Spillovers under Information and Search Frictions: Experimental Evidence from an Online Platform in Pakistan*

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Abstract

Information communications technology is shown to reduce search and information frictions in developing markets. Yet the mechanisms through which such interventions trigger strategic responses, spillovers, and adjustments to the market in developing economies remain under-explored. We causally identify spillover effects and their mechanisms via a randomized control trial on a major online listing platform for used vehicles in Pakistan, where there is limited publicly available price information. We provide estimates of transaction prices privately to sellers who create new posts and capture their pricing, advertising, and transaction outcomes. We also identify direct and spillover effects via a saturation design at the vehicle-model level. We find that the information intervention brings sellers' listing prices closer to our price estimates, but increases transaction probability only for the spillover group. The findings point to two mechanisms: 1) effects of price information are mediated by advertising tools that could countervail effects of list-pricing choices, and 2) spillovers could propagate direct effects of information intervention via adjustments by competing sellers.
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1 Introduction

Information and search frictions are often cited as causes of high price levels and dispersion and a significant share of trade costs in developing markets (Allen 2014; Startz 2016). Information communications technology (ICT), such as mobile phones and apps, has been shown to reduce by making price information more accessible (e.g., Aker 2010; Jensen 2007). Reducing search and information frictions via ICT could also lead to productivity gains and other knock-on effects through supply chains, suggesting the benefit of information interventions beyond the direct effects of reducing information friction (e.g., Jensen and Miller 2018, Hasanain et al. 2019). Large online marketplaces and platforms, which have increased their prominence in developing economies, may further reduce search costs by making it easier to acquire information about competing products (Fu et al. 2021). Yet, evidence of persistent price dispersion and search frictions in developed and developing economies' online markets suggests that platforms would not entirely eliminate information and search friction (Einav et al. 2015; Horton 2019; Fradkin 2015; Bai et al. 2020). Such persistent frictions point to the importance of understanding what types of search and information frictions agents in developing economies face and how they internalize such frictions with available tools.

The mechanisms through which ICT- or platform-based information interventions trigger spillovers, strategic responses, and adjustments in developing economies remain underexplored. First, price information interventions may generate spillover effects through improved market-wide access to information. Yet, we know little about the mechanisms through which such spillovers happen, due in part to the difficulty in scaling interventions and monitoring the mechanisms market-wide. Second, information and search frictions may also affect agents' choices on a wider range of decisions beyond pricing, causing further frictions. There is evidence that search friction and congestion in emerging online markets negatively affect the growth of high-quality traders (Bai et al. 2020). Yet, further empirical evidence is needed on how traders internalize and overcome such market frictions, such as pricing and advertising, particularly in a context where individuals' actions may cause spillovers on other market participants and have implications on market efficiency.

To provide insights into the links between information friction, individual choices, and spillovers in a developing market, we conducted a randomized controlled trial (RCT) in the used car market in Pakistan. In the intervention, we provide transaction-price estimates—called the Price Calculator—privately to sellers on a leading and nationally recognized online classified listing platform for used vehicles in Pakistan, PakWheels.com. With the experimental variation, we address the following research questions; first, we identify agents' pricing and advertising choices under search and information frictions and their internal mechanisms (i.e., changes in beliefs) behind those choices. Second, we identify if and how the information intervention induces spillover effects in the presence of information and search frictions. Specifically, we are able to capture a range of outcomes, such as prices, advertising, and transaction, as well as the direction of the impact relative to the direct treatment effects.

We overcome several traditional challenges in estimating the market-wide impact of an intervention. First, we relax the logistical constraint in conducting large-scale interventions in markets in developing economies through an extensive partnership with a popular and dominant online listing platform, an increasingly popular form of transaction in developing economies. Second, the collaboration with the platform allows us to conduct a natural field experiment, where the intervention is nested within the user interface of a popular platform, and the study sample consists of the vast majority of new listings (List 2007). Third, we measure changes in detailed, individual-level outcomes such as sellers' strategic choices and buyer-side responses using unique data captured by the platform. Forth, we generate variation in treatment saturation at the market sub-section level via a two-stage randomization saturation design. This partial identification strategy allows us to estimate the direct treatment effect, spillover, and saturation effects. Fifth, by privately providing the Price Calculator estimate, we rule out direct information spillovers and instead show knock-on effects of *choices* that treated sellers make.

We pre-specify and measure direct treatment and spillover effects on a) changes to the listing price, b) occurrence of the transaction, c) transaction price, d) usage of advertising tools, and e) index of buyer attention. We find that the intervention brings listing prices closer to our price suggestions for directly treated sellers. We find, however, that the intervention improves transaction outcomes for the spillover group by 1 percentage point, from the base of about 33%, but not for the directly treated group. We also find that the intervention increases the potential buyers' attention to spillover posts and reduces advertisement usage by the directly treated sellers. The findings point to two mechanisms: 1) effects of price information are mediated by advertising tools that could countervail effects of list-pricing choices, and 2) spillovers could propagate direct effects of information intervention via adjustments by competing sellers.

To further clarify the potential mechanisms that drive our main pre-specified results, we exploit our pre-specified model of static search with information friction. We find some alignment between the theoretical predictions and the empirical findings. Where there is misalignment, we speculate that the intervention induces shifts in sellers' beliefs that we need to account for in the theoretical framework. As such, we investigate shifts in sellers' beliefs as a potential mechanism using non-pre-specified outcomes from the endline survey of 2,311 sellers in our experimental sample.

From the survey, we find evidence that suggests that beliefs and their adjustments drive the set of results we find in the pre-specified outcomes. First, we find that price information intervention adjusts beliefs about the demand for the treated sellers. We also find that the intervention affects their beliefs about search frictions and market conditions, suggesting that sellers believe list pricing and advertising are substitutes. Importantly, these effects on beliefs are not detected for the spillover sellers, suggesting that they are responding to publicly visible choices of competitors but not adjusting their beliefs.

Our findings offer insights into the direct and spillover effects of information interventions on online markets' information environment in a developing economy, as well as mechanisms behind pricing and other choice parameters agents in the market have. We contribute to a strand of literature on search and information frictions in developing markets, motivated by a body of evidence that suggests the high transaction costs in trade (Allen 2014; Atkin and Donaldson 2015; Startz 2016; Aggarwal et al. 2022). We follow a body of work focused on ICT-based information intervention on price convergence and extend the knowledge into both detailed individual mechanisms and spillovers at the market level (Aker 2010; Aker and Mbiti 2010;Andrabi et al. 2017; Jensen 2007). Our work is also related to an emerging body of work on the effect of information interventions on spillovers up the supply chain and the roles of market structure in determining such effects (Jensen and Miller 2018, Hasanain et al. 2019; Mitra et al. 2018). Our work also generally addresses the externalities generated from information intervention but focuses on spillovers *within markets*, sellers' strategic choices, and the implications on market structure.

Our work is also motivated by a body of work documenting persistent price dispersion in online market platforms and sellers' and platform operators' incentives in those marketplaces (Dinerstein et al. 2018; Einav et al. 2015; Horton 2019; Fradkin 2015). The question is why search and information frictions persist in a world with plausibly low search and information costs. One view is that price dispersion and friction on online platforms are, in part, endogenous choices that platform operators make relative to other objectives, such as the extent of competitive pressure they want to induce. Dinerstein et al. (2018), for instance, shows using a redesign on eBay that balance between low information friction and competitive pressure is key to efficient online markets. This trade-off may be even more salient in developing economies with higher existing frictions and other market failures.

Lastly, our work contributes to an emerging body of work on the roles of online platforms in emerging economies, with a focus on reducing information and search frictions (Bai et al. 2020; Falcao Bergquist and McIntosh 2021 Couture et al. 2018; Fernando et al. 2020; Jeong 2020). On the extensive-margin access to platforms, Couture et al. (2018), find that while the benefits of access to e-commerce for rural markets in China are sizable, most of the gains accrue to the consumption side and to a minority of younger and richer users. The findings suggest that simply increasing access does not induce investments required to drive adaptation to e-commerce. On the intensive margin, Bai et al. (2020) suggest that search and information frictions still plays a major source of inefficiency on online platforms in developing countries, as they show that positive shocks to demand and information improve firms' performance in the long run, independent of productivity or quality. This suggests that market dynamics may generate inefficient firms and low-quality goods to persist in markets with information and search frictions based on the luck of having received positive initial demand shocks.

The remainder of this paper is organized as follows; Section 2 describes the context in which we conduct our intervention, and Section 3 the research design. Section 4 describes the pre-specified outcomes. Section 4.1 describes the identification strategy for our pre-specified analysis and our approach to multiple-hypothesis testing. Section 5 presents the pre-specified results, followed by an overview of the pre-specified theoretical framework to rationalize the results in Section 6. We then present results on non-pre-specified outcomes, primarily in the endline survey, in Section 7. Section 8 concludes.

2 Used car markets in Pakistan

Trading of used vehicles is a capital-intensive and frictional market in Pakistan, where vehicle ownership is low at around 6% (Pakistan Bureau of Statistics 2020). Anecdotally, trade has traditionally remained within existing social circles or through used car dealerships, with limited peer-to-peer transactions outside of their networks. There have been forums in which people exchange information on online bulletin boards and other social media. The most notable of such platforms, PakWheels.com, has evolved into a listing platform in which sellers and buyers can find each other and can find other information such as insurance and taxes. The platform receives approximately 100,000 new valid listings per month and has a similar level of active posts in a given time as its main competitor, olx.com.

Pricing high-value heterogeneous goods is challenging in a context without publicly available transaction price information. There is no publicly accessible and reliable information on transaction prices for used vehicles in Pakistan, where there is no equivalent to services like kbb.com. At the moment, the most comprehensive and publicly accessible price signals are the listing prices from online listing platforms like PakWheels. The lack of information may generate variation in market participants' beliefs about market prices, which, in turn, they emit into publicly accessible information in the form of listing prices.

There are three data points that corroborate this problem. First, our baseline data shows that only 33% of all listings are reported to have been sold, highlighting the underlying search and matching frictions. Second, we show in Figure 1 that even in the subset of listings that reported to have sold their vehicles, there are significant deviations and variations between the listing price and the transaction price. Third, the management at PakWheels has anecdotally indicated that their listing sellers are not pricing their vehicles "right," which led to the collaboration in which we provide price information to the sellers.

3 Experimental design

We conduct a field experiment in which we privately provide Price Calculator estimates to a randomly chosen subset of sellers. The Price Calculator estimates are based on a machine learning model using data on self-reported transaction prices from previous listings collected by PakWheels. The experiment is conducted within PakWheels' web and mobile platforms, where sellers create new posts. We assign treatment via a blocked, two-step randomization procedure with two saturation levels. The intervention is conducted over the course of 8 weeks to a flow of new posts. The sample selection and randomization procedure are described in the following sections 3.1 and 3.2. Figure 2 also shows the breakdown of posts into our sample and into treatment groups.

3.1 Sample selection

The platform receives up to 100,000 valid listings per month. Our experimental sample is new posts on the platform during the intervention period, except those for which PakWheels do not have sufficient data points to provide a Price Calculator estimate. The exact criteria for inclusion into the sample are masked for confidentiality reasons, but we include approximately 88% of all new posts into the study sample, consisting of approximately 70 distinct make-models. We arrive at the sample restrictions via the following steps.

First, we restrict our sample to the listings for which PakWheels would be able to provide Price Calculator estimates, i.e., vehicle types with large enough transaction volume with reported transaction prices. For instance, we do not include certain rare models for which PakWheels deemed not suitable to provide price estimates, such as luxury models or commercial vehicles, or large trucks. We cannot disclose further details on PakWheels' inclusion criteria into the Price Calculator estimation sample, but the resulting sample constitutes the vast majority of all listings.

Second, we impose restrictions based on when listings are created. For the primary analysis, we restrict the sample to listings created during the 8-week experimental period. For the secondary analysis of spillover effects, on the other hand, we also include listings created eight weeks prior to the start of the experimental period. This allows us to include model- (and model-version) fixed effects and run two-way fixed effect models, allowing for higher power of detecting treatment effects under an assumption on time trends. We discuss the benefit of these approaches in Section **??** and implications for power in Appendix Section E.1.

3.2 Two-step treatment assignment procedure

Our two-stage randomization process is as follows. In step 1, we block-randomize market clusters, defined as the make-model (e.g., Toyota Corolla), into two treatment (high vs. medium saturation) and control groups. In step 2, we randomize posts into treatment based on the last digit of the user ID on PakWheels. The assignment probability is 50 percent for the medium saturation group and 90 percent for the high group. In order to ensure that treatment and control groups are comparable in the primary outcome variables, we test for balance using listings data from a pre-treatment period with the same sample inclusion criteria and randomization procedure as the experiment. We bootstrap-sample and iterate this randomization procedure over 500 times and identify seeds for which we fail to reject differences in all primary outcome variables (described in Section 4), adjusted for false discover rate at 5%. We then randomly select one of those qualified seeds.

In step 1 of the two-step process, we run block-randomize make-model clusters into high-treatment, mid-treatment, and control groups. We use standardized cluster-level means of the five primary outcomes, as described in Section 4, and the cluster size.[1] Based on the

[1] Blocking is done with R's *blockTools* package (Moore 2012), which uses the optimal-greedy algorithm

blocks, we assign 50 percent of the clusters to control and 25 percent each to high- and low-treatment groups. Our choice of shares of clusters to treatment arms is informed by the literature on the optimal design of saturation design and our own Monte Carlo simulations using real data from the platform.[2]

In step 2, we assign treatment to posts based on the last digit of sellers' user-ID on PakWheels.[3] Treatment digits are chosen by a random number generator in *R*. The choice of digits for treatment is fixed across clusters and time in order to limit the extent of potential interference and for logistical simplicity. In other words, if a seller with user-ID i is in a treatment group for model m, then all other posts by i in m are treated, as well as any other model m' that is treated at the same saturation intensity as m. Treatment intensity of 50% or 90% stays constant for the cluster over the course of the experimental period.

3.2.1 Interference between clusters

One potential empirical challenge is interference between assignment clusters at the first stage of the randomization procedure. One may be concerned that if clusters are defined too narrowly, and pricing or advertising choices in one cluster could affect those in another, we would violate the Stable Unit Treatment Value Assumption (SUTVA). We allay this concern by using a relatively broad definition of clusters—the make-model—based on aggregated search logs data. We also address possible ways in which interference across clusters could still occur and their potential magnitude.

The aggregated search engine logs tell us which combinations of terms are used most frequently by viewers on PakWheels. For the randomization, our objective is to minimize concerns about inter-cluster interference but also retain as many randomization clusters for the step as possible. The aggregate search logs data are taken from the month of August 2020, containing approximately 68 million searches. The data contain numbers of searches per any combination of search terms (e.g., make, model, model-years in range, city, range of listing prices). We capture 35,000 most common search combinations, which account for 93% of all searches. We do not have information on the remaining 7% percent of less frequent combinations of specified search terms due to the capacity constraint of the partnering firm to address our data requests.

First, we observe that a majority (58%) of specified searches for posts on PakWheels included the make-model and the majority of those 58% also had additional terms (e.g., model year, city, price ranges). On the other hand, 32 percent of specified searches did not include make-models but instead included other fairly broad terms such as city name only

over the Mahalambois distance. We weigh the five main outcome variables twice as heavily as the cluster size variable. Our choice of weights is admittedly arbitrary, but the rationale is that the primary objective is to balance over main outcome variables and then with cluster size.

[2]We provide further detail on this process in Appendix Section E.

[3]The reason for this randomization procedure, as opposed to some others that does not rely on the user-ID, is partly for its simplicity in implementation, but also because we are assigning treatment to a *flow* of new listings (and some new users), meaning that we cannot pre-assign treatment to posts.

(e.g., “Lahore”), vehicle make only (e.g., “Honda”), or all posts with pictures. We infer that these broad searches are mostly speculative and unlikely to lead to meaningful price comparisons between posts. We would have been worried about interference if, for instance, a significant portion of users searched for vehicles of specific characteristics (e.g., “mid-size sedans”) that contain multiple make-models (e.g., Honda Civic and Toyota Corolla). Overall, the breakdown of specified searches indicates that the make-model is likely a reasonable and perhaps conservative level of clustering and that we are unlikely to have meaningful interference between clusters.

Second, interference across make-model clusters is likely minor because we provide private information that is specific to treated posts’ characteristics, making it unlikely that there would be large direct information spillovers from one make-model to another. We confirm our intuition from our pilot telephone endline survey, in which almost none of the sellers reported having looked at listing prices of other models besides one of their own vehicles.

Yet, the following are some of the ways in which interference *across* make-models could occur, violating SUTVA across treatment clusters:

- Large enough shifts in the distribution of listing prices could eventually induce information spillovers. Such large shifts in list-price distribution could also lead to changes in transaction probability, transaction price, and congestion, which in turn may affect price distributions and market outcomes of similar models.
- Changes to the listing prices or advertising in treated clusters may shift buyers’ attention to/from untreated make-models. Changes in buyer attention in untreated make-models may affect sellers’ pricing and advertising choices.

3.3 Treatment assignment and take-up

The intervention is designed to minimize non-compliance; those randomly assigned treatments are automatically shown the Price Calculator estimate on the interface while they create a post. One exception is if the seller uses an older version of PakWheels’ mobile app that does not yet contain the intervention tools. This may generate selection into treatment based on a) users’ preference for PakWheels’ mobile app as opposed to the web platform, which does not suffer from this issue, and b) their propensity to update the app. In order to mitigate this issue, PakWheels launched a new version of the app with the disabled intervention tool weeks in advance of the experimental period. The timing gave users plenty of time to update the new app before the intervention tool was enabled. Yet, it is possible, but unfortunately unverifiable, that a small fraction of users assigned to treatment did not receive it. As such, we identify both intend-to-treat and treatment-on-treated effects, as highlighted in Sections 4.2 and 4.3.

3.4 Intervention instrument: The Price Calculator

We provide estimates of the transaction price for used vehicles on PakWheels while sellers are creating their posts. The price information, which PakWheels calls "the Price Calculator", is based on a machine learning model trained to predict self-reported transaction prices using the firm's database of historical listings. The model estimate is conditional on the self-reported occurrence of the transaction, and we use observable attributes of the vehicle, but not of sellers' characteristics, as explanatory variables. Our hypothesis is that this information would help sellers identify realistic transaction prices and set listing prices accordingly.

To identify an error-minimizing forecast model, we take a gradient-boosting approach primarily for two reasons. First, gradient boosting—a method of ensemble predictions based on tree-based models—allows us to construct a predictive model that does not require estimating each of the make-model-modelyear fixed effects. We are, therefore, able to predict transaction prices for vehicles that had a relatively small number of observations within their own make-model-modelyear, but for which we had sufficient information to provide predictions. Second is that the gradient boosting approach performed best in most measures of error against other approaches in our initial design process, in line with the success of gradient boosting models in recent prediction competitions.

3.4.1 Display of the Price Calculator estimate

On PakWheels' web platform and mobile apps, sellers can create a new post by clicking on "Post an Ad." Sellers are first asked to log in so that PakWheels' platform can identify the user ID associated with each post. Users would not know their own user ID, as it is internal to PakWheels, or for which last digits we provide the Price Calculator estimates. Once logged in, sellers are asked to provide information about the vehicle they intend to sell, as shown in Figure 3. They then set the listing price in a box shown in Figure 4. If the seller is assigned to treatment, they are then shown a Price Calculator estimate, i.e., the machine-learning-based transaction price forecast, as well as the 10th and 90th percentiles of reported transaction prices for the make-model-model year (or make-model-modelyear-version for frequently traded models). These percentile measures would be labeled as "Lower end" and "Upper end" of transaction prices. Figure 5 shows how the Price Calculator estimate is displayed along with a brief description. Treated sellers are then given a chance to update their listing price, but not the untreated sellers. All sellers are then guided through the rest of the posting process.

3.5 User-experience after selecting the listing price

After providing information on the vehicle and selecting a listing price, sellers put their posts "live" on the platform and can be contacted by potential buyers. Sellers can adjust their listing price at any time as they gather more information about market conditions and be contacted by interested buyers. While list pricing is one of the primary choices that sellers make during initial posting and over the duration of the post's life, advertising is another way in which sellers can try to affect the outcome on PakWheels.com. The following are the three principal advertising strategies on which we create an indexed outcome variable for our analysis.

First, sellers can purchase "bump" credits and use them on their posts. The credits allow sellers to bring their post to the top of the result page in the default, reverse-chronological listing order. This effectively increases the post's visibility as more people look at the first pages of listings. Second, a "feature" credit would put their post in a few reserved spots at the top of the result page and label the listing as a "featured ad," much like promoted ads on Google searches. Posts are otherwise listed in the reversed chronological order within the class of featured ads. Third, sellers can provide signals of vehicle quality by requesting in-person inspections by PakWheels' mechanics. Based on a pre-specified rubric, the inspection would result in scores (out of 100) on eight dimensions: engine, brakes, suspension, interior, AC, electrical, exterior, and tires. The vehicles will pass the inspection if the unweighted average of scores over these eight dimensions is above a threshold. They can then be marked as "PakWheels certified" on the platform for an additional fee.

Sellers and potential buyers connect via the contact information listed on the post, negotiate, and transact outside the platform. As PakWheels.com is only a listing platform, it does not directly observe if a transaction occurs and, if so, to whom and at what price. Instead, the platform contacts the users regularly to request that the sellers self-report the transaction outcomes when they make a sale or want to take down the post. The posts expire after 90 days from the initial posting when sellers are again asked to report the transaction outcome.

3.6 Data sources

We leverage access to PakWheels' database, which contains all historical and live posts, to estimate the market-wide impact of our natural field experiment. At the post level, the database contains information on the posted vehicles' attributes, such as the make, model, model year, version, engine capacity, transmission, fuel type (e.g., petrol or CNG), color, and if it was assembled domestically. These fields are required for the sellers to post their vehicle and thus are consistently available for most listings. The database also contains additional information about the vehicles' attributes, such as stereo, air conditioning, and other amenities, that sellers have the option of reporting. For both the construction of our Price Calculator model and for analysis, we use required fields as inputs.

The database also has information on list pricing and advertising choices, and page views over the course of time per post. For list pricing and advertising choices, the database tracks when prices are adjusted, and advertising tools are activated with timestamps. The page-views variable, on the other hand, is a live count and is updated every 12 hours in the database. Because of the time-varying nature of these variables, we measure them after all posts are expected to go offline, i.e., we collect data 90 days after the last day of the experiment. Similarly, seller-reported transaction outcomes, which we expect to be logged when the posts go offline, at the same time as we collect the time-varying data.

There are two primary potential challenges with relying entirely on PakWheel's database. First, transaction outcomes and prices are self-reported for a subset of sellers and may suffer from selection bias or cannot be verified. Second, PakWheels' database does not contain information about the sellers' beliefs about market conditions, expectations on transaction outcomes, and other measures of experience in the marketplace on a representative sample. As such, we also collect data from the telephone endline survey of 3,000 representative sellers on their transaction outcomes, unincentivized beliefs, and other measures of experience.

Other data items we use for design and secondary analysis include i) aggregated search engine results in terms of keywords and their combinations, ii) daily search listing orders from PakWheels, and iii) a usage log of a previous iteration of the Price Calculator, which preceded the experiment. Further details about each of the data sources can be found in Appendix Section A.

4 Prespecified outcomes

The primary objective of this paper is to address how a price information intervention induces direct and spillover effects on sellers' pricing and advertising choices, transaction outcomes, and mechanisms. As such, we pre-specify five primary outcome variables, for which we address the issues of multiple hypothesis testing. The five outcomes are a) changes to the listing price, b) occurrence of the transaction, c) transaction price, d) usage of advertising tools, and e) index of buyer attention, which are defined as follows.

4.0.1 log-absolute difference in prices

We consider changes to listing prices as the "first-stage" effect of our intervention, in that impact on other primary outcomes hinges on the changes to listing prices and their distributions. We expect that sellers would adjust their listing price toward the Price Calculator estimate, plus some margin for expected bargaining. In order to capture this type of convergence, we define our primary price outcome to be the natural-log transformation of the absolute difference between the final listing price and the Price Calculator estimate that the seller received or would have received. PakWheels calculates and provides the Price Calculator estimate only to treated posts, so we estimate the prices that control posts *would have received* using the identical model as the one PakWheels uses for this experiment.

As discussed in Section 3.5, sellers can update prices and other features as long as their posts are active on the platform. Direct effects of the Price Calculator estimate may happen when the post is created, while indirect effects may occur even after the post is created through feedback from buyers and competition with other posts. We use the listing price at the end of posts' active status for our primary outcome so that all changes to the listing prices are factored in.

4.0.2 Transaction outcome and price

Sellers on PakWheels can take down their posts once they no longer wish to receive inquiries or the post expires after 90 days since the initial posting. When the post is taken down, sellers are asked if they have sold their vehicles. They are required to respond in order to have their ads taken off. They are given options on the form (e.g., sold via PakWheels' website, sold via others, chose not to sell, etc.), and most sellers choose one of them. However, some respond as "Other" yet report in the comment section that they have sold the vehicle. Our transaction outcome variable accounts for this to the best extent possible by string cleaning responses classified as "Other." The transaction variable is 1 if the seller reported a sale and 0 otherwise.

Sellers are also prompted to report the transaction price on the online form if they report having sold their vehicle. The value is missing for those who do not report their transaction outcome. We also remove inputs outside of the reasonable price range for their given make-model. We use the natural log of the transaction price as the outcome variable.

These self-reported outcome data are likely the best source of information on transactions and prices across a wide range of vehicle characteristics and locations in Pakistan. However, they may be vulnerable to biases and are checked against values collected via a telephone survey described in Section A.4. We plan on using responses from this survey to construct analogous outcome variables for robustness checks.

4.0.3 Advertisement index

One of our main hypotheses is that, when faced with novel price information, sellers adjust their strategic choices along two margins; list pricing and advertising. We capture sellers' choices on advertising with data on paid services on PakWheels. As discussed in Section 3.5, sellers can increase the visibility of their posts and/or signal quality by "bumping", "featuring," and requesting an inspection for their vehicle. In order to capture both intensive and extensive usage of advertising tools, we construct an index measure consisting of the following variables:

- number of "bumps" the seller applies to the post
- number of weeks the seller "features" the post
- 1 if the seller requests PakWheels to have the vehicle inspected.[4]

[4] Given that certification is endogenous to vehicle quality, we use the data on whether or not the vehicle was ever inspected, as opposed to certified.

4.0.4 Buyer-attention index

We also hypothesize that the price information intervention, and causal effects on pricing and advertising, affect treated posts' visibility on the platform. In order to capture this effect on the post's visibility and buyer attention, we construct an indexed measure from data discussed in Section A.3. The index consists of the following variables:

- page views (i.e., clicks on the post)
- clicks on the "Show Phone Number" button within the post to contact the seller.

4.1 Empirical strategy

4.2 Intend-to-treat effects

We estimate the intent-to-treatment effect of being provided the Price Calculator estimate using Equation 1, where the coefficients of interest are β_1, β_2, and β_3:

$$\begin{aligned} Y_{i,p,m,w} = \beta_0 + \beta_1 * Assign_{i,m} + \beta_2 * Cluster_m + \beta_3 * ClusterHigh_m \\ + \bar{Y}_{m,w\in[-15,-8]} + \psi_w + X'_{i,p}\rho + \epsilon_{i,p} \end{aligned} \tag{1}$$

The subscripts used in the equation above indicate the following:

- i: individual user identifier (defined by PakWheel's user ID)
- p: post (multiple posts could belong to a given i)
- m: vehicle make-model cluster
- w: posting week. $w = 1$ is the first week of the experimental phase.

This estimating equation is fitted to data of listings that were created during the 8-week experimental period and for which Price Calculator estimates could be generated, as discussed in Section 3.1.

$\hat{\beta}_1$, $\hat{\beta}_2$, and $\hat{\beta}_3$ capture the ITT effects. $Assign$ is the binary direct treatment variable, $Cluster$ is a dummy variable that equals 1 if the model is selected for first-stage assignment (of either saturation level) and zero otherwise. $ClusterHigh$ is a dummy variable for high-saturation cluster-level treatment. Since we cannot have model fixed effects, we include the pre-experimental, model-level means of the outcome variable from weeks -15 to -8. We select this time period as it would be sufficiently far from the experimental time frame, and the vast majority of posts created in weeks -15 to -8 would already be taken down week 1. ψ_w denotes the week fixed effects, and $X'_{i,p}$ is a vector of controls for vehicle and seller characteristics, as follows:

- Vehicle characteristics:
  - vehicle's age (by model year)
  - log(mileage)
  - engine capacity
  - transmission
  - fuel type (e.g., petrol, CNG)
  - color
  - assembly (domestic or imported)
- Seller's characteristics:
  - Seller's city
  - 1 if professional dealer, as observed through PakWheels' account information
  - log(number of listings ever made on PakWheels)
  - log(months since first listing on PakWheels)

For all dependent variables other than the binary transaction outcome, we use linear regressions. For the binary outcome variable, we use the logit model. We cluster the error at the make-model level, as the first stage of the randomization is conducted at this level. We also estimate these models using heteroskedasticity-robust standard errors as a robustness check.

4.3 Treatment-on-the-treated

As discussed in Section 3.3, we may encounter some treatment non-compliance by sellers with old versions of the PakWheels app that does not include the intervention tools. This type of non-compliance is rare but likely non-random, so we instrument for the treatment take-up using the assignment variable.

The treatment-on-the-treated (TOT) effect is estimated via 2SLS, with $Assign$ instrumenting for $Treat$, and $Cluster$ and $ClusterHigh$ included as controls.

$$\begin{aligned} Y_{i,p,m,w} = \theta_0 + \theta_1 * \widehat{Treat}_{i,p} + \theta_2 * Cluster_m + \theta_3 * ClusterHigh_m \\ + \bar{Y}_{m,w\in[-15,-8]} + \psi_w + X^{'}_{i,p}\rho + \epsilon_{i,p} \end{aligned} \tag{2}$$

The first-stage specification for $\widehat{Treat}$ is as follows:

$$Treat_{i,p} = \phi_0 + \phi_1 * Assign_{i,m} + \bar{Y}_{m,w\in[-15,-8]} + \psi_w + X^{'}_{i,p}\tau + \xi_{i,p} \tag{3}$$

$\hat{\theta}_1$ represents the estimated TOT effects. The specifications include controls ψ_w, γ_m , and $X'_{i,p}$ in the first and second stages, as we did for the ITT effect. $\xi_{i,p}$ is error term in the first stage.

4.4 p-value adjustments

In order to address the issue of multiple hypothesis testing, we follow Romano and Wolf 2005 and correct for the false discovery related to tests on the five primary outcomes: log(absolute difference between listing price and Price Calculator estimate), binary transaction outcome, log(transaction price), indexed measure of advertisement usage, and buyer-attention index. Given that we consider direct treatment and spillovers as separate hypotheses, we adjust their critical values separately. We, therefore, report adjusted critical values at five percent, based on the Romano-Wolf procedure. There are three groups of five null hypotheses related to the primary outcomes; that the coefficients on three main exogenous variables ($Assign$, $Cluster$, and $ClusterHigh$ for ITT) in the regressions of five pre-specified primary outcomes are not statistically different from zero. We report both the unadjusted p-values and adjusted q-values for these primary hypotheses and only unadjusted p-values for tests on secondary outcomes.

5 Results from pre-specified analysis

5.1 Balancing checks

We begin by examining the balance of outcomes in the pre-treatment period. Our test differs from a standard approach in which one measures outcome variables and covariates at baseline and tests for statistically significant differences between treatment and control groups. In our context, we do not observe baseline measures of the experimental sample because we treat a subset of the flow of new listings, and outcomes are observed only after treatment, i.e., the presentation of the Price Calculator estimates.

As such, we conduct a "placebo" test of balance by using listings data between 8th November 2021 and 9th January 2022, the 8-week pre-treatment period from which the data for randomization comes. The null hypothesis is that the coefficients on the placebo treatment variables assigned to the pre-treatment listings would not be statistically different from zero, based on the intent-to-treat estimators discussed in Section 4.2. We conduct the tests on five pre-specified primary outcome variables and adjust for multiple hypothesis testing as described in Section 4.4.

Table 1 shows the means and standard deviations by placebo treatment groups of the pre-treatment period listings, and Table 2 presents the tests of balance via placebo regressions. We present coefficients "Assignment," "Spillover," and "Spillover (high)" corresponding to the direct treatment effect ("Assign"), as high- ("Spillover (high)") and low- ("Spillover") saturation spillover effects based on Equation 1, respectively. We report unadjusted statistical significance with stars next to the coefficients and adjusted q-values at the bottom three rows of the table.

The balance test shows that we fail to reject the null on almost all outcome variables at conventional levels after controlling for the false-discovery rate. First, unadjusted p-values are below 0.05 for one of the fifteen tests and between 0.10 and 0.05 for two tests. When we adjust for false discovery rates via the Romano-Wolf procedure, however, we find that only one q-value ("Spillover" for Column 4, "Page-view index") is between 0.10 and 0.05, and none below 0.05. Although not a direct test for balance on the experimental sample, this placebo test during the period preceding the intervention is the closest conceptual approximation to the standard baseline test for balance.

5.2 Treatment effects on primary outcomes

Through the analysis of pre-specified primary outcomes, we find that our price information intervention caused changes to both sellers' choices and their transaction outcomes. Table 3 shows the ITT estimates, as specified in Equation 1 on pre-specified outcomes. Table 4 shows the TOT effects that are qualitatively indistinguishable from the ITT counterparts, so we focus our interpretation based on the ITT estimates. Table 5 also shows ITT results by treatment groups (i.e., assigned and spillover groups for high and low saturation models), following an alternative specification to the pre-registered empirical model. First, we find evidence that the intervention brings the listing price closer to the Price Calculator estimates as a direct treatment effect and suggestively as spillovers. We find, however, that it increases the transaction probability for the spillover group but not for the directly treated group. We find effects on two potential mechanisms: the use of advertising tools and the resulting shift in buyer attention. We find that the direct treatment reduces advertisement usage for the directly treated group. This effect leads to fewer page views for the directly treated and more for the spillover group.

Column 1 in Table 3 indicates that the intervention reduces the absolute difference between the listing price and the Price Calculator estimate by 3.3% as a result of direct assignment and a further 7.8% as a result of spillovers. The direct assignment effect survives the critical value adjustment, but the spillover effect does not. There is also no statistically significant *additional* effect of a higher saturation, though the coefficient estimates of the higher saturation are less precisely estimated across the rest of the outcomes. The set of results from Column 1 shows the direct effect of price signals on reducing price variations away from the Price Calculator estimate and some suggestive evidence of a spillover. Appendix Table F.1 also shows results on non-primary price outcomes. We do not find evidence of adjustments in listing price **levels**, but evidence of increased listing price adjustments for the high saturation group.

Column 2 in Table 3 shows that the direct treatment effect on the reported transaction is negative and statistically significant, yet positive and statistically significant in the equal magnitude for the spillover. Because these effects are additive, the direct effect and the spillover coefficients cancel out for the direct treatment group. On the other hand, the net effect of treatment is positive for the spillover group. The magnitude of the direct and spillover effects is 1 percentage point, as shown with a linear probability model in Appendix Table F.2. Column 3 Table 3 also shows that the intervention does not significantly affect the log transaction price either directly or through spillovers, although there is an endogenous selection of the sample by those who report their transactions and prices. Overall, we find a set of counter-intuitive results that the Price Calculator intervention improves the transaction probability of spillover groups but not the directly treated ones, the mechanisms behind which we explore in the remainder of our analysis.

Column 4 in Table 3 shows that the direct treatment effect on the buyer attention index is negative, but the spillover effect is positive. The effects are relatively small, at -0.02 SD for the direct effect and 0.03 for the spillover effect. Appendix Table F.3 breaks down the effects on the index by its components of the effect. The table shows that direct assignment reduces the number of page views by 60, and the spillover effect increases it by 57. Similarly, the direct assignment reduces phone number views by 0.41, and the spillover effect increases it by 1.15. Table 5 also indicates that, when assessed by treatment groups, the effects on increased page views are found for spillover groups. We, therefore, find robust evidence indicating that buyer attention increases as a result of spillovers, but the effects are somehow muted for direct treatment groups. We hypothesize that the effects on the page-view index are a result of differential choices by directly treated and spillover groups.

Lastly, Column 5 in Table 3 shows that the direct treatment effect on advertising usage is negative, at 0.01 SD, but the spillover effect is statistically and economically insignificant. Appendix Table F.4 shows the results on each component of the advertising index, as well as an unwinsorized index outcome for reference. We find that direct assignment reduces advertising usage in all components: bumps, features, and certifications. We also find positive and significant effects for high-saturation spillover effects on bumps, certifications, and the un-winsorized index but do not find it in the pre-specified winsorized outcome.

5.3 What mechanisms explain the set of pre-specified results?

We find in our analysis of pre-specified outcomes that the intervention reduced the list price's deviation from the Price Calculator estimate and induced positive transaction outcomes through increased page views for the spillover group but not for the directly treated. We also find reduced use of advertising by the direct treatment group. Yet, questions remain as to how to make sense of the set of results in combination. What are the overarching mechanisms that relate pricing, advertising, buyer attention, and transaction outcomes in the context of search and information frictions? And how do we account for spillovers into the mechanisms, particularly when the signs of the effects differ from those on the direct treatment counterpart?

To address these challenges, we use a pre-specified conceptual framework of static search and a set of comparative statics, with which we derive a set of predictions. The conceptual framework is specified in the pre-analysis plan and summarized in Section 6. We then evaluate how closely the pre-specified empirical results align with theoretical predictions. We then reconcile any deviations of empirical results from the model predictions or confirm alignment by providing further evidence on survey-based measures. In this analysis in Section 7, we focus on sellers' beliefs, which we highlight in the model as underlying mechanisms.

6 Conceptual framework

We present a simple search framework that addresses various mechanisms of search and information frictions incurred by agents in a developing market. The objective of this exercise is to identify mechanisms through which lack of access to information could generate losses in unrealized transactions or may induce externalities in terms of search and information frictions. We combine our theoretical predictions with empirical results to demonstrate how sellers facing such frictions set listing prices, promote their posts through advertising, and respond to information about market conditions.

There are several channels through which search and information frictions may affect prices and transactions in highly frictional markets. First, the sheer lack of access to, or high cost of accessing, price information could result in variations in otherwise optimal choices. Second, even with access to price information and signals, individuals' beliefs about market conditions and the signal quality may vary, leading to variations in their otherwise rational chocies. Third, sellers may generate spillovers through, for instance, spillovers of information itself, changes in their pricing and advertising decisions, and their choices affecting potential buyers responses that then back to affect sellers' choices.

We address a wide range of possible channels listed above with the conceptual framework while staying with a simple and tractable model to generate clear predictions. We use a static search model that derives inspiration from canonical frameworks such as Stigler (1961) and Diamond (1982). Most contemporary models that focus on the effect of access to price information assume full knowledge of parameters on market friction and demand distributions (Baye et al. 2007). We introduce the following deviations from a standard search framework:

- We allow for supply-side heterogeneity of access to information and resulting beliefs about the demand-side distribution. In effect, sellers have biased or noisy beliefs about the distribution of buyers' willingness-to-pay (WTP).
- This, along with possibly noisy beliefs about the match rate and efficacy of advertising, would lead to biased or noisy beliefs about the probability of sale and to suboptimal list pricing.
- We allow the match rate with potential buyers to be endogenous with respect to advertising choices sellers make. They can influence the match rate by engaging in costly

actions, i.e., advertising.

Our approach is similar to that of Bai et al. (2020), who model and empirically estimate the search and information frictions buyers experience and resulting firm and market dynamics. Unlike Bai et al. (2020), who focus on the demand side, we address the role of information friction on the supply-side and search friction that sellers experience. Our focus on mechanisms is founded on previous work such as Bergquist and McIntosh (2021) and Bai et al. (2020), who show that the existence of, or mere access to, online platforms does not resolve issues of search and information frictions and that frictions that persist on such platforms deserve attention.

We set up a model in which a seller i is endowed with an asset and certain unobservable characteristics s_i, as well as information set I_i. The search process is composed of the following steps:

1. Seller i forms beliefs about the distribution of buyers' WTP based on information I_i.
2. Seller i chooses a listing price p_i^l and amount of advertisements a to optimize expected returns from participating in the marketplace.
3. Seller i matches with a potential buyer via a Poisson process.
4. Once matched, seller i makes a take-it-or-leave-it (TIOLI) offer p_i^t below p_i^l to the potential buyer.
5. Transaction occurs if the matched buyer's WTP is higher than p_i^t.

We provide further detail on the set-up and derive the model in Appendix C. Section C.1 lays out the setup of our model and provides definitions of terms and parameters. Section C.2 defines the objective function and the maximization problem in terms of the listing price and advertising choices. Section C.3 gives optimality conditions in the case of no information friction. Section C.4 shows how individual choices may be altered when there is noise in beliefs about the demand and how price information signals would induce updates in beliefs and alter input decisions. Section C.5 concludes by providing predictions on the role of information friction and noisy beliefs on demand in terms of sellers' choice variables and transaction outcomes.

6.1 Theoretical predictions

We derive the following predictions from the theoretical framework:

1. The price information intervention brings the listing price p_i^l closer to what it would be under no noise in beliefs about demand.

2. The information intervention increases expected returns from the search process.

3. The information intervention increases the consumption of advertising a if sellers' beliefs about expected returns from search are adjusted upward.

4. Spillover effects could occur through lower noise in publicly available price signals, which could increase returns from the platform and from advertising.

5. Spillover effects could occur if the intervention affects transaction outcomes and, consequently, the Poisson match rate in a treated market segment.

6.2 Do the theoretical predictions align with empirical results?

The first prediction is consistent with the empirical results that show that the listing price move close to the Price Calculator estimates. One assumption that underlies this conclusion is that the optimal listing price p_i^{l*} under no noise in beliefs about demand and the optimal transaction price p_i^{t*}, which the Price Calculator estimates, is close enough that a seller moving their listing price closer to p_i^{l*} would also reduce the distance to p_i^{t*}.

The second prediction that the information intervention increases expected returns is not explicitly supported by the empirical results of the pre-specified outcomes. If the effect of direct treatment is increased expected returns, then one might expect positive effects on transaction probability, transaction price, or the revenue from the search process. One possibility is that the intervention affects other mechanisms, such as advertising and buyer attention, that may, in turn, negatively affect the transaction outcomes. The other possibility is that directly treated sellers increased their expectations of transaction price and held more optimistic view on the market conditions, but such effects are not detected by reported transaction outcomes. To explore these possibilities, we analyze survey-based outcomes in Section 7.

The third prediction is that the information intervention would increase advertising if the seller's belief about expected returns from search is adjusted upward. Empirically, we find that the intervention *reduces* advertising. This suggests that either sellers' beliefs about expected returns are negatively altered or their beliefs about other factors, such as the extent of information and search frictions they face, are affected. The latter is a possibility if directly treated sellers believe that they face lower information friction. We confirm the effects on beliefs about frictions and market conditions in Section 7.

The fourth prediction is that spillovers could occur through lower noise in publicly available price signals, which could increase returns from the platform and from advertising. Empirically, we find positive spillovers in terms of the pricing choices but not on the use of advertising. Fifth, the model predicts that the spillover effects could occur if the intervention affects transaction outcomes and, consequently, the Poisson match rate in a treated market segment. Our effects on increased page views for the spillover group seem to confirm this view. In Section 7, we also test if the beliefs of sellers in the spillover group about search and information are affected to narrow in on specific mechanisms behind the spillover effects.

Overall, we find that some of the theoretical predictions line up with the empirical findings, but not perfectly. We speculate that the changes in sellers' beliefs and how the intervention induces it may help explain the deviations between theoretical predictions and empirical findings. As such, we analyze the results of survey outcome measures on sellers' beliefs in the next section.

7 Results from non-pre-specified analysis

7.1 The endline survey measures on beliefs

We provide insights into mechanisms put forth by the conceptual framework and the empirical mechanisms through data from a telephone endline survey. We conducted the survey on a representative subsample of 3,000 sellers, balanced across make-model clusters and within-cluster treatment assignments. The survey is conducted on sellers 4 to 6 weeks after their posting in order for the timing to be early enough to minimize recall bias and keep a high response rate, but late enough that sellers have engaged with potential buyers and transaction outcomes mostly determined. We received responses from 2,311 of them (77% response rate) of the sampled respondents. The original intention of the survey was to confirm the self-reported outcomes, but additional questions are included to capture potential mechanisms of the treatment and spillover effects.

We test for both direct treatment and spillover effects on these belief measures, using the specifications listed in Section 4.1. We ask a series of questions pertaining to sellers' beliefs about the market conditions, perceptions of market frictions, and perceptions about the Price Calculator instrument. These questions are meant to capture changes in sellers' beliefs about the demand distribution, i.e., the possibility that tailored price information leads sellers to have less noisy beliefs about the eventual transaction price. The belief outcome measures that we test can be categorized as follows:

- sellers' expectations about transaction prices and their willingness to negotiate
- sellers' beliefs about search and information friction
- sellers' demand for the Price Calculator tools and beliefs about the effectiveness of advertising tools.

7.2 Treatment effects on survey outcomes

First, we find that the intervention moves sellers' price expectations toward the Price Calculator estimates and makes them more willing to bargain. Table 6 shows that treatment assignment moves sellers' expected transaction prices closer to the Price Calculator estimates by 21.3%, as shown in column 1. The treatment also increases sellers' willingness to bargain by PKR 5,600. Importantly, statistically significant effects are only detected for direct treatment effects and for spillovers. The set of results confirms the idea that sellers' beliefs about prices are adjusted based on exogenously shifted price signals, i.e., the estimates they receive from the Price Calculator. The result of willingness to bargain may also indicate that sellers are more flexible on pricing to market conditions. The set of results conforms with Theoretical Prediction 1.

Second, we find that directly treated sellers' beliefs about search and information frictions are more optimistic. Table 7 shows that directly treated sellers' perceptions about the difficulty of getting inquiries and good prices improve by 0.05 to 0.07 on the Likert scale. Again, we do not see similar impact as spillover effects. The results on sellers' beliefs about search and information frictions may help explain the reduction in advertising by directly treated sellers, especially given adjustments in their listing price. If sellers adjust their listing prices according to new information, and they believe they reduce friction from search and information asymmetry, then they may opt to spend less on advertising. This may help explain discrepancies between the pre-specified empirical results and Theoretical Predictions 2. and 3.. We also note, however, that we do not find higher demand for the Price Calculator tools or for advertising as a result of direct treatment, as shown in Table 8.

Third, we note that almost all effects on beliefs are direct treatment effects, not spillovers. All tables mentioned above from survey outcomes do not show statistically significant effects on the spillover coefficients, with the exception of Column 3 in Table 7.[5] Table 9 also shows that sellers believe they are exposed to the Price Calculator as a result of direct treatment and not through spillovers. The set of results seems to indicate that the spillovers to sellers who are not directly treated occur through changes to the market conditions caused by choices of the directly treated.

The findings from the endline survey measures suggest that changes in sellers' beliefs about demand and search frictions may play a role in the effects of information interventions. However, changes in beliefs seem to only occur when the agents are directly confronted with new information; spillover sellers' beliefs are not altered but are merely responding to publicly visible choices of competitors. Combined with the effects we see on advertising in Table 3, the evidence seems to point out that pricing and advertising are substitutes, as directly treated sellers adjust their listing price but also lower their advertising usage.

8 Conclusion

In this study, we address challenges sellers and consumers face in developing markets in the form of information and search frictions. These challenges are mitigated yet persistent in a

[5]We note that we do not correct for multiple hypothesis testing in the non-pre-specified outcomes and that this is the one statistically significant result out of 26 coefficients in question on Tables 6 to 8.

world with access to information and communication technologies and online marketplaces (Aker 2010; Jensen 2007; Dinerstein et al. 2018; Bai et al. 2020). We conduct a natural field experiment with a theoretical framework to understand how sellers on online markets under persistent frictions internalize such frictions with available tools and how spillovers occur when an information environment is exogenously altered. We conduct a natural field experiment with a dominant online listing platform for used vehicles, PakWheels.com, in which we privately provide Price Calculator estimates to sellers when they create a new post. We track the sellers' list pricing choices, advertising usage, number of page views received, and transaction outcomes.

We find that the information intervention affects observed choices on list prices and advertising. The intervention brings the listing price closer to the Price Calculator estimates yet increases transaction probability only for the spillover group. We also find that the intervention increases the potential buyers' attention to spillover posts and reduces advertisement usage by the directly treated sellers. We hypothesize that these effects may be driven by changes in beliefs about demand distributions and search frictions. We apply a pre-specified conceptual framework to evaluate the set of pre-specified empirical results and assess the validity of the mechanisms we propose via survey outcome measures on sellers' beliefs.

We find evidence consistent with the idea that sellers' beliefs about demand and search frictions play a role in the effects of information interventions. First, the information intervention only affects the beliefs about the price levels, information and search frictions, and other market conditions of those who are directly treated. Second, we find evidence that is consistent with the view that sellers use advertising as a substitute for list-pricing. As such, adjustments via changes to the consumption of advertising tools could countervail the effects of information intervention. Third, the information intervention can generate significant spillover effects, not only through the direct spillover of information itself but also through changes in choices like pricing and advertising usage made by the directly treated. This set of results suggests that spillovers and general-equilibrium effects could propagate direct effects of information intervention through sellers' choices of list pricing and advertising.

Our findings show how information interventions via an online platform in developing economies affect price dispersion and market outcomes and offer novel mechanisms through which spillovers can occur. Our findings are in line with previous ICT-based information interventions, such as Aker (2010), Aker and Mbiti (2010), and Jensen (2007), in that we find significant reductions in price dispersion even in the context of heterogeneous goods. Our work also shows that information interventions can cause spillovers and knock-on effects not only through the supply chains as in Jensen and Miller (2018) and Hasanain et al. (2019), but also *horizontally* through choices that competing sellers make on pricing and advertising. Our work shows the importance of accounting for externalities generated from information intervention on sellers within markets for policymakers considering scaling information interventions.

Our work also highlights the potentials and limitations of improving market access and reducing frictions via online marketplaces in developing economies (Bai et al. 2020; Falcao Bergquist and McIntosh 2021 Couture et al. 2018; Fernando et al. 2020; Jeong 2020). Our work highlights the importance of complementary and substitute tools to pricing that sellers use to counter market frictions in developing economies, even in the context in which information and search frictions are reduced through technology. More work is needed to understand how information and marketing strategies interact with other channels on which entrepreneurs in developing economies rely, such as social networks and relational contracting. Lastly, our findings may also be relevant to the literature on small-to-medium enterprises in developing economies and business training interventions, which have had relatively low cost-effectiveness due to high cost (McKenzie and Woodruff 2014; Blattman and Ralston 2015). Our work shows the potential of improving information access to small-scale traders at scale and also points out the importance of advertising as a jointly determined tool.

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9 Tables

Table 1: **Balance table: mean by treatment group**

	Pure control (N=63242)		Assigned (N=50619)		Spillover (high) (N=2185)		Spillover (low) (N=30514)	
	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.	Mean	Std. Dev.
log(Price difference)	11.422	1.814	11.197	1.910	11.168	1.806	11.262	1.849
1 if sold	0.339	0.474	0.351	0.477	0.358	0.480	0.356	0.479
log(Trnsaction price)	14.342	0.650	14.091	0.713	13.947	0.712	14.179	0.712
Page view index	-0.003	0.580	-0.004	0.574	0.053	0.608	-0.015	0.572
Advertising index	-0.052	0.523	-0.108	0.442	-0.107	0.444	-0.111	0.437

Notes: Mean outcomes by the mutually exclusive treatment group. "log(Price difference)": log of the absolute difference between listing price and Price Calculator estimate. "1 if sold": a binary outcome that is 1 if the seller reports the car as sold. "log(Transaction price)": log of the self-reported transaction price. "Page-view index": a standardized index of page-view measures. "Advertising index": a standardized index of advertising usage by the seller.

Table 2: **Balance table: placebo regressions (ITT) on pre-specified main outcomes**

	log(Price difference)	1 if sold	log(Transaction price)	Page-view index	Advertising index
	(1)	(2)	(3)	(4)	(5)
	OLS	Logit	OLS	OLS	OLS
Assignment	-0.0209	-0.0317*	-0.0015	-0.0067	0.0061*
	(0.0195)	(0.0190)	(0.0025)	(0.0065)	(0.0035)
Spillover	0.0074	0.0318	-0.0063	0.0512**	-0.0094
	(0.0543)	(0.0420)	(0.0556)	(0.0197)	(0.0075)
Spillover (high)	-0.0120	0.0304	-0.0751	-0.0136	0.0056
	(0.0540)	(0.0431)	(0.0662)	(0.0212)	(0.0063)
Observations	104,485	116,314	19,222	117,715	117,715
Squared Correlation	0.05454	0.01119	0.89064	0.09523	0.25887
Pseudo R^2	0.01385	0.00882	1.0964	0.05739	0.21179
BIC	419,391.9	151,714.6	-1,993.4	195,543.5	133,310.6
Q-values: Assignment	0.383	0.237	0.565	0.383	0.237
Q-values: Spillover	0.91	0.749	0.91	0.053	0.535
Q-values: Spillover (high)	0.826	0.655	0.655	0.655	0.655

Notes: The outcomes are as defined in Section 4. "log(Price difference)": log of the absolute difference between listing price and Price Calculator estimate. "1 if sold": a binary outcome that is 1 if the seller reports the car as sold. "log(Transaction price)": log of the self-reported transaction price. "Page-view index": a standardized index of page-view measures. "Advertising index": a standardized index of advertising usage by the seller. The specification is as shown in Equation 1. Standard errors are reported in parentheses and clustered at the make-model level. The stars show the two-tailed significance in p-values: p<0.1*; p<0.05**; p<0.01***. Registered tests of statistical significance are q-values, which are reported in the bottom three rows.

Table 3: **Experimental results (ITT) on main pre-specified outcomes**

	log(Price difference)	1 if sold	log(Transaction price)	Page-view index	Advertising index
	(1)	(2)	(3)	(4)	(5)
	OLS	Logit	OLS	OLS	OLS
Assignment	-0.0327**	-0.0499***	-0.0008	-0.0172***	-0.0095***
	(0.0135)	(0.0172)	(0.0037)	(0.0039)	(0.0025)
Spillover	-0.0779*	0.0488***	-0.0401	0.0332***	-0.0005
	(0.0443)	(0.0140)	(0.0335)	(0.0106)	(0.0042)
Spillover (high)	0.0736	-0.0138	-0.0031	-0.0092	0.0062
	(0.0537)	(0.0333)	(0.0457)	(0.0169)	(0.0059)
Observations	101,750	111,309	14,084	117,891	117,891
Squared Correlation	0.10797	0.01471	0.92874	0.12322	0.29329
Pseudo R^2	0.02959	0.01197	1.2997	0.08546	0.24067
BIC	383,275.7	141,831.7	-6,886.7	167,975.7	131,198.5
Q-values: Assignment	0.023	0.006	0.835	0.000	0.001
Q-values: Spillover	0.14	0.002	0.293	0.006	0.912
Q-values: Spillover (high)	0.741	0.848	0.945	0.848	0.741

Notes: The table presents the intent-to-treat estimates on the main, pre-specified outcomes. The specification is as shown in Equation 1. The outcomes are as defined in Section 4. "log(Price difference)": log of the absolute difference between listing price and Price Calculator estimate. "1 if sold": a binary outcome that is 1 if the seller reports the car as sold. "log(Transaction price)": log of the self-reported transaction price. "Page-view index": a standardized index of page-view measures. "Advertising index": a standardized index of advertising usage by the seller. Standard errors are reported in parentheses and clustered at the make-model level. The stars show the two-tailed significance in p-values: p<0.1*; p<0.05**; p<0.01***. Registered tests of statistical significance are q-values, which are reported in the bottom three rows.

Table 4: **Experimental results (ToT) on main pre-specified outcomes**

	log(Price difference)	1 if sold	log(Transaction price)	Page-view index	Advertising index
	(1)	(2)	(3)	(4)	(5)
Treatment	-0.0440**	-0.0160***	-0.0011	-0.0252***	-0.0140***
	(0.0180)	(0.0060)	(0.0055)	(0.0067)	(0.0041)
Spillover	-0.0775*	0.0103***	-0.0401	0.0329***	-0.0007
	(0.0442)	(0.0030)	(0.0335)	(0.0106)	(0.0042)
Spillover (high)	0.0743	-0.0043	-0.0032	-0.0110	0.0053
	(0.0541)	(0.0075)	(0.0456)	(0.0174)	(0.0061)
Observations	101,750	111,312	14,084	117,891	117,891
R^2	0.10787	0.01464	0.92873	0.12284	0.29303
Within R^2	0.02809	0.00451	0.75561	0.01610	0.00609
Q-values: Assignment	0.022	0.015	0.835	0.001	0.003
Q-values: Spillover	0.14	0.004	0.293	0.006	0.869
Q-values: Spillover (high)	0.705	0.705	0.945	0.705	0.705

Notes: The table presents the treatment-on-the-treated estimates on the main, pre-specified outcomes. The 2SLS specification is as shown in Equation 2. The outcomes are as defined in Section 4. "log(Price difference)": log of the absolute difference between listing price and Price Calculator estimate. "1 if sold": a binary outcome that is 1 if the seller reports the car as sold. "log(Transaction price)": log of the self-reported transaction price. "Page-view index": a standardized index of page-view measures. "Advertising index": a standardized index of advertising usage by the seller. Standard errors are reported in parentheses and clustered at the make-model level. The stars show the two-tailed significance in p-values: p<0.1*; p<0.05**; p<0.01***. Registered tests of statistical significance are q-values, which are reported in the bottom three rows.

Table 5: **Experimental results (ITT) on main pre-specified outcomes by treatment group**

	log(Price difference) (1) OLS	1 if sold (2) Logit	log(Transaction price) (3) OLS	Page-view index (4) OLS	Advertising index (5) OLS
GroupSatAssigned(high)	-0.0316	-0.0238	-0.0441	0.0060	-0.0018
	(0.0372)	(0.0207)	(0.0370)	(0.0113)	(0.0053)
GroupSatAssigned(low)	-0.1135***	0.0046	-0.0407	0.0166	-0.0090**
	(0.0408)	(0.0212)	(0.0353)	(0.0122)	(0.0043)
GroupSatSpillover(high)	-0.0511	0.1117**	-0.0421	0.0312**	0.0162**
	(0.0552)	(0.0552)	(0.0427)	(0.0119)	(0.0066)
GroupSatSpillover(low)	-0.0749*	0.0431***	-0.0401	0.0327***	-0.0007
	(0.0435)	(0.0145)	(0.0335)	(0.0106)	(0.0042)
Observations	101,750	111,309	14,084	117,891	134,781
Squared Correlation	0.10799	0.01474	0.92874	0.12322	0.29973
Pseudo R^2	0.02960	0.01199	1.2997	0.08546	0.25504
BIC	383,285.7	141,840.4	-6,877.1	167,986.9	142,368.9
Q-values: Assignment group (high)	0.663	0.626	0.626	0.729	0.729
Q-values: Assignment group (low)	0.036	0.83	0.315	0.293	0.094
Q-values: High spillover group	0.359	0.072	0.359	0.038	0.038
Q-values: Low spillover group	0.15	0.007	0.292	0.007	0.872

Notes: The table presents the intent-to-treat estimates on the main, pre-specified outcomes. The specification is *not* identical to Equation 1, but instead, we regress outcomes on the following dummies: assigned to treatment in high-saturation model ("GroupSatAssigned(high)"), assigned to treatment in low-saturation model ("GroupSatAssigned(low)"), not directly assigned to treatment in high-saturation model ("GroupSatSpillover(high)"), and not directly assigned to treatment in low-saturation model ("GroupSatSpillover(low)"). The outcomes are as defined in Section 4. "log(Price difference)": log of the absolute difference between listing price and Price Calculator estimate. "1 if sold": a binary outcome that is 1 if the seller reports the car as sold. "log(Transaction price)": log of the self-reported transaction price. "Page-view index": a standardized index of page-view measures. "Advertising index": a standardized index of advertising usage by the seller. Standard errors are reported in parentheses and clustered at the make-model level. The stars show the two-tailed significance in p-values: p<0.1*; p<0.05**; p<0.01***. Registered tests of statistical significance are q-values, which are reported in the bottom three rows.

Table 6: **Regressions on survey measures: Sellers' expectations on prices**

	log(absdff(Expectation))			Amt. bargain	Searched listings
	(1)	(2)	(3)	(4)	(5)
	OLS	OLS	OLS	OLS	Logit
Assignment	-0.2128***	-0.0531	-0.1945	5,561.1***	0.1338
	(0.0765)	(0.0743)	(0.1692)	(1,670.5)	(0.1845)
Spillover	0.0233	-0.1126	0.0777	-642.9	-0.1345
	(0.1025)	(0.1005)	(0.1069)	(2,697.6)	(0.1537)
Spillover (high)	0.1712	0.1324	0.1398	3.690	-0.0276
	(0.1239)	(0.1246)	(0.1510)	(3,644.3)	(0.1391)
Observations	2,046	2,045	2,045	2,321	2,185
Squared Correlation	0.09972	0.16112	0.10842	0.08766	0.05688
Pseudo R^2	0.02475	0.04524	0.02552	0.00370	0.05098
BIC	9,618.9	8,733.0	10,113.1	58,576.7	3,010.0

Notes: The table presents the treatment-on-the-treated estimates on the survey outcomes. "log(diff(Expectation))": log absolute difference between the Price Calculator estimate and their expected transaction price, as measured through the survey. We asked price expectations in three ways: their initial expectation (column 1), the highest price they could have received (column 2), and the lowest price they could have received (column 3). "Amt. bargain": The amount in PKR that the seller is willing to bargain. "Searched listings": a binary outcome that is 1 if the seller reported having searched for or looked at other similar ads to their vehicles. The ITT specification is as shown in Equation 1. Standard errors are reported in parentheses and clustered at the make-model level. The stars show the two-tailed significance in p-values: p<0.1*; p<0.05**; p<0.01***.

Table 7: **Regressions on survey measures: Sellers beliefs about search and information frictions**

	Difficult to get inquiry (1)	Difficult to get good price (2)	Buyers have good info (3)	Sellers have good info (4)
Assignment	-0.0658**	-0.0532**	0.0275	0.0239
	(0.0324)	(0.0232)	(0.0477)	(0.0392)
Spillover	-8.42×10^{-5}	-0.0163	0.0457	0.0221
	(0.0286)	(0.0221)	(0.0316)	(0.0368)
Spillover (high)	0.0197	0.0469	-0.0868**	-0.0176
	(0.0282)	(0.0306)	(0.0394)	(0.0292)
Observations	2,311	2,311	2,310	2,310
R^2	0.11718	0.13054	0.10224	0.10057
Within R^2	0.00257	0.00212	0.00299	0.00100

Notes: The table presents the treatment-on-the-treated estimates on the survey outcomes. The outcomes on this table are in the Likert scale, where 1 = "strongly disagree" and 5 = 'strongly agree." "Difficult to get inquiry": It is difficult to get inquiries from potential buyers. "Difficult to get good price": It is difficult to get good price offers from potential buyers. "Buyers have good info": Buyers have good information about what fair prices for used vehicles are. "Sellers have good info": Sellers have good information about what fair prices for used vehicles are. The ITT specification is as shown in Equation 1. Standard errors are reported in parentheses and clustered at the make-model level. The stars show the two-tailed significance in p-values: p<0.1*; p<0.05**; p<0.01***.

Table 8: **Regressions on survey measures: Valuation of Price Calculator and advertising tools**

	1 if WTP at Rs100 (1) Logit	WTP (2) OLS	Ad useful-high price (3) OLS	Ad useful-sell faster (4) OLS
Assignment	-0.2485	-5.674	0.0239	0.0251
	(0.1624)	(3.584)	(0.0392)	(0.0400)
Spillover	0.0314	3.710	0.0221	0.0252
	(0.1094)	(3.503)	(0.0368)	(0.0323)
Spillover (high)	0.1901	3.429	-0.0176	-0.0152
	(0.1313)	(2.107)	(0.0292)	(0.0286)
Observations	2,247	2,261	2,310	2,301
Squared Correlation	0.05775	0.08011	0.10057	0.10072
Pseudo R^2	0.04635	0.00781	0.06402	0.06679
BIC	3,578.5	25,149.6	4,764.8	4,597.6

Notes: The table presents the treatment-on-the-treated estimates on the survey outcomes. "1 if WTP at Rs100": a binary outcome that is 1 if the seller reports to be willing to pay PKR 100 for a Price Calculator estimate per post. "WTP": seller's willingness to pay for a Price Calculator estimate per post. "Ad useful-high price": features and bumps are useful for selling the vehicle faster, on a Likert scale (5 = 'strongly agree."). "Ad useful-sell faster": features and bumps are useful for getting a higher price for the vehicle, on a Likert scale (5 = 'strongly agree."). The ITT specification is as shown in Equation 1. Standard errors are reported in parentheses and clustered at the make-model level. The stars show the two-tailed significance in p-values: p<0.1*; p<0.05**; p<0.01***.

Table 9: **Regressions on survey measures: Price Calculator and transaction outcomes**

	Seen PC (1) Logit	Others seen PC (2) Logit	1 if sold (3) Logit	log(Transaction price) (4) OLS
Assignment	0.7749***	0.4057**	0.0207	-0.0130
	(0.1287)	(0.1622)	(0.1224)	(0.0179)
Spillover	0.0013	0.1678	0.1606	0.0219
	(0.1596)	(0.1060)	(0.1430)	(0.0372)
Spillover (high)	-0.0925	-0.0550	0.0078	-0.0694
	(0.1238)	(0.1712)	(0.1727)	(0.0463)
Observations	2,202	2,195	2,280	1,397
Squared Correlation	0.09259	0.06356	0.09186	0.76180
Pseudo R^2	0.08516	0.06132	0.06989	0.65933
BIC	3,150.7	2,823.5	3,820.6	2,034.9

Notes: The table presents the treatment-on-the-treated estimates on the survey outcomes. "Seen PC": a binary outcome that is 1 if the seller reports to have seen the Price Calculator estimate. 'Seen PC": a binary outcome that is 1 if the seller reports that other sellers have received the Price Calculator estimates. '1 if sold" and "log(Transaction price)": analogous to main outcomes but collected through the survey instead of the platform's database. The ITT specification is as shown in Equation 1. Standard errors are reported in parentheses and clustered at the make-model level. The stars show the two-tailed significance in p-values: p<0.1*; p<0.05**; p<0.01***.

10 Figures

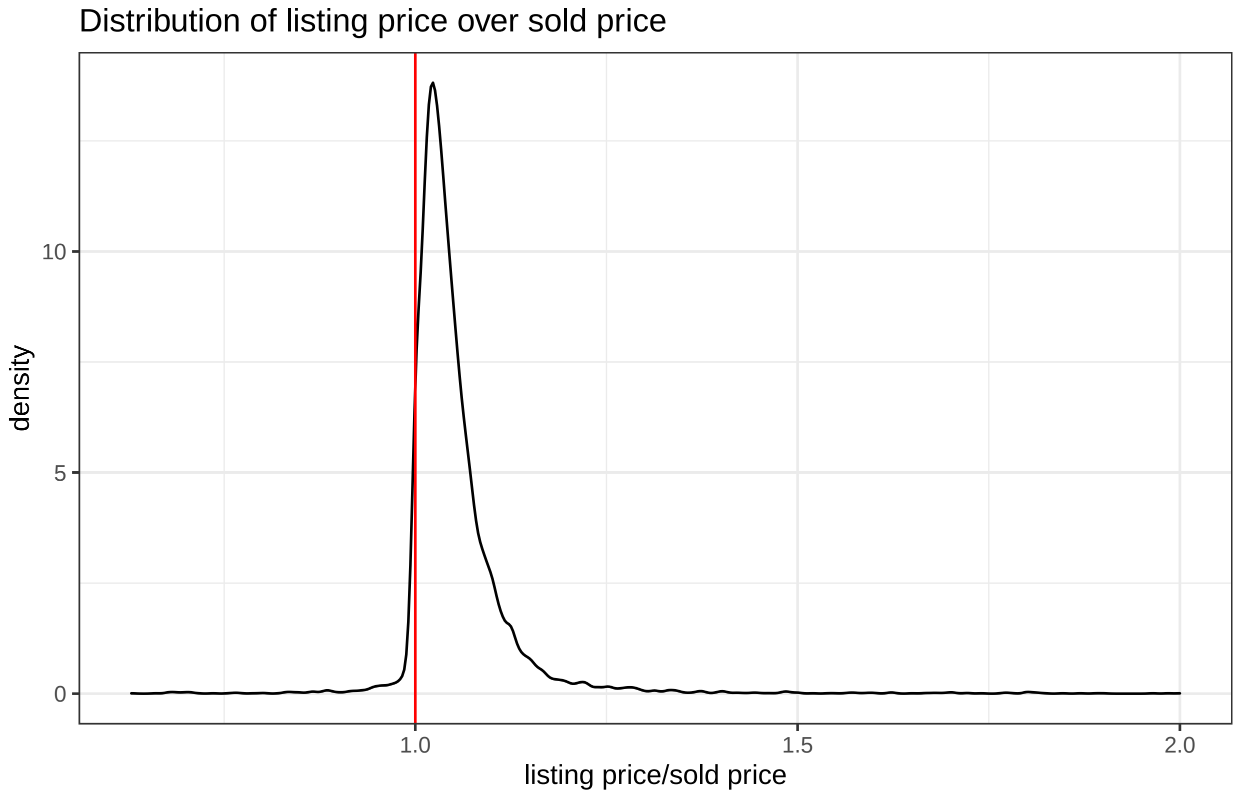

Figure 1: Distribution of the share of listing price to transaction price

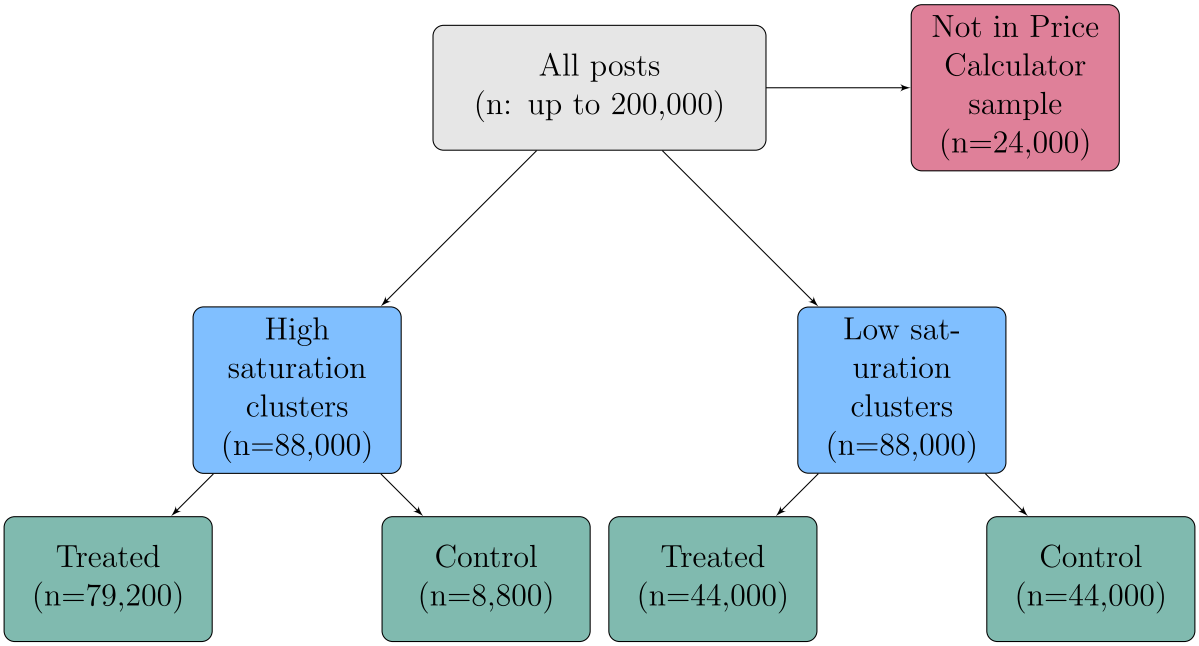

Figure 2: Treatment Groups

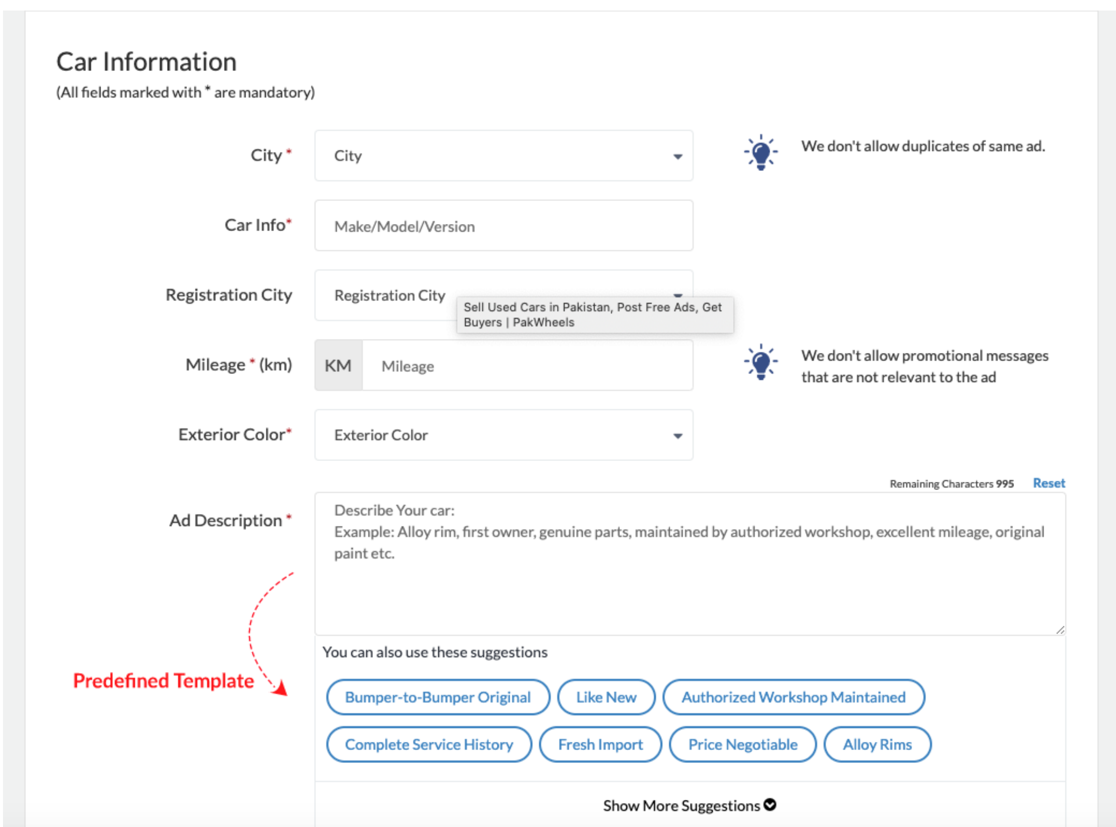

Figure 3: Making of a listing: Vehicle information

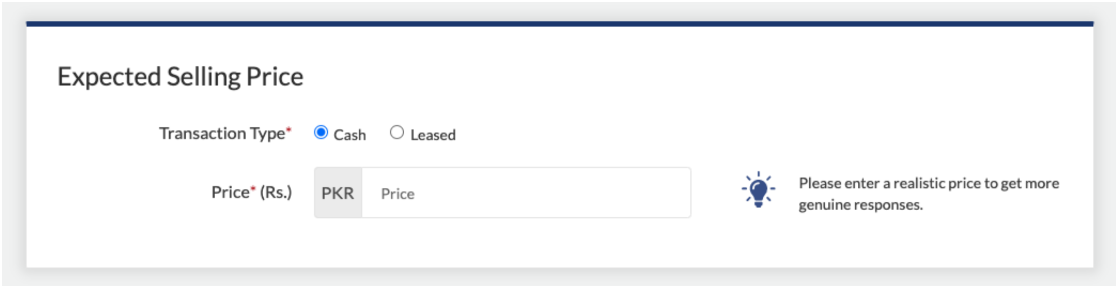

Figure 4: Making of a listing: Vehicle price

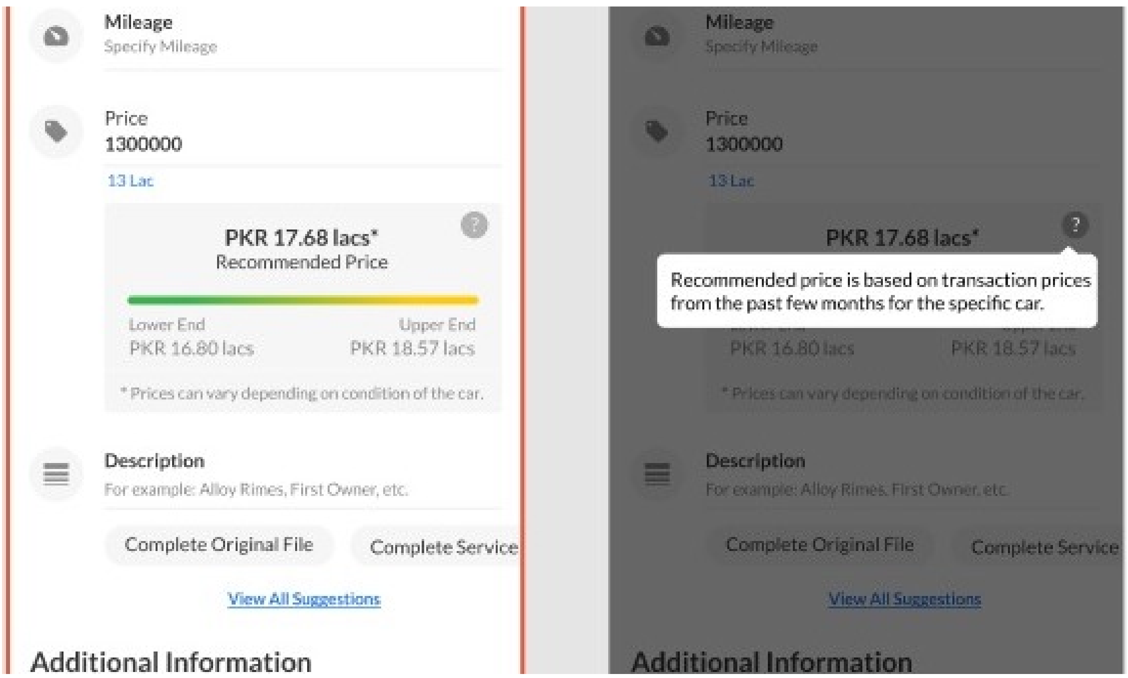

Figure 5: Display of the Price Calculator estimate

A Data

A.1 Posts

PakWheels' database tracks every post on the platform. Once a post is created, it is vetted against spam or fraud, made publicly available on the platform, then removed after 90 days or once the user asks for it to be taken down. We collect the following measures from the database:

- timing of the post's creation, approval, and closure
- vehicle characteristics
  - basic information such as make, model, model year, mileage, sellers' location, and registration city
  - additional information about vehicle characteristics, such as version, assembly, engine size, and capacity
- listing price
- self-reported transaction outcome (e.g., sold to a customer on the platform, sold through other means, decided not to sell)
- self-reported transaction price, if sold.

The database also tracks any updates to variables over the course of posts' active status. This allows us to capture sellers' initial choice of the listing price before and after exposure to the Price Calculator estimate.

A.2 Advertising tools and vehicle inspection services

PakWheels' database also tracks users' platform-credit purchases and usages, which we consider to be measures of sellers' advertising efforts. Users on PakWheels have two primary tools for advertising: "bump" and "feature" credits. A "bump" credit allows sellers to bring their post to the top of the result page in the default, reverse-chronological listing order. This effectively increases the post's visibility as more people look at the first pages of listings. On the other hand, a "feature" credit would put their post in a few reserved spots at the top of the result page and label it as a "featured ad", in a similar way as promoted ads on Google searches. Posts are otherwise listed in the reversed chronological order within the class of featured ads.

Another way for sellers to attract buyers' attention to their posts is to provide signals of vehicle quality. In order to do so, sellers can request in-person inspections by PakWheels' mechanics, who give scores (out of 100) on eight dimensions (engine, brakes, suspension, interior, AC, electrical, exterior, and tires) based on a pre-specified rubric. The vehicles will pass the inspection if the unweighted average of scores over these eight dimensions is above a threshold. They can then be marked as "PakWheels certified" on the platform for an additional fee. Because certification is endogenous to vehicle quality, we use the data on whether or not the vehicle was ever inspected, as opposed to certified.

A.2.1 Expenditures on advertising tools

Another way of expressing sellers' advertising choices would be in terms of the amount paid to the platform for advertising. This is made difficult, however, by the fact that credits for bumps and features are purchased in bundles, and users can apply them to any posts that they own. We, therefore, do not plan on using this measure as a primary outcome. Nonetheless, we collect data on advertising expenditures for robustness checks where the unit of analysis is the user. The data set contains information on purchase timing, descriptions of the bundles or services, quantity, and prices.

A.3 Buyer-attention measures

One of our hypotheses is that the price information intervention and resulting changes to pricing and advertising would affect buyers' attention to certain posts. In order to construct measures of buyer attention, we access PakWheels' data on views and clicks at the post level. We are able to collect cumulative measures of the following:

- page views (i.e., clicks on the post)
- clicks on the "Show Phone Number" button within the post to contact the seller.

We also capture the number of times each post appears on search listings. We run an analysis including this measure in the index as an alternative specification, as well.

A.4 Endline survey

We use self-reported transaction prices to train and test the Price Calculator estimates. These self-reported data are collected in an online form whenever sellers choose to take their posts down. There are concerns about the accuracy of reported transaction prices, particularly for the following reasons:

1. Transaction prices may be selectively reported (e.g., if those who fetched a higher price are more likely to report).

2. Conditional on reporting, sellers may obfuscate the true value, leading to more noise in the price estimate.

3. Conditional on reporting, sellers may feel that the reported price should be inflated or deflated (because of, for example, their beliefs about a fair transaction price or the desire to appear successful).

4. Conditional on reporting, sellers may find it easy just to repeat the listing price they have already given for their post.

5. Conditional on reporting, sellers may simply put a random number down to "get it out of the way."

We can identify the extent of number 4. by comparing transaction prices with listing prices and address 5. via data cleaning. Concerns like number 2. may introduce noise but should not bias the Price Calculator estimate or our empirical analysis.

We are unable to directly address concerns 1. and 3. from the data, nor is it realistic for us to request sales receipts or access other independent sales records. Instead, we randomly selected 3,000 listings (stratified over the vehicle model) and conducted a short phone survey of the sellers. We survey owners of the posts between 3 to 5 weeks after their initial listing. The primary objective of the survey is to collect an independent measure of self-reported transactions and transaction prices from the platform-collected counterparts. In addition, we ask about sellers' expectations about transaction prices, beliefs about search and information frictions, and demand for, and perceived effectiveness of, the Price Calculator and advertising tools. [6]

A.5 Search engine logs

Aggregated search engine logs tell us which combinations of terms are used most frequently by viewers on PakWheels. We use these aggregate statistics for our justifications for market cluster groupings. Our objective is to minimize concerns about inter-cluster interference but also retain as many randomization clusters for the step as possible. Our aggregate search logs data are taken from the month of August 2020. They represent tens of millions of searches over the month, and our data contain numbers of searches per combination of search terms (e.g., make, model, model-years in range, city, range of listing prices). We capture 35,000 most common search combinations, which account for 93% of all searches. We use these data for our definition of clusters in Section 3.2.1.

[6] The survey questionnaire is included in Appendix Section G.

A.6 Listing orders

Beyond the primary analysis, in which we measure the average spillover effects on treated clusters, we plan to assess the extent to which the spillover effects depend on the "proximity" to treated posts, such as how close a given ad is to treated peers in ad listings. For this, we web-scrape listing orders in their default, reverse-chronological order on a daily basis for each make-model cluster in the sample.

A.7 Use of an old Price Calculator

We also track the usage of a previous version of the Price Calculator, which our intervention replaces. The previous iteration of the Price Calculator was designed and implemented prior to the beginning of our research collaboration with PakWheels. It was contained in a separate module on PakWheels' website and mobile apps, unintegrated with the posting process, and was discontinued at the end of December 2020. The old Price Calculator offered predictions to only a handful of make-model-model years of certain colors, locations, and mileage. PakWheels keeps a log of all price estimates the old Price Calculator provided at each instance. This dataset contains the user ID, search inputs (make, model, model year, location, mileage, and if seller or buyer), the price estimates, and the time stamp.

B Secondary outcomes listed in the pre-analysis plan

B.0.1 Survey data

The first-order objectives of the endline phone survey are to confirm reported transaction outcomes on PakWheels' platform and elicit sellers' beliefs about transaction prices. Those outcomes are listed in bold below. We also collect the following measures from survey respondents:

1. validation of self-reported transaction outcome
    - **1 if the vehicle is sold**
    - **transaction price (if sold)**
    - reasons for not selling the vehicle (if not sold)
    - relationship with the buyer (if sold)
2. price elicitation (stated beliefs)
    - **Expected transaction price at the time of initial posting**
    - lower and upper bounds of the expected transaction price

3. purchase price
4. number of vehicles previously traded
5. recall and salience of the Price Calculator instrument
    - 1 if the seller recalls seeing the Price Calculator estimate
    - recall of the Price Calculator estimate
    - beliefs about Price Calculator's accuracy
6. search and information acquisition
    - if the seller searches for other posts on PakWheels
    - terms used for the search
    - other sources of information
7. stated beliefs about challenges and frictions on the market
    - if the seller believes it is difficult to receive enough inquiries on PakWheels
    - if the seller believes it is difficult to receive acceptable price offers on PakWheels
8. stated beliefs about the usefulness of the advertising tools offered by PakWheels (i.e., bumps and features)
9. stated willingness to pay for the Price Calculator estimates

B.0.2 Post's duration on the platform

PakWheels' database reports when each post is created and taken down, so we can calculate the duration of the post's active status on the platform. One challenge is that posts may be left inactive for a period of time, so this would not be a measure of sellers' active participation in the market. This makes it difficult to interpret the meaning of any causal effect on this variable other than in aggregate as a measure of market congestion. For this reason, we consider this as a secondary outcome.

B.0.3 Price changes, and convergence to estimated price

We have chosen the logged absolute difference between the listing price and the Price Calculator estimate as a primary outcome variable. It is possible, however, that the treatment effect on the list price may be better captured if the Price Calculator induces a level shift in price or affects whether sellers ever adjust their initial listing prices. It is also possible that the treatment effect on the listing price is asymmetrical around the Price Calculator estimate. We intend to address this possibility with following alternative outcomes pertaining to the listing price as robustness checks:

- log(list price)
- 1 if the listing price is ever modified
- difference between the initial and final listing prices.

B.0.4 Cluster-level outcomes

Our two-stage randomization procedure allows us to estimate the impact on cluster-level outcomes because of the two-stage design. Part of our secondary analysis focuses on cluster-level aggregate measures of moments of prices, page views, and post duration. We construct the following variables at the cluster-day level:

- number of new posts
- number of active posts
- standard deviation and kurtosis of the listing price
- standard deviation and kurtosis of page views.

B.0.5 Spillovers based on listing order

For our primary analysis, we use the post-level data to identify the treatment effects of treatment and spillover assignment when the posts are initially created. An alternative conception of potential spillover, however, maybe that it is a function of exposure to treated posts over the listing space over time. To create a measure of potential spillover intensity based on proximity to treated posts, we web-scrape data on the listing order from PakWheels.

We define outcomes from PakWheels' scraped data as follows:

- number of days a post is on the first page of the make-model level search result
- average page number of the search results over the course of its active status.

We also construct the following variables as proxies of exposure to treated posts:

- number of days spent being adjacent to at least one treated post
- average number of treated posts within its listing result page (i.e., if the post resides on page 5 in a given day, then we take the number of treated posts on page 5), throughout the post's active status.

C Details on Conceptual Framework

C.1 Set-up

Suppose that we have a seller i, who is endowed with an asset. The asset- and seller-characteristics are denoted as s_i, and information set $\mathcal{I}_i$. The search and transaction process is as follows:

- Seller i forms a prior belief about the demand distribution for their asset based on the information set $\mathcal{I}_i$ and characteristics s_i.
- Some sellers are provided with an information signal, i.e., the Price Calculator estimate denoted as x_i.
- If treated, seller i forms a posterior belief about the demand distribution based on the information signal x_i, their belief in the quality of the signal, and their prior.
- Seller i chooses a listing price p^l and amount of advertisements a, based on their posterior belief about the demand distribution and their characteristics s_i.
- Choices of p^l and a affect the distribution of potential buyers with whom seller i is matched via a Poisson process.
- Once a match occurs, seller i makes a take-it-or-leave-it (TIOLI) offer p^t below p^l to the potential buyer.
- Transaction occurs if matched buyer's WTP is higher than p^t.

We denote the probability density function (PDF) of true buyer WTP as $f(\theta)$, and the distribution of potential buyers that get matched to the seller, conditional on p^l, as $g(\theta; p^l)$ and their cumulative equivalents, F and G. The distinction between $F(\theta)$ and $G(\theta; p^l)$ is key since we assume that the seller's choice of the listing price p^l skews the distribution of potential buyers (who may click on the post depending on the listing price) towards p^l itself. Setting too high of p^l also comes at a cost, as we make the following assumption:

$$\int_{-\infty}^{\infty} g(\theta; p^l) d\theta \leq 1 \tag{4}$$

$$\frac{\delta}{\delta p^l} \int_{0}^{\infty} g(\theta; p^l) d\theta < 0 \tag{5}$$

In other words, the distribution g is a subset of f and does not add up to one. In other words, g does not add up to one, and high values of p^l effectively reduce the pool of buyers to match with. The relationship between p^l and $g(\theta; p^l)$ are also described schematically in Figure C.1:

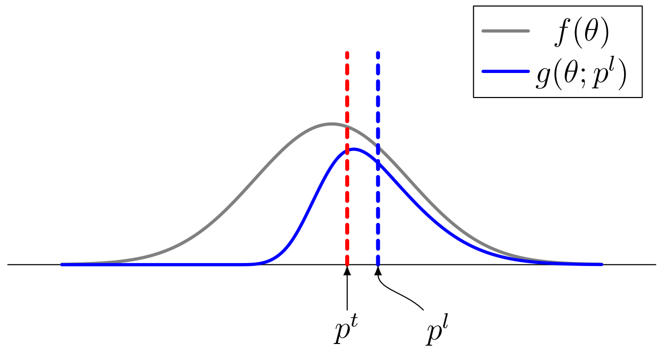

Figure C.1: Relationship between list price, the buyer it draws, and the TIOLI price

C.2 The objective function

Under no information friction, seller i chooses the listing price and advertisements to maximize the following:

$$V(p^l, a; s_i) = -c - k(a) + \gamma(a) \int \max_{p^t} [\mathbb{E}\pi(p^t; p^l, s_i)] g(\theta; p^l)\, d\theta \tag{6}$$

Sellers incur a constant cost of search, denoted as c. They also incur a variable cost $k()$, based on the amount spent on advertising, a. The term $\gamma(a)$ is a Poisson match rate between the seller and a potential buyer, and it is an increasing function with respect to a. We denote the seller's utility from the transaction as $\pi(p^t; s_i)$. This is a function not of the listing price but of the eventual offer price p^t, which is discussed below. π is also not strictly a profit term, as sellers may also have preferences over how quickly to sell the vehicle, as captured in s_i. We assume the function is continuously differentiable and concave with respect to its only choice variable p^t, so that there is a single global maximum that is conditional on individual characteristics s_i.

C.2.1 The TIOLI price p^t

Seller i sets the listing price p^l, keeping in mind the distribution of buyers the list price attracts and the (TIOLI) offer price p^t that seller i would then select. We assume that there is a one-to-one correspondence between p^l and p^t conditional on seller i's characteristics. We also assume that potential buyers cannot perfectly infer p^t from p^l, because this depends on the seller's individual characteristics s_i as well as $\mathcal{I}_i$. This allows us to express the seller's problem of maximizing the value function $V()$ as choices of p^l and a.

Based on the mapping we assume between p^l and p^t, we can also express Equation 6 as follows:

$$V(p^l, a; s_i) = -c - k(a) + \gamma(a)\pi(p^t(p^l; s_i))\Omega(p^t(p^l), s_i), \tag{7}$$

$$\text{where } \Omega(p^t(p^l), s_i) = \int_{p^t(p^l)}^{\infty} g(\theta; s_i, p^l) d\theta \tag{8}$$

Ω is a function that represents the probability that a potential buyer's willingness to pay is greater than the TIOLI offer price, given the listing price p^l chosen by the seller. In order to ensure a unique and interior solution to the problem, we assume that Ω is decreasing and concave with respect to p^l; As p^l increases, fewer buyers are drawn to the listing and have a WTP greater than the TIOLI price associated with p^l. This ensures that the objective function in Equation 7 is quasiconcave with respect to its argument p^l.

C.3 Identifying optimal p^l and a

Taking the first-order condition of Equation 7 with respect to p^l gives the following expression, where we see that the choice of optimal p^l is independent of a under no information friction.

$$0 = \frac{dV}{dp^l} = \gamma(a)[\pi'(p^t)\frac{dp^t}{dp^l}\Omega(p^t(p^l), s_i) + \pi(p^t(p^l; s_i))\frac{d\Omega(p^t(p^l), s_i)}{dp^l}\frac{dp^t}{dp^l}] \tag{9}$$

Rearranging and simplifying Equation 9, we get:

$$\Omega(p^t(p^l), s_i)\pi'(p^t)\frac{dp^t}{dp^l} = -\frac{d\Omega(p^t(p^l), s_i)}{dp^l}\frac{dp^t}{dp^l}\pi(p^t(p^l; s_i)) \tag{10}$$

The left-hand side of Equation 10 is an expression of "marginal benefit" of price adjustment, i.e., the marginal change in the seller's payoff ($\pi'(p^t)\frac{dp^t}{dp^l}$) times the probability that a matched buyer accepts the TIOLI price ($\Omega(p^t(p^l), s_i)$). The right-hand side is an expression of the "marginal cost" of price adjustment, i.e., the marginal effect of the changes in listing price on the probability of TIOLI price's acceptance ($\frac{d\Omega(p^t(p^l),s_i)}{dp^l}\frac{dp^t}{dp^l} < 0$) times the payoff ($\pi(p^t(p^l; s_i))$). As for the second order conditions, we have made assumptions about the functional forms of $\pi()$ and Ω such that we can show that the "marginal benefit" from Equation 10 is decreasing and "marginal cost" increasing.

Similarly, taking the first-order condition of Equation 7 with respect to a and rearranging gives the following expression that identifies the optimal a is conditional on a choice of p^l.

$$\frac{d\gamma}{da}\pi(p^t(p^l; s_i))\Omega(p^t(p^l), s_i) = k'(a) \tag{11}$$

A component of the left-hand side of Equation 11 is the marginal gain from advertising, which is a product of changes in the Poisson match rate ($\frac{d\gamma}{da}$) and expected payoff ($\pi(p^t(p^l; s_i))\Omega(p^t(p^l), s_i)$). This marginal gain is equal to the right-hand side term $k'(a)$, i.e., the marginal cost of advertising. As for the second-order condition, we assume the functional forms of the Poisson matching function $\gamma()$ and the cost function $k()$ such that a unique solution of a exists.[7]

C.4 Information friction and beliefs

The solutions above hinge on the assumption that sellers have accurate beliefs about buyers' WTP, other parameters, and functional forms (e.g., Poisson match rate function). However, if there is noise in sellers' beliefs about buyers' WTP, how would it affect sellers' decisions? We explore this possibility while assuming that beliefs on other parameters and functional forms are accurate.

Suppose that seller i possesses noisy information about the distribution of buyers' WTP. We assume that their beliefs are accurate on average over all sellers to focus on a point about noise rather than bias. Individual sellers hold beliefs over $f(\theta)$, and the distribution of buyers they get matched to conditional on p^l(i.e., $g(\theta)$) also depends on their belief over $f(\theta)$. We denote seller i's belief on f as $\hat{f}(\theta|\mathcal{I}_i)$ and their resulting belief over g as $\hat{g}(\theta_0|\mathcal{I}_i)$, where $\mathcal{I}_i$ denotes information quality individuals possess to form a prior belief. The resulting optimality conditions then simply replace f with $\hat{f}(\theta_0|\mathcal{I}_i)$ and g with $\hat{g}(\theta_0|\mathcal{I}_i)$.

The idea behind our intervention is that a randomly selected subset of sellers would update their beliefs based on the information signals contained in the Price Calculator estimates. Signal x_i is drawn from the true distribution of the WTP, f. If treated sellers engage in a rational Bayesian updating process, their posterior beliefs $\hat{f}(|x_i, \mathcal{I}_i)$ and $\hat{g}(|x_i, \mathcal{I}_i)$ from their equivalents under no information friction. The schematic representation of Bayesian belief updating is shown in Figure C.2.

C.4.1 Bayesian belief updating

We assume that sellers engage in a Bayesian belief-updating process when they receive information signals in the form of Price Calculator estimates. We note that, in reality, some sellers may not be Bayesian and exhibit behavioral deviations (e.g., motivated beliefs). We stay away from such complications and focus on a rational framework, which we believe is more relevant to the main treatment effects we expect to see. Furthermore, we may expect sellers to have heterogeneous strategic responses to the information signal. Formalizing the

[7] It is likely reasonable to assume that the Poisson match rate function $\gamma()$ is concave given the diminishing returns to advertising. The potential issue is with the cost function $k()$, including the financial cost of advertising. PakWheels offers quantity discounts of advertising credits, making the per-unit cost of advertisement use cheaper as sellers use more. We will check with data to see if the use of advertising tools in excess (e.g., bumping their ads at a high frequency) is a concern.

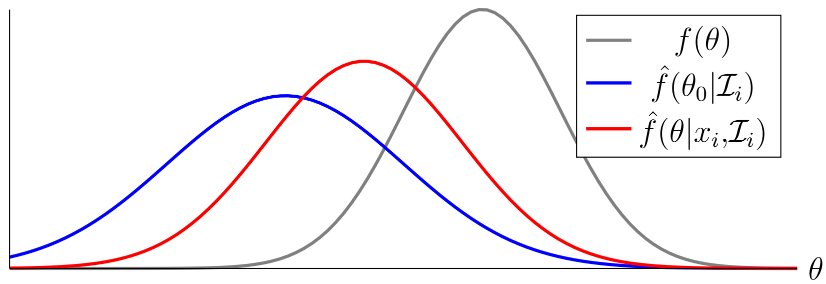

Figure C.2: Beliefs about$f$ based on information set $\mathcal{I}_i$ and signal x_i

belief updating process thus allows us to separate the strategic responses (expressed in the functional form of $\pi()$) from the changes in beliefs (parameters of $\hat{f}()$, which we elicit in the endline survey.

We assume that both buyers' WTP and sellers' prior beliefs about it are normally distributed. We make this assumption to simplify the distributional forms of prior and posterior beliefs, as the normal distribution is its own conjugate prior. We also note that the sellers' prior beliefs are based on information they already have access to, i.e., $\mathcal{I}_i$. We express the prior beliefs and true distributions as follows:

- Prior belief about demand distribution: $\hat{f}(\theta_0|\mathcal{I}_i) \sim \ N(\mu_{i,0}, \sigma_0^2)$
- True demand distribution: $f(x) \sim \ N(\mu, \sigma^2)$

Signals that sellers receive are drawn from the true demand distribution x. If sellers are Bayesian, they will update θ based on x. Both the prior belief as well as the signals are continuous, so the posterior belief function is as follows:

$$\hat{f}(\theta|x_i, \mathcal{I}_i) \sim \ N(\frac{a\mu_0 + bx}{a+b}, \frac{1}{a+b}), \tag{12}$$

where $a = \frac{1}{\sigma_0^2}$, and $b = \frac{1}{\hat{\sigma}^2}$. We assume that sellers have *perceptions* about the quality of information signals they receive, whether that is the variance of f and/or the standard error of the information signal we deliver in practice. We therefore use $\hat{\sigma}^2$ instead of σ^2 to include an individual's perception about the credibility, or variance, of the information signal. Furthermore, we could have specified that $\hat{\sigma}^2$ is a function of some argument (e.g., the difference between data and prior mean: $\sigma^2 = \phi(|x - \mu_{i,0}|)$). Instead, we stay agnostic about factors that correlate with $\hat{\sigma}^2$ and leave this as an empirical exercise after estimating $\hat{\sigma}^2$.

C.5 Model predictions: direct treatment effects

C.5.1 Information intervention reduces deviation of p^l from p^{l*})

The optimality condition for p^l in Equation 10, under noisy beliefs, can be rearranged as follows.

$$\frac{\pi'(p^t)}{\pi(p^t(p^l; s_i))} = -\frac{\frac{d\Omega(p^t(p^l;\hat{f}(\theta_0|\mathcal{I}_i),s_i)}{dp^l}}{\Omega(p^t(p^l;\hat{f}(\theta_0|\mathcal{I}_i),s_i))} \tag{13}$$

Following the logic from Equation 10, Equation 13 shows that the seller sets their listing price p^l such that their *beliefs* about the expected payoff equals their *beliefs* about the cost. Their choice of p^l based on their belief about $\hat{f}()$, however, does not necessarily equal that based on true $f()$. In other words, it is generally true that given a choice of p^l made under information friction (with access only to $\mathcal{I}_i$) and prior belief $\hat{f}()$:

$$\frac{\frac{d\Omega(p^t(p^l;\hat{f}(\theta_0|\mathcal{I}_i),s_i)}{dp^l}}{\Omega(p^t(p^l;\hat{f}(\theta_0|\mathcal{I}_i),s_i))} \neq \frac{\frac{d\Omega(p^t(p^l;f(\theta),s_i)}{dp^l}}{\Omega(p^t(p^l;f(\theta),s_i))} \tag{14}$$

We make assumptions about the structure of the belief-updating process in Section C.4.1 to show how the Price Calculator estimate could help sellers update beliefs about the demand distribution $\hat{f}()$, on average toward the truth $f()$. Given our assumption that the form of the objective function with respect to the choice variable p^l is quasiconcave, we make the following prediction:

- Prediction 1.: Information intervention brings p^l closer to what it would be under no information friction about the demand distribution (call this p^{l*}), if the updated belief brings the posterior distribution $\hat{f}(\theta|x_i,\mathcal{I}_i)$ closer to $f(\theta)$ from $\hat{f}(\theta|\mathcal{I}_i)$. (Research question 1.1.)

C.5.2 Information intervention increases *ex post* payoffs

If information friction results in beliefs about $f()$ and the objective function are quasiconcave with respect to p^l, then the choice of p^l under information friction is *ex-post* suboptimal. It follows that the Price Calculator information signal would increase the *ex-post* payoff, as posterior beliefs about $f()$ are more accurate and would result in p^l closer to p^{l*} on average. In other words, we can show that:

$$\pi(p^t(p^l;\hat{f}(\theta|x_i,\mathcal{I}_i),s_i))\Omega(p^t(p^l;\hat{f}(\theta|x_i,\mathcal{I}_i),s_i)) \geq \pi(p^t(p^l;\hat{f}(\theta_0|\mathcal{I}_i),s_i))\Omega(p^t(p^l;\hat{f}(\theta_0|\mathcal{I}_i),s_i)) \tag{15}$$

This leads to the next prediction of our conceptual framework:

- Prediction 2.: Information intervention increases sellers' *ex-post* returns from the platform if the updated belief brings the posterior distribution $\hat{f}(\theta|x_i, \mathcal{I}_i)$ closer to $f(\theta)$ from $\hat{f}(\theta|\mathcal{I}_i)$. (Research question 1.2.)

C.5.3 Information intervention may increase a

We have so far shown that the choice of listing price can be affected by noise in sellers' beliefs about the demand and that *if* the Price Calculator estimate leads to an updated belief that is closer to the truth, then it would bring the listing price toward the optimum and improve their payoff from engaging with the marketplace. How would their choice of advertising then be affected by information friction? From Equation 11, we see that under no information friction, sellers use advertising up to the point where the expected marginal benefit of its use equals its marginal cost. Under information friction, however, sellers consume advertising tools to the point where their *beliefs* about the expected marginal benefit equals marginal cost. The following equation makes this point by modifying Equation 11, and putting the term corresponding to beliefs about expected payoffs in a (very) wide hat:

$$\frac{d\gamma(a; s_i, \mathcal{I}_i)}{da} \widehat{\pi(p^t(p^l; \hat{f}(\theta_0|\mathcal{I}_i), s_i))\Omega(p^t(p^l; \hat{f}(\theta_0|\mathcal{I}_i), s_i))} = k'(a; s_i, \mathcal{I}_i) \tag{16}$$

The information intervention improves sellers' *ex-post* payoffs (Equation 15). The Price Calculator intervention may also shift sellers' expectations, i.e., they themselves believe that their *ex-post* payoffs would improve, meaning:

$$\widehat{\pi(p^t(p^l; \hat{f}(\theta|x_i, \mathcal{I}_i), s_i))\Omega(p^t(p^l; \hat{f}(\theta|x_i, \mathcal{I}_i), s_i))} \geq \widehat{\pi(p^t(p^l; \hat{f}(\theta_0|\mathcal{I}_i), s_i))\Omega(p^t(p^l; \hat{f}(\theta_0|\mathcal{I}_i), s_i))} \tag{17}$$

If Equation 17 is true, then, combined with Equation 16 we see that

$$\frac{d\gamma(a; s_i, \mathcal{I}_i)}{da} \widehat{\pi(p^t(p^l; \hat{f}(\theta|x_i, \mathcal{I}_i), s_i))\Omega(p^t(p^l; \hat{f}(\theta|x_i, \mathcal{I}_i), s_i))} \geq k'(a; s_i, \mathcal{I}_i) \tag{18}$$

Then, it follows that $a^*(s_i, x_i, \mathcal{I}_i) \geq a(s_i, \mathcal{I}_i)$. In other words:

- Prediction 3.: Information intervention increases a if sellers' expectations about their *ex-post* returns from the platform are updated upward when they receive the price information signal.(Research question 1.3.)

C.6 Model predictions: spillovers and their mechanisms

The optimality conditions and predictions above are based on the assumption that exogenous information shocks via the experiment only affect individual choices. We also hypothesize that individuals' access to information and their choices may generate spillovers, happening through multiple mechanisms. In this sub-section, we discuss three possibilities: information spillovers, distribution of buyer attention, and congestion.

C.6.1 Information spillovers

The first possibility is that sellers' choices of p^l may generate changes to the quality of information signals available in the market, therefore affecting $\mathcal{I}_i$ for all seller i in the market segment. This point is captured in research question 2.1.1.. An exogenous shift in the information set available in a market segment would affect sellers' prior beliefs about the distribution of buyers' WTP. The choices of p^l and a made in a treated market segment would therefore be closer to those under no information friction than in an untreated market segment.

In other words, suppose that part of a market segment is exposed to the Price Calculator treatment, and their choices of p^l are closer to the values they would choose under no information friction. Define the resulting information set in this market segment to be a union of the existing information set and information contained in treated p^l's, i.e., $\mathcal{J}_i \equiv \mathcal{I}_i \cup I(\bigcup_{i \in T} p_i^l)$, where I is a function that maps a set of prices and T is a set of treated individuals. Then we get:

$$\pi(p^t(p^l; \hat{f}(\theta|\mathcal{J}_i), s_i))\Omega(p^t(p^l; \hat{f}(\theta|\mathcal{J}_i), s_i)) \geq \pi(p^t(p^l; \hat{f}(\theta_0|\mathcal{I}_i), s_i))\Omega(p^t(p^l; \hat{f}(\theta_0|\mathcal{I}_i), s_i)) \quad (19)$$

- Prediction 4.: Information spillovers from treated individual sellers in a given market segment would weakly improve the information set of all sellers in the market segment and would bring the prior beliefs about $f()$ and p^l closer to those under no information friction, increase *ex-post* returns, and would increase a if expected returns increase. (Research question 2.1.1.)

C.6.2 Congestion and the match rate

Lastly, the treatment may affect quantities of sellers and buyers actively participating in the market, affecting congestion and the speed at which sellers and buyers are matched. The match rate is expressed via a function $\gamma()$. The spillover effect of changes in congestion levels may depend on what types of sellers and buyers are taken out of the market as a result of the Price Calculator intervention. Complex assumptions about the resulting composition of sellers and buyers are outside the scope of this framework.

One simple scenario we explore is what would happen if the intervention relaxes congestion in the matching process overall and the match rate increases for all sellers. In other words, treated market segments would have $\tilde{\gamma}(a) \geq \gamma(a)$, $\forall a$. Then we get:

$$a^*|_{\tilde{\gamma}} \geq a^*|_{\gamma} \tag{20}$$

This is because the marginal benefit of advertising is now higher in treated market segments for a given a, while the cost function is unchanged. This would lead to further consumption of a to the point where marginal cost equals the benefit. This does not affect the choice of p^l, as it is a separate problem from the choice of a.

- Prediction 5.: A higher match rate as a result of reduced congestion in treated market segments results in higher consumption of advertising tools than in untreated market segments. (Research question 2.1.3.)

D Pre-specified research questions

We pre-specified the research questions in the pre-analysis plan, which was initially submitted and modified both prior to the beginning of the intervention. We divided our research questions into two; the first set of questions pertains to direct treatment effects of price information on listing prices, transaction outcomes, and mechanisms at the individual level. Our hypothesis is that the information intervention reduces noise in sellers' beliefs about the distribution of demand, affects their pricing decisions, and improves their market outcomes. We also posit that sellers do not make strategic choices beyond pricing, such as advertising, and those choices are contingent upon their pricing decisions and beliefs. We, therefore, hypothesize that contingent strategic choices like advertising could be affected by price information intervention.

The second set of questions concerns spillovers and other market-level impacts of the information intervention. Possible channels include a) diffusion of information itself via shifts in the distribution of listing prices, b) competing sellers' pricing and advertising choices to treated individuals' strategic choices, and c) reduction in search friction and congestion in the market. Our empirical objectives, therefore, are to identify spillover effects on our primary outcome variables and narrow down on channels of such spillovers.

Following is the list of primary (in bold) and secondary questions, with links to the theoretical predictions in Section 6.

1. Does the price information intervention induce direct effects on pricing, advertising, and transaction outcomes?
   - 1.1. **Do sellers adjust their listing prices toward the price signal they receive?** (Prediction 1.)
     - 1.1.1. Does the intervention affect sellers' stated beliefs about the distribution of transaction prices?
   - 1.2. **Does the price information intervention improve sellers' returns from the platform?** (Prediction 2.)
     - 1.2.1. **Does it increase page views?**
     - 1.2.2. **Does it increase the transaction probability?**
     - 1.2.3. **Does it affect the transaction price?**
   - 1.3. **Do sellers respond to the intervention by making strategic adjustments in advertising?** (Prediction 3.)
   - 1.4. Across what characteristics do we observe heterogeneous treatment effects?
     - sellers' experience
     - product heterogeneity in market clusters
     - availability and variation of price information at baseline

2. Does the price information intervention create spillovers and other knock-on effects?
    2.1. **Does the intervention induce spillovers in terms of listing prices, transaction outcomes, and the use of advertising?**
        2.1.1. Are these spillovers induced by changes in listing prices and advertising by competing, treated sellers? (Prediction 4.)
            2.1.1.1. Are there spillover effects on the stated belief about the distribution of transaction prices?
        2.1.2. Do spillovers occur through a zero-sum shift in buyer attention toward treated sellers?
        2.1.3. Do spillovers occur through changes in congestion? (Prediction 5.)

E Statistical Power

We make choices on the following dimensions to maximize the statistical power of detecting treatment, spillover, and saturation effects:

- shares of clusters assigned to control, high treatment, and medium treatment groups
- share of posts into treatment assignment for both high- and medium groups.

We take as given the cluster sizes, as it depends on a fixed experimental duration of 8 weeks. We also take as given the number of clusters, as it depends on the number of models PakWheels could offer Price Calculator estimates without risking providing noisy information to infrequently traded vehicle models.

We take a hybrid approach based on theoretical optimal design and Monte Carlo simulations. For the latter, we use real historical data with assumptions about the reduced-form structure and relative effect sizes between direct treatment, spillovers, and saturation. First, we set the share of control clusters to 0.5, and the rest split evenly between high- and medium-treatment groups, based on insight from Baird et al. (2018). Their setup and assumptions are similar to ours, such as that they allow for intracluster correlation and only partial interference (i.e., within clusters but not across). We deviate from the procedure by Baird et al. (2018) on our choices of saturation levels. We assign second-stage randomization based on the last digit of the sellers' user ID, and we expect some level of treatment non-compliance as discussed in Section 3.3. As such, we have chosen the high treatment assignment to be 9 out of 10 digits and middle treatment 5 out of 10. With a conservative assumption on treatment take-up of about 70 percent, then treatment intensities would be symmetrical around 0.5, as recommended by Baird et al. (2018).

Based on the saturation levels and the range of control group size chosen by the process above, we run Monte Carlo simulations to estimate power under several assumptions. We use actual data from PakWheels and estimate the statistical power of detecting a range of effect sizes for direct impact, spillovers, and saturation. We use different data samples and specifications for direct and spillover effects, as described in Section 4.1. We bootstrap-sample the data 100 times, stratified over the make-model. We then assign treatment according to the method described in Section 3.2, and construct outcome variables conditional on cluster and individual assignments into treatment. We assume that direct and spillover treatment effects are linear and additive, except for the transaction outcome. [8] Spillovers are assumed to occur within the make-model cluster evenly for both treated and untreated posts. Using real historical data, we assume that intra-cluster correlation is already built in. We assume no inter-cluster interference.

The outcome variables, which are standardized and identical to the primary outcomes described in Section 4, are the following:

- log(absolute difference between listing price and Price Calculator estimate)
- 1 if reported as sold
- log(self-reported transaction price)
- advertising index[9]
- buyer-attention index.

We estimate the power of detecting the intend-to-treat (ITT) effects of direct treatment and spillovers for a range of relatively small effect sizes (0.025 to 0.2 standard deviation). We explore two scenarios of spillover and saturation effect sizes relative to direct treatment effects:

1. spillover effect in high-saturation is 50% of the direct treatment effect, and in medium saturation 25%.
2. spillover effect in high-saturation is 100% of the direct treatment effect, and in medium saturation 50%.

We identify the optimal division of clusters into treatment arms based on the following proposition by Baird et al. (2018):

$$\psi^* = \frac{-\kappa + \sqrt{\kappa^2 + (1-\rho)\kappa}}{1-\rho} \tag{21}$$

[8] Given that the transaction outcome is binary, we assumed that assignment into treatment would increase the probability of transaction by X%, where X is a standardized effect size based on the standard deviation of the binary variable.

[9] There is a minor difference in definitions of constituent variables due to limitations in the pre-intervention data

where ψ is share of control clusters, $\kappa \equiv 1 + (n-1)$, ρ the intracluster correlation, and n cluster size. The boundary values of ψ^* are $\sqrt{2} - 1$ and 0.5. Plugging in our parameter values to Equation 21 resulted in a control share close to 0.5.

We use the identical estimating equations to estimate intent-to-treat effects as in the main analysis in Section 4.2. In other words, we run the *logit* model for the binary outcome and linear regressions for all other outcomes. These models include the same set of controls as the ones used for the primary analysis. We use data from an 8-week period that approximates the actual experimental timing. We also present results that include data from 8 weeks prior in addition to data from the experimental period. This is to gauge how much power gains we could make in detecting spillovers by a larger sample and with cluster fixed effects, as described in Section **??**. In both approaches, we report the false-discovery-rate-adjusted q-values based on five p-values corresponding to the main outcomes. These adjustments are made separately for direct treatment, spillover, and high-saturation effects.

E.1 Results of power calculations

The results of power simulations from specifications containing only data from the 8-week experimental period are shown in Figures E.3 and E.4, corresponding to scenarios 1. and 2., respectively. These figures reveal that the power to detect direct treatment effects of 0.05 SD is 80% or greater for all five primary outcomes. The effect size of 0.05 SD translates into 11,594 PKR (65.73 USD at 176.4 PKR to USD) in an absolute difference between the listing price and Price Calculator estimate (level mean: 305,434 PKR), 2.44 percentage-points in transaction probability (mean: 0.394), and 55,473 PKR (314.47 USD) in transaction price (level mean: 1,893,626 PKR).

Figures E.3 and E.4 also show that we are able to detect some spillover and saturation effects at 80% power or greater with the specification for primary analysis, depending on the effect sizes and assumptions about their relative sizes to direct effects. Figure E.3 suggests that under assumption 1. we would have greater than 80% power to detect a spillover effect of 0.05 SD on advertisement, as well as saturation effects of 0.1 SD on advertisement and demand. Figure E.4 suggests that under assumption 2., we would have greater than 80% power to detect spillover effects of 0.1 SD on transaction, demand, and advertisement, and saturation effect of 0.2 SD on all outcomes. Figures E.5 and E.6 also show that using the two-way fixed-effect specification from secondary analysis in Section **??** would improve power on some of the spillover outcomes, as compared to Figures E.3 and E.4, respectively.

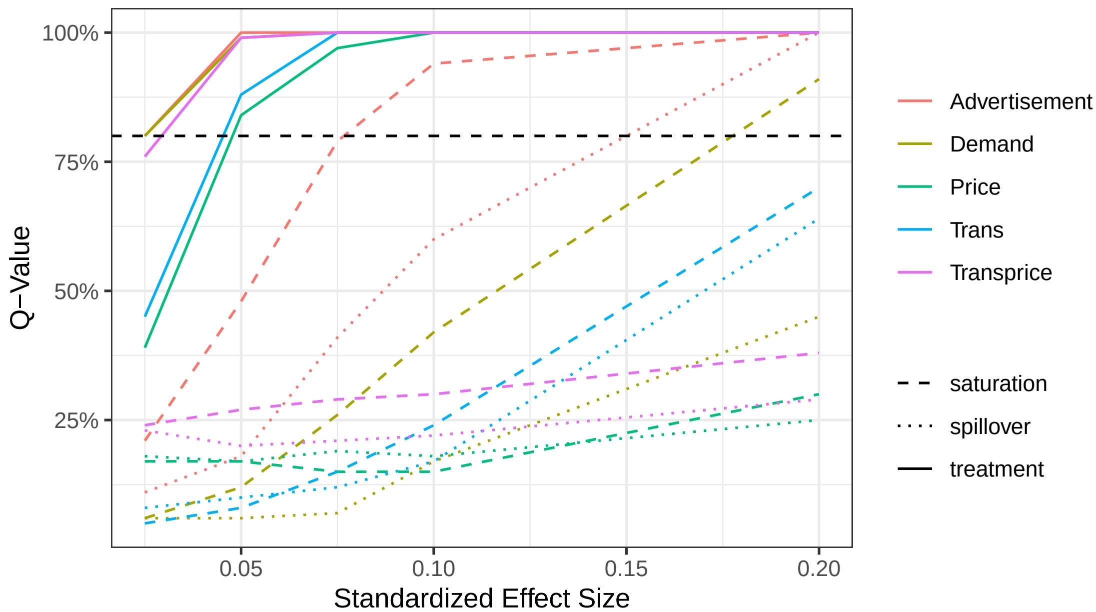

Figure E.3: Power estimates: Scenario 1. and data over 8 weeks

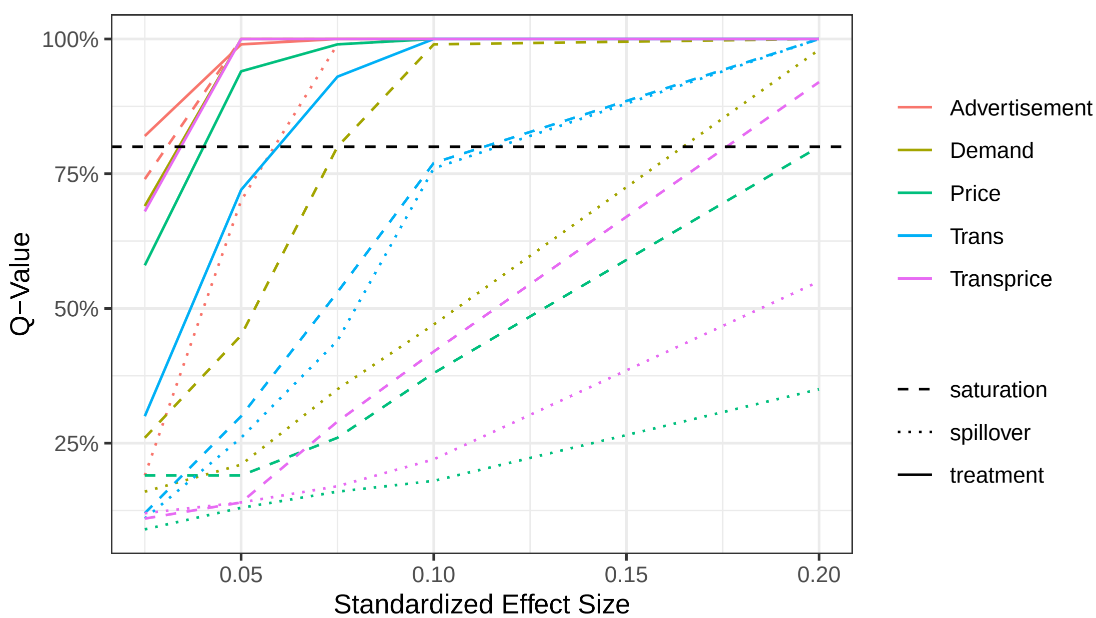

Figure E.4: Power estimates: Scenario 2. and data over 8 weeks

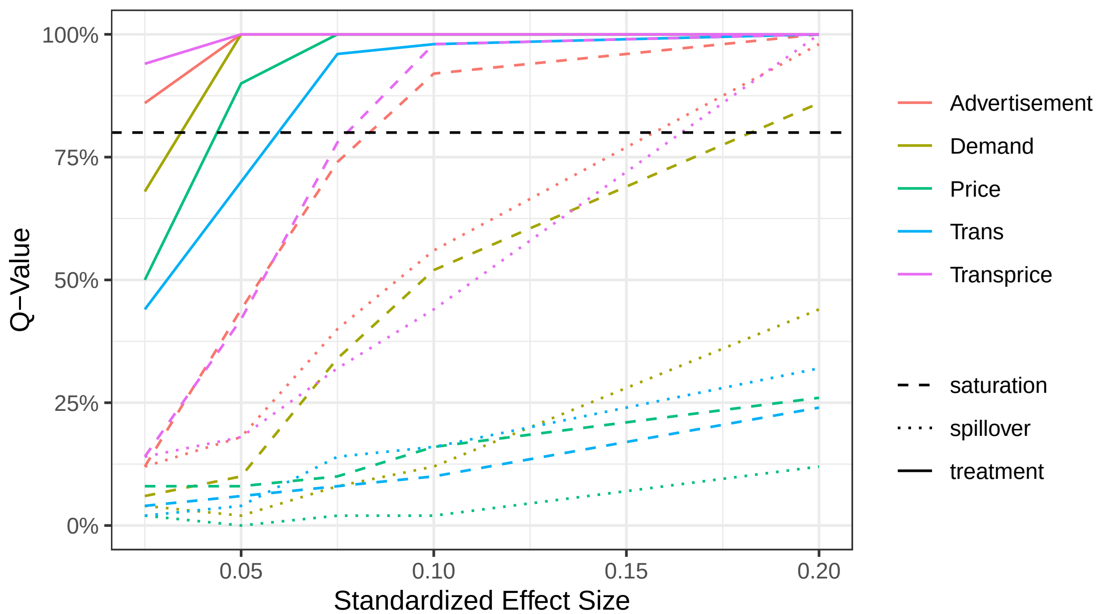

Figure E.5: Power estimates: Scenario 1. and data over 16 weeks

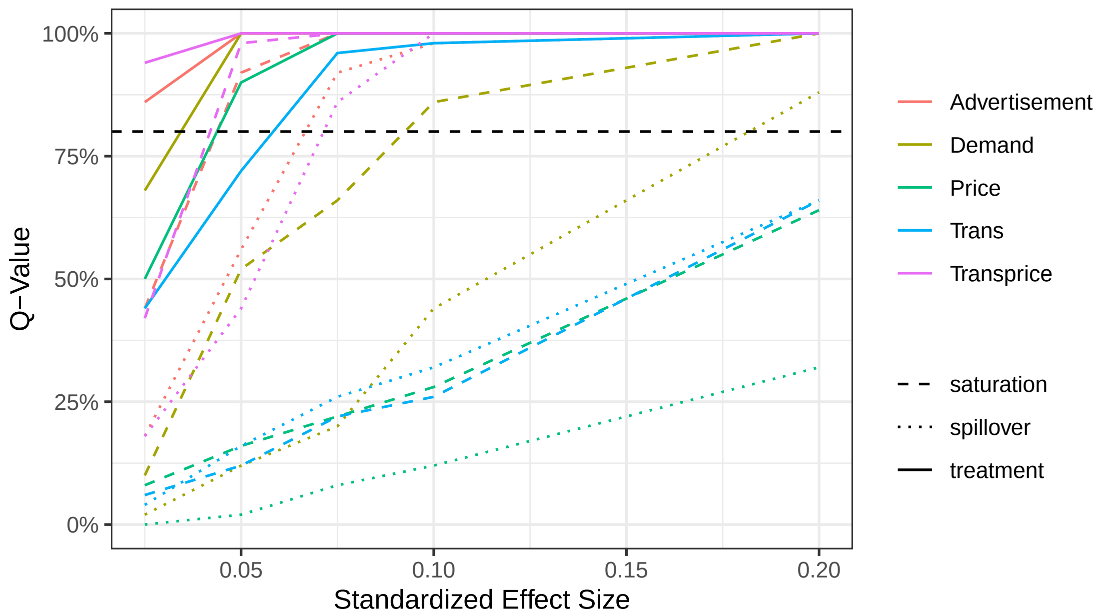

Figure E.6: Power estimates: Scenario 2. and data over 16 weeks

F Additional Tables

Table F.1: **ITT estimates on price-related outcomes**

	log(Listing price)	Price updated	N. price updates	log(Abs. price change)
	(1)	(2)	(3)	(4)
	OLS	Logit	OLS	OLS
Assignment	-0.0005	-0.0144	0.0092	-0.0585
	(0.0014)	(0.0194)	(0.0096)	(0.0511)
Spillover	-0.0211	-0.0230	-0.0401**	-0.0451
	(0.0256)	(0.0222)	(0.0166)	(0.0505)
Spillover (high)	-0.0034	0.0587***	0.0361*	0.1356***
	(0.0418)	(0.0226)	(0.0194)	(0.0488)
Observations	117,891	117,891	117,891	117,891
Squared Correlation	0.93107	0.02790	0.03762	0.03023
Pseudo R^2	1.2508	0.02253	0.01069	0.00500
BIC	-61,139.6	144,326.4	420,509.3	721,911.2

Notes: OLS estimates. "log(Listing price)": log of the final listing price. "Price updated": a binary outcome that is 1 if the seller ever updates the listing price. "N. price updates": number of times the seller updates the listing price. "log(Abs. price change)": log of the absolute difference between the initial and final listing price. The specification is as shown in Equation 1. Standard errors are reported in parentheses and clustered at the make-model level. The stars show the two-tailed significance in p-values: p<0.1*; p<0.05**; p<0.01***.

Table F.2: **ITT estimates on transaction-related outcomes**

	1 if sold	log(Transaction price)	log(Seller revenue)
	(1)	(2)	(3)
Assignment	-0.0110***	-0.0008	-0.0457
	(0.0038)	(0.0037)	(0.0363)
Spillover	0.0105***	-0.0401	0.0185
	(0.0030)	(0.0335)	(0.0407)
Spillover (high)	-0.0033	-0.0031	0.0307
	(0.0071)	(0.0457)	(0.0367)
Observations	111,312	14,084	117,891
R^2	0.01478	0.92874	0.01112
Within R^2	0.00465	0.75562	0.00343

Notes: OLS estimates. "1 if sold": a binary outcome that is 1 if the seller reports the car as sold. "log(Transaction price)": log of the self-reported transaction price. "log(Seller revenue)": log of the self-reported transaction price if sold, and 0 otherwise. The specification is as shown in Equation 1. Standard errors are reported in parentheses and clustered at the make-model level. The stars show the two-tailed significance in p-values: p<0.1*; p<0.05**; p<0.01***.

Table F.3: **ITT estimates on variables included in the page-view index outcome**

	Page views	Phone number views	Page-view index (not winsorized)	Page-view index
	(1)	(2)	(3)	(4)
Assignment	-59.91***	-0.4050**	-0.0335***	-0.0172***
	(12.37)	(0.2041)	(0.0070)	(0.0039)
Spillover	57.39***	1.151***	0.0499***	0.0332***
	(18.23)	(0.3713)	(0.0145)	(0.0106)
Spillover (high)	8.431	-0.3183	-0.0054	-0.0092
	(25.02)	(0.7047)	(0.0245)	(0.0169)
Observations	117,891	117,891	117,891	117,891
R^2	0.10683	0.06462	0.09405	0.12322
Within R^2	0.01673	0.00938	0.01082	0.01652

Notes: OLS estimates. "Page views': The number of times the individual listing page is viewed. "Phone number views": The number of times the button in the individual listing page that shows the seller's phone number is clicked. "Page-view index (not winsorized)": The standardized index consisting of page views and phone number views, non-winsorized. "Page-view index": The standardized index consisting of page views and phone number views, winsorized. The specification is as shown in Equation 1. Standard errors are reported in parentheses and clustered at the make-model level. The stars show the two-tailed significance in p-values: p<0.1*; p<0.05**; p<0.01***.

Table F.4: **ITT estimates on variables included in the advertising index outcome**

	N. bumps	1 if featured	1 if certified	Advertising index (not winsorized)	Advertising index
	(1)	(2)	(3)	(4)	(5)
	OLS	Logit	Logit	OLS	OLS
Assignment	-0.0456***	-0.0948**	-1.182***	-0.0578***	-0.0095***
	(0.0153)	(0.0398)	(0.1601)	(0.0108)	(0.0025)
Spillover	0.0146	-0.0525	0.0984	0.0165	-0.0005
	(0.0159)	(0.0488)	(0.0933)	(0.0108)	(0.0042)
Spillover (high)	0.0472***	0.0638	0.7654***	0.0415***	0.0062
	(0.0174)	(0.0618)	(0.1381)	(0.0136)	(0.0059)
Observations	117,891	116,346	91,159	117,891	117,891
Squared Correlation	0.07426	0.29549	0.11528	0.21337	0.29329
Pseudo R^2	0.02057	0.29896	0.26361	0.08105	0.24067
BIC	435,153.9	50,519.6	7,744.6	322,880.4	131,198.5

Notes: OLS estimates. "N. bumps": the number of times the seller applies the "bump" tool on the listing. "1 if featured": a binary outcome that is 1 if the seller ever features the listing. "1 if certified": a binary outcome that is 1 if the car in the listing is certified. "Advertising index (not winsorized)": The standardized index consisting of N. bumps, 1 if featured, and 1 if certified, non-winsorized. "Advertising index": The standardized index consisting of N. bumps, 1 if featured, and 1 if certified, winsorized. The specification is as shown in Equation 1. Standard errors are reported in parentheses and clustered at the make-model level. The stars show the two-tailed significance in p-values: p<0.1*; p<0.05**; p<0.01***.

G Endline telephone survey questions

begin group	section_1	Background	
select_one yn_noad	s1_q1	Our records show that you recently listed [make] [model] [model year] in [city location] on PakWheels. Have you already sold this vehicle you posted?	کیا آپ نے اپنی گاڑی بیچی ہے. ہمارے ریکارڈز کے مطابق آپ نے ${make_b}, ماڈل ${model_b} سال ${year_b} شہر ${city_b} کا اشتہار لگایا تھا . کیا آپ نے جس گاڑی کا اشتہار پاک ویلز پر لگایا تھا وہ بیچ دی ہے؟
integer	s1_q2	What was the price you sold this car at? We would like to remind you again that your answers will stay anonymous and be used for research purposes only	اگر گاڑی بیچ دی ہے تو اسکی قیمت کیا تھی. آپ نے کس قیمت پر گاڑی بیچی ؟ ہم آپکو دوبارہ یاد کروا دیں کے آپ کے جوابات کو گم نام رکھا جائے گا اور صرف تحقیقی مقاصد کے لیے استعمال کیا جائے گا.
integer	s1_q3	What was the "expected" price? At what price did you expect to sell this car at, when you initially posted it on PakWheels?	جب اپنے ابتدائی طور پر اِس گاڑی کا اشتہار پاک ویلز پر لگایا تھا تو آپ کو کیا تواقع تھی کے یہ گاڑی کتنے کی بک جائے گی ؟
integer	s1_q4	What is realistically the highest price you could have gotten for your car?	آپ کے خیال میں، آپ کی گاڑی زیادہ سے زیادہ کتنی قیمت پر بیچی جا سکتی تھی؟
integer	s1_q4a	What is realistically the lowest price you could have gotten for your car?	آپ کے خیال میں، آپ کی گاڑی کم سے کم کس قیمت پر بیچی جا سکتی تھی؟
integer	s1_q5	How much did you pay for this vehicle when you first bought it?	آپ نے یہ گاڑی کتنی قیمت پر خریدی تھی؟
select_multiple reason_sell	s1_q6	Why have you not sold the car? (Allow the respondent to elaborate and ask follow-up questions to determine which of the following apply. You can choose more than one options.)	آپ نے گاڑی کیوں نہیں بیچی ؟
text	s1_q6_o	Please specify other	دیگر کی وضاحت کریں
select_one reasons_who	s1_q7	How and to whom did you sell the car? (Allow the respondent to elaborate and ask follow-up questions to determine which of the following apply.)	آپ نے یہ گاڑی کس کو اور کس طرح بیچی؟
integer	s1_q8	In the past 12 months, how many cars did you try to sell in total, not just on PakWheels?	پچھلے 12 مہینوں میں ، آپ نے مجموعی طور پر کتنی گاڑیاں فروخت کرنے کی کوشش کی؟ کل گاڑیاں ,صرف وہ نہیں جن کا اشتیار آپ نے پاک ویلز پہ لگایا ہو
end group	section_1		
begin group	section_2	Price Calculator	
calculate	treat_2nd		کچھ لوگوں کو گاڑی کا اشتہار پاک ویلز کی ویب سائٹ پر بناتے وقت ایک پوپ اپ یا گرافک باکس دکھایا گیا تھا ، جس میں نئے پرائس کیلکو لیٹر کی مدد سے
select_one yesno_dk	s2_q1	When creating the post for a car (in the "Post an Ad" process), a random subset of people were shown a pop-up or graphic box containing price estimate from the new Price Calculator, along with higher and lower end estimates. If you were selected, you would have seen this when you first selected your listing price while creating the post. Do you remember seeing this particular Price Calculator estimate?	کچھ لوگوں کو گاڑی کا اشتہار پاک ویلز کی ویب سائٹ پر بناتے وقت ایک پوپ اپ یا گرافک باکس دکھایا گیا تھا ، جس میں نئے پرائس کیلکو لیٹر کی مدد سے گاڑی کی اندازاً قیمت اور گاڑی کی کم سے کم اور زیادہ سے زیادہ اندازاً قیمت دی گئی تھی۔ اگر آپ ان کچھ لوگوں میں شامل ہیں توآپ نے یہ پرائس کیلکولیٹر سب سے پہلے تب دیکھا ہوگا جب آپ پوسٹ لکھتے وقت لسٹ پرئس سیلیکٹ کر رہے ہوں گے۔ کیا آپ کو یہ پرائس کیلکو لیٹر کی مدد سے لگائی گئی گاڑی کی یہ اندازاً قیمت یاد ہے؟
integer	s2_q2	What was the estimate you were given?	اگر آپکو پرائس کیلکو لیٹر دیکھنا یاد ہے ، آپکو گاڑی کی اندازاً قیمت کیا دی گئی تھی ؟
select_one enum_note	s2_q2_e	Did the respondent give you estimates from the Price Calculator provided to them on the sell-form (the "Post an Ad" process) from the intervention? Or did they give you something else?	کیا جواب دھندہ نے پرائس کیلکو لیٹر پوسٹ این ایڈ پروسیس والے کے مطابق جواب دیا یا کچھ اور جواب دیا ؟
select_one too_hl	s2_q3	Did you think that this Price Calculator box during the "Post an Ad" process gave you a reasonable estimate of transaction price? Or was it too low, or high?	آپ کے خیال سے پرائس کیلکو لیٹر پوسٹ این ایڈ پروسیس والے کے ذریعے جو گاڑی کی قیمت پاک ویلز کی جانب سے بتائی گئی وہ مناسب تھی یا بہت زیادہ تھی یا بہت کم تھی ؟
select_one yesno_dk	s2_q5	Do you know of any other sellers who have gotten the Price Calculator estimates from PakWheels?	کیا آپ کوئی اور گاڑی فروخت کرنے والوں کو جانتے ہیں جن کو پاک ویلز استعمال کرتے وقت پرائس کیلکو لیٹر کی مدد سے لگائی گئی اندازاً قیمت ملی ہو ؟
end group	section_2		
begin group	section3		

select_one yesno_dk	s3_q1	Now I would like to ask you a few questions about searching for similar posts and the choice of listing price. Did you searched for, or looked at other posts that are similar to your vehicle on PakWheels?	اب میں آپ سے کچھ سوال آپ کی گاڑی کی پوسٹ سے ملتی جُلتی پوسٹ اور لسٹ پرائس کے بارے میں پوچھنا چاہتا ہوں. کیا آپ نے اپنی گاڑی سے ملتے جلتے دوسرے اشتہار پاک ویلز پر سرچ کیے ؟
select_one search	s3_q2	Did you search for posts only for ${model_b}, or did you also search for other models?	کیا آپ نے صرف ${model_b} پوسٹس کی تلاش کی یا دوسرے ماڈلوں کی بھی تلاش کی؟
text	s3_q2_o	Please specify which models	آپ نے کون سے ماڈل کی سرچ کی ؟
select_multiple search_m	s3_q3	Did you restrict your search by any other terms? For example, your model year, version, or your city?	آپ نے اپنی سرچ کو درج زیل میں سے کن چیزوں پر محدود کیا ؟ جیسے کے ماڈل کا سال ، ماڈل ورژن یا آپکا شہر وغیرہ
text	s3_q3_o	Please specify other	دیگر کی وضاحت کریں
select_multiple search_op	s3_q4	Besides other listings from PakWheels, what other information or experience did you base your initial listing price on?	پاک ویلز پر دوسری پوسٹس دیکھنے کے علاوہ ، اور کون سی معلومات یا تجربہ کی بنیاد پر آپ نے اپنی گاڑی کی ابتدائی قیمت کو مقرر کیا؟
text	s3_q4_other	Please explain this	پاک ویلز کی دی ہوئی قیمت کے علاوہ کوئی دیگر معلومات کی وضاحت کریں
text	s3_q4_o	Please specify other	دیگر کی وضاحت کریں
select_one yesno_dk	s3_q5	Have you changed your listing price on PakWheels after you have set it initially	کیا آپ نے اپنی گاڑی کی جو شروع میں قیمت پاک ویلز پر مقرر کی تھی اُسے بعد میں تبدیل کیا.
select_multiple price_adj	s3_q6	Could you tell us why you did so?	آپ نے اپنی گاڑی کی ایک قیمت مقرر کی تھی، لیکن بعد میں اِس قیمت کو تبدیل کر دیا۔ کیا آپ ایسا کرنے کی وجہ بیان کر سکتے ہیں؟
text	s3_q6_o	Please specify other	دیگر کی وضاحت کریں
end group	section3		
begin group	section4		
integer	s4_q1	How much (in PKR) were you willing to bargain from the listing price you chose when you created the listing?	آپ اپنے اشتہار میں مقرر کردہ قیمت میں کتنی کم و پیشی کرنے پر رضامند تھے؟
begin group	s4_preamble	Please tell us if you agre with the following statements	مندرجہ ذیل بیانات سے آپ کتنا متفق یا غیرمتفق ہیں؟ بلکل متفق سے بلکل غیر متفق کے پیمانے پر جواب دیں.
select_one likert_agreedk	s4_q2	It was difficult to get enough inquiries for your post on PakWheels	آپنی پوسٹ سے متعلق مطلوبہ انکوائریز پاک ویلز پر حاصل کرنا مشکل تھا
select_one likert_agreedk	s4_q3	It was difficult to get a price offer that you would accept for your car on PakWheels.	آپنی پوسٹ سے متعلق، قابل قبول قیمت پاک ویلز پر حاصل کرنا مشکل تھا
select_one likert_agreedk	s4_q5	Most (around 3 out of 4 or more) of potential buyers of ${make_b} ${model_b} have good information about what are fair used car prices.	گاڑی ${model_b} {make_b} خریدنے والے زیادہ تر (چار میں سے تِین) افراد کو مناسب قیمتوں کا اندازہ ہوتا ہے۔
select_one likert_agreedk	s4_q6	Most (around 3 out of 4 or more) of other sellers of ${make_b} ${model_b} have good information about what are fair used car prices.	گاڑی ${model_b} {make_b} بیچنے والے زیادہ تر (چار میں سے تِین) افراد کو مناسب قیمتوں کا اندازہ ہوتا ہے۔
end group	s4_preamble		
begin group	s4a_preamble	Please tell us if you agre with the following statements	مندرجہ ذیل بیانات سے آپ کتنا متفق یا غیرمتفق ہیں؟ بلکل متفق سے بلکل غیر متفق کے پیمانے پر جواب دیں.
select_one helpful_scale	s4_q7	As you may know, sellers like you can feature your ad, use "bumps" to get your post to be more visible, or requested for vehicle inspections by PakWheels. In a scale of 1 to 5, 5 being the most useful, how useful are these features, bumps, and inspections to get people to buy your car at a higher price?	جیسا کہ آپ جانتے ہیں، جو لوگ آپ کی طرح پاک ویلز کی ویبسائٹ پر اپنی گاڑی کو بیجنے کےلئے اشتہارلگاتے ہیں وہ اپنے اشتہار کو فیچر کر سکتے ہیں یا مشہور بنانے کے لئے "بمپس" کا استعمال کرسکتے ہیں یا پاک ویلز کی طرف سے اپنی گاڑی کی انسپیکشن کرواسکتے ہیں۔ ایک سے پانچ کے پیمانے پر بتاین کے ان سہولیات کا استعمال آپ کو اپنی گاڑی کو زیادہ قیمت پر بیچنے میں کتنا مدد گار ثابت ہو سکتا ہے ؟ ایک کا مطلب ہے بلکل مدد کار نہیں اور پانچ کا مطلب ہے بہت زیادہ مدد کار
select_one helpful_scale	s4_q8	Again in a scale of 1 to 5, how useful are these features, bumps and inspections to increase the chance that it sells, or sells faster?	ایک سے پانچ کے پیمانے پر بتاین کے آپ کی گاڑی کے بکنے کے امکانات میں اضافہ کرنے میں بمپز اور فیچر کا استعال کتنا مدد گار ثابت ہو سکتا ہے ؟ ایک کا مطلب ہے بلکل مدد کار نہیں اور پانچ کا مطلب ہے بہت زیادہ مدد کار
select_one yesno_dk	s4_q9	The Price Calculator is currently provided for free. But in the future if it were offered for 100 rupees per post, would you be willing to pay for it?	فل حال پرائس کیلکولیٹر فری ہے، لیکن اگر مستقبل میں اس کی قیمت ایک سو روپے فی پوسٹ مقرر کردی جائے تو کیا آپ اس کے استعمال کے لئے پیسے دیں گیں؟
integer	s4_q10	What is the maximum amount (PKR) that you would be willing to pay for the Price Calculator, per post?	پرائس کیلکولیٹر کے استعال کے لئے آپ زیادہ سے زیادہ کتنی قیمت فی پوسٹ ادا کرنا چاہتے ہیں؟
end group	s4a_preamble		
end group	section4		