Repairing the Damage: The Effect of Price Expectations on Auto-Repair Price Quotes*

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Abstract

We show that price expectations alter outcomes in a negotiated price environment. By experimentally manipulating the price expectations that consumers communicate to firms, we show that consumers' price expectations alter outcomes by directly changing firms' behavior. We implement a large-scale field experiment in which callers request price quotes from automotive repair shops. We find that repair shops quote higher prices if they know that callers' perception of the market price is high. We find that women are quoted higher prices than men when callers signal that they are uninformed about market prices. However, gender differences disappear when callers mention an expected price for the repair. Finally, we find that repair shops are more likely to offer a price concession if asked to do so by a woman than a man.

1 Introduction

In an environment in which prices customarily are negotiated, it can be difficult and time-consuming for consumers to gather enough information to create a good estimate of the market price. This phenomenon is common to many industries: buying a car entails lengthy negotiations in which dealers want to know about a consumer's current vehicle, her financing preferences, the available funds for a down-payment, and the consumer's urgency of purchase. Getting price quotes on home repairs involves meeting multiple contractors at home and sharing preferences. Similarly, it is costly to learn about market prices for mattresses, houses, and car repairs, among others. This is because firms frequently want consumers to share some information before engaging in price negotiation and also because informative prices are only revealed after costly negotiations.

By lowering the cost for consumers to gather market information and for firms to disseminate it, the Internet has made it much easier for consumers to form market-based price expectations. For example, Redfin.com and Trullia.com allow consumers access to historical and current pricing information for real estate listings. TrueCar.com and Edmunds.com provide consumers with distributions of vehicle market prices obtained in recent transactions. AutoMD.com and Repairpal.com inform consumers about the cost of car repair. Better access to price information seems to have helped consumers. For example, Brown and Goolsbee (2002) have shown that the Internet has lowered the price of term life insurance by 8% to 15%. Zettelmeyer, Scott Morton, and Silva-Risso (2006) show that new car buyers who use the Internet pay on average 1.5% lower transaction prices than consumers who do not. Finally, Busse, Silva-Risso, and Zettelmeyer (2006) show that knowledge of manufacture rebates on automobiles allows consumers to appropriate a larger share of the rebates.

What is less clear from prior work are the precise mechanisms by which consumers obtain lower prices. Within a negotiated price environment, price expectations can affect outcomes in (at least) two ways. First, consumer price expectations may alter the optimal stopping rule of consumers; a consumer will continue soliciting price quotes among sellers as long as she determines that the prices she has been offered so far are high relative to her expected price. Second, consumer price expectations may directly alter firm behavior by conveying information about consumer types, or by serving as a reference price in negotiations.

In this paper we show that the benefit of market-based price expectations extends beyond helping consumers find low-priced firms.[1] We show that the price a consumer expects to pay can alter his or her negotiation with individual firms directly by changing the price offers made by sellers.

[1] By "market-based price expectation" we mean an estimate of the market price that is based on information about actual market transactions, as opposed to a consumer's subjective information, or her sense of what would be a "fair price," or inferences based on the market prices of other goods or services.

Our empirical approach for demonstrating this is the following: We design an experiment in which callers obtain price quotes from automotive repair shops. By experimentally varying whether and how callers use price expectations while obtaining price quotes, our experimental price outcomes are all driven by how firms respond to callers' price expectations, not by unobserved differences between consumers.

In our experiment we implement price expectations as an "expected price," by which we mean a consumer's estimate of the market price for a repair. Since the price for a given repair may well vary across repair shops, the "market price" can be thought of as an average or median price.[2] We can think of three ways in which an expected price may affect the price quote obtained by a consumer. First, communicating an expected price provides a signal about the consumer's price knowledge. This may be a positive or negative signal depending on whether the expected price accurately reflects market conditions. Of course, not communicating an expected price may also be a signal. Moreover, this signal may differ depending on the prior a firm has about the price knowledge of a particular consumer type (for example, males versus females). Second, communicating an expected price may provide an anchor for the initial price quoted by a firm. An extensive body of evidence on anchoring and adjustment suggests that anchors may affect outcomes (see Furnham and Boo (2011) for a comprehensive review). Note that by serving as an anchor, an expected price could lead to a price quote that is higher or lower than the quote the shop would have given in the absence of an anchor.[3] Third, an expected price may serve as a readily available reference in negotiations. For example, if a consumer asks a shop to match her expected price, the shop may infer that the expected price is what the consumer thinks is her outside option.

We perform two field experiments. In our first and main experiment we test the effect of mentioning an expected price on firm behavior by comparing price quotes obtained by callers who don't refer to any particular price (the "no expected price" condition) to those given to callers who mention that they have learned that the requested repair should cost $365 (the "market-based expected price," i.e., the approximate market average for the requested repair) or $510 (the "upward-biased expected price," i.e., a price that exceeds the market average price for the requested repair). Since demographic differences have been known to affect outcomes in price negotiations, we vary the gender of callers. We anticipate that firms may assign different priors to the price knowledge of male and female callers.

We find that price expectations affect firms' price quotes. When callers don't communicate an

[2] In other contexts, a modal price could be thought of as the "market price."

[3] An anchor is any kind of numerical cue or trigger, including numbers that are introduced into an experimental setting, that have no connection to the content of the experiment (for example, the day of the month on which the subject was born, or the last two digits of the subject's phone number). Experimental results have shown that people are influenced by anchors, even when there is no logical reason for the anchor to be relevant for the experimental task. A reference price is an anchor that is suggested as the price of something within the experimental setting.

expected price, the price quote is, on average, no different than when they communicate market-based expected prices. However, repair shops quote significantly higher prices to callers who mention upward-biased expected prices than to those who communicate market-based or no expected prices.

Female callers are quoted higher prices on average. Moreover, this effect is driven by differences in the "no expected price" condition. Women who do not mention any expected price are quoted significantly higher prices than men in the same condition or women who mention market-based price expectations. However, once callers refer to a price, gender differences disappear. We find some evidence that men do better in the no expected price condition than they do in the market-based expected price condition; we find the opposite effect for women.

To test the effect of an expected price as a readily available reference in negotiations, we instructed callers to ask the repair shop for a price concession if the shop's initial price quote exceeded the expected price used in the relevant experimental condition. In the condition where callers did not mention any expected price, callers requested that the market-based price expectation ($365) be matched. We find that asking for a price concession leads to a 13% average reduction relative to the quoted price, regardless of whether callers had previously mentioned an expected price. This average price reduction notwithstanding, only 26% of requests led to a price concession of any amount. We find that women are more likely by 10 percentage points than men to obtain a price concession at all. However, conditional on obtaining a price concession, the magnitude of the price concession does not vary by gender.

When we combine our findings on initial price quotes and price concessions, our results suggest that women are best off mentioning market-based price expectations upfront. This lowers the initial quote while not negatively affecting any subsequent match concession. In contrast, our results suggest that men are better off inserting their price expectation into the negotiation late: mentioning a market-based expected price upfront may increase the initial price quote,[4] but does not increase the benefit of using it later to ask for a price concession.

After we finished collecting most of the data for the first experiment, we conducted a second experiment to see whether a "downward-biased" expected price condition (an expected price of $310) would lead to a lower price than the no expected price condition for both men and women. We did not find that downward-biased price expectations changed prices relative to no price expectations (after pooling the data across both experiments), relative to market-based price expectations.

As described above, the key contribution of our paper is its focus on the role that consumer (buyer) price expectations have on altering firm (seller) behavior. We can summarize what we

[4] We find this result in our main estimates, but the result does not hold up throughout all of our robustness checks.

believe our results indicate about the behavior of repair shops with regards to price expectations as follows. First, shops appear to respond to the available information about how well price-informed the caller is. When the caller does not provide any information about his or her price expectation, the shop must rely on its priors. If we group all callers together, it appears that shops assume that callers know the market average price. However, if we split our analysis by the callers' genders, it appears that the shops have different priors about different genders, and that they infer that women will accept a price that is higher than the market average and that men will only accept a price below it. However, once a caller provides information about his or her price expectation, the shops treat men and women the same, whether the information is that the caller's expectation is too high, too low, or correct. In other words, information about price expectations supersedes for the shops any information they might infer from the caller's gender. In this sense, our results imply that our estimated gender effects are statistical discrimination rather than animus: shops don't quote women higher prices no matter what—they just believe that women expect higher prices than men and respond accordingly. Finally, we find that women are more likely than men to be offered a price concession if they ask for one. We discuss possible reasons for this later in the paper in conjunction with the results themselves.

A key implication of our study is that consumers can use price expectations strategically to affect price negotiations. Having an expected price in mind enables consumers to use bargaining strategies that they might otherwise not use, such as mentioning the expected price or negotiating for a price concession, which in turn may lead firms to offer different prices from what they would have offered otherwise. As we have shown, the extent to which expected prices help consumers depends on whether the expected prices accurately reflect market prices, how they are used, and what consumer type uses them.

Our paper proceeds as follows. In Section 2 we describe the connections between this paper and related research. In Section 3 we describe the experiment design and summarize the data. In Section 4 we describe economic models of consumer price search, and the predictions suggested by such models for the outcomes of our experiments. In Section 5 we present the findings from our experiments. Section 6 explores the robustness of our findings. We conclude in Section 7.

2 Related literature

Our paper connects to three literatures: negotiation and bargaining, reference prices, and gender and race discrimination. We discuss the connection between our paper and each of these literatures in turn.

There is a vast literature on negotiation. Tsay and Bazerman (2009) provide a review that emphasizes factors that lead to negotiated outcomes deviating from rational negotiation outcomes. There are a variety of such factors, including negotiators' social relationships, a negotiator behaving in an egocentric manner, negotiators' illusions about their own negotiation skills, negotiators' emotions, and their reliance on intuition. The section of this literature that most closely relates to our paper is that which examines the effect of information on negotiation outcomes. Stigler (1961) asserted the importance, in general, of price information that buyers are able to obtain. The effect of information on bargaining outcomes specifically is studied by Valley, Blount White, Neal, and Bazerman (1992), who find that, in a setting in which agents serve as information brokers between buyers and sellers, settlement prices are higher when the agent knows the buyer's maximum reservation price, and lower when the agent knows the seller's minimum reservation price.

Our study is unusual in this literature in that the experimental manipulation is not to vary the information setting, and then evaluate the equilibrium bargaining outcome that results from the two parties interacting. Instead, our experiment focuses on only a part of the negotiation process: namely, how the seller's offer differs depending on how informed about prices the buyer appears to be. We do this by having buyers operate from a script that keeps the buyers' side of the negotiation fixed across information conditions, leaving sellers' responses to the buyers' price inquiry as the only factor that varies across the information conditions.

The second literature to which our paper relates is reference prices. (See Biswas, Wilson, and Licata (1993), Kalyanaram and Winer (1995), and Mazumdar, Raj, and Sinha (2005) for reviews.) A large part of this literature is devoted to understanding how customers form reservation prices. In our experiment, the experimental design specifies reservation prices for our callers to present as their own, which means that this is not the part of the reference price literature that is relevant for our paper. Our paper is more closely related to a literature that studies the effect of reservation prices on negotiations, such as Galinsky and Mussweiler (2001), Van Poucke and Buelens (2002), and Moosmayer, Schuppar, and Siems (2012). This literature has focused on identifying which of three possible references prices—the reservation price (the price at which the negotiator would be indifferent between accepting the offer and walking away), the aspiration price (the best expected result given the other negotiator), and the first offer price—has the greatest effect on the negotiated outcome. Again, since we are using a script in which we fixed the reference prices that our callers present, and specify what the callers should say at every juncture in the script, our experiment really focuses on the effect that a buyer's reservation price has on sellers' responses. This effect is certainly part of the overall price determination studied in other papers in this literature. Our paper is unusual in that we have designed an experiment to isolate just this effect.

The last literature to which our paper is related is gender and race discrimination. The aim of many of the papers in this literature is to understand whether one set of customers (distinguished by gender or race) is able to obtain lower prices than another. Castillo, Petrie, Torero, and Vesterlund (2012) and Gneezy, List, and Price (2012) are two examples; they investigate this effect in the market for taxi rides in Lima, Peru, and for new cars in the United States, respectively. While our paper does investigate the effect of gender on price, our emphasis differs in that we are interested primarily in whether the effects of different information conditions vary by gender; put simply, we are interested more in the interaction than in the main effect of gender.

The paper that most closely relates to ours is Ayres and Siegelman (1995). In this paper the authors conduct a field experiment in which they send buyers into car dealerships to negotiate for a new car using a prepared script. They find that white men receive lower initial price quotes and are also able to negotiate lower final prices than black or female buyers. Using observations made by testers during the negotiation, the authors argue that the price quote differences mainly arise from dealerships assuming that different groups have different reservations prices. Our paper is similar in that it scripts the behavior of buyers in order to try to hold buyer behavior constant across observations, and therefore focuses primarily on the seller response to experimentally manipulated differences across observations. We differ from Ayres and Siegelman (1995) in that we experimentally manipulate whether consumers present themselves to firms as being uninformed, well informed, or poorly informed. Ayres and Siegelman (1995) focus on gender and race effects, while our paper focuses on the effect of price expectations. We expand on the gender results in Ayres and Siegelman (1995) by showing that gender discrimination depends on firms' perception about how well informed consumers are.

Our paper is also related to a literature on consumer price search. We will describe the connections between our paper and the price search literature in Section 4, after we describe our experiment in detail in the next section. In Section 4, we will discuss specifically what such models suggest about the results of our experiment.

3 Experiment

We conducted the experiment in cooperation with the company AutoMD.com, an online firm that specializes in auto repairs by helping consumers diagnose car problems and providing consumers with information on repair costs.[5] In order to provide repair cost estimates to consumers, AutoMD operates a call center in which agents call local repair shops on a daily basis and ask for price quotes for specific types of repairs. For that purpose, agents may or may not introduce themselves as employees of AutoMD, and may at times mention AutoMD's estimated prices to shops. AutoMD

[5]Any results and conclusions contained in this paper are solely the responsibility of the authors and do not necessarily reflect the views of AutoMD.com nor are they endorsed by AutoMD.com.

allowed us to prepare a set of scripts for calls that varied the information that their call center agents provided in order to create a set of experimental treatment and control conditions. AutoMD also instructed the agents to make calls following the scripts and to vary which scripts they used for which calls, according to a randomization protocol we designed. Making our scripted calls was very similar to what agents regularly did in their job. They incorporated our experimental calls into their normal workflow, and were paid their regular salary by AutoMD. The call center consisted of nine agents, of which four were males and five females, who conducted our assigned calls over a period of 16 weeks in summer and fall 2012.

We instructed the agents to request a price quote for a radiator replacement for a 6-cylinder 2003 Toyota Camry LE. We chose this model because it was one of the vehicles in the AutoMD database for which consumers were most likely to request information on repair costs. We chose a radiator replacement because radiator leaks are relatively common among 10-year-old vehicles, and because radiator leaks are easily diagnosed by consumers. We held the car model, model year, and repair constant across all conditions and weeks of the experiment. The estimated average price in the AutoMD database for a radiator repair for this specific model and model year was $365, with minor regional variations.

3.1 Main experiment

In our main experiment we randomly assigned repair shops and call center agents to one of three experimental conditions. These conditions were codified in the form of call center scripts that the agents were instructed to follow as closely as possible. In the first script, after introducing themselves with, "Hello, my name is John/Jennifer," agents said, "So I have a 6-cylinder 2003 Toyota Camry LE. *I just visited the website AutoMD.com, and for this area they say the cost should be $365 to replace the radiator on my car.* Could you tell me how much you charge?" We refer to this script as the "market-based expected price" condition (or "market-based EP" condition for short).

In the second script, after introducing themselves, agents said, "So I have a 6-cylinder 2003 Toyota Camry LE. *I have no idea how much it is to replace a radiator.* Could you tell me how much you charge?" This is the "no expected price" condition.

In the third script, again after introducing themselves, agents said, "So I have a 6-cylinder 2003 Toyota Camry LE. *I just visited the website AutoMD.com, and for this area they say the cost should be $510 to replace the radiator on my car.*" We refer to this condition as the "upward-biased expected price" condition. (See page Appendix-1 for an outline of the scripts used by the agents.)

We chose the price in the market-based EP condition to signal that the consumer was aware of the prevailing market price. In the upward-biased EP condition we wanted to signal that the caller had inaccurate information. In consultation with AutoMD.com we chose \$510 because it was substantially higher than \$365 (by 40%) but not so high that it would be impossible for a repair shop to have given such a quote. We chose the wording in the no expected price condition to signal that the consumer was not informed about prices in the market and had no expected price in mind.

After agents received a price quote, they asked shops to make sure that the price included antifreeze and parts and labor, and was the total price before tax. The agents then recorded the price quote that incorporated any adjustments for these elements. We refer to this price as the "initial price quote."

Next, agents were instructed to compare the initial price quote to the expected price they had mentioned at the beginning of the call in the market-based or upward-biased price condition (\$365 or \$510). If the initial price quote exceeded the respective expected price, agents said, "So, I have a question. Would you match the price of \$365 (or \$510) that the website AutoMD said it should cost in this area?" Agents would then record "no," "yes," or a revised price offer if the shop offered a price concession that differed from a match of the expected price. Recall that in the no EP condition agents had not mentioned an expected price at the beginning of the call. Hence we instructed them to say, "So, I have a question. I just visited the website AutoMD.com, and for this area they say the cost should be \$365. Would you match this price?" Doing so allowed us to later determine whether callers were better off mentioning the expected price at the outset of the call (market-based EP condition) or after the initial price quote (no EP condition). The agents concluded the call after obtaining a response to the request for a price concession or—if the initial price quote had been below \$365 or \$510, depending on the condition—after obtaining the initial price quote.

Agents were provided with weekly spreadsheets containing a list of shops to call for each day of the week, and a script assignment for that shop. In order to minimize agent error in following the script, we assigned agents to use the same script for all the calls they made in a given day (although agents used different scripts on different days). Calls were made during weekdays. Shops were randomly assigned across agents. In order to study within-shop variation, we tried to obtain two price quotes from each shop, with a randomly assigned pair of scripts in random order, keeping at least four weeks between the two calls.[6] We held the gender of the agent constant in the second call to the same shop to be able to measure differences between scripts with less variance. We decided on the number of shops to call before the start of the experiment and ended the experiment once we had exhausted our list of shops. The list of repair shops consisted of all independent repair shops (not franchised new car dealerships) listed by AutoMD as providing radiator replacements

[6] We asked agents to record if a shop indicated that they noticed that we already called. We recorded only 94 such occasions among the 4,603 total quotes.

for Toyota vehicles in Designated Market Areas (DMAs) with 150 or more repair shops. These DMAs correspond to the most heavily populated DMAs in the country.

We collected quotes from a total of 2,778 shops over a period of 16 weeks. Of these, 1,825 provided price quotes under two different experimental conditions. We obtained only one quote from the remaining shops because the shop could not be reached a second time, the shop closed, or the shop refused to provide a quote over the phone despite having done so under a different condition. Overall, we obtained 4,603 price quote observations in our main experiment.[7]

3.2 Experiment 2

After collecting data for 10 weeks we were told that there was enough call center capacity to add a downward-biased expected price condition. We chose \$310 for the downward-biased expected price, which corresponded to the 20th percentile of the initial price quotes we had collected so far in the no EP condition.

Since our original list of shops from DMAs with 150 or more shops would be exhausted in the process of completing our ongoing main experiment, for our calls in experiment 2 we used a new list of shops, this time from DMAs with 70 to 149 repair shops. These calls began in week 12. As a result, experiment 2 was based on shops in different, smaller, markets and in different, later, weeks than our original experiment (henceforth experiment 1). Because we knew that we could not consider the conditions in which we collected observations in experiment 1 to be experimentally valid counterfactuals for the downward-biased EP condition in experiment 2, in experiment 2 we replicated the no EP condition along with the downward-biased EP condition. This way we had one experimentally valid counterfactual for the downward-biased EP condition.

Overall, both experiments ran concurrently for three weeks. During that time we called repair shops from experiment 1 for the second time with a different, randomly assigned, condition relative to the first condition with which those shops were called. We also called repair shops from experiment 2 with the no expected and downward-biased EP conditions, for a total of 1,941 price quotes across the two experiments. Table 1 summarizes the differences across conditions in the two experiments.

3.3 Experimental paradigm

The experiments used in this paper follow the experimental paradigm commonly used to study race and gender discrimination (Ayres and Siegelman 1995, Bertrand and Mullainathan 2004, Gneezy,

[7] We obtained an additional 85 quotes but removed them from the analysis because agents had erroneously used a pre-test version of the call script. See Section 6.1.1 for a discussion.

Table 1: Experimental Conditions

Variable	No EP	Market-Based EP	Upward-Biased EP	Downward-Biased EP
Expected price mentioned at outset of call	None	$365	$510	$310
Price used in request for price concession	$365	$365	$510	$310
Observations in experiment 1	1,613	1,509	1,481	0
Observations in experiment 2	1,012	0	0	929

List, and Price 2012). In this paradigm researchers pose as consumers/job seekers to measure the reaction of firms to different experimental stimuli. By design, firms cannot give informed consent without invalidating the purpose of the experiment. Because researchers typically do not end up engaging in a purchase or taking a job, this experimental paradigm imposes costs on firms. As a result, researchers have to be mindful to balance the benefits from the insights of the experiments with the costs imposed on firms.

While this is fundamentally a subjective judgment, we believe that the cost imposed by this experiment on a repair shop was small, for three reasons: First, these shops receive calls from "mystery shoppers" on a regular basis (a term used in the industry to refer to the practice of agents of firms pretending to be a consumer in order to obtain price and/or product information). In fact, the business model of AutoMD and its competitors is based on sampling prices for various repairs and vehicles from repair shops. While our script prescribed specific wording, calls made during the experiment did not differ in nature from calls made by AutoMD call center agents on a daily basis. Second, the duration of our calls was short—typically three minutes or so. At a wage of $15 for a clerical worker, the opportunity cost of this time was about 75 cents. Third, our calls were unlikely to have further consequences for these firms. (Our calls were not made in high enough volume to trigger updated estimates of anticipated demand, nor were they likely to trigger an emergency response by firms such as might be caused by reports of food poisoning in restaurants, for example.)

4 Consumer price search

In this section we discuss how economic theory illuminates our understanding of the results of our experiments. The economic models that most closely match our experimental setting are models of consumer price search, a literature initiated by Stigler (1961). Although our call center agents are following scripts in an experimental protocol rather than actually searching for prices, we have designed the protocol so that the agents mimic the behavior of consumers who are calling individual repair shops in order to obtain price quotes for a service they intend to purchase in the near future.

The aim of our experiment is to elicit the responses that repair shops would give to different types of price-searching potential customers.

Modeling consumer price search is tricky because two benchmark models lead to results that are paradoxical because they imply that there will be no price dispersion in equilibrium, a result inconsistent with what is generally observed in most markets. The first result is the Bertrand paradox (Bertrand 1883), which says that if consumer search costs are zero, the prices set by all firms will be driven to marginal cost.[8] The second result is the Diamond paradox (Diamond 1971), which says that if search costs are positive, the only Nash equilibrium is for all firms to charge the monopoly price. The intuition for this result is simple. If all firms are charging the monopoly price and search is costly, there is no incentive for consumers to search at more than one firm; and if consumers are searching at only one firm, there is no reason for firms to offer a price less than the monopoly price.[9] These two results allow only a small set of equilibrium pricing outcomes: uniform prices across firms, either at marginal cost or at the monopoly price.

Of course, in most real-world markets one can observe considerable pricing dispersion, even for identical products. In response to the stark mismatch between the predictions of the Bertrand and Diamond paradoxes and the actual occurrence of price dispersion, a sizable theoretical literature has attempted to show how price dispersion can arise in a model of consumer price search. See Stahl (1989) and Anderson and de Palma (2005) for discussions of this literature. Summarizing broadly, price dispersion can arise if consumers vary in how costly it is for them to search. Consumers will employ a search rule in which they continue searching as long as the expected gain to an additional search is worth the cost of conducting an incremental search. In practice, this translates to a "reservation price" rule in which consumers search until they find a price that is below their "reservation price." It is important to note that in this context "reservation price" is *not* the consumer's gross utility of consuming the good or the consumer's maximum willingness-to-pay for the good. These could be labelled as the "reservation value" in order to clarify the distinction. In this context, the reservation price is the price at which the cost of an additional search just equals the amount by which the next price quote would be expected to beat the reservation price.

On the other side of the market in these models, firms make a trade-off in choosing prices that closely resembles the monopoly pricing trade-off: a higher price increases the profits made on

[8] Of course if products are horizontally differentiated or if marginal costs are asymmetric, these results are relaxed. But the effect of search itself, in the simplest model, is to drive all prices to the uniform marginal cost.

[9] More precisely, the Diamond model has zero search cost for the first price quote and positive search costs for all subsequent price quotes. As Stiglitz (1979) points out, if there is a positive search cost for all searches, the market will unravel in the same manner as a lemons market because consumers with very low valuations for the good will not find it worthwhile to enter the market all, leading firms to raise their prices, driving out consumers with the next lowest valuations, and so on. Diamond (1987) notes, however, that if consumers have downward sloping demands and all consumers have high enough valuations for at least the first unit of the good, the original Diamond (1971) equilibrium is restored.

each individual sale, but also increases the possibility that the price is above a potential buyer's reservation price, and hence reduces the probability of making a sale. In the models, consumers are assumed to know the distribution of prices offered in the market, although they don't know the price offered by any individual firm until they search. Solving for equilibrium consists of finding a price distribution that is an equilibrium response by firms to the search behavior that consumers would conduct if they believed that to be the price distribution. One simple version of such a model has two types of consumers, with high and low search costs, and firms that divide into two groups, one that offers a low price (and sells to low-search-cost customers and to lucky high-search-cost customers) and another group that offers a high price and sells only to unlucky high-search-cost customers.[10]

While these models accomplish the goal of showing how price dispersion can arise with consumer search, they are not very informative to our question, which is how a seller should respond to an individual buyer who appears to be uninformed, well informed, or poorly informed. In the simplest of the two-type models, only low search cost customers will search, and they will be perfectly informed. The models do not accommodate consumers whose belief about the price distribution available in the market is wrong, or consumers who learn and update their beliefs about the price distribution as they conduct their price search.

The paper that comes closest to addressing our question is Salop (1977). Salop's main purpose is to show that a monopolist who sells to consumers who vary in both search cost and valuation of the good can use price dispersion to simultaneously segment consumers and charge higher prices to the high valuation (or low elasticity) consumers. For our purposes, the most relevant part of Salop's paper is his model of the reservation prices that consumers use to decide whether to continue searching or accept a current offer. Salop shows that consumers with higher search cost will have higher reservation prices. While search cost can be understood as the disutility or opportunity cost of time spent searching, or of the financial cost of accessing information sources, Salop argues that search cost could also be understood more broadly. He says, "Consumers may differ in effective search costs due to differences in their abilities to analyze and process data gathered. A consumer with an incorrect prior on [the distribution of prices offered in the market] may also be treated as having a higher [search cost]. One can think of high cost consumers simply as being less adept in economic decision making."

In our context, one could imagine that women have a higher search cost for obtaining information about car repair costs. For example, there is a prevalent stereotype that women are, on

[10] Varian (1980) and Butters (1977) present related models in which consumers are simply informed or uninformed. Their aim is to explain why firms have sales. They solve for mixed strategy equilibrium in which firms randomize over prices in a range, trading off the profits of selling at a high price to a share of the uninformed consumers vs. the probability of being the low price firm and selling, at a lower price, to all of the informed consumers.

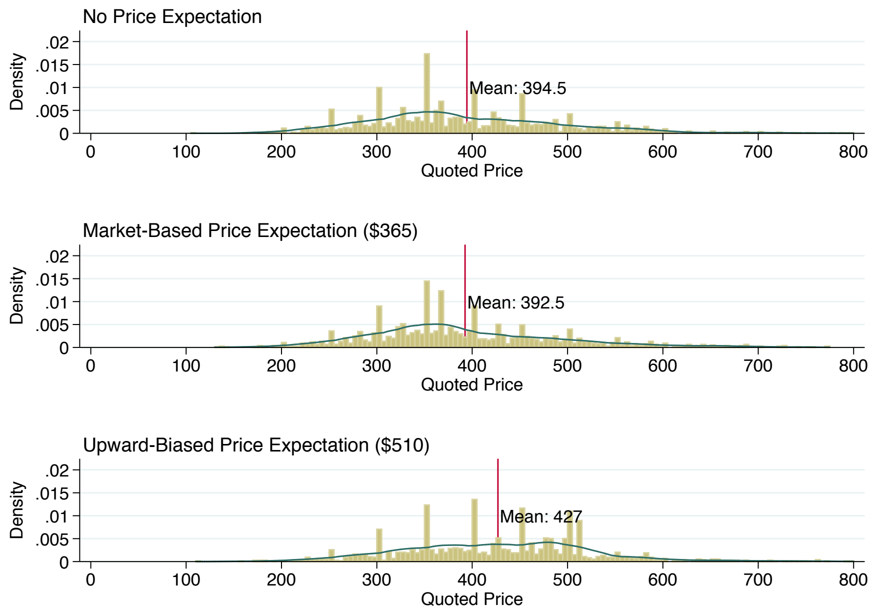
average, less interested in cars than men. If this is so, then gathering information about car repair costs may be inherently less interesting to the average woman than to the average man, or it may be more difficult to evaluate price quotes, since understanding what needs to be repaired and why and whether a fair price is being quoted are all aided by knowing more about cars themselves. Even if women understand price quotes just as easily as men do, women may dislike being treated by repair shops as if they know less about cars, or may dislike interacting in an environment in which they suspect repair shops will try to rip them off because they are perceived (correctly or incorrectly) to be poorly informed. There is a sense in which this is self-fulfilling: if women dislike interacting with repair shops, they may do it less, leading them to be less well-informed about prices, and thus justifying the repair shops' beliefs that they are poorly informed which leads women to dislike interacting with repair shops. Notice also that it need not be the case that women actually *are* less well-informed about cars. As long as repair shops *believe* that women are less well-informed, and this leads them to interact with women in a way women dislike, the cycle begins.[11]

If it is indeed the case that, on average, women have higher search costs for obtaining information about car repair shops, then a shop fielding a request from a woman should assume, in the absence of any other information, that she has a higher reservation price than a man and quote her a higher price. However, if either a man or a woman announces a reservation price, then the gender of the caller is no longer informative, since the gender was being used as a way to infer the reservation price. In our experimental scripts, the call center agent says "I just visited the website AutoMD.com, and for this area they say the cost should be $365 (or $510) to replace the radiator on my car." We assert that this price (which we call the "expected price" in this paper), should communicate to the repair shops something very similar to Salop's reservation price; we believe that the implicit message to the shop is "If you don't match or beat this price I will keep searching," which is exactly the idea of a reservation price.

If the repair shops respond to the price expectations announced by our call center agents as if they are the reservation prices of price-searching consumers who differ by gender in their search costs, that would lead to the basic pattern of results we find: Women who announce no price expectation (reservation price) are quoted higher prices than men who announce no price expectation, but both men and women are offered the same price quotes on average if they announce a price expectation. Finally, men and women who announce higher price expectations are offered higher price quotes than those who announce a lower price expectation.

[11] Salop suggests that one way some consumers can have higher search costs is if they are "less adept in economic decision-making." While it is definitely neither our intention, nor Salop's, to suggest that women are less adept decision-makers, repair shops may believe they are. If this leads them to treat women in a way that women dislike, and shy away from, then this could lead to women having higher search costs, and thus higher reservation prices, even if the repair shops' belief has no basis in reality. In some aspects, the repair shops' belief may become self-perpetuating.

Figure 1: Main experiment: Distribution of price quotes by condition



5 Results

In this section we report on the results of our main experiment. We begin with an analysis of how communicating different expected prices affects the initial price quote given to a consumer (henceforth *PriceQuote*). Next, we analyze how the effect of communicating an expected price differs by gender. We then show how firms react when consumers request that the firm match their expected price. Finally, we report an addition of a fourth condition in our second experiment.

5.1 Comparing different expected price conditions

The distribution of initial price quotes in our main experiment are graphed in Figure 1 on page 14. Consumers who communicated no expected price were quoted, on average, a price of $394.5; the median initial quote was $375. If consumers communicated a market-based expected price, the average price quote was $392.5 (median $370).[12] Communicating an upwards-biased expected price led to an average quote of $427 (median $425).

To analyze whether the quotes are statistically different from each other in the three conditions, we estimate the following specification:

[12]Note that the median price in these conditions is very close to our market-based expected price.

$$PriceQuote_{ijt} = \alpha_0 + \alpha_1 \mathbf{Condition_i} + \alpha_2 \mathbf{Controls_{ijt}} + \epsilon_{ijt} \tag{1}$$

PriceQuote is the initial price quote obtained from shop j called under condition i in week t. **Condition** contains indicator variables for the different EP conditions. The omitted category is the market-based EP condition. While the randomization of repair shops to conditions and call center agents should make control variables unnecessary, we use control variables in **Controls** as a randomization check and to convince ourselves of the robustness of our findings. In different specifications we will control for week fixed effects, DMA fixed effects, call order fixed effects, and repair shop fixed effects.

The results of estimating Equation 1 with varying controls are reported in Table 2 on page 16. Column 1 shows the results of a specification without control variables, which corresponds to a test of whether the means in the histogram are different from each other. We find no difference in the prices quoted to callers between the no EP and the market-based EP conditions. However, consumers who communicated upward-biased expected prices paid, on average, \$34 more than consumers in the market-based EP condition ($p<0.01$). In columns 2 and 3 we report the results of estimating Equation 1 with week fixed effects only, or week and DMA fixed effects. The results in column 2 and 3 indicate that our randomization of shops to conditions worked well—the estimates of the experimental effects are essentially unchanged by the addition of these controls. Column 4 also includes a dummy if the price quote was obtained from a shop that had already been called once before under a different condition. While the coefficient on *SecondCall* in column 4 suggests that shops quoted somewhat higher prices when they had been called before, the coefficients on the condition indicator variables suggest that this does not change the estimated difference between conditions.

As we described in Section 3, we tried to call each shop twice. In practice, some shops could not be reached a second time or simply refused to give a quote when we called them the second time. As a result, 953 of the 4,603 quotes in our main experiment are from shops that only quoted one price. For the remaining 3,650 price quotes we can add another control, namely, repair shop fixed effects.[13] Column 5 reports the results of this regression. As before, we find no difference in the prices quoted to callers between the no EP and the market-based EP conditions. In addition, we still find that callers who communicated an upward-biased expected price paid, on average, more than consumers whose expected price was market-based, although we estimate an effect of \$25 when we include shop fixed effects, an estimate that is smaller in magnitude than the \$35 effect

[13]We take out DMA fixed effects in specifications that use shop fixed effects, since they are collinear with shop fixed effects.

Table 2: Effects of information condition

Dependent Variable	(1) Price Quote	(2) Price Quote	(3) Price Quote	(4) Price Quote	(5) Price Quote
No EP	1.9 (3.8)	2.1 (4.1)	2.7 (4)	2.5 (4)	-.068 (3.3)
Upward-Biased EP	34** (3.8)	35** (4.1)	35** (4.1)	35** (4.1)	24** (3.8)
Week 2		-1.1 (18)	-3.7 (17)	-3.8 (17)	-45* (20)
Week 3		-3 (20)	-2.6 (20)	-2.5 (20)	-14 (23)
Week 4		-3.8 (17)	-7.7 (16)	-7.8 (16)	-36 (23)
Week 5		-8.6 (17)	-12 (16)	-13 (16)	-39+ (22)
Week 6		-1.5 (17)	-4.8 (16)	-7.2 (16)	-46+ (24)
Week 7		-9.4 (17)	-13 (16)	-17 (17)	-44+ (25)
Week 8		-7.2 (17)	-12 (16)	-23 (17)	-51 (34)
Week 9		21 (19)	14 (18)	4.5 (18)	-46 (32)
Week 10		12 (19)	9.1 (19)	-4.4 (19)	-66+ (37)
Week 11		2.3 (17)	-1.9 (17)	-15 (18)	-60+ (34)
Week 14		-9.9 (18)	-14 (17)	-28 (18)	-67+ (36)
Week 15		-24 (18)	-25 (17)	-38* (18)	-73* (36)
Week 16		-1.3 (19)	-8.3 (18)	-22 (19)	-53 (36)
Boston (Manchester)			15+ (8.7)	16+ (8.7)	
Charlotte			2.8 (11)	2.8 (11)	
Chicago			29** (8.7)	29** (8.6)	
Cleveland-Akron (Canton)			32* (14)	33* (14)	
Dallas-Ft. Worth			-7.8 (10)	-7.3 (10)	
Denver			24** (9.4)	24** (9.3)	
Detroit			1.9 (9)	1.8 (9)	
Indianapolis			18 (11)	18 (11)	
Los Angeles			-14+ (7.1)	-14* (7.1)	
Miami-Ft. Lauderdale			2.8 (11)	3.2 (11)	
New York			-5.7 (7.8)	-5.4 (7.8)	
Orlando-Daytona Bch-Melbrn			7.4 (9.7)	8.1 (9.6)	
Philadelphia			55** (9.3)	56** (9.3)	
Phoenix (Prescott)			5.6 (9.2)	5.4 (9.2)	
Portland, Or			5.8 (12)	5.9 (12)	
Sacramnto-Stkton-Modesto			17 (11)	17 (11)	
Salt Lake City			-8.5 (10)	-8.4 (9.9)	
San Francisco-Oak-San Jose			30** (9)	30** (9)	
Seattle-Tacoma			29** (9)	29** (9)	
Tampa-St. Pete (Sarasota)			4.4 (9.4)	4.4 (9.3)	
Washington, DC (Hagrstwn)			65** (9.7)	65** (9.7)	
Second Call				13* (5.4)	19 (15)
Shop Fixed Effects					✓
Constant	393** (2.7)	396** (17)	388** (17)	388** (17)	438** (22)
Observations	4,603	4,603	4,603	4,603	3,650
R-squared	0.022	0.026	0.067	0.068	0.811

estimated in columns 1-4.

In summary, we find that communicating an expected price that exceeds the market price induced repair shops to provide price quotes that were higher by $25 to $35 than when a market-based expected price or no expected price was communicated. The main inference we draw from this result is that shops change the prices they quote in response to information communicated to them by potential customers. In particular, if a customer indicates that he or she has a substantially inflated price expectation, the shop will try to capitalize on that by quoting a higher price. This inference is particularly strong because it holds in a within-shop comparison (column 5, which includes repair shop fixed effects). However, we find no difference in price quotes when callers communicate market-based expected prices or no expected prices. An interpretation of this result might be that repair shops, without any information about consumers' price expectations, infer that everyone has an expected price that corresponds to the market price. We will argue next, in our presentation of gender effects, that this interpretation is too simple.

5.2 Expected prices and gender

In addition to randomly assigning conditions to repair shops, we also randomized whether calls made to a given shop were made by male or female call center agents. In this section we report on the results of analyzing the effect of different conditions by the gender of the caller.

Figure 2 on page 18 shows the distribution of prices by condition and gender. Visual inspection of the means displayed in the figure suggests that men and women are quoted similar prices in both the market-based and upward-biased EP conditions, but that women are quoted higher prices than men in the no EP condition. In order to assess the size and statistical significance of these comparisons, we add interaction effects between condition and gender to the specification in Equation 1, thereby obtaining Equation 2.

$$PriceQuote = \beta_0 + \beta_{\mathbf{1}}\mathbf{Condition} \times \mathbf{Gender} + \beta_{\mathbf{2}}\mathbf{Controls} + \nu_i \tag{2}$$

where *Gender* j is an indicator variable for the gender of agents making calls to shop j.[14]

In Table 3 we report the results from estimating Equation 2. In the table, the omitted category is the market-based EP condition for male callers. Hence, all coefficients measure the effect of different conditions for male or female callers relative to the market-based EP condition for male callers. As in Table 2, the first four columns of Table 3 differ in the controls they use: column 1 reports on the results of Equation 2 without controls and column 4 reports on the estimation with

[14] An individual shop received calls from a single gender of agents. In some cases, the calls were made by different agents of the same gender; in others both calls were made by the same agent.

Figure 2: Distribution of price quotes by condition and gender

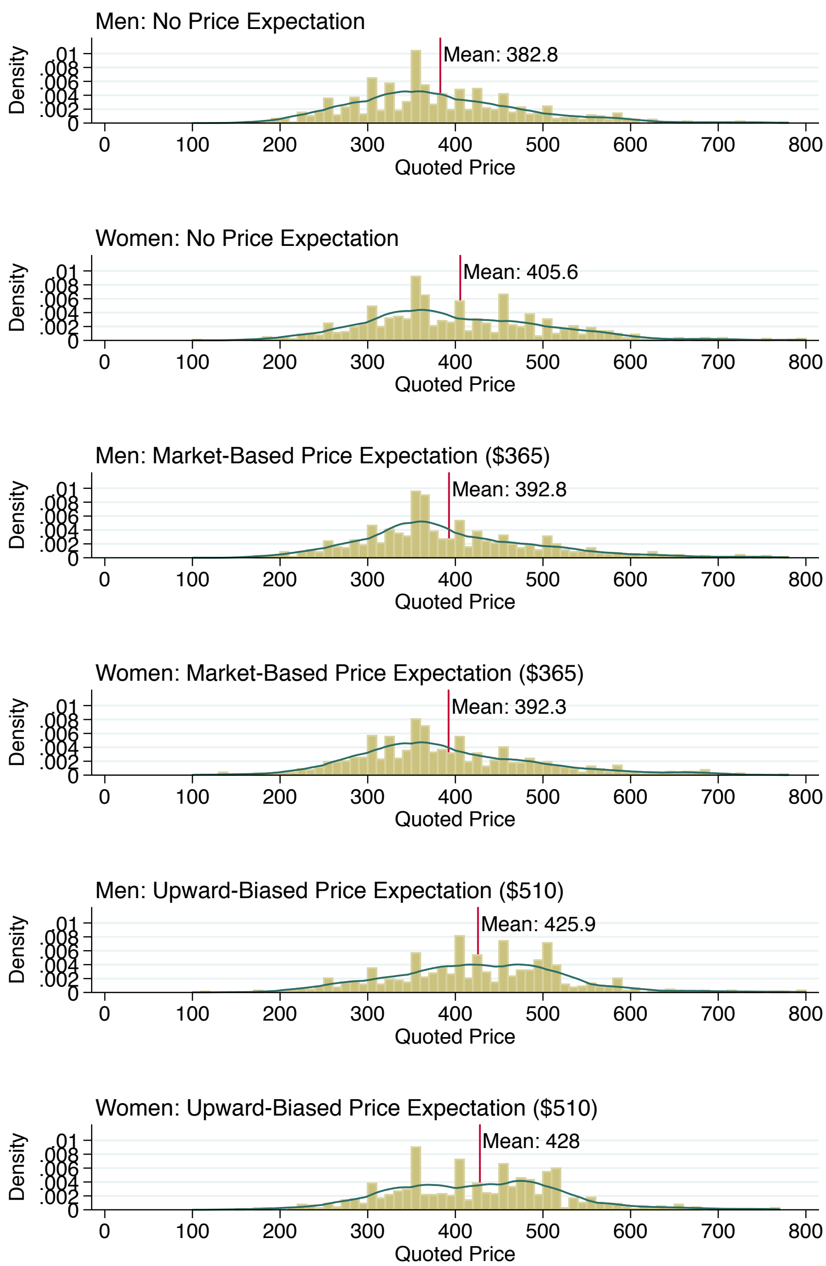


Table 3: Effects of information condition by gender

Dependent Variable	(1) Price Quote	(2) Price Quote	(3) Price Quote	(4) Price Quote
Market-Based EP, Women	-.45	-2.7	-2.8	-2.7
	(5.4)	(5.5)	(5.4)	(5.4)
No EP, Women	13*	11*	12*	11*
	(5.4)	(5.6)	(5.5)	(5.5)
No EP, Men	-9.9+	-11*	-11+	-10+
	(5.3)	(5.7)	(5.6)	(5.6)
Upward-Biased EP, Women	35**	34**	34**	34**
	(5.3)	(5.6)	(5.5)	(5.5)
Upward-Biased EP, Men	33**	33**	32**	33**
	(5.4)	(5.6)	(5.5)	(5.6)
Second Call				11*
				(5.4)
Week Fixed Effects		✓	✓	✓
DMA Fixed Effects			✓	✓
Constant	393**	395**	387**	388**
	(3.8)	(17)	(17)	(17)
Observations	4,603	4,603	4,603	4,603
R-squared	0.026	0.030	0.070	0.071

full controls.[15] Results vary little as controls are added. Therefore we focus on column 1.

We find that repair shops quote men and women the same price on average after they communicate market-based expected prices, as indicated by the statistically insignificant coefficient estimate of -0.45 for "*Market-Based EP, Women*." Similarly, after communicating upward-biased expected prices, men and women are quoted the same price, as indicated by the statistically indistinguishable coefficient estimates of 35 for "*Upward-Biased EP, Women*" and 33 for "*Upward-Biased EP, Men*."[16]

However, our prior finding that callers who communicated no expected price were quoted the same prices as callers who communicated a market-based expected price no longer holds when we analyze the effect by gender. Specifically, female callers are quoted prices that are \$13 *higher* (p=0.01) when they do not mention an expected price than when they communicate a market-based expected price.[17] In contrast, we estimate that male callers are quoted prices that are \$9.9 *lower* (p=0.07) when they do not mention an expected price than when they communicate a market-based expected price. This estimate is of borderline statistical significance across the four columns,

[15]Note that one consequence of our decision to use a single gender for all calls made to an individual shop is that we cannot identify gender effects on price quotes in a specification that also uses shop fixed effects. This is because results estimated using shop fixed effects are identified by within-shop variation, and there is no within-shop variation in caller gender, only in condition. We can still estimate the differences between conditions using shop fixed effects, but the estimated effects should be interpreted as the average within-shop differences between conditions for male callers (for shops that received calls from male agents) and for female callers (for shops that received calls from female agents). We report such results separately for the subsample of shops that received calls from male agents and for the subsample of shops that received calls from female agents in Table A-1.

[16]An F-test of the hypothesis that these are equal yields a p-value of 0.95.

[17]An estimate of \$13 for this effect is obtained from the regression coefficients by subtracting the coefficient of -0.45 for "*Market-Based EP, Women*" from the coefficient of 13 for "*No EP, Women.*" The p-value is derived from an F-test.

however. Taken together, these results imply that female callers pay on average almost $23 more than male callers when they don't communicate an expected price to the repair shop.

The gender results presented in this section show an interesting pattern. When callers reveal an expected price to shops, they are treated the same whether male or female. This is true whether the expected price reveals the caller to be well-informed (market-based expected price of $365) or poorly informed (upward-biased expected price of $510). However, when shops are given no direct indication of a caller's price information, they appear to draw inferences from the caller's gender that lead them to offer male and female callers different initial price quotes. Female callers who reveal no expected price are offered prices that are higher than male callers in the same condition, and higher than is offered to female callers with market-based expected prices. Male callers who reveal no expected price are offered lower prices than female callers in the same condition and lower than the prices offered to male callers with market-based expected prices. In short, shops appear to respond to whatever information they have about consumers' price knowledge, drawing inferences from gender if that is all they have to go on, but disregarding gender if provided more direct information on consumers' price expectations.

5.3 Expected prices as a bargaining reference

So far we have investigated how mentioning an expected price changes the initial price quote provided by repair shops. We have interpreted the estimated results as evidence that repair shops adjust their initial price quotes according to how well-informed a shop thinks a caller is, conveyed in part by the expected price a caller communicates. While signaling price knowledge is one role that an expected price can play in such an interaction, it can also play a second role—as a reference in bargaining with the repair shop. To investigate the role of expected prices as a bargaining reference we instructed callers—immediately after obtaining the initial price quote—to check whether the initial price quote exceeds the expected price. If so, callers asked shops whether they would match the reference price. In the market-based and upwards-biased EP condition we instructed them to say, "So, I have a question. Would you match the price of $365 ($510) that the website AutoMD said it should cost in this area?" In the no EP condition we had callers request a match if the initial price quote exceeded $365: "So, I have a question, I just visited the website AutoMD.com, and for this area they say the cost should be $365. Would you match this price?" We then had the agents record whether the repair shop revised their price quote. We refer to the difference between the initial price quote and the revised price quote as the *Price Concession*.

Overall, we find that shops agree to some form of price concession 26% of the time, i.e., 74% of caller requests to modify the price were not successful. How frequently shops agree to make a price concession varies between the EP conditions. As shown in Table 4, shops are much more likely to modify the price when asked to match \$510 than when asked to match \$365 (p-value <0.01 in a χ^2 test).[18]

Table 4: Likelihood of price concession by condition

	Price Concession		
Condition	$= 0$	> 0	Total
Market-Based EP	73.8%	26.2%	100.0%
No EP	76.6%	23.4%	100.0%
Upward-Biased EP	58.1%	41.9%	100.0%
Total	73.6%	26.4%	100.0%

One might think that this is because lowering the price to \$510 requires less of a concession than lowering the price to \$365. Consistent with this, the average amount by which the initial price quote exceeds the expected price is lower by \$19 in the upward-biased EP condition than in the other conditions. To see whether shops are more likely to modify the price when the request is to match \$510 rather than \$365, even when controlling for the magnitude of the requested concession, we estimate the following specification:

$$I(\text{PriceConcession} > 0)_{ijt} = \delta_0 + \delta_{\mathbf{1}}\mathbf{Condition}_i + \delta_2\text{RequestedConcession}_{ijt} + \mu_i \tag{3}$$

Condition contains indicator variables for the different EP conditions. RequestedConcession controls for the difference between the initial price quote and requested price (\$365 or \$510). In column 1 of Table 5 we control for this difference linearly. In column 2 we control for this difference non-parametrically by including indicator variables for deciles of the difference between the initial price quote and requested price. The results show that shops remain significantly more likely to agree to a price concession (by 16 percentage points) in the upward-biased EP condition than in other conditions, even when holding constant the magnitude of the requested concession. An alternative explanation, which we cannot separately identify with our data, is that the shops' willingness to agree to a price concession depends on the absolute magnitude of the price; perhaps shops are more likely to make a price concession when they know that their price quote is high, irrespective of condition.

Next, we explore whether men and women are equally likely to obtain price concessions when they bargain. We estimate this in column 3 of Table 5 by replacing the condition indicators in Equation 3 with a gender indicator. The results show that women remain significantly more likely

[18]This tests the probability of matching in the upward-biased condition versus the probability of matching in the market-based and no EP conditions pooled.

Table 5: Price concession results

Dependent Variable	(1) I(Price Concession > 0)	(2) I(Price Concession > 0)	(3) I(Price Concession > 0)	(4) I(Price Concession > 0)	(5) PriceConcession if Concession > 0
No EP	-.027	-.022		.013	4.1
	(.022)	(.021)		(.03)	(6.5)
Upward-Biased EP	.14**	.16**		.076	5.2
	(.041)	(.041)		(.061)	(7.2)
Women			.11**		
			(.021)		
No EP, Women				.057*	3.4
				(.029)	(4.8)
Market-Based EP, Women				.12**	5.3
				(.031)	(6.1)
Upward-Biased EP, Women				.24**	5.4
				(.074)	(4.7)
RequestedConcession	-.00076**				
	(.00011)				
RequestedConcession Decile 2		-.041	-.055	-.052	19**
		(.048)	(.046)	(.047)	(1.1)
RequestedConcession Decile 3		-.24**	-.21**	-.26**	27**
		(.066)	(.071)	(.064)	(2.6)
RequestedConcession Decile 4		-.13**	-.13**	-.14**	36**
		(.051)	(.05)	(.05)	(2.1)
RequestedConcession Decile 5		-.24**	-.25**	-.24**	57**
		(.046)	(.045)	(.045)	(2.9)
RequestedConcession Decile 6		-.25**	-.27**	-.27**	73**
		(.054)	(.053)	(.053)	(5.2)
RequestedConcession Decile 7		-.2**	-.22**	-.22**	94**
		(.05)	(.049)	(.049)	(4.8)
RequestedConcession Decile 8		-.26**	-.27**	-.26**	121**
		(.048)	(.047)	(.048)	(7)
RequestedConcession Decile 9		-.27**	-.29**	-.28**	171**
		(.047)	(.046)	(.047)	(6.7)
RequestedConcession Decile 10		-.26**	-.27**	-.27**	256**
		(.048)	(.047)	(.048)	(19)
Constant	.34**	.43**	.39**	.37**	4.1
	(.021)	(.039)	(.038)	(.041)	(5.8)
Observations	1,738	1,738	1,738	1,738	458
R-squared	0.038	0.064	0.065	0.081	0.814

than men (by 11 percentage points) to obtain price concessions from repair shops. Note that because we control directly for the size of the requested concession, this result is not an artifact of women receiving higher initial price quotes in the no EP condition.

Finally, we would like to know whether the likelihood of obtaining a price concession depends on the interaction of gender and condition. In column 4 of Table 5 we add condition and gender interaction effects to the specification in column 4. We find that repair shops are more likely to give a price concession to a female caller than to a male caller, irrespective of condition (although the gender difference for the no EP condition is only marginally significant at p=.052). However, the gender difference seems most pronounced in the upward-biased EP condition (even though we control for the size of the requested concession).

If we look in column 4 at the results across condition for a single gender rather than across gender for a single condition, we see that men are statistically equally likely to obtain a price concession in the three conditions, controlling for the size of the requested concession. For women, however, the probability of obtaining a concession is highest in the upward-biased EP condition, even controlling for the size of the requested concession. Most interesting, however, is the result that women are more likely to obtain a concession in the market-based EP condition than in the no EP condition (p-value = .09), even though in both cases the caller is asking the shop to match a price of $365. This result suggests that for women, there is a double benefit to revealing a market-based price expectation: doing so not only leads on average to a lower initial price quote (Table 3), it also leads to a higher probability of obtaining a match, should the initial quote exceed the market-based expect price. Together these suggest that a woman in this context has a distinct advantage in revealing good price knowledge early on.

One might worry that the results for the No EP condition could be affected by selection for the following reason.[19] Agents only ask for a price concession if the original price quote exceeds $365 (in the No EP and market-based EP condition) or $510 (in the upward-biased EP condition). In the market-based and upward-biased conditions, men and women are quoted the same prices on average. This means that, since agents are assigned randomly to shops, there should not be systematic differences in the shops where women are asking for price concessions and where men are asking for price concessions.[20] However, in the No EP condition, women receive higher initial price quotes than men do, which means that women are asking for price concessions at a greater number of shops than men are. One could think of there being a set of shops where a woman would be quoted a price above $365 and therefore ask for a concession, but where a man would be

[19] We thank Stephan Seiler and seminar participants at Stanford GSB for pointing this possibility out to us.

[20] Women and men are also offered the same prices on average if we restrict the sample to calls in which the original price quote was at least $365 in the market-based condition or $510 in the upward-biased condition.

quoted a price below \$365 and therefore would not ask for a concession. The selection question is whether this set of shops are more likely or less likely to offer price concessions *irrespective of the gender of the agent* than the shops which would quote both men and women prices above \$365. If this were true, it would be to bias our estimate of the difference between men and women in the probability of receiving a price concession.

It turns out that our empirical results are inconsistent with this type of selection for the following reasons. Suppose that the set of shops at which only a woman would be quoted an initial price above \$365 offer concessions *more frequently*, irrespective of the gender of the caller, than the shops at which both men and women would be offered prices above \$365. If this were the case, then in column 4, the estimated coefficient for "No EP, Women" should be higher than the coefficient on "Market-based EP, Women" and "Upward-biased EP, Women" since the No EP condition would contain a positive selection bias that the other two coefficients would not. (Women would receive concessions more frequently relative to men in the No EP condition than in the other two conditions, because of the sample of shops in the No EP condition at which only women are asking for concessions.) Instead, the reverse is true. The female indicator variable is lower in the No EP condition than in the other two conditions. Suppose, on the other hand, that the set of shops at which only women would be quoted a price above \$365 offers concessions *less frequently*, irrespective of the caller's gender, than shops that would quote prices above \$365 to both men and women. If this is the case, then the coefficient on "No EP, Women" should be negative. (Women in the No EP condition should receive concessions less often on average than men because some of the shops at which women are asking for concessions grant them less frequently.) This is the opposite of what we estimate. Our estimates are consistent with neither direction of the possible selection bias.[21]

Having established that women are significantly more likely than men to obtain price concessions from repair shops, we analyze next whether the magnitude of the price concession differs by condition and gender. We estimate the following specification using data only from calls for which PriceConcession > 0:

$$\text{PriceConcession}_{ijt} = \gamma_0 + \gamma_{\mathbf{1}}\mathbf{Condition}_i \times \mathbf{Gender}_j + \gamma_2 \text{RequestedConcession}_{ijt} + \nu_{ijt} \qquad (4)$$

where **Condition** $\times$ **Gender** contains interaction variables for gender and condition. The results are reported in column 5. According to the estimates, the magnitude of price concessions vary neither by condition nor by gender. Furthermore, there are no interaction effects between condition

[21]The negative selection bias could make the "No EP, Women" coefficient smaller than it otherwise would be, but this can't be the dominant determinant of this coefficient, since the coefficient is positive rather than negative.

and gender.[22]

In summary, we have shown that likelihood of obtaining a price concession depends on both how high the initial price quote is and how large a concession is requested. We have also shown that women are significantly more likely than men to obtain price concessions from repair shops. However, conditional on obtaining a price concession, the size of the concession varies neither by gender nor by expected price condition.[23] The design of our experiment does not enable us to do more than speculate about why women are more likely to obtain a price match than men are. We have evidence that most of the repair shop employees to whom callers spoke were male.[24] It may be that men are more likely because of social or cultural conditioning to respond positively to requests made by women. Research by Babcock, Laschever, Gelfand, and Small (2003) indicates that women are less likely to ask for things such as raises (or perhaps price concessions) in negotiations. Further, Leibbrandt and List (2012) demonstrate that women are likely to negotiate in environments where negotiability is explicit, but deter from negotiations when they do not perceive a situation as a negotiation opportunity. Women may not consider asking for a quote or shopping for prices as a negotiation opportunity, thereby further reducing the likelihood that a woman would ask for a price reduction. If these findings are true on average for women in the repair shop context, repair shops may interpret a woman asking for a match as being a signal that she is more dissatisfied with the price offer she has received (because it has actually prompted her to take the relatively uncommon step of asking for a price match) than a man is when he asks for a match.

5.4 Downward-biased expected prices

After we concluded most of the data collection for the first experiment, we decided to investigate the effect of a "downward-biased" EP condition, for which we chose an expected price of \$310. We wanted to compare the price quotes that callers obtained in the downward-biased EP condition to price quotes obtained in other conditions. However, the data gathered on various conditions in experiment 1 could not be used to construct an experimentally valid counterfactual for the

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[22] Note that we have estimated these results only in the subset of the sample where the initial price offer was greater than either the expected price of either \$365 or \$510. (Asking the repair shop to match the expected price when the initial price quote was actually *below* the expected price would have been nonsensical.) Thus, our results should be interpreted as the effect of gender on the size of a concession when the request is to match an announced expected price. In other words, it is the average treatment effect on a particular treatment group, defined by the kind of concession we had agents ask for. If we had had another rule for the concession the agents were to ask for—for example, "Ask for a price \$30 lower, whatever the initial price quote"—then we could estimate the effect of concession requests in the entire sample.

[23] All these results are robust to inclusion of week, DMA, and call order controls (see Table A-2 on page Appendix-4).

[24] We did not ask our callers to record the gender of the repair shop employee to whom they spoke. Nonetheless, in 915 cases agents recorded the name of the employee they spoke to of their own accord. In this sample, 814 of the names were male, 75 were female, and the rest could be either. Due to the small sample, we cannot test the hypothesis that male repair shop employees are more likely than female employees to make concessions to female callers.

downward-biased EP condition in experiment 2 for two reasons. First, we started the second experiment in the 12th week of the first experiment. The two experiments ran concurrently for only three weeks. In addition, during these three weeks we were conducting only second calls to repair shops in experiment 1, whereas we were calling repair shops in experiment 2 for the first time. (Because experiment 2 was initiated near the end of our experimental period, we did not have enough time to call the shops in experiment 2 for the first time, wait a few weeks, and then follow-up with a second call under another condition. As a result, shops in experiment 2 were called only once, with a single experimental condition.) Second, for experiment 1 we called all shops that were located in DMAs with 150 or more repair shops. These DMAs largely correspond to the most populous DMAs in the nation. As a result, for experiment 2 we had to call repair shops in smaller DMAs, specifically shops in DMAs with 70 to 149 repair shops.

Because we knew that we would step outside the experimental paradigm if we pooled the data from both experiments, in experiment 2 we replicated the no EP condition along with the downward-biased EP condition. This way we had one experimentally valid counterfactual for the downward-biased EP condition.[25]

The initial price quote results of experiment 2 are reported in Table 6. In column 1 we analyze whether mentioning a downward-biased expected price yields a lower initial price quote than not mentioning an expected price. Column 2 repeats the analysis while controlling for week and DMA fixed effects. In these two columns our point estimates indicate that average price quotes are lower in the downward-biased EP condition, by \$7.4 and \$8.4, respectively, but these differences are not statistically different from zero (p=.12 in column 1 and p=.11 in column 2). Moreover, when we investigate interactions between gender and the two expected price conditions in column 3 of Table 6, we find no statistically significant effects. (We will revisit the lack of gender effects in this experiment in the robustness section.)

We can also investigate the likelihood of obtaining a price concession in experiment 2. Table 7 shows that callers are less likely to obtain a price concession when they ask the shop to match a price of \$310 than when they ask the shop to match a price of \$365 (p-value <0.01 in a χ^2 test).[26] Similar to what we found in subsection 5.3, this difference seems to be driven not by the condition directly, but instead by the size of the requested concession. In column 1 of Table 8, we reestimate Equation 3 using data from experiment 2. We find no statistically significant difference between conditions in the probability of obtaining a price concession, once we control for the size of the requested concession. We also find that women are (statistically weakly) more likely to

[25] In the robustness section we will show results obtained by pooling the data from both experiments, with appropriate caveats.

[26] For consistency with experiment 1 we instructed callers to request \$365 in the no EP condition.

Table 6: Experiment 2, initial price quote results

Dependent Variable	(1) PriceQuote	(2) PriceQuote	(3) PriceQuote
	Experiment 2:	Experiment 2:	Experiment 2:
Downward-Biased EP	-7.4	-8.4	-8.9
	(4.6)	(5.2)	(8)
No EP, Women			-5.7
			(6.9)
Downward-Biased EP, Women			-5.2
			(6.9)
Week 13		-3.8	-3.6
		(6.4)	(6.4)
Week 14		-1.7	-1.5
		(7)	(7.1)
Week 15		-6.7	-6.5
		(8.6)	(8.6)
Week 16		-24+	-22
		(14)	(14)
Week Fixed Effects		✓	✓
DMA Fixed Effects		✓	✓
Constant	399**	384**	388**
	(3.3)	(13)	(14)
Observations	1,941	1,941	1,941
R-squared	0.001	0.055	0.056

Table 7: Likelihood of price concession by condition

	PriceConcession		
Condition	= 0	> 0	Total
No EP	78.7%	21.3%	100.0%
Downward-Biased EP	83.3%	16.7%	100.0%
Total	81.2%	18.8%	100.0%

Table 8: Experiment 2, price concession results

Dependent Variable	(1) I(Price Concession > 0)	(2) I(Price Concession > 0)	(3) PriceConcession if Concession > 0
Downward-Biased EP	-.021	.03	2.5
	(.022)	(.036)	(8.4)
No EP, Women		.062+	-.26
		(.035)	(7.5)
Downward-Based EP, Women		-.018	4.9
		(.029)	(6.2)
RequestedConcession Decile 2	-.16**	-.16**	18**
	(.058)	(.057)	(1.9)
RequestedConcession Decile 3	-.24**	-.24**	25**
	(.058)	(.057)	(3.1)
RequestedConcession Decile 4	-.2**	-.19**	38**
	(.057)	(.057)	(3.2)
RequestedConcession Decile 5	-.31**	-.31**	61**
	(.052)	(.052)	(4.3)
RequestedConcession Decile 6	-.29**	-.29**	66**
	(.052)	(.052)	(7.1)
RequestedConcession Decile 7	-.36**	-.37**	94**
	(.048)	(.048)	(8.3)
RequestedConcession Decile 8	-.32**	-.32**	131**
	(.054)	(.053)	(7.7)
RequestedConcession Decile 9	-.3**	-.3**	178**
	(.053)	(.052)	(12)
RequestedConcession Decile 10	-.3**	-.3**	210**
	(.053)	(.052)	(24)
Constant	.45**	.41**	10
	(.044)	(.048)	(6.6)
Observations	1,259	1,259	236
R-squared	0.073	0.076	0.822

obtain a price concession than men in the no EP condition, but no difference across genders in the downward-biased EP condition exists, as shown in column 2 of Table 8. This is consistent with our prior finding that the female effect seemed to be smaller for lower requested prices (column 4 of Table 5). Also, as in experiment 1, the magnitude of price concessions vary neither by condition nor gender.

6 Robustness

In this section we explore the robustness of our findings. We begin by investigating agents' adherence to our experimental protocol. Next, we pool the data from our two experiments to see whether our conclusions would be the same if we considered the data to have been generated by a single experiment.

6.1 Execution of the experiment

To make sure that calls were conducted in the way we intended, we put several safeguards in place. First, we regularly communicated with the call center supervisor to discuss the progress of the experiment and react to unexpected issues. Second, the call center supervisor held weekly meetings with agents where she assigned them spreadsheets that we created, which contained each agent's randomly assigned shops and randomly assigned conditions for the week. During those meetings she communicated any instructions we wanted implemented. Third, to make sure that agents were following the correct script for each experimental condition, we printed on each script a 3-digit "script code" (e.g., "181"). We changed the script code whenever we changed the wording of the script, or updated instructions on how to fill out the spreadsheet.[27] Agents were required to manually enter these codes in a spreadsheet column at the end of each call. Fourth, we monitored comments made by agents in the spreadsheet in which they noted anything that struck them as noteworthy during the call. Analyzing these comments serves the purpose of both recording shop behavior that we had not anticipated as well as monitoring agent behavior by observing what agents felt was necessary to point out.

In the following subsections we investigate how our estimated results change if we make data corrections to account for inconsistent and/or problematic behavior by agents. We begin with script codes and then move to three types of comments made by agents in the comment field.

6.1.1 Script codes

Out of the 6,544 price quotes obtained during the course of the two experiments, we found that in 249 instances (3.8%) the agent had recorded the incorrect script code. Out of these, 85 quotes were obtained at the beginning of the experiment using script wording that we changed after making some calls in a pre-test. These 85 calls recorded the pre-test script codes, indicating that the calls were probably made using the pre-test script wording. In the analysis presented in this paper we have already eliminated these 85 quotes. However, the remaining 164 quotes with incorrect script codes are still in the data. This is because the scripts to which the codes referred had identical wording to those that the agents should have used.[28] Since all of these cases occurred during the first six weeks of data collection, they can only potentially affect the results from experiment 1.

To explore the robustness of our result to incorrect script code entries, we rerun the main regressions from experiment 1 (column 4 of Table 3, and columns 4 and 5 of Table 5) after dropping all 249 quotes with incorrect script codes. The results are reported in column 2 of Table A-3 and columns 2 and 6 of Table A-4. While the point estimates change slightly, none of our conclusions

[27]This occurred when we updated the wording of scripts between the pre-test and the beginning of the experiment, when we updated instructions on how to fill out the spreadsheet before week 3 and again week 5, and when we introduced experiment 2.

[28]We changed the script code because we updated instructions on how to fill out the spreadsheet, not because the wording of the scripts themselves changed.

are affected by dropping quotes with incorrect script codes.[29]

6.1.2 Agents' comments: call-backs

We reviewed the comments entered by agents when they considered something out of the ordinary. We noticed that for 13% of price quotes, shops asked agents to call back before they gave a price quote. This reflects situations in which the shop could not pull together a price quote while staying on the phone, perhaps because of other demands of the shop employee's attention. For an additional 1% of the quotes in our data, agents were asked to call back after they had obtained the initial price quote but before they were given a revised price quote in response to their request to match one of the expected prices. Both of these requests for a call-back are potentially problematic because we don't know whether our experimental manipulation had the same effect on quotes that were assembled once the agent was off the phone. As a result, we reestimate our main specifications after eliminating all quotes for which an agent noted that the shop requested a call-back before the quote was obtained. The conclusions from experiment 1 do not change (see column 3 of Table A-3, columns 3 and 7 of Table A-4).[30] Similarly, we did not find any significant changes in experiment 2 (see column 2 of Table A-5, columns 2 and 5 of Table A-6).

6.1.3 Agents' comments: rough estimates

For 3.4% of price quotes, agents noted that the shop had referred to the price quote it gave as a "rough estimate." To ensure that our findings are not driven by these observations, we reestimate our main specifications after eliminating all quotes that were classified as "rough estimates." The conclusions from experiment 1 do not change; see column 4 of Table A-3, columns 4 and 8 of Table A-4.[31] In experiment 2, the results in column 3 of Table A-5 and column 6 of Table A-6 are unchanged. However, the result that women are more likely to obtain a price concession in the no EP condition falls in magnitude and is no longer statistically significant even at the 10% confidence level (see column 3 of Table A-6).

6.1.4 Agents' comments: inconsistent call behavior

The final issue we detected in agents' comments was an inconsistency over time in how they applied scripts and specifically in whether they insisted on including particular aspects of the radiator repair

[29] The most significant differences in the results are in two p-values in Table A-3. The p-value for the "No expected price, Women" coefficient goes from just over .05 to just under, while the p-value for the "No expected price, Men" coefficient does the reverse.

[30] Again, the p-values of the "No expected price, Women" and "No expected price, Men" coefficients in Table A-3 vacillate between just over and just under .05, but with little change in the coefficients themselves.

[31] In Table A-3, the p-values on the "No expected price, Women" and "No expected price, Men" coefficients are now both just under .05.

in the recorded price quotes. To understand the context of this problem, recall that the script calls for a radiator replacement. In pre-testing we identified that a radiator replacement always requires new antifreeze and often (but not always) requires a cooling system flush. To reduce the variance in repair quotes we tried to standardize quotes by asking shops whether the quote included new antifreeze and a cooling system flush, and if not, to requote the price to include these items. To be precise, after the shop quoted the initial price the agent would say, "OK, thanks. Would that also include antifreeze liquid? [If the answer is "no"]: Can you give me the price including antifreeze? [If necessary, adjust price.] OK, thanks. Would that also include a cooling system flush? [If the answer is "no"]: Can you give me the price including a cooling system flush? [If necessary, adjust price.]"

Regrettably, in copying the script into a flowchart format, as requested by the call center, we instructed agents to say (differences are underlined), "OK, thanks. Would that also include antifreeze liquid? [If the answer is "no"]: Can you give me the price including antifreeze? [If necessary, adjust price.] OK, thanks. Would that also include a cooling system flush? [If the answer is "no"]: Can you give me the price including antifreeze? [If necessary, adjust price.]" In other words, while agents asked whether the price included a cooling system flush, they were *not* instructed to request a price that included the flush.

This mistake would be of serious concern if it had only been present in some scripts (conditions) but not others. However, the error was uniformly present across all conditions. The likely effect of the mistake is to increase the variance of price quotes, because some of the quotes would include a cooling system flush while others do not. This is likely to have decreased the power of our estimates.

Another consequence is that the mistake creates some room for interpretation by the agents. Specifically, while agents are instructed to follow the script exactly, some agents might perceive an inconsistency and therefore request an updated price including the cooling system flush, even if they were not instructed to do so. To investigate whether this occurred, we analyzed how frequently agents made reference in the comment field of the spreadsheet to accounting for a cooling system flush when requesting price quotes. The results from this analysis are in Table 9.

As we can see from these results, most agents never refer to a cooling system flush. Agents 5 and 8 are much more likely to mention accounting for a cooling system flush when requesting price quotes, but not equally across all weeks of the experiment. Agent 5 seems to have accounted for a cooling system flush during the first half of the experiment but followed the script more closely during the second half. Agent 8 exhibits the opposite pattern: the agent seems to have accounted for a cooling system flush during the second half of the experiment but not during the first half.[32]

[32]The reader may note a low percentage for agent 5 during weeks 1 and 3 and a high percentage for agent 8 during week 2. During these weeks these two agents made very few calls, resulting in high sampling variation.

Table 9: Percentage of calls in which agent notes that price quote includes cooling system flush

	Agent ID								
Week	1	2	3	4	5	6	7	8	9
1	0	0	0	0	0	0	0	0	0
2	0	0	0	0	17	0	0	14	0
3	0	0	0	0	0	0	0	0	0
4	1	0	0	0	19	1	0	0	1
5	0	0	0	0	27	2	0	2	0
6	2	0	0	0	24	0	0	0	0
7	0	0	0	0	9	0	0	5	0
8	0	0	0	0	5	1	0	0	0
9	0	0	0	0	0	0	0	0	0
10	0	0	0	0	0	0	0	0	0
11	0	0	0	0	6	0	0	6	0
12	0	0	0	0	2	1	0	15	0
13	0	0	0	0	6	0	0	28	3
14	0	0	0	0	8	1	0	12	8
15	0	0	0	0	3	1	0	15	0
16	0	0	0	0	0	0	0	11	0

Agents 5 and 8 are both male, a detail whose relevance will become apparent shortly.

We account for the variation in agent behavior by using three different approaches. First, we add the information in Table 9 as a control variable in all estimations. This means that for each agent in each week we control for the percentage of calls in which the agent refers to accounting for a cooling system flush when requesting price quotes. We also add the percentage squared in this specification. Second, we eliminate all observations for an agent in a week in which that agent refers to accounting for a cooling system flush more than 7.5% of the time, the 90th percentile in Table 9. Third, we eliminate all observations associated with agents 5 and 8 from the sample. That is, we remove observations where these agents' behavior was inconsistent compared to other agents, but also instances where they behaved in accordance with other agents. Removing these two agents from the dataset altogether eliminates 25% of the observations from our sample.

Table A-7 reports the results of comparing the quoted prices across the different conditions of experiment 1, using the three different methods to account for agents' inconsistent behavior with respect to cooling system flush. Column 1 presents the original results from column 4 of Table 2, and columns 2-4 reestimate this specification in three variations of accounting for flush references: controlling for flush reference, eliminating agent-weeks with a high frequency of flush references, and eliminating the two inconsistent agents altogether. The results remain unchanged. Specifically, there is no statistical difference between the average price quotes in the market-based EP and the no EP conditions. In addition, there is no change in the effect of the upward-biased EP condition, which remains about $35 higher than the market-based EP condition.

Table A-8 reports on the results of reestimating Equation 2 by incorporating the three approaches for accounting for a flush reference. Column 1 presents the original results from column 4 of Table 3, and columns 2-4 reestimate this specification in three variations of accounting for flush references. Upon first inspection, it appears that many of the condition–gender interaction terms that capture our main results have changed. Closer inspection reveals that the results have not changed by as much as initially appears.

We turn our attention to column 2 of Table A-8. The "% Flush Reference" and "% Flush Reference Squared" coefficients imply that the initial price quotes for an agent-week in which the agent refers to a cooling system flush in 5% of his calls will be higher by \$21.38 on average. For a 10% flush reference rate, the implied average effect on the initial price quote is a \$33.61 increase; \$30.64 for a flush reference rate of 20%.

The second point to note is that agents 5 and 8, the agents who had the highest flush reference rates, were male. Given the effects described in the previous paragraph, these agents may well have recorded higher average initial quotes than the remaining three male agents, since agents 5 and 8 appear to have more often recorded a price quote that included a cooling system flush, an add-on that would increase a price quote.

This means that in column 4 of Table A-8, the specification in which we eliminate agents 5 and 8 from the sample, we redefine our benchmark group (male agents) to be a subgroup of male agents who would be expected to obtain lower price quotes on average (because they did not include cooling system flushes) than did agents 5 and 8. This can be seen in the estimated constant term of \$370 (which captures the average for male callers in the market-based EP condition) in column 4 compared to an estimated constant term of \$388 in column 1.

In column 4, many of the condition–gender interaction terms have higher estimated coefficients than in column 1. But this is in large part because they estimate the incremental effect relative to the "left out" group (male callers in the market-based EP condition), which is a lower average price quote group in column 4 than in column 1 because the (male) high flush reference agents are not included in the sample in column 4. These effects can be seen visually in Figure 3, which plots the average initial price quotes for each of our six condition–gender interactions as predicted by the estimates in Table A-8. For each condition-gender combination, the bar on the left shows the predicted initial price quotes based on the estimates in column 1, while the bar on the right is based on column 4. It is noteworthy that the predicted average initial price quotes for women are very similar in the two specification, which we might have expected since the sample of women callers did not change between columns 1 and 4.

What this tells us is that the differences between outcomes obtained by men and women, and the differences in outcomes between the upward-biased and market-based conations may be somewhat bigger than what we have reported in the main section of the paper. However, this is for the very simple reason that some of the male agents in the sample had artificially high initial price quotes because they were more likely to include a cooling system flush in the price quote they recorded.

Figure 3: Predicted average initial price quotes by condition and gender, full sample ("Col 1") and subsample ("Col 4") estimates

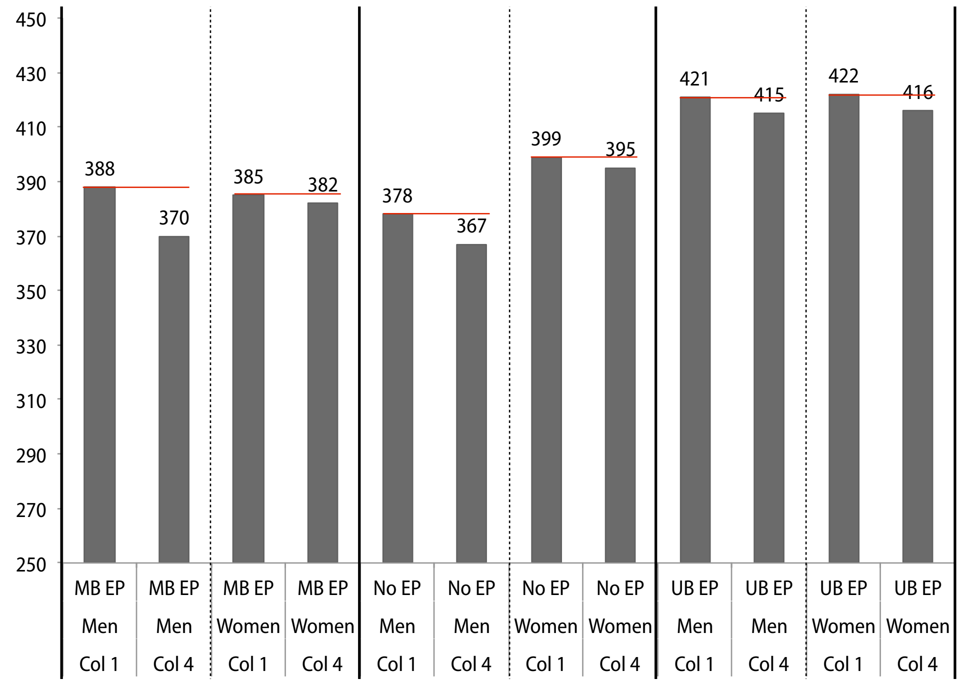

Table A-9 reports on the results of price concession incidence and levels, accounting for the flush references. Columns 1 and 5 present the original results from columns 4 and 5 of Table 5, and columns 2-4 and 6-8 reestimate these specifications in three variations of accounting for flush references. The results after adding the flush reference variables as controls and eliminating agents in weeks where their relative frequency of flush reference was high are consistent with the original results that overall, women are more likely to obtain price concessions. In these specifications, the result for women in the no EP condition becomes stronger ($p<0.05$). In the specification where we eliminate agents 5 and 8, the directional result that women are more likely to obtain price concessions remains, but the results are statistically weaker for all of the experimental conditions, and insignificant for the no EP condition.

Table A-10 and Table A-11 report the analysis of experiment 2 while accounting for the flush reference. Column 1 in Table A-10 presents the original results from column 3 of Table 6, and columns 2-4 reestimate this specification in three variations of accounting for flush references. As in Table 6, the price quotes in the downward-biased EP condition are not statistically different than in the no EP condition. When adding the condition and gender interactions, we again do not find significant differences between women and men.

Table A-11 reports the results of price concession incidence and levels in experiment 2, accounting for the flush references. Columns 1 and 5 present the original results from columns 2 and 3 of Table 8, and columns 2-4 and 6-8 reestimate these specifications in three variations while accounting for flush references. The (statistically weak) result that women in the no EP condition in experiment 2 are more likely to obtain price concessions no longer holds in experiment 2 when accounting for flush reference, although the result does hold for experiment 1 when accounting for flush reference, as described previously.

6.2 Pooling data from experiments 1 and 2

In this section, we pool data from experiments 1 and 2, with the caveat that these experiments do not provide clearly valid counterfactuals for each other. Recall that a) the two experiments were conducted using different DMAs (experiment 1 was conducted in higher-population DMAs than experiment 2); and b) experiment 2 started after most of the data for experiment 1 was collected. The two experiments ran concurrently only for a period of three weeks, a period during which we collected the first (and only) call to each shop in experiment 2, but during which we were making second calls to shops in experiment 1.

In order to understand how much these differences matter, we begin by comparing the results in experiment 1 and experiment 2 for the no EP condition, which is the one condition present in both experiments. In Table 10 we report that the average initial price quote obtained in the no EP condition of experiment 1 was \$394.5, compared to \$398.8 obtained in the same condition in experiment 2. A t-test of means cannot reject that the two experiments had equal average initial price quotes in this condition (p-value = .31). Table 10 also reports the standard deviation of initial price quotes in the two experiments in the no EP condition; a variance ratio test fails to reject equal variances (p-value = .16). From this we conclude that it will not be obviously misleading to pool the data from experiments 1 and 2. We repeat our main analysis using the pooled dataset. The results are reported in Tables A-12, A-13, and A-14.

Table 10: Initial price quotes in no EP condition in experiments 1 and 2

	Mean	Std. Dev.
Experiment 1	394.5	108.4
Experiment 2	398.8	104.1

Table A-12 reports on the differences between experimental conditions in the pooled sample that includes all four conditions: market-based EP, no EP, upward-biased EP, and downward-biased EP for both experiments. In column 3 we reestimate the specification of column 4 in Table 2 using the pooled sample, and in columns 4-6 we repeat the specification, using the three different strategies to account for flush references. Across all of these specifications, the results of the pooled regression do not change our prior conclusions. In particular, we estimate that initial price quotes in the upward-biased condition are \$35 higher on average than initial price quotes in the market-based condition (similar to our estimate of \$34 in experiment 1, reported for comparison in column 1 of Table A-12). In addition, we do not find statistically significant differences between initial price in the market-based EP condition and initial prices in either the no EP condition or the downward-biased EP conditions (similar to our findings in experiments 1 and 2).

Table A-13 reports the results of reestimating Equation 2, which included condition–gender interactions, for the pooled sample. Column 3 presents the main results using the pooled sample. (For ease of comparison, column 1 re-reports the results of this specification from experiment 1—originally reported in column 4 of Table 3; column 2 re-reports the results from experiment 2—originally reported in column 3 of Table 6.) Columns 4-6 reestimate condition–gender interaction specification using the three variations to account for flush references. Without accounting for flush references, the result that women in the no EP condition are quoted higher prices than men in the same condition and higher prices than men in the market-based condition is no longer statistically significant. Additionally, the result that men in the no EP condition are quoted lower prices than men in the market-based EP condition is also no longer statistically significant. However, once we account for flush references, the result that women in the no EP condition are quoted higher prices than both men in the no EP and men in the market-based EP conditions returns to statistical significance.

Table A-14 reports the results of price concession incidence and levels, both with and without accounting for the flush references. Columns 1 and 5 present the results using the full sample with the specification of columns 4 and 5 of Table 5, and columns 2-4 and 6-8 reestimate these specifications with three variations of accounting for flush references. Overall, women in the market-based EP and women in the upward-biased EP conditions are more likely to obtain price concessions. However, the result from experiments 1 and 2 showing women in the no EP condition are also more likely to obtain price concessions holds only in columns 1 and 2 of Table A-14.

6.3 Summary

In summary, the majority of our results are robust to different considerations about the execution of the experiment. Regarding the initial price quote results, we find, across all our specifications, that the price quotes in the upward-biased EP condition are higher than in the other conditions by about \$35. In addition, we consistently find that women in the no EP condition are quoted higher prices than men in that condition, and than men and women in the market-based EP condition. The only specification where we did not find this result was when pooling experiment 1 and experiment 2 without applying any corrections for the agent inconsistency issue. While we did find some evidence in experiment 1 that men in the no EP condition were quoted lower prices than men in the market-based EP condition, that result was not robust to different specifications, although the direction of the results remained throughout most of the specifications.

Regarding the price concession results, we find, across all specifications, that women in the market-based EP and the upward-based EP conditions are more likely to obtain positive price concessions compared to men in those conditions. We also find in experiment 1 and in its subsequent robustness tests, that women in the no EP condition are also more likely to obtain price concession. However, once we pool the data, that result is maintained only in some of the specifications. Recall that the result that women in the no EP condition are more likely to obtain price concession for experiment 2 are not robust to accounting for inconsistency in agents' behavior, which could explain why the result does not hold when pooling the sample.

7 Concluding remarks

In this paper we have presented the results of an experiment designed to measure the differences in the price quotes sellers offer to customers who present themselves as being uninformed, well-informed, or poorly informed about price. Our experiment also varies the gender of buyers, in order to understand whether the effects of information differ across genders. By scripting the buyer's side of the negotiation, this experimental design focuses specifically on the seller's response to buyers who are differentially informed.

In our results we find that callers who ask for repair shop quotes after announcing that they have no idea what the repair should cost are quoted the same prices on average as callers who announce first that they have learned that the repair should cost a price that is approximately the market average. The overall averages, however, mask differences between genders: female callers who say they are uniformed about prices are quoted prices about $20 higher than male callers who present themselves the same way. However, there is no difference between genders for callers who present themselves as accurately informed about prices. There is also no difference between genders for callers who announce that they have learned that the repair should cost an amount that is much higher than the actual market average price.

We note that one way we could observe such an outcome would be if repair shops believe that women are on average less well informed about prices than men, and price accordingly when callers present no direct information about how informed they are. In order to explain the rest of our observed results, however, we must conclude that when presented with direct information that a caller is either well or poorly informed, the repair shops price according to that information, and no longer find gender a useful basis on which to price discriminate.

In our experiment, we also directed the callers to ask for price matches if their initial quote exceeded the expected price in the relevant information condition. Our results also show that shops are more willing to agree to a price concession the higher their initial price quote, and also more likely, in all information conditions, to agree to a price concession for a female than for a male caller. In addition, shops were more likely to agree to a price concession for a female caller if she first presented herself as well informed than if she first presented herself as uninformed, even though the price the shops were being asked to match was the same in those two conditions. These results together suggest that there is a double-benefit to women to presenting themselves as well informed in these negotiations: first, they obtain lower initial price quotes on average, and second, if they obtain a high initial price quote, they are more likely to be able to negotiate a price concession.

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Appendix

Outline of call center scripts

- Dial number
  - "Hello, my name is John/Jennifer. I have a leak in the radiator on my 2003 Camry and I need to replace it. Is that something you work on?"
- If the answer is no
  - "Thank you" (and hang up)
- If the answer is yes
  - "So I have a 6 Cylinder 2003 Toyota Camry LE."
  - *No Expected Price Condition:* "I have no idea how much it is to replace a radiator."
  - *Market-Based Price Condition:* "I just visited the website AutoMD.com, and for this area they say the cost should be $365 to replace the radiator on my car."
  - *Upward-Biased Price Condition:* "I just visited the website AutoMD.com, and for this area they say the cost should be $510 to replace the radiator on my car."
  - *Downward-Biased Price Condition:* "I just visited the website AutoMD.com, and for this area they say the cost should be $310 to replace the radiator on my car."
  - "Could you tell me how much you charge?"
- If shop refuses to give quote over the phone type "Refused" in column I (do not leave column empty).
  - "Thank you" (and hang up)
- Otherwise, insert amount into column I.
  - "OK, thanks. Would that also include antifreeze liquid?"
- If the answer is "no":
  - "Can you give me the price including antifreeze?" (If necessary, update amount in column I.)
  - "OK, thanks. Would that also include a cooling system flush?"
- If the answer is "no":
  - "Can you give me the price including a cooling system flush?" (If necessary, update amount in column I.)
  - "OK, thanks. That is the total price without tax, right?"
- If the answer is "no":

  - "Can you give me the total price before tax?" (If necessary, update amount in column I.)
  - "Just to make sure: does the price include parts and labor?"

- If the answer is 'yes":
  - "OK, thank you"

- If the answer is "no":
  - "Can you give me the total price before tax?" (If necessary, update amount in column I.)

- If the price in column I is LESS than or EQUAL to column J: ($365 for *Market-Based Price* and *No Expected Price Condition*, $510 for *Upward-Biased Price Condition*, $310 for *Downward-Biased Price Condition*)
  - "All right, let me think about this" (and hang up)

- If the price in column I is MORE than column J:
  - "So, I have a question:"
  - *No Expected Price Condition:* "I just visited the website AutoMD.com, and for this area they say the cost should be $365. Would you match this price?"
  - *Market-Based Price Condition:* "Would you match the price of $365 that the website AutoMD said it should cost in this area?"
  - *Upward-Biased Price Condition:* "Would you match the price of $510 that the website AutoMD said it should cost in this area?"
  - *Downward-Biased Price Condition:* "Would you match the price of $310 that the website AutoMD said it should cost in this area?"

- If the answer is "yes," type "yes" in column K

- If the answer is "no" with a REVISED offer, type amount of revised offer in column K. If they mention certain terms (proof/local shops) write them.

- If the answer is "no" WITHOUT a revised offer, type "no" in column K. If they mention certain terms (proof/local shops) write them.

- "All right, let me think about this" (and hang up)

Additional Tables

Table A-1: Analysis of shops with two quotes

Dependent Variable	(1) Men Pricequote	(2) Women Pricequote
No EP	-2.8	7.5
	(5.1)	(4.7)
Upward-Biased EP	25**	27**
	(5.1)	(5.8)
Second Call	22	12
	(21)	(21)
Week Fixed Effects	✓	✓
Shop Fixed Effects	✓	✓
Constant	395**	460**
	(23)	(32)
Observations	1,626	2,024
R-squared	0.842	0.795

Table A-2: Price concession results with controls

Dependent Variable	(1) Price Concession	(2) Price Concession > 0	(3) Price Concession if Concession > 0
No EP		.0036	12
		(.034)	(7.8)
Upward-Biased EP		.064	5.7
		(.062)	(8)
Women	.096**		
	(.021)		
No EP, Women		.05	-2.9
		(.031)	(5.4)
Market-Based EP, Women		.11**	9.2
		(.033)	(6.7)
Upward-Biased EP, Women		.22**	5.2
		(.074)	(5.9)
RequestedConcession Decile 2	-.048	-.045	20**
	(.046)	(.047)	(2.2)
RequestedConcession Decile 3	-.21**	-.25**	31**
	(.073)	(.067)	(4.6)
RequestedConcession Decile 4	-.12*	-.12*	37**
	(.05)	(.05)	(3.2)
RequestedConcession Decile 5	-.24**	-.23**	59**
	(.045)	(.046)	(3.6)
RequestedConcession Decile 6	-.25**	-.24**	71**
	(.052)	(.053)	(6.4)
RequestedConcession Decile 7	-.22**	-.21**	99**
	(.049)	(.049)	(5.2)
RequestedConcession Decile 8	-.27**	-.27**	124**
	(.047)	(.047)	(7.3)
RequestedConcession Decile 9	-.29**	-.27**	169**
	(.046)	(.047)	(6.6)
RequestedConcession Decile 10	-.26**	-.26**	255**
	(.047)	(.048)	(18)
Second Call	.046	.049	9.2
	(.04)	(.04)	(6.6)
Week Fixed Effects	✓	✓	✓
DMA Fixed Effects	✓	✓	✓
Constant	.63**	.63**	6.3
	(.12)	(.12)	(9.4)
Observations	1,738	1,738	458
R-squared	0.104	0.117	0.833

Table A-3: Initial price quote results, robustness checks

Robustness Check	(1) *Original Results*	(2) Script Code	(3) Call-Back	(4) Rough Estimate
Dependent Variable	*Price Quote*	Price Quote	Price Quote	Price Quote
Market-Based EP, Women	*-2.7*	-3.7	-3.3	-2.8
	(5.4)	(5.5)	(6)	(5.6)
No EP, Women	*11**	9.7+	13*	11+
	(5.5)	(5.6)	(6.2)	(5.6)
No EP, Men	*-10+*	-12*	-11+	-11+
	(5.6)	(5.8)	(6.2)	(5.8)
Upward-Biased EP, Women	*34***	33**	34**	33**
	(5.5)	(5.6)	(5.9)	(5.6)
Upward-Biased EP, Men	*33***	31**	33**	32**
	(5.6)	(5.6)	(6.1)	(5.8)
Second Call	*11**	11*	13*	11*
	(5.4)	(5.5)	(6.1)	(5.5)
Week Fixed Effects	✓	✓	✓	✓
DMA Fixed Effects	✓	✓	✓	✓
Constant	*388***	389**	389**	390**
	(17)	(17)	(19)	(17)
Observations	*4,603*	4,439	4,022	4,455
R-squared	*0.071*	0.071	0.073	0.073

Table A-4: Price concession results, robustness checks

Robustness Check	*(1) Original*	(2) Script Code	(3) Call-Back	(4) Rough Estimate	*(5) Original*	(6) Script Code	(7) Call-Back	(8) Rough Estimate
Dependent Variable	*I(Price Concession > 0)*	I(Price Concession > 0)	I(Price Concession > 0)	I(Price Concession > 0)	*PriceConcession if Concession > 0*	PriceConcession if Concession > 0	PriceConcession if Concession > 0	PriceConcession if Concession > 0
No EP	*.013*	.0052	-.0048	.0012	*4.1*	3.8	6.1	3.8
	(.03)	(.031)	(.033)	(.032)	*(6.5)*	(6.5)	(7.7)	(6.7)
Upward-Biased EP	*.076*	.079	.081	.061	*5.2*	5.5	11	5.3
	(.061)	(.062)	(.066)	(.062)	*(7.2)*	(7.2)	(7.5)	(7.4)
No EP, Women	*.057**	.065*	.071*	.053+	*3.4*	3.8	5.3	3.8
	(.029)	(.029)	(.03)	(.03)	*(4.8)*	(5)	(5.2)	(5)
Market-Based EP, Women	*.12***	.12**	.11**	.1**	*5.3*	5.2	6.9	5.3
	(.031)	(.032)	(.034)	(.032)	*(6.1)*	(6.2)	(7.4)	(6.3)
Upward-Biased EP, Women	*.24***	.24**	.23**	.24**	*5.4*	5.5	-2	5.5
	(.074)	(.076)	(.079)	(.075)	*(4.7)*	(4.7)	(2.7)	(4.7)
RequestedConcession Decile 2	*-.052*	-.063	-.052	-.058	*19***	19**	20**	19**
	(.047)	(.048)	(.05)	(.049)	*(1.1)*	(1.2)	(1.2)	(1.2)
RequestedConcession Decile 3	*-.26***	-.27**	-.25**	-.27**	*27***	27**	30**	27**
	(.064)	(.065)	(.071)	(.065)	*(2.6)*	(2.7)	(2.1)	(2.6)
RequestedConcession Decile 4	*-.14***	-.15**	-.15**	-.15**	*36***	36**	40**	36**
	(.05)	(.051)	(.054)	(.052)	*(2.1)*	(2.2)	(2.1)	(2.2)
RequestedConcession Decile 5	*-.24***	-.26**	-.25**	-.25**	*57***	58**	56**	57**
	(.045)	(.046)	(.048)	(.047)	*(2.9)*	(2.8)	(3.4)	(2.9)
RequestedConcession Decile 6	*-.27***	-.28**	-.25**	-.27**	*73***	73**	76**	73**
	(.053)	(.054)	(.058)	(.055)	*(5.2)*	(5.2)	(5.5)	(5.1)
RequestedConcession Decile 7	*-.22***	-.24**	-.19**	-.23**	*94***	93**	95**	94**
	(.049)	(.05)	(.054)	(.051)	*(4.8)*	(5.2)	(4.7)	(4.8)
RequestedConcession Decile 8	*-.26***	-.28**	-.24**	-.27**	*121***	121**	126**	121**
	(.048)	(.049)	(.052)	(.05)	*(7)*	(7.2)	(5.8)	(7.1)
RequestedConcession Decile 9	*-.28***	-.29**	-.26**	-.28**	*171***	170**	171**	171**
	(.047)	(.048)	(.051)	(.049)	*(6.7)*	(6.9)	(7.1)	(6.7)
RequestedConcession Decile 10	*-.27***	-.28**	-.26**	-.28**	*256***	256**	255**	256**
	(.048)	(.049)	(.051)	(.049)	*(19)*	(20)	(21)	(19)
Constant	*.37***	.39**	.37**	.4**	*4.1*	4.2	1.7	4
	(.041)	(.043)	(.045)	(.044)	*(5.8)*	(5.8)	(7)	(6)
Observations	*1,738*	1,669	1,513	1,682	*458*	443	397	450
R-squared	*0.081*	0.086	0.077	0.081	*0.814*	0.807	0.810	0.813

Table A-5: Experiment 2, initial price quote results, robustness checks

Robustness Check	(1) *Original Results*	(2) Call-Back	(3) Rough Estimate
Dependent Variable	*Price Quote*	Price Quote	Price Quote
Downward-Biased EP	*-8.9*	-14	-8.9
	(8)	(9.3)	(8.1)
No EP, Women	*-5.7*	-11	-5.2
	(6.9)	(7.9)	(7)
Downward-Biased EP, Women	*-5.2*	-3	-4
	(6.9)	(7.7)	(7.1)
Week 13	*-3.6*	-3.2	-4.4
	(6.4)	(7)	(6.5)
Week 14	*-1.5*	-3.8	-1.9
	(7.1)	(7.9)	(7.2)
Week 15	*-6.5*	-11	-5.4
	(8.6)	(9.5)	(8.9)
Week 16	*-22*	-38**	-23
	(14)	(15)	(14)
DMA Fixed Effects	✓	✓	✓
Constant	*388***	395**	389**
	(14)	(16)	(14)
Observations	*1,941*	1,629	1,864
R-squared	*0.056*	0.061	0.055

Table A-6: Experiment 2, price concession results, robustness checks

Robustness Check	*(1) Original*	(2) Call-Back	(3) Rough Estimate	*(4) Original*	(5) Call-Back	(6) Rough Estimate
Dependent Variable	*I(Price Concession > 0)*	I(Price Concession > 0)	I(Price Concession > 0)	*PriceConcession if Concession > 0*	PriceConcession if Concession > 0	PriceConcession if Concession > 0
Downward-Biased EP	*.03*	.037	.016	*2.5*	3.5	-1.1
	(.036)	(.043)	(.04)	*(8.4)*	(10)	(8.4)
No EP, Women	*.062+*	.068+	.044	*-.26*	-2.5	-.83
	(.035)	(.039)	(.037)	*(7.5)*	(9.3)	(7.5)
Downward-Based EP, Women	*-.018*	-.014	-.022	*4.9*	3.1	7.9
	(.029)	(.033)	(.03)	*(6.2)*	(7.1)	(6.3)
RequestedConcession Decile 2	*-.16***	-.22**	-.18**	*18***	17**	17**
	(.057)	(.063)	(.06)	*(1.9)*	(2)	(1.9)
RequestedConcession Decile 3	*-.24***	-.25**	-.26**	*25***	23**	25**
	(.057)	(.067)	(.06)	*(3.1)*	(3.5)	(3.1)
RequestedConcession Decile 4	*-.19***	-.21**	-.23**	*38***	37**	38**
	(.057)	(.064)	(.059)	*(3.2)*	(3.7)	(3.3)
RequestedConcession Decile 5	*-.31***	-.32**	-.34**	*61***	60**	61**
	(.052)	(.06)	(.054)	*(4.3)*	(4.7)	(4.7)
RequestedConcession Decile 6	*-.29***	-.31**	-.32**	*66***	63**	66**
	(.052)	(.059)	(.054)	*(7.1)*	(7.9)	(7.4)
RequestedConcession Decile 7	*-.37***	-.4**	-.39**	*94***	96**	94**
	(.048)	(.054)	(.05)	*(8.3)*	(8.5)	(8)
RequestedConcession Decile 8	*-.32***	-.34**	-.35**	*131***	138**	131**
	(.053)	(.061)	(.056)	*(7.7)*	(2.4)	(7.5)
RequestedConcession Decile 9	*-.3***	-.32**	-.33**	*178***	175**	179**
	(.052)	(.06)	(.055)	*(12)*	(12)	(12)
RequestedConcession Decile 10	*-.3***	-.34**	-.33**	*210***	212**	203**
	(.052)	(.059)	(.054)	*(24)*	(27)	(24)
Constant	*.41***	.43**	.45**	*10*	12	11+
	(.048)	(.056)	(.052)	*(6.6)*	(8.3)	(6.6)
Observations	*1,259*	1,054	1,205	*236*	207	231
R-squared	*0.076*	0.077	0.084	*0.822*	0.818	0.823

Table A-7: Effects of information condition, robustness to inconsistent behavior

Robustness Check	*(1) Original Results*	(2) % Flush Reference	(3) Eliminate High Flush Reference Agent-Weeks	(4) Eliminate High Flush Reference Agents
Dependent Variable	*Price Quote*	Price Quote	Price Quote	Price Quote
No EP	*2.5*	3	3	5.4
	(4)	(4)	(4.2)	(4.5)
Upward-Biased EP	*35***	35**	33**	37**
	(4.1)	(4.1)	(4.3)	(4.9)
% Flush Reference		354**		
		(102)		
% Flush Reference Sq		-1299**		
		(445)		
Second Call	*13**	14**	12*	9.2
	(5.4)	(5.4)	(5.3)	(6.1)
Week Fixed Effects	✓	✓	✓	✓
DMA Fixed Effects	✓	✓	✓	✓
Constant	*388***	388**	387**	382**
	(17)	(17)	(17)	(17)
Observations	*4,603*	4,603	4,104	3,496
R-squared	*0.068*	0.071	0.069	0.077

Table A-8: Effects of information condition by gender, robustness to inconsistent behavior

Robustness Check	*(1)* *Original Results*	(2) % Flush Reference	(3) Eliminate High Flush Reference Agent-Weeks	(4) Eliminate High Flush Reference Agents
Dependent Variable	*Price Quote*	Price Quote	Price Quote	Price Quote
Market-Based EP, Women	*-2.7*	6.6	3.5	12+
	(5.4)	(5.8)	(6.1)	(6.8)
No EP, Women	*11**	20**	16**	25**
	(5.5)	(5.9)	(6)	(6.8)
No EP, Men	*-10+*	-7.9	-9.4	-3.3
	(5.6)	(5.6)	(6.3)	(7.5)
Upward-Biased EP, Women	*34***	43**	38**	46**
	(5.5)	(5.9)	(6)	(6.8)
Upward-Biased EP, Men	*33***	33**	31**	45**
	(5.6)	(5.5)	(6.4)	(8.8)
% Flush Reference		519**		
		(112)		
% Flush Reference Sq		-1829**		
		(472)		
Second Call	*11**	11*	8.8	7.4
	(5.4)	(5.4)	(5.4)	(6.1)
Week Fixed Effects	✓	✓	✓	✓
DMA Fixed Effects	✓	✓	✓	✓
Constant	*388***	381**	382**	370**
	(17)	(17)	(17)	(17)
Observations	*4,603*	4,603	4,104	3,496
R-squared	*0.071*	0.076	0.074	0.083

Table A-9: Price concession results, robustness to inconsistent behavior

Robustness Check	*(1) Original*	(2) %Flush Reference	(3) Eliminate High FR Agent-Weeks	(4) Eliminate High FRAgents	*(5) Original*	(6) %Flush Reference	(7) Eliminate High FR Agent-Weeks	(8) Eliminate High FR Agents
Dependent Variable	*I(Price Concession > 0)*	I(Price Concession > 0)	I(Price Concession > 0)	I(Price Concession > 0)	*PriceConcession if Concession > 0*	PriceConcession if Concession > 0	PriceConcession if Concession > 0	PriceConcession if Concession > 0
No EP	*.013*	.025	.006	.026	*4.1*	3.1	12	2.8
	(.03)	(.032)	(.034)	(.045)	*(6.5)*	(5.8)	(8.6)	(7.2)
Upward-Biased EP	*.076*	.081	-.014	.045	*5.2*	4.6	12	1
	(.061)	(.06)	(.066)	(.098)	*(7.2)*	(7.6)	(11)	(11)
No EP, Women	*.057**	.074*	.066*	.028	*3.4*	4.1	3.8	4.1
	(.029)	(.03)	(.03)	(.034)	*(4.8)*	(5.2)	(5.2)	(5.3)
Market-Based EP, Women	*.12***	.15**	.12**	.1*	*5.3*	5.2	13	5.2
	(.031)	(.035)	(.034)	(.042)	*(6.1)*	(5.9)	(8.1)	(7)
Upward-Biased EP, Women	*.24***	.27**	.33**	.25*	*5.4*	5.7	7.1	9.4
	(.074)	(.074)	(.077)	(.1)	*(4.7)*	(6.9)	(8.5)	(8.7)
% Flush Reference		.84				-71		
		(.71)				(150)		
% Flush Reference Sq		-1.8				427		
		(3)				(595)		
RequestedConcession Decile 2	*-.052*	-.049	-.051	-.066	*19***	19**	20**	19**
	(.047)	(.046)	(.049)	(.053)	*(1.1)*	(1.2)	(1.4)	(1.2)
RequestedConcession Decile 3	*-.26***	-.26**	-.25**	-.25**	*27***	28**	27**	27**
	(.064)	(.063)	(.067)	(.075)	*(2.6)*	(2.6)	(3)	(3)
RequestedConcession Decile 4	*-.14***	-.14**	-.13*	-.11+	*36***	37**	36**	36**
	(.05)	(.05)	(.053)	(.059)	*(2.1)*	(2.2)	(2.3)	(2.3)
RequestedConcession Decile 5	*-.24***	-.24**	-.25**	-.25**	*57***	57**	57**	57**
	(.045)	(.045)	(.047)	(.052)	*(2.9)*	(3)	(3.3)	(3.2)
RequestedConcession Decile 6	*-.27***	-.26**	-.25**	-.27**	*73***	73**	74**	76**
	(.053)	(.053)	(.057)	(.06)	*(5.2)*	(5.1)	(4.9)	(4.5)
RequestedConcession Decile 7	*-.22***	-.21**	-.21**	-.23**	*94***	94**	94**	93**
	(.049)	(.049)	(.052)	(.056)	*(4.8)*	(4.8)	(5)	(5.1)
RequestedConcession Decile 8	*-.26***	-.26**	-.26**	-.28**	*121***	122**	122**	123**
	(.048)	(.048)	(.05)	(.055)	*(7)*	(7.1)	(7.6)	(7.8)
RequestedConcession Decile 9	*-.28***	-.28**	-.28**	-.3**	*171***	172**	169**	169**
	(.047)	(.046)	(.05)	(.053)	*(6.7)*	(6.7)	(7.4)	(7.5)
RequestedConcession Decile 10	*-.27***	-.27**	-.27**	-.29**	*256***	257**	252**	266**
	(.048)	(.048)	(.051)	(.055)	*(19)*	(19)	(20)	(19)
Constant	*.37***	.34**	.37**	.4**	*4.1*	4	-3.9	4.1
	(.041)	(.045)	(.045)	(.053)	*(5.8)*	(5.7)	(8)	(6.8)
Observations	*1,738*	1,738	1,583	1,384	*458*	458	417	394
R-squared	*0.081*	0.084	0.084	0.081	*0.814*	0.815	0.803	0.844

Table A-10: Experiment 2, initial price quote results, robustness to inconsistent behavior

Robustness Check	(1) *Original*	(2) %Flush Reference	(3) Eliminate High FR Agent-Weeks	(4) Eliminate High FR Agents
Dependent Variable	*Price Quote*	Price Quote	Price Quote	Price Quote
Downward-Biased EP	*-8.9*	.72	1.4	-16
	(8)	(8.8)	(9.9)	(15)
No EP, Women	*-5.7*	11	8.6	-10
	(6.9)	(9.4)	(8.9)	(13)
Downward-Biased EP, Women	*-5.2*	1.5	.46	.23
	(6.9)	(7.5)	(7.4)	(8.7)
% Flush Reference		300*		
		(143)		
% Flush Reference Sq		-690		
		(546)		
Week 13	*-3.6*	-5.4	-3.9	-5.2
	(6.4)	(6.5)	(6.6)	(7.7)
Week 14	*-1.5*	-7.7	-11	-12
	(7.1)	(7.3)	(8)	(8.6)
Week 15	*-6.5*	-11	-11	-14
	(8.6)	(8.8)	(9.2)	(11)
Week 16	*-22*	-24+	-23	-22
	(14)	(14)	(14)	(14)
DMA Fixed Effects	✓	✓	✓	✓
Constant	*388***	375**	378**	408**
	(14)	(15)	(15)	(18)
Observations	*1,941*	1,941	1,651	1,513
R-squared	*0.056*	0.059	0.070	0.072

Table A-11: Experiment 2, price concession results, robustness to inconsistent behavior

Robustness Check	*(1) Original*	(2) %Flush Reference	(3) Eliminate High FR Agent-Weeks	(4) Eliminate High FRAgents	*(5) Original*	(6) %Flush Reference	(7) Eliminate High FR Agent-Weeks	(8) Eliminate High FR Agents
Dependent Variable	*I(Price Concession > 0)*	I(Price Concession > 0)	I(Price Concession > 0)	I(Price Concession > 0)	*PriceConcession if Concession > 0*	PriceConcession if Concession > 0	PriceConcession if Concession > 0	PriceConcession if Concession > 0
Downward-Biased EP	*.03*	-.025	-.078	-.013	*2.5*	-2.1	.98	8.5
	(.036)	(.041)	(.062)	(.072)	*(8.4)*	(10)	(10)	(16)
No EP, Women	*.062+*	-.018	-.068	-.021	*-.26*	-6.7	-2	-1.6
	(.035)	(.049)	(.059)	(.067)	*(7.5)*	(10)	(9.5)	(15)
Downward-Based EP, Women	*-.018*	-.039	-.043	-.056	*4.9*	4.6	3.6	-4
	(.029)	(.033)	(.032)	(.039)	*(6.2)*	(6.2)	(6.4)	(6.6)
% Flush Reference		-.55				-306		
		(.56)				(238)		
% Flush Reference Sq		-1.4				1915		
		(1.8)				(1679)		
RequestedConcession Decile 2	*-.16***	-.16**	-.18**	-.15*	*18***	17**	18**	19**
	(.057)	(.057)	(.064)	(.067)	*(1.9)*	(2.1)	(1.9)	(2.1)
RequestedConcession Decile 3	*-.24***	-.23**	-.29**	-.22**	*25***	26**	25**	28**
	(.057)	(.057)	(.065)	(.071)	*(3.1)*	(3.3)	(2.9)	(2.8)
RequestedConcession Decile 4	*-.19***	-.2**	-.27**	-.24**	*38***	39**	39**	43**
	(.057)	(.057)	(.061)	(.064)	*(3.2)*	(3.2)	(3.3)	(2.7)
RequestedConcession Decile 5	*-.31***	-.3**	-.37**	-.34**	*61***	61**	62**	62**
	(.052)	(.052)	(.057)	(.06)	*(4.3)*	(4.3)	(4.1)	(4.3)
RequestedConcession Decile 6	*-.29***	-.28**	-.34**	-.35**	*66***	67**	69**	71**
	(.052)	(.052)	(.058)	(.059)	*(7.1)*	(7.2)	(7.7)	(9.5)
RequestedConcession Decile 7	*-.37***	-.36**	-.43**	-.39**	*94***	98**	90**	101**
	(.048)	(.048)	(.052)	(.054)	*(8.3)*	(9.7)	(9.7)	(7.2)
RequestedConcession Decile 8	*-.32***	-.32**	-.36**	-.34**	*131***	130**	131**	132**
	(.053)	(.053)	(.06)	(.061)	*(7.7)*	(7.8)	(7.8)	(8.2)
RequestedConcession Decile 9	*-.3***	-.3**	-.37**	-.35**	*178***	181**	175**	200**
	(.052)	(.052)	(.057)	(.058)	*(12)*	(10)	(14)	(4.5)
RequestedConcession Decile 10	*-.3***	-.3**	-.34**	-.33**	*210***	210**	210**	206**
	(.052)	(.052)	(.058)	(.059)	*(24)*	(24)	(24)	(25)
Constant	*.41***	.48**	.58**	.51**	*10*	17+	12	9.5
	(.048)	(.059)	(.07)	(.078)	*(6.6)*	(9.2)	(8.6)	(15)
Observations	*1,259*	1,259	1,063	984	*236*	236	215	199
R-squared	*0.076*	0.085	0.103	0.094	*0.822*	0.823	0.815	0.854

Table A-12: Effects of information condition, pooled data

Robustness Check	*(1) Experiment 1*	*(2) Experiment 2*	(3) Pooled	(4) Pooled % Flush Reference	(5) Pooled Eliminate High Flush Reference Agent-Weeks	(6) Pooled Eliminate High Flush Reference Agents
Dependent Variable	*Price Quote*	*Price Quote*	Price Quote	Price Quote	Price Quote	Price Quote
No EP	*2.5*		2.3	3.1	3.4	5.8
	(4)		(4)	(4)	(4.1)	(4.5)
Upward-Biased EP	*35***		35**	35**	34**	37**
	(4.1)		(4)	(4)	(4.3)	(4.9)
Downward-Biased EP		*-8.4*	-4.2	-2.3	-2.9	-2.6
		(5.2)	(6.3)	(6.4)	(6.7)	(7.3)
% Flush Reference				294**		
				(74)		
% Flush Reference Sq				-981**		
				(324)		
Second Call	*13**		13*	14**	12*	9.2
	(5.4)		(5.4)	(5.4)	(5.3)	(6.2)
Week Fixed Effects	✓	✓	✓	✓	✓	✓
DMA Fixed Effects	✓	✓	✓	✓	✓	✓
Constant	*388***	*388***	404**	404**	406**	412**
	(17)	*(14)*	(22)	(22)	(23)	(23)
Observations	*4,603*	*1,941*	6,544	6,544	5,755	5,009
R-squared	*0.068*	*0.056*	0.065	0.068	0.071	0.076

Table A-13: Effects of information condition by gender, pooled data

Robustness Check	*(1)* *Experiment 1*	*(2)* *Experiment 2*	(3) Pooled	(4) Pooled % Flush Reference	(5) Pooled Eliminate High Flush Reference Agent-Weeks	(6) Pooled Eliminate High Flush Reference Agents
Dependent Variable	*Price Quote*	*Price Quote*	Price Quote	Price Quote	Price Quote	Price Quote
Market-Based EP, Women	*-2.7*		-3	6.1	2.4	11+
	(5.4)		(5.4)	(5.7)	(6)	(6.7)
No EP, Women	*11**	*-5.7*	5.3	17**	14*	21**
	(5.5)	*(6.9)*	(5.2)	(5.6)	(5.8)	(6.7)
No EP, Men	*-10+*		-5	-5.2	-7.2	.92
	(5.6)		(5.3)	(5.3)	(6)	(7.3)
Upward-Biased EP, Women	*34***		33**	43**	38**	45**
	(5.5)		(5.5)	(5.8)	(6)	(6.8)
Upward-Biased EP, Men	*33***		32**	33**	30**	44**
	(5.6)		(5.6)	(5.5)	(6.4)	(8.9)
Downward-Biased EP, Women		*-14+*	-7	6	3.9	11
		(7.4)	(7.5)	(7.9)	(8.2)	(9.1)
Downward-Biased EP, Men		*-8.9*	-1.7	2.7	3.7	11
		(8)	(8)	(8)	(8.7)	(11)
% Flush Reference				457**		
				(84)		
% Flush Reference Sq				-1480**		
				(351)		
Second Call	*11**		12*	11*	9.1+	7.9
	(5.4)		(5.4)	(5.4)	(5.4)	(6.1)
Week Fixed Effects	✓	✓	✓	✓	✓	✓
DMA Fixed Effects	✓	✓	✓	✓	✓	✓
Constant	*388***	*388***	405**	394**	396**	396**
	(17)	*(14)*	(22)	(22)	(23)	(24)
Observations	*4,603*	*1,941*	6,544	6,544	5,755	5,009
R-squared	*0.071*	*0.066*	0.066	0.072	0.074	0.079

Table A-14: Price concession results, pooled data

Robustness Check	(1)	(2) %Flush Reference	(3) Eliminate High FR Agent-Weeks	(4) Eliminate High FRAgents	(5)	(6) %Flush Reference	(7) Eliminate High FR Agent-Weeks	(8) Eliminate High FR Agents
Dependent Variable	I(Price Concession > 0)	I(Price Concession > 0)	I(Price Concession > 0)	I(Price Concession > 0)	PriceConcession if Concession > 0	PriceConcession if Concession > 0	PriceConcession if Concession > 0	PriceConcession if Concession > 0
No EP	.0024	.0054	.03	.036	1.2	.36	9	-.014
	(.027)	(.028)	(.033)	(.043)	(6.4)	(6.3)	(8.3)	(7.2)
Upward-Biased EP	.065	.068	-.022	.035	2.9	3	8.8	-2
	(.061)	(.061)	(.067)	(.1)	(7.5)	(7.7)	(12)	(11)
Downward-Biased EP	.0083	.0072	.028	.032	-2.4	-3.4	5	4.3
	(.032)	(.032)	(.037)	(.049)	(8.7)	(8.8)	(10)	(8.6)
No EP, Women	.054*	.051*	.03	.0067	1	-.49	.4	1.3
	(.022)	(.025)	(.025)	(.029)	(3.9)	(4.4)	(4.4)	(4.8)
Market-Based EP, Women	.12**	.12**	.12**	.11*	2.6	.51	9.7	1.9
	(.031)	(.033)	(.034)	(.042)	(6)	(5.9)	(7.8)	(6.5)
Upward-Biased EP, Women	.25**	.25**	.34**	.26*	3.9	1.2	5.1	7.8
	(.074)	(.074)	(.078)	(.1)	(5.1)	(6.4)	(9)	(9.4)
Downward-Biased EP, Women	-.018	-.017	-.041	-.056	8.7	8.3	9	.87
	(.029)	(.031)	(.032)	(.039)	(6.4)	(6.3)	(6.8)	(6.2)
% Flush Reference		.18				-154		
		(.41)				(98)		
% Flush Reference Sq		-1.3				709+		
		(1.7)				(396)		
RequestedConcession Decile 2	-.096**	-.096**	-.11**	-.1*	18**	18**	19**	19**
	(.036)	(.036)	(.039)	(.041)	(.97)	(1)	(1.1)	(.87)
RequestedConcession Decile 3	-.21**	-.21**	-.22**	-.18**	28**	28**	28**	29**
	(.044)	(.044)	(.048)	(.054)	(1.8)	(1.9)	(2)	(1.7)
RequestedConcession Decile 4	-.19**	-.19**	-.22**	-.2**	39**	40**	38**	40**
	(.036)	(.036)	(.039)	(.042)	(1.8)	(1.9)	(2)	(1.8)
RequestedConcession Decile 5	-.27**	-.27**	-.3**	-.28**	58**	58**	58**	59**
	(.035)	(.035)	(.038)	(.041)	(2.6)	(2.7)	(2.8)	(2.7)
RequestedConcession Decile 6	-.28**	-.28**	-.3**	-.31**	70**	71**	72**	75**
	(.037)	(.037)	(.04)	(.042)	(4.2)	(4.2)	(4.2)	(4.4)
RequestedConcession Decile 7	-.28**	-.28**	-.3**	-.3**	94**	94**	94**	94**
	(.034)	(.034)	(.037)	(.039)	(4)	(4)	(4.2)	(4.2)
RequestedConcession Decile 8	-.29**	-.29**	-.31**	-.31**	127**	128**	128**	130**
	(.037)	(.037)	(.039)	(.042)	(5.5)	(5.5)	(5.9)	(5.6)
RequestedConcession Decile 9	-.3**	-.3**	-.32**	-.33**	172**	173**	170**	179**
	(.035)	(.035)	(.037)	(.039)	(7)	(6.8)	(7.8)	(6.1)
RequestedConcession Decile 10	-.28**	-.28**	-.29**	-.3**	241**	242**	238**	243**
	(.036)	(.036)	(.039)	(.041)	(14)	(14)	(15)	(15)
Constant	.4**	.4**	.41**	.43**	8.3	11+	1.2	8
	(.034)	(.036)	(.038)	(.046)	(5.8)	(5.7)	(7.6)	(6.4)
Observations	2,997	2,997	2,646	2,368	694	694	632	593
R-squared	0.082	0.082	0.088	0.088	0.814	0.815	0.804	0.836