Match-Making is Difficult: Experimental & Quasi-Experimental Evidence from a Carpooling Platform

SUBMISSION #20

Early in their development, commercial platforms overcome the "chicken-and-egg" problem through subsidies and promotional campaigns, spurring growth through network effects and complementarities. But platforms are also used to coordinate *non-commercial* activities. In non-commercial settings, conventional prices may play a much smaller role in users' participation decisions and little is known about the strategies that promote growth. We study the growth of non-commercial platforms in a field experiment with drivers on a peer-to-peer carpooling platform in Singapore. In particular, we test whether the tools that drive market expansion on commercial platforms also do so in non-commercial settings. We randomize messages to dormant drivers: Some drivers receive information about favorable market conditions, other drivers receive subsidies for picking up passengers, and yet other drivers receive reminders that picking up a peer could spawn a new friendship. Our results suggest that the theories about how promotional strategies encourage market expansion on commercial platforms *do not* carry over to non-commercial platforms. The information interventions in some cases backfire, while the subsidies sometimes have a small positive effect during the promotional period, but, if anything, decrease drivers' probability of participation in the market after the subsidy period has ended. We explore several explanatory channels: drivers are persistent and thus unlikely to update their beliefs about the market, drivers may adjust their selectivity when they know they are in high demand, and price-based incentives may crowd out prosocial behavior. Our study generates novel insights about the growth strategies of non-commercial platforms: promotional campaigns designed for platforms with conventional pricing may be ineffective—or even backfire—in contexts with rigid prices and substantial heterogeneity in user preferences over non-price dimensions of the service.

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1 INTRODUCTION

Platforms coordinate a vast and growing portion of commercial activity. Platforms and intermediaries also coordinate non-commercial and socially beneficial activities.[1] The activities coordinated on commercial and non-commercial platforms vary widely. And while prices play a key role on platforms that coordinate commercial activities, financial incentives for participation in non-commercial activities are minimal or absent.

Despite these differences, both types of platforms face similar obstacles to growth. Namely, creating a valuable service for one side of the market (e.g. buyers, food banks) requires getting the other side (e.g. sellers, food donors) on board first. The value of the service depends on others' participation. As a result, equilibrium levels of participation in the market can be inefficiently low. Starting out in the presence of network effects is especially hard: With no participants on either side, at first, it can be difficult to induce the participation that would allow the platform to grow. The presence of such cross-side participation externalities is known as the "chicken-and-egg problem" [Caillaud and Jullien, 2003].

In commercial markets, platforms often use price subsidies and promotional campaigns to overcome the chicken-and-egg problem and drive new market expansion. Uber offered large discounts to early riders as well as subsidies to early drivers, as did Lyft. Amazon offers substantial discounts and free trial periods when entering new product segments. There are two primary ways that such strategies promote the growth of new markets. The first comes from the logic of experience goods: the use of a new intermediary is difficult to evaluate ex ante. So, getting potential participants into the marketplace with a promotion can introduce users to the platform's benefits, and make it more likely that these users will continue to use the platform future when the promotional period ends.

The second key objective of promotional campaigns early in a platform's development comes from the theory of two-sided platform markets, which have been widely studied in market design and industrial organization [Parker and Van Alstyne, 2005, Rochet and Tirole, 2003]. In these markets—where benefits from participation depend on network externalities—a positive shock for one set of participants (e.g. sellers) can increase the number of participants on the other side of the market (e.g. buyers). This feedback loop leverages the complementarities between participation decisions of different participants. By this logic, temporary discounts and publicity campaigns can play a coordinating role, moving the market to a different and thicker equilibrium.

The benefits of promotional campaigns for *commercial* platforms are well established theoretically, but less so empirically, beyond the data unlocked for research by a few private firms.[2] Even less well-understood—theoretically or empirically—is the growth of *non-commercial* platforms that coordinate socially beneficial activities. On one hand, it may be that even when the rewards to one side of the market are non-pecuniary, the standard platform logic applies. On the other hand, it may be that when incentives for participation are not oriented toward financial profit, motivating participation is more delicate. For example, it is well documented that financial incentives can crowd out prosocial behavior [Gneezy and Rustichini, 2000], and so subsidies may work against

[1] Altruistic organ donors are matched to patients in need of a transplant [Roth et al., 2007]. International refugee resettlement programs match willing host nations to refugees [Delacrétaz et al., 2016]. Adoption agencies match foster children to host parents [Baccara et al., 2014]. Some of these uses of matching technology for non-commercial purposes are implemented entirely by centralized clearinghouses. Others, such as the allocation of food to food banks [Prendergast, 2017] and the coordination of drivers and passengers for carpooling, are run on decentralized platforms.

[2] See, for example, Cullen and Farronato [2020], Farronato and Fradkin [2018], Fradkin [2015].

coordination on non-commercial platforms where prosocial motivations play a role in participation decisions.[3]

This paper reports results from a set of experiments designed to test whether the promotional strategies commonly used on commercial platforms can generate durable positive effects in non-commercial peer-to-peer markets. We test whether a package of interventions impact the longer term participation of either the direct beneficiaries of these interventions (through the experience good channel) or the other participants in the market (through network externalities and complementarities in decision-making).

In particular, we study the impact of subsidies and information campaigns on coordination in an emerging market for carpooling in Singapore. Carpooling is often a win-win for drivers and passengers. Drivers can fill their empty car with others who are going in roughly the same direction. They incur a small cost (e.g. time lost to picking up and dropping off their rider, hassle costs of arranging the trip) in return for a small gain (e.g. shared petrol costs, companionship, altruism, environmentalism). Passengers may find carpooling more convenient, quicker, or cheaper than other transport options. However, carpooling cannot occur without efficient coordination, especially if there are a group of people not necessarily going to the same location at the same time each day [Ostrovsky and Schwarz, 2019].[4]

Worldwide, intermediaries have entered such markets. These intermediaries aim to solve the two-sided matching problem in carpooling by developing platforms (where drivers and passengers post information about rides they want to give or take), algorithms (which present potential riders to potential drivers) and distinctive marketing tactics (which emphasize the beneficial externalities of the service, e.g. by creating community cohesion and by reducing environmental damage relative to single passenger vehicles).[5] In Singapore, GrabHitch was launched in November 2015 to fulfill this role.[6] In the GrabHitch app, vehicle owners can submit plans in advance to take up to two passengers per day. Passengers can log onto the system and enter in ride requests that get matched to potential drivers through GrabHitch's algorithm. Drivers then see a list of potential passengers and determine which, if any, of the passengers to share their ride with. If accepted for a ride, passengers pay a small fee based solely on distance, intended to help offset drivers' petrol costs.

We worked with GrabHitch to conduct a large-scale randomized experiment aimed at understanding the impact of temporary subsidies and information on coordination in this non-commercial setting. We identified 11,800 drivers in Singapore who had signed up for GrabHitch, but who were not currently active. We then randomized drivers to either one of two control conditions—pure control or placebo message—or one of the following treatment groups: a pure publicity intervention that reminded the drivers that giving a ride makes the drive more attractive; a subsidy intervention in which drivers were offered a subsidy per ride which could be either high or low for rides given within a stipulated period; and a set of demand information interventions that drew the drivers' attention to the fact that demand had gone up in the recent period (the highlighted increment was

[3]For a review of when financial incentives do and do not work in the context of public goods provision, education and lifestyle changes, see Gneezy et al. [2011].

[4]In addition to individual benefits from these transactions, carpooling also provides positive externalities, particularly in dense, urban settings: Hanna et al. [2017] show that the sudden removal of a mandated carpooling policy in Jakarta worsened overall traffic conditions.

[5]Carpooling coordination services are successfully running in a number of countries. BlaBlaCar, a carpooling service coordinating between-city rides that started in France, and now operates throughout Europe, has amassed over 70 million users and over €60 million in yearly revenue. Didi Chuxing, the leading mobile ride-hailing application in China, has a peer-to-peer service ("Hitch") that reached over 2 million rides on a peak day in 2017.

[6]GrabHitch is a vertical on the mobile application of Grab, a ride-sharing service whose core service resembles Uber or Lyft in the United States. With operations in 8 countries in Southeast Asia, and having merged with Uber's Southeast Asian operations in March 2018, Grab is the leading mobile ride-sharing company in the region.

either large or small and this was randomized) or that there was a substantial increase in the *excess* demand in the market.

The results provide no support for either of the theories of why market-makers should adopt these promotional strategies. The large subsidy had a positive effect on both plans made and rides during the week of the subsidy, but then they both fall relative to the control in the week and month after the subsidies expire. The smaller bonus created a similar pattern to the larger subsidy, although treatment effects were more muted in magnitude and significance: The small bonus had a small and insignificant positive effect on both plans made and rides given in the week of the subsidy, but a significant and somewhat larger negative effect on plans made and rides given in the week or month after the subsidy. In other words, if anything, the subsidy reduced market participation in the post-subsidy period. Furthermore, the companionship treatment had no observable effect on drivers.

On the question of whether drivers react to positive information about demand conditions (the complementarity effect), we find somewhat counterintuitive results. The high demand intervention actually reduced the number of plans made in the week and month after the intervention (and generated no effect during the weeklong subsidy period). There was a similar pattern with the high demand treatment's impact on rides given, but the effect was not significant. The treatment telling drivers about lower levels of increase in demand had no effects on behavior; in fact, we can safely rule out any meaningful effects. Furthermore, the negative effect is not being driven by drivers inferring that the market is more competitive due to the increase in demand: The excess demand intervention had a negative effect on plans made in the week and month after the intervention. The effect on rides given was negative in the week of the intervention, but insignificant afterwards. In other words, drivers did not respond to information about the thickness of the market by increasing participation as the idea of complementarities would suggest.

It is possible that the treatments—and in particular, the demand information treatments—failed because drivers were optimizing on dimensions other than the number of rides. For example, maybe they were instead becoming pickier about other dimensions of a ride, such as the detour that they would be willing to take in order to pick up a passenger, or the gender of the potential passenger. Using extensive pre-experiment, administrative data on driver choices among possible passengers, we estimate a model of driver preferences. We use these to preferences to weight the rides given by the drivers during the subsidy period and thereafter. With preference-weighted rides, the negative effects of the demand information intervention are no longer even close to significant. This suggests that it is perhaps possible that the drivers are reacting to good news about demand by becoming pickier, an idea consistent with the price-theoretic framework for matching markets of Azevedo and Leshno [2016], in which the "selectivity" of one side of the market adjusts endogenously to clear markets in the absence of prices. However, on net, it remains that the complementarity mechanism is not working here–news of more demand is not ultimately bringing forth more rides.

To explore the question of complementarities further, we ask whether the package of interventions, which did increase offers of rides (plans made) during the subsidy period, had any effect on the number of rides given in the following week. If the fact that there were more rides available had a positive effect on demand in the future this would be evidence that the complementarity mechanism works. We use an event study approach to look at this question, exploiting the timing of our experiment. Again, we find no effect on subsequent demand or efficiency in the market.

Finally, we investigate potential explanations for why these interventions failed to generate lasting effects on rides using administrative data. First, to better understand the "experience good" channel, we explore how potential market participants react to their failure to get a match. We find that both drivers and passengers are persistent in the sense that failures to match do not seem to discourage them from trying again. This might be a part of the explanation of why the treatments fail to generate changes in subsequent behavior, as drivers do not appear to be easily moved by positive or negative information (i.e. whether they receive or fail to receive a match). Second, we look at local market conditions to better understand heterogeneity in users' prior beliefs about match rates, market density and the relationship between match rates and market density. Over some intervals, match rates are increasing with market density, while over other intervals, match rates are decreasing with market density. So, some drivers may hold the belief that higher passenger density implies worse match rates, which could explain why drivers reacted negatively to our demand information treatments. Our exploration of local market conditions also provides further suggestive evidence that drivers adjust their selectivity when they know they are scarce relative to passengers, which aligns with the framework of Azevedo and Leshno [2016]. Finally, we discuss a potential explanation suggested by previous theories [Bénabou and Tirole, 2006] and empirical observations [Gneezy and Rustichini, 2000]: financial incentives may "crowd out" prosocial behavior.

This study contributes to the theoretical and empirical literature on platform economics, dynamic matching, and peer-to-peer marketplaces. Foundational theoretical work on platform economics focused on how to set prices as a function of user participation decisions [Caillaud and Jullien, 2003, Parker and Van Alstyne, 2005, Rochet and Tirole, 2003, Weyl, 2010], and later considered how information disclosure and search influences design considerations [Boudreau and Hagiu, 2009, Casadesus-Masanell and Halaburda, 2014]. Empirical studies of peer-to-peer online markets document how geographic heterogeneity generates frictions in growth [Cullen and Farronato, 2020], how ranking algorithms influence user choice [Hitsch et al., 2010], how search inefficiencies [Fradkin, 2015] and congestion [Arnosti et al., 2014, Horton, 2010] affect coordination, and how different pricing mechanisms influence equilibrium [Einav et al., 2018].[7]

To this literature we contribute empirical insights into how obstacles to platform growth interact with obstacles to incentivizing non-commercial activities in which participation motivations may be altruistic or prosocial. Such issues, as far as we know, have not been discussed in the context of platform economics. We find that promotional strategies designed for two-sided platforms with conventional prices may fail in peer-to-peer contexts in which non-price characteristics of the service are more salient for users. This finding has direct implications for the design and growth strategies of non-commercial intermediaries, and suggests new avenues for theoretical and empirical work investigating the interaction between platforms and prosocial behavior.

2 SETTING AND EXPERIMENTAL DESIGN

2.1 Setting

Set in a young peer-to-peer carpooling market in Singapore, this project empirically explores the growth of platforms that coordinate non-commercial activity. We partner with Grab, a privately-held South East Asian mobile ride-hailing company with operations in Singapore, Malaysia, Indonesia, Vietnam, Myanmar, Cambodia, Thailand and the Philippines. Having merged with Uber's Southeast Asian operations in March 2018, Grab is the leading mobile ride-sharing company in the region. The Grab app has many products and services including standard on-demand personal cars for hire (GrabCar and GrabTaxi), on-demand motorbikes (GrabBike), on-demand food delivery (GrabFood) as well as in-app platforms for messaging (GrabChat) and payment (GrabPay).

We focus on one vertical on Grab's smartphone-based platform, called "GrabHitch." GrabHitch has been in operation since November 2015 in Singapore. The "Hitch" vertical is a social carpooling service that aims to match passengers and non-commercial drivers who are traveling the same approximate route. Drivers are not professional drivers—they sign up to take passengers at their

[7] For a survey of the literature on online peer-to-peer markets, see Einav et al. [2016]

convenience. The platform automatically charges a small fare, paid to the drivers, intended to help offset petrol costs. For passengers, empirically observed prices for a given route on GrabHitch are 20-40 percent lower than the commercial rideshares or municipal taxis. Pricing is based solely on distance between origin and destination, and there is no surge pricing based on demand. Advertising for GrabHitch stresses the social, environmental and economic benefits of peer-to-peer car-sharing. For example, GrabHitch marketing emphasizes the potential to "expand your social network" while saving money and making Singapore a "car-light and friendly city."

Signing up as a driver for GrabHitch is simple relative to the much lengthier processes required for becoming a professional driver on other verticals.[8] Drivers must be over 18, have a valid license with at least 1 year of driving experience, and have a private car with valid auto insurance. Upon submitting documentation, Grab verifies the driver information and runs a background check, usually approving drivers within two days.

Once approved, drivers can propose "plans" on the app, entering their origin, destination, and desired departure time.[9] Meanwhile, passengers submit their "bookings" on the app, keying in their origins, destinations, and desired start times for their trips. Passengers can also indicate whether there are pets or other passengers traveling with them, and they can optionally choose to only be matched to drivers of the same gender. While the interface allows for passengers to make bookings as few as 5 minutes in advance and as many as 7 days in advance, GrabHitch recommends, for example, booking "the night before for a morning commute or 2 hours ahead of your evening ride home."

After the driver enters her plan, she sees a list of passenger candidates. The list includes the passenger's name, booking details, and photo (if provided). The list is dynamic: As more passengers make bookings that are compatible with the driver's plan, new passenger bookings appear. The GrabHitch algorithm for selecting the list of compatible passenger bookings for a given driver's plan works as follows. First, the algorithm considers only passenger bookings that have starting points within a 10km radius of the driver plan. Then, the algorithm excludes potential bookings based on user-entered constraints (e.g. number of seats, gender preference). Finally, the algorithm creates a plan-booking compatibility "score" using a proprietary algorithm based on passenger and driver trip details. The driver sees the eligible bookings—as many as fit on her smartphone screen (usually a maximum of 5-15 bookings fit on the screen)—ordered by the compatibility score. The driver can also scroll down to see the full set of compatible bookings.

The driver can then select a passenger from the list of candidates at any point up to 15 minutes before the trip. When selected, the passenger receives a notification along with the driver's name, mobile phone number, license plate, car type, car color, and photo (if available). The matched pair can communicate through an in-app messaging service or via SMS. If all goes smoothly, the driver will pick up the passenger at her requested origin at the requested time. After completing a trip, passengers typically pay their drivers through the built-in payment platform or in cash. Drivers can complete a maximum of two GrabHitch rides per day due to legal restrictions on non-commercial driving in Singapore.

Note that it is possible for either the driver or the passenger to cancel the ride after the parties have been matched. Cancelling the ride−−even after the match is confirmed by both parties−−incurs no cost to either party. However, if a passenger or driver cancels frequently, they can be barred from using the service in the future.

[8] GrabHitch initially required that both passengers and drivers sign in through their Facebook accounts, but this requirement was dropped.
[9] Drivers also have the option to indicate whether they would like to make a recurring or non-recurring plan.

2.2 Experimental Design

The experiment aimed to understand how information about market conditions affects the probability that a driver enters the market (i.e. makes a plan), as well as the ultimate probability and quality of a match (i.e. a ride). Specifically, we randomized the information content of messages that were sent to dormant drivers, defined as those who had not completed a ride in the 60 days prior to the experiment. We chose dormant drivers since market growth depends not just on attracting participation but also maintaining participation; thus, understanding the behavior of those who join but drop out can give us insights into the role of information in overcoming coordination failures. We included the entire universe of dormant drivers for the study, generating a sample size of 11,883 drivers.

The first set of treatments mimic typical promotions that offer subsidies to try the product or advertise particular features of it. Specifically, we randomly offered small and large bonuses per ride for a promotion period. The amounts and timeframes were determined by GrabHitch to be consistent with their practices. We offered $S4 (small bonus) or $S8 (large bonus) for any ride given on the day the SMS was sent or in the 5 days thereafter.

Second, we randomized some users into receiving information about the "social" benefit of carpooling, i.e. making new friends. This “Companionship” treatment group received messages that read: “We miss you! Offer a ride to a fellow commuter today and meet new friends!”

The next set of treatments are the "density treatments." We provide drivers in this treatment arm with information about how the passenger side of the market has changed since they last took a ride (see Appendix Table A2 for details). Specifically, drivers received messages that read "We miss you! Offer a ride to a fellow commuter today! The number of passengers on Hitch has increased X% since you last completed a ride!” where X was randomly assigned to be either 48 percent (“High density treatment”) or 24 percent (“low density treatment). These figures were calculated from the administrative data, with a base period of the last two months (i.e. 24 percent) or the last nineteen months (i.e. 48 percent). We choose these two base periods since no drivers had completed rides within the last two months, and the furthest rides were saw in the data were about 19 months.

Importantly, the density treatment not only signals an increase in passengers, but also an increase in drivers due to network effects. Thus, it is possible that drivers might infer from the density messages that the market is now too competitive, and it is not worth it for them to join. In order to isolate the effect of information about *excess demand* from demand, we also included a SMS message (“excess demand” treatment) that provided information about the growth of unmatched passengers in the last 60 days prior to the experiment: “Since your last ride, the monthly number of unmatched passengers has grown by 814,643!”

To evaluate the effect of these various treatments, we randomly assigned drivers to one of two control groups. First, we sent a placebo message that included just the first sentence in the treatment arms: “We miss you! Offer a ride to a fellow commuter today!” This treatment allows us to separate out the effect of the reminder of the existence of GrabHitch from the content of each message, and it is the primary control group that we compare each treatment to. Second, we also randomize some drivers into a “pure” control group that received no text messages, so we can measure the overall treatment effect of the messages.

2.3 Randomization and Experimental Implementation

We classified drivers into two strata: (1) “more dormant stratum” that consisted of drivers who had *neither made a plan nor completed a ride* in the 60 days before the experiment, and (2) “less dormant stratum” that consisted of drivers who had *made at least one plan* but had *not completed a ride* in the 60 days prior to the experiment. Within these two strata, we randomized drivers into roughly equal sized bins across the experimental treatments (see Table A1).

The experiment launched on July 11, 2017. GrabHitch sent the SMSs to the drivers.

2.4 Data Collection

We worked with Grab's data under strict confidentiality conditions, with our access limited to the purposes of this study. We had temporary access to a dataset of all plans made by drivers, bookings made by passengers, as well as ultimate matches and rides completed. For each of these, the data fields included request times, pick-up and drop-off locations, detours to pick up passengers, prices, and the gender of the drivers and passengers. No personally-identifiable data was used.

In addition, we also had temporary access to data on the backend universe of candidate bookings that were compatible with each driver plan, i.e. the choice set of passengers shown to drivers for a given ride. This will allow us to gain some insight into which characteristics drivers prefer (i.e. how much of a detour they are willing to make, do they prefer female passengers, etc.) and allows us to estimate the "quality" of a given ride by constructing a model of driver preferences.

3 BASIC MARKET DESCRIPTION

Before we turn to our experimental results, it is important to understand the evolution of the market, from GrabHitch's launch up until our experiment.

Table 1. **Summary statistics**

Panel A	
Market Level Statistics	
Total Drivers	111,248
Total Passengers	730,324
Total Completed Rides	4,493,903
Percent Drivers w> 0 Rides	71.4
Percent Passengers w> 0 Rides	59.0
Percent Drivers Female	18.2
Percent Passengers Female	54.1
Panel B	
Driver Level Statistics	
Mean Total Plans	122.3
Mean Total Rides	57.4
Median Plan Dist. (km)	21.9
Median Detour Completed Rides	2.6
Panel C	
Passenger Level Statistics	
Mean Total Bookings	18.2
Mean Total Rides	6.2
Mean Booking Dist. (km)	14.3

NOTE. Summary statistics of the GrabHitch market in Singapore, between January 1, 2016 and July 10, 2017. Panel A reports market level statistics. Panel B reports statistics about the universe of drivers, while Panel C reports statistics within the universe of passengers.

The total number of completed GrabHitch rides in Singapore from its inception to just before our intervention is 4,493,303 (Table 1, Panel A). The number of completed rides steadily increased over time, peaking at 217,717 rides in the week of June 12, 2017 (Figure A1). Note that the number of completed rides is subject to market-wide shocks, such as promotions.

We next examine key facts about the market participants, defined as anyone who has entered a booking as a passenger or plan as a driver, regardless of whether they have ever actually completed a ride.[10] The total number of participants in the market includes 111,248 drivers and 730,324 passengers. Only about 18 percent of the drivers are female, despite a more even split in the gender of passengers. The first key fact that we observe is that the total number of active drivers has increased steadily over time (Figure 1, Panel A). We define active drivers to be drivers who make at least one plan in a given week. Meanwhile, the number of new drivers signing up each week also increased over time, albeit less steadily (Figure 1, Panel B).

[10]One can be both a driver and a passenger. We treat one individual who participates in both sides of the market as distinct when they are in their passenger vs. driver role.

Second, drivers appear more active than passengers. On average, drivers made 122 plans, and took 57 rides. In contrast, passengers made an average of 18.2 bookings and took about six rides.

Third, most bookings tend to occur during commuting hours. Figure A3 shows the frequency of passenger bookings and driver plans, by time of day. There are sharp peaks in passenger bookings around 8:30am and 6pm. There are also corresponding peaks for drivers, but they are less pronounced and drivers are more likely to make a plan for the middle of the day than passengers.

Next, we observe some differences in the level of activities across neighborhoods, but not large differences in patterns over time. We used GrabHitch's geographic boundaries, which classifies the city into 54 areas.[11] The area with the most activity in terms of passenger requests is Tampines, a residential suburb. Other active areas (in the next-to-darkest shade of green on the heatmap) include the Woodlands, Houjang, Bedok, Downtown Core, Bukit Merah, and Jurong West. We then graph completed rides by neighborhood for the ten most active neighborhoods (Figure A2).[12] The neighborhood graphs are interesting: One might think that the overall trend in market growth and development may be masking neighborhood differences in growth trajectories—small initial differences in market conditions could lead to large differences due to economies of scale. However, we do not find any observable evidence of this.

Finally, we examine detours, as they provide a sense of how much someone is willing to change their plans in order to pick up a passenger. Overall, drivers plan longer trips than passengers book: The median driver plan is 21.9 kilometers (Table 1, Panel B) while the median passenger booking is 14.3 kilometers (Panel C). For reference, a trip from the center of the largely residential neighborhood of Tampines to the Downtown Central Business District is about 20 kilometers. For completed rides, the median detour that drivers take to pick up their passenger—defined as the distance from the passenger's pick-up point to the driver's origin—is 2.6 kilometers, or about 18 percent of the median distance of completed rides.

4 DO TEMPORARY BONUSES OR INFORMATIONAL MESSAGES CHANGE BEHAVIOR?

4.1 Experimental Results

We now turn to the results of our field experiment conducted in cooperation with GrabHitch, which offers insights into the role of information in platform participation and coordination. As we discuss above, we focus on "dormant" drivers, defined as those who had not picked up a passenger in the previous 60 days.

We estimate the impact of the different information treatments (T_d) on two key outcomes (y_d): whether the driver d makes a plan, i.e. whether the treatment moves a driver to enter the market, and whether the driver ultimately provides gives a ride to a passenger. Specifically, we estimate using OLS:

$$y_d = \beta_0 + \sum_T \beta_T T_d + \delta_d + \varepsilon_d \tag{1}$$

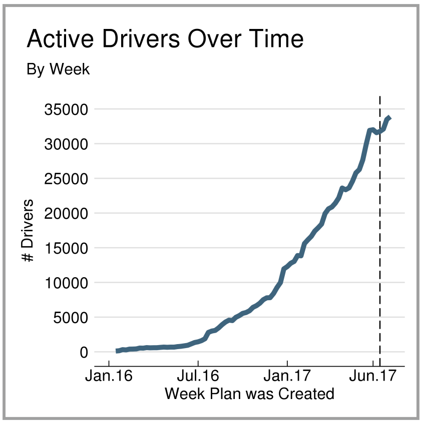
Where δ_d is an indicator variable for whether the driver is more or less dormant (i.e. the strata). Standard errors are Huber-White robust standard errors.[13] We estimate the treatment effects for three different time periods. First, we examine what happens when the information has just been received and the subsidies are still active (July 11-16). Since drivers learn more about the market

[11] Areas that are not included in these neighborhoods are coded in our analyses as "other."

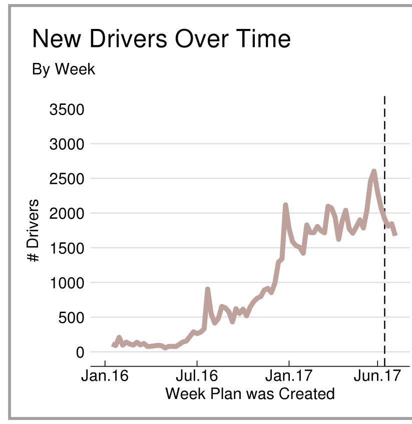
[12] In order to find the "top 10 neighborhoods," we first ordered the 54 neighborhoods by the total number of completed rides that had their pick-up point in that neighborhood, over our entire observation period.

[13] As we randomized treatment, our coefficient estimates capture the causal effect of the treatments. However, as an additional robustness check, we include the total number of plans made prior to the experiment as an additional control variable to correct for any small sample imbalances. Table A4 shows that this specification provides near identical results.

Fig. 1. **Drivers over time**



(a)



(b)

NOTE. Panel A shows the number of active drivers in each week from the inception of GrabHitch to our intervention. A driver is considered "active" in a given week if she enters a plan in that week. Panel B shows the number of new drivers each week from the inception of GrabHitch to our intervention, where new drivers are defined as drivers who have never made a plan before. In both panels, the dotted line indicates the date of our intervention.

through experience, and as since market conditions change week to week−−we then examine what happens after the subsidy period. Specifically, we examine the week after the subsidy period ends (July 17-22) and the month after (July 17-August 15).

For this analysis, we drop the pure control group from the estimation sample and we compare the information treatments (T_d) to the placebo message treatment. Therefore, each β_T captures the independent impact of receiving the content of each treatment message, as distinct from the impact of receiving a message. However, in Table A3 we estimate the impact of receiving any message treatment relative the pure control and we find that receiving a message increases the probability of making a plan during the subsidy period by 26 percent, but has no observable lasting effect in the subsequent periods or on the probability of taking a ride. This confirms that the messages were being read by drivers, enough so to impact their behavior in some ways.

Table 2 provides our findings of the impact of the treatments on drivers' choices to entering a plan and to give a ride (Equation 1).[14] We have two key sets of findings. First, we examine the subsidy results. Recall that a key motivation for the prevalence of temporary bonuses among rideshares is that the use of a new intermediary is always partly an experience good, and so getting potential

[14]While Table 2 considers whether the treatment affected intensive margin decisions (whether you undertook a plan or ride), Table A5 considers the effect of treatment on both intensive and extensive margin outcomes. Specifically, we examine the number of plans made and the number of rides taken. We find similar effects that the excess demand information reduces the number of plans made, as well as rides taken. However, while we find that the large bonus affects the decision to enter the market, we find no observable impact on the number of rides taken.

participants to use the market-place may make it more likely that they continue to do so in the future.

Table 2. **Treatment effects: Made plan or gave ride**

	(1) Made Plan (During Subsidy)	(2) Made Plan (Week After)	(3) Made Plan (Month After)	(4) Gave Ride (During Subsidy)	(5) Gave Ride (Week After)	(6) Gave Ride (Month After)
Density Low	0.011	-0.008	-0.004	0.002	0.000	0.007
	(0.009)	(0.010)	(0.014)	(0.006)	(0.006)	(0.009)
Density High	0.002	-0.018*	-0.035***	0.001	-0.007	-0.001
	(0.009)	(0.010)	(0.013)	(0.006)	(0.006)	(0.009)
Excess Demand	-0.001	-0.026***	-0.042***	-0.012**	-0.008	-0.001
	(0.009)	(0.009)	(0.013)	(0.005)	(0.006)	(0.009)
Small Bonus	0.008	-0.019**	-0.028**	-0.002	-0.006	-0.009
	(0.009)	(0.010)	(0.014)	(0.006)	(0.006)	(0.009)
Large Bonus	0.016*	-0.021**	-0.027**	0.011*	-0.010*	-0.003
	(0.009)	(0.009)	(0.014)	(0.006)	(0.006)	(0.009)
Companionship	0.007	-0.012	-0.022*	-0.003	-0.006	-0.003
	(0.009)	(0.010)	(0.014)	(0.005)	(0.006)	(0.009)
Observations	10401	10401	10401	10401	10401	10401
Control Mean	0.066	0.085	0.199	0.025	0.029	0.066

NOTE. Each column in this table comes from a separate OLS regression of respective outcome on the treatment and strata fixed effects. The control group is drivers who received message with no informational content. Huber-White robust estimates of the standard errors are reported in parentheses. Asterisks are based on standard p-values ($^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$).

However, if anything, we find the opposite. While drivers take advantage of the bonuses while the bonuses are available, they are then less likely to use the service afterwards, when the bonus is no longer available. The larger bonus (S$8) increased activity during the subsidy period: it increased the probability that the driver entered a plan during the subsidy period by 24 percent relative to the control group (significant at the 10 percent level), as well as the probability that they gave a ride by almost 45 percent. However, in the post-subsidy week, drivers in the larger bonus group reduced the probability that they entered a plan relative to the control group by about 24 percent (significant at the 10 percent level) and they were 34 percent less likely to complete a ride. The small bonus effects were more muted, but also showed declines after the subsidy period: The small bonuses (S$4 per ride) had no noticeable effect on making a plan nor giving a ride during the subsidy period, and led to a 14 percent decrease in the probability that a driver would make a plan over the course of the month after the subsidy ended. In short, rather than inducing drivers into the market through experience, the temporary subsidies appeared to simply shift activity to the subsidy period.

Second, we find no observable evidence that drivers respond to information about the thickness of the market by increasing participation as the idea of complementarities would suggest. The lower density treatment had no observable effect on behavior, while the high density information decreased the probability that a driver entered a plan. Ultimately, neither treatment had any impacts on the probability that a ride was provided.

Of course, it is possible that the density treatments also conveyed information that supply had increased alongside demand—in which case drivers may believe the market is too congested. Thus, we turn to the excess demand treatment, which isolated information on the number of unmatched passengers. Again, we find that, if anything, the excess demand information led to decreases in the probability of entering a plan relative to the control group. In the first few days after the information was sent, those who received the excess demand information were 48 percent less likely to take a ride (significant at the 5 percent level). Note that this effect is materially large, as the percent change is equivalent to the size of the large bonus treatment.

One possible alternative explanation of why information about market thickness did not induce participation is that drivers may respond to the information by being pickier about the *quality* of their rides (e.g. how much of a detour it is, the gender of their passenger, etc) rather than just increasing rides.[15] Thus, we next examine two measures of quality in Table 3

First, we examine a very simple measure of quality: the detour that driver makes if they take a ride (Table 3, Columns 1 - 3). Note that we only observe the detour if someone takes a ride, and we know the treatments induce different probabilities of taking a ride across treatments, so these results are suggestive rather than causal.[16] Those who receive the low- or high- density treatments do not take different detours relative to those in the control. However, those in the excess demand group appear picker than the control: They give rides that require detours that are 0.128 kilometers shorter than the detours taken by the control group (0.413), representing a 31.6 percent decrease.

Table 3. **Treatment effects: Quality of rides given**

	Relative Detour			Quality Weighted Rides		
	(1) During Subsidy	(2) Week After	(3) Month After	(1) During Subsidy	(2) Week After	(3) Month After
Density Low	-0.006	-0.014	-0.003	-0.065	0.131	0.091
	(0.061)	(0.052)	(0.033)	(0.180)	(0.303)	(0.839)
Density High	0.015	0.018	0.005	0.071	-0.205	-0.156
	(0.065)	(0.053)	(0.037)	(0.210)	(0.244)	(0.844)
Excess Demand	-0.128**	0.008	-0.030	-0.143	-0.148	0.395
	(0.053)	(0.056)	(0.035)	(0.182)	(0.250)	(0.895)
Small Bonus	-0.047	0.072	-0.029	0.455	-0.050	0.201
	(0.063)	(0.057)	(0.037)	(0.449)	(0.249)	(0.864)
Large Bonus	-0.020	0.066	0.008	0.513*	-0.219	-0.485
	(0.057)	(0.066)	(0.036)	(0.280)	(0.249)	(0.792)
Companionship	-0.023	-0.054	-0.034	-0.129	-0.203	0.195
	(0.074)	(0.043)	(0.035)	(0.171)	(0.234)	(0.865)
Observations	170	171	517	10401	10401	10401
Control Mean	0.412	0.346	0.384	0.491	0.692	3.216

NOTE. Each column in this table comes from a separate OLS regression of respective outcome on the treatment and strata fixed effects. Huber-White robust estimates of the standard errors are reported in parentheses. Asterisks are based on standard p-values ($^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$).

Second, we make use of the detailed administrative data on the driver's choice set of passengers for a given plan to construct a new outcome called "quality-weighted rides." This measure weights a ride given by a driver by a proxy for the idiosyncratic "quality" of that passenger's booking. Our

[15] This idea is akin to Azevedo and Leshno [2016]—when drivers know that they are scarce relative to passengers, they adjust their "selectivity" much like suppliers in standard frameworks would increase their prices.

[16] In Table A6, we also examine detours across the full sample. In particular, we code the detour taken as "0" if one did not take a ride, and so the measure captures both the extensive margin of detour and the intensive margin of whether you would take a detour. The results are similar in that those in the drivers who received the excess demand treatment are less likely to take a detour, and so are perhaps choosing high quality matches.

quality weights take into account six observable measures: (i) relative detour (the distance that the passenger booking adds to the driver's entered plan, divided by the driver's entered plan, both in kilometers); (ii) passenger gender; (iii) an interaction term for the driver gender and the passenger gender; (iv) the number of seats requested by the passenger; (v) the difference between the start time of the passenger's booking and the driver's plan (in minutes); (vi) whether the passenger has a photo.[17] Details about the construction of quality weights are provided in Appendix A.

We find no observable impact of the information about density or excess demand on quality weighted rides (Table 3, Columns 4 - 6). Particularly striking is that while the excess demand treatment led to a 48 percent decrease in rides taken in the week the information was received, we find no observable impact on quality adjusted rides. This fact suggests that when drivers did give a ride, they tended to give rides of high quality. While not conclusive, the evidence about driver detours and quality adjusted rides suggests that one reason why drivers may have reduced their entry behavior is that they became pickier about the quality of their ride, knowing that there were plenty of passengers to match with.

4.2 Did Our Intervention Improve Coordination Overall?

To explore the question of complementarities further, we ask whether the package of interventions, which did increase offers of rides (plans made), had any effect on the number of rides given in the week following the messages. As we reported in Table A3, the experimental treatments as a whole increased drivers' probability of making a plan by nearly 26 percent. We thus can see if the introduction of the experiment led to subsequent changes in the market.

In order to investigate whether our intervention had an effect on the full market, we implemented an event study design. Figure 3 shows driver and passenger activity and match rates in the days prior to and after the intervention in the market as a whole. That is, each observation in Figure 3 is the match rate or activity level in all of Singapore on the day indicated on the x axis.[18]

To avoid the seasonal noise driving our results, we chose to focus our event study on a small window around the intervention: 10 days before and 6 days after. This window is shown in Figure 3 in light grey dotted lines. The period of 6 days after the intervention coincides with the period during which the subsidy was active. Overall, we find no effect. In addition to the graphical evidence, Table A8 shows the coefficients on our event study specification. Effects on activity outcomes reported in Panel A of Table A8 (plans, bookings, completed rides, number of driver and passengers) are positive but very small in magnitude and not statistically significant. The same is true for match rates, with the exception of passenger match rates, which decreased (Panel B, Table A8).

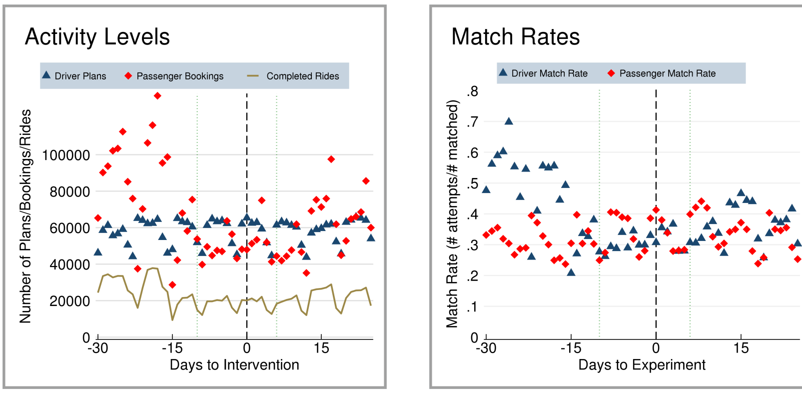
5 MECHANISMS

Thus far, our results suggest that temporary bonuses did not lead to sustained participation in the market. And, while the various types of information about the thickness of the market may have somewhat improved the quality of the matches, for the most part, the informational interventions either had no effect on or reduced market participation. We next explore reasons why our interventions—contrary to logic of experience goods and of network effects and complementarities—for the most part failed in this non-commercial setting.

[17] Anecdotal evidence is consistent with the importance of these factors, see for example: https://cnalifestyle.channelnewsasia.com/trending/how-to-get-grab-hitch-booking-driver-accept-pick-up-secrets-10256366. Table A7 confirms that detour, passenger gender, booking-plan time difference and number of seats are all important predictors of a driver choosing a passenger.

[18] The driver match rate in neighborhood i in week t is simply the total number of driver plans originating in neighborhood i in week t divided by the total number of completed plans in the same neighborhood and week. The passenger match rate is defined similarly.

Fig. 3. **Event study: Activity before vs after intervention**



(a) (b)

NOTE. Panel A shows driver and passenger activity, and the number of completed rides, in the entire market in the 30 days before and after our intervention. Panel B shows driver match rates (number of completed plans/number of driver plans) and passenger match rates (number of completed plans/number of passenger bookings) in the entire market before and after our intervention. In both panels, the black dashed line indicates the date of our intervention, while the light green dotted lines indicate the start and end days of the period used in our event study specification.

We consider two possible explanations using the extensive administrative data available. First, we examine whether our treatments failed because individuals are persistent, i.e. they nonetheless continue to participate in the market even if they do not get successful matches. Second, we consider whether individuals incorporate more localized information about their neighborhood—rather than market information—in making their choices, which would drive them to possibly value the overall market information less.

We conclude this section with a third potential explanation suggested by previous theory and empirical studies. To the extent that drivers see themselves as performing an altruistic act when they give rides to passengers, it may be that financial incentives crowd out prosocial behavior.

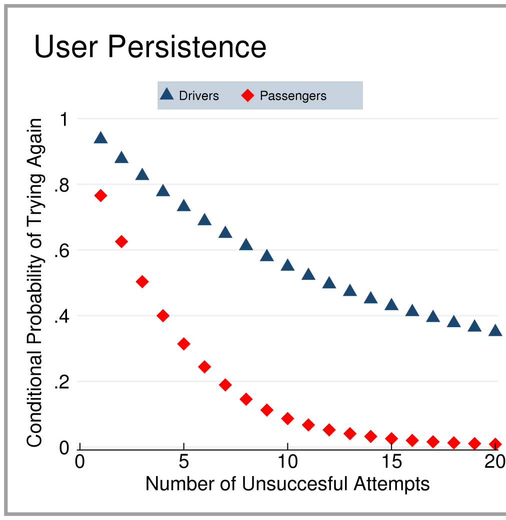
5.1 User Persistence

In this section, we examine whether our treatments failed because individuals are persistent, i.e. they nonetheless continue to participate in the market, even if they do not get successful matches. To do so, we examine the extensive observation data from the entire GrabHitch platform since its launch up until our experimental launch.

In particular, we ask: what is the probability that a driver enters a plan into the app again –i.e. tries again—conditional on having an unsuccessful attempt—i.e. enters a plan into the app that does not lead to a ride? We provide a measure of persistence in Figure 3, where we graph the probability that a driver makes an additional attempt after failing X times. Drivers appear to be persistent: 93.7 percent of drivers try again after their first attempt is unsuccessful, while 87.7 percent of people try again after two unsuccessful attempts. In fact, after 10 unsuccessful tries (and no successful ones), 54.9 percent of drivers try again, and even after 20 unsuccessful tries, 35.0 percent of drivers try again.

We construct a similar graph for passengers to help put the driver findings into context. We find that passengers are less persistent than drivers: 76.6 percent try again after the first failure,

Fig. 5. **User persistence**



Note. This figure reports the conditional probability that drivers and passengers make X+1 attempts given that X attempts have been unsuccessful. These conditional probabilities are calculated off the universe of administrative data from January 2016 until the day before our intervention in July 2017. Passenger persistence is marked in red, while driver persistence is marked in blue.

but only 31.4 percent try again after 5 failures in a row. By 10 failures in a row, only 8.6 percent try again. There are a number of reasons why passengers may be less persistent. For example, passengers have other rideshare verticals available to them while drivers do not. In addition, drivers and passengers receive different information about why they failed to get a match.

These measures of persistence provide suggestive evidence that one reason why the treatments failed to generate sustained changes is that the market participants are not updating their beliefs based on signals, such as whether they get a match or not. So perhaps information about market conditions is also unlikely to move beliefs.

Furthermore, the persistence of drivers could be linked to their pickiness. Since drivers are in control of choosing among passengers presented to them, their persistence may indicate their rejection of the passengers shown to them, and their choice to opt instead to try again in hopes of getting better options.

5.2 Neighborhood Market Conditions

While much of our earlier analysis considered Singapore as a whole, there is considerable heterogeneity in market conditions across different neighborhoods at different times (Figure A6). This heterogeneity suggests the possibility that the market is very localized (i.e. drivers tend to pick up passengers who live near them). Therefore, we next explore patterns in the data across neighborhoods to help provide a better understanding of why the treatments failed to lead to sustainable increases in matching.

Two key facts emerge. First, it is possible that our information treatments about density provided bad news for some drivers. Depending on when and where drivers in our sample made their plans in the past, they could have experienced very different market conditions, which informed their prior belief about the efficiency of the market. And, given that drivers are fairly persistent, as described in the subsection above, it's likely that they have several data points from prior experience with the app that inform their beliefs about the market. These beliefs may be not just about overall match efficiency, but in fact how match efficiency relates to market density. The passenger to driver ratio serves as a useful measure of market density. Overall passengers' match efficiency decreases with passenger density (Figure A8, Panel A) while drivers' match efficiency increases with passenger density (Figure A8, Panel B). However, a closer look at match rates plotted against deciles of passenger density shows that while driver match rates increase over the lowest five deciles (starting at 0.13), they then level off around the sixth decile (at a mean match rate of 0.33) before decreasing slightly to 0.29 in the 10th decile (Figure A10). Some drivers whose previous market experience lies in this region may have learned that efficiency is decreasing as passenger density increases. These drivers might update negatively upon receiving our information treatments, which could help explain why our messages about excess demand led to fewer drivers making plans and completing rides.

Second, as in section 4, we find additional evidence that drivers may become picker when there are more passengers to choose from, as described by Azevedo and Leshno. Initially, as passengers become more available relative to drivers, the match rate increases (Figure A10). However, as the ratio increases beyond a certain point—roughly at a ratio of 1.5, the driver match rate flattens out (Figure A10). This pattern in the data is consistent with our earlier finding, that driver may have preferences about other characteristics of the ride, and so information on demand conditions may simply make them picker on dimensions other than price.

5.3 Did Incentives Crowd Out Prosocial Behavior?

Financial incentives have psychological effects on their recipients. These psychological effects are modeled in Bénabou and Tirole [2006], where individuals' utility has three components. First individuals value "extrinsic" rewards (like money). Second, individuals enjoy doing certain activities. And third, individuals care intrinsically about their image—both how they appear to themselves (self-image) and to others (social image). This model highlights one channel through which incentives may backfire: material rewards increase the first component (extrinsic reward) but in so doing decrease the benefits from the third component (self- or social-image concerns). If the increase to the first component is smaller than the decrease in the third component, monetary incentives may actually deter individuals from engaging in the incentivized activity.

To the extent that drivers' motivation to provide rides comes from a prosocial or altruistic impulse, it may be the case that our intervention—especially the subsidies—did exactly this. Since drivers are unlikely to provide rides for financial gain, a significant component of their benefit from giving a ride may come through the image channel. If this is the case, then financial incentives may have made giving a ride seem less attractive. This effect is consistent with many empirical findings in the literature reviewed in Gneezy et al. [2011], including Meier [2007] who found dynamic effects consistent with ours: price incentives for charitable giving increased donations while the incentives were in place, but then decreased donations relative to the baseline when the incentives expired. This effect is similar to ours, in which drivers who received financial incentives participated more while the subsidy was active but then decreased their participation relative to the baseline when the subsidy lapsed.

6 CONCLUSION

When developing new markets, platforms often use subsidies and information campaigns to draw new users. Platforms pursue these tactics with two theories in mind: that the service they provide is an experience good which must be experienced to attract and retain a user base, and that inducing entry of one side of the market can spur growth of the market as a whole through complementarities in user decisions. Through an experiment with drivers on a new carpooling platform in Singapore, we find no evidence to support either theory for why market-makers should adopt these strategies. One reason why "the experience good" logic breaks down in this setting is that users are persistent−−they do not react much to market signals. Further, we find suggestive evidence that drivers get pickier about which passengers they choose when they know they are scarce relative to passengers, which blocks the putative feedback loop driven by complementarities in supply and demand decisions.

Our findings have important implications for the growth strategies of non-commercial peer-to-peer platforms—which may differ substantially from growth strategies for commercial counterparts—and suggest several directions for future research. This study implies that growth strategies that work for platforms with conventional pricing−−such as promotional campaigns designed with network effects in mind−−may not work in matching markets without flexible prices in which users on each side have highly idiosyncratic preferences over the other. Our results suggest that more work must be done to adapt the rich insights from the literature on platform economics, developed with commercial settings in mine, to platforms that leverage prosociality and aim to create community around new public goods.

A valuable avenue for future research is to better understand, theoretically and empirically, why these strategies fail in settings like the one we studied, and what strategies could be better tailored to these settings. For example, it would be valuable to better understand *why* users are so persistent, and thus unlikely to react to information. In addition, more work must be done to understand how intermediaries can better match users along non-price dimensions using models of user choice estimated from historical databases. These research avenues, together, could generate insights into how platforms and match-makers can deploy growth strategies that involve user-specific subsidies to the *quality* dimension of matches, analogous to subsidies in settings where prices move users in and out of the market.

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A CONSTRUCTION OF THE QUALITY-WEIGHTED RIDE METRIC

We constructed our measure of quality weighted rides as follows. We looked at a database that shows the compatible passenger bookings shown to each driver when they enter a plan. Then, we analyzed driver choices. Note that when a driver chooses a passenger booking, they do not necessarily complete the ride because passengers could still cancel or the driver may change her mind.

For each driver, we looked at how six factors influence their choice of passenger. The six factors we look at are: relative detour−−that is distance that the passenger booking adds to the driver's entered plan, divided by the driver's entered plan (both in kilometers); passenger gender; an interaction term for the driver gender and the passenger gender; the number of seats requested by the passenger; the difference between the start time of the passenger's booking and the driver's plan (in minutes); whether the passenger has a photo.

We trained the data on histories of driver choices before our intervention, using all of drivers in our sample. For every driver plan p and booking i we estimated the following OLS model

$$\begin{aligned} y_{ip} &= \alpha_p + \beta_1(\text{relative detour})_{ip} + \beta_2\ (\text{passenger female})_{ip} \\ &\quad + \beta_3(\text{passenger female})_{ip} \times (\text{driver female})_{ip} + \beta_4(\text{booking-plan time difference})_{ip} \\ &\quad + \beta_5(\text{\# of seats requested})_{ip} + \beta_6(\text{passenger has photo})_{ip} + \varepsilon_{ip} \end{aligned} \tag{2}$$

and a Logit equivalent (letting $\mathbf{x}_{ip}$ be the vector of passenger characteristics used in Equation 2)

$$\mathbb{P}(y_{ip} = 1 | x_{ip}, \alpha_p) = \frac{e^{\alpha_p + \beta \mathbf{x}_{ip}}}{1 + e^{\alpha_p + \beta \mathbf{x}_{ip}}} \tag{3}$$

As shown in Table A7, relative detour, passenger gender, booking-plan time difference, and number of seats requested are all strong predictors of driver choice.

After estimating the coefficients above, we predict a fitted quality "score" for each passenger candidate in the message treatment groups. So, for each passenger booking i that was ultimately given a ride by driver in our sample (excluding pure control) who made plan p, we estimated the plan-booking-specific probability that that booking would be chosen by that driver. In other words, for each passenger booking p we estimated $\hat{y}_{ip}$ for OLS and $\mathbb{P}(\hat{y}_{ip} = 1 | x_{ip}, \alpha_p))$ for Logit. Then, we normalize these values by dividing by the maximum score in the entire set of drivers who gave rides. Then, our "quality-weighted rides" outcome is simply the number of rides given by a driver weighted by this quality score, which is on a 0-100 scale.

To define this measure more formally, let $\mathcal{D}$ be the set of drivers who complete a ride in our sample. Let P_d be the set of plans logged by driver d and let $C(P_d) \subseteq P_d$be the subset of plans completed by driver d, with generic element p_d. Define $\hat{Y}$ as

$$\max_{d \in \mathcal{D}, p_d \in C(P_d)} \hat{y}_{ip} = \hat{Y}. \tag{4}$$

and q_d as,

$$q_d = \frac{\hat{y}_{ip}}{\hat{Y}} \times 100. \tag{5}$$

Our quality weighted rides outcome for driver d is Q_d, which is defined using Equation 4 and Equation 5. It is the sum of driver d's completed rides weighted by the quality score of each completed ride, i.e.

$$Q_d = \sum_{p_d \in P_d} q_d \mathbf{1}[p_d \in C(P_d)] \tag{6}$$

where 1 is the indicator function.

B SUPPLEMENTARY MATERIAL

Table A1. Sample selection

	More Dormant	Less Dormant	Total
Control (No Message)	1,121	361	1,482
Control (Message)	1,131	364	1,495
Density Low	1,133	349	1,482
Density High	1,127	366	1,493
Excess Demand	1,127	358	1,485
Small Bonus	1,124	360	1,484
Large Bonus	1,111	355	1,466
Companionship	1,131	365	1,496
Total	9,005	2,878	11,883

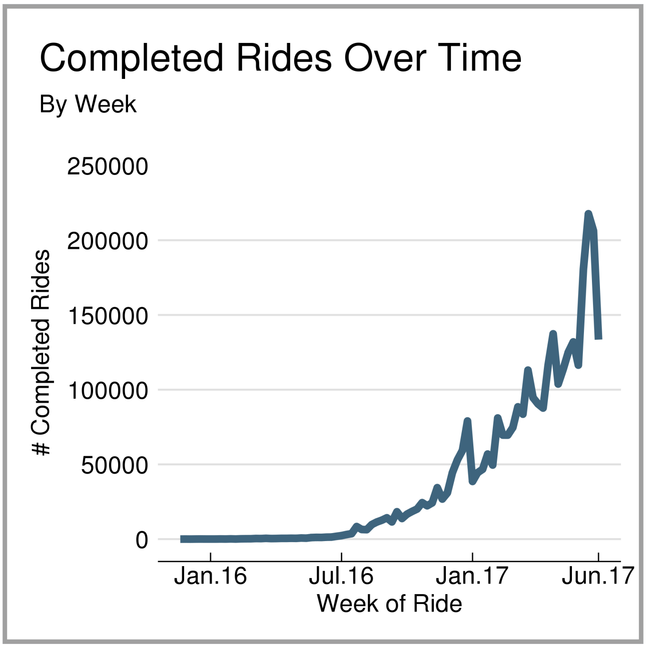
NOTE. This table lists the total number of drivers from each stratum (more dormant, less dormant) assigned to each treatment arm.

Table A2. Messages to treatment groups

- **Density Low:** “We miss you! Offer a ride to a fellow commuter today! The number of passengers on Hitch has increased 24% since you last completed a ride!"
- **Density High:** “We miss you! Offer a ride to a fellow commuter today! The number of passengers on Hitch has increased 48% since you last completed a ride!"
- **Excess Demand:** “We miss you! Offer a ride to a fellow commuter today! Since your last ride, the monthly number of unmatched passengers has grown by 814,643!"
- **Small Bonus:** “We miss you! Offer a ride to a fellow commuter on 11-16 July & earn S$4 EXTRA per ride! T&C apply."
- **Large Bonus:** “We miss you! Offer a ride to a fellow commuter on 11-16 July & earn S$8 EXTRA per ride! T&C apply."
- **Companionship:** “We miss you! Offer a ride to a fellow commuter today and meet new friends!"
- **Control (Message):** “We miss you! Offer a ride to a fellow commuter today!"

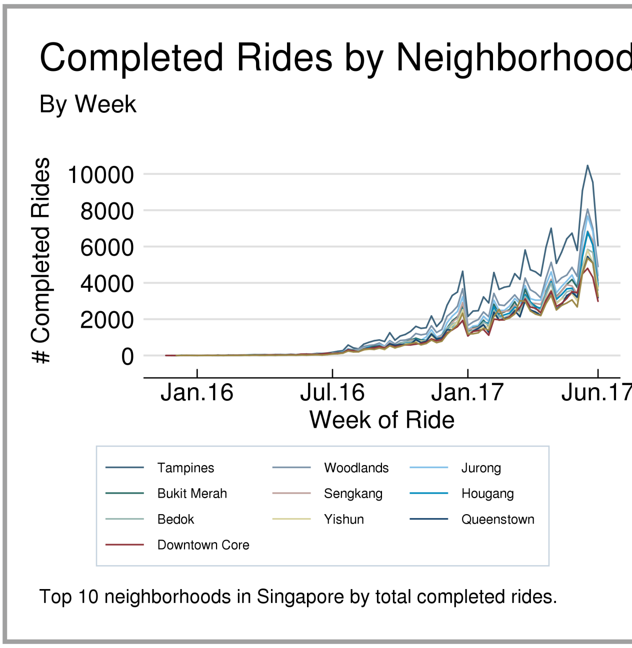
Note. Text (verbatim) of messages sent via SMS to drivers in the experimental sample.

Fig. A1. Completed rides over time



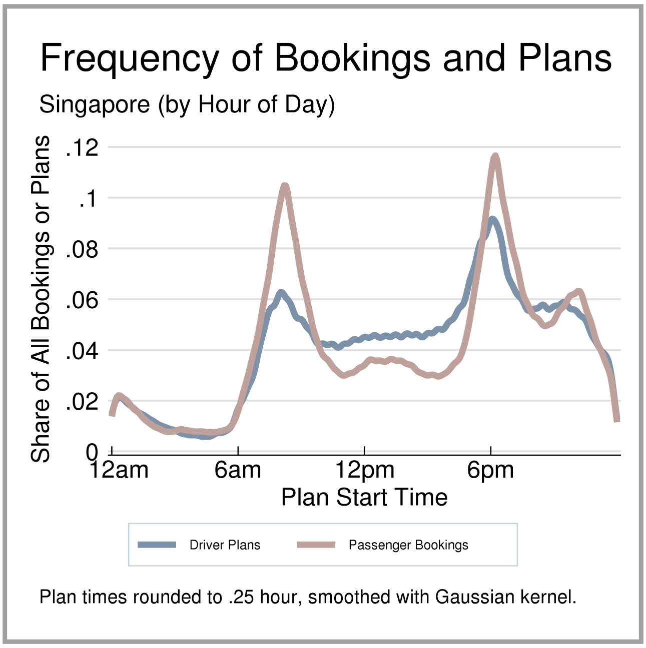
NOTE. This figure shows the number of completed rides per week from the inception of GrabHitch to the month after our intervention. The dashed line indicates the date of our intervention.

Fig. A2. Completed rides over time: Top 10 neighborhoods



Note. This figure shows the number of completed rides per week from the inception of GrabHitch to the month after our intervention in the top 10 neighborhoods. The top 10 neighborhoods are the 10 neighborhoods with the highest number of total completed rides on the platform before our intervention.

Fig. A3. Frequency of plans and bookings by time requested



Note. This figure shows the frequency of passenger bookings and driver plans by hour of day. Plan and booking times are rounded to the quarter-hour, and the distribution is smoothed with a Gaussian kernel.

Table A3. Treatment effects: Effect of any message on driver activity

Panel A: Made Plan and Gave Ride

	(1) Made Plan (During Subsidy)	(2) Made Plan (Week After)	(3) Made Plan (Month After)	(4) Gave Ride (During Subsidy)	(5) Gave Ride (Week After)	(6) Gave Ride (Month After)
Message	0.015**	0.001	-0.014	0.003	0.006	0.006
	(0.006)	(0.007)	(0.010)	(0.004)	(0.004)	(0.007)
Observations	11883	11883	11883	11883	11883	11883
Control Mean	0.057	0.070	0.191	0.022	0.018	0.059

Panel B: Number of Plans Made and Number of Rides Given

	(1) # Plans (During Subsidy)	(2) # Plans (Week After)	(3) # Plans (Month After)	(4) # Rides (During Subsidy)	(5) # Rides (Week After)	(6) # Rides (Month After)
Message	0.431**	0.197	0.179	0.228	0.118	0.136
	(0.196)	(0.189)	(0.146)	(0.283)	(0.309)	(0.203)
Observations	11883	11883	11883	11883	11883	11883
Control Mean	0.217	0.271	1.955	0.059	0.060	0.349

Note. Each column in this table comes from a separate OLS regression of respective outcome on the treatment (received any message at all) and strata fixed effects. The control group is drivers who received no message. Huber-White robust estimates of the standard errors are reported in parentheses. Panel A shows binary outcomes—*whether* a given driver made a plan or gave a ride. Panel B shows counts—*how many* plans and rides were given by drivers. Asterisks are based on standard p-values ($^{*}p < 0.1$,$^{**}p < 0.05$,$^{***}p < 0.01$).

Table A4. Treatment effects: Made plan or gave ride (with driver history)

	(1) Made Plan (During Subsidy)	(2) Made Plan (Week After)	(3) Made Plan (Month After)	(4) Gave Ride (During Subsidy)	(5) Gave Ride (Week After)	(6) Gave Ride (Month After)
Density Low	0.011	-0.008	-0.003	0.003	0.000	0.007
	(0.009)	(0.010)	(0.014)	(0.006)	(0.006)	(0.009)
Density High	0.002	-0.018*	-0.035***	0.001	-0.007	-0.001
	(0.009)	(0.010)	(0.013)	(0.006)	(0.006)	(0.009)
Excess Demand	-0.001	-0.026***	-0.042***	-0.012**	-0.008	-0.001
	(0.009)	(0.009)	(0.013)	(0.005)	(0.006)	(0.009)
Small Bonus	0.008	-0.018*	-0.027**	-0.002	-0.006	-0.009
	(0.009)	(0.009)	(0.014)	(0.006)	(0.006)	(0.009)
Large Bonus	0.016*	-0.021**	-0.027**	0.011*	-0.010*	-0.003
	(0.009)	(0.009)	(0.014)	(0.006)	(0.006)	(0.009)
Companionship	0.007	-0.012	-0.022	-0.003	-0.006	-0.003
	(0.009)	(0.010)	(0.014)	(0.005)	(0.006)	(0.009)
Observations	10401	10401	10401	10401	10401	10401
Control Mean	0.066	0.085	0.199	0.025	0.029	0.066

NOTE. Each column in this table comes from a separate OLS regression of respective outcome on the treatment, strata fixed effects, and drivers' number of previous plans. The control group is drivers who received message with no informational content. Huber-White robust estimates of the standard errors are reported in parentheses. Asterisks are based on standard p-values ($^{*}p < 0.1,^{**}p < 0.05,^{***}p < 0.01$).

Table A5. Treatment effects: Number of plans made and rides given

	(1) # Plans (During Subsidy)	(2) # Plans (Week After)	(3) # Plans (Month After)	(4) # Rides (During Subsidy)	(5) # Rides (Week After)	(6) # Rides (Month After)
Density Low	-0.060	0.002	0.017	-0.235	0.112	0.059
	(0.240)	(0.235)	(0.215)	(0.301)	(0.292)	(0.241)
Density High	-0.080	-0.190	-0.161	-0.184	0.065	0.006
	(0.254)	(0.237)	(0.218)	(0.299)	(0.332)	(0.240)
Excess Demand	-0.593**	-0.255	-0.303	-1.147***	-0.240	-0.164
	(0.247)	(0.227)	(0.193)	(0.369)	(0.308)	(0.232)
Small Bonus	-0.115	-0.151	-0.230	-0.087	-0.021	-0.210
	(0.262)	(0.233)	(0.216)	(0.329)	(0.310)	(0.259)
Large Bonus	-0.032	-0.221	-0.164	0.076	0.001	-0.116
	(0.235)	(0.245)	(0.206)	(0.279)	(0.319)	(0.235)
Companionship	-0.295	-0.362*	-0.195	-0.939***	-0.337	-0.219
	(0.239)	(0.216)	(0.211)	(0.295)	(0.309)	(0.224)
Observations	10401	10401	10401	10401	10401	10401
Control Mean	0.373	0.376	2.641	0.096	0.070	0.431

Note. Each column in this table comes from a separate Poisson regression of respective outcome on the treatment and strata fixed effects. The control group is drivers who received message with no informational content. Huber-White robust estimates of the standard errors are reported in parentheses. Asterisks are based on standard p-values ($^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$).

Table A6. Driver's relative detour

	(1) During Subsidy	(2) Week After	(3) Month After
Density Low	-0.001	-0.000	0.002
	(0.002)	(0.002)	(0.004)
Density High	0.000	-0.002	-0.000
	(0.002)	(0.002)	(0.004)
Excess Demand	-0.005***	-0.002	-0.001
	(0.002)	(0.002)	(0.003)
Small Bonus	-0.002	0.001	-0.003
	(0.002)	(0.002)	(0.003)
Large Bonus	0.003	-0.001	0.001
	(0.002)	(0.002)	(0.004)
Companionship	-0.001	-0.002	-0.001
	(0.002)	(0.002)	(0.003)
Observations	10401	10401	10401
Control Mean	0.007	0.007	0.019

NOTE. Each column in this table comes from a separate OLS regression of driver's relative detour distance on the treatment and strata fixed effects. Relative detour is the distance from the passenger's pickup location to the driver's start location, divided by the driver's total trip distance. Huber-White robust estimates of the standard errors are reported in parentheses. Asterisks are based on standard p-values ($^{*}p < 0.1,^{**} p < 0.05,^{***} p < 0.01$).

Table A7. Construction of quality metric

	(1) Logit	(2) OLS
Relative Detour	-6.5048***	-0.1056***
	(0.2499)	(0.0037)
Passenger Female	0.5408***	0.0108***
	(0.0681)	(0.0013)
Passenger Female × Driver Female	-0.1620	-0.0023
	(0.2108)	(0.0042)
Plan-Booking Time Difference	-0.0472***	-0.0008***
	(0.0023)	(0.0000)
Number of Seats Requested	-0.3678***	-0.0058***
	(0.0621)	(0.0011)
Passenger Has Photo	-0.0629	-0.0010
	(0.0567)	(0.0012)
Observations	14291	78677
R^2		0.020
Pseudo R^2	0.213	
Overall Match Rate	0.1342	0.0246

NOTE. Panel 1 shows the coefficient of a logit regression on plan-booking compatibility characteristics. Panel 2 shows the coefficient of an OLS regression on the same characteristics. These coefficients are used in the construction of our quality-weighted ride metric described in Appendix 1.

Table A8. Event study coefficients

Panel A: Activity Levels

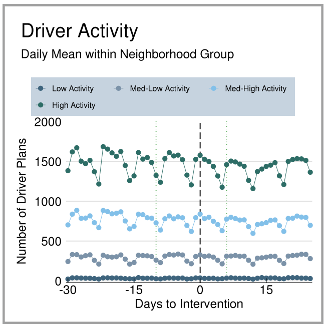
	(1) # Plans	(2) # Bookings	(3) # Completed	(4) # Drivers	(5) # Passengers
During Subsidy Period	551.27	4092.90	556.700	403.43	3049.96
	(4023.52)	(5053.95)	(1883.00)	(1897.41)	(3330.76)
Observations	16	16	16	16	16
Control Mean	57389.63	50907.94	18090.56	30945.69	43590.44

Panel B: Match Rates

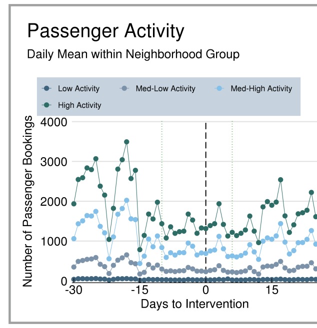
	(1) Pax Match Rate	(2) Dax Match Rate	(3) Pax Assign Rate	(4) Dax Assign Rate
During Subsidy Period	-0.006	0.019	0.000	0.027
	(0.031)	(0.018)	(0.038)	(0.024)
Observations	16	16	16	16
Control Mean	0.333	0.310	0.466	0.439

NOTE. Each column in this table comes from a separate OLS regression of respective outcome on a dummy for whether the day is after the subsidy period. Panel A reports outcomes that have to do with activity levels in the entire market, while Panel B reports match rates in the entire market. Huber-White robust estimates of the standard errors are reported in parentheses. Asterisks are based on standard p-values ($^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$).

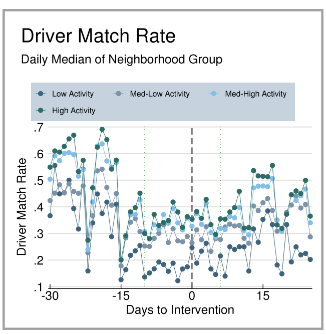
Fig. A4. Event study: Activity before vs after intervention (by quartiles)



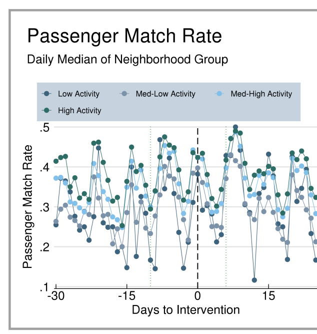
(a)



(b)



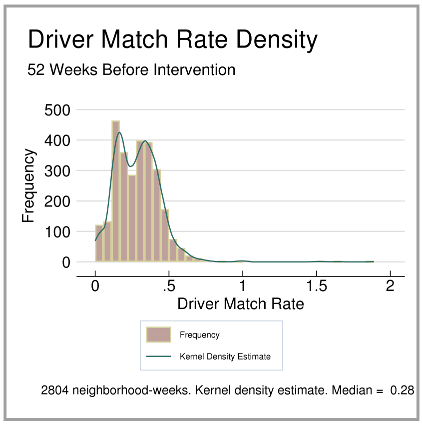
(c)



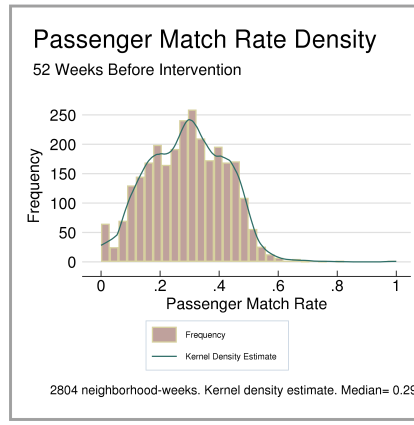
(d)

Note. These figures show market statistics by neighborhood quartiles: driver activity (Panel A), passenger activity (Panel B), driver match rates (Panel C), and passenger match rates (Panel D). Quartiles are defined using the number of completed rides in each neighborhood in the 60 days before our intervention. The black dashed line indicates the date of our intervention, while the light green dotted lines indicate the start and end days of the period used in our event study specification.

Fig. A6. Histogram of match rates (area-week observations, 1 year before intervention)



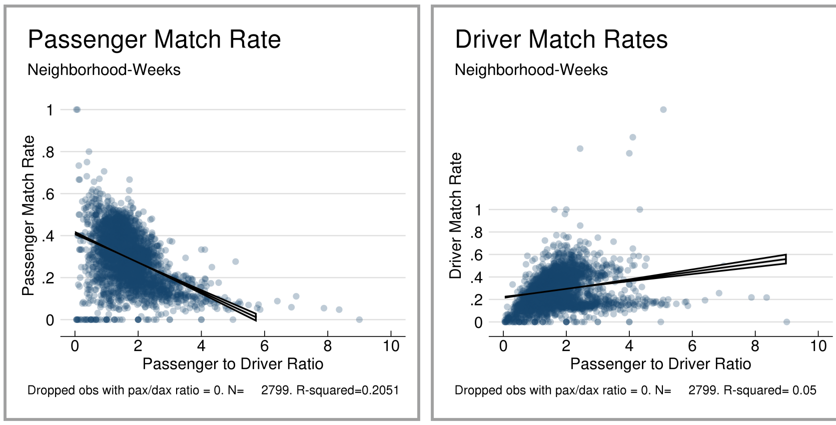
(a)



(b)

NOTE. These figures show a histogram of driver match rates (Panel A) and passenger match rates (Panel B) in all neighborhood-weeks in Singapore in the 52 weeks before our intervention. The green lines show kernel density estimates of the distribution.

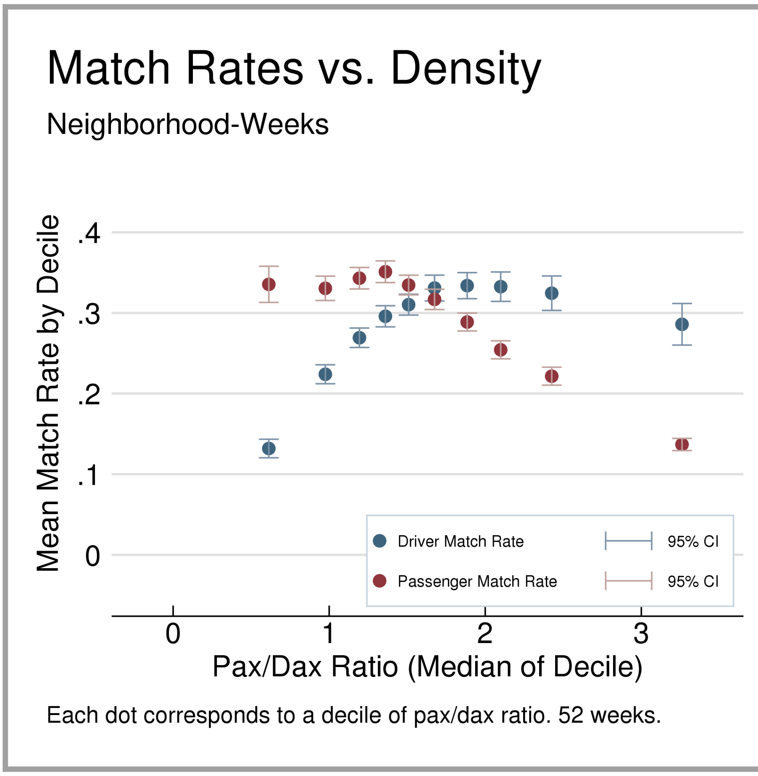
Fig. A8. Passenger and driver match rates (week-neighborhood, 52 weeks before intervention)



(a) (b)

Note. These figures show a scatter plot of passenger-to-driver ratios versus driver match rates (Panel A) and versus passenger match rates (Panel B) in all neighborhood-weeks in Singapore in the 52 weeks before our intervention. The black lines are best-fit linear regressions.

Fig. A10. Match rates by decile of passenger/driver ratio



Note. This figure breaks all neighborhood-week observations into deciles of passenger-to-driver ratio. The median passenger-to-driver ratio value is plotted against the mean match rate within that decile. Bars around markers indicate 95% confidence intervals.