Intermediaries in Bargaining: Evidence from Business-to-Business Used-Car Inventory Negotiations*

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Abstract

We analyze 75,000 alternating-offer, business-to-business negotiations in the wholesale used-car market, each mediated (over the phone) by a third-party. We find that it matters who intermediates: high-performing mediators are 18.07 percentage points more likely to close a deal than low performers, and these differences are not due to statistical noise. Better mediators achieve trades quickly, and mediator skill improves with experience. We estimate buyer and seller value distributions using a structural model of two-sided incomplete-information in which mediators of different skill levels correspond to different equilibria. High-skilled mediators improve efficiency and overcome some of the inefficiency inherent in incomplete-information settings.

JEL Codes: C7, D8, L1, L81

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1 Introduction

Many real-world bargaining situations—among nations, businesses, investors, consumers, or litigants—involve a third-party *intermediary* or *mediator*. These mediators are often at the center of massive transactions and are highly paid for their role: consider investment banks handling firm acquisitions or lawyers mediating pre-trial settlement. A 2011 survey of Fortune 1000 companies found that 98% of respondents used a mediator at least once in the previous three years (Stipanowich and Lamare 2014).[1] To date, however, there is little real-world evidence on whether intermediaries make a difference for negotiation outcomes and welfare.

We analyze 75,000 business-to-business negotiations from the wholesale used-car market. The industry consists of hundreds of auction house locations nationwide that facilitate trade between manufacturers, fleet companies, banks, and used and new car dealerships, with more than $110 billion worth of cars traded annually.[2] Each car is auctioned individually in a rapid process. When the auction price (the highest bid at the auction) fails to reach the seller's secret reserve price, it frequently occurs that a *mediator* facilitates bargaining between the highest bidder and seller over the phone. The buyers, sellers, and mediators in this market are professionals; they frequently engage in negotiations, as each auction house location sells hundreds to thousands of cars on a fixed day each week. Stakes are high, especially for smaller buyers or sellers, where each transaction can make the difference for preferred inventory levels or profit margins. The primary goal of the auction house company throughout this process is to maximize the trade probability (see discussions of the industry in Treece 2013, Lacetera et al. 2016).

The dataset we study is rich, containing information from six auction house locations owned by one company. Collectively, these locations sold hundreds of thousands of cars from 2006–2010. For each attempt to sell a car, the data records the auction price, the seller's secret reserve price, every action taken by each party in the negotiation process, and — critically for our study — the mediator's identity, which varies across transactions. The data also contains detailed vehicle characteristics, the timing and location of each transaction, and identities of buyers and sellers. Such data is rare in the literature — only a handful of existing studies analyze information on offers

[1]Stipanowich and Lamare (2014) showed that the use of mediators by major companies has grown since 1997 in every category of dispute: commercial contracts, employment, consumer issues, corporate finance, environmental issues, intellectual property, personal injury, product liability, real estate, and construction.

[2]https://www.autoremarketing.com/ar/how-vehicle-volumes-sales-rates-performed-at-naaa-member-auctions-last-year/.

and counteroffers within real-world bargaining, and we know of only one other study containing information on both counteroffers and mediation.[3] We view this market as a rare opportunity to study bargaining intermediaries in the field.

In the data, negotiations end in agreement 62% of the time. In the remaining 38%, the buyer and seller fail to trade. Such breakdown is consistent with the presence of *two-sided incomplete information*, where the buyer and seller both have private values (i.e., unknown to the opposing party) for the car, and inconsistent with complete information.[4] Two-sided incomplete information is a primary reason why the wholesale used-car market is an interesting place to study mediator heterogeneity and its effects on welfare. This is for two reasons. First, in such an environment, the number of equilibria of a sequential bargaining game is infinite (Ausubel et al. 2002), with equilibria known to have vastly different efficiency outcomes, ranging from no trade (Perry 1986, Ausubel and Deneckere 1992) to relatively efficient trade (Cramton 1992, Ausubel and Deneckere 1993). Thus, if one mediator can implement a different equilibrium than another, there is scope for her to increase trade probabilities and realized gains from trade. Second, with incomplete information, there is an inherent trade off between efficiency and either party's rent extraction, and thus bargaining is not zero-sum: mediators who increase agreement can potentially also increase the size of the pie.

To analyze whether different mediators differentially affect welfare, we introduce a structural model that allows for different mediators to correspond to distinct pure strategy Bayes Nash equilibria (BNE) of a two-sided incomplete-information game. As a precursor for this structural analysis, we first document descriptive evidence on the assignment of mediators to specific negotiations and on the heterogeneity in observable outcomes across mediators. Our conversations with sales managers suggest that mediators are largely randomly assigned, although empirically we find some evidence that assignment is not entirely random and can depend on features determined before bargaining starts, such as car characteristics, buyer and seller identities, and outcomes of the pre-bargaining

[3] In contemporaneous work, Dindaroglu and Ertac (2024) studied a little over 1,000 manually collected negotiations in Turkey's sheep market leading up to the Festival of Sacrifice. 7 percent of these involved a third-party mediator. Mediated negotiations had a higher trade probability and showed no difference in prices, consistent with our findings.

[4] In a complete-information model, such as the Rubinstein (1982) noncooperative model or a cooperative Nash bargaining model, parties would only begin negotiating if agreement is known a priori to be efficient. The same is true in many incomplete-information models where the incomplete information is about features other than values, such as discount rates (as in Rubinstein 1985), or in models of reputational bargaining (Abreu and Gul 2000), where all rational types eventually trade and trade is always the efficient outcome. In contrast, with two-sided incomplete information about values, and with overlapping support for buyer and seller values, *disagreement* will sometimes be the efficient outcome, and impasse will generally occur with positive probability even when gains from trade do exist (Myerson and Satterthwaite 1983).

stages of the game (the auction). Our detailed data allows us to control for these features, and we show that, conditional on controls, variation in mediator assignment is indeed largely random. We further show that our key findings are similar with or without these controls.

We document heterogeneity in mediators' average trade probability through various regressions of an agreement indicator on mediator fixed effects, following the analysis of human auctioneer heterogeneity in Lacetera et al. (2016). We observe significant dispersion: the 75th percentile mediator trades 32.9 percentage points more often than the 25th percentile mediator. Large differences persist as we add rigorous controls for features of the negotiation. In our most saturated specification, the 75–25-percentile gap is 18.07 percentage points. Some variation in outcomes would occur in any finite data due to statistical noise even if underlying population probabilities were equal across mediators. Through a bootstrap simulation exercise, we show that mediator heterogeneity is a real phenomenon, wider than can be explained by statistical error. While mediators have large effects on trade probability, their effects on prices are indistinguishable from zero.[5] In this light, we refer to mediators with a higher trade probability as having higher *skill* or higher *performance*.

Having established heterogeneity in mediators' trade probability, we then examine how mediators differ in *when* they come to agreement and how outcomes differ by mediators' experience. Negotiations handled by effective mediators have a higher likelihood of ending in the first bargaining round, rather than after substantial back-and-forth action between buyers and sellers. Effective mediators stand out in their ability to achieve trade in difficult negotiations, where the auction price is not already close to the seller's secret reserve price. More experienced mediators — those who have been employed by the company longer — are more effective on average at reaching agreement, and within-mediator performance improves to some extent over time. Importantly, however, there is substantial residual variation in mediator ability that is not explained by experience.

These results are consistent with any number of possible channels through which mediators might influence outcomes. The lens through which we view this for our structural exercise is that different mediators implement different equilibria, some with higher probabilities of trade. The model is closely related to that of Larsen (2021). We first identify the buyer value distribution using an order statistics argument that relates buyer values to auction prices. We bound the seller value distribution using revealed preference arguments from the seller's decision to accept or reject

[5]As highlighted above, a lack of price effects is consistent with statements of industry participants, who suggest that the primary aim of the auction house is to facilitate trade independent of price. In our bootstrap simulation exercise, we confirm that any differences in average prices across mediators can be explained by statistical noise.

the auction price. Finally, for different mediators, we identify the function determining whether a given buyer and seller pair trade. This final step relies on a revelation-principle argument: any BNE has a corresponding *direct mechanism*. The direct mechanism for a given mediator skill level can be identified from trade probabilities in the data conditional on auction prices and reserve prices. Our identification and estimation also incorporate unobserved game-level heterogeneity — information that agents observe but that is unobservable to the econometrician.

Our estimates of the direct mechanisms for different mediator types imply that, for cases where the buyer's value is high relative to the seller's, mediator skill makes little difference: the probability of agreement is high independent of the mediator. Where mediators matter substantially is in cases of intermediate gaps in private values: high-skilled mediators are more likely to get agreement to occur for the same pair of buyer and seller types.

With these estimated direct mechanisms and distributions of buyer and seller values, we compute surplus (the gains from trade) realized in the trade-maximizing (first-best efficient) mechanism and the second-best efficient mechanism, as well as the real-world surplus achieved by mediators of different skill levels.[6] We find that mediators who achieve a higher trade probability also achieve greater gains from trade. This implies that higher-skilled mediators are not only capturing additional low-surplus trades (i.e., cases where the buyer values the car only slightly more than the seller). Rather, these higher trade-volume mediators increase the total realized *gains from trade*, creating significant value. As highlighted above, this reflects the feature that, under two-sided incomplete information about values, the game is not zero sum.

2 Related Literature

Our study examines mediator heterogeneity. A related theoretical and empirical literature compares mediated bargaining to unmediated bargaining. On the theory side, Jarque et al. (2003) (JPS) and Fanning (2021) studied continuous-time, war-of-attrition models where mediators can improve outcomes by speaking privately to agents and only revealing the opposing parties' offers once they agree. The models differ in how they arrive at this result: JPS studied a double auction with two-sided incomplete information about values. Fanning (2021) considered reputation, with two-sided incomplete information about whether an agent is rational or not. In the latter model, mediators

[6]The first-best mechanism maximizes trade gains as well as probability. With incentive constraints, the second-best mechanism that maximizes trade gains does not necessarily maximize probability. We choose to evaluate both the first-best and surplus-maximizing second-best.

introduce randomness in when they reveal offers compatible with agreement.[7] Our model differs from Fanning (2021) in that we consider incomplete information about values, as in JPS.[8] We differ from JPS by considering alternating offers and discrete time, realistic features of our setting.[9]

Theory also considers mediation in one-shot bargaining games. In Goltsman et al. (2009), a mediator can improve outcomes over unmediated bargaining by filtering information and adding noise, akin to Fanning (2021).[10] In Kydd (2003), mediators must be biased toward one party or the other in order to send credible messages.[11] Hörner et al. (2015) modeled international conflict, finding that mediation can be effective when mediators do not fully reveal when a country is weak. Li and Zhang (2024) demonstrated theoretically that more informed mediators can improve efficiency.[12]

In our study, we aim for a structural model that is patterned closely after the real-world game and that allows for the wide variety of outcomes we observe while still being empirically tractable. This motivates our choice of a model with the following traits: two-sided incomplete information about values, continuous value distributions, discrete time, and both parties being allowed to make multiple offers. None of the above models of mediation in bargaining fits these requirements. We therefore adapt the two-sided incomplete-information model of Larsen (2021) to allow for multiple mediator types.[13] Our model does not incorporate some potential roles of mediators from the theoretical models above, such as mediators having different information than agents or being

[7]The model of Copic and Ponsati (2008) is built on JPS, while that Fanning (2023) is built on Fanning (2021) to derive the optimal mediation protocol in reputational bargaining. Basak (2024) also studied information in reputational bargaining. Fanning (2021) offered a thorough literature review that we build on.

[8]Gottardi and Mezzetti (2024) modeled two-sided incomplete information about values, where mediators know more about agents' values than do agents. Mediators restrict information flow to agents to allow gradual learning and improve efficiency. In our setting, mediators do not know more than agents about the car; Table 3 and Figure 7 of Lacetera et al. (2016) presented survey evidence consistent with this for auctioneers, whose level of knowledge is similar to auction house mediators in this market.

[9]JPS highlighted that modeling their setting in continuous time greatly facilitates the theoretical analysis.

[10]Goltsman et al. (2009), as well as Fanning (2023), found that arbitration is generally more effective than mediation. Like mediation, arbitration is considered a form of alternative dispute resolution (ADR), but arbitrators can enforce (rather than only recommend) an outcome. Unlike mediation, arbitration usage by firms has declined in recent decades (Stipanowich and Lamare 2014). Kong et al. (2021) presented a structural analysis of arbitration.

[11]Kim (2020) showed theoretically that a mediator who maximizes trade (not revenue) can send more credible messages. The author finds supporting evidence in Korean real estate.

[12]Glode and Opp (2016) considered an intermediary who buys from a seller and resells to a buyer, finding that efficiency improves if the intermediary is more informed than one of the negotiators.

[13]The broader bargaining theory literature also tends to focus on special cases of the game we study, such as take-it-or-leave-it offers or one-sided incomplete information. Sequential bargaining with two-sided incomplete information about values is known to yield infinitely many, qualitatively different BNE, with no complete characterization (Ausubel et al. 2002), posing challenges for theoretical and empirical work. Equilibrium multiplicity is driven by off-equilibrium beliefs, which can sustain a large range of on-path behavior. A feature that supports tractability in our setting is that negotiation occurs *after* an auction, so auction actions (e.g., auction prices and secret reserve prices) help us identify primitives while remaining agnostic about the equilibrium of the post-auction bargaining subgame.

able to elicit private information to selectively (and potentially randomly) disclose. Instead, in our model, mediator types differ in which of infinitely many equilibria they implement, leading to different agreement rates and trade gains.

A higher-skilled mediator in our framework can be viewed as one that helps agents play an equilibrium closer to the theoretical second-best described in Myerson and Satterthwaite (1983) and Williams (1987). For example, a good mediator may be better at withholding trades between players when necessary — even when she knows the buyer values the good more than the seller — to keep agents' reporting incentive compatible.[14] In theory work building on Myerson and Satterthwaite (1983), Eilat and Pauzner (2021) showed that, if mediators cannot commit to the trade-withholding necessary to maintain incentive compatibility, efficiency declines.[15] How close real-world bargaining gets to the Myerson-Satterthwaite-Williams frontier, and whether some mediators get closer than others, is an empirical question.

Empirical evidence on mediation effectiveness is mixed. Several laboratory experiments have studied mediation: in Bazerman et al. 1992, mediation decreases the trade probability and increases price, depending on mediators' incentives and knowledge; Casella et al. (2020) found that mediation does not increase agreement; Eisenkopf and Bachtiger (2013) found the opposite, but only when mediators can punish uncooperative agents. In international conflict, Dixon (1996) found that mediation increases the likelihood of peaceful resolution, while Fey and Ramsay (2010) found no effect.[16] In law, McEwen and Maiman (1981) found that mediation increases compliance and satisfaction rates in small claims suits, and Emery et al. (1991) showed positive effects in child custody cases in which parents were randomly assigned to mediated vs. unmediated negotiations.

An important distinction of our study from previous empirical work — as well as previous theoretical work — is that we study *differences across mediators* rather than comparing mediated to unmediated bargaining. To our knowledge, in the theoretical or empirical studies highlighted above, all mediation is considered equal — there is no scope for any one mediator to *do* something

[14]Myerson and Satterthwaite (1983) considered the second-best efficient bilateral trade mechanism, maximizing the gains from trade, with equal weights on buyer and seller surplus, under the constraints of individual rationality, incentive compatibility, and ex-ante budget balance. Williams (1987) extended this analysis to the full Pareto efficient frontier with unequal welfare weights.

[15]Recent theoretical work (Saran 2011) suggests that efficiency in the Myerson and Satterthwaite (1983) framework can also be affected — indeed, improved — by the presence of naive traders who do not strategically shade their actions relative to their true values.

[16]Bercovitch and Jackson (2001) found that mediation is preferred in international conflicts (over unmediated bargaining) in cases of outcome uncertainty or unequal bargaining power.

better or inherently *be* better than another, no notion of *mediator types* or *mediator skill*.[17] This contrasts starkly with the way mediators are discussed in popular writing and negotiation training, where discussions abound of the existence of high-quality vs. low-quality mediators.[18] Our study attempts to fill this gap by analyzing *mediator heterogeneity*.

Our study also relates to structural empirical work studying bargaining under two-sided incomplete information (Keniston 2011, Li and Liu 2015, Freyberger and Larsen 2021, Larsen 2021, Larsen and Zhang 2021).[19] Our work contributes by analyzing the impact of intermediaries.[20] The data we use overlaps to some extent with that of Larsen (2021) and Larsen and Zhang (2021), although those studies do not exploit mediator identities. Larsen (2021) provided a structural welfare analysis of the real-world average bargaining efficiency relative to theoretical benchmarks, and Larsen and Zhang (2021) analyzed the division of surplus in this context. Both studies leave unanswered the question of *what explains* variation in outcomes across negotiating pairs. Our paper takes a first step in this direction. Our model builds on Larsen (2021) to identify buyer and seller values, but differs in allowing the *direct mechanism* to depend on the mediator.

Our hope is that insights from this paper can inform several audiences. For practitioners, the results suggest that mediator heterogeneity can have sizeable effects on outcomes, much like takeaways from the teacher value-added literature, and hence interventions to improve mediator skill may have positive effects on trade and welfare.[21] For bargaining theory, we hope our findings can motivate models incorporating this heterogeneity, rather than equal treatment of all mediation. Finally, for empirical bargaining research, which typically models negotiated prices as arising from some form of Nash bargaining, where mediators can play no role, our finding that mediators play a

[17]Theory work by Kim (2017) sheds light on one reason why empirical analysis of mediator *heterogeneity* can be challenging: to avoid being revealed as a weak negotiator, both parties favor a mediator who is best for strong negotiators. This leads to complicated selection in settings where a mediator is chosen by the negotiators. In our setting, negotiators do not choose the mediator.

[18]See, for example, the training material offered by the Harvard Law School Program on Negotiation at https://www.pon.harvard.edu/freemium/mediation-secrets-for-better-business-negotiations-top-techniques-from-mediation-training-experts/. Evidence in Stipanowich and Lamare (2014) also pointed to the existence of variation in quality: responding Fortune 1000 firms prefer mediators whom they know from previous experience or who come from private ADR firms, and only 37.7% of respondents rank mediators they work with as "very qualified."

[19]Empirical bargaining studies of one-sided incomplete information include Silveira (2017) and Ambrus et al. (2018).

[20]Biglaiser et al. (2020) offered a model and empirical evidence of how intermediaries can help reduce information asymmetries in retail used-car markets.

[21]Other empirical studies have documented heterogeneous outcomes in sales situations in the field, such as Barwick and Pathak (2015), Robles-Garcia (2020), and Gilbukh and Goldsmith-Pinkham (2023), studying real estate agents/brokers; Lacetera et al. (2016), studying auctioneers in wholesale used-car markets; Bruno et al. (2018), studying art auctions; and Jindal and Newberry (2022), studying heterogeneity across sellers in large-scale appliances. Relative to much of this literature, the heterogeneity we find is quantitatively large.

large role may offer insights into the validity of such abstractions and the importance of incomplete information more broadly.

3 Institutional Background and Data

The wholesale used-car auction industry is the backbone of the supply-side of the used-car market, both in the U.S. and much of the world. Millions of used cars arrive each year to used-car dealerships as trade-in vehicles and then are never sold at those dealerships, but are instead brought by the dealerships (referred to as dealer sellers) to a wholesale used-car auction house location, where the inventory is sold to other car dealerships. Inventory at used-car auction houses also comes from large sellers (referred to as fleet/lease sellers), including fleet companies (such as Wheels, selling old fleet cars), rental car companies (such as Hertz), banks (such as Bank of America, selling repossessed cars), or manufacturers (such as Ford or GM, selling off-lease or lease-buy-back vehicles). Total industry revenue is currently more than $110 billion annually.

Transactions involve a secret reserve price ascending auction potentially followed by post-auction bargaining. The role of the auction stage is to ensure that it is the highest-value bidder (rather than some other bidder) who trades with the seller through the auction or who faces the seller in a post-auction negotiation. For each car, an auctioneer runs a rapid (approximately 90-second) ascending auction. Prior to the auction, the seller chooses a reserve price, which is reported to the auction house but hidden from bidders. If the auction price (the highest bid) fails to reach the seller's secret reserve price, the buyer is given the option to walk away from the transaction. If the buyer chooses not to walk away, bargaining ensues between the high bidder (whom we will call the *buyer*) and the seller. For most sales, this bargaining stage simply involves the seller immediately accepting or rejecting the auction price in person.[22] About 26% of auctions end with the auction house facilitating negotiations over the phone through an auction house employee — a *mediator*.[23]

[22]In some cases, if the seller is not present and the auctioneer observes a sufficiently wide gap between the auction price and the reserve price, the auctioneer may decide to reject the auction price on behalf of the seller.

[23]This 26% is not conditional on the auction failing, but rather represents the percentage of observations for which we see a mediator identity recorded among all attempts to sell the vehicle in our full auction-plus-bargaining dataset, described in Section 5.5. Observations *without* a mediator recorded are those fall into one of the following cases: (i) the car sells through the auction price exceeding the reserve price, (ii) the auction price is below the reserve price and is accepted or rejected by the seller in person (meaning a mediator is never involved), or (iii) the auction price is so far below the reserve price that the auctioneer opts against contacting the seller. The 26% of transactions with mediated negotiation — which we will refer to as our *mediated negotiation data* — constitute the primary dataset we use in this and the following sections to study mediator heterogeneity. In Section 5, we bring in observations corresponding to cases (i)–(iii) as well, as these observations are key for identifying buyer and seller value distributions.

The mediator first calls the seller and relays the auction price.[24] The seller can choose to accept this price, counter, or quit (ending the negotiation). If the seller counters, the mediator calls the buyer. This process continues until one party accepts or quits. The auction house company records all actions taken by either party, the identity of each party, and the mediator's identity.

The mediated negotiation data consists of several hundred thousand realizations of bargaining sequences at six different auction house locations (in distinct geographic markets) run by the same parent company from 2006–2010. We will frequently use the term *thread* to refer to a bargaining sequence. The data storage system creates a new record for each action taken during a given bargaining thread, allowing us to see that some threads involve multiple mediators facilitating different stages of the negotiation. Among all bargaining threads, 67.06% are handled by one mediator per thread, 30.34% are handled by two mediators, and the remaining by more than two. We limit our analysis to threads involving single mediators to facilitate our attribution of outcomes to mediators.[25] Because we want a sufficient number of observations per mediator to estimate mediator-specific outcomes, we restrict our sample to mediators whom we observe in at least 50 separate bargaining threads. Appendix B describes additional data cleaning. In the end, we have 114 mediators and 75,090 threads with 6,226 distinct sellers and 6,258 distinct buyers.

The industry's primary performance indicator is the probability with which trade occurs; the auction company's goal is to operate a liquid two-sided market that can attract both buyers and sellers (Treece 2013, Lacetera et al. 2016).[26] Each mediator also has this same goal. We will largely evaluate differences in mediator performance by this trade probability metric.[27] We also look at heterogeneity in the final price of successful trades. To make prices comparable across cars, for much of our analysis, we normalize them by the auction company's estimated market value of the car (which we will refer to as the *book price*).

Table 1 shows thread-level statistics for our main sample.[28] Agreement occurs in 62.3% of

[24] The mediator's identity is not known to the buyer or seller before the negotiation begins.

[25] Appendix B compares the single-mediator threads to those with multiple mediators and presents evidence suggesting that dropping these observations does not drastically affect our results.

[26] In contrast, price maximization (a common auction design objective) attracts sellers but not buyers.

[27] Mediators are not paid a commission based on individual trades they facilitate; they are paid a salary. However, our conversations with individual mediators, and our in-person observations of the mediation process, make it clear that mediators' primary objective is a high trade probability.

[28] Appendix Table B.1 shows means from Table 1 separately by the six auction house locations. Locations vary in the number of negotiations they handle, ranging from 6,766 to 19,293, with smaller locations having fewer mediators (this ranges from 10 to 31 across locations). Locations also differ in the fraction of cars sold by fleet/lease sellers, ranging from 0.396 to 0.536. See Appendix B for more discussion. In our analysis of mediator heterogeneity below, we control for these differences in a number of ways, both through location fixed effects and through controls varying

Table 1: Descriptive Statistics at Bargaining Thread Level

	Mean	Std. Dev.	0.1 Quantile	0.9 Quantile
Agreement reached	0.623	0.485	0	1
Final price ($)	5,605	4,940	850	12,700
Book price ($)	6,958	5,264	1,550	14,475
Auction price ($)	5,572	4,938	800	12,700
Reserve Price ($)	7,347	5,407	1,900	15,000
No reserve	0.255	0.436	0	1
# Offers in a thread	1.393	0.704	1	2
Length of a thread (hours)	5.769	14.934	0.337	15.6
Fleet/lease car	0.474	0.499	0	1
Car age (years)	6.353	3.605	2	12
Mileage	93,341	51,099	30,161	158,011
Engine displacement (liters)	3.598	1.525	2	5.7
No. Threads	75,090			

Notes: Table presents descriptive statistics at the thread level. Final price is conditional on trade occurring.

threads. *Final price* refers to final negotiated price when trade occurs. This price may coincide with the auction price if the seller accepts that price in the negotiation (the auction price is the first offer in the negotiation stage). The average final price for a successful trade is $5,605, lower than the average book price of $6,958. The average final price is also between the average auction price of $5,572 and average secret reserve price of $7,347, though it is much closer to the former.[29] A bargaining thread ends after 1.39 offers on average, and within six hours. An average car is 6.35 years old and has 93,341 miles on it.[30] Roughly half of threads correspond to fleet/lease sellers.

Table 2 shows descriptive statistics at the mediator level. Some of these outcomes are thread-level characteristics averaged within threads handled by a given mediator. Among the 114 mediators, 45.8% are female.[31] The average mediator has worked at her current auction house for four years.[32]

at the thread-level within a location.

[29]Recall that any transaction that reaches the negotiation stage is one in which the reserve price exceeds the auction price, and hence trade failed to occur at the auction stage.

[30]Table 1 shows that, for 25.5% of observations, there is no reserve price reported. This typically implies that (i) the seller plans to be present at the auction to signal to the auctioneer whether to let the car sell at the current bid (effectively acting as a shill bidder) or (ii) the seller wants an auction house mediator to call her regardless of the auction price to let her accept or reject this price over the phone. The probability of agreement is similar in cases where the reserve price is missing vs. not (see Online Appendix D.1.1 of Larsen 2021).

[31]We find no difference in trade probabilities by mediator gender (see Appendix Table B.5).

[32]We observe each mediator's employment start date at an auction house. The reported years of employment in Table 2 are computed by first calculating the mediator's employment length up through the date of each bargaining thread and then taking the average of this quantity across all of her threads in our data.

Table 2: Descriptive Statistics at Mediator Level

	Mean	Std. Dev.	0.1 Quantile	0.9 Quantile
Agreement reached	0.604	0.196	0.374	0.899
Final price/book price	0.834	0.0751	0.754	0.932
Final price/reserve price	0.790	0.0626	0.725	0.861
Final price/auction price	1.022	0.0359	1	1.04
Female	0.458	0.501	0	1
# Threads mediated	659	686	102	1,473
Years of employment	4.150	5.308	0.379	9.96
No. Mediators	114			

Notes: Table presents descriptive statistics at the mediator level.

In our data, she handles 659 bargaining threads and successfully facilitates 60.4% of them. While our sample restrictions enforce that each mediator handle at least 50 negotiations, Table 2 shows that even the 10th percentile mediator handles more than twice this amount. The achieved final price is, on average, 79% of the reserve price and 2.2% higher than the auction price. Dispersion in average trade probability is quite large across mediators, with a standard deviation of 0.196 (while the dispersion in prices is much smaller). The following section analyzes this dispersion.

4 Heterogeneity in Outcomes Across Mediators

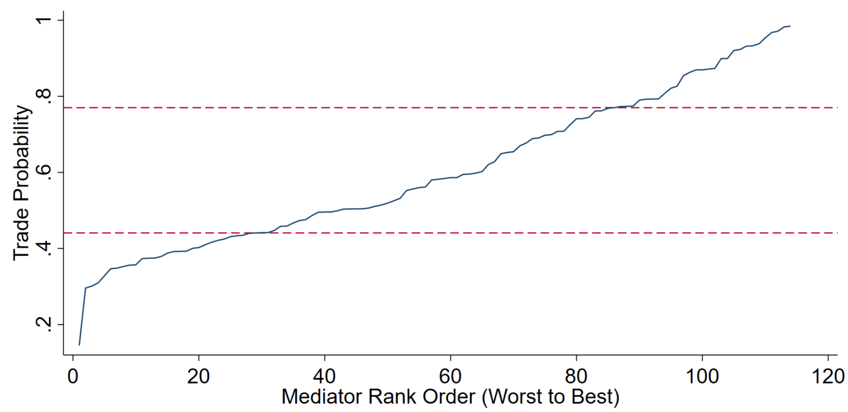
In this section, we provide evidence on mediators' assignment and their heterogeneous performance as measured by their trade probability. We show that this heterogeneity is not due to sampling error. We also examine differences in their final negotiated prices. Finally, we examine how mediators differ in *when* they achieve trade, how experience correlates with performance, and how different mediators perform when faced with greater disparity in buyer and seller values.

4.1 Mediator Effects on Agreement. We first report raw differences in trade probability across the 114 mediators. Let $Agree_i$ be an indicator variable representing whether trade occurs for thread i, which is mediated by intermediary $k(i)$. Let $\mathcal{J}_k$ be the set of all threads mediated by k, and $|\mathcal{J}_k|$ be the number of elements in this set. The average of $Agree_i$ over the subset of k's threads is

$$\overline{Agree}_k \equiv \frac{1}{|\mathcal{J}_k|} \sum_{i \in \mathcal{J}_k} Agree_i. \tag{1}$$

Figure 1 shows these probabilities, sorted from smallest to largest. A core stylized fact of our study

Figure 1: Mediator Differences in Trade Probability



Notes: Figure shows average trade probability for each mediator, ranking them from low to high. Dashed lines represent the trade probability achieved by the 25th and 75th percentile mediators.

is that different mediators have very different success probabilities. The best to worst mediators span a difference of 0.840. The 90–10 percentile spread is 0.525. The 25th-percentile of mediator trade probabilities is 0.441 and the 75th-percentile is 0.770 (shown with horizontal dashed lines in Figure 1). We will refer to higher-trade-probability mediators as *higher-skilled*.

To better understand the industry, we spent time observing the bargaining/mediation process and interviewing mediators, buyers, and sellers. Through this effort, we learned about potential drivers of mediator heterogeneity and the mediator assignment process. These conversations revealed that each seller typically has an auction house employee assigned to manage the relationship between that seller and the auction house, and, where possible, this employee mediates negotiations involving that seller. A seller's default mediator is often unavailable, however, and in such cases alternative mediators handle negotiations. At one location, we were told that negotiations involving fleet/lease sellers, rather than dealer sellers, are more often handled by a pre-assigned mediator, but even for fleet/lease sellers this is not always the case. At this location, we were able to observe that, for several exciting hours each week, the room in which the phone calls occur for dealer sellers is alive with activity, akin to a miniature stock market trading floor, with no obvious protocol determining which mediator handles a given thread.[33]

[33] Throughout the body of the paper, we use a sample of negotiations that includes both dealer sellers and fleet/lease sellers. In Appendix D we replicate all tables and figures from the body of our paper on separate samples for dealers vs. fleet/lease sellers and find that our results are largely similar across the two seller types.

Mediator assignment for a given thread thus involves an element of randomness, but is unlikely to be completely random. Nonrandom assignment is potentially problematic for our goal of measuring mediator heterogeneity if good mediators are systematically assigned to threads with characteristics associated with higher trade probabilities; for example, if effective mediators are systematically assigned to less-expensive cars, cars with lower reserve prices or higher auction prices, or sellers or buyers who have a higher propensity to agree. We evaluate the extent of this non-random assignment and identify features of the negotiation that help control for non-random assignment.

We first construct a leave-one-out estimate of a mediator's agreement rate, given by $\overline{Agree}_{-i,k(i)} \equiv \frac{1}{|\mathcal{J}_k|-1}\sum_{\{j:j\neq i,j\in\mathcal{J}_k\}} Agree_j$. This is the average trade probability for all threads *other than* i that are mediated by k. We construct a similar leave-one-out average of the trade probability for the seller and buyer of thread i.[34] We then estimate the following regression:

$$Agree_i = Z_i'\alpha + \epsilon_i, \tag{2}$$

and an alternative version of this regression where the outcome variable is instead $\overline{Agree}_{-i,k(i)}$.

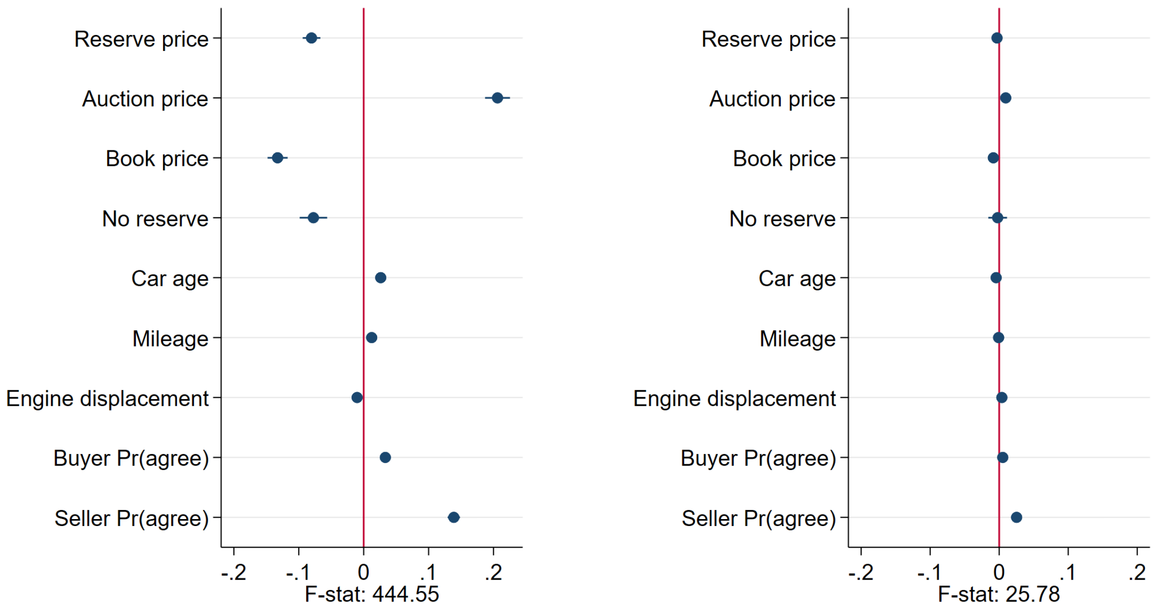
Z_i contains characteristics of thread i that are determined before the negotiation begins (and hence before any mediator influence). The auction price and reserve price are conceptually quite important to include in Z_i. All observations in our mediated negotiation data are cases where the auction price is below the reserve price (the condition that leads to auction failure and entering negotiations). However, observations in which the auction price is closer to the reserve price are more likely cases where gains from trade exist and agreement is feasible. This is because a higher auction price should reflect a higher willingness to pay of the buyer, and a lower reserve price a lower value of the seller. We formalize this in our structural analysis in Section 5, where auction prices and reserve prices play key roles in identifying buyer and seller values.

Z_i also includes other factors that potentially relate to mediator assignment and to trade probabilities: fixed effects for the auction house location and negotiation date; vehicle mileage, age, engine displacement, and book price; and the leave-one-out agreement rates of the seller and buyer. The first regression, (2) with $Agree_i$ as the outcome, allows us to explore whether thread charac-

[34] The leave-one-out agreement rate for seller $s(i)$ is the empirical frequency with which $s(i)$ comes to agreement based on threads other than i that involved s. In similar notation to the mediator leave-one-out rate, this can be written $\overline{Agree}^{seller}_{-i,s(i)} \equiv \frac{1}{|\mathcal{J}_s|-1}\sum_{\{j:j\neq i,j\in\mathcal{J}_s\}} Agree_j$, where $\mathcal{J}_s$ is the set of all threads with s as the seller. The buyer leave-one-out agreement rate can be written $\overline{Agree}^{buyer}_{-i,b(i)} \equiv \frac{1}{|\mathcal{J}_b|-1}\sum_{\{j:j\neq i,j\in\mathcal{J}_b\}} Agree_j$, where $\mathcal{J}_b$ is the set of all threads with b as the buyer.

Figure 2: Mediator Assignment Test

(A) Agreement (B) Leave-one-out Agreement



Notes: Figure shows coefficients from (2). Panel A shows coefficients when the outcome is $Agree_i$ and panel B where the outcome is the leave-one-out agreement rate $\overline{Agree}_{-i,k(i)}$. In addition to the variables listed in the figure, regressions include fixed effects for auction house locations and negotiation date. Covariates are standardized to have standard deviations equal to 1. 95% confidence intervals, computed by clustering at the mediator level, surround each point estimate. Buyer Pr(agree) and Seller Pr(agree) represent buyer and seller leave-one-out agreement.

teristics correlate with the likelihood that the thread ends in agreement. The second regression, (2) with the leave-one-out agreement as the outcome, speaks to whether these characteristics correlate with a mediator's average agreement rate on the mediator's *other* threads. Comparing estimates from these two regressions allows us to examine whether mediators with higher agreement rates are systematically matched to cars, buyers, or sellers that are more likely to result in an agreement.

Figure 2.A shows estimates from the first regression, with 95% confidence intervals about each estimate. Other than engine displacement, each feature is significantly related to the likelihood that a thread ends in agreement. In particular, threads are more likely to end in agreement if they have a lower reserve price or higher auction price (as foreshadowed in our discussion above), if the car is a less-expensive car (lower book price), or if the buyer or seller is a priori a more agreeable agent (i.e., more likely to agree as measured by the buyer's or seller's leave-one-out agreement rate).

Figure 2.B shows the coefficients from the second regression, where the outcome is $\overline{Agree}_{-i,k(i)}$,

the leave-one-out agreement rate. If good mediators are not systematically assigned to threads that are a priori more likely to end in agreement, we would expect coefficients from this regression to be much closer to zero. Figure 2.B confirms this. The results still attest that assignment is not completely random: better mediators—those with a higher leave-one-out average trade probabilities — tend to be assigned to threads with slightly higher auction prices and with sellers who are a priori slightly more likely to agree. However, these effects are small relative to the magnitudes in panel A. Indeed, the F-statistic decreases from 445 in the first regression to 26 in the second, and the coefficients are about five times smaller. Thus, while thread-level features do predict realized agreement, they have much less predictive power for leave-one-out agreement rates. These results suggest that, while some of the raw heterogeneity in trade probability across mediators arises from other thread-level characteristics, these characteristics themselves are not strongly correlated with the assigned mediator's overall performance in terms of trade probability. We will control for this non-mediator-related heterogeneity in our analysis below.[35]

To measure mediator performance incorporating these controls, we expand (1) to estimate an effect for each mediator conditional on thread-level characteristics, as follows:

$$Agree_i = \beta_{k(i)} + X_i'\phi + \epsilon_i. \tag{3}$$

In (3), $\beta_{k(i)}$ is the effect for mediator k and X_i is a vector varying across specifications.[36] Following Lacetera et al. (2016), we reposition estimated mediator effects to be mean zero:

$$\hat{\beta}_{norm,k} = \begin{cases} \hat{\beta}_k - \frac{1}{M}\sum_{j=2}^{M}\hat{\beta}_j & for\ k \neq 1 \\ 0 - \frac{1}{M}\sum_{j=2}^{M}\hat{\beta}_j & for\ k = 1, \end{cases}$$

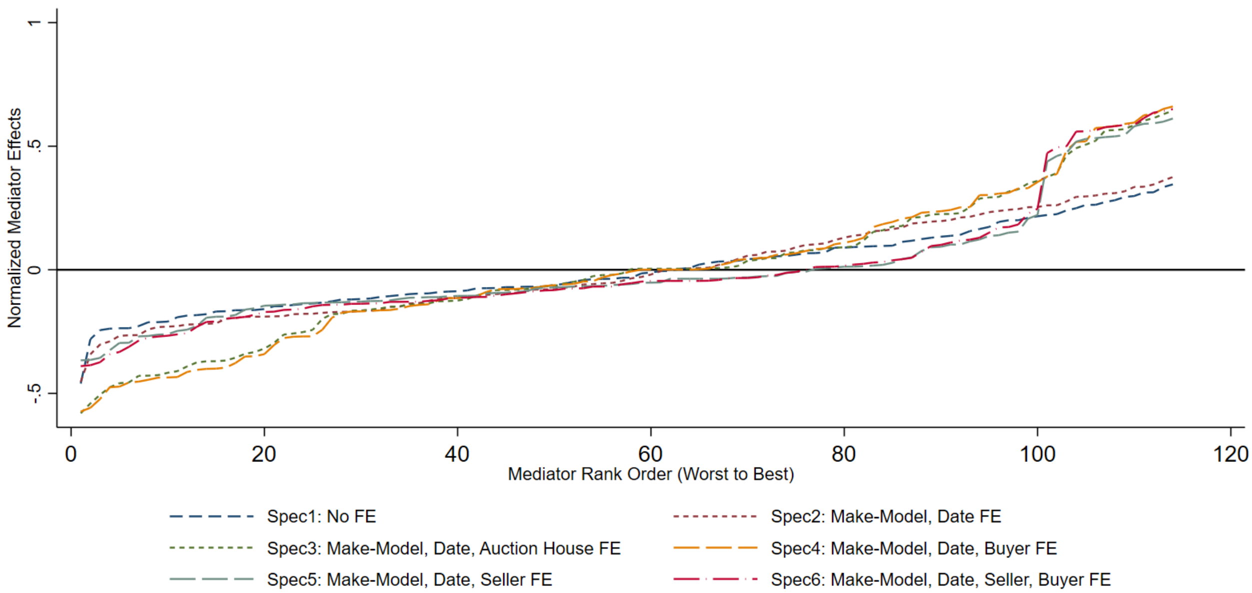
where 1 denotes the omitted mediator in regression (3).

Figure 3 displays estimates of mediator fixed effects from increasingly stringent specifications for X_i. In the baseline case, which we denote *spec 1*, X_i includes various thread-level features: the car's age, book price, engine displacement and mileage; a dummy for whether the car is sold by a

[35] Appendix B offers an alternative test of mediator assignment that suggests that mediators are largely randomly assigned to car types (make and model) and to buyers and that mediator to sellers is less random. This highlights the importance of controlling for seller fixed effects in our analysis, as we do below.

[36] We denote this vector X_i to distinguish it from Z_i in (2), as the controls in the two regressions only partially overlap. In particular, depending on the specification, X_i can include seller and buyer fixed effects, rather than just the buyer and seller leave-one-out agreement rates found in Z_i.

Figure 3: Mediator Fixed Effects for Trade Probability Under Different Specifications



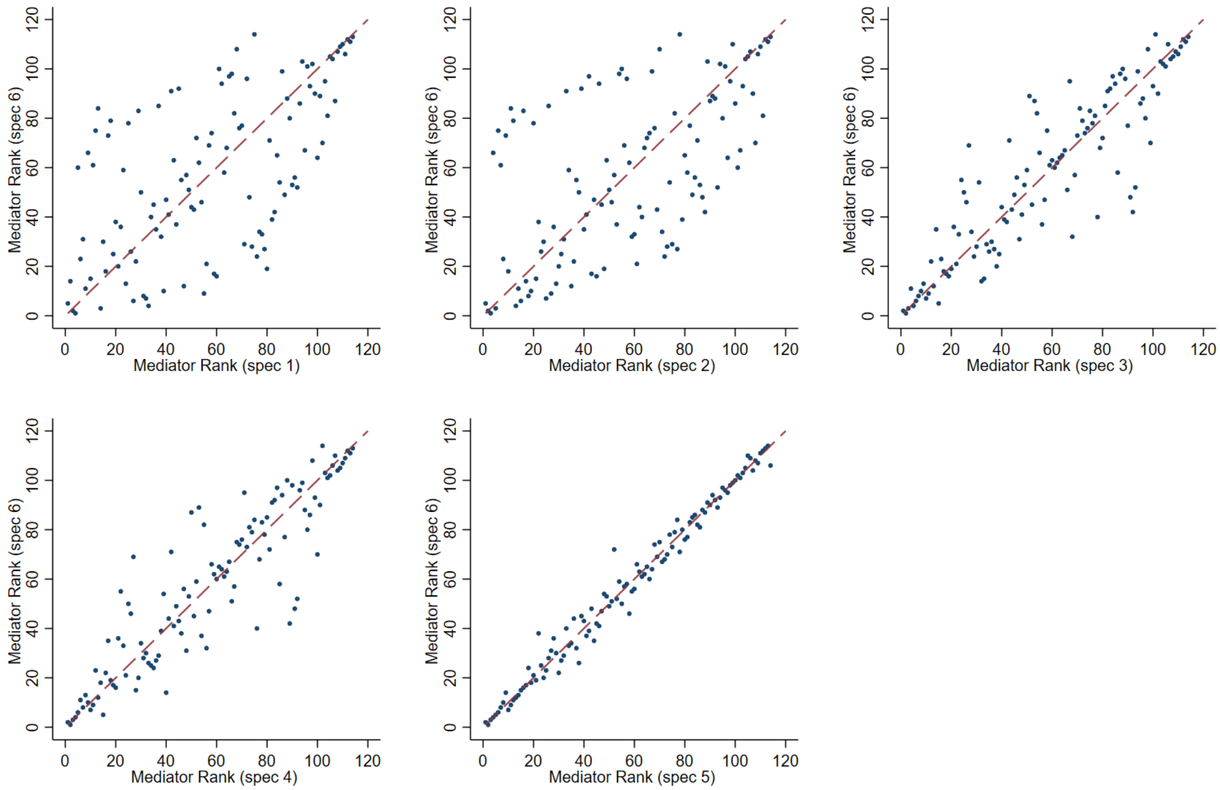
Notes: Figure shows normalized mediator fixed effect estimates for trade probability under increasingly stringent specifications for X_i from regression (3).

fleet/lease seller; and reserve and auction prices. In spec 2, we add fixed effects for the car's make-model combination and the negotiation date. Spec 3 adds auction house location fixed effects and spec 4 adds buyer fixed effects.[37] Spec 5 replaces buyer with seller fixed effects. The most-saturated version (spec 6) includes all controls. Seller fixed effects are of particular interest given that Figure 2 suggests that assignment of mediators to sellers may not be entirely random, and certain sellers may be more prone to agree independent of the mediator. Seller fixed effects help absorb variation in mediators' trade probabilities that is due solely to the seller. Spec 5 and spec 6 allow us to examine whether mediator heterogeneity remains even after controlling for seller effects.

Figure 3 shows results comparing these different specifications, ranking mediator fixed effects from small to large as in Figure 1. Controls change the shape of the mediator effects curve, but the heterogeneity remains relatively consistent and large across all specifications. The interquartile

[37]Our data contains separate lists of buyer, seller, and mediator identities for each auction house location. We therefore treat these identities as specific to a given location. In practice, it is possible, for example, that the same seller operates at two different locations in our data, but our analysis will treat this as two distinct sellers. This is not necessarily a downside for the analysis; rather, we highlight this to clarify that any time we control for seller, buyer, or mediator identities, those indicators will naturally absorb auction house location effects. Note also that some fixed-effect cells are singletons (e.g., some seller or buyer cells), and these are observations are naturally dropped from regressions.

Figure 4: Mediator Rank Across Specifications



Notes: Figure presents mediators' rank in trade probabilities across different specifications. Each data point represents a mediator. Vertical axis shows mediator fixed effects from spec 6, horizontal axis shows fixed effects from other specifications, and dashed line shows 45-degree line.

range is also large enough to be economically meaningful in each specification: the minimum value of the 75th to 25th percentile gap is 18.07 percentage points. Figure 4 illustrates how mediator fixed effects are correlated across specifications. In each panel, vertical axes show mediators' ranks (from 1 to 114) as measured by her fixed effect from spec 6. Horizontal axes show mediators' ranks from other specifications (1–5). Rankings are highly correlated across specifications.[38]

4.2 Mediator Effects on Prices. We repeat estimation of (3) using final negotiated prices

[38] Appendix B includes additional analysis of mediator heterogeneity. Appendix Table B.4 adds additional controls to the Figure 3 regressions, controlling for the number of previous interactions between a mediator-buyer pair, mediator-seller pair, or buyer-seller pair. These controls have little to no effect. Appendix Table B.6 examines incremental changes in adjusted R^2 from adding (to a regression where the right-hand side only includes X_i from spec 1) or subtracting (from spec 6) any of the following fixed effects: mediator, buyer, seller, auction house location, or make-model combination. The latter two have negligible effects; the other three help explain variation in outcomes.

(normalized by book prices), denoted $Price_i$, as the outcome of interest rather than $Agree_i$. Figure 5.B shows the estimated mediator fixed effects under the saturated model (spec 6), along with pointwise 95% confidence intervals. Panel A shows the trade-probability effects (also under spec 6) for comparison. The estimated mediator effects for final prices are smaller in magnitude than those for agreement. A one-standard-deviation increase in mediator performance is only associated with a 3.8 percentage point increase in the final price, but a 24.9 percentage point increase in the trade probability. Confidence intervals for each estimated effect are also wider for the price measure: for 113 out of the 114 mediators, the effects are not significantly different from zero. These results are unsurprising given mediators' objectives, which the industry clearly dictates as increasing trade.

4.3 Sampling Error. A concern with any study of heterogeneity is that some (and potentially all) heterogeneity may be driven by sampling error: even if true mediator performance were constant across mediators, some variance in outcomes would arise in any finite sample. Here we quantify the variation due to sampling error through a parametric bootstrap approach that takes into account other factors — such as seller identity or the make and model of the car — that may also affect trade probability.[39] We begin with the null hypothesis that mediators have no effect on trade probability. We regress $Agree_i$ on X_i from spec 6 of (3), without including mediator fixed effects. Let $\widehat{Agree}_i$ denote the predicted probability of trade from this regression for observation i. We then generate a large number of simulated datasets, indexed by ℓ, of identical size to the original data. For simulated dataset ℓ, we generate a synthetic trade indicator for observation i, $\widetilde{Agree}_i^{\ell}$, which is equal to 1 with probability $\widehat{Agree}_i$ and 0 otherwise.[40]

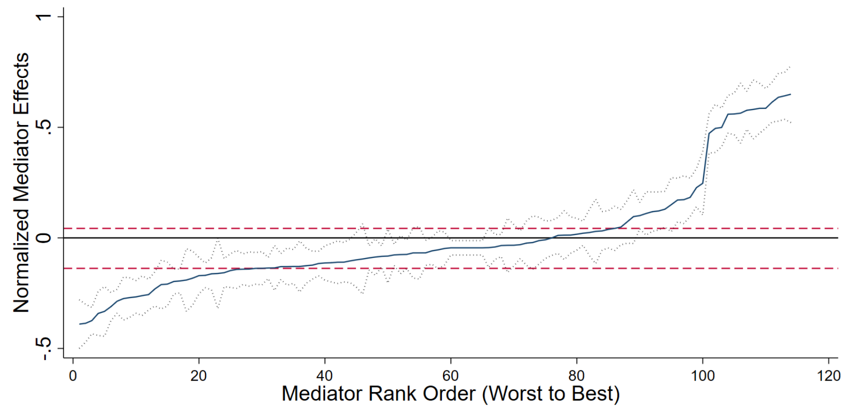
This resampling method preserves much of the structure of the data. It preserves, for example, the size distribution of mediators; the correlations between trade probabilities and features such as auction house locations, sellers, buyers, car models, and other controls in X_i; and the correlation between these features and mediator assignments. For example, if a certain seller tends to have a higher trade probability in the raw data, the procedure ensures that this seller will also have higher trade probabilities in the simulated data. If certain mediators are systematically more likely to interact with particular sellers or car types, this is also preserved, because the *outcomes* are synthetically produced but not covariates. The main part of the data structure that is destroyed in

[39] See Section 10.4 of Efron and Hastie (2021) for a description of the parametric bootstrap. We are grateful to an anonymous referee whose comments inspired this approach.

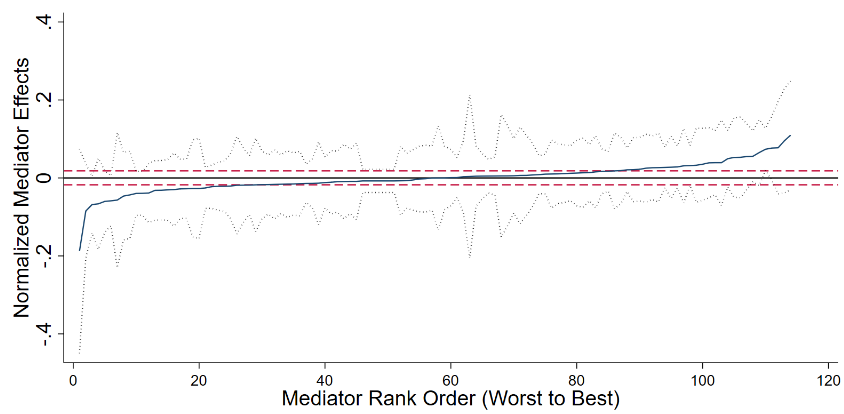
[40] Because we use a linear probability model, in a small fraction of the data, the estimated probabilities $\widehat{Agree}_i$ are greater than 1 or less than 0; in these cases, we simply set the probability to equal to 1 or 0, respectively.

Figure 5: Mediator Fixed Effects Under Most-Saturated Specification

(A) Trade probability



(B) Normalized final price

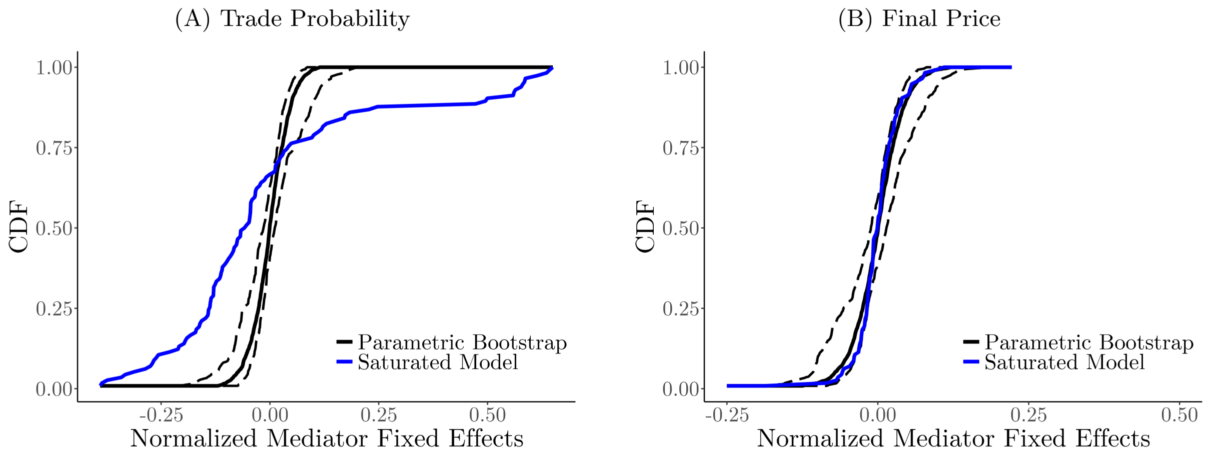


Notes: Panel A shows mediator fixed effect estimates for trade probability along with 95% confidence intervals under spec 6 of (3). Panel B shows results from the same exercise but using normalized final price as the outcome variable. Horizontal dashed lines represent the estimated fixed effect for the 25th- and 75th-percentile mediators.

the synthetic data is any correlation between mediator identities and trade probabilities, conditional on the controls X_i, which is exactly the variation that identifies mediator effects.

We simulate 100 such synthetic datasets and estimate mediator fixed effects in each as in spec 6 of (3). Because mediator identities have no effect on trade probabilities in the synthetic data, any variance in mediator effects in the synthetic data reflects pure sampling noise. Figure 6.A plots the CDF of the median bootstrap estimates across these 100 datasets, along with 95% confidence bands. The figure also shows the CDF of the actual mediator fixed effect estimates from spec 6

Figure 6: Parametric Bootstrap Test of Heterogeneity from Sampling Error



Notes: Panel A shows parametric bootstrap test results for trade probability. Panel B presents results for final price normalized by book price. To obtain the bootstrap estimates and confidence intervals for trade probabilities, we estimate a model of trade probabilities using all variables in spec 6 of (3), except that we exclude mediator fixed effects. For each bootstrap sample, we then generate simulated trade indicators, equal to 1 with probability equal to the predicted probabilities from the estimated model. We similarly estimate a model of prices using spec 6 of (3), and then simulate prices using the regression predictions combined with the regression estimate of the error variance. In each case, we use 100 bootstrap samples. Blue lines show the estimated distribution of fixed effects from the actual data and black lines show the bootstrap median and 95% pointwise confidence bands.

of (3). The distribution of trade probability fixed effects is quite dispersed, lying well outside the 95% confidence bands of the bootstrap distribution, suggesting that the heterogeneity we find is not purely due to noise. This same approach admits a one-sided test of the null hypothesis that the standard deviation of actual mediator fixed effects is no larger than what we would expect solely due to sampling error. We solidly reject this null: the standard deviation of the actual mediator fixed effect estimates (0.25) is well outside the one-side 95% bootstrapped confidence value (0.06).

We adopt a similar method to evaluate the effects of mediators on prices, regressing $Price_i$ on X_i from spec 6 of (3). Let $\widehat{Price}_i$ be the predicted price from this regression for observation i and let $\hat{\sigma}$ be the estimated standard deviation of the residuals. Because prices are continuous rather than discrete, simulating prices requires a stronger stance on the data generating process. For simulation ℓ, we generate a synthetic price outcome for observation i, $\widetilde{Price}_i^{\ell}$, by drawing from a $N(0, \hat{\sigma})$ and adding the realizations to $\widehat{Price}_i$. We create 100 such datasets and estimate mediator effects in each as in spec 6 of (3) but using $\widetilde{Price}_i^{\ell}$ as the outcome.

The results of this exercise are shown in Figure 6.B. The estimated mediator fixed effects for prices fall within the 95% confidence band of our bootstrap estimates. Thus, unlike our effects on trade probabilities, we cannot reject the null that mediators have no effect on prices, and that any apparent effect observed in Figure 5.B is due to sampling error. This is also confirmed by the standard deviation of the actual mediator fixed effects for prices, which is 0.038, whereas the 95th percentile standard deviation across parametric bootstrap samples is 0.06.[41]

4.4 Other Differences Across Mediators. This section examines several differences across mediators. First, we show that effective mediators' threads have a higher tendency to end in agreement in the first period, and that this fully explains their better performance. Let $Agree_i^1$ be equal to 1 if thread i ends in agreement in the first period (meaning the seller accepts the auction price, the first bargaining offer), and 0 otherwise. Let $Agree_i^{>1}$ be equal to 1 if thread i ends in agreement some time after the first period, and 0 otherwise. Thus, mechanically, $Agree_i = Agree_i^1 + Agree_i^{>1}$. We define three tercile groups of mediators, which we call *low*, *medium*, and *high*, ranked by their estimated fixed effects for trade probability from spec 6 of (3).[42] We then analyze the three components $Agree_i$, $Agree_i^1$, and $Agree_i^{>1}$ in these groups.

Table 3 column 1 displays estimates from regressing $Agree_i$ on dummies for tercile groups, with the lowest group as the omitted category. The vector X_i from spec 6 of (3) is also included. We find higher trade probabilities for medium and high mediators; this is mechanical given their grouping by trade probabilities. Column 2 displays results when the outcome is instead the first-round agreement indicator, $Agree_i^1$. The coefficients for the medium and high groups are positive and significant, and similar in magnitude to those in column 1. Column 3 uses instead the later-round agreement indicator, $Agree_i^{>1}$. The coefficients are insignificant in both cases, and the standard errors are tight enough to reject economically large effects. Thus, effective mediators' increased propensity to agree in the first period appears to fully explains their better performance.[43]

We next examine how mediators' experience affects their trade probabilities. First, Figure 7.A

[41] We find similar results using final prices normalized by the auction price or reserve price rather than book price or using an alternative version of the bootstrap test relying only on seller fixed effects (which, again, absorb auction house location effects) in X_i; See Appendix Figure B.3. Appendix B also proposes a test based on splitting our main sample in half that also confirms these results.

[42] For this exercise, as well as for the structural model in Section 5, we take estimated mediator rankings as truth; that is, we treat the spec 6 estimates from (3) as though they control well enough for covariates that we have a correct understanding of who the best and worst (and middle-ground) mediators truly are.

[43] Appendix D replicates all analyses separately for fleet/lease vs. dealer sellers. We find that the null effect in column 3 of Table 3 is driven by a positive effect for dealer cars and a negative effect for fleet/lease cars. In both samples, the strong positive correlation between a mediator's overall trade probability and her first-round trade probability (column 2) remains.

Table 3: Agreement Rates in First vs. Later Rounds and Mediator Experience

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	$Agree_i$	$Agree_{1,i}$	$Agree_{>1,i}$	$Agree_i$	$Agree_{1,i}$	$Agree_{>1,i}$	$Agree_i$	$Agree_{1,i}$	$Agree_{>1,i}$
High	0.254***	0.250***	0.004						
	(0.010)	(0.010)	(0.006)						
Medium	0.102***	0.098***	0.004						
	(0.006)	(0.006)	(0.004)						
Experience (years)				0.005***	0.004***	0.001*	0.010***	0.019***	-0.009***
				(0.001)	(0.001)	(0.001)	(0.004)	(0.004)	(0.002)
Mediator FE							Y	Y	Y
R2	0.371	0.438	0.233	0.398	0.467	0.257	0.416	0.484	0.262
N	71,154	71,154	71,154	51,589	51,589	51,589	51,589	51,589	51,589

Notes: "High" and "Medium" are dummies for a given thread i being mediated by a mediator in the top and middle tercile of mediator trade probability fixed effects, respectively, where the fixed effects are spec 6 of (3). In Columns 1-3, where we analyze mediator terciles, the benchmark group is the bottom tercile of mediators. Experience is measured for a given thread as the time elapsed between the thread mediator's start date, which we observe in the data, to the date of the bargaining thread. The dependent variable in columns 1, 4, and 7 is a dummy for the thread ending in agreement. In columns 2, 5, and 8, the dependent variable is a dummy for the thread ending in agreement in the first period, and in columns 3, 6, and 9, the dependent variable is a dummy for the thread ending in agreement later than the first period. The vector X_i from spec 6 of (3) is included as a control in all regressions. The number of observations is fewer than in Table 1 because experience is not available for some mediators and because singleton fixed effect cells are dropped from the analysis. Significance levels: *: $p < 0.10$, **: $p < 0.05$, and ***: $p < 0.01$.

shows a scatter plot of mediator experience against mediators' estimated fixed effects from spec 6 of (3). Experience is measured, for a given thread, as the time elapsed between the mediator's employment start date and the start time of the current thread; we then average this across threads a mediator handles.[44] We observe that more experienced mediators are more effective. However, a large degree of mediator skill is unexplained by experience.

We also quantify experience effects through variants on the following regression:

$$Agree_i = X_i'\xi + \psi Experience_{k,i} + \omega_i \tag{4}$$

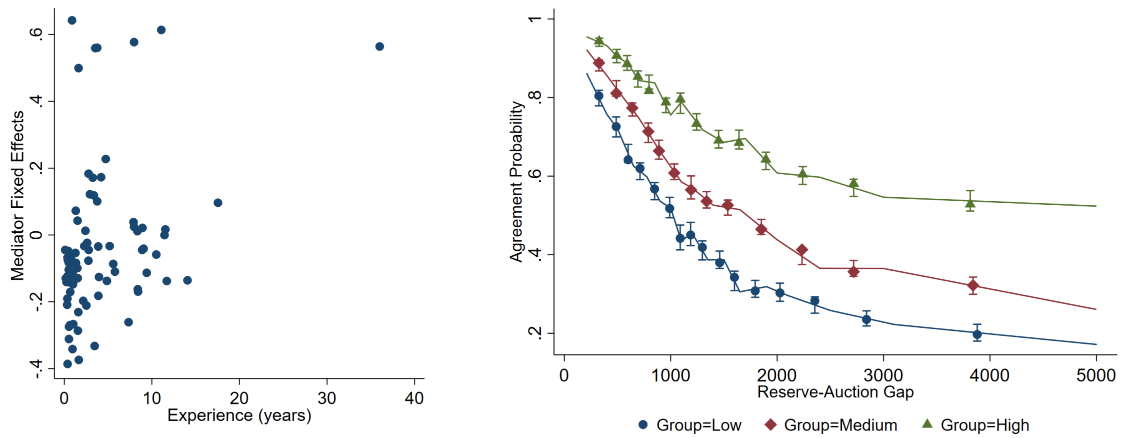
where $Experience_{k,i}$ denotes the experience level of mediator k up to the point where thread i begins, and X_i is as in spec 6 of (3). Table 3 column 4 shows the results: mediator experience is positively correlated with agreement, an effect that is small in magnitude but statistically significant.

We augment (4) to include mediator fixed effects, exploring how individual mediators' performance changes as they gain experience. Table 3 column 7 shows the results. The coefficient on experience is positive and significant, suggesting that individual mediators, even those who are poor performers on average, improve in their trade probability with additional experience, becoming more like high-skilled mediators. The effects are quantitatively modest: A one-year increase in experience

[44] We observe mediators' employment start date in the data.

Figure 7: Agreement vs. Mediator Experience and Pr(Agreement) vs. Reserve-Auction Gap

(A) Agreement vs. Experience | (B) Agreement vs. Reserve-Auction Gap



Notes: Panel A displays a scatter plot of mediators' estimated fixed effects from spec 6 of (3) vs. average work experience in years. Panel B shows a binned scatter plot with the probability of agreement on the vertical axis and the reserve-auction gap on the horizontal axis, with the plot created separately for negotiations handled by high-, medium-, and low-skilled mediators. The binned scatter plot sets the polynomial degree and number of smoothness constraint to 1 and uses the bias-corrected confidence interval of Cattaneo et al. (2024).

is associated with a one-percentage point increase in trade probability, a movement upward of about three slots (relative to the 25th percentile) in the mediator rankings from spec 6 of (3). Mediator performance thus appears to improve with experience, but some variation in effectiveness remains that experience cannot explain. More experienced mediators have higher first-round agreement probabilities (columns 5 and 8) and lower rates of later-round agreement (column 9; the result in column 6 is not significant at the 0.05 level).[45]

Table 3 offers two takeaways. First, effective mediators appear to succeed by convincing sellers to concede in the first period more often and this channel appears to explain the entirety of high-skilled mediators' better performance.[46] Second, experience appears to be an important driver of mediators' effectiveness: more experienced mediators have higher trade probability fixed effects, and mediators (even low-skilled ones) become more effective as they gain experience. A narrative consistent with these stylized facts is that effective mediators may be able to coordinate buyers and

[45]In Appendix D we show that the strong positive effects in columns 7 and 8 are driven by negotiations involving fleet/lease sellers. The negative effects in column 9 appear in both the dealer and fleet/lease samples.

[46]Our result that better mediators achieve agreement in the first period also suggests that these mediators reduce delay, which is costly in many bargaining models. The structural analysis in Section 5 allows us to examine whether the realized welfare gains, including bargaining costs, are higher for these higher-trade-probability mediators.

sellers on playing an equilibrium that involves sellers conceding faster. The structural exercise in Section 5 will allow for these possibilities by viewing mediators of different skill levels as corresponding to different equilibria. Our results in Table 3 suggest that the skills required to implement faster agreement may be difficult to acquire: mediators improve slightly as they become more experienced, but there is large variation in mediators' abilities that is not explained by experience. This finding is consistent with a number of other papers which have documented persistent productivity differences among individual employees (e.g., salespeople in Jindal and Newberry 2022 or auctioneers in Lacetera et al. 2016) despite strong incentives for firms to attempt to remove heterogeneity through training, suggesting that some skills may be difficult to transfer from high to low performers.

Our final investigation in this section looks for evidence regarding the *surplus*, or *gains from trade*, in trades that are consummated by different mediators. The gains from trade can be defined as the buyer's willingness to pay (the buyer's value) less the price at which seller is willing to sell (the seller's value). While these objects are unobserved in the data, it is intuitive that a seller with a higher value would report a higher secret reserve price and that the buyer's value is related to the auction price: the last bidder standing in the auction must have a value higher than the auction price. The model in Section 5 formally derives these and other properties. Here, we simply use these arguments to motivate that the reserve price minus the auction price — we call this the *reserve-auction gap* for brevity — is negatively related to the true surplus.[47]

Figure 7.B shows a binned scatter plot with trade probability on the vertical axis and the reserve-auction gap on the horizontal. We plot this separately using high-, medium-, and low-skilled mediators. Within each group, the higher the reserve-auction gap, the lower the probability of agreement. This is because a larger reserve-auction gap reflects a greater chance that gains from trade are small or nonexistent (nonexistent meaning the seller values the car more than the buyer). Figure 7.B shows that the different mediators achieve a relatively similar probability of agreement when the reserve-auction gap is small. These low-gap cases likely reflect transactions where the surplus is large enough for any mediator, regardless of skill, to achieve agreement. Where mediator skill diverges is for higher reserve-auction gaps. Here, where the true gains from trade are likely smaller, and hence negotiators are less likely to naturally agree, high-skilled mediators substantially outperform medium-skilled mediators, who outperform low-skilled mediators. This

[47]The reserve-auction gap is always a positive number for any observation that enters bargaining: the auction price being less than the reserve price is why the auction fails and bargaining begins.

raises the question of whether better mediators largely only capture additional *low-surplus* trades missed by other mediators. If so, better mediators may have only minimal effects on welfare, in spite of their increased trade probability. This question requires a model, to which we now turn.

5 The Effect of Mediators on Welfare

In this section, we construct a structural model to quantify how mediators' performance gaps affect buyer, seller, and total welfare. Our goal is to analyze whether mediators who achieve a higher trade probability also achieve higher overall efficiency, or whether instead these mediators are simply capturing low-surplus trades (i.e., cases where the buyer values the car only slightly more than the seller, which adds little to overall efficiency). For this analysis, we construct a structural model building on the incomplete-information, mechanism design framework for bilateral trade of Myerson and Satterthwaite (1983) and Williams (1987). Our empirical approach applies that of Larsen (2021), extended to allow for different mediators to implement different equilibria.

5.1 Conceptualizing Mediators as Implementing Different Equilibria. Mediators could potentially influence outcomes in a number of ways. To our knowledge, the theory literature offers no model of mediator *heterogeneity*, but we can extract possible theories that might generate heterogeneity from the literature that compares *mediated* to *unmediated* bargaining. For example, several theories discussed in Section 2 suggest that mediation can improve efficiency when a mediator elicits private information from each party and selectively discloses it (e.g., JPS, Goltsman et al. (2009), Hörner et al. 2015, Fanning 2021). Heterogeneity could therefore potentially arise if some mediators are better at information elicitation or optimal disclosure. Alternatively, heterogeneity might arise if some mediators are better informed than others about agents' values, a feature of Gottardi and Mezzetti (2024). Finally, in a setting like ours with multiple equilibria, different mediators could correspond to distinct equilibria. For our model, we take the strong view that mediators influence outcomes only through the final channel; better mediators do not have (or extract) different information from other mediators, but have the ability to coordinate agents on a higher-trade equilibrium. This structure turns out to be sufficiently flexible to capture large mediator effects on outcomes. This is because sequential-offer, incomplete-information bargaining games have a rich set (indeed, a continuum) of qualitatively different equilibria (Ausubel et al. 2002).[48] Even with

[48]This feature of multiple, qualitatively different equilibria is also discussed in Gul and Sonnenschein (1988) in the context of sequential-offer bargaining games and Satterthwaite and Williams (1989) in the context of simultaneous

a fixed bargaining protocol and rational agents, a backdrop where mediators implement different equilibria can yield a wide array of trade probabilities and welfare results across equilibria.[49]

In addition to its flexibility, this framework is empirically appealing due to several properties derived in Larsen (2021) that identify model primitives while remaining largely agnostic about equilibria. By the revelation principle (Myerson 1979), each BNE has a corresponding direct mechanism pinning down the probabilities with which buyer and seller types trade. As we show, these different direct mechanisms are nonparametrically identified in our data. The data also allows us to identify (or partially identify) agents' value distributions, which, combined with the direct mechanisms, are sufficient to quantify welfare under different mediators. Our identification arguments require data not only from transactions involving mediated bargaining, but also those that ended at other stages of the game. For example, we need auction and reserve price data for successful auctions. We use the estimated primitives (buyer values, seller values, and mediators' direct mechanisms) to estimate welfare solely within the bargaining stage of the game under different mediator skill levels.[50]

5.2 Model Setup. Our discussion follows Larsen (2021). Identification relies heavily on a basic model of the pre-bargaining (auction) stage. We maintain the following assumptions:

(A1) *$N \geq 2$ risk-neutral bidders participate in an ascending button auction with zero participation costs. For $i = 1, ..., N$, each buyer i has a value $Y'\gamma + W + B_j$, with $B_j \sim F_B$ and $W \sim F_W$, and with $(X,W,N,\{B_j\}_{j=1}^N)$ mutually independent.*

(A2) *A risk-neutral seller has a value $Y'\gamma + W + S$, with $S \sim F_S$ and with S independent of $(Y,W,N,\{B_j\}_{j=1}^N)$.*

(A3) *Post-auction bargaining lasts for up to $T < \infty$ periods; buyers incur a common bargaining cost, $\eta_B > 0$ (and sellers η_S), for each offer made.*

For each auction, we treat each bidder j as having a private value B_j drawn from F_B (with density f_B and support $[\underline{b}, \overline{b}]$) and the seller as having a private value, $S \sim F_S$ (with density f_S and support $[\underline{s}, \overline{s}]$), where B_j and S are assumed to be independent for all j. Note that when referring

offers. Larsen (2021) described several examples of equilibria of the bargaining subgame at used-car auctions.

[49] This is not the case for all games. For example, consider a sealed-bid second-price auction, where, if we were to assume all agents play dominant-strategies, all agents simply bid their values. There is then no scope for third-party influence through equilibrium coordination, because equilibrium outcomes are fully pinned down.

[50] Larsen and Zhang (2018) studied the full auction-plus-bargaining game in this market, finding that increases in the number of bidders can improve efficiency, consistent with theoretical implications of Williams (1999).

to the value of the highest bidder – who potentially enters bargaining with the seller – we use the notation B, without a j subscript. The number of bidders in a given auction is a random variable N. In addition to their own private values, all agents, the seller and bidders, observe a game-level heterogeneity component, $Y'\gamma + W$, where Y is a vector observable to the econometrician, W is a scalar unobserved by the econometrician, and γ is a parameter vector to be estimated.

$A1$–$A2$ imply that B_j, S, N, Y, and W are mutually independent. We allow bidders' values to be correlated with one another and with sellers, but only through Y and W. We invoke this assumption because of evidence elsewhere in the literature that an independent private values (IPV) model, or IPV with unobserved heterogeneity, fits well for used-car auctions, and because allowing for additional correlation — or for interdependent/common values — is beyond the current state of empirical methodologies for ascending auctions.[51] In estimation we will parse out this game-level heterogeneity. Here we describe the game *conditional* on a realization of game-level heterogeneity, $Y'\gamma + W$. We omit dependence on this realization in our notation, and return to the role of heterogeneity in Proposition 5. See Appendix C for additional model discussion.

We review the timing of the game, described in Section 3, in the context of our model. Let t represent the period of the game. Period 0 begins with nature assigning an intermediary, κ (relevant only if the game reaches mediated bargaining), and with the seller choosing a secret reserve price R. In period 1, N bidders participate in an ascending auction (which we assume, for simplicity, follows a button auction format), resulting in an auction price, P^A. If $P^A \geq R$, the seller and high-bidder trade. Otherwise, the high-bidder is given the opportunity to walk away. If she does not walk away, the game continues to period 2, the realization of κ is revealed, and the seller can accept, reject, or counter in response to the auction price. If the seller counters, the game continues to period 3, the buyer's turn, and so on.[52] Bargaining lasts for up to T periods, which, by $A3$, is finite (but potentially large). The final bargained price is above P^A; this is the standard outcome in the game, and we treat it here as a requirement of the auction company (although in practice this is not explicitly imposed).

We allow κ to take values in $\{L, M, H, \emptyset\}$, standing for low, medium, or high skill levels; a value

[51]Structural models of similar used-car auctions (but in Korean) in Roberts (2013) and Kim and Lee (2014), found evidence consistent with IPV. Earlier used-car auctions work (Genesove 1993) found weak evidence of a common values component at used-car auctions; sellers' information disclosure requirements have improved since then, arguably reducing potential adverse selection concerns that could arise in common values setting and rendering the environment of IPV with unobserved heterogeneity more palatable.

[52]We will use the terms "high bidder" and "buyer" interchangeably.

of $\kappa = \emptyset$ means that the negotiation stage took place without a mediator or that the negotiation failed to meet our sample criteria from Section 3. As described in Section 3, a negotiation can occur without a mediator if the seller is physically present at the auction sale and accepts or rejects P^A before any mediator involvement. It is also possible for the auctioneer to reject P^A on behalf of the seller if the seller is absent. A negotiation falls outside of our Section-3 sample criteria, for example, if it is handled by multiple mediators or by a mediator who handled fewer than 50 negotiations (meaning we lack data to classify the mediator's skill level). We assume the following:

(A4) κ *is independent of* $(Y, W, N, S, \{B_j\}_{i=1}^N)$.

Section 4 provides some evidence that mediator assignment is largely random conditional on observables. For our structural exercise, $A4$ is a stronger, unconditional independence assumption, but we still take a first step of residualizing actions against a large set of observables (a standard "homogenization" approach in the auction literature; Haile et al. 2003). Conditional independence would be weaker but would require continued conditioning on observables — or an index of observables — in subsequent estimation steps, greatly increasing the computational burden.

Payoffs are as follows. If trade occurs at a price P, a buyer with value B_j receives $B_j - P$ and the seller receives a payoff of P. If trade fails, the buyer receives 0 and the seller receives S (her private value for the car). Agents also face any incurred bargaining costs. These costs, described in $A3$, consist of a disutility η_B that the buyer pays for each period he chooses to continue the game (by making an offer or by not opting out initially) and similarly for the seller.

Let $\rho(S) = R$ be the seller's secret reserve price strategy. Let ζ_j to be the drop-out price of bidder j in the auction. When $R > P^A$, let $\pi^B(P^A, B)$ be the high bidder's expected value of not opting out of bargaining, conditional on his value and on the auction price. Let $D_1^B = 1$ represent the buyer's decision to walk away when informed that $P^A < R$ (and $D_1^B = 0$ means not walking away). Let $D_t^B \in \{A, C, Q\}$ be the buyer's decision to accept, counter, or quit at odd $t > 1$. Let P_t^B be the buyer's counteroffer (if the buyer counters) in period t. Let D_t^S and P_t^S be defined similarly for even periods t. Let H_t be the history of publicly observed actions up through period $t-1$ of the game. These actions include P^A and all previous bargaining offers and period-specific decisions.

We focus on pure strategy BNE of this game.[53] An equilibrium in this game is a history-contingent set of actions $\sigma_j^B(B_j, \kappa) = \{\zeta_j, \chi, \{D_t^B | H_t, \kappa\}_j, \{P_t^B | H_t, \kappa\}_j\}$, where the decisions D_t^B

[53]In incomplete-information sequential bargaining, refinements (such as perfect Bayes equilibrium) do little or nothing to refine equilibria because beliefs after off-path actions sustain a wide range of behavior (Gul and Sonnenschein

and offers P_t^B included are those for periods in which it is the buyer's turn. The strategy of a seller of type S is a history-contingent set of actions $\sigma^S(S,\kappa) = \{\rho, \{D_t^S | H_t, \kappa\}, \{P_t^S | H_t, \kappa\}\}$. A set of strategies $\{\sigma^{B*}(B_j,\kappa)\}_{j=1}^N$ for all buyers and $\sigma^{S*}(S,\kappa)$ for the seller is a BNE if, for each player, her strategy is a best response to opponents' strategies and players update their beliefs about opponent values using Bayes rule at each history of the game reached with positive probability. Equilibrium actions that occur after mediator assignment is revealed are allowed to depend on κ.

5.3 Model Properties. We present several properties that help identify primitives. We continue to ignore game-level heterogeneity, $Y'\gamma + W$, and we return to it in Proposition 5.

Buyer Values. Our first result addresses bidding behavior in the auction:

Proposition 1. *Suppose* $A1$ *and* $A3$ *hold and consider an arbitrary bidder* j*. In any BNE, holding fixed the strategies of all players in the continuation game, strategies of other bidders, and the reserve price strategy of the seller, it is a weak best response for* j *to play the following: (i) bid truthfully in the auction and (ii) enter bargaining only if doing so yields a non-negative expected payoff.*

This is akin to the standard truthtelling result in button auctions but here the result is only a weak best response rather than dominant because of the post-auction subgame, which can potentially condition on auction outcomes in ways that make truthtelling not dominant. The proof of Proposition 1 shows that the key model features for this result are (i) if a bidder learns $P^A < R$ and she is the high bidder, she can costlessly opt out of bargaining, eliminating any downward bid shading; and (ii) the bargaining price is weakly greater than P^A, eliminating incentive to shade auction bids upward. As in many prior empirical studies of ascending auctions, having shown existence, we assume bidders follow this truthtelling strategy, allowing us to infer the distribution of buyer values from the distribution of auction prices using order statistics.

(A5) Bidders bid truthfully in the auction and enter bargaining only if doing so yields a non-negative expected payoff.

Under $A5$, the distribution of auction prices, F_{P^A}, is equal to the distribution of the second-highest order statistic of buyer values, and hence F_B is related to F_{P^A} and $\Pr(N = n)$ (the proba-

1988). Mixed-strategy equilibria may also exist; we focus on pure strategies to facilitate the model properties we exploit for identification while still being sufficiently flexible to admit a wide range of behavior.

bility mass function of N) as follows, evaluated at any number v:

$$F_{P^A}(v) = \sum_n \Pr(N = n) \left[nF_B(v)^{n-1} - (n-1)F_B(v)^n\right] \tag{5}$$

For any v, $F_{P^A}(v)$ can be estimated from data on auction prices, and the right-hand side is monotonic in $F_B(v)$, and thus $F_B(v)$ is identified.[54]

Seller Values. Let $D^S = A$, without a t subscript (to distinguish this from the period-specific action described in Section 5.2), represent the event in which the seller takes an action in period 1 or 2 that results in the game ending in agreement at P^A. This event occurs either when 1) $P^A \geq R$ or 2) $P^A < R$, the high bidder does not opt out of bargaining, and the seller accepts the auction price on her first bargaining turn. Similarly, let $D^S = Q$ represent the event in which the seller takes an action in period 2 that results in the game ending in disagreement in that period. This event happens when $P^A < R$, the high bidder does not opt out of bargaining, and the seller rejects P^A or quits on her first bargaining turn (rather than accepting P^A or making a counteroffer).[55]

In any BNE, the seller will not accept P^A if $P^A < S$ (the seller would be better off quitting and getting S instead of P^A) and will not quit if $S < P^A$ (the seller would be better off accepting, receiving P^A instead of S). This implies the following bounds:

Proposition 2. *Under A1–A5, for any* $v \in [\underline{s}, \overline{s}]$, $F_S(v) \in [E_\kappa\{\Pr(D^S = A|P^A = v, \kappa)|P^A = v\}, E_\kappa\{\Pr(D^S \neq Q|P^A = v, \kappa)|P^A = v\}] = [\Pr(D^S = A|P^A = v), \Pr(D^S \neq Q|P^A = v)]$.

The notation E_κ indicates an expectation taken over mediator assignment, which still yields bounds on F_S because, by *A4–A5*, $\Pr(S \leq v|P^A = v, \kappa) = \Pr(S \leq v)$. The property underlying these bounds is similar to that of the English-auction bounds in Haile and Tamer (2003) but applied to the seller's decision to accept or reject the first bargaining offer (P^A). As explained above, period 2 may or may not involve a mediator interaction. We do not use mediator assignment in estimating seller bounds but instead estimate bounds that average over mediator assignment (averaging over both whether a mediator is involved beginning in period 2 and, if so, which mediator is assigned). This

[54] Equation (5) can be equivalently written $F_{P^A}(v) = \sum_n \Pr(N = n) \left[F_B^n(v) + n\,(1 - F_B(v))\, F_B^{n-1}(v)\right]$. Note that, throughout the paper, for random variables that can take on *infinitely many* values (e.g., Y, W, N, S, B_j, R, P^A), we use corresponding lowercase letters to denote realizations. We use v as a generic argument in various functions.

[55] For purposes of bounding F_S, we treat cases where $P^A < R$ and the auctioneer rejected P^A on behalf of the seller as cases where the seller herself rejected P^A.

allows us to obtain a single set of bounds on F_S and to focus on the different equilibria/mechanisms mediators implement, which we address below. See Appendix C.9 for additional discussion.

Belief Updating After Auctions. If $P^A < R$, the buyer can opt out of bargaining, and does so if $\pi^B(P^A, B) < 0$. Define $\chi(B)$ by $\pi^B(\chi(B), B) = 0$. When bargaining ensues, both the buyer and seller are aware that the support of buyer types who would enter bargaining is truncated: $B \geq \chi^{-1}(P^A)$. Similarly, both parties are aware that $P^A < R$ means S is truncated: $P^A < \rho(S)$ (recall that $\rho(S) \equiv R$.) Proposition 3 below states this result, along with monotonicity properties satisfied by ρ and χ that rely on two additional assumptions:

(A6) The seller's expected payoff in the bargaining subgame is continuous in the auction price.

(A7) The density f_B is positive on $[\underline{b}, \overline{b}]$.

A6 is a technical condition that yields differentiability of the seller's payoff and allows us to apply the Edlin and Shannon (1998) theorem to prove strict monotonicity of $\rho(\cdot)$. *A7* is invoked in the proof of Proposition 3 to avoid dividing by zero.

Proposition 3. *Under A1–A7, conditional on $P^A = p^A$, the support of buyer and seller types in bargaining is $[\chi^{-1}(p^A), \overline{b}]$ and $[\rho^{-1}(p^A), \overline{s}]$, where $\rho(\cdot)$ and $\chi(\cdot)$ are strictly increasing.*

In addition to *A6*–*A7*, a component of proving strict monotonicity of $\rho(\cdot)$ is buyers' ability to opt out of bargaining after the auction if the expected payoff is negative. This disciplines sellers to set finite reserve prices. By Proposition 3, the density of seller and buyer types entering bargaining conditional on $P^A = p^A$ are $f_S(s|p^A) \equiv \frac{f_S(s)}{1-F_S(\rho^{-1}(p^A))}$ and $f_B(b|p^A) \equiv \frac{f_B(b)}{1-F_B(\chi^{-1}(p^A))}$. These reflect Bayes-updated beliefs at the beginning of the bargaining stage. Proposition 3 implies

$$\Pr(D_1^B = 0 | P^A = p^A, P^A < R) = \frac{1 - F_B(\chi^{-1}(p^A))}{1 - F_B(p^A)}, \tag{6}$$

an expression that we use below in estimating $\chi^{-1}(\cdot)$.

Direct Mechanism. We consider different mediator types κ to be different equilibria of the bargaining subgame. By the revelation principle (Myerson 1979), any BNE has an equivalent *direct mechanism* in which agents truthfully report types to a mechanism designer who ensures outcomes occur as if the original game had played out. A mechanism is described by an *allocation* *function* specifying the probability with which a given seller and buyer trade and a *transfer function* specifying transfers between agents. For our purposes, we do not need to identify the entire transfer function, only the expected price of consummated trades, which is observable in the data. Identifying the allocation function, however, is essential and requires more work.

The allocation function for a given mediator type can be written $x_\kappa(s, b; p^A)$, as the mechanism can depend on mediator type and P^A; this is because outcomes at the auction directly depend on the realization of P^A and outcomes in bargaining can also because P^A is the first bargaining offer, affecting belief updating. We prove the following:

Proposition 4. *Under A1–A7, in any BNE, the allocation function for any mediator type κ can be written $x_\kappa(r, b; p^A) \equiv 1\left\{b \geq g_\kappa\left(r, p^A\right)\right\}$, where $g_\kappa(r, p^A)$ is weakly increasing in r.*

This threshold form for the allocation function is a common result in bargaining mechanisms. Ausubel and Deneckere (1993) referred to this property as the "Northwestern Criterion" as it implies that trade occurs if and only if players' types lie northwest of a boundary defined by g_κ.[56] Proposition 4 also incorporates the result from Proposition 3 that $\rho(\cdot)$ is strictly increasing, and thus we define the allocation function as a function of r (which is observable to the econometrician) rather than s (which is privately known only to the seller). Proposition 4 implies that the function g_κ in $1\{b \geq g_\kappa(r; p^A)\}$ is related to the conditional probability of agreement as follows:

$$\Pr(\mathcal{A}|R = r, P^A = p^A, \kappa) = \frac{1 - F_B\left(g_\kappa(r, p^A)\right)}{1 - F_B\left(p^A\right)}. \tag{7}$$

where $\mathcal{A}$ is the event that trade occurs. The denominator accounts for the fact that any buyer in bargaining has $B \geq P^A$. The conditional probability on the left of (7) is the key to identifying g_κ: because F_B is identified by (5), at a given $P^A = p^A$ and $R = r$, the quantile of $(1 - F_B(\cdot))/(1 - F_B(p^A)$ that matches the conditional trade probability reveals the lowest buyer type that trades with a seller who reports a reserve of $R = r$. This lowest buyer type is $g_\kappa(r, p^A)$.

Game-level Heterogeneity, We now re-introduce game-level heterogeneity, $Y'\gamma + W$:

Proposition 5. *Fix any mediator type κ (thus also holding fixed the BNE) and suppose A1–A7 hold. Suppose, when $Y'\gamma + W = 0$, the equilibrium involves reserve price r; auction price p^A; a*

[56]Note that, in a more general framework of mixed strategies, equilibria could exist that correspond to random allocation functions that yield trade with some probability in $(0, 1)$.

lowest buyer type who would choose to bargain $\chi^{-1}(p^A)$; and, for each period t at which the game arrives, the bargaining offer is $P_t = p_t$ and the decision to accept, quit, or counter is $D_t = d_t$. Then, for any $Y'\gamma + W = z$, the equilibrium will involve reserve price $\tilde{r} = r + z$; auction price $\tilde{p}^A = p^A + z$; the lowest buyer type who would choose to bargain $\chi^{-1}(\tilde{p}^A - z) + z$; the period t decision d_t; and, for any period t offer that is accepted with positive probability, the period t offer is $p_t + z$.

This result is similar in spirit to other homogenization results (Haile et al. 2003) but applied here to this specific case of a secret reserve auction followed by bargaining. It implies that the game is location invariant, allowing us to parse out game-level observable heterogeneity using a linear regression and unobserved heterogeneity via a deconvolution exercise, described below.

5.4 Evaluating Gains from Trade. The expected gains from trade in the bargaining stage of the game, under allocation function $x_\kappa(r, b; p^A)$, can be evaluated as

$$\int_{\underline{b}}^{\overline{b}} \left[\int_{\chi^{-1}(p^A)}^{\overline{b}} \int_{\rho^{-1}(p^A)}^{\overline{s}} (b - s) x_\kappa(\rho(s), b; p^A) f_S(s|p^A) f_B(b|p^A) ds\, db \right] f_{p^A}(p^A) dp^A \tag{8}$$

We also subtract from this quantity an estimated bound on the loss due to bargaining costs, described in Appendix C. We evaluate these gains separately for low-, medium-, and high-skilled mediators. As a counterfactual benchmark, we also compute the infeasible first-best mechanism, which is simply $1\{B \geq S\}$ — trading whenever gains from trade exist. We also compute the second-best following Myerson and Satterthwaite (1983) and Williams (1987).[57] The Myerson-Satterthwaite Theorem shows that a gap exists between the second- and first-best; the latter is infeasible because each party has incentives to retain information rents. Larsen (2021) showed that inefficiency in the real-world mechanism is even larger than this gap, as the real-world outcome lies below even the second-best. Our setting allows us to unpack some of this shortfall to examine whether certain mediators are better at reducing this inefficiency.

Expression (8) shows that the key objects required for evaluating the trade gains are the allocation functions for each mediator type, $x_\kappa(r, b; p^A)$; the conditional densities $f_S(s|p^A)$ and $f_B(b|p^A)$; the truncated support bounds for the types who enter bargaining, $\chi^{-1}(p^A)$ and $\rho^{-1}(p^A)$; and the density of auction prices f_{P^A}. The truncated supports allow us to compute welfare solely within the bargaining stage of the game, which is the only stage mediators can potentially influence.

[57] Appendix Figure C.2 shows the estimated welfare under different mediators relative to the full Pareto frontier of Williams (1987), of which the Myerson-Satterthwaite second-best is the highest-utility point.

5.5 Estimation Sample and Steps. Our estimation relies on data that merges our indicators of mediator type from the mediated negotiation data with an auction-plus-bargaining dataset of 264,996 observations from Larsen (2021). The latter only partially overlaps with the mediated negotiation data: some observations in the auction-plus-bargaining data end at the auction stage and have no mediator identifiers, and some observations in the mediated negotiation data are missing variables found in the auction-plus-bargaining data that are important for our analysis.

The merge yields the following number of observations handled by mediators whom we can identify as low, medium, or high-skilled, respectively: 11,764 (denote this set by N_L), 10,235 (N_M), 10,043 (N_H). In 36,400 observations, a mediator identity is recorded but the negotiation does not meet our sample criteria from Section 3 for classifying mediator skill; e.g., the mediator handles too few observations. We denote this set N_ω. Let N_A denote the set of 196,524 observations that ended at the auction stage, ended with the buyer opting out of bargaining, or ended through bargaining but with *no* mediator recorded. Let N_B be the union of N_L, N_M, N_H, and N_ω (and thus the union of N_A and N_B is the full dataset).

The model properties yield identification arguments and corresponding estimation approaches. To conserve space, we summarize estimation steps here and relegate details to Appendix C. Steps 1–5 do not require conditioning on mediator type, and these steps follow Larsen (2021) closely. Step 6 incorporates mediator type, where we estimate the g_κ functions for each mediator type κ.

1. **Observed Heterogeneity.** By Proposition 5, auction and reserve prices are additively separable in game-level heterogeneity. We control for this heterogeneity via a stacked linear regression of auction and reserve prices on a rich set of game-level observables Y, yielding an estimate of γ in $Y'\gamma + W$. Residuals from this regression correspond to $\tilde{R} \equiv R + W$ and $\tilde{P}^A \equiv P^A + W$, where R and P^A are the homogenized reserve and auction prices. This step uses all observations in $N_A \cup N_B$, in addition to some observations in which auction or reserve prices are missing (but not both). This improves estimation of γ given the large number of fixed effects we control for. See Appendix C.

2. **Unobserved Heterogeneity.** By Proposition 5, the residuals from the Step 1 regressions are additively separable in W. By independence of R, P^A, and W, a convolution argument implies that the marginal densities f_R, f_W, and f_{P^A} are identified from the joint distribution of $\tilde{R}$ and

$\tilde{P}^A$.[58] We estimate these marginal densities f_{P^A}, f_R, and f_W and their corresponding CDFs using a flexible maximum likelihood approach. This step uses all observations in $N_A \cup N_B$.

3. **Buyer Values.** By $A5$ and Proposition 1, we can use the CDF F_{P^A}, estimated in Step 2, along with $\Pr(N = n)$, to identify and estimate F_B by directly solving (5) on a grid of values. Appendix C contains details about the number of bidders and how $\Pr(N = n)$ is estimated. This step uses all observations in $N_A \cup N_B$.

4. **Seller Values.** Ignoring unobserved game-level heterogeneity, the lower and upper bounds on F_S in Proposition 2 are probabilities conditional on P^A. Accounting for unobserved heterogeneity, the objects we can estimate in the data instead condition on $\tilde{P}^A$, a noisy measure of the auction price with unobserved heterogeneity included. These objects are $\Pr(D^S = A|\tilde{P}^A = v)$ and $\Pr(D^S \neq Q|\tilde{P}^A = v)$, which we estimate via local linear regressions of the events $D^S = A$ and $D^S \neq Q$ on the noisy auction price, $\tilde{P}^A$. Considering the lower bound, the theoretical counterpart for this conditional probability is a convolution of $\Pr(D^S = A|P^A = v)$ with f_W. The expression for this convolution (in Appendix C) can be used to estimate bounds on F_S, parsing out W using a flexible parameterization of the bounds (piecewise linear splines) and minimizing the distance between the estimates of $\Pr(D^S = A|\tilde{P}^A = v)$ and its theoretical counterpart. The upper bound follows similarly. This step uses all observations in $N_A \cup N_B$.

5. **Belief Updating (Functions $\rho(\cdot)$ and $\chi^{-1}(\cdot)$).** For any function $F_S(\cdot)$ lying in the estimated bounds, $\rho(s)$ can be constructed as $\rho(s) = F_R^{-1}(F_S(s))$, with F_R replaced with $\hat{F}_R$ from Step 2. To estimate $\chi^{-1}(\cdot)$, we modify (6) to incorporate unobserved heterogeneity, which becomes another convolution, and minimize the distance between the left and right-hand sides of (6), parameterizing $\chi^{-1}(\cdot)$ via piecewise linear splines. As indicated by the conditional probability in (6), this step conditions on observations from $N_A \cup N_B$ in which $R > P^A$.

6. **Direct Mechanisms.** To estimate each $g_\kappa(\cdot)$, we modify (7) to include game-level unobserved heterogeneity – another convolution – and minimize the distance between the left and right-hand sides of (7), parameterizing $g_\kappa(\cdot)$ via bilinear splines. We create appropriate pseudo samples to estimate these g_κ functions separately by mediator type. To estimate g_L, for example, we combine the set N_A with $|N_B|$ observations drawn with replacement from the

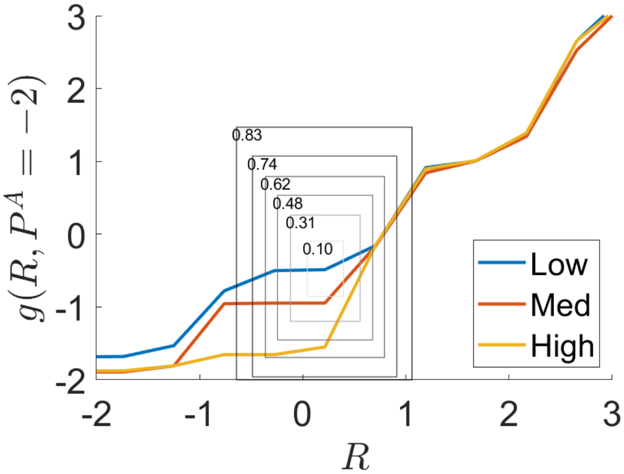
[58] A similar convolution argument was first applied to auction-level heterogeneity by Krasnokutskaya (2011).

N_L observations mediated by low-skilled mediators (where $|\cdot|$ denotes the number of elements in a set). We create similar pseudo samples for medium- and high-skilled mediators. This allows each g_κ function to be estimated as if using a full auction-plus-bargaining dataset with only one mediator type while maintaining the same size of the dataset and same proportion of observations ending at the auction or bargaining stage.

5.6 Estimated Allocation Threshold Functions by Mediator Type. The estimated $g_\kappa(R, P^A)$ functions for the three mediator types are shown in Figure 8, with units in \$1,000. This figure contains similar information to the model-free reserve-auction gap plot in Figure 7.B, but offers additional insights through the lens of the structural model, which allows us to plot and interpret a function of values (e.g., B) rather than just an object related to values (P^A). To aid visualization, we plot R on the horizontal axis and B on the vertical, and hold P^A fixed at \$2,000 below the game-level heterogeneity components, $Y'\gamma + W$. After subtracting the heterogeneity components, because heterogeneity enters additively, values and prices (such as R, P^A, and B) will range from negative to positive. For example, a reserve price of $R = -1$ in Figure 8 means \$1,000 below the car's game-level heterogeneity component. Each mechanism yields trade if B lies above a given $g_\kappa(R, P^A)$ line. The contour plot and corresponding numbers show the amount of mass under the joint pdf of B (conditional on $B > P^A$) and R.

Figure 8 shows a clear ordering of mediator types: low-skilled mediators implement a higher threshold function, consummating fewer efficient trades, than do medium-skilled mediators, who in turn consummate fewer efficient trades than high-skilled mediators (the lowest function vertically). This structural exercise reveals that, at very high reserve prices (or, equivalently in our model, high seller values), the assigned mediator makes little difference, suggesting that, when the seller's value for the car is high relative to the buyer's, the negotiation is likely to fail regardless of the mediator skill. For very low reserve prices, the different g_κ functions again align almost perfectly, suggesting that the negotiation is likely to succeed regardless of mediator skill. It is for the middle range of reserve prices — around \$700 below to \$700 above the game-level heterogeneity component — where mediator skill appears to have an effect. At a reserve price equal to the game-level heterogeneity component (shown as zero on the horizontal axis), a low-skilled mediator only successfully closes a deal for buyers who have a value of at least -\$500 (i.e., \$500 below the heterogeneity component). Faced with the same seller reserve price situation, a medium-skilled mediator is able to generate

Figure 8: Direct Mechanisms $g(\cdot)$ Functions for Different Mediator Types



Notes: Figure displays estimates of $g(R, P^A)$ for low- (blue), medium- (red), and high-skilled (yellow) mediators, where the auction price is held at a value of \$2,000 below the heterogeneity value ($Y\gamma + W$) for the car; that is, $P^A = -2$, as units are \$1,000. Trade occurs if $B \geq g(R, P^A)$. Contour plot and corresponding numbers show the amount of mass under the joint pdf of B (conditional on $B > P^A$) and R.

trades for buyers with values approximately above -\$900, whereas a high-skilled mediator captures trades even for lower buyer values, when the buyer's value exceeds about -\$1,700.

5.7 Welfare Results. Because F_S is only partially identified, we obtain bounds on trade gains. We first evaluate bounds on the difference in trade gains, from (8), between a given real-world mediator's mechanism and the first- or second-best mechanism, for the average negotiation.[59] Figure 9.A shows, in blue, bounds on the difference between the first-best and real-world outcomes, separately for negotiations mediated by low-, medium-, or high-skilled mediators.[60] These represent bounds on the deadweight loss in bargaining — gains that real-world bargaining fails to capture but that

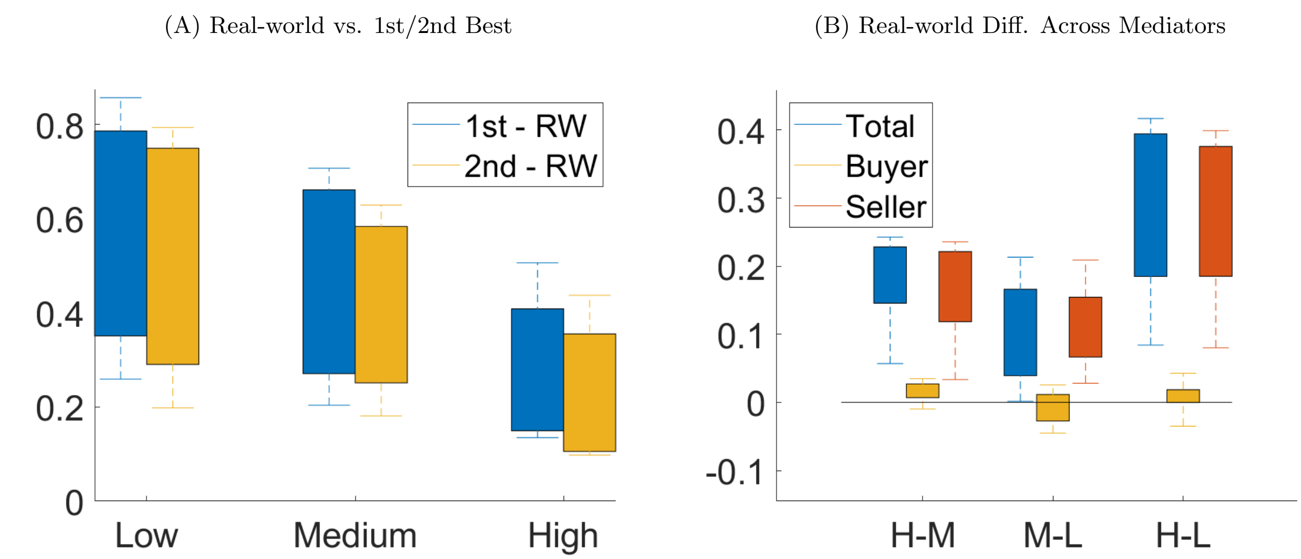
[59]Proposition 6 of Larsen (2021) showed that, across all possible F_S within the seller CDF bounds, an upper bound on the gap between trade gains in the first-best and in the real-world bargaining is given by the difference between (8) evaluated at the upper bound seller CDF and (8) evaluated at the lower bound seller CDF. A lower bound on the trade gains difference is given analogously. This same bounding property applies to the gap between trade gains under any two different mediator types (high- vs. low-skill, say). For consistency, we follow this same approach (evaluating trade gains at the lower bound and upper bound seller CDFs) when evaluating the *gap between the real-world and second-best* outcomes, but, unlike the gap between the real-world and *first-best* outcomes, we have no theoretical result proving that this yields a bound on the difference across all seller CDFs. The qualitative implications from examining first- and second-best mechanisms (relative to any of the mediator mechanisms) are quite similar; this is because the first- and second-best are similar in this market; see Larsen (2021).

[60]Dashed lines show 95% bootstrapped confidence sets for these bounds. Confidence bounds throughout this section are constructed via a nonparametric percentile bootstrap, using 200 bootstrap replications of our estimation procedure and reporting the 0.025 quantile of the estimated lower bound on an object of interest and the 0.975 quantile of the estimated upper bound; these are conservative by a Bonferroni-style argument.

would be realized in a first-best world because they correspond to a buyer valuing the car more than the seller. A larger number in this figure thus represents a larger loss relative to the first-best efficient outcome. Yellow bars show similar bounds comparing to the second-best outcome.

In Figure 9.A, under each mediator type, the outcome is slightly closer to the second-best than to the first-best; this is because the second-best falls short of the first-best (the inefficiency described by Myerson and Satterthwaite 1983). The interesting finding from this figure is that high-skilled mediators appear better at reducing some of the deadweight loss between the real-world outcome and these theoretical benchmarks. This deadweight loss is at least \$100, regardless of the mediator, but for high-skilled mediators we can reject the largest efficiency shortfalls (those greater than \$500), and we fail to reject these levels for low-skilled mediators.

Figure 9: Surplus Differences Across Mediators



Notes: Panel A displays, in solid blue, bounds on the difference in total surplus between first-best (1st) and real-world bargaining (RW), separately for threads mediated by low, medium, and high-skilled mediators; and solid yellow displays bounds on the difference between the second-best (2nd) and real-world outcomes. Panel B displays, in solid blue, bounds on the difference in total surplus in the real-world mechanism between high vs. medium-skilled mediators (H-M), medium vs. low-skilled (M-L), and high vs. low-skilled (H-L); solid red lines display bounds on the difference in seller surplus; and solid yellow lines display bounds on the difference in buyer surplus. All bounds are computed by evaluating differences at the upper and lower bounds on F_S. Whiskers show 95% confidence intervals on these bounds constructed via bootstrapping with 200 replications. Units on the vertical axis are \$1,000.

Our primary result is in Figure 9.B, where we show realized surplus differences for the average negotiation mediated by high- vs. medium-skilled (H-M), medium- vs. low-skilled (M-L), and high- vs. low-skilled (H-L) mediators. These total surplus differences are in solid blue. Solid yellow and red bars show buyer and seller surplus separately. We observe a statistically significant difference in total surplus for all three pairwise comparisons, with a surplus increase of about \$150–250 between high- vs. medium-skilled mediators, \$50–175 for medium- vs. low-skilled, and \$200–400 for high- vs. low-skilled. This implies that mediators who achieve higher trade probabilities also achieve a statistically significant increase in total welfare. Relative to the shortfall from the theoretical benchmarks shown in Figure 9.A, these quantities represent a meaningful improvement in efficiency.

A priori, it is unclear whether mediators' improvements in surplus would accrue mostly to buyers or sellers: a high-skilled mediator could achieve higher total surplus by affecting one or both of the agents' surplus. Figure 9.B shows that higher-skilled mediators tend to improve seller surplus, and that we cannot reject a null effect on buyer surplus. This result, combined with the fact that mediators have little effect on trade prices, suggests that, regardless of mediator assignment, more surplus tends to accrue to sellers than to buyers, and hence increases in trade probability and total surplus affect seller surplus more than buyer surplus.[61] Part of this asymmetry is attributable to the finding in Figure 8 that the shift from $g_L(\cdot)$ to $g_H(\cdot)$ is largely a vertical shift, meaning that the additional sellers who trade under high-skilled mediation are quite similar to those who trade under low-skilled mediation, whereas the additional buyers are low-value buyers, and thus the improvement in seller surplus is larger.

Table 4: Auction House Revenue Under Different Mediator Types

	Low	Medium	High
Revenue	0.1352	0.1683	0.2025
	(0.002)	(0.002)	(0.001)
	High - Medium	Medium - Low	High - Low
Revenue Difference	0.0341	0.0331	0.0672
	(0.002)	(0.002)	(0.002)

Notes: Table displays average auction house revenue per sales attempt under different mediator types, as well the differences between average revenues. Standard errors, computed via 200 bootstrap replications, are in parentheses.

A natural question is how large the revenue gains are for the auction house company from having a better mediator. The company collects fees from buyers and sellers only when trade occurs (see Appendix C.8). These fees are observable in the auction-plus-bargaining data, and thus we can estimate the fees collected under different mediator skill levels. Table 4 displays average

[61] The evidence in Larsen (2021), which does not condition on mediator assignment, is consistent with this idea. There, like in this paper, only bounds on surplus are available, but the upper bound on seller surplus is about 2 to 3.5 times larger than the upper bound on buyer surplus; see Table 3 and Table A.4 of Larsen 2021. Appendix C.10 of the current paper, where we discuss the full Pareto frontier, contains additional discussion.

auction house revenue per sales attempt under high-, medium-, and low-skilled mediators, where this revenue, for negotiations handled by a given mediator type, is simply the trade probability multiplied by the average auction house fees collected from negotiations that ended in trade. Average revenue per sales attempt is \$203 for high-skilled mediators, \$168 for medium-skilled mediators, and \$135 for low-skilled mediators. Thus, high-skilled mediators improve auction house revenue by about 50% relative to low-skilled mediators. These numbers are also meaningful relative to the total surplus differences between high- and low-skilled mediators discussed above (\$200–400).

6 Conclusion

In this paper, we have shown that mediators have statistically significant and economically large effects on bargaining outcomes. The 75th-percentile intermediary is 18.07 percentage points more likely to close a deal than the 25th percentile intermediary. The estimated mediator effects are robust to a variety of different controls, and are not driven by sample noise. Effective mediators tend to achieve trade quickly, and particularly stand out above low-performing mediators in situations where agreement does not look like a foregone conclusion (i.e., when the auction price is not close to the reserve price). Mediators appear to become more effective with experience, but there is substantial variation in mediator effectiveness left unexplained by experience. Our structural exercise demonstrated that better mediators not only increase trade probabilities but also have real effects on realized gains from trade. Myerson and Satterthwaite (1983) showed that incomplete-information bilateral bargaining under overlapping supports generically results in deadweight losses — cases where a buyer values the item more than the seller but they nonetheless fail to trade. These represent potential gains from trade left on the table. Our findings suggest that real-world bargaining falls even farther from the first-best outcome than implied by Myerson and Satterthwaite (1983), but that some of this shortfall can be overcome by high-performing mediators who are able to execute a mechanism/equilibrium lying closer to the efficient outcome. We see these findings as potential starting points for future experimental, theoretical, and empirical work in incomplete-information bargaining settings. A natural next step would be to design laboratory experiments to analyze more in-depth the question of *how* good mediators achieve a higher trade probability or to identify interventions that could shift play to better equilibria. We hope these findings can also motivate theoretical investigations explicitly allowing for mediator heterogeneity.

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A Proofs

Proofs in this section follow Larsen (2021) with minor modifications to accommodate mediators.

A.1 Preliminaries. We first state preliminary lemmas. Let $H_t \equiv \{P_\tau\}_{\tau=1}^{t-1}$ represent the set of offers made in periods 1 through $t-1$ of bargaining.[62] Let $D_t^S \in \{A, Q, C\}$ represent the seller's decision at t, and let $D_{t+1}^B \in \{A, Q, C\}$ represent the buyer's decision at $t+1$. As in Section 5, $\kappa \in \{L, M, H, \emptyset\}$ is the mediator type.

The seller's payoff at period t of the bargaining is as follows. Conditional on a realization of $H_t = h_t$, which includes the buyer's most recent offer (p_{t-1}^B), a seller of type $S = s$, chooses to accept (A), quit (Q), or counter (C), yielding the following payoffs for $t < T-1$ and for any κ:

$$\mathrm{A} : p_{t-1}^B$$

$$\mathrm{Q} : s$$

$$\begin{aligned}\mathrm{C} : V_t^S\left(s|h_t,\kappa\right) = \max_p \Bigg\{ & p \Pr\left(D_{t+1}^B = A|\{h_t,p\},\kappa\right) + s \Pr\left(D_{t+1}^B = Q|\{h_t,p\},\kappa\right) - \eta_S \\ & + \Pr\left(D_{t+1}^B = C|\{h_t,p\},\kappa\right) E_{P_{t+1}^B}\left[\max\left\{P_{t+1}^B, s, V_{t+2}^S\left(s|\{h_t,p,P_{t+1}^B\},\kappa\right)\right\} \middle| \{h_t,p\}, D_{t+1}^B = C, \kappa\right]\Bigg\}\end{aligned}$$

where p is the counteroffer chosen by the seller. The seller's counteroffer payoff takes into account that the buyer may either accept, quit, or counter. In the latter case, the seller receives her expected payoff from being faced with the decision at $t+2$ to accept, quit, or counter.

The buyer's payoff at $t+1$ is defined similarly, the buyer receiving $b-p$ if he accepts a price p, 0 if he quits, and an expected counteroffer payoff if he counters. Conditional on a realization of $H_{t+1} = h_{t+1}$, which includes the seller's most recent offer (p_t^S), a buyer of type $B = b$ faces the following payoffs for $t < T-2$ and for any κ:

$$\mathrm{A} : b - p_t^S$$

$$\mathrm{Q} : 0$$

$$\mathrm{C} : V_{t+1}^B\left(b|h_{t+1},\kappa\right) = \max_p \Bigg\{ (b-p)\Pr\left(D_{t+2}^S = A|\{h_{t+1},p\},\kappa\right) - \eta_B + \Pr\left(D_{t+2}^S = C|\{h_{t+1},p\},\kappa\right)$$

[62] The player with the period t turn has not yet made an offer, so this offer does not enter into H_t.

$$\times E_{P^S_{t+2}}\left[\max\left\{b - P^S_{t+2}, 0, V^B_{t+3}\left(b|\{h_{t+1}, p, P^S_{t+2}\}, \kappa\right)\right\}\middle|\{h_{t+1}, p\}, D^S_{t+2} = C, \kappa\right]\Bigg\}$$

where p is the counteroffer chosen by the buyer. The expected payoff of a buyer of type $B = b$ from entering bargaining when $R > p^A$, conditional on winning the auction, is

$$\pi^B(p^A, b) = E_\kappa\Bigg[(b - p^A)\Pr\left(D^S_2 = A|p^A, \kappa\right)$$
$$+ \Pr\left(D^S_2 = C|p^A, \kappa\right) E_{P^S_2}\left[\max\left\{b - P^S_2, 0, V^B_3\left(b|\{p^A, P^S_2\}, \kappa\right)\right\}\middle|p^A, D^S_2 = C, \kappa\right]\Bigg] - \eta_B$$

This expression is the payoff to the buyer from stating the auction price as a counteroffer, which is how the bargaining game begins. Note that $\pi^B(p^A, b)$ does not depend on κ, the mediator type, as this is modeled as being revealed only after the buyer's decision to opt out of bargaining or not.

Lemma 1. *If A1–A3 are satisfied, then for any finite T, any realized history up to period $t < T$, and any κ, the payoff from countering (for the player whose turn it is to counter at period t) is weakly increasing in the player's type.*

Proof. Fix κ. The proof proceeds by induction on the number of periods remaining. WLOG, we prove the result in the case where the buyer moves last. Suppose one period remains (the seller's) out of T total, after which the buyer can only accept or quit. Suppose this node is reached on path. At a given realization of $H_{T-1} = h_{T-1}$, the seller's payoff from countering at price p is

$$U^S_{T-1}(s, p|h_{T-1}, \kappa) \equiv p\Pr(D^B_T = A|\{h_{T-1}, p\}, \kappa) + s(1 - \Pr(D^B_T = A|\{h_{T-1}, p\}, \kappa)) - \eta_S$$

Let $p^*(s|h_{T-1}, \kappa) \in \arg\max_p U^S_{T-1}(s, p|h_{T-1}, \kappa)$, so $V^S_{T-1}(s|h_{T-1}, \kappa) = U^S_{T-1}(s, p^*(s|h_{T-1}, \kappa)|h_{T-1}, \kappa)$. Let $V_{T-1}(s, s'|h_{T-1}, \kappa)$ (with four arguments rather than three) be the payoff to a type-s seller who mimics type $s' < s$. Clearly $V_{T-1}(s, s|h_{T-1}, \kappa) \geq V_{T-1}(s, s'|h_{T-1}, \kappa)$ because $V_{T-1}(s, s|h_{T-1}, \kappa)$ is the maximized counteroffer payoff given the seller's true value, s. It remains to be shown that $V_{T-1}(s, s'|h_{T-1}, \kappa) \geq V_{T-1}(s', s'|h_{T-1}, \kappa)$.

Below, $p^*(s'|h_{T-1}, \kappa)$ is the optimal offer for a type-s' seller given h_{T-1}. Observe that

$$V_{T-1}(s, s'|h_{T-1}, \kappa) = p^*(s'|h_{T-1}, \kappa)\Pr(D^B_T = A|\{h_{T-1}, p^*(s'|h_{T-1}, \kappa)\}, \kappa)$$
$$+ s(1 - \Pr(D^B_T = A|\{h_{T-1}, p^*(s'|h_{T-1}, \kappa), \kappa\})) - \eta_S, \quad \text{and}$$

$$V_{T-1}(s', s'|h_{T-1}, \kappa) = p^*(s'|h_{T-1}, \kappa) \Pr(D_T^B = A|\{h_{T-1}, p^*(s'|h_{T-1}, \kappa)\}, \kappa)$$
$$+ s'(1 - \Pr(D_T^B = A|\{h_{T-1}, p^*(s'|h_{T-1}, \kappa)\}, \kappa)) - \eta_S$$

Thus,

$$V_{T-1}(s, s'|h_{T-1}, \kappa) - V_{T-1}(s', s'|h_{T-1}, \kappa) = (s - s')(1 - \Pr(D_T^B = A|\{h_{T-1}, p^*(s'|h_{T-1}, \kappa)\}, \kappa)) \geq 0$$

Therefore, $V_{T-1}(s, s|h_{T-1}, \kappa) \geq V_{T-1}(s', s'|h_{T-1}, \kappa)$, and the seller's counteroffer payoff is weakly increasing in her type when there is one period remaining.

To complete the proof, let $V^S_{T-(t-1)}(s|h_{T-(t-1)}, \kappa)$ denote the seller's counteroffer payoff with $t-1$ periods remaining, and suppose $V^S_{T-(t-1)}(s|h_{T-(t-1)}, \kappa)$ is weakly increasing in s. For $s' < s$, when there are t periods remaining, $V_{T-t}(s, s|h_{T-t}, \kappa) \geq V_{T-t}(s, s'|h_{T-t}, \kappa)$ by the same argument as above. It remains to be shown that $V_{T-t}(s, s'|h_{T-t}, \kappa) \geq V_{T-t}(s', s'|h_{T-t}, \kappa)$. Note,

$$\begin{aligned}
&V_{T-t}(s, s'|h_{T-t}, \kappa) - V_{T-t}(s', s'|h_{T-t}, \kappa) \\
&= (s - s') \Pr\left(D^B_{T-(t-1)} = Q|\{h_{T-t}, p^*(s'|h_{T-t}, \kappa)\}, \kappa\right) + \Pr\left(D^B_{T-(t-1)} = C|\{h_{T-t}, p^*(s'|h_{T-t}, \kappa)\}, \kappa\right) \\
&\times E_{P^B_{T-(t-1)}}\Big[\max\left\{P^B_{T-(t-1)}, s, V^S_{T-(t-2)}\left(s, s|\{h_{T-t}, p^*(s'|h_{T-t}, \kappa), P^B_{T-(t-1)}\}, \kappa\right)\right\} \\
&- \max\left\{P^B_{T-(t-1)}, s', V^S_{T-(t-2)}\left(s', s'|\{h_{T-t}, p^*(s'|h_{T-t}, \kappa), P^B_{T-(t-1)}\}, \kappa\right)\right\}\Big|\{h_{T-t}, p^*(s'|h_{T-t}, \kappa)\}, D^B_{T-(t-1)} = C, \kappa\Big]
\end{aligned}$$

is non-negative. Therefore, $V_{T-t}(s, s|h_{T-t}, \kappa) \geq V_{T-t}(s', s'|h_{T-t}, \kappa)$, completing the proof. The proof that the buyer counteroffer payoff is increasing in b follows by the same steps. □

Lemma 2. *In any equilibrium with truthtelling in the auction, f_B being positive everywhere on the support of B implies f_{PA} is positive everywhere on the support of B.*

Proof. $f_B > 0$ implies F_B is strictly increasing. Equation (5) shows that, under truthtelling in the auction, F_{PA} is then also strictly increasing, and so $f_{PA} > 0$ on the support of B. □

Proof of Proposition 1: Consider an arbitrary bidder of type $B = b$. A bidder's strategy is the price at which to stop bidding. Suppose the current auction price is $\bar{p}$ and suppose the bidder is one of at least two bidders still remaining. The auction eventually ends at some price $p^A \geq \bar{p}$.

Suppose, for now, that, off the equilibrium path, when $p^A < R$, the bidder only enters bargaining if doing so yields a non-negative payoff. This is always satisfied *on* path, but if all bidders were to drop out of bidding immediately, a bidder would only face the decision to enter bargaining or not *off* path, where optimality is not required.

If $b > \bar{p}$, it is optimal for the bidder to remain in the auction, as dropping out would yield 0 and staying in would yield a non-negative expected payoff because there is some chance the bidder wins at $p^A < b$. To see the expected payoff of remaining is non-negative, consider the case where $p^A \geq R$; in this case, the car sells through the auction and the bidder receives a positive payoff. Consider the case where $p^A < R$; in this case, the bidder has the option to enter bargaining, and only chooses to if it yields a non-negative expected payoff. Recall that the bidder only pays bargaining cost η_B if he chooses not to opt out.

If $b < \bar{p}$, the buyer cannot receive a positive expected payoff from remaining in the auction. To see this, note that if the bidder remains in the auction there is a chance that he will win at $p^A > b$. If this occurs and $p^A \geq R$, the car sells through the auction and the bidder receives $b - p^A < 0$. If, on the other hand, the bidder wins and $p^A < R$, the bidder's payoff conditional on entering bargaining will necessarily be negative because the final bargained price is required to be greater than p^A (as stated in Section 5.2) and hence, in this case, the bidder opts out of bargaining, receiving 0.

Now suppose the equilibrium involves (off path) a bidder choosing to enter bargaining even when doing so yields a negative payoff. Holding all other parts of equilibrium strategies fixed as described in Proposition 1, the bidder would have a weak best response of bidding truthfully and only entering bargaining when doing so yields a non-negative payoff. $\square$

Proof of Proposition 3: Note that for $b' > b$, $\pi^B(\chi(b), b') > 0$. This follows by several points: (i) by Lemma 1, $V_3^B(\cdot)$ is weakly increasing in b for any κ; (ii) the term $(b - p^A)$ appearing in the $E_\kappa[\cdot]$ expression in $\pi^B(p^A, b)$ is strictly increasing in b; (iii) combining facts (i) and (ii) and averaging over κ implies that $\pi^B(p^A, b)$ is strictly increasing in b. Thus, $\chi(b') > \chi(b)$, and hence χ is strictly increasing, and χ^{-1} exists and is also strictly increasing.

To see that $\chi^{-1}(p^A) > p^A$, note that a buyer pays $\eta_B > 0$ if he does not opt out of bargaining, and the best possible outcome a buyer can expect from bargaining would be to only have to pay p^A. Therefore, for any auction price p^A, there exists some buyer with type close to p^A, say $p^A + \varepsilon$, where $\varepsilon < \eta_B$, who would prefer to opt out of bargaining rather than receive a payoff of (at most)

$\varepsilon - \eta_B$, which is negative. Strict monotonicity of $\rho(\cdot)$ is shown in Lemma 3.

When bargaining occurs, it is common knowledge among the two bargaining parties s satisfies $\rho(s) \geq p^A$ and b satisfies $\chi(b) \geq p^A$, implying $s \in [\rho^{-1}(p^A), \overline{s}]$ and $b \in [\chi^{-1}(p^A), \overline{b}]$. $\square$

Lemma 3. *If A1–A7 are satisfied, then in any BNE the seller's optimal secret reserve price, $\rho^*(s)$, is strictly increasing in s and satisfies $\rho^*(s) \geq s$.*

Proof. Suppose for now that there is some positive probability that the buyer does not opt out of bargaining when $R > P^A$. A seller of type $S = s$ chooses $\rho(s)$ to maximize

$$E_{P^A}\left[E_B\left[P^A * 1\left\{P^A \geq \rho(s)\right\} + s * 1\left\{P^A < \rho(s), \pi^B(P^A, B) < 0\right\}\right.\right. \tag{9}$$
$$\left.\left.+ \pi^S\left(P^A, s\right) * 1\left\{P^A < \rho(s), \pi^B(P^A, B) \geq 0\right\}\middle| P^A\right]\right]$$

This term consists of 1) P^A, which the seller receives if it exceeds the reserve price; 2) s, which the seller receives if the auction price is below the reserve price and the buyer opts out of bargaining; and 3) the seller's bargaining payoff, $\pi^S\left(P^A, s\right) = E_\kappa[\max\left\{P^A, s, V_2^S\left(s|P^A, \kappa\right)\right\}]$, which the seller receives when $P^A < R$ and bargaining occurs. (9) can be re-written as

$$\int_\rho^{\overline{b}} p^A f_{P^A}(p^A)dp^A + \int_{\underline{b}}^\rho \left[\int_{p^A}^{\chi^{-1}(p^A)} s f_B(b)db + \int_{\chi^{-1}(p^A)}^{\overline{b}} \pi^S\left(p^A, s\right) f_B(b)db\right] \frac{f_{P^A}(p^A)}{1 - F_B(p^A)}dp^A$$
$$= \int_\rho^{\overline{b}} p^A f_{P^A}(p^A)dp^A + \int_{\underline{b}}^\rho \left[s\left(F_B(\chi^{-1}(p^A)) - F_B(p^A)\right) + \pi^S\left(p^A, s\right)\left(1 - F_B(\chi^{-1}(p^A))\right)\right] \frac{f_{P^A}(p^A)}{1 - F_B(p^A)}dp^A$$

A6 implies $\pi^S(\cdot, s)$ is continuous and thus the payoff is differentiable. Differentiating using Leibniz Rule yields the following, with respect to ρ:

$$\frac{\partial}{\partial\rho} = f_{P^A}(\rho)\left[-\rho + s\frac{F_B(\chi^{-1}(\rho)) - F_B(\rho)}{1 - F_B(\rho)} + \pi^S(\rho, s)\frac{1 - F_B(\chi^{-1}(\rho))}{1 - F_B(\rho)}\right] \tag{10}$$

We next show that $f_{P^A}(\rho) > 0$ for any ρ in the support of R. To see this, first note that $f_{P^A}(v) > 0$ for all $v \in [\underline{b}, \overline{b}]$ by Lemma 2. Second, choosing any $\rho < \underline{b}$ would be dominated by a reserve price of $\underline{b}$ because every buyer has a value of at least $\underline{b}$. Third, a seller would be indifferent between any reserve price above $\overline{b}$ (because no buyer would be willing to pay more than $\overline{b}$). Therefore, any $\rho \notin [\underline{b}, \overline{b}]$ would be weakly dominated, and so we can remove $f_{P^A}(\rho)$ from the above expression without dividing by zero. Also note that $1 - F_B(\rho) > 0$ because f_B is positive by $A7$.

By Lemma 1, $V_2^S\left(s|P^A, \kappa\right)$ is weakly increasing in s for any κ, and thus $\pi^S\left(p^A, s\right)$ is weakly increasing in s, so $\frac{\partial}{\partial\rho}$ is weakly increasing in s. Topkis's Theorem then implies that $\rho^*(s)$ is weakly increasing in s. A stronger, strictly increasing result for $\rho^*(s)$ is then obtained as follows. The proof of Proposition 3 demonstrates (due to costly bargaining) that $\chi^{-1}(p^A) > p^A$, and thus $F_B(\chi^{-1}(\rho)) > F_B(\rho)$. Combining these arguments implies that $\frac{\partial}{\partial\rho}$ is strictly increasing in s, which in turn implies, by the Edlin and Shannon (1998) Theorem, that $\rho^*(s)$ is strictly increasing on the interior of the support of R. Let $\underline{r}$ and $\overline{r}$ be the infimum and supremum of $\rho(s)$ that are optimal for any s. If the support of R is an interval, then $\rho^*(s)$ will be strictly increasing on $(\underline{r}, \overline{r})$. Now suppose the support of R is not an interval, i.e. suppose there exists a point or points on the interior of $[\underline{r}, \overline{r}]$ that are not optimal for any $s \in [\underline{s}, \overline{s}]$. Such points are discontinuities of the function $\rho^*(s)$. By the weakly increasing result from above (due to Topkis's Theorem), any such discontinuities are positive jumps in the function $\rho^*(s)$, and therefore $\rho^*(s)$ is strictly increasing on $(\underline{r}, \overline{r})$ even if the support of R is not an interval. This same argument implies $\rho^*(s)$ is strictly increasing at $\underline{r}$ and $\overline{r}$.

The fact that $\rho^*(s) \geq s$ follows from a simple rationality argument (no seller would offer a reserve price less than s given that a reserve price of s yields a weakly higher payoff), but it can also be seen by noting that the first-order condition above implies that the reserve price is given by a convex combination of s and a quantity weakly greater than s (i.e. $\pi^S(\rho, s)$).

If the buyer opts out of bargaining with probability one when $R > P^A$ then the expression in (10) becomes $\frac{\partial}{\partial\rho} = f_{P^A}(\rho)\left[-\rho + s\right]$, implying $\rho^*(s) = s$, again satisfying the proposition. $\square$

Proof of Proposition 4: Theorem 1 of Storms (2015) (included below as Lemma 4, adapted to this setting) implies that, in any BNE of this game, conditional on κ and on $P^A = p^A$, for each seller type s, there is a cutoff value $g_{\kappa,0}(s, p^A)$ such that trade occurs if and only if the buyer's type b satisfies $b \geq g_{\kappa,0}(s, p^A)$. Given the strict monotonicity of $\rho(\cdot)$ (Proposition 3), such a cutoff function also exists with realizations of S replaced with realizations of the reserve price R. $\square$

Lemma 4. *(Due to Storms 2015) If $A1$–$A7$ are satisfied, then, conditional on any realization of the auction price $P^A = p^A$, in any BNE of the bargaining subgame, for each seller type s there is a cutoff value $g_{\kappa,0}(s, p^A)$ such that s trades with a buyer b if and only if $b \geq g_{\kappa,0}(s, p^A)$.*

Proof. Fix κ throughout this proof and omit dependence on it for the sake of brevity. Fix $P^A = p^A$. We first prove a preliminary property. Fix any arbitrary BNE. Let $\Pr(\mathcal{A} = 1|b, h^t)$ represent the trade probability for a buyer who mimics the strategy of a buyer of type b when the history so far in the game is h_t. Here, $\mathcal{A} \in \{0,1\}$ is a random variable indicating whether or not trade occurs, where, from the buyer's perspective, the seller's value is unknown. Let $y(b, h_t)$ represent the expected transfer from playing such an action. Also, let $h_t(s, b)$ denote the history of the game when players' types are s, b and when they play their equilibrium strategies.

We will discuss properties that must hold at histories that have a positive probability of being played in equilibrium (i.e., histories that at least some buyer and seller pair would play). In such histories, in any BNE, each buyer type must weakly prefer to play his own strategy from any history onward to playing that of another type. Thus, for $b' > b$, we have $b \Pr(\mathcal{A} = 1|b, h_t) - y(b, h_t) \geq b \Pr(\mathcal{A} = 1|b', h_t) - y(b', h_t)$ and $b' \Pr(\mathcal{A} = 1|b', h_t) - y(b', h_t) \geq b' \Pr(\mathcal{A} = 1|b, h_t) - y(b, h_t)$. Combining inequalities demonstrates that, for $b' > b$ (and similarly for $s' < s$),

$$\Pr(\mathcal{A} = 1|b', h_t) \geq \Pr(\mathcal{A} = 1|b, h_t) \tag{11}$$

$$\Pr(\mathcal{A} = 1|s', h_t) \geq \Pr(\mathcal{A} = 1|s, h_t) \tag{12}$$

Lemma 4 can then be shown by contradiction. Such a contradiction would be a triple s, b, and b' with $b' > b$ such that s eventually (at some unspecified time period of the game) trades with b, but does not at any period of the game reach agreement with a type b'. For the sake of clarity, we will give such triples a name, referring to them as *Type A* triples. Let h_t^* be the longest history of play *among all Type A triples* such that the strategy for b is the same as that for b' up to time t when the seller's type is s (that is, $h_t^* = h_t(s, b) = h_t(s, b')$). Throughout the remainder of the proof, let s, b, and b' be a Type A triple at which h_t^* is achieved. The result in (11) implies that b' must trade with weakly greater probability than b from h_t^* onward. This weak inequality, combined with (s, b, b') being a Type A triple, implies that there must be some seller type s' who reaches history h_t^* against both b and b' and who trades with b' but not b. Now consider two cases:

1. Case where $s' > s$. Since s does not trade with b' from the history $h_{t+1}(s, b')$, s cannot trade with any types $\tilde{b} < b'$ from $h_{t+1}(s, b')$, or else $(s, \tilde{b}, b')$ would form a counterexample to h_t^* because it would constitute a Type A triple with buyers having $t+1$ periods of identical strategies. But by (12), s must trade more often than s' conditional on the history $h_{t+1}(s, b')$, and hence there must be some type $b'' > b'$ such that b'' eventually trades with s but not with s' when the history is $h_{t+1}(s, b')$. The triple (s', b', b'') then gives a contradiction because it

constitutes a Type A triple with buyers having $t+1$ stages of their strategies being identical.

2. Case where $s' < s$. Since s trades with b from the history $h_{t+1}(s,b)$, s must trade with all types $\tilde{b} > b$ from $h_{t+1}(s,b)$, or else $(s,b,\tilde{b})$ would form a counterexample to h_t^* because it would constitute a Type A triple with buyers having $t+1$ periods of identical strategies. By (12), s' must trade more often than s conditional on the history $h_{t+1}(s,b)$. It follows that there must be some type $b''' < b$ that trades with s' but not s. The triple (s', b''', b) then gives a contradiction because it constitutes a Type A triple with buyers having $t+1$ stages of their strategies being identical. □

Proof of Proposition 5: Let $\tilde{B} \equiv B+W \sim F_{\tilde{B}}$, with density $f_{\tilde{B}}$, and $\tilde{S} \equiv S+W$. For this proof, let the realization of W be w. That the auction price will be additively separable in w is obvious, given that there is no incentive for bidders to deviate from truthful bidding by Proposition 1. To show that bargaining offers are additively separable, the proof proceeds by induction on the number of periods remaining. Before proving this result, we highlight here that Proposition 5 only states that bargaining offers will be additively separable if they are accepted with positive probability; the equilibrium framework does not rule out equilibria in which a player makes an offer that would not be accepted by any type, and additive separability will not necessarily hold for such offers.

Suppose there is one period remaining in the bargaining game: it is the seller's turn and after her turn the buyer can only accept or quit (we prove the result in the case where the buyer moves last; analogous reasoning proves that the result also holds if the seller moves last). Suppose for simplicity that the equilibrium does not entail the buyer rejecting all offers with probability one in the final period (if not, the seller would not choose to counter in period $T-1$). In the final period, a buyer with type $\tilde{B} = \tilde{b}$ will accept a price, $\tilde{p}$, if and only if $\tilde{p} \leq \tilde{b}$. In period $T-1$, the seller of type $\tilde{S} = \tilde{s}$ chooses $\tilde{p}^*$ to solve the following problem (where $\tilde{h}_{T-1}$ is the history of offers prior to period $T-1$ and κ is the mediator's type):

$$\begin{aligned}\tilde{p}^* &\in \arg\max_{\tilde{p}} \left\{ \tilde{p}\Pr(\tilde{B} \geq \tilde{p} | \tilde{h}_{T-1}, \kappa) + \tilde{s}\Pr(\tilde{B} < \tilde{p} | \tilde{h}_{T-1}, \kappa) - \eta_S \right\} \\ &= w + \arg\max_{p} \left\{ p\Pr(B \geq p | \tilde{h}_{T-1}, \kappa) + s\Pr(B < p | \tilde{h}_{T-1}, \kappa) - \eta_S \right\}\end{aligned}$$

Therefore, the penultimate bargaining offer in the heterogeneous setting will be w above the bargaining offer from the homogeneous good setting, and similarly for the seller's maximized payoff.

Note also that, if $\tilde{p}^B_{T-2} = p^B_{T-2} + w$, then the seller would also then quit, accept, or counter in period $T-1$ with the same probability as in the homogeneous case.

Now suppose $\tilde{p}^S_{T-3} = p^S_{T-3} + w$ and consider the buyer's payoff in period $T-2$ from accepting, quitting, or countering. Let all $\tilde{(\cdot)}$ expressions represent the heterogeneous model expressions.

$$
\begin{aligned}
&\mathrm{A} : \tilde{b} - \tilde{p}^S_{T-3} = b - p^S_{T-3} \\
&\mathrm{Q} : 0 \\
&\mathrm{C} : \tilde{V}^B_{T-2}\left(\tilde{b}|\tilde{h}_{T-2}, \kappa\right) = \max_{\tilde{p}} \left\{ (\tilde{b} - \tilde{p}) \Pr\left(D^S_{T-1} = A|\{\tilde{h}_{T-2}, \tilde{p}\}, \kappa\right) - \eta_B \right. \\
&\quad \left. + \Pr\left(D^S_{T-1} = C|\{\tilde{h}_{T-2}, \tilde{p}\}, \kappa\right) E_{\tilde{P}^S_{T-1}}\left[\max\left\{\tilde{b} - \tilde{P}^S_{T-1}, 0\right\} \middle| \{\tilde{h}_{T-2}, \tilde{p}\}, D^S_{T-1} = C, \kappa\right] \right\} \\
&\quad = V^B_{T-2}\left(b|h_{T-2}, \kappa\right)
\end{aligned}
$$

The last line follows by removing w from each expression. Thus, the buyer's payoffs in period $T-2$ are the same as in the homogeneous case, and hence the buyer's probabilities of accepting, countering, or quitting at period $T-2$ are the same as in the homogeneous case, and the buyer's counteroffer is w higher than in the homogeneous case.

Now suppose $\tilde{p}^B_{T-4} = p^B_{T-4} + w$ and consider the seller's payoff at period $T-3$:

$$
\begin{aligned}
&\mathrm{A} : \tilde{p}^B_{T-4} = w + p^B_{T-4} \\
&\mathrm{Q} : \tilde{s} = w + s \\
&\mathrm{C} : \tilde{V}^S_{T-3}\left(\tilde{s}|\tilde{h}_{T-3}, \kappa\right) = \max_{\tilde{p}} \left\{ \tilde{p} \Pr\left(D^B_{T-2} = A|\{\tilde{h}_{T-3}, \tilde{p}\}, \kappa\right) + \tilde{s} \Pr\left(D^B_{T-2} = Q|\{\tilde{h}_{T-3}, \tilde{p}\}, \kappa\right) \right. \\
&\quad + \Pr\left(D^B_{T-2} = C|\{\tilde{h}_{T-3}, \tilde{p}\}, \kappa\right) \\
&\quad \left. \times E_{\tilde{P}^B_{T-2}}\left[\max\left\{\tilde{P}^B_{T-2}, \tilde{s}, \tilde{V}^S_{T-1}\left(\tilde{s}|\{\tilde{h}_{T-3}, \tilde{p}, \tilde{P}^B_{T-2}\}, \kappa\right)\right\} \middle| \{\tilde{h}_{T-3}, \tilde{p}\}, D^B_{T-2} = C, \kappa\right] - \eta_S \right\} \\
&\quad = w + \max_{p} \left\{ p \Pr\left(D^B_{T-2} = A|\{h_{T-3}, p\}, \kappa\right) + s \Pr\left(D^B_{T-2} = Q|\{h_{T-3}, p, \kappa\}\right) \right. \\
&\quad + \Pr\left(D^B_{T-2} = C|\{h_{T-3}, p\}, \kappa\right) \\
&\quad \left. \times E_{P^B_{T-2}}\left[\max\left\{P^B_{T-2}, s, V^S_{T-1}\left(s|\{h_{T-3}, p, P^B_{T-2}\}, \kappa\right)\right\} \middle| \{h_{T-3}, p\}, D^B_{T-2} = C, \kappa\right] - \eta_S \right\}
\end{aligned}
$$

Thus, the seller's payoffs and counteroffer at period $T-3$ are w higher than in the homogeneous case, and the seller's probabilities of accepting, quitting, or countering are the same as in the homogeneous case.

To complete the proof, assume these same separability properties hold in periods $T-(t-1)$ and $T-(t-2)$. It follows (by the same reasoning as above), that at $T-t$ (the seller's turn), the seller's payoffs and counteroffer are w higher than in the homogeneous case, and the seller's probabilities of accepting, quitting, or countering are the same as in the homogeneous case; at $T-(t+1)$ (the buyer's turn), the buyer's payoffs and probability of accepting, quitting, or countering are the same as in the homogeneous case and the buyer's counteroffer is w higher. We omit the steps as they are analogous to those above. This completes the proof regarding actions in bargaining.

The separability result for χ^{-1} then follows immediately: given that the buyer's bargaining payoff is the same as in the homogeneous good case, the probability of the buyer not walking away from bargaining must also be the same, i.e., $\Pr(\tilde{B} \geq \tilde{\chi}^{-1}(\tilde{p}^A)) = \Pr(B \geq \chi^{-1}(p^A))$. This in turn implies $\tilde{\chi}^{-1}(\tilde{p}^A) = \chi^{-1}(p^A) + w$.

Now consider the seller's secret reserve price in the setting with game-level heterogeneity w. Suppose for now that the equilibrium does not entail the buyer opting out of bargaining with probability 1 when $R > P^A$. From the proof of Lemma 3, the derivative of the seller's payoff with respect to the seller's choice of secret reserve price, $\tilde{r} = \rho(\tilde{s})$, is

$$\frac{\partial}{\partial \tilde{r}} = f_{\tilde{P}^A}(\tilde{r}) \left[-\tilde{r} + \tilde{s} \frac{F_{\tilde{B}}(\tilde{\chi}^{-1}(\tilde{r})) - F_{\tilde{B}}(\tilde{r})}{1 - F_{\tilde{B}}(\tilde{r})} + \tilde{\pi}^S(\tilde{r}, \tilde{s}) \frac{1 - F_{\tilde{B}}(\tilde{\chi}^{-1}(\tilde{r}))}{1 - F_{\tilde{B}}(\tilde{r})} \right] \tag{13}$$

where $\tilde{\pi}^S$ denotes the seller's bargaining payoff similarly as in the proof of Lemma 3 but for the heterogeneous scenario. In particular, $\tilde{\pi}^S(\tilde{r}, \tilde{s}) = E_\kappa \left[\max \left\{ \tilde{r}, \tilde{s}, \tilde{V}_2^S(\tilde{s}|\tilde{r}, \kappa) \right\} \right]$. From the additive separability property of the seller's bargaining payoff shown above, $\tilde{V}_2^S(\tilde{s}|\tilde{r}, \kappa) = V_2^S(s|\tilde{r} - w, \kappa) + w$, and it follows that

$$\tilde{\pi}^S(\tilde{r}, \tilde{s}) = E_\kappa \left[\max \left\{ \tilde{r} - w, s, V_2^S(s|\tilde{r} - w, \kappa) \right\} + w \right] = \pi^S(\tilde{r} - w, s) + w,$$

where we applied the independence of W and κ to pull w out of the expectation. Using (13), we the replace $\tilde{s}$ with $s+w$ and $\tilde{\pi}^S(\tilde{r},\tilde{s})$ with $\pi^S(\tilde{r}-w,s)+w$ and rearrange to obtain

$$\begin{aligned}\frac{\partial}{\partial\tilde{r}} &= f_{P^A}(\tilde{r}-w)\left[-\tilde{r}+(s+w)\frac{F_B(\chi^{-1}(\tilde{r}-w))-F_B(\tilde{r}-w)}{1-F_B(\tilde{r}-w)}+\left(\pi^S(\tilde{r}-w,s)+w\right)\frac{1-F_B(\chi^{-1}(\tilde{r}-w))}{1-F_B(\tilde{r}-w)}\right]\\ &= f_{P^A}(\tilde{r}-w)\left[-(\tilde{r}-w)+s\frac{F_B(\chi^{-1}(\tilde{r}-w))-F_B(\tilde{r}-w)}{1-F_B(\tilde{r}-w)}+\pi^S(\tilde{r}-w,s)\frac{1-F_B(\chi^{-1}(\tilde{r}-w))}{1-F_B(\tilde{r}-w)}\right]\end{aligned}$$

Therefore, the optimal secret reserve price in the heterogeneous setting is w above the optimal reserve in the homogeneous setting. Now suppose the equilibrium does involve the buyer opting out of bargaining with probability 1 when $R > P^A$. In this case, the expression in (13) becomes $\frac{\partial}{\partial\tilde{r}} = -\tilde{r}+\tilde{s}$, and thus the optimal secret reserve price is $s+w$, again satisfying the proposition.

This also implies a generalization of Proposition 4: For any κ, at a general realization $W=w$, trade occurs if and only if $\tilde{b} \geq g_\kappa(\tilde{r},\tilde{p}^A) \Rightarrow b \geq g_\kappa(\tilde{r}-w,\tilde{p}^A-w) = g_\kappa(r,p^A)$. □

B Additional Results Related to Sections 3–4.

We first describe additional data-cleaning steps. In 9.66% of threads, a buyer who is not the high bidder contacts the auction house with an offer to be considered if negotiations with the high bidder fall through or if the seller (or auctioneer) already rejected the auction price. We drop these threads. We also drop observations in which the following variables lie outside their respective 0.005 and 0.995 percentiles: the auction price, reserve price, book price, and final price normalized by book price. We drop observations for which key variables are missing or a bargaining sequence is clearly misrecorded or incomplete. Finally, we drop a small number of bargaining threads (less than 0.02% of threads) that extend longer than one week.

As highlighted in Section 3, when the auction price falls short of the reserve price, the buyer may sometimes walk away before negotiations begin. In observations where this is indicated in the data, a mediator identity is sometimes recorded, suggesting this decision may be influenced by a mediator, but text notes (available only for some observations in the data) often indicate that, in such cases, the mediator is recording a walk-away decision that happened prior to any mediator involvement. We focus our analysis in Section 4 only on stages of the game occurring *after* the buyer has passed the opt-out stage of the game. Section 5 models (and uses data from) these walk-away decisions as a strategic decision of the buyer taking place before mediator involvement.

B.1 Descriptive Statistics by Auction House Location. In Table B.1, we report means from

Table B.1: Descriptive Statistics at Bargaining-Thread Level by Auction House Location

	Auction House 1	Auction House 2	Auction House 3	Auction House 4	Auction House 5	Auction House 6
Agreement reached	0.600	0.560	0.514	0.700	0.645	0.630
Final price ($)	5,193	7,603	5,657	5,054	5,803	5,760
Book price ($)	6,089	9,082	7,184	6,561	6,852	7,135
Auction price ($)	4,890	7,662	5,543	4,994	5,597	5,880
Reserve price ($)	6,574	9,228	7,690	6,682	7,122	7,703
No reserve	0.265	0.126	0.275	0.264	0.154	0.312
# Offers in a thread	1.346	1.577	1.593	1.215	1.435	1.426
Length of a thread (hours)	7.719	5.928	5.357	5.777	4.131	5.073
Fleet/lease car	0.396	0.446	0.440	0.536	0.530	0.475
Car age (years)	7.057	4.938	6.584	6.529	6.623	5.985
Mileage	97,257	75,367	94,090	97,548	97,549	91,050
Engine displacement (liters)	3.359	3.789	3.523	3.618	3.803	3.646
No. Mediators	16	19	11	31	10	27
No. Sellers	975	746	614	1,802	778	1,311
No. Buyers	1,065	725	441	1,894	701	1,432
No. Threads	13,846	6,998	9,057	19,130	6,766	19,293

Notes: This table shows the means from Table 1 separately by auction house, as well as the number of mediators, sellers, buyers, and threads at each auction house.

Table B.2: Descriptive Statistics at Mediator-Thread Level by Auction House Location

	Auction House 1	Auction House 2	Auction House 3	Auction House 4	Auction House 5	Auction House 6
Agreement reached	0.572	0.540	0.518	0.663	0.632	0.625
Final price/book price ($)	0.816	0.884	0.816	0.808	0.868	0.833
Final price/reserve price ($)	0.788	0.839	0.766	0.764	0.793	0.795
Final price/auction price ($)	1.013	1.027	1.032	1.024	1.022	1.017
Fleet/lease car	0.421	0.385	0.324	0.491	0.436	0.475
Female	0.500	0.353	0.222	0.517	0.600	0.462
# Threads mediated	865.375	368.316	823.364	617.097	676.600	714.556
Years of employment	4.218	3.451	2.191	6.684	8.806	1.547
No. Mediators	16	19	11	31	10	27

Notes: This table shows the means from Table 2 separately by auction house.

Table 1 separately by the six auction house locations. These locations differ somewhat in their volume, inventory, and negotiation outcomes. Locations 1, 4, and 6 have the most negotiations and most distinct buyers and sellers. About half of all mediators are at locations 4 or 6. Average agreement rates differ across locations, ranging from 0.514 to 0.70. Location 2 stands out with newer (lower car age and higher mileage) and higher-priced cars. The fraction of cars sold by fleet/lease sellers is lowest at location 1 (0.396) and highest at location 4 (0.536). Table B.2 reports means at the mediator level, as in Table 2, separately by auction house location. The average years of employment for a mediator range from 1.547 in location 6 to 8.806 in location 5. Location 3 has the smallest fraction of female mediators (0.222), and location 5 the largest (0.6).

B.2 Threads Handled by Multiple Mediators. Our main sample limits to threads for which a single mediator handled a given thread. We now examine statistics for threads where one mediator handles the first offer of the bargaining thread but at some point in the thread a different mediator takes over. Table B.3 replicates Table 1 among threads handled by multiple mediators, showing that threads handled by multiple mediators tend to correspond to newer and more expensive cars; tend to take longer, both in terms of the number of offers in the thread and the length of time; and are also less likely to reach agreement, by about 41 percentage points. This difference in agreement rates persists even after controlling for all controls (other than mediator identities) in spec 6 of (3).[63] These threads may represent more challenging (less-likely-to-agree) cases that are passed from one mediator to another.

Table B.3: Threads with Multiple Mediators

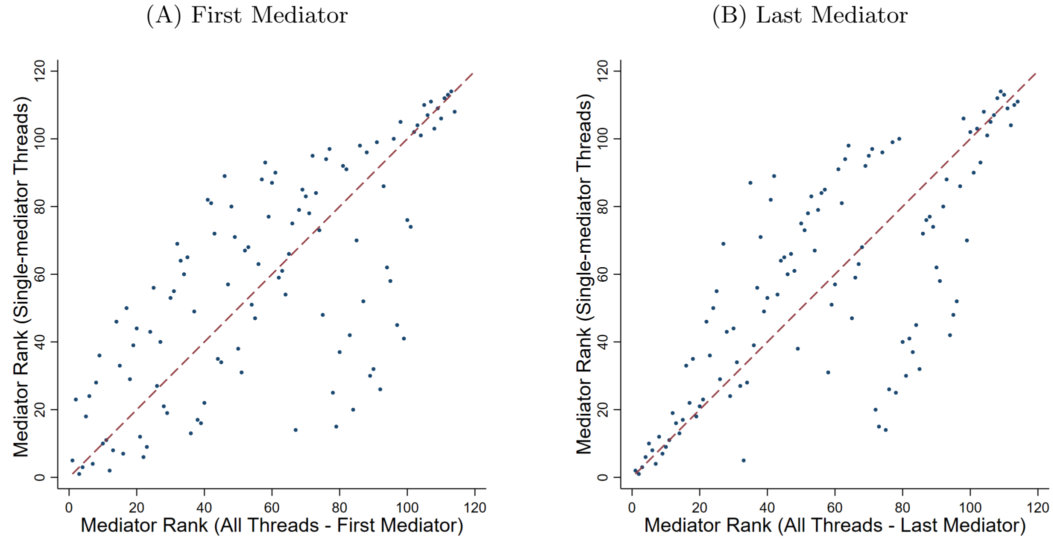
	Mean	Std. Dev.	0.1 Quantile	0.9 Quantile
Agreement reached	0.212	0.408	0	1
Final price	6,958	4,962	1,500	13,750
Bluebook price	7,680	5,098	1,925	14,750
Auction price	5,896	4,664	950	12,500
Reserve Price	7,853	5,117	2,300	15,000
# Offers in a thread	2.401	0.802	2	4
Length of a thread (hours)	12.626	21.529	1.47	28.2
Fleet/lease car	0.575	0.494	0	1
Car age (years)	5.394	3.533	1	11
Mileage	86,313	50,100	26,912	152,006
Engine displacement (liters)	3.648	1.443	2	5.7
No. Threads	37,486			

Notes: Table shows descriptive statistics as in Table 1 but for threads handled by multiple mediators.

As a test of whether the exclusion of multiple-mediator threads is consequential, we replicate spec 6 of (3) using an augmented sample that pools single-mediator and multiple-mediator threads. We do this in two different ways. First, we code the mediator who *starts* a thread as though she handles the whole thread. We then repeat but code the mediator who *ends* thread as though she handles the whole thread. Figure B.1 shows a scatter plot of the main mediator rankings (spec 6 of (3)) against these alternative versions, with the starting-mediator results in the left panel and the ending-mediator results in the right panel. In both cases, we observe a strong positive correlation with our main mediator rankings, suggesting that the main rankings are not substantially driven by our choice to omit multiple-mediator threads. We focus only on single-mediator threads in the body

[63] This is based on a regression (not shown) of the agreement indicator on these controls using all single- and multiple-mediator threads.

Figure B.1: Multiple-Mediator Threads vs. Single-Mediator Threads

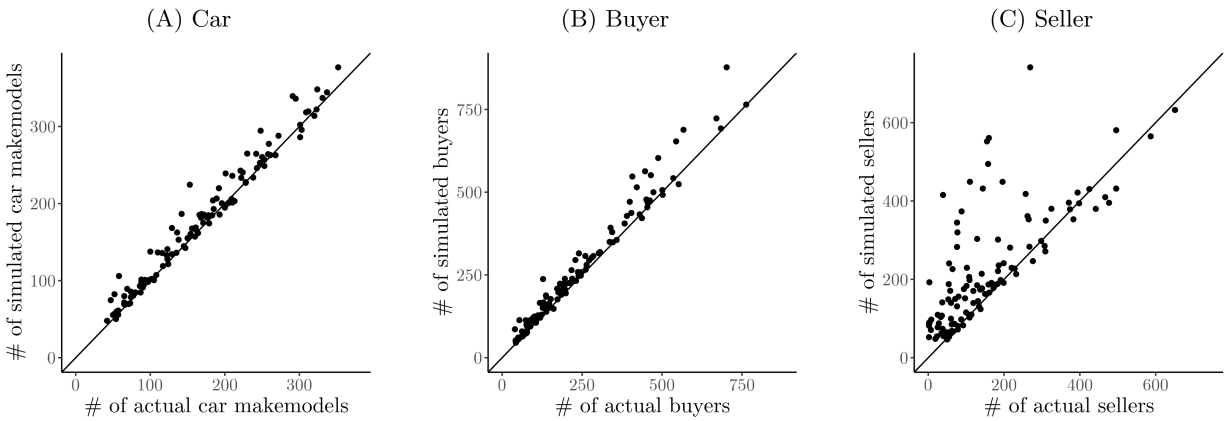


Notes: Each plot shows mediators' ranks based on the estimated mediator fixed effects from spec 6 of (3) using single-mediator threads (on the vertical axis) and compares it to rankings (on the horizontal axis) constructed from an augmented sample that includes threads handled by a single mediator and threads handled by multiple mediators. For estimates in the left (right) panel, we code the mediator who starts (ends) a thread as though she handles the whole thread. We add a 45 degree dashed line as a reference.

of the paper because these are the threads where it is more straightforward to attribute outcome variation to mediator variation.

B.3 An Alternative Assignment Test. Here we randomly shuffle a car-type (make-by-model) identifier across observations within a given auction house location, year, and month. We repeat this 500 times. We perform an analogous procedure shuffling buyers or sellers instead of car types. Figure B.2 plots the number of unique car makes, buyers, and sellers that a mediator interacts with in the simulated data, against the mean number of unique car makes, buyers, and sellers that the mediator interacts with in the real data. Each data point represents one mediator. If mediators are indeed randomly assigned to buyers, for example, mediators should interact with roughly the same number of unique buyers in the real data as in the shuffled data, so all points in panel (B) should lie close to the 45-degree line; conversely, if buyers are assigned mediators non-randomly, a mediator should see *more* unique buyers in the shuffled dataset than in the real dataset.

Figure B.2: Random Assignment Test



Notes: Each data point is a mediator. The x-axis shows the number of car make-models (panel A), buyers (panel B), or sellers (panel C) that a mediator interacts with in the data. The y-axis shows the corresponding numbers of interactions in the randomly reshuffled data.

In panels (A) and (B), most points lie close to the 45-degree line, implying that assignment of mediators to car types and to buyers is approximately random by this measure. Panel (C) shows that mediators interact with fewer sellers in the real data than they do in the shuffled data, suggesting that assignment of mediators to sellers is less likely to be completely random, and highlighting the importance of the seller fixed effects we include in our preferred specification, spec 6 of (3).

B.4 Controlling for Previous Interactions Between Mediators/Agents. Here we examine the extent to which heterogeneity is due to repeated relationships between mediators and agents. For example, it is possible that mediators we identify as highly skilled are only effective when working with particular buyers. To explore this possibility, we re-estimate spec 6 of (3) (from Figure 3) with additional controls for the number of interactions (other than the current thread) between the current mediator and buyer, mediator and seller, or seller and buyer. Table B.4 shows that the coefficients on these interaction terms are quantitatively small and, for most of the terms, not statistically significant at conventional levels. We also find that mediator fixed effects estimated under the specifications in Table B.4 are nearly perfectly correlated with our main estimates.

B.5 Differences in Agreement by Gender. Table B.5 shows differences in agreement by mediator gender. In column 1 we regress thread-level agreement on mediator gender, with no other controls. Columns 2–3 include all controls from spec 6 of (3) except for mediator fixed effects.

Table B.4: Effects of Repeated Interactions

	(1)	(2)	(3)	(4)
Mediator-buyer interactions	-0.000165*			-0.000226**
	(0.000093)			(0.000105)
Mediator-seller interactions		0.000025		0.000026
		(0.000024)		(0.000025)
Buyer-seller interactions			0.000234	0.000549
			(0.000302)	(0.000350)
Mean(interactions)	2.772	4.183	1.403	.
Median(interactions)	1	1	1	.
N	71,093	71,093	71,093	71,093

Notes: Table shows estimates of (3) under spec 6 with additional controls for number of previous interactions between pairs of agents. An observation is a thread. The outcome variable is the agreement dummy. Mean(interactions) refers to the mean number of interactions between a given mediator-buyer pair, mediator-seller pair, or buyer-seller pair (and similarly for the median). The number of observations is fewer than in Table 1 because singleton fixed effect cells are dropped from the analysis. Significance levels: *: $p < 0.10$, **: $p < 0.05$, and ***: $p < 0.01$.

Table B.5: Mediator Performance Variation by Gender

	(1)	(2)	(3)
	Agreement	Agreement	Agreement
Gender(Female=1)	0.112***	-0.003	-0.003
	(0.004)	(0.005)	(0.014)
Specification	No Controls	All Controls	All Controls
R2	0.013	0.386	0.386
N	73,262	69,290	69,290

Notes: Table shows differences in agreement by mediator gender. In column 1 we regress thread-level agreement on mediator gender, with no controls. Columns 2–3 include all controls from spec 6 of (3) except for mediator fixed effects. Columns 1–2 report heteroskedasticity-robust standard errors, and column 3 mediator-level clustering. The number of observations is fewer than in Table 1 both because the gender indicator is not available for some observations and because singleton fixed effect cells are dropped from the analysis.

Columns 1–2 report heteroskedasticity-robust standard errors, and column 3 uses clustering at the mediator level. Column 1 suggests that female mediators outperform male mediators, but after including other controls this effect is small and statistically indistinguishable from zero.

B.6 Quantifying Variation Explained by Mediators and Other Factors. In Table B.6 we display the adjusted R^2 of regression (3) under various scenarios, both with the agreement indicator as the outcome variable and with final price (normalized by book price) as the outcome variable. The controls in our regressions, including the fixed effect indicators, are correlated, and thus the implications for the adjusted R^2 from additional controls varies greatly depending on the *order* in which we add them. In this analysis, we take two different approaches to quantify the contribution of a given set of controls to the adjusted R^2. Panel A reports the adjusted R^2 when we only include our baseline controls (reserve price, auction price, book price, car age, mileage, engine displacement, and date fixed effects), as in spec 1 of (3) and then when we add only one set of fixed effects to those baseline controls. Panel B shows the adjusted R^2 from spec 6 of (3) and with one set of fixed effects *removed* from that full model. Qualitative implications are similar in both panels. For the agreement regressions, we find that buyer, seller, and mediator fixed effects all change the adjusted R^2 noticeably, with seller fixed effects having the largest impact, and other fixed effects have little impact on the adjusted R^2. For example, panel A shows that adding seller fixed effects to the baseline controls increases the adjusted R^2 from 0.0603 to 0.2612, whereas adding mediator fixed effects to the baseline controls increases the adjusted R^2 from 0.0603 to 0.195. For the price regressions, make-model fixed effects and buyer fixed effects have the largest effects, but the shifts are smaller than for the agreement regressions.

Table B.6: Adjusted R^2 Under Various Specifications

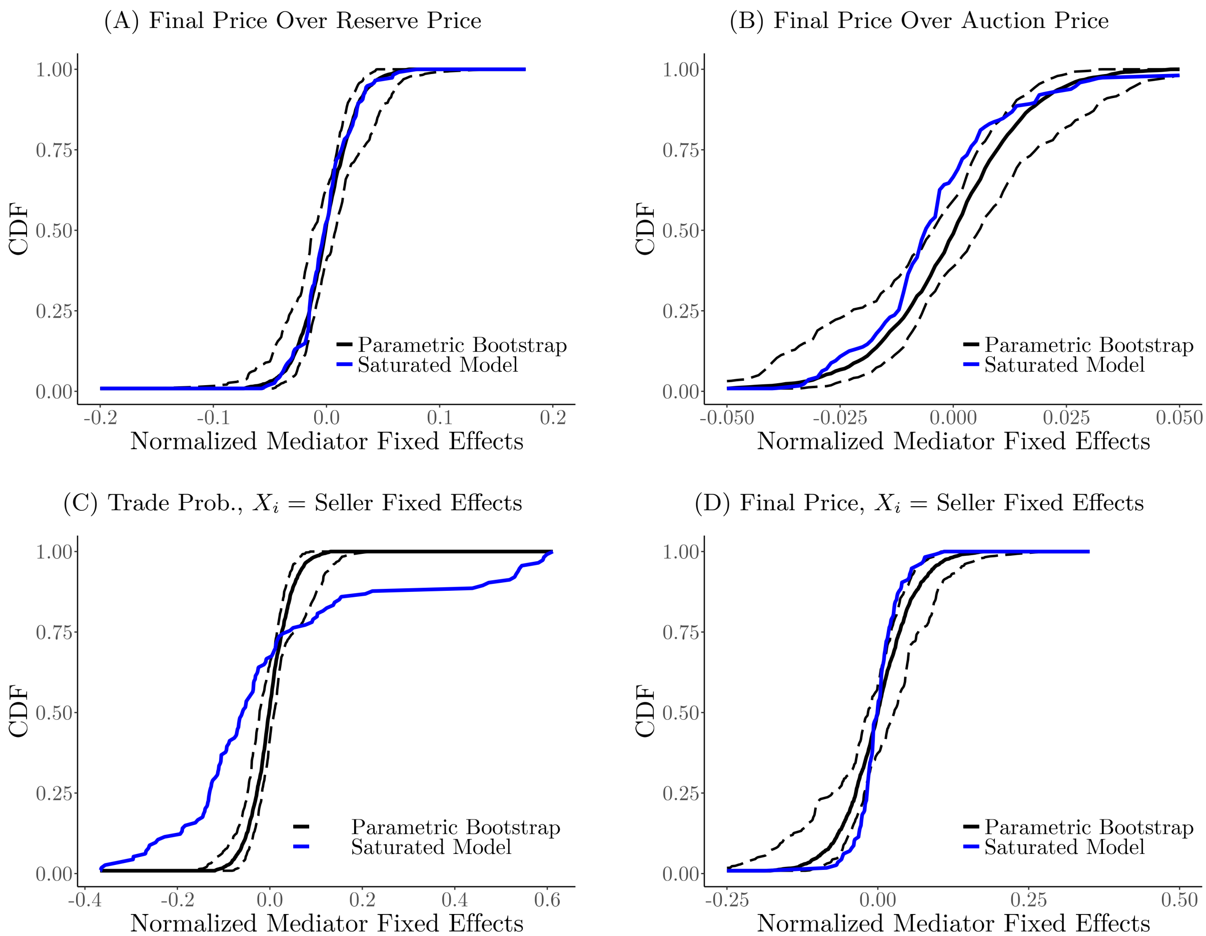
Panel A	Agreement Adjusted R^2	Final Price Adjusted R^2
Baseline	0.0603	0.4077
Baseline + Make-Model FE Only	0.0689	0.4486
Baseline + Auction House Location FE Only	0.0681	0.4081
Baseline + Buyer FE Only	0.1091	0.4481
Baseline + Seller FE Only	0.2612	0.4339
Baseline + Mediator FE Only	0.195	0.4099

Panel B	Agreement Adjusted R^2	Final Price Adjusted R^2
All variables from Spec 6	0.3074	0.4908
All except Make-Model FE	0.3046	0.4632
All except Auction House Location FE	0.3074	0.4908
All except Buyer FE	0.286	0.4674
All except Seller FE	0.2247	0.4798
All except Mediator FE	0.2861	0.4906

Notes: Table shows adjusted R^2 from estimating (3) under various scenarios. Panel A shows adjusted R^2 when we only include our baseline controls (reserve price, auction price, book price, car age, mileage, engine displacement, and date fixed effects) and then when we add various sets of fixed effects to those baseline controls. Panel B shows the adjusted R^2 in the most saturated specification (spec 6) and then again with one set of fixed effects removed.

B.7 Alternative Bootstrap Tests of Variation Due to Sampling Error. Figure B.3 panels A and B show results of the bootstrap procedure for prices as described in Section 4.3 (using X_i from spec 6 of (3)) but normalizing by reserve prices (panel A) or auction prices (panel B) rather than book prices. Figure B.3 panels C and D show results of the bootstrap test from Section 4.3 but using only seller fixed effects in X_i.[64] The results are similar to those in the main text: the distribution of mediator fixed effects for trade probabilities lies well outside the bootstrap confidence intervals, whereas the distribution of fixed effects for prices lies largely within the confidence intervals.

Figure B.3: Alternative Bootstrap Tests of Heterogeneity from Sampling Error



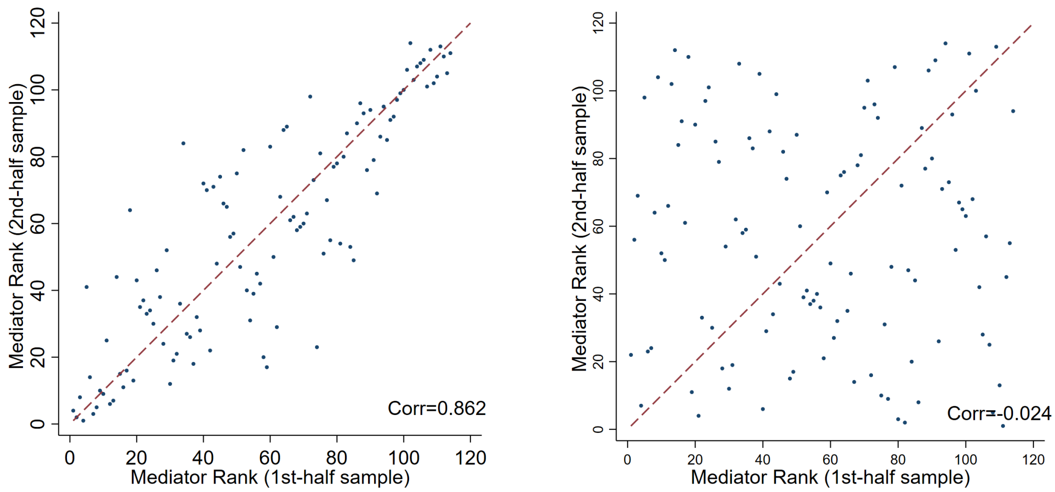
Notes: Panels A and B show results as in Figure 6.B but where the outcome is final price divided by reserve price in panel A and final price divided by auction price in panel B. Panels C and D show results as in Figure 6 but where we only use seller fixed effects (which also absorb auction house location fixed effects) to predict trade probabilities and prices. In each case, we use 100 bootstrap samples. Blue lines show the estimated distribution of fixed effects from the actual data and black show the bootstrap median and 95% pointwise confidence bands.

B.8 Split-sample Test of Fixed Effect Significance. As an alternative test of whether esti-

[64]Recall that seller (and buyer) identifiers in our data are unique for each auction house, and thus seller (or buyer) fixed effects also absorb auction house fixed effects.

Figure B.4: Split-sample Test of Fixed Effect Significance

(A) Trade Probability (B) Final Price



Notes: Each data point is a mediator. The x-axis shows mediators' rank in the first-half sample. The y-axis shows mediators' rank in the second-half sample. Dashed line is 45-degree line. Correlation coefficients are also shown. Trade probability fixed effects are in panel A and price fixed effect in panel B.

mated mediator heterogeneity is due to random chance, we randomly split the main dataset into two halves and estimate mediator fixed effects using the first and second halves separately. Figure B.4 plots the estimated mediators' rank from the first subsample on the x-axis and the rank from the second subsample on the y-axis. Under the null hypothesis that mediators have no effect on trade outcomes, the entirety of the estimated dispersion in mediator fixed effects would be driven by sampling error. As a result, in the split-sample experiment, mediators' ranks in the two samples would be uncorrelated. Panel A shows a strong positive correlation (with a correlation coefficient of 0.862) for trade probability fixed effects. Panel B shows no correlation (-0.024) for price fixed effects. These results are consistent with our findings that mediators primarily differ in their ability to help agents reach agreement, not in the prices at which trade occurs.

C Additional Model and Estimation Details for Welfare Analysis

Before fleshing out estimation details, we first make several remarks regarding the model. While we must choose a specific ordering for the purposes of our model, several aspects of the end of period 1 and the beginning of period 2 can sometimes differ in practice from the way we model them. First, in some cases, the high bidder chooses to walk away soon after the auction, while in other cases he waits several hours to call the auction company to opt out (as long as the seller has not yet accepted or rejected P^A). Our data do not allow us to distinguish between cases, but we can see whether agents agree or not, and at what price, which suffices for estimating model primitives.

Second, there are some sales in which the auction house states that P^A is considered *binding*, meaning the highest bidder is not allowed the initial walk-away option. We do not have clear indicators in the data as to which observations had this binding feature, although we are told this is quite common for fleet/lease sales; this is one motivation for our exploration of results separately by dealers and fleet/lease sellers in Appendix D. Incorporating binding P^A would complicate the model by making bids depend on anticipated bargaining outcomes, potentially incentivizing bidders to shade their bids to avoid bargaining costs. A binding auction price scenario also complicates the proof of strictly increasing secret reserve prices, as buyers no longer have the option to opt out. For simplicity, we treat all auctions as though, when $R > P^A$, the highest bidder is always given the option to walk away from bargaining *before* the seller can accept or reject P^A.

Third, as described in Section 3, there are some cases where the seller does not report a reserve price, usually meaning the seller wishes the auction house to contact her regardless of the outcome, similar to a situation with a binding P^A. Several steps in our identification and estimation strategy require observing reserve prices, so we do drop these observations except in controlling for observable heterogeneity (as described in Section 5.5) and we do not model the seller's extensive-margin decision to report a secret reserve price vs. not. As described in Section 3, agreement is equally likely in observations with or without reserve prices reported.

We now turn to estimation details. Estimation requires several assumptions beyond those described in the model setup. Below, let F_R, F_{P^A}, and F_W represent CDFs of R, P^A, and W. Define the *full game* as the game beginning with period 0 (as described in Section 5.2) and a κ *bargaining subgame* as the subgame that begins after a buyer chooses not to opt out of bargaining and the mediator's type κ is revealed to all parties.

(A8) *F_R, F_{P^A}, and F_W have densities f_R, f_{P^A}, and f_W satisfying the following: (i) the characteristic functions of f_R and f_W have only isolated real zeros; (ii) the real zeros of the characteristic function of f_{P^A} and the real zeros of its derivative are disjoint; and (iii) E[W]=0.*

(A9) *Random variables $(S,\{B_j\}_{j=1}^{N},W,N)$ are identically and independently distributed across instances of the game.*

(A10) All observations in the data arise from the same equilibrium of the full game. All observations in the data with the same assigned mediator type κ are generated by the same equilibrium of a κ bargaining subgame.

$A8$ lists sufficient conditions from Evdokimov and White (2012) for proving identification of f_R, f_{P^A}, and f_W from the joint distribution of $R+W$ and P^A+W. $A9$ is common in the empirical literature on games, abstracting away from dynamics *across* instances of the game. Versions of $A10$, a data generation assumption, are common in structural work. In particular, in a setting with multiple equilibria, to estimate equilibrium objects, the researcher must typically assume the data is generated by a single equilibrium. In our setting, the analogous assumption is that equilibrium objects that do not depend on mediator assignment (such as ρ and χ) are constant (generated by a single equilibrium) across all observations in the data and equilibrium objects that do depend on mediator assignment (g_κ) are constant across observations in which the assigned mediator was κ.

C.1 Observed Heterogeneity. For observation i, the raw reserve and auction prices, denoted R_i^{raw} and $P_i^{A,raw}$, are modeled as

$$\begin{bmatrix} R_i^{raw} \\ P_i^{A,raw} \end{bmatrix} = \begin{bmatrix} Y_i'\gamma \\ Y_i'\gamma \end{bmatrix} + \begin{bmatrix} \tilde{R}_i \\ \tilde{P}_i^A \end{bmatrix}, \tag{14}$$

where $\tilde{R}_i = R_i + W_i$, $\tilde{P}_i^A = P_i^A + W_i$. As in the standard *homogenization* approach (Haile et al. 2003), we estimate γ through a linear regression of reserve and auction prices on observables Y_i, and the residuals provide an estimate of $\tilde{R}_i$ and $\tilde{P}_i^A$. Variation in these two quantities can then be attributed to unobserved game-level heterogeneity and to players' private values.

For this linear regression, we include a rich set of controls, far more than in our specifications in Section 4 because it comes from the broader dataset of all cars running through the auction mechanism (not just observations that reached mediated negotiation), where more controls are available, and because a rigorous set of controls helps to make the independence assumptions of the model as plausible as possible. As in Larsen (2021), Y_i includes the following: fifth-order polynomials in the book price, odometer reading, run number within an auction-house-by-day combination, and run number within an auction-house-by-day-by-lane combination (where run number refers to the order in which cars are auctioned); the number of previous attempts to sell the car; the number of pictures displayed for the car on the auction company website; a dummy for whether or not the odometer reading is considered accurate, and the interaction of this dummy with the odometer reading; the interaction of the odometer reading with car-make dummies; dummies for each make-model-year-trim-age combination (where age refers to the age of the vehicle in years); dummies for condition report grade (ranging from 1-5, observed only for fleet/lease vehicles); dummies for the year-month combination and for auction house location interacted with hour of sale; dummies for 32 different vehicle damage categories recorded by the auction house; dummies for each seller who appears in at least 500 observations; dummies for discrete odometer bins (four equally sized bins for mileage in $[0, 20000)$, eight equally sized bins for mileage in $[20000, 100000)$, four equally sized bins for mileage in $[100000, 200000)$, one bin for mileage in $[200000, 250000)$, and one bin for mileage greater than 250000); several measures of the thickness of the market during a given sale.[65]

Note that, in this homogenization regression, observable heterogeneity, $Y_i'\gamma$, enters both auction and reserve prices identically, a result that follows from Proposition 5. To examine the validity of this property, we perform the homogenization step in two separate regressions, one using only auction prices as the outcome and one using only reserve prices, using the same vector of observables Y_i in each case. This exercise yields predicted values that we denote $Y_i'\hat{\gamma}_{P^A}$ and $Y_i'\hat{\gamma}_R$. The correlation between the $Y_i'\hat{\gamma}$ we use in the body of the paper and either of these two different predicted values is about 0.99. The correlation between residual auction prices using these two different regressions (that is, the correlation between $P_i^A - Y_i'\hat{\gamma}_{P^A}$ and $P_i^A - Y_i'\hat{\gamma}_R$) is 0.7812. The analogous correlation for reserve price residuals is 0.7732. Thus, the outputs from this process (which are then used as inputs for Step 2 of our estimation) are highly correlated regardless of whether we use reserve or auction prices as the regression outcome with which we estimate γ. Our main observable heterogeneity results pool both outcomes together.

C.2 Unobserved Heterogeneity. We apply a result due to Kotlarski (1967), which implies that observations of $\tilde{R} = R + W$ and $\tilde{P}^A = P^A + W$ are sufficient to recover f_W, f_R, and f_{P^A}. We estimate these using a flexible likelihood approach. The likelihood of the joint density of $(\tilde{R}, \tilde{P}^A)$ is

$$\mathcal{L}(f_{P^A}, f_R, f_W) = \prod_i \left[\int f_{P^A}(\tilde{p}_i^A - w) f_R(\tilde{r}_i - w) f_W(w) dw \right] \tag{15}$$

[65]These market thickness measures are computed as follows: for a given car on a given sale date at a given auction house, we compute the number of remaining vehicles still in queue to be sold at the same auction house on the same day lying in the same category as the car under consideration. The six categories we consider are make, make-by-model, make-by-age, make-by-model-by-age, age, or seller identity.

We approximate each of the densities f_{P^A}, f_R, and f_W using fifth-order Hermite polynomials (as in Gallant and Nychka 1987). A similar identification argument was applied in Li and Vuong (1998) and then applied to unobserved auction-level heterogeneity first by Krasnokutskaya (2011). The authors use kernel estimation approaches rather than a likelihood approach. We found the flexible likelihood approach stable and straightforward, as in Freyberger and Larsen (2022).

C.3 Buyer Values. The left-hand side of (5) is F_{P^A}, which is estimated in Step 2. The object $\Pr(N = n)$ is the distribution of the number of bidders in the auction. Larsen (2021) discussed several approximations for $\Pr(N = n)$ and showed that welfare estimates are relatively insensitive to these choices. In particular, Larsen (2021) demonstrated that, in a certain class of specifications for $\Pr(N = n)$ (Poisson), the distribution of valuations for the bidder who enters the bargaining game is analytically invariant to the distribution of N. Moreover, even under non-Poisson distributions, Larsen (2021) showed that the approximation for $\Pr(N = n)$ makes little difference for final welfare estimates: the choice of $\Pr(N = n)$ affects estimates of F_B, but has little to no effect on the distribution of buyer values *conditional* on the auction price, $\frac{F_B(b)}{1-F_B(\underline{b}(p^A))}$, which is what matters for evaluating (8). Following Larsen (2021), we approximate $\Pr(N = n)$ using the distribution of the lower bound on the number of bidders in each auction. This lower bound is observable on an auction-by-auction basis for a subset of auctions for which we have *bid logs*, recording each bid placed during the auction process. We observe these bid logs for a total of 113,497 auctions (primarily fleet/lease sales). With estimates of F_{P^A} and $\Pr(N = n)$, F_B is nonparametrically estimable by solving (5) on a grid of points v. The estimate of F_B is shown in Figure C.1.A.

C.4 Seller Values. To estimate lower and upper bounds on F_S from Proposition 2, but taking into account unobserved heterogeneity, we parameterize each bound as a flexible piecewise linear spline, denoted $F_S^L(\cdot, \theta^{S,L})$ and $F_S^U(\cdot, \theta^{S,U})$. Denote the fixed vector of spline knots $\{v_k^S\}_{k=1}^{K_S}$, which we choose to be $K_S = 200$ uniformly spaced knots between the 0.001 and 0.999 quantiles of $\tilde{P}^A$. We use minimum distance to estimate $\theta^{S,L}$ and $\theta^{S,U}$:

$$\min_{\theta^{S,L},\theta^{S,U}} \sum_{k=1}^{K_S} \left\{ \left[\widehat{\Pr}(D^S = A|\tilde{P}^A = v_k^S) \left(\int \widehat{M}_S(v_k^S, z)dz\right) - \int F_S(v_k^S - w; \theta^{S,L})\widehat{M}_S(v_k^S, w)dw\right]^2 \right.$$

$$\left. + \left[\widehat{\Pr}(D^S \neq Q|\tilde{P}^A = v_k^S) \left(\int \widehat{M}_S(v_k^S, z)dz\right) - \int F_S(v_k^S - w; \theta^{S,U})\widehat{M}_S(v_k^S, w)dw\right]^2 \right\} \quad (16)$$

Here, $M_S(v,w) \equiv f_{P^A}(v-w)f_W(w)$ is the joint density of P^A and W, an estimate of which can be constructed using the estimated densities from Step 2. The objects $\Pr(D^S = A|\tilde{P}^A = v)$ and $\Pr(D^S \neq Q|\tilde{P}^A = v)$ are, respectively, the probabilities that a seller accepts or does not quit, conditional on the realization of $\tilde{P}^A$. We estimate these objects via local linear regressions.

Following Larsen (2021), we impose the following constraints in (16): (i) F_S^L lies graphically above F_R and graphically below F_S^U; (ii) F_S^L and F_S^U lie in $[0,1]$; (iii) F_S^L and F_S^U are weakly increasing; and (iv) $F_S^L(v)$ and $F_S^U(v)$ are equal to 0 for any $v < v_1^S$ and equal to 1 for any $v > v_{K_S}^S$. These last three constraints ensure that F_S^L and F_S^U will correspond to proper distribution functions. Condition (iv) merits additional discussion. By construction, the seller bounds are conditional on a buyer action (the auction price). If the support of auction prices in the data is wide enough that sellers accept or reject extreme auction prices with probability 1, the bounds will be surjective, mapping to each point in [0,1]. Otherwise, while still valid, the bounds may be wide near the tails, failing to reach 0 or 1. We find that this is not an issue for F_S^L, which is surjective, or for the right tail of F_S^U, which attains a value of 1, meaning condition (iv) does not bind for these cases. It does bind, however, for the left tail of F_U^S. This means that, without condition (iv), we would be unable to reject the true F_S containing mass well below v_1^S. We choose to assign all of this mass exactly at v_1^S. While somewhat arbitrary, this choice has the following motivation: Because v_1^S approximates the lowest auction price (and hence, the lowest realization of B), condition (iv) essentially imposes that $\underline{s} \geq \underline{b}$, i.e., the lowest price any seller would accept is bounded below by the lowest buyer value. More importantly, however, Figure 8 suggests that the left tail of F_S is relatively inconsequential for our comparison of efficiency for different mediator skill levels, as differences across mediators are small when dealing with very low-value sellers. Figure C.1.A shows estimates of F_S^L and F_S^U, as well as F_B. F_B (and hence, F_{P^A}) has very little mass below -\$5,000, illustrating the point that, at these low values, an assumption is needed to truncate F_S^U.

C.5 Belief Updating. Taking unobserved heterogeneity into account, (6) becomes

$$\Pr(D_1^B = 0|\tilde{P}^A = \tilde{p}^A, \tilde{P}^A < \tilde{R}) = \int \frac{1 - F_B(\chi^{-1}(\tilde{p}^A - w))}{1 - F_B(\tilde{p}^A - w)} \left(\frac{M_\chi(\tilde{p}^A, w)}{\int M_\chi(\tilde{p}^A, z)dz} \right) dw \qquad (17)$$

where $M_\chi(\tilde{p}^A, w) \equiv f_{P^A}(\tilde{p}^A - w)(1 - F_R(\tilde{p}^A - w))f_W(w)$ is the likelihood of the event $(P^A = \tilde{p}^A - w, \tilde{P}^A < \tilde{R}, W = w)$. We approximate $h_\chi(\cdot) \equiv 1 - F_B(\chi^{-1}(\cdot))$ as a flexible piecewise linear spline. We estimate $\Pr(D_1^B = 0|\tilde{P}^A = \tilde{p}^A, \tilde{P}^A < \tilde{R})$ via local linear regression and estimate

$h_\chi(\cdot)$ by minimizing the distance between the left and right-hand sides of (17). We then back out $\hat{\chi}^{-1}(p^A) = \hat{F}_B^{-1}(1 - \hat{h}_\chi(p^A))$.

C.6 Direct Mechanisms. Taking unobserved heterogeneity into account, (7) becomes

$$\Pr(\mathcal{A}|\tilde{R} = \tilde{r}, \tilde{P}^A = \tilde{p}_A) = \int \frac{1 - F_B\left(g_\kappa(\tilde{r} - w, \tilde{p}^A - w)\right)}{1 - F_B\left(\tilde{p}^A - w\right)} \left(\frac{M_{g_\kappa}(\tilde{r}, \tilde{p}^A, w)}{\int M_{g_\kappa}(\tilde{r}, \tilde{p}^A, z)dz}\right) dw \tag{18}$$

where $M_{g_\kappa}(\tilde{r}, \tilde{p}^A, w) \equiv f_R(\tilde{r} - w) f_{P^A}(\tilde{p}^A - w) f_W(w)$ is the joint density of (R, P^A, W). We approximate $h_{g_\kappa}(r, p^A) \equiv \frac{1 - F_B(g_\kappa(r, p^A))}{1 - F_B(p^A)}$ using a flexible bilinear spline parameterized by 25 knots in each dimension, uniformly spaced between the 0.001 and 0.999 quantiles of $\tilde{R}$ and $\tilde{P}^A$. We estimate the spline parameters by minimizing the distance between the left- and right-hand sides of (18).[66]

This requires first estimating the conditional probability $\Pr(\mathcal{A}|\tilde{R} = \tilde{r}, \tilde{P}^A = \tilde{p}_A)$, which we do using a tensor product of cubic b-spline functions with fifteen uniformly spaced knots in each dimension. With estimates of $\hat{h}_{g_\kappa}(r, p^A)$, we then obtain $\hat{g}_\kappa(r, p^A) = \hat{F}_B^{-1}(1 - (1 - \hat{F}_B(p^A))\hat{h}_{g_\kappa}(r, p^A))$.

C.7 Bargaining Cost Bounds. To incorporate bargaining costs into the surplus estimates, we subtract an *upper* bound on expected losses due to bargaining costs from our surplus estimates. This yields a *lower* bound on surplus measures incorporating bargaining costs. In contrast, the upper bound on our surplus measures does not need to be adjusted, because treating the expected loss due to bargaining costs as though it were zero yields an upper bound on these surplus measures.

We bound these losses following Larsen (2021). First, bounds on η_B and η_S can be obtained from the difference between consecutive bargaining offers. Consider offers made in periods 1 through 3 of the bargaining game. Because an agent who counters must prefer the situation in which (i) she must pay the cost of countering (η_S or η_B) but her counteroffer is accepted with probability one to the situation in which (ii) she accepts the most recent opponent offer, it must be true that $P_2^S - \eta_S \geq P_1^B$ for the seller and $B - P_3^B - \eta_B \geq B - P_2^S$ for the buyer. Note that if (i) is not preferred to (ii), the agent should not counter. Rearranging these inequalities yields $p_2^S - p_1^B \geq \eta_S$ and $p_2^S - p_3^B \geq \eta_B$. We compute the minimum (across all observations) of $p_2^S - p_1^B$ in 200 nonparametric bootstrap samples and use as our (conservative) upper bound on η_S the 0.95 quantile of these minima across bootstrap replications. We follow a similar procedure with $p_2^S - p_3^B$ for an upper bound on η_B.

[66]We impose several constraints in estimation: (i) $h_g(r, p^A) \in [0, 1]$; $h_g(r, p^A)$ being decreasing in r and (ii) $g_\kappa(r, p^A) \geq \underline{g}_\kappa(r, p^A) \equiv \max\{p^A, \rho^{-1}(r)\} \Rightarrow h_{g_\kappa} \leq \frac{1 - F_B(\underline{g}_\kappa(r, p^A))}{1 - F_B(p^A)}$. We enforce (ii) by evaluating $\rho^{-1}(r) = F_S^{-1}(F_R(r))$ at $F_S = \hat{F}_S^L$. Condition (ii) ensures that the estimated g_κ do not allow trade when $S > B$.

With these bounds on η_B and η_S, we compute upper bounds on the total expected loss for a buyer or seller due to bargaining costs as follows. Recall that the bargaining costs are incurred each time an agent makes an offer. Let $\mathcal{T}$ be a random variable representing the period the game ends. The buyer's and seller's expected loss incurred due to bargaining costs are given by $\eta_B E\left[\lfloor \mathcal{T}/2 \rfloor\right]$ and $\eta_S E\left[\lfloor (\mathcal{T}-1)/2 \rfloor\right]$, respectively, because by period t of the game the buyer has made a total of $\lfloor \frac{t}{2} \rfloor$ offers (where $\lfloor \cdot \rfloor$ is the floor function), and similarly for the seller. Because different mediators (i.e., different equilibria of the bargaining subgame) may take a different number of periods in bargaining, we compute this upper bound on losses separately for each κ. Our estimated upper bounds on the total loss to buyers due to bargaining costs are \$1, \$40, and \$20 for low-, medium-, and high-skilled mediators, respectively. The corresponding estimated bounds for sellers are \$4, \$6, and \$2.

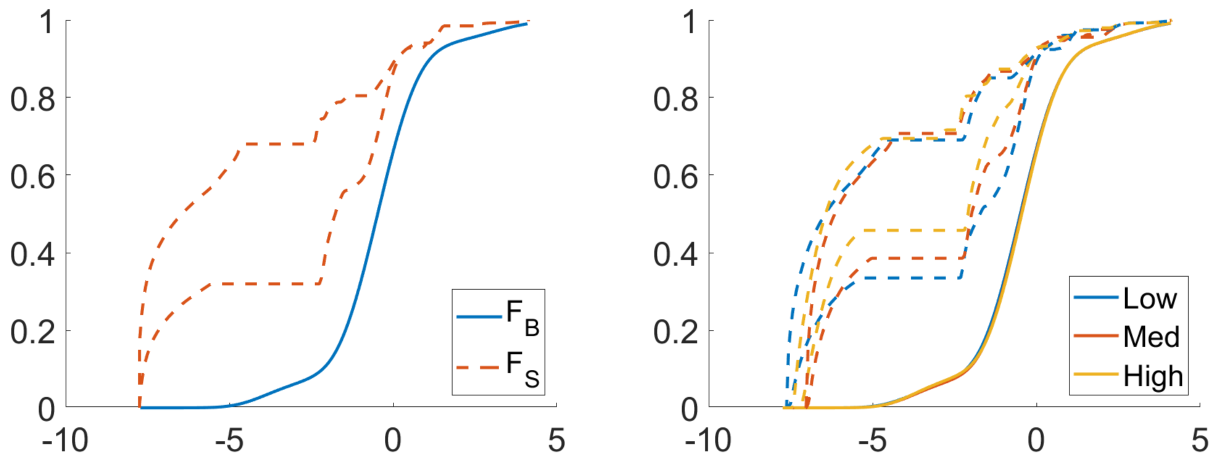
C.8 Auction House Fees. As explained in Appendix E of Larsen (2021), these fees are largely fixed fees (with only a small price-based commission), and we treat them as fully comprised of a fixed fee. Our notion of buyer and seller values, B and S, embed these fees. Specifically, let h^B and h^S represent the fee paid to the auction house by the buyer and seller if trade occurs, and let B^* be the buyer's willingness to pay ignoring this fee, and S^* the seller's willingness to sell ignoring this fee. The random variables of which we identify (or bound) distributions of $B = B^* - h^B$ and $S = S^* + h^S$. Our estimates of total surplus under different mediators (as well as our calculations of first-best and second-best surplus) do not include any surplus accrued to the auction house from fees. We treat fees as fixed, to be paid in any mechanism, including theoretical benchmarks.

C.9 Estimating Distributions Separately by Mediator Type. Figure C.1.A displays the estimated F_B in the solid blue line and the estimated bounds on F_S in the dashed red line. To explore whether mediator types indeed appear to face the same distribution of buyer and seller values (as assumed in $A4$), Figure C.1.B displays these same objects estimated separately using the pseudo samples described in Section 5.5, one for each mediator type. The different estimates of F_B in panel B are indistinguishable from one another, lending support to $A4$. For F_S, we only have bounds, and thus we cannot concretely confirm or deny whether the true F_S is the same across mediator types. However, if we were to find large regions of the CDF bounds that do not overlap between different mediator types, this would be evidence that the F_S is *not* the same across mediator types. We find the opposite: the estimated bounds on F_S differ slightly by mediator type but overlap substantially, allowing for a single F_S (faced by all mediators) contained in the bounds.

Figure C.1: Estimated Value Distributions

(A) Main Estimates

(B) Estimation by Mediator Type



Notes: Panel A displays, in solid blue, the estimated F_B and, in dashed red, the bounds on F_S from the body of the paper. Panel B shows these same objects estimated separately using the different pseudo samples (described in Section 5.5) for different mediator types. In panel B, the solid lines represent estimates of F_B and the dashed lines bounds on F_S estimated separately using the different mediator pseudo samples (represented by different colors). The estimates of F_B in panel B are indistinguishable across mediator types. Units on the vertical axes are $1,000.

The small differences we do see in the F_S bounds across mediator types can be explained by how the bounds are constructed. The bounds use all observations of the game, including those that end by a successful auction (where $P^A \geq R$). As explained in the discussion of Proposition 2, the lower bound corresponds to the probability, conditional on P^A, that the seller takes an action at or before the second period of the game that leads to trade; this action is either to choose a low enough R such that $R \leq P^A$ or, when $R > P^A$, to choose to accept the P^A in person or through mediated bargaining over the phone. Only in mediated bargaining (reached by 26% of observations; see Section 3) can mediators influence trade and, consequently, estimated F_S bounds.

Mediator-specific F_S bounds are less useful for our main evaluation of efficiency, where we evaluate welfare at different choices of F_S within the bounds. Evaluating welfare at *different* bounds for different mediator types, when these bounds overlap, would mistakenly attribute *differences in the bounds* at which we evaluate welfare to differences in actual welfare across mediator types. Instead, we use a fixed set of F_S bounds in evaluating welfare; these bounds average over mediator types, which Section 5.3 shows contain the true CDF of seller values under the model's assumptions.

C.10 Pareto Frontier. Figure C.2 displays the second-best (ex ante efficient) Pareto frontier, which can be referred to as the Myerson-Satterthwaite and Williams frontier, along with the first-best frontier.[67] Dashed lines indicate 95% confidence intervals. We also show, in the colored dots, the estimated surplus/gains from trade in the real world mechanisms corresponding to each mediator type, with lines protruding from those points indicating 95% confidence intervals in each dimension (expected buyer gains on the vertical axis and expected seller gains on the horizontal axis). Panels on the left show the full Pareto frontier, and panels on the right zoom in to show the region surrounding the mediator-specific outcomes. Panels on top are evaluated at the F_S lower bound and panels on the bottom at the F_S upper bound. The results are consistent with higher-ability mediators achieving outcomes that are more efficient (i.e., further to the northeast).

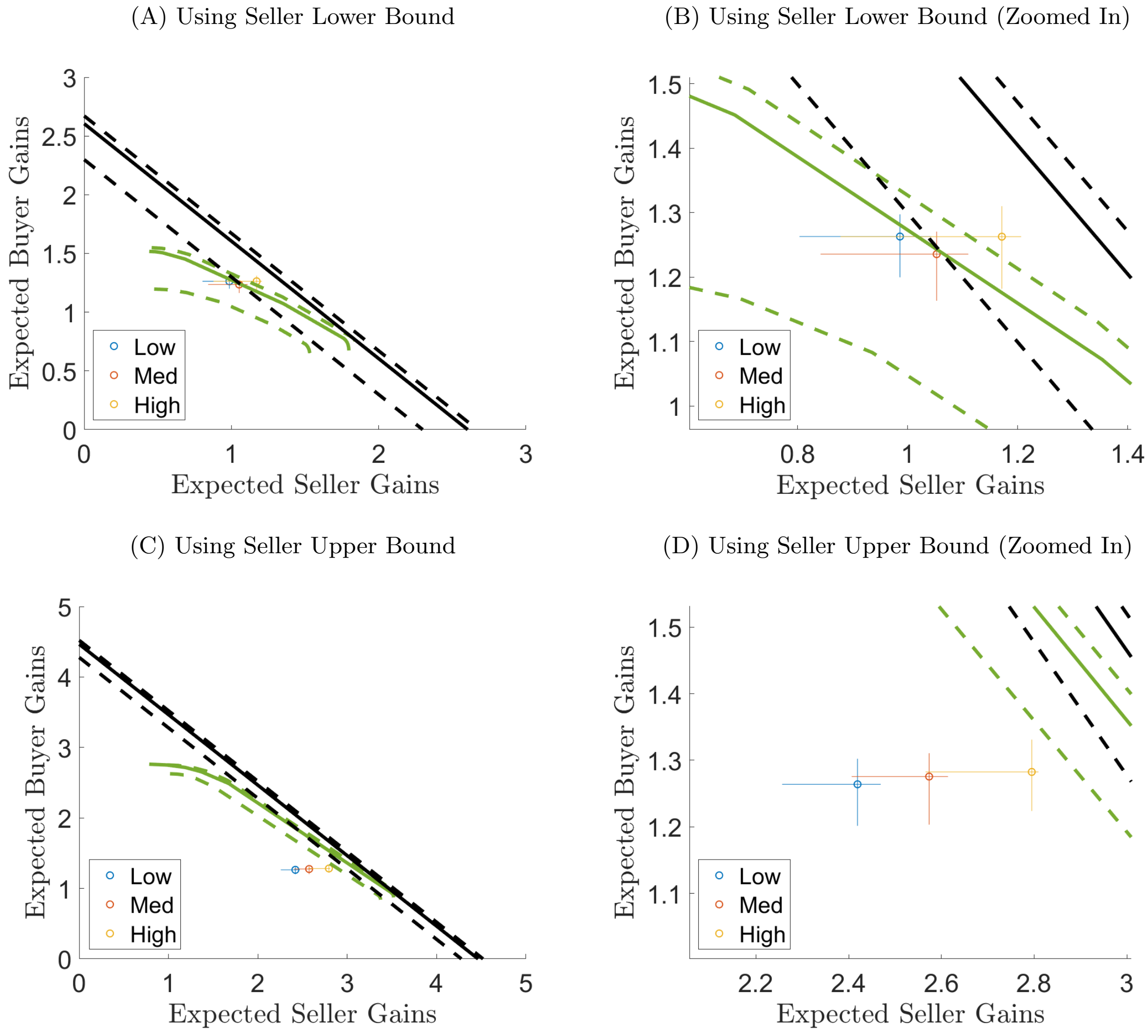
The up-close results using the F_S lower bound (panel B) show some outcomes lying beyond the Pareto frontier, but with confidence intervals overlapping those of the frontier, and thus we cannot reject that the ordering is due to sampling error. However, it is possible to obtain such an ordering within our framework because theory only suggests that the real-world mechanism should lie within the frontier at the *true* distributions F_B and F_S, but we only have bounds on the latter. We evaluate the frontier at the upper and lower bounds while holding fixed the conditional probabilities of trade in that data that are used to estimate the g_κ functions. We estimate F_B and bounds on F_S (and these together determine the second-best frontier) separately from our estimation of the real-world mechanism, without enforcing that the latter lie within the frontier. The point estimates in Figure C.2.B suggest that enforcing such a constraint could potentially tighten the lower bound on F_S. Imposing such a constraint in estimating F_S bounds would not be straightforward, however, as the second-best frontier is itself a solution to an optimization problem (described in Myerson and Satterthwaite 1983 and Williams 1987) that depends on F_B as well as F_S.

The extreme left and right points on the frontier correspond respectively to the buyer-optimal mechanism (which can be implemented by a take-it-or-leave-it offer by the buyer) and seller-optimal mechanism (a take-it-or-leave-it offer by the seller); see Williams (1987). Under the lower bound on F_S, the real-world outcomes lie relatively close to the middle of the frontier, whereas they lie relatively close to the seller-optimal mechanism under the F_S upper bound. This result is at least partially explained by the fact that the theoretical buyer-optimal mechanism could involve the buyer

[67] Myerson and Satterthwaite (1983) characterized the mechanism on the frontier that is closest to the first-best (ex post efficient) outcome line. Williams (1987) characterized the full Pareto frontier. Our numerical computation of these mechanisms follows the construction in these papers.

making an offer that lies well below the auction price, whereas the real-world mechanism enforces that post-auction bargaining offers lie above the auction price, as explained in Section 5.2.

Figure C.2: Pareto Frontier



(A) Using Seller Lower Bound

(B) Using Seller Lower Bound (Zoomed In)

(C) Using Seller Upper Bound

(D) Using Seller Upper Bound (Zoomed In)

Notes: Each panel displays estimated expected seller and buyer surplus on the ex-post efficient frontier (in black), on ex-ante efficient frontier (in green), and in real-world bargaining under different mediator types (low in blue, medium in red, and high in yellow). Top panels use F_S lower bound and bottom panels use upper bound. Panels on right are zoomed-in versions of the left. Units = $1,000.

D Analyzing Fleet/lease and Dealer Sales Separately

This section analyzes dealer vs. fleet/lease sellers separately. This analysis is motivated by several comments we gathered in conversations with industry participants. First, as described in Section

4.1, we are told that fleet/lease sellers are more likely than dealer sellers to be assigned their default mediator in negotiations. Second, in at least one auction house location, mediators handling the two different types of sellers do so in different rooms, with mediators handling fleet/lease sellers being instructed to be less aggressive. Third, as described in Appendix C, auction sales for fleet/lease cars often have a binding auction price, meaning that the highest bidder is not allowed to walk away from negotiations before the seller has the opportunity to decide whether to accept or reject the auction price. Finally, fleet/lease sellers generally have more cars to sell on a given day and hence may have less information about any given car. Any of these reasons could lead to different implications for mediator heterogeneity for the two different types of sellers.

The sample in the body uses mediators who handled at least 50 total negotiations, whether these correspond to dealers or fleet/lease sellers. Thus, some mediators in the main sample may handle few dealer cars or few fleet/lease cars. Here we exploit a sample (the *dealers sample*) involving only dealer sellers and limiting to mediators who handled at least 25 such dealer sales. There may be mediators in our main dataset who handled 50 total sales but only 10 dealer sales, and thus these mediators would not be included in the dealers sample. There may also be mediators who handled 30 dealer sales but less than 50 sales in total, and thus the dealers sample is not a strict subset of the main sample. The same construction and arguments apply to the *fleet/lease sample* we use here. We impose this 25-negotiations criterion to create samples with a sufficient number of observations per mediator. Tables D.1–D.4 and Figures D.1–D.9 are replications of all tables and figures from the body of the paper using these dealers and fleet/lease samples separately.

The results are largely qualitatively similar in the two samples. In both samples, and in the main sample, we document significant heterogeneity in mediator fixed effects that persists after including a rich set of controls and that is not solely due to sampling error. We also find that better mediators (those who achieve a higher trade probability) achieve more efficient outcomes. Some results differ between the two samples. We now discuss similarities and differences in more detail.

Table D.1 shows that dealer sales involve less agreement, lower prices, older (and higher-mileage) cars, and slightly more bargaining offers per thread. Table D.2 shows that mediators of the two different types of cars handle a similar number of threads on average. The average mediator in the fleet/lease sample has slightly more years of employment and is more likely to be female. The ratio of the final price to the auction price or reserve price is similar in the two samples. Figures D.1–D.6 show strikingly similar qualitative and quantitative results in the two samples.

In Table D.3, we find that, in both samples, higher-skilled mediators are more likely than lower-skilled mediators to end the negotiation with agreement in the first period of bargaining (column 2), and that the effects of mediator skill on first-round agreement are much larger than those on later agreement (comparing columns 2 and 3). However, the small effects we do see in column 3 are different in sign between the two samples, suggesting that high-skilled mediators are more likely than low-skilled mediators to achieve later round agreement when interacting with dealer sellers but not with fleet/lease sellers. The effects on later periods in the main analysis (Table 3) average over these two effects, finding a null effect of mediator skill on later period agreements. A key takeaway is that the effect sizes are small in column 3 relative to column 2 in both panels, consistent with the main results. Table D.3, columns 4–6 (as well as Figures D.7.A and D.7.C) confirm that mediator experience correlates positively with agreement. Columns 7–8 of Table D.3, which include mediator fixed effects, show a significant and positive effect for fleet/lease cars of within-mediator experience on the overall probability of agreement or first-round agreement. We find an insignificant effect in these same columns for dealer cars. In both samples, column 9 shows that mediator experience is associated with a lower likelihood of ending in agreement at later bargaining periods.

Figure D.7 panels B and D show a larger gap between the agreement probabilities (as a function of the reserve-auction gap) for different mediator types in the fleet/lease sample than in the dealers sample. In both cases, the agreement probability is decreasing as the reserve-auction gap increases, and in both cases the gap between the lines corresponding to high-skilled vs. medium-skilled mediators appears to widen as the reserve-auction gap increases. The gap between the lines corresponding to high- and low-skilled mediators appears to be closer to constant.

In Figure D.8, where we show the estimated g_κ functions, among dealer cars (panel A), we find a clear ordering between high- and low-skilled mediators, with high-skilled mediators facilitating more efficient trades; this can be seen by the yellow line lying largely below the blue line, meaning more buyer types trade under the high-skilled mediator mechanism. Medium-skilled mediators are not clearly ordered in dealer sales, appearing to allow fewer buyer types to trade than low-skilled mediators. Among fleet/lease sales (panel B), the order is instead quite clear across the low, medium, and high $g_\kappa(\cdot)$ functions, with higher-skilled mediators allowing more buyer types to trade.[68] Finally, the key takeaways from comparing efficiency at different mediator skill levels are

[68]Note, as mentioned in the body of the paper, this figure only shows cross-sections (evaluated at P^A=-\$2,000) of the two-dimensional surfaces corresponding to the g_κ functions. Across the full surface, the $g_M(\cdot)$ function implies more trade than the $g_L(\cdot)$ function even in the dealers sample, just not at the cross-section in Figure D.8.A.

similar in the two samples (Figures D.9.A and D.9.C). One distinction between the two samples is that, in the dealers sample (Figures D.9.B), we find high-skilled mediators increase seller surplus, with no statistically significant effect on buyers, and we find the opposite in the fleet/lease sample (Figures D.9.D). The effect on total surplus of high-skilled vs. low-skilled mediation is positive in both samples. The implications for fees (Table D.4) are also similar in the two samples.

Table D.1: Dealer vs. Fleet/lease Sales: Descriptive Statistics at Bargaining-Thread Level

Panel A: Dealer Sales

	Mean	Std. Dev.	0.1 Quantile	0.9 Quantile
Agreement reached	0.475	0.499	0	1
Final price ($)	5,578	5,209	800	13,300
Book price ($)	6,795	5,318	1,500	14,600
Auction price ($)	5,551	5,114	800	13,100
Reserve Price ($)	7,212	5,574	1,800	15,400
No reserve	0.193	0.394	0	1
# Offers in a thread	1.602	0.803	1	3
Length of a thread (hours)	6.358	16.374	0.298	21.3
Car age (years)	7.036	3.705	2	12
Mileage	97,602	51,253	31,820	162,679
Engine displacement (liters)	3.619	1.490	2	5.7
No. Threads	39,563			

Panel B: Fleet/lease Sales

	Mean	Std. Dev.	0.1 Quantile	0.9 Quantile
Agreement reached	0.789	0.407	0	1
Final price ($)	5,618	4,741	900	12,200
Book price ($)	7,133	5,189	1,650	14,350
Auction price ($)	5,591	4,728	850	12,200
Reserve Price ($)	7,521	5,166	2,000	14,700
No reserve	0.323	0.467	0	1
# Offers in a thread	1.159	0.476	1	2
Length of a thread (hours)	5.066	13.005	.376	6.68
Car age (years)	5.602	3.329	2	10
Mileage	88,707	50,437	28,718	151,612
Engine displacement (liters)	3.575	1.563	2	5.7
No. Threads	35,369			

Notes: Statistics at the thread level in the dealers (panel A) vs. fleet/lease (panel B) samples, as in Table 1.

Table D.2: Dealer vs. Fleet/lease Sales: Descriptive Statistics at Mediator Level

Panel A: Dealer Sales

	Mean	Std. Dev.	0.1 Quantile	0.9 Quantile
Agreement reached	0.484	0.146	0.325	0.639
Final price/book price	0.854	0.0872	0.76	0.951
Final price/reserve price	0.804	0.0584	0.739	0.869
Final price/auction price	1.031	0.0441	1	1.05
Female	0.357	0.482	0	1
# Threads mediated	439	524	30	1,090
Years of employment	3.933	4.469	0.375	10.5
No. Mediators	90			

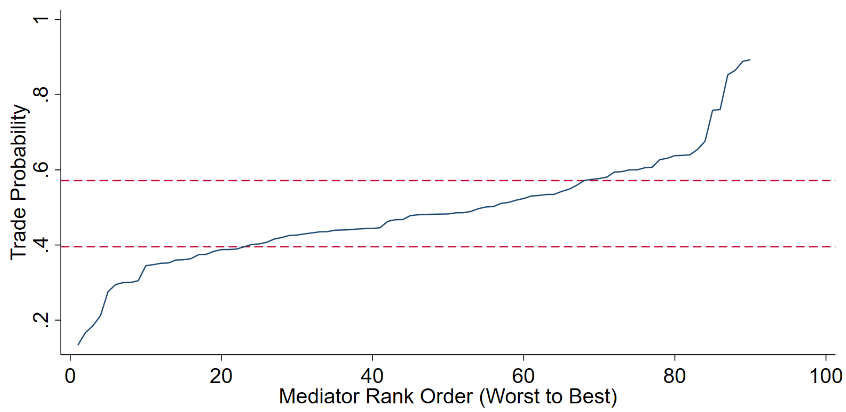
Panel B: Fleet/lease Sales

	Mean	Std. Dev.	0.1 Quantile	0.9 Quantile
Agreement reached	0.705	0.199	0.444	0.929
Final price/book price	0.813	0.0885	0.712	0.916
Final price/reserve price	0.799	0.077	0.711	0.898
Final price/auction price	1.009	0.016	1	1.02
Female	0.525	0.502	0	1
# Threads mediated	431	654	33	1,091
Years of employment	4.753	5.625	0.41	10.6
No. Mediators	82			

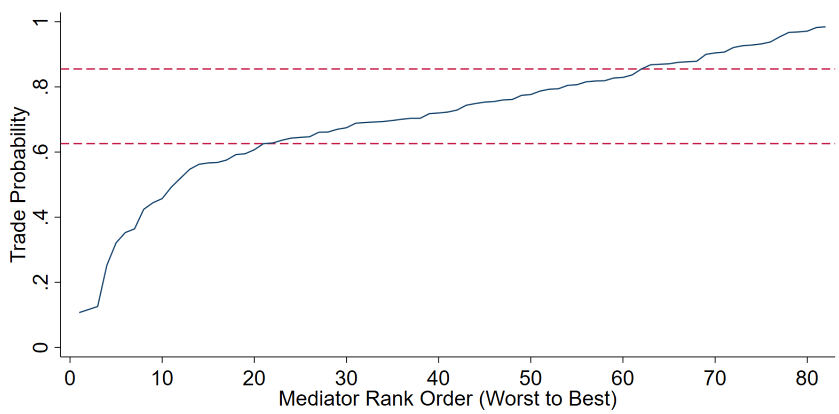
Notes: Statistics at the thread level in the dealers (panel A) vs. fleet/lease (panel B) samples, as in Table 2.

Figure D.1: Dealer vs. Fleet/lease Sales: Mediator Differences in Trade Probability

(A) Dealers

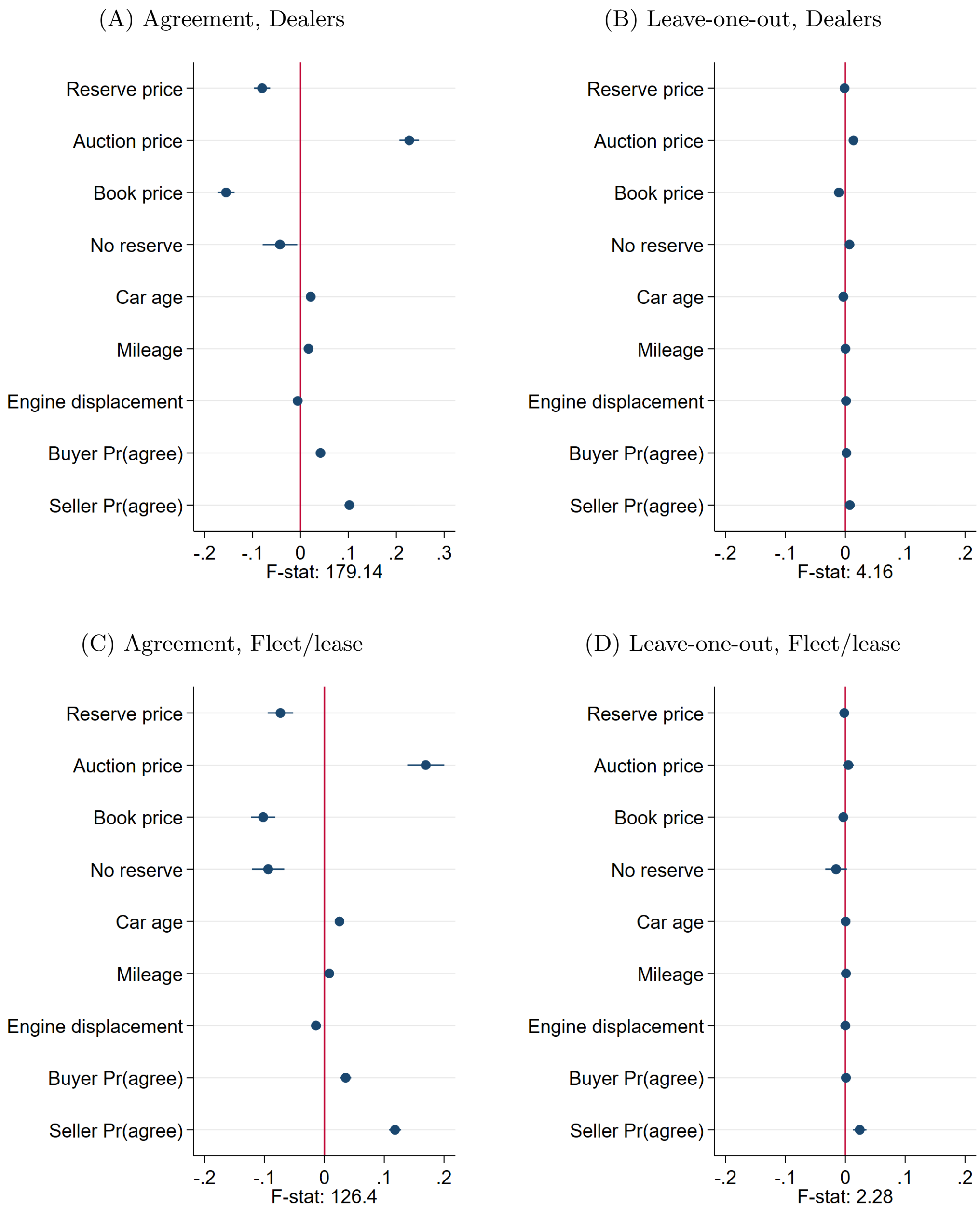


(B) Fleet/lease



Notes: Replication of Figure 1 on the dealers and fleet/lease samples separately.

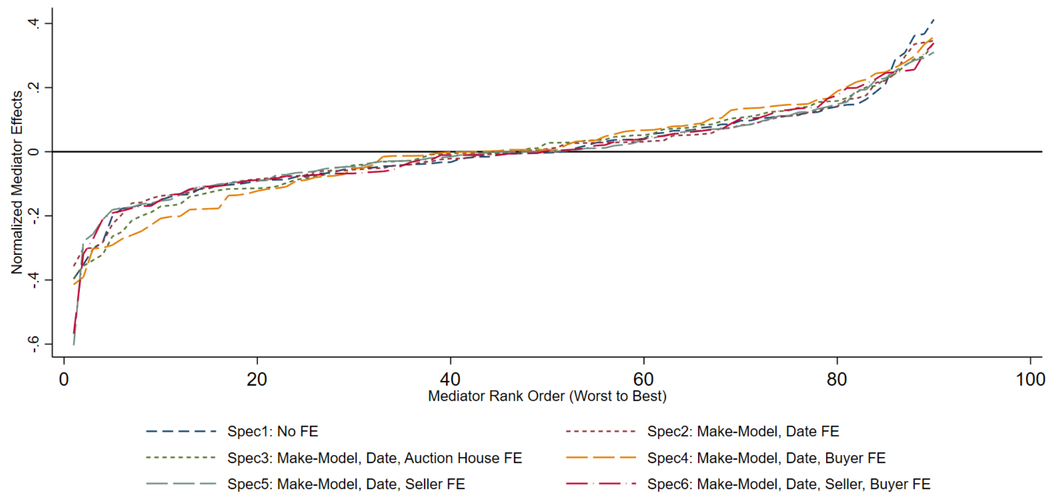
Figure D.2: Dealer vs. Fleet/lease Sales: Assignment Test



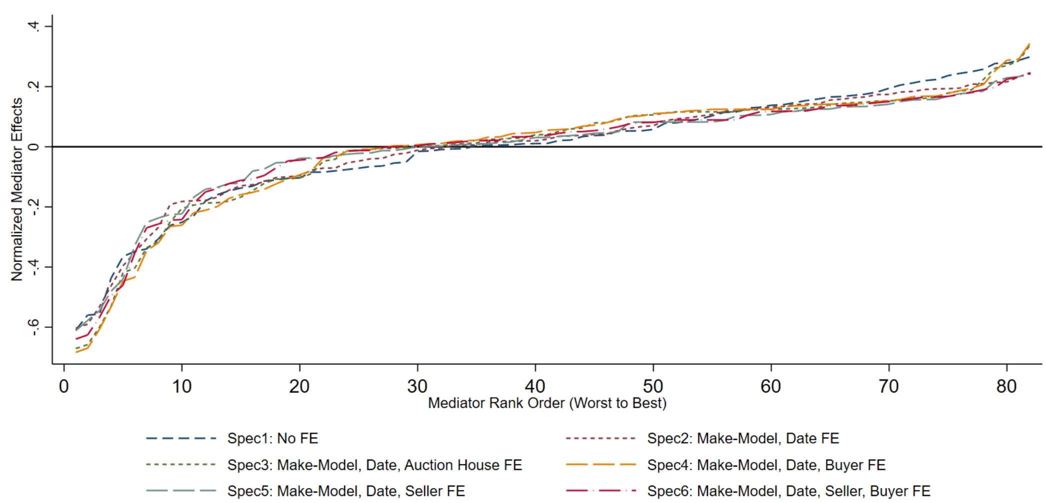
Notes: Coefficients of (2), as in Figure 2 but estimated separately on the dealers sample (panels A and B) and fleet/lease sample (panels C and D).

Figure D.3: Dealer vs. Fleet/lease Sales: Mediator Fixed Effects for Trade Probability Under Different Specifications

(A) Dealers



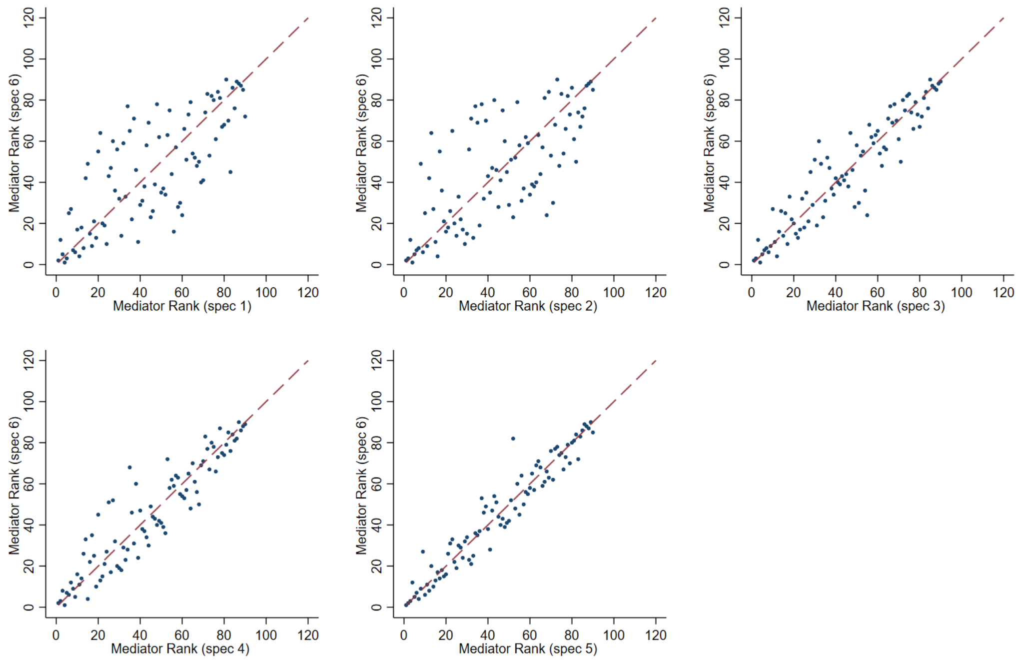
(B) Fleet/lease



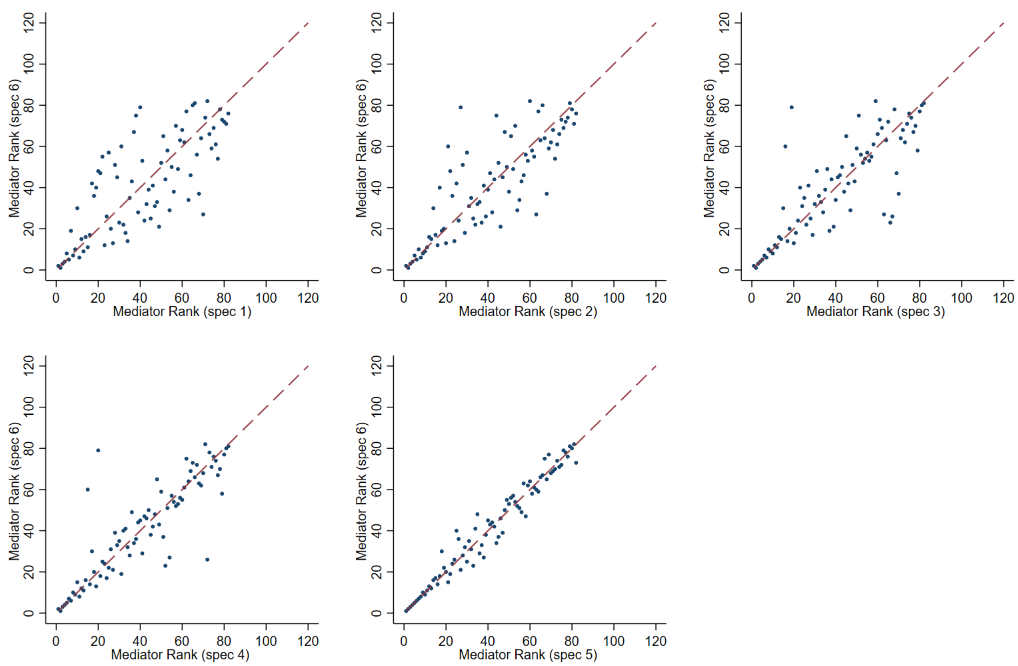
Notes: Replication of Figure 3 separately on the dealers and fleet/lease samples.

Figure D.4: Dealer vs. Fleet/lease Sales: Mediator Rank Across Specifications

(A) Dealers



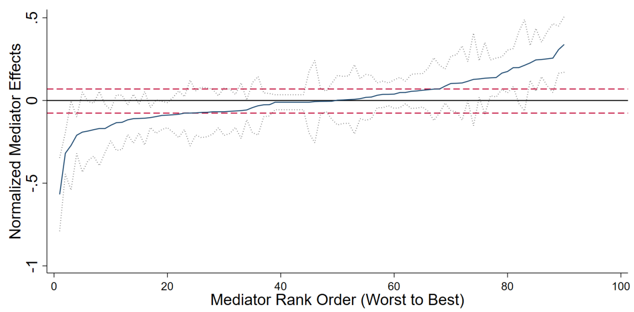
(B) Fleet/lease



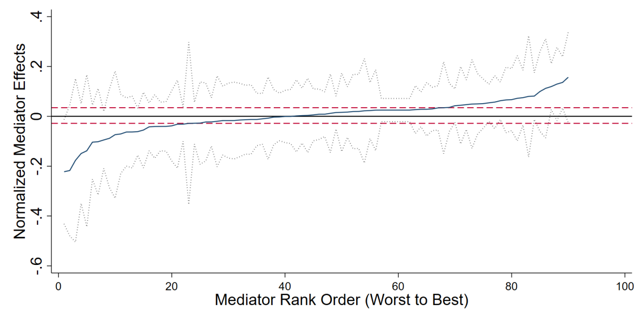
Notes: Replication of Figure 4 separately for the dealers and fleet/lease samples.

Figure D.5: Mediator Fixed Effects Under Most-Saturated Specification

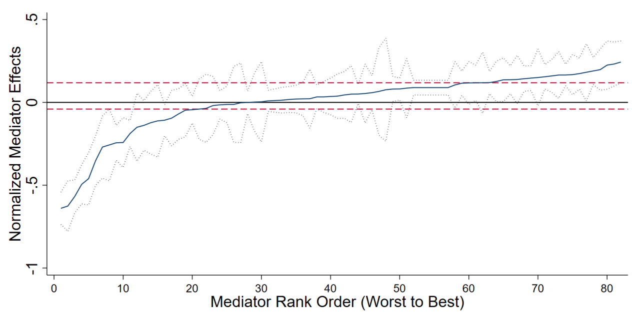
(A) Trade Probability, Dealers



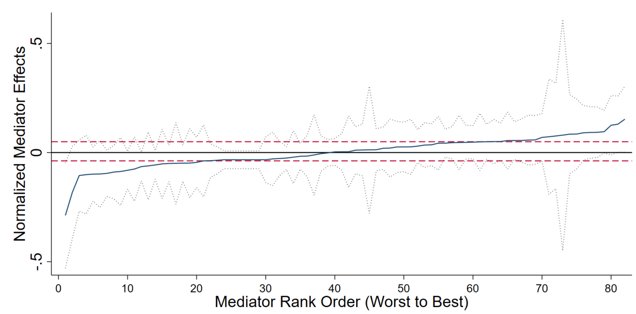
(B) Normalized Final Price, Dealers



(C) Trade Probability, Fleet/lease

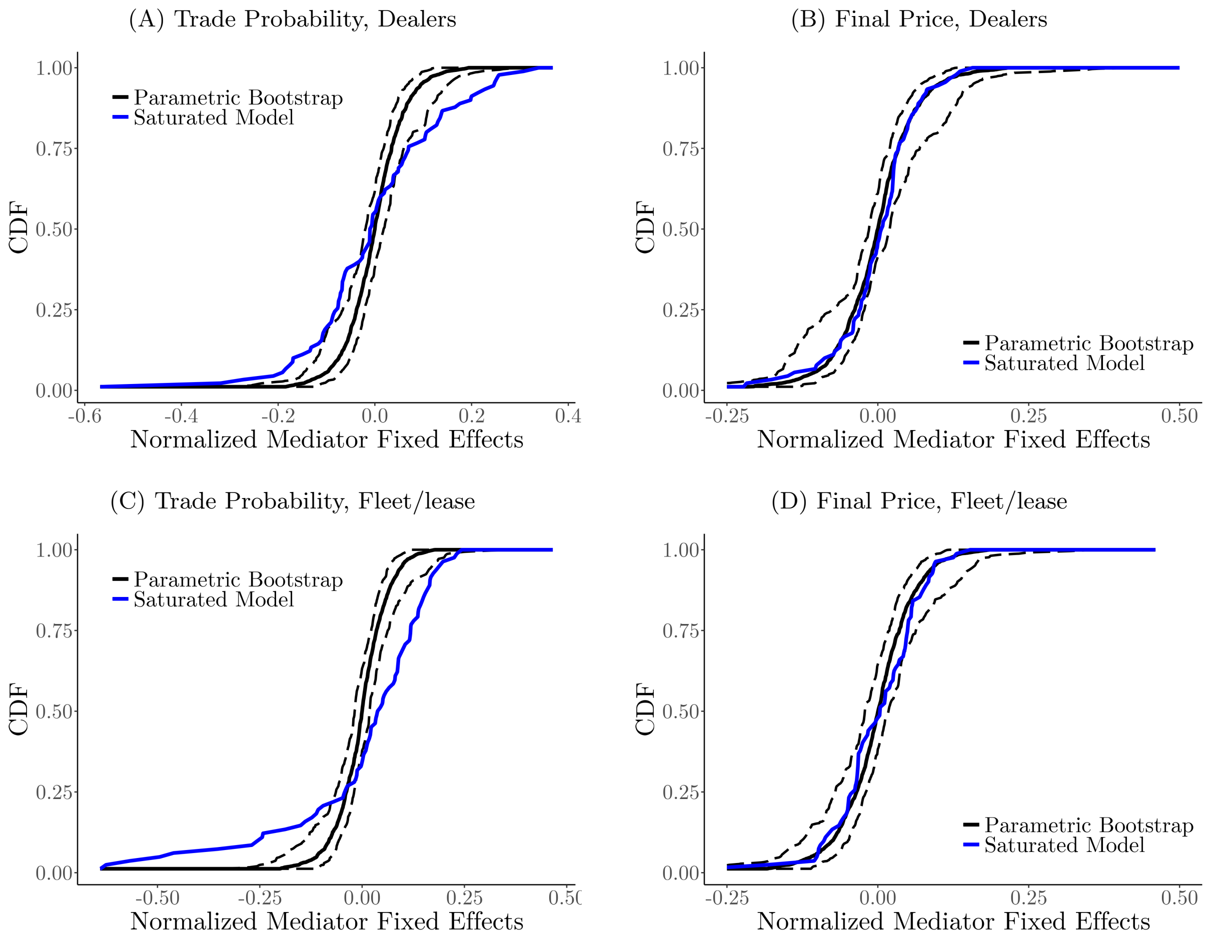


(D) Normalized Final Price, Fleet/lease



Notes: Replication of Figure 5 separately on the dealers and fleet/lease samples.

Figure D.6: Dealer vs. Fleet/lease Sales: Bootstrap Test of Heterogeneity from Sampling Error



Notes: Parametric bootstrap test results, as in Figure 6, but using the dealers sample (panels A and B) and fleet/lease sample (panels C and D).

Table D.3: Agreement Rates in First Later Rounds and Mediator Experience

(a) Dealers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	$Agree_i$	$Agree_{1,i}$	$Agree_{>1,i}$	$Agree_i$	$Agree_{1,i}$	$Agree_{>1,i}$	$Agree_i$	$Agree_{1,i}$	$Agree_{>1,i}$
High	0.210***	0.181***	0.028***						
	(0.012)	(0.011)	(0.009)						
Medium	0.103***	0.085***	0.018***						
	(0.008)	(0.007)	(0.006)						
Experience (years)				0.010***	0.008***	0.002*	-0.006	0.004	-0.011**
				(0.001)	(0.001)	(0.001)	(0.006)	(0.006)	(0.005)
Mediator FE							Y	Y	Y
R2	0.333	0.348	0.255	0.363	0.385	0.293	0.373	0.394	0.298
N	36,481	36,481	36,481	25,392	25,392	25,392	25,392	25,392	25,392

(b) Fleet/lease

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	$Agree_i$	$Agree_{1,i}$	$Agree_{>1,i}$	$Agree_i$	$Agree_{1,i}$	$Agree_{>1,i}$	$Agree_i$	$Agree_{1,i}$	$Agree_{>1,i}$
High	0.307***	0.335***	-0.028***						
	(0.010)	(0.011)	(0.005)						
Medium	0.197***	0.233***	-0.035***						
	(0.010)	(0.011)	(0.005)						
Experience (years)				0.002**	0.001	0.001*	0.023***	0.030***	-0.007***
				(0.001)	(0.001)	(0.001)	(0.004)	(0.005)	(0.002)
Mediator FE							Y	Y	Y
R2	0.350	0.383	0.202	0.361	0.392	0.213	0.397	0.429	0.219
N	33,066	33,066	33,066	24,730	24,730	24,730	24,730	24,730	24,730

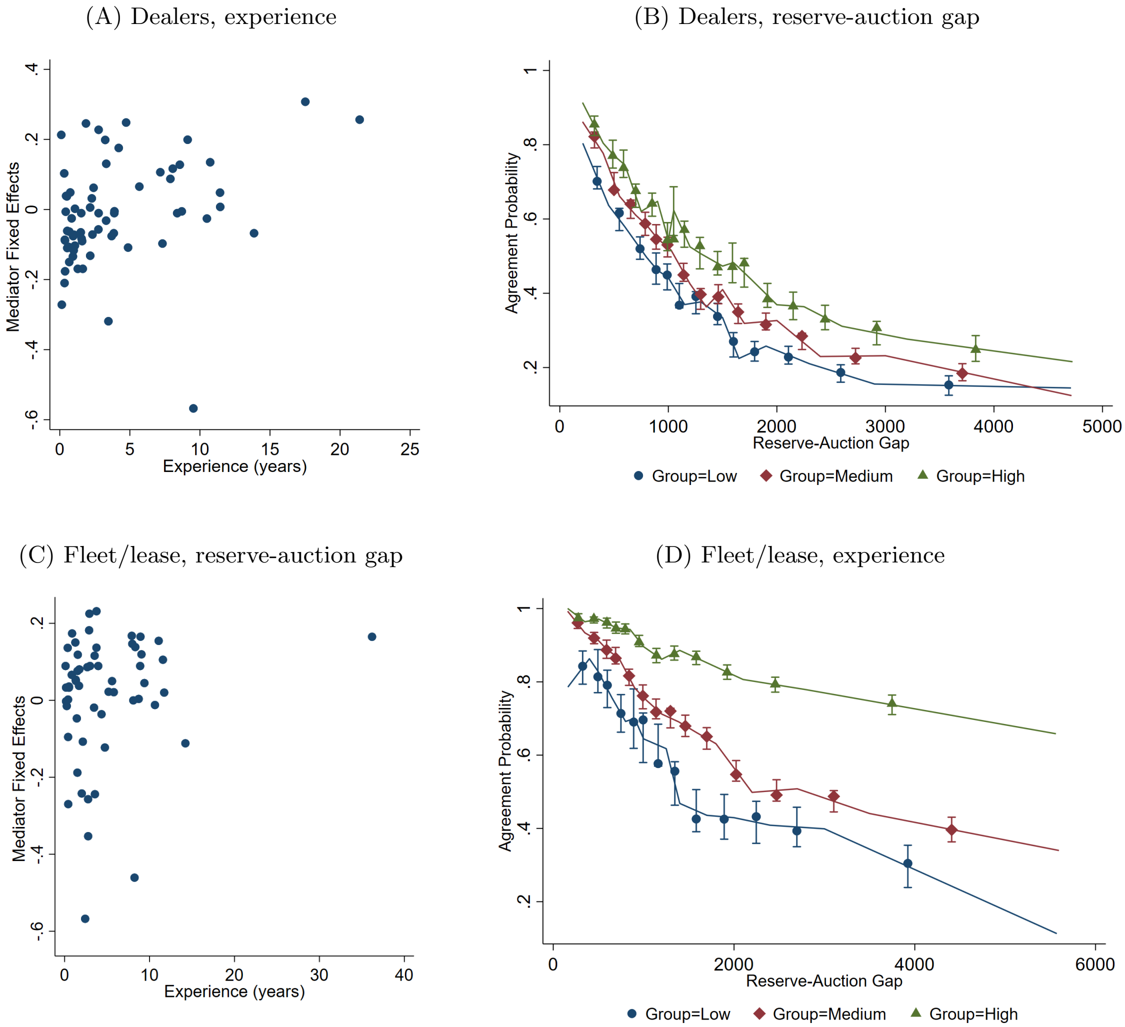
Notes: Estimates as in Table 3, but separately using dealers and fleet/lease samples.

Table D.4: Auction House Revenue Under Different Mediator Types

A. Dealers	Low	Medium	High
Revenue	0.1171	0.1363	0.1486
	(0.002)	(0.002)	(0.002)
	High - Medium	Medium - Low	High - Low
Revenue Difference	0.0124	0.0192	0.0315
	(0.003)	(0.003)	(0.003)
B. Fleet/lease	Low	Medium	High
Revenue	0.1515	0.2103	0.2469
	(0.005)	(0.002)	(0.001)
	High - Medium	Medium - Low	High - Low
Revenue Difference	0.0366	0.0589	0.0954
	(0.002)	(0.005)	(0.005)

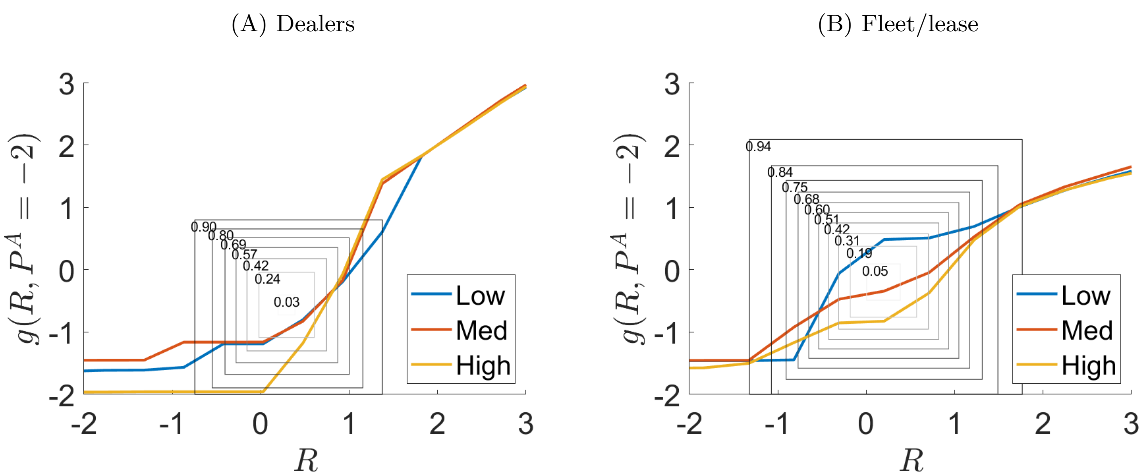
Notes: Results as in Table 4 but estimated separately using the dealers vs. fleet/lease samples.

Figure D.7: Agreement vs. Mediator Experience and Pr(Agreement) vs. Reserve-Auction Gap



Notes: Estimates as in Figure 7, but separately using the dealers vs. fleet/lease samples.

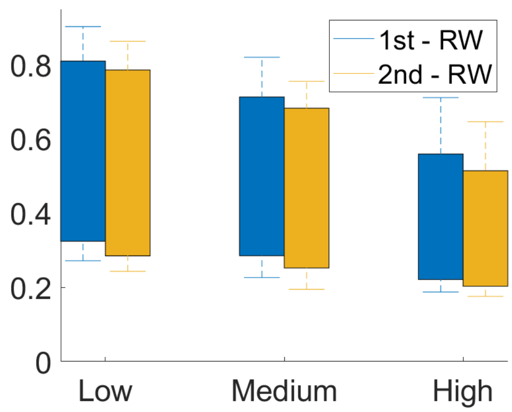
Figure D.8: Dealer vs. Fleet/lease Sales: Direct Mechanisms $g(\cdot)$ Functions for Different Mediator Types



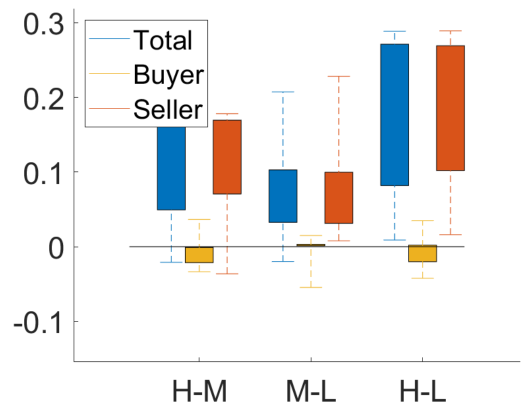
Notes: Estimates of $g(R, P^A)$, as in Figure 8, but separately using the dealers vs. fleet/lease samples.

Figure D.9: Dealer vs. Fleet/lease Sales: Surplus Differences Across Mediators

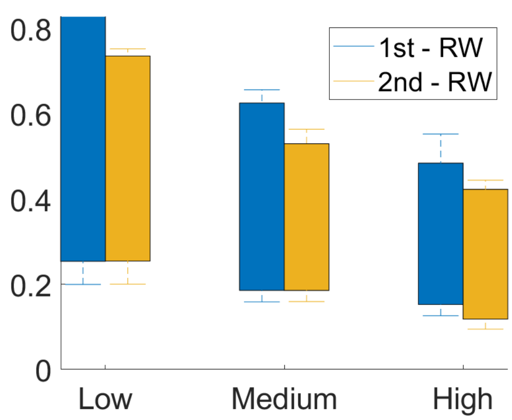
(A) RW vs. 1st/2nd Best, Dealers



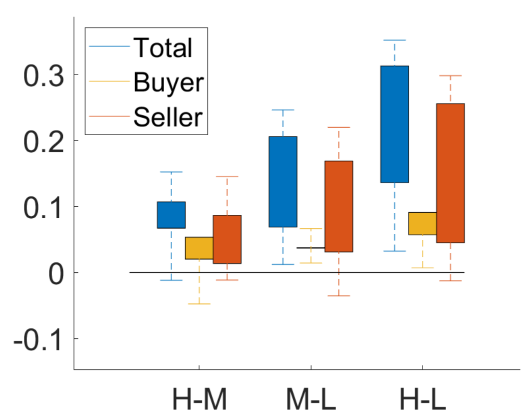
(B) Diff. Across Mediators, Dealers



(C) RW vs. 1st/2nd Best, Fleet



(D) RW vs. 1st/2nd Best, Fleet



Notes: Estimates as in Figure 9 but estimated separately using the dealers vs. fleet/lease samples.