# How to Prevent Traffic Accidents

## Moral Hazard, Inattention, and Behavioral Data

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**Abstract**

Risk mitigation is an essential aspect of risk management, but it has largely evaded attention by auto insurers that optimize selective risk-sharing and claim mitigation instead. We use novel sensor data to study drivers' risky phone use behavior and how behavior-based insurance contract can prevent accidents. We first measure moral hazard. We find handheld phone use ("HPU") to be risky but insensitive to both insurance changes and weather shocks that increase its riskiness. However, an experiment with a one-time text-message warning led to a persistent 15% HPU reduction. Drivers' inattention to risk thus limits moral hazard while inducing inefficiently high HPU. Based on this finding, we develop a structural model to distinguish nudging effect, risk aversion, and the price elasticity of HPU. This facilitates counterfactual simulations of optimal contracts with full insurance and a direct price on HPU (Holmström 1979). The "first-best" can be achieved with a 40-cent average charge per mile of HPU. A 62-cent charge can resolve additional externalities on traffic congestion and injuries.

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# Introduction

Recent advance in data and AI technologies has made detailed individual behaviors easily contractable. Sensor data have proliferated with the ubiquity of smartphones. Advance in machine learning techniques also allows accurate extraction of behaviors from the data.

For auto insurers, behavioral contracting is a fundamentally new way to manage risk. For centuries, managers have focused on selective risk-sharing and claim mitigation. The former relies on risk-rating, refusing coverage, and reinsurance, while the latter discourages accident reporting and overspending via monetary punishments.[1] These strategies take driving behavior and accident probabilities as fixed. They are effective at limiting the insurers' own risk exposure, but do little to reduce risk and prevent accidents.

The theoretical benefit of behavioral contracting is well understood in the presence of *moral hazard*. Driving safely requires costly efforts, and insuring people against accident losses may discourage those efforts. Contracting on driving behavior provides an escape from this trade-off between risk-sharing and incentive problem (Arrow 1978; Holmström 1979). However, empirical evidence on how driving behavior and accident probability respond to incentives is sparse (Einav and Finkelstein 2018).[2] The few studies on this subject all rely on realized insurance claims that are contaminated by accident under-reporting (Jeziorski, Krasnokutskaya, and Ceccarini 2017; Jin and Vasserman 2019). This is an important gap in the literature, especially given the extreme cost of traffic accidents and risky driving.[3]

In this paper, we collaborate with a transportation company that provides on-demand rides and self-insures against traffic accidents incurred by its drivers. Using drivers' smartphone sensor data, we are able to accurately identify handheld phone use ("HPU") behavior during rides. We use this behavioral data to quantify moral hazard directly and to study behavioral pricing in a "first-best" insurance contract *a la* Holmström (1979).

[1] In the U.S., deductibles and low coverage upper-limits are unavoidable. Drivers responsible for an accident also face higher future prices and potential rejection.

[2] Einav and Finkelstein (2018) points out that "'ex ante moral hazard' has received very little subsequent attention in empirical work [after a body of theoretical work]." In contrast, "ex-post" moral hazard on the utilization of services after losses occur has been well studied empirically.

[3] Traffic accidents killed 1.4 million people in 2019. In the U.S. alone, the estimated annual damage amounts to $995 billion in 2020 dollars (NHTSA 2015b). This includes property damage, quality-adjusted life year cost, medical cost, congestion costs, etc. Police reports show that 94% of fatalities involve risky driving (NHTSA 2015a).

Our main result is that moral hazard in risky driving is limited by inattention. Contracting on accident outcome therefore harms risk-sharing without inducing safer driving ("second-best"). Specifically, we find HPU to be risky but insensitive to both insurance reduction and to weather shocks that increase its riskiness. However, a one-time text-message warning on HPU led to a persistent 15% HPU reduction. Drivers' inattention to risk thus limits moral hazard while inducing inefficiently high HPU. Based on this finding, we develop a structural model to distinguish nudging effect, risk aversion, and the price elasticity of HPU. This facilitates counterfactual simulations of the "first-best" contract with full insurance and a salient price on HPU. It includes a 40-cent average charge per mile of HPU. A 62-cent charge can resolve additional externalities on traffic congestion and injuries.

We first introduce our behavioral data and provide background information. HPU is identified from phone sensor records based on AI algorithms. It significantly increases the likelihood of accident and passenger complaint. In other words, its *implicit price* is high. Meanwhile, a variance decomposition reveals that the across-driver HPU heterogeneity accounts for 68% of all data variation. This suggests that HPU may be more stable than implied by full rationality.

Our main analyses begin by laying out a simple rational framework of HPU. We then test its reduced-form implications using exogenous weather events and insurance coverage change. Overall, we find evidence rejecting the rational benchmark. First, precipitation strongly heightens the riskiness of HPU, but they have minute deterrence effect on it. This holds true for both across- and within-trip precipitation changes. Second, we study state-level regulatory changes that reduced drivers' auto insurance coverage. We use a synthetic control approach and find that the insurance reduction led to no detectable HPU change. Further, drivers in different states were differentially affected. We detect no differential HPU response to that variation either. We thus argue that moral hazard is weak; exposing drivers to more risk does little to discipline and deter their risky behavior.

We then conduct an experiment to explore the role of inattention and the effect of nudges. We sent a simple text message to a random subset of drivers with high HPU. The message brings attention to their behavior. It also informs them of a small added incentive: potential suspension when they receive passenger complaints on unsafe driving. The treatment effect is large and persistent. HPU drops by one-third on the first day, with weekly treatment effect stabilizing at 15% after a month. Meanwhile, no change is detected in driving hours or in other types of risky behavior such as harsh acceleration. More importantly, the experiment imposes no added incentives in trips without passengers. Yet we find a large drop in HPU nonetheless. The size of this drop is also larger in trips with precipitation and among drivers that stand to lose more in suspension. Therefore, inattention limits moral hazard, while nudges catalyze drivers' responsiveness to risk.

Based on the findings above, we develop a structural model of HPU to study contracting implications. Conceptually, we marry the canonical insurance framework with the sufficient statistics approach common in studies of inattention (Einav, Finkelstein, and Levin 2010; Allcott, Lockwood, and Taubinsky 2019). The key innovation is the separate identification of nudging effect, risk aversion, and the price elasticity of HPU. These primitives are necessary and sufficient in counterfactual simulations of optimal contracts. In exchange, we make three additional assumptions on driver expectation: correct risk perception when nudged, correct interpretation of our treatment incentives, and a CRRA-like HPU private benefit function.

Concretely, we first propose a risk model to translate the risks and incentives associated with HPU into implicit prices. The latter is defined as the certainty equivalent of the incremental accident and suspension risk caused by each mile of HPU. Due to the rarity of accidents, we rely on large observational data, applying exhaustive observable controls and fixed effects as well as a filter that remove post-accident HPU to shut down reverse causality. HPU's implicit price is then a function of these cost estimates and drivers' risk aversion.

Our behavior model builds upon the rational benchmark. The important addition is an attention latent parameter. It is modeled as a random effect with driver-level unobservables, and it scales HPU's implicit price to form the *perceived* price. It also includes a time-varying component to capture short-term Hawthorne effect. In addition, both driver attention and HPU's price elasticity can vary based on driver and trip characteristics.

Identification relies on similar data variation as our reduced-form and experimental analyses. First, the attention effect is identified from our intervention. Based on our assumption, the attention latent parameters are normalized to one when drivers are nudged. Second, risk aversion is identified based on how HPU responds to the variance of its associated risk, conditional on the expected cost of that risk. Lastly, with the cost estimates and risk aversion, we identify HPU price elasticity from two sources: precipitation and variation in treatment intensity due to pre-treatment earnings. The latter may be endogenous to driver-level unobservables. To account for this, we use a control function approach with lagged quarterly gas-price-change as instrument.

Estimation results put the average price elasticity of HPU at -0.73. Moreover, inattention is pervasive. The average driver under-perceives HPU's implicit price by 44% during normal times. This magnitude also varies substantially across driver and trip observables. Meanwhile, drivers are reasonably risk averse, with an average absolute risk aversion parameter of 1.6e-4. Taken together, our estimates highlight two sources of inefficiencies: (1) high HPU due to inattention, and (2) under-insurance, which not only undermines risk-sharing but also fails to discipline HPU due to inattention.

Finally, we use our model to study optimal contract design. Our goal is to replace under-insurance with direct prices on HPU so as to recover the "first-best" *a la* Holmström (1979). We take a three-step approach. We start from a counterfactual contract with full insurance and driver attention to HPU. This scenario has the most efficient risk-sharing. It also incorporates an attention effect that is inherent to direct prices on HPU. Second, we find the optimal HPU level for each trip by maximizing the private benefit, net all associated costs. Third, we search for the personalized HPU prices that bridge the HPU gap between steps one and two. Among high-HPU drivers, the "first-best" contract includes a 40-cent average charge per mile of HPU. A 62-cent average charge can resolve additional externalities on congestion and injuries.

**Related Literature** This paper brings together and contributes to three strands of literature. First, we study the micro-foundation of "ex-ante" moral hazard in insurance. We directly examine consumers' riskbearing decisions and empirically recover the "first-best" contract (Holmström 1979). In contrast, existing studies have largely focused on plan choice and utilization of services, not the underlying risk (Einav, Finkelstein, Ryan, Schrimpf, and Cullen 2013; Brot-Goldberg, Chandra, Handel, and Kolstad 2017; Finkelstein, Hendren, and Luttmer

2019; Ho and Lee 2020).[4] Closest to us is Jeziorski, Krasnokutskaya, and Ceccarini (2017), which use dynamic insurance data to identify moral hazard.[5] Their key finding is that risk class influences claim occurrence even for the same driver. Our behavior-based investigation extend their result by addressing limitations due to strategic claim under-reporting[6] and time-varying preferences. We also study direct behavioral price as opposed to insurance reduction as a solution to moral hazard. Methodologically, our framework is capable of simulating optimal contracts. It also maps novel behavioral data onto the set of primitives often used to explain insurance choices.

We also draw insights from the literature on inattention and bounded rationality. Our findings are consistent with the dual-process theory in the psychology of attention (Miller 1956; Stanovich 2009; Kahneman 2003; Gabaix 2019). Drivers must process a myriad of navigation, earning, and risk factors at any given time, but they can only consciously monitor few. We show that this results in an inattention to risk, which increases risky behavior while reducing its sensitivity to risk.

Lastly, we contribute to the literature on digitization and contracting innovations. Jin and Vasserman (2019) study a monitoring program in U.S. auto insurance. They find that consumers become 30% safer when monitored. We study the determinants of such large effects and the broader implications on insurance contract design. We also relate to two papers comparing Uber to traditional taxis. Liu, Brynjolfsson, and Dowlatabadi (2018) find that the ability for riders to monitor and to rate drivers limits moral hazard with respect to detours. Athey, Castillo, and Chandar (2019) study similar risky behaviors as us. They find that nudges, rider preference, and Uber's rating system can partially explain reduced risky driving compared to taxis. Our work covers similar channels, but the main focus is on

[4]Conceptually, both the historical origin and the common connotation of moral hazard emphasize perverse incentives on behaviors that heighten the probability of risk. See Rowell and Connelly 2012. ”Heavy insurance also increases the moral hazard, by developing motive for crime.” (Aetna Insurance Co., 1867) ”The owner...has little or no incentive for safekeeping hazard...” (Crosby, 1905) ”...an individual may choose to invest less preventive effort than otherwise to avoid the insured outcome.” (Shavell 1979) Consistent with the popular ”hidden action” interpretation, this is often referred to as “ex-ante” moral hazard. But it has largely evaded attention in the empirical insurance literature (Einav and Finkelstein 2018).

[5]Chiappori and Salanie (2000) discusses the difficulty in separately identifying adverse selection from moral hazard with static insurance data.

[6]Their paper focuses on liability claims and do not find that higher risk class leads to more severe reported claims. But claim severity is notoriously noisy and concentrated on small values. Therefore, a lack of statistical significant selection on claim severity may not rule out moral hazard on reporting. Similar studies include Abbring, Chiappori, and Zavadil (2008), Wang, Chung, and Tzeng (2008), Schneider (2010), and Weisburd (2015).

driver preference, accident impact, and insurance design. Finally, we relate to studies on usage-based pricing. For example, our modeling of risky behavior with uncertain payoff draws parallel with models of dynamic cellphone use under three-part tariff *a la* Nevo, Turner, and Williams (2016).

# 1 Background, Data, and Stylized Facts

## 1.1 Data

**Telematics** Our telematics data is derived from drivers' cell phone sensor record, whose collection is necessary for the transportation company to dispatch its drivers to passengers. Table 1 summarizes most key variables in our analysis.[7] The average driver is 43 years old and male. The average trip is 4.4 miles long and lasts for 16.8 minutes. The handheld phone use ("*HPU*") frequency in the average trip is 13.9 meters per mile.

There are many types of phone movements that are not HPU. Many drivers keep their phones in cup-holders or on dashboards. Disruptive vehicle movements such as going through speed bumps or collision accidents can also cause large sensor data fluctuations. As a result, the telematics event identification requires that the phone orientation be aligned with the car movement, that the car be traveling at nonzero speed, and that phone movement instability be detected for a nontrivial time duration. Sophisticated machine learning techniques and validation datasets are used to improve event detection.[8]

**Precipitation** Almost a third of all trips in our data experience precipitation, which partially reflect increased passenger demand in such weather conditions. We collect detailed and real-time data on precipitation type and intensity from Darksky's public API. We merge our datasets based on the geohash12 codes and timestamps at the start and the end of each trip. Table 1 includes summary statistics of our precipitation data.

**Insurance** The transportation company self-insures its drivers and offers no choice in coverage. The cost of insurance is passed onto the drivers in the form of a per-mile charge

[7] We are unable to provide accident statistics to protect company information.
[8] We briefly discuss the implications of our results on optimal precision-recall choice in the conclusion section.

that is actuarially fair based on historical claim records. All drivers enjoy close-to-unlimited coverage for third-party liability but face high deductible for repairing property damage to their own cars in at-fault accidents. There is additional coverage for drivers' own medical payment or when the at-fault counter-parties of an accidents fail to pay liability in a timely manner. This coverage varies by state and time, which we exploit in Section 3.2.

Table 1: Summary Statistics of Select Data Variables

| Variable | | Mean | Std. Dev. | Pctl(10) | Median | Pctl(90) |
|---|---|---|---|---|---|---|
| *Driver-level* | | | | | | |
| Driver characteristics | Age (years) | 43.4 | 12.3 | 27.9 | 42.1 | 60.8 |
| | Is female | 0.13 | 0.34 | 0.00 | 0.00 | 1.00 |
| | Past rides completed | 734.4 | 530.7 | 98.0 | 640.0 | 1505.0 |
| Vehicle characteristics | Vehicle age (years) | 5.6 | 3.3 | 2.0 | 5.0 | 10.0 |
| | Is SUV | 0.12 | 0.33 | 0.00 | 0.00 | 1.00 |
| | Is luxury | 0.05 | 0.22 | 0.00 | 0.00 | 0.00 |
| *Trip-level* | | | | | | |
| Trip characteristics | Distance (miles) | 4.4 | 3.8 | 1.4 | 3.2 | 8.6 |
| | Duration (minutes) | 16.8 | 8.5 | 8.3 | 14.8 | 27.8 |
| | Percent of drive-time with passenger | 0.72 | 0.15 | 0.50 | 0.74 | 0.89 |
| | Is weekend | 0.29 | 0.45 | 0.00 | 0.00 | 1.00 |
| | Is night-time | 0.41 | 0.49 | 0.00 | 0.00 | 1.00 |
| | Population density in geohash6 of pickup (1/sqmi/1k) | 17.5 | 21.4 | 1.4 | 9.0 | 44.9 |
| | Requests in same hour and geohash6 of pickup | 30.6 | 48.7 | 0.0 | 9.0 | 93.0 |
| Precipitation | Is raining | 0.27 | 0.45 | 0.00 | 0.00 | 1.00 |
| | Average rain intensity (in/hr/sqmi/1k) | 10.6 | 21.7 | 0.5 | 1.9 | 27.8 |
| | Change in rain intensity (in/hr/sqmi/1k) | -0.0 | 4.7 | -0.5 | 0.0 | 0.5 |
| | Abs. val. change in rain intensity (in/hr/sqmi/1k) | 1.6 | 6.1 | 0.0 | 0.0 | 2.8 |
| | Is snowing | 0.05 | 0.22 | 0.00 | 0.00 | 0.00 |
| | Average snow intensity (in/hr/sqmi/1k) | 4.4 | 6.9 | 0.4 | 1.4 | 10.4 |
| | Change in snow intensity (in/hr/sqmi/1k) | 0.0 | 1.2 | -0.1 | 0.0 | 0.1 |
| | Abs. val. change in snow intensity (in/hr/sqmi/1k) | 0.4 | 1.5 | 0.0 | 0.0 | 0.8 |
| Risky behavior | Meters of handheld phone use per mile | 13.9 | 64.4 | 0.0 | 0.0 | 0.0 |
| | Harsh acceleration incidents per 1k miles | 65.7 | 206.6 | 0.0 | 0.0 | 232.1 |
| | Safety complaints per 1k miles | 1.1 | 9.4 | 0.0 | 0.0 | 0.0 |

*Notes:* This table provides summary statistics of key data variables based on our observational data, which comes from a random sub-sample of drivers and trips from April 2019 to May 2020. We filter out uncommon trips defined by the start-geohash5-end-geohash5 combination having fewer than 10 occurrences. Some variables, such as driver earnings and accidents, are hidden to protect company private information. Risky behavior is recorded only when the vehicle is moving. Our sample size is more than 10 million trips.

## 1.2 Road Accidents and Cellphone Use

Handheld cellphone use while driving is outlawed in 19 U.S. states, while texting while driving is outlawed in all states (NHTSA 2019b). However, the law is poorly enforced. Accidents without DUI or speeding are defaulted as simple negligence, and even severe accidents rarely lead to trials. As a result, only 3% of first-respondent police reports for traffic fatalities involve definitive mentioning of cellphone use (NHTSA 2019a). In other regions in which camera and phone records are collected for police reports, the proportion of phone-use-caused road fatalities are far higher: 29.6% in Shanghai in 2019,[9] for example. This severe verification and under-reporting issue makes police-report-based risk inference invalid (Levitt and Porter 2001). It has also prompted some fleet owners to adopt telematics or in-vehicle surveillance solutions in some commercial settings (Jin and Vasserman 2019; Hubbard 2003). Therefore, although cellphone use is widely accepted as an important risk factor in road accident, it has nevertheless not received similar legal emphasis as other factors such as DUI and speeding.

## 1.3 Riskiness of handheld phone use

This subsection presents results from several observational studies that reveal the riskiness of HPU. The ideal test would be to conduct an experiment that change HPU for a randomized subset of drivers. However, the extreme rarity of accidents mean that tens of millions of trips must be in the treatment and control groups to achieve statistical significance.

We therefore rely on observational studies. Specifically, we focus on two channels: accidents and passenger complaints. To obtain a causal estimate, we perform four procedures. First, we add trip- and driver-level control variables. This includes all available variables on passenger demand (number of passengers, request frequency at the time and location of trip start, etc.), company actions, environmental factors such as precipitation, various time and location fixed effects (hour-of-day, day-of-week, month-year, start-end-geohash5), as well as other types of risky behavior detected from phone sensors. Second, for trips with accidents, we remove all post-accident HPU records. This limits reverse causal effects: accidents may

[9] https://www.chinadaily.com.cn/china/2014-11/14/content_18914543.htm.

lead to phone use, inflating the causal effect of interests. Then, we add driver fixed effects to arrive at our main specification, which corresponds to our investigation of moral hazard and behavioral improvement in latter sections:

$$\frac{\mathbf{1}_{\text{acc},it}}{\text{miles}_{it}} = p_b b_{it} + x'_{it}\beta + FEs^{i,\text{loc}_t,\text{time}_t} + \varepsilon_{it} \tag{1}$$

Here, $b$ is HPU/mile, $x$ are observable covariates, and $FEs$ are the fixed effects. We find that a 1% increase in HPU/mile leads to a 0.06% increase in accident/mile and a 0.02% increase in passenger safety complaints. This magnitude is large: holding phones for mere 2.3 more meters per mile leads to a one percent increase in accident rate. The results reported here is in elasticity. Their counterparts in levels are used directly as cost estimates in Section 5.

**Fact 1** Handheld phone use increases accident likelihood.

Table A.1 shows the cost estimates for each of the steps above. For robustness, we further absorb driver-month-location-pair average to control for driver-specific unobserved location and time factors (row 5 of Table A.1). This specification dramatically reduces the number of effective observations since most driver-month-location-pairs in our dataset have only a few observations with even less variations in HPU or accident.

## 1.4 Behavioral Stability

We regress trip-level HPU frequency on exhaustive trip-level covariates, as well as driver, location (geohash6-pair of start and ending locations), and time (hour-of-week and week-year) fixed effects to understand the relative importance of across- vs. within-driver behavioral differences. The exercise suggests that across-driver variation accounts for 68% of the overall behavioral variance. This surprising stability of HPU within drivers may contradict rationality given a myriad of environmental risk factors.

$$b_{it} = x'_{it}\beta + FEs^{i,\text{loc}_t,\text{time}_t} + \varepsilon_{it} \tag{2}$$

**Fact 2** Across-driver variation accounts for a majority of HPU behavioral variation.

# 2 Conceptual Framework

In this section, we lay out a simple rational expectation benchmark to explain handheld phone use ('HPU') behavior. The framework is used to develop several testable implications, and to formalize moral hazard and externalities of traffic accidents.

## 2.1 Rational Benchmark

Each unit of HPU carries minute incremental accident risk. As a result, we rely on the Arrow-Pratt approximation to model its utility impact. HPU, denoted as $b$ influences consumption $h$ by causing accidents with Poisson rate $\lambda(b, x|p)$ and by providing private benefit $g(b|\beta)$. Here, $x$ represents environmental factors, while $p$ and $\beta$ are key primitives to be estimated: marginal contribution of HPU on accidents and the incentive (implicit price) elasticity of HPU, respectively. When accidents occur, consumers suffer loss $\ell$ based on their insurance $y$, which are exogenously set by the transportation company.

$$h(b|\Omega) = -\lambda(b, x; p)\ell(y) + g(b; \beta) \tag{3}$$

$$u(b|\Omega; \theta) = \mathbb{E}[h] - \gamma\mathbb{V}[h] \tag{4}$$

$$= \underbrace{g(b; \beta) - \lambda(b, x; p)}_{\text{risk occurrence}} \underbrace{\ell(y)\left[1 + \gamma\ell(y)\right]}_{\text{ex-post utilization}} \tag{5}$$

$$\text{where } \Omega = \{x, y, p\} \text{ and } \theta = \{\beta, \gamma\} \tag{6}$$

WLOG, this model assumes that (1) the arrival of accidents follows a Poisson process with rate $\lambda$. (2) accident loss $\ell$ is deterministic in drivers' expectation conditional on $y$. (3) $g(b)$ is increasing and concave. (4) We adopt a quadratic utility function, or the "negligible third-derivative" approach (Barseghyan, Molinari, O'Donoghue, and Teitelbaum 2018). As in expression 5, the utility impact can be decomposed into risksharing/insurance-related terms, which governs the OOP expenditure and the utilization of accident remedies after accidents occur. The other terms, governing the arrival of accidents, is our main focus.

**Testable Implications** We model $\lambda$ as follows, where the marginal impact of $b$ on accidents vary with $x$ based on $p(x)$ (Equation 7). Then the implicit price of HPU, denoted as $\xi$, can be defined as the certainty equivalent of the additional risk exposure caused by each unit of $b$.

$$\lambda(b,x) := p(x)b + f(x) \tag{7}$$

$$\xi(x,y;\gamma) := -p(x)\cdot \ell(y)(1+\gamma\ell(y)) \tag{8}$$

We can generate the following testable implications ("TI").

**TI 1 Environmental Sensitivity** Risky behavior is suppressed in environments that heighten their riskiness. (Equation 10)

The link between riskbearing and risksharing is represented in Equation 11:

**TI 2 Moral Hazard** More insurance coverage encourages risky behavior.

$$\underset{\text{FOC}}{\Longrightarrow} \quad g'(b^\star) = \xi(x,y;\gamma) \tag{9}$$

$$b^\star_x = \frac{g'(b)}{g''(b)}\frac{p_x(x)}{p(x)} \propto -p_x(x) \tag{10}$$

$$b^\star_y = \frac{g'(b)}{g''(b)}\frac{\xi_y(x,y;\gamma)}{\xi(x,y;\gamma)} \propto -\ell'(y) \tag{11}$$

## 2.2 Private and Social Optima

Consistent with the classic principal-agent setting, the private optimum, or the "first-best" contract *a la* Holmström (1979) consists of optimal risk-sharing (full insurance). It also sets the optimal HPU level to maximize the difference between HPU's private benefit and its expected cost for each driver. The socially optimal level of HPU may differ due to additional negative externalities of accidents. In our setting, drivers are automatically covered by close-to-unlimited liability insurance that all but exempt them from financial liabilities in auto accidents.[10] Therefore, we primarily look at non-financial losses (quality-adjusted life year, or "QALY") as well as the time cost of traffic congestion.

[10]The liability upper-limit is only exceeded in rare cases in which long-term disabilities or severe injuries are caused. In these cases, the injured party will typically seek payment in trials or settlements with the company as opposed to with the drivers at fault.

# 3 Reduced-Form Evidence

This section conducts reduced-from tests for TI1 (environmental sensitivity) and TI2 (moral hazard). Here, we take advantage of repeated and high-frequency observations for the same driver as well as plausibly exogenous changes to drivers' risk exposure due to weather shocks and to state-level insurance mandate changes. Our results reject strong environmental sensitivity and moral hazard.

## 3.1 Behavioral Response to Weather Shocks

Precipitation significantly increases accident likelihood. Despite slower traffic, the overall risk of a fatal crash increases by 34% during active precipitation (Stevens, Schreck III, Saha, Bell, and Kunkel 2019). Precipitation also makes HPU riskier. Adding an interaction term between HPU frequency and precipitation indicator to our HPU riskiness analysis (Equation 1 and Table A.1 row 3), we find that the average precipitation event increases the marginal accident impact of HPU by 38.7% (10.9%).

The heightened riskiness of HPU should suppress HPU based on TI1 (environmental sensitivity). We test this hypothesis by directly regressing trip-level HPU frequency on precipitation indicator, average precipitation level of a trip, as well as changes in precipitation level within a trip (from beginning to end). The main specification includes trip-level controls specified in Table 1: demand conditions at the time and location at trip start, company actions such as the type of passenger(s) assigned for the trip, vehicle type, as well as driver, time, and location-pair fixed effects.

$$b_{it} = \begin{pmatrix} \kappa_{\text{rain}} \\ \\ \kappa_{\text{snow}} \end{pmatrix}_{it} \times (\underbrace{Inch_t}_{\substack{\text{across-trip} \\ \text{precip change}}}, \underbrace{\Delta Inch_t}_{\substack{\text{within-trip} \\ \text{precip change}}}) + x'_{it}\beta + FEs^{i,\text{loc}_t,\text{time}_t} + \varepsilon_{it} \tag{12}$$

Figure 1 reports the regression results and the percentage change in HPU induced by different precipitation events. Within the left panel, different colors (top to bottom) represent the HPU impact of across-trip difference in average precipitation, measured in thousandth inches per hour. Along the X axis, we plot the effect of within-trip precipitation change on HPU. This complements the first estimate by mitigating the effect of potential selection bias: drivers may be more likely to stop driving as precipitation increases.

The estimates are precise, and rain precipitation have economically small impact on HPU. This is especially so since the mean and median rain precipitation levels are only 11.4e-3 and 1.6e-3 inch/hour. Snow precipitation events elicit larger HPU response. However, the HPU response is still economically small: the mean and median snow precipitation levels are 4.6e-3 and 1.2e-3 inch/hour.

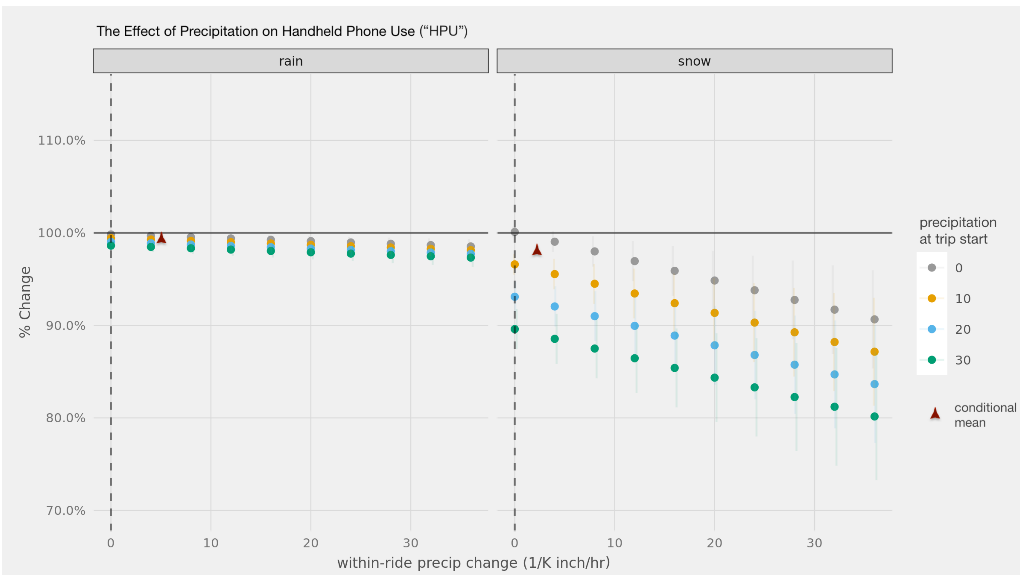



Figure 1: HPU impact of within-trip precipitation change

*Notes:* This graph reports the result of Equation 12: the effect of precipitation on HPU (handheld phone use frequency). It shows the impact (on HPU) of both within- (x-axis/left-to-right) and across-trip (color/top-to-bottom) precipitation change, for rain and snow weather (left vs. right panels). Controls include demand and company/dispatch factors such as ride type and passenger request density at trip start time and location (geohash6), as well as driver, time, and start-geohash5-end-geohash5 fixed effect.

The main assumption is that precipitation does not directly alter the private benefit of phone use while driving. It may be violated if, for example, drivers enjoy large private benefit from hand-holding their phones to check real-time weather forecasts, and such benefits scale with precipitation both across- and within-trips.

## 3.2 Behavioral Response to Insurance Changes

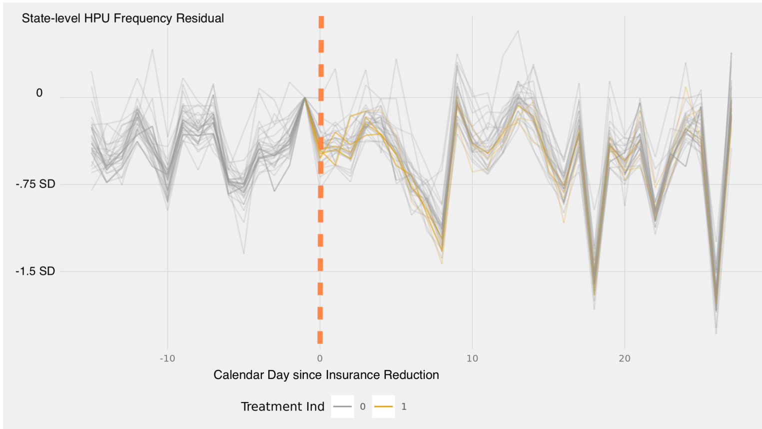
As discussed in Section 1.1, drivers at the transportation company are essentially exempt from any liabilities associated with road accidents. However, protection against losses incurred by the drivers themselves are not as well-insured. Drivers use their own cars for the trips and still face high deductibles to repair damages. Drivers can also potentially incur high out-of-pocket expenditures from their medical treatments, which are only partially covered. TI2 (moral hazard) implies that the more exposed drivers are to these losses, the less they should exhibit risky behavior such as HPU.

In this subsection, we directly test for moral hazard (TI2) with changes in insurance mandate across several states. As a result, coverage limits on own medical payments reduced from a quarter million to the new state mandatory minimum levels, which in some states are no coverage at all. This, combined with differences in the frequency and utilization of this claim type across states, led to significant variation in the impact on drivers' expected loss in accidents (Figure 2(c)). Drivers in most states saw small impact ($0.01 per trip), but those in three states experienced changes greater than $1 per trip.This insurance change was unanticipated; drivers were informed via email the night before the change.

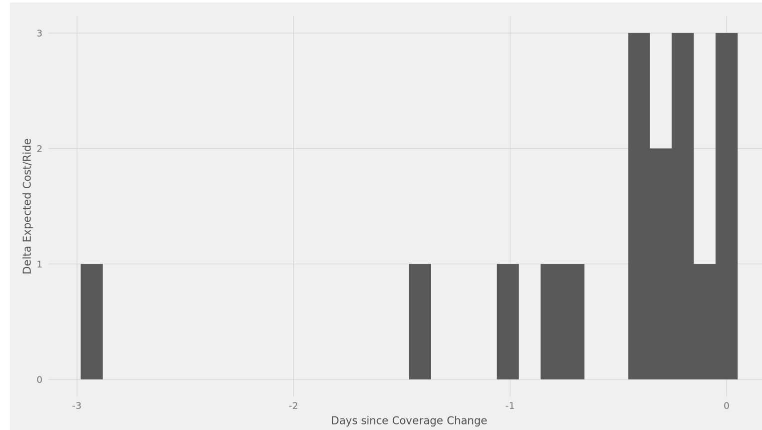
We use a two-step synthetic control approach to estimate the treatment effect of this insurance change (Abadie, Diamond, and Hainmueller 2010; Xu, Liu, and Xu 2017). In the first step, we estimate residual HPU progression across different states by regressing HPU on trip-level controls and fixed effects. It is similar to Equation 2, with the addition of state and day interactive fixed effects, which is plotted in Figure 2(a) with colors representing treatment status, lines for state, and X-axis for date relative to insurance change. We see that almost all states experienced a drop in HPU on the day of treatment regardless of insurance change. This is most likely a result of the treatment timing being on the first of the month. It also highlights the importance of unobserved time trends. We bootstrap standard errors. In the first step, for each state, we bootstrap 100 daily HPU residual estimates (i.e. 100 lines as in Figure 2(a)). In the second step, we also bootstrap 100 times, each using the corresponding bootstrap from the first step.

Figure 2(c) plots the treated data against the synthetic control estimates and the 90%

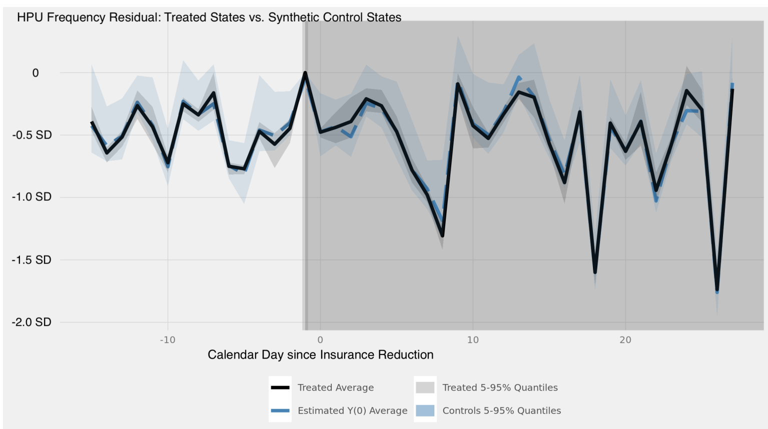
confidence band. It is clear that the mean synthetic counterfactual closely tracks real data (the black line largely covers the blue dotted line in the shaded region). To further test moral hazard and HPU response to risk, we separate treated states into three groups based on the cost impact of the insurance change. Figure 2(d) reports the average ATT. The average cost impact among the high-impact group is over ten times that of the low-impact group. However, our results in Figure 2(d) suggest that higher OOP impact did not deter HPU.



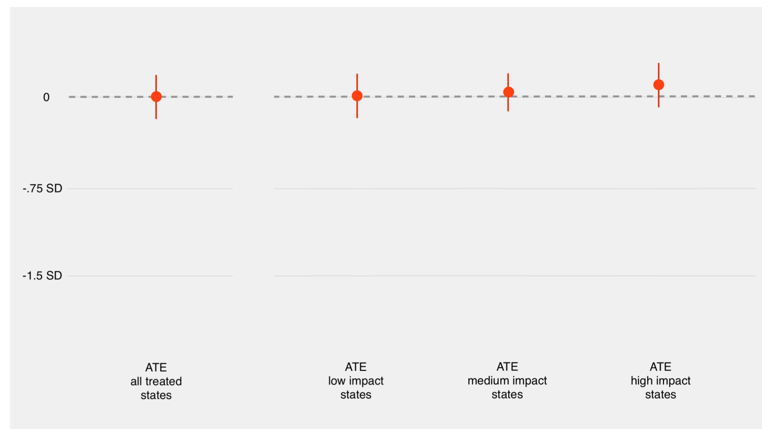

(a) HPU Progression by State - Insurance Change




(c) Cost Implications (Treatment Intensity) by State




(b) Treated Data vs. Synthetic Control Counterfactual




(d) ATT by Treatment Intensity

Figure 2: Behavioral Response to Insurance Change

*Notes:* These graphs present the synthetic control estimates of the treatment effect of the insurance regulatory coverage change on handheld phone use frequency. In Fig **??**, we plot the daily fixed effect estimated in a regression where the outcome variable is HPU frequency, and the controls include demand and platform factors, as well as driver and location fixed effects. Fig **??** is a permutation test/placebo test for the significance of the day-to-day change estimated before and after the insurance coverage change. Notice that a cross-sectional-RD-like estimator will likely produce spurious LATE due to unobserved time trends, especially given that the insurance changes happened on the first calendar date of the respective months.

# 4 Experimental Evidence

our findings in the last section reveal a general behavioral insensitivity to risk and a lack of moral hazard. We now conduct an experiment with a simple nudge. It sheds light on the role of inattention HPU and HPU response to incentives when drivers do pay attention.

## 4.1 Experimental Design

- **Treatment**: we sent the following warning to drivers via a one-time text message: "[*Our*] *app shows you may be holding your phone while driving. Passenger reports of unsafe driving, like handheld phone use, can lead to suspension.*"

  This intervention is designed (1) to call for drivers' *attention* to the riskiness of HPU and (2) to increase the financial risk associated with practicing HPU so that any additional passenger complaints will likely lead to suspension. The text message ends with a link to an FAQ page containing legal primers, explanations of how telematics data are collected, as well as details about suspension, including investigation procedures to verify passenger complaints and categorize them as unsafe driving.

- **Target population**: drivers with prior passenger complaints and are in the top 5% percentile HPU over the two-week period prior to intervention. We focus on drivers who work at least 20 hours a week on average over the last quarter.

- **Logistics**: we pre-select a 30-min window randomly (3-3:30pm EST). On the intervention day, all targeted drivers logging on and starting a shift during this period are in our experimental sample. To ensure safety, we automatically exclude drivers who are traveling at non-trivial speed when they log on. There are 467 treated drivers, to whom we sent the treatment text message right when they log on. The main control group consists of 512 hold-out drivers, including 25 drivers who were randomized into treatment but were unreachable by text message due to engineering issues (connectivity or device issues). We continue to include trip-level controls and fixed effects as in the reduced-from section. Standard errors are clustered at the driver level.

### 4.2 Average Treatment Effects

The average treatment effect is large and persistent. As shown in Figure 3(a), there is a 1/3 HPU reduction within the 24-hour window after the intervention. This likely includes a Hawthorne effect: drivers behaving better when they first realize that they are being watched. However, this effect is typically short-lived,[11] which seems to hold up in our case: the first week sees an average treatment effect of 21%, which reduced to stabilize around 15% in subsequent weeks: 14% for weeks 2 and 3; 16% for week 4.

Consistent with the reduced-form analyses, we estimate treatment effects with the regression below, in which $\tau$ represents calendar day or week relative to the intervention time.[12]

$$b_{it} = \alpha_t + \delta_\tau D_{it} + x'_{it}\beta + FEs^{i,\text{loc}_t,\text{time}_t} + \varepsilon_{it} \tag{13}$$

### 4.3 Inattention and Heterogeneous Treatment Effect

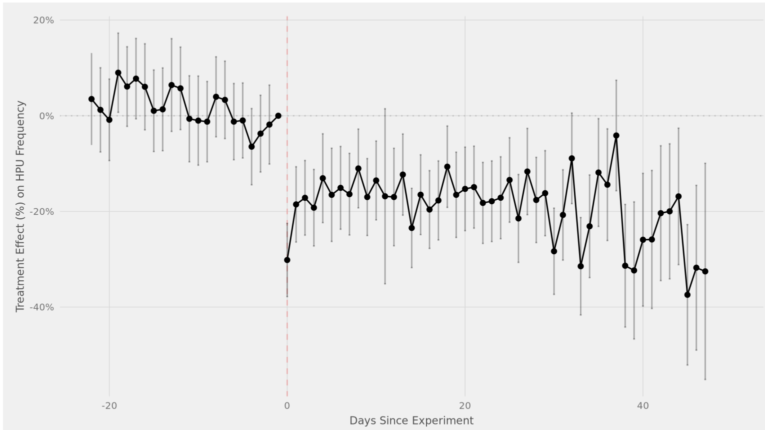
This subsection argues that driver attention is the reason why our experiment generates seemingly contrasting results from our reduced-form tests. First, the intervention generates no detectable change in other types of risky behavior. Figure 3(c) reports treatment effect on harsh acceleration. The intervention increases the financial risk of all behavior that caused safety complaints, but only specifically called drivers' attention to HPU. It is therefore plausible that driver attention played a key role in the big treatment effect on HPU.

Moreover, the additional treatment incentives only apply when there are passengers in the car. In reality, drivers spend a good amount of time going to pickup assigned passengers (Table 1). During these trips, they are paid similarly and are exposed to the same amount of accident risk as trips with passengers.[13] Figure 3(d) reports the differential treatment effects using a "difference-in-differences" estimator. Surprisingly, treatment effect is larger for non-

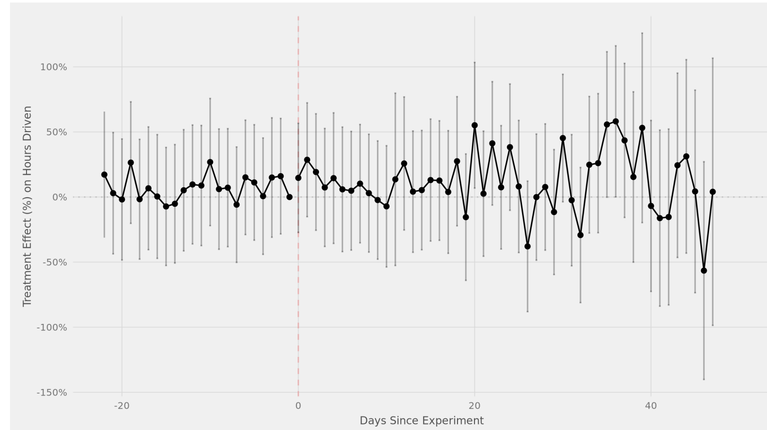
[11] Friedman and Gokul (2014) provides an empirical survey of Hawthorne effect: "Of note is that the Hawthorne effect was rather short lived... teachers habituate quite rapidly to video observation and return to ânormalâ levels of practice within a day or so after the introduction of the camera."

[12] Including driver fixed effects resembles a block design and puts higher regression weights on drivers with higher variance in the treatment variable, which are those who did not change the number of trips driven before and after treatment. We do not detect statistically significant treatment effect on driving time (Figure 3(b)).

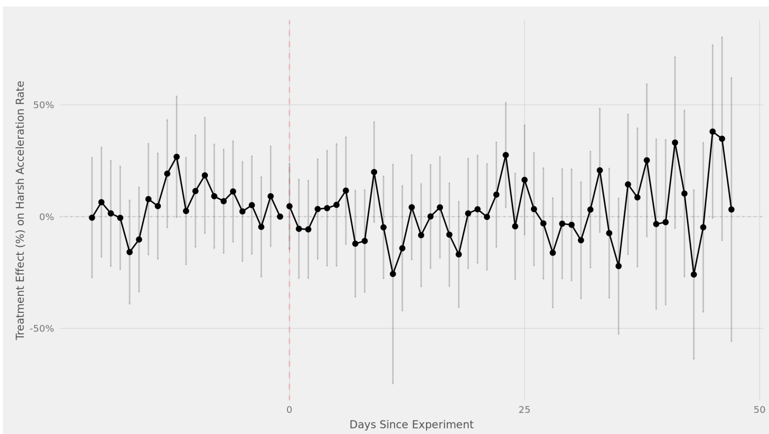
[13] Passenger injuries and loss productivity are fully insured by the company.



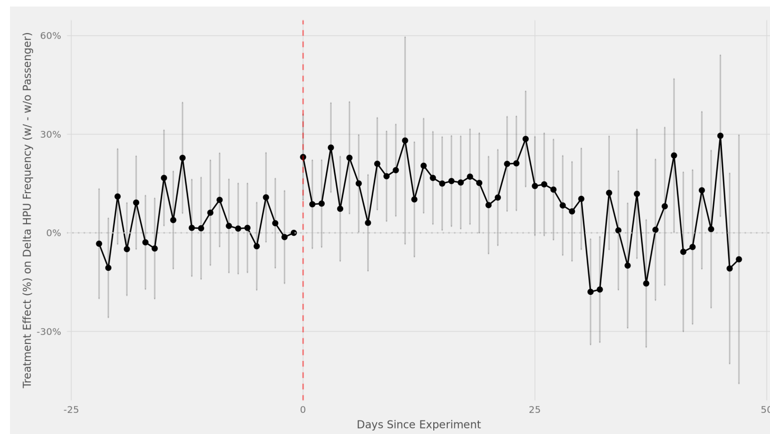

(a) Daily % ATE on HPU Frequency




(b) Daily % ATE on Hours Driven




(c) Daily % ATE on Harsh Acceleration Frequency




(d) Differential TE for Trip w/ and w/o Passengers

Figure 3: Direct Intervention: Experimental Results

*Notes:* Figures (a), (b), (c) plot the estimates on the interaction between daily fixed effects and the treatment indicator, converted to percentage terms based on the pre-treatment average level of the outcome variable. Figure (d) further interact this interaction term with an indicator of whether the trip has passengers and therefore is subject to the treatment incentives. (a) and (d) use the frequency of HPU as outcome variable, controls for demand and platform factors such as ride type and trip request density, as well as driver, time-of-day, and weekday fixed effects. (c) reports the same estimates as Figure (a), replacing the outcome variable with the frequency of harsh acceleration. (b) is on the driver-day-level and therefore uses the total driving hour as the outcome variable, and average out trip-level controls.

-passenger trips with no added incentives. This isolates the attention effect. As we will see in the next section, the larger treatment effect in non-passenger trip is due to higher pre-treatment HPU, so a smaller proportional treatment effect translates into larger drop in HPU level. In fact, quantile regressions reveal that proportional treatment effect is almost identical across drivers with different pre-treatment levels of HPU (Figures B.6(a) and B.6(b)). Higher HPU drivers therefore saw much higher decrease in absolute levels.

Combined with our reduced-form findings, we argue that drivers' inattention to HPU risk leads to both inefficiently high HPU and an insensitivity to risk, limiting moral hazard. As a result, nudges and direct prices will be much more effect at deterring HPU.

# 5 Structural Estimation and Results

Building upon the rational benchmark in Section 2, this section develops and estimates an econometric model to formalize how HPU behavior respond to incentives. Our model closely relates to the sufficient statistics approach used in many studies on inattention (Allcott, Lockwood, and Taubinsky 2019). Our innovation is to further distinguish attention effect and risk aversion from the (implicit) price elasticity of behavior. This is theoretically important in optimal contract design: as we move towards "first-best," we replace under-insurance with direct price on the (now contractable) risky behavior (Holmström 1979). For drivers, this removes uncertainty and triggers their attention. The former corresponds to the resolution of moral hazard, while the latter resolves inefficiency from inattention to risk.

To achieve that, our structural model makes three additional assumptions: (1) drivers' interpretation of treatment incentives is correct; (2) drivers correctly perceive incentives with nudges ; (3) a utility function that disciplines how inattention influences driver behavior.

## 5.1 Risk Model: Translate Incentives to Implicit Price of HPU

Similar to a canonical insurance model *a la* Einav, Finkelstein, and Levin (2010), we first model the implicit price of HPU $\xi$. It is defined as the certainty equivalent of the incremental risk from accidents or reduced earning (due to suspension) caused by each unit of HPU. The first source is out-of-pocket expenditure and money-metric non-financial cost $\underline{C}$ of accidents.

$$\xi_{it}^{\text{accident}}(x'_{it}, y_i; \gamma) = p^a(x'_{it}) \cdot \left[ (\underline{C}_{it} + y_i) \cdot (1 + \frac{\gamma}{2} \cdot (\underline{C}_{it} + y_i)) \right]$$

We inherit notations from Section 2: $x_{it}$ is trip-level observable characteristics, and $y_i$ is the insurance deductible for driver $i$. $p^a(x'_{it})$ is the marginal contribution of HPU ($b$) on accident as calculated in Section 1.3. $x'_{it}$ are precipitation variables as in Section 3.1. We assume that (1) drivers have no uncertainty over the size of losses conditional on accidents; (2) as in Cohen and Einav (2007), drivers expect to pay the deductible in an accident;[14] (3) drivers have homogeneous risk aversion $\gamma$; and (4) drivers have homogeneous expectation for $\underline{C}$.

$$\underline{C}_{it} = 43,676 \cdot .35 \cdot .57 \cdot (1 + \kappa N_{\text{pas,it}}) \tag{14}$$

We calibrate $\underline{C}$ based on NHTSA (2015b): the average quality-adjust-life-year cost per accident with injury (\$43,676) is weighted by the proportion of accidents with injury (35%). 57% of the injuries are on drivers (57%), and we allow drivers to exhibit altruist concerns for passengers in passenger trips based on parameter $\kappa$, which can also be interpreted as intrinsic motives to increase service quality and avoid accidents. (Kolstad 2013).

Drivers face earning risk associated with passenger ratings and complaints similar to those discussed in Athey, Castillo, and Chandar (2019). Without the experimental treatment, passengers rate drivers after each trip and suspension happens when a driver's rolling-average rating $r$ over the last $N$ trips drop below a certain threshold $\underline{R}$, for which the driver loses his or her earnings $w_i$ (and do not have an easy way to compensate for the loss). $w_i$ is the average weekly fare and tips minus gas spending (weekly region-level gas price scaled by total miles traveled and divided by vehicle mpg) during the pre-treatment period. The implicit price of HPU due to complaint risk can be expressed as:

$$\xi_{it}^{\text{complaint}}(x'_{it}, r_{it}, w_i; \gamma) = \underbrace{\frac{\partial \Pr(r' \leq \underline{R} | b, r_{it}, x'_{it})}{\partial b}}_{:= p^c(x'_{it}, r_{it})} \cdot \left[ w_i (1 + \frac{\gamma}{2} w_i) \right]$$

In reality and in our estimation, suspension rule is $\{\underline{R}_j, W_j\}_{j=1,\ldots,J}$, in which $\underline{R}_j$s are rating cutoffs while $W_j$ are weeks suspended (capped at 4).

[14] Notice that the insurance change discussed in 3.2 happened after our experiment ends. Before the insurance change, coverage is close-to unlimited except for the deductibles.

Lastly, suspension risk increases for treated drivers. Passenger complaints related to unsafe driving would automatically trigger suspension regardless of driver ratings:

$$\xi_{it}^{\text{treat}}(x'_{it}, r_{it}, w_i; \gamma) = \underbrace{\frac{\partial \Pr\left(\text{safety complaints \& } r' > \underline{\text{R}} | b, r_{it}, x'_{it}\right)}{\partial b}}_{:=p^D(x'_{it}, r_{it})} \cdot \left[w_i(1 + \frac{\gamma}{2} w_i)\right]$$

**Estimation** We rely on large observational data to estimate $p$'s and hence the mapping from $\{\gamma, \kappa\}$ to $\xi$. As in Section 1, we apply exhaustive observable controls, driver, time, and location fixed effects, and a filter that remove all post-accident behavioral data to shut down reverse causality. HPU's implicit price (sometimes abbreviated as "IP") is then the sum of the $\xi$ components modeled above, each a function of cost estimates and $\{\gamma, \kappa\}$.

$$\xi_{it} = \underbrace{\xi_{it}^{\text{accident}}(x_{it}, y_i; \gamma, \kappa)}_{\text{accident IP}} + \mathbf{1}^{pas} \cdot \left[\underbrace{\xi_{it}^{\text{complaint}}(x_{it}, r_{it}, w_i; \gamma, \kappa)}_{\text{complaint IP}} + D_n \underbrace{\xi_{it}^{\text{treat}}(x_{it}, r_{it}, w_i; \gamma, \kappa)}_{\text{treatment IP}}\right]$$

## 5.2 Behavior Model: HPU Response to Implicit Prices

With the implicit price of HPU modeled above, we now model the corresponding HPU behavioral response. We adopt a CRRA-like functional form to facilitate clean separation of moral hazard and attention effects as in Equation 16.

$$g(b) = \frac{1}{\alpha} \frac{b^{1+\frac{1}{\beta}}}{1 + \frac{1}{\beta}} + \text{const} \tag{15}$$

$$b = (\alpha \xi)^{\beta} \tag{16}$$

$\beta$ is the elasticity of HPU with respect to (implicit) prices. Holding $\alpha$ constant, the moral hazard effect depends on $\beta$ and risk aversion $\gamma$:

$$\frac{b_y}{b} = \beta \frac{\xi_y}{\xi} = \beta \frac{(1 + \gamma y_i)}{\xi}$$

In addition, $\alpha$ is the attention factor. It changes HPU behavior by scaling its implicit price. We therefore term $\alpha\xi$ as the "*perceived implicit price*" of HPU. Our experimental treatment effect can be expressed as an attention effect that scales the perceived implicit price $\alpha$ plus an incentive effect from actual $\xi$ change, both of which move behavior based on elasticity $\beta$.

$$\Delta^{\text{treat}} \log b = \beta \left[ \Delta^{\text{treat}} \log \alpha + \Delta^{\text{treat}} \log \xi \right]$$

**Heterogeneity** The attention and price elasticity latent parameters are both heterogeneous across drivers and trips based on observable characteristics. The attention parameters also vary based on *driver-level unobservables*. We further include a time-varying component to capture Hawthorne effect.

$$\beta_{it} = \underbrace{x'_{it}\theta_\beta}_{\text{price elasticity}} \tag{17}$$

$$\log \alpha_{it} = -(1 - D_{it}) \cdot \left[ \underbrace{x'_{it}\theta_\alpha + \varepsilon_i}_{\substack{\text{attention} \\ \text{nudge effect}}} \right] + D_{it} \cdot \left[ \underbrace{\frac{\alpha_H^0(x_{it})}{(1 + \alpha_H^1 \cdot t)}}_{\text{Hawthorne}} \right] \tag{18}$$

$$\varepsilon_i \overset{i.i.d.}{\sim} \mathcal{N}(0, \sigma_\alpha) \tag{19}$$

Equation 20 is the main estimating equation. We use a Poisson-logit-normal mixture distribution to accommodate the discrete-continuous nature of HPU and the left-skewness of the log positive HPU distribution, which is why we don't use log-normal.

$$\log \mu_{it}^b = \beta_{it} \cdot [\log \xi_{it} + \log \alpha_{it}] \tag{20}$$

$$\text{logit}(\tilde{b}_{it}) \sim \mathcal{N}(\mu_{it}^{\text{logit}(b)}, \sigma) \tag{21}$$

$$b_{it} = \text{Poisson}(\delta_0 + \delta_1 \mu_{it}^b) \cdot \tilde{b}_{it} \tag{22}$$

### 5.3 Identification of Behavioral Primitives

Our focus is to separately identify heterogeneous price elasticity of HPU ($\beta$), heterogeneous nudging effect ($\alpha$), and the average level of risk preference $\gamma$. Conditional on trip-level characteristics $x_{it}$, the intuition for identification is as follows. The experimental treatment effect on trips without passengers allows separate identification of the persistent nudging effect on attention as well as the transitory Hawthorne effect on attention $\alpha_H$, conditional on $\{\beta, \gamma\}$. Here, our assumption that drivers correctly perceive incentives when nudged provides necessary normalization. The HPU price elasticity with nudges serves as the baseline upon which the effect of inattention (without nudges) and that of the short-term

Hawthorne effect are identified (Equation 18).

Separate identification of risk aversion is important in our setting as many counterfactuals focus on expanding risksharing and removing driver uncertainty. Relying on theory, risk aversion is identified based on how HPU responds to the variance of its associated risk, conditional on the expected cost of that risk. Across drivers, variation in the spread of HPU risk exists due to predetermined earning and rating differences. Within-driver variation comes from our treatment and from precipitation. To better visualize our functional form restriction, assume that we only have one source of HPU implicit price: a lottery with payoff $s$ at probability $\lambda$, where each HPU mile increases $\lambda$ by $p$. Then with a nudge, we have:

$$\begin{aligned}\log b =& \beta \cdot (\log p + \log s + \log(1 + \frac{\gamma}{2}s)) \\ \approx& \beta \cdot (\log p + \log s + \frac{\gamma}{2}s) \text{ for small } \gamma\end{aligned}$$

This reflects how $\gamma$ is capturing higher-order effect of risky payoffs on behavior.

To account for potential endogeneity issues between the pre-treatment earnings and unobserved attention or incentive factors, we employ a control function approach.[15] Our instrument $z_i$ is city-level gas-price-change in the quarter before the pre-treatment period.

$$w_i = x_i'\theta_w + \theta_z z_i + \nu_i \tag{23}$$

$$\varepsilon_i = \zeta z_i + \psi \nu_i + \sigma_{\alpha,z}\epsilon_i \tag{24}$$

The endogeneity is modeled through the correlation between $\varepsilon_i$ (Equation 19) and $\nu_i$. The key assumption for the validity of the instrument is that the unobserved driver-level component of the attention factor $\varepsilon_i$ is conditionally independent of $w_i$ given $z_i$ and $\nu_i$.

Drivers exhibit far less HPU behavior when passengers are present. $\kappa$ is identified based on how this differential varies based on the number of passengers assigned to the driver in each trip. Lastly, $\delta$'s are identified based on how the fitted $\mu^b$ and $\mu^{\log b}$ differ in their explanatory power for HPU occurrence versus positive HPU magnitude.

**estimation** We estimate the model using a simulated maximum likelihood approach. For each coefficient proposal, we generate 50 draws of $\varepsilon_i$ for each driver $i$. Then we average over

[15] We do not focus on rating-related endogeneity since the implicit price due to ratings is zero for the vast majority of drivers and trips. Only drivers immediately above the suspension thresholds see non-zero implicit prices. We therefore assume that these drivers are plausibly similar along unobservable dimensions.

the corresponding $\mu_b$ (not $\log \mu_b$) to obtain the simulated latent parameter. Conditional on estimates of $\xi$'s from Section 5.1, we estimate the behavioral model on a randomized 80% subsample of our experimental sample, saving the remaining 20% trips for validation. We bootstrap 50 times within the training sample to obtain bootstrap standard error, each time using a random draw of $\xi$'s based on the incentive estimates (middle left panel in Table 2).

### 5.4 Estimation Result

Figures 4(a) and (b) plot the fit of our model in and out of training sample. Table 2 reports our estimation results. The average price elasticity of HPU is $-0.73$ (Column 1). Given our CRRA-like private benefit function, $\beta > -1$ means that HPU private benefit is high close-to-zero, but quickly diminishes. Across drivers, selection on driver acquisition channel and experience are important: drivers acquired through paid channels, often poached from other firms, are more price sensitive, and so are drivers with more trip and accident experience.

The nudging effect is large (Column 2). Based on our estimates, drivers only perceive 66 cents for every dollar of implicit price on HPU in normal times. Since we assume that drivers correctly perceive incentives when nudged, larger coefficients in Column 2 (higher nudging effect) indicates more under-perception in normal times. In particular, high risk awareness significantly (in the statistical sense) correlate with trip and accident experience, drivers being referred by passengers, as well as weekday commuting trips. There is also a short-lived Hawthorne effect ($\alpha^H$) with a half life of 0.67 days.

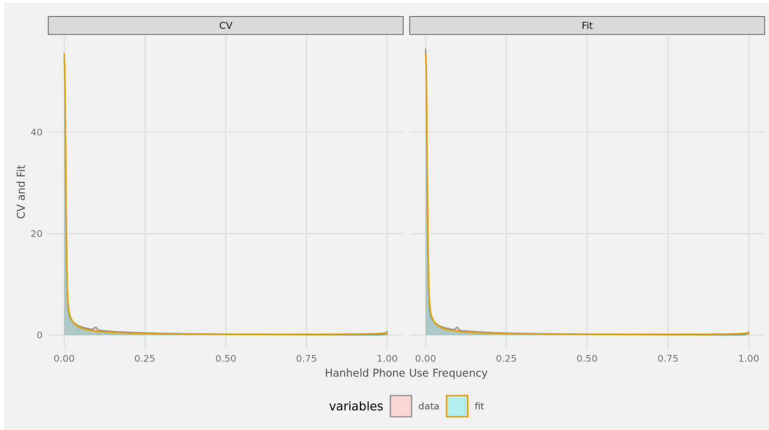
Drivers are risk averse, with $\gamma = 1.16e - 4$. They also have large altruistic motives: the deterrence effect of non-financial costs borne by passengers is 3 times as large as those borne by the drivers themselves (Kolstad (2013)). Lastly, the mid bottom panel shows the distribution of implicit prices. High HPU risk and risk aversion combined to put the average HPU implicit price from accidents at $3 per HPU mile. Those from ratings and our treatment are small. However, the variance of rating implicit price is large: negligible for most but very high for those that are just above suspension cutoffs.

Table 2: **Estimation Result and Counterfactual Contract Simulation**

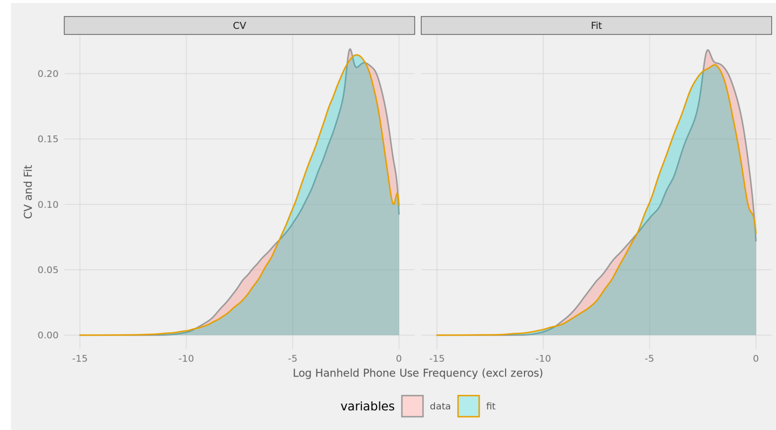
| | **Price elasticity** $\beta_{it}$ | **Log Nudge Effect** $\log \alpha_{D,it}$ | **First Stage** $w$ | **Private Optima** $\tau_p$ | **Social Optima** $\tau_s$ |
|---|---|---|---|---|---|
| Mean [sd] | -0.73 [0.11] | -0.70 [0.52] | 808.3 [591.1] | 40 [43] | 62 [66] |
| Converted Mean [sd] | | 0.44 [0.27] | | | |
| – intercept | -0.58*** (0.25) | 1.34*** (0.45) | 569.7 [14.9] | 0.69 | 0.71 |
| *driver characteristics* | | | | | |
| – age | -0.02 (0.05) | -0.13 (0.49) | 1.6 (1.2) | -0.02 | -0.02 |
| – male | -0.04 (0.04) | 0.20 (0.71) | 1.3* (0.7) | -0.66 | -0.92 |
| – tenure | -0.03 (0.03) | -0.12 (0.07) | 2.6*** (1.1) | -0.01 | -0.02 |
| – lux | -0.03* (0.01) | 0.03** (0.00) | 0.03** (0.00) | 0.09 | 0.14 |
| – referral lead | 0.24* (0.13) | -1.52*** (0.62) | -0.0 (0.0) | -0.51 | -1.24 |
| – organic lead | 0.04 (0.03) | 0.01 (0.30) | -0.0 (0.0) | -0.45 | -0.67 |
| – paid lead | 0.07*** (0.02) | -0.74 (0.68) | -1.0 (0.8) | -0.49 | -0.71 |
| *driver experience* | | | | | |
| – past trip | 0.14** (0.07) | -1.06** (0.53) | 10.7 (8.0) | 0.35 | 0.47 |
| – past accident | 0.03*** (0.01) | -0.48** (0.23) | 2.7 (2.0) | 0.00 | 0.01 |
| – past complaint | 0.03* (0.02) | 0.24 (0.17) | 5.0 (3.7) | 0.02 | 0.04 |
| *trip characteristics* | | | | | |
| – distance | -0.10** (0.03) | 0.74 (0.66) | | -0.79 | -1.08 |
| – weekday | 0.02*** (0.01) | -0.09 (0.20) | | -0.64 | -0.84 |
| – weekday commute | -0.02 (0.03) | -0.25*** (0.13) | | -0.62 | -0.67 |
| – wknd day | 0.02 (0.01) | -0.16 (0.13) | | -0.80 | -0.99 |
| – wknd night | 0.02 (0.02) | -0.21 (0.32) | | -0.77 | -1.00 |
| – pop dens | -0.00 (0.01) | -0.07 (0.20) | | 0.05 | 0.08 |
| – req dens | -0.01** (0.01) | -0.17 (0.74) | | -0.34 | -0.47 |
| – passenger ind | 0.00 (0.00) | -0.14 (0.65) | | -10.56 | -15.98 |
| – comms | 0.00 (0.00) | 0.06 (0.28) | | 0.02 | 0.03 |
| – precipitation | 0.01** (0.02) | 0.17 (0.26) | | 36.06 | 66.47 |
| *other* | | | | | |
| – $\psi$ control function | | 0.0 (0.00) | | | |
| – $\sigma_{\alpha,z}$ random coef. | | 0.11*** (0.02) | | | |
| – $z$ trailing gas price shock | | -0.26* (0.16) | 175.0*** (1.4) | | |
| – $\sigma_w$ | | | 364.5*** (0.4) | | |

| **Incentives: other** | | **Attention other** | | **Implicit Prices** | | **Link Function** | |
|---|---|---|---|---|---|---|---|
| Risk Aversion $\gamma$ | 1.16e-4*** (1.22e-7) | Hawthrone Halflife $1/\alpha_H^1$ | 0.68*** (0.01) | $\xi_{\text{accident}}$ [sd] | 3.00 [2.26] | $\delta_0$ | 0.00*** (0.00) |
| Altruistic Motive $\kappa$ | 2.99*** (0.29) | – Log Magnitude $\alpha_H^0$ | 0.06*** (0.00) | $\xi_{\text{complaint}}$ [sd] | 0.08 [15.53] | $\delta_1$ | 2.36*** (0.48) |
| | | | | $\xi_{\text{treat}}$ [sd] | 0.05 [0.07] | $\sigma$ | 2.78*** (0.03) |

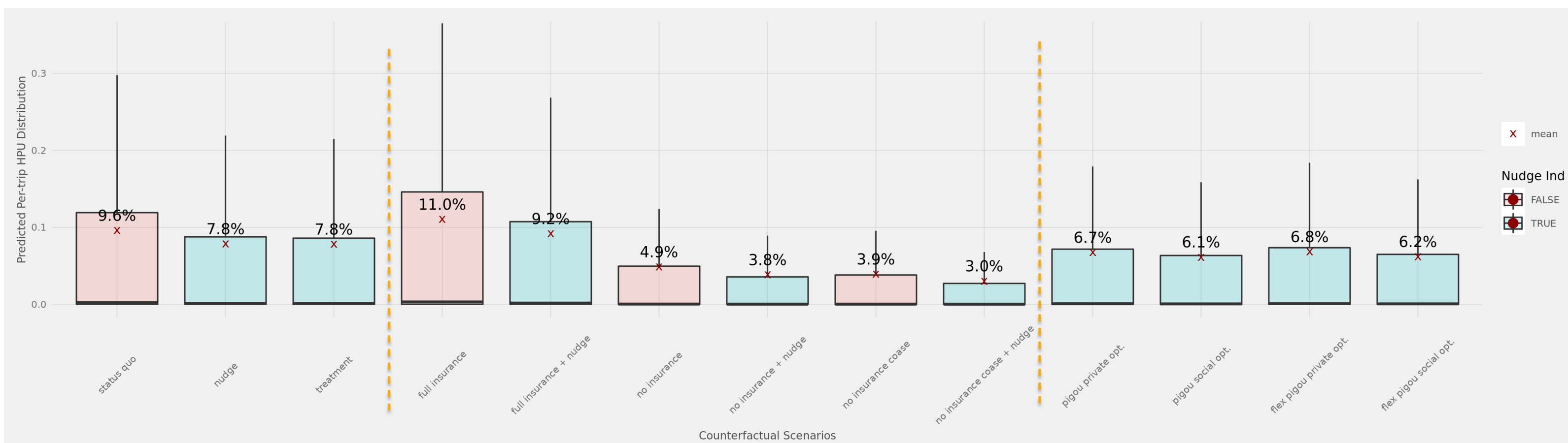
*Notes:* This table shows our main estimation results. Latent parameters are those with square brackets: mean and standard deviation are shown before and in the brackets. We also show estimates of key primitives and the corresponding bootstrap standard error. For the nudging effect, the "Converted Mean" shows the percentage increase in implicit price perception in absolute terms as opposed to in log differences. 'lux': indicator for luxury cars; 'popdens': population density; 'reqdens': passenger request density at the pickup location. The rightmost two vertical columns shows the flexible HPU pricing rules that correspond to our counterfactual simulations of private and social optima.




(a) Fit and CV - Handheld Phone Use Frequency




(b) Fit and CV - Log Handheld Phone Use Frequency




(c) HPU Distributions and Means under Counterfactual Contracts

Figure 4: Estimation Result and Counterfactual Contract Simulation

*Notes:* There figures report estimation results and counterfactual simulations. (a) plot model fit (yellow line green region) on top of data (shaded area) for both the training and validation samples. Our training sample consists of 80% of trips in the experimental sample. All remaining trips are used for validation. (b) plots the fit and cross validation of log HPU magnitude conditional on HPU being positive. (c) plots the distribution (boxes and lines) and the means (red crosses) of counterfactual HPU under various counterfactual scenarios/contracts. The first group focuses on the effect of nudges, the second group looks at the effect of insurance change, and the third group includes direct HPU prices and corresponds to the "first-best" private optima and social optima that internalizes additional externalities on traffic congestion and injuries. Red and blue colors distinguish whether the counterfactual contract in each scenario includes nudging or not. See Section 6 for more detail.

# 6 Counterfactual and Optimal Contract

We now conduct counterfactual simulations to study optimal contract design. We conduct three groups of simulations to quantify the impact of nudges, large insurance changes, and to calculate the direct HPU prices needed to "first-best" *a la* Holmström (1979).

The first group of counterfactuals are shown in the left panel of Figure 4(c). Starting from the **"status quo"**, in which we assume no treatment was conducted, we simulate two scenarios with intervention to all drivers: **"nudge"** corresponds to a nudge-only treatment; **"treatment"** is the experimental treatment in Section 4. Similar to our prior estimates, the treatment reduces HPU by 18%, but the vast majority of the reduction comes from the nudge.

The second group of counterfacutals focus on insurance and risksharing (middle panel of Figure 4(c)). **"full insurance"** and **"no insurance"** scenarios are self-explanatory. With full insurance, we eliminate the deductibles. In the no-insurance scenario, we assume that drivers pay all of the counterfactual claims out-of-pocket and can not insure elsewhere.[16][17] The **"no insurance Coase"** scenario forces drivers to pay all negative externalities in an accident. This is the sum of the average traffic congestion time cost ($2,059) plus all non-financial costs suffered by passengers, pedestrians, and other drivers ($6,541).[18] To better visualize the degree to which inattention leads to inefficiently high HPU and hence accidents, for each of the three counterfactuals in this group, we calculate a scenario with and another without nudges. For example, **"full insurance + nudge"** shows that a simple nudge can almost compensate for the moral hazard effect induced by completely eliminating deductibles.

The third group recovers "first-best" contracts *a la* Holmström (1979) with direct HPU prices, whose effect is isomorphic to that of a Pigouvian tax. **"Pigou private opt."** and **"flex pigou private opt."** use a uniform HPU price and a trip-specific HPU price to recover the private optimum, respectively. Their counterparts for the social optimum are **"pigou social opt."** and **"flex pigou social opt."**. For all scenarios in this group, we take a three-step approach. First, we start from "full insurance + nudge", which has the most efficient

[16]One caveat that may bias our results upwards is that we maintain our assumption that drivers expect deterministic loss conditional on accidents, which may be less appropriate in the "no insurance" scenario.

[17]We are unable to disclose the exact magnitude of claim, but the magnitude is over $10,000.

[18]See NHTSA (2015b). The latter part is calculated similar to Equation 14.

risk-sharing and the nudging effect inherent to direct behavioral prices. Second, we find the optimal *HPU levels* that our Pigouvian prices aim to recover. The optimal behavior maximizes drivers' private benefit (from HPU) minus all associated costs. For a risk-neutral driver, the optimal HPU levels in private and social optima correspond to the "no insurance + nudge" and the "no insurance Coase + nudge" scenarios. We thus use hypothetical risk-neutral drivers to re-calculate HPU levels in those two scenarios. Third, we search for the direct HPU prices that help us achieve the optimal HPU benchmarks in step 2.[19] Lastly, we assume the insurer is able to pass all insurance cost as well as revenue raised from HPU charges back to the drivers at actuarially fair rate.[20] We also abstract away from participation constraints.

When charging a uniform HPU price, \$0.77 per HPU mile (traveled while holding phones) is enough to recover the private optima, and \$1.20 is needed for the social optima. This translates into 5.4 and 6.7 cents and per mile driven on average based on the corresponding reduced HPU levels they induce. A more sophisticated insurer may want to charge personalized (and trip-specific) HPU prices based on driver- and trip-level characteristics. We assume the firm can charge an HPU price that is linear in characteristics with a zero-lower bound. The optimal charges are reported in Table 2. The superiority of personalized pricing is easy to see - it targets risky trips, particularly precipitation and non-passenger ones, as well as drivers that are sensitive to prices and those for whom nudging alone would not be as effective: those with lots of driving/trip experience, for instance. The overall result is striking: for the average trip, a 40-cent charge per HPU mile is enough to recover the private optima "first-best" contract, and a 62-cent charge can recover the social optima.

Optimality does not mean that HPU is minimized. We see higher HPU in optimal scenarios compared to "no insurance + nudge" or "no insurance Coase + nudge". This represents a fundamental increase in total surplus due to the resolution of moral hazard, in which "first-best" risk-sharing facilitated by the behavioral price allows more welfare-positive riskbearing (HPU). Nonetheless, with our socially optimal HPU price, even with full insurance, 2% accidents can be prevented by deterring HPU alone.[21]

[19] We optimize based on a sum-of-squared-errors loss function.

[20] This is a reasonable simplification for our setting. The insurer do not make money from insurance and charges a per-mile price for drivers based on historical claim costs.

[21] Based on the overall percent drop in HPU frequency and our accident elasticity estimates in Table A.1.

## Conclusions

In this paper, we obtain novel data on driving behavior to directly answer two questions: the magnitude of "ex-ante" moral hazard and the importance of inattention in shaping risky driving and hence accident risk. Focusing on handheld phone use (HPU) while driving, the central finding is that inattention to risk is prevalent, limiting moral hazard while inducing inefficiently high HPU and hence accident risk.

These findings have important implications on the design of auto insurance contract. Instead of raising accident punishment–essentially reducing insurance–to deter risky driving, raising attention to or directly pricing on such behavior are far more effective.

In order to simulate the optimal insurance contract *a la* Holmström (1979), we develop a structural model that focuses on identifying three key parameters: HPU's price elasticity, risk aversion, and a nudging effect that is inherent to any direct behavioral prices. When HPU prices can vary flexibly with driver and trip characteristics, a 40-cent average charge per mile of HPU plus full insurance can achieve the "first-best;" a 62-cent charge can further resolve congestion and injury externalities.

We end by discussing broader implications of our results on other dimensions of insurance and nudge design. First, there is no insurance plan choice in our setting. However, heterogeneity in the HPU price elasticity suggests that nudges may trigger adverse selection when insurance choices are available (Handel 2013). In this case, a coverage-dependent personalized HPU price should be used to curb adverse selection (on observables).

Second, we rely on a machine-learning algorithm to translate noisy phone sensor data to HPU behavior. An important decision is choosing a point on the "precision-recall frontier": higher precision trades false positives for false negatives and is thus considered as more conservative. Our findings suggest that drivers are quite risk averse, but simple nudges can trigger large attention effect essentially "for free." Therefore, the optimal precision-recall choice is a mix: nudges should rely on ML algorithms with high recall, while direct prices on HPU should only be charged on precisely-identified occurrences. Lastly, we do not model heterogeneity in risk aversion for simplicity, but attention and risk aversion compound to deter risky behavior. When determining the timing of nudges, targeting accidents or near-accident events such as extreme harsh-brakes, which are shown to temporarily increase risk preference, can be especially effective (Deng, Hu, and Zhu 2018; Shum and Xin 2019).

Of course, the cause of accident risk is diverse, and handheld phone use is but one risk factor. At the same time, "ex-post" moral hazard on insurance utilization remains an important reason to limit coverage so as to contain unnecessary spending on accident remedies. However, as phone sensors and surveillance become ubiquitous, and with increasing standardization of car repair and injury treatment procedures, there is a real possibility for insurers to identify all major risky behavior and move towards full insurance.

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# A Additional Figures and Tables

Table A.1: Robustness - HPU Risk Model

| | Specification | Accident | Safety Complaints |
|---|---|---|---|
| 1 | OLS | -0.074 | -0.404 |
| | | (0.017) | (0.052) |
| 2 | 1 + trip & driver controls + location/time fixed effects | 0.034 | 0.032 |
| | | (0.019) | (0.007) |
| 3 | 2 + post-accident filter | 0.056 | 0.033 |
| | | (0.019) | (0.007) |
| 4 | 3 + Driver fixed effects | 0.060 | 0.021 |
| | | (0.021) | (0.008) |
| 5 | 3 + Driver-month-location-pair fixed effects | 0.042 | 0.010 |
| | | (0.021) | (0.008) |

*Notes:* This table reports estimates from our observational estimates of the riskiness of HPU. The results are presented in elasticity terms. 'OLS' shows the coefficient of regressing accident or safety complaints directly on trip-level HPU. Location fixed effects use the pair of start and finish geohash5 codes for each trip. Time fixed effects include hour-of-day, day-of-week, month-year dummies. Post-accident filter removes all HPU records that occurs after accidents for all accident trips.

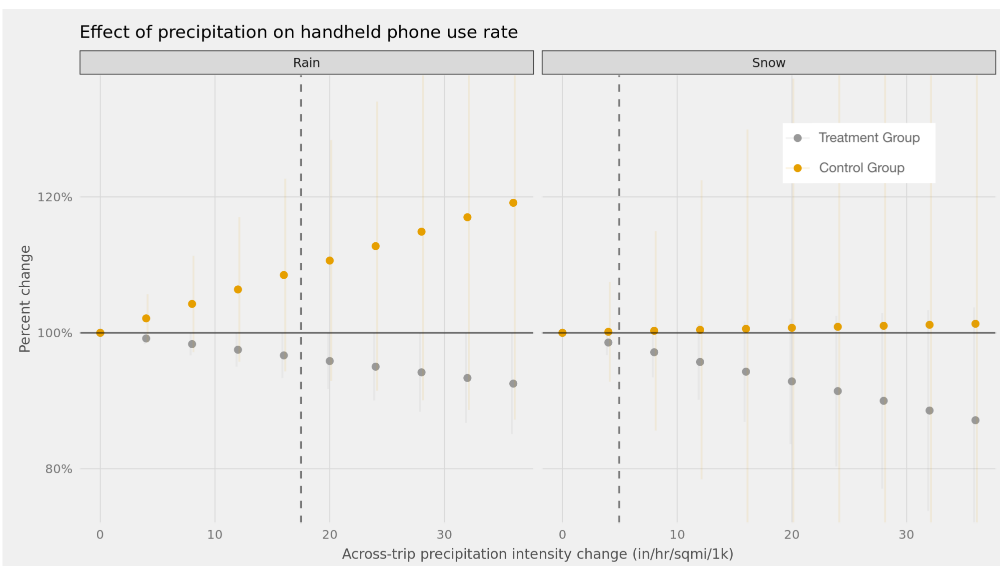



Figure A.1: HPU impact of across-trip precipitation change by phase

*Notes:* This graph reports key coefficients of the regressions of HPU (handheld phone use frequency) on precipitation by treatment status. It shows across-trip precipitation change, for rain and snow weather (left vs. right panels), for the treatment and the control groups. Control variables include demand and company/dispatch factors such as ride type and passenger request density at trip start time and location (geohash6), as well as driver-start-geohash5-end-geohash5-month fixed effect.

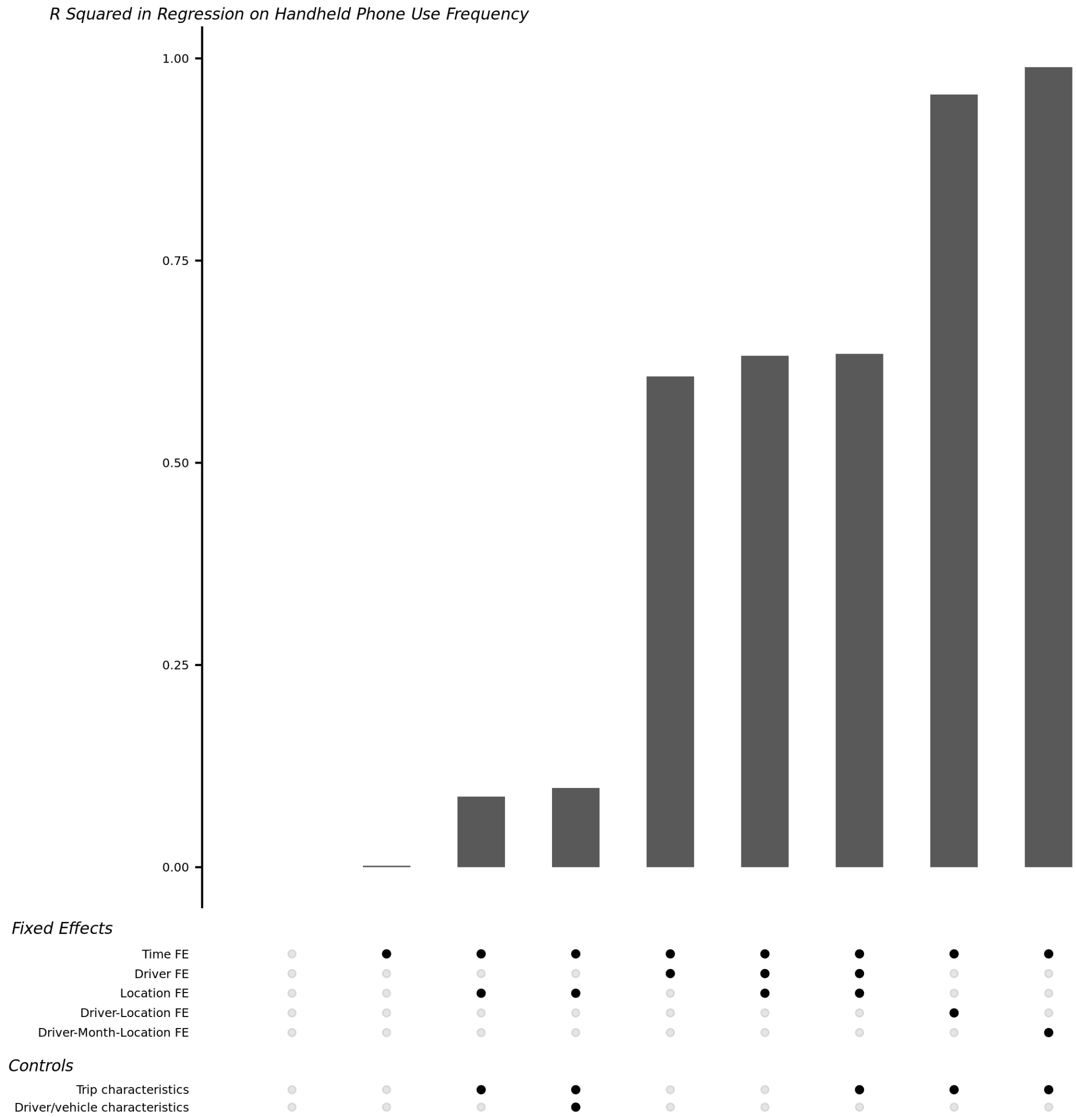



Figure A.2: Robustness Checks - R Square of HPU Regressions

*Notes:* This graph is a robustness check for our regression in Equation 2. It also helps visualize the marginal explanatory power of including various covariates and/or fixed effect.

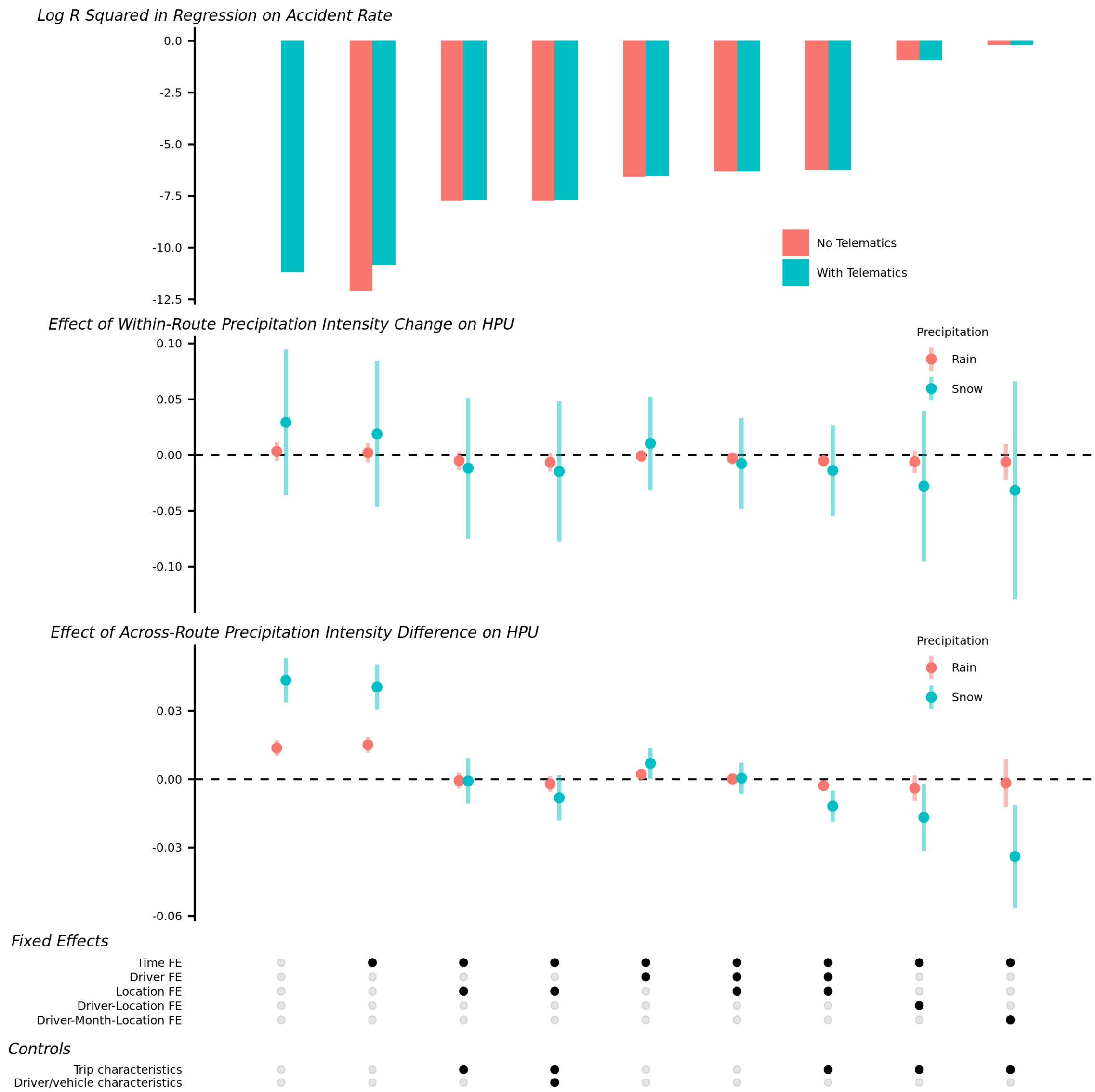



Figure A.3: Robustness Checks - Reduced-Form Analyses

*Notes:* This graph presents robustness check for our regressions in Equation 1 (top panel) and in Equation 12/Figure 1. It helps visualize the marginal explanatory power and impact of including various covariates and/or fixed effect. The mid panel corresponds to the impact on HPU from across-trip precipitation differences. The bottom panel shows the impact of within-trip precipitation change.

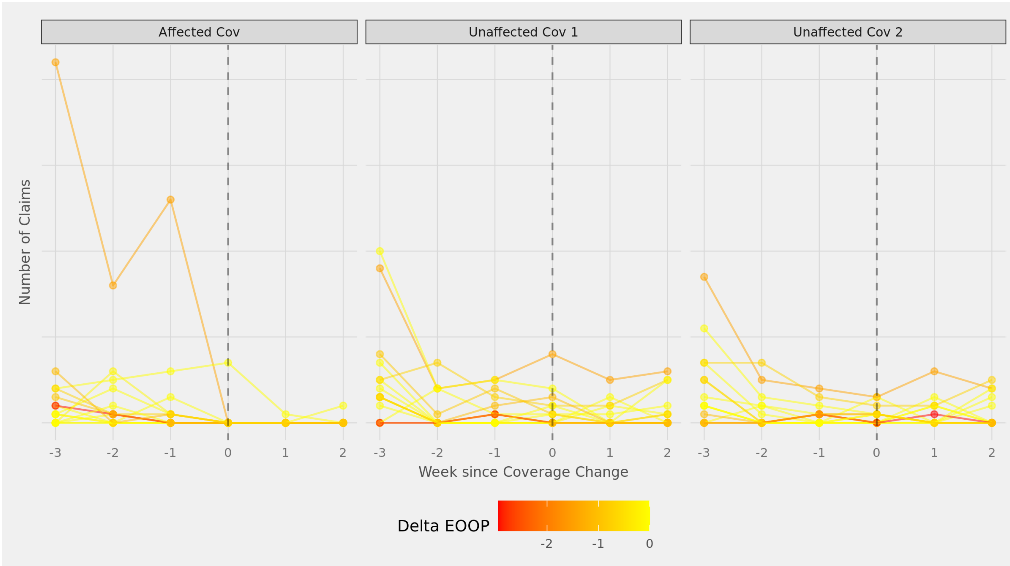



Figure A.4: Insurance Change: Claims Progression by State and by Coverage

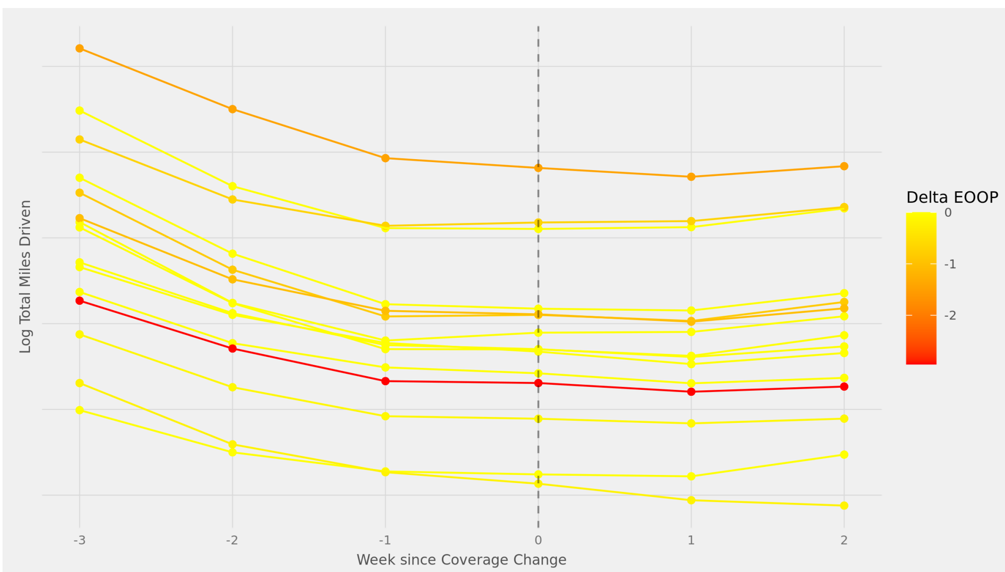
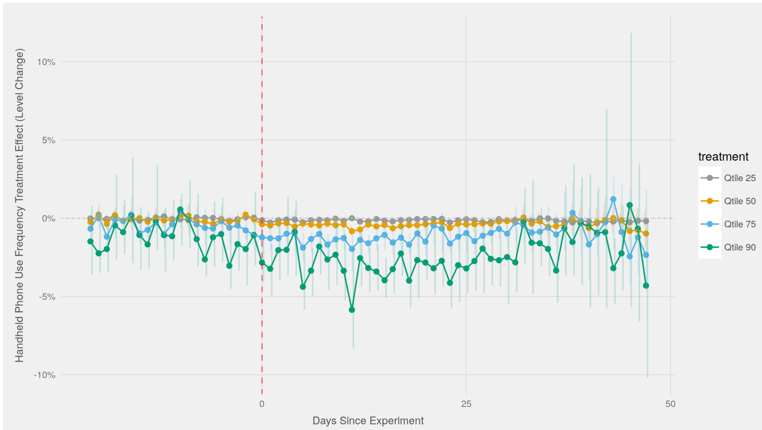



Figure A.5: Insurance Change: Miles Driven Progression by State

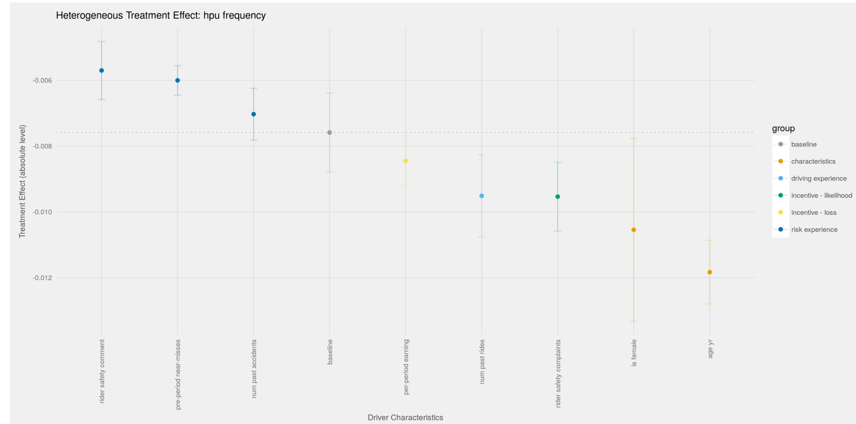
*Notes:* These graphs describe case 1 of the insurance change analyzed in the reduced form section, in which the affected coverage was removed at week zero (Fig A.4). Different lines represent different states, while their color represent the treatment intensity: Delta EOOP means the change in expected out of pocket expenditure per trip due to the insurance change. States/lines that are colored yellow were unaffected by the insurance change. Both claim counts and miles driven suffer time trends due to macroeconomic and public health events but both have largely stabilized two weeks before the insurance change.

# B Heterogeneous and Quantile Treatment Effect

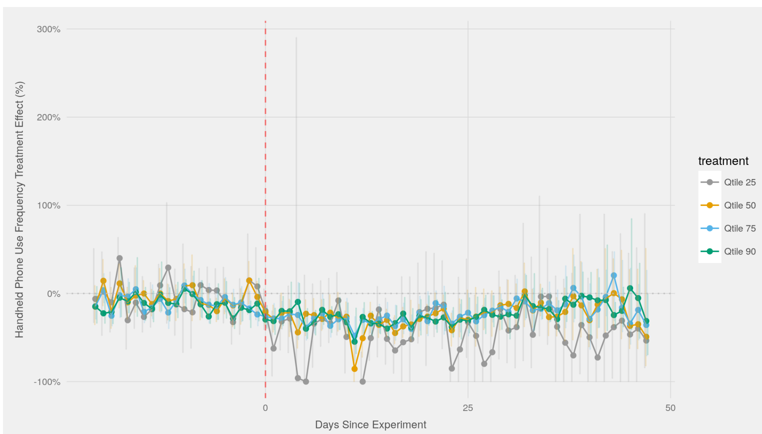
We explore how the treatment effects differ by driver observable characteristics. We regress HPU on the interaction between driver-level heterogeneity variables and the treatment indicator. Therefore, each heterogeneous treatment effect is derived conditional on the other factors. We focus on three groups of observable characteristics that are intuitively linked to elements of our main framework. The "incentives" group includes pre-treatment daily earning and existing safety complaints that increase suspension likelihood. Unsurprisingly, these strongly increase the magnitude of treatment and moral hazard effects. The "preference/characteristics" group includes driver age and gender that are notably linked with risk preference and the private benefit of phone use. Older and female drivers experience larger treatment effects, consistent with them being more risk averse and having lower private benefits from phone use. The "risk experience" group includes past accidents and preexisting number of passenger safety comments, as well as near-misses identified by severe harsh braking events during the two-week period before treatment. These factors may increase drivers' attention to risky behaviors and their risk aversion Shum and Xin 2019. Consistent with that hypothesis, the estimate show that risk experience reduces the magnitude of the treatment effect. Conversely, the more "driving experience" a driver has conditional on the same risk experience, the larger the treatment effect is.



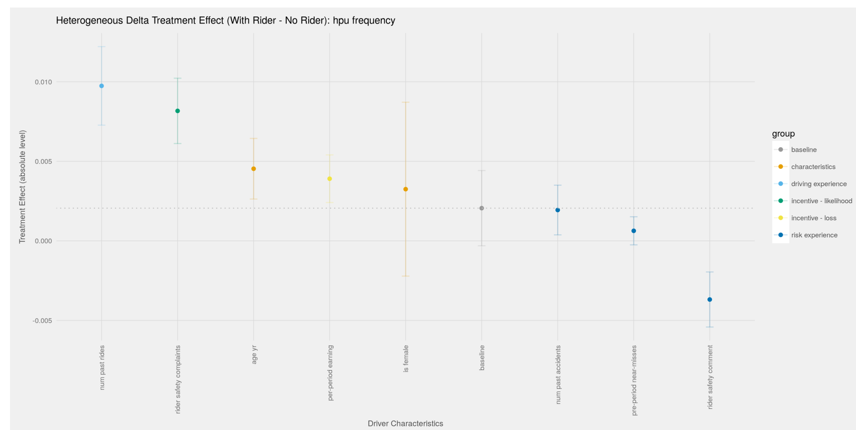

(a) HPU Quantile Treatment Effects




(c) Het. Treatment Effects by Driver Char.




(b) HPU % Quantile Treatment Effects




(d) Het. Differential TE by Driver Char. (w/ - w/o Passenger)

Figure B.6: Heterogeneous Treatment Effects

*Notes:* These figures report heterogeneous treatment effects in our direct intervention experiment. Quantile treatment effects in handheld phone use frequency (HPU) are reported in levels in (a) (5% means a 0.05 drop in HPU frequency) and in percentage terms in (b) by dividing the average pre-treatment levels of HPU for each quartile. (c) and (d) reports report heterogeneous treatment effects on HPU frequency by observable driver features. In (c), the coefficients reported are the ones on the interaction between treatment indicator, post treatment indicator, and the corresponding driver feature. Controls are the same as the main regression, including driver fixed effects. (d) adds passenger indicator to the interaction term. The coefficient reports how the treatment effect differ with and without passengers. In (c) and (d), driver characters are selected to represent key factors in the structural model. Specifically, "incentives" includes pre-treatment daily earning and existing safety complaints that increase suspension likelihood. "Risk experience" includes past accidents and passenger safety comments, as well as near-misses identified by severe harsh braking events.