Peer Information at Work: Evidence from Two Field Experiments*

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Abstract

I study how recurring peer earnings disclosure affects labor supply using two large-scale field experiments with over 3,000 ride-hailing drivers. During each six-week intervention, drivers received twice-weekly updates on their own earnings, and I randomized whether these updates also reported the average earnings of ten similar drivers. I repeated the intervention about eight months later under substantially weaker market conditions. An incentivized baseline measure of demand for peer information predicts who responds in both experiments: peer information has sizable effects among information-seeking drivers, while non-information-seeking drivers show no detectable response. Among information-seeking drivers, peer information increases platform labor supply by 6% in the first experiment but reduces it by 5% in the second. By contrast, cross-randomized monetary incentives increase labor supply similarly for both types. I present a signal-extraction framework in which peer earnings help workers infer shared market conditions and thus the returns to work. Using independent trip-request data, I show that peer earnings contain information about unexpected market conditions beyond drivers' own earnings. Consistent with market learning, the effects of peer information become more positive when conditions are unexpectedly favorable but vary little with predicted conditions. Information demand predicts *who* responds, while what peer earnings reveal about shared earning opportunities helps explain *how* they respond.

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1 Introduction

Information about coworkers' pay or performance, which I refer to as *peer information*, is widely used as a management tool to motivate workers.[1] Such disclosures can meaningfully shape effort and labor supply (Cullen, 2024; Villeval, 2020). Yet peer information can operate through different channels, making it difficult to predict which workers will respond and in what direction. Prior work has largely interpreted these effects through social comparison, emphasizing concerns about fairness, status, and competition (see, e.g., Card et al., 2012; Breza et al., 2018; Cohn et al., 2014; Charness et al., 2014). Peer outcomes, however, can also serve a distinct informational role: they can reveal prevailing economic conditions and help workers infer the returns to work. This role is especially relevant in jobs with performance-based pay, which have become increasingly prominent with the growth of platform-mediated work.[2] In such settings, workers choose how much labor to supply as shared earning opportunities fluctuate over time and are imperfectly observed. By helping workers better assess these opportunities, peer information may improve labor allocation, with implications for organizational efficiency.

In this paper, I test this informational channel among ride-hailing drivers, who operate under a common pay structure and have substantial flexibility over whether, when, and how much to work. Their realized earnings reflect both individual performance and shared earning opportunities. I refer to this shared, time-varying component of the returns to work as *market conditions*. A driver observing only their own earnings must infer whether low earnings reflect weak market conditions, their own performance, or simply bad luck. Because other drivers on the platform share exposure to market conditions, their earnings provide an additional signal about the shared component of returns. Peer earnings disclosure can therefore increase or decrease labor supply depending on whether peer outcomes convey positive or negative news about the returns to work. I ask whether workers use peer information to learn about market conditions, and whether the direction of their labor supply response depends on what peer earnings reveal.

The effects of peer information may depend not only on what the information reveals but also on workers' preferences for receiving it. Recent work documents substantial heterogeneity in demand for peer information and suggests that elicited demand can pre-

[1] For example, Whole Foods historically maintained an open-book salary policy, while General Electric famously used a relative-performance ranking system known as the "vitality curve" under Jack Welch. Today, Salesforce markets sales-analytics tools, including leaderboards and team-benchmark dashboards.

[2] Performance-based compensation arises in a broad class of jobs, including commission sales and piece-rate production (Gittleman and Pierce, 2013; Lemieux et al., 2009). Recent evidence documents substantial growth in platform-mediated gig work, with the number of platform gig workers in the U.S. tripling between 2019 and 2023 (Garin et al., 2025).

dict subsequent responses to feedback (Lim, 2025; Tekleselassie et al., 2025). This raises a second question: can workers' ex ante demand for peer information identify who will respond to recurring disclosure, and does it remain predictive across implementations under different market conditions? The latter is especially relevant because workplace feedback often recurs as economic conditions change. A measure elicited once that remains predictive over time could provide organizations with a simple tool for identifying responsive workers and tailoring subsequent disclosure.

I address these questions using two large-scale field experiments involving over 3,000 drivers, conducted in partnership with TADA, a major ride-hailing platform in Singapore. The experiments implemented recurring peer earnings disclosure with the same underlying driver cohort approximately eight months apart, but under substantially different market conditions. Among drivers who expressed demand for peer information at baseline, disclosure increases labor supply in the first experiment and reduces it in the second, when market conditions were weaker. Other drivers show little response in either. Using within-experiment variation, I show that the effects of peer information among these responsive drivers become more positive when realized market conditions are better than expected, consistent with market learning. Thus, information demand predicts *who* responds, while what peer earnings reveal about shared earning opportunities helps explain *how* they respond.

Each field experiment comprised a six-week intervention implemented as part of "TADA Insights," a new program launched with TADA to better support drivers' needs and earnings. Experiment 1 ran from September to November 2025, and Experiment 2 from June to July 2026. In both experiments, all participating drivers received twice-weekly updates on their own cumulative trip earnings. At the driver level, I randomized whether these updates also reported the average earnings of a randomly assigned reference group of ten drivers with similar baseline work hours. The design thus generates exogenous variation in the peer earnings benchmark observed by otherwise comparable drivers. Reference-group drivers were drawn from outside the experimental sample and did not receive the intervention, so the disclosed peer earnings did not reflect treatment responses by those drivers. The earnings updates were delivered by email, with push notifications and in-app notices alerting drivers when each update became available.

Before treatment assignment in Experiment 1, I elicited drivers' demand for peer information using an incentive-compatible mechanism (Butera et al., 2022; Lim, 2025). As part of the onboarding survey, drivers indicated whether they wanted to receive peer earnings information during the program, preferred not to receive it, or were indifferent

under three scenarios in which their past earnings were above, similar to, or below their peer group average. In Experiment 1, with one-third probability, the driver's stated choices determined whether they received peer information. With the remaining two-thirds probability, receipt of peer information was randomized independently of their choices. This design provides an ex ante measure of information demand while preserving random assignment of peer information for estimating treatment effects.

In Experiment 2, I re-randomized peer information assignment within the same underlying driver cohort about eight months later, when market conditions were substantially weaker. Drivers' information demand was re-elicited only after the intervention, in the endline survey. The second implementation therefore tests whether the baseline measure remains predictive of responsiveness to peer information in a different market environment, while the endline elicitation provides a separate measure of the temporal stability of information demand.[3]

A potential concern is that drivers who seek peer earnings information may simply be more attentive to platform communications or have more elastic labor supply. To distinguish these explanations from responsiveness specific to peer information, I cross-randomized monetary incentives with the peer information treatment in each field experiment. Each driver received each incentive type once, with the incentive weeks and order randomized across drivers. In Experiment 1, these consisted of a 10% increase in trip earnings or a fixed bonus for reaching a daily trip target, both offered over a full day. In Experiment 2, they consisted of 10% and 20% increases in trip earnings during a four-hour window.[4] The incentives were communicated through the same channels as the earnings updates, making them a useful benchmark for assessing these alternative explanations.

I present three main sets of results. First, baseline information demand consistently identifies which workers respond to peer earnings disclosure in both field experiments. As preregistered, drivers are classified as *info-seeking* if they express strict positive demand for peer information in at least one of three relative-earnings scenarios. At baseline, 52% of drivers fall into this group. The remaining drivers are classified as *non-info-seeking*; in each scenario, they are either indifferent or willing to pay to avoid receiving it. Peer information produces sizable labor supply effects among info-seeking drivers in both experiments, while non-info-seeking drivers show no detectable effect in either.[5] These effects differ

[3]The endline elicitation was unincentivized. Drivers were asked whether they would want peer earnings information under the same three scenarios if the firm introduced in-app earnings updates in the future.

[4]The incentive designs differed across experiments because they were also developed to address a separate research question on income targeting, which I study in a forthcoming companion paper.

[5]Further splitting the non-info-seeking drivers into those who are always indifferent and those who express strict negative demand in at least one scenario yields similar treatment effects across the two

significantly between the two information-preference types within each experiment.

As an important validation, cross-randomized monetary incentives increase labor supply on the platform similarly among info-seeking and non-info-seeking drivers in both experiments, even though these incentives are communicated through the same channels as the earnings updates. Information demand itself also exhibits meaningful temporal stability. At the Experiment 2 endline, nearly one year after the original elicitation, baseline info-seeking drivers were 27 percentage points more likely than baseline non-info-seeking drivers to seek peer information again. Together, these results show that an incentivized measure of information demand elicited once at baseline captures a meaningful and temporally stable dimension of heterogeneity in responses to peer information, even when disclosure is repeated under substantially different market conditions.

Second, the direction of the labor supply response to peer information among info-seeking drivers reverses across the two experiments. My preregistered primary measure of labor supply on the platform is daily utilization hours, which measure time spent serving trips from trip acceptance through drop-off. Peer information increases platform labor supply among info-seeking drivers by about 6% in Experiment 1 but reduces it by about 5% in Experiment 2. The margin of adjustment also differs across experiments. In Experiment 1, I find no detectable change in the probability of working on the platform; instead, drivers work more intensively conditional on working, completing more trips and earning more. In Experiment 2, peer information reduces the probability of working on the platform by 2.2 percentage points, with no detectable changes in utilization hours, trips completed, or earnings conditional on working.

One competing explanation for this reversal is that drivers faced different relative earnings positions across experiments and responded differently depending on whether they earned above or below their peers. Complementary tests exploiting variation in drivers' relative earnings provide little support for this account as the primary explanation.

The two field experiments also took place under markedly different market conditions. Using trip-request data from a fixed cohort of drivers outside the experimental sample, I construct an independent measure of market conditions, which I call *trip-request availability*, based on the daily volume of unique passenger trip requests shown to these drivers. This measure was about 20% lower during Experiment 2, and the peer earnings disclosed to treated drivers were also lower. These differences are directly relevant to market learning: if workers use peer earnings to assess shared earning opportunities, the effects of disclosure should depend on what those earnings reveal about market conditions.

subgroups; see Appendix Section A.10.

To formalize this inference problem, I develop a simple learning framework in which drivers use their own and peers' earnings to assess the returns to work. Own earnings provide a noisy signal of market conditions. Peer earnings provide an additional signal of market conditions, but because drivers can differ in how they work, they may also be informative about the returns to alternative work strategies. Drivers therefore face a signal extraction problem: higher-than-expected peer earnings may reflect more favorable market conditions, more effective strategies, or idiosyncratic shocks. The framework distinguishes learning about shared market conditions ("market learning") from learning about the effectiveness of alternative work strategies ("strategic learning"). The central prediction for market learning is that the labor supply effects of peer information become more positive when market conditions are better than expected. A complementary prediction for strategic learning is that peer information is more likely to induce changes in work strategy when peer earnings are higher relative to the driver's own earnings.

The third set of results provides evidence consistent with market learning. Exploiting variation in market conditions across earnings-update episodes within each experiment, I find that treatment effects among info-seeking drivers become more positive as market conditions improve. A one standard deviation improvement in market conditions, corresponding to about 13% greater trip-request availability, is associated with a 0.04-hour increase in the treatment effect on daily utilization hours. Among non-info-seeking drivers, the corresponding treatment gradient is close to zero and not statistically significant.

To test the framework's market-learning prediction more directly, I separate realized market conditions into expected and unexpected components. Under rational expectations, drivers' expectations already incorporate predictable variation in market conditions. I therefore forecast trip-request availability using only information available before each earnings update, treating the forecast as a proxy for expected market conditions and the forecast error as the unexpected component, which I call the *market surprise*. Peer earnings strongly predict these market surprises even after controlling for drivers' own earnings and are substantially more predictive than own earnings. Thus, peer earnings contain additional information about market surprises beyond drivers' own earnings.

Among info-seeking drivers, the effect of peer information becomes substantially more positive following favorable market surprises. A market surprise corresponding to trip-request availability about 13% higher than forecast is associated with a 0.18-hour increase in the effect on daily utilization hours. By contrast, the treatment gradient with respect to predicted market conditions is close to zero, and equality of the two gradients is strongly rejected. Among non-info-seeking drivers, the market-surprise gradient is also close to

zero. Together, these findings provide strong evidence consistent with market learning.

As a complementary test, I find little evidence for strategic learning: peer information does not significantly affect whether drivers change when or where they work relative to their usual work patterns, nor does this response become more pronounced when peer earnings are higher relative to their own earnings.

Overall, this paper has implications for the design of workplace information policies. First, peer information can complement monetary incentives in shaping labor supply. Whereas incentives directly increase the returns to work, peer information can help workers infer those returns when they face shared demand or production conditions that are imperfectly observed. By making labor supply more responsive to underlying earning opportunities, peer information may improve the allocation of labor across changing market conditions. Second, organizations should account for heterogeneous information preferences rather than adopt a one-size-fits-all approach to disclosure. A nontrivial share of workers are willing to pay to avoid peer information.[6] Uniform disclosure of peer information may therefore impose direct utility costs on these workers, suggesting a role for using elicited preferences to tailor subsequent disclosure.[7]

This paper makes three contributions to the literature. The main contribution is to provide, to my knowledge, the first field evidence that peer earnings disclosure can affect labor supply by helping workers learn about shared, time-varying earning opportunities. This contributes to two related literatures on pay transparency and relative performance feedback (see Cullen, 2024; Villeval, 2020, for reviews, respectively), and relates more broadly to research on social incentives in organizations (Ashraf and Bandiera, 2018). A prominent focus is how information about coworkers' pay or performance affects behavior through fairness, status, and social-comparison concerns (Card et al., 2012; Breza et al., 2018; Blanes i Vidal and Nossol, 2011; Charness et al., 2014; Huet-Vaughn, 2025). A close precedent is Barankay (2012), who considers whether rank feedback helps commission salespeople filter common shocks and infer market demand, but finds evidence more consistent with status and self-image concerns. Distinguishing these channels is empirically challenging because higher peer earnings can simultaneously signal more favorable market conditions and worse relative performance.

[6] In my setting, 24% of drivers are willing to give up a small monetary stake to avoid receiving peer information in at least one of the three scenarios. This includes 40% of non-info-seeking drivers, equivalent to 19% of the full sample.

[7] Relatedly, Butera et al. (2022) show that public recognition can impose significant utility losses on low-performing individuals through feelings of shame, highlighting the importance of considering net welfare effects rather than behavioral responses alone.

I make progress on this distinction using randomized variation in peer earnings benchmarks and an independent measure of market conditions. Tests exploiting variation in drivers' relative earnings provide little support for relative performance concerns as the primary explanation for the cross-experiment pattern. By contrast, peer earnings strongly predict market surprises beyond drivers' own earnings, and treatment effects among info-seeking drivers vary substantially more with unexpected than predicted market conditions. Together, these results show that learning about shared market conditions can be first-order in shaping labor supply responses to peer earnings disclosure.

Second, this paper contributes to the literature on endogenous information acquisition and avoidance (see Golman et al., 2017, for a review). One strand documents substantial heterogeneity in demand for performance, ego-relevant, and social comparison feedback (Eil and Rao, 2011; Möbius et al., 2022; Castagnetti and Schmacker, 2022; Coffman and Klinowski, 2025), including in field settings involving resource use and education (Allcott and Kessler, 2019; Andor et al., 2023; Castagnetti and Rury, 2026). The closest workplace precedent is Cullen and Perez-Truglia (2022), who document substantial willingness to pay (WTP) for information about coworker and manager salaries at a commercial bank. Their elicitation, however, does not capture information avoidance. Relatedly, recent work asks whether elicited information demand predicts subsequent responses to feedback (Lim, 2025; Tekleselassie et al., 2025). To my knowledge, prior work has not examined demand for and avoidance of peer earnings information under performance-based pay. I contribute by showing that workers' ex ante preferences for this information strongly predict which workers respond to recurring disclosure and retain predictive power over time. Methodologically, I adapt the incentive-compatible strategy-method elicitation of WTP in Butera et al. (2022) and Lim (2025) into a simple measure of information demand that can be readily implemented in organizations and other field settings.

Finally, this paper contributes to research on labor supply and worker management in platform markets. Existing work on ridesharing studies the value of work flexibility (Chen et al., 2019) and heterogeneity in drivers' labor supply elasticities (Chen et al., 2025b), and examines drivers' responses to compensation schemes (Angrist et al., 2021), financial incentives (Brandon et al., 2024; Allon et al., 2023), outside employment options (Caldwell and Oehlsen, 2023), pay timing and flexibility (Chen et al., 2025a), team contests (Ai et al., 2023; Ye et al., 2022), and feedback about workers' own driving behavior (Athey et al., 2019). I add peer earnings disclosure as a non-monetary information intervention and show that its effects on platform labor supply depend both on workers' demand for the information and on what peer outcomes reveal about earning opportunities.

The remainder of the paper proceeds as follows. Section 2 introduces the rideshare setting and data. Section 3 describes the experimental design. Section 4 presents the main results. Section 5 develops the conceptual framework. Section 6 presents evidence on learning mechanisms. Section 7 concludes.

2 Rideshare Setting and Data

2.1 Study Context

The study is conducted in collaboration with TADA, a major ride-hailing platform in Singapore. Drivers use their own or leased vehicles to provide on-demand transportation services and have substantial flexibility over their labor supply. They decide whether to work on a given day, when and where to work, and how long to remain active on the platform. While active on the app, drivers receive trip requests displaying the fare, trip distance, and pickup and drop-off locations. They can choose whether to accept or skip each request. Appendix Figure A1 shows an example of the trip-request screen (left panel) and the pickup interface after a request is accepted (right panel).

Drivers are compensated on a per-trip basis and retain the trip fare net of a small, fixed platform fee.[8] Their earnings depend jointly on their own labor supply and work strategy and on the earning opportunities available on the platform. The availability of trip requests varies across days and times of day, exposing drivers to fluctuations in shared earning opportunities. Drivers can influence their earnings by working more hours, changing when and where they work, or choosing which trips to accept.

The ride-hailing setting is particularly well suited to studying market learning. Drivers operate under a common pricing and dispatch system and are thus exposed to shared earning opportunities that fluctuate substantially over time. As a result, the earnings of other drivers with similar baseline work hours can contain information about the shared market conditions they face.

The setting also offers several advantages for studying the effects of peer information more generally. First, drivers work largely independently and do not routinely observe other drivers' earnings. Before the study, the firm had not provided its drivers with any form of peer information, so the intervention introduced a new source of workplace information. Second, the decentralized nature of the work limits opportunities for infor-

[8] TADA operates a zero-commission model rather than the traditional commission-based model in which drivers pay a percentage of each fare to the platform.

mation exchange between drivers, reducing the scope for treatment contamination. Third, ride-hailing generates high-frequency administrative records of drivers' earnings and trip activity, making recurring peer earnings updates feasible and allowing me to study labor supply responses to repeated disclosure over time.

2.2 Data Sources

I draw on three main sources of data: administrative driver records, trip-request records, and survey responses. These sources are linked at the driver level using anonymized identifiers. Together, they allow me to measure each driver's platform activity alongside the shared market conditions they face.

Administrative driver records contain information on account status, platform tenure, ratings, and basic demographic characteristics. These records allow me to identify the experimental sample and track individual drivers across both experiments.

Trip-request records provide detailed information on every request shown to drivers, regardless of whether it was accepted. For each request, I observe when it was shown, its pickup and drop-off locations, estimated pickup and trip distances, trip earnings, and the driver's acceptance decision.[9] For accepted requests, I also observe the subsequent trip status, including whether the trip was completed or canceled by the driver or passenger. For completed trips, I additionally observe utilization time, defined as the time from trip acceptance through drop-off. These records allow me to measure drivers' platform activity, trip-acceptance decisions, and work patterns. I also use trip-request records from a fixed cohort of over 10,000 drivers outside the experimental sample to measure shared market conditions.

Baseline and endline surveys supplement the administrative data. Before Experiment 1, the baseline survey elicited drivers' demand for peer earnings information, as described in Section 3.3, and collected information on demographics, driving experience, and work patterns. Following Experiment 2, the endline survey re-elicited information demand nearly one year after the original elicitation and collected information on drivers' experiences during the experiment. I describe the relevant survey modules alongside the analyses that use them.

For each experiment, I aggregate the trip-request records to the driver-day level and combine them with administrative and survey data to construct a harmonized panel

[9]The earnings field records realized earnings for completed trips and estimated earnings for trips that were not completed.

dataset. The Experiment 1 panel covers June 30 to November 2, 2025, and the Experiment 2 panel covers March 30 to July 12, 2026. Each panel includes a pre-intervention (baseline) period and the corresponding six-week intervention period. The resulting panel contains one observation per driver per calendar day, including days with no platform activity.

2.3 Labor Supply Outcomes

Using the trip-request records, I construct daily labor supply measures using the same definitions across both experiments. I use daily *Utilization Hours* as my preregistered primary measure of platform labor supply. This outcome sums the time a driver spends completing trips during the day, measured from trip acceptance through drop-off and expressed in hours. Days with no completed trips are assigned zero utilization hours. This unconditional measure captures whether a driver works on the platform and how much time they spend serving trips on working days.

A limitation of the data is that I do not directly observe the total time each driver spends logged into the app. *Utilization Hours* therefore excludes time spent waiting for trip requests or repositioning between trips and provides a lower-bound measure of total platform labor supply. I use it as my primary measure because it is directly observed and does not require imputing online time from patterns of trip-request activity.

For the extensive margin, *Working* equals one if the driver accepts at least one trip request during the day, regardless of whether it is subsequently completed or canceled. I use this measure to capture daily participation on the platform.[10]

For the intensive margin, I examine *Utilization Hours*, *Completed Trips*, and *Trip Earnings* conditional on working, following the preregistered analysis plan. *Completed Trips* counts the number of trips completed during the day, while *Trip Earnings* sums realized earnings (in S$) from completed trips, excluding additional incentive payouts. Because the decision to work may itself be affected by treatment, these conditional estimates should be interpreted with caution, as conditioning on working may introduce post-treatment selection.

[10] As a supplementary measure, I construct an *Online* indicator equal to one if the driver receives at least one trip request during the day, regardless of whether it is accepted. I use *Working* as my preferred measure of platform participation because it captures active engagement with trips, whereas receiving a request alone does not necessarily reflect a decision to undertake work. The extensive-margin findings are qualitatively and quantitatively similar under this broader definition of platform participation.

3 Experimental Design

I conducted two randomized field experiments with the same underlying cohort of drivers, about eight months apart. In both experiments, drivers received recurring updates on their own earnings, and I randomized whether these updates also included peer earnings information. In Experiment 1, I elicited drivers' demand for peer information before treatment assignment. In Experiment 2, I independently re-randomized peer information assignment without re-eliciting information demand beforehand. The two experiments took place under substantially different realized market conditions. I first document this contrast, then describe the common design features and the details of each experiment. Figure 1 summarizes the timeline and design of both experiments.

3.1 Timing and Market Conditions

Each field experiment comprised a six-week intervention. Experiment 1 ran from September 22 to November 2, 2025, while Experiment 2 ran from June 1 to July 12, 2026. Experiment 1 overlapped with the Formula 1 Grand Prix weekend (October 3–5), whereas the first four weeks of Experiment 2 coincided with Singapore's Term 2 school holidays.

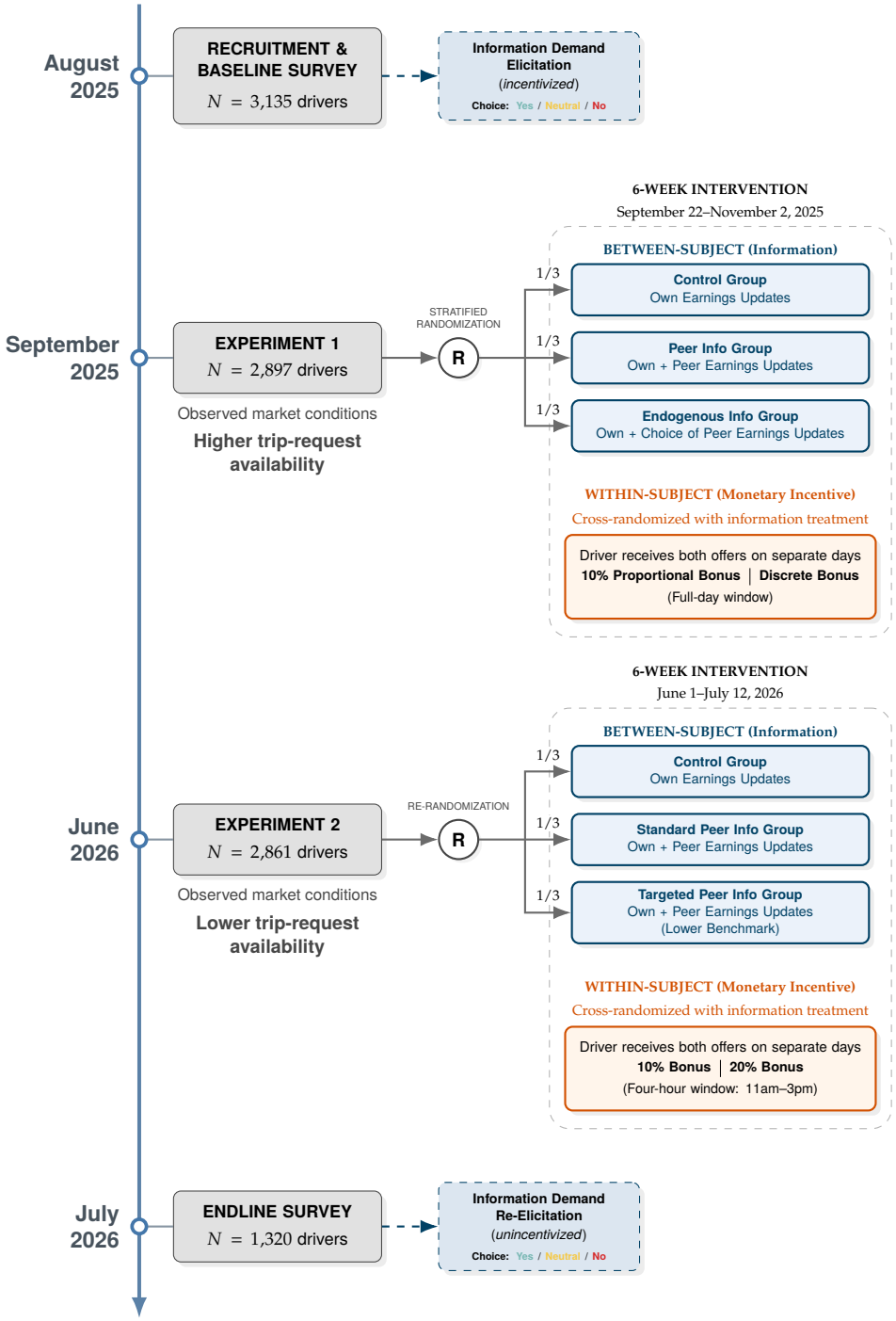
To characterize the shared earning opportunities on the platform, I construct a *Trip-Request Availability Index* from the daily number of unique passenger requests shown to a fixed cohort of over 10,000 drivers outside the experimental sample. I use this index as my primary measure of *market conditions*, proxying for the rate at which trip requests arrive.[11] Higher values indicate that trip opportunities are more readily available. Figure 2 plots weekly averages of this index alongside reference-group earnings, with each series separately normalized to a mean of 100 during the Experiment 1 baseline period.

During the Experiment 1 intervention, the trip-request availability and reference-group earnings indices averaged 107 and 103, respectively, compared with 85 and 80 during the Experiment 2 intervention. Trip-request availability was therefore about 20% lower during Experiment 2, indicating substantially weaker market conditions, while reference-group earnings showed a similar decline.

Market conditions also varied substantially within each experiment. In Experiment 1, trip-request availability rose sharply around the Formula 1 Grand Prix period and subsequently declined. In Experiment 2, availability was particularly low during the school holiday period and recovered toward the end of the intervention. These within-

[11] Section 5 formalizes this notion of market conditions as the common, time-varying component of trip-request arrival rates faced by drivers.

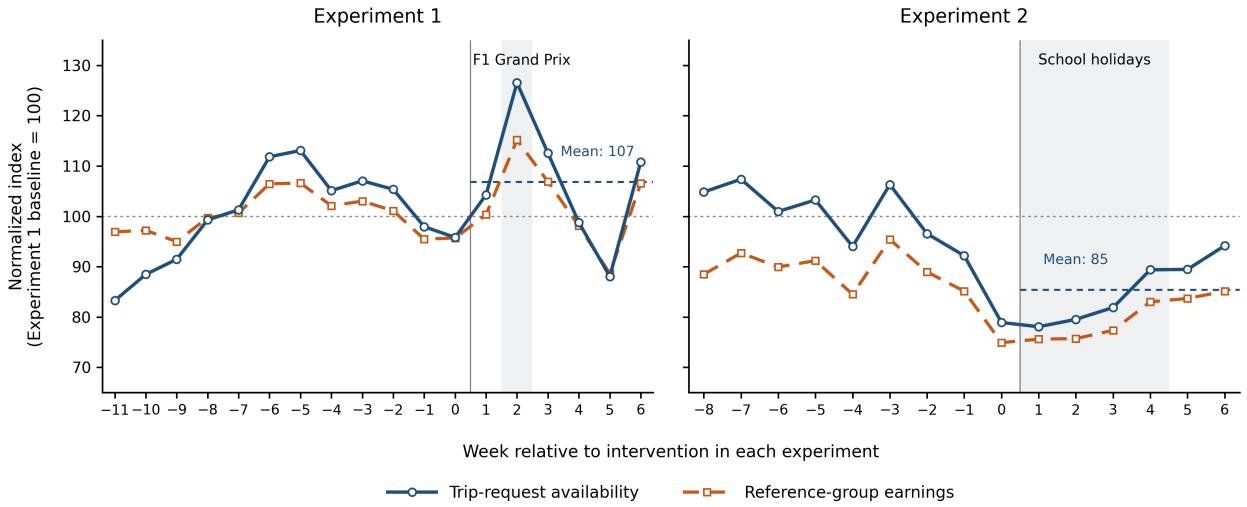
Figure 1: Design and Timeline of the Two Field Experiments



Notes. This figure provides a schematic overview of the design and timeline of the two field experiments.

experiment fluctuations in trip-request availability provide additional variation in market conditions across successive earnings updates.

Figure 2: Trip-Request Availability and Reference-Group Earnings Across Experiments



Notes. The solid line plots the weekly average of the daily number of unique passenger requests shown to a fixed cohort of drivers who participated in neither experiment and were observed during both baseline periods. The dashed line plots average weekly earnings of the reference groups assigned to drivers in the *Control* and *Peer Info* groups in Experiment 1, and the *Control* and *Standard Peer Info* groups in Experiment 2. Reference-group earnings were not disclosed to control drivers. Drivers assigned to the *Targeted Peer Info* group in Experiment 2 are excluded. Each series is normalized to have a mean of 100 during the Experiment 1 baseline period. Week 0 is the final baseline week, and vertical lines separate the baseline and intervention periods. Shaded areas indicate the Formula 1 Grand Prix period in Experiment 1 and the school holiday period in Experiment 2.

As a complementary measure of market conditions, I construct the average time between a trip request that a driver rejects and the next request shown to the same driver. Longer waiting times indicate a lower arrival rate of trip requests. This measure reveals the same cross-experiment pattern: drivers waited substantially longer for another request during Experiment 2 (Appendix Figure A2). The trip-request availability index and this waiting-time measure are strongly negatively correlated ($\rho = -0.95$), suggesting that both capture the same underlying variation in arrival rates.

More generally, shared earning opportunities can vary through both the arrival rate of trip requests and the returns from serving them. In this setting, however, average earnings per completed trip, per kilometer, and per utilization hour each differed by about 1% or less across the two intervention periods (see Appendix A.2). Thus, the weaker market conditions in Experiment 2 reflected lower trip availability rather than lower trip returns. Throughout the paper, I therefore use market conditions primarily to refer to variation in trip availability—how readily drivers can obtain another trip on the platform.

3.2 Common Design Features

Peer Earnings Updates. In both experiments, all participating drivers received twice-weekly email updates on their own earnings and platform activity. A midweek update reported cumulative outcomes for the week to date, while an end-of-week update reported outcomes for the completed week. Each update displayed the driver's total trip earnings, excluding incentive payouts, together with utilization hours and implied earnings per utilization hour. Push and in-app notifications alerted drivers when the updates became available.

Each driver was randomly assigned a reference group of ten other drivers with similar baseline weekly utilization hours. Drivers randomized to receive peer information additionally observed their reference group's average total trip earnings, utilization hours, and earnings per utilization hour. These averages were displayed alongside the driver's own outcomes in the same email.[12] The earnings updates were delivered privately to each driver, limiting social-image concerns that could arise if workers' relative earnings were publicly disclosed (Butera et al., 2022). Figure 3 shows examples of the own-only and peer earnings updates.

Reference Groups. The drivers' reference groups were constructed using administrative trip-level data from the two months preceding each intervention. Under the main matching rule, I grouped drivers into bins based on their average weekly utilization hours and randomly matched each participant to ten reference drivers from the same bin. Experiment 2 additionally included a *Targeted* variant in which reference drivers were selected to have lower baseline earnings within the same utilization-hours bin; I describe this manipulation in Section 3.4.

All reference drivers were drawn from outside the experimental sample and did not receive the earnings updates, preventing the intervention from directly affecting the peer outcomes reported to treated drivers. They were also not informed that their earnings were being used as benchmarks, avoiding the reflection problem that can arise under simultaneous peer interactions (Manski, 1993; Senn et al., 2026).[13] Within each experiment, reference-group assignments remained fixed throughout the six-week intervention.

Monetary Incentives. Both experiments also cross-randomized monetary incentives with the peer information treatment. In each experiment, every driver received two incentive

[12] Drivers could already observe their own weekly earnings through the TADA driver app. During the intervention, both control and treated drivers received the same own-outcome updates through the same channels, allowing me to isolate the incremental effect of peer earnings information.

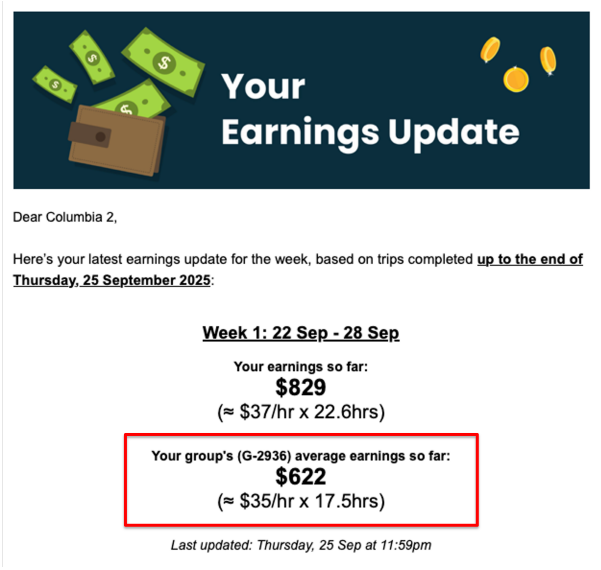
[13] For example, reference drivers might change their behavior if they were aware that their earnings were being observed by other drivers.

Figure 3: Earnings Updates Delivered During the Intervention

(a) Own Earnings Update



(b) Own and Peer Earnings Update



Notes. Panel (a) displays an example of an own earnings update, reporting the driver's total trip earnings (excluding incentive payouts), utilization hours, and implied earnings per utilization hour. Panel (b) displays the corresponding update received by drivers assigned to peer information, which additionally reports the same outcomes averaged across the driver's assigned reference group of ten drivers with similar baseline weekly utilization hours.

offers during the six-week intervention. I use this variation to test whether drivers who differ in baseline information demand also differ in their responsiveness to monetary incentives. The incentives therefore provide a benchmark for assessing whether heterogeneity in responses to peer information reflects broader differences in labor supply responsiveness or attention to platform communications. I describe the experiment-specific incentive designs in Section 3.5.

3.3 Implementation of Experiment 1

Recruitment and Sample. In August 2025, the firm invited drivers to enroll in a new program called "TADA Insights". Drivers were informed that the program would provide exclusive updates and additional earnings incentives, while leaving all other aspects of their work unchanged. Participation required completing a baseline onboarding survey and providing informed consent.

A total of 3,135 drivers enrolled.[14] Before the intervention began, 38 had their accounts

[14]The platform's total number of registered drivers is commercially confidential. For scale, there were just under 60,000 valid Private Hire Car Driver's Vocational Licence (PDVL) holders in Singapore as of August 2025, so the enrolled sample represents roughly 5% of the PDVL-holder population. This is a conservative

deactivated and were excluded, while another 200 participated in an initial pilot. The remaining 2,897 drivers comprise the main Experiment 1 sample. I preregistered the design and analysis plan in the AEA RCT Registry (AEARCTR-0016393).

Information Demand Elicitation. In the baseline survey, I elicited drivers' demand for peer earnings information before treatment assignment. Using the strategy method, each driver considered three scenarios in which their historical weekly earnings were above, approximately equal to, or below the average earnings of their assigned reference group. In each scenario, drivers selected "Yes," "Neutral," or "No" to indicate whether they wanted to receive peer information. To shed light on the motives behind their choices, drivers were also asked to provide a brief open-ended explanation. Appendix Figure A7 displays the survey instrument.

To incentivize truthful responses, I implemented an incentive-compatible procedure adapted from Butera et al. (2022) and Lim (2025). Each driver received an additional S$0.10 bonus on top of the survey incentive. With one-third probability, corresponding to assignment to the *Endogenous Info* arm described below, the driver's choice in the scenario matching their historical relative-earnings position was implemented. Drivers who selected "Yes" or "No" forfeited the bonus and their stated choice was implemented. Drivers who selected "Neutral" retained the bonus and receipt of peer information was determined randomly. With the remaining two-thirds probability, assignment to peer information was randomized independently of the driver's stated choices. The small monetary stake therefore distinguishes drivers willing to pay to receive or avoid peer information from those who are indifferent.[15]

Pilot testing indicated that simple survey instructions were important for comprehension in this field setting. I therefore used this discrete-choice procedure rather than a full Becker-DeGroot-Marschak elicitation of willingness to pay (Becker et al., 1964).

As preregistered, I classify drivers who selected "Yes" in at least one of the three scenarios as *info-seeking* and all remaining drivers as *non-info-seeking*. The latter group therefore comprises drivers who never expressed strict demand for peer information, including those who were indifferent and those willing to pay to avoid receiving it. This classification is based entirely on choices elicited before treatment assignment. I use it to test whether baseline information demand predicts heterogeneity in the treatment effects

benchmark as not all licensed drivers are necessarily working on any ride-hailing platform.

[15] A potential concern is that drivers could answer strategically to infer their historical relative-earnings position rather than report truthfully. Such inference is limited because drivers were not told whether they were in the *Endogenous Info* arm, and receipt of peer information was randomized for most drivers. Ex post, this concern is further mitigated since over 86% of drivers made the same choice across all three scenarios.

of peer information.

Information Treatment Arms. Drivers were randomly assigned with equal probability to one of three experimental groups. The *Control* group received only their own earnings updates. The *Peer Info* group additionally received the corresponding averages for their assigned reference group. In the *Endogenous Info* group, receipt of peer earnings information depended on the driver's elicited choice in the scenario corresponding to their historical relative-earnings position.

The causal effect of peer earnings information is therefore identified by comparing the *Control* and *Peer Info* groups, for whom receipt of peer information was randomly assigned independently of elicited choices. Treatment assignment remained fixed throughout the six-week intervention. Randomization was stratified by baseline utilization hours and platform tenure.

Sample Characteristics and Balance. Appendix Table A2 presents summary statistics and balance tests. Drivers are on average 51 years old, 93% are male, and 62% report driving as their full-time job. They report working approximately six days per week. At baseline, drivers earn an average of S$480 (US$370) and complete about 30 trips on the platform per week. Baseline characteristics are well balanced across the three treatment arms; the p-values from tests of joint equality of means are well above 0.10 for all characteristics.

3.4 Implementation of Experiment 2

Sample and Re-randomization. Experiment 2 drew from the same original cohort of 3,135 drivers who enrolled in the program before Experiment 1. By May 2026, 2,861 drivers remained active and in good standing on the platform and were re-randomized for Experiment 2.[16]

Information preferences were not re-elicited before Experiment 2. The treatment heterogeneity analyses therefore use the same baseline measure elicited approximately ten months earlier, before treatment assignment in Experiment 1. This provides a second test of whether baseline information demand continues to predict heterogeneous responses to peer information. I preregistered the Experiment 2 design and analysis plan in the AEA RCT Registry (AEARCTR-0018613).

Information Treatment Arms. Experiment 2 retained the same recurring earnings-update

[16]The Experiment 2 sample comprises 2,667 drivers from the main Experiment 1 sample, 174 drivers from the initial pilot, and 20 drivers who were inactive when Experiment 1 began but subsequently reactivated their accounts.

format but included two variants of the *Peer Info* treatment that differed in how the reference group was constructed. Drivers were randomly assigned with equal probability to the *Control*, *Standard Peer Info*, or *Targeted Peer Info* group. The *Control* group received only their own earnings updates, while drivers in either *Peer Info* variant additionally received the corresponding averages for their assigned reference group.

The *Standard Peer Info* group followed the same matching rule as Experiment 1, with reference drivers randomly drawn from the same baseline-utilization-hours bin. In the *Targeted Peer Info* group, reference drivers were instead selected to have lower baseline earnings within the same bin, increasing the likelihood that the driver's own earnings exceeded the reference-group average.[17] As intended, the treatment shifted the distribution of relative earnings in a more favorable direction (Appendix Figure A18).

As preregistered, I pool the two peer-information variants in the main analysis and compare them with the *Control* group. I also report separate estimates for each peer-information variant. Randomization in Experiment 2 was stratified by baseline utilization hours, platform tenure, and Experiment 1 information assignment. Reference-group assignments again remained fixed throughout the six-week intervention.

Balance. Appendix Table A3 presents balance tests. Baseline characteristics are again well balanced across the three randomized information arms. Tests of joint equality of means yield p-values above 0.10 for all characteristics.

3.5 Monetary Incentives

As described in Section 3.2, each driver received two monetary incentive offers in each experiment. The two offers were assigned to separate Wednesdays during the six-week intervention. The incentive weeks and the order of the two offers were randomized across drivers, with at least one intervening week without an incentive.

In Experiment 1, each driver received one *Proportional Bonus* and one *Discrete Bonus*. The former increased trip earnings by 10% for the incentive day, while the latter paid S$21 if the driver completed at least ten trips that day. The discrete bonus was calibrated to generate an average payout similar to that of the proportional bonus. Drivers were informed of each incentive on the afternoon before the incentive day.

In Experiment 2, each driver received one 10% and one 20% *Proportional Bonus*. Each bonus applied to trip earnings during a four-hour window from 11am to 3pm on the

[17] The *Targeted Peer Info* group was motivated by suggestive evidence in Huet-Vaughn (2025) that responses to peer information may depend on the relative earnings of the comparison group.

assigned Wednesday. Drivers were informed of each incentive either shortly before the incentive window or on the previous afternoon, with notice timing randomized across drivers. The incentive design was also developed to provide a clean experimental test of income-targeting behavior, a longstanding and much-debated empirical question that I examine in a forthcoming companion paper.

3.6 Internal Validity

I assess the four exclusion restrictions required for identifying unbiased treatment effects (List, 2026). First, the stable unit treatment value assumption (SUTVA) is likely to hold in my setting. The earnings updates were delivered privately and, as discussed in Section 2.1, drivers work largely independently, limiting opportunities for information spillovers across treatment groups.[18] Peer earnings were also constructed from reference-group drivers outside the experimental sample, so the peer averages were not mechanically affected by the intervention. Second, observability is satisfied because I observe labor supply outcomes for all participating drivers from administrative data throughout the experimental periods. Third, compliance is complete as drivers received the respective earnings updates and monetary incentive offers according to their randomized assignments. Finally, statistical independence follows from random assignment to treatment and control within strata in each experiment.

4 Main Results

In this section, I first characterize information demand elicited at baseline and its temporal stability, then estimate how the effects of peer information vary with information demand. I next assess whether this treatment heterogeneity is specific to peer information, before presenting robustness checks of the cross-experiment results.

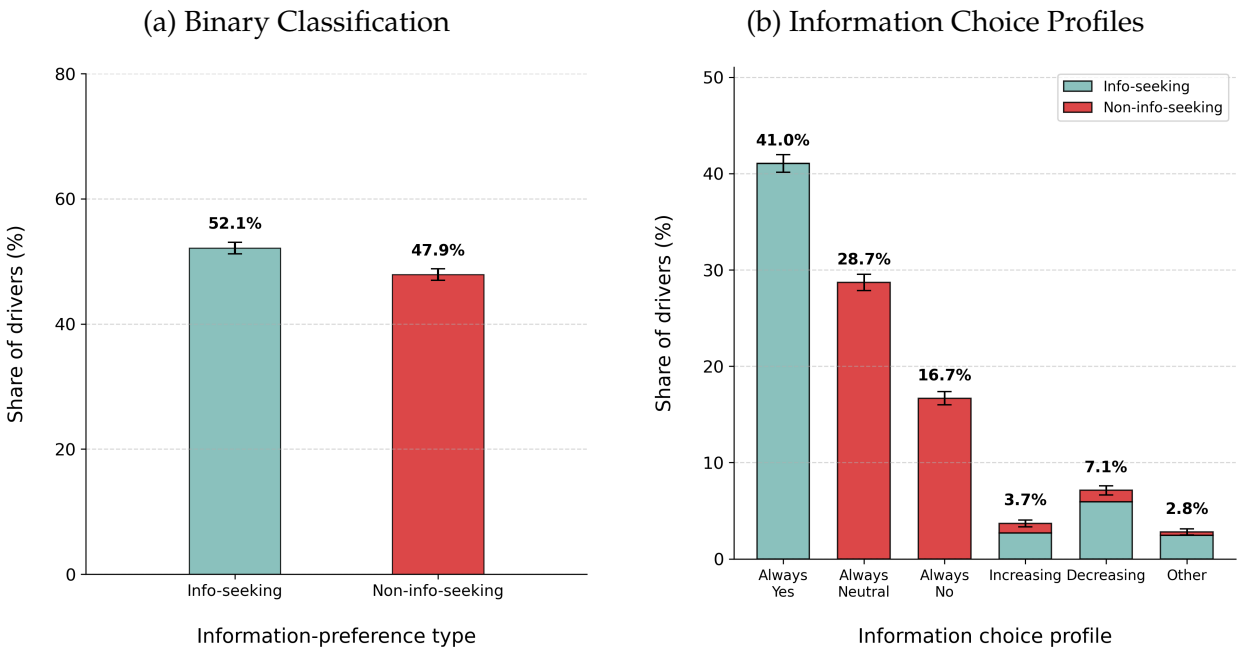
4.1 Demand for Peer Information

Information-Preference Types. Figure 4 presents the distribution of information demand elicited at baseline. Panel (a) shows that 52.1% of drivers are info-seeking, while the remaining 47.9% are non-info-seeking. Panel (b) further shows how drivers' choices vary across the three relative-earnings scenarios. The largest category, comprising 41.0% of

[18] Although informal information sharing between drivers cannot be ruled out entirely, the decentralized nature of the work makes such spillovers less likely. I also found no public discussion of the experiments on the main Facebook groups for ride-hailing drivers in Singapore during the intervention periods.

drivers, chose to receive peer information in all three scenarios ("Always Yes"). Another 28.7% expressed indifference throughout ("Always Neutral"), while 16.7% always chose not to receive peer information ("Always No"). A further 10.8% exhibited monotonic choice profiles ("Increasing" or "Decreasing"), becoming more or less willing to receive peer information as their relative earnings position improved.[19] The remaining 2.8% exhibited other choice patterns. The elicitation therefore reveals substantial heterogeneity in drivers' demand for peer earnings information.

Figure 4: Distribution of Information Demand



Notes. Panel (a) reports the share of drivers classified as *info-seeking* and *non-info-seeking*. Info-seeking drivers are those who choose "Yes" in at least one of the three relative-earnings scenarios (below, around, or above average relative to their assigned peer group); non-info-seeking drivers are those who never choose "Yes". Panel (b) provides a finer breakdown into six mutually exclusive information choice profiles: "Always Yes," "Always Neutral," and "Always No" refer to drivers who make the same choice across all three scenarios. "Increasing" and "Decreasing" refer to nonconstant choice profiles in which willingness to receive peer information weakly increases or decreases, respectively, as relative earnings position improves. "Other" includes all remaining choice profiles. The error bars display ± one standard error of the mean.

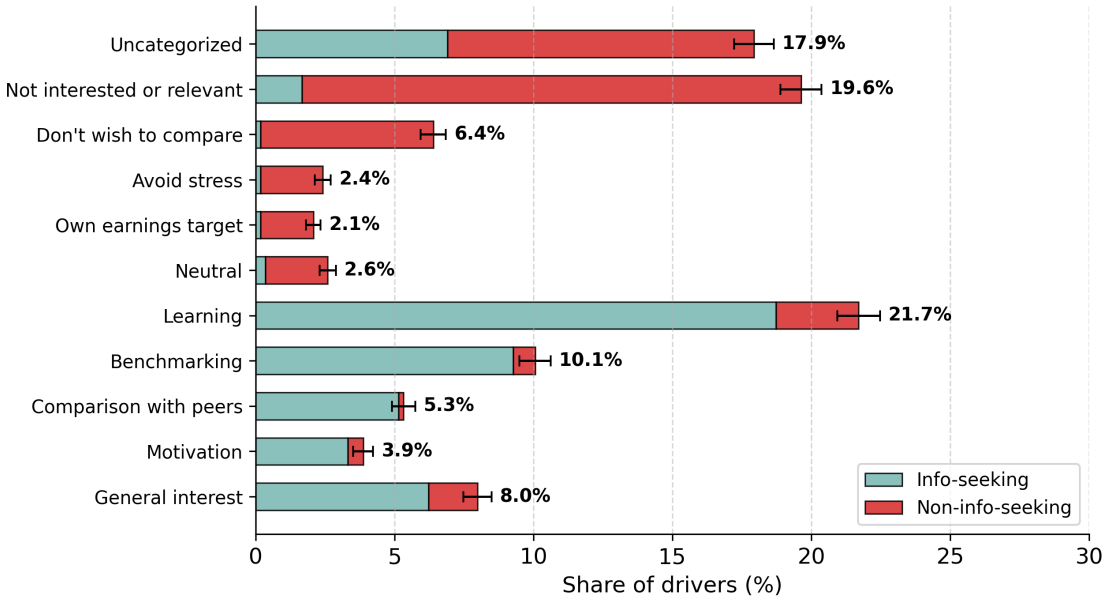
Temporal Stability in Information Demand. Information demand also exhibits meaningful temporal stability. When demand was re-elicited at the Experiment 2 endline nearly one year later, baseline info-seeking drivers were 26.6 percentage points more likely than baseline non-info-seeking drivers to choose to receive peer information in at least one scenario ($p < .001$), with a correlation of 0.28 between baseline and endline info-seeking

[19] Because the elicitation records only three discrete choices rather than willingness to pay (WTP), the observed share of monotonic choice profiles may be understated. For example, an "Always Yes" driver may nevertheless have WTP that varies monotonically with relative earnings.

status.[20] Appendix Section A.6 reports these results in greater detail, including persistence of information choices within each relative-earnings scenario and across different treatment arms.

Stated Motives for Information Choices. To better understand what underlies these information choices, I examine drivers' open-ended explanations for why they wanted or did not want peer earnings information. An independent research assistant assigned each response to one of eleven mutually exclusive categories using the coding scheme in Appendix Table A6.[21] Figure 5 reports the resulting distribution by information-preference type. The stated motives align closely with drivers' elicited choices: responses describing peer information as irrelevant, undesirable, or potentially stressful are concentrated among non-info-seeking drivers, whereas explanations emphasizing learning from peers, benchmarking or comparison, and motivation are concentrated among info-seeking drivers.

Figure 5: Motives Underlying Information Choices



Notes. This figure presents the share of drivers citing different motives for seeking or avoiding peer earnings information. At baseline, after choosing whether they would like to receive peer information across three relative-earnings scenarios, drivers were asked to provide an open-ended explanation for their choices. Specifically, they were asked, "Please tell us in a few words why you made the above choices." Responses are hand-coded into one of eleven mutually exclusive categories (see Table A6 for definition of categories and example responses). The error bars display ± one standard error of the mean.

[20] As a benchmark, Meier and Sprenger (2015) report one-year correlations of 0.25 for individual discount factors and 0.36 for present bias parameters as evidence of meaningful temporal stability in time preferences.

[21] This manual classification is validated against an independent LLM classification using GPT-5.6 Sol, which yields similar results (see Appendix Figure A11).

Among info-seeking drivers, learning-related motives are the most common, accounting for 36% of their explanations. The responses point to two broad, overlapping forms of learning. Some drivers described peer earnings as informative about prevailing market conditions, helping them assess the earning opportunities available on the platform.[22] Others anticipated that peer information could help them reassess their work strategies and identify adjustments that might improve earnings. These included when and where to drive, how many hours to work, or which trips to accept.[23] A separate set of responses emphasized benchmarking, comparison with peers, or motivation. Taken together, these responses suggest that drivers sought peer information to learn about market conditions and work strategies, or for reasons related to relative performance. I treat these motives as descriptive evidence of how drivers expected to use peer information and return to these potential channels after presenting the treatment effect results.

Correlates of Information Demand. Appendix Table A7 examines whether info-seeking status can be predicted from observable characteristics. Female drivers are about 9 percentage points less likely to be info-seeking, while each additional year of education is associated with a 2.8 percentage point higher likelihood of being info-seeking. By contrast, baseline platform activity, measured by earnings and completed trips, is not significantly associated with info-seeking status. Overall, the full set of demographic, administrative, and self-reported measures explains only 5.5% of the variation in info-seeking status, suggesting that information demand is not readily proxied by standard observables.[24] I next test whether this baseline measure predicts heterogeneous responses to peer earnings disclosure across two implementations under substantially different market conditions.

4.2 Effects of Peer Information

Empirical Strategy. I estimate the effects of peer earnings information across the two experiments. As preregistered, I examine treatment effect heterogeneity by drivers' baseline information demand, distinguishing between info-seeking and non-info-seeking drivers. In Experiment 1, I restrict the analysis to drivers assigned to the *Control* and *Peer Info* groups, for whom receipt of peer information was randomly assigned. In Experiment

[22] For example, some drivers wrote that peer earnings would "inform us of market/platform conditions" or be "helpful to know if demand is up or down generally." Another described using peer earnings to determine whether weak earnings reflected "my issue or the market."

[23] One driver wrote that peer information could help "optimise our pickup strategy (e.g. timeslot and location choices)." Another mentioned it would help them consider whether to "drive extra or less hours" each day. A third asked, "Are other drivers being picky for trips? Or do they just accept whatever comes?"

[24] This conclusion is robust to more flexible prediction methods. A tuned random forest classifier using the same observables also achieves modest out-of-sample predictive performance (AUC = 0.65); see Appendix Figure A12.

2, I pool the *Standard Peer Info* and *Targeted Peer Info* groups and compare them with the *Control* group. I report variant-specific estimates separately below.

Let i index drivers, $e \in \{1, 2\}$ index experiments, and t index dates. Define $\text{Treat}_{iet} \equiv \text{PeerInfo}_{ie} \times \text{Post}_{et}$, where PeerInfo_{ie} indicates assignment to peer information in experiment e and Post_{et} equals one beginning with the first earnings update in that experiment. Let $S_i = \mathbf{1}\{\text{info-seeking}\}$. I stack the two experiment panels and estimate a single regression, allowing the coefficients to vary by experiment:

$$\begin{aligned} y_{iet} = \alpha_{ie} + \delta_t + \beta_e^S \left(\text{Treat}_{iet} \times S_i\right) + \beta_e^N \left(\text{Treat}_{iet} \times (1 - S_i)\right) + \lambda_e \left(\text{Post}_{et} \times S_i\right) \\ + \gamma_e^S \left(\text{Incentive}_{iet} \times S_i\right) + \gamma_e^N \left(\text{Incentive}_{iet} \times (1 - S_i)\right) + \varepsilon_{iet}, \end{aligned} \tag{1}$$

where y_{iet} denotes the outcome of driver i on date t in experiment e. α_{ie} and δ_t are driver-by-experiment and date fixed effects, respectively. Incentive_{iet} is an indicator for whether driver i was offered a monetary incentive on date t in experiment e. The interaction $\text{Post}_{et} \times S_i$ allows changes in outcomes following the first earnings update to differ between info-seeking and non-info-seeking drivers within each experiment. Standard errors are clustered at the driver level.

The coefficients β_e^S and β_e^N identify the peer information effects for info-seeking and non-info-seeking drivers, respectively, in experiment e. Within each experiment, I test whether $\beta_e^S = \beta_e^N$ to assess whether elicited information demand predicts differential labor supply responses to peer information. The coefficients γ_e^S and γ_e^N capture the corresponding responses to monetary incentives, providing a benchmark for interpreting the peer information effects. I also test whether treatment effects differ across experiments within each type, $\beta_1^S = \beta_2^S$ and $\beta_1^N = \beta_2^N$, and whether the treatment effect difference between types changes across experiments: $\beta_1^S - \beta_1^N = \beta_2^S - \beta_2^N$. I additionally report average treatment effects from the corresponding specification without interactions with info-seeking status.

My primary measure of platform labor supply is daily utilization hours, defined in Section 2. I also examine the extensive margin using the probability of working on the platform and, conditional on working, utilization hours, completed trips, and trip earnings. Because treatment may affect which drivers work on a given day, conditioning on working can introduce post-treatment selection. I therefore interpret these intensive-margin estimates with caution, particularly when treatment affects work participation.

Treatment Effects by Information Demand. Figure 6 presents the estimated effects of peer earnings information by baseline information demand across both experiments.

Panel (a) reports effects on unconditional daily utilization hours, while panels (b) and (c) report effects on the probability of working and utilization hours conditional on working, respectively. Table 1 extends the analysis to all five main labor supply outcomes and reports formal tests of equality across information-preference types and experiments.

For comparison, Panel A of Table 1 reports the average treatment effects in each experiment. The effects on daily utilization hours are small and statistically insignificant: 0.03 hours in Experiment 1 and −0.01 hours in Experiment 2. The average effects on work participation are also close to zero. These averages, however, mask substantial heterogeneity by elicited information demand.

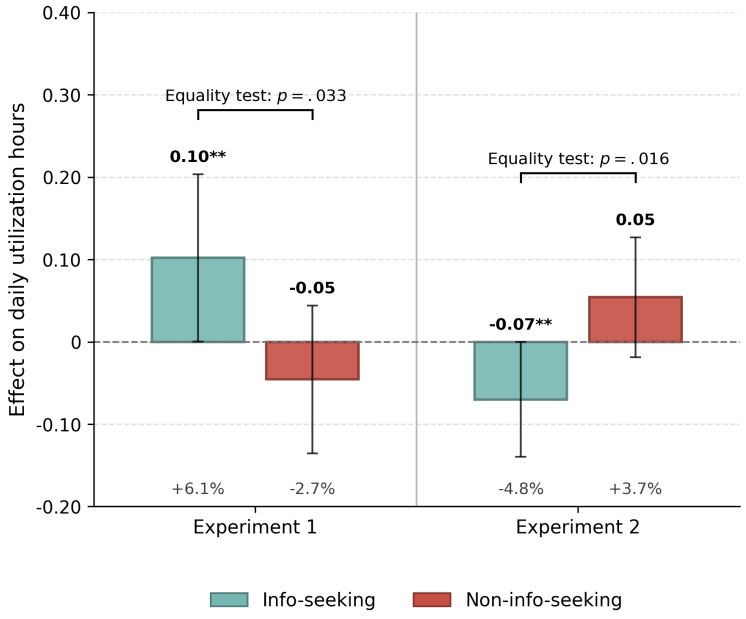
In Experiment 1, peer information increases daily utilization hours among info-seeking drivers by 0.10 hours, or approximately 6% relative to baseline ($p < .05$). The corresponding estimate for non-info-seeking drivers is −0.05 hours and is not statistically significant. The treatment effects differ significantly between the two preference types ($p = .033$).

Experiment 2 again exhibits significant treatment heterogeneity by information demand, but the response among info-seeking drivers reverses sign. Peer information reduces their daily utilization hours by 0.07 hours, or approximately 5% relative to baseline ($p < .05$). The corresponding estimate for non-info-seeking drivers is 0.05 hours and is not statistically significant. The treatment effects again differ significantly between the two types ($p = .016$). Thus, baseline information demand identifies the responsive worker type in both experiments, although the direction of the labor supply response changes across experiments.

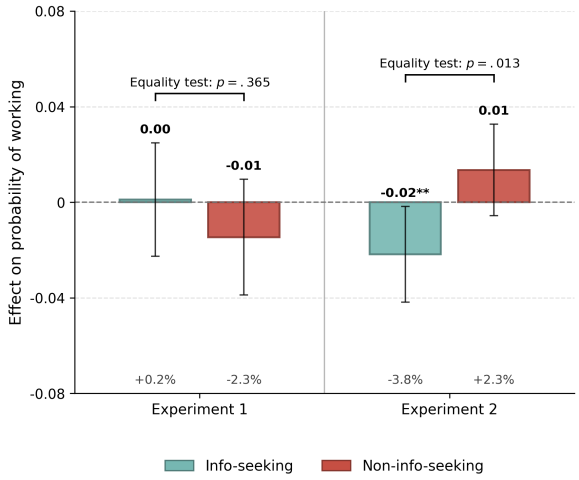
Adjustment Margins. The labor supply responses are also concentrated along different observed margins. In Experiment 1, peer information does not measurably change the probability that info-seeking drivers work on the platform. Conditional on working, however, peer information increases utilization hours by 0.14 hours, completed trips by 0.34, and trip earnings by S$4.97 per day. These estimates correspond to increases of approximately 5.1%, 4.9%, and 4.6% relative to their respective baseline means, and are all significant at the 1% level. By contrast, I find no detectable effects on these outcomes among non-info-seeking drivers. The differences between the two preference types are statistically significant for conditional utilization hours ($p = .011$), completed trips ($p = .027$), and trip earnings ($p = .009$). Thus, the Experiment 1 increase in daily utilization hours is concentrated in greater work intensity on working days, with no detectable change in platform participation.

Figure 6: Effects of Peer Information by Information Preference

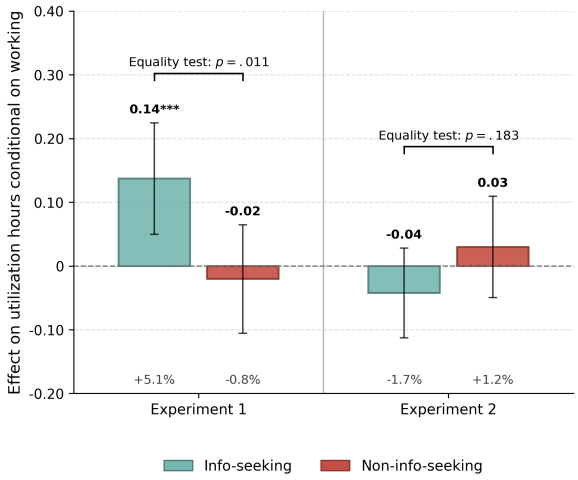
(a) Daily Platform Labor Supply



(b) Extensive Margin



(c) Intensive Margin



Notes. This figure reports the estimated effects of peer earnings information on daily labor supply, separately for info-seeking and non-info-seeking drivers in each experiment. Panel (a) reports unconditional daily utilization hours. Panel (b) reports the probability of working, while Panel (c) reports utilization hours conditional on working. Experiment 1 compares the *Control* and *Peer Info* groups. Experiment 2 pools the *Standard Peer Info* and *Targeted Peer Info* groups and compares them with the *Control* group. Percentages shown below the estimates express the effects relative to the corresponding baseline mean. All specifications include driver-by-experiment and date fixed effects. Equality tests compare the effects between info-seeking and non-info-seeking drivers within each experiment. The error bars represent 95% confidence intervals. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 1: Effects of Peer Information on Daily Labor Supply

Dependent variable:	Platform labor supply	Extensive margin	Intensive margins (conditional on working)		
	Hours	Working	Hours	Trips	Earnings
	(1)	(2)	(3)	(4)	(5)
Panel A: Average Treatment Effects by Experiment					
Peer information: Experiment 1 (β_1)	0.03	-0.01	0.06**	0.16*	1.93
	(0.03)	(0.01)	(0.03)	(0.08)	(1.23)
Peer information: Experiment 2 (β_2)	-0.01	-0.00	-0.01	-0.04	-0.49
	(0.03)	(0.01)	(0.03)	(0.07)	(1.09)
Panel B: Treatment Effects by Experiment and Information Preferences					
Experiment 1 × Info-seeking (β_{1S})	0.10**	0.00	0.14***	0.34***	4.97***
	(0.05)	(0.01)	(0.04)	(0.12)	(1.76)
Experiment 1 × Non-info-seeking (β_{1N})	-0.05	-0.01	-0.02	-0.04	-1.45
	(0.05)	(0.01)	(0.04)	(0.12)	(1.70)
Experiment 2 × Info-seeking (β_{2S})	-0.07**	-0.02**	-0.04	-0.14	-1.83
	(0.04)	(0.01)	(0.04)	(0.10)	(1.44)
Experiment 2 × Non-info-seeking (β_{2N})	0.05	0.01	0.03	0.07	0.93
	(0.04)	(0.01)	(0.04)	(0.11)	(1.65)
Baseline mean: Experiment 1	1.68	0.63	2.67	6.66	105.46
Baseline mean: Experiment 2	1.46	0.58	2.52	6.33	103.47
p-values of equality tests:					
ATE: $\beta_1 = \beta_2$	0.338	0.893	0.091	0.074	0.144
Experiment 1: $\beta_{1S} = \beta_{1N}$	0.033	0.365	0.011	0.027	0.009
Experiment 2: $\beta_{2S} = \beta_{2N}$	0.016	0.013	0.183	0.138	0.209
Info-seeking: $\beta_{1S} = \beta_{2S}$	0.007	0.153	0.002	0.002	0.003
Non-info-seeking: $\beta_{1N} = \beta_{2N}$	0.087	0.079	0.397	0.490	0.318
Diff-in-diff: $\beta_{1S} - \beta_{1N} = \beta_{2S} - \beta_{2N}$	0.002	0.024	0.005	0.008	0.006
No. of observations	543,585	543,585	319,814	319,814	319,814
No. of drivers: Experiment 1	1,930	1,930	1,869	1,869	1,869
No. of drivers: Experiment 2	2,861	2,861	2,611	2,611	2,611

Notes. This table reports the effects of peer earnings information on daily labor supply for both experiments. Panel A reports average treatment effects; Panel B reports effects by elicited information-preference type. Experiment 1 compares the *Control* and *Peer Info* groups. Experiment 2 pools the *Standard* and *Targeted Peer Info* groups and compares them with the *Control* group. In Column 1, *Hours* denotes total daily utilization hours and equals zero on days without completed work. *Working* equals one if at least one completed or canceled trip was recorded. Columns 3–5 report utilization hours, completed trips, and trip earnings conditional on *Working*. Earnings are reported in S$ and exclude incentive payouts. All specifications include driver-by-experiment and date fixed effects. Standard errors clustered at the driver level are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In Experiment 2, by contrast, peer information reduces the probability of working on the platform by 2.2 percentage points ($p < .05$) among info-seekers, while the corresponding effect for non-info-seeking drivers is not significant. The extensive-margin effects differ significantly between the two types ($p = .013$). Conditional on working, I find no detectable effects on utilization hours, completed trips, or trip earnings for either preference type. Thus, in Experiment 2, the reduction in platform labor supply among info-seeking drivers operates through less frequent platform participation rather than lower work intensity on working days.

The formal cross-experiment tests reinforce this contrast. Among info-seeking drivers, the treatment effects differ significantly across experiments for total utilization hours and the intensive-margin outcomes ($p < .01$ for all). The extensive-margin estimate also changes sign, although the cross-experiment difference on this margin is estimated less precisely ($p = .153$).

4.3 Interpreting Heterogeneity by Information Demand

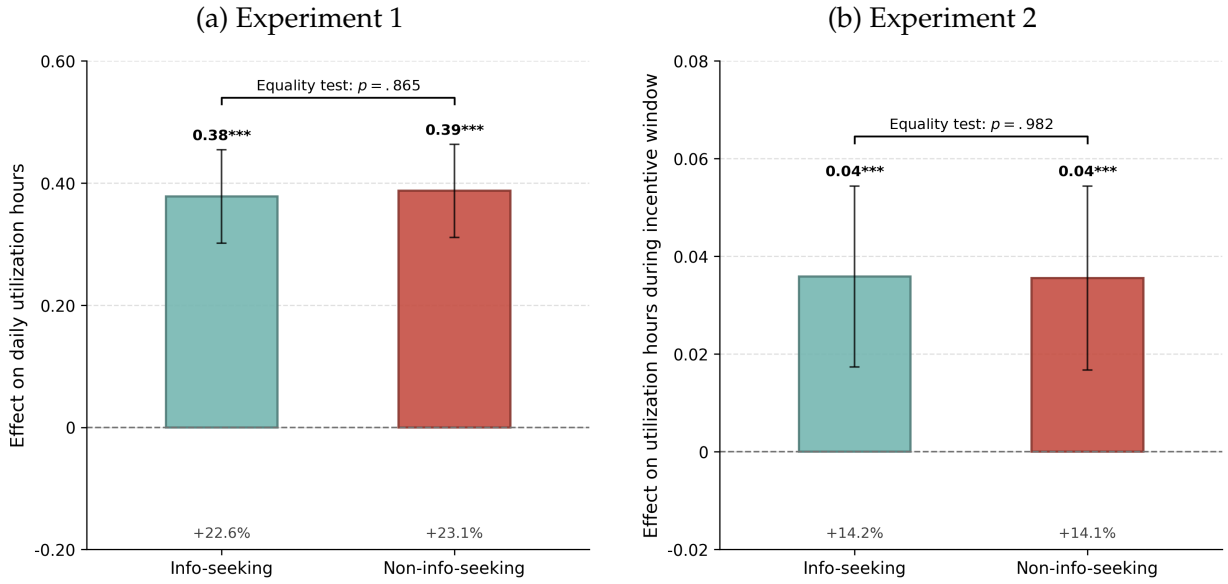
I next examine whether the treatment heterogeneity is specific to peer information or instead reflects two broader explanations: (1) info-seeking drivers may be generally more responsive to workplace interventions, either because they have more elastic labor supply or pay greater attention to platform communications; or (2) they may have less access to information from other drivers outside the experiment.

Monetary Incentives as a Benchmark. The cross-randomized monetary incentives provide a useful test of the first explanation. If the heterogeneous peer information effects reflected broader differences in labor supply responsiveness, info-seeking drivers should also respond more strongly to monetary incentives. As the incentives were communicated through the same channels as the earnings updates, non-info-seeking drivers should likewise respond less to the incentives if they were less attentive to platform communications. Instead, the two types respond very similarly to the incentives in both experiments.

Figure 7 reports the estimated effects of monetary incentives on utilization hours in both experiments. In Experiment 1, shown in Panel (a), the single-day incentives increase daily utilization hours by 0.38 hours among info-seeking drivers and 0.39 hours among non-info-seeking drivers, and the equality of these effects cannot be rejected ($p = .865$). The incentives also increase work participation, conditional hours, completed trips, and earnings, with no significant differences between the two types across any of the five outcomes ($p > 0.10$ for all outcomes; see Appendix Table A8).

Experiment 2, shown in Panel (b), yields the same qualitative pattern. During the four-hour incentive window, the incentives increase utilization hours by approximately 0.04 hours among both info-seeking and non-info-seeking drivers. Again, none of the estimated incentive effects differs significantly between the two types (all equality-test p-values are well above 0.10; see Appendix Table A9). Estimates using full-day outcomes in Appendix Table A8 yield the same conclusion. The results are also robust to estimating the effects separately by incentive type within each experiment (Figures A13 and A14).

Figure 7: Effects of Monetary Incentives by Information Preference



Notes. This figure reports the estimated effects of monetary incentives on utilization hours, separately for info-seeking and non-info-seeking drivers. In Experiment 1, the incentive applied throughout the day, and Panel (a) reports effects on total daily utilization hours. In Experiment 2, the incentive applied only during the 11am–3pm window, and Panel (b) reports effects on utilization hours during that four-hour window. Percentages shown below the estimates express the effects relative to the corresponding baseline mean. Equality tests compare the incentive effects between info-seeking and non-info-seeking drivers within each experiment. The error bars represent 95% confidence intervals. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Access to Other Sources of Peer Information. The second explanation is that baseline information demand reflects differences in drivers' access to information about earnings and market conditions from other drivers outside the experiment. Drivers with fewer outside sources of such information may be more likely to seek peer information and more responsive when it is provided. I proxy for such access using drivers' self-reported membership in online driver groups and communities.[25]

The data provide little support for this explanation. Drivers who report belonging

[25]In the baseline survey, drivers were asked, "Are you part of any online groups or chats for private-hire or taxi drivers?" They were presented with options to select, such as Facebook, WhatsApp, Telegram, etc.

to any online driver group are, if anything, 7.2 percentage points more likely to be info-seeking ($p < .001$), conditional on baseline observables (Appendix Table A7). Moreover, the same treatment effect pattern appears among drivers with and without online-group membership: info-seeking drivers respond more positively than non-info-seeking drivers in Experiment 1 and more negatively in Experiment 2. The difference in peer information effects between the two types does not differ significantly by online-group membership in either Experiment 1 ($p = .69$) or Experiment 2 ($p = .54$); see Appendix Table A10.

Taken together, these results support interpreting baseline information demand as capturing a dimension of worker heterogeneity that is specifically relevant to peer information, rather than simply proxying for broader labor supply responsiveness, attention to platform communications, or access to peer information outside the experiment.

4.4 Robustness Checks

Common-Driver Sample. The composition of participating drivers changes modestly between the two field experiments. I therefore re-estimate the main specification using the same 2,667 drivers who participated in both experiments. The main conclusions are unchanged in this common-driver sample (see Appendix Table A11). Among info-seeking drivers, the effect on daily utilization hours remains positive in Experiment 1 and negative in Experiment 2, and the two effects differ significantly ($p = .034$). The effects of peer information also continue to differ significantly by information-preference type within each experiment ($p < .05$ for both).

Prior Exposure to Peer Information. Experiment 2 independently re-randomized drivers who had and had not received peer information in Experiment 1, allowing me to compare first-time and repeat recipients. Among returning drivers originally randomized to the *Control* or *Peer Info* groups in Experiment 1, the Experiment 2 effects on daily utilization hours and the probability of working do not differ significantly by prior exposure. Among info-seeking drivers, the estimated effects are negative under both first and repeat exposure, and their equality cannot be rejected for either outcome ($p = .559$ and $p = .589$, respectively; Appendix Table A12).

These robustness checks indicate that the cross-experiment pattern is not explained by changes in sample composition or prior exposure to peer information. Baseline information demand consistently predicts *who* responds to peer earnings disclosure, but does not by itself explain the direction or margin of response. The next section considers potential mechanisms and develops a learning framework for interpreting these responses.

5 Mechanisms and Conceptual Framework

Peer earnings can affect labor supply not only because workers may care directly about their earnings relative to their peers, but also because such information may help them learn about the returns to work. In my setting, this learning can operate through two channels. First, peer earnings may help workers learn about shared market conditions that fluctuate over time and are imperfectly observed ("market learning"). Second, peer outcomes may provide information about how effectively others work and thereby about the returns to alternative work strategies ("strategic learning"). These channels are particularly relevant because drivers face shared earning opportunities on the platform and are matched by design to peers with similar baseline work hours. They are also consistent with drivers' stated motives for seeking peer information at baseline.

Before formalizing these learning channels, I assess whether relative pay and performance concerns emphasized in prior work can account for the key empirical patterns.

5.1 Relative Pay and Performance Concerns

Prior research identifies several forms of relative pay and performance concerns, including fairness or inequity concerns (Breza et al., 2018; Cohn et al., 2014), status or rank concerns (Barankay, 2012; Gill et al., 2019), and competitive preferences (Charness et al., 2014). A priori, fairness concerns are unlikely to be central in my setting because drivers operate under a common performance-based compensation rule, so earnings differences do not arise from employer-set pay differences. Consistent with this distinction, Breza et al. (2018) find that pay inequality has no discernible effect on worker output when it can be clearly justified by productivity differences between peers.

Status and competitive motives remain plausible because workers may value relative performance even without direct pecuniary incentives. On their own, however, these mechanisms do not naturally explain why peer information raises labor supply among info-seeking drivers in Experiment 1 but lowers it in Experiment 2. They could nevertheless generate a reversal if treated drivers faced systematically different relative earnings positions across the two experiments and responded differently to being above or below their peers. The experimental design provides two complementary tests of this possibility.

Effects by Peer-Information Variants. First, Experiment 2 directly tests the role of relative earnings position through randomized variation in average peer earnings. The *Targeted Peer Info* variant assigns lower-earning reference drivers than the *Standard Peer Info* variant, shifting treated drivers toward more favorable relative earnings positions (Appendix

Figure A18). Despite this substantial shift, the labor supply effects of peer information are nearly identical across the two variants (Appendix Table A13). Among info-seeking drivers, both treatments reduce daily utilization hours by 0.07 hours and the probability of working by about 2 percentage points. Equality of the variant-specific effects cannot be rejected for either outcome ($p = .982$ and $p = .715$, respectively).

Treatment Heterogeneity by Relative Earnings. Second, the reference-group design provides another source of variation in average peer earnings: drivers are matched to ten randomly selected reference drivers, so otherwise similar drivers face different peer benchmarks. I use this variation to examine whether treatment effects vary with relative earnings position, measured by (1) whether the driver's own earnings are above or below their reference-group average and (2) the relative earnings gap, defined as average peer earnings minus own earnings at each update.[26] Appendix Table A14 reports the estimates. Among info-seeking drivers, the above- and below-average treatment effects do not differ significantly in either Experiment 1 ($p = .924$) or Experiment 2 ($p = .830$). The continuous specifications yield the same conclusion: the treatment-effect gradient with respect to relative earnings is statistically insignificant in both experiments ($p = .980$ and $p = .317$, respectively). The tests among non-info-seeking drivers are also insignificant.

Overall, these results provide little support for relative pay or performance concerns as an explanation for the cross-experiment pattern. Accordingly, I abstract from preferences over relative earnings in the framework below and focus on the informational role of peer earnings. The framework generates testable predictions for market and strategic learning, which guide the analysis in the next section.

5.2 Learning Framework

Set-up. I model the driver's labor supply and strategy choices following an earnings update at the end of period t. After receiving the update, driver i chooses platform hours $H \geq 0$ for the next period and whether to retain their current work strategy (e.g., when or where to drive) or try a different one.[27]

Let λ_{it} denote the rate at which driver i encounters trip requests while available for work in period t under their current strategy. I assume that $\lambda_{it} = A_i M_t$, where $A_i > 0$

[26] In Experiment 2, this analysis compares the *Standard Peer Info* and *Control* groups, whose reference drivers are randomly selected from the same baseline-utilization-hours bins. For control drivers, I construct the measures using their assigned but undisclosed peer averages.

[27] Here, H denotes total time made available to the platform. The administrative data measure utilization hours, which exclude idle waiting time. Holding strategy and the trip arrival rate fixed, expected utilization hours increase with H, so the model's directional predictions carry over to observed utilization hours.

captures how driver i's current strategy affects exposure to trip requests and $M_t > 0$ is the common latent market state. Taking logs gives

$$\log \lambda_{it} = \underbrace{\log A_i}_{a_i} + \underbrace{\log M_t}_{m_t}. \tag{2}$$

Thus, the log trip arrival rate reflects both strategy effectiveness, a_i, and common market conditions, m_t.

Let $w(\lambda)$ denote expected hourly wage when the trip arrival rate is λ. For simplicity, I abstract from changes in the distribution of trip earnings and durations as the arrival rate varies.[28] A higher arrival rate reduces expected waiting time between trip requests, increasing the share of platform hours spent completing trips, so $w'(\lambda) > 0$.[29] Thus, a more effective work strategy or more favorable market conditions increase the arrival rate and thereby increase the expected hourly return on the platform.

I assume that driver i knows the effectiveness of their current strategy, a_i, but is uncertain about the effectiveness of other available strategies. Because their reference group consists of drivers with similar baseline labor supply, peer outcomes are informative about the distribution of strategy effectiveness relevant to driver i. Let $a_{g(i)}$ denote the mean of this distribution.

Earnings Signals and Belief Updating. In each update, driver i observes their own total earnings, which generate an *own signal*, s_{it}^O. If treated with peer information, the driver additionally observes the average total earnings of their reference group, which generate a *peer signal*, s_{it}^P. I model the information that drivers extract from these earnings as noisy signals of the log arrival rates underlying their own and their reference group's earnings, respectively:[30]

$$s_{it}^O = a_i + m_t + e_{it}, \qquad s_{it}^P = a_{g(i)} + m_t + u_{it}, \tag{3}$$

where $e_{it} \sim N(0, \sigma_O^2)$ and $u_{it} \sim N(0, \sigma_P^2)$ are independent signal errors.

Let $\mathcal{I}_{it}^-$ denote the driver's information set before the current update. I assume the

[28] Appendix A.2 shows that average earnings per completed trip, per kilometer, and per utilization hour are very similar across the two experiments, despite substantial differences in trip-request availability.

[29] For example, if trip requests arrive according to a Poisson process with rate λ per hour, the expected waiting time until the next request is $1/\lambda$.

[30] To see why total earnings are informative about the underlying arrival rate, consider a benchmark with known wage and cost mappings. Let $H^*(w) \equiv \arg\max_{H>0}\{wH - C(H)\}$ denote optimal hours supplied at hourly wage w. Total earnings are given by $Y(\lambda) = w(\lambda)H^*(w(\lambda))$. Since both $w(\lambda)$ and $H^*(w)$ are strictly increasing, $Y(\lambda)$ is strictly increasing in λ and can therefore be inverted to recover λ, and equivalently, $\log \lambda$. Equation (3) expresses the information extracted from earnings in log arrival-rate units and allows for imperfect inference through the signal errors.

driver's prior beliefs are $m_t \mid \mathcal{I}_{it}^- \sim N(\mu_{it}^m, \tau_{it}^2)$ and $a_{g(i)} \mid \mathcal{I}_{it}^- \sim N(\mu_{it}^a, \omega_{it}^2)$ and that m_t and $a_{g(i)}$ are independent conditional on $\mathcal{I}_{it}^-$. The signal errors e_{it} and u_{it} are also independent of these latent components.

For expositional convenience, consider first the driver's own signal, and let $\mathcal{I}_{it}^0 = \mathcal{I}_{it}^- \cup \{s_{it}^O\}$. By assumption, a_i is known to the driver, so their own signal about the market state is $s_{it}^O - a_i = m_t + e_{it}$. Bayesian updating gives

$$\widehat{m}_{it}^0 \equiv \mathbb{E}_i[m_t \mid \mathcal{I}_{it}^0] = \kappa_{it}(s_{it}^O - a_i) + (1 - \kappa_{it})\mu_{it}^m, \qquad \kappa_{it} \equiv \frac{\tau_{it}^2}{\tau_{it}^2 + \sigma_O^2}. \tag{4}$$

The remaining uncertainty about the market state is $\widetilde{\tau}_{it}^2 \equiv \mathrm{Var}_i(m_t \mid \mathcal{I}_{it}^0) = (1 - \kappa_{it})\tau_{it}^2$. Because their own signal contains no information about $a_{g(i)}$, the driver's posterior mean remains unchanged: $\widehat{a}_{it}^0 \equiv \mathbb{E}_i[a_{g(i)} \mid \mathcal{I}_{it}^0] = \mu_{it}^a$.

A treated driver additionally observes the peer signal, so $\mathcal{I}_{it}^1 = \mathcal{I}_{it}^0 \cup \{s_{it}^P\}$. Define the *peer-signal surprise* as $z_{it} \equiv s_{it}^P - \mathbb{E}_i[s_{it}^P \mid \mathcal{I}_{it}^0] = s_{it}^P - \left(\widehat{m}_{it}^0 + \widehat{a}_{it}^0\right)$. Bayesian updating then gives the posterior means of m_t and $a_{g(i)}$:

$$\widehat{m}_{it}^1 \equiv \mathbb{E}_i[m_t \mid \mathcal{I}_{it}^1] = \widehat{m}_{it}^0 + \eta_{it} z_{it}, \qquad \eta_{it} \equiv \frac{\widetilde{\tau}_{it}^2}{\widetilde{\tau}_{it}^2 + \omega_{it}^2 + \sigma_P^2}, \tag{5}$$

$$\widehat{a}_{it}^1 \equiv \mathbb{E}_i[a_{g(i)} \mid \mathcal{I}_{it}^1] = \widehat{a}_{it}^0 + \gamma_{it} z_{it}, \qquad \gamma_{it} \equiv \frac{\omega_{it}^2}{\widetilde{\tau}_{it}^2 + \omega_{it}^2 + \sigma_P^2}. \tag{6}$$

Thus, a higher-than-expected peer signal raises the driver's posterior beliefs about both shared market conditions and the mean effectiveness of strategies available to them. I refer to these channels as *market learning* and *strategic learning*, respectively.

Learning Across Repeated Updates. Because the driver's reference group remains fixed throughout the intervention, $a_{g(i)}$ is a persistent group-specific component of strategy effectiveness that can be learned over repeated updates. Early in the intervention, with only a few peer signals observed, the driver remains relatively uncertain about $a_{g(i)}$: low peer earnings may reflect weak market conditions or a relatively low-earning reference group. As the driver observes additional earnings updates from the same group, uncertainty about $a_{g(i)}$ falls and persistent differences in peer earnings are increasingly incorporated into expectations about the reference group, making subsequent peer signals more informative about market conditions.[31] The predictions below characterize responses to a

[31] Appendix C formalizes how repeated peer signals from a fixed reference group reduce uncertainty about $a_{g(i)}$.

given update, conditional on the beliefs entering that update.

Strategy and Labor Supply Choices. If driver i tries a different strategy, its effectiveness $\widetilde{a}_i$ is uncertain and follows $\widetilde{a}_i \mid a_{g(i)} \sim N(a_{g(i)}, \sigma_a^2)$. The driver chooses whether to try a different strategy before observing $\widetilde{a}_i$, and the new strategy is used in the next period. Let $d \in \{0, 1\}$ denote the driver's choice, where $d = 0$ means retaining the current strategy and $d = 1$ means trying a different one, and define

$$a_i(d) \equiv \begin{cases} a_i, & d = 0 \ \text{(retain strategy)}, \\ \widetilde{a}_i, & d = 1 \ \text{(change strategy)}. \end{cases}$$

I assume that drivers view market conditions in the current period as informative about the next period. Specifically,

$$m_{t+1} = \delta_{it} + \rho_i m_t + \nu_{i,t+1}, \tag{7}$$

where δ_{it} captures other predictors of subsequent market conditions known before the current update, $\rho_i > 0$, and $\nu_{i,t+1}$ is an independent mean-zero innovation. Thus, more favorable current market conditions predict more favorable conditions in the next period.

The request-arrival rate under strategy choice $d \in \{0, 1\}$ in the next period is therefore $\lambda_{i,t+1}(d) = \exp\{a_i(d) + m_{t+1}\}$. The expected hourly wage associated with strategy choice d is then

$$\bar{w}^k_{i,t+1}(d) \equiv \mathbb{E}_i \left[w(\lambda_{i,t+1}(d)) \mid \mathcal{I}^k_{it} \right], \tag{8}$$

where $\mathcal{I}^0_{it}$ denotes the information set of a control driver (own signal only) and $\mathcal{I}^1_{it}$ denotes that of a treated driver (own and peer signals). When $d = 1$, the expectation also integrates over uncertainty about the effectiveness $\widetilde{a}_i$ of the new strategy.

Assuming utility is linear in monetary earnings, the driver jointly chooses their strategy d and total platform hours H to maximize expected utility for the next period:

$$\max_{d \in \{0,1\},\, H \geq 0} \left\{ \bar{w}^k_{i,t+1}(d) H - C_i(H) - K_i \mathbf{1}\{H > 0\} \right\}, \tag{9}$$

where $k = 0$ denotes own information only and $k = 1$ denotes own and peer information. The function $C_i(H)$ denotes the variable cost of supplying platform hours, including the disutility of effort and the opportunity cost of time, with $C_i(0) = 0$, $C_i' > 0$, $C_i'' > 0$, and $\lim_{H \to \infty} C_i'(H) = \infty$. The term $K_i > 0$ captures the fixed cost of working on the platform.

A higher posterior mean of m_t shifts beliefs about next-period market conditions

upward, and thus raises the expected hourly wage under either strategy. By contrast, a higher posterior mean of $a_{g(i)}$ raises the expected hourly wage from trying a different strategy and therefore makes doing so more attractive. Conditional on working, optimal hours satisfy $C_i'(H) = \bar{w}_{i,t+1}^k(d)$, so a higher expected hourly wage increases hours supplied. The fixed participation cost also allows a higher expected hourly wage to affect whether the driver works at all.

Empirical Predictions. The learning model yields two key predictions that I take to the data. First, consider the *market learning* channel. Define $\xi_{it} \equiv m_t - \mu_{it}^m$ as the unexpected component of current market conditions relative to the driver's prior mean, which I call *market surprise*. Using equation (4), the peer-signal surprise can be written as

$$z_{it} = (1 - \kappa_{it})\xi_{it} + (a_{g(i)} - \mu_{it}^a) - \kappa_{it} e_{it} + u_{it}. \tag{10}$$

It follows that $\mathbb{E}_i \left[z_{it} \mid \xi_{it}, \mathcal{I}_{it}^- \right] = (1 - \kappa_{it})\xi_{it}$. The peer-signal surprise is therefore positive (negative) on average when market conditions are better (worse) than expected.

Combining this result with equation (5) yields

$$\frac{\partial}{\partial \xi_{it}} \mathbb{E}_i \left[\widehat{m}_{it}^1 - \widehat{m}_{it}^0 \mid \xi_{it}, \mathcal{I}_{it}^- \right] = \eta_{it}(1 - \kappa_{it}) > 0. \tag{11}$$

Thus, as realized market conditions become more favorable relative to prior expectations, the effect of peer information on the driver's posterior belief about the market state becomes more positive. Because a higher posterior belief about the market state raises the expected return to working on the platform, the model yields the following prediction:

Prediction 1. (Market Learning) *The effect of peer earnings information on platform labor supply becomes more positive as market conditions improve relative to prior expectations.*

Since drivers' beliefs are not directly observed, I operationalize this prediction using a rational expectations benchmark: forecasts based only on pre-update information proxy for prior expectations, and the corresponding forecast errors proxy for market surprises.

Next, consider the *strategic learning* channel. Define the peer signal relative to the driver's own signal as $r_{it} \equiv s_{it}^P - s_{it}^O = (a_{g(i)} - a_i) + (u_{it} - e_{it})$, where the common market state m_t cancels out. Conditional on the driver's own signal and prior information, equations (5) and (6) imply

$$\left. \frac{\partial \widehat{m}_{it}^1}{\partial r_{it}} \right|_{s_{it}^O, \mathcal{I}_{it}^-} = \eta_{it} > 0, \qquad \left. \frac{\partial \widehat{a}_{it}^1}{\partial r_{it}} \right|_{s_{it}^O, \mathcal{I}_{it}^-} = \gamma_{it} > 0. \tag{12}$$

Thus, a higher relative signal raises the driver's posterior beliefs about both the common market state and the effectiveness of alternative strategies.

To link these belief updates to the driver's strategy choice, let

$$V_{it}^1(d;r) \equiv \max_{H \geq 0} \left\{ \bar{w}_{i,t+1}^1(d;r)H - C_i(H) - K_i \mathbf{1}\{H > 0\} \right\} \tag{13}$$

denote the optimized expected utility from strategy choice $d \in \{0, 1\}$ after receiving peer information with relative signal r, and define $\Delta V_{it}^1(r) \equiv V_{it}^1(1;r) - V_{it}^1(0;r)$ as the expected gain from trying a different strategy rather than retaining the current one. The driver changes strategy whenever $\Delta V_{it}^1(r) > 0$, retains the current strategy otherwise.

A higher posterior mean of the effectiveness of alternative strategies, $\widehat{a}_{it}^1$ raises the expected return from trying a different strategy while leaving the known effectiveness a_i of the driver's current strategy unchanged. By contrast, a higher posterior mean of the market state, $\widehat{m}_{it}^1$, raises expected returns under both strategies, so its effect on the relative value of switching is ambiguous. Combining these two belief updates, the total effect of a higher relative peer signal on the gain from switching strategies is

$$\frac{d\Delta V_{it}^1(r)}{dr} = \eta_{it} \frac{\partial \Delta V_{it}^1}{\partial \widehat{m}_{it}^1} + \gamma_{it} \frac{\partial \Delta V_{it}^1}{\partial \widehat{a}_{it}^1} \tag{14}$$

I impose a single-crossing condition on the strategy choice: if changing strategy is optimal at some relative peer signal r, it remains optimal at any higher relative peer signal $r' > r$. Formally, $\Delta V_{it}^1(r) > 0 \implies \Delta V_{it}^1(r') > 0$, for all $r' > r$.[32] This yields the following prediction:

Prediction 2. (Strategic Learning) *The effect of peer earnings information on the likelihood of changing work strategy is weakly increasing in the peer signal relative to the driver's own signal.*

6 Evidence on Learning Mechanisms

In this section, I examine whether the informational role of peer earnings formalized above helps explain the labor supply responses to peer information. I first examine *market learning* by estimating how the labor supply effects of peer information vary with the

[32] For example, this condition holds under the constant-elasticity hourly wage function $w(\lambda) = c\lambda^\alpha$, with $c > 0$ and $\alpha > 0$. Under the maintained assumptions, the ratio of expected hourly wages under the alternative and current strategies is $B_{it} \exp\{\alpha(\widehat{a}_{it}^1(r) - a_i)\}$, where $B_{it} > 0$ collects all terms that do not vary with r. Since $\partial \widehat{a}_{it}^1 / \partial r = \gamma_{it} > 0$, this ratio is increasing in r, yielding the single-crossing property.

market conditions summarized by each earnings update. To test this channel more directly, I then decompose realized market conditions into (1) predicted conditions and (2) *market surprises*. I examine whether peer earnings contain information about these surprises beyond drivers' own earnings and whether the labor supply effects vary more strongly with market surprises than with predicted conditions (Prediction 1). Finally, I examine *strategic learning* by testing whether peer information induces drivers to reallocate work across times or locations relative to their baseline patterns, and whether these responses vary with peer earnings relative to their own (Prediction 2).

6.1 Market Learning

Recall that Experiment 2 was designed in part to test whether the effects of peer information would generalize to a different market environment. As documented in Section 3.1, the two experiments took place under markedly different average market conditions, with substantially lower trip-request availability in Experiment 2. The learning framework suggests that these differences may matter because peer outcomes convey information about shared earning opportunities. However, the cross-experiment comparison alone cannot establish this mechanism, since other features of the environment may also have changed. I therefore first test whether the same market-state dependence appears *within* experiments by examining whether the labor supply effects of peer information become more positive following updates that summarize more favorable market conditions.

6.1.1 Peer Information Effects by Realized Market Conditions

Measuring Market Conditions Summarized by Each Update. I use the trip-request availability measure introduced in Section 3.1, constructed from a fixed cohort of drivers outside the experimental sample. For each earnings update, I average daily log trip-request availability over the dates newly summarized by that update: Monday through Thursday for the midweek update and Friday through Sunday for the end-of-week update. I standardize this update-level measure using the mean and standard deviation across baseline update episodes pooled across the two experiments. Denote the resulting measure by M_{eu}, where e indexes experiments and u indexes earnings-update episodes. A value of zero corresponds to average trip-request availability during the baseline periods, while a one-unit increase corresponds to 0.125 log points, or approximately 13% greater trip-request availability.

Persistence of Market Conditions. For recently realized market conditions to guide subsequent labor supply decisions, they must be informative about the earning opportunities

drivers will face next. This corresponds to the persistence assumption in equation (7). I therefore regress market conditions during the subsequent earnings-update window, $M_{e,u+1}$, on those summarized by the current update, M_{eu}. A one-standard-deviation improvement in current market conditions predicts a 0.590-standard-deviation improvement in conditions during the subsequent window ($p < .001$). The relationship is positive in both experiments, and the persistence coefficients do not differ significantly across them ($p = .492$; Appendix Table B1). Thus, market conditions summarized by an earnings update are strongly predictive of the earning opportunities drivers face when making their subsequent labor supply decisions.

Empirical Specification. Let $u(t)$ denote the most recent earnings update available on date t. I match drivers' platform activity on that date to the market conditions summarized by that update, $M_{e,u(t)}$. As before, define $\text{Treat}_{iet} \equiv \text{PeerInfo}_{ie} \times \text{Post}_{et}$. I estimate the following specification in the full sample and separately for info-seeking and non-info-seeking drivers:

$$y_{iet} = \alpha_{ie} + \delta_t + \beta_e \text{Treat}_{iet} + \rho\, \text{Treat}_{iet} M_{e,u(t)} + \zeta_e\, \text{PeerInfo}_{ie} M_{e,u(t)} + \theta_e \text{Incentive}_{iet} + \varepsilon_{iet}, \tag{15}$$

where y_{iet} is daily utilization hours, and α_{ie} and δ_t are driver-by-experiment and date fixed effects, respectively. The interaction $\text{PeerInfo}_{ie} M_{e,u(t)}$ allows the relationship between market conditions and labor supply to differ between treatment groups during the baseline period. Standard errors are two-way clustered by driver and earnings-update date.

The coefficient β_e captures the peer-information effect in experiment e when market conditions are at their baseline-period mean ($M = 0$). The coefficient ρ measures how this treatment effect changes with a one-standard-deviation increase in realized market conditions. I allow β_e to differ across experiments while constraining ρ to be common across experiments. Thus, identification of ρ comes from variation in market conditions across earnings-update episodes *within* each experiment rather than from the difference in average market conditions between the two experiments. The implied peer information effect in experiment e at realized market condition M is therefore $\tau_e = \beta_e + \rho M$.

Results. Table 2 reports the estimates. Among info-seeking drivers, a one-standard-deviation improvement in realized market conditions is associated with a 0.041-hour more positive effect of peer information on daily utilization hours ($p = .011$). The corresponding gradient is 0.022 hours in the full sample ($p = .060$) and essentially zero among non-info-seeking drivers (0.001 hours, $p = .943$). The point estimates therefore suggest that the market-state dependence of peer information effects is concentrated among info-

seeking drivers, although the difference between the two type-specific market gradients is imprecisely estimated ($p = .105$).

Appendix Table B2 reports an alternative specification that additionally constrains the peer information effect at baseline market conditions ($M = 0$) to be common across experiments ($\beta_1 = \beta_2$). Under this restriction, the market gradient draws on both within- and cross-experiment variation in market conditions and treatment effects. The estimated gradient remains positive, and if anything becomes larger, among info-seeking drivers (0.062 hours, $p < .001$). Thus, the cross-experiment comparison reinforces rather than drives the positive relationship found using within-experiment variation.

Table 2: Peer Information Effects by Realized Market Conditions

Dependent variable:	Daily utilization hours		
	Full sample (1)	Info-seeking (2)	Non-info-seeking (3)
Peer information at $M = 0$: Experiment 1	0.019	0.092*	-0.061
	(0.033)	(0.052)	(0.045)
Peer information at $M = 0$: Experiment 2	0.028	0.002	0.057
	(0.028)	(0.041)	(0.045)
Peer information × Realized market conditions (M)	0.022*	0.041**	0.001
	(0.011)	(0.016)	(0.018)
p-value: equal market gradients across preference types		0.105	
No. of observations	543,585	282,660	260,925
No. of drivers	3,013	1,576	1,437
No. of earnings-update episodes	66	66	66

Notes. The table reports how the effects of peer information on daily utilization hours vary with realized market conditions. Market conditions are measured by average daily log trip-request availability over the dates newly summarized by each earnings update: Monday–Thursday for midweek updates and Friday–Sunday for end-of-week updates. The measure M is centered at its pooled-baseline mean and divided by its pooled-baseline standard deviation of 0.125 log points. The peer-information coefficients report treatment effects at $M = 0$. The interaction coefficient reports the change in the treatment effect associated with a one-standard-deviation improvement in market conditions. Within each column, treatment effects at $M = 0$ may differ across experiments, while the market gradient is constrained to be common. Column (1) uses the full sample; Columns (2) and (3) restrict the sample to info-seeking and non-info-seeking drivers, respectively. All regressions include driver-by-experiment and date fixed effects, an incentive-offer indicator with experiment-specific coefficients, and experiment-specific interactions between peer-information assignment and market conditions. Standard errors in parentheses are two-way clustered by driver and earnings-update date.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6.1.2 Predicted Market Conditions and Market Surprises

The preceding results show that peer information effects vary with realized market conditions, particularly among info-seeking drivers. This relationship alone does not establish market learning. Prediction 1 concerns market conditions relative to drivers' prior expectations, $m_t - \mu_{it}^m$. If market conditions were already anticipated by the drivers, peer

earnings need not provide *new* information about shared earning opportunities. I therefore decompose realized market conditions into predicted conditions and market surprises and test whether peer information effects vary more positively with the latter.

Constructing Market Surprises. Because drivers' subjective expectations are unobserved, I use one-day-ahead forecasts of daily log trip-request availability to proxy for predictable market conditions. Let $\ell_d = \log R_d$, where R_d denotes trip-request availability on day d. For each day, I recursively estimate an AR(1) model using an expanding estimation window containing only observations available through the preceding day. The model includes controls for day of week, school holidays, public holidays, and the Formula 1 Grand Prix period. Denote the resulting forecast by $\widehat{\ell}_{d|d-1}$. The daily forecast error is $\ell_d - \widehat{\ell}_{d|d-1}$.

For each update episode, I average these forecasts and forecast errors over the dates newly summarized by the update, denoted $\mathcal{D}_{eu}$. Using the same baseline-period mean $\bar{\ell}_0$ and standard deviation s_0 used to standardize realized market conditions, I construct

$$M^P_{eu} = \frac{|\mathcal{D}_{eu}|^{-1} \sum_{d \in \mathcal{D}_{eu}} \widehat{\ell}_{d|d-1} - \bar{\ell}_0}{s_0}, \qquad M^U_{eu} = \frac{|\mathcal{D}_{eu}|^{-1} \sum_{d \in \mathcal{D}_{eu}} (\ell_d - \widehat{\ell}_{d|d-1})}{s_0}. \tag{16}$$

I refer to M^P as *predicted market conditions* and M^U as the *market surprise*. By construction, $M_{eu} = M^P_{eu} + M^U_{eu}$. Both components use the same scale: one unit corresponds to 0.125 log points of trip-request availability.

A positive market surprise indicates that trip-request availability was higher than predicted using information available beforehand. Under rational expectations, the model object $m_t - \mu^m_{it}$ is the forecast error in market conditions relative to driver i's prior information. I use M^U_{eu} as an empirical proxy for this object.[33] The econometrician's information set need not coincide with the information set or subjective beliefs of driver i. I therefore interpret M^U_{eu} as a proxy for unexpected market conditions rather than a direct measure of drivers' belief surprises.

Information Content of Peer Earnings. For peer earnings to help drivers learn about shared market conditions, they must contain information beyond that already available from drivers' own outcomes. I therefore test whether assigned peer earnings predict

[33]The measure averages daily forecast errors, allowing the forecasting information set to evolve within each update episode. It therefore captures unexpected daily market conditions over that window, rather than the forecast error for the entire window based on information available at its beginning.

market surprises conditional on drivers' own earnings:

$$M^U_{eu} = \alpha_{ie} + \pi_O \,\text{OwnEarnings}_{ieu} + \pi_P \,\text{PeerEarnings}_{ieu} + \lambda M^P_{eu} + \mathbf{X}'_{eu}\gamma + \varepsilon_{ieu}, \tag{17}$$

where own and assigned peer earnings are average daily earnings over the same earnings-update window, expressed in S$10 units. The specification includes driver-by-experiment fixed effects and predicted market conditions, while $\mathbf{X}_{eu}$ contains experiment-specific indicators for update type (i.e., midweek or end-of-week) and the intervention period. Testing $\pi_P = 0$ assesses whether peer earnings contain incremental predictive information about market surprises beyond the driver's own earnings.

Appendix Table B3 reports the estimates. S$10 higher average daily peer earnings are associated with a 0.035-unit higher market surprise ($p < .001$), conditional on the driver's own earnings and predicted market conditions. The corresponding coefficient on own earnings is 0.004 ($p < .001$), and equality of the two coefficients is rejected ($p < .01$). Peer earnings therefore contain substantial incremental information about market surprises beyond what drivers' own earnings reveal. The larger coefficient on peer earnings is also consistent with the peer-group average providing a more precise signal of shared earning opportunities by averaging out some of the idiosyncratic variation in an individual driver's earnings.

Empirical Specification. I next examine whether the labor supply effects of peer information become more positive following favorable market surprises, as in Prediction 1. I replace the interaction with realized market conditions in equation (15) with separate interactions with the market surprises and predicted market conditions, which enter jointly:

$$\begin{aligned} y_{iet} = \alpha_{ie} + \delta_t + \beta_e \text{Treat}_{iet} + \rho_U \,\text{Treat}_{iet} M^U_{e,u(t)} + \rho_P \,\text{Treat}_{iet} M^P_{e,u(t)} \\ + \zeta_{eU} \,\text{PeerInfo}_{ie} M^U_{e,u(t)} + \zeta_{eP} \,\text{PeerInfo}_{ie} M^P_{e,u(t)} + \theta_e \text{Incentive}_{iet} + \varepsilon_{iet}. \end{aligned} \tag{18}$$

As before, I allow the peer information effect at $M^P = M^U = 0$, captured by β_e, to differ across experiments, while constraining the treatment gradients ρ_U and ρ_P to be common across experiments. I estimate the specification in the full sample and separately for info-seeking and non-info-seeking drivers. The coefficients ρ_U and ρ_P measure how the effect of peer information changes with a one-unit increase in market surprises and predicted market conditions, respectively, holding the other component fixed. Both components use the same scale: one unit corresponds to 0.125 log points of trip-request availability. Both gradients are identified using variation across earnings-update episodes within each experiment.

Prediction 1 motivates testing whether favorable market surprises make the labor supply effect of peer information more positive ($\rho_U > 0$). I also compare this gradient with the corresponding gradient for predicted market conditions. Because realized market conditions decompose as $M = M^P + M^U$, a linear specification in which the treatment effect depends only on realized market conditions, regardless of whether those conditions were anticipated, implies $\rho_U = \rho_P$. A more positive gradient for market surprises, $\rho_U > \rho_P$, would provide sharper evidence that workers respond to the new information about shared earning opportunities conveyed by peer outcomes.

Results. Table 3 reports the estimates. Among info-seeking drivers, a market surprise corresponding to trip-request availability 0.125 log points higher than predicted is associated with a 0.184-hour more positive effect of peer information on daily utilization hours ($p < .001$). By contrast, the corresponding gradient for predicted market conditions is only 0.010 hours ($p = .516$). Equality of the two gradients is strongly rejected ($p = .001$).

The same pattern appears in the full sample, although the difference between the two gradients is less precisely estimated. The market-surprise gradient is 0.074 hours ($p = .016$), compared with 0.011 hours for predicted market conditions ($p = .377$), with a test of equality yielding $p = .066$. Among non-info-seeking drivers, neither the market-surprise gradient (−0.044 hours, $p = .452$) nor the predicted-condition gradient (0.010 hours, $p = .591$) is statistically significant. Moreover, the market-surprise gradient differs significantly between info-seeking and non-info-seeking drivers ($p = .009$), while the predicted-condition gradients are nearly identical ($p = .996$).

Appendix Table B4 shows that this pattern extends to the other labor supply outcomes. Among info-seeking drivers, the market-surprise gradients are positive and statistically significant across all reported outcomes, whereas none of the corresponding gradients is statistically significant among non-info-seeking drivers.

Robustness Checks. The positive market-surprise gradient among info-seeking drivers is robust to alternative forecasting specifications. Appendix Figure B2 compares forecasts based on calendar controls alone with specifications that sequentially add a one-day lag, a seven-day lag, and the recent 28-day mean and trend in trip-request availability. The market-surprise gradient among info-seeking drivers remains positive and statistically significant across all five specifications, whereas none of the corresponding gradients among non-info-seeking drivers is statistically significant. The positive relationship is therefore not specific to the benchmark AR(1) forecasting specification.

The market-surprise gradient among info-seeking drivers also remains positive and

Table 3: Peer Information Effects by Predicted Conditions and Market Surprises

Dependent variable:	Daily utilization hours		
	Full sample (1)	Info-seeking (2)	Non-info-seeking (3)
Peer information at $M^P = M^U = 0$: Experiment 1	0.007	0.080*	-0.071*
	(0.031)	(0.045)	(0.042)
Peer information at $M^P = M^U = 0$: Experiment 2	0.039	0.023	0.057
	(0.028)	(0.038)	(0.045)
Peer information × Market surprise (M^U)	0.074**	0.184***	-0.044
	(0.030)	(0.045)	(0.058)
Peer information × Predicted conditions (M^P)	0.011	0.010	0.010
	(0.012)	(0.016)	(0.019)
p-value: $\rho_U = \rho_P$	0.066	0.001	0.399
No. of observations	543,585	282,660	260,925
No. of drivers	3,013	1,576	1,437
No. of earnings-update episodes	66	66	66

Notes. The table reports the treatment effect of peer information on daily utilization hours and how it varies with predicted and unexpected market conditions. Column (1) uses the full sample; Columns (2) and (3) restrict the sample to info-seeking and non-info-seeking drivers, respectively. Experiment 1 compares the Control and Peer Info groups; Experiment 2 pools the Standard and Targeted Peer Info variants. All specifications include driver-by-experiment and date fixed effects, experiment-specific controls for randomized monetary incentives, and all estimable lower-order interactions, which may differ across experiments. Standard errors in parentheses are two-way clustered by driver and earnings-update date.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

similar in magnitude when I exclude observations following updates that summarize dates overlapping the Formula 1 Grand Prix, public holidays, or school holidays, as well as when all three event periods are excluded together (Appendix Figure B3).

Finally, I conduct a "leave-one-out" analysis in which I re-estimate equation (18), excluding one earnings-update episode from the sample at a time. The market-surprise gradient remains remarkably stable across these iterations, indicating that the result is not driven by any single update (Appendix Figure B4).

Taken together, the results show that among info-seeking drivers, the effect of peer information varies substantially more with unexpected than with predicted market conditions. Combined with the evidence that assigned peer earnings contain incremental information about market surprises beyond drivers' own earnings, this pattern supports the market learning channel in Prediction 1.

6.2 Strategic Learning

Peer earnings may also reveal scope to earn more through alternative work strategies, leading drivers to change how they work. I examine this strategic learning channel

by measuring departures from drivers' baseline allocation of work along two margins: (1) times of day and (2) locations. I first test whether peer information increases these departures on average. I then test Prediction 2 by examining whether the effects become more positive as assigned peer earnings increase relative to the driver's own earnings. I define the two measures of strategy reallocation below before presenting the results.

Time-of-Day Reallocation Measure. To measure whether peer information changes *when* drivers work, I first divide each day into five time blocks: 12–6 am, 6–10 am, 10 am–5 pm, 5–8 pm, and 8 pm–12am. The cutoffs reflect distinct patterns of trip-request availability over the course of the day (Appendix Figure B5).

I aggregate drivers' platform activity into two analysis windows each week, Monday–Thursday and Friday–Sunday. During the intervention, these correspond to the periods following the end-of-week and midweek earnings updates, respectively. Let s_{ieub} denote driver i's share of utilization hours allocated to time block b during the period following earnings update u in experiment e, and let s^0_{ieub} denote the baseline share for the corresponding window type. I define the time-of-day reallocation measure as $D^T_{ieu} = 100 \times \frac{1}{2} \sum_{b=1}^{5} \left| s_{ieub} - s^0_{ieub} \right|$. The index ranges from 0 to 100 and measures the absolute distance between the driver's current and baseline allocation across the five time blocks. Higher values indicate larger departures from the driver's usual allocation of work across times of day. For example, a value of 20 means that 20% of the driver's allocation across time blocks would need to be redistributed to match their baseline work pattern.

Spatial Reallocation Measure. To measure whether peer information changes *where* drivers work, I measure location using Singapore's 55 planning areas. For each driver-hour, I infer the driver's location as the planning area with the highest number of pickup points among the trip requests they received.[34] I then measure how far the resulting spatial distribution departs from the driver's baseline pattern. Appendix Section B.6 shows that the spatial distribution of trip requests varies substantially over the course of the day. As changes in when a driver works could mechanically change their observed spatial distribution, I compare spatial patterns separately within each of the same five time blocks used above.

Let p_{ieubg} denote driver i's share of observed hours assigned to planning area g within time block b during the period following earnings update u in experiment e, and let p^0_{ieubg} denote the corresponding baseline share for the same window type and time block.

[34] Within each driver-hour, I retain distinct trip requests with an estimated pickup distance of no more than 2 km, regardless of whether the request was accepted. Ties across planning areas are broken using the smaller mean pickup distance.

Let N_{ieub} denote the number of observed hours with an inferred location in time block b, and let $N_{ieu} = \sum_b N_{ieub}$. I define the spatial reallocation measure as $D^S_{ieu} = 100 \times \sum_{b:N_{ieub}>0} \frac{N_{ieub}}{N_{ieu}} \left[\frac{1}{2}\sum_g \left|p_{ieubg} - p^0_{ieubg}\right|\right]$. For each time block, the term in brackets measures the distance between the driver's current and baseline distributions across planning areas. I then average these distances across time blocks, weighting each block by its share of observed hours in the response period. The resulting index ranges from 0 to 100, with higher values indicating larger departures from the driver's usual work locations. For example, a value of 20 means that 20% of the driver's observed hours would need to be reassigned across planning areas, holding their time block fixed, to match their baseline spatial pattern.

Results. Appendix Table B5 reports the pooled estimates across both experiments. Among info-seeking drivers, peer information has no detectable average effect on either measure of work strategy reallocation. The estimated effects are 0.082 points for time-of-day reallocation ($p = .823$) and -0.108 points for spatial reallocation ($p = .622$). I likewise find little support for Prediction 2 when allowing the treatment effect to vary with relative earnings. The estimated treatment-effect gradients are essentially zero: 0.0047 points for time-of-day reallocation ($p = .800$) and -0.0001 points for spatial reallocation ($p = .991$) for each S$10 increase in assigned peer earnings relative to the driver's own earnings. Neither the average effects nor the relative-earnings gradients are statistically significant among non-info-seeking drivers as well.

Overall, I find little evidence that drivers respond to peer earnings information by systematically changing when or where they work. A caveat is that while these measures capture two important dimensions of work strategy, they do not rule out other ways in which drivers might adjust how they work.

7 Conclusion

This paper shows that the labor supply effects of recurring peer earnings disclosure depend both on who receives the information and on what peer outcomes reveal about shared earning opportunities. Across two field experiments with around 3,000 ride-hailing drivers, an incentivized measure of information demand consistently identifies the workers who respond to peer information. Info-seeking drivers increase platform labor supply by about 6% in Experiment 1 but reduce it by about 5% in Experiment 2, while non-info-seeking drivers show no detectable response in either experiment. By contrast, the two worker types respond similarly to monetary incentives. These findings under-

score that elicited information demand captures a persistent and behaviorally meaningful dimension of heterogeneity in responses to peer information.

The results also highlight a distinct informational channel through which peer information can affect labor supply, beyond social comparison. In performance-based settings where workers face shared, time-varying earning opportunities, peer outcomes can reveal information about the underlying market state that workers cannot fully infer from their own outcomes alone. Consistent with this market learning channel, peer earnings contain incremental information about unexpected market conditions beyond what drivers' own earnings reveal. Among info-seeking drivers, the labor supply effect of peer information becomes substantially more positive following unexpectedly favorable market conditions, while showing little relationship with predicted conditions. By contrast, I find limited evidence that drivers use peer information to learn about alternative work strategies, such as when or where to work.

Of course, this paper comes with caveats that point to directions for future work. First, while the evidence points to market learning as the leading channel, I do not randomize market conditions or directly observe drivers' expectations about them. Instead, I distinguish predicted from unexpected market conditions using a forecast based on information available beforehand. The constructed market surprises therefore serve as a proxy for what drivers themselves did not anticipate, since the information set underlying my forecasting model need not exactly coincide with the information available to drivers. Future work could directly elicit beliefs before and after information provision, or randomize whether market conditions are revealed directly to drivers, to provide a more direct test of belief updating and the market learning channel.

Second, I find limited evidence that peer information induces drivers to change when or where they work, but this does not rule out strategic learning more generally. The intervention was deliberately light-touch: drivers observed peers' earnings outcomes, but not their work strategies. Drivers could therefore learn that peers were earning more without knowing how to translate that information into a different work strategy. Future interventions could pair peer earnings with information about peers' work practices (e.g., how higher-earning drivers allocate their work across times or locations), providing a more direct test of whether peer information can facilitate strategic learning and, in turn, improve worker earnings and productivity.

More broadly, these findings have implications for the design of information policies within organizations. The simple incentivized elicitation introduced here could be incorporated into workplace surveys to help firms identify ex ante which workers are likely

to respond to peer feedback. The fact that nearly one-quarter of drivers in my setting were willing to forgo a small payment to avoid such information in at least one scenario cautions against treating peer disclosure as a one-size-fits-all policy and underscores the importance of accounting for workers' information preferences. Yet peer outcomes can contain useful information about shared earning opportunities. Understanding what drives this information avoidance, and whether alternative presentations of peer feedback can preserve its instrumental value while reducing the costs associated with receiving it, is a promising direction for future research. Finally, from an efficiency perspective, the market learning channel points to a broader economic role for peer information beyond concerns about relative performance or status. By helping workers infer common shocks, peer information may improve labor allocation by enabling workers to adjust their labor supply to changes in the returns to work.

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Online Appendix

Peer Information at Work: Evidence from Two Field Experiments

Zhi Hao Lim

A Supplementary Figures and Tables **2**

A.1 Driver App Interface 2
A.2 Trip Availability and Trip Returns Across Experiments 3
A.3 Recruitment Materials and Program Participation 7
A.4 Elicitation of Information Demand 9
A.5 Summary Statistics and Balance 10
A.6 Temporal Stability in Information Demand: Endline Survey 12
A.7 Coding and Validation of Stated Motives 18
A.8 Predicting Information Demand from Observables 20
A.9 Effects of Monetary Incentives by Information Preference 23
A.10 Disaggregating the Non-Info-Seeking Group 27
A.11 Effects by Membership in Online Driver Groups 30
A.12 Robustness Checks 31
A.13 Tests of Relative Performance Mechanisms 34

B Additional Mechanism Evidence **37**

B.1 Persistence of Market Conditions 37
B.2 Effects of Peer Information by Market Conditions: Common Treatment Effect . . . 38
B.3 Predicted Market Conditions and Market Surprises 39
B.4 Robustness of Market-Surprise Gradients 42
B.5 Intraday Distribution of Trip Requests 45
B.6 Spatial Distribution of Trip Requests 46
B.7 Effects of Peer Information on Work Strategy 48

C Theoretical Appendix **49**

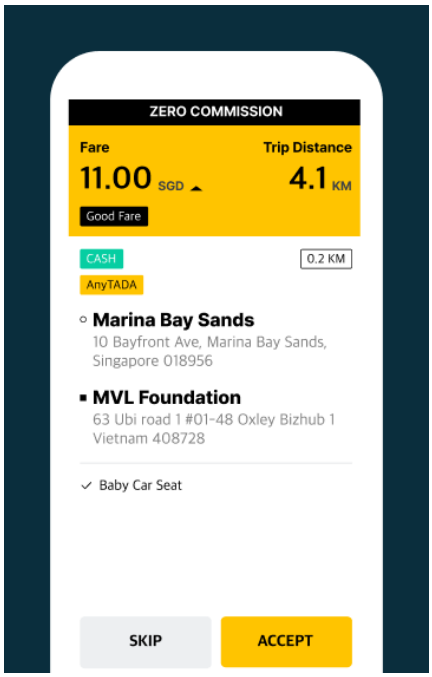
D Generalizability **51**

A Supplementary Figures and Tables

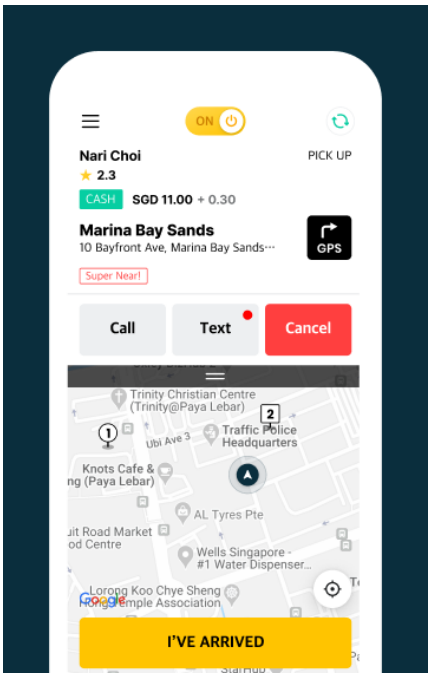
A.1 Driver App Interface

Figure A1: The TADA Driver App

(a) Trip Request



(b) Pickup Interface

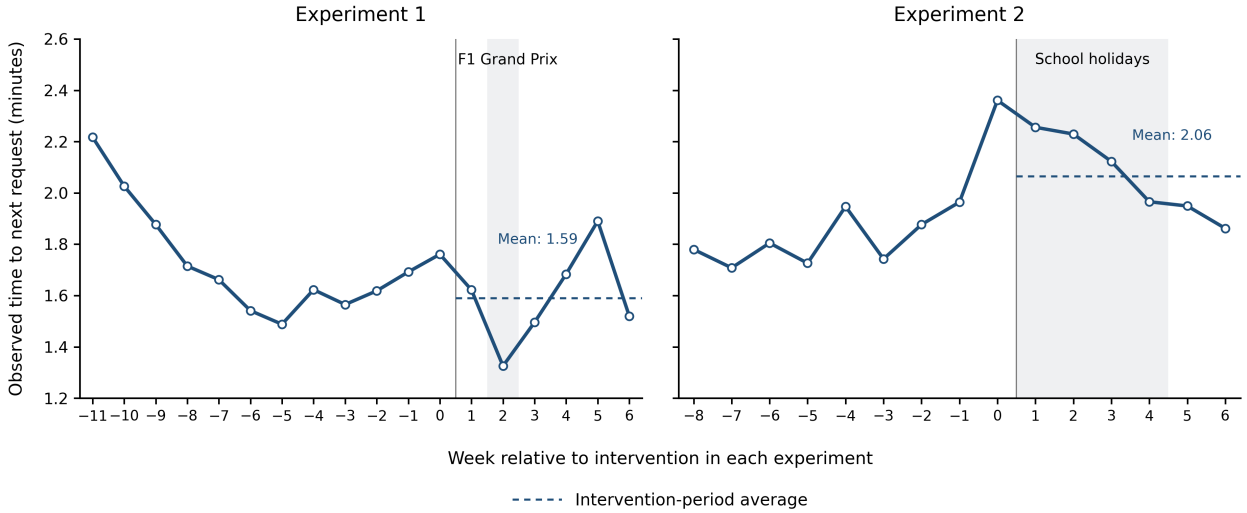


Notes. Panel (a) shows the trip-request screen before a driver accepts or skips a request. Panel (b) shows the pickup interface after a request is accepted.

A.2 Trip Availability and Trip Returns Across Experiments

Trip availability. I complement the trip-request availability index with the observed waiting time between a rejected request and the next request shown to the same driver. Figure A2 shows that waiting times were longer during Experiment 2, consistent with the lower availability of trip opportunities documented in the main text.

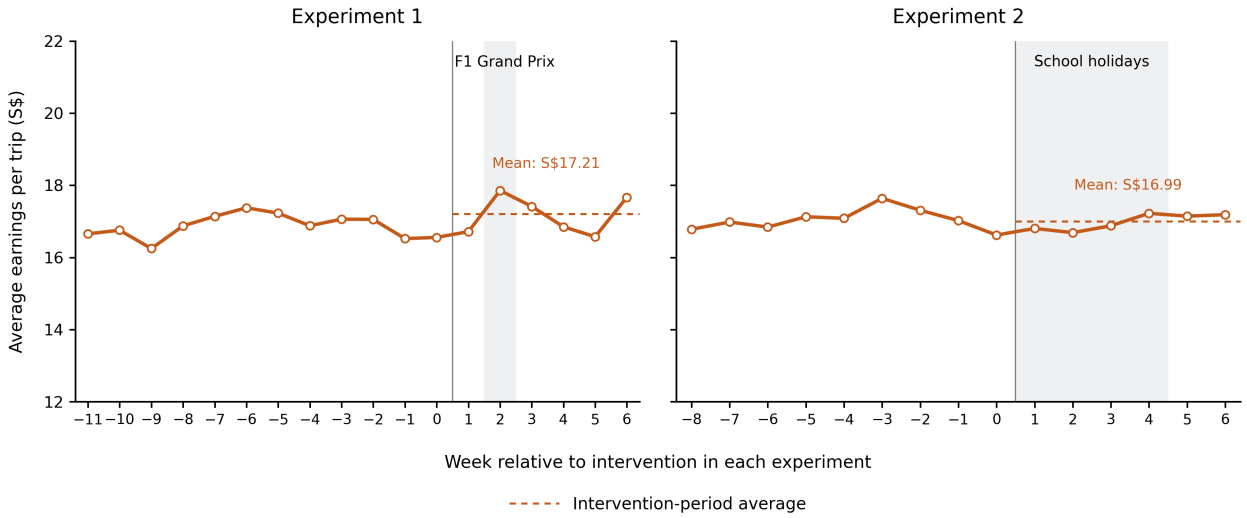
Figure A2: Average Waiting Time to Next Trip Request Across Experiments



Notes. The figure plots the weekly average waiting time from a rejected trip request to the next request shown to the same driver that day. Waiting times are capped at 120 minutes; rejected requests with no subsequent request observed that day are excluded. The sample consists of the same fixed cohort of nonexperimental drivers observed during both baseline periods. Week 0 is the final baseline week, and vertical lines separate the baseline and intervention periods. Shaded areas indicate the Formula 1 Grand Prix period in Experiment 1 and the school holiday period in Experiment 2.

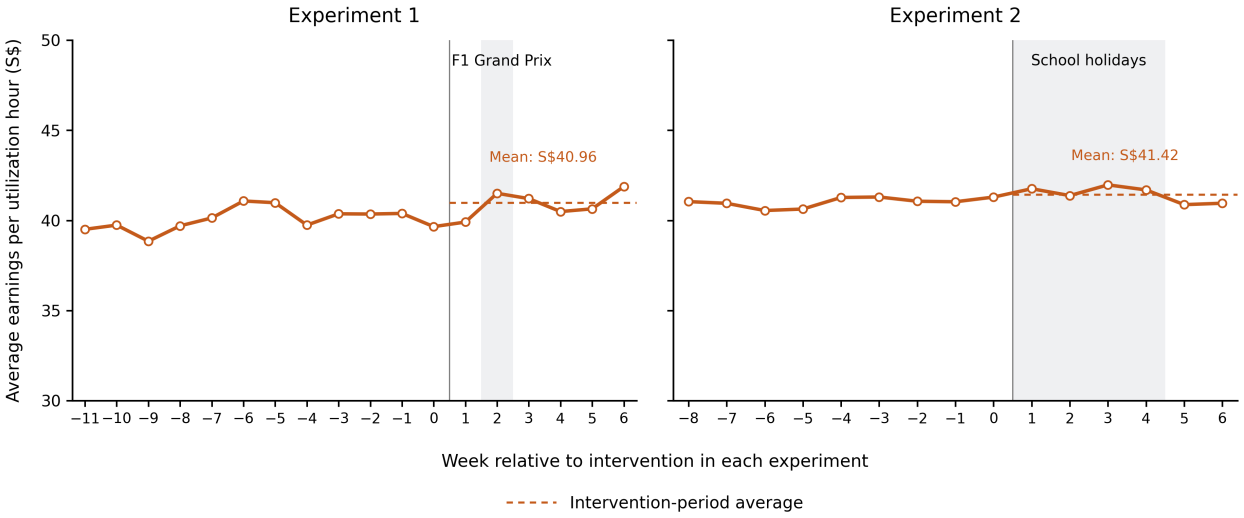
Trip returns. I next examine whether lower trip availability was accompanied by lower trip quality, measured by the returns from completing trips. Figures A3, A4, and A5 report earnings per completed trip, per utilization hour, and per kilometre, respectively. These measures were very similar across the two interventions, despite substantially lower trip-request availability in Experiment 2. This contrast motivates the focus on trip availability as the primary measure of market conditions.

Figure A3: Average Earnings per Trip Across Experiments



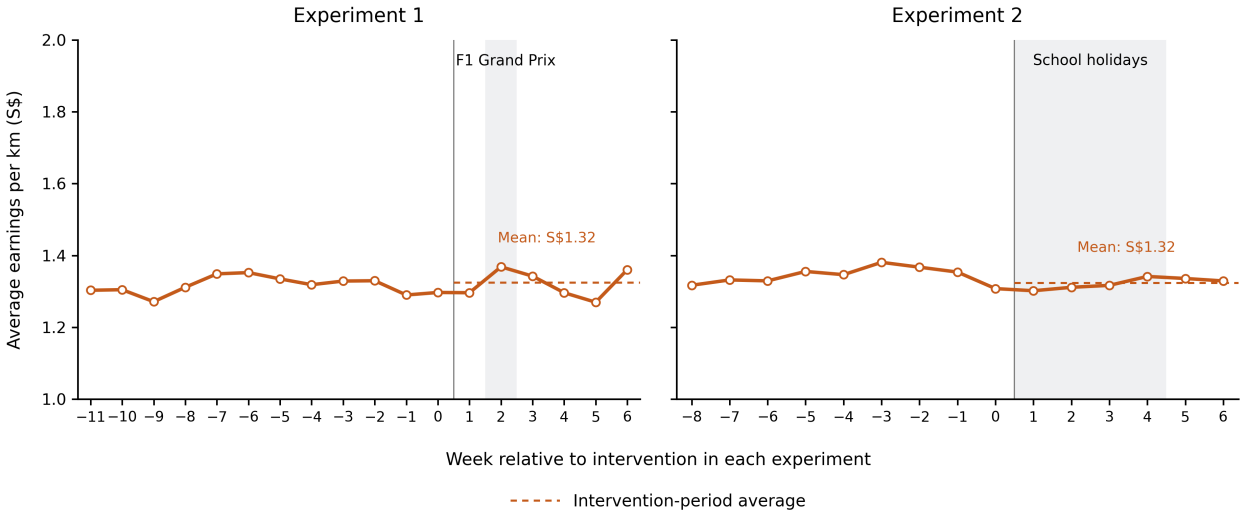
Notes. The figure plots average earnings per trip in each week, calculated as total earnings from completed trips divided by the number of completed trips. The sample consists of the same fixed cohort of nonexperimental drivers observed during both baseline periods. Week 0 is the final baseline week, and vertical lines separate the baseline and intervention periods. Shaded areas indicate the Formula 1 Grand Prix period in Experiment 1 and the school holiday period in Experiment 2.

Figure A4: Average Earnings per Utilization Hour Across Experiments



Notes. The figure plots the average earnings per utilization hour in each week, calculated as total earnings from completed trips divided by total utilization hours for those trips. The sample consists of the same fixed cohort of nonexperimental drivers observed during both baseline periods. Week 0 is the final baseline week, and vertical lines separate the baseline and intervention periods. Shaded areas indicate the Formula 1 Grand Prix period in Experiment 1 and the school holiday period in Experiment 2.

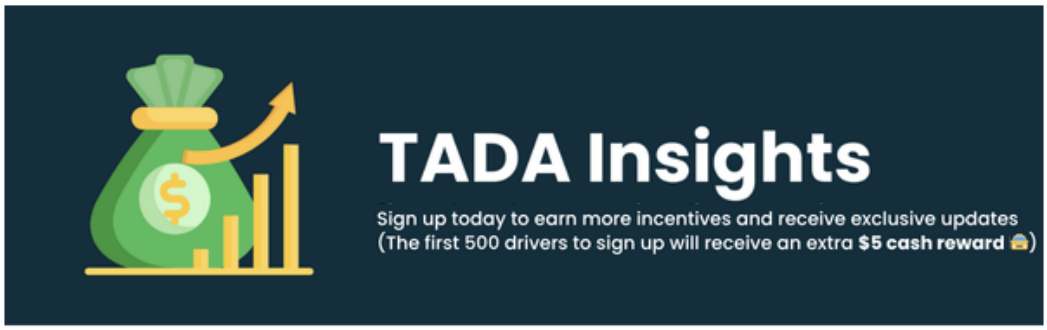
Figure A5: Average Earnings per Kilometre Across Experiments



Notes. The figure plots average earnings per kilometre in each week, calculated as total earnings from completed trips divided by total recorded distance. Distance includes travel to the pickup location and from pickup to drop-off. The sample consists of the same fixed cohort of nonexperimental drivers observed during both baseline periods. Week 0 is the final baseline week, and vertical lines separate the baseline and intervention periods. Shaded areas indicate the Formula 1 Grand Prix period in Experiment 1 and the school holiday period in Experiment 2.

A.3 Recruitment Materials and Program Participation

Figure A6: Recruitment Email



Dear [Driver Name],

We are inviting you to join **TADA Insights**, a new pilot program to help you **earn more** and **track your progress**. It is completely free to join, and you can opt out at any time.

If you join, you will:

- Receive a **$5 cash reward** if you are among the first 500 drivers to sign up
- Earn **extra incentives**, such as **5% more on every trip** on certain days, if you are selected
- Get **exclusive updates** on your driving activity

To sign up, simply complete a short 5-10 minute survey by Sunday, 24 Aug 2025. The survey is available in English, Chinese, Malay, and Tamil.

Take the Survey & Sign Up Here

Thank you for driving with TADA and we hope you will be part of this new program.

Best Regards,
TADA Team

Notes. This figure shows the recruitment email sent to eligible drivers inviting them to enroll in the new program "TADA Insights" before Experiment 1. The program was presented as a new initiative offering drivers additional earnings incentives and exclusive updates on their driving activity. Drivers enrolled by completing the onboarding survey linked in the email, in which their information demand was elicited subsequently.

Baseline Characteristics by Program Participation. Appendix Table A1 compares baseline characteristics of drivers who enrolled in the "TADA Insights" program with those who did not. Participants and nonparticipants are very similar in age and gender. Although differences in platform tenure and driver ratings are statistically significant, they are economically small in magnitude. The main difference is in baseline platform activity: participating drivers completed substantially more trips and earned more on the platform at baseline. Thus, program participation was concentrated among more active drivers, while the two groups were otherwise broadly similar on observed characteristics. I discuss the generalizability of the findings in Appendix Section D.

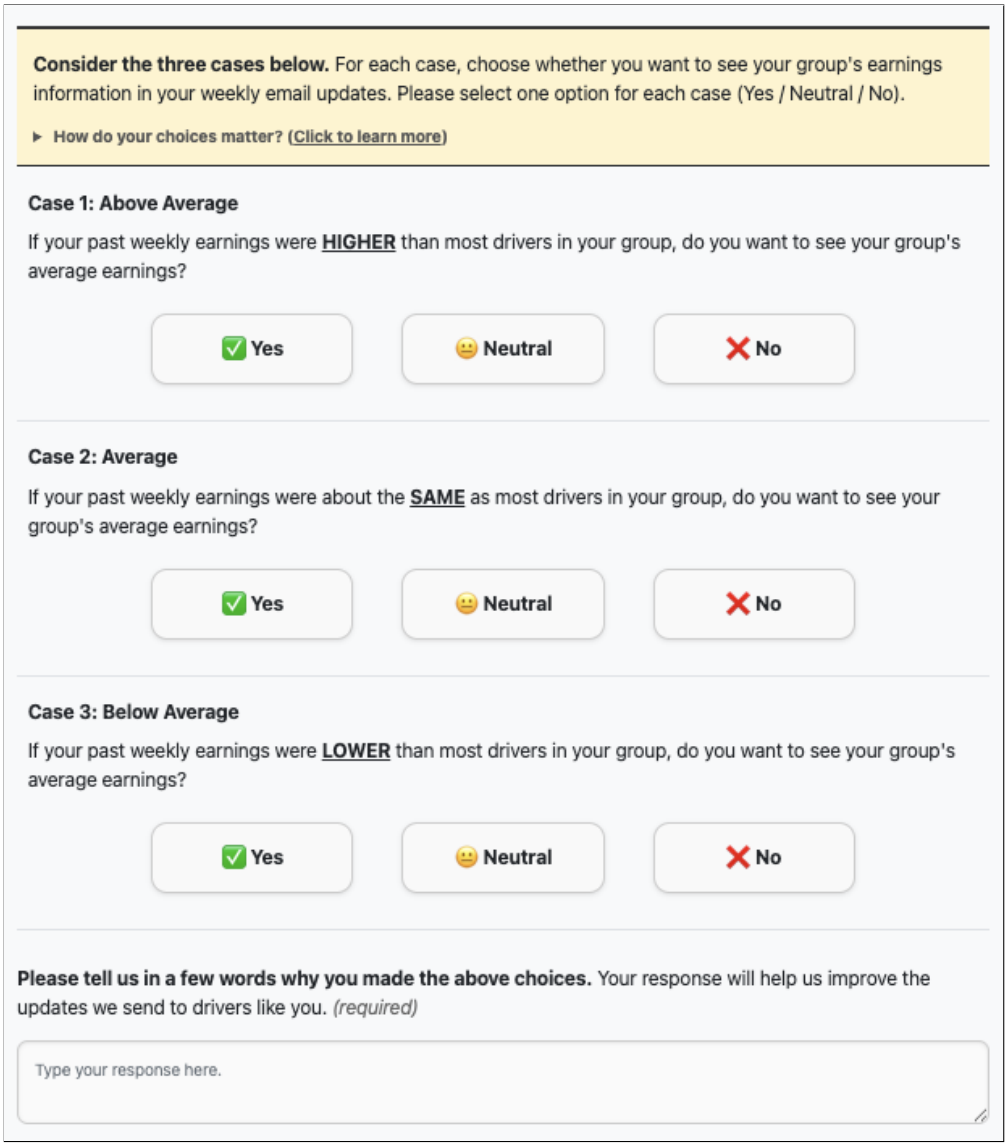
Table A1: Baseline Characteristics of Participating and Non-Participating Drivers

	Participants (1)	**Non-participants** (2)	**Difference in means** (3)
Demographics			
Age (Years)	50.754	50.923	-0.169
	(10.101)	(10.870)	
1 if Female	0.071	0.066	0.005
	(0.257)	(0.249)	
Baseline Labor Supply on Platform			
Average Weekly Earnings (S$)	474.279	225.714	248.565***
	(403.222)	(309.013)	
Average Weekly Trips Completed	29.910	13.409	16.501***
	(26.994)	(19.267)	
Driver Profile			
Tenure With Platform (Years)	3.256	3.374	-0.119**
	(2.380)	(2.258)	
Driver Rating (5-Point Scale)	4.951	4.960	-0.008***
	(0.079)	(0.075)	
No. of drivers	3,135	Undisclosed	–

Notes. Columns (1) and (2) report means, with standard deviations in parentheses, for participating and non-participating drivers, respectively. Participants include all enrolled drivers; the nonparticipating sample consists of drivers who did not enroll in the "TADA Insights" program. The exact size of the nonparticipating sample is withheld to protect commercial confidentiality. Column (3) reports difference in means, with stars indicating significance from two-sided two-sample t-tests. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.4 Elicitation of Information Demand

Figure A7: Information Demand Survey Module



Notes. This figure shows the baseline survey module used to elicit drivers' preferences for receiving peer earnings information. Using a strategy-method approach, drivers choose "Yes," "Neutral," or "No" in three contingent scenarios: when their historical weekly earnings were above, around, or below their reference group average. After making these choices, drivers are asked to provide a brief open-ended explanation. To make elicitation incentive-compatible, with probability 1/3, the choice corresponding to the driver's realized relative-earnings scenario was implemented; otherwise, assignment to peer information was random. In the implemented scenario, selecting "Yes" (respectively, "No") means the driver receives (does not receive) peer information and forgoes a small bonus (S$0.10). Selecting "Neutral" allows the driver to keep the bonus, with receipt of peer information determined randomly.

A.5 Summary Statistics and Balance

Table A2: Sample Characteristics and Balance Tests (Experiment 1)

	Full Sample (1)	**Control Group** (2)	**Peer Info Group** (3)	**Endogenous Info Group** (4)	**p-value (*F*-test)** (5)
Panel A: Demographics					
Age (Years)	50.702	50.895	50.688	50.524	0.718
	(10.019)	(10.089)	(10.027)	(9.949)	
1 if Female	0.073	0.076	0.067	0.077	0.670
	(0.260)	(0.265)	(0.250)	(0.266)	
1 if Chinese (Ethnicity)	0.795	0.802	0.792	0.791	0.789
	(0.404)	(0.398)	(0.406)	(0.407)	
Years of Education	12.205	12.188	12.137	12.290	0.478
	(2.806)	(2.854)	(2.709)	(2.854)	
Panel B: Baseline Platform Labor Supply					
Average Weekly Earnings (S$)	477.323	475.274	478.279	478.401	0.982
	(402.466)	(394.880)	(405.126)	(407.636)	
Average Weekly Trips Completed	30.134	29.633	30.381	30.384	0.781
	(27.018)	(25.943)	(27.561)	(27.531)	
Panel C: Driver Profile and Work Patterns					
Years of Driver Experience	6.057	6.014	6.252	5.905	0.417
	(5.904)	(5.854)	(6.117)	(5.735)	
Tenure with Platform (Years)	3.251	3.235	3.261	3.257	0.967
	(2.386)	(2.380)	(2.398)	(2.381)	
1 if Full-Time Driver	0.616	0.630	0.616	0.603	0.485
	(0.486)	(0.483)	(0.487)	(0.490)	
Average Hours Worked Per Day	5.414	5.456	5.384	5.402	0.548
	(1.495)	(1.482)	(1.523)	(1.481)	
Average Days Worked Per Week	5.814	5.799	5.856	5.787	0.433
	(1.242)	(1.247)	(1.194)	(1.285)	
Driver Rating (5-Point Scale)	4.951	4.953	4.951	4.950	0.701
	(0.079)	(0.077)	(0.082)	(0.077)	
Panel D: Other Self-Reported Measures					
1 if Multi-Homing	0.818	0.810	0.820	0.825	0.659
	(0.386)	(0.393)	(0.384)	(0.380)	
1 if Has Income Target	0.767	0.769	0.761	0.770	0.858
	(0.423)	(0.422)	(0.427)	(0.421)	
1 if on Online Driver Groups	0.523	0.530	0.530	0.510	0.590
	(0.500)	(0.499)	(0.499)	(0.500)	
No. of observations	2,897	961	969	967	–

Notes. Column (1) reports means and standard deviations for the full experimental sample. Columns (2) through (4) report the corresponding statistics for the *Control*, *Peer Info*, and *Endogenous Info* groups. Column (5) reports p-values from F-tests of the joint equality of means across the three groups.

Table A3: Balance Tests (Experiment 2)

	Control (1)	**Standard Peer Info** (2)	**Targeted Peer Info** (3)	***p*-value (*F*-test)** (4)
Panel A: Demographics				
Age (Years)	51.123	50.652	50.638	0.489
	(9.820)	(10.130)	(10.221)	
1 if Female	0.057	0.073	0.074	0.277
	(0.232)	(0.260)	(0.262)	
1 if Chinese (Ethnicity)	0.790	0.807	0.793	0.612
	(0.407)	(0.395)	(0.405)	
Years of Education	12.092	12.235	12.202	0.506
	(2.729)	(2.843)	(2.818)	
Panel B: Baseline Platform Labor Supply				
Average Weekly Earnings (S$)	424.941	422.731	427.473	0.967
	(408.858)	(402.670)	(394.825)	
Average Weekly Trips Completed	25.956	25.997	25.876	0.995
	(26.591)	(26.247)	(25.528)	
Panel C: Driver Profile and Work Patterns				
Years of Driver Experience	6.296	6.092	6.134	0.730
	(6.012)	(5.789)	(5.956)	
Tenure with Platform (Years)	3.249	3.349	3.334	0.608
	(2.320)	(2.389)	(2.381)	
1 if Full-Time Driver	0.631	0.615	0.615	0.704
	(0.483)	(0.487)	(0.487)	
Average Hours Worked Per Day	5.438	5.405	5.432	0.877
	(1.431)	(1.513)	(1.529)	
Average Days Worked Per Week	5.871	5.823	5.791	0.358
	(1.214)	(1.222)	(1.260)	
Driver Rating (5-Point Scale)	4.951	4.952	4.953	0.853
	(0.075)	(0.076)	(0.082)	
Panel D: Other Self-Reported Measures				
1 if Multi-Homing	0.830	0.813	0.822	0.597
	(0.376)	(0.390)	(0.382)	
1 if Has Income Target	0.771	0.757	0.772	0.680
	(0.420)	(0.429)	(0.420)	
1 if on Online Driver Groups	0.507	0.534	0.537	0.347
	(0.500)	(0.499)	(0.499)	
Panel E: Prior Experimental Measures				
1 if Info-Seeking	0.525	0.514	0.526	0.855
	(0.500)	(0.500)	(0.500)	
1 if Previously Treated	0.563	0.538	0.550	0.562
	(0.496)	(0.499)	(0.498)	
No. of observations	949	955	957	–

Notes. Columns (1)–(3) report means and standard deviations by information arm; Column (4) reports *p*-values from *F*-tests of joint equality of means across treatment arms. Baseline earnings and trips are averaged over the nine pre-intervention weeks. Information preferences were elicited before Experiment 1. *Previously Treated* indicates receipt of peer information in Experiment 1 or its initial pilot. The sample contains 2,861 drivers and varies slightly across rows because of missing values.

A.6 Temporal Stability in Information Demand: Endline Survey

Following the six-week intervention in Experiment 2, participating drivers were invited by email in July 2026 to complete a short endline survey. The survey re-elicited drivers' demand for peer earnings information and collected additional self-reported outcomes.

Endline Survey Participation. Of the 2,861 drivers in the Experiment 2 experimental sample, 1,320 completed the endline survey, corresponding to a response rate of 46.1%. Appendix Table A4 shows that response rates are similar across the *Control*, *Standard Peer Info*, and *Targeted Peer Info* groups in Experiment 2, and equality across treatment arms cannot be rejected ($p = .963$). The same result holds when examining response rates separately by baseline info-seeking status. Thus, there is no evidence of differential endline survey participation by treatment assignment.

Table A4: Endline Survey Response Rates

	Control (1)	Standard Peer Info (2)	Targeted Peer Info (3)	p-value (4)
Panel A: Full sample				
Survey response rate	0.463	-0.005	0.001	0.917
		[0.828]	[0.971]	
Joint F-test: equal response rate across arms				0.963
No. of drivers	949	955	957	
Panel B: Info-seeking drivers				
Survey response rate	0.472	-0.027	-0.014	0.450
		[0.384]	[0.654]	
Joint F-test: equal response rate across arms				0.685
No. of drivers	498	491	503	
Panel C: Non-info-seeking drivers				
Survey response rate	0.452	0.018	0.017	0.534
		[0.587]	[0.594]	
Joint F-test: equal response rate across arms				0.824
No. of drivers	451	464	454	

Notes. This table reports response rates for the endline survey following Experiment 2. Panel A includes all 2,861 drivers in the Experiment 2 experimental sample. Panels B and C restrict the sample to info-seeking and non-info-seeking drivers, respectively. Column (1) reports the raw response rate in the *Control* group. Columns (2) and (3) report treatment–control differences from a joint OLS regression with separate indicators for the *Standard Peer Info* and *Targeted Peer Info* groups; corresponding p-values are reported in brackets. Column (4) reports the p-value from a test of equal response rates between the pooled peer-information group and the *Control* group. The F-test row reports the joint test of equal response rates across all three treatment arms. The number of drivers includes both survey respondents and nonrespondents. All regressions include randomization-stratum fixed effects and use robust standard errors.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Characteristics of Endline Survey Respondents. Table A5 compares observed characteristics of endline survey respondents and nonrespondents. The share of drivers classified as info-seeking is nearly identical across the two groups (52.2% vs 52.1%), and the share previously assigned to peer information in Experiment 1 does not differ significantly. However, respondents were substantially more active on the platform at baseline, earning an average of S$510 and completing 31.5 trips per week, compared with S$353 and 21.2 trips among nonrespondents. They also differ from nonrespondents along several demographic characteristics. These differences warrant caution when generalizing the survey findings to the full experimental sample.

Temporal Stability in Information Demand. In the endline survey following Experiment 2, I re-elicited drivers' demand for peer earnings information using the same three relative-earnings scenarios as at baseline (Figure A7). The endline elicitation differed from the baseline measure in two respects. First, the endline information choices were not incentivized. Second, they concerned hypothetical earnings updates in the future. Drivers were asked to consider a setting in which the firm might provide weekly in-app updates and whether they would also want to receive information about the average earnings of ten similar drivers when their own earnings were above, around, or below their peer average. In addition, the endline elicitation occurred after drivers had already participated in the "TADA Insights" program, so their stated demand may partly reflect their experience with it.

With these caveats in mind, Figure A8 documents meaningful, though imperfect, temporal stability in information demand. Panel (a) shows that baseline info-seeking status is strongly predictive of endline info-seeking status nearly one year later. Among drivers classified as info-seeking at baseline, 50.1% choose to receive peer information in at least one scenario at endline, compared with 23.5% of baseline non-info-seeking drivers. The difference of 26.6 percentage points is statistically significant ($p < .001$), and the Pearson correlation between the baseline and endline info-seeking indicators is 0.28.

Panel (b) further examines persistence within each relative-earnings scenario while retaining all three response categories, ordered *No* < *Neutral* < *Yes*. The baseline-endline Spearman rank correlations range from 0.25 to 0.28 across the three scenarios ($p < .001$ for all). As a benchmark, Meier and Sprenger (2015) report one-year correlations of 0.25 for individual discount factors (δ) and 0.36 for present bias parameters (β) as evidence of meaningful temporal stability in time preferences. The correlations in information demand documented here are therefore of a similar order of magnitude.

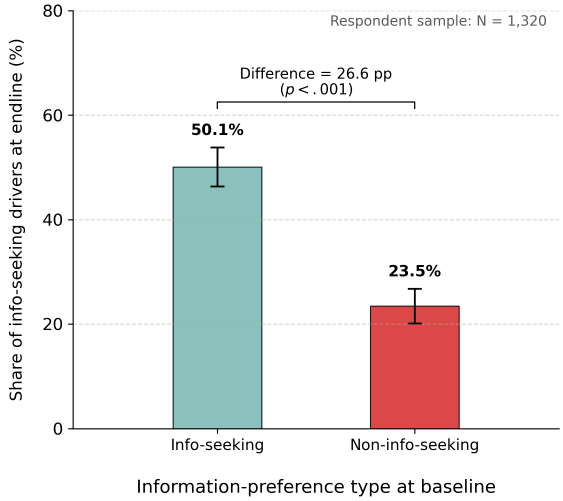
Table A5: Characteristics of Endline Survey Respondents and Nonrespondents

	Respondent sample (1)	Nonrespondent sample (2)	Difference in means (3)
Panel A: Demographics			
Age (Years)	50.907	50.716	0.191
	(9.764)	(10.306)	
1 if Female	0.054	0.080	-0.025***
	(0.227)	(0.271)	
1 if Chinese (Ethnicity)	0.840	0.760	0.080***
	(0.367)	(0.427)	
Years of Education	12.425	11.963	0.462***
	(2.731)	(2.836)	
Panel B: Baseline Platform Labor Supply			
Average Weekly Earnings (S$)	509.607	352.620	156.987***
	(412.405)	(378.257)	
Average Weekly Trips Completed	31.458	21.219	10.239***
	(26.997)	(24.371)	
Panel C: Driver Profile and Work Patterns			
Years of Driver Experience	6.004	6.318	-0.314
	(5.481)	(6.267)	
Tenure With Platform (Years)	3.423	3.214	0.209**
	(2.339)	(2.381)	
1 if Full-Time Driver	0.603	0.635	-0.032*
	(0.489)	(0.482)	
Average Hours Worked Per Day	5.377	5.466	-0.089
	(1.499)	(1.484)	
Average Days Worked Per Week	5.858	5.803	0.054
	(1.224)	(1.239)	
Driver Rating (5-Point Scale)	4.950	4.954	-0.004
	(0.077)	(0.078)	
Panel D: Other Self-Reported Measures			
1 if Multi-Homing	0.808	0.834	-0.026*
	(0.394)	(0.372)	
1 if Has Income Target	0.767	0.767	0.000
	(0.423)	(0.423)	
1 if on Online Driver Groups	0.557	0.500	0.057***
	(0.497)	(0.500)	
Panel E: Prior Experimental Measures			
1 if Info-Seeking	0.522	0.521	0.001
	(0.500)	(0.500)	
1 if Previously Treated	0.557	0.544	0.012
	(0.497)	(0.498)	
No. of drivers	1,320	1,541	–

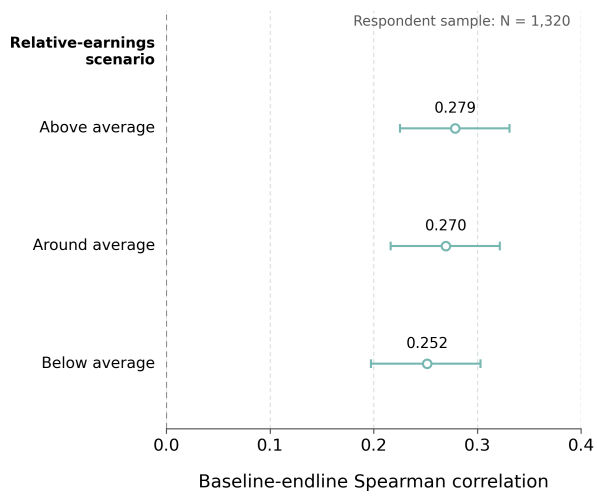
Notes. Columns (1) and (2) report means, with standard deviations in parentheses, for endline survey respondents and nonrespondents in the Experiment 2 analysis sample. Column (3) reports differences in means, with stars indicating significance from two-sided two-sample t-tests.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A8: Temporal Stability in Information Demand

(a) Endline Info-Seeking Status by Baseline Type



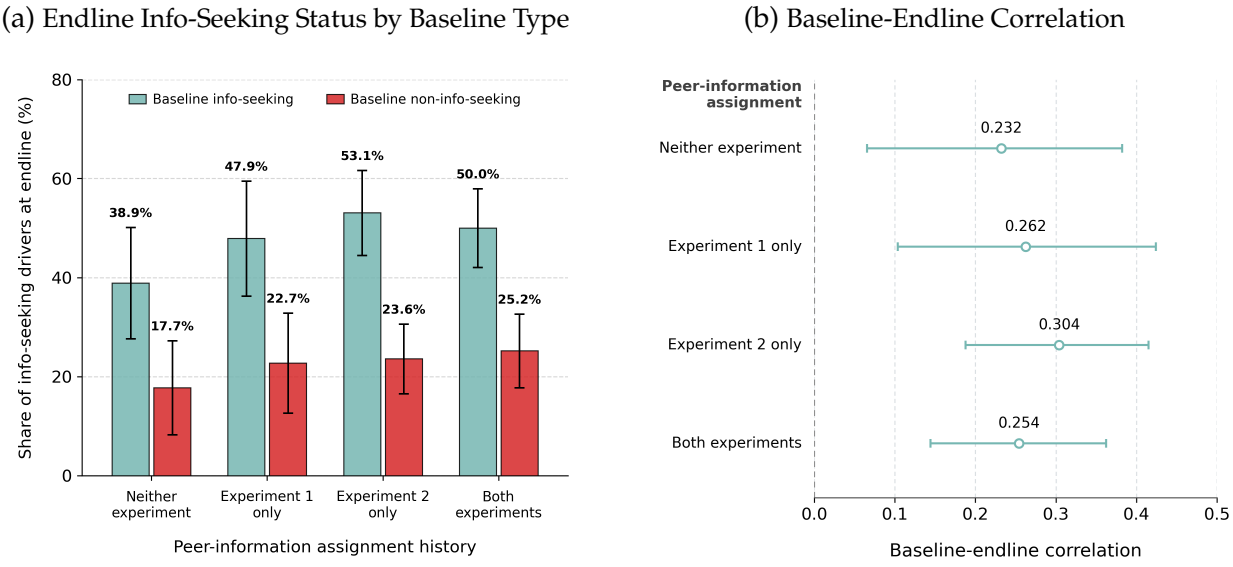
(b) Baseline-Endline Rank Correlation by Scenario



Notes. Panel (a) reports the share of drivers classified as *info-seeking* at endline, separately for drivers classified as info-seeking and non-info-seeking at baseline. Info-seeking drivers are those who choose "Yes" in at least one of the three relative-earnings scenarios; non-info-seeking drivers are those who never choose "Yes". Panel (b) reports Spearman rank correlations between baseline and endline information choices separately for scenarios in which own earnings are above, around, or below the peer-group average. Error bars represent 95% confidence intervals. The respondent sample consists of 1,320 drivers who completed the endline survey following Experiment 2.

Persistence by Peer Information Assignment History. Because the endline elicitation occurred after drivers had participated in both experiments, I further examine whether persistence in information demand varies with drivers' peer information assignment history. Figure A9 distinguishes four groups: drivers randomly assigned to peer information in neither experiment, Experiment 1 only, Experiment 2 only, or both experiments. Baseline information demand remains predictive of endline information demand across all four assignment histories, with correlations ranging from 0.23 to 0.30. A joint test does not reject that the difference in endline info-seeking rates between baseline info-seeking and non-info-seeking drivers is the same across the four assignment histories ($p = .840$).

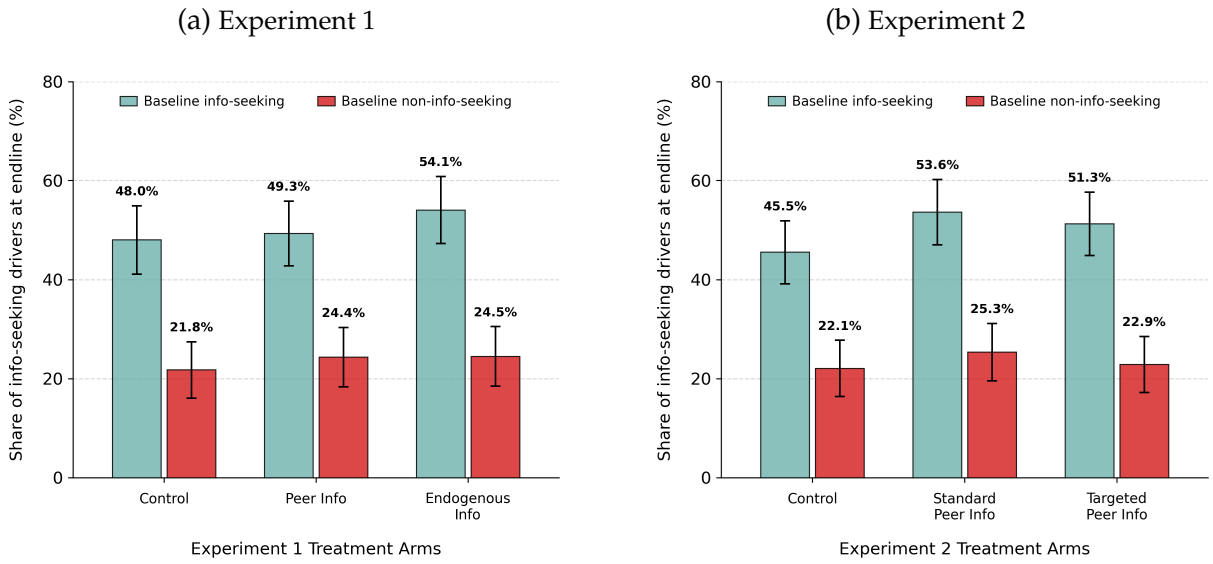
Figure A9: Temporal Stability by Peer Information Assignment History



Notes. The figure reports temporal stability in information demand across four peer information assignment histories: neither experiment, Experiment 1 only, Experiment 2 only, and both experiments. Panel (a) reports the share of drivers classified as *info-seeking* at endline, separately by baseline info-seeking status within each treatment assignment history. Panel (b) reports Pearson correlations between the binary baseline and endline info-seeking indicators within each assignment history. Error bars represent 95% confidence intervals. The sample consists of 824 Experiment 2 endline respondents from the *Control* and *Peer Info* groups of the Experiment 1 sample; the *Endogenous Info* group is excluded. The *Standard Peer Info* and *Targeted Peer Info* groups in Experiment 2 are pooled.

Persistence by Experimental Treatment Arm. Figure A10 reports endline information demand separately by information treatment arm in each experiment. Within each arm, baseline info-seeking drivers remain substantially more likely than baseline non-info-seeking drivers to seek peer information at endline, with differences ranging from 23.5 to 29.6 percentage points ($p < .001$ for all).

Figure A10: Endline Information Demand by Treatment Arm in Each Experiment



Notes. Both panels report the share of drivers classified as *info-seeking* at endline, separately by baseline info-seeking status and information treatment arm. Panel (a) groups 1,229 endline respondents from the Experiment 1 main sample by their original assignment to *Control*, *Peer Info*, or *Endogenous Info*. Panel (b) groups all 1,320 Experiment 2 endline respondents by assignment to *Control*, *Standard Peer Info*, or *Targeted Peer Info*. Each panel pools across treatment arms in the other experiment. Error bars represent 95% confidence intervals.

A.7 Coding and Validation of Stated Motives

Table A6: Coding Scheme with Example Responses

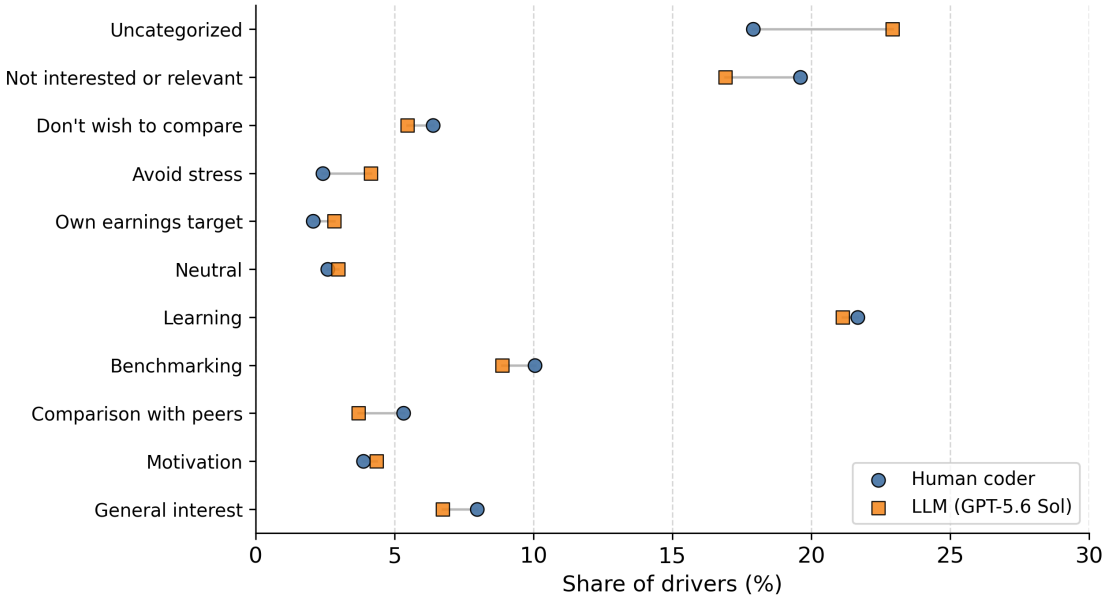
Category	Definition	Example Responses
Uncategorized	The driver's response is blank, unintelligible, off-topic, or otherwise cannot be classified.	"N/A"; "No comment"
Not Interested or Relevant	The driver indicates that they are not interested in other drivers' earnings or view the information as irrelevant, unnecessary, or not useful. This category also includes responses expressing privacy concerns, a preference not to know for reasons unrelated to stress, or the view that the information is not meaningful for their circumstances (e.g., working part-time).	"It does not matter to me how much others earn."; "Knowing the results will have zero impact on my earnings and the way I am currently driving."; "Not interested to know about other drivers' earnings."; "I'm a part-time driver, thus may not be comparable to others."
Do Not Wish to Compare	The driver indicates that they do not want to compare their earnings with those of other drivers.	"I do not want to compare my earnings with others."; "I drive at my own pace, . . . there's no comparison needed."; "There is no point comparing."
Avoid Stress	The driver indicates that knowing other drivers' earnings could cause stress, discouragement, or disappointment, or other negative effects on their well-being.	"Don't want to stress out about who's earning more or who's earning less."; "Can be discouraging."; "Don't want to have unnecessary stress which may lead to accident."
Own Target	The driver indicates that they rely on their own earnings goal or target to evaluate their performance, rather than information about other drivers' earnings.	"I have my own daily targets."; "I'm contented with hitting the goals that I set out for myself."; "I set my own target; I don't need to know what other drivers are earning."
Neutral	The driver indicates that they have no strong preference and are comfortable with having or not having the information.	"I'm fine with both, actually."; "I have no preference."; "I don't mind either way; the information isn't very important to me."
Learning	The driver indicates that they would use the information to learn from other drivers, better understand market conditions or attainable earnings, identify opportunities for improvement, or adjust their driving behavior or strategy to improve earnings.	"The drivers' earnings will also inform us of market/platform conditions."; "At least can know the market trend vs. own self performance."; "Knowing how much other drivers earn can let us have a feel of actual supply and demand. This also allows us to optimise our pickup strategy (e.g. timeslot and location choices)."; "Information is good to know to optimise driving hours and platform usage."
Benchmarking	The driver indicates that they would use average earnings as a benchmark or reference point for evaluating their own earnings, without explicitly emphasizing comparison with other drivers.	"It's a good gauge to see whether I'm doing it right."; "Average I think is fair, as a reference."; "Regardless of what you earn, it is still good to know the benchmark."
Comparison with Peers	The driver specifically indicates that they want to compare their earnings with those of other drivers.	"I want to know how I compare to my peers."; "I [am] able to compare if earn higher or lower."; "I want to know how much I earn as compared to the other drivers who are in my same group."
Motivation	The driver indicates that the information would motivate or push them to work harder, without indicating that it would teach them something or help them identify how to improve.	"I just want to see the average so I can push myself."; "It will spur me to work harder."; "It would help to motivate me to aim for higher earnings."
General Interest	The driver indicates that they are generally interested in or curious about the information without identifying a specific use.	"I would like to know."; "Always good to get more info."; "I'm just keen to see how this works out."

Notes. This table provides an overview of the qualitative coding scheme used to categorize drivers' open-ended responses. Each driver's response is assigned to exactly one category based on its primary stated motive.

LLM Validation. As a validation exercise, I used a large language model (GPT-5.6 Sol) to independently classify drivers' open-ended responses. The LLM received the category definitions and illustrative examples reported in Table A6, but not the research assistant's classifications, and assigned each response to exactly one primary category. Figure A11 compares the share of responses assigned to each category by the research assistant and the LLM. The resulting distributions are broadly similar.

Across the 11 categories, correlations between the corresponding human- and LLM-coded indicators range from 0.53 to 0.77. Overall agreement is also substantial: Cohen's κ is 0.64, comparable to the mean human–LLM κ of 0.60 reported in a similar validation exercise by Bursztyn et al. (2026). Restricting the sample to responses that both coders assigned to a substantive category (i.e., excluding *Uncategorized*) yields a Cohen's κ of 0.70.

Figure A11: Human and LLM Coding of Drivers' Stated Motives



Notes. This figure compares the share of drivers assigned to each motive by the research assistant and the LLM (GPT-5.6 Sol). Both use the coding scheme in Appendix Table A6. The LLM was supplied with the category definitions and illustrative examples but not the research assistant's classifications. Circles denote the research assistant's coding, squares denote the LLM coding, and lines connect the corresponding shares for each category.

A.8 Predicting Information Demand from Observables

This appendix subsection examines whether drivers' elicited information demand can be predicted from characteristics observed at baseline. Table A7 reports linear probability models in which the dependent variable is an indicator for whether a driver is info-seeking. Column 1 includes demographic characteristics only; Column 2 adds baseline earnings and completed trips; Column 3 adds driving experience, platform tenure, usual work patterns, and driver ratings; and Column 4 adds indicators for multihoming, income-targeting, and membership in online driver groups and communities.

Several baseline characteristics are individually associated with information demand. In Column 4, the richest specification, female drivers are 9.1 percentage points (pp) less likely to be info-seeking, while each additional year of education is associated with a 2.8 pp higher likelihood of being info-seeking. Among the additional self-reported measures, drivers who multihome are 8.7 pp more likely to be info-seeking, while those who are members of online driver groups are 7.2 pp more likely to be info-seeking.

By contrast, baseline platform activity, measured by average weekly earnings and trips completed, is not significantly associated with information demand. Despite these individual associations, the richest specification explains only 5.5% of the variation in info-seeking status.

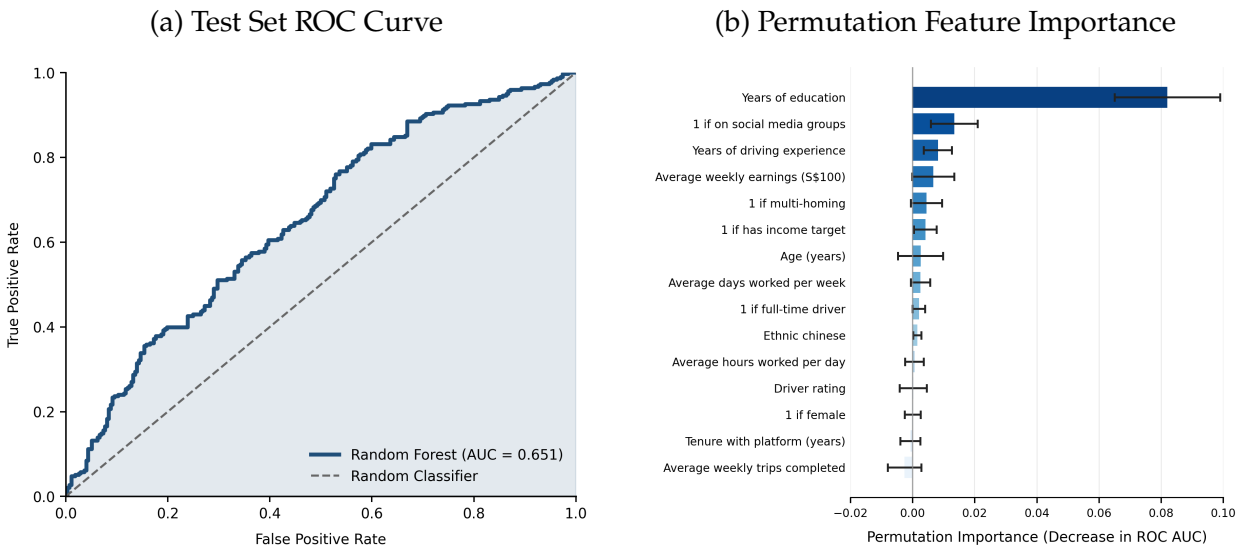
To allow for nonlinear relationships and interactions among the observables, I also estimate a tuned random forest classifier using the same baseline characteristics. Figure A12 reports performance on a held-out test set. The model achieves an AUC of 0.65, indicating only modest out-of-sample predictive power. Panel (b) reports the corresponding permutation-based feature importance measures. Education has the highest predictive importance, followed by membership in driver social media groups and years of driving experience, with substantially smaller importance values for the remaining characteristics.

Table A7: Correlates of Information Demand

Dependent variable:	1 if classified as "info-seeking"			
	(1)	(2)	(3)	(4)
Demographics				
Age (Years)	-0.001	-0.001	-0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)
1 if Female	-0.096***	-0.090**	-0.092***	-0.091**
	(0.035)	(0.035)	(0.036)	(0.035)
1 if Chinese (Ethnicity)	0.016	0.011	0.007	-0.003
	(0.023)	(0.023)	(0.023)	(0.023)
Years of Education	0.030***	0.030***	0.030***	0.028***
	(0.003)	(0.003)	(0.004)	(0.004)
Baseline Labor Supply on Platform				
Average Weekly Earnings (S$100)		0.007	0.003	0.004
		(0.008)	(0.008)	(0.008)
Average Weekly Trips Completed		-0.000	0.000	0.000
		(0.001)	(0.001)	(0.001)
Driver Profile and Work Patterns				
Years of Driver Experience			-0.007***	-0.006***
			(0.002)	(0.002)
Tenure with Platform (Years)			0.012***	0.010**
			(0.004)	(0.004)
1 if Full-Time Driver			0.034	0.032
			(0.024)	(0.024)
Average Hours Worked Per Day			0.016**	0.011
			(0.008)	(0.008)
Average Days Worked Per Week			-0.008	-0.012
			(0.008)	(0.008)
Driver Rating (5-Point Scale)			-0.024	-0.031
			(0.119)	(0.120)
Other Self-Reported Measures				
1 if Multi-Homing				0.087***
				(0.025)
1 if Has Income Target				0.043*
				(0.022)
1 if on Online Driver Groups				0.072***
				(0.019)
R-squared	0.033	0.035	0.043	0.055
No. of observations	2,897	2,897	2,840	2,840

Notes. This table reports correlates of info-seeking status from linear probability models where the dependent variable equals one if a driver is classified as "info-seeking" (i.e., chooses to receive peer information in at least one relative-earnings scenario). Robust standard errors in parentheses. The reduction in observations from Columns (2) to (3) reflects missing driver ratings for a small subset of drivers with insufficient rated trips. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A12: Out-of-Sample Prediction of Information Demand



Notes. This figure reports out-of-sample predictive performance from a tuned random forest classifier that predicts whether a driver is info-seeking using the full set of baseline observables. Panel (a) shows the receiver operating characteristic (ROC) curve evaluated on a held-out test set, with an area under the curve (AUC) of 0.65, indicating modest predictive performance. Panel (b) reports permutation-based feature importance, measured as the average decrease in AUC when each variable is randomly permuted. Higher values indicate greater predictive importance. The error bars display ± one standard deviation across 30 permutation repeats.

A.9 Effects of Monetary Incentives by Information Preference

Table A8: Effects of Monetary Incentives on Daily Labor Supply

Dependent variable:	Platform labor supply	Extensive margin	Intensive margins (conditional on working)		
	Hours	Working	Hours	Trips	Earnings
	(1)	(2)	(3)	(4)	(5)
Panel A: Average Treatment Effects by Experiment					
Monetary incentive: Experiment 1 (γ_1)	0.38***	0.02**	0.52***	1.59***	17.80***
	(0.03)	(0.01)	(0.03)	(0.09)	(1.23)
Monetary incentive: Experiment 2 (γ_2)	0.03*	0.01	0.04	0.16***	0.54
	(0.02)	(0.01)	(0.02)	(0.06)	(0.90)
Panel B: Treatment Effects by Experiment and Information Preferences					
Experiment 1 × Info-seeking (γ_{1S})	0.38***	0.02**	0.49***	1.54***	16.79***
	(0.04)	(0.01)	(0.04)	(0.12)	(1.62)
Experiment 1 × Non-info-seeking (γ_{1N})	0.39***	0.01	0.54***	1.65***	18.93***
	(0.04)	(0.01)	(0.05)	(0.12)	(1.81)
Experiment 2 × Info-seeking (γ_{2S})	0.02	0.00	0.03	0.12	0.19
	(0.02)	(0.01)	(0.03)	(0.08)	(1.23)
Experiment 2 × Non-info-seeking (γ_{2N})	0.04*	0.01	0.04	0.21**	0.93
	(0.02)	(0.01)	(0.03)	(0.09)	(1.23)
Baseline mean: Experiment 1	1.68	0.63	2.67	6.66	105.46
Baseline mean: Experiment 2	1.46	0.58	2.52	6.33	103.47
p-values of equality tests:					
Experiment 1: $\gamma_{1S} = \gamma_{1N}$	0.865	0.461	0.407	0.506	0.366
Experiment 2: $\gamma_{2S} = \gamma_{2N}$	0.569	0.620	0.714	0.442	0.660
No. of observations	543,585	543,585	319,814	319,814	319,814
No. of drivers: Experiment 1	1,930	1,930	1,869	1,869	1,869
No. of drivers: Experiment 2	2,861	2,861	2,611	2,611	2,611

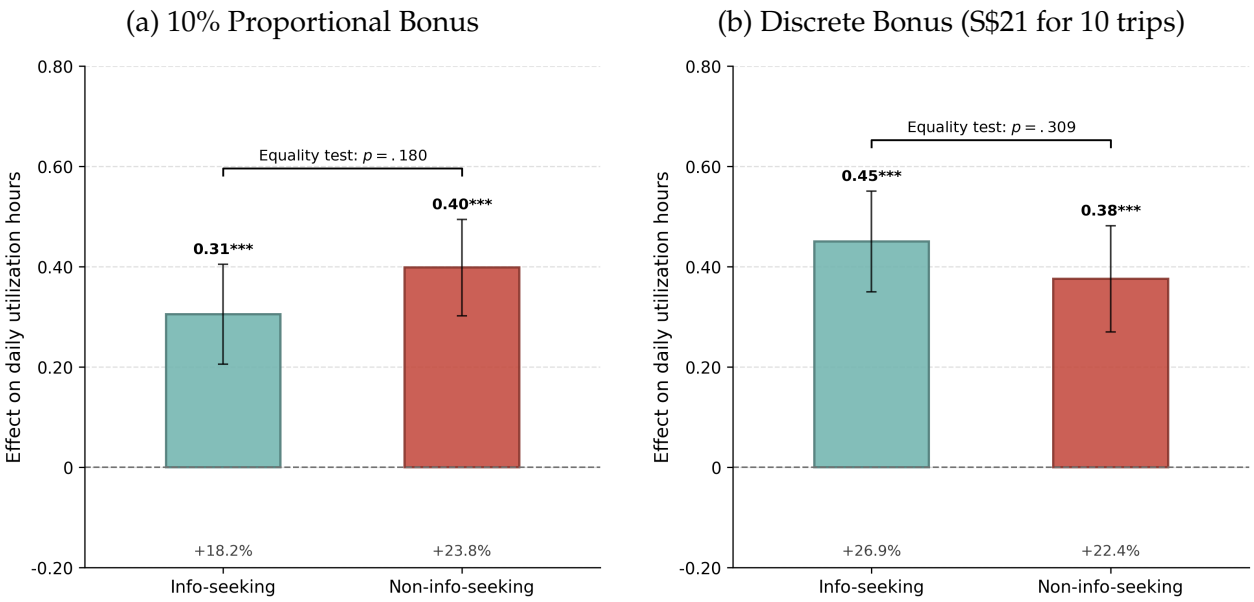
Notes. This table reports the effects of monetary incentives on daily platform labor supply for both experiments. Panel A reports average treatment effects; Panel B reports effects by elicited information-preference type. The two randomized incentive offers are pooled within each experiment. The estimation samples and specifications match those in Table 1. All outcomes cover the full day, including in Experiment 2, when incentives applied during a four-hour window. In Column 1, *Hours* denotes total daily utilization hours. *Working* equals one if at least one completed or canceled trip was recorded. Columns 3–5 report utilization hours, completed trips, and trip earnings conditional on *Working*. Earnings are reported in S$ and exclude incentive payouts. All specifications include driver-by-experiment and date fixed effects, experiment-specific Post × Info-seeking interactions, and experiment-specific controls for the randomized peer-information treatment. In Panel B, peer-information effects also vary by information-preference type. Standard errors clustered at the driver level are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A9: Effects of Monetary Incentives on Labor Supply During Incentive Window (Experiment 2)

Dependent variable:	Platform labor supply	Extensive margin	Intensive margins (conditional on working)		
	Hours	Working	Hours	Trips	Earnings
	(1)	(2)	(3)	(4)	(5)
Panel A: Average Treatment Effects					
Incentive (γ)	0.04***	0.01	0.14***	0.48***	4.74***
	(0.01)	(0.01)	(0.02)	(0.06)	(0.73)
Panel B: Treatment Effects by Information Preferences					
Incentive × Info-seeking (γ_S)	0.04***	0.01	0.13***	0.42***	4.25***
	(0.01)	(0.01)	(0.03)	(0.09)	(1.01)
Incentive × Non-info-seeking (γ_N)	0.04***	0.01	0.15***	0.55***	5.22***
	(0.01)	(0.01)	(0.03)	(0.09)	(1.03)
Baseline mean	0.25	0.26	0.97	2.64	34.50
p-values of equality test:					
Incentive effects equal: $\gamma_S = \gamma_N$	0.982	0.769	0.670	0.314	0.498
No. of observations	42,915	42,915	10,478	10,478	10,478
No. of drivers	2,861	2,861	1,517	1,517	1,517

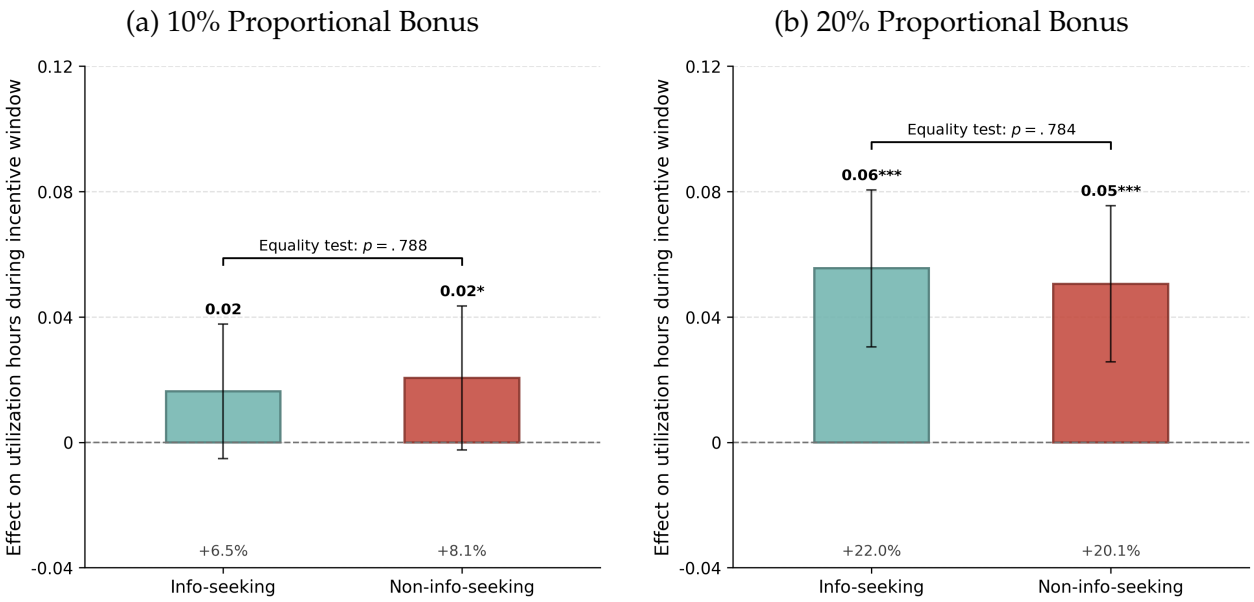
Notes. This table reports the effects of assignment to either monetary incentive during the 11 am–3 pm window in Experiment 2. The sample includes nine baseline and six intervention Wednesdays. Baseline, explicit no-incentive, and washout Wednesdays are coded as untreated. In Column 1, *Hours* denotes utilization hours during the window and equals zero when no completed work is observed. *Working* equals one if the driver accepted at least one trip request during the window. Columns 3–5 report utilization hours, completed trips, and trip earnings during the window conditional on *Working*. Earnings are reported in S$ and exclude incentive payouts. All specifications include driver and date fixed effects, Post × Info-seeking, and controls for the randomized peer-information treatment. In Panel B, peer-information effects are allowed to differ by information-preference type. Standard errors clustered at the driver level are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figure A13: Effects of Monetary Incentives by Incentive Type and Information Preference (Experiment 1)



Notes. This figure reports the effects of the two monetary incentives in Experiment 1 separately for info-seeking and non-info-seeking drivers. The outcome is total daily utilization hours. The two incentive indicators are included jointly in the regression, with their effects allowed to vary by information-preference type. All specifications include driver and date fixed effects. Percentages express effects relative to the Experiment 1 baseline mean. Equality tests compare the incentive effects between info-seeking and non-info-seeking types. Error bars represent 95% confidence intervals based on standard errors clustered at the driver level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure A14: Effects of Monetary Incentives by Incentive Type and Information Preference (Experiment 2)

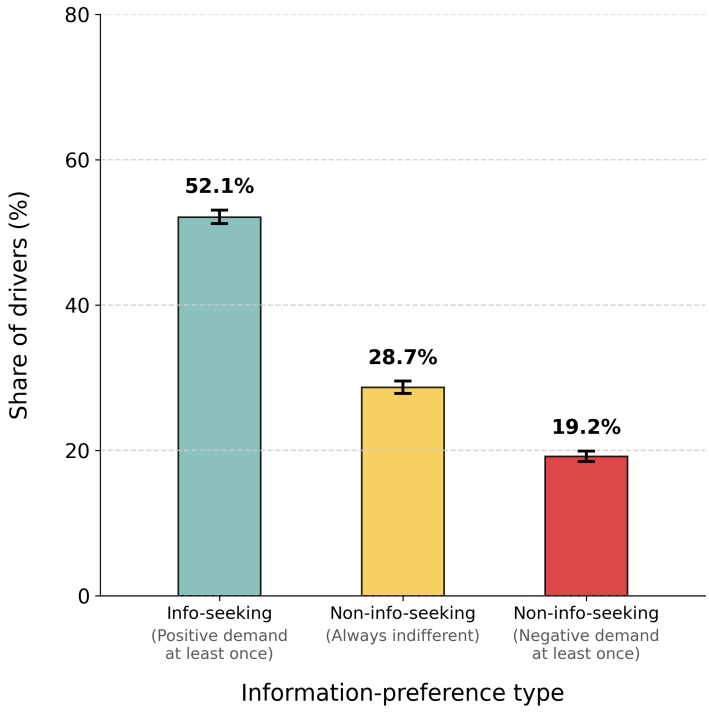


Notes. This figure reports the effects of the two monetary incentives in Experiment 2 separately for info-seeking and non-info-seeking drivers. The outcome is utilization hours during the 11am–3pm incentive window. The two incentive indicators are included jointly in the regression, with their effects allowed to vary by information-preference type. All specifications include driver and date fixed effects. Percentages express effects relative to the Experiment 2 baseline mean for the incentive window. Equality tests compare the incentive effects between info-seeking and non-info-seeking types. Error bars represent 95% confidence intervals based on standard errors clustered at the driver level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.10 Disaggregating the Non-Info-Seeking Group

In the main analysis, I classify drivers as *info-seeking* if they express strict positive demand for peer information in at least one of the three relative-earnings scenarios and as *non-info-seeking* otherwise. As a robustness check, I examine whether this binary classification masks meaningful heterogeneity by further splitting the non-info-seeking group into drivers who are always indifferent and those who express strict negative demand in at least one scenario. Figure A15 shows the resulting three-way classification. Info-seeking drivers comprise 52.1% of the sample, while 28.7% are always indifferent, and 19.2% fall into the negative demand group.

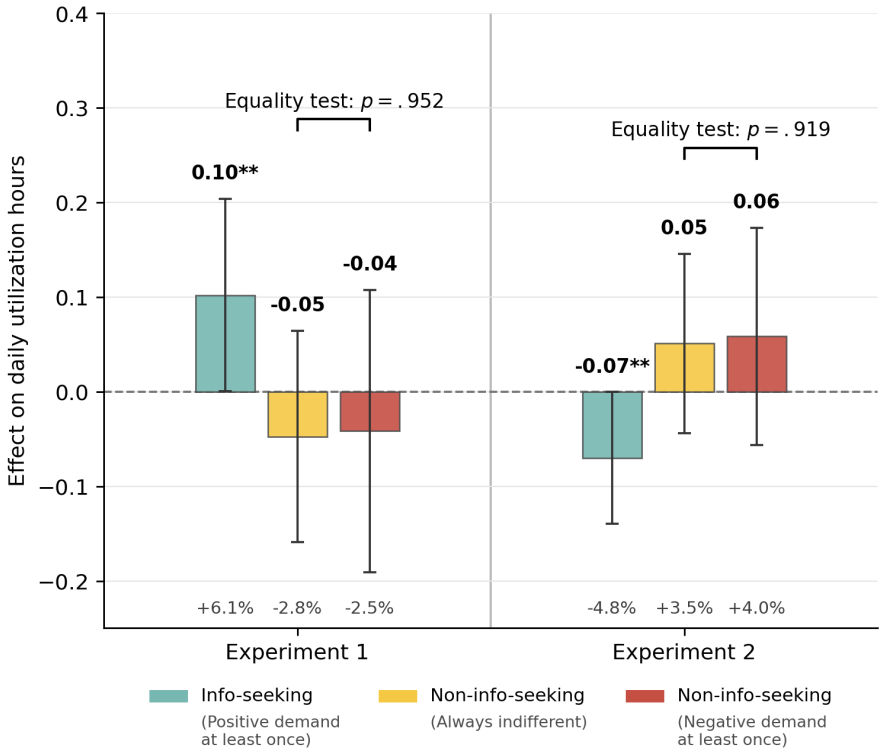
Figure A15: Distribution of Information Preferences: Finer Classification



Notes. This figure reports the share of drivers across three information-preference types. Info-seeking drivers are those who choose "Yes" in at least one of the three relative-earnings scenarios (below, around, or above average relative to their assigned peer group); non-info-seeking drivers are those who never choose "Yes". Among non-info-seeking drivers, I further distinguish between those who are indifferent in all three scenarios and those who choose "No" in at least one scenario. Error bars display ± one standard error of the mean.

Figure A16 re-estimates the effects of peer information on daily utilization hours, using this finer classification. The treatment effects are very similar across the two non-info-seeking subgroups, supporting their pooling in the main analysis. In Experiment 1, the estimated effects are −0.05 hours among always-indifferent drivers and −0.04 hours among negative-demand drivers; neither estimate is statistically significant, and equality of the two effects cannot be rejected ($p = .952$). In Experiment 2, the corresponding estimates are 0.05 and 0.06 hours; neither is statistically significant, and equality of the two effects again cannot be rejected ($p = .919$). Thus, there is little evidence that the binary classification used in the main analysis masks differential responses within the non-info-seeking group.

Figure A16: Effects of Peer Information by Three-Way Classification

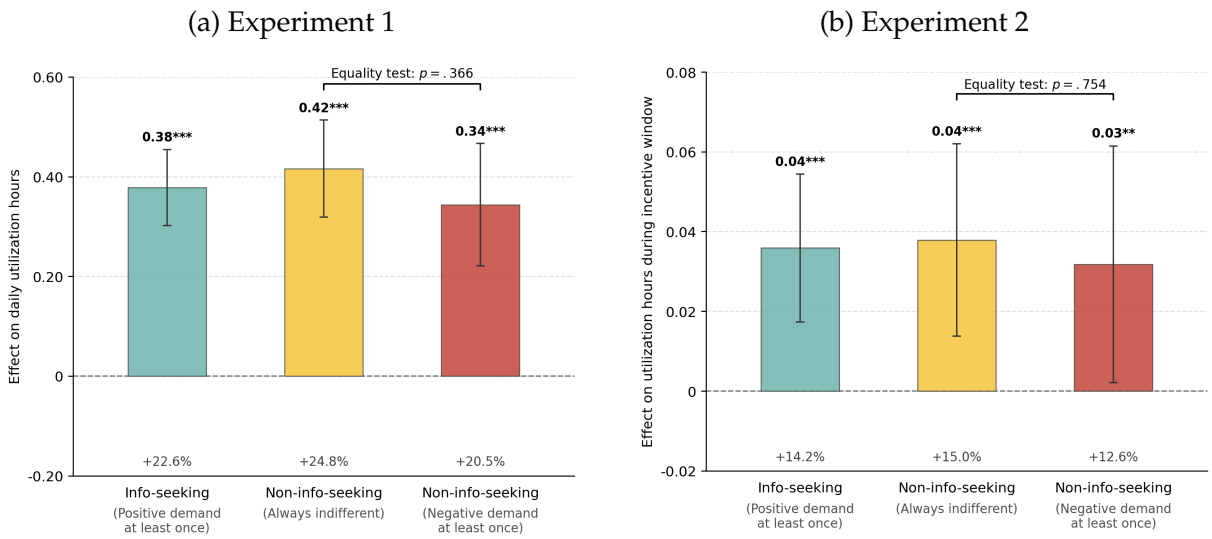


Notes. This figure reports the estimated effects of peer earnings information on daily utilization hours separately for info-seeking drivers, always-indifferent, and negative-demand drivers. Experiment 1 compares the *Control* and *Peer Info* groups. Experiment 2 pools the *Standard Peer Info* and *Targeted Peer Info groups* and compares them with the *Control* group. Percentages shown below the estimates express the effects relative to the corresponding baseline mean. Equality tests compare the treatment effects between the two non-info-seeking subgroups within each experiment. The error bars represent 95% confidence intervals. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

The same pattern appears in drivers' labor supply responses to the monetary incentives in both experiments. Figure A17 shows that the incentive effects are positive and similar in magnitude across all three information-preference types. In Experiment 1, the estimated effects on daily utilization hours are 0.38, 0.42, and 0.34 hours for info-seeking, always-indifferent, and negative-demand drivers, respectively. In particular, the effects for the two non-info-seeking subgroups are statistically indistinguishable ($p = .366$). In Experiment 2, the corresponding effects during the four-hour incentive window are 0.04, 0.04, and

0.03 hours, and the effects for the two non-info-seeking subgroups are again statistically indistinguishable (p = .754).

Figure A17: Effects of Monetary Incentives by Three-Way Classification



Notes. This figure reports the estimated effects of monetary incentives on utilization hours separately for info-seeking drivers, always-indifferent drivers, and negative-demand drivers. In Experiment 1, the incentive applied throughout the day, and Panel (a) reports effects on total daily utilization hours. In Experiment 2, the incentive applied only during the 11am–3pm window, and Panel (b) reports effects on utilization hours during that four-hour window. Percentages shown below the estimates express the effects relative to the corresponding baseline mean. Equality tests compare the incentive effects between the two non-info-seeking subgroups within each experiment. The error bars represent 95% confidence intervals. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.11 Effects by Membership in Online Driver Groups

Table A10: Effects of Peer Information by Membership in Online Driver Groups

Dependent variable:	Daily utilization hours	
	In Online Driver Groups (1)	Not in Online Driver Groups (2)
Experiment 1 × Info-seeking (β_{1S})	0.11*	0.09
	(0.06)	(0.09)
Experiment 1 × Non-info-seeking (β_{1N})	-0.01	-0.08
	(0.07)	(0.06)
Experiment 2 × Info-seeking (β_{2S})	-0.04	-0.10*
	(0.05)	(0.06)
Experiment 2 × Non-info-seeking (β_{2N})	0.05	0.06
	(0.05)	(0.05)
Baseline mean: Experiment 1	1.60	1.77
Baseline mean: Experiment 2	1.40	1.52
p-values of equality tests:		
Experiment 1: $\beta_{1S} = \beta_{1N}$	0.21	0.10
Experiment 2: $\beta_{2S} = \beta_{2N}$	0.18	0.05
Info-seeking: $\beta_{1S} = \beta_{2S}$	0.07	0.06
Non-info-seeking: $\beta_{1N} = \beta_{2N}$	0.48	0.10
Diff-in-diff: $\beta_{1S} - \beta_{1N} = \beta_{2S} - \beta_{2N}$	0.08	0.01
p-values of tests across group membership:		
Experiment 1: $(\beta_{1S} - \beta_{1N})_{G=1} = (\beta_{1S} - \beta_{1N})_{G=0}$	0.69	
Experiment 2: $(\beta_{2S} - \beta_{2N})_{G=1} = (\beta_{2S} - \beta_{2N})_{G=0}$	0.54	
No. of observations	286,923	256,662
No. of drivers: Experiment 1	1,023	907
No. of drivers: Experiment 2	1,505	1,356

Notes. The table reports the effects of peer information on total daily utilization hours (including zeros), separately by baseline membership in online driver groups. In the baseline survey, drivers were asked whether they were part of any online groups or chats for private-hire or taxi drivers. $G = 1$ indicates membership in at least one driver online group (e.g., Facebook, WhatsApp); $G = 0$ otherwise. Experiment 1 compares the *Control* and *Peer Info* groups. Experiment 2 pools the *Standard* and *Targeted Peer Info* groups and compares them with the *Control* group. Each column reports estimates from a separate regression with driver-by-experiment and date fixed effects. Standard errors clustered at the driver level are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

A.12 Robustness Checks

Table A11: Effects of Peer Information on Daily Labor Supply: Common-Driver Sample

Dependent variable:	Platform labor supply	Extensive margin	Intensive margins (conditional on working)		
	Hours	Working	Hours	Trips	Earnings
	(1)	(2)	(3)	(4)	(5)
Panel A: Average Treatment Effects by Experiment					
Peer information: Experiment 1 (β_1)	0.00	-0.01*	0.06*	0.14	1.65
	(0.03)	(0.01)	(0.03)	(0.09)	(1.25)
Peer information: Experiment 2 (β_2)	-0.01	-0.00	-0.00	-0.03	-0.38
	(0.03)	(0.01)	(0.03)	(0.08)	(1.13)
Panel B: Treatment Effects by Experiment and Information Preferences					
Experiment 1 × Info-seeking (β_{1S})	0.08	-0.01	0.13***	0.29**	4.44**
	(0.05)	(0.01)	(0.05)	(0.12)	(1.78)
Experiment 1 × Non-info-seeking (β_{1N})	-0.08*	-0.02*	-0.02	-0.03	-1.43
	(0.04)	(0.01)	(0.04)	(0.12)	(1.75)
Experiment 2 × Info-seeking (β_{2S})	-0.06*	-0.02*	-0.04	-0.13	-1.65
	(0.04)	(0.01)	(0.04)	(0.10)	(1.52)
Experiment 2 × Non-info-seeking (β_{2N})	0.05	0.01	0.03	0.07	0.96
	(0.04)	(0.01)	(0.04)	(0.11)	(1.69)
Baseline mean: Experiment 1	1.70	0.64	2.67	6.67	105.75
Baseline mean: Experiment 2	1.45	0.58	2.50	6.31	103.09
p-values of equality tests:					
ATE: $\beta_1 = \beta_2$	0.776	0.391	0.141	0.130	0.235
Experiment 1: $\beta_{1S} = \beta_{1N}$	0.025	0.374	0.027	0.062	0.019
Experiment 2: $\beta_{2S} = \beta_{2N}$	0.039	0.032	0.250	0.174	0.250
Info-seeking: $\beta_{1S} = \beta_{2S}$	0.034	0.448	0.007	0.009	0.010
Non-info-seeking: $\beta_{1N} = \beta_{2N}$	0.030	0.032	0.468	0.537	0.327
Diff-in-diff: $\beta_{1S} - \beta_{1N} = \beta_{2S} - \beta_{2N}$	0.003	0.042	0.016	0.022	0.013
No. of observations	616,077	616,077	369,172	369,172	369,172
No. of drivers: Experiment 1	2,667	2,667	2,599	2,599	2,599
No. of drivers: Experiment 2	2,667	2,667	2,437	2,437	2,437

Notes. This table re-estimates the main specifications using the same 2,667 drivers from the Experiment 1 main sample who returned for Experiment 2. Drivers originally assigned to the *Endogenous Info* group are retained as a separate experimental arm in the Experiment 1 specifications; the reported Experiment 1 peer information effects compare the *Control* and *Peer Info* groups. Panel A reports average treatment effects and Panel B reports effects by elicited information-preference type. Experiment 2 pools the *Standard Peer Info* and *Targeted Peer Info* groups and compares them with *Control*. In Column 1, *Hours* denotes total daily utilization hours and equals zero on days without completed work. *Working* equals one if at least one completed or canceled trip was recorded. Columns 3–5 report utilization hours, completed trips, and trip earnings conditional on *Working*. Earnings are reported in S$ and exclude incentive payouts. All specifications include driver-by-experiment and date fixed effects. Standard errors clustered at the driver level are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Prior Exposure to Peer Information. I examine whether the Experiment 2 treatment effects depend on prior exposure to peer earnings information. I restrict the analysis to the 1,778 returning drivers originally randomized to the *Control* or *Peer Info* groups in Experiment 1, so prior exposure is itself randomly assigned. Drivers originally assigned to *Control* receive peer information for the first time if assigned to peer information in Experiment 2, while those originally assigned to *Peer Info* receive it again. The *Standard Peer Info* and *Targeted Peer Info* groups in Experiment 2 are pooled. Within each prior-assignment group, I compare drivers assigned to peer information in Experiment 2 with those assigned to *Control*. I estimate these effects for the full sample and separately by information-preference type.

Table A12 reports the results. I find no statistically detectable difference between first and repeat exposure for daily utilization hours ($p = .276$) or the probability of working ($p = .350$). Among info-seeking drivers, the estimated effects are negative under both first and repeat exposure, and their equality cannot be rejected for either outcome ($p = .559$ and $p = .589$, respectively). The difference in treatment effects between info-seeking and non-info-seeking drivers also does not vary significantly with prior exposure for these outcomes ($p = .842$ and $p = .936$, respectively).

Taken together, I find no evidence that prior exposure to peer information in Experiment 1 moderates the Experiment 2 effects on overall labor supply or work participation. The only suggestive difference is for completed trips conditional on working, where the first- and repeat-exposure estimates differ marginally in the full sample ($p = .052$). Because this comparison conditions on working and may therefore be subject to post-treatment selection, I interpret this result cautiously.

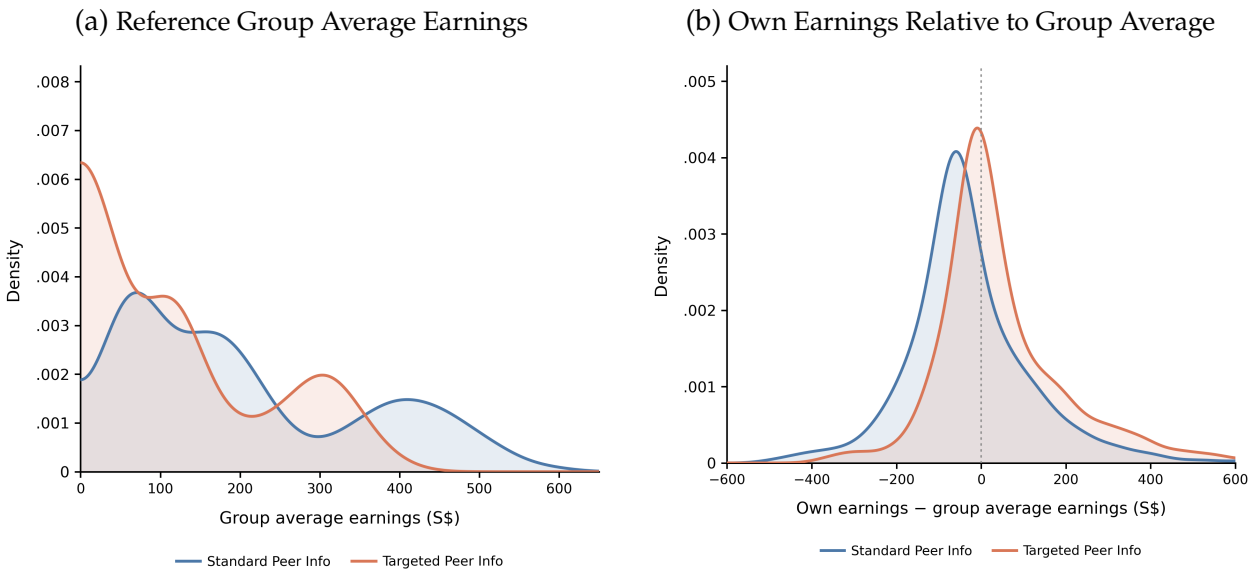
Table A12: Effects of Peer Information on Daily Labor Supply by Prior Exposure (Experiment 2)

Dependent variable:	Platform labor supply	Extensive margin	Intensive margins (conditional on working)		
	Hours	Working	Hours	Trips	Earnings
	(1)	(2)	(3)	(4)	(5)
Panel A: Treatment Effects by Prior Exposure					
First exposure (β_F)	-0.05	-0.01	-0.06	-0.17	-2.27
	(0.05)	(0.01)	(0.04)	(0.12)	(1.77)
Repeat exposure (β_R)	0.02	0.00	0.05	0.16	1.71
	(0.04)	(0.01)	(0.05)	(0.12)	(1.92)
Panel B: Treatment Effects by Prior Exposure and Information Preferences					
First exposure × Info-seeking ($\beta_{F,S}$)	-0.08	-0.02	-0.07	-0.24	-2.97
	(0.07)	(0.02)	(0.06)	(0.17)	(2.42)
Repeat exposure × Info-seeking ($\beta_{R,S}$)	-0.03	-0.01	0.05	0.18	2.07
	(0.06)	(0.02)	(0.06)	(0.16)	(2.47)
First exposure × Non-info-seeking ($\beta_{F,N}$)	-0.01	0.00	-0.04	-0.10	-1.53
	(0.06)	(0.02)	(0.06)	(0.16)	(2.61)
Repeat exposure × Non-info-seeking ($\beta_{R,N}$)	0.07	0.02	0.04	0.14	1.37
	(0.06)	(0.02)	(0.07)	(0.18)	(2.94)
Baseline mean	1.46	0.58	2.51	6.32	103.34
p-values of equality tests:					
ATE: $\beta_F = \beta_R$	0.276	0.350	0.109	0.052	0.128
Info-seeking: $\beta_{F,S} = \beta_{R,S}$	0.559	0.589	0.151	0.073	0.145
Non-info-seeking: $\beta_{F,N} = \beta_{R,N}$	0.346	0.459	0.394	0.337	0.460
Joint exposure equality by type	0.541	0.657	0.248	0.126	0.263
Preference gaps equal across prior exposure	0.842	0.936	0.731	0.587	0.682
First exposure: $\beta_{F,S} = \beta_{F,N}$	0.443	0.263	0.748	0.554	0.686
Repeat exposure: $\beta_{R,S} = \beta_{R,N}$	0.275	0.187	0.867	0.853	0.855
No. of observations	186,690	186,690	105,667	105,667	105,667
No. of drivers	1,778	1,778	1,621	1,621	1,621

Notes. This table reports Experiment 2 peer information effects by randomized prior exposure. The sample consists of returning drivers originally assigned to the Experiment 1 *Control* or *Peer Info* groups. *First exposure* denotes drivers originally assigned to *Control*; *Repeat exposure* denotes drivers originally assigned to *Peer Info*. Within each prior-assignment group, the reported effects compare drivers assigned to peer information in Experiment 2 with those assigned to *Control*. The *Standard Peer Info* and *Targeted Peer Info* groups in Experiment 2 are pooled. In Column 1, *Hours* denotes total daily utilization hours and equals zero on days without completed work. *Working* equals one if at least one completed or canceled trip was recorded. Columns 3–5 report utilization hours, completed trips, and trip earnings conditional on *Working*. Earnings are reported in S$ and exclude incentive payouts. All specifications include driver and date fixed effects, Prior Peer Info × Post, Post × Info-seeking, and controls for the randomized monetary incentives. Panel B additionally includes Prior Peer Info × Post × Info-seeking and allows incentive effects to differ by information-preference type. Standard errors clustered at the driver level are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

A.13 Tests of Relative Performance Mechanisms

Figure A18: Distributions of Peer Earnings Information by Treatment Variant



Notes. This figure shows the distributions of the earnings information reported in the first earnings update of Experiment 2, delivered on June 5, 2026. The update summarized cumulative earnings for the week to date. Panel (a) reports the reference group's average earnings. The mean was S$207.6 for the *Standard Peer Info* group and S$135.9 for the *Targeted Peer Info* group ($p < .001$). Panel (b) reports the difference between the driver's own earnings and the group average. Positive values indicate that the driver earned at least as much as their reference group. The share at or above the peer group average was 32.0% in the *Standard Peer Info* group and 56.1% in the *Targeted Peer Info* group ($p < .001$). Panel (b) is displayed from −S$600 to S$600; 0.7% of observations lie above this range and none lie below it. The curves are kernel-density estimates. Within each panel, the two treatment groups are estimated using the same bandwidth. Panel (a) applies a boundary correction at zero.

Table A13: Effects of Peer Information on Daily Labor Supply by Treatment Variant

Dependent variable:	Platform labor supply	Extensive margin	Intensive margins (conditional on working)		
	Hours	Working	Hours	Trips	Earnings
	(1)	(2)	(3)	(4)	(5)
Panel A: Average Treatment Effects					
Standard peer information (β_{Std})	-0.01	-0.01	0.00	-0.04	-0.35
	(0.03)	(0.01)	(0.03)	(0.08)	(1.26)
Targeted peer information (β_{Tgt})	-0.01	-0.00	-0.01	-0.04	-0.64
	(0.03)	(0.01)	(0.03)	(0.08)	(1.24)
Panel B: Treatment Effects by Information Preferences					
Standard × Info-seeking ($\beta_{\text{Std},S}$)	-0.07*	-0.02**	-0.03	-0.14	-1.59
	(0.04)	(0.01)	(0.04)	(0.11)	(1.66)
Targeted × Info-seeking ($\beta_{\text{Tgt},S}$)	-0.07*	-0.02*	-0.05	-0.15	-2.06
	(0.04)	(0.01)	(0.04)	(0.11)	(1.66)
Standard × Non-info-seeking ($\beta_{\text{Std},N}$)	0.05	0.01	0.04	0.06	0.97
	(0.04)	(0.01)	(0.05)	(0.12)	(1.90)
Targeted × Non-info-seeking ($\beta_{\text{Tgt},N}$)	0.06	0.02	0.02	0.08	0.89
	(0.04)	(0.01)	(0.05)	(0.12)	(1.85)
Baseline mean	1.46	0.58	2.52	6.33	103.47
p-values of equality tests:					
ATE: $\beta_{\text{Std}} = \beta_{\text{Tgt}}$	0.873	0.453	0.602	0.959	0.814
Info-seeking: $\beta_{\text{Std},S} = \beta_{\text{Tgt},S}$	0.982	0.715	0.658	0.894	0.779
Non-info-seeking: $\beta_{\text{Std},N} = \beta_{\text{Tgt},N}$	0.808	0.462	0.778	0.825	0.964
Joint arm equality by type	0.971	0.714	0.871	0.967	0.961
Preference gaps equal across variants	0.880	0.795	0.913	0.802	0.872
Standard: $\beta_{\text{Std},S} = \beta_{\text{Std},N}$	0.042	0.042	0.269	0.238	0.312
Targeted: $\beta_{\text{Tgt},S} = \beta_{\text{Tgt},N}$	0.028	0.021	0.220	0.154	0.236
No. of observations	300,405	300,405	169,393	169,393	169,393
No. of drivers	2,861	2,861	2,611	2,611	2,611

Notes. This table reports variant-specific effects of peer earnings information in Experiment 2. The *Standard Peer Info* group uses a randomly selected reference group within the same baseline utilization-hours bin; the *Targeted Peer Info* group uses lower-earning reference drivers within the same bin. In Column 1, *Hours* denotes total daily utilization hours and equals zero on days without completed work. *Working* equals one if at least one completed or canceled trip was recorded. Columns 3–5 report utilization hours, completed trips, and trip earnings conditional on *Working*. Earnings are reported in S$ and exclude incentive payouts. All specifications include driver and date fixed effects, Post × Info-seeking, and controls for the randomized monetary incentives. In Panel B, incentive effects are allowed to differ by information-preference type. Standard errors clustered at the driver level are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A14: Effects of Peer Information by Relative Earnings Position

Dependent variable:	Daily utilization hours			
	Experiment 1		Experiment 2: Standard	
	(1)	(2)	(3)	(4)
Panel A: Info-seeking drivers				
Peer information: Below average	0.082		-0.062*	
	(0.052)		(0.037)	
Peer information: Above average	0.089		-0.048	
	(0.060)		(0.064)	
Peer information ($r = 0$)		0.063*		-0.047
		(0.033)		(0.030)
Peer information × Relative earnings (r)		0.0005		-0.0166
		(0.0197)		(0.0166)
p-value: Above = Below	0.924		0.830	
p-value: Relative-earnings gradient = 0		0.980		0.317
Panel B: Non-info-seeking drivers				
Peer information: Below average	-0.032		0.061	
	(0.048)		(0.038)	
Peer information: Above average	-0.037		-0.028	
	(0.056)		(0.058)	
Peer information ($r = 0$)		-0.047		0.032
		(0.031)		(0.028)
Peer information × Relative earnings (r)		0.0139		-0.0119
		(0.0186)		(0.0156)
p-value: Above = Below	0.936		0.167	
p-value: Relative-earnings gradient = 0		0.455		0.446
No. of observations	235,450	235,450	192,304	192,304
No. of drivers	1,930	1,930	1,904	1,904

Notes. This table reports the effects of peer information by relative earnings in the latest update. Experiment 1 compares *Peer Info* with *Control*; Experiment 2 compares *Standard Peer Info* with *Control*. Columns (1) and (3) classify drivers as below or above their assigned peer average using the latest cumulative earnings gap. Columns (2) and (4) instead interact treatment with relative earnings r, defined as assigned-peer minus own cumulative trip earnings in the latest update, expressed in S$100 units. The interaction term measures the change in the treatment effect per S$100 larger peer-minus-own gap. Control drivers use assigned but undisclosed average peer earnings. All specifications include driver and date fixed effects. Standard errors clustered by driver are in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B Additional Mechanism Evidence

B.1 Persistence of Market Conditions

To assess whether the market conditions summarized by an earnings update predict the market conditions drivers subsequently face in the next update window, I estimate

$$M_{e,u+1} = \eta_e + \rho M_{eu} + \gamma \,\text{Midweek}_{eu} + \lambda_e \,\text{Post}_{eu} + \varepsilon_{eu}, \tag{19}$$

where M_{eu} measures market conditions over the dates newly summarized by update u in experiment e, and $M_{e,u+1}$ measures market conditions during the subsequent earnings-update window. η_e denotes experiment fixed effects, Midweek_{eu} indicates a midweek update, and Post_{eu} indicates the intervention period, with its coefficient allowed to differ across experiments. The coefficient ρ measures the persistence of market conditions across consecutive earnings-update windows, conditional on these controls.

Table B1 reports the estimates. In the pooled specification, a one-standard-deviation improvement in current market conditions predicts a 0.590-standard-deviation improvement in conditions during the next update window ($p < .01$). The experiment-specific coefficients are 0.720 in Experiment 1 ($p < .01$) and 0.529 in Experiment 2 ($p = .054$). Both estimates are positive, although the Experiment 2 estimate is less precise. Equality of the two coefficients cannot be rejected ($p = .492$).

Table B1: Persistence of Market Conditions Across Earnings-Update Windows

Dependent variable:	Market conditions in the next update window ($M_{e,u+1}$)		
	Pooled (1)	Experiment 1 (2)	Experiment 2 (3)
Current market conditions (M_{eu})	0.590***	0.720***	0.529*
	(0.142)	(0.079)	(0.264)
p-value: equal coefficients across experiments		0.492	
R^2	0.585	0.722	0.411
No. of observations	66	36	30

Notes. This table reports whether market conditions summarized by an earnings update predict market conditions in the next earnings-update window. Observations are earnings-update episodes from the baseline and intervention periods. Current market conditions, M_{eu}, average daily log trip-request availability over the dates newly summarized by the update u in experiment e: Monday–Thursday for midweek updates and Friday–Sunday for end-of-week updates. The dependent variable, $M_{e,u+1}$, measures market conditions over the immediately following window: Friday–Sunday after a midweek update and Monday–Thursday after an end-of-week update. Trip-request availability is measured using the fixed cohort of drivers outside the experimental sample. Both measures are centered at the pooled-baseline mean of current conditions and divided by its standard deviation of 0.125 log points. Column (1) pools both experiments and includes experiment indicators, an update-type indicator, and experiment-specific intervention-period indicators. Columns (2) and (3) estimate the corresponding specifications separately by experiment, controlling for update type and the intervention period. Newey-West standard errors allowing for serial dependence across update episodes in parentheses.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.2 Effects of Peer Information by Market Conditions: Common Treatment Effect

Table B2: Peer Information Effects by Realized Market Conditions: Common Intercept Across Experiments

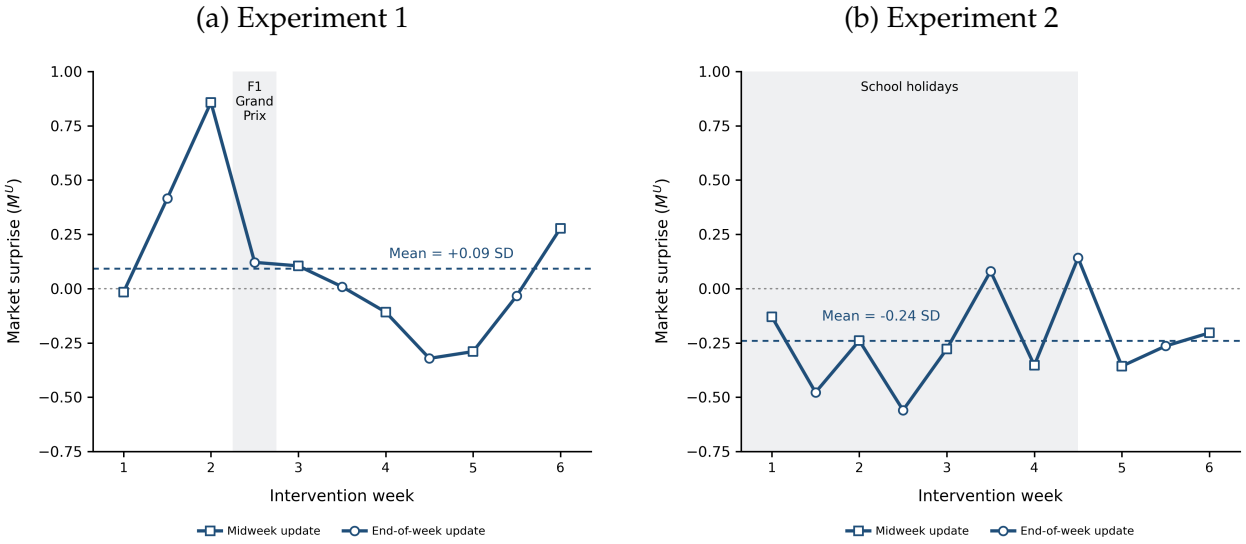
Dependent variable:	Daily utilization hours		
	Full sample (1)	Info-seeking (2)	Non-info-seeking (3)
Peer information at $M = 0$	0.023	0.056	-0.014
	(0.022)	(0.034)	(0.033)
Peer information × Realized market conditions	0.020	0.062***	-0.026
	(0.012)	(0.017)	(0.018)
p-value: equal market gradients across preference types		< 0.001	
No. of observations	543,585	282,660	260,925
No. of drivers	3,013	1,576	1,437
No. of earnings-update episodes	66	66	66

Notes. The table reports how the effects of peer information on daily utilization hours vary with realized market conditions under a common-intercept specification. Market conditions are measured by average daily log trip-request availability over the dates newly summarized by each earnings update: Monday–Thursday for midweek updates and Friday–Sunday for end-of-week updates. The measure M is centered at its pooled-baseline mean and divided by its pooled-baseline standard deviation of 0.125 log points. The peer-information coefficient reports the treatment effect at $M = 0$. The interaction coefficient reports the change in the treatment effect associated with a one-standard-deviation improvement in market conditions. Within each column, both the treatment effect at $M = 0$ and the market gradient are constrained to be common across experiments. This restriction allows the market gradient to draw on variation both within and across experiments. Column (1) uses the full sample; Columns (2) and (3) restrict the sample to info-seeking and non-info-seeking drivers, respectively. All regressions include driver-by-experiment and date fixed effects, an incentive-offer indicator with experiment-specific coefficients, and experiment-specific interactions between peer-information assignment and market conditions. Standard errors, reported in parentheses, are two-way clustered by driver and earnings-update date.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

B.3 Predicted Market Conditions and Market Surprises

Figure B1: Market Surprises During the Intervention Periods



Notes. This figure plots the market surprise, M^U, summarized by each earnings update during the six-week intervention of each experiment. Market surprises are constructed from one-day-ahead forecast errors in log trip-request availability using the AR(1) model with controls for day of week, school holidays, public holidays, and the Formula 1 Grand Prix period, as described in Section 6.1.2. The measure is in standard deviation units, where one unit corresponds to 0.125 log points of trip-request availability. The dashed horizontal line reports the mean market surprise during each intervention period. Shaded areas indicate the Formula 1 Grand Prix period in Experiment 1 and the school holiday period in Experiment 2.

Table B3: Information Content of Own and Peer Earnings

Dependent variable:	Market surprise (M^U)		
	Own earnings	Peer earnings	Own and peer earnings
	(1)	(2)	(3)
Own earnings	0.005***		0.004***
	(0.001)		(0.001)
Assigned peer earnings		0.036***	0.035***
		(0.010)	(0.009)
p-value: equal own and peer coefficients			< 0.001
R^2	0.382	0.395	0.397
No. of observations	150,516	150,516	150,516
No. of drivers	3,013	3,013	3,013
No. of earnings-update episodes	64	64	64

Notes. The table reports regressions of market surprises on own and assigned peer earnings, pooling baseline and intervention observations across both experiments. The sample includes the *Control* and *Peer Info* arms in Experiment 1 and all three information arms in Experiment 2. Each observation is a driver–update episode. The dependent variable, M^U, is the average daily forecast error in log trip-request availability over the dates newly summarized by each update, divided by the pooled-baseline standard deviation of realized update-level market conditions (0.125 log points). Forecasts use the expanding-window AR(1) model with calendar controls described in the text. Both earnings measures are expressed in units of S$10 per day. For midweek updates, reported earnings are divided by four. For end-of-week updates, the preceding midweek update is subtracted from the full-week update and the difference is divided by three. This transformation is applied to both own earnings and the assigned reference group's reported average earnings. Assigned peer earnings are included regardless of whether they were disclosed to the participant. All regressions include driver-by-experiment fixed effects, predicted market conditions (M^P), and experiment-specific indicators for update type and the intervention period. Column (3) reports the test of equality between the own- and peer-earnings coefficients. Standard errors in parentheses are two-way clustered by driver and earnings-update date.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

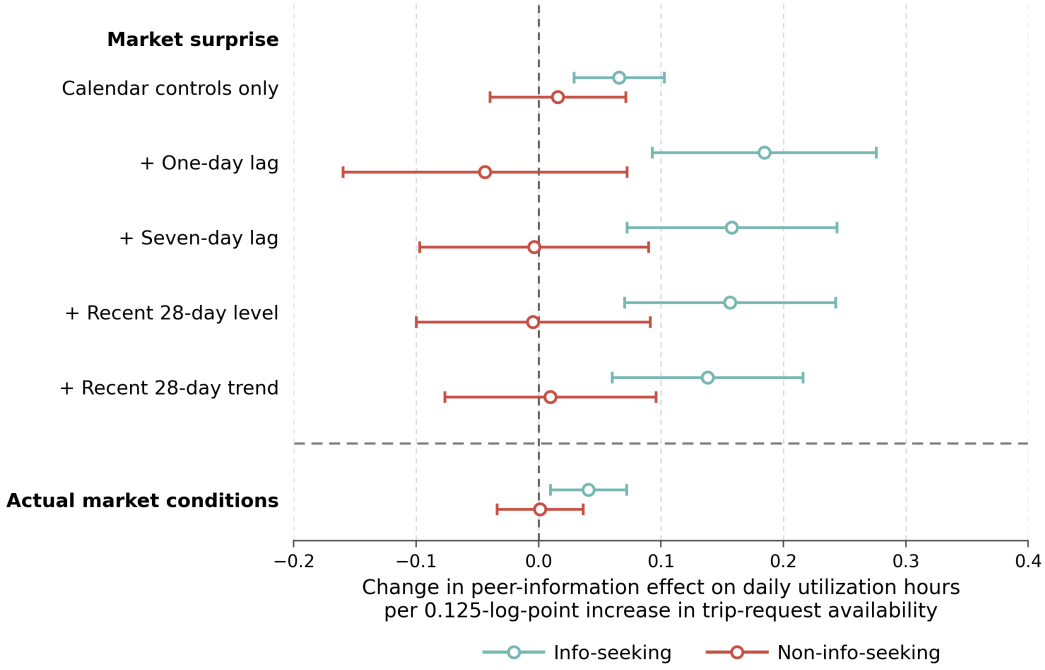
Table B4: Peer Information Effects by Predicted Conditions and Market Surprises Across Labor Supply Outcomes

Dependent variable:	Platform labor supply	Extensive margin	Intensive margins (conditional on working)		
	Hours	Working	Hours	Trips	Earnings
	(1)	(2)	(3)	(4)	(5)
Change in Peer-Information Effect Per 0.125-Log-Point Increase					
Unexpected market conditions (M^U)					
Info-seeking treatment gradient (ρ_U^S)	0.184***	0.038***	0.157***	0.413**	5.101**
	(0.046)	(0.013)	(0.052)	(0.162)	(2.136)
Non-info-seeking treatment gradient (ρ_U^N)	-0.044	0.004	-0.096	-0.183	-4.350
	(0.058)	(0.013)	(0.065)	(0.137)	(2.696)
Predicted market conditions (M^P)					
Info-seeking treatment gradient (ρ_P^S)	0.010	0.001	0.005	-0.021	-0.055
	(0.016)	(0.004)	(0.019)	(0.050)	(0.864)
Non-info-seeking treatment gradient (ρ_P^N)	0.010	-0.002	0.020	0.056	1.013
	(0.019)	(0.005)	(0.025)	(0.061)	(1.005)
p-value of equality test:					
Unexpected gradients equal: $\rho_U^S = \rho_U^N$	0.009	0.127	0.003	0.007	0.014
Predicted gradients equal: $\rho_P^S = \rho_P^N$	0.996	0.716	0.600	0.259	0.339
Unexpected = predicted: info-seeking $\rho_U^S = \rho_P^S$	0.001	0.012	0.011	0.015	0.037
Unexpected = predicted: non-seeking $\rho_U^N = \rho_P^N$	0.399	0.696	0.133	0.156	0.087
No. of observations	543,585	543,585	319,814	319,814	319,814
No. of drivers	3,013	3,013	2,879	2,879	2,879
No. of earnings-update episodes	66	66	66	66	66

Notes. The table reports how the treatment effect of peer information varies with predicted and unexpected market conditions. Predicted conditions are one-day-ahead forecasts from an expanding-window AR(1) model with a one-day lag, day-of-week indicators, and controls for school holidays, public holidays, and the Formula 1 Grand Prix period. Unexpected conditions are the corresponding forecast errors. Both components are expressed in units of 0.125 log points, corresponding to one pooled-baseline standard deviation of actual market conditions, so that the predicted- and unexpected-condition gradients are directly comparable. One standard deviation of the market-condition innovation itself is 0.058 log points. Experiment 1 compares the Control and Peer Info groups; Experiment 2 pools the Standard and Targeted Peer Info variants. In Column 1, *Hours* denotes total daily utilization hours and equals zero on days without completed work. *Working* equals one if at least one completed or canceled trip was recorded. Columns 3–5 report utilization hours, completed trips, and trip earnings conditional on *Working*. Earnings are reported in S$ and exclude incentive payouts. All regressions include driver-by-experiment and date fixed effects, experiment-specific controls for randomized monetary incentives, and all estimable lower-order interactions, which may differ across experiments. Standard errors, reported in parentheses, are two-way clustered by driver and earnings-update date.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

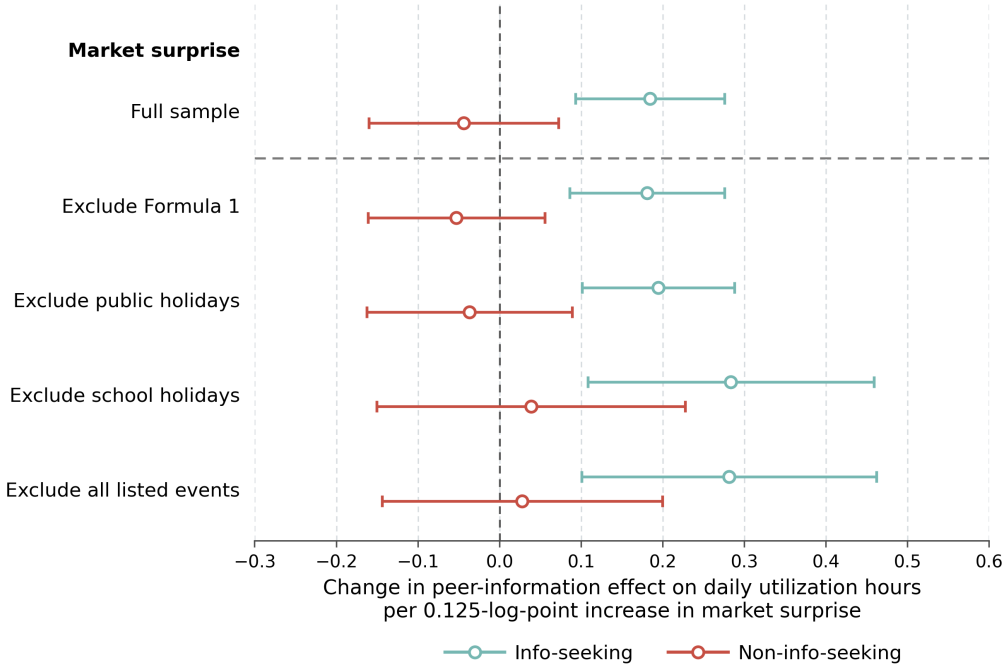
B.4 Robustness of Market-Surprise Gradients

Figure B2: Market-Surprise Gradients Across Forecasting Models



Notes. The figure reports how the effect of peer information on daily utilization hours varies with market surprises constructed using alternative forecasting models, separately for info-seeking and non-info-seeking drivers. Starting with calendar controls alone, the specifications sequentially add a one-day lag, a seven-day lag, and the trailing 28-day mean and trend in log trip-request availability. All forecasts use expanding estimation windows. The bottom row reports the corresponding gradients for realized market conditions without separating predicted conditions from market surprises. All estimates are expressed per 0.125-log-point increase in the corresponding market measure. Horizontal bars indicate 95% confidence intervals, with standard errors two-way clustered by driver and earnings-update date.

Figure B3: Market-Surprise Gradients Excluding Event Periods

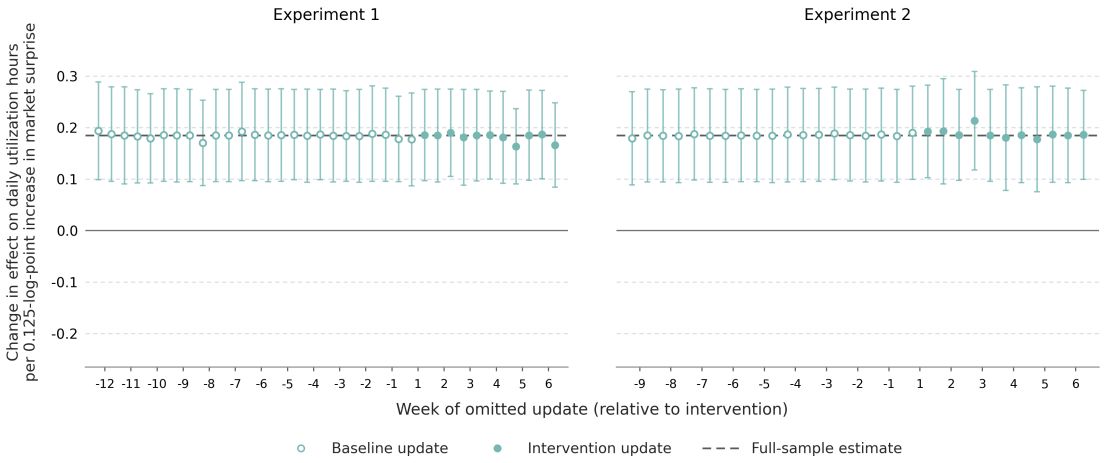


Notes. The figure reports how the effect of peer information on daily utilization hours varies with market surprises, separately for info-seeking and non-info-seeking drivers. The first row reports the market-surprise gradients estimated using the full sample. Subsequent rows exclude observations between an earnings update and the next update if the earlier update includes dates that overlap the Formula 1 Grand Prix, public holidays, school holidays, or any of these listed events, respectively. Treatment interactions with market surprises and predicted market conditions enter jointly in every specification. All estimates are expressed per 0.125-log-point increase in market surprises. Horizontal bars indicate 95% confidence intervals, with standard errors two-way clustered by driver and earnings-update date.

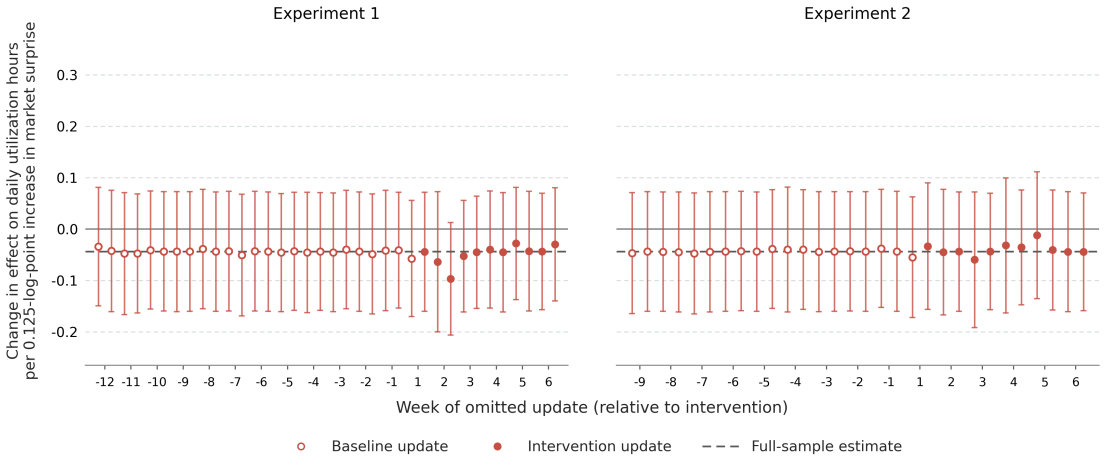
Leave-one-out Estimation. I assess whether the estimated market-surprise gradient is driven by any single earnings-update episode using a "leave-one-out" analysis that re-estimates the main specification 66 times for each information-preference type, each time excluding the observations associated with one update episode. Figure B4 shows that the estimated gradients remain very stable across these iterations for both info-seeking and non-info-seeking drivers.

Figure B4: Leave-one-out Estimates of Market-Surprise Gradients

(a) Info-seeking drivers



(b) Non-info-seeking drivers

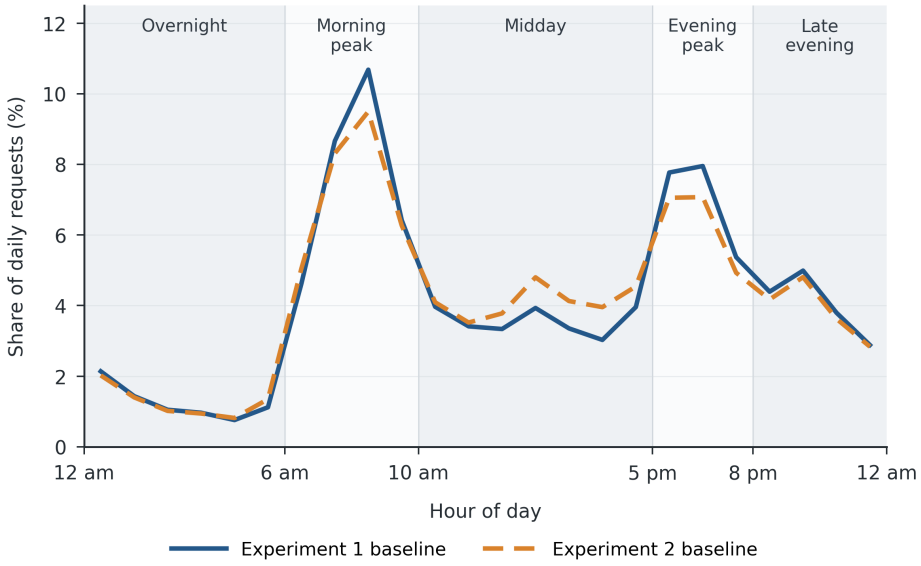


Notes. The figure reports market-surprise gradients estimated after omitting one earnings-update episode at a time, separately for info-seeking and non-info-seeking drivers. Each estimate excludes all driver-day observations between the omitted update and the next update. Treatment interactions with market surprises and predicted market conditions enter jointly in every specification. Week 1 denotes the first week of intervention in each experiment; negative numbers denote preceding baseline weeks. Within each week, the two points correspond to the midweek and end-of-week updates, respectively. Hollow circles denote baseline updates and filled circles denote intervention updates. Dashed horizontal lines indicate the full-sample estimates. All estimates are expressed per 0.125-log-point increase in market surprises. Vertical bars indicate 95% confidence intervals, with standard errors two-way clustered by driver and earnings-update date.

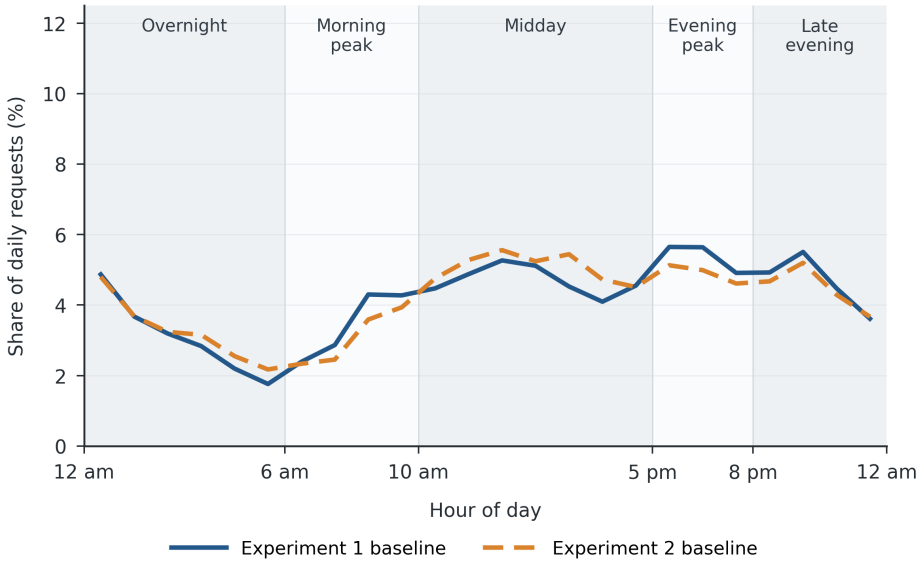
B.5 Intraday Distribution of Trip Requests

Figure B5: Intraday Distribution of Trip Requests Across Baseline Periods

(a) Weekdays



(b) Weekends and Public Holidays

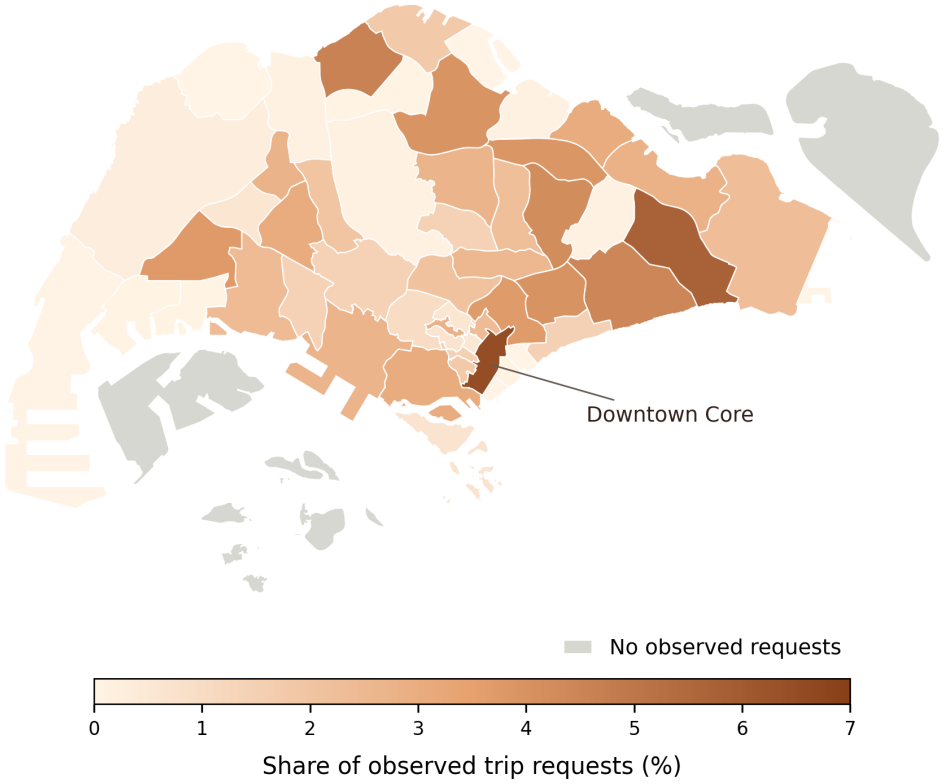


Notes. This figure plots the average share of daily trip requests observed in each hour of the day during the baseline periods of Experiments 1 and 2. Panel (a) reports weekdays and Panel (b) reports weekends and public holidays. Trip requests are measured using the same fixed cohort of drivers outside the experimental sample used to construct the trip-request availability index. Vertical lines indicate the five time blocks used in the time-of-day reallocation analysis: midnight–6 a.m., 6–10 a.m., 10 a.m.–5 p.m., 5–8 p.m., and 8 p.m.–midnight.

B.6 Spatial Distribution of Trip Requests

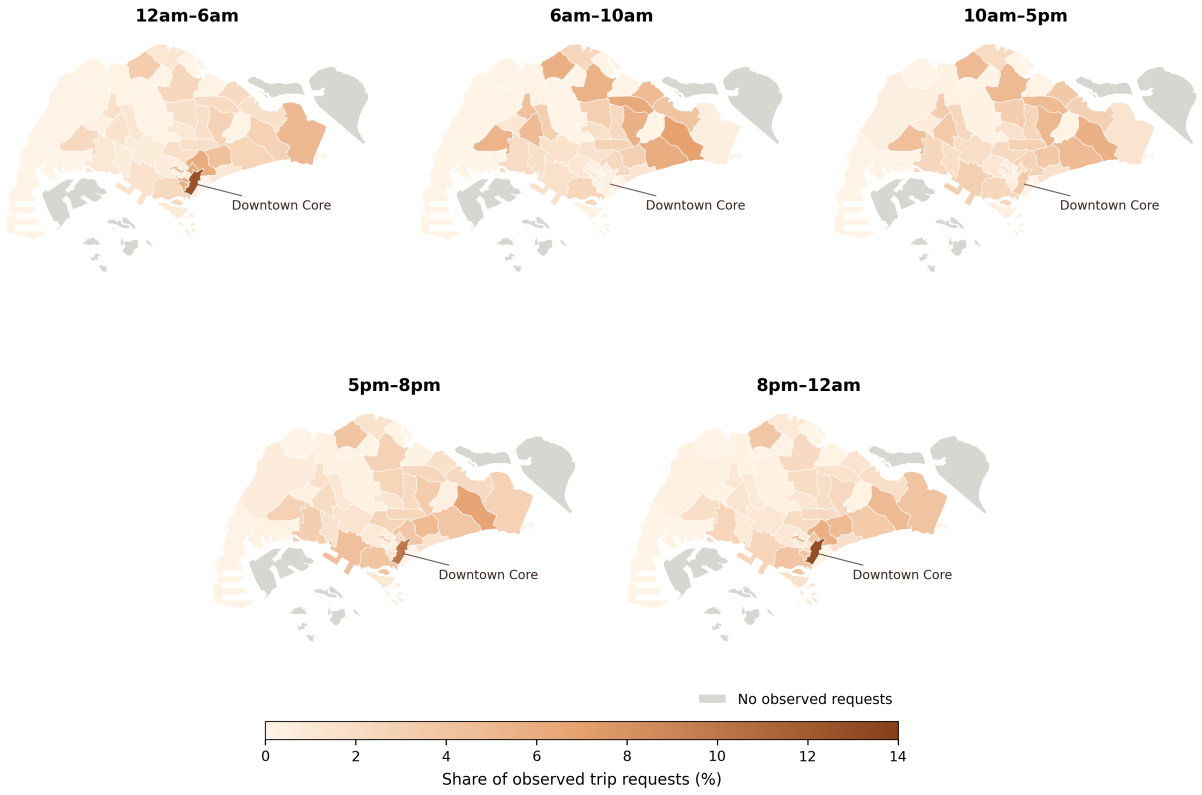
Figure B6 shows the overall spatial distribution of trip-request pickup locations during the baseline periods of both experiments. Figure B7 presents the corresponding distributions separately across the five time blocks, showing that these spatial patterns also vary across times of day.

Figure B6: Spatial Distribution of Trip Requests at Baseline



Notes. This figure shows the geographic distribution of passenger pickup requests observed during the baseline periods of Experiments 1 and 2, pooled across both periods. Boundaries correspond to the 55 planning areas defined in Singapore's Urban Redevelopment Authority Master Plan 2019. Shading indicates each area's share of trip requests observed by drivers in the original enrolled cohort, regardless of whether the trip was accepted. Grey areas have no observed trip requests during either baseline period.

Figure B7: Spatial Distribution of Trip Requests by Time of Day



Notes. This figure shows the geographic distribution of passenger pickup requests within each of the five time blocks used in the reallocation analysis, pooling the baseline periods of Experiments 1 and 2. Boundaries correspond to the 55 planning areas defined in Singapore's Urban Redevelopment Authority Master Plan 2019. Shading indicates each area's share of trip requests observed by drivers in the original enrolled cohort within the indicated time block, regardless of whether the trip was accepted. Grey areas have no observed trip requests during either baseline period.

B.7 Effects of Peer Information on Work Strategy

Table B5: Effects of Peer Information on Time-of-Day and Spatial Reallocation

Dependent variable:	Time-of-day reallocation		Spatial reallocation	
	(1)	(2)	(3)	(4)
Panel A: Info-seeking drivers				
Peer information	0.082	0.265	−0.108	−0.078
	(0.365)	(0.396)	(0.218)	(0.237)
Peer information × Relative earnings		0.0047		−0.0001
		(0.0183)		(0.0094)
No. of observations	57,639	55,774	55,784	53,989
No. of drivers	1,412	1,412	1,410	1,410
Panel B: Non-info-seeking drivers				
Peer information	0.022	0.325	−0.109	−0.099
	(0.396)	(0.402)	(0.254)	(0.284)
Peer information × Relative earnings		0.0032		0.0019
		(0.0181)		(0.0108)
No. of observations	52,492	50,796	50,541	48,924
No. of drivers	1,283	1,283	1,283	1,282

Notes. This table reports the effects of peer information on time-of-day and spatial reallocation across Experiments 1 and 2, estimated separately by information-preference type. Observations are driver–update response windows, defined as Monday–Thursday or Friday–Sunday. Both outcomes are 0–100 indices measuring departures from drivers' own baseline work patterns. *Time-of-day reallocation* compares the allocation of utilization hours across five time blocks. *Spatial reallocation* compares the distribution of inferred work locations across Singapore's planning areas within each time block and averages these distances across blocks using the share of observed hours in each block. Baseline distributions are constructed separately by experiment and window type; for baseline observations, the evaluated window is excluded from its own reference distribution. Columns (1) and (3) report the pooled average treatment effects. Columns (2) and (4) additionally interact assignment to peer information with *Relative earnings*, defined as assigned-peer earnings minus the driver's own cumulative earnings reported in the latest update, in S$10 units. These specifications also control for own earnings and the corresponding lower-order interactions. All specifications include driver-by-experiment and update-date fixed effects and experiment-specific controls for the randomized monetary incentives. Experiment 2 pools the *Standard* and *Targeted Peer Info* groups. Standard errors two-way clustered by driver and update date are reported in parentheses.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

C Theoretical Appendix

Learning Across Repeated Earnings Updates. The main framework characterizes belief updating at a given earnings update, taking the driver's prior beliefs at that update as given. In this appendix, I show how repeated signals from the same reference group allow drivers to learn its persistent component of strategy effectiveness. Reference-group assignments are fixed throughout each intervention, and I assume that $a_{g(i)}$ is constant over the intervention.

Recall that the own and peer signals are

$$s_{it}^{O} = a_i + m_t + e_{it}, \qquad s_{it}^{P} = a_{g(i)} + m_t + u_{it}.$$

Both signals contain the same market state, so taking their difference cancels out m_t. By assumption, the driver knows the effectiveness of their current strategy, a_i, so define

$$q_{it} \equiv s_{it}^{P} - s_{it}^{O} + a_i = a_{g(i)} + \varepsilon_{it}, \qquad \varepsilon_{it} \equiv u_{it} - e_{it}. \tag{C.1}$$

Under the maintained independence assumptions, $\varepsilon_{it} \sim N\left(0, \sigma_P^2 + \sigma_O^2\right)$. Thus, for a treated driver, each earnings update provides a noisy signal of $a_{g(i)}$.

Suppose that before observing the first peer earnings update, the driver's prior belief is $a_{g(i)} \sim N\left(\mu_{i0}^{a}, \omega_{i0}^2\right)$. For tractability, assume that the errors ε_{it} are independent across updates. Define $\sigma_q^2 \equiv \sigma_P^2 + \sigma_O^2$.

Consider the driver's posterior belief based on the relative earnings signals $q_{i1}, ..., q_{in}$. After observing n signals of the form in equation (C.1), the posterior variance of $a_{g(i)}$ is

$$\omega_{in}^2 = \left(\frac{1}{\omega_{i0}^2} + \frac{n}{\sigma_q^2}\right)^{-1}. \tag{C.2}$$

The corresponding posterior mean is

$$\mu_{in}^{a} = \omega_{in}^2 \left(\frac{1}{\omega_{i0}^2}\mu_{i0}^{a} + \frac{1}{\sigma_q^2}\sum_{r=1}^{n} q_{ir}\right). \tag{C.3}$$

It follows immediately from equation (C.2) that posterior uncertainty, ω_{in}^2, decreases as the number of observed signals increases. Equation (C.3) also shows that persistent differences in the group's earnings relative to the driver's own are incorporated into the driver's expectation of $a_{g(i)}$ over time.[35]

This learning affects how subsequent peer signals are interpreted by the driver. At any given update t, recall that the weight placed on the peer-signal surprise when updating

[35] With correlated Gaussian signal errors, the closed-form expressions change, but an additional signal still weakly reduces posterior uncertainty after conditioning on earlier signals.

beliefs about market conditions is $\eta_{it} = \frac{\widetilde{\tau}_{it}^2}{\widetilde{\tau}_{it}^2 + \omega_{it}^2 + \sigma_P^2}$. Holding $\widetilde{\tau}_{it}^2$ and σ_P^2 fixed,

$$\frac{\partial \eta_{it}}{\partial \omega_{it}^2} = -\frac{\widetilde{\tau}_{it}^2}{\left(\widetilde{\tau}_{it}^2 + \omega_{it}^2 + \sigma_P^2\right)^2} < 0. \tag{C.4}$$

Thus, as repeated signals reduce uncertainty about $a_{g(i)}$, a given peer-signal surprise is more readily attributed to current market conditions rather than uncertainty about the reference group.

At the same time, under the maintained assumptions, the posterior mean converges to $a_{g(i)}$ as the number of observed signals increases. Persistent differences in peer earnings are therefore incorporated into the driver's expectation for their reference group over time. Persistently lower (higher) peer earnings therefore need not be interpreted at every update as evidence of weaker (stronger) market conditions.

D Generalizability

In this section, I discuss the external validity of my findings using the SANS framework of List (2020), which considers selection, attrition, naturalness, and scaling.

Selection. Participation in "TADA Insights" was voluntary, so the experimental sample need not be representative of all registered drivers on the platform. Administrative data allow me to directly assess this margin of selection by comparing participants and nonparticipants on observed characteristics. As shown in Appendix Table A1, the two groups were similar in age and gender composition, with economically small differences in platform tenure and driver ratings. The main difference was baseline platform activity: participating drivers earned approximately S$474 and completed 29.9 trips per week, compared with S$226 and 13.4 trips among nonparticipants, on average. The results therefore generalize most directly to relatively active drivers, who are also the relevant target population for a recurring workplace information intervention.

More broadly, there is also selection into ride-hailing work itself. Ride-hailing drivers may differ from workers in other occupations or industries along unobserved dimensions that shape their demand for and responses to peer earnings information. This does not affect the internal validity of my results, but it may limit how they generalize to other workplace settings. My companion paper provides complementary evidence from a real-effort experiment with US-based workers recruited through Prolific: elicited WTP for peer information systematically predicts subsequent responses to feedback (Lim, 2025). Together, the two studies support the broader relevance of heterogeneity in information demand and of elicited demand as a strong predictor of responses to peer information.

Attrition. There was no outcome attrition within each experiment, as I observe the labor supply of all participating drivers throughout each experimental period, including days on which they do not work on the platform. The composition of the sample does change slightly between experiments: Experiment 2 re-randomized drivers from the original cohort who remained active and in good standing on the platform. However, the cross-experiment results are quantitatively and qualitatively similar when I restrict the analysis to the same 2,667 drivers who participated in both experiments (Appendix Table A11).

Naturalness. The experiments have a high degree of naturalness in the setting and choice task. "TADA Insights" was introduced as a new driver program to help drivers stay informed and manage their time and earnings more efficiently, as part of the firm's commitment to transparency. Drivers continued to make their usual labor supply decisions on the platform (i.e., whether and when to work, how long to work, and which trips to accept), with the underlying work environment otherwise unchanged. The firm implemented the earnings updates and monetary incentives through its existing email, push-notification, and in-app notice channels. The main outcomes were measured using high-frequency administrative records of actual platform activity rather than hypothetical choices or self-reported outcomes. In terms of time frame, each intervention period lasted six weeks. This recurring format more closely resembles an ongoing workplace information policy than a one-time information treatment and allows drivers to respond to successive updates as shared earning opportunities change over time.

Scaling. Within the firm, the information intervention is operationally straightforward to scale, since the platform already collects high-frequency trip-level data and can deliver the earnings updates through its existing communication channels. The results also inform who should receive the intervention when it is scaled. Instead of disclosing peer information to all drivers, the firm could tailor disclosure based on elicited information demand, given that a nontrivial share of drivers are averse to receiving peer information and show no detectable response to receiving it. More broadly, the market learning channel highlights the value of peer information: by helping workers adjust their labor supply in response to changes in market conditions, peer information may improve the allocation of labor across periods and thereby enhance allocative efficiency.

Importantly, the estimates from this paper are partial equilibrium effects. A platform-wide rollout would require accounting for general equilibrium responses. If disclosure changes the labor supply of a sufficiently large share of drivers, it could itself alter market conditions on the platform. Quantifying these equilibrium effects and their implications for worker welfare and firm performance is an important direction for future work.

Across organizations, the market learning channel may extend beyond ride-hailing to other workplaces in which workers face shared, time-varying earning opportunities that are imperfectly observed. Peer information may similarly affect labor supply by changing workers' beliefs about the returns to work. Whether this channel extends to other settings will likely depend on how informative peer outcomes are about shared earning opportunities and how flexibly workers can adjust their labor supply.