A New Experimental Method for Estimating the Value of a Statistical Life

With an application to road safety in Kenya*

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January 15, 2024

Abstract

This paper introduces and implements a new experimental method for estimating the value of a statistical life (VSL) that is robust to leading methodological concerns with observational approaches. Estimates typically assume that mortality risk is exogenous conditional on observables and that subjective beliefs about the risk of choices exactly equal empirical estimates. This study relaxes both assumptions by randomly updating agents' information sets to create an instrument for subjective beliefs about the efficacy of a safety technology, then eliciting their demand for the product. I apply this method in the context of motorcycle helmets in Nairobi, Kenya and compare the results to those obtained from observational methods across the same sample. I find that observational estimates fall outside of experimental confidence sets and present evidence that subjective beliefs differ systematically from empirical risk estimates. I also contribute to a growing literature studying VSL in low and middle-income countries (LMICs), estimating a low willingness to pay for safety among urban Kenyans. My preferred point estimate of VSL is just 2022 USD PPP $224, and I can reject values above USD PPP $429 with 95% confidence. (*JEL* O18, R49, J17)

*I thank Edward Miguel for excellent feedback, advising, and help implementing this project. This project benefited from suggestions by Kurt Lavetti, Jeremy Magruder, Marco Gonzalez-Navarro, Sam Wang, Elif Tasar, Carlos Paramo, Catherine Che, and attendants of the UC Berkeley Development Lunch. I also thank William Jack, Whitney Tate, Nyambaga Muyesu, Josephine Okello and the team at the *Georgetown University Initiative on Innovation, Development, and Evaluation* for support with the implementation of the study. 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.

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1 Introduction

The value of a statistical life (VSL) – willingness to pay for reductions in mortality risk – has been studied extensively by economists because of the importance of mortality risk in a wide range of individual choices and public policies. The parameter is widely used by policymakers that face the problem of balancing lives saved from policies with their financial costs; academics that need to value reductions in mortality risk when studying decision making or optimal policy in risky environments; and, in the context of economic development, aid workers and donors that aim to direct funds to programs that maximize social welfare. Despite the large empirical literature about VSL, the number of high quality studies from low and middle income (LMIC) settings is limited, and economists have recently raised concerns about the credibility of estimates across contexts.

This paper introduces a new experimental method that produces statistically precise estimates of the VSL of urban Kenyans that are robust to several prominent concerns that have been documented in the VSL literature. Furthermore, this study was designed to facilitate comparisons between experimental and observational estimates of the parameter across the same sample in order to provide one of the first test of the performance of commonly used methods. In the central finding of the study, I find that consumers have a remarkably low VSL in this context: the preferred estimate rejects values above USD PPP $429 with 95% confidence. I further demonstrate that two observational revealed preference approaches to estimating VSL that have been used in recent publications produce point estimates that fall outside of experimental confidence sets and differ significantly from each other. Analysis of data on elicited mortality beliefs suggests that this discrepancy is driven at least in part by the fact that consumers' beliefs about the mortality risk of actions align poorly with empirical estimates, which are typically used as a proxy for beliefs.

A large number of studies have estimated VSL in high-income settings (for a review see Banzhaf, 2014). Recent methods include examining demand for safety equipment (Rohlfs et al., 2015), comparing lives saved by public policies such as speed limits to economic costs (Ashenfelter and Greenstone, 2015), and estimating wage premiums for high risk jobs (Lee and Taylor, 2019).

Although there is a rich literature about VSL from rich countries, knowledge of demand for safety is limited in poor contexts. Common tools such as hedonic wage models depend on assumptions about perfect markets that contradict empirical evidence in less developed markets (Breza et al., 2021). The most credible studies of VSL in LMICs have thus turned to revealed preference approaches that estimate demand models for choices where fatality risk is salient (Kremer et al., 2011; León and Miguel, 2017; Berry et al., 2020; Ito and Zhang, 2020). However, observational revealed preference estimates (in addition to hedonic methods) are still subject to three leading methodological concerns with VSL estimates documented in Ashenfelter (2006).[1] First, risks may be endogenous, for instance due to selection into risky behaviors (Ashenfelter and Greenstone, 2004). Second, econometricians generally use empirical risk estimates as a proxy for beliefs, but agents may not have perfect information about risks. Third, VSL is likely heterogeneous across samples, and many studies estimate the parameter on selected populations.

This paper introduces an experimental approach to VSL estimation that makes progress on these three limitations of existing estimates. I first present motorcycle taxi passengers with randomly varying information about the fatality risk of motorcycles and the safety benefits of wearing helmets. I then measure the posterior belief that a helmet will save the life of each respondent and

[1] Ashenfelter (2006) also notes that agency problems raise concerns in some VSL estimates, but these do not apply to revealed preference estimates.

elicit willingness to pay for a helmet using a Becker et al. (1964), hereafter referred to as BDM, mechanism. This design allows one to estimate VSL using treatment assignment as an instrument for elicited beliefs in a two-stage least squares regression. Random assignment addresses concerns of endogeneity and the use of elicited beliefs avoids the need to use empirical estimates as a proxy. The design of the experiment does not directly address external validity concerns, but the use of motorcycle taxis is near universal in the context of Nairobi, Kenya and essentially no taxi passengers use helmets. Hence, the VSL estimates may be interpreted as an average over a broad urban sample, and I further show that VSL heterogeneity is limited in this setting.

The estimates of VSL indicate that urban Kenyan adults have low demand for safety. The preferred estimate is just 2022 PPP USD $224, and a weak-instrument robust confidence set rejects values greater than $429 with 95% confidence.[2] Two characteristics of VSL provide insight into why the parameter is so low in this context. First, VSL is inversely proportional to the marginal utility of a dollar, so theory predicts it will increase non-linearly with income (Hall and Jones, 2007). Consistent with this view, I estimate that VSL increases by about 90 cents for each 1 dollar increase in wages in this sample. Given income differences between the sample in this study and León and Miguel (2017), which estimates a VSL (adjusted to 2022 terms) over $700,000 among wealthy West Africans, the two VSL estimates can be reconciled by a coefficient of relative risk aversion below 3.[3] Consistent with this view, Berry et al. (2020) and (Kremer et al., 2011) estimate similarly low VSLs ranging from about 2022 USD 0 to 4,145 in low-income settings. Second, VSL is a function of one's utility from being alive less one's expected utility from not being alive. This parameter may be less than the present value of one's lifetime consumption, for instance if the

[2] I use the World Bank's 2021 PPP conversion factor to convert from Kenyan shillings to USD in this paper, which was 43.8 Kenyan shillings per USD at the time it was accessed.

[3] This study was selected for comparison because it reports incomes and is one of the highest revealed preference VSL incomes from an LMIC country.

agent believes in an afterlife. These estimates have policy relevant welfare implications: I show later in the paper that they are an order of magnitude lower than VSL estimates used in 5 recently published benefit-cost analyses of health programs, causing the benefit-cost ratio to fall below 1 in 4 of 5 instances.

The study was designed to further facilitate comparisons between experimental and observational VSL estimates in order to evaluate the performance of revealed preference methods in practice. I test two revealed preference approaches similar to those used in recent publications. First, I use cross-sectional variation in empirical mortality risk as a proxy for beliefs to estimate a demand model. This approach is similar to León and Miguel (2017) and Ito and Zhang (2020). Second, I use empirical mortality risk as a proxy for beliefs and assume that agents only value helmets for their life saving potential as in Berry et al. (2020) and Kremer et al. (2011). I find that the two approaches, which produce estimates ranging from USD -4,359 to over 380,000, typically produce estimates that fall outside of experimental confidence sets, in some cases by orders of magnitude, and that they differ from each other by a statistically significant margin.

Data on subjective beliefs indicate that empirical risk estimates are a poor proxy, helping explain these results. Respondents overwhelmingly reported using their own experiences and experiences of friends and family to generate beliefs, not empirical data sources. Empirical determinants of one's risk of dying in a motorcycle accident, such as the frequency with which they use them and the length of an average trip, are not positively correlated with beliefs. However, indicators for whether a respondent or someone they know has suffered a prior accident, which is likely determined by luck after controlling for rich covariates, are strongly predictive of beliefs. In addition, respondents reported about double the probability of dying in a motorcycle accident over a 5 year span if it happened to be raining the day of the survey.

To my knowledge, this is the first study to produce a statistically precise experimental estimate of VSL and the first to test the performance of observational approaches against an experimental benchmark.[4] This builds on recent revealed preference estimates of VSL from low-income countries, weakening identifying assumptions and estimating VSL over a broad urban sample. This helps address a need identified in Greenstone and Jack (2015) for measures of willingness to pay for health among low-income populations. This study also contributes to the broader VSL literature by testing whether observational VSL estimates match an experimental benchmark. This provides among the first evidence about the degree to which the concerns identified in Ashenfelter (2006) bind in practice and offers a new methodology that is robust to many concerns about observational approaches.

Aside from the estimates of VSL, this study contributes to a literature summarized in Delavande (2014) demonstrating that agents subjective beliefs about stochastic events affect their decisions in LMICs (e.g. Delavande, 2008; Attanasio and Kaufmann, 2017; McKenzie et al., 2013; Shrestha, 2020). I introduce a new two-step survey instrument that can elicit informative measurements of beliefs about low probability events. Furthermore, I show how the malleability of biased beliefs can be leveraged to identify parameters of a utility function.

Finally, the study contributes to a small literature about road safety. Traffic accidents are a leading cause of death in East Africa. Several studies have examined buses (e.g. Habyarimana and Jack, 2011, 2015). However, I am not aware of work studying motorcycle safety. I show that there is high demand for helmets despite low adoption in the absence of liquidity constraints, suggesting that improving accessibility of helmets could reduce traffic injuries and deaths.[5]

[4] Shrestha (2020) attempted to estimate VSL using experimental variation, but a weak first stage results in confidence intervals that are not informative.

[5] High helmet demand exists despite low VSL primarily because agents have a high utility from characteristics of the helmet other than mortality risk reduction. Respondent anecdotally stated that they valued the protection from

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 unique features which are ideal to study demand for safety. First, motorcycle taxis are near universally used and it is rare for taxi 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 at the individual level since it is a function of ridership volume. This facilitates comparisons between observational and experimental VSL estimates.

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.[6] Transportation, including on motorcycles, is notoriously dangerous. Data from the National Transport and Safety Authority (NTSA) reports that 1,722 motorcycle drivers and passengers died in 2021, compared to 715 recorded deaths 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.[7]

Despite the high risks of motorcycles, helmet use among motorcycle taxi passengers is rare. Bachani et al. (2017) measured passenger helmet use at 3% in an observational study. At that time, helmeted passengers typically borrowed them from the driver. However, anecdotal accounts suggest that consumers became concerned with the cleanliness of borrowed helmets during the

non-fatal injuries, which they viewed as costly due to lost work and medical expenses.

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

[7]"New initiative to tackle road crash deaths in Kenya," *World Health Organization*

covid-19 pandemic, prompting the *Kenyan Bureau of Standards* to release regulations for sanitary inserts that have not gained traction. Consistent with the view that the use of borrowed helmets fell, under 1% of motorcycle taxi passengers approached reported regular access to a helmet.

The low use of helmets suggests that demand may be low. However, the availability of effective helmets within the budget set of average Kenyans is a recent development. The *FIA Foundation Safe and Affordable Helmet Program* began offering helmets in Kenya in 2021, and a local Kenyan producer *Boda Plus* began manufacturing effective and low-cost helmets the same year. Prior to the introduction of those products, consumers typically only had access to high-cost imports above USD $100 or low-cost helmets with limited safety benefits.

One concern with this sample is that consumers with a weak preference for safety may select into riding motorcycles more often. A strength of this study is that the sample of motorcycle commuters is policy relevant even if it is not representative. In addition, I show that results are robust to re-weighting to account for selection into motorcycle trips at the intensive margin, and qualitative evidence suggests that selection into motorcycle use is limited. I am not aware of representative evidence documenting the prevalence of motorcycle taxi use, but the study team estimated that at least 85% of urban Kenyan adults use motorcycle taxis. Back of the envelope calculations also suggest that ridership needs to be high among urban adults to rationalize ridership volumes.[8] Selection may be limited because alternative modes of public transportation are dangerous, crowded and uncomfortable (Habyarimana and Jack, 2011).

[8]There were an estimated 22 million trips per day in Kenya in 2022, and a population of 53 million. 40% of Kenyans are farmers and at least 10% are children aged 5 or younger. Respondents in our sample report taking about 7.5 trips per week on average. This would suggest that about 80% of urban Kenyans adults ride motorcycle taxis ($22 \approx 53 \cdot 0.5 \cdot \frac{7.5}{7} \cdot 0.78$).

2.2 Recruitment

This study recruited consumers from a 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 geographic constituencies. The stands were selected for broad geographic and demographic coverage. However, areas with very high crime rates were excluded for the safety of the team. Survey locations are plotted over a map of Nairobi in Figure 1.

The study leveraged arrival times of consumers to sample from the population of the passengers at each location. Surveyors attempted to recruit the first individual to arrive at a taxi stand after completing a survey. Consumers that did not report regular access to a motorcycle helmet were informed that they could choose a free helmet or a cash gift if they completed a 15-30 minute survey. The high value of the gifts (about $5 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, and under 1% were excluded because they regularly used a helmet.

Demographic information presented in Table 1 shows that the study reached a broad demographic 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 (Nairobi is the richest county in Kenya).[9] The sample is not perfectly representative. Significantly more males (981) than females (444) were surveyed, explained by the fact that that men are more likely to commute. Education is relatively high, with an average of about 12 years of schooling completed.

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

2.3 Information treatment: Motorcycle fatality risks and helmet effectiveness

This study implemented a randomized information treatment to produce the variation in beliefs needed to estimate VSL. There are four experimental arms: a pure control group, a control group, and two treatment arms. The two treatment arms were presented with information about the mortality risk of motorcycle taxis in Kenya and the effectiveness of helmets at preventing death. The two control arms received no information.

The pure control and control arms vary in the questions that they were asked. The pure control was not asked about motorcycle safety. In contrast, the control group was asked detailed questions about the risks of using motorcycle taxis and surveyors elicited beliefs about the effectiveness of helmets. The pure control group was included because I hypothesized that asking respondents about the risks of motorcycles and the benefits of helmets may increase helmet demand, changing the public policy implications of results. Pure control observations are excluded from VSL estimates since subjective beliefs were not elicited. In practice, there were no differences in helmet demand between the pure control and control group during the first wave, so the pure control was excluded from the second wave.

The two treatment arms were presented different studies estimating the effectiveness of motorcycle helmets. All respondents in these treatment arms first received an estimate of their baseline empirical mortality risk over the 5 year lifespan of a helmet, estimated based on their ridership volume using data provided by the NTSA.[10] Respondents were then presented with one of two studies about the effectiveness of helmets at preventing death. Those in a "low treatment" group were presented with the results of Liu et al. (2008) which reports the results of a meta-analysis

[10] I calculated per trip mortality risk estimates for the average Kenyan from NTSA data, then estimated 5 year risks based on the respondent's expected ridership volume over that period.

of studies of helmet effectiveness. The authors estimate that helmets reduce one's likelihood of dying by 42%. Respondents assigned to a "high treatment" arm were presented with the results of Ouellet and Kasantikul (2006), who estimate that high quality helmets reduce fatality risk by about 70% in Thailand. All respondents were informed about the sources of information they were presented with so that they could make judgements about its credibility.

Both studies are credible and no respondents were given misleading information. There is a strong consensus that motorcycle helmets are effective, but there is considerable uncertainty about exactly how well they work. In fact, the 70% estimate reported in Ouellet and Kasantikul (2006) is approximately the upper bound of the 95% confidence interval from Liu et al. (2008). Furthermore, there is a trade-off between quality and contextual similarity between the studies. Liu et al. (2008) is a high-quality meta analysis, but the authors mainly consider studies from rich countries where roads quality and traffic speeds are higher. Ouellet and Kasantikul (2006) reports results from a single study, but the authors examine a LMIC setting.

2.4 Helmet valuations

The study measured demand for a high quality 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 Ksh 0 and 600. 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 on the spot. The study used a willingness to accept rather than a willingness to pay mechanism to ensure that liquidity constraints did not bind, which would bias VSL.

The maximum draw was set to Ksh 600 based on assessments by experts at an NGO and the helmet manufacturer that most valuations would fall below this threshold. The helmet model used in the study sold at a wholesale price of 580 Kenyan shillings (Ksh), about 15% of weekly wages for the median respondent. A limitation of the willingness to accept mechanism is that disclosing one's true valuation is not the unique weakly dominant strategy if the agent knows that their valuation exceeds the maximum draw. To hedge against the risk of setting the max payout too low, and to avoid anchoring or signaling effects, enumerators did not disclose the upper bound or value of the helmet prior to the game.[11]

In practice, helmet valuations exceeded the wholesale price of the helmets in about $3/4$ of cases. Figure 2 demonstrates that there is no unusual behavior in the distribution of bids near the maximum draw, consistent with accurate valuation data.

2.5 Randomization

Respondents were assigned to the information arms using a pseudo random number drawn using the survey software. During the first wave, respondents were assigned to the pure control group with a probability of 0.1 and each of the other three groups with a probability of 0.3. During the second wave, the pure control group was eliminated and respondents were assigned to the three remaining arms with equal probability. An independent random draw determined the cash payment amount offered to each respondent.

3 Model and identification

This section presents a simple model of demand for motorcycle helmets. This demonstrates how the experiment identifies VSL. Furthermore, the model illustrates the assumptions needed to iden-

[11] Surveyors were instructed to reveal this information if asked about it, but there was only a single report of this happening.

tify VSL under observational revealed preference approaches and demonstrates that these estimates are unbiased only if all individuals have mortality beliefs perfectly aligned with the econometrician's estimate of empirical risk.

Consider a set of individuals indexed by $i \in \{1, \ldots, I\}$. I assume that individuals maximize expected utility and that their beliefs about motorcycle mortality risk and helmet effectiveness evolve according to a Bayesian process. Belief formation is modeled as Bayesian to illustrate how common learning models can generate bias in observational estimates that use empirical risk as a proxy for beliefs. To identify VSL, I require only that the information presented in the study changes beliefs, which I show 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

$$Pr(D|H; \mathcal{I}_0) \sim Beta(\alpha_{0H}, \beta_{0H}) \tag{1}$$

where $\mathcal{I}_0$ denotes the individual's baseline information set. I temporarily exclude the subscript i for clarity, but I do not assume that beliefs are constant across individuals. Taking the expectation

$$H_0 \equiv \mathbb{E}[Pr(D|H; \mathcal{I}_0)] = \frac{\alpha_{0H}}{\alpha_{0H} + \beta_{0H}} \tag{2}$$

Now suppose that the consumer receives a signal from the surveyor that the estimated efficacy of helmets is $\theta_H \sim Binomial\left(\alpha_{EH} + \beta_{EH}, \alpha_{EH}/(\alpha_{EH} + \beta_{EH})\right)$. Then their posterior beliefs about the efficacy of helmets are given by

$$Pr(D|H;\mathcal{I}_1) \sim Beta(\alpha_{0H} + \alpha_{EH}, \beta_{0H} + \beta_{EH}) \tag{3}$$

and the expected value is

$$H_1 \equiv \mathbb{E}[Pr(D|H;\mathcal{I}_1)] = \frac{\alpha_{0H} + \alpha_{EH}}{\alpha_{0H} + \alpha_{EH} + \beta_{0H} + \beta_{EH}} \tag{4}$$

If $\frac{\alpha_{0H}}{\alpha_{0H}+\beta_{0H}} \neq \frac{\alpha_{EH}}{\alpha_{EH}+\beta_{EH}}$, the consumer's posterior mean will differ from their prior. The degree to which their posterior will update depends on the magnitude of bias in initial beliefs, how diffuse their prior is, and how diffuse the signal is.

Similarly, suppose that the agent has a prior about the probability per trip of getting into a fatal motorcycle accident without a helmet

$$Pr(A|\mathcal{I}_0) \sim Beta(\alpha_{0A}, \beta_{0A}) \tag{5}$$

If agents learn about risk through their own experiences or their social network then their beliefs will likely vary from empirical estimates. Learning from one's own experiences is subject to survivorship bias and learning through one's social network is prone to hasty generalization since accidents are rare (Rabin, 2002). In addition, a higher likelihood of learning about fatal trips, which recent research suggests is likely, would cause agents to overestimate risk (Fang and Ben-Miled, 2017).

Suppose the consumer completes n motorcycle taxi rides over the lifespan of a helmet. I assume that $n_i = n$ is constant for simplicity, but results are similar using consumers' expectations of the parameter. Their baseline expectation of mortality risk over this time frame without a helmet is given by

$$r_{in} = 1 - \int_{A=0}^{1} (1-A)^{n_i} Pr(A|\mathcal{I}_0) \tag{6}$$

I assume that individuals involved in an accident that would otherwise be fatal are deterred from continuing to use motorcycles.[12] Hence, the agent's subjective probability of suffering a fatal motorcycle accident as a function of their information set $\mathcal{I}_i$ is given by

$$r_{ih}(\mathcal{I}) = \begin{cases} H_0 \cdot r_{in}, \mathcal{I}_i = \mathcal{I}_{i0} \\ H_1 \cdot r_{in}, \mathcal{I}_i = \mathcal{I}_{i1} \end{cases} \tag{7}$$

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. Denote their flow utility of consumption by $u(c_i; x_i)$ and denote that expected utility from not being alive by $u_d(x_i)$.[13] The agent's expected utility from purchasing a helmet is given by

$$U_{ih} = \zeta_h + (1 - r_{ih}) \cdot u_a(x_i) - p_i \cdot u'(c_i; x_i) + r_{ih} \cdot u_d(x_i) + \epsilon_{ih} \tag{8}$$

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

$$U_{in} = (1 - r_{in}) \cdot u_a(x_i) + r_{in} \cdot u_d(x_i) + \epsilon_{in} \tag{9}$$

[12]This is based on discussions with respondents during the pilot and the fact that people often quit using motorcycles after serious accidents. Absent this assumption, $r_{ih0} = \frac{H_0 \cdot r_{in}}{1 - H_0 \cdot r_{in}}$. Results are similar using this calculation, and the differences between the two values is small since the probability of suffering two accidents serious enough to be fatal without a helmet is low.

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

Setting $\epsilon_i = \epsilon_{ih} - \epsilon_{in}$ and $\Delta r_i(\mathcal{I}_i) = r_{in} - r_{ih}(\mathcal{I}_i)$

$$U_{id} = U_{ih} - U_{in} = \zeta_h + \Delta r_i \cdot (u_a(x_i) - u_d(x_i)) - p_i \cdot u'(c_i; x_i) + \epsilon_i \tag{10}$$

Our goal is to identify VSL, the marginal change in income needed to compensate an agent for a marginal change in mortality risk. Totally differentiating,

$$dU_{id} = \frac{\partial U_{id}}{\partial \Delta r_i} d\Delta r_i + \frac{\partial U_{id}}{\partial p_i} dp_i \tag{11}$$

Setting $dU_{id} = 0$,

$$VSL \equiv \frac{dp_i}{d\Delta r_i} = \left(\frac{\partial U_{id}}{\partial \Delta r_i}\right) \Big/ \left(\frac{\partial U_{id}}{\partial p_i}\right) = \frac{u_a(x_i) - u_d(x_i)}{u'(c_i; x_i)} \tag{12}$$

This expression illustrates why VSL is likely to vary with income. Theory suggests $u_a(x_i)$ will increase and $u'(c_i; x_i)$ will permanent income.

For simplicity, suppose that $\beta = u_a(x_i) - u_d(x_i)$ and $\alpha = u'(c_i; x_i)$ are homogeneous. Denote the econometrician's estimate of the probability that a helmet will save an individual's life by Δr_i^*. A common set of identifying assumptions in the VSL literature is that $\Delta r_i = \Delta r_i^*$ for all i and that ϵ_i is distributed IID logit with mean 0 (eg León and Miguel, 2017). Then

$$Pr(y_i = 1|\mathcal{I}) = \Lambda\left(\zeta_h + \beta\Delta r_i - \alpha p_i\right) \tag{13}$$

and α, β may be identified from data on helmet purchases, empirical risk, and prices. This argument depends on a strong assumption of rational expectations. If $Pr(\Delta r_i \neq \Delta r_i^*) > 0$, the estimate will not be consistent.[14] This assumption has been made because of a lack of alternative methodology capable of producing meaningful VSL estimates since subjective beliefs are measured with noise.[15]

This study avoids the need to use empirical risk estimates by exogenously updating agents' information sets to produce an instrument for beliefs. Let v_i denote individual i's willingness to pay for a helmet. Then

$$\zeta_h + \beta\Delta r_i - \alpha v_i + \epsilon_{ih} = \epsilon_{in}$$

$$\alpha v_i = \zeta_h + \beta\Delta r_i + \epsilon_{ih} - \epsilon_{in}$$

$$v_i = \frac{1}{\alpha}\zeta_h + VSL\Delta r_i + \frac{1}{\alpha}\epsilon_i \tag{14}$$

Hence, VSL is identified from data on v_i and potentially mismeasured subjective beliefs $\Delta r_i(\mathcal{I})$ using exogenous variation in $\mathcal{I}$. It may be estimated using two-stage least squares or generalized method of moments without requiring any distributional assumptions about unobserved components of utility.

Assuming that the econometrician also observes data on expected remaining lifespan, LS_i, then one may show that the value of a statistical life year (VSLY) is identified since

$$v_i = \frac{1}{\alpha}\zeta_h + VSLY\Delta r_i \cdot LS_i + \frac{1}{\alpha}\epsilon_i \tag{15}$$

[14]Even if empirical beliefs and calculated risks align on average, but not for all individuals, there will be attenuation bias unless an instrument is used.

[15]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."

4 Data and empirical specification

4.1 Data

I use data from 1,571 surveys completed in two waves. The first wave was conducted between October and December 2022, and the second between February and March 2023. The first wave consisted of a total of 921 surveys, including pure control observations, and the second wave included 650 surveys.

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 during a pilot exercise. Details of the final approach are presented in Appendix A and the full survey is available on the AEA RCT Registry. The survey elicits priors about the per trip mortality risk of a motorcycle taxi, 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.

Table 1 summarizes demographics of the sample and demonstrates balance across treatment arms. Table 2 is similar, but it examines motorcycle taxi use and prior beliefs. These outcomes are also generally balanced across experimental groups, although there is some imbalance between the pure control and other arms.[16]

[16] Appendix Table A1 and Appendix Table A2 demonstrate that non-response rates are similar across treatment arms and that response rates are high. Despite the sensitive nature of questions about one's mortality risk, only about 2% of respondents declined to answer.

4.2 Experimental VSL estimation

The primary estimate of the value of a statistical life is obtained via the two-stage least squares regression model

$$
\begin{aligned}
v_i &= \zeta_h + VSL\Delta r_i + X_i'\gamma_0 + \gamma_1 r_{0,i} + \epsilon_i \\
\Delta r_i &= Z_i'\pi + X_i'\pi_c + \pi_r r_{0,i} + \nu_i
\end{aligned}
\tag{16}
$$

where v_i is the respondent's willingness to pay for a helmet, Δr_i is the reduction in mortality risk that a respondent perceives over the lifespan of a helmet, X_i is a vector of controls, and $r_{0,i}$ is the respondent's baseline belief about their mortality risk without a helmet. Z_i denotes a vector of excluded instruments. I report results with two sets of instruments. First, I consider an "interacted" set of instruments $Z_i = (T_i', r_{0,i} \cdot T_i')'$ where T_i is a vector of treatment assignment indicators. The second set of instruments consists of treatment assignment only, $Z_i = T_i$.

The preferred instrument is the interacted version because it absorbs baseline heterogeneity in beliefs, leading to power gains unless VSL heterogeneity is correlated with $r_{0,i}$. Intuitively, if there is heterogeneity in the perceived risk of motorcycles, then Δr_i will vary both due to differences in risk and beliefs about the efficacy of helmets. If heterogeneity in perceived motorcycle accident risk is large then VSL will be less precisely estimated using treatment assignment only than the interacted instrument that accounts for priors. This set of instruments is similar to that studied in Abadie et al. (2023), which shows that the interacted first stage yields improved asymptotic mean squared error.[17]

[17] I pre-specified two sets of instruments before the first wave of the experiment, but the interacted version varied from what we use in the paper. The original PAP specified the use of n_i, the number of motorcycle taxi trips taken in a typical week, in place of $r_{0,i}$. The logic was the same, but I made the assumption that n_i, which is important in determining empirical risk, would be an effective proxy for beliefs. Baseline beliefs were in fact orthogonal to n_i during wave 1, so it does not help account for heterogeneity. Hence, I filed a PAP amendment before wave 2 to instead use $r_{0,i}$. The treatment only model is unmodified from the original PAP.

Controls are selected using single-post LASSO. The set of possible controls includes demographic variables, motorcycle trip characteristics, and the information sources used to construct beliefs about mortality risk. Estimates also include surveyor fixed effects because surveyors were assigned to different parts of the city.[18]

I follow the PAP and report results over two samples. First, I use data from the control and both treatment arms. Second, I restrict the sample to treated respondents to demonstrate that results are not driven by an endorsement effect associated with informing individuals that academic studies show helmets are effective.

I report homoskedastic standard errors and weak instrument robust confidence sets in the primary tables. Results are similar using GMM with heteroskedastic robust errors or 2SLS and two-stage cluster bootstrapped inference (Abadie et al., 2022). Appendix B provides details, including justifications for these decisions.

4.3 Observational VSL estimation procedures for comparison

I implement two observational revealed preference approaches to estimating VSL similar to those used in recent publications. First, I estimate the OLS regression

$$v_i = \zeta_h + VSL\Delta r_i^* + X_i'\gamma_0 + \epsilon_i$$

where Δr_i^* is the empirical likelihood that a helmet will save the respondent's life, estimated from ridership and treating the point estimate from one of the two helmet studies as true. This approach

[18]Due to an error, the initial PAP only listed the demographic variables. The PAP amendment filed before wave 2 specified the full set of potential controls. Enumerator FEs were also added in the PAP amendment. Results are similar if they are excluded.

is similar to León and Miguel (2017) and Ito and Zhang (2020) which estimate logit demand models using empirical risk estimates as a proxy for beliefs. I similarly estimate a demand using empirical estimates of the mortality risk from a helmet, but the specification is linear since willingness to pay is observed. Following León and Miguel (2017), I do not instrument for risk.

Second, I report the estimates

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

This approach assumes that agents do not receive utility from any characteristics of helmets other than their life saving potential and that $\Delta r_i = \Delta r_i^*$ for all i. This specification is identical to Berry et al. (2020) and is similar to Kremer et al. (2011). The estimator is applied in the context of safety products, so the assumption that agents only consider the safety benefits of products is plausible. This approach allows one to estimate VSL in settings where cross-sectional variation in mortality risk is small or not observed. Neither approach separates willingness to pay for non-fatal illness or injury prevention from the agents' demand for mortality risk reduction, so authors often encourage readers to interpret estimates as upper bounds (e.g. León and Miguel, 2017; Kremer et al., 2011).

5 Results

5.1 Validating belief measurement

Elicited beliefs about the likelihood of suffering a fatal motorcycle accident fall in plausible ranges and are strongly correlated across measurements. This suggests that the data capture an informative signal about respondents' true beliefs.

The mean baseline five year mortality risk is 0.034, the median is .001, the 1st percentile is 10^{-7}

and the 99th percentile is 0.5. I estimated that the median empirical risk was about 1 in 4,000, but the NTSA data used to calculate empirical risk may underestimate deaths because the NTSA only record fatalities in which a victim is dead on arrival (Muguro et al., 2020). Responses about the number of deaths per 10,000 passengers per 5 years and the assessments of one's own risk were highly correlated ($p < .01$, $R^2 > 0.1$), although the median consumer believed that they were only about 20% as likely to die as an average Kenyan. This would be consistent with overconfidence in one's own safety.

Beliefs about the 5 year and 1 year mortality rates of motorcycle passengers are strongly correlated. The R^2 between the measures exceeds 0.57, and for over 60% of respondents, the 5 year estimate is between 4 and 6 times their 1 year estimate. On average, the 5 year response was 5.24 times the 1 year response. Only 15 respondents (less than 1%) stated that they believed more people would die over the next 1 year than the next 5 years, which is impossible.[19] The respondents' belief about the 5 year risk of an average Kenyan were also positively related with their beliefs about the per trip risk of a fatal accident ($p < .01, R^2 = 0.2$).

The right tail of responses raises a concern that some respondents may have trouble understanding or communicating probabilities. It is striking that one would choose to ride motorcycles if they felt that the risk of dying is 50% over 5 years. However, respondents that gave very high responses reported them consistently high across measurement approaches. Surveyors were also instructed to ask after the survey why they chose to ride motorcycle taxis if they felt the risk was so high. Typical responses were that they had been told that motorcycles were extremely dangerous, which they interpreted as a value such as 1 in 2. When asked why they still chose to use them, respondents indicated that they either had no safer way of commuting for work or that they were

[19]VSL estimates are similar if these observations are excluded.

aware that the risk was extreme but felt that it was acceptable, often due to strong religious beliefs. Hence, extreme responses seem to reflect very high assessed probabilities of suffering a fatal accident, not measurement error.

5.2 Correlates with beliefs

Respondents reported forming beliefs primarily based on their own experiences and information from their social network. Consistent with these accounts, I find that variables indicating whether the respondent or someone in their social network suffered a prior accident are strongly correlated with beliefs. This provides further evidence that subjective belief data is informative and raises concerns that empirical estimates may be a poor proxy for beliefs.

Figure 3 plots the information sources that respondents reported using to form their beliefs about the risks of motorcycle taxis. The most common source is own experience (79%), followed by family members (48%) and social media (38%). Over 95% of respondents stated that they considered their own or others' experiences. In contrast, under a third of respondents reported that media or government sources, which are more objective, informed their views about safety. Beliefs should therefore be correlated with the accident history of oneself and their social network. Furthermore, the fact that consumers overwhelmingly form their beliefs based on personal experiences suggests that they are unlikely to coincide with empirical estimates.[20] This suggests that beliefs need not be correlated with empirical determinants of accident risk.

Table 3 confirms these predictions. In columns 1, I estimate the least squares fit of one's prior belief of dying in a motorcycle accident on ridership volume. Panel a reports results without

[20]As noted in Section 3, even if the agent's social network is perfectly representative and survived trips are just as likely to be reported as deaths, accidents are sufficiently rare that agents forming beliefs based on their social network's experiences are likely to be biased.

controls, and panel b reports estimates with taxi terminal fixed effects and covariates selected using double-post LASSO (Belloni et al., 2014). The correlation is negative in panel a and positive but insignificant once covariates and fixed effects are added. This suggests that agents do not learn from unbiased sources since empirically those that ride motorcycles more often are more likely to die on them. In fact, a 1 percent increase in reported motorcycle taxi trips in a typical week is associated with about a 0.124 percentage point increase in the probability of reporting a prior accident, and this relationship is strong with or without taxi terminal fixed effects ($p < .01$). Column 2 show that the length of one's average trip, which one would again expect to be correlated with unbiased estimates of risk, is also not strongly correlated with beliefs.

Columns 3 and 4 examine the correlation between beliefs and an indicator for whether the respondent suffered a prior motorcycle accident and an indicator for whether a member of the respondent's social network suffered a motorcycle accident. Consistent with stated information sources, those that suffered a prior accident perceive higher risks of using motorcycle taxis, although this relationship is not statistically significant once controls and terminal fixed effects are added. Individuals that know someone that has experienced an accident also perceive motorcycle taxis as significantly more dangerous, and this relationship survives the addition of controls.

The final column of table 5 reports regressions of elicited beliefs about suffering a fatal accident over the next 5 years on an indicator equal to one if it was raining during the survey. If agents have rational expectations, then their assessment of the long-run dangers of motorcycles should be independent of the weather when responding. However, the psychology and economics literature suggests that rainfall could make the dangers of motorcycles more salient (Bordalo et al., 2013). Respondents surveyed on a day that it rained assessed that they were almost twice as likely to die in a motorcycle accident ($p = .013$). This suggests that agents do not have rational expectations, so leveraging elicited belief data is important to consistently estimate VSL.

5.3 First stage effects on beliefs

Table 4 demonstrates that randomized information exposure had a statistically significant effect on the agent's posterior belief that a motorcycle helmet will save their life.

I first examine the effect of receiving an estimate of one's empirical likelihood of dying in a motorcycle accident without a helmet on beliefs in columns 1 and 2. Respondents did not update their beliefs based on this information. There is not a significant difference between either treatment arm, who received identical information, and the control group. Surveyors reported that respondents understood the information being presented, but they did not trust the estimates because of a view that the NTSA is ineffective at reporting accidents. The fact that respondents did not report updated beliefs in response to this information, and that they felt confident telling surveyors that they did not trust the information, provides evidence that surveyor demand effects are not present in this experiment.

Respondents did update their beliefs about the effectiveness of helmets based on the studies presented in the low and high treatments, which is the primary variation used by the interacted instruments. Contradicting the study team's priors, respondents overestimated the effectiveness of helmets at baseline and updated their beliefs downward when exposed to information. Table 4 reports treatment effects on beliefs about the effectiveness of helmets in columns 3 and 4. The mean belief about helmet effectiveness reported in the control group was about 79%, and many control respondents stated effectiveness beliefs over 90%. The average in the high treatment group was about 75%, and that in the low treatment group was about 65%. The differences between the treatments arms are significant at the 1% level.

During piloting and the start of data collection respondents' beliefs about the effectiveness of helmets did not significantly update. Surveyors reported that respondents were asking questions about the studies that they did not know how to answer, such as inquiring about causes of death observed with helmets. Difficulty responding to these questions caused respondents to doubt the credibility of the information. Surveyors were trained with details about the studies so that they could respond to questions. After this training, the information treatments affected beliefs and average posteriors fell between the study estimates and the control average posterior. The fact that respondents did not update beliefs until surveyors could provide adequate details about the studies provides further evidence that surveyor demand effects are not binding.

Despite a lack of updating about motorcycle accident risks, the change in beliefs about helmet effectiveness were sufficient to detect a change in the probability that a helmet would save the respondent's life. Columns 5 - 6 of Table 4 report the change in the perceived lives saved per 10,000 people associated with helmet use over 5 years. Beliefs about accident risk and helmet effectiveness among those assigned to the low treatment arm correspond to around 80 fewer lives saved on average compared to the control group. This effect is driven in part by outlying beliefs: the difference falls to about 46 fewer lives if data is winsorized at the 2nd and 98th percentiles. These differences are significantly different from the control and high treatment beliefs at the 1% level. There is not a significant difference between the high treatment and control means, although point estimates change in the expected direction. Overall, one can reject the null hypothesis that the information treatments did not update beliefs, meaning that treatment assignment is a viable instrument for estimating VSL.

5.4 The value of a statistical life

Table 5 demonstrates that demand for safety is low in this sample. The preferred estimate of the value of a statistical life uses the interacted set of instruments and controls selected using LASSO. This value is reported in column 3 of panel a. The point estimate of the value of a statistical life is just USD PPP $224, and a weak instrument robust confidence set rejects values below $34 and above $429 with 95% confidence. Results are similar without controls, using the non-interacted instrument, and if willingness to pay data is winsorized. However, there is a drop in power in these specifications.

Panel b shows that results are similar if the sample is restricted to those that were treated. These results are robust to potential confounds associated with an endorsement effect driven by presenting respondents with rigorous data showing that helmets are effective, but there is a loss in power since $1/3$ of the sample is excluded. Despite the loss in power, estimates place informative bounds on VSL. One may reject VSL estimates above $1,038 with 95% confidence using the preferred specification and above $2,726 across all specifications.

Estimates of VSL are similar under a battery of robustness checks. Table 5 exclude 33 observations that the field team reported were contaminated by motorcycle drivers pretending to be passengers and submitting false survey responses in order to receive a free helmet.[21] Results do not substantially change, although confidence sets are wider. Appendix Table A7 demonstrates that results also hold if beliefs about the probability that a helmet will save one's life are winsorized. Appendix Table A6 shows that the results are robust to changes in future ridership due to receiving

[21] This occurred on the second day of data collection because teams returned to an area that they had already visited and motorcycle drivers recognized them. I exclude all observations from this day to be cautious. Survey protocols were adjusted to avoid this after that point. This decision was filed in a PAP amendment before the second wave of data collection. Appendix Table A3 reports first stage results excluding these observations and Appendix Table A5 reports results with the problematic surveys included.

a helmet (panel a) or subjective beliefs about the lifespan of a helmet (panel b). Panel c of Appendix Table A6 suggests that results do not disproportionately weight individuals that select into high motorcycle ridership because of a weak preference for safety.

Although estimates of VSL are low in this sample, they are not inconsistent with theory or much higher estimates among wealthy populations. Recall from section 3 that VSL is a function of one's expected utility from being alive less one's expected utility from not being alive normalized by the marginal utility of consumption. I show in section 5.6 that the differences between these estimates and estimates over $700,000 from León and Miguel (2017) can be rationalized by admissible curvature of the marginal utility of consumption with income.

More speculatively, qualitative evidence suggests that strong beliefs in an afterlife may also contribute to low demand for safety. Religious beliefs are strong in Kenya, and there is near universal belief in an afterlife. The fact that VSL falls well below annual income could indicate that agents expected utility after death is high. Qualitative responses from respondents match this interpretation. However, the study does not include data on religious beliefs, so this hypothesis cannot be rigorously tested. León and Miguel (2017) similarly suggest that fatalistic beliefs may lower the VSL of some consumers.

Estimates of the value of a statistical life year (VSLY), presented in Table 6, similarly show low demand for safety. Result above $61 are rejected across all specifications.

5.5 Heterogeneity in the value of a statistical life

If there is large heterogeneity in VSL then estimating the parameter over a selected population may yield estimates that vary from the average VSL over populations relevant to policy decisions. The estimates presented in this paper may be interpreted as the average VSL over a broad sample of urban commuters, a sample that is of a priori interest for transportation policy. However, I examine the degree to which VSL is heterogeneous across 8 different variables in Table 7 to provide insight into the extent to which the average VSL over all residents of Nairobi may differ from these estimates. I find evidence of heterogeneity that is consistent with economic theory, but estimates are sufficiently small that it is unlikely to change the conclusion that willingness to pay for safety is low.

In column 1, I examine heterogeneity by age. Since a younger respondent has more life years remaining in expectation, age would be negatively correlated with VSL if VSLY were homogeneous. However, I find that VSL is the same among respondents below and above the median age of 32. These results are consistent with Aldy and Viscusi (2007) which shows that VSL peaks around age 40 in high-income settings.

As discussed in the model, theory predicts that VSL should increase with income. Consistent with the theory, I find an economically significant interaction between VSL and wages in column 2. The second row reports an estimate of the interaction between the coefficient on the mortality risk reduction offered by a helmet and the log of the respondent's income, demeaned so that the first row captures the average VSL in the sample. The point estimate indicates that a \$1 increase in wages is associated with about a \$0.9 increase in VSL. If VSL increases linearly with income, this point estimate suggests that a consumer with income in the 90th percentile in this sample has a VSL over twice as large as the average respondent. Column 3 shows that agents with above median expected future wage growth also have much higher demand for safety, providing further evidence that VSL varies positively with income.

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.

5.6 Experimental versus observational estimates

Analysis of belief data reported in section 5.2 suggests that individuals do not have rational expectations. Consistent with this fact, I show in Table 8 that observational estimates of VSL generally do not fall in experimental confidence sets. In addition, different observational approaches produce dramatically different estimates of demand for safety. In order to calculate the empirical likelihood that a helmet will save an individual's life, the econometrician must take a stance of the efficacy of helmets. Panel a reports results assuming that the Liu et al. (2008) estimate of 42% is the truth, and panel b reports estimates using 70% efficacy as estimated in Ouellet and Kasantikul (2006). My discussion focuses on panel a.

Columns 1 - 4 report estimates of VSL obtained from the demand model that uses empirical risk estimates as a proxy for beliefs. The first column does not include any covariates. Columns 2 - 4 are estimated using double-post LASSO to select controls. Column 3 adds enumerator fixed effects and column 4 includes taxi terminal fixed effects. Estimates are generally somewhat sensitive to the covariates and fixed effects included, although confidence intervals are wide. The point estimate is over \$15,000 without controls and is negative in column 3. Columns 2 and 4 produce point estimates that are close to experimental estimates, about \$2,700 and just under \$2,000 respectively. This provides the most promising evidence that demand estimates that assume rational expectations can consistently estimate VSL. However, standard errors are large and these results may simply be driven by a weak correlation between willingness to pay and ridership, rather than the approach accurately estimating demand for safety.

The fact that standard errors are so large even in a context where mortality risk is high illustrates the challenges of estimating VSL in a setting where willingness to pay for safety is low. With low demand for safety, demand residuals may be high even if beliefs are accurately measured because variation in safety benefits may not be the primary driver of demand. Since the asymptotic variance of VSL estimates is essentially the average squared residual of the demand estimate normalized by variance in mortality risk, VSL is difficult to precisely estimate if it is low since the percentage point likelihood of any given safety product saving the life of a representative person is small. This may make observational estimates of VSL in LMICs particularly prone to publication bias since researchers may interpret a point estimate near zero with wide confidence intervals as evidence that they lack identifying variation. This paper is able to produce precise experimental estimates of VSL despite this challenge in part because respondents tend to believe that risks are higher than empirical data suggests.

An alternative approach to estimating VSL from observational data that can produce precise estimates even if the parameter is low is to assume that agents only value safety products because of their life saving potential. Berry et al. (2020) reports VSL that use this approach and elicited willingness to pay data, and Kremer et al. (2011) report estimates that depend on similar assumptions. I report estimates using this approach in column 5. The mean VSL estimate obtained from this approach is over $380,000, and the confidence interval excludes values below $354,000. Hence, one may easily reject the null hypothesis that the estimates match experimental estimates of VSL (recreated in column 6).

Column 7, which re-weights an experimental estimate to match OLS, shows that the differences between observational and experimental estimates cannot be rationalized by differences in the local average treatment effects that they estimate.[22]

[22]Assuming the OLS model is correct, it weights observations proportionally to the square of empirical risk re-

5.7 Comparisons to existing revealed preference VSL estimates

The VSL estimates presented in this paper fall below most existing revealed preference estimates from low-income settings. However, values are similar to estimates calculated over populations with similar income levels. Figure 4 plots point estimates and standard errors from all revealed preference estimates of VSL from LMICs that I am aware of.[23] This includes Kremer et al. (2011), León and Miguel (2017), Berry et al. (2020), Ito and Zhang (2020), and Shrestha (2020). In addition, I include a revealed preference estimate from Greenberg et al. (2021) which examines a population of US soldiers for comparison to a high-income setting.

The dispersion of VSL estimates is large across existing studies. León and Miguel (2017) estimate an average VSL over \$700,000 among wealthy international travelers, with an upper confidence interval over \$1.4 million. In contrast, one of the median VSL estimates presented in Berry et al. (2020) is negative, and the confidence interval reported in Shrestha (2020) includes values below -\$600,000. To accommodate this range of values, I plot estimates using a modified scale that is approximately linear near zero but logarithmic away from zero and truncate values below -\$1.

Point estimates presented in this paper are lower than most prior values. However, estimates are similar to those among populations with similar income levels. For instance, Kremer et al. (2011) estimate a value of about USD \$1,000 to averting a child death in rural Kenya, and Berry et al. (2020) estimate median VSLs below \$0 and just over \$4,000 in Ghana. Estimates orders of

duction after partialling out controls, $(\Delta \dot{r}_i^*)^2$. I compare these to a simplified experimental estimate that pools high treatment and control observations and uses only low treatment assignment as an instrument, in which case observations are weighted proportionally to $\pi_1 + \pi_2 r_{0,i}$ where π_1 is the first stage coefficient on low treatment and π_2 is the first stage coefficient on $r_{0,i}$ interacted with low treatment assignment, which I estimate. Hence, I assign weights $w_i = (\Delta \dot{r}_i^*)^2/(\hat{\pi}_1 + \hat{\pi}_2 r_{0,i})$.

[23] I convert estimates to 2022 dollars using the change in the CPI from January of the paper's publication year to November, 2022.

magnitude larger from Ito and Zhang (2020) and León and Miguel (2017) examine populations with much higher incomes close to \$10,000 and \$75,000 respectively.

If we take all VSL estimates as given, there is non-linear growth of VSL with income consistent with Hall and Jones (2007). As a thought experiment, one may assume that one's utility from being alive is constant across income levels and then calculate what curvature of marginal utility of consumption would be required to rationalize the differences in VSL observed across low and high-income settings. Assuming a CRRA utility function, the differences between my preferred point estimate of VSL and those reported in León and Miguel (2017) would imply a coefficient of relative risk aversion of $\theta < 3$, and $\theta < 2$ if I instead consider a VSL estimate of \$3,000 from the upper range of confidence intervals.[24] These values fall within admissible estimates of θ. In fact, Havránek (2015) find a mean elasticity of inter-temporal substitution of around $1/3$ in a meta-analysis of the parameter, which translates to $\theta = 3$ under power utility.

5.8 Policy implications of experimental VSL estimates

The welfare implications of the experimental VSL estimates presented in this paper depend on the values that are currently used in benefit-cost analysis in sub-Saharan Africa. If decision makers employ values similar to this paper, such as Kremer et al. (2011), then benefit-cost ratios will be similar to those implied by this paper. But if larger VSL estimates are considered, then switching to the estimates from this paper could yield large welfare gains by better aligning policy with preferences.

I selected 5 recently published benefit-cost analyses in Kenya to examine how these VSL esti-

[24] Assume $\beta_i = \beta, \alpha_i = Y_i^{-\theta}$ where Y_i is income. Then $\frac{VSL_1}{VSL_2} = \left(\frac{Y_1}{Y_2}\right)^{\theta} \Rightarrow \theta = \log\left(\frac{VSL_1}{VSL_2}\right) / \log\left(\frac{Y_1}{Y_2}\right)$. Then plugging in the estimates from the two studies, $\theta \approx \frac{\log(700{,}000/224)}{\log(75{,}000/4{,}750)} = 2.91$

mates affect policy conclusions. I identified relevant studies by searching for benefit-cost analyses on Google Scholar that referenced Kenya and utilized VSL (or VSLY). The 5 analyses selected, which come from 4 different studies, are the 5 most recent that I was able to locate. Two benefit-cost analyses are from Babagoli et al. (2022) which evaluates a program to provide menstrual cups and sanitary pads to young women, one is from Mwai et al. (2023) which examines primary health care investment, one is from Hamze et al. (2017) examining a cleft lip and palate repair program, and the final value was published in Oyugi et al. (2023) which studies a free maternity program.[25] I examine how my VSL estimates change the policy conclusions of these studies by replacing the original VSL and VSLY estimates used to value health benefits with the preferred estimates from this paper.

Figure A2 shows that benefit-cost ratios (BCRs) drop substantially when I replace the studies' VSL and VSLY values with the estimates from this paper. BCRs fall by over 99% on average, and in 4 out of 5 instances the ratio falls from a value above 1 to a value below 1, changing the conclusion about whether the programs are welfare maximizing. These differences are driven by the fact that the studies value lives and life years saved an order of magnitude higher than consumers in this study: the studies value a disability-adjusted life year saved at over $3,000 or a statistical life saved at over $200,000.

If the VSL estimates from this paper are correct, development assistance donations may also be significantly misallocated. For instance, GiveWell, a large non-profit that matches donor funds to charities which has directed over USD 1 billion in donations, weights averting a death of someone aged 15-49 104 times as highly as doubling their consumption for a year.[26] These "moral weights,"

[25]If multiple estimates were published for the same program, I selected the one that produces the smallest benefit-cost ratio.

[26]Based on the 2023 update on GiveWell's moral weights.

an application of VSL in practice, have concrete affects on how aid is distributed. The value is used to calculate the cost effectiveness of programs, which informs which programs are recommended and eligible for certain funds. Consistent with their high estimate of demand for safety, the four "Top Charities" identified by GiveWell in their July 2023 update all focused on preventing death, rather than reducing poverty.

These weights are based in part on stated preference VSL estimates of over USD $38,000 from a rural Kenyan context (Redfern et al., 2019). In contrast, the VSL estimates presented in this paper indicate that consumers would value a doubling of their annual income more than 10 times as highly as averting a death on average.[27] Hence, this paper suggests that weighting consumption gains more strongly compared to health improvements in policy and aid decisions could yield substantial social welfare gains.[28] The following section discusses potential limitations to these results which could affect this conclusion.

5.9 Potential threats to the validity of VSL estimates

Evidence presented in section 5.4 shows that VSL estimates are robust to a large set of robustness checks. Three other concerns with the design of this experiment that warrant discussion are that experimenter demand effects, cognitive frictions understanding or articulating small probabilities, or inaccurate willingness to pay elicitation could bias estimation of the parameter. Although it is difficult to definitively reject these confounds, the design of the experiment and evidence suggest

[27] Stated preference VSL elicitation is a non-incentivized approach in which agents are asked how much they would be willing to pay for certain mortality reductions for themselves or others. Walker et al. (2023) also estimate willingness to pay per DALY at USD PPP 3,611 using a stated preference approach, but note that a revealed preference estimate from the same sample derived from Kremer et al. (2011) produces an estimate of USD PPP 67. Stated preference estimates thus seem to overestimate willingness to pay for health compared to incentivized designs.

[28] This argument assumes that funds would be directed towards poverty alleviation, in which case the relevant trade-off is between consumption and safety in a low-income setting. If donations would instead not occur or be made in a high-income setting, this argument may not hold since the trade-off is between safety in a low-income setting and consumption in a high-income setting. The conclusion explains this point in more detail.

that these factors are unlikely to confound estimates.

It is unlikely that experimenter demand effects confound because respondents did not report updated beliefs in response to all of the information they were presented with. If demand effects were present, we would expect respondents to report changes in beliefs in response to information about motorcycle risks and helmet effectiveness, but there were no changes in posteriors about the risks of motorcycles. Furthermore, there were only changes in beliefs about helmet effectiveness after surveyors learned to respond to detailed questions about the information they were presenting and respondents rarely reported that their posterior equaled a study estimate exactly.

Cognitive frictions in understanding small probabilities could similarly cause respondents to systematically misreport beliefs. The study was designed to limit this risk by focusing on one of the largest sources of mortality risk that is faced by a broad segment of the population. Furthermore, respondents that responded with risks well above empirical estimates provided sensible reasons for departing from the data, and we found that stated beliefs were broadly consistent across different measures. These results suggest that the survey elicited a meaningful signal about beliefs, particularly since treatment assignment is used as an instrument to create robustness against measurement error.

The similarity of estimates based on the interacted and non-interacted version of the IV also provides evidence against this confound. The interacted instrument weights individuals that believe motorcycles are dangerous much more heavily compared to the treatment only estimates. If respondents that communicated high risks struggled to communicate small probabilities but truly believed that the risks were lower, then we would expect VSL estimates to be higher using the treatment only IV. However, estimates are similar across the two sets of IVs, and a Hansen J test fails to reject the equality of the estimates ($p > 0.5$). In addition, Table 7 shows that effects are not heterogeneous with respect to education or performance on a digit span recall test, which one would expect to be positively correlated with VSL if this confound were binding.

Third, inaccurate measures of willingness to pay could bias estimates. One possibility is capital constraints. The experiment aimed to mitigate this risk relative to existing work by using a willingness to accept instead of a willingness to pay mechanism, meaning respondents received either a free helmet or a cash payment rather than paying for a helmet. This ensures that liquidity constraints do not bind, but it is possible that capital market frictions could still inflate the value of cash. This is unlikely to significantly bias results since average willingness to pay is small relative to incomes. A second possible avenue of bias is from the Becker et al. (1964) mechanism. A literature suggests that different versions of the mechanism, such as willingness to pay versus willingness to accept, can produce different valuations (Isoni et al., 2011). But these differences are unlikely to bias VSL estimates since the paper leverages changes in WTP with respect to changes in beliefs and not levels, and Cole et al. (2020) and Berry et al. (2020) demonstrate that BDM mechanisms generate variation similar to take it or leave it offers among similar populations. Moreover, the survey implemented a practice round of the game to limit confusion, and surveyors reported strong understanding of the game among respondents.

Although the results in this paper are strikingly robust and the method relaxes many of the strongest assumptions used in VSL estimation, it is important to note that it still relies on assumptions which cannot be tested in this setting that could affect results. One core assumption is that agents maximize expected utility, which a behavioral literature suggests may be inaccurate in certain settings (e.g. List and Mason, 2011). A second is that lives lost do not impose any externalities, which could be inaccurate if agents do not internalize the utility that their loved ones receive from interacting with them or do not bear medical and funeral costs incurred after their death. These topics are outside of the scope of this paper, but they represent an important area of future research.

6 Public policy implications of helmet demand

The primary goal of this study is to generate experimental estimates of VSL and examine how they compare to observational estimates. However, this study is among the first to rigorously estimate demand for motorcycle helmets in East Africa. Helmet producers and road safety organizations attributed low helmet use to a lack of demand prior to this study. Although this study is not designed to evaluate programs to improve helmet use, it tests whether demand is in fact low at observed prices. In addition, the study provides insight into the extent to which information about the risks of motorcycle taxis, the effectiveness of helmets at preventing death, or subsidies could affect demand.

Table 9 indicates that helmet demand is not low because consumers do not understand the safety benefits of helmets. The effects of the treatment are small and average, and presenting consumers with information reduces their willingness to pay for helmets.

Although use of helmets is rare in Nairobi, Figure 5 suggests that demand for helmets may not be low. The figure plots inverse demand and the elasticity of demand among control respondents. [29] The wholesale price of helmets is denoted by a vertical red line. I estimate that over 75% of motorcycle taxi passengers were willing to forego a cash payment at least as large as the wholesale cost of helmets to receive one, and about 60% were willing to forego Ksh 1,000 or more, which is over 170% of the wholesale cost. This suggests that there may be unmet demand for helmets, or that liquidity constraints often bind.

[29] The elasticity of demand is estimated using a local polynomial regression adapted from Berry et al. (2020).

High demand is reconciled with low VSL estimates by two facts. First, as outlined in section 5.1, consumers view motorcycles as extremely dangerous. So the change in willingness to pay induced by the information interventions is small relative to the change in mortality risk. Second, consumers value characteristics of helmets other than protection against fatal accidents. I estimate that the average willingness to pay for a helmet with zero mortality risk reduction would be about \$28 in this sample ($p < .01$).[30] This study was not designed to rigorously identify determinants of helmet demand other than mortality risk protection. However, respondents qualitatively reported valuing protection against non-fatal injuries, which could result in large financial losses due to foregone wages and medical bills.

Further supporting the view of unmet demand, the helmet manufacturer sent vans to taxi stands after seeing these results and reported easily selling a stock of helmets. This suggests that the retail market for helmets may be thin. Qualitative evidence supports this conclusion. Many respondents reported that they never seen a helmet in a store or believed that they were bundled with motorcycles. The low retail availability of helmets may be driven in part by the fact that the manufacturer used for this study had only been producing helmets for about a year and had not engaged in significant advertising at the time of the study. This evidence is of course only suggestive. However, it indicates that research studying the retail market may be valuable. This topic is the focus of ongoing follow-up work studying barriers to helmet adoption.

7 Conclusion

This study presents among the first experimental estimates of the value of a statistical life and introduces a method for experimentally estimating VSL that can be applied in other contexts. I

[30]This is based on a two-stage least squares regression of willingness to pay on an intercept and mortality risk reduction instrumented for via treatment assignment.

estimate that urban East African consumers have low demand for safety. These estimates are much lower than those typically used to inform decisions in low-income settings, suggesting that there is room to substantially improve welfare by better aligning the decisions of governments and NGOs with individuals' preferences. Consistent with this view, I find that the conclusions of 4 out of 5 benefit-cost analyses that I examined flipped when the experimental VSL estimates from this paper were used in place of the originally used values, and I show that the VSL estimates presented in this paper imply that a prominent development aid organization may improve welfare by weighting consumption gains more heavily when recommending programs.

I caution that these estimates of VSL are valid only in instances in which the opportunity cost of a safety program is consumption among Kenyans. This trade-off is applicable when aid organizations decide between programs aimed at improving incomes versus health or for East African governments choosing whether to fund health and safety programs. But it is not relevant when the financial cost of a program is paid for by high-income consumers since in these cases the welfare costs of the program are a function of the high-income, not low-income, consumer's marginal utility of consumption. For instance, if a rich country government were conducting a benefit-cost analysis of a program aimed at reducing an externality causing excess mortality in East Africa, then rich country and not East African consumers would bear the costs of the program. The correct VSL in this case would be a function of an East African's average utility of averting a death normalized by a rich country consumer's marginal utility of consumption. This value cannot be identified without additional structure since utils are subjective (Robbins, 1934). But since economic theory indicates that the marginal utility of a dollar is strongly related to income levels, a reasonable rule of thumb would be to price lives saved by a high-income VSL.

In addition to producing experimental estimates of VSL, this paper is among the first to compare experimental and observational estimates. This provides some of the first evidence about the extent to which biased mortality beliefs and endogeneity, potential problems in observational VSL estimates that have been identified in studies such as Ashenfelter (2006), affect VSL estimates in practice. I find that cross-sectional estimates of VSL are biased, and I present evidence that empirical estimates of mortality risk align poorly with beliefs because agents do not learn from representative data. Barriers to objective information about risk – such as low internet access, low access to high quality journalism, and weak government institutions – raise the concern that agents in other low-income settings are also likely to have biased beliefs. Given the considerable expert disagreement that exists over the precise safety benefits of products such as motorcycle helmets, it also seems possible that high-income consumers may also have beliefs that different from econometric estimates of the safety implications of their decisions. This suggests the need for future research to examine whether beliefs about risk are accurate in other settings. If not, then estimates of VSL that use elicited beliefs rather than empirical risk estimates are important to guide optimal trade-offs between safety and risk.

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Figures

Figure 1: Survey locations

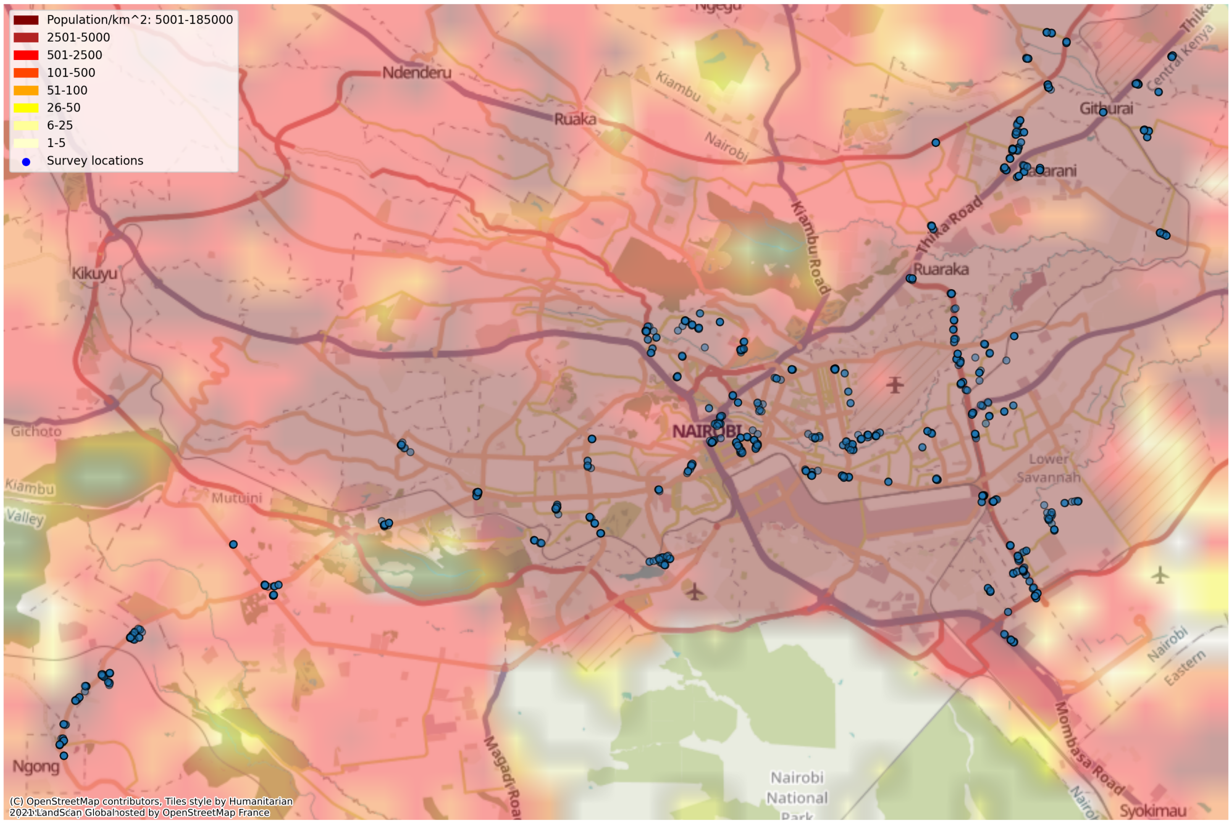
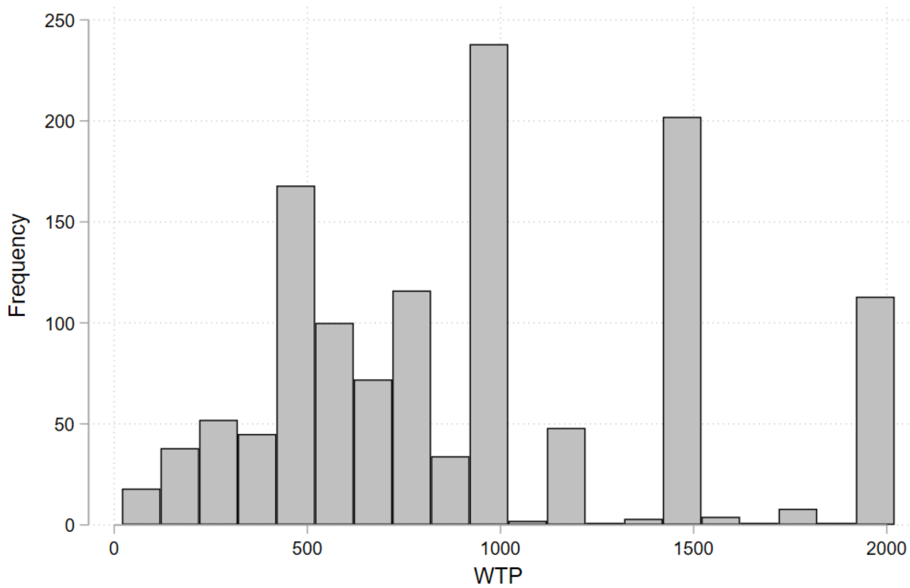


Figure 1 plots survey locations over a map of Nairobi. Color denotes population, where no color denotes no residents. The 2019 census estimated a population of about 4.4 million within the city. Map data is from Open Street Map. Population data is from the LandScan Global 2021 data set produced by Oak Ridge National Laboratory.

Figure 2: Distribution of helmet bids (Kenyan shillings)

(a) A. Histogram of bids, restricted axis



(b) B. Histogram of bids, full distribution

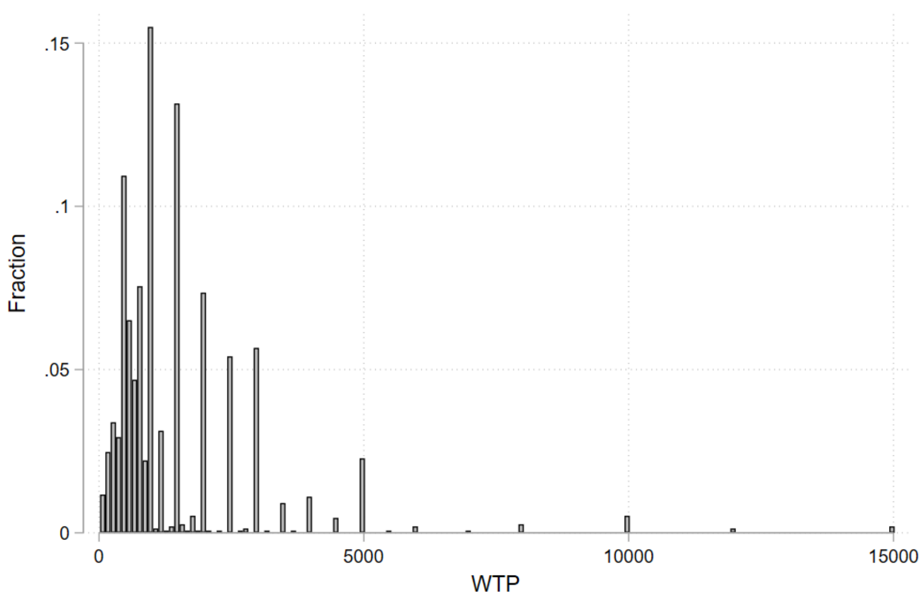


Figure 2 plots the distribution of willingness to pay for helmets in Kenyan shillings (Ksh). Panel A reports a histogram of bids, excluding outliers above Ksh 2,000 for clarity. Panel B reports the same histogram across the full distribution of bids. Figures present Ksh rather than USD to illustrate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

Figure 3: Information sources used to form beliefs about motorcycle safety

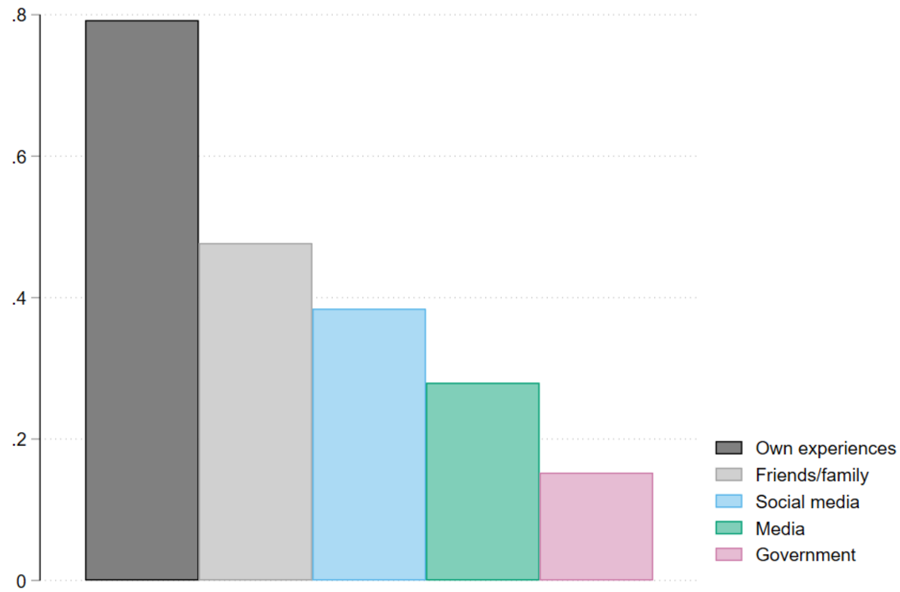


Figure 3 plots the information sources that respondents reported using to form beliefs. Respondents were able to select multiple options, so the columns do not add to 1.

Figure 4: Revealed-preference VSL estimates across studies

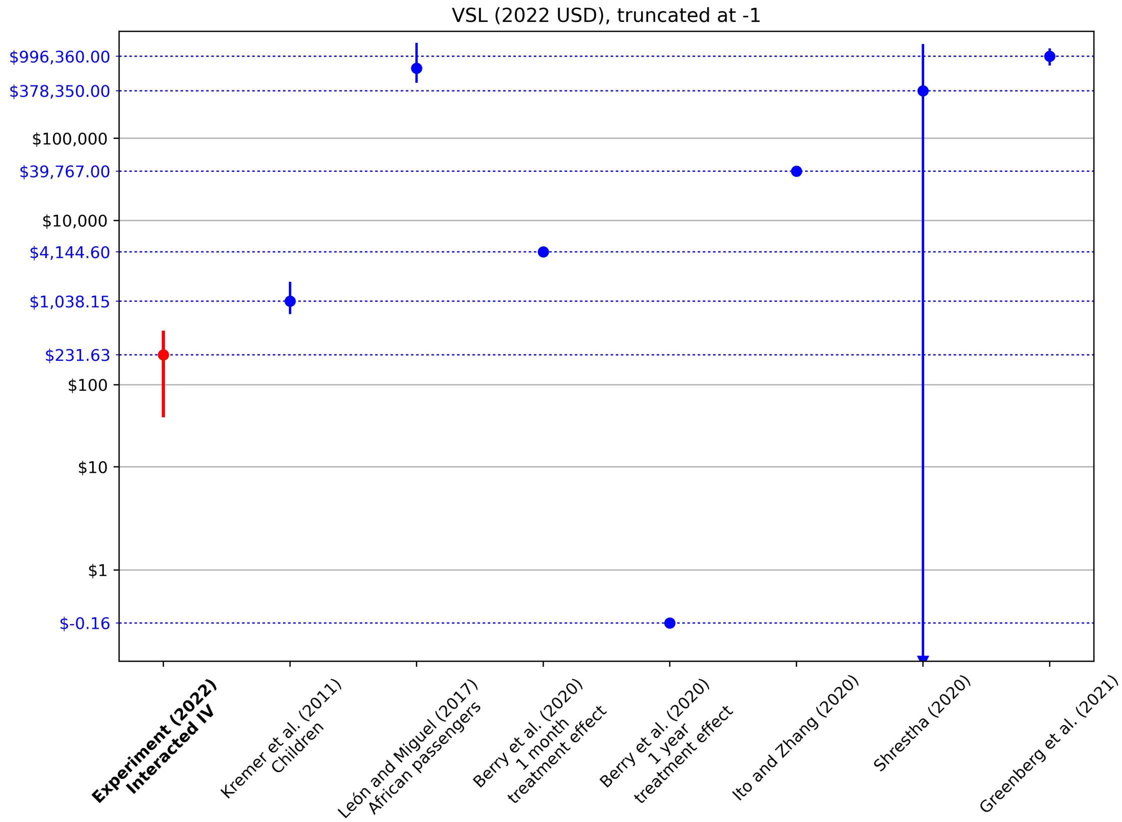
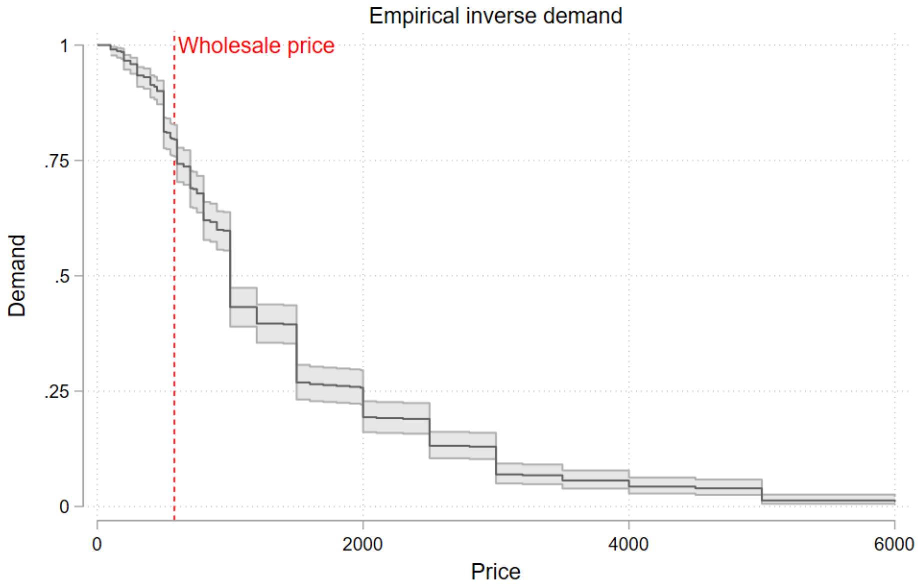


Figure 4 plots revealed-preference VSL estimates and (where available) 95% confidence intervals from this paper, Kremer et al. (2011), León and Miguel (2017), Berry et al. (2020), Ito and Zhang (2020), Shrestha (2020), and Greenberg et al. (2021). Greenberg et al. (2021) is included for comparison to 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. Appendix Figure A1 plots a version of the figure without truncation.

Figure 5: Demand for helmets

(a) Inverse demand, control group



(b) Elasticity of demand, control group

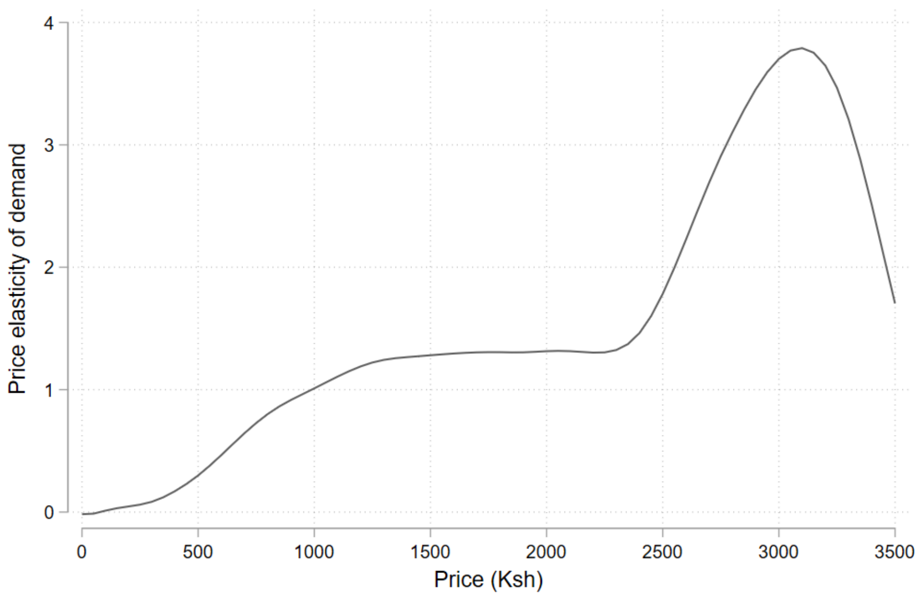


Figure 5 plots estimates of demand for helmets among observations in the control and pure control arms. The plot of demand includes a pointwise confidence interval. The plot of the elasticity of demand is based on a local polynomial estimation adapted from Berry et al. (2020). The vertical line denotes 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.

Tables

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	0.275	0.667	0.392
	[8.126]	(1.159)	(0.572)	(0.589)	(0.590)
Female	0.352	-0.039	-0.064**	-0.049	0.014
	[0.478]	(0.060)	(0.030)	(0.030)	(0.029)
Health (1-5)	3.403	-0.034	-0.058	-0.009	0.049
	[0.667]	(0.086)	(0.043)	(0.045)	(0.043)
Life expectancy	81.438	-0.134	0.462	-0.124	-0.586
	[6.947]	(0.840)	(0.427)	(0.439)	(0.413)
Employed	0.898	-0.051	-0.041*	-0.046**	-0.005
	[0.303]	(0.043)	(0.022)	(0.022)	(0.022)
Income (PPP, ’000s USD)	6.866	-0.150	0.638	1.032	0.394
	[8.427]	(1.472)	(0.834)	(0.862)	(0.955)
$\mathbb{E}$[Wage in 5 years/Wage today]	6.166	-0.636	-0.907	-1.353*	-0.446
	[11.685]	(1.761)	(0.719)	(0.743)	(0.674)
1(children)	0.774	0.033	-0.002	-0.013	-0.011
	[0.418]	(0.053)	(0.027)	(0.028)	(0.027)
Digit span recall	3.020	-0.156	-0.005	-0.011	-0.007
	[1.391]	(0.183)	(0.089)	(0.091)	(0.088)
Years of education	12.111	-0.468	0.162	-0.026	-0.188
	[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	-0.032
	[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	0.173	0.204	0.273

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: 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	0.192	-0.375	-0.567*
	[5.242]	(0.725)	(0.348)	(0.358)	(0.343)
Average trip length (minutes)	19.593	0.334	-0.725	-1.276*	-0.551
	[12.567]	(1.538)	(0.726)	(0.746)	(0.672)
$\mathbb{E}$[Deaths/10,000 passengers, 1 year]	371.584	NA	-56.005	7.543	63.547
	[1,186.706]	NA	(78.123)	(80.394)	(78.179)
$\mathbb{E}$[Deaths/10,000 passengers, 5 years]	977.118	NA	-179.625	283.175	462.800**
	[2,479.942]	NA	(196.470)	(202.182)	(208.987)
10000*Pr(Fatal accident, 5 years)	354.133	NA	-26.685	-34.493	-7.808
	[973.710]	NA	(57.110)	(58.763)	(53.724)
Previous accident	0.488	NA	-0.011	-0.029	-0.018
	[0.500]	NA	(0.032)	(0.033)	(0.032)
Know accident victim	0.947	NA	-0.022	-0.036**	-0.013
	[0.224]	NA	(0.017)	(0.017)	(0.017)
Use motorcycle taxi: Commuting	0.781	0.027	-0.031	0.008	0.039
	[0.414]	(0.054)	(0.027)	(0.028)	(0.027)
Shopping	0.420	-0.031	-0.034	-0.017	0.018
	[0.494]	(0.064)	(0.031)	(0.032)	(0.031)
Leisure	0.261	0.057	0.074**	0.035	-0.039
	[0.440]	(0.058)	(0.029)	(0.030)	(0.029)
Deliveries	0.095	-0.012	0.018	-0.004	-0.022
	[0.294]	(0.014)	(0.019)	(0.019)	(0.019)
Emergency/hospital transportation	0.095	-0.008	-0.007	-0.011	-0.004
	[0.294]	(0.014)	(0.018)	(0.018)	(0.018)
Reason for use: Speed	0.816	0.114**	0.018	0.004	-0.014
	[0.388]	(0.045)	(0.024)	(0.025)	(0.024)
Convenience	0.717	-0.005	0.006	-0.047	-0.053*
	[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*	-0.009	0.018
	[0.253]	(0.033)	(0.015)	(0.015)	(0.014)
Enjoyment	0.011	-0.012	-0.002	0.008	0.010
	[0.105]	(0.015)	(0.007)	(0.007)	(0.007)
Risk information: Own experiences	0.819	NA	-0.037	-0.041	-0.004
	[0.386]	NA	(0.026)	(0.027)	(0.026)
Friends/family	0.454	NA	0.030	0.037	0.006
	[0.498]	NA	(0.032)	(0.033)	(0.032)
Social media	0.414	NA	-0.048	-0.035	0.013
	[0.493]	NA	(0.031)	(0.032)	(0.031)
Media	0.288	NA	0.016	-0.030	-0.045
	[0.453]	NA	(0.029)	(0.030)	(0.028)
Government	0.135	NA	0.016	0.026	0.010
	[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	0.116	0.180	0.157

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 3: 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
					(80.32)
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	0.19				
	(3.68)				
Trip length		0.92			
		(1.59)			
Previous accident			68.72		
			(52.65)		
Contact in accident				207.03	
				(56.29)	
Raining					257.42
					(103.79)
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.

Table 3 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 4: 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	-26.44	-14.08	-14.26	-81.12	-46.41
	(21.77)	(21.97)	(0.97)	(0.99)	(18.38)	(14.12)
High treatment	6.77	4.86	-3.98	-4.36	-29.46	-0.45
	(24.91)	(25.28)	(0.88)	(0.93)	(19.70)	(16.12)
Control mean	330.97	330.97	78.68	78.68	221.79	228.62
Pr(High treatment = low treatment)	0.08	0.09	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 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 5: Value of a statistical life: Primary estimates

Panel A: Full sample

	Raw WTP				Winsorized WTP	
	(1) Interacted	(2) Treatment only	(3) Interacted	(4) Treatment only	(5) Interacted	(6) Treatment only
VSL	215.23	347.26	223.90	307.88	99.12	250.38
	(98.38)	(259.49)	(97.88)	(262.00)	(71.01)	(193.25)
Cragg-Donald F-stat	40.12	10.99	40.75	10.63	40.75	10.63
Weak IV Robust Confidence Set	[24.10, 420.73]	[-160.17, 1,066.93]	[34.22, 428.34]	[-215.85, 1,028.44]	[-40.85, 244.30]	[-129.96, 744.77]
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	396.03	774.94	394.52	823.28	177.94	431.11
	(253.28)	(478.06)	(257.04)	(513.43)	(177.92)	(340.79)
Cragg-Donald F-stat	16.84	11.24	16.07	9.90	16.07	9.90
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]	[-174.90, 596.09]	[-190.31, 1,591.40]
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. All estimates are in 2022 USD PPP units. 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; Chernozhukov and Hansen, 2008). Results exclude 35 observations for which surveyors reported motorcycle taxi drivers pretending to be passengers.

Table 6: Value of a statistical life year: Primary estimates

Panel A: Full sample

	Raw WTP				Winsorized WTP	
	(1) Interacted	(2) Treatment only	(3) Interacted	(4) Treatment only	(5) Interacted	(6) Treatment only
VSLY	3.18	5.41	3.39	5.27	1.47	4.50
	(1.52)	(4.62)	(1.52)	(4.64)	(1.11)	(3.43)
Cragg-Donald F-stat	54.55	10.80	55.91	10.92	55.91	10.92
Weak IV Robust Confidence Set	[0.24, 6.32]	[-3.70, 18.55]	[0.45, 6.53]	[-3.89, 18.05]	[-0.70, 3.71]	[-2.16, 13.30]
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
VSLY	5.55	16.65	4.82	15.23	1.89	7.78
	(4.01)	(10.64)	(3.95)	(10.05)	(2.77)	(6.67)
Cragg-Donald F-stat	23.75	8.78	24.06	9.32	24.06	9.32
Weak IV Robust Confidence Set	[-2.10, 14.90]	[-0.84, 60.48]	[-2.80, 13.89]	[-1.69, 53.71]	[-3.65, 7.94]	[-4.54, 31.07]
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; Chernozhukov and Hansen, 2008). Results exclude 35 observations for which surveyors reported motorcycle taxi drivers pretending to be passengers.

Table 7: Heterogeneity in the value of a statistical life

	(1) 1(Age > median)	(2) Wage	(3) 1(wage growth > median)	(4) 1(health > median)	(5) 1(children)	(6) 1(digit recall score > median)	(7) 1(education > median)	(8) 1(female)
VSL	295.01	208.52	54.13	42.85	250.85	60.51	215.75	223.03
	(148.85)	(130.43)	(126.78)	(169.85)	(123.04)	(197.61)	(180.79)	(119.15)
VSL x Interaction	-0.20	0.89	670.88	497.87	-97.05	297.54	69.28	260.45
	(269.42)	(0.43)	(335.59)	(254.27)	(247.69)	(242.17)	(232.66)	(238.62)
Cragg-Donald F-stat	17.70	22.13	10.05	17.01	21.70	12.10	18.89	16.78
Observations	1,425	1,425	1,425	1,425	1,425	1,425	1,425	1,425
Controls	LASSO	LASSO	LASSO	LASSO	LASSO	LASSO	LASSO	LASSO
Enumerator FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Standard errors in parenthesis.

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. The estimates exclude 35 observations for which surveyors reported motorcycle taxi drivers pretending to be passengers.

Table 8: Value of a statistical life: Comparison of non-experimental to experimental estimates

Panel A: 42% helmet effectiveness treated as truth

	Non-experimental					Experimental	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	LASSO	LASSO	LASSO	$\frac{WTP_i}{\Delta r_i^*}$	Preferred IV	Weighted IV
VSL	15,244.30	2,744.41	-4,358.96	1,962.01	381,326.62	223.90	535.97
	(10,900.69)	(10,630.65)	(9,927.91)	(10,836.30)	(13,859.27)	(97.88)	(396.62)
95% CI/Confidence set	[-6,121.06, 36,609.66]	[-18091.67, 23,580.49]	[-23817.66, 15,099.74]	[-19277.15, 23,201.17]	[354,162.44, 408,490.79]	[34.22, 428.34]	[-181.23, 1,744.48]
Cragg-Donald F-stat						40.75	12.42
Observations	1,425	1,425	1,425	1,425	1,425	1,425	1,425
Controls	None	LASSO	LASSO	LASSO	LASSO	LASSO	LASSO
Enumerator FE	No	No	Yes	No	Yes	Yes	YES
Taxi Terminal FE	No	No	No	Yes	No	No	No

Panel B: 70% helmet effectiveness treated as truth

	Non-experimental					Experimental	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	LASSO	LASSO	LASSO	$\frac{WTP_i}{\Delta r_i^*}$	Preferred IV	Weighted IV
VSL	9,146.58	1,646.65	-2,615.38	1,177.21	228,795.97	223.90	535.97
	(6,540.42)	(6,378.39)	(5,956.75)	(6,501.78)	(8,315.56)	(97.88)	(396.62)
95% CI/Confidence set	[-3,672.63, 21,965.79]	[-10855.00, 14,148.30]	[-14290.60, 9,059.85]	[-11566.29, 13,920.70]	[212,497.47, 245,094.48]	[34.22, 428.34]	[-181.23, 1,744.48]
Cragg-Donald F-stat						40.75	12.42
Observations	1,425	1,425	1,425	1,425	1,425	1,425	1,425
Controls	None	LASSO	LASSO	LASSO	LASSO	LASSO	LASSO
Enumerator FE	No	No	Yes	No	Yes	Yes	YES
Taxi Terminal FE	No	No	No	Yes	No	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) is the preferred VSL estimate and replicated for comparison. Column (7) re-weights the non-interacted experimental estimate to calculate the same LATE as the observational estimate reported in column (3). Experimental estimates in columns (6) - (7) 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.

Table 9: Reduced form effect of information on demand

	(1)	(2)	(3)	(4)	(5)	(6)
	Individual treatments				Pooled treatments	
	WTP	WTP	log(WTP)	log(WTP)	WTP	WTP
Pure control	2.24	1.99	0.04	0.02		
	(4.58)	(4.49)	(0.10)	(0.10)		
Low treatment	-2.16	-2.23	-0.07	-0.08	-3.30	-1.99
	(1.90)	(1.84)	(0.05)	(0.05)	(1.59)	(1.79)
High treatment	1.65	1.95	-0.01	-0.01		
	(2.32)	(2.28)	(0.05)	(0.05)		
Low x BL Belief						-27.65
						(25.04)
Control mean	33.28	33.28			34.35	34.15
Pr(Low = High)	0.08	0.05	0.28	0.13		
Observations	1,536	1,536	1,536	1,536	1,536	1,427
Controls	None	LASSO	None	LASSO	LASSO	LASSO
Enumerator FE	No	Yes	No	Yes	Yes	Yes

Robust standard errors in parenthesis.

This table reports reduced form regressions of willingness to pay for a helmet, in PPP 2022 USD, on the information treatment that the respondent received. All models include wave fixed effects. The pure control was assigned with a probability of 0.1 in the first wave and 0 in the second wave of the experiment. The other treatment arms were assigned with equal probability. Columns 1-4 examine individual treatment assignments. Columns 5-6 pool control and high treatment observations, which had similar beliefs about the effectiveness of helmets. 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 detailed demographic information about respondents. Second, we obtained information about their use of motorcycle taxis, including weekly ridership volume, standard trip length, trip types (e.g. commuting vs leisure), and reasons for using motorcycle taxis versus other modes of transportation (e.g. speed or cost). 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 since the survey was identical across arms through this point (excluding the pure control), and the surveyors were not yet informed about treatment assignment. During the second component, surveyors presented empirical risk estimates to respondents in the treatments arms and elicited posteriors 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 (those that asked questions about the risks of motorcycles and perceived safety benefits of helmets) 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. These events were chosen because all individuals experience them with the same frequency, whereas indexing to something like the prevalence of HIV deaths would be correlated with socioeconomic status.

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 asked over two time horizons to help the respondent think through risks incrementally and as a method of testing data quality since the 5 year response should be larger than the 1 year response.

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 exact belief within the range. Piloting revealed that this two step approach helped respondents answer accurately. A design feature of this survey is that the questions about one's own risk assessment and the number of deaths per 10,000 passengers over 5 years are asked in different ways but measure a similar outcome. The first question fixes the denominator and asks for a numerator, whereas the question about one's own risk asks for the risk in terms of a fraction, percent or decimal. These two questions need not perfectly align since the first considers an average Kenyan whereas the second asks about the respondent's own risk, but we may test whether they are broadly consistent to verify the quality of the measurements.

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 and 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 than that used to elicit beliefs about the risk of a fatal accident without a helmet. 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%. Surveyors communicated this information by stating that for every 100 individuals that would die if no one wore a helmet, the study estimates that 42 (or 70) would survive if all had worn a helmet. 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 and VSLY 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 since random assignment of T_i guarantees that it is independent of unobserved determinants of utility. Recall that the two-stage least squares model which identifies VSL is given by

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

$$\Delta r_i = Z_i'\pi + X_i'\pi_c + \pi_r r_{0,i} + \nu_i$$

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

If $Z_i = (T_i', r_{0,i} \cdot T_i')'$, then errors may be heteroskedastic with respect to $r_{0,i}$. However, controls for $r_{0,i}$ will ensure homoskedasticity because $r_{0,i} \cdot T_i$ adds no information about ϵ_i^2 after accounting for $r_{0,i}$, so homoskedastic standard errors about VSL will still be accurate.

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

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

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

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

$$= \mathbb{E}[(\Delta\hat{r}_i)^2]^{-1}\sigma^2(r_{0,i})\,\mathbb{E}[(\Delta\hat{r}_i)^2]\,\mathbb{E}[(\Delta\hat{r}_i)^2]^{-1}$$

$$= \epsilon_i^2(r_{0,i})\,\mathbb{E}[(\Delta\hat{r}_i)^2]^{-1} := V(r_{0,i})$$

where $\Delta\hat{r}_i = P_Z\Delta r_i$.

Hence, by the Law of Total Variance,

$$Avar(\sqrt{N}(\widehat{VSL} - VSL)) = E[V(r_{0,i})] + Var(\mathbb{E}[\sqrt{N}(\widehat{VSL} - VSL)|r_{0,i}])$$

$$= E[V(r_{0,i})] = \mathbb{E}[\epsilon_i]^2\,\mathbb{E}[(\Delta\hat{r}_i)^2]^{-1}$$

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. Hence, homoskedastic errors are a reasonable approximation of the data which 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* (Finlay et al., 2016) 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 (Andrews et al., 2007).

For robustness, I also report estimates constructed using continuously updating GMM (CUE) with heterodekastic robust standard errors in Appendix Table A4. Weak instrument robust confidence sets are reported based on inversion of a CLR test statistic. Although standard errors are marginally larger, 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.

Randomization was conducted at the individual level. As a result, clustering standard errors is not necessary for inference over the sampled population. However, I report two-stage cluster bootstrap (TSCB) standard errors and confidence intervals over the full sample in Appendix Table A4. I use 1,000 bootstrap iterations.

I report TSCB rather than analytic clustered standard errors because Abadie et al. (2022) demonstrate that clustered standard errors are too conservative in cases where randomization is assigned at the individual level, and the authors demonstrate that correlation between residuals within clusters does not necessitate clustering given this treatment assignment mechanism. Furthermore, the TSCB, which is based on study design, captures clustered sampling when the number of sampled clusters is a large share of the population number of clusters which accurately describes this setting. I report boostrapped standard errors because Abadie et al. (2022) do not derive analytic solutions for instrumental variable regression and to allow for asymmetric confidence sets since there may be more uncertainty in the upper than lower tail. Standard errors increase with the use of TSCB standard errors, but results are similar and one may reject VSL estimates above about $1,000 with the interacted instrument and $3,000 if only using treatment as an instrument.

C Appendix Figures

A1: VSL estimates across studies, not truncated

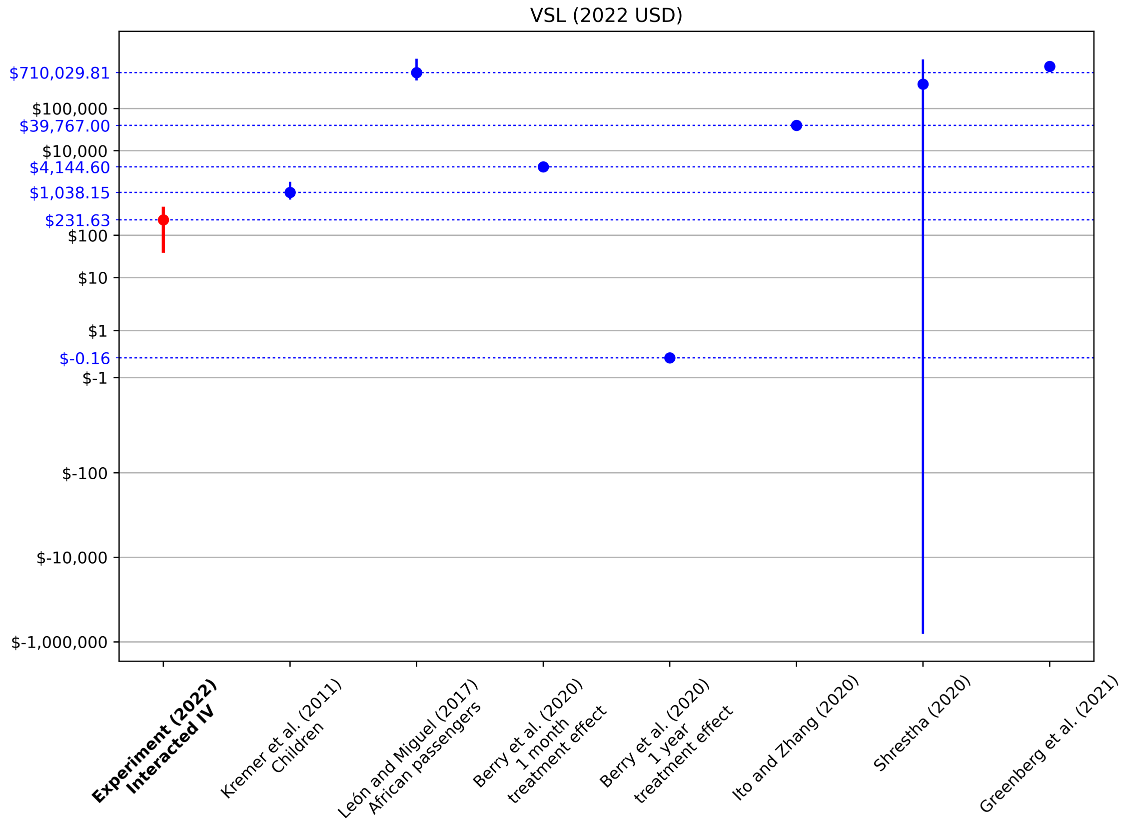


Figure A1 plots revealed-preference VSL estimates and (where available) 95% confidence intervals from this paper, Kremer et al. (2011), León and Miguel (2017), Berry et al. (2020), Ito and Zhang (2020), Shrestha (2020), and Greenberg et al. (2021). Greenberg et al. (2021) is included for comparison to 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.

A2: Effect of study VSL on published benefit-cost ratios

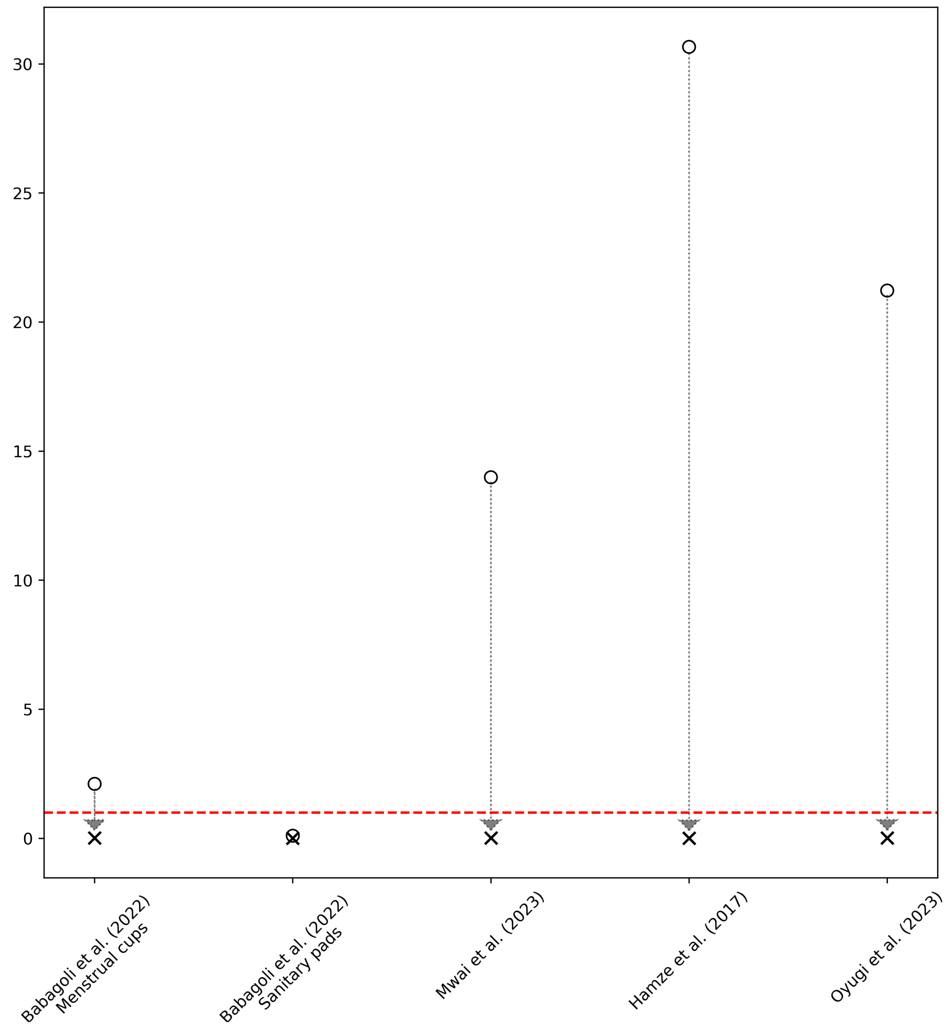


Figure A2 examines how the benefit-cost ratios (BCRs) of published benefit-cost analyses of Kenyan programs change when the study's original VSL or VSLY is replaced with the preferred estimates from this study. Hollow circles denote the original BCR estimate, and an x denotes the revised estimate. The 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.

D Appendix Tables

A1: Non-response rates and balance: Demographics

	(1) Control Summary statistics	(2) Pure control - control	(3) Low treatment - Control	(4) High treatment - Control	(5) High treatment - low treatment
Age	0.000	0.000	0.000	0.004*	0.004
	[0.000]	(0.006)	(0.002)	(0.002)	(0.003)
Female	0.000	0.000	0.000	0.000	0.000
	[0.000]	(0.000)	(0.000)	(0.000)	(0.000)
Health (1-5)	0.000	0.000	0.000	0.000	0.000
	[0.000]	(0.000)	(0.000)	(0.000)	(0.000)
Life expectancy	0.015	-0.016	0.003	0.004	0.000
	[0.124]	(0.018)	(0.008)	(0.008)	(0.009)
Employed	0.000	0.000	0.000	0.000	0.000
	[0.000]	(0.000)	(0.000)	(0.000)	(0.000)
Wage (Ksh/hr)	0.111	0.055	0.042*	0.059**	0.017
	[0.314]	(0.045)	(0.023)	(0.023)	(0.023)
$\mathbb{E}$[Wage in 5 years/Wage today]	0.115	0.047	0.043*	0.058**	0.015
	[0.319]	(0.046)	(0.023)	(0.024)	(0.024)
1(children)	0.000	0.000	0.000	0.000	0.000
	[0.000]	(0.000)	(0.000)	(0.000)	(0.000)
Digit span recall	0.000	0.000	0.000	0.000	0.000
	[0.000]	(0.000)	(0.000)	(0.000)	(0.000)
Years of education	0.007	-0.008	0.001	-0.002	-0.003
	[0.081]	(0.010)	(0.005)	(0.005)	(0.005)
1(primary school complete)	0.007	-0.008	0.001	-0.002	-0.003
	[0.081]	(0.010)	(0.005)	(0.005)	(0.005)
1(secondary school complete)	0.007	-0.008	0.001	-0.002	-0.003
	[0.081]	(0.010)	(0.005)	(0.005)	(0.005)
1(college degree)	0.007	-0.008	0.001	-0.002	-0.003
	[0.081]	(0.010)	(0.005)	(0.005)	(0.005)
N (exc. control in col. 2-4)	452	80	531	473	
p-value of joint orthogonality		0.271	0.416	0.138	0.652

Standard deviations in brackets. Standard errors in parenthesis.

Column (1) reports the mean and standard deviation of the indicated variable across the full sample. 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.

A2: Non-response rates and balance: Motorcycle taxi use

	(1) Control Summary statistics	(2) Pure control - control	(3) Low treatment - Control	(4) High treatment - Control	(5) High treatment - low treatment
Trips/week	0.000	0.000	0.000	0.000	0.000
	[0.000]	(0.000)	(0.000)	(0.000)	(0.000)
Average trip length (minutes)	0.000	0.000	0.000	0.000	0.000
	[0.000]	(0.000)	(0.000)	(0.000)	(0.000)
$\mathbb{E}$[Deaths/10,000 passengers over 5 years]	0.004	NA	0.001	0.006	0.005
	[0.066]	NA	(0.005)	(0.005)	(0.006)
Baseline belief: 10000*Pr(Fatal accident over 5 years)	0.018	NA	0.001	0.006	0.004
	[0.132]	NA	(0.009)	(0.009)	(0.009)
Motorcycle trip types	0.000	0.000	0.000	0.000	0.000
	[0.000]	(0.000)	(0.000)	(0.000)	(0.000)
Reasons for using motorcycles	0.000	0.000	0.000	0.000	0.000
	[0.000]	(0.000)	(0.000)	(0.000)	(0.000)
Risk information sources	0.000	NA	0.000	0.000	0.000
	[0.000]	NA	(0.000)	(0.000)	(0.000)
N (exc. control in col. 2-4)	452	80	531	473	
p-value of joint orthogonality		.	0.962	0.554	0.652

Standard deviations in brackets. Standard errors in parenthesis.

Column (1) reports the mean and standard deviation of the indicated variable across the full sample. 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.

A3: Effect of information on beliefs: Estimated including manipulated surveys

	(1) Posterior risk	(2) Posterior risk	(3) Helmet effectiveness	(4) Helmet effectiveness	(5) Risk reduction	(6) Risk reduction, winsorized
Low treatment	-20.56	-20.83	-14.06	-14.25	-77.09	-41.52
	(21.72)	(21.93)	(0.97)	(0.99)	(18.46)	(14.28)
High treatment	7.58	5.88	-4.12	-4.54	-28.33	0.66
	(24.44)	(24.86)	(0.87)	(0.92)	(19.35)	(15.87)
Control mean	339.71	339.71	78.85	78.85	226.92	233.96
Pr(High treatment = low treatment)	0.13	0.15	0.00	0.00	0.00	0.00
Observations	1,455	1,455	1,457	1,457	1,455	1,455
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 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. These estimates include 35 observations for which surveyors reported motorcycle taxi drivers pretending to be passengers.

A4: Value of a statistical life: Estimates with heteroskedasticity and clustering

Panel A: GMM Estimation with heteroskedastic consistent standard errors

	Raw WTP				Winsorized WTP	
	(1) Interacted	(2) Treatment only	(3) Interacted	(4) Treatment only	(5) Interacted	(6) Treatment only
VSL	249.34	349.35	245.74	308.12	102.70	252.59
	(138.64)	(233.33)	(141.87)	(238.51)	(76.48)	(193.40)
Cragg-Donald F-stat	8.23	11.84	8.16	10.96	8.16	10.96
Weak IV Robust Confidence Set	[-19.64, 704.97]	[-103.35, 1,005.30]	[-40.74, 689.51]	[-154.63, 978.63]	[-63.84, 323.75]	[-122.64, 750.35]
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: Two-stage cluster bootstrap

	Raw WTP				Winsorized WTP	
	(1) Interacted	(2) Treatment only	(3) Interacted	(4) Treatment only	(5) Interacted	(6) Treatment only
VSL	215.23	347.26	223.90	307.88	99.12	250.38
	(401.61)	(686.53)	(469.47)	(948.40)	(276.89)	(465.95)
Cragg-Donald F-stat	40.12	10.99	40.75	10.63	40.75	10.63
95% CI	[-386.22, 1,079.49]	[-315.63, 2,902.88]	[-495.32, 1,088.54]	[-366.03, 3,536.83]	[-243.22, 683.24]	[-237.14, 1,300.50]
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 a reports robust standard errors in parenthesis and weak instrument robust confidence sets in brackets. Panel b reports TSCB standard errors in parenthesis and bootstrapped confidence intervals in brackets.

This table reports VSL estimates with alternative standard error assumptions. Panel a reports estimates constructed using continuous updating GMM (CUE) and heteroskedastic robust standard errors. Weak instrument robust confidence sets are calculated using conditional likelihood ratio test inversion. Panel b reports inference conducted using the two-stage cluster bootstrap (TSCB) from Abadie et al. (2022) constructed using 1,000 iterations. TSCB confidence intervals are included in brackets. 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. These estimates exclude 35 observations for which surveyors reported motorcycle taxi drivers pretending to be passengers.

A5: Value of a statistical life: Estimates including manipulated surveys

Panel A: Full sample

	Raw WTP				Winsorized WTP	
	(1) Interacted	(2) Treatment only	(3) Interacted	(4) Treatment only	(5) Interacted	(6) Treatment only
VSL	39.86	273.66	59.07	211.15	44.53	232.41
	(98.86)	(278.59)	(99.06)	(282.85)	(70.46)	(204.71)
Cragg-Donald F-stat	41.88	9.99	42.05	9.57	42.05	9.57
Weak IV Robust Confidence Set	[-159.33, 240.49]	[-297.40, 1,054.76]	[-159.33, 240.49]	[-395.81, 984.34]	[-96.19, 187.12]	[-178.22, 770.64]
Inversion test	CLR	CLR	CLR	CLR	CLR	CLR
Observations	1,455	1,455	1,455	1,455	1,455	1,455
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	29.28	761.86	-26.67	779.79	26.24	424.19
	(251.24)	(534.02)	(256.07)	(568.27)	(175.23)	(369.34)
Cragg-Donald F-stat	17.18	9.63	16.16	8.46	16.16	8.46
Weak IV Robust Confidence Set	[-516.47, 609.45]	[-158.62, 2,727.95]	[-605.12, 556.92]	[-194.57, 3,080.62]	[-351.33, 418.50]	[-259.50, 1,809.29]
Inversion test	CLR	AR	CLR	AR	CLR	AR
Observations	1,006	1,006	1,006	1,006	1,006	1,006
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; Chernozhukov and Hansen, 2008). These estimates include 35 observations for which surveyors reported motorcycle taxi drivers pretending to be passengers.

A6: 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.

A7: 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; Chernozhukov and Hansen, 2008). 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.