates a VSL of $224, near the left tail of estimates. Existing methods for estimating VSL produce skewed results, driven by severe violations of rational expectations. These findings rationalize low demand for health products and suggest that billions of dollars of development aid is misallocated.

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 three anonymous referees at the *Quarterly Journal of Economics*, Paul Niehaus, Christopher Snyder, Kurt Lavetti, Patrick Kline, Jeremy Magruder, Kelsey Jack, Marco Gonzalez-Navarro, Elif Tasar, Catherine Che, Matteo Saccararola, the UC Berkeley development community and participants of the University of Chicago Causal Inference Conference. 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 and the University of California, Berkeley. AEA Trial Registry RCT ID: AEARCTR-0010101. Email: gkilleen@berkeley.edu

1 Introduction

Many goods that are important to welfare – such as clean air and water, health, privacy 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), consumer products often change exposure to or consumption of such goods (e.g. air or water 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. I also do not require that agents fully update beliefs in response to information, or that individuals report expectations without error. The primary identifying assumption is that measurement error in reported beliefs 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 types of belief misreporting such as rounding. 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 job choice, healthcare, environmental regulation and transportation. Policymakers directly use VSL in benefit-cost analyses, and NGOs rely on the parameter to allocate billions of dollars of 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 is used as a proxy for beliefs. This approach is biased if agents have beliefs about mortality risk that differ from statistical estimates (i.e. they do not have rational expectations). Moreover, estimates may be prone to omitted variable bias if agents select into dangerous behavior (Ashenfelter and Greenstone, 2004). VSL may also be heterogeneous, and many studies estimate it over selected populations where there is quasi-random variation in risk (Greenberg et al., 2021). Consistent with potential bias, estimates of VSL vary by orders of magnitude: in Africa, they range from about \$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. Motorcycle taxi passengers 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). I then elicited respondents' beliefs about the likelihood that a helmet would save their life and estimated helmet

[1] For 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.

[2] Stated 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.

demand using a Becker et al. (BDM, 1964) mechanism. 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 in a minimally selected population. Traffic accidents are a leading cause of death in Kenya and 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 urban Kenyans have low average demand for safety, with a VSL of PPP USD $224 (5% of annual income).[3] Exposure to the results of Liu et al. (2008) reduces average beliefs about the likelihood that a helmet will save an agent's life (a product of accident risk and helmet efficacy) from 2.22% to 1.41%. This reduces willingness to pay for a helmet by $3. I recover informative bounds on VSL, rejecting values below $34 and above $429 with 95% confidence.

The low VSL estimates are consistent with theory because demand for risk reduction is inversely proportional to the marginal utility of consumption. Therefore VSL should rise with income as the opportunity cost of funds falls. The difference between my estimate and a $700,000 VSL reported by León and Miguel (2017), among a sample with incomes near $75,000, can be fully explained by published estimates of utility curvature.[4] I also find heterogeneity consistent with theory: VSL is $500 higher among wealthier individuals, with an estimated income elasticity of 1.14, and higher among healthier respondents.

Is it plausible that the VSL in this sample is far lower than annual income? This finding aligns closely with results finding low demand for preventative health in LMICs. For instance, Cohen and Dupas (2010) find that only 40% of pregnant women purchase insecticide treated 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 in-kind incentives

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

[4] I 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.

substantially increases uptake of free childhood vaccinations. Dupas and Miguel (2017) documents that similar results hold consistently across products.

The second component of this study validates that beliefs are measured without non-classical error. Two leading threats are experimenter demand effects and systematically misreported beliefs. Since the concern is a gap between reported and decision-relevant subjective beliefs – not accuracy relative to empirical truth – tools like bunching tests are uninformative. But two design features help test for bias. First, the two treatment arms provide signals of varying intensity. If beliefs measures 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. I show that any combination of arms yields similar estimates. Moreover, willingness to pay falls in response to the lower signal, providing revealed preference evidence of belief updating.

The other 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 proportionally to priors squared, yielding similar results. The stability of estimates suggests that estimates are not substantially affected by misreporting correlated with true beliefs, which I show includes most cases where agents systematically under or over-report probabilities (e.g. rounding or difficulty articulating small probabilities).

In additional checks, I demonstrate that helmet demand is similar between the control and a pure control group not asked questions about mortality risk, that there is no detectable heterogeneity with respect to education or performance on a digit span recall test, and that distinct measures of beliefs are highly correlated. Treated respondents were also presented with information about the risks of motorcycles and efficacy of helmets but only reported updating beliefs about helmets. VSL estimates are also similar if I instrument for beliefs using an observational belief shifter: an indicator for knowing a motorcycle accident victim. The BDM game was implemented over a high-stakes and realistic decision since evidence indicates that the mechanism is reliable in similar settings (Berry et al., 2020).

The third part of the paper tests if rational expectations, a core assumption required by most methods for estimating VSL, holds in this context. The data support the view that beliefs depart systematically 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 their probability of suffering a fatal accident to be high, but those that ride motorcycles frequently do not perceive a higher 5-year mortality risk. Respondents also report 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. In 4 of 5 cases, they cause benefit-cost ratios 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 gains, producing misallocation.

The findings should not be interpreted as a call to eliminate health funding for LMICs. They do suggest that redirecting some preventative health investments towards poverty alleviation may better reflect recipients' preferences. But theory does not support applying the results to allocate external health funding (e.g. from the WHO) since the low VSL likely reflects a high opportunity cost of funds. When funds are earmarked for health and the decision is over where to send them, no LMIC consumer faces a health-consumption trade-off. This challenges the application of VSL in recent studies (e.g. Agrawal et al., 2023). Moreover, VSL reflects demand for *marginal* reductions in risk, the appropriate parameter for most policies that modestly reduce risk. But willingness to pay for non-marginal reductions (e.g. antiretrovirals) may be higher since the transfers needed to compensate agents for large risks likely lower the marginal value of consumption.

This paper advances the VSL literature by introducing and validating a method for estimating the parameter that does not rely on rational expectations and is robust to selection into risk. To my knowledge, this is among the first studies to produce a precise revealed preference estimate of VSL without assuming rational expectations and to test the assumption.[5] 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 how deviations from rational expectations affect demand for clean air. 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 and my design ensures that results are not biased by cash on hand (Redfern et al., 2019).

This methodology could be applied to non-market amenities beyond safety. Goods such as time, privacy, reputation, and environmental quality are economically important but difficult to value, especially in LMICs (Campbell and Brown, 2003; Greenstone and Jack, 2015). Policy decisions often rely on estimates that are not incentivized (e.g. contingent valuation surveys) when the variation needed for revealed preference estimates is unavailable (Mendelsohn and Olmstead, 2009). Revealed preference methods also tend to assume rational expectations, which contradicts recent evidence (Baylis et al., 2023). The framework I introduce could estimate demand for these amenities by applying it to products such as ride sharing services, cyber security software, 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 beliefs in some cases (León and Miguel, 2017).

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

[6] Kremer 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 wealthy populations.

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

The use of motorcycle taxis is widespread and growing in East Africa, attributed to low costs and road congestion. In Kenya, there are an estimated 2.4 million drivers providing taxi services, combining for about 22 million trips per day.[7] Transportation is notoriously dangerous. Data from the National Transport and Safety Authority (NTSA) reports that 1,722 motorcycle drivers and passengers died 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 Kenyans aged 5-70.[8]

Despite the high risks of motorcycles, helmet use among taxi passengers is rare. Bachani et al. (2017) measure passenger helmet use at 3%. At that time, helmeted passengers typically borrowed one from the driver, which became rare after the COVID-19 pandemic due to hygiene concerns and may have further suppressed passenger helmet use. The low use of helmets suggests that demand may be low. However, the availability of effective helmets affordable to Kenyans is recent. The *FIA Foundation* began offering helmets in Kenya in 2021, about a year before the experiment was implemented, and a local producer began manufacturing helmets near 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.

[7] Fred Matiang'i, "The urgency of bodaboda reforms", *Nation.Africa*, 2022.
[8] "New initiative to tackle road crash deaths in Kenya," *World Health Organization*.

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. 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 estimates 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.

Unlike the extensive margin, ridership volume may be selected. The sampling was designed so that one's probability of being sampled is a known function of ridership, allowing for estimates that use re-weighting to test for selection. Section 5.1 presents details and shows that re-weighting is inconsequential, suggesting that substantial selection on safety preferences is unlikely. The VSL of motorcycle taxi users is also policy relevant even if the population is not representative.

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, reaching 13 of Nairobi's 17 constituencies. The stands were selected for broad geographic and demographic coverage. However, areas with high crime rates were excluded for safety. Survey locations are plotted over 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. Consumers that did not report regular access to a mo-

[9]There 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 ($22 \approx 53 \cdot 0.5 \cdot \frac{7.5}{7} \cdot 0.78$).

torcycle 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 participate lacked time.[10]

Demographic information presented in Table 1 shows that the study reached a broad sample. This suggests that the VSL estimates are informative when considering other East African samples. Income aligns closely with representative samples of the population. The mean annual income of control respondents 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. Education is relatively high, with an average of about 12 years of schooling completed.

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 motorcycle taxis and the effectiveness of helmets at preventing death. The control arms received no 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 group 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 affects demand. Pure control observations are excluded from VSL estimates since beliefs were not elicited. In practice, there are no differences in demand between the pure control and control groups.

The treatments presented different studies of helmet effectiveness. Respondents first re-

[10]Surveyors 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.

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

ceived 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" group were presented the results of Liu et al. (2008) which estimates that helmets reduce one's likelihood 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 to present the information, so only the study and results vary across arms. The sources of information and the fact that the studies of helmet efficacy were conducted outside of Kenya were disclosed.

Both studies of helmet efficacy are from peer reviewed publications. There is a strong consensus that motorcycle helmets are effective, but there is uncertainty about exactly how well they work, particularly across contexts and helmet types. The 42% figure presented in Liu et al. (2008) is often presented by NGOs operating in Kenya, motivating its use in this study. But their meta analysis primarily considers studies of high-income settings where drivings 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 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 accept 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 leveraged willingness to accept, rather than pay, to

[12] I 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.

[13] An ethics appendix details steps taken by the study to ensure that no respondents were harmed.

ensure that cash on hand did not determine demand.

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 policy efforts to communicate that affordable helmets were available. 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 accept 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.[14]

In practice, helmet valuations often exceed the wholesale price.[15] Appendix Figure A2 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 ($p < 0.1$ and $p < .05$ if baseline risk is above 1 in 100,000), validating that valuations affect real outcomes. I also estimate VSL using an IV probit regression of an indicator for receiving a helmet on the BDM draw (price) and risk reduction (instrumented for with treatment) as a robustness check. This is not influenced by variation in valuations above the maximum draw and produces similar results. 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 and respondents were assigned to the remaining arms with equal likelihood.

[14]In principle, one could infer the upper bound 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. During the pilot, field officers verified that respondents could not state the upper bound. The maximum was revealed when requested, which happened once.

[15]High 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.

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 $i \in \{1, \ldots, \infty\}$. 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 beliefs, which I test empirically.

Each consumer has a prior about the probability of dying with 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 $\theta_H \sim Binomial\left(a_{EH} + b_{EH}, a_{EH}/(a_{EH} + b_{EH})\right)$. 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, 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 priors and the signal. Modeling belief formation helps think about what information sources are likely to violate rational expectations. If agents learn through their experiences or their social network then their beliefs will likely vary from empirical estimates. Learning from one's experiences is subject to survivor bias and learning through one's social network 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 likelihood of an accident and the effectiveness of a helmet conditional on suffering an accident. Therefore, suppose that the agent has a prior about the probability per trip of getting into 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 parameters of this 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

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

$[1 - Pr(A = 1|\mathcal{I}_i)]^{n_i}$ 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 3.

I assume that helmeted individuals involved in an accident that would otherwise be fatal are deterred from continuing to use motorcycles.[16] 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

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

which is the unhelmeted risk times the perceived chance that a helmet fails to prevent death. The subscript u refers to the agent's perceived unhelmeted risk and h to their helmeted risk. Let p_i denote the price of a helmet. The present value of the agent's utility from being alive is given by $u_a(x_i)$ where x_i is a vector of characteristics, such as income and health. Denote their flow utility of consumption by $u(c_i; x_i)$ and denote the expected utility from not being alive by $u_d(x_i)$.[17] The agent's expected utility from purchasing a helmet is

$$U_{ih}(z_i) = \zeta_h + [1 - r_{ih}(z_i)] \cdot u_a(x_i) - p_i \cdot u'(c_i; x_i) + r_{ih}(z_i) \cdot u_d(x_i) + \epsilon_{ih} \tag{2}$$

The parameter ζ_h captures average utility from characteristics of helmets other than safety, and ϵ_{ih} denotes idiosyncratic variation in utility. Without purchasing a helmet, expected utility is

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

[16] Absent this assumption, $r_{ih0} = \frac{H_{i0} \cdot r_{in}}{1 - H_{i0} \cdot r_{in}}$. Results are similar using this calculation since the probability of suffering two serious accidents is low.

[17] This may not be zero, for instance if the agent believes in an afterlife.

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

$$VSL_i \equiv \frac{dp_i}{d\Delta r_i} = \left(\frac{\partial U_{i,h-u}}{\partial \Delta r_i} \right) \Big/ \left(\frac{\partial U_{i,h-u}}{\partial p_i} \right) = \frac{u_a(x_i) - u_d(x_i)}{u'(c_i; x_i)} \tag{3}$$

This predicts that VSL will vary with income. Theory suggests $u_a(x_i)$ will increase and $u'(c_i; x_i)$ will fall as agents become richer. The falling marginal utility of consumption is likely to be particularly important since it would drop linearly with income under log utility and faster under other common utility representations. This reflects the fact that the opportunity cost of funds used to invest in safety is lower when agents are wealthier, so their VSL will be higher even if they obtain no more utility from averting a death.

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. Let Δr_i^* be a statistical estimate of the likelihood that a helmet will save an individual's life. A common set of identifying assumptions in the VSL literature (but not this paper) is

$$\text{Full Information Rational Expectations:} \quad \Delta r_i = \Delta r_i^* \quad \forall i \tag{4a}$$

$$\text{Exogeneity and Logit Errors:} \quad \epsilon_{ih}, \epsilon_{iu} \sim_{iid} EV1 \tag{4b}$$

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

$$Pr(y_i = 1) = \Lambda\left(\zeta_h + \beta \cdot \Delta r_i^* - \alpha \cdot p_i\right) \tag{5}$$

Λ is the Logistic CDF and so α, β (and $VSL = \frac{\beta}{\alpha}$) are identified.[18]

Assumption 4a 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 those estimates match beliefs in expectation. This assumption is plausible when consumers have the same information as the researcher, but fails if they possess private information (e.g. personal experiences) or lack access to the data used by the researcher. Rational expectations has been assumed despite these limitations because of a lack of an incentive-compatible alternative (prior to this paper) capable of producing meaningful estimates since beliefs are noisily measured.[19]

Assumption 4b 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 rely on 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 a natural way to identify VSL since presenting agents with information about hel-

[18]In practice studies often estimate mixed logit models, but the assumptions are similar.

[19]For 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."

met efficacy will change the expected utility they obtain from the life saving potential of a helmet (unless their priors align with the signal or are degenerate). Updating beliefs about helmet efficacy produces 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

$$\text{Exclusion Restriction:} \quad Cov\left(z_i, \epsilon_i\right) = 0 \tag{6a}$$

$$\text{Instrument Relevance:} \quad Cov(\Delta r_i, z_i) \neq 0 \tag{6b}$$

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

$$\begin{aligned}
\epsilon_{in} &= \zeta_h + \beta \cdot \Delta r_i(z_i) - \alpha \cdot v_i + \epsilon_{ih} \\
\alpha v_i &= \zeta_h + \beta \cdot \Delta r_i(z_i) + \epsilon_i \\
v_i &= \frac{1}{\alpha}\zeta_h + \frac{\beta}{\alpha} \cdot \Delta r_i(z_i) + \frac{1}{\alpha}\epsilon_i = \zeta_h' + VSL \cdot \Delta r_i(z_i) + \epsilon_i' \qquad (7)
\end{aligned}$$

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 randomly assigned, then denoting agents' true beliefs by Δr_i^t, assumption 6a is equivalent to

$$\text{Classical Measurement Error:} \quad \mathbb{E}[\Delta r_i - \Delta r_i^t] = 0 \tag{6a$'$}$$

This motivates the focus on testing for non-classical error in the empirical application.

The model shows that relevance is 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 $Cov(\Delta r_i, z_i) \neq 0$.

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. This highlights the need to estimate VSL in contexts where risk is sufficiently high that results generalize to targeted policies.

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.

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.[20] The survey elicits 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). Then respondents were then asked to list a more precise belief within the interval. This two-step approach reduced rounding in piloting.[21]

Table 1 summarizes demographics of the sample and demonstrates balance across treat-

[20]The instrument is appended to the end of the PAP amendment.

[21]There 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.

ments. Appendix Table A1 is similar, but it examines motorcycle taxi use and beliefs. These outcomes are generally balanced across arms, although there is some imbalance between the pure control (which had a small number of respondents) and other arms.[22]

4.2 Experimental VSL estimation

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

$$\begin{aligned} v_i &= \zeta_h' + VSL\Delta r_i + X_i'\gamma_0 + \gamma_1 r_{0,i} + \epsilon_i' \\ \Delta r_i &= Z_i'\pi + X_i'\pi_c + \pi_r r_{0,i} + \nu_i \end{aligned} \tag{9}$$

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, 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. This set of instruments is similar to that studied in Abadie et al. (2023), which shows that similar instruments improve asymptotic mean squared error.[23]

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.[24]

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

[22] Non-response rates are also similar across treatment arms and that response rates are high.

[23] I pre-specified two sets of IVs. The original pre-analysis plan (PAP) specified 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 $r_{0,i}$.

[24] Due 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.

rule out confounds. The primary tables report homoskedastic standard errors and weak instrument robust 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 implement two observational revealed preference approaches to estimate VSL similar to recent publications. First, I estimate

$$v_i = \zeta_h + VSL\Delta r_i^* + X_i'\gamma_0 + \epsilon_i \tag{10}$$

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

$$VSL_i = \frac{v_i}{\Delta r_i^*} \tag{11}$$

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 in the context of health or safety products, where assumptions are plausible. This allows one to estimate VSL in settings where variation in risk across the sample is small.

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 likelihood of a fatal motorcycle accident fall within plausible ranges and are strongly correlated across measures, supporting their use in estimating VSL. The mean reported 5-year mortality risk is 0.034, with a median of 0.001.[25] For comparison, the estimated median empirical risk is about 1 in 4,000, though deaths may be under-reported. Beliefs about deaths per 10,000 passengers and one's own risk are highly correlated ($R^2 > 0.1$), though respondents view themselves as less at risk than average.

Beliefs about 1-year and 5-year mortality risk are also strongly correlated ($R^2 = 0.57$). 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.[26] Respondents' 5-year risk beliefs are also correlated 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 agent's belief that a motorcycle helmet will save their life.

I first presented respondents with estimates of their unhelmeted mortality risk constructed using data from the NTSA, which has a low level of trust. Consistent with mistrust, respondents reported no change in beliefs in response to this information as shown in columns 1 and 2. There is not a significant difference between either treatment arm, which received identical information, and the control group. This suggests agents did not feel compelled by experimenter demand effects to misreport changes in beliefs.

Respondents did update beliefs about the effectiveness of helmets based on the studies presented in the treatments, the primary variation used by the interacted instrument. Respondents *overestimated* the effectiveness of helmets at baseline and updated their beliefs downward when exposed to information. Table 2 reports treatment effects on beliefs about the effectiveness of helmets in columns 3 and 4. The mean belief about helmet effective-

[25] VSL estimates are robust to excluding implausible responses or winsorizing beliefs.
[26] Results are unchanged if these are excluded.

ness reported in the control group was 79%, and many respondents stated beliefs over 90%. Exposure to the 70% (high) and 42% (low) effectiveness estimates reduced beliefs to 75% and 65%, respectively (columns 3–4), with the treatment gap significant at the 1% level.[27]

The change in beliefs about helmet effectiveness was sufficient to shift the perceived probability that a helmet will save a respondent's life (the product of the prior two variables). The dependent variable in Columns 5 - 6 of Table 2 equals 10,000 times beliefs. This is 80 units (36%) lower on average in the low treatment versus control group, significant at the 1% level. The high treatment group does not differ significantly from control (due to similar priors), but point estimates move in the expected direction. These results reject the null hypothesis that information had no effect on beliefs, confirming that treatment assignment is a relevant instrument for perceived mortality risk reduction.

The value of a statistical life

Primary estimates of the value of a statistical life are reported in Table 3. The preferred estimate in column 3 is \$224, indicating low demand for safety. 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; 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 5 of panels a and b: an indicator for knowing a motorcycle accident victim (conditioning on taxi terminal fixed effects). Estimates are similar to those obtained from random variation. Column 6 uses both victim exposure and treatment assignment as IVs. A J-test fails to reject the equality of estimates.

Appendix Table A4 demonstrates that results hold if beliefs about the probability that

[27] Under 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.

a helmet will save one's life are winsorized. Appendix Table A3 shows that the results are robust to changes in planned future ridership (panel a) or subjective beliefs about helmet lifespan (panel b). Panel c suggests that results do not disproportionately weight individuals that select into high ridership because of low demand for safety.[28] Appendix Table A2 shows that IV probit results regressing receipt of a helmet on price (the BDM draw) and perceived risk reduction (instrumented with treatment assignment) also produces similar results.

Although demand for safety is low, theory suggests these estimates are consistent with much higher VSLs in richer settings, where the opportunity cost of funds is lower. Comparing results from this paper to estimates across contexts reveals that my estimates are of a similar order of magnitude to those from similar income levels, but much smaller than those from wealthier samples. Some differences may be due to methodology, as discussed in this paper, but this exercise helps to confirm the first order importance of income.

Figure 1 plots point estimates and standard errors from all LMIC revealed preference estimates of VSL that I am aware of.[29] I also include an estimate from Greenberg et al. (2021), which examines a population of US soldiers using related methods, for comparison. Estimates from this paper are lower than most existing values, but more similar to those from populations with similar incomes. For instance, Kremer et al. (2011) estimate a value 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. Ito and Zhang (2020) and León and Miguel (2017) find much higher VSLs among wealthier populations.

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. This provides an upper bound on the implied curvature if quality of life improves with income. Under a CRRA utility function, the

[28]Estimates exclude observations from the second day of data collection, as specified in the PAP amendment, because 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.

[29]This includes Kremer et al. (2011), León and Miguel (2017), Berry et al. (2020), Ito and Zhang (2020), and Shrestha (2020). I use the consumer price index to adjust estimates for inflation.

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 $\theta < 3$.[30] 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 also drive gaps in estimates, this exercise demonstrates that large cross-country variation in VSL can be rationalized by differences in the marginal utility of consumption.

Is an average VSL below annual income plausible?

The average VSL estimated in this study is only 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, especially if individuals believe in an afterlife (87% of Kenyans)[31] or have fatalistic views (León and Miguel, 2017). Estimates also align with a consistent finding that demand for effective preventive health products in LMICs is low (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 an insecticide-treated 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).

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[30] 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 $\theta \approx \frac{\log(700{,}000/224)}{\log(75{,}000/4{,}750)} = 2.91$

[31] Gallup International Association. (2022). *More Prone to Believe in God than Identify as Religious. More Likely to Believe in Heaven than in Hell.*

Is low demand simply due to capital constraints or inaccurate information? Dupas and Miguel (2017) document that demand for preventative health products remains well below 100% when such constraints are alleviated, consistent with the VSL estimated.

When does theory support the use of LMIC VSL estimates?

These results should not be interpreted as a call to eliminate health funding to LMICs because the use of the estimates is not theoretically supported to value all health investments. The estimates are only valid for decisions where a population faces a mortality risk-consumption trade-off. This informs many important decisions such as those discussed in the next section. But it does not apply when funds are earmarked for health.

To illustrate this, suppose that there are homogeneous high and low-income consumers indexed by h and 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 w.p. p. The cost is $c \cdot N$ paid for by N high-income consumers. The global welfare gain, in utils, is

$$W = N \cdot p \cdot \beta_\ell - N \cdot c \cdot \alpha_h \tag{12}$$

This is positive if $N \cdot p \cdot \frac{\alpha_\ell}{\alpha_h} \cdot VSL_\ell > N \cdot c$. Thus, evaluating policies by estimating lives saved, valuing them at the low-income country's VSL, and comparing benefits to costs will understate welfare gains by $\frac{\alpha_\ell}{\alpha_h}$. This 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 program costs, low-income consumers face no consumption tradeoff.[32]

One example where this distinction matters is vaccine policy. For instance, Ahuja et al. (2021) examine how changes in vaccine procurement could reduce deaths, valuing lives saved by VSLs adjusted to income levels. Agrawal et al. (2023) argue that more equitable COVID-19 vaccine allocation may have saved 670,000 lives but reduced welfare due to VSL differences. But LMICs relied on donated COVID-19 vaccines (Ahuja et al., 2021).

[32] If high-income consumers choose between donations to health or consumption programs, then there is an LMIC health-consumption trade-off so applying the LMIC VSL is appropriate.

Hence, these studies likely underestimate the welfare gains of lives saved in poor countries.

In addition, applying VSL to value large changes in risk – such as HIV treatment that prevents near certain death – may be inaccurate. Preferences for mortality risk may be convex if transfers large enough to offset major risks affect the marginal value of consumption. Thus, the estimated VSL should be applied only to policies involving modest mortality risks where consumers face a consumption-risk tradeoff.

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.

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.[33] I re-calculate benefit-cost ratios (BCRs) using my VSL estimates 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.

Results also suggest that development assistance is misallocated. GiveWell, an NGO that has matched over $1 billion to charities, weights averting the death of someone aged 15-49 104 times doubling their consumption for a year.[34] These weights directly determine which charities are recommended. The VSL estimated in this paper suggests that these weights under-value the benefits of consumption gains by orders of magnitude, suggesting that allocating more funding towards programs like cash transfers could improve 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 particular interest for transportation policy. To assess broader applicability, I examine VSL heterogeneity on observables in Table 4. Results align closely with theo-

[33] These include Babagoli et al. (2022) (menstrual cups and sanitary pads), Mwai et al. (2023) (primary health investment), Hamze et al. (2017) (cleft lip and palate repair), and Oyugi et al. (2023) (maternity care).

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

retical predictions, but the magnitudes are modest, suggesting that the conclusion – that willingness to pay for safety is low – is unlikely to change across similar populations.

Column 1 shows that VSL is similar among respondents below and above the median age. This is consistent with Aldy and Viscusi (2007), which shows that VSL peaks around age 40. This suggests that the value of a statistical life year is not age invariant.

Columns 2 and 3 show a strong relationship between VSL and income, matching theory. Column 2 reports heterogeneity with respect to asinh(wages), demeaned so the first coefficient captures average VSL.[35] Estimates indicate that a 1% increase in wages is 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 \$500 higher.

Column 4 provides evidence that VSL is also increasing with health. There is not significant heterogeneity with respect to having children, performance on a digit span recall test, 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 gap between reported and decision-relevant beliefs is driven only be classical error. Decision-relevant need not be true beliefs, since research shows that agents base decisions off of rounded values (Lacetera et al., 2012). Tests for mismeasured beliefs support the view that misreporting is classical and that VSL estimates are consistent.

The experiment 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 experimental arms included under classical error. However, if beliefs were misreported – unless misstatement was proportional across arms – estimates would differ. For example, if the reduction in perceived helmet efficacy in the low treatment arm reflected experimenter demand effects, estimates using the low treatment and control groups would be smaller

[35] 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(wage).

than those using the high treatment and control groups. Table 3 shows that estimates are similar across all combinations of arms.

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 beliefs were subject to non-classical measurement error correlated with true beliefs, the two IVs would yield different estimates. Appendix C presents a formal argument, based on the literature on local average treatment effects (e.g. see 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 motorcycle risks, $r_{0,i}$, helmet effectiveness and their VSL. The 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 typically remain proportional to $r_{0,i}$. Thus the IVs will typically produce different estimates, unless errors are orthogonal to true beliefs.

For instance, some agents report very high risks, resulting in average beliefs much larger than empirical estimates. Are they overstating their true belief? This would result in the interacted instrument producing a lower estimate, as long as those reporting extremely high beliefs have above average true expectations. The appendix argues that common forms of rounding and demand effects would also cause the estimates to diverge.[36] 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.[37]

Experimented demand effects are further ruled out by the reduction in willingness to pay in the low control arm, providing revealed preference of belief updating. And helmet

[36] Appendix Figure A4 illustrates this in the case of rounding.

[37] Estimates are also similar if only those with priors between 1:100,000 and 1:200 are considered, but instruments are only strong when treatments are pooled and controls are included so I do not report the result.

demand is similar in the pure control and control arms, despite the fact that the control was asked questions highlighting the risks of motorcycles and benefits of helmets. Lastly, VSL estimates that leverage quasi-exogenous variation from accident victim exposure, which was measured before treatment delivery, yields statistically indistinguishable estimates.

To further address concerns about bias from agents struggling to articulate small probabilities, I construct a 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 small probability elicitations. I estimate a similar VSL of \$151 (95% confidence set \$53 to \$273). The lack of VSL heterogeneity with respect to education or digit span recall test performance 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 biased.

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. Measurement error could obscure a true relationship, so I instrument for final beliefs using a prior survey measure, 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 (rational expectations) with 99.9% confidence. However, since this test is sensitive to correlated errors across belief measures, I turn to two other sources of data to examine how agents form expectations.

First, the survey asked respondents to list the information sources they used to form their beliefs. Figure 2 plots the responses. 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 variables that shift empirical risk and should influence beliefs (but not necessarily true risk) if respondents' reported information sources are accurate. I use trip volume and duration as empirical shifters, since more frequent motorcycle use mechanically increases mortality risk, particularly when conditioning on taxi terminal fixed effects. Consistent with this, a 1% increase in ridership is associated with a 0.13 percentage point increase in reporting a prior accident ($t \approx 5$). To test if agents form beliefs based on personal or social network experiences, I also consider indicators for having personally experienced a motorcycle accident or knowing someone who has.

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 suggest a violation of rational expectations.

One concern is that accident history may be driven by risk heterogeneity observed to the respondent but not the econometrician, in which case rational expectations could hold under a more complete model of empirical risk. At minimum, the findings demonstrate the sensitivity of results to the researcher's data. To further test this, I examine if respondents reported higher 5-year risks if it rained the day of the survey. The weather today should not affect true long-run risk, but rain increases the salience of conditions when motorcycles are dangerous. Respondents reported about twice the risk if it rained, suggesting a departure from rational expectations.

I next examine if the approaches typically used to estimate VSL, which assume rational expectations, consistently estimate VSL in Table 6.[38] Estimates from traditional methods generally fall outside the experimental confidence sets, and different observational approaches yield starkly different results.

[38] One 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.

Columns 1 - 4 report estimates of VSL obtained from Equation 10. 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 fixed effects, 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.

I report estimates using Equation 11 in column 5. The mean estimate is over $380,000, and the confidence interval excludes values below $354,000.

The experimental approach relaxes rational expectations and addresses endogeneity, raising the question of which force primarily drives differences between estimates. 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 endogeneity. The resulting estimate, about $110, is smaller than but not statistically different from the experimental point estimate. While suggestive, this indicates that deviations from rational expectations may be more important than endogeneity in this setting, consistent with subjective beliefs being shaped by idiosyncratic experiences, such as accident realizations.

5.4 Other threats to identification

There are several other concerns with this identifying procedure warranting discussion.

First, the treatments may have influenced beliefs about the efficacy of helmets at injury prevention, not just mortality. To limit this, the information excluded injury content and no injury-related questions were asked. If injury beliefs did shift, the VSL estimates would represent upper bounds, not affecting the conclusion that safety demand is low.

Second, the exclusion restriction may fail if information prompted agents to change other beliefs. One would expect such updating to be most prominent in a control versus treatment comparison since all treated subjects received information about how risky motorcycles are. However, when restricting the analysis to treated respondents – who received the same informational aside from the specific statistic shown – estimates are stable.

Third, VSL may be overestimated if agents did not plan to regularly use helmets. Scoping discussions indicated that respondents anticipated using it on all trips. However, some respondents did report a risk they would lose the helmet. Appendix Table A3 shows that estimates are similar when adjusting for respondents' expected duration of helmet use.

Fourth, monotonicity violations could affect the interpretation of estimates as a weighted average VSL. In practice, 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.

Finally, agents could face capital constraints. To limit liquidity concerns, demand was elicited through a helmet-versus-cash choice. This ensures estimates are not constrained by agents' ability to pay for a helmet. While temporary liquidity needs may still exist, VSL remains only \$500 among respondents with above median income and median willingness to pay is $1/4$ weekly wages, suggesting substantial bias is unlikely. Estimates are about \$290 if the unemployed are excluded, who are the most likely to face constraints. 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 Conclusion

This study introduces a framework for experimentally estimating demand for non-market amenities using subjective belief data and validates the approach by estimating the value of a statistical life. I leverage the fact that products often exist which affect an agent's exposure to such goods. This paper demonstrates that researchers may update agents' beliefs about how a product affects the amenity of interest, then examine how product demand changes. This method is tractable and low-cost in many settings where exogenous variation in attribute exposure or product characteristics does not exist, and it avoids the need to assume that agents' beliefs satisfy rational expectations. I document evidence that this assumption is violated in this context, adding to recent evidence from demand for air quality (Baylis et al., 2023), highlighting the value of leveraging subjective beliefs.

I estimate low demand for mortality risk reduction in Kenya. The low estimate is consistent with an extensively documented fact that consumers have low demand for highly effective preventative health products in LMICs. Development economists have long sought to understand the reasons that demand for such products is low, with no clear answer. It is certainly possible that a feature of decision making may deflate demand for such products and bias estimates of VSL in this paper. However, these results suggest another explanation: perhaps low demand for safety simply reflects a high marginal utility of consumption.

These results suggest scope to improve welfare by better aligning government and NGO decisions with recipients' preferences. Highlighting potential misallocation, the conclusions of four out of five benefit-cost analyses change when applying this paper's VSL estimates. I also show that a major aid organization underweights consumption gains by orders of magnitude relative to the efficient level implied by these estimates. This has direct welfare consequences, as billions of dollars are allocated to preventative health programs at the expense of cash transfers.

This result does not mean that health programs are not valuable. Rather, it suggests that aid recipients may value income gains so highly that the welfare benefits of higher consumption can exceed those of some preventative health investments. However, theory supports applying this logic only when LMIC consumers face a direct health-consumption tradeoff. Thus, these findings should not be used to justify reallocating health program funds toward richer populations.

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Figure 1: Revealed-preference VSL estimates across studies

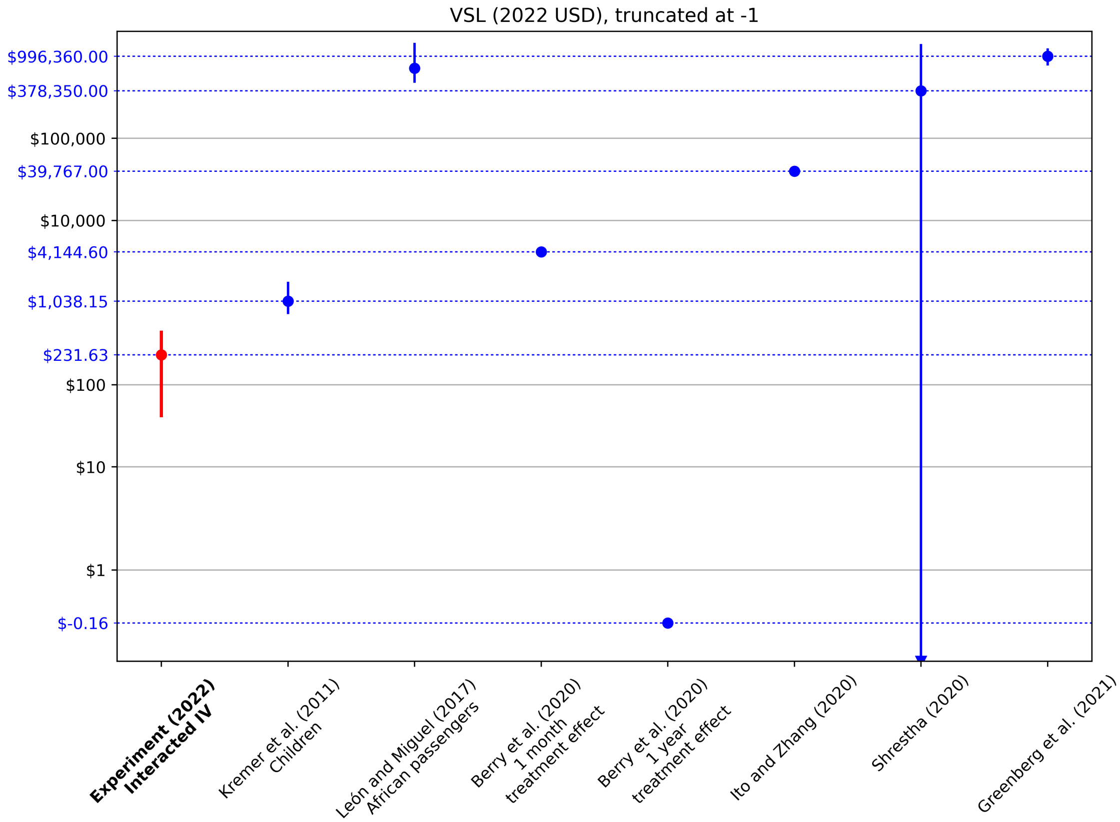


Figure 1 plots revealed-preference VSL estimates and (where available) 95% confidence intervals from this paper, Kremer et al. (2011), León and Miguel (2017), Berry et al. (2020), Ito and Zhang (2020), Shrestha (2020), and Greenberg et al. (2021). Greenberg et al. (2021) is included for comparison to 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

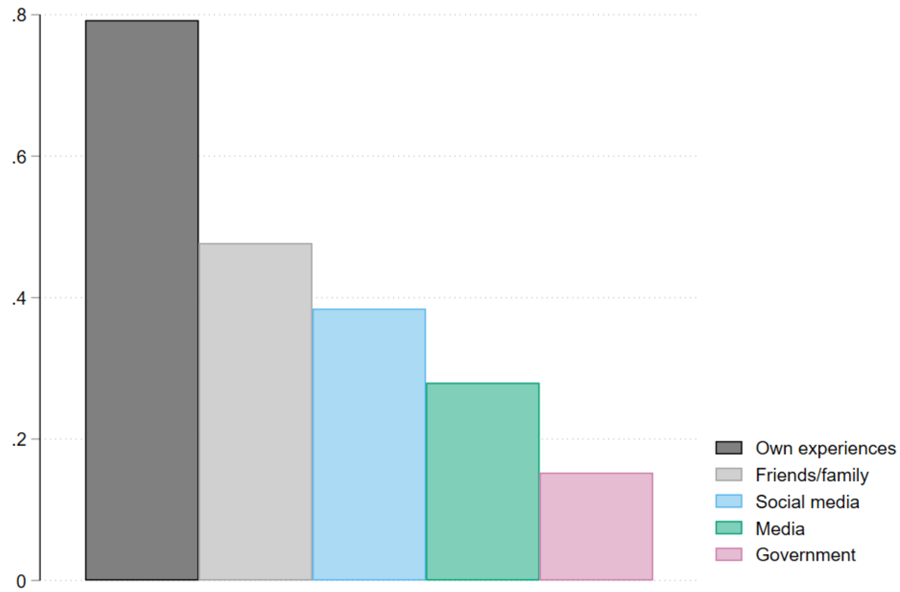


Figure 2 plots the information sources that respondents reported using to form beliefs about the mortality risk of motorcycles. Respondents were able to select multiple options, so the columns do not add to 1.

Table 1: Summary statistics and balance: Demographics

	(1) Control Summary statistics	(2) Pure control - control	(3) Low treatment - Control	(4) High treatment - Control	(5) High - low treatment
Age	32.735	0.105	0.282	0.659	0.377
	[8.126]	(1.159)	(0.572)	(0.589)	(0.590)
Female	0.352	-0.039	-0.064**	-0.050	0.014
	[0.478]	(0.060)	(0.030)	(0.030)	(0.029)
Health (1-5)	3.403	-0.034	-0.056	-0.012	0.044
	[0.667]	(0.086)	(0.043)	(0.044)	(0.043)
Life expectancy	81.438	-0.134	0.457	-0.119	-0.577
	[6.947]	(0.840)	(0.427)	(0.439)	(0.413)
Employed	0.898	-0.051	-0.041*	-0.047**	-0.006
	[0.303]	(0.043)	(0.022)	(0.022)	(0.022)
Income (PPP, ’000s USD)	6.866	-0.150	0.635	1.017	0.384
	[8.427]	(1.472)	(0.834)	(0.862)	(0.956)
$\mathbb{E}$[Wage in 5 years/Wage today]	6.166	-0.636	-0.925	-1.422*	-0.491
	[11.685]	(1.761)	(0.710)	(0.734)	(0.667)
1(children)	0.774	0.033	-0.002	-0.014	-0.012
	[0.418]	(0.053)	(0.027)	(0.028)	(0.027)
Digit span recall	3.020	-0.156	-0.006	-0.010	-0.002
	[1.391]	(0.183)	(0.089)	(0.091)	(0.088)
Years of education	12.111	-0.468	0.161	-0.025	-0.186
	[2.869]	(0.371)	(0.184)	(0.189)	(0.181)
1(primary school complete)	0.964	-0.035	0.007	-0.022	-0.029**
	[0.186]	(0.026)	(0.013)	(0.013)	(0.013)
1(secondary school complete)	0.722	-0.047	0.024	-0.008	-0.031
	[0.449]	(0.058)	(0.029)	(0.029)	(0.028)
1(college degree)	0.241	-0.054	-0.003	0.014	0.018
	[0.428]	(0.055)	(0.028)	(0.028)	(0.027)
N (exc. control in col. 2-4)	452	80	531	473	
p-value of joint orthogonality		0.743	0.183	0.193	0.271

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.

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	-25.90	-14.08	-14.08	-80.66	-45.76
	(21.77)	(21.77)	(0.97)	(0.97)	(18.31)	(14.00)
High treatment	6.77	6.77	-3.98	-3.98	-28.11	1.30
	(24.91)	(24.91)	(0.88)	(0.88)	(19.55)	(15.92)
Control mean	330.97	330.97	78.68	78.68	221.79	228.62
Pr(High treatment = low treatment)	0.08	0.08	0.00	0.00	0.00	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) Treatment only	(3) Interacted	(4) Treatment only	(5) Know victim	(6) Treatment + Know victim
VSL	215.23	347.26	223.90	307.88	559.99	510.63
	(98.38)	(259.49)	(97.88)	(262.00)	(225.82)	(189.43)
Cragg-Donald F-stat	40.12	10.99	40.75	10.63	16.24	6.32
Weak IV Robust Confidence Set	[24.10, 420.73]	[-160.17, 1,066.93]	[34.22, 428.34]	[-215.85, 1,028.44]	[117.39, 1,002.59]	[188.10, 1,238.20]
Inversion test	CLR	CLR	CLR	CLR	Wald	CLR
Observations	1,425	1,425	1,425	1,425	1,427	1,427
Controls	BL Risk	BL Risk	LASSO	LASSO	None	None
Enumerator FE	Yes	Yes	Yes	Yes	No	No

Panel B: Treated respondents only

	(1) Interacted	(2) Treatment only	(3) Interacted	(4) Treatment only	(5) Know victim	(6) Treatment + Know victim
VSL	396.03	774.94	394.52	823.28	824.37	769.37
	(253.28)	(478.06)	(257.04)	(513.43)	(405.92)	(343.19)
Cragg-Donald F-stat	16.84	11.24	16.07	9.90	8.17	5.68
Weak IV Robust Confidence Set	[-80.04, 1,021.81]	[-39.13, 2,403.92]	[-88.12, 1,038.34]	[-36.41, 2,726.94]	[28.78, 1,619.95]	[239.42, 2,087.47]
Inversion test	CLR	AR	CLR	AR	Wald	CLR
Observations	982	982	982	982	983	983
Controls	BL Risk	BL Risk	LASSO	LASSO	None	None
Enumerator FE	Yes	Yes	Yes	Yes	No	No

Panel C: Interacted estimates leaving 1 arm out

	(1) High treatment	(2) Low treatment	(3) Control	(4) High treatment	(5) Low treatment	(6) Control
VSL	211.18	138.12	396.03	213.28	178.31	394.52
	(88.71)	(190.17)	(253.28)	(87.39)	(174.54)	(257.04)
Cragg-Donald F-stat	79.07	19.80	16.84	82.35	23.50	16.07
Weak IV Robust Confidence Set	[38.16, 391.40]	[-39.13, 2,403.92]	[-80.04, 1,021.81]	[43.23, 390.86]	[-170.17, 559.50]	[-88.12, 1,038.34]
Inversion test	CLR	CLR	CLR	CLR	CLR	CLR
Observations	963	905	982	963	905	982
Controls	BL Risk	BL Risk	BL Risk	LASSO	LASSO	LASSO
Enumerator FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parenthesis.

Panel A reports regressions using observations in all experimental arms. Panel B excludes control observations. 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. Panel C reports "interacted" estimates leaving one experimental arm out, listed in the column title. All models control for baseline risk beliefs. Column (5) of panels A and B report estimates that instrument for beliefs using an indicator equal to 1 if the respondent knows a motorcycle accident victim, and this value plus treatment assignment in column (6). Heteroskedastic-robust standard errors are used for these estimates. 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). Just-identified models with heteroskedastic errors in columns (5) - (6) report confidence sets constructed via Wald test inversion. Excludes 35 observations where surveyors reported motorcycle taxi drivers pretending to be passengers.

Table 4: Heterogeneity in the value of a statistical life

	(1) 1(Age > median)	(2) asinh(Wage)	(3) 1(Wage > median)	(4) 1(health > median)	(5) 1(children)	(6) 1(digit recall score > median)	(7) 1(education > median)	(8) 1(female)
VSL	286.64	340.44	8.11	42.85	250.85	60.51	214.97	223.03
	(148.10)	(155.09)	(186.16)	(169.85)	(123.04)	(197.61)	(181.00)	(119.15)
VSL x Interaction	4.39	386.85	501.51	497.87	-97.05	297.54	70.90	260.45
	(268.81)	(127.27)	(273.24)	(254.27)	(247.69)	(242.17)	(232.93)	(238.62)
	[0.987]	[0.002]	[0.066]	[0.050]	[0.695]	[0.219]	[0.761]	[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 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)	(2)	(3)	(4)	(5)	(6)
	OLS	LASSO	LASSO	LASSO	$\frac{WTP_i}{\Delta r_i^*}$	Beliefs
VSL	15,244.30	2,744.41	-6,167.09	1,490.08	381,326.62	110.96
	(10,900.69)	(10,630.65)	(10,840.79)	(10,747.15)	(13,859.27)	(53.74)
95% CI/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]
Cragg-Donald F-stat						19.73
Observations	1,425	1,425	1,425	1,425	1,425	1,425
Controls	None	DPLASSO	DPLASSO	DPLASSO	None	None
Enumerator FE	No	No	Yes	No	No	Yes
Taxi Terminal FE	No	No	No	Yes	No	No

Panel B: 70% helmet effectiveness treated as truth

	Rational expectations					Subjective beliefs
	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	LASSO	LASSO	LASSO	$\frac{WTP_i}{\Delta r_i^*}$	Beliefs
VSL	9,146.58	1,646.65	-3,700.25	894.05	228,795.97	110.96
	(6,540.42)	(6,378.39)	(6,504.48)	(6,448.29)	(8,315.56)	(53.74)
95% CI/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]
Cragg-Donald F-stat						19.73
Observations	1,425	1,425	1,425	1,425	1,425	1,425
Controls	None	DPLASSO	DPLASSO	DPLASSO	None	LASSO
Enumerator FE	No	No	Yes	No	No	Yes
Taxi Terminal FE	No	No	No	Yes	No	No

Standard errors in parenthesis.

Columns (1) - (4) report VSL estimates obtained by estimating a regression of willingness to pay on the empirical risk reduction offered by a helmet. Column (5) estimates VSL as valuation normalized by the mortality risk reduction offered by a helmet. The mean VSL values and standard errors of the mean are reported in this column. In Panel A, the estimated helmet effectiveness from Liu et al. (2008) is used in constructing empirical estimates, while in Panel B the estimated effectiveness of helmets in Thailand from Ouellet and Kasantikul (2006) is used. Column (6) reports estimates using subjective beliefs about helmet efficacy estimated using a separate survey measure as an instrument to minimize measurement error without ruling out endogeneity. All models exclude 35 observations for which surveyors reported motorcycle taxi drivers pretending to be passengers.

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 week long 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 every 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.

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 instance, for a passenger that takes 6 trips per week on average, 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 group with the Liu et al. (2008) estimate that helmets reduce one's likelihood of dying by 42% and the high treatment group with the Ouellet and Kasantikul (2006) estimate 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

My primary estimates of VSL report homoskedastic standard errors along with weak IV robust confidence sets. This analytic choice 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

$$
\begin{aligned}
v_i &= \zeta_h + VSL\Delta r_i + X_i'\gamma_0 + \gamma_1 r_{0,i} + \epsilon_i \\
\Delta r_i &= Z_i'\pi + X_i'\pi_c + \pi_r r_{0,i} + \nu_i
\end{aligned}
$$

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, we have from the asymptotic variance formula for two-stage least squares that

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

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 unobserved determinants of utility. The standard deviation of willingness to pay is about 41, so even if the average deviation of VSL were as large as the mean, the contribution to ϵ_i would be just $.022 \cdot 223$ or 10% of the standard deviation of WTP on average, 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 under 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 A2. I report estimates using efficient GMM since 2SLS is less efficient under heteroskedasticity and to show that results are similar under different weighting of the 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 will apply the Frisch-Waugh-Lovell theorem to partial out the constant in the treatment only first stage and r_{i0} from the interacted version. I will denote 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

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

Showing the estimate weights the VSL of individuals linearly in their prior, r_{i0}. In the interacted case, observe that first stage coefficients on L_i and H_i will be zero in expectation, therefore

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

Therefore the interacted instrument weights 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,

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

Hence, $V\hat{S}L_{2SLS}^{TO} \neq V\hat{S}L_{2SLS}^{INT}$ 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

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

Therefore one typically has $V\hat{S}L_{2SLS}^{TO} \neq V\hat{S}L_{2SLS}^{INT}$ unless ζ_i and r_{i0} are independent, for instance if $\zeta_i = c$. Observe that if individuals with low beliefs round down and those with high beliefs round up, $\mathbb{E}[\zeta_i r_{i0}] > 0$. Similarly, 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.

Ouellet and Kasantikul (2006) leverages observational accident report 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 helmet 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

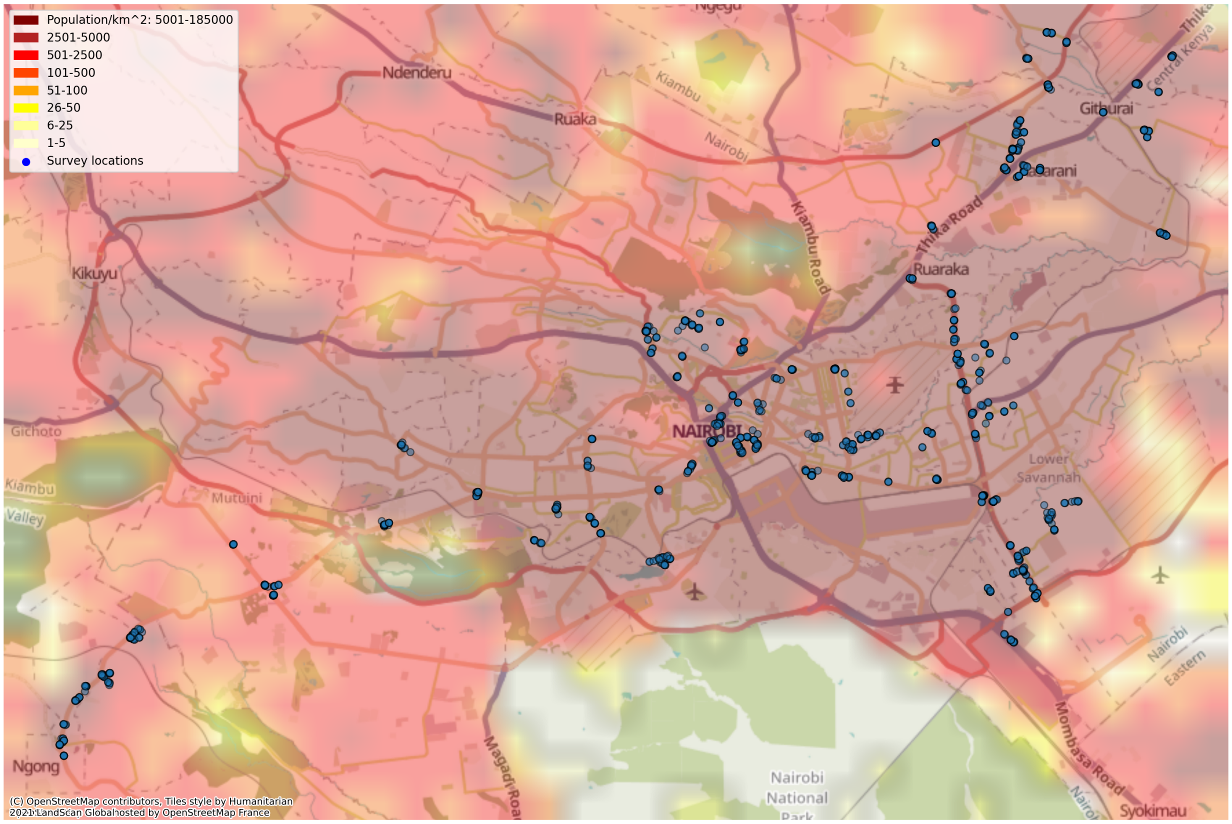
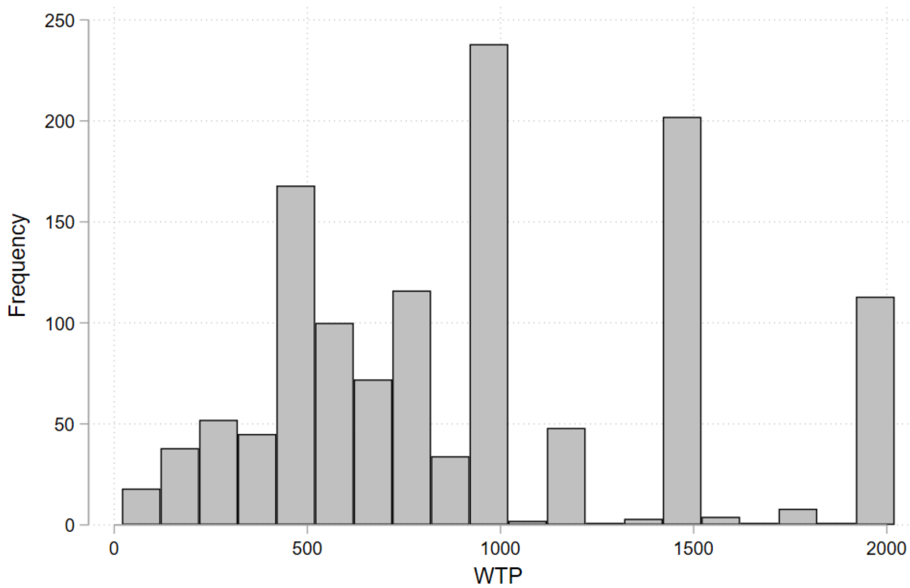


Figure A1 plots survey locations over a map of Nairobi. Color denotes population, where no color denotes no residents. The 2019 census estimated a population of about 4.4 million within the city. Map data is from Open Street Map. Population data is from the LandScan Global 2021 data set produced by Oak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

A2: Distribution of helmet bids (Kenyan shillings)

(a) A. Histogram of bids, restricted axis



(b) B. Histogram of bids, full distribution

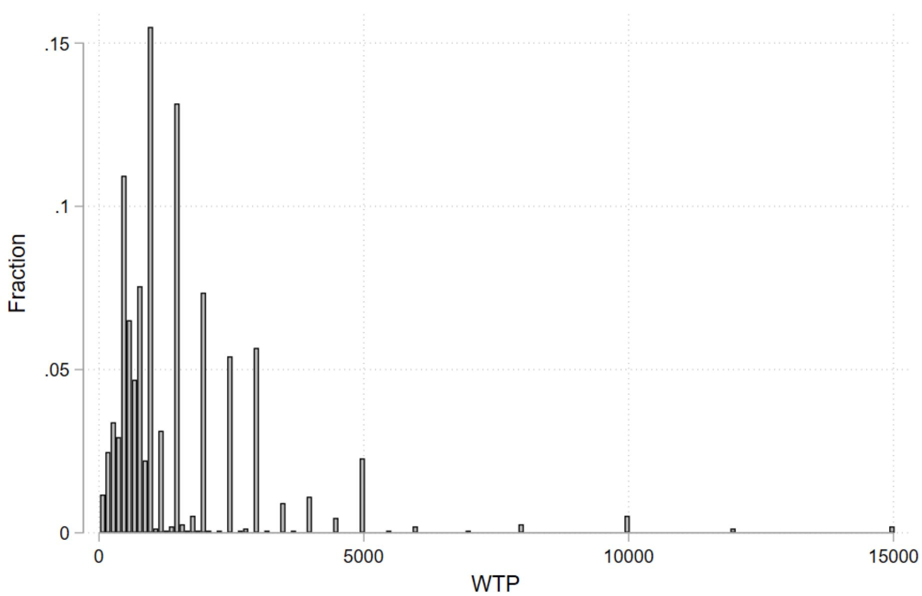


Figure A2 plots the distribution of willingness to pay for helmets in Kenyan shillings (Ksh). Panel A reports a histogram of bids, excluding outliers above Ksh 2,000 for clarity. Panel B reports the same histogram across the full distribution of bids. Figures present Ksh rather than USD to illustrate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

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

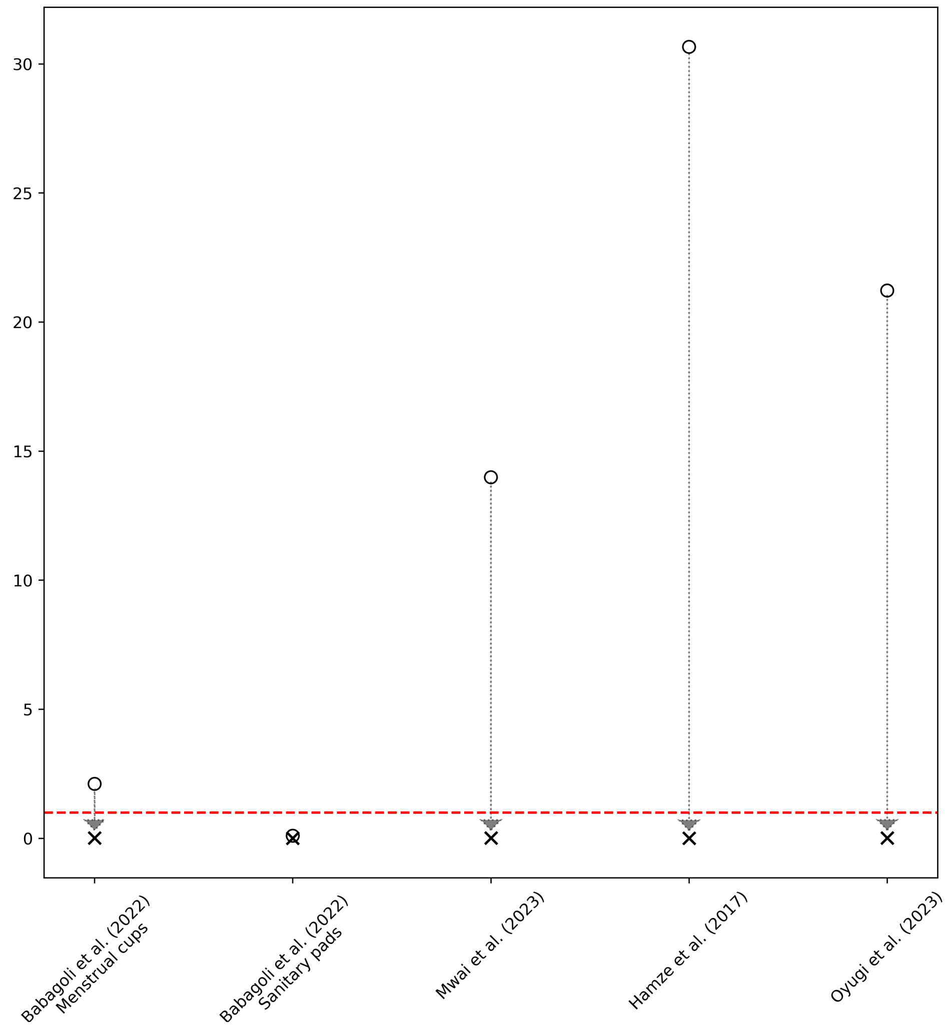
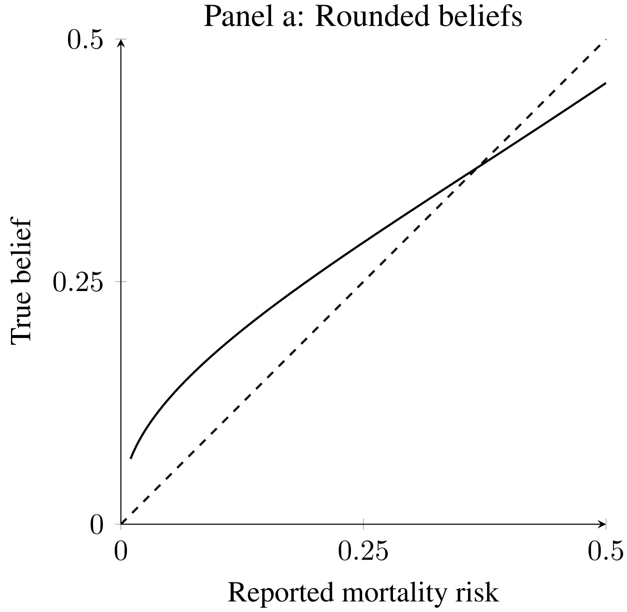


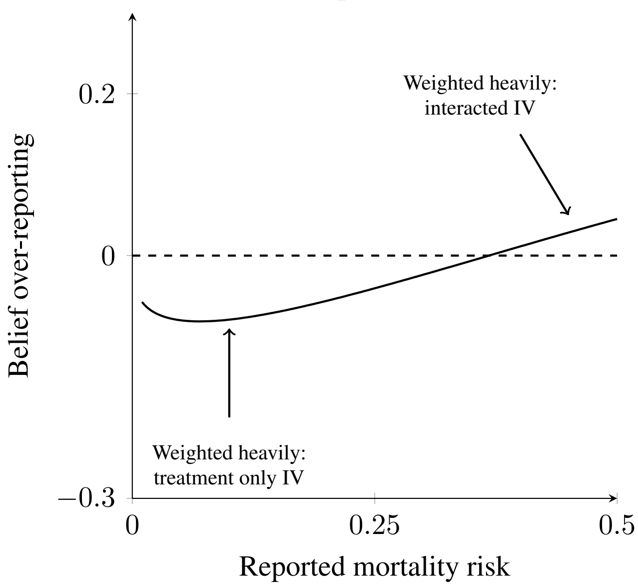
Figure A3 examines how the benefit-cost ratios (BCRs) of published benefit-cost analyses of Kenyan programs change when the study's original VSL or VSLY is replaced with the preferred estimates from this study. Hollow circles denote the original BCR estimate, and an x denotes the revised estimate. The 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

Panel a: Rounded beliefs



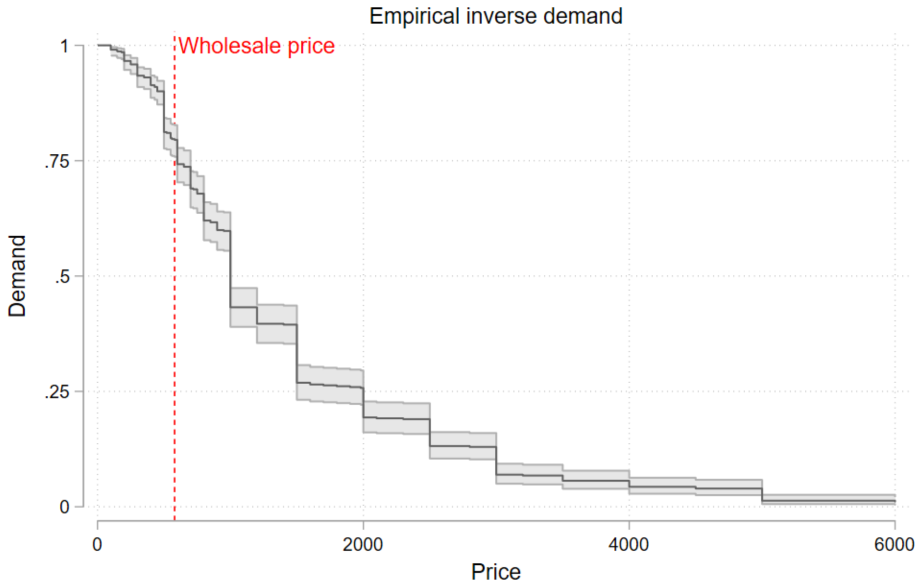
Panel b: Difference between reported and decision-weighted beliefs



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

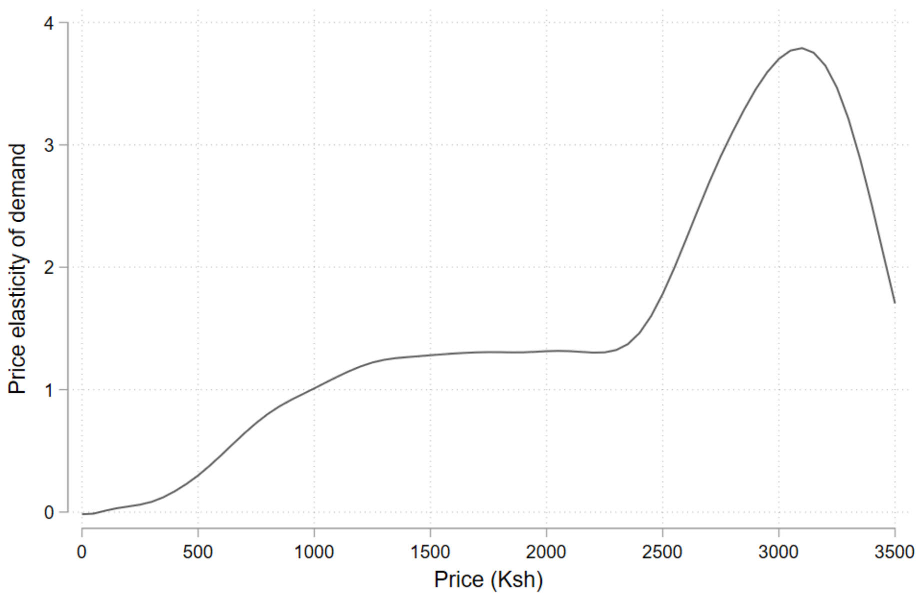


Figure A5 plots estimates of demand for helmets among observations in the control and pure control arms. The plot of demand includes a pointwise confidence interval. The plot of the elasticity of demand is based on a local polynomial estimation adapted from Berry et al. (2020). The vertical line denotes 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	1.178	0.196	-0.379	-0.593*
	[5.242]	(0.725)	(0.348)	(0.358)	(0.343)
Average trip length (minutes)	19.593	0.334	-0.704	-1.301*	-0.583
	[12.567]	(1.538)	(0.725)	(0.745)	(0.672)
$\mathbb{E}$[Deaths/10,000 passengers, 1 year]	371.584	NA	-46.930	-0.326	46.539
	[1,186.706]	NA	(76.351)	(78.569)	(76.478)
$\mathbb{E}$[Deaths/10,000 passengers, 5 years]	977.118	NA	-154.821	266.926	420.383**
	[2,479.942]	NA	(192.351)	(197.928)	(205.063)
Confidence in beliefs	3.461	NA	0.034	0.011	-0.022
	[0.699]	NA	(0.042)	(0.044)	(0.041)
10000*Pr(Fatal accident, 5 years)	354.133	NA	-19.377	-40.568	-20.464
	[973.710]	NA	(56.011)	(57.630)	(52.741)
Previous accident	0.488	NA	-0.011	-0.030	-0.019
	[0.500]	NA	(0.032)	(0.033)	(0.032)
Know accident victim	0.947	NA	-0.022	-0.036**	-0.014
	[0.224]	NA	(0.017)	(0.017)	(0.017)
Use motorcycle taxi: Commuting	0.781	0.027	-0.032	0.008	0.038
	[0.414]	(0.054)	(0.027)	(0.028)	(0.027)
Shopping	0.420	-0.031	-0.033	-0.018	0.016
	[0.494]	(0.064)	(0.031)	(0.032)	(0.031)
Leisure	0.261	0.057	0.074**	0.036	-0.037
	[0.440]	(0.058)	(0.029)	(0.030)	(0.029)
Deliveries	0.095	-0.012	0.015	-0.000	-0.015
	[0.294]	(0.014)	(0.018)	(0.018)	(0.018)
Emergency/hospital transportation	0.095	-0.008	-0.009	-0.007	0.003
	[0.294]	(0.014)	(0.017)	(0.017)	(0.017)
Reason for use: Speed	0.816	0.114**	0.019	0.003	-0.017
	[0.388]	(0.045)	(0.024)	(0.025)	(0.024)
Convenience	0.717	-0.005	0.005	-0.045	-0.049*
	[0.451]	(0.061)	(0.029)	(0.030)	(0.029)
Only option	0.168	0.007	-0.025	-0.010	0.015
	[0.374]	(0.047)	(0.023)	(0.024)	(0.023)
Price	0.106	0.037	-0.008	-0.026	-0.018
	[0.308]	(0.038)	(0.019)	(0.019)	(0.018)
Safety/Avoid dangerous areas	0.069	-0.017	-0.027*	-0.010	0.017
	[0.253]	(0.033)	(0.015)	(0.015)	(0.014)
Enjoyment	0.011	-0.012	-0.002	0.008	0.009
	[0.105]	(0.015)	(0.007)	(0.007)	(0.007)
Risk information: Own experiences	0.819	NA	-0.036	-0.042	-0.007
	[0.386]	NA	(0.026)	(0.027)	(0.026)
Friends/family	0.454	NA	0.029	0.038	0.009
	[0.498]	NA	(0.032)	(0.033)	(0.032)
Social media	0.414	NA	-0.049	-0.034	0.016
	[0.493]	NA	(0.031)	(0.032)	(0.031)
Media	0.288	NA	0.014	-0.027	-0.042
	[0.453]	NA	(0.029)	(0.029)	(0.028)
Government	0.135	NA	0.015	0.026	0.011
	[0.342]	NA	(0.023)	(0.023)	(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: 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 A2 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.

A3: 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	287.46	154.61	264.60	66.63	199.55
	(64.91)	(187.29)	(65.85)	(195.50)	(48.09)	(144.70)
Cragg-Donald F-stat	45.37	10.37	44.74	9.52	44.75	9.52
Weak IV Robust Confidence Set	[13.66, 274.52]	[-82.08, 803.62]	[25.46, 290.05]	[-130.39, 811.47]	[-28.66, 164.14]	[-86.79, 578.53]
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
Enumerator FE	Yes	Yes	Yes	Yes	Yes	Yes

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	332.13	277.79	298.61	144.55	240.87
	(89.62)	(245.11)	(89.07)	(246.66)	(64.47)	(182.05)
Cragg-Donald F-stat	41.49	10.38	42.24	10.06	42.24	10.06
Weak IV Robust Confidence Set	[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]
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
Enumerator FE	Yes	Yes	Yes	Yes	Yes	Yes

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	96.56	286.78	86.40	145.99	110.97
	(100.91)	(311.00)	(98.70)	(304.39)	(72.20)	(225.65)
Cragg-Donald F-stat	43.20	7.97	45.41	8.26	45.41	8.26
Weak IV Robust Confidence Set	[77.43, 482.10]	[-637.11, 865.77]	[96.84, 492.58]	[-632.16, 837.48]	[4.88, 293.38]	[-388.60, 671.41]
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
Enumerator FE	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parenthesis.

Panel A considers 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 fully interacted with treatment assignment. Panel B considers uses the respondent's stated belief about the lifespan of the helmet, rather than the manufacturers suggestion. Panel C weights each observation by the inverse of motorcycle trips in a typical week to account for selection into ridership. All models control for baseline beliefs. I report weak instrument robust confidence sets constructed using inversion of a conditional likelihood ratio (Moreira, 2003). All models exclude 35 observations for which surveyors reported motorcycle taxi drivers pretending to be passengers.

A4: 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	671.97	601.70	653.89	282.14	436.77
	(252.81)	(416.68)	(280.05)	(452.65)	(195.98)	(326.19)
Cragg-Donald F-stat	9.54	7.38	8.03	6.23	8.03	6.23
Weak IV Robust Confidence Set	[64.21, 1,197.10]	[-90.81, 2,026.93]	[64.21, 1,197.10]	[-187.78, 2,297.45]	[-108.59, 772.92]	[-194.93, 1,492.94]
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
Enumerator FE	Yes	Yes	Yes	Yes	Yes	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	969.44	690.84	1,022.45	323.45	535.40
	(401.69)	(595.22)	(414.44)	(633.37)	(281.93)	(420.67)
Cragg-Donald F-stat	10.67	10.97	9.95	9.83	9.95	9.83
Weak IV Robust Confidence Set	[-66.05, 1,785.13]	[-49.48, 3,012.90]	[-72.23, 1,869.17]	[-45.73, 3,365.04]	[-237.52, 1,067.29]	[-239.47, 1,959.91]
Inversion test	CLR	AR	CLR	AR	CLR	AR
Observations	982	982	982	982	982	982
Controls	BL Risk	BL Risk	LASSO	LASSO	LASSO	LASSO
Enumerator FE	Yes	Yes	Yes	Yes	Yes	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.