Incentives Justifying Nonconformity

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Abstract

What role do financial incentives play in mitigating harmful peer norms? This paper studies whether financial incentives can be more powerful when they help justify choices that have social image costs among peers. I test this hypothesis in the high-stakes context of road safety in low-income countries. I run multiple experiments with 360 motorcycle taxi drivers in urban Uganda, offering financial incentives to avoid speeding. First, I provide incentivized evidence that peers view speeding as admirable. In a Demand Experiment, I randomize the visibility of incentives to coworkers and show that (i) drivers are more likely to take up financial incentives when they can be used as justification. In an Impact Experiment, I find that randomly offering visible incentives with justification properties (ii) is twice as effective in promoting compliance with speed regulation relative to private incentives, and (iii) increases driver productivity. At least since Coase, economists have considered financial incentives as a tool to reward desirable behavior. This paper illustrates that they can also reduce the social image costs to defy peer norms, achieving the same behavioral change with lower but visible monetary incentives.

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1 Introduction

Financial incentives have long been thought of as a way to induce behaviors that are desirable for society. Financial incentives are not only a cornerstone of economic theory, they are also the main lever that policymakers typically have. From subsidies in childcare and education to taxation and fines for behaviors that have negative externalities, such as smoking, traffic violations, or carbon emissions, most policies rely on monetary incentives.

The traditional economic view suggests that financial incentives influence behavior by changing the extrinsic rewards associated with certain actions. Whether financial incentives are effective may depend on features of their design other than the level of monetary benefits or costs. One other key motivating force behind people's choices is reputation. Individuals care about how others in their social environment perceive them and, as a consequence, can engage in costly and potentially harmful behaviors (Bursztyn and Jensen, 2017). Although extrinsic and reputational incentives have separate effects on the choice of individuals, it is well understood that they can also interact (Ariely et al., 2009; Bénabou and Tirole, 2006; Heyman and Ariely, 2004).

A long tradition of thought in psychology - and more recently in economics - suggests that having a way to excuse yourself matters (Bursztyn et al. (2022); see Shalvi et al. (2015) for a review of the literature in psychology). Specifically, when a valid excuse is available, the ability of others to infer the true motivation of the deviant is reduced, as well as the social image costs associated with the action. Despite the reason-giving nature of people (Wolff, 2014), we know little about whether and how financial incentives, a very common policy lever, can help people justify contravening harmful peer norms, particularly in high-stakes environments.

Understanding the interaction between financial incentives and social image concerns is key to designing policy interventions to reduce behaviors that are desirable among peers but harmful to society. This class of policy problems is common. Consider behaviors in the health sphere, such as smoking, which can be viewed as socially desirable in a local peer group, but is not desirable in a global sense. The same applies to social dynamics in education: in settings where educational investment is stigmatized (Austen-Smith and Fryer, 2005), studying less may be socially desirable "locally" (among friends) but may not be optimal "globally" as it typically reduces upward mobility and exacerbates inequality.

This paper experimentally tests the hypothesis that people value financial incentives more when they provide a credible justification to peers for violating norms, and I study the economic implications of providing financial incentives with and without the justification property.

I study this problem and how it maps onto real outcomes in the context of "global road safety epidemics" (World Bank, 2019). I present evidence from two field experiments with 360 motorcycle taxi drivers working in organizations in Uganda, where fellow drivers view speeding as admirable.

Speeding is a high-stake behavior that has negative externalities on people's safety and the environment, calling for regulatory interventions. The traditional approach to overcome the externalities is to introduce appropriate Pigouvian taxes or subsidies so that the price reflects the optimal incentive to reduce speed. In the presence of social image concerns, optimal pricing can also depend on social costs or benefits of driving fast (Bénabou and Tirole, 2011). In this paper, I demonstrate that when harmful behavior is caused by social image concerns, simple financial incentives that have justification properties can lower the social image cost of not conforming to the norm, thereby mitigating the behavior that induces negative externalities.

In the study setting, motorbike taxi drivers regularly exceed speed limits in urban areas.[1] Survey evidence suggests that speeding is not considered a desirable characteristic by customers and drivers are aware of it. With survey data from a sample of motorcycle taxi commuters (N=386), I document that customers are willing to pay a premium of 8% to avoid speeding.

Why do drivers not respond to customer preferences? Evidence from high-frequency spatial data on drivers' mobility and survey data from my study sample of drivers suggests that drivers see speeding as a way to win their peers' admiration. I build causal evidence on this mechanism with a preregistered *Beliefs experiment*. The results of this survey experiment show that drivers who regularly speed excessively are perceived to have a greater influence on group decisions (0.3 standard deviations, $p = 0.00$), to be cooler (0.45 standard deviations, $p = 0.00$) and more skilled drivers (0.32 standard deviations, $p = 0.00$). However, respondents do not view speeding drivers as higher earners, and they perceive the monetary costs of speeding (for example, fuel and maintenance) to be 15% higher than those of not speeding ($p = 0.00$). This evidence suggests that speeding is normalized among drivers and results from concerns about their social image.

In January-February 2023, I conducted two field experiments with a sample of 360 motorcycle taxi drivers operating out of separate taxi stations in the Greater Kampala Metropolitan Area. The design of the first field experiment—the *Demand Experiment*—is guided by a simple conceptual framework grounded in Bénabou and Tirole's (2006) theory

[1] Speed-limit compliance in this setting is a key issue for local authorities, but my contribution extends beyond the specific location to the growing literature on road safety in economics (Bauernschuster and Rekers, 2022; Ashenfelter and Greenstone, 2004).

regarding social signaling and how individuals' utility depends on intrinsic, extrinsic, and reputational incentives. I establish that incentives that financially reward not speeding can be used to justify contravening the speeding norm when they are visible to colleagues. This justification property is a key driver of the demand for those incentives.

In the experiment, I elicit the reservation price of a contract that offers high-powered financial incentives to avoid speeding. I ask drivers to make a series of choices from a menu with two options: financial incentives and unconditional payment. The amount of the unconditional payment varies between choices, which allows me to estimate the reservation price, defined as the highest unconditional payment one is willing to forgo to receive the incentives. To make the financial rewards offer a credible justification for reducing speeding, colleagues need to be credibly informed about the level of incentives. I provide incentives that can be used as justification by experimentally manipulating the visibility of the amount of financial incentives. I inform the driver that the financial rewards offered, if chosen, would be disclosed to his colleagues by the field officer of the research team. Besides social image concerns, there may be other reasons why drivers value visible incentives differently from equivalent but confidential ones. I attempt to isolate these alternatives by creating another condition in which not only the incentive amount but also the outside option (that is, the unconditional payment) is revealed to colleagues.

My design helps identify the justification channel: the higher the outside option, the less powerful visible financial incentives to avoid speeding can be in justifying slow driving. The intuition is simple. What makes visible incentives a credible justification is the fact that, when incentives are visible, colleagues may find it harder to answer the questions: Why is the driver slow? Is he doing it for money? A key assumption for the justification mechanisms to play a role is that earning an extra dollar is typically viewed as a way to promote financial stability rather than greediness.

Compared to the scenario in which the incentive contract and the outside option are confidential, drivers are willing to pay 25%–37% more ($p = 0.00$) for the contract if it is made visible and thus has the justification property. And while 43% of the drivers are willing to forgo some unconditional payment for the contract in the first scenario, the share increases by 22 percentage points (55% increase, $p = 0.00$) when the amount of incentives is visible to peers. The justification property of the incentive contract also flattens the slope of its demand curve relative to the same contract offered privately. When the forgone outside option is also revealed, the justification property of the visible incentives is muted, while the incentives' visibility is maintained the same as in the scenario in which the incentives are visible and the outside option confidential. I find that the demand is 34 percentage points lower than in the scenario in which the incentive contract and the outside option are confidential.

The results of the *Demand Experiment* are consistent with the hypothesis that visible financial incentives have justification properties valued by drivers. The welfare consequences of the demand for justification depend on whether and to what extent this property affects driving behavior and labor market outcomes. I implement a second field experiment, the *Impact Experiment*, to study the implications of financial incentives with and without justification properties on driving behavior and labor market outcomes.

To create random variation in exposure to financial incentives, a randomly selected subsample of study participants was offered the incentive contract proposed in the Demand Experiment for 10 days. To net out income effects, the remaining individuals were offered an unconditional cash transfer calibrated to match the expected payment of the incentives. Among those assigned the contract, the drivers were randomly assigned into two groups. One group received the contract fully privately; the other group received one whose value was disclosed to colleagues. No information was shared about the decision to forgo the unconditional payment, thus preserving the incentives' justification property.

I find that incentives increase compliance with the speed limit 72% more if they are visible rather than private ($p = 0.04$). Visible incentives increase compliance by 9.2 percentage points relative to a mean of 14% when drivers receive the unconditional payment, while private incentives boost compliance only 4.5 percentage points. Visible incentives also reduce average speed conditional on speeding above 50 kph by 17% ($p = 0.00$), relative to a mean excess speed of 5.1 kph. The results are robust to alternative measures of speeding.

Who are the drivers for whom visible incentives are more effective? Heterogeneity analysis shows that the drivers who value the justification property of visible incentives respond systematically more to the visible incentives than other drivers. Descriptive evidence also suggests that these drivers are more likely to work at large taxi stations, are more risk averse, and earn less at baseline. These patterns are consistent with the hypothesis that the behavioral grip of peer pressure loosens more among drivers who value the signal that their motivation is financial stability.

Finally, the justification property has economic implications for drivers. While labor supply and earning are unaffected by both visible and private incentives relative to unconditional payment, fuel costs drop significantly for both incentives. Visible incentives reduce marginal cost by 15.2% ($p = 0.00$) and private incentives by 11% ($p = 0.01$). This pattern reflects a productivity increase of 20% ($p = 0.01$) and 15% ($p = 0.02$), respectively. The results are only suggestive of a multiplicative effect of the justification channel on productivity, as the difference between the two types of incentives is not statistically significant.

This paper contributes to two main strands of the literature. The first is the growing literature on social incentives and image concerns. Research shows that social pressure and status concerns shape behaviors in various contexts: the labor market (Bursztyn et al., 2020), the marriage market (Bursztyn et al., 2017), education Bursztyn et al. (2019), the workplace (Ashraf and Bandiera, 2018; Ager et al., 2022), health care (Karing, 2018; Macchi, 2021), and voting (Dellavigna et al., 2017). A complementary strand of the literature studies the role of intrinsic, extrinsic, and social motives (Bénabou and Tirole, 2006, 2011). My work is closely aligned with Bursztyn et al. (2022), whose authors investigate how rationales (in the form of media-provided information) are strategically used to justify dissent. The idea that individuals strategically use events or information to justify their own choices is deeply explored in the psychology literature on self-convincing (Shalvi et al., 2015). I contribute by examining how financial incentives can justify contravening harmful norms and by tracing out the behavioral and economic consequences of the justification property in a high-stakes environment.

Isolating the justification effect of incentives is difficult. It requires separating the justification mechanism from the monetary effect of the incentives and from other social incentive channels. I do so by varying aspects of visibility with the aim of holding fixed the level of the expected monetary rewards and the other possible social motivations while varying the extent to which the incentives can provide a credible justification. Furthermore, I examine this mechanism within a high-stakes context, which is rarely explored, with Ager et al. (2022) being a notable exception.

My work also contributes to the vast literature on incentives to exert effort in organizations. Dellavigna et al. (2017) compares the role of financial and psychological incentives and shows that psychological incentives are effective but less so than financial incentives. Ashraf and Bandiera (2018) provides a comprehensive review of the role of social incentives in organizations in facilitating interaction between individuals. In particular, my work is related to the literature on incentive transparency (Deserranno et al., 2023). I contribute by studying incentive transparency vis-à-vis peers rather than customers, shedding light on the role of social dynamics within an organization. Existing evidence shows that when social incentives are at odds with financial incentives, disclosing the latter limits their effects, possibly because the financial incentives send a negative signal. I contribute to this literature by showing that disclosure of some features of financial incentives can be beneficial even if, at baseline, financial and social incentives are at odds.

The remainder of this paper is structured as follows. In Section 2, I describe the setting and present descriptive and experimental evidence that motivated this study. In Section 3, I present the conceptual framework. In Section 4, I present the *Demand experiment* and discuss the findings through the lens of the model. In Section 5, I present the *Impact Experiment*. Section 6 concludes.

2 Study Setting and Motivating Evidence

2.1 Motorcyle Taxi and Speeding in Kampala

The setting of this study is the motorcycle transport sector in Kampala, Uganda. The transportation industry employs about 30% of the workforce in urban Uganda, and motorcycle taxis represent 9 out of 10 providers of urban public transportation (KCCA, 2020).

Motortaxi Organizations. The Ugandan motorcycle taxi industry is organized in stations, which provides a setting for regular interactions among colleagues. The study population consists of motorcycle taxi drivers operating from stations in the urban area of Kampala.[2] Despite the effort to regulate the industry by local authorities, taxi stations remain mostly informal organizations. Drivers pay a fee to become a member, which typically corresponds to a one-time entry cost that can be diluted in one to three years, making mobility between stations limited. The amount of fees ranges from Ugx 50k (Usd 13.5) to Ugx 800k (Usd 216).[3] The size of the stations is heterogeneous, ranging from 5 to 50 members. There is a defined hierarchy in place: members elect both the stage chief and the budget overseer, suggesting that social image can have significant economic implications, as seen in many organized groups.

Motortaxi Drivers: Labor Supply, Earnings, and Productivity. Existing work documents that productivity in African countries remains low (Dieppe, 2021; Bloom et al., 2010). This is also true in Uganda and its motorcycle-transit market. In particular, the Ugandan transportation market is characterized by an oversupply of service providers: the Kampala Capital City Authority estimates there are more than 200,000 motorcycle-

[2]Affiliation with a station is compulsory for operating a motorcycle taxi. However, the Kampala City Authority estimates that less than 40% of drivers are affiliated.

[3]Throughout the paper, I use a Usd-to-Ugx exchange rate of 3,700, the exchange rate at the time of the data collection.

taxi drivers in the city, one for every eight people in the city (KCCA, 2020).[4] In my data, motorcycle drivers work 11.45 hours a day, six days a week. The hourly net income of the workers is estimated to be Usd 0.7 (Ugx 2,700), serving an average of 16.7 customers on a typical working day and earning Usd 9.9 (Ugx 38,500). Initial focus groups suggested low levels of productivity, as drivers spend 80% of their work hours looking for customers. Additional survey evidence suggests that 75% of the time used to search for customers is spent at the taxi station, with on average seven other members.[5] These patterns highlight the importance of interactions between drivers on the job.

Road Safety and Speeding Behavior. The global road safety epidemic, with 1.35 million deaths per year and 90% occurring in developing countries, has become a critical issue for economic development (WHO, 2018).[6] According to the Kampala City Authority, Uganda loses on average 10 people per day in road traffic accidents. The total annual cost of road accidents is currently estimated at approximately Usd 1.2 billion, representing 5% of Uganda's gross domestic product (KCCA, 2020). In the Ugandan context, a key policy-relevant concern is that motorcycle taxis are an unsafe form of transport due to lack of adherence to traffic rules. Data from police traffic reports indicate that motorcycles alone contributed to 64% of traffic accidents in the country in 2017. In my data at baseline, 67% of the drivers report having experienced road accidents that involved repair costs; of those, 43% also required medical care. Among these drivers, 78% state that accidents involved excess speed.[7] Bespoke GPS data demonstrate that speeding is a widespread behavior. Figure A1 shows the general distribution of speed conditional on moving (on the left) and conditional on speeding—that is, speed above 50 kph (on the right), which corresponds to an average of 8.6% of driving time. Are speeding violations concentrated on specific rides, or are they systematically repeated over time? I aggregate speeding data to investigate the distribution of average drivers' speeding-violation occurrences across trips and days before the implementation of the field experiments. In particular, Figure A2 shows the distribution of the share of trips per driver for which at least one speeding violation is detected. Figure A3 shows the share of days in which at least one speeding violation was

[4]For comparison, the number of Uber drivers in New York City is about 1 for every 85 people.

[5]According to the GPS data on travel behavior of study participants, drivers spend on average 67% of the working time at the taxi station.

[6]According to the World Health Organization, the number of deaths from road accidents worldwide is projected to be 1.9 million by 2030, making accidents the fifth-highest cause of death. Road accidents disproportionately affect the poor, making road safety an issue of economic development. A conservative estimate of road accidents' economic costs varies between 1% and 3% of gross national product for low- and middle-income countries (World Bank, 2014).

[7]More than 70% of the accidents reported by Mulago Hospital, the largest health care facility in Uganda, involve speeding.

recorded. On average, drivers exceed speed limits at least once in 84% of trips and 80% of workdays.

2.2 Demand and Supply Motives for Speeding

Why do drivers drive fast? This section provides descriptive evidence on the demand- and supply-driven motivations for speeding.[8]

Consumers' Preferences for Speed. Do customers have a preference for fast driving or do they rather value drivers that limit speeding? Survey data from commuters (N=386) who regularly use a motorbike taxi as their main means of transport in this setting suggest that customers value safety. The customers in my sample were approached by field officers in the vicinity of the taxi stations of the drivers in the pilot study. Only 8% of the respondents report that fast driving is a desirable characteristic of a trip on a motorbike taxi, and 88% report safety as the most desirable characteristic. No customers who value safety report fast driving as desirable. These patterns are consistent with the assumption that people believe that fast driving decreases road safety. Are customers willing to pay a premium for safety? To further investigate the preferences of customers, I elicit their willingness to pay for a trip operated by drivers with different speeding habits. I use an incentive-compatible elicitation procedure described in Section C of the Appendix. Seventy-three percent of customers are willing to pay more for a ride with a driver who exceeds the speed limit on less than half of their trips, as opposed to a driver who exceeds the speed limit on more than half of their trips. The opposite is true for 12% of the sample, while 15% are indifferent. Importantly, virtually all customers associate speeding with a faster trip, suggesting that customers trade off safety with time. Among the survey respondents, the average premium for "slow driving" corresponds to 8% of the cost of the ride. Taken together, this descriptive evidence suggests that customers value safety and are willing to pay for it.

Beliefs of Drivers about Customers' Preferences. If customers do not value fast driving, why do drivers violate speed limits so often? One hypothesis is that drivers are not aware of the preferences of customers. Second-order beliefs of drivers about customers' preferences show no systematic misperception.[9] On average, drivers report

[8] I consider a speeding violation to be any speed detected by the GPS tracker above 50 kph prior to the experiment, which allows me to examine the intensity and spatial distribution of speeding behavior absent the incentives. All study participants were equipped with GPS tracking technology. The trackers are extensively described in Section E in the Appendix.

[9] I leverage data from the customer willingness-to-pay elicitation exercise to incentivize drivers' beliefs about the share of customers willing to pay more for a safe ride. I incentivize answers by offering a lottery

that 8 out of 10 customers are willing to pay more for a safe ride and are more likely to ask for their contact for future services. Furthermore, 14% of the drivers report any customer requesting that they drive fast in a typical working day, and only 9% mention requests from clients as a reason for speeding.

Reputation at the Workplace and Visibility of Speeding. An alternative explanation for speeding is peer pressure from colleagues. Initial focus groups suggested that drivers are concerned about their reputation among peers and adopt risky behaviors to increase their social status. Baseline survey data show that 90% of drivers prefer to disclose to coworkers the acceptance of risky actions, rather than not. When asked about their disclosure preferences to fellow drivers not affiliated with the station, 95% were indifferent. Furthermore, drivers perceive fast driving as a signal of social status on the job: as illustrated in Figure 1, 78% of the drivers who were recruited in the experiment (N=360) mentioned fast driving as a characteristic associated with drivers with a high social status among coworkers. Whether speeding matters to the social image of a driver among coworkers depends on its visibility. The spatial data from the high-frequency GPS trackers installed on the motorbikes of the study participants were then used together with the survey data from the study participants' colleagues[10] to document the visibility of driving behavior to peers. First, speeding reported by peers is a strong predictor of speeding recorded by the GPS. At baseline, 82% speeding violations are recorded within a radius of 500 meters from the motorcycle-taxi station, while only 37% of the driving time is spent in the same area.

2.3 Beliefs Experiment: Speeding and Perceived Social Status

Motivated by descriptive evidence on the importance of approval from colleagues and visibility of driving behaviors, I designed the *Belief Experiment* to test whether speeding behavior causally affects beliefs about social status among colleagues and to what extent speeding is perceived by peers as a signal of profitability and on-the-job competence.[11]

Design. In the *belief experiment*, 362 drivers are asked to examine and rate a sequence of four fellow drivers randomly selected from a set of profiles, for a total of 1,448 evaluations. To estimate the causal impact of speeding behavior on beliefs, the design randomizes past speeding behavior using pilot information. Each real pilot profile is ma-

ticket to win Ugx 10,000 for correct guesses.

[10] I interviewed a sub-sample of 54 colleagues randomly selected from the listing exercise.

[11] Details about the experimental design and empirical strategy are extensively discussed in section D in the Appendix.

nipulated to obtain a triplet of identical profiles with the exception of the frequency of GPS-recorded excess speeding in the last seven working days. Respondents are informed that some profiles are real and some are hypothetical. I incentivize the elicitation of beliefs by offering a financial reward for correctly guessing a randomly selected characteristic that respondents are asked to rate for one of the yet-to-be-known real profiles.[12]

Outcomes. Three sets of outcomes were preregistered: profitability, driving competences, and social image. In particular, each respondent evaluates profiles along six characteristics, in random order, for which I have information for the real profiles: daily earnings, daily marginal costs, driving ability, capacity to influence group decisions, driver's social standing, willingness to make a referral for the profile.[13]

Results. Figure 3 graphically summarizes the effects of treatment on the standardized outcome measure, while Table 2 shows the estimate of the corresponding regression framework in levels. Beliefs about earnings are not affected by the frequency of speed limit violations. However, drivers with a high speeding frequency (highest tercile of the distribution of speeding frequency) are perceived to incur 15.6% higher costs than drivers with a median speeding frequency (central tercile), thus reducing the perceived disposable income by 11.2% (0.21 standard deviations, $p = 0.001$) from a control mean of Usd 7.7 (Ugx 28,150). Low speeding behavior (lowest tercile of speeding frequency distribution) is associated with a reduction of 0.14 SD ($p = 0.013$) in perceived driving ability compared to the median speeding frequency, while high speeding behavior is associated with a 0.31 SD ($p = 0.000$) increase in perceived ability. The two pre-registered measures of social status have similar patterns. Drivers with low speeding frequency are rated 0.2 SD ($p = 0.001$) less capable of influencing decisions at work compared to drivers with median speeding behavior, while profiles above the median are rated 0.3 SD ($p = 0.000$) more capable of influencing decisions. Finally, slow driving reduces the probability of being referred by 15 percentage points ($p = 0.000$) from a control mean of 49%, while I find no statistically significant evidence that fast driving affects referrals.

Discussion. On average, fast drivers are perceived as less profitable, more skilled and with a higher social status. The results are consistent with observational evidence

[12] I built on Kessler et al.'s (2019) Incentive Resume Rating paradigm, while departing from it in two substantial ways. First, among the profiles to be rated, at least one profile evaluated by each respondent is real. The respondent is informed that one is real but is not told which one. Profiles' additional information is manipulated in reasonable ranges to minimize the likelihood of respondents' identifying ex-ante which profiles are real and which ones are hypothetical. For each profile, I created two additional versions with randomly manipulated information on speeding so that, for each profile, three versions were available: slow, average, or fast driver. The categorization of speeding behavior is based on the speeding distribution of the pilot sample and corresponds to its terciles.

[13] Details about the measurements of the outcome variables are reported in Appendix section D.

of limited supply-driven incentives to drive fast: drivers associate speeding behavior with higher fuel costs and lower disposable income. The effect is sizeable: above-median speeding behaviors are perceived as a monetary loss comparable to two customers less per day, the average trip paying Usd 0.44 (Ugx 1,678). However, speeding improves the social image of the driver among colleagues as measured by the ability to influence decisions at their station and is a direct measure of "coolness". Perceived driving skills increase with speeding habits, suggesting that the adoption of risk-taking behaviors helps to gain social approval.

Taken together, descriptive evidence and results from the *belief experiment* suggest that (i) speeding behavior is visible to coworkers operating from the same taxi station; (ii) drivers use speeding behavior to update their beliefs on other drivers' social status; and that (iii) speeding is not perceived to have a financial upside.[14]

3 Conceptual Framework

I adapt the framework of incentives and prosocial behavior from Bénabou and Tirole (2006) to study conformity to harmful norms and the role of financial incentives in defying the norms. I augment the framework by allowing the real financial incentives received by the agent to differ from what others perceive those financial incentives to be. The model serves three purposes: (i) it introduces the main objects considered in the experimental design; (ii) it delivers testable predictions about the mechanisms through which the visibility of monetary incentives impacts norm defiance; (iii) it guides the analysis and the interpretation of the results.

3.1 Drivers' Preferences

I study the behavior of drivers who choose the extent of their slow driving.[15] Each driver selects the level of slow driving a from a choice set A that can be discrete, as it is in the experiment where $a \in \{fast, slow\}$, or continuous. Following Bénabou and Tirole (2006), a driver has three sets of motivations to drive slow: intrinsic, extrinsic, and reputational. Each driver has idiosyncratic valuations for safe driving, v_a (for example, risk aversion), and for money, v_m (for example, financial stability, business acumen). Valuations defining

[14] It is possible that higher social status reflects indirect financial benefits. In my data, on average, drivers report comparable numbers of clients regardless of the years of experience at the stations. However, group leaders earn more and report higher job satisfaction.

[15] Based on the descriptive evidence described in Section 2, I use the terms *slow driving* and *safe driving* interchangeably.

driver's type $\mathbf{v} = (v_a, v_m)$ are private information and are drawn from a joint distribution $G(\mathbf{v})$. Choosing a entails a direct payoff of m and a utility cost $C(a)$. The material payoff m is modeled as the expected monetary incentives y of the contract that rewards safe driving more, net of the fixed monetary value o of the outside option relative to the incentives: $m = y - o$.

Based on the evidence described in Section 2, I assume that the driver's behavior a is visible to others within his social group.[16] In addition to the intrinsic motivation to drive safe, captured by $v_a a$, and the direct financial motivation given by $v_m(y - o)a$, drivers care what other drivers think about them. In particular, drivers care about others' expectations of their private valuations v_a and v_m. Others' learning about v_a depends on two possibly informative signals: the action a and the perceived financial incentives $\hat{y}$ to drive safely. As the monetary compensation o is fixed regardless of driving behavior, the value of the outside option does not reveal anything about the driver's motivation to drive safely. Others' learning about v_m depends on the action a, the perceived monetary value $\hat{y}$ of the incentives that reward safe driving, and the perceived alternative monetary compensation $\hat{o}$.[17] I adapt the reputational payoff structure of Bénabou and Tirole (2006) to reflect the patterns in my data. First, being perceived as richer comes with reputational benefits μ_m. Second, being perceived as a risk-averse driver has a reputational cost μ_a. This parameter captures the harmful norm of speeding in my empirical context. As in Bénabou and Tirole (2006), I assume that the levels of image concerns μ_m and μ_a are the same for all types, and the reputational motives are a linear combination of the expectations of the driver about others' beliefs over his type given the information on driving behavior a and the perceived material-reward system $(\hat{y}, \hat{o})$:[18]

$$R(a, \hat{y}, \hat{o}) \equiv \mu_m E\left(v_m \mid a, \hat{y}, \hat{o}\right) - \mu_a E\left(v_a \mid a, \hat{y}\right) \tag{1}$$

[16] The assumption that driving behavior is fully visible comes without loss of generality as far as it is visible to some extent. The consequences of how visible driving behavior is make the representational concerns more or less binding but do not change the essence of the interaction with the other motives.

[17] The distinction between real and perceived financial incentives maps onto my main empirical exercise, where I leverage the visibility of y alone and in conjunction with o to manipulate $\hat{y}$ and $\hat{o}$ and isolate the mechanism through which the visibility of financial incentives influences behavior.

[18] In particular, I vary what the respondent believes their colleagues believe the financial-reward system to be. For simplicity, I refer to the perceived reward system as one's beliefs about others' beliefs about the payments.

3.2 Drivers' Problem

Taken together, a driver of type (v_a, v_m) solves

$$\max_{a \in A} \{v_a a + v_m(y-o)a - C(a) + R(a, \hat{y}, \hat{o})\} . \tag{2}$$

Consider the first-order condition for a driver's choice of a:

$$c(a) = v_a + v_m(y-o) + r(a, \hat{y}, \hat{o}) \tag{3}$$

Here, $c(a)$ is the marginal cost of a and $r(a, \hat{y}, \hat{o})$ is the marginal reputational return of a:

$$r(a, \hat{y}, \hat{o}) \equiv \mu_m \frac{\partial E\left(v_m \mid a, \hat{y}, \hat{o}\right)}{\partial a} - \mu_a \frac{\partial E\left(v_a \mid a, \hat{y}\right)}{\partial a} \tag{4}$$

It is useful to interpret the equilibrium condition given by equation (3) in the discrete case, where the driver chooses whether to drive fast (that is, any speeding) or slow (that is, no speeding), as it maps onto my main empirical exercise. When the marginal cost of a is equal to the sum of the marginal returns for intrinsic, extrinsic, and reputational motives, the driver is indifferent between driving fast or slow. At the margin, a driver of type $\mathbf{v}$ decides to drive slow if their utility from choosing a is greater than the utility from not choosing it:

$$v_a + v_m(y-o) \geq c(a) - r(a, \hat{y}, \hat{o}) \tag{5}$$

Panel (a) of Figure 2 illustrates how the set of drivers deciding to drive slow, characterized by equation (5), increases with the net expected reward. Consider y fixed: the larger the outside option, the smaller the net expected payoff of the incentives for driving slow $(m = y - o)$. Graphically, when o is large, the share of drivers that slow down corresponds to area A. When o decreases, the slope $-1/(y-o)$ of types who are indifferent changes: drivers in area B+C now prefer to choose a and drive slowly.

The framework is, of course, extremely simplified and ignores heterogeneity in social concerns, among other complexities. However, it is useful for illustrating how the visibility of financial incentives to reduce speeding can mitigate reputational concerns about safe driving.

3.3 Visibility Conditions of Financial Incentives

I now analyze comparative statics under different visibility conditions for $(\hat{y}, \hat{o})$. The key part of this simple framework is that the perceived monetary rewards from the point of view of the observers can differ from the real ones received by the driver. My experiment aims to vary the perceived monetary incentives to drive safe ($\hat{y}$) and the value of the perceived outside option ($\hat{o}$), while holding constant the direct extrinsic incentives (that is, the standard effect of incentives on payoffs). The goal is to study the mechanisms through which financial incentives can reduce the reputational costs of defying the norm. In particular, the model identifies two channels through which, in equilibrium, financial incentives to drive slowly influence the reputational concerns attached to choosing a. Panels (b), (c) and (d) of Figure 2 illustrate the forces at work.

If the observers are not informed about the monetary-compensation system, the driver knows that the introduction of (y, o) is uninformative about their type from the point of view of the observers: $(\hat{y}, \hat{o}) = (0, 0)$. In this case, no reputational adjustments are triggered, as depicted in Panel (a) of Figure 2: others do not learn about v_m when they observe action a, as they associate only types in area E as those who prefer slow speed. Now consider the case where the driver knows that the observer is informed about the monetary incentives for safe driving (y) but not about the outside option (o). This means that $(\hat{y}, \hat{o}) = (y, 0)$. Two reputational effects of the visibility of y are captured by the model. First, expectations about v_a adjust as illustrated in Panel (c) of Figure 2. The marginal drivers who now switch from fast to slow driving under the monetary-reward system $y > 0$ are perceived by others as having a lower v_a compared to the average of the set of slow drivers when $\hat{y} = 0$, thus making the types in area J choose a low speed. However, the change in the average v_a for fast drivers after making y visible has the same sign, possibly making types in area I fast drivers. Taken together, the model leaves the sign of the overall predicted effect ambiguous. This mechanism exists because observers' posteriors about v_a are now updated based on the shift of drivers in area F depicted in Panel (a) from fast to slow . Second, expectations about v_m also adjust as depicted in Panel (d) of Figure 2. The model provides clear predictions for this channel. From an observer's perspective, the introduction of monetary incentives y makes the choice about a informative about the compositional change in the valuation of money. Marginal drivers (area F in Panel (b)) have a higher v_m than the average, thus increasing the reputation of slow drivers and decreasing the reputation of fast drivers. When y is visible, others are led to attribute less of a role to risk aversion in explaining the decision to drive slowly, making drivers in area M in Panel (d) drive slow. Throughout this paper, I refer to this channel as the *justification mechanism*.

My experiment aims to isolate the justification mechanism. One way to achieve this is to isolate the reputational effect that passes through v_a for comparison. The model does not have a clear prediction for this channel: if the effect is to increase slow driving, the difference in behavior between nonvisibility and visibility of y is an upper bound of the justification effect; if the effect is to decrease slow driving, then the same difference is a lower bound for the justification effect. In the model, the effect on posterior expectations of v_m also depends on the perceived outside option $\hat{o}$. Disclosing o to the observers makes $\hat{o} = o$, thus decreasing the average v_m of the drivers switching under the disclosure condition relative to the nondisclosure condition. When $\hat{o} = \hat{y}$, the reputational channel passing through $E(v_m \mid a, \hat{y}, \hat{o})$ is muted.

Finally, the visibility of financial incentives can change the perceived monetary payoff depending on whether a sharing tax exists in the social group. If a sharing tax exists, the net monetary incentives are lower when the incentives are visible compared to when they are kept private, reducing the demand for the incentives when visible to peers. This dynamic would reduce the differences between visible and private incentives, making the justification effect harder to detect in my experiment.

4 Demand Experiment: Incentives to Justify Nonconformity

Do drivers value monetary incentives to not speed more when they provide a credible social justification for defying the speeding norm? This section presents experimental evidence that the demand for such incentives depends on their visibility to colleagues operating from the same taxi station. Guided by the theoretical framework of Section 3, I hypothesize that financial incentives provide a justification for nonconforming when (i) they are visible to others and (ii) they reduce how informative the behavior is about one's motivation to conform. One way speeding becomes less informative about one's motivation to conform is when speeding also becomes more informative about one's motivation to earn more. When speeding reveals the sum of these two motivations, rather than only the motivation for social conformity, observers face a more difficult signal-extraction problem: is the driver slowing down because they value less the membership to the group or because they earn more?

In my experiment, I manipulate the informativeness of speeding by making financial incentives to drive slow visible to colleagues. How informative the visible incentives are depends on the extent to which the incentives are perceived to increase earning: the larger the expected increase in earning, the stronger the justification effect. To isolate the justification property from other mechanisms through which the visibility of incentives can impact the demand for the incentives, I also manipulate the visibility of the unconditional payment drivers can take instead of the incentives. When the fixed payment drivers are giving up by choosing the incentives is similar to the value of the contract, speeding is less informative about the driver's valuation of money. In practice, revealing the outside option makes it harder for drivers to justify driving slow by claiming that they are doing it for money.

4.1 Experimental Design

4.1.1 Sample Selection

Individuals were selected through a three-step procedure: listing, screening, and trial period. The upper section of Figure 4 illustrates the timeline of the sample selection activities.[19]

Listing. I first listed 1,450 drivers operating from 512 taxi stations throughout the city. I successfully contacted 1,156 drivers.

Recruitment and Screening. Individuals were eligible to proceed to the next stage if they met the following screening criteria: (i) between 18 and 60 years of age, (ii) English or Luganda speaker, (iii) had worked as a driver at least four days a week on average in the month before recruitment, (iv) reported no plans to permanently leave the industry or the metropolitan area of Kampala, (v) were affiliated with a taxi station in Kampala, and (vi) were interested in installing a tracking device on their motorcycle. If the driver met all the criteria, he was invited to identify the location of his stage. Among the drivers who completed the recruitment survey, 632 (55%) were eligible to participate in the experiment. Selected individuals reported working out of 415 taxi stations. From the eligible pool of drivers, I randomly selected 1 individual per taxi station. I expect most spillovers to be within stations. The purpose of this choice was to minimize spillover effects among study participants and ensure that the experimental design met the stable unit treatment value assumption (SUTVA).[20] I collected information on the social ties of participants with others inside and outside the station. The participants had strong ties with an average of 7.3 members of the station. Much more limited social ties existed between drivers working out of different stations. On average, study participants reported

[19]Section F in the Appendix provides more details on the timeline for both the sample selection and the implementation of the experiments.

[20]Exploring spillovers within organizations is beyond the scope of this paper. However, I collected contact information from drivers who belong to the same taxi stations of the study sample for future work

having 1.2 connections to other taxi stations, supporting the assumption that the relevant social group is composed of colleagues at one's station and that the potential interaction between study participants was minimal.

Office Visit. I restricted the study sample to 362 individuals, 1 per station, due to limited access to GPS technology at the time of the project's implementation.[21] Each participant was invited to the office of the local technology company with which I partnered. A token of appreciation amount of Ugx 15,000 (Usd 3.95) was provided to compensate drivers for time subtracted from the working. During the visits, GPS tracking devices were installed. The entire sample showed up at the office visit, and the devices were successfully installed and tested by a team of two technicians.

Trial Period and Attrition. After the installation, participants went through a trial period of approximately four weeks. This period served to allow me to gather baseline data on driving behavior in the absence of incentives and to screen individuals based on interest and trust in the tracking technology. Participants got acquainted with the technology and learned how to use it: 98% of the sample reported being satisfied with the tracking service, of which 78% reported being willing to pay a monthly subscription fee below Usd 1.5.[22] In my study, attrition was a particularly relevant threat to the validity of the experimental results. GPS devices had to consistently report reliable information to build the trust of participants in the underlying mobility data, and participants were required to follow up daily during the treatment period. To limit attrition, participants were requested to meet a field officer in person at their taxi station before the experiment to confirm their consent to participate. Participants who did not confirm their interest in the technology or did not schedule an in-person meeting were substituted with other eligible candidates. Three drivers did not confirm their interest in participating and were substituted with other eligible candidates.

Sample Characteristics and Representativeness. Table B1 summarizes the characteristics of the sample. The study sample consists of 362 drivers aged 20 to 60 years. The average workplace station has 22.7 members, and participants spend roughly half of their working time at the station. Participants reported spending time at the station with on average 7 other members. On average, participants work 11.45 hours a day, 6 days a week. They serve 16.7 customers on a typical working day and earn an average of Usd 9.9 per day. The study population comprises motorcycle-taxi drivers who work from Kampala stations. Figure 5 illustrates the spatial distribution of the participants'

[21] The COVID-19 pandemic brought a worldwide shortage of semiconductor-chip production: https://www.bloomberg.com/graphics/2021-semiconductors-chips-shortage/.

[22] The willingness-to-pay elicitation was not incentivized.

stations.[23] To investigate the representativeness of the study sample, participants were compared with workers in the transport industry as reported in the Uganda National Panel Survey 2019–2020 (UBoS, 2020). My sample is on average marginally older, with a comparable household composition and level of education. However, the respondents in my study have a higher net income and systematically more access to technology.

4.1.2 Eliciting Demand for the Incentives for Not Speeding

Contract: Incentives' Value and Duration. I designed a contract that offered financial incentives to avoid speeds over 50 kph in the urban area of Kampala. The payment scheme was the following: daily payment of Ugx 2,000 (Usd 0.5) regardless of driving behavior and Ugx 6,000 (Usd 1.6) if the respondent did not exceed 50 kph. The contract covered a period of 10 days. The amount represented a relatively high-powered incentive for speed compliance, given the average reported labor income of Ugx 21,000 (Usd 5.7).

Incentives' Credibility. The information used to define whether the contract was fulfilled comes from GPS data. The experimental design includes a trial period between the GPS installation and the experiment with the main purpose of letting the respondents familiarize themselves with the technology and build trust in the payment system.[24]

After the trial period, the respondents and field officers met on a scheduled visit in the vicinity of the respondents' station. In the first part of the visit, drivers were asked to confirm their interest in participating in the study and to answer an additional set of questions related to the previously installed GPS. We then asked the respondents to leave the taxi station premises to be interviewed without interruption by colleagues. After a brief set of questions about satisfaction with the technology provided, the contract was carefully explained. The respondents were then asked to make a series of binary decisions between the contract and various levels of unconditional payments. During the explanation of the task, the enumerators placed particular emphasis on the trade-off between the contract and the cash drop (unconditional payment) as a function of driving behavior.[25]

[23] Locations reported are aggregated at the ward level for confidentiality reasons.

[24] Drivers were asked to perform the following incentivized task at the time of the in-person visit. The drivers were told to drive about 200 meters down the road in front of the office during the office visit. The goal was to get as close to 50 kph as possible without exceeding it. For any performance between 45 and 50 kph recorded by the GPS tracker, a payment of Ugx 1,000 was delivered in cash right after the task.

[25] The respondents were guided through several examples explaining how much money was left on the table if they chose the contract over the payment and vice versa.

I introduced a trial round to improve familiarity with the task and then began with a choice between unconditional payments of Ugx 1,000 and Ugx 2,000. This choice had two purposes: first, it allowed me to test drivers' basic understanding of the task;[26] second, it allowed me to have one choice where, with certainty, an unconditional payment would be chosen. This feature allowed for incentive-compatible random assignment of an unconditional payment in the Impact Experiment.

Elicitation Procedure. The Demand Experiment is designed to elicit the reservation price for the contract that rewards not speeding. Enumerators elicited drivers' preferences for the financial incentives over unconditional payments of varying amounts. The interview was held in the vicinity of the taxi stations. Participants were asked to choose between the contract that paid for not speeding and an unconditional payment. The level of unconditional payments varied between Ugx 1,000 and Ugx 9,000. As shown in Figure 1, each participant made nine consecutive binary decisions. Eliciting preferences for the contract over different unconditional payments allows me to study the distribution of drivers' demand for financial incentives.[27] To address potential concerns about anchoring effects, I randomized the order of choices. Half of the respondents made binary choices from the lowest to the highest unconditional payment amount, while the other half made binary choices from the highest to the lowest unconditional payment amount.

The payment size was extensively tested and represented a sizable amount given the reported labor income. The range of cash-drop amounts was designed with two purposes in mind. First, it was meant to ensure that for at least one binary, the contract would be unequivocally preferred over the cash drop. This allowed me to randomly assign fixed cash drops in the Impact Experiment while maintaining an incentive-compatible preference-elicitation procedure in the *Demand Experiment*. For this reason, one binary choice was between an unconditional payment lower than the minimum payment ensured by the contract. The second objective was to be able to examine the demand for a commitment to not speeding: unconditional payments between Ugx 6,000 and Ugx 9,000 dominate the contract weakly or strictly. The contract paid up to Ugx 6,000 if the driver did not drive faster than 50 kph, and Ugx 2,000 otherwise.

Before making their choices, respondents were instructed to take all choices seriously,

[26] If the respondent did not pick the highest amount, the field officer was prompted to go through the instructions once more.

[27] The behavioral and experimental literature offers various ways to elicit the willingness to pay (Burchardi et al., 2021). Given the low average level of education of the study sample, the design seeks to minimize the complexity of the decisions. Sequential binary decisions are well understood by study participants and virtually no inconsistencies are detected (for example, there are no cases where the contract is preferred over a given unconditional payment, but not over a lower unconditional payment).

as each choice had a positive probability of being implemented, and one would be randomly selected to be implemented.[28]

4.1.3 Randomization of Visibility Conditions: Financial Incentives as Justification

The goal of the experimental design is to manipulate the availability of a credible justification provided by financial incentives for complying with speed limits while holding other potential motives for choosing the financial incentives. To do so, I randomly varied the visibility of either the value of the contract offered, if chosen, or both the value of the contract and the value of the unconditional payment offered to the participant.

Between-Subject Randomization. Study participants were randomly assigned, with equal probabilities, to one of the three treatment conditions that manipulated the disclosure to colleagues of different aspects of the choice between the contract and unconditional payments. Figure 6 illustrates the experimental flow.

- **Private Menu and Private Incentives:** Respondents were informed before making the sequence of choices that, for each choice, (i) their decision to opt into the contract and give up the unconditional payment offered would remain private and (ii) the financial incentives received (that is, the monetary rewards offered based on their speeding behavior) would also remain private. If the contract was rejected, the choice and value of the outside options would also remain private. In other words, both the fact that drivers were offered a choice between the contract and unconditional payment and the level of monetary rewards proposed in the contract would remain private to the respondent.

- **Private Menu and Visible Incentives:** Respondents were informed before making their sequences of choices that, for each choice, (i) their decision to opt into the contract and give up an unconditional payment would remain private, and (ii) the financial incentives, if selected, would be disclosed by the enumerator to the other members of the taxi stations, right after the interview, and would be presented as a contract offered to the respondents. If the contract was rejected, the choice and value of the outside options would also remain private. In other words, the fact that drivers were offered a choice from a menu with two options would remain confiden-

[28]Study participants were informed that all binary choices had a positive probability of being selected; however, they were not informed about the probabilities attached to each binary choice.

tial, while the value of the incentives offered with the contract, if chosen, would be disclosed to peers.

- **Visible Menu and Visible Incentives:** Respondents were informed before making their series of choices that, for each choice, (i) their decision to opt into the contract when faced with the choice between the contract and the unconditional payment and (ii) the financial incentives, if selected, would both be publicly disclosed to the other members of the taxi stations, right after the interview, by the enumerator. In other words, under this visibility scenario, respondents knew that, if they chose the contract, colleagues would be informed that the offer was the consequence of a decision between two options: the contract and unconditional payment.

Under all experimental conditions, respondents faced the same expected monetary gains from the contract. The experimental manipulations changed the reputational incentives associated with the contract, conditional on the extrinsic motive to avoid speeding. Making the contract offer visible allowed the respondents to use the financial incentives as a justification for not speeding. As the visibility of the offered contract could change respondents' interest in the contract for other reasons than its informativeness about the respondents' monetary motivation (that is, their valuation of money, v_m), I teased out these alternative channels by keeping the contract visible while reducing the informativeness of the choice about v_m. I did so by revealing that the contract offered was the result of the decision to opt into the contract rather than accept an unconditional payment. The larger the unconditional payment, the harder it became to use the contract as a justification.[29]

Within-Subject Randomization. After eliciting preferences for the contract on the set of unconditional payments under the assigned treatment condition in the between-subject design, the study participants were randomly assigned to the other visibility conditions sequentially so that the reservation price for the contract would be elicited for each respondent under all three visibility conditions. This design choice had two purposes. First, it allowed me to estimate the treatment effects of contracts and outside-option visibility by exploiting both within-subject and between-subject variations. Both designs were preregistered. In particular, the main identification strategy uses the between-subject design, which exploits variation across subjects in their first round of binary decisions under

[29]The design aims at avoiding complicated interventions for two reasons: First, given the low level of education of the study sample, I wanted to avoid heavy-handed interventions. Second, the treatment conditions have policy relevance in designing incentive schemes, as they manipulate two typically controllable margins: public disclosure of the procedure for accessing the incentives and public disclosure of the offer.

one of the three treatment conditions. The second design is within subject and exploits the variation of choices made by each participant under the three visibility conditions. The second purpose of the within-subject design was to allow me to assign the visibility condition for the real offer of the contract in the subsequent Impact Experiment, regardless of the assignment made in the between-subject design of the Demand Experiment.

Randomization Balance. Table B2 presents the balance of baseline covariates on the demographic and socioeconomic variables of the study participants, their labor market outcomes, and the organizational structure of their workplace with respect to the between-subject design. While most baseline variables are balanced, a few variables are unbalanced, as expected with a large number of comparisons. Nine out of 58 coefficients are statistically significantly different at the 10% level. The results are robust to specifications with and without controls for unbalanced variables.

4.2 Empirical Analysis

In this section, I describe the empirical strategies used to identify the causal effect of the justification property of financial incentives.

I analyze the variation between subjects by focusing on the first round of preference elicitation that was preregistered, in which study participants were randomly assigned to one of the three treatment conditions with equal probability and went only through the practice rounds before randomization.[30]

My empirical strategy consists of estimating two objects under the three visibility conditions: (i) the mean reservation price for the contract; (ii) the distribution of take-up of the contract at different prices. Guided by the theoretical framework, I am interested in the difference in outcomes between (i) the treatment condition in which, if chosen, the financial incentives were disclosed to peers and the outside option was kept private and (ii) the treatment conditions in which both the financial incentives and the outside option (that is, the menu of options) were disclosed to peers. I interpret this difference as the justification effect.

To quantify the causal effect of social justification, I estimate the following regression model:

$$\begin{aligned} Y_i^d = \beta_0 + \beta_1 \text{PrivateMenu}_i * \text{VisibleIncentives}_i + \\ + \beta_2 \text{VisibleMenu}_i * \text{VisibleIncentives}_i + \mu_k + u_i \end{aligned} \tag{6}$$

[30] I also preregistered the within-subject design, and I present the main results in Section G of the Appendix.

Here, Y_i^d denotes outcome d for respondent i, VisibleMenu$_i$ is a dummy indicating whether the decision of respondent i to opt into the contract over an unconditional payment is revealed to their colleagues, while PrivateMenu$_i$ indicates that the decision to opt into the contract over the unconditional payment is kept private. VisibleIncentives$_i$ is a dummy indicating that the financial incentives offered to the respondent i, if chosen, are revealed to the colleagues of i'. The omitted category corresponds to the fully private case, in which neither the decision to opt in to the contract (that is, PrivateMenu$_i = 1$) nor the financial incentives are publicly disclosed (that is, PrivateIncentives$_i = 1$). μ_k is a dummy that captures the order of the elicitation procedure. I preregistered two main outcomes: a dummy indicating the preference for the contract over the weakly dominating unconditional payment and a multivalued variable indicating the willingness to pay for the contract. For the sake of completeness, I report the full distribution of the demand for the contract.

In summary, the coefficient β_1 captures the differential effect of revealing the financial incentives to peers on the demand for the contract. The coefficient β_2 captures the perceived reputational cost associated with the disclosure of the decision to give up an unconditional payment on the demand for the contract. The difference between the two coefficients captures the justification effect.

4.3 Main Results

In this section, I present the results of the Demand Experiment. First, I illustrate how visibility conditions impact the share of drivers willing to forgo some money to get the contract that rewards not speeding. Second, I present the impact of the visibility conditions on the distribution of drivers' demand for the contract. Third, I show how visibility conditions affect the average reservation price for the contract.

The Choice of Dominated Contract. Drivers are 52% (22.2 percentage points, $p = 0.000$) more likely to choose the incentives over an unconditional payment equivalent to the largest possible payment under the contract when the financial incentives offered are visible to peers, compared to an average of 42.7% when the contract offer is fully private. When the financial incentives are visible and the unconditional payment is 50% larger than the best possible payment realization under the contract, the likelihood of choosing the contract is higher. Specifically, it is 80.5% (34.4 percentage points, $p = 0.000$) more likely that the driver chooses the contract if the offer is publicly disclosed while the decision to give up the unconditional payment remains confidential relative to the probability of choosing the contract when the decision to choose it over the unconditional payment is also disclosed. Figure 9 illustrates the results. Table 3 reports the estimates of equation (6), including strata fixed effects, for the share of the sample that chooses the contract over the unconditional payment.[31] Columns 1-4 show to what extent the probability of selecting the contract changes when the alternative unconditional payment is equal to or up to 50% higher than the largest payment possible under the contract.

The Distribution of Demand for the Contract. Figure 8 shows the demand distribution for the contract under the three disclosure conditions. Essentially no driver chooses the unconditional payment if the contract pays at least as much as the minimum offered by the contract. The percentage of drivers who choose the contract decreases to 42% when the gap between the highest possible payment under the contract and the unconditional payment is nil (both correspond to Ugx 6,000, which is roughly one-third of average daily earnings). The percentage of drivers choosing the contract when faced with the same monetary trade-off is considerably higher, at 65% ($p = 0.000$), when compared to the fully private condition, while it decreases to 30% ($p = 0.060$) when both the decision to accept the contract over the unconditional payment and the financial incentives offered are disclosed to colleagues. When the gap between the unconditional payment and the highest possible realization of the contract increases to Ugx 3,000 (50% increase relative to the value of the unconditional payment), the percentage of drivers choosing the contract decreases to 14% and 9% under fully visible and fully private conditions, respectively, with no detectable differences at conventional significance levels ($p = 0.213$). The slope of the demand curve for the contract is much flatter when the contract offered is publicized but the decision to accept the contract remains confidential. At Ugx 3,000 gap between the unconditional payment and the best contract outcome, the percentage of drivers choosing the contract is 45%. Acceptance is three times greater than in the fully visible scenario ($p = 0.000$).

The Mean Reservation Price for the Contract. Figure 9 illustrates the main experimental results on the reservation price for the contract (that is, the minimum amount of money for which the respondent prefers an unconditional payment over the incentive contract). The average reservation price for the contract increases by 25% ($p = 0.001$) when the contract offer is made visible and the decision to choose the contract over the unconditional payment remains confidential, relative to a mean of Ugx 5,400 (Usd 1.45) when the contract offer is fully confidential. Upon disclosure of the financial incentives offered, drivers are 37% more likely to choose the contract if the decision to

[31] Results are robust to the inclusion of unbalanced covariates reported in Table B2; results are shown in Table G1.

choose it over the outside option remains private.[32]

To understand at what price the contract is not chosen by any of the drivers, respondents who choose the contract over the highest unconditional payment offered (Ugx 9,000) are asked to report how much they would give up to receive the contract. The elicitation procedure for high values is not incentivized. When the contract and the decision to give up an unconditional payment are private, virtually no driver chooses the contract when the alternative unconditional payment is greater than Ugx 12,000.[33] The same applies to the scenario in which the decision to accept the contract over the cash drop is disclosed. When only the financial incentives offered in the contract are disclosed, the maximum price recorded is Ugx 14,000.[34] Table 4 shows that treatment effects are robust to the inclusion of covariates that are unbalanced at baseline, as reported in Table B2.[35] Columns 1 and 2 report estimates based on the reservation prices that are elicited with the incentive-compatible procedure (truncated at Ugx 9,000), with and without controls. Columns 3 and 4 report estimates that include the non-incentivized elicitation above Ugx 9.000.[36] On average, drivers are willing to pay 33%–39% more to receive the contract when the contract offer is publicized and the decision to accept remains confidential, relative to the full-disclosure scenario.

4.4 Visibility Treatment Effects: Discussion

Linking the Experiment to the Theoretical Framework. I proceed by discussing the results through the lens of the conceptual framework of Section 3. In the model, the visibility conditions in the experiment influence the reputational incentives $R(a, \hat{y}, \hat{o})$ by manipulating the perceived financial reward system $(\hat{y}, \hat{o})$. The benchmark case is where both the contract offered, if chosen, and the decision to choose the contract over the outside option remain confidential. In this case, reputational incentives operate only through

[32] These estimates are lower bounds of the treatment effects, as the reservation prices elicited with the incentive-compatible procedure are truncated at Ugx 9,000, as far as there are no inconsistencies with higher values (for example, respondents refusing the contract over Ugx 10,000 but choosing it over Ugx 11,000).

[33] Values are winsorized at the 99% level, as three participants in the control group reported extreme values of Ugx 18,000 to 25,000.

[34] Reservation prices above Ugx 9,000 were not incentivized and are not used in the preferred empirical specification as in equation (8) and were not preregistered.

[35] The covariates added as controls to the main specification are dummies for no education and high risk preference, number of working days per week, share of recurrent clients from the stage, and average kilometers per trip.

[36] The regression analysis based on the within-subject design is presented in Section G of the Appendix. Results are similar in magnitude to those obtained with the design between subjects as illustrated in Table G2.

visibility of driving behavior. Perceived material rewards can influence reputational incentives to drive slowly through two channels. One channel is how people build expectations about the valuation of money, v_m. When unconditional payment ($\hat{o}$) remains confidential, but the financial incentives offered (y) are made visible to colleagues, the perceived and real value of the incentives to not speed up align ($\hat{y} = y$) while the expected unconditional payment remains null ($\hat{o} = 0$). Under these visibility conditions, slow drivers are now, on average, perceived as more concerned with financial stability, whereas fast driving reveals that they care less about money. Slow driving reveals more concern for financial incentives relative to scenarios where the net financial rewards are lower. It is harder for others to answer the question: Does the driver drive slow because they are risk averse (hence, uncool) or because they care about earning more? I interpret this channel as the social justification property of financial incentives. If drivers are aware of this, they can use the contract as an excuse, as long as the net incentives perceived by others are large enough. In my experiment, perceived net financial incentives are not a credible justification in two cases: when the entire financial reward system is confidential ($(\hat{y}, \hat{o}) = (0, 0)$) and when it is fully disclosed ($(\hat{y}, \hat{o}) = (y, o)$, therefore $m < y$). In the first case, informing others about one's extra earnings is cheap talk from the point of view of colleagues—there is no way for the colleagues to verify that the driver received more money to drive slow. In the second case, the net incentives are revealed to be lower than the value of the contract, therefore reducing the possibility of using money as a justification. The second channel through which incentives influence reputational concerns is how people build expectations about one's valuation of safe driving, v_a. Once you can receive money to drive slow, do you do it more or less if others know about the financial incentives? The model has an ambiguous prediction for this mechanism, which is active as long as the contract is visible (regardless of the decision to choose it over an unconditional payment).

In the regression framework illustrated in equation (6), the coefficient β_1 captures both mechanisms described above. Meanwhile, β_2, for a sufficiently large perceived outside option $\hat{o}$, mutes the *justification* channel (that is, driving behavior is not informative about one's valuation of money, conditional on having chosen the contract). I am interested in estimating the role of the justification property of the contract in shaping the demand for the contract, which corresponds to the difference between β_1 and β_2. The results presented in Section 4.3 demonstrate the importance of the ability of financial incentives to justify deviant behavior as a key motivating force behind the demand for incentives. On average, drivers demand financial incentives that offer a credible justification more than twice as often as the equivalent monetary incentives that do not offer a credible justification. The availability of a social justification provided by the contract makes drivers less sensitive about their taste for money: as the unconditional payment proposed as an outside option increases, the demand for the contract decreases with a lower gradient when the contract can provide a justification.[37]

Alternative Mechanisms: Sharing Tax and Signaling Social Conformity. There is abundant empirical evidence on the existence of a sharing tax within groups, and this pattern is particularly accentuated in low-income countries. If a sharing tax is present, the expected net monetary value of the financial contract is reduced for the respondent when the contract terms are visible. The sharing-tax mechanism may interact with the effects estimated in equation (6). A sharing tax increases the extrinsic incentive to choose the contract in the fully private scenario, suggesting that β_1 identifies a lower bound for the impact of disclosing the financial incentives to colleagues and β_2 does the same for the effect of fully revealing the decision-making process and the offered contract. Under the assumption that the sharing tax has equal or less bite when the outside option is also disclosed,[38] the sharing-tax mechanism reduces the difference between β_2 and β_1. As a consequence, we can interpret the estimated difference $\hat{\beta}_2 - \hat{\beta}_1$ as a lower bound of the justification effect. Another mechanism through which the informativeness of driving behavior changes as a function of the visibility of the monetary incentives is another form of signaling social conformity: drivers might choose the contract over the unconditional payment and then decide to speed and not receive the payment. This would reduce the difference between β_2 and β_1, the main hypothesis would be less likely to hold as this mechanism operates in contrast with the justification mechanims.[39]

Who Wants a Justification? I calculate the difference in the reservation price under different visibility conditions for each of the study participants. On average, drivers have a reservation price Ugx 1,500 higher when the justification is available than when it is not. Figure 10 illustrates the distribution of the differences in the reservation price for the contract between the visible-with-justification condition (Private Menu and Visible Incentives) and the visible-without-justification condition (Visible Menu and Visible Incentives). I interpret this measure as the within-subject value of the justification property of financial incentives. Drivers who associate a positive price with the justification are on

[37]Importantly, this experiment estimates the differences in take-up of a specific daily contract that is sizable (approximately one-third of daily income). For this reason, we should be careful in extrapolating the treatment effect of the visibility conditions for contracts offering different payment schemes (amount, timing, etc.).

[38]For instance, one can imagine that the sharing tax is applied to the net incentives.

[39]Payment was communicated only if the reward was strictly positive. The goal was to minimize the incentives to countersignal.

average less risk loving, work from larger taxi stations in less central areas, and earn less. Most importantly, drivers who want to pay for a justification mention peer pressure as the primary motive for speeding. Despite the purely observational nature of these correlations, they shed light on the type of driver that may be strategically interested in financial incentives that provide an excuse. The patterns are reassuring, as the drivers interested in the justification are also the most vulnerable to peer pressure and less financially stable.

5 Impact Experiment: The Justification Effect of Financial Incentives on Behavior

Drivers are willing to incur significant costs to publicly rather than privately receive contracts that reward not speeding. The Demand Experiment demonstrates that visible financial incentives have justification properties valued by drivers. The welfare consequences of the demand for justification by means of financial incentives depend on whether and to what extent this property impacts driving behavior and labor market outcomes. This section analyzes a field experiment that randomizes the visibility of the offer of the contract for not speeding and benchmarks it with an unconditional payment to control for income effects. In particular, I analyze two sets of outcomes. First, I study to what extent social-justification properties impact driving behavior. Second, I turn to labor market outcomes to identify the consequences for labor supply, income, and productivity.

5.1 Experimental Design

5.1.1 Randomization: Incentives and Unconditional Cash Transfer

The main treatment of interest is the visibility of the contract for not speeding, which I interpret as the effect of the social justification property of the contract. To isolate the impact of this property while holding fixed the monetary gains from the contract, I also compare outcomes from a treatment arm with the publicly offered contract to a treatment arm with the same contract but offered in private. These two conditions correspond to the *Private Menu and Visible Incentives* and *Private Menu and Private Incentives* conditions of the Demand Experiment. I also benchmark the social-justification effect with the financial-incentive effect, while controlling for the income effects. I do so by introducing a third treatment arm in which participants receive an unconditional cash transfer.

The study sample for this experiment is the same as for the Demand Experiment. This design choice has the advantage of linking the preferences for financial incentives with their behavioral and economic impact. To deal with the issue of selection into receiving the contract based on the participants' preference, while maintaining incentive compatibility in the demand-elicitation procedure in the *Demand Experiment*, the binary choice implemented was drawn from a distribution with high masses on the choice of Ugx 1,000 or the contract (decision (1), illustrated in Table 1) and on the choice between two unconditional payments. The choice between two unconditional payments was offered in the incentivized round before the elicitation of the reservation price). Consequently, participants received the payment scheme randomly assigned to them, regardless of their willingness to pay. Figure 11 illustrates the randomization procedure.

- **Private Menu and Visible Incentives:** Respondents in this group were randomly assigned to receive their preferred payment scheme in the binary choice of Ugx 1,000 or the contract in the Private Menu and Visible Incentives arm of the Demand Experiment. The take-up of the contract over cash was universal in this binary choice (corresponding to binary choice (1) in Table 1).

- **Private Menu and Private Incentives:** Participants in this group were randomly assigned to receive what they preferred in the binary choice of Ugx 1,000 or the contract in the Private Menu and Private Incentives arm of the Demand Experiment. Under this disclosure condition, all respondents in the Demand Experiment preferred the contract over the cash.

- **Unconditional Cash Transfer:** The control group was assigned to receive their preferred payment scheme elicited in the binary choice confronting cash transfers. To maintain similar average payments between treatment groups, drivers in this group received a larger payment than the one presented in the Demand Experiment. In particular, the unconditional payment was set at Ugx 4,500 (Usd 1.2) daily for the 10-day duration of the contract. This amount was calculated based on the average realized payment under the private-incentives contract in the pilot sample. This amount is in line with the expected payment of the private incentives at baseline. The size of the cash transfer was preregistered.

Participants were randomly assigned to the three treatment groups described above with the following procedure. First, to create random variation in the exposure to financial incentives for not speeding, a randomly selected subsample of participants was offered financial incentives to comply with the speed limit of 50 kph for a period of 10 days. The remaining individuals were assigned *unconditional cash transfers* regardless of their driving behavior (55 drivers). Second, to estimate the effect of financial incentives when they could be used as a social justification for not speeding, a randomly selected subset of drivers among those assigned to receive the no-speeding contract was offered the incentive scheme under the Private Menu and Visible Incentives condition (125 drivers). The remaining participants were offered the same incentive scheme under the Private Menu and Private Incentives condition (180 drivers).

As nothing impeded the drivers themselves from publicizing the incentives received, the payment system was designed in the following way: the payment was made with no reference to the driving behavior and the contractual agreement, but it was mentioned by the enumerator at the end of the daily phone surveys during the duration of the contract. Payments were made through mobile money after three days of the contract (to confirm credibility toward the payment system) and at the end of the contract period. Essentially, the payment system was designed to ensure the credibility of the contract from the perspective of the colleagues when the contract offer was publicized by the field officer responsible for administering the baseline survey. However, any communication in the case of the private contract would have had less credibility (essentially cheap talk), as it would not have come from the field officer at any point during the experiment.

5.1.2 Outcomes

The main outcomes of interest in this study are (i) driving behavior and (ii) labor market participation, earnings, costs, and productivity.

Driving Behavior. To measure changes in driving behavior, I use GPS spatial data collected at high frequency. My main preregistered outcome of interest is the share of days when speed did not exceed the limit of 50 kph, as specified in the contract. To investigate whether the treatments changed extreme driving behavior, two measures were used: the average excess speed conditional on violating the speed limit on a given day and the maximum speed registered daily.[40]

Primary Labor Market Outcomes. To study individual labor market patterns, all individuals were surveyed at baseline, in daily surveys during the Impact Experiment, and at endline, one month after the end of the treatment period. Throughout these surveys, respondents were asked to report the following variables: number of hours at work, daily revenues, costs, disposable income, and number of customers who asked for

[40] Speed is winsorized at the 99% level to exclude possible misreporting by the GPS trackers. Also, since GPS data are reported every two seconds when the bike is in motion, the speed reported after every speed change over 20 kph from the previous observation is coded as missing. This data cleaning procedure affected only 0.000002% of the information collected by the GPS.

their contact information. Based on these variables, in the main analysis, the following labor market outcomes are used: labor supply, earnings, costs, productivity, and number of new recurrent customers.

The randomization balance is presented in Table B3, and attrition is discussed in Appendix Section H. As in the Demand Experiment, while most baseline variables are balanced, a few variables are unbalanced, as expected with a large number of comparisons. Ten out of 58 coefficients are statistically significantly different at the 10% level. The results are robust to specifications with and without controls for unbalanced variables.

5.2 Empirical Strategy

To quantify the impact of the social justification provided by the incentives, I estimate the intention-to-treat effects following the analysis of covariance (ANCOVA) specification for worker i in strata s, t days into the experiment:

$$\begin{aligned} Y^d_{ist} = {} & \beta_1 \text{PrivateMenu}_i * \text{PrivateIncentives}_i + \\ & + \beta_2 \text{PrivateMenu}_i * \text{VisibleIncentives}_i + \\ & + \gamma y_{i0} + \lambda_s + u_{ist} \end{aligned} \tag{7}$$

Here, Y^d_{it} is the outcome of interest d for driver i, and y^d_{i0} is the outcome of interest d at baseline; and λ_s are strata fixed effects, namely net income at baseline, a dummy for positive willingness to pay for the visible contract in the Demand Experiment, share of recurrent customers, and average kilometers traveled per working day in the month before the experiment. As randomization was at the driver level, I use robust standard errors clustered at the respondent level.

The coefficient β_1 measures the causal impact of the incentive scheme in addition to the expected financial rewards. The coefficient β_2 captures the impact of the incentive scheme when publicly offered, relative to the unconditional cash payment. The main hypothesis is that there is a difference between β_1 and β_2 and that it captures the effect of the social-justification property of the incentives averaged over the 10 days of exposure to treatment. In particular, I hypothesize that visible contracts that offer social justification are more effective at reducing speeding relative to the same contract offered in private. Drivers who receive the contract in private are subject to the same reputational costs as in the absence of incentives, while drivers who receive the publicly offered contract face lower reputational costs. Publicly offered incentives mitigate the behavioral grip of the speeding norm, allowing participants to deviate at lower social cost. As the take-up of the contract, when offered, was universal, the intention-to-treat effect corresponds to the average treatment effect on the intensive margin—that is, conditional on take-up.

5.3 Results

5.3.1 The Impact of the Justification Property on Driving Behavior

This section reports the treatment effect of the contract when its offer is disclosed to colleagues and the effect of the same contract offered in private, relative to receiving the unconditional cash transfer.

Does the Justification Property of the Contract Increase Compliance with Speed Limits? Table 5 reports the treatment effects of incentives under the fully private condition and the Private Menu and Visible Incentives condition compared to the unconditional cash transfer. I report estimates for regression model 7. As reported in column 1, private incentives decrease the share of speeding days by 4.6 percentage points ($p = 0.06$), corresponding to a reduction of 5.4% relative to a mean of 0.86 under the unconditional cash transfer. The effects are substantially higher in the Private Menu and Visible Incentives condition: drivers comply with the speed limits set by the contract by 9.3 percentage points ($p = 0.00$) more than absent the incentives, an effect 72% larger than the one estimated under the private contract.

Does the Justification Property Affect the Distribution of Speeding Behavior? Columns 2 and 3 of Table 5 show how the treatments also shifted the distribution of speeding behavior. When incentives are private, the average speed over 50 kph decreases by 5% and is not statistically different from zero at conventional levels ($p = 0.24$). When the incentives are visible and the underlying choice is private, the impact is considerably larger, with the average excessive speed decreasing by 17% ($p = 0.00$), a decrease of 0.87 kph relative to a control mean of 5.11 kph conditional on speeding. I further explore the effect on speeding behavior by looking at the maximum speed. The average treatment effect corresponds to a 6.2% decrease ($p = 0.00$) under the Private Menu and Visible Incentives condition and a 2.8% decrease ($p = 0.06$) under private incentives, relative to a mean of 61.8 kph under the unconditional cash transfer. The differential treatment effects between the fully private condition and the Private Menu and Visible Incentives condition are systematically different from zero at conventional levels of statistical significance. Column 4 reports that there is no statistically significant treatment effect on the average distance covered in a day.

The results suggest that the social justification property increases the effectiveness of the contract in two ways: first, drivers whose excess speed was close to the limit of 50 kph systematically reduced their speed and drove below the limit; second, incentives shifted downward the distribution of speeding behavior, and more so for high speed when a social justification existed. These patterns are consistent with the idea that incentives that provide social justification impact risky behaviors, such as high speed, even when they do not directly affect the likelihood of fulfilling the contract. Table H1 shows that the results are robust to the inclusion of covariates that were unbalanced at the beginning of the study.

5.3.2 The Impact of the Justification Property of the Contract on Labor Market Outcomes

Do incentives with the justification property impact labor market outcomes more than incentives with the same payoff that offer no justification? Turning to the impact of incentives on labor market outcomes, I test the hypothesis that safety is traded off against productivity. To estimate the average treatment effects of the contract under different disclosure conditions, I estimate regression model 7. Table 7 reports the regression estimates. Incentives did not substantially affect labor supply and revenues. Column 1 shows how the treatment reduces the number of hours worked by 20 to 25 minutes, relative to a control mean of 8 hours and 7 minutes. The estimates are not statistically distinguishable from zero ($p = 0.34$) or from each other ($p = 0.29$). Similar patterns are revealed for earnings, as shown in column 2. In particular, earnings increase by about Ugx 1,000 (Usd 0.26), a 3.3% increase, under both visible and private incentives compared to the cash transfer, but they are not statistically significant at conventional levels. Column 3 shows that incentives under the Private Menu and Visible Incentives condition reduce marginal daily costs by Ugx 2,100 (Usd 0.55) ($p = 0.00$) relative to a control mean of Ugx 14,270 (Usd 3.75). Incentives under the Private Menu and Private Incentives condition also decrease, to a similar extent, the marginal daily costs (Ugx 1,500 $p = 0.01$), the two effects being statistically indistinguishable from each other ($p = 0.23$). The same pattern holds for productivity, which increases by Ugx 370–425 (Usd 0.1–0.12) for private and visible contracts, relative to a control mean of Ugx 2,083 (Usd 0.54). The effects correspond to a 15%–20% increase in net revenues per hour worked. Although the positive impact is larger for the visible contract, the difference is not statistically significant ($p = 0.60$). Table H2 shows that the results are robust to the inclusion of covariates that were unbalanced at baseline.

5.3.3 Further Evidence on the Justification Mechanism: Heterogeneity by Demand for Justification

Do drivers who value a justification respond more to the contract that provides it? If the contract with the justification property helps drivers loosen the behavioral grip of the speeding norm, then drivers who demand more contracts with the justification property relative to the contract without the property should change their behavior more. I use the individual measure of willingness to pay for the justification reported in Figure 10 to investigate whether drivers' demand for justification is associated with behavioral responses to offering contracts with such properties. Drivers who have a positive willingness to pay for the justification (that is, reservation price higher under the Private Menu and Visible Incentives condition than the Visible Menu and Visible Incentives condition) decrease their speeding behavior substantially more. Table 8 shows the heterogeneous treatment effects on driving behavior. Drivers who value the justification property are more likely to comply with the speed limit; the same group of drivers also explains the impact of the justification property on the distribution of speeding behavior. Speeding decreases significantly among drivers who value the justification property and are offered the contract with that property. The patterns are consistent for both speeding measures reported in columns 2 and 3. The differential effects on labor market outcomes by whether drivers are willing to pay more for a contract that provides a justification are reported in Table 9. In particular, the reduction in marginal costs (column 3) caused by the incentives that provide the justification is mostly explained by those drivers who valued the justification at baseline. The reduction in costs is also reflected in higher productivity gains, as reported in column 5.

Taken together, the experimental evidence illustrates how publicly offered contracts promoting no-speeding behavior are more effective in reducing speeding than equivalent contracts delivered confidentially. Furthermore, these incentives have a sizable positive economic impact on the profits and productivity of drivers.

6 Conclusions

This paper hypothesized that visible financial incentives can provide a justification for breaking harmful social norms. It tested this hypothesis in relation to an intrinsically important issue—road safety in the Global South—using motorcycle-taxi drivers in Kampala, Uganda, as the study setting. After providing descriptive and experimental evidence to document that speeding serves as a means of gaining social approval among colleagues, I explored how financial incentives can be designed to help break the speeding norm. These incentives work by not only increasing direct financial gains but providing a justification to defy the norm.

In the *Demand Experiment*, I demonstrated that the demand for the incentive contract depends greatly on its ability to justify a driver's decision to contravene the norm. In the *Impact Experiment*, I showed that the justification property significantly reduces speeding, especially when this behavior is driven by social image concerns.

Importantly, the effects faded after the incentives ended, persisting for approximately one week after treatment. No effects on labor market outcomes were detected in the endline survey administered one month after the intervention. This evidence is consistent with the justification mechanisms and suggests the justification has to be credible to make the effects persist. Anecdotally, after receiving the visible contract, drivers report interest in other ways to credibly justify not speeding. For example, they mention recurrent customers as a valid motivation for responding to customers' preference for speed-limit compliance.

In my experimental setting, the consequences of speeding call for regulatory intervention for many reasons. One first-order reason is that speeding imposes two massive negative externalities: (i) on others' safety and the health care system and (i) on the environment. In Uganda, the total annual cost of road accidents is estimated at approximately Usd 1.2 billion, representing 5% of Uganda's gross domestic product, and costs the lives of 10 people per day. Furthermore, the World Bank declares that "decarbonizing road transport is one of the most critical challenges of our time." The carbon footprint produced in low-income countries is an order of magnitude lower than in high-income countries but is growing extremely quickly due to the low quality of the (typically imported) motor vehicles, with important implications for emissions, safety, and health. During the field activity, information was collected on the quality of motorbike engines (as a proxy for fuel efficiency) in my study sample. These data, paired with fuel-consumption data and a strong assumption about the relationship with CO_2 emissions, allow me to ballpark the effect of visible incentives: a 5% to 25% decrease in the carbon footprint of an average motorbike, relative to no incentives. These calculations are very rough approximations and require more work.

The second reason is that speeding is associated with harmful norms among drivers that undermine the functioning of the market: drivers do not respond to consumer preferences, despite being aware that this limits their own profits. Designing financial incentives to mitigate the behavioral grip of the norm can help drivers respond to consumer preferences.

This paper offers a proof of concept for the social-justification mechanism as a key factor determining whether financial incentives are effective in combating harmful practices. The results highlight the need to consider this channel when designing incentives to promote desirable behaviors. However, caution must be exercised in the design and implementation of policies that leverage the justification property of incentives, as they depend on the joint presence of undesirable local norms and colleagues' positive perception of drivers' choice of financial incentives.

An exciting direction for future research is exploring policy alternatives that leverage customers' willingness to pay for safe rides as a verifiable justification. It is also important to understand how this mechanism operates when larger shares of individuals within an organization are offered a justification, triggering possible general equilibrium effects.

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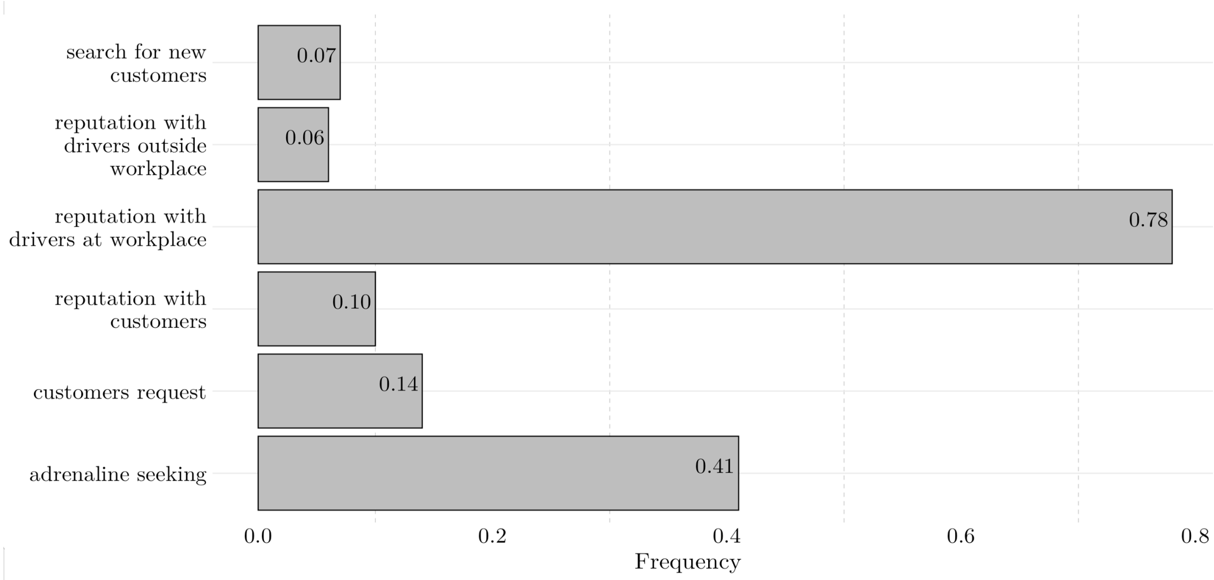
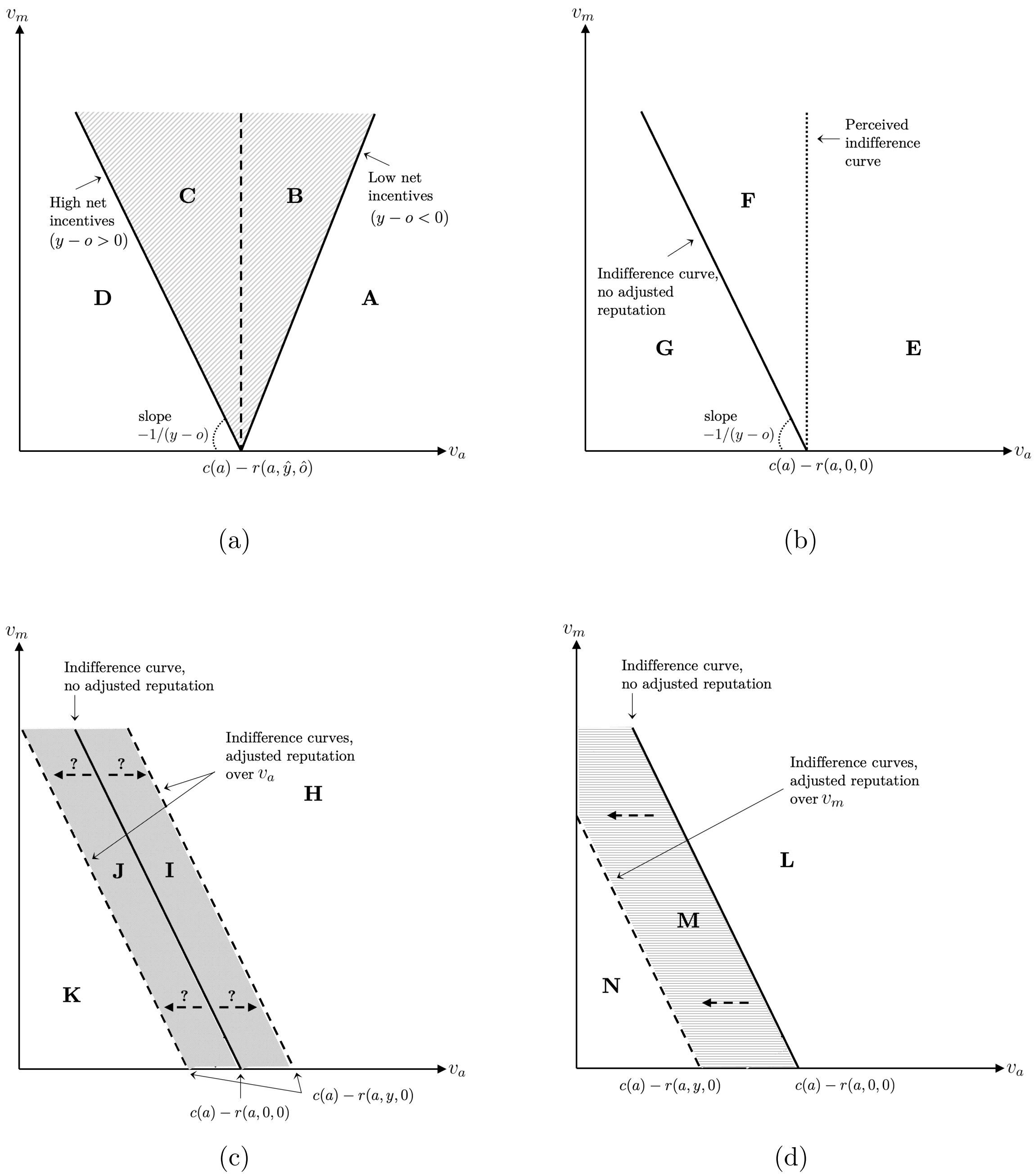
7 Main Figures

Figure 1: Descriptive Evidence: Drivers' Reasons for Speeding



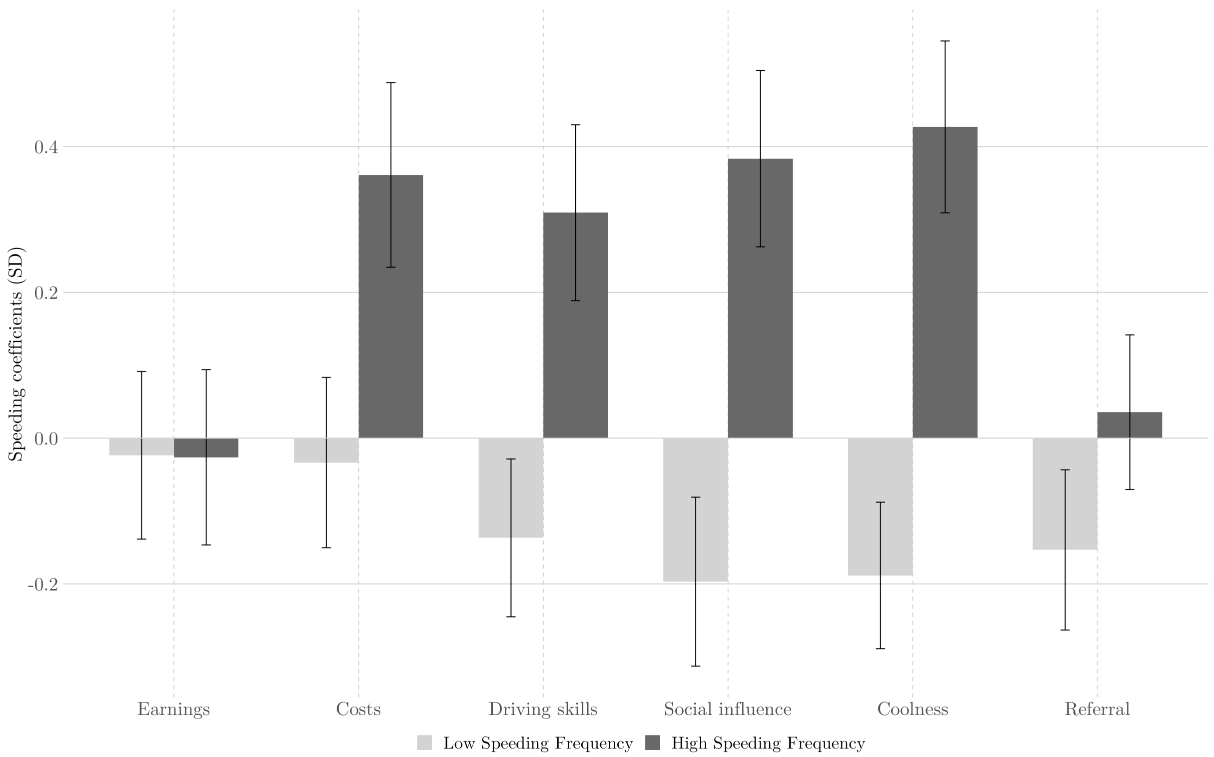
Notes: This figure presents descriptive statistics from the baseline survey on drivers' reasons to exceed speed limits of 50 kph. All the variables are binary indicators and the horizontal bars display the frequency of drivers agreeing with the statement in the baseline survey. For all variables, I used the entire sample (N = 362) surveyed at baseline, before the installation of GPS. Among those, two individuas dropped out of the study. The order in which these questions were asked was randomized to avoid anchoring effects.

Figure 2: Graphical Intuition of the Theoretical Framework



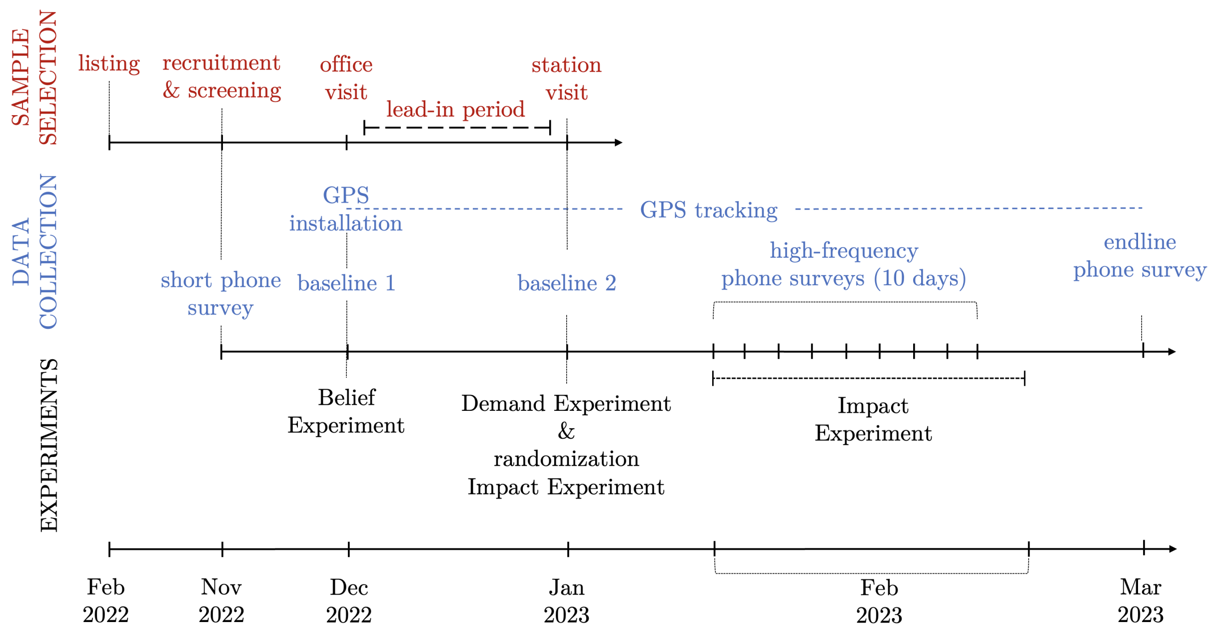
Notes: This figure illustrates how the incentives to avoid speeding $\hat{y}$ and the outside option $\hat{o}$ perceived by others influence the social image motives to choose the contract under different visibility conditions. All panels report on the x-axis the valuation v_a for safety and on the y-axis the valuation v_m for money. Panel (a) illustrates how changes in the levels of incentives for slow driving affect the group composition of slow and fast drivers. Panel (b) shows how differences in private monetary incentives affect behavior when they are private, thus the perceived monetary incentives are null. Panel (c) depicts the ambiguous effect of disclosing the financial incentives on v_a. Panel (d) illustrates the predicted effect of disclosing the financial incentives on v_m.

Figure 3: Beliefs Experiment Results



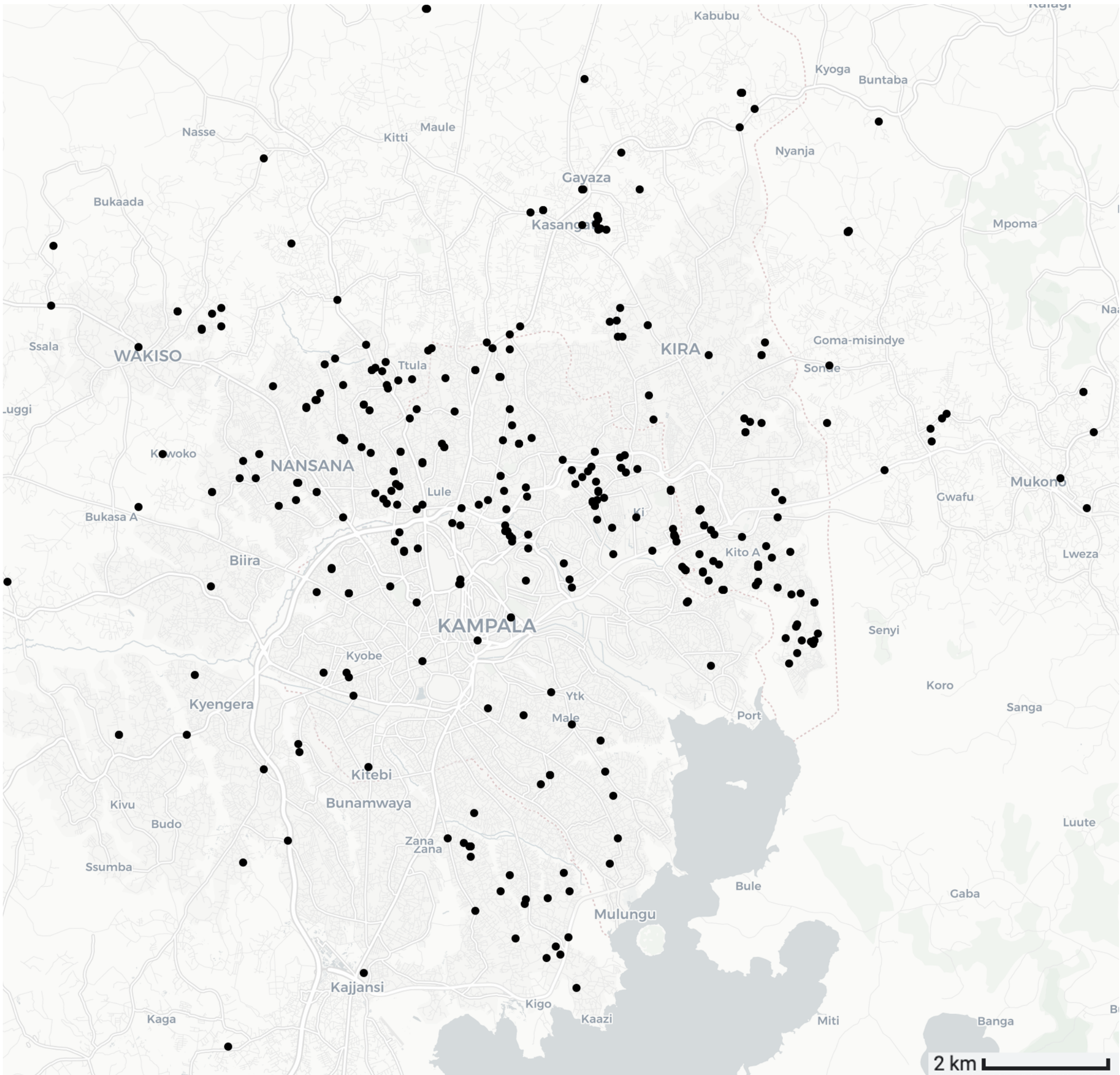
Notes: This figure illustrates the main results of the Beliefs Experiment. The bars are 95 percent confidence intervals. A total of 362 respondents rate 4 profiles of other drivers, for a total of 1,448 evaluations. The graph plots the speeding coefficients from a regression including all evaluations, standardized outcomes, profile and respondent fixed effects, and standard errors clustered at the respondent level. The omitted category is composed of drivers profiles with a history of speeding frequency falling into the central tercile of the speeding frequency distribution of the reference sample of drivers of the study pilot. *Low speeding frequency* group corresponds to profiles with history of speeding frequency belonging to the lowest tercile of the same speeding distribution. *High speeding frequency* coefficient refers to the profiles belonging to the highest tercile. The evaluation of the characteristics of the profile by the study participants is incentivized, with the exception of the variables *coolness* and whether they are willing to make a referral.

Figure 4: Timeline of Sample Selection, Data Collection and Experiments



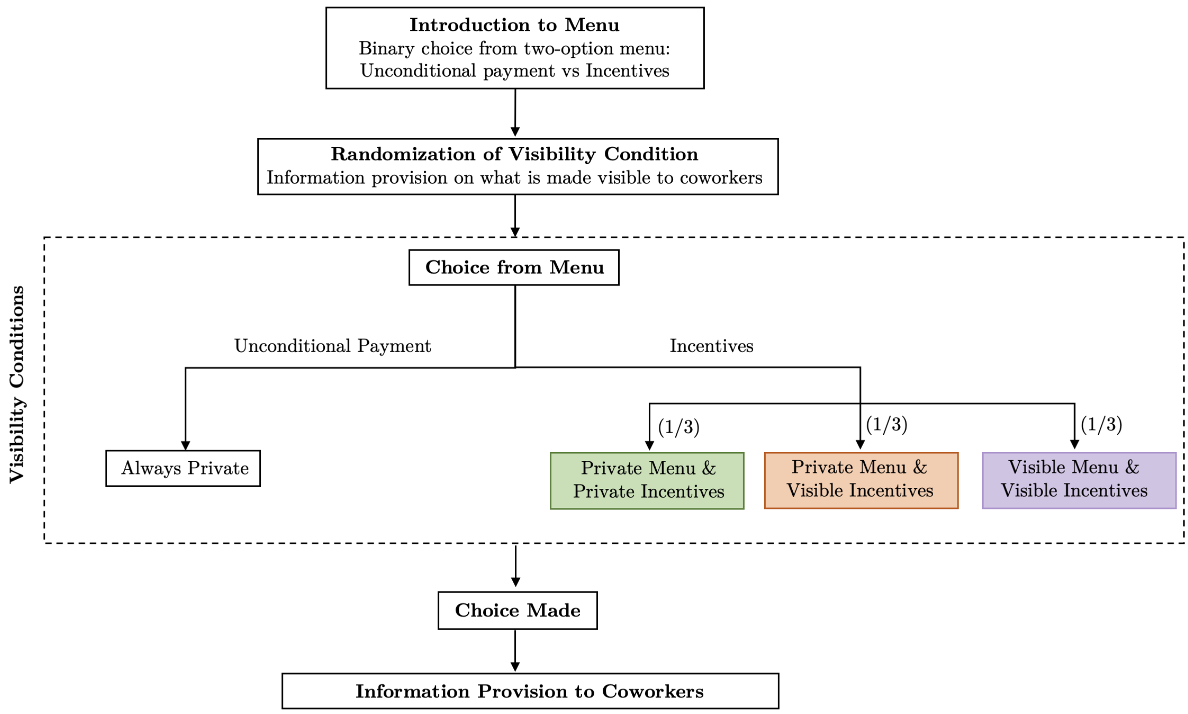
Notes: This figure details the main activities of the study. The top section of the figure illustrates the steps done to select the study sample. The mid section reports the main data collection activities. The bottom section outlines the timing of the experiments. The timeline does not report piloting work performed between 2019 and 2021.

Figure 5: Spatial Distributions of Motortaxi Stations



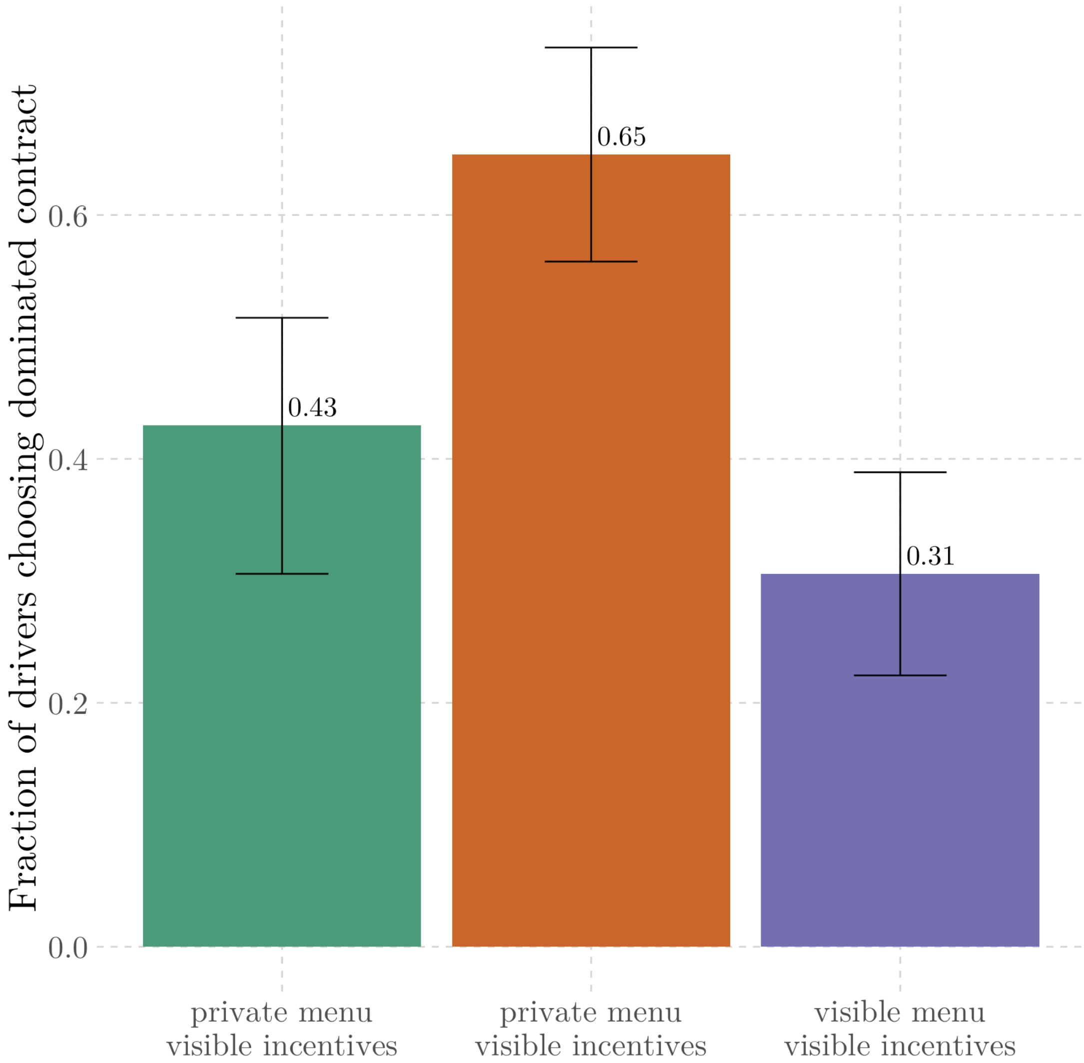
Notes: This figure reports the locations of motorcycle taxi stations of the study sample in the metropolitan area of Kampala, Uganda. To protect privacy of the participants, locations accuracy has been reduced by showing a random positions within 100m radius of the actual location for each taxi station.

Figure 6: Demand Experiment - Design Chart



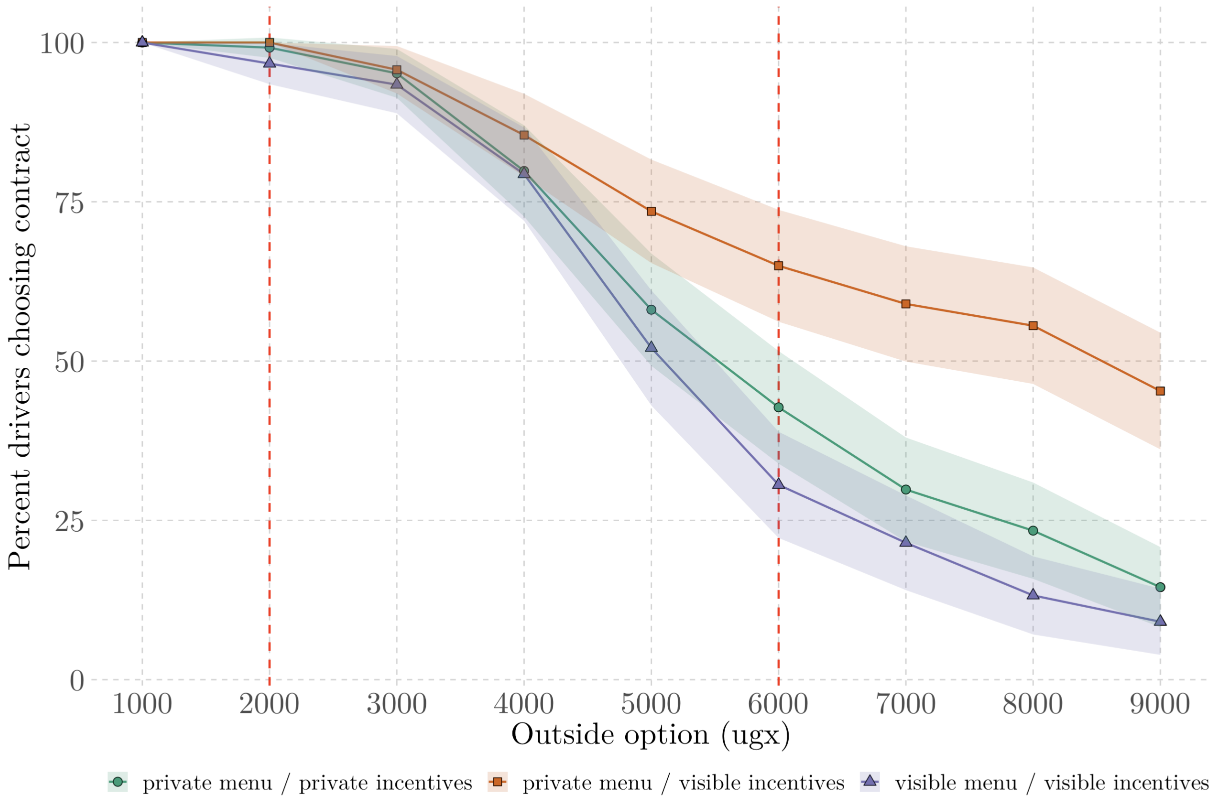
Notes: This figure describes the experimental flow of the *Demand Experiment*. Study participants are first introduced to the contract that provides incentives to avoid speeding, and the task of indicating their favorite option between the incentives and an unconditional payment is exampled. Participants who pass comprehension checks (N = 360) are randomized into three equally sized groups. Before decisions are made, the field officer informs the study participant about what will be disclosed to the coworkers at his taxi station, after the survey. The information disclosed by field officers varies by the visibility conditions of the treatment. The study participant then chooses from a given menu. Each participant repeats the choice for multiple menus where the incentives are the same, while the unconditional payment varies. The study participant is also informed that all menus have a positive probability of being selected and that their preferred option for the selected menu is offered to him. The selected option is the binding option, while the others are not offered, and the other decisions made remain confidential. After the choices are made and a random one is selected for implementation, the information is then disclosed to coworkers according to the treatment assignment.

Figure 7: Demand Experiment Results - The Choice of Dominated Contract



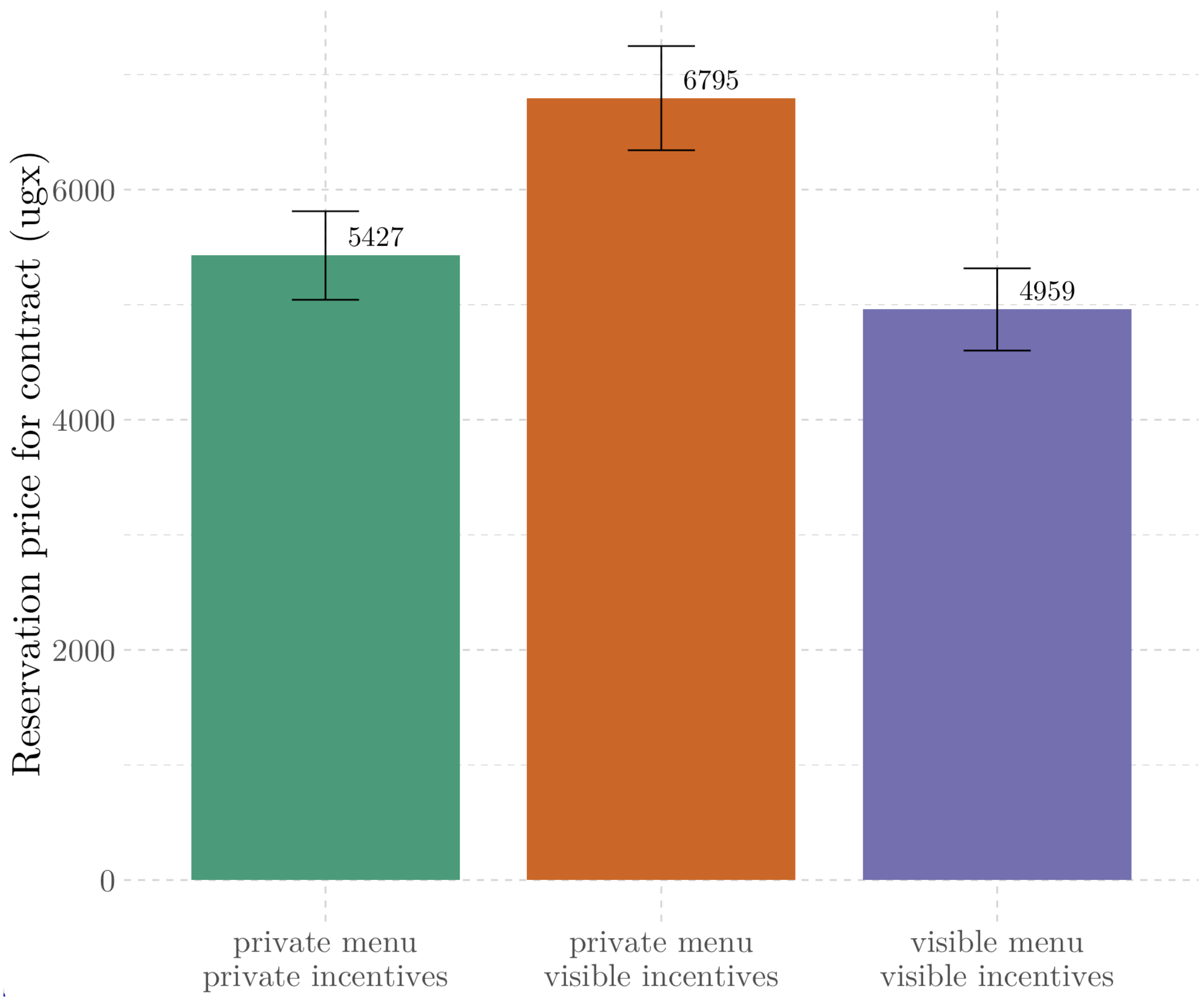
Notes: This figure reports the proportion of drivers who choose the contract, which rewards up to Ugx 6,000 if no speeding is recorded by the GPS, over an unconditional payment of Ugx 6,000 by treatment visibility conditions in the between-subject design (N=360). The choice of the contract is weakly dominated by the unconditional payment as the contract can pay at most as much. On the y-axis of the graph, I report the share of drivers in each treatment group that chooses the dominated contract. The bars report 95 percent confidence intervals.

Figure 8: Demand Experiment Results - The Distribution of Demand for the Contract



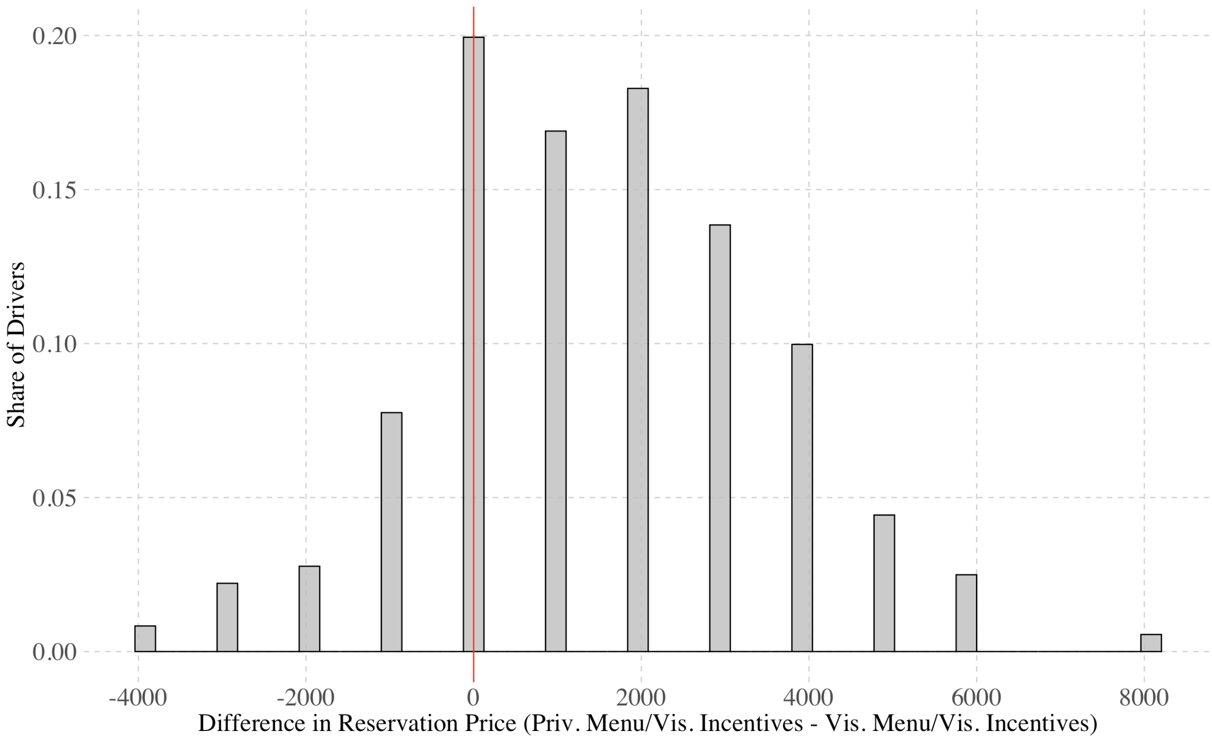
Notes: This figure summarizes the main results of the Demand Experiment. The plot reports the distribution of the demand for the contract by treatment group for the between-subject design. The preferences of the respondents (N = 360) for the contract are elicited against nine alternative unconditional payments by making nine binary choices, for a total of 3,240 choices made. The choices are randomly presented in ascending or descending order of the unconditional payment to half of the sample, respectively. The x-axis plots the value of the unconditional payment offered as an outside option. The y-axis plots the percentage of drivers by treatment group. The two vertical red dashed lines indicate the possible payment realization offered in the contract: the contract pays Ugx 6,000 per day for ten days if speed does not exceed 50 kph and pays Ugx 2,000 otherwise. The shaded areas report 95 percent confidence intervals.

Figure 9: Demand Experiment Results - Reservation Price for the Contract



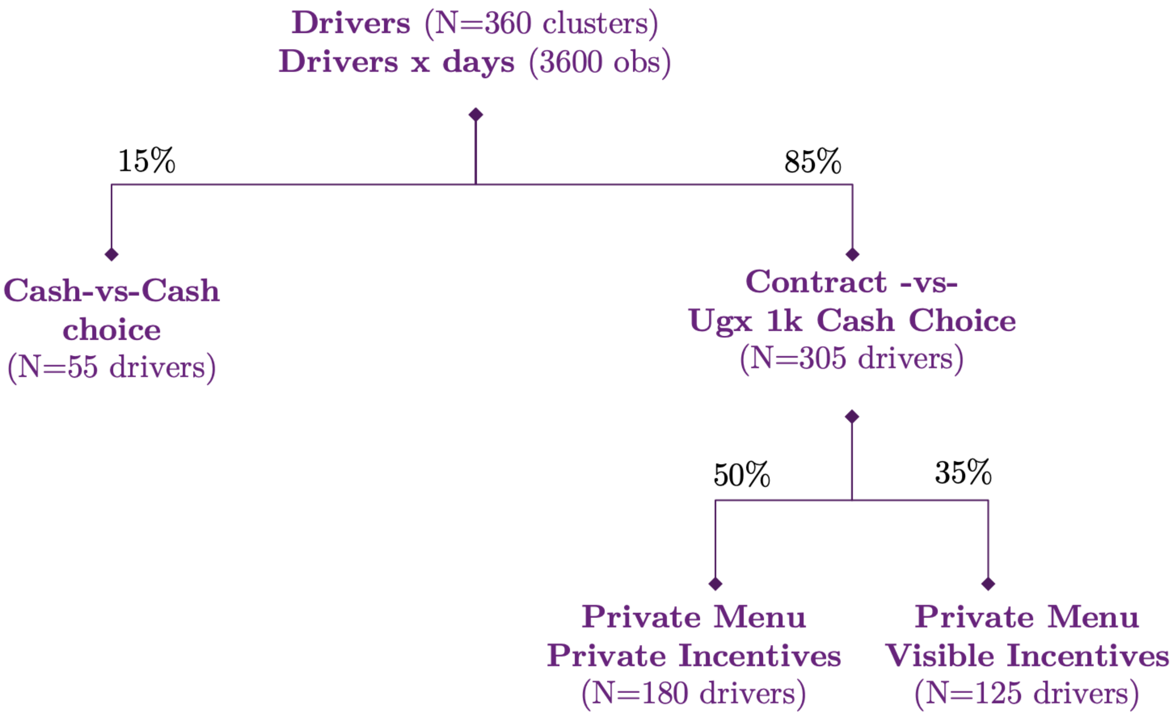
Notes: This figure reports the average reservation price for the contract elicited from drivers (N=360) in the between-subject design. The reservation price for the contract is defined as the maximum unconditional payment the driver is willing to give up to receive the contract. On the y-axis of the graph, I report the average reservation price for the contract in Ugandan Shillings by contract visibility condition. The bars report 95 percent confidence intervals. Reservation prices are truncated at a maximum of Ugx 9,000 (prices obtained with incentive-compatible elicitation procedure).

Figure 10: Distribution of Within-subject Difference in Reservation Price for the Contract



Notes: In the *Demand Experiment*, I elicit the reservation price for the contract of each participants under all three visibility conditions. This figure shows the distribution of the differences within subject of the reservation prices (Ugx) for the contract between visibility conditions: *Private Menu and Visible Incentives* and *Visible Menu and Visible Incentives*. On the x-axis, this graph reports the difference in reservation prices, while the y-axis is the fraction of the sample corresponding to it (N = 360). For instance, a value of Ugx 2,000 means that a driver has a reservation price for the contract publicly offered but privately chosen Ugx 2,000 higher than the same contract publicly offered and publicly chosen.

Figure 11: Impact Experiment - Design Chart



Notes: This figure describes the randomization procedure of the *Impact Experiment*. The randomization exploits the within-subject design of *Demand Experiment*. Each participant in the *Demand Experiment* faced three sets of nine binary choices plus one choice between two unconditional payments made privately. The binary choice to which participants were assigned was drawn from a distribution with high masses on the choice between unconditional payments and the decision "Ugx 1,000 vs. contract". Drivers who are randomly assigned to the cash-to-cash option are offered an unconditional payment of Ugx 4,500. The amount is calibrated based on the behavior of the average payment made to drivers in the pilot sample who received the contract (N=25). Drivers randomly assigned to choice (1) illustrated in Table 1 are then randomly assigned to the *Private Menu and Private Incentives* condition or the *Private Menu and Visible Incentives* condition. In both cases, study participants are offered their preferred choice in the assigned scenario.

8 Main Tables

Table 1: Reservation Price Elicitation for Incentives

	Binary Choices								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Contract (Ugx)	No Speeding: 6000 / Any Speeding: 2000								
Unconditional Payment (Ugx)	1000	2000	3000	4000	5000	6000	7000	8000	9000

Notes: The table illustrates the sequence of binary choices made by the respondents (N=360). Each respondent chooses sequentially from a menu with two options: the contract that offers incentives to refrain from speeding and unconditional payment. The contract pays Ugx 6,000 if the GPS tracker detects no speeding and Ugx 2,000 otherwise. The unconditional payment varies across choices and ranges from Ugx 1,000 to Ugx 9,000. Based on random assignment, half of the respondents are presented with a sequence of choices in ascending order ((1) to (9)) and the other half in descending order ((9) to(1)). Answers are incentivized as described in the design section 4.1. If a respondent prefers the contract over the highest unconditional payment, he is asked to report the maximum amount of money he would give up to get the contract. The responses were not incentivized.

Table 2: Beliefs Experiment Results - Perceived Outcomes

	(1) earnings	(2) costs	(3) disp. income	(4) driving skills	(5) social influence	(6) coolness	(7) referral
Low Speeding	-333	-257	-76	-0.143**	-0.208***	-0.199***	-0.088***
	[829]	[456]	[850]	[0.057]	[0.062]	[0.054]	[0.032]
High Speeding	-372	2,773***	-3,145***	0.322***	0.406***	0.452***	0.021
	[867]	[494]	[905]	[0.064]	[0.065]	[0.063]	[0.031]
Obs	1,448	1,448	1,448	1,448	1,448	1,448	1,448
Ctrl Mean	45,615	17,467	28,148	2.09	2.14	2.08	0.49

Notes: The table summarizes the results of the *Beliefs Experiment*. All regressions include drivers profile and respondent fixed effects. Each respondent (N=362) evaluates 4 profiles for a total of 1,448 evaluations. All monetary outcomes are reported in Ugandan Shilling. *Earnings* variable is the perceived daily earnings; *costs* is the perceived daily marginal costs associated with the motorcycle (e.g. fuel costs); *disp. income* is the perceived daily disposable income calculated as the difference between perceived earning and costs; *driving skills* is the response's guess of the score in a driving drill around four cones (1-4 scale) performed by the driver's profile; *social influence* is the capacity to influence the decision to introduce a new member at his taxi station (0-4 scale); coolness is the respondent's perceived coolness of the rated profile (1-4 scale); *referral* is a dummy indicating if the respondent would be willing to make a referral for the profile in his own taxi station. The table reports the estimated coefficients for high- and low-speeding frequency drivers. *Low speeding frequency* group corresponds to profiles with a history of speeding frequency that belong to the lowest tercile of the same speeding distribution. *High speeding frequency* coefficient refers to the profiles belonging to the highest tercile. The omitted category used a control group is composed of drivers profiles with a history of speeding frequency falling into the central tercile of the speeding frequency distribution of the reference sample of drivers of the study pilot. Robust standard errors clustered at the respondent level are in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Demand Experiment Results - Choice of Dominated Contracts

	(1) weakly dominated	(2) dominated +1k	(3) dominated +2k	(4) dominated +3k
Priv. Menu / Vis. Incentives	0.226***	0.294***	0.326***	0.310***
	[0.062]	[0.061]	[0.059]	[0.056]
Vis. Menu / Vis. Incentives	-0.117*	-0.080	-0.096*	-0.052
	[0.062]	[0.059]	[0.049]	[0.052]
p-value Priv. vs Vis. Menus	0.000	0.000	0.000	0.000
Observations	360	360	360	360
Mean in Priv. Menu & Incent.	0.43	0.30	0.23	0.15

Notes: The table reports treatment effects on the fraction of drivers who choose the contract when they are weakly or strictly dominated by the unconditional payment offered as an outside option in the between-subject design (N=360). I define the contract as weakly dominated when the alternative unconditional payment is equal to the maximum possible payment of Ugx 6,000 under the contract (column 1). The contract is strictly dominated when the unconditional payment is larger than the highest payment under the contract, such as Ugx 7,000, 8,000 and 9,000 (columns 2, 3 and 4). *Priv. Menu / Vis. Incentives* variable is an indicator function of whether the respondent was randomly assigned to make binary choices between the contract and a set of unconditional payments knowing that the decision to accept the contract would remain private while the financial incentives offered would be publicized among colleagues at the taxi station by the field officer. *Vis. Menu / Vis. Incentives* is an indicator function of whether the respondent was randomly assigned to taking decisions knowing that their choice to take up the contract over an unconditional payment would be revealed together with the financial incentives and the size of the unconditional payment forgone. The omitted category is the fully private condition *Priv. Menu / Priv. Incentives* where both the preference of the contract over unconditional payment and the contract terms are kept private. I report the p-value for the difference between *Priv. Menu / Vis. Incentives* and *Vis. Menu / Vis. Incentives.* Robust standard errors are reported in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Demand Experiment Results - Reservation Price for the Contact

	(1) only incentivized	(2) only incentivized	(3) all	(4) all
Priv. Menu / Vis. Incentives	1,388***	1,379***	1,727***	1,706***
	[299]	[300]	[373]	[375]
Vis. Menu / Vis. Incentives	-444*	-438	-523	-515
	[266]	[267]	[327]	[327]
p-value Priv. vs Vis. Menus	0.00	0.00	0.00	0.00
Observations	360	360	360	360
Mean in Priv. Menu & Incent.	5,427	5,427	5,694	5,694
Covariates	N	Y	N	Y

Notes: The table reports treatment effect on the reservation price for the contract in the between-subject design (N=360). Amounts are reported in Uganda Shilling. Reservation prices are elicited with an incentive-compatible procedure up to Ugx 9,000. Reservation prices higher than Ugx 9,000 are elicited by a direct survey question. Columns 1 and 2 report the results for the reservation prices truncated at Ugx 9,000; columns 3 and 4 report the results for the full support. Columns 2 and 4 report the estimated coefficient from Equation 6 by including unbalanced covariates at baseline, as reported in Table B2. *Priv. Menu / Vis. Incentives* variable is an indicator function of whether the respondent was randomly assigned to make binary choices between the contract and a set of unconditional payments knowing that the decision to accept the contract would remain private while the financial incentives offered would be publicized among colleagues at the taxi station by the field officer. *Vis. Menu / Vis. Incentives* is an indicator function of whether the respondent was randomly assigned to take decisions knowing that their choice to take up the contract over an unconditional payment would be revealed together with the financial incentives and the size of the unconditional payment forgone. The omitted category is the fully private condition *Priv. Menu / Priv. Incentives* where both the preference of the contract over the unconditional payment and the contract terms are kept private. I report the p-value for the difference between *Priv. Menu / Vis. Incentives* and *Vis. Menu / Vis. Incentives.* Robust standard errors are reported in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Impact Experiment Results - Driving Behavior

	(1) Share Any Overspeed	(2) Mean Overspeed	(3) Max Speed	(4) Distance per day (km)
Priv. Menu / Vis. Incentives	-0.092***	-0.87***	-3.84***	3.97
	[0.026]	[0.21]	[0.94]	[4.27]
Priv. Menu / Priv. Incentives	-0.045*	-0.23	-1.75*	0.86
	[0.024]	[0.21]	[0.91]	[3.89]
p-value Priv. vs Vis. Incent.	0.042	0.000	0.003	0.33
Observations	3,549	2,751	3,549	3,549
Mean in UCT	0.86	5.11	61.4	94.7

Notes: The table reports treatment effect on the driving outcomes. I report ANCOVA regression estimates on treatment group indicators, outcome values at baseline, and strata fixed effects. The unit of observation is the driver x day (N=3,549). In absence of attrition, the sample would have been composed of 3,600 observations (360 drivers for 10 days): however, the GPS failed to provide information in 7 instances, while 31 drivers did not work 10 days in the two-week period of data collection. The outcome in column 1 is a dummy that indicates whether any speeding violation has been committed on a given day (that is, GPS-recorded speed exceeding 50 kph during the incentivized day). Column 2 reports the average excess speed, that is, the average speed conditional on a speed greater than 50 kph (N = 2,751). Column 2 should be interpreted as the intensive margin, conditional on speeding. Column 3 reports the maximum speed in kph, winsorized at the top 1%. Column 4 reports treatment effects on the distance covered in a day, measured in kilometers (km). *Priv. Menu / Vis. Incentives* variable is an indicator function of whether the respondent was randomly assigned to make binary choices between the contract and a set of unconditional payments knowing that the decision to accept the contract would remain private while the financial incentives offered would be publicized among colleagues at the taxi station by the field officer. *Priv. Menu / Priv. Incentives* is an indicator function of whether the respondent was randomly assigned to take decisions knowing that their preference for the contract over the unconditional payment and the contract terms were kept private. The omitted category is the *Unconditional Cash Transfer* of Ugx 4,500 (UCT), calibrated based on the average realized payment under private contract in the pilot sample. I report the p-value for the difference between *Priv. Menu / Vis. Incentives* and *Priv. Menu / Priv. Incentives.* Robust standard errors clustered at the respondent level are in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: Impact on Compliance

	(1) Complied (day)	(2) Share Extreme Speed (day)	(3) Share Extreme Speed (time)	(4) Distance Daily (km)
Priv. Menu / Vis. Incentives	0.092***	-0.042**	-0.046**	3.28
	[0.026]	[0.016]	[0.022]	[4.65]
Priv. Menu / Priv. Incentives	0.044*	-0.027*	-0.017	0.34
	[0.024]	[0.014]	[0.022]	[4.29]
p-value Priv. vs Vis. Incent.	0.042	0.113	0.038	0.331
Observations	3,549	3,579	3,579	3,518
Control Mean	0.14	0.08	0.09	94.75

Standard errors in brackets

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Impact Experiment Results - Labor Market Outcomes

	(1) Hours Worked	(2) Revenue	(3) Marginal Cost	(4) Net Revenue	(5) Producti-vity
Priv. Menu / Vis. Incentives	-0.34	932	-2,116***	3,257**	425***
	[0.34]	[1679]	[624]	[1291]	[153]
Priv. Menu / Priv. Incentives	-0.042	997	-1,573***	2,773**	371**
	[0.30]	[1515]	[576]	[1181]	[145]
p-value Priv. vs Vis. Incent.	0.228	0.957	0.233	0.609	0.610
Observations	3,525	3,525	3,525	3,525	3,141
Mean in UCT	8.12	30,310	14,270	16,039	2,083

Notes: The table reports treatment effect on the driving outcomes. I report ANCOVA regression estimates on treatment group indicators, outcome values at baseline, and strata fixed effects. The unit of observation is the driver x day (N=3,525). In absence of attrition, the sample would have been composed of 3,600 observations (360 drivers for 10 days): however, we failed to contact a handful of drivers for 1 to 3 days during the intervention. Furthermore, productivity is not calculated if no hours worked were reported (N=3,141). Monetary outcomes are reported in Ugandan Shilling. Column 1 reports the labor supply measured in hours; Column 2 reports daily revenues; Column 3 reports daily marginal costs (i.e. fuel and daily expenses); Column 4 reports the disposable income measures as the reported by the respondent; Column 5 reports the treatment effects on productivity measured as net revenues per hour worked. *Priv. Menu / Vis. Incentives* variable is an indicator function of whether the respondent was randomly assigned to make binary choices between the contract and a set of unconditional payments knowing that the decision to accept the contract would remain private while the financial incentives offered would be publicized among colleagues at the taxi station by the field officer. *Priv. Menu / Priv. Incentives* is an indicator function of whether the respondent was randomly assigned to make decisions knowing that their preference for the contract over the unconditional payment and the contract terms were kept private. The omitted category is the *Unconditional Cash Transfer* of Ugx 4,500 (UCT), calibrated based on the average realized payment under private contract in the pilot sample. I report the p-value for the difference between *Priv. Menu / Vis. Incentives* and *Priv. Menu / Priv. Incentives*. Robust standard errors clustered at the respondent level are in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Impact Experiment - Heterogeneity by Demand for Justification on Driving Behavior

	(1) Share Any Overspeed	(2) Mean Overspeed	(3) Max Speed	(4) Distance Daily (km)
Priv. Menu / Vis. Incentives	-0.105***	-1.15***	-4.70***	0.95
× Value justification	[0.0367]	[0.28]	[1.293]	[5.91]
Priv. Menu / Priv. Incentives	-0.059*	-0.49*	-2.20*	-0.42
× Value justification	[0.033]	[0.27]	[1.27]	[5.39]
Value justification	-0.028	-0.62*	-1.14	-0.62
	[0.0384]	[0.35]	[1.53]	[7.88]
Priv. Menu / Vis. Incentives	0.035	0.68	2.11	5.75
	[0.053]	[0.44]	[1.89]	[9.71]
Priv. Menu / Priv. Incentives	0.038	0.63	1.37	2.77
	[0.049]	[0.41]	[1.81]	[9.02]
Observations	3,549	2,751	3,549	3,518
Control Mean	0.86	5.11	61.43	94.75
p-value Priv. vs Vis. Incent.	0.084	0.007	0.005	0.676

Notes: The table summarizes the heterogeneous treatment effect of the disclosure conditions of the contract by drivers' demand for a justification on the driving outcomes. I capture the demand for justification with a dummy variable called *Value justification* that indicates whether a driver has a higher reservation price for the contract when the contract choice is private relative to when the choice is disclosed to colleagues. I report ANCOVA regression estimates on treatment group indicators, outcome values at baseline, and strata fixed effects. The unit of observation is the driver x day (N=3,549). The outcome in column 1 is a dummy that indicates whether any speeding violation has been committed on a given day (that is, GPS-recorded speed exceeding 50 kph during the incentivized day). Column 2 reports the average excess speed, that is, the average speed conditional on speed exceeding 50 kph (N = 2,751). Column 2 should be interpreted as the intensive margin, conditional on speeding. Column 3 reports the maximum speed in kph, winsorized at the top 1%. Column 4 reports treatment effects on the distance covered in a day, measured in kilometers (km). *Priv. Menu / Vis. Incentives* variable is an indicator function of whether the respondent was randomly assigned to make binary choices between the contract and a set of unconditional payments knowing that the decision to accept the contract would remain private while the financial incentives offered would be publicized among colleagues at the taxi station by the field officer. *Priv. Menu / Priv. Incentives* is an indicator function of whether the respondent was randomly assigned to make decisions knowing that their preference for the contract over the unconditional payment and the contract terms were kept private. The omitted category is the *Unconditional Cash Transfer* of Ugx 4,500 (UCT), calibrated based on the average realized payment under private contract in the pilot sample. I report the p-value for the difference between *Priv. Menu / Vis. Incentives* and *Priv. Menu / Priv. Incentives*. The coefficient of interest is the interaction of *Priv. Menu / Vis. Incentives* variable with *Value justification*, which capture the differential effect of the disclosure of the financial incentives when people value the contract as social justification. Robust standard errors clustered at the respondent level are in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

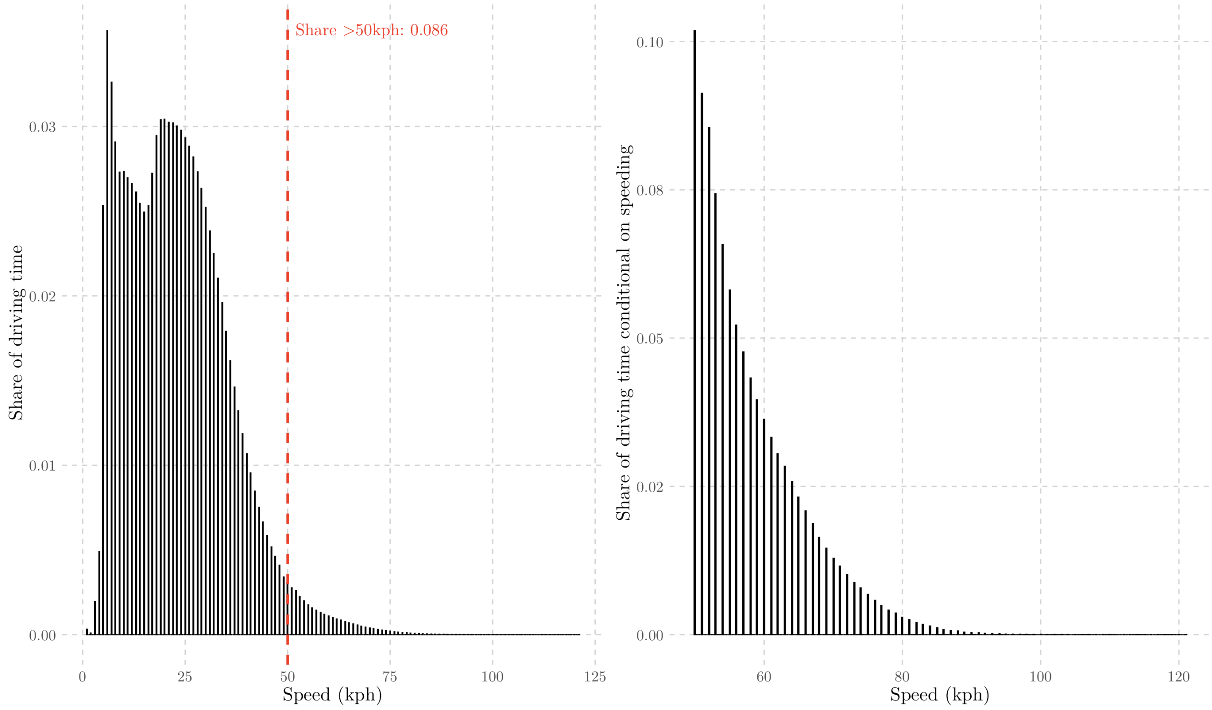
Table 9: Impact Experiment - Heterogeneity by Demand for Justification on Labor Market Outcomes

	(1) Hours Worked	(2) Revenues	(3) Marginal Costs	(4) Disposable Income	(5) Productivity
Priv. Menu / Vis. Incentives	-0.49	-263	-2302**	2046	353*
× Value justification	[0.49]	[2326]	[916]	[1703]	[184]
Priv. Menu / Priv. Incentives	-0.043	920	-1233	2302	327*
× Value justification	[0.44]	[2146]	[869]	[1596]	[182]
Value justification	-0.063	-3730	1528	-2365	199
	[0.56]	[2971]	[1068]	[2319]	[275]
Priv. Menu / Vis. Incentives	0.32	2084	238	2185	120
	[0.70]	[3524]	[1287]	[2735]	[317]
Priv. Menu / Priv. Incentives	0.013	-239	-782	658	60
	[0.63]	[3215]	[1185]	[2512]	[304]
Observations	3,525	3,525	3,525	3,525	3,141
Control Mean	8.12	30,310	14,270	16,039	2,083
p-value Priv. vs Vis. Incent.	0.324	0.712	0.003	0.432	0.905

Notes: The table summarizes the heterogeneous treatment effect of the disclosure conditions of the contract by drivers' demand for a justification on the driving outcomes. I capture the demand for justification with a dummy variable called *Value justification* that indicates whether a driver has a higher reservation price for the contract when the contract choice is private relative to when the choice is disclosed to colleagues. I report ANCOVA regression estimates on treatment group indicators, outcome values at baseline, and strata fixed effects. The unit of observation is the driver x day (N=3,525). Monetary outcomes are reported in Ugandan Shilling. Column 1 reports the labor supply measured in hours; Column 2 reports daily revenues; Column 3 reports daily marginal costs (i.e., fuel and daily expenses); Column 4 reports the disposable income as reported by the respondent; Column 5 reports the treatment effects on productivity measured as net revenues per hour worked (N=3,141). *Priv. Menu / Vis. Incentives* variable is an indicator function of whether the respondent was randomly assigned to make binary choices between the contract and a set of unconditional payments knowing that the decision to accept the contract would remain private while the financial incentives offered would be publicized among colleagues at the taxi station by the field officer. *Priv. Menu / Priv. Incentives* is an indicator function of whether the respondent was randomly assigned to make decisions knowing that their preference for the contract over the unconditional payment and the contract terms were kept private. The omitted category is the *Unconditional Cash Transfer* of Ugx 4,500 (UCT), calibrated based on the average realized payment under private contract in the pilot sample. I report the p-value for the difference between *Priv. Menu / Vis. Incentives* and *Priv. Menu / Priv. Incentives*. The coefficient of interest is the interaction of *Priv. Menu / Vis. Incentives* variable with *Value justification*, which capture the differential effect of the disclosure of the financial incentives when people value the contract as social justification. Robust standard errors clustered at the respondent level are in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

A Descriptive Evidence: Tables and Figures

Figure A1: Distribution of Speed at Baseline



Notes: This figure illustrates the speed distribution of study participants (N = 360) for a period of one month before the implementation of the Demand Experiment. I refer to this period as the "lead-in" period during which drivers had the opportunity to familiarize themselves with GPS technology. GPS captures speed information every 2 seconds when the driver is in movement, summing up more than 50 million observations. The graph on the left shows the distribution of the speed at any recorded speed. I winsorize speed at the top 1%. The x-axis reports the speed in kilometers per hour (kph). The y-axis reports the fraction of average driving time per driver spent at a given speed. The vertical red dashed line indicates the speed at 50 kph and the share of driving time spent over it. The right-hand graph zooms in on the speed distribution conditional on exceeding 50 kph

B Summary Statistics and Balance Tables

Table B1: Descriptive Statistics

	Mean	Standard Deviation	Minimum	Median	Maximum	Sample Size
Panel A: Demographics						

Table B1 continued from previous page

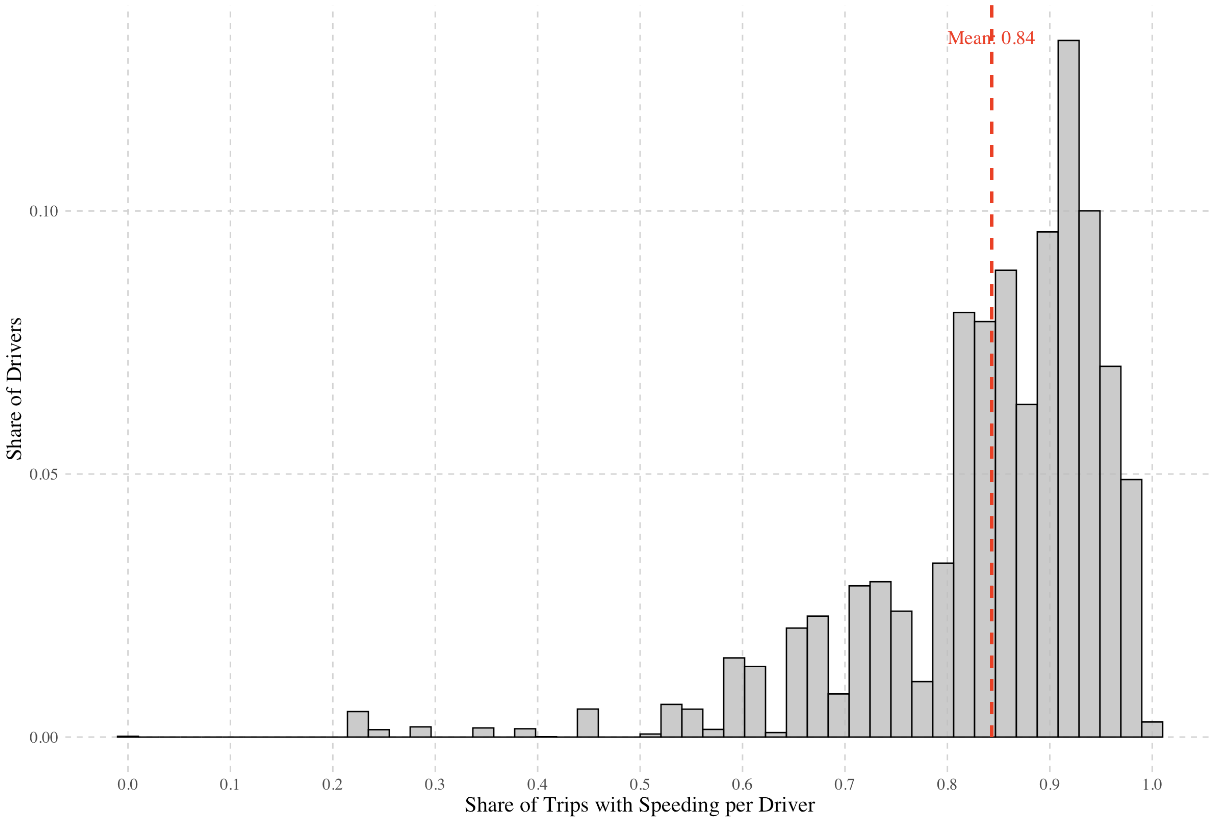
	Mean	Standard Deviation	Minimum	Median	Maximum	Sample Size
male	1.00	0.00	1.00	1.00	1.00	360
age	32.73	7.25	20.00	32.00	60.00	360
any child	0.91	0.28	0.00	1.00	1.00	360
own smartphone	0.84	0.36	0.00	1.00	1.00	360
no educ	0.02	0.13	0.00	0.00	1.00	360
prim. educ	0.40	0.49	0.00	0.00	1.00	360
sec. educ	0.50	0.50	0.00	0.50	1.00	360
tert. educ	0.08	0.28	0.00	0.00	1.00	360
multiple income	0.32	0.47	0.00	0.00	1.00	360
savings	27.25	22.02	0.00	26.75	240.77	360
road adren	0.40	0.49	0.00	0.00	1.00	360
risk pref	3.88	2.40	0.00	4.00	10.00	360
risk pref drive	3.52	2.52	0.00	3.00	10.00	360
sophistication	0.70	2.28	-8.00	0.00	10.00	360
Panel B: Labor Supply						
work days	6.30	0.70	3.00	6.00	7.00	360
hours worked	11.45	2.31	5.00	12.00	19.00	360
revenues	9.88	3.04	2.68	9.36	21.40	360
fuel costs	4.10	1.27	0.40	4.55	6.15	360
disp. income	5.78	3.10	-2.68	5.35	16.85	360
productivity	0.89	0.32	0.32	0.85	3.21	360
clients per day	16.77	7.44	3.00	15.00	50.00	360
share stage clients	0.42	0.20	0.00	0.40	0.90	360
share street clients	0.58	0.20	0.10	0.60	1.00	360
recur. cust.	0.41	0.22	0.00	0.40	1.00	360
recur. cust wish	0.80	0.27	0.00	1.00	1.00	360
new clients	0.41	0.22	0.00	0.40	1.00	360
job satisf.	2.59	0.79	1.00	3.00	4.00	360
stress	2.22	0.76	1.00	2.00	4.00	360
Panel C: Motortaxi Stations						
years as driver	6.41	4.41	0.00	5.00	31.00	360
years in stage	4.79	4.61	0.00	3.00	31.00	360
stage size	22.68	10.59	2.00	20.00	53.00	360
stage fee	115.52	61.24	0.00	107.01	214.02	360
pressure to earn	0.08	0.28	0.00	0.00	1.00	360
pressure to speed	2.73	0.97	1.00	3.00	4.00	360
popularity	3.50	0.72	1.00	4.00	4.00	360
stage friends	0.61	0.33	0.00	0.75	1.14	360
Panel D: Driving Behavior						
any overspeed	0.80	0.25	0.00	0.89	1.00	357

Table B1 continued from previous page

	Mean	Standard Deviation	Minimum	Median	Maximum	Sample Size
avg speed	21.67	3.80	4.75	22.17	31.05	357
avg max speed	57.95	10.82	7.00	58.84	83.97	357
avg num trips	26.25	13.36	0.00	26.91	62.93	357
avg km trip	3.42	1.97	0.00	3.20	20.15	346
fines	0.82	0.39	0.00	1.00	1.00	360
driving ability	3.36	0.81	1.00	4.00	4.00	360
under 50kph	6.47	2.53	0.00	6.00	10.00	360
under 50kph goal	7.18	2.72	0.00	8.00	10.00	360
speed test	48.34	3.30	40.00	48.00	59.00	360
Panel E: Assets						
bike owned	0.55	0.50	0.00	1.00	1.00	360
bike on loan	0.45	0.50	0.00	0.00	1.00	360
own helmet	0.02	0.13	0.00	0.00	1.00	360
use helmet	0.65	0.48	0.00	1.00	1.00	360
odometer	0.60	0.49	0.00	1.00	1.00	360
bike cond.	3.08	0.62	1.00	3.00	4.00	360
service costs	0.49	0.35	0.00	0.41	2.85	360
repair costs	0.89	0.69	0.00	0.86	2.68	360
wish to rent	0.60	0.49	0.00	1.00	1.00	360

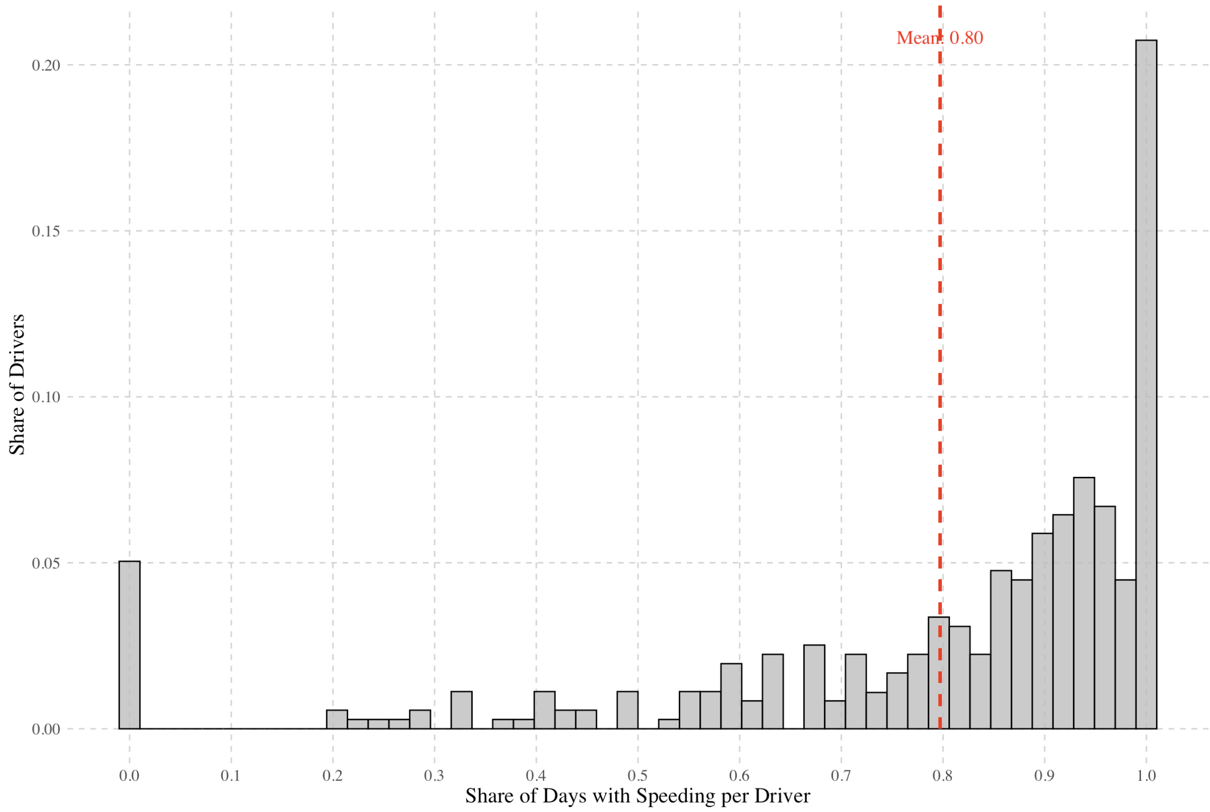
Note: The table reports the summary statistics of the study sample. Monetary values are expressed in Usd. *Panel A* illustrates a set of sociodemographic characteristics. The savings variable refers to monthly savings. The sophistication measure is adapted from Carrera et al. (2022) and is measured as the standardized gap between the goal and the predicted attendance. *Panel B* reports characteristics of the labor supply. Job satisfaction and stress measure are self-reported on a scale of 1 to 4. Productivity is calculated as revenues per hour worked. In *Panel C*, I report statistics for organized groups, taxi stations. The pressure to earn is a binary indicator equal to one when respondents report feeling pressured by peers. The pressure-to-speed variable is a 1-4 self-reported measure of how pressure to drive fast the respondent feels vis-à-vis peers. *Panel D* describes the statistics on driving behavior. Speed information comes from GPS data. "Under 50kph" measures out of 10 days, the respondent expects to comply with the 50kph speed limit. The same logic holds for "Under 50kph goal" that measures the percentage of days drivers wish to comply with speeding regulation. Finally, *Panel E* reports statistics on reversal and assets related to taxi drivers.

Figure A2: Distribution of Speeding over 50 kph Aggregated by Trip



Notes: This figure reports the distribution of the share of trips by study participants (N = 360) in which at least one speeding violation is recorded. I define a speeding violation as a speed exceeding 50 kph in the metropolitan area of Kampala, excluding the main roads where the speed limit regulation is explicitly set higher than 50 kph. Data refer to a period of one month before the implementation of the Demand Experiment. I refer to this period as the "lead-in" period during which drivers had the opportunity to familiarize themselves with GPS technology. I identify a total of 220,000 trips, regardless of the presence of a passenger, in the GPS data. On the x-axis, I plot the share of trips for which at least one speeding violation occurred. The vertical red dashed line reports the average share of trips with at least one speeding violation. The y-axis reports the share of drivers.

Figure A3: Distribution of Speeding over 50 kph Aggregated by Day



Notes: This figure reports the distribution of the share of working days in which at least one speeding violation is recorded by study participants (N = 360). I define a speeding violation as a speed exceeding 50 kph in the metropolitan area of Kampala, excluding the main roads where the speed limit regulation is explicitly set higher than 50 kph. The data refer to a period of one month prior to the implementation of the Demand Experiment. I refer to this period as the "lead-in period" during which drivers had the opportunity to familiarize themselves with GPS technology. I observe a total of 8748 working days. A working day is defined as a day where one of the following conditions is met: the driver reached the taxi station at least once, or the driver spent more than an hour in movement. On the x-axis, I plot the share of trips for which at least one speeding violation occurred. The vertical red dashed line reports the average share of working days with at least one speeding violation. The y-axis reports the fraction of drivers in the study sample.

Table B2: Balance Table - *Demand Experiment*

	Pri Choice Pri Offer (0,0)	Pri Choice Pub Offer (0,1)	Vis. Menu Pub Offer (1,1)	(0,0)-(0,1)	(0,0)-(1,1)	(0,1)-(1,1)	N
Panel A: Socio-demographics							
male	1.000	1.000	1.000	0.000	0.00	0.00	360
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	
age	33.088	32.817	31.636	0.290	1.60	1.26	360
	(6.98)	(7.63)	(6.59)	(0.84)	(1.09)	(1.08)	
any child	0.936	0.900	0.909	0.030	0.03	-0.01	360
	(0.25)	(0.30)	(0.29)	(0.03)	(0.05)	(0.05)	
own smartphone	0.872	0.833	0.818	0.040	0.06	0.01	360
	(0.34)	(0.37)	(0.39)	(0.04)	(0.06)	(0.06)	
no educ	0.008	0.028	0.000	-0.020	0.01	0.03	360
	(0.09)	(0.16)	(0.00)	(0.01)	(0.01)	(0.02)**	
prim. educ	0.376	0.400	0.455	-0.010	-0.05	-0.04	360
	(0.49)	(0.49)	(0.50)	(0.06)	(0.08)	(0.08)	
sec. educ	0.528	0.506	0.418	0.010	0.11	0.06	360
	(0.50)	(0.50)	(0.50)	(0.06)	(0.09)	(0.08)	
tert. educ	0.088	0.067	0.127	0.020	-0.06	-0.06	360
	(0.28)	(0.25)	(0.34)	(0.03)	(0.05)	(0.05)	
multiple income	0.304	0.306	0.382	0.000	-0.06	-0.06	360
	(0.46)	(0.46)	(0.49)	(0.05)	(0.08)	(0.08)	
savings	27.946	27.977	23.295	-0.210	4.31	4.53	360
	(21.52)	(23.84)	(16.11)	(2.66)	(3.43)	(2.93)	
road adren	0.360	0.444	0.345	-0.080	0.03	0.08	360
	(0.48)	(0.50)	(0.48)	(0.06)	(0.08)	(0.08)	
risk pref	4.072	3.928	3.273	0.120	0.77	0.81	360
	(2.48)	(2.38)	(2.19)	(0.29)	(0.36)**	(0.38)**	
risk pref drive	3.712	3.606	2.782	0.130	1.09	0.89	360
	(2.49)	(2.56)	(2.39)	(0.30)	(0.40)***	(0.38)**	
sophistication	0.696	0.872	0.164	-0.190	0.49	0.88	360
	(2.20)	(2.25)	(2.52)	(0.27)	(0.38)	(0.36)**	
Panel B: Labor Supply							
work days	6.296	6.261	6.455	0.040	-0.15	-0.20	360
	(0.66)	(0.74)	(0.63)	(0.08)	(0.12)	(0.11)*	
hours worked	11.320	11.572	11.364	-0.170	-0.13	0.17	360
	(2.45)	(2.18)	(2.39)	(0.27)	(0.43)	(0.38)	
revenues	9.825	10.022	9.560	-0.070	0.16	0.13	360
	(3.01)	(3.12)	(2.87)	(0.26)	(0.36)	(0.35)	
fuel costs	4.206	4.066	3.986	0.120	0.37	0.13	360
	(1.10)	(1.32)	(1.48)	(0.14)	(0.23)	(0.21)	
disp. income	5.618	5.955	5.572	-0.190	-0.22	-0.01	360
	(3.01)	(3.20)	(3.01)	(0.25)	(0.33)	(0.31)	
productivity	0.893	0.895	0.884	0.000	0.01	-0.02	360
	(0.28)	(0.33)	(0.40)	(0.03)	(0.06)	(0.06)	
clients per day	16.536	16.683	17.582	0.030	-1.13	-1.14	360
	(7.55)	(7.14)	(8.21)	(0.85)	(1.37)	(1.20)	

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Table B2 Continued from previous page

	Pri Choice Pri Offer (0,0)	Pri Choice Pub Offer (0,1)	Vis. Menu Pub Offer (1,1)	(0,0)-(0,1)	(0,0)-(1,1)	(0,1)-(1,1)	N
share stage clients	0.380	0.447	0.429	-0.070	-0.04	0.02	360
	(0.21)	(0.19)	(0.20)	(0.02)***	(0.03)	(0.03)	
share street clients	0.620	0.553	0.571	0.070	0.04	-0.02	360
	(0.21)	(0.19)	(0.20)	(0.02)***	(0.03)	(0.03)	
recur. cust.	0.393	0.412	0.424	-0.020	-0.01	0.01	360
	(0.21)	(0.22)	(0.24)	(0.02)	(0.02)	(0.02)	
recur. cust wish	0.807	0.775	0.882	0.030	-0.07	-0.09	360
	(0.27)	(0.28)	(0.22)	(0.03)	(0.04)*	(0.04)**	
new clients	0.393	0.412	0.424	-0.020	-0.01	0.01	360
	(0.21)	(0.22)	(0.24)	(0.02)	(0.02)	(0.02)	
job satisf.	2.584	2.611	2.509	-0.020	0.09	0.08	360
	(0.82)	(0.77)	(0.77)	(0.09)	(0.14)	(0.12)	
stress	2.256	2.211	2.182	0.020	0.13	0.05	360
	(0.69)	(0.78)	(0.84)	(0.09)	(0.14)	(0.14)	
Panel C: Motortaxi Station							
years as driver	6.236	6.590	6.224	-0.380	0.09	0.63	360
	(4.17)	(4.72)	(3.93)	(0.53)	(0.69)	(0.70)	
years in stage	4.528	5.172	4.109	-0.650	0.24	1.11	360
	(4.06)	(5.17)	(3.69)	(0.56)	(0.62)	(0.63)*	
stage size	23.614	22.510	21.125	1.130	2.01	1.91	360
	(11.25)	(9.91)	(11.20)	(1.27)	(1.95)	(1.67)	
stage fee	108.764	120.504	114.549	-11.550	-6.85	4.70	360
	(58.61)	(64.61)	(54.96)	(7.15)	(9.78)	(9.33)	
pressure to earn	0.104	0.067	0.091	0.040	0.02	-0.03	360
	(0.31)	(0.25)	(0.29)	(0.03)	(0.05)	(0.05)	
pressure to speed	2.760	2.750	2.618	0.010	0.19	0.13	360
	(0.95)	(0.97)	(1.03)	(0.11)	(0.18)	(0.17)	
popularity	3.480	3.533	3.418	-0.070	0.07	0.13	360
	(0.78)	(0.69)	(0.71)	(0.09)	(0.13)	(0.11)	
stage friends	0.600	0.641	0.511	-0.050	0.08	0.10	360
	(0.36)	(0.31)	(0.33)	(0.04)	(0.06)	(0.05)*	
Panel D: Driving Behavior							
any overspeed	0.805	0.789	0.807	0.020	0.02	-0.01	357
	(0.26)	(0.26)	(0.24)	(0.02)	(0.03)	(0.03)	
avg speed	21.608	21.749	21.538	-0.180	0.70	0.63	357
	(4.17)	(3.68)	(3.31)	(0.43)	(0.53)	(0.50)	
avg max speed	58.366	57.692	57.873	0.640	1.49	0.79	357
	(11.56)	(11.07)	(8.00)	(1.12)	(1.19)	(1.08)	
avg num trips	25.698	26.268	27.466	-0.480	-1.43	-0.70	357
	(13.72)	(13.66)	(11.60)	(1.41)	(1.82)	(1.72)	
avg km per trip	3.628	3.323	3.241	0.310	0.55	0.19	346
	(2.08)	(2.04)	(1.38)	(0.24)	(0.26)**	(0.23)	
fines	0.824	0.789	0.891	0.040	-0.08	-0.09	360
	(0.38)	(0.41)	(0.31)	(0.05)	(0.07)	(0.06)	
driving ability	3.488	3.311	3.200	0.170	0.27	0.15	360

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Table B2 Continued from previous page

	Pri Choice Pri Offer (0,0)	Pri Choice Pub Offer (0,1)	Vis. Menu Pub Offer (1,1)	(0,0)-(0,1)	(0,0)-(1,1)	(0,1)-(1,1)	N
	(0.74)	(0.77)	(1.03)	(0.09)*	(0.16)*	(0.15)	
under 50kph	6.672	6.211	6.891	0.440	-0.24	-0.71	360
	(2.47)	(2.58)	(2.45)	(0.30)	(0.43)	(0.43)*	
under 50kph goal	7.368	7.083	7.055	0.260	0.25	0.18	360
	(2.57)	(2.75)	(2.97)	(0.31)	(0.49)	(0.48)	
speed test	48.358	48.412	48.042	-0.110	0.36	0.40	360
	(3.30)	(3.26)	(3.50)	(0.38)	(0.60)	(0.55)	
Panel E: Assets							
bike owned	0.560	0.550	0.509	0.020	0.03	0.02	360
	(0.50)	(0.50)	(0.50)	(0.06)	(0.08)	(0.08)	
bike on loan	0.440	0.450	0.491	-0.020	-0.03	-0.02	360
	(0.50)	(0.50)	(0.50)	(0.06)	(0.08)	(0.08)	
own helmet	0.008	0.022	0.018	-0.020	0.00	0.00	360
	(0.09)	(0.15)	(0.13)	(0.01)	(0.02)	(0.02)	
use helmet	0.640	0.650	0.673	-0.010	-0.01	0.03	360
	(0.48)	(0.48)	(0.47)	(0.06)	(0.08)	(0.07)	
odometer	0.632	0.567	0.618	0.070	-0.03	-0.07	360
	(0.48)	(0.50)	(0.49)	(0.06)	(0.08)	(0.07)	
bike cond.	3.064	3.106	3.018	-0.050	0.05	0.08	360
	(0.61)	(0.60)	(0.73)	(0.07)	(0.11)	(0.10)	
service costs	0.507	0.485	0.493	0.020	0.02	0.01	360
	(0.35)	(0.35)	(0.33)	(0.04)	(0.06)	(0.05)	
repair costs	0.909	0.891	0.843	-0.010	0.13	0.05	360
	(0.75)	(0.66)	(0.67)	(0.08)	(0.11)	(0.10)	
wish to rent	0.632	0.583	0.582	0.060	0.04	-0.01	360
	(0.48)	(0.49)	(0.50)	(0.06)	(0.08)	(0.08)	

Note: The table presents a comparison of baseline characteristics of respondents by treatment condition for Experiment 1, the *Demand experiment*. Variables reported are organized in five panels as in Table B1. The *Differences* columns are generated by a regression of each outcome on a treatment dummy with robust standard errors. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The *Differences* reports all pair-wise comparison across treatment conditions. Tha last column reports the sample size of the regression used to run the difference-in-means tests.

Table B3: Balance Table - *Impact Experiment*

	Public Offer [Pub]	Private Offer [Pri]	Control [C]	Differences [Pub-Pri]	[Pub-C]	[Pub-C]	N
Panel A: Socio-demographics							
male	1.000	1.000	1.000	0.000	0.00	0.00	360
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	
age	33.088	32.817	31.636	0.290	1.60	1.26	360
	(6.98)	(7.63)	(6.59)	(0.84)	(1.09)	(1.08)	

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Table B3 Continued from previous page

	Public Offer [Pub]	Private Offer [Pri]	Control [C]	Differences [Pub-Pri]	[Pub-C]	[Pub-C]	N
any child	0.936	0.900	0.909	0.030	0.03	-0.01	360
	(0.25)	(0.30)	(0.29)	(0.03)	(0.05)	(0.05)	
own smartphone	0.872	0.833	0.818	0.040	0.06	0.01	360
	(0.34)	(0.37)	(0.39)	(0.04)	(0.06)	(0.06)	
no educ	0.008	0.028	0.000	-0.020	0.01	0.03	360
	(0.09)	(0.16)	(0.00)	(0.01)	(0.01)	(0.02)**	
prim. educ	0.376	0.400	0.455	-0.010	-0.05	-0.04	360
	(0.49)	(0.49)	(0.50)	(0.06)	(0.08)	(0.08)	
sec. educ	0.528	0.506	0.418	0.010	0.11	0.06	360
	(0.50)	(0.50)	(0.50)	(0.06)	(0.09)	(0.08)	
tert. educ	0.088	0.067	0.127	0.020	-0.06	-0.06	360
	(0.28)	(0.25)	(0.34)	(0.03)	(0.05)	(0.05)	
multiple income	0.304	0.306	0.382	0.000	-0.06	-0.06	360
	(0.46)	(0.46)	(0.49)	(0.05)	(0.08)	(0.08)	
savings	27.946	27.977	23.295	-0.210	4.31	4.53	360
	(21.52)	(23.84)	(16.11)	(2.66)	(3.43)	(2.93)	
road adren	0.360	0.444	0.345	-0.080	0.03	0.08	360
	(0.48)	(0.50)	(0.48)	(0.06)	(0.08)	(0.08)	
risk pref	4.072	3.928	3.273	0.120	0.77	0.81	360
	(2.48)	(2.38)	(2.19)	(0.29)	(0.36)**	(0.38)**	
risk pref drive	3.712	3.606	2.782	0.130	1.09	0.89	360
	(2.49)	(2.56)	(2.39)	(0.30)	(0.40)***	(0.38)**	
sophistication	0.696	0.872	0.164	-0.190	0.49	0.88	360
	(2.20)	(2.25)	(2.52)	(0.27)	(0.38)	(0.36)**	
Panel B: Labor Supply							
work days	6.296	6.261	6.455	0.040	-0.15	-0.20	360
	(0.66)	(0.74)	(0.63)	(0.08)	(0.12)	(0.11)*	
hours worked	11.320	11.572	11.364	-0.170	-0.13	0.17	360
	(2.45)	(2.18)	(2.39)	(0.27)	(0.43)	(0.38)	
revenues	9.825	10.022	9.560	-0.070	0.16	0.13	360
	(3.01)	(3.12)	(2.87)	(0.26)	(0.36)	(0.35)	
fuel costs	4.206	4.066	3.986	0.120	0.37	0.13	360
	(1.10)	(1.32)	(1.48)	(0.14)	(0.23)	(0.21)	
disp. income	5.618	5.955	5.572	-0.190	-0.22	-0.01	360
	(3.01)	(3.20)	(3.01)	(0.25)	(0.33)	(0.31)	
productivity	0.893	0.895	0.884	0.000	0.01	-0.02	360
	(0.28)	(0.33)	(0.40)	(0.03)	(0.06)	(0.06)	
clients per day	16.536	16.683	17.582	0.030	-1.13	-1.14	360
	(7.55)	(7.14)	(8.21)	(0.85)	(1.37)	(1.20)	
share satge clients	0.380	0.447	0.429	-0.070	-0.04	0.02	360
	(0.21)	(0.19)	(0.20)	(0.02)***	(0.03)	(0.03)	
share street clients	0.620	0.553	0.571	0.070	0.04	-0.02	360
	(0.21)	(0.19)	(0.20)	(0.02)***	(0.03)	(0.03)	
recur. cust.	0.393	0.412	0.424	-0.020	-0.01	0.01	360
	(0.21)	(0.22)	(0.24)	(0.02)	(0.02)	(0.02)	
recur. cust wish	0.807	0.775	0.882	0.030	-0.07	-0.09	360
	(0.27)	(0.28)	(0.22)	(0.03)	(0.04)*	(0.04)**	

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Table B3 Continued from previous page

	Public Offer [Pub]	Private Offer [Pri]	Control [C]	Differences [Pub-Pri]	[Pub-C]	[Pub-C]	N
new clients	0.393	0.412	0.424	-0.020	-0.01	0.01	360
	(0.21)	(0.22)	(0.24)	(0.02)	(0.02)	(0.02)	
job satisf.	2.584	2.611	2.509	-0.020	0.09	0.08	360
	(0.82)	(0.77)	(0.77)	(0.09)	(0.14)	(0.12)	
stress	2.256	2.211	2.182	0.020	0.13	0.05	360
	(0.69)	(0.78)	(0.84)	(0.09)	(0.14)	(0.14)	
Panel C: Motortaxi Station							
years as driver	6.236	6.590	6.224	-0.380	0.09	0.63	360
	(4.17)	(4.72)	(3.93)	(0.53)	(0.69)	(0.70)	
years in stage	4.528	5.172	4.109	-0.650	0.24	1.11	360
	(4.06)	(5.17)	(3.69)	(0.56)	(0.62)	(0.63)*	
stage size	23.614	22.510	21.125	1.130	2.01	1.91	360
	(11.25)	(9.91)	(11.20)	(1.27)	(1.95)	(1.67)	
stage fee	108.764	120.504	114.549	-11.550	-6.85	4.70	360
	(58.61)	(64.61)	(54.96)	(7.15)	(9.78)	(9.33)	
pressure to earn	0.104	0.067	0.091	0.040	0.02	-0.03	360
	(0.31)	(0.25)	(0.29)	(0.03)	(0.05)	(0.05)	
pressure to speed	2.760	2.750	2.618	0.010	0.19	0.13	360
	(0.95)	(0.97)	(1.03)	(0.11)	(0.18)	(0.17)	
popularity	3.480	3.533	3.418	-0.070	0.07	0.13	360
	(0.78)	(0.69)	(0.71)	(0.09)	(0.13)	(0.11)	
stage friends	0.600	0.641	0.511	-0.050	0.08	0.10	360
	(0.36)	(0.31)	(0.33)	(0.04)	(0.06)	(0.05)*	
Panel D: Driving Behavior							
any overspeed	0.805	0.789	0.807	0.020	0.02	-0.01	357
	(0.26)	(0.26)	(0.24)	(0.02)	(0.03)	(0.03)	
avg speed	21.608	21.749	21.538	-0.180	0.70	0.63	357
	(4.17)	(3.68)	(3.31)	(0.43)	(0.53)	(0.50)	
avg max speed	58.366	57.692	57.873	0.640	1.49	0.79	357
	(11.56)	(11.07)	(8.00)	(1.12)	(1.19)	(1.08)	
avg num trips	25.698	26.268	27.466	-0.480	-1.43	-0.70	357
	(13.72)	(13.66)	(11.60)	(1.41)	(1.82)	(1.72)	
avg km trip	3.628	3.323	3.241	0.310	0.55	0.19	357
	(2.08)	(2.04)	(1.38)	(0.24)	(0.26)**	(0.23)	
fines	0.824	0.789	0.891	0.040	-0.08	-0.09	360
	(0.38)	(0.41)	(0.31)	(0.05)	(0.07)	(0.06)	
driving ability	3.488	3.311	3.200	0.170	0.27	0.15	360
	(0.74)	(0.77)	(1.03)	(0.09)*	(0.16)*	(0.15)	
under 50kph	6.672	6.211	6.891	0.440	-0.24	-0.71	360
	(2.47)	(2.58)	(2.45)	(0.30)	(0.43)	(0.43)*	
under 50kph goal	7.368	7.083	7.055	0.260	0.25	0.18	360
	(2.57)	(2.75)	(2.97)	(0.31)	(0.49)	(0.48)	
speed test	48.358	48.412	48.042	-0.110	0.36	0.40	360
	(3.30)	(3.26)	(3.50)	(0.38)	(0.60)	(0.55)	

Continued on next page

Table B3 Continued from previous page

	Public Offer [Pub]	Private Offer [Pri]	Control [C]	Differences [Pub-Pri]	[Pub-C]	[Pub-C]	N
Panel E: Assets							
bike owned	0.560	0.550	0.509	0.020	0.03	0.02	360
	(0.50)	(0.50)	(0.50)	(0.06)	(0.08)	(0.08)	
bike on loan	0.440	0.450	0.491	-0.020	-0.03	-0.02	360
	(0.50)	(0.50)	(0.50)	(0.06)	(0.08)	(0.08)	
own helmet	0.008	0.022	0.018	-0.020	0.00	0.00	360
	(0.09)	(0.15)	(0.13)	(0.01)	(0.02)	(0.02)	
use helmet	0.640	0.650	0.673	-0.010	-0.01	0.03	360
	(0.48)	(0.48)	(0.47)	(0.06)	(0.08)	(0.07)	
odometer	0.632	0.567	0.618	0.070	-0.03	-0.07	360
	(0.48)	(0.50)	(0.49)	(0.06)	(0.08)	(0.07)	
bike cond.	3.064	3.106	3.018	-0.050	0.05	0.08	360
	(0.61)	(0.60)	(0.73)	(0.07)	(0.11)	(0.10)	
service costs	0.507	0.485	0.493	0.020	0.02	0.01	360
	(0.35)	(0.35)	(0.33)	(0.04)	(0.06)	(0.05)	
repair costs	0.909	0.891	0.843	-0.010	0.13	0.05	360
	(0.75)	(0.66)	(0.67)	(0.08)	(0.11)	(0.10)	
wish to rent	0.632	0.583	0.582	0.060	0.04	-0.01	360
	(0.48)	(0.49)	(0.50)	(0.06)	(0.08)	(0.08)	

Note: The table presents a comparison of baseline characteristics of respondents by treatment condition for the *Impact Experiment*. Variables reported are organized in five panels as in Table B1. The *Differences* columns are generated by a regression of each outcome on a treatment dummy with robust standard errors. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The *Differences* reports all pair-wise comparison across treatment conditions. Tha last column reports the sample size of the regression used to run the difference-in-means tests.

C Elicitation Procedure for Customers' Preferences

I elicit the willingness to pay for the customer's upcoming ride with a driver from our pilot sample. The customer is informed that the driver's speed has been monitored for the past week. The customer is then asked to make a series of choices between a cash payment or a free ride with our driver. The cash payment varies between choices and ranges between Ugx 1,000 and Ugx 5,000 (Ugx 500 intervals); the choices are asked to be made for two hypothetical scenarios, administered in random order. In one scenario, the driver exceeded a speed of 50 kph less than half of the trips made in the last seven working days; in the other scenario, the driver exceeded a speed of 50 kph more than half of the trips. Typically, the price bargaining occurs at the end of the trip, making it possible to reflect the quality of the ride.

The customer is then informed that a) one of the two scenarios is true and that it will be revealed after the choices are made; b) one of the choices made under the true scenario will be picked at random; and finally c) whatever the customer chooses under the selected scenario is offered. The revealed willingness to pay for the ride under the two scenarios shows that 73% of the customers are willing to pay a premium for the driver with a lower speeding rate, while 12% are willing to pay more for the driver with a higher speeding rate, and 15% are indifferent. The average premium for slow driving corresponds to 8% of the cost of the ride.

D Belief Experiment: Design Details

I conducted a belief experiment during an in-person visit to the office as part of the baseline survey to test (i) whether fast driving is perceived as a salient signal of social status at the workplace compared to labor market outcomes and (ii) whether fast drivers are perceived as more skilled or higher earners.

In the experiment, I ask the drivers to evaluate the profile of four other drivers, based on the incentive resume rating exercise developed by Kessler et al. (2019). Each profile contains the following information: age, years of working experience as a motor-taxi driver, location of current work station, days with recorded speeding violations within 7 days. Speeding violation is defined as driving more than 50 kph in the urban area of Kampala. A day is defined as a speeding day when at least one speeding violation is recorded. Speed is recorded by GPS technology also installed on the respondents' motorbikes.

At baseline, I ask the respondent to rate whether a driver is perceived as slow, medium speed, or fast based on the number of days out of 7 working days in which they commit at least one speeding violation. Speeding violation is defined as exceeding speed limits of 50 kph in urban areas and 90 kph on main national roads in the metropolitan area. I classify as "slow" when days with speeding are 1, 2 or 3; medium if 4, 5; fast if 6 or 7. This classification is based on the distribution of perceived speeding behavior according to the drivers in the list.

Each respondent is shown four profiles, randomly selected from a list of 45. The list is the result of a random manipulation of the speeding behavior of 15 real drivers. For each driver, I create 3 types: a slow, medium and fast version of it where one is the real profile and two are manipulated versions. The classification of speeding types follows the categories reported above. Each profile shown to the respondent includes information on age, years of work experience as drivers, location of the current work station, and speeding behavior. Respondents receive extensive explanations about the technology used to trace speeding behavior, which is also offered to them in the framework of the same project. Respondents have also already gained experience with the technology prior to being exposed to this survey experiment: the goal is to establish trust in the technology and make the information shown to the driver to be perceived as reliable. The 15 real profiles have been exposed the following during the pilot phase: ability S-shaped test, task to get a new driver accepted in his taxi station.

I exploit random variation within profile across respondents to estimate the causal effect of speeding behavior on the perceived social and monetary returns. The order in which the profiles are shown to each respondent is also random. In the regression analysis, I control for ordering, profile fixed effects, and respondent fixed effects.

The set of profiles is constructed as follows. I use 15 real drivers from the pilot of this project. Each driver has been interviewed and offered a GPS tracker that allows them to monitor their driving behavior and locate their bike position the day before. The same technology is also offered to the subjects of this project. I classified their driving behavior of the four weeks of work before the beginning of this project and classified each driver as slow, medium, or fast according to the rating distribution from the listing exercise. For each driver, I then created two manipulated versions corresponding to the two missing speeding types. Conditional on the type, the number of days with speeding violations is assigned randomly.

Respondents are not informed of which of the profiles shown are real. As I have information about the real profiles, I incentivize answers for using them. Beliefs about others' beliefs are also incentivized using the answers from the pilot data.

Each respondent is assigned to view either the slow, medium, or fast version of a given profile. Each respondent sees at least one profile for each type of speed classification, in random order. 1/3 of the sample is assigned to slow, 1/3 to medium and 1/3 to fast driving types.

E Spatial Data: Tracking Devices and Smartphone Application

GPS trackers. The trackers have two main purposes in this study. First, the combination of these three technology components and the high frequency nature of the data allows me to measure a wide range of driving behaviors. In particular, I am interested in building a credible measure of speeding behavior. The information provided by the GPS is used to inform the fulfillment of the speed requirement set in the financial incentives used in the experiment.

The second purpose is to produce reliable information underlying the incentive schemes offered to participants. We develop a smartphone application that allows users to access information about their own driving behavior. The application is paired with the automated message system to reach people without access to smartphones. The goal of the application is two-fold. Firstly, we equalize the access to reliable information that users have about their own driving behavior. Second, this technology allows me to establish the trust of the study participants over the reliability of the driving information provided in the smartphone application and the SMS system. Panel A of Figure E2 shows the interface of the application, while panel B shows the SMS structure. Both technologies provided driving statistics only for the previous day. The rationale behind this is twofold: first, I designed the app to share information that informed the payment schemes provided in the experiment while minimizing the risk of incentivizing phone usage while driving. I saw virtually no log-in to the application while the motorbike was moving. Limiting access to live information implied that any changes in driving behavior induced by the experimental variation could not be explained by a change in the self-monitoring of a participant's own behavior.

To verify that the technology was used effectively, the participants had to open the application at least once in the last seven days of the trial period during the sample selection phase: 1.4% of the sample did not. [41]

The tracking devices feature GPS technology, a gyroscope, and an accelerometer. Data are captured at a 2-second frequency for the study period when the motorcycle is moving and every 30 seconds when it does not.

I use Teltonika FMB920 trackers. Production is based in Ukraine and shipping took more than 3 months due to the pandemic-related microchip shortage. This device is a slim design, easily fitted tracker with GNSS/GPS internal antennas, flash memory, integrated backup battery, accelerometer, and beacon support. This product has been designed to track light commercial vehicles and passenger cars. It is mostly used in insurance telematics, rental and car sharing, recovery of stolen cars, public safety and courier delivery services, taxi, corporate fleets. The model has been designed for light commercial vehicles and passenger cars tracking in insurance telematics, rental and cars sharing, recovery of stolen cars, public safety and courier delivery services, taxis, corporate fleets. Figure E1 illustrates the type of spatial data available.

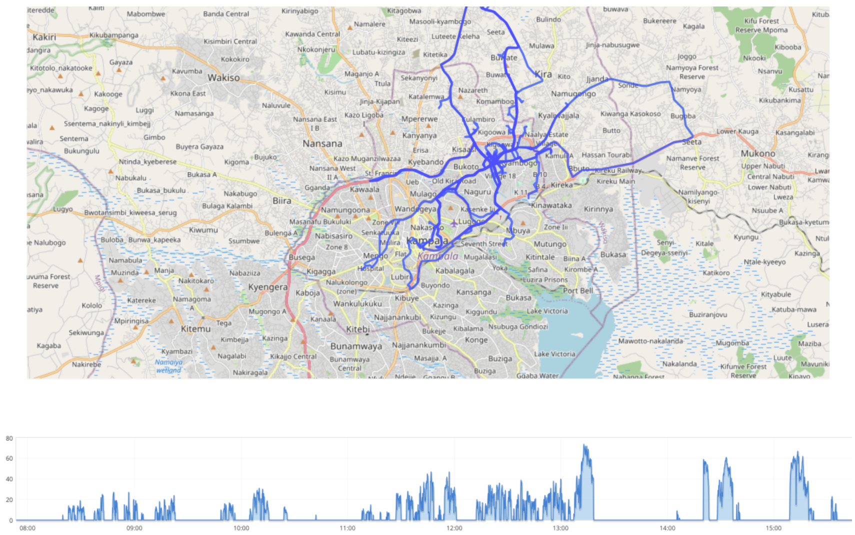
The great advantage of this device is that it can be calibrated remotely. I used a random forest to calibrate the detection of mild accidents and passengers. In particular, I used a fleet of 15 pilot drivers to establish the threshold conditions for tagging an event that corresponded to an unusual condition (as a proxy of accidents) and the presence of customers based on the frequency of vibrations detected by the accelerometer. To improve the precision of the prediction, I am exploring the possibility of making use of the survey data collected during the experiment to increase the size of the training database.

Smartphone Application. Figure E2 shows the main interfaces used to communicate information on driving behavior. Together with the local IT company with which I partnered, we developed an ad-hoc app that provided information about speeding together with kilometers moved, geo-location, and last reported information. Panel A shows the main screen of the app. To avoid prompting the user to access the app while driving, I blocked live information. All data shared refers to the day before. To provide the same information via automated SMS systems as reported in panel B.

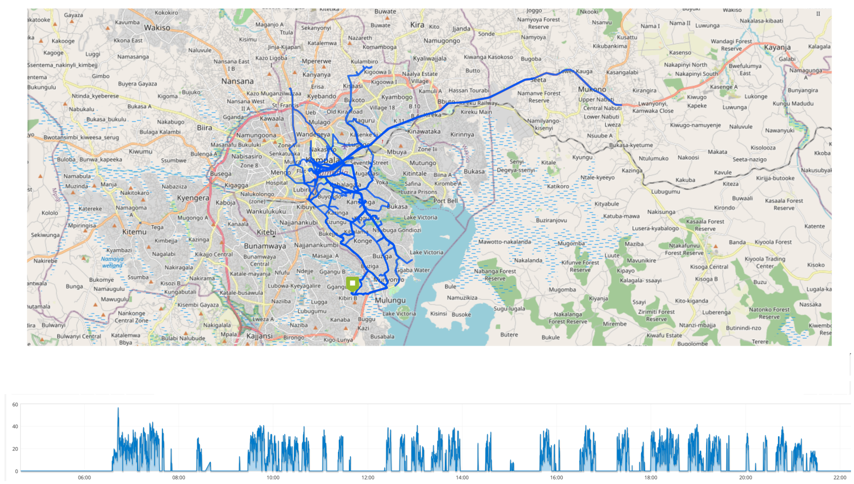
Installation procedure. Tracking devices were installed and tested during the office visit. Since then, high-frequency data on driving behavior were collected throughout the study period. We paired the GPS installation with instructing drivers about the smartphone application and the SMS system. The participants were guided through the installation and usage of the interface by the field officer and went through a real driving test in the office surroundings. In particular, the participants conducted two activities. First, drivers were asked to drive through 4 cones on an eight-shaped path, then ride for about 200 m on a straight and come back. The objective was to reach a maximum speed of 50 kph. Drivers were informed that they could participate in a lottery to win Ugx 5000 (approximately Usd 1.3) if the maximum recorded by the GPS was between 45 and 50 kph. The purpose of this exercise was

[41] I have access to the timestamp of each login into the application. Five respondents did not use the application: three were drivers who had no interest in participating and two additional drivers did not pass a comprehension test. The test consisted of reporting the information provided on the app or by SMS about whether a speeding violation was committed the day before.

Figure E1: Spatial Data: Examples



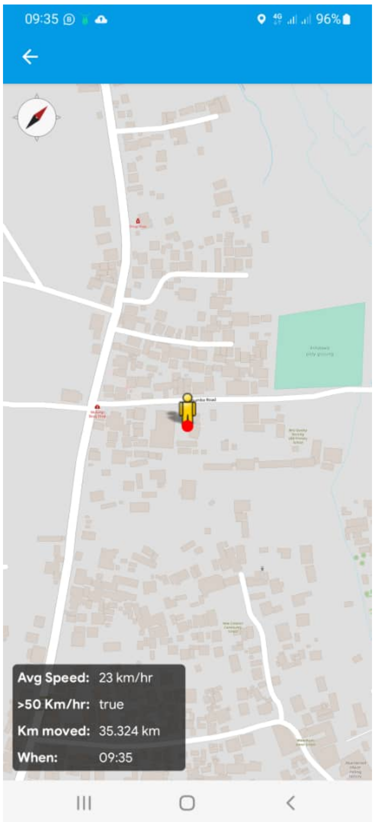
(a) example 1



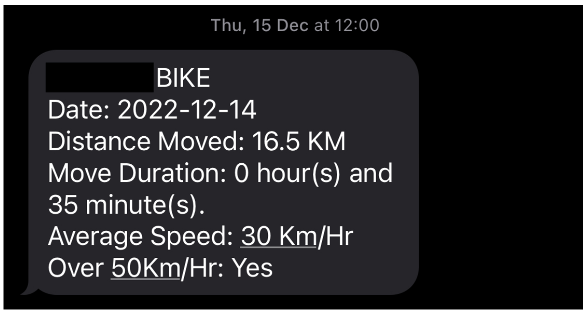
(b) example 2

Notes: This figure illustrates two examples of the spatial distribution of a driver's rides and the time series of his speed during the day.

Figure E2: App and SMS interface



(a) Smartphone App



(b) Automated SMS

Notes: Panel (a) of the figure illustrates the interface of the smartphone application developed to share information about driving behavior and own excess speeding. Information shows to a driver is about his own driving behavior from the previous day while the location is from 3 hours before the time of the access to the application. Panel (b) shows how the SMS that the driver receives about the driving behavior from the day before looks like.

two-fold: to get acquainted with the app and SMS information and to build trust in the quality of the GPS information[42].

F Timeline Details

I conducted three pre-registered experiments in December 2022 and January 2023 with a sample of 360 motorbike taxis operating from separate stations in the Greater Kampala Metropolitan Area. The experiments were implemented sequentially.

The *Belief Experiment* is a survey experiment, which has been implemented as part of the first baseline. The *Demand Experiment*) studies the impact of visibility of financial incentives to reduce excessive speeding on the demand for these incentives. The last intervention is the (henceforth *Impact Experiment*), which evaluates the impact of financial incentives under different disclosure policies on driving behavior and labor market outcomes.

Figure 4 describes the timing of the implementation of the experiments, together with the survey activities and the collection of GPS data. All participants were part of three blocks of activities: sample selection, data collection, and experiment implementation. To measure the potential effects of the intervention beyond the incentivized period, the surveyors contacted the drivers once again, one month after the end of the intervention. Both experiments make use of spatial data collected with GPS trackers. These data informed the design of the contract used in both the Demand experiment and the Impact Experiment. In particular, the respondent had access to information on their own speeding violations based on GPS data regardless of their treatment group.

The first contact of the research team with study participants was during the recruitment phase: listed drivers were contacted by phone and asked to answer a brief set of questions that corresponded to the eligibility criteria. Recruited drivers were asked to visit the office of our tech company partner. While the GPS was installed on the bike, the baseline survey (baseline 1) was administered to the participant. As part of the survey, the *Belief experiment* was implemented and the phone application paired to the GPS was explained. After the trial period of approximately four weeks, an in-person visit of a field officer was made in the proximity of the respondent's taxi station. During the visit, a second baseline survey (baseline 2) was administered with three goals: (i) drivers were asked to confirm interest in participating in the study; (ii) the *Demand experiment* was implemented, and (iii) individuals were randomly assigned to the experimental conditions of the *Impact Experiment* for the subsequent 10 days. During that period, I collected daily survey data through phone interviews. The study period ended with an end-of-contract survey that took place one month after the end of the contract.

[42]As the information about driving behavior reported in the version of the app for the participants is restricted to the day before, we used a version available on the smartphone of the field manager and a test bike available on at the office.

G Demand Experiment: Robustness, Within-Subject Design and Results

G.1 Between-Subject Robustness Checks

Table G1 summarizes the treatment effects of the main empirical specification when controlling for covariates that were unbalance at baseline as reported in Table B2.

G.2 Within-Subject Design and Results

The experimental design also allows me to exploit within-subject variation to compare the demand for the contract under different treatment conditions, holding fixed individual characteristics. To estimate the causal effect of social justification under the manipulation of the order-of-preference elicitation task, we estimate the following regression model:

$$\begin{aligned} Y_{ir}^{d} = \beta_0 + \beta_1 \text{PrivateMenu}_{ir} * \text{VisibleIncentives}_{ir} + \\ + \beta_2 \text{VisibleMenu}_{ir} * \text{VisibleIncentives}_{ir} + \mu_k + \delta_i + u_{ik} \end{aligned} \tag{8}$$

where Y_{ir}^{d} corresponds to outcome d for respondent i in round $r \in \{1, 2, 3\}$ of the preference elicitation procedure.

Table G2 reports the results of the regression analysis to estimate 8 on the following outcomes: the reservation price and the share of respondents who choose dominated contracts, when the contract is dominated.

H Field Experiment: Randomization Balance, Attrition, and Robustness Checks

The design features to ensure attendance throughout the study period translated into limited attrition, with no statistically significant differences between the treatment groups, as shown in Figure H1.

Table B3 presents the balance of the baseline covariates on the demographic and socioeconomic variables of the study participants, the labor market outcomes, and the organizational structure of their workplace. As in the Demand Experiment, some variables are unbalanced, as expected with a large number of comparisons. Ten out of 58 coefficients are statistically significantly different at the 10 percent level. The results are robust to specifications with and without controls for unbalanced variables. As I discuss in 5.2, in the main results we use ANCOVA specifications that control for the value of the baseline outcomes.

Table G1: Demand Experiment Results with Unbalanced Covariates - Choice of Dominated Contracts

	(1) weakly dominated	(2) dominated +1k	(3) dominated +2k	(4) dominated +3k
Priv. Menu / Vis. Incentives	0.225***	0.295***	0.324***	0.305***
	[0.063]	[0.063]	[0.060]	[0.057]
Vis. Menu / Vis. Incentives	-0.116*	-0.0793	-0.0952*	-0.0513
	[0.0617]	[0.0560]	[0.0494]	[0.0411]
p-value Priv. vs Vis. Menus	0.000	0.000	0.000	0.000
Observations	360	360	360	360
Mean in Priv. Menu & Incent.	0.43	0.30	0.23	0.15

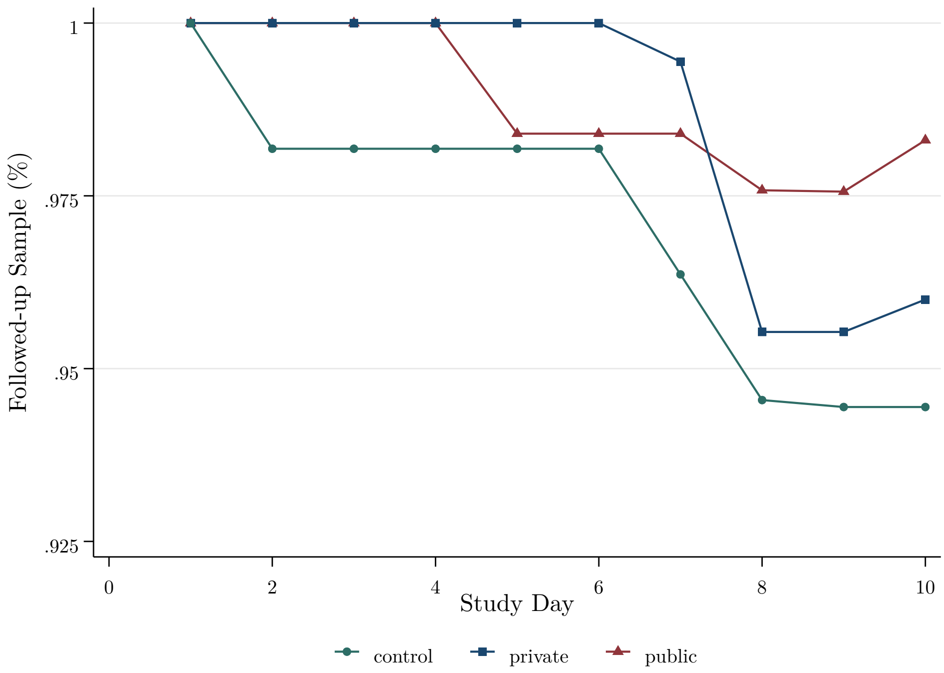
Notes: The table reports treatment effects on the fraction of drivers who choose the contract when they are weakly or strictly dominated by the unconditional payment offered as an outside option in the between-subject design (N=360). I report regression estimates from Equation 6, controlling for covariates that were not balanced at baseline as reported in Table B2. I define the contract as weakly dominated when the alternative unconditional payment is equal to the maximum possible payment of Ugx 6,000 under the contract (column 1). The contract is strictly dominated when the unconditional payment is larger than the highest payment under the contract, such as Ugx 7,000, 8,000 and 9,000 (columns 2, 3 and 4). *Priv. Menu / Vis. Incentives* variable is an indicator function of whether the respondent was randomly assigned to making binary choices between the contract and a set of unconditional payments knowing that the decision to accept the contract would remain private while the financial incentives offered would be publicized among colleagues at the taxi station by the field officer. *Vis. Menu / Vis. Incentives* is an indicator function of whether the respondent was randomly assigned to taking decisions knowing that their choice to take up the contract over an unconditional payment would be revealed together with the financial incentives and the size of the unconditional payment forgone. The omitted category is the fully private condition *Priv. Menu / Priv. Incentives* where both the preference of the contract over the unconditional payment and the contract terms are kept private. I report the p-value for the difference between *Priv. Menu / Vis. Incentives* and *Vis. Menu / Vis. Incentives*. Robust standard errors are reported in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table G2: Results Within-subject Design

	(1) res. price incentivized	(2) res. price all	(3) weakly dominated	(4) dominated +1k	(5) dominated +2k	(6) dominated +3k
Priv. Menu/ Vis. Incentives	1083*** [89.05]	1245*** [122.7]	0.265*** [0.0301]	0.215*** [0.0270]	0.166*** [0.0263]	0.105*** [0.0258]
Vis. Menu/ Vis. Incentives	-234*** [50]	-256*** [82]	-0.039** [0.016]	-0.069*** [0.016]	-0.022 [0.014]	-0.011 [0.016]
p-value Priv. vs Vis. Menu	0.00	0.00	0.00	0.00	0.00	0.00
Observations	1,086	1,086	1,086	1,086	1,086	1,086
Mean	5,373	5,616	0.29	0.23	0.12	0.10

Notes: The table reports treatment effects from the within-subject design (N=1,086) for 362 drivers exposed to all three visibility conditions on the reservations prices and on the fraction of drivers who choose the contract when they are weakly or strictly dominated by the unconditional payment offered as an outside option. Monetary amounts are reported in Ugandan Shilling. Columns 1 and 2 report the treatment effects on the reservation price for only the incentivized values (trucated at Ugx 9,000) or for full support, respectively. I define the contract as weakly dominated when the alternative unconditional payment is equal to the maximum possible payment of Ugx 6,000 under the contract (column 3). The contract is strictly dominated when the unconditional payment is larger than the highest payment under the contract, such as Ugx 7,000, 8,000 and 9,000 (columns 4, 5 and 6). *Priv. Menu / Vis. Incentives* variable is an indicator function of whether the respondent was randomly assigned to make binary choices between the contract and a set of unconditional payments knowing that the decision to accept the contract would remain private while the financial incentives offered would be publicized among colleagues at the taxi station by the field officer. *Vis. Menu / Vis. Incentives* is an indicator function of whether the respondent was randomly assigned to take decisions knowing that their choice to take up the contract over an unconditional payment would be revealed together with the financial incentives and the size of the unconditional payment forgone. The omitted category is the fully private condition *Priv. Menu / Priv. Incentives* where both the preference of the contract over the unconditional payment and the contract terms are kept private. I report the p-value for the difference between *Priv. Menu / Vis. Incentives* and *Vis. Menu / Vis. Incentives*. Robust standard errors are reported in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Figure H1: Impact Experiment: Attrition



Notes: This figure illustrates attrition by treatment conditions at daily level. *Private* is the Private Menu and Private Incentives visibility condition while *visible* is the Private Menu and Visible Incentives condition.

Table H1: Impact Experiment Results with Unbalanced Covariates - Driving Behavior

	(1) Share Any Overspeed	(2) Mean Overspeed	(3) Max Speed	(4) Distance Daily (km)
Priv. Menu / Vis. Incentives	-0.09***	-0.876***	-3.861***	3.28
	[0.026]	[0.21]	[0.95]	[4.65]
Priv. Menu / Priv. Incentives	-0.04*	-0.24	-1.77*	0.33
	[0.024]	[0.21]	[0.91]	[4.28]
p-value Private vs Visible	0.048	0.000	0.004	0.413
Observations	3,549	2,751	3,549	3,518
Control Mean	0.86	5.1	61.4	94.7

Notes: The table reports treatment effect on the driving outcomes. I report ANCOVA regression estimates on treatment group indicators, outcome values at baseline, and strata fixed effects and controls for unbalanced covariates as reported in Table B3. The unit of observation is the driver x day (N=3,549). In absence of attrition, the sample would have been composed of 3,600 observations (360 drivers for 10 days): however, the GPS failed to provide information in 7 instances, while 31 drivers did not work 10 days in the two-week period of data collection. The outcome in column 1 is a dummy that indicates whether any speeding violation has been committed on a given day (that is, GPS-recorded speed exceeding 50 kph during the incentivized day). Column 2 reports the average excess speed, that is, the average speed conditional on a speed greater than 50 kph (N = 2,751). Column 2 should be interpreted as the intensive margin, conditional on speeding. Column 3 reports the maximum speed in kph, winsorized at the top 1%. Column 4 reports treatment effects on the distance covered in a day, measured in kilometers (km). *Priv. Menu / Vis. Incentives* variable is an indicator function of whether the respondent was randomly assigned to make binary choices between the contract and a set of unconditional payments knowing that the decision to accept the contract would remain private while the financial incentives offered would be publicized among colleagues at the taxi station by the field officer. *Priv. Menu / Priv. Incentives* is an indicator function of whether the respondent was randomly assigned to take decisions knowing that their preference for the contract over the unconditional payment and the contract terms were kept private. The omitted category is the *Unconditional Cash Transfer* of Ugx 4,500 (UCT), calibrated based on the average realized payment under private contract in the pilot sample. I report the p-value for the difference between *Priv. Menu / Vis. Incentives* and *Priv. Menu / Priv. Incentives.* Robust standard errors clustered at the respondent level are in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table H2: Impact Experiment Results with Unbalanced Covariates - Labor Market Outcomes

	(1) Hours Worked	(2) Revenues (ugx)	(3) Marginal Costs (ugx)	(4) Disposable Income (ugx)	(5) Productivity (ugx/hour)
Priv. Menu / Vis. Incent.	-0.354	706	-2,113***	3,036**	427***
	[0.350]	[1,774]	[654]	[1,367]	[158]
Priv. Menu / Priv. Incent.	-0.059	735	-1,544**	2,566**	377**
	[0.314]	[1,623]	[609]	[1,261]	[149]
p-value Private vs Visible	0.242	0.982	0.220	0.626	0.634
Observations	3,525	3,525	3,525	3,525	3,141
Control Mean	8.12	30,310	14,270	16,039	2,083

Notes: The table reports treatment effect on the driving outcomes. I report ANCOVA regression estimates on treatment group indicators, outcome values at baseline, and strata fixed effects and controls for unbalanced covariates as reported in Table B3. The unit of observation is the driver x day (N=3,525). In absence of attrition, the sample would have been composed of 3,600 observations (360 drivers for 10 days): however, we failed to contact a handful of drivers for 1 to 3 days during the intervention. Furthermore, productivity is not calculated if no hours worked were reported (N=3,141). Monetary outcomes are reported in Ugandan Shilling. Column 1 reports the labor supply measured in hours; Column 2 reports daily revenues; Column 3 reports daily marginal costs (i.e. fuel and daily expenses); Column 4 reports the disposable income measures as the reported by the respondent; Column 5 reports the treatment effects on productivity measured as net revenues per hour worked. *Priv. Menu / Vis. Incentives* variable is an indicator function of whether the respondent was randomly assigned to make binary choices between the contract and a set of unconditional payments knowing that the decision to accept the contract would remain private while the financial incentives offered would be publicized among colleagues at the taxi station by the field officer. *Priv. Menu / Priv. Incentives* is an indicator function of whether the respondent was randomly assigned to make decisions knowing that their preference for the contract over the unconditional payment and the contract terms were kept private. The omitted category is the *Unconditional Cash Transfer* of Ugx 4,500 (UCT), calibrated based on the average realized payment under private contract in the pilot sample. I report the p-value for the difference between *Priv. Menu / Vis. Incentives* and *Priv. Menu / Priv. Incentives.* Robust standard errors clustered at the respondent level are in brackets. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.