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PRICE DISCRIMINATION BY NEGOTIATION: A FIELD EXPERIMENT IN RETAIL ELECTRICITY*

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We use a field experiment to study price discrimination in a market with price posting and negotiation. Motivated by concerns that low-income consumers do poorly in markets with privately negotiated prices, we built a call center staffed with actors armed with bargaining scripts to reveal negotiated prices and their determinants. Our actors implement sequential bargaining games under incomplete information in the field. By experimentally manipulating how information is revealed, we generate sequences of price offers that allow us to identify price discrimination in negotiations based on retailer perceptions of consumers' search and switching costs. We also document differences in price distributions between entrants and incumbents, reflecting differences in captivity of their respective consumer bases. Finally, we show that higher prices paid by lower-income subsidy recipients in our market is not due to discriminatory targeting; they can be explained by variation in consumer willingness and ability to search and bargain. *JEL Codes:* D83, L13, Q41.

I. Introduction

In many markets, firms post one set of prices publicly and negotiate discounts with customers privately. Prominent examples include automobiles, mortgages, health care, retirement

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savings, private schooling, telecommunications, and energy. In such settings, oligopolistic firms leverage their market power to price discriminate through posted and negotiated prices, segmenting customers based on their willingness and ability to search and negotiate. For public policy, this naturally raises the question of who searches and bargains and whether high- or low-income customers ultimately pay higher prices. These questions become even more policy relevant when the public sector intervenes to subsidize low-income households.

Despite the prevalence and policy relevance of price posting and negotiation, there is relatively little research on such pricing structures. This lack of research is in part because oligopolistic models of price discrimination are generally challenging to solve (Stole 2007), but also because data on negotiated prices are difficult to access.

In this article, we demonstrate the power of field experiments to recover negotiated price data, reveal how firms use price posting and negotiation to price discriminate, identify channels through which discrimination-based price dispersion arises (namely, search and switching costs), and discuss potential distributional consequences of public price posting and private negotiations. Our approach is based on audit studies, which have been used extensively in studying labor market discrimination but have been overlooked for their potential applications to oligopolistic industries until now.[1]

We ran our audit study in a deregulated retail electricity market. We created fictitious customers with randomly allocated combinations of characteristics, including stated and revealed willingness and ability to search, while holding other characteristics related to demand and cost of service fixed. We hired actors to represent these customers and provided them with scripts, effectively creating a call center to negotiate with firms' call centers. These calls yielded multiple price quotes that vary with perceived customer characteristics across and within calls, as our customers gradually revealed individual characteristics, recording sequences of price offers as they revealed new information.

1. Bertrand and Mullainathan (2004) is perhaps the most well-known audit study. They examine racial discrimination in the labor market using fictitious résumés that vary whether job applicants have African American– or white-sounding names. Bertrand and Duflo (2017) overview audit studies of discrimination.

We describe the industry context in Section II and the experimental design in Section III.

Embedding this field experiment with sequential bargaining in an oligopoly yields two key innovations. The first relates to measurement: we uncover post-and-negotiate pricing structures that are otherwise unobservable due to limited availability of negotiated price data. Section IV examines the pricing structures at the market level. Established oligopoly models of price discrimination provide little guidance as to the form such pricing structures should take because they abstract from search frictions and private negotiation (Stole 2007). The market that we study contains both large incumbent retailers, the utilities that existed before retail market deregulation 10 years before, and entrants. We find that incumbent retailers post high prices and are willing to negotiate modest discounts. Entrants, in contrast, post lower prices and are heterogeneous in negotiations, with some aggressively discounting posted rates. Negotiated discounts represent a 27% reduction in retailers' profit margins for a typical customer in the market.[2]

We complement these findings with institutional evidence emphasizing that incumbents and entrants serve different customer segments, with incumbents serving a larger share of "captive" customers who are less willing to search and bargain. In this context, our novel empirical finding of asymmetric post-and-negotiate price distributions among incumbents and entrants supports theoretical results from Armstrong and Vickers (2019). Their model of oligopolistic price discrimination predicts that firms endowed with an exogenous larger share of captive customers who are uninformed about rival prices will, in equilibrium, offer price distributions with higher prices than rival firms with fewer captive customers.[3] The incumbent and entrant price distributions that we uncover align with these predictions.

Section V turns to the second key advance of our IO audit study: it enables us to examine how incomplete information

2. Section II.B discusses how the national antitrust authority published margin estimates just months after our field experiment using proprietary customer data from the retailers on prices, revenues, and costs.

3. Fabra and Reguant (2020) propose an oligopolistic model of search-based price discrimination, while Anderson, Baik, and Larson (2020) develop a novel model of price discrimination involving price posting and personalized discounts. Both papers focus on symmetric equilibria, unlike Armstrong and Vickers (2019).

shapes bargaining at the individual level. Although theoretical research on bargaining with incomplete information dates back at least 40 years, few empirical studies on the topic exist. The difficulty in measuring negotiated prices has limited empirical research. Furthermore, even when price data from negotiations are available, real-world bargaining tends to be unstructured, making it challenging to use bargaining models to interpret data. Further, incomplete-information bargaining models are notoriously complex, with many equilibria that depend on assumptions about how the beliefs of buyers or sellers evolve on and off of the equilibrium path (Ausubel, Cramton, and Deneckere 2002). Such complexity and multiplicity limit these models' ability to generate testable predictions to bring to bargaining data, even in settings where bargaining is structured.

Our field experiment overcomes these challenges to examine how incomplete information over sources of customer captivity—search and switching costs—affects negotiations. In particular, through our experimental design, we impose structure on a real-world high-stakes bargaining process that enables us to develop testable implications regarding information revelation and bargaining outcomes using Sobel and Takahashi's (1983) incomplete-information bargaining model. In their model, a seller makes sequential price offers to a buyer. The buyer is privately informed about the utility of accepting an offer and makes accept/reject decisions. The model's sequential equilibrium predicts that a seller initially offers high prices and gradually reduces them in later bargaining rounds as they update their beliefs about the buyer's private utility, given previous offers and the buyer's decisions to reject them.

We tailor their model to our experiment and research context and assume prospective electricity customers enter negotiations with retailers with two sources of private information: a reference price from the market and a utility value from switching to a given retailer. Incorporating institutional features into our experimental design, we examine how retailers' sequences of price offers vary as a function of customers' (i) awareness of reference prices; and (ii) switching costs that affect their utility value from switching. Our model predicts price discrimination in negotiations whereby customers who are more informed about reference prices or have lower switching costs are offered lower prices in equilibrium. These predictions are borne out in our data, with reference

price awareness and switching costs having economically meaningful effects on negotiated prices.

Our results add to emerging empirical research on bargaining with incomplete information.[4] This research ranges from descriptive analyses of price sequences from back-and-forth bargaining (Backus et al. 2020), to reduced-form studies of final negotiated prices that exploit shocks to buyer informedness of rival offers to estimate causal effects on negotiated price outcomes (Grennan and Swanson 2020), to structural analyses that study how incomplete information affect negotiations and measure associated market inefficiencies (Sieg 2000; Keniston 2011; Silveira 2017; Ambrus, Chaney, and Salitskiy 2018; Allen, Clark, and Houde 2019; Larsen 2021; Larsen and Zhang 2021).[5] Our audit study approach overcomes the data availability problem and, through experimental design, can align the data-generating process for negotiated prices with a theory of bargaining to study how incomplete information shapes bargaining outcomes and underlying mechanisms.

In the latter part of Section V, we further use our experiment to study mechanisms that lead low-income households to pay higher electricity prices and to incomplete pass-through. In our setting, the government subsidizes a fraction of bills faced by low-income customers, and there is public concern that retailers fail to pass through subsidies to low-income customers. Again, market power and search frictions potentially shape these outcomes. It can be profitable for firms to charge subsidy recipients higher prices when they have market power and subsidies are

4. This research departs from the established "Nash in Nash" empirical bargaining framework (Crawford and Yurukoglu 2012) which assumes complete information, and hence that bargaining is efficient.

5. More broadly, this research connects to empirical research documenting observable determinants of real-world bargaining outcomes. See, for example, Ayres and Siegelman (1995), Scott Morton, Zetttelmeyer, and Silva-Risso (2003), List (2004), Gneezy, List, and Price (2012), Castillo et al. (2013), Busse, Israeli, and Zettelmeyer (2017), Chandra, Gulati, and Sallee (2017), Jindal and Newberry (2018), Bagwell, Staiger, and Yurukoglu (2020), and Shelegia and Serman (2022). We more closely relate to four of these papers. Shelegia and Serman (2022) and Keniston (2011) employ auditors and bargaining scripts to generate negotiated price data in the field but do not implement experimental designs to study how information revelation affects bargaining outcomes nor study underlying mechanisms. Gneezy, List, and Price (2012) document statistical discrimination on perceived search differences in a nonsequential bargaining experiment. Busse, Israeli, and Zettelmeyer's (2017) bargaining experiment reveals gender-based differences in the value of price information in negotiations.

targeted (Akerlof 1978), leading to a fraction of the low-income subsidy going to the supplier and not benefiting the intended customer. However, subsidy recipients could be more costly to serve or less likely to search for many reasons (including the subsidy itself reducing the expected gains from search (Gulati, McAusland, and Sallee 2017)), leading them to pay higher prices.

To overcome this identification problem, in the negotiations we randomize revealed subsidy status, separately from randomized revealed intensity of search. Despite public concern over this issue, we find no evidence that retailers engage in targeted discrimination against subsidy recipients. This result, together with results from Byrne and Martin (2021) showing that low-income customers in the market are far less likely to search and negotiate, point to differences in the rate of search and bargaining over the income distribution as the underlying driver of regressive price dispersion observed in the market. Our findings add to burgeoning research documenting incomplete pass-through of targeted government subsidies across many contexts: tuition (Turner 2017), hybrid electric vehicles (Gulati, McAusland, and Sallee 2017), childcare (Rodgers 2018), private Medicare Advantage plans (Cabral, Geruso, and Mahoney 2018), housing (Collinson and Ganong 2018), and ethanol-based fuel (Lade and Bushnell 2019). We introduce audit studies to test targeted discrimination as a potential mechanism for incomplete subsidy pass-through.

Section VI summarizes and concludes the article. We discuss how the search cost income gradient creates a potentially undesirable regressive form of price dispersion in markets with price posting and negotiation. We explore implications of search and switching frictions for energy market design, contributing to ongoing policy debates worldwide over the costs and benefits of deregulating retail electricity markets.

II. Industry

Our research context is the electricity market of Victoria, Australia, a state with 6.3 million people, 4.4 million of which live in the city of Melbourne. The power market has four parts: generation, transmission, distribution, and retail. Generators bid supply into an exchange that determines dispatch and the marginal wholesale cost of generating electricity through uniform price auctions. Transmission and distribution companies are regulated monopolists who own the electricity grid's wires and poles and

manage geographically distinct networks. Retailers compete downstream, paying network fees upstream to distributors, supplying electricity to residential and commercial end-users.[6]

We focus on the market for retail customers, which in 2017 has 13 retailers. The retailers include the "Big 3"—AGL, Origin, and Energy Australia—vertically integrated retailers that compete in electricity generation and retailing, and who served customers as price-regulated monopolists in their respective geographies before retail competition was introduced in 2009. Each of these incumbents serve more than two million customers; collectively, they serve 62% of customers in 2017.[7] The remaining 10 retailers are postderegulation entrants with a 38% collective share. They are heterogeneous in size, serving between 20,000 and 500,000 customers each.

II.A. Customer Inertia

Retailers engage in TV, online, print, and telemarketing advertising to encourage customers to switch retailers. Each year 26% of customers make such a switch. There is, however, considerable inertia with electricity contracts, especially among the incumbents. For instance, 35% of customers of the Big 3 retailers have been with their electricity supplier for more than two years. In contrast, just 20% of entrants' customers have been with their retailer that long. There is thus segmentation between incumbents and entrants based on customers' willingness to compare and switch retailers.[8] One of the entrants emphasizes this segmentation, and the incumbency advantage for the Big 3 that comes with it, in their submission to the ACCC's investigation into the market in 2018:[9]

6. See Australian Energy Market Commission (AEMC) (2017) for a detailed description of Victoria's electricity market design.

7. All figures discussed in Section II draw from four major government inquiries into the retail market: Australian Competition and Consumer Commission (ACCC) (2017), Australian Energy Regulator (2017), AEMC (2017), and a state-level retail electricity market review by the Victorian Government (Thwaites, Mulder, and Faulker 2017).

8. Hortaçsu, Madanizadeh, and Puller (2017) similarly find customer inertia and incumbency advantage among electricity suppliers who were in the retail market prederegulation in Texas using switching and posted price data.

9. Momentum Energy, *Submission to ACCC Issues Paper* (6) for ACCC (2018), June 30, 2017.

> Despite the strong rate of customer switching in the market, retailers with an incumbent base have the luxury of "sticky" customers who are not price-sensitive and can be relied upon to provide a dependable revenue stream. Customers who have switched to a second-tier retailer have demonstrated a willingness to seek out a better electricity deal and are likely to do so again if their new retailer does not meet their needs.

Search and switching costs underlie the customer inertia. Anecdotal evidence from government and customer advocacy reports (e.g., Thwaites, Mulder, and Faulker 2017; Johnston 2016) suggest that the combination of electricity contract complexity and a barrage of marketing campaigns leave customers feeling confused. These challenges for customers create large search costs that limits their levels of engagement in researching and obtaining lower prices.[10] To help customers overcome search costs, the state and federal governments provide online price comparator websites for comparing electricity pricing contracts.[11]

Customers also face switching costs associated with exit fees for terminating a current contract, disconnection and new connection fees when transitioning from an old to a new retailer, and transaction costs from coordinating between old and new retailers during the switching process (AEMC 2017). Nine of the 13 retailers have early termination exit fees of either $20 or $22, with one exception at $80 (Johnston 2016).

II.B. Retail Pricing

As in many electricity markets, retail contracts in Victoria consist of a two-part tariff: a fixed daily charge and a variable per kilowatt-hour (kWh) charge. Some retailers offer variable rates that increase with the quantity consumed or vary by time of day. Our experimental design, discussed below, focuses on the most common prices in the market: contracts with constant per kWh charges.

Electricity contracts reflect either posted prices or negotiated prices. Firms promote posted prices through online, television, and

10. Previous studies have documented customer search costs and inattention in retail electricity markets including Hortaçsu, Madanizadeh, and Puller (2017) and Guilietti, Wildenbeest, and Waterson (2014).

11. Energy Made Easy is the national website (https://www.energymadeeasy.gov.au/). Victorian Energy Compare is the state-run website (https://compare.energy.vic.gov.au/).

print advertising.[12] Posted prices are most commonly valid for one year and can change frequently. At any given time retailers can offer a range of prices with discounts for automatic payment, on-time payments, loyalty affiliations, one-time sign-up discounts or other promotions, and premiums for green power commitments. Customers can sign up for posted prices through retailer websites or by calling sales centers directly.

There are also negotiated prices: customers can call their current electricity supplier or rival retailers to bargain directly. Before our study, there was anecdotal evidence of large potential gains from negotiating rates in the market (Johnston 2016). But previously reported aggregate industry statistics focused exclusively on default and posted prices, and limited customer survey data did not distinguish between retailers' posted and negotiated prices as the latter are unobserved. This fundamental measurement problem is what we designed our field experiment to address.

II.C. Government Subsidies

Because high electricity bills can exacerbate cycles of debt for low-income customers, many governments worldwide subsidize electricity costs for vulnerable groups.[13] In Victoria, the state government subsidizes annual electricity costs for low-income households, pensioners with moderate-to-low income, and veterans. Specifically, for eligible households, the state pays 17.5% of all electricity bills.[14]

There is suggestive evidence that subsidies may be partially captured by the retailers. The ACCC obtained proprietary data from retailers for its 2018 antitrust investigation into the retail electricity market. In ACCC (2018), they show that subsidy

12. A subset of posted prices are default prices. If a customer fails to renew or renegotiate their retail electricity contract after expiration, they may automatically revert to their retailer's default price. According to the ACCC, approximately 5% of Victorian households are on default price contracts.

13. Examples include social tariffs in France, the United States Low Income Home Energy Assistance and National Grid Energy Affordability Programs, and the United Kingdom Warm Home Discount (Brattle Group 2018).

14. Customers are responsible for the first $171.60 of annual costs. Should households have solar panels, the subsidy is taken after any solar credits are applied. In terms of implementation, the rebate is automatically deducted from total nominal costs on each bill. Customers observe the nominal cost, subsidy, and net payable amount on their bills. Subsidy-eligible customers are required to provide evidence to their electricity supplier to prove their entitlement.

recipients indeed pay higher base prices in all Australian states. In Victoria, the average price of electricity, in cents per kWh, variable charges and all fixed charges averaged over the level of use, is 31.4 cents for regular customers and 32.7 cents per kWh for subsidy recipients. This difference implies that 24% of the 5.5 cent subsidy per kWh goes to retailers in the form of higher prices.

Of course, there are many reasons subsidy recipients could pay higher prices. Recipients may be more expensive to serve if they have different ratios of fixed to variable costs or are more likely to default on bill payments. If more costly to serve, there is a clear efficiency case for higher prices. Alternatively, subsidy recipients could rationally choose to search less for good deals. Gulati, McAusland, and Sallee (2017) provide a simple theoretical framework that explains why, when bargaining is costly to customers, subsidy status may in itself lower the amount of search and negotiation that occurs. In this case, there are potential efficiency and distributional gains from reducing customer search costs.

Separate from cost-to-serve or search and bargaining cost–based explanations, higher prices could also be due to retailers explicitly targeting subsidy recipients. In this case, there is a strong argument for regulatory action. By exogenously varying customer characteristics and collecting prices for combinations of characteristics that may be infrequently observed in practice, our experimental design allows us to disentangle some of these different factors that potentially explain why subsidy recipients pay higher prices.

III. The Experiment

We designed a natural field experiment (Harrison and List 2004) to study oligopolistic price posting and negotiation and to identify channels through which negotiated price dispersion arises. Our experimental approach targets three channels: (i) retailers' initial perception of a customer's informedness about reference prices and switching costs at the start of negotiations; (ii) customers' within-negotiation revelation of their knowledge of reference prices; and (iii) customers' low-income subsidy eligibility. In what follows, we describe our experimental design and the call center that we created to implement our experiment.

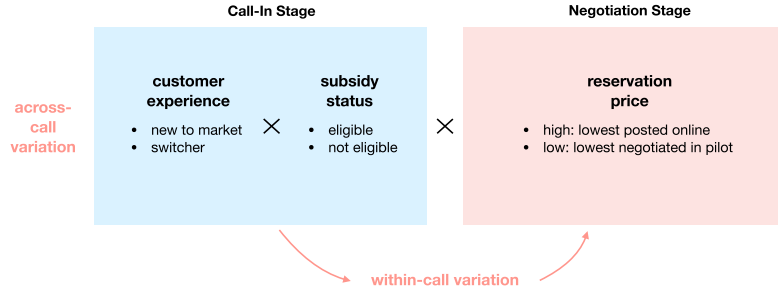

FIGURE I

Experimental Design

III.A. Design

Our experiment is an audit study whereby fictitious customers call electricity retailers under different experimental conditions to negotiate prices. To identify the channels that give rise to price dispersion, we created eight unique customer types, where each type is a combination of one of three characteristics:

- customer experience: switcher is a customer who already lives in the city of our study, is with a rival retailer, and is considering switching retailers versus new-to-market, who is a customer who just moved to the city of our study and needs to establish a new connection.
- low-income subsidy status: eligible for a 17.5% electricity bill subsidy versus not eligible.
- reference price: high price is the lowest posted price, that is, the best price obtained on the government's online search platform before the experiment, versus low price, which is the lowest price we obtained via negotiation during our pilot calls.

Figure I depicts our experimental design, highlighting that each call consisted of two stages. In the call-in stage, callers immediately reveal their customer experience and subsidy status, along with contract standardizations that we describe momentarily. After revealing this information, our actors ask for an initial price quote, recording the daily fixed price and per kWh variable price. The call then moves to the negotiation stage where our actors reveal a reference price and ask for an updated price offer from their retailer. Below, we formulate hypotheses regarding the

effect of our experimental variations on retailers' price offers using a sequential bargaining model with incomplete information.

III.B. Recruiting and Training Actors

Negotiating retail prices involves calling frontline employees at retailer call centers, who are the first point of contact for customers. The challenge in implementing an audit study in this environment is to produce conversations that flow naturally and are also consistent across calls. Callers need to sound spontaneous while simultaneously following a carefully scripted conversation. Calls can neither leave out nor add key elements that could bias the negotiations. They also need to have enough irrelevant variation in case our callers reach the same call center employee more than once.[15]

To assemble our team of callers, we held a casting call at the University of Melbourne, where we auditioned both actors and economics students using hypothetical bargaining scripts. The actors excelled at adding the extraneous detail that helped conversations flow naturally. We hired 18 different actors for the experiment, 9 of which were women. Each actor played a variety of customer types; no actor called the same retailer call center twice.

All successful recruits participated in a four-hour training session where we told them about the study and provided background on the Victorian retail electricity market. We had the actors practice negotiating electricity contracts using our bargaining scripts, where one acted as the electricity retailer, and the other was the customer. During the mock calls, we encouraged actors to develop their voices in a prescribed way. We finished training by having actors engage in pilot negotiations with actual electricity retailers using different bargaining scripts.[16]

III.C. Standardizing Customer and Contract Characteristics

A key part of any audit study is standardizing characteristics that are not experimentally manipulated. In our context, we need to hold constant, to the best of our ability, a customer's

15. This was an ex ante concern in our implementation strategy. To our knowledge, this never happened.

16. Online Appendix A contains the bargaining script used in negotiations and the price sheets that our callers and (silent) research assistants filled out in collecting price offer data during the call-in and negotiate stage.

perceived cost of service. Because electricity bills have fixed and variable components, we fixed the expected electricity usage level. All of our customers obtained price quotes for a two-bedroom rental apartment with an average monthly usage of 300 kWh, which is the average usage level for a two-person household in Melbourne (AEMC 2017).

We shared usage information in all calls, being careful to not signal extreme familiarity with electricity bills in the process. All of our callers reported a level of use of "about 300" (kWh/month). Although callers recorded daily fixed charges and per kWh variable charges for each price quote, we encouraged them to negotiate using total annual bills to facilitate comparison across offers. Because of the potential for lower variable prices to be offset by higher daily fixed prices, our empirical analyses in Sections IV and V focus on total annual bills. Online Appendix B shows that all of our main conclusions are robust to defining price exclusively as the per kWh variable charge.

During our pilot calls, we learned that customer addresses were required to establish credibility with call center personnel to initiate negotiations. We selected and randomly assigned home addresses for our fictitious customers from two-bedroom units available on a large online rental listing website. All home addresses were from the catchment of a single Melbourne-based electricity distribution network, United Energy. This feature guaranteed that electricity network charges would also be identical across customers.

We also normalized several key contract characteristics. Our fictitious customers all negotiated one-year contracts. The properties had existing natural gas connections that our callers did not want to link at the time. Our callers were also not interested in green power plans or time-of-day plans, both of which are relatively rare in the Victorian retail market. If asked, callers said that they did not know their meter type and wanted to know prices for the most common type. Our switcher customers switched from the same rival retailer for all calls, and this retailer (an entrant) was excluded from the experiment.

Finally, after some pilots where call center personnel appeared to strategically switch between pre– and post–sales tax prices, we insisted that callers note whether each price quote include 10% Value Added Tax (VAT). We present prices exclusive of VAT.

III.D. The Call Center

We randomly assigned actors to each of the eight caller types and called 12 of the 13 retailers in the market with every caller type. We randomly allocated addresses and actors to each retailer to ensure we would not call a retailer twice for the same address or by the same actor. Each caller type–retailer combination was repeatedly implemented up to four times, resulting in a total of 336 customer-retailer interactions.[17]

The calls took place at our call center, a set of private offices in the University of Melbourne's Faculty of Business and Economics, during the third week of March 2017, between 9 am and 4 pm. We provided the actors with disposable subscriber identity module (SIM) cards that they inserted into their mobile phones. Using mobile phones enabled us to disable caller identification.

Armed with a bargaining script, an actor dialed each designated retailer on speakerphone. A silent research assistant sat next to each caller, taking duplicate notes during the call-in and negotiate stages of the call to ensure data quality. Calls were not otherwise recorded, as specified in our agreement with human ethics. The study's authors also participated silently in many calls to further ensure quality control and uniformity across calls. After each call, the actor and research assistant compared notes to finalize data collected from the call. As with previous audit studies (Bertrand and Mullainathan 2004), our experiment involved deception: the retailers' call center employees were not told that they were participating in a research study.

To minimize the burden on call center staff, we limited all calls to 20 minutes. We also encouraged actors to publicize good deals to friends and family after the experiment. The university's ethics review board prevented us from using data on a caller's or call center employee's gender or race in our analyses. They also prevented us from revealing statistics based on individual retailers. They did, however, allow reporting of empirical results based on whether a retailer was an incumbent or entrant, as defined in Section II.

17. Our original design further varied information sources for reference prices. However, that distinction was underpowered. The caller types with high reference prices had four different information sources, whereas the caller types with low reference prices had three different information sources, yielding $4 \times 4 \times 12 + 4 \times 3 \times 12 = 336$ customer-retailer interactions. We refer interested readers to Online Appendix A, which discusses the information sources. All empirical results include information source controls and are robust to their exclusion.

1. *Retailers' Call Centers.* To learn about the retailer call centers, we combed through employment ads for call center personnel and websites where employees share their experiences. Some retailers run their call centers in-house, while others contract from call center service companies.[18] Employees of inbound customer service and sales providers are paid by the hour, with financial bonuses and in-kind rewards for outcomes like customer acquisitions, total sales, call times, and ratings on postcall customer experience surveys.[19] Job advertisements mention what appear to be rewards that are linear in outcomes and bonuses for individuals and group outcomes that exceed discrete thresholds or meet minimum expectations. According to employee posts, some rewards come in the form of intangibles like preferential scheduling. Importantly for our study, call center staff have incentives not just to close deals but to close profitable deals, and to do so promptly.

IV. Price Posting and Negotiation

A key innovation of our IO audit study is its ability to make otherwise proprietary negotiated price data accessible. In this section, we leverage this aspect of our audit study to study price posting and negotiation at the market level. The data set for our analysis contains 336 pairs of call-in and negotiated prices. We further scraped retailers' websites each day of the trial to obtain the posted prices corresponding to our call-in and negotiated prices. For each of our 336 caller-retailer combinations, we have a set of three prices, {posted, call-in, negotiated}, implying 1,008 price offers in total.

To normalize prices across retailers, we calculate a total annual bill based on average electricity use of 300 kWh/month. The total annual bill estimate includes connection fees and discounts. Figure II presents the distribution of these bills. The average annual bill based on posted and call-in prices is around $1,150. The distribution of bills from negotiated prices shifts left and is centered around $1,050 a year. The negotiated price distribution also has a mass to the left of $1,000 that is not

18. To the best of our knowledge, all centers are located in Australia. We identified several centers in the greater Melbourne area, Geelong, and Tasmania.

19. Wages posted online range between AUD$25–35; longer-term contracts include benefits like annual leave.

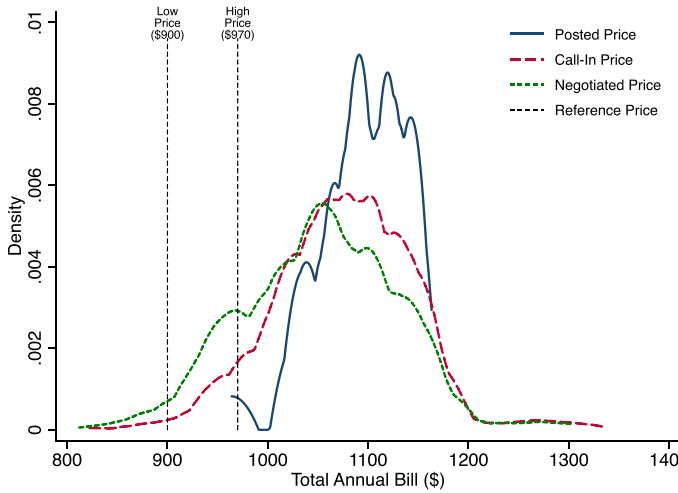

FIGURE II

Distribution of Annual Bills Associated with Posted, Call-In, and Negotiated Contracts

Contracts assume 300 kWh/month usage for all customers in the experimental sample. The figure presents all standardized posted, call-in, and negotiated price offers collected from three incumbent retailers and nine entrants (1,221 offers total). High Price ($970) and Low Price ($970) are annual electricity costs from the two reference prices cited during the negotiation stage of our experiment in Figure I. The High Price contract is the lowest price obtained on the government's online search platform before the experiment. The Low Price contract is the lowest price that we obtained via negotiation during our pilot calls.

present in the posted or call-in distributions. Because our experiment standardizes all other customer and contract characteristics, we can interpret the shift in negotiated prices as being driven solely by our callers' ability to negotiate using a reference price.

Figure II displays the range of prices for what is essentially a homogeneous product. Annual electricity costs can be as high as $1,400 (upper end of the call-in price distribution) and as low as $800 (lower end of the negotiated price distribution). The price distributions highlight significant dispersion both across and in posted, call-in, and negotiated contracts. Retailers offer nonzero discounts in an overwhelming 93% of our negotiations. Negotiation leads, on average, to a 9% discount on annual electricity bills relative to posted contracts. In the best-case scenario, a caller

obtains a 35% discount off a retailer's posted rates when negotiating.[20]

IV.A. Baseline Estimates of Negotiated Price Discounts

Motivated by Figure II, we develop baseline results that quantify discounting rates within-retailer between posted, call-in, and negotiated prices. Specifically, we regress the log of the total annual bill for call i from retailer j, Bill_{ij}, on the way the associated price was obtained:

$$\log(\text{Bill}_{ij}) = \beta_0 + \beta_1 \text{Call-In}_{ij} + \beta_2 \text{Negotiate}_{ij} + \gamma_1 Ncall_k + \gamma_2 Ncall_k^2 + \alpha_k + \rho_j + \delta_t + \epsilon_{ij}, \quad (1)$$

where Call-In_{ij} equals 1 if Bill_{ij} corresponds to the price obtained during the call-in stage of a call and Negotiate_{ij} equals 1 if Bill_{ij} corresponds to the negotiating stage of a call. To implement these regressions, we stack three prices for each call: {posted, call-in, negotiated} prices. As the omitted category in equation (1) is the best price posted on retailer j's website given our customer and contract standardizations, the β_1 and β_2 correspond to percentage discounts relative to posted prices for call-in and negotiated prices. Randomization of callers to retailers and our standardizations allow us to consistently estimate these discounts by OLS. We also report results in levels where Bill_{ij} is the dependent variable to shed light on dollar amounts.

To improve the efficiency of our coefficient estimates, our regression includes actor k, retailer j, and date-of-call t fixed effects: respectively, α_k, ρ_j, and δ_t. We also include the cumulative number of calls made by an actor by the time they made call i and its square, $Ncall_k$ and $Ncall_k^2$, to account for any actor-experience effects on negotiated offers. To further improve precision, we use data from 71 pilot calls, which brings our total sample size to $(336 + 71) \times 3 = 1{,}221$ observations total.[21] We cluster standard errors at the retailer–contract type level, which follows standard

20. The dispersion in total annual electricity bills associated with variable rates and fixed daily rates is similar.

21. While including pilot calls that are identical in terms of execution helps to improve the precision of our regression model estimates, they have no effect on any of our coefficient estimates. Results that exclude pilot calls are available on request.

TABLE I
BASELINE RESULTS

	Log bill (1)	Bill (2)
Call-in	−0.014***	−13.24***
	(0.003)	(3.746)
Negotiated	−0.040***	−39.83***
	(0.004)	(4.197)
R-squared	0.502	0.479
Observations	1,221	1,221

Notes. The dependent variable is the log (column (1)) or level (column (2)) of an annual electricity bill assuming 300 kWh/month usage. The omitted category is the retailer's posted price. Standard errors are clustered to allow for arbitrary covariance at the retailer and contract type level. All regressions include actor, retailer, and date fixed effects, the number of calls made by an actor up to that point, and its square. *10%,** 5%,*** 1% significance level.

approaches to inference in audit studies used in studies of labor market discrimination (e.g., Bertrand and Mullainathan 2004).

Table I presents our baseline results. We find that on average, customers obtain a 1.5% discount ($13.24 savings) from calling to obtain a price quote. When they further negotiate during the call, this discount rises to 4.0% ($39.83 savings). These discounts are individually statistically significant, and a joint test of their equality rejects the null at the 1% level. That is, our experiment yields statistically significant negotiated discounts.

1. *Economic Magnitudes.* The negotiated price reductions are statistically different from zero, but are they economically meaningful? Some back-of-the-envelope calculations suggest that they are.

Consider first the retailer's perspective. Through the federal government's unique access to customer-level contract data and retailer cost data, ACCC (2018) estimates that Victorian retailers earn an 11 percentage point profit margin on average. In dollar terms, this implies that $160 of a customer's $1,457 annual before-tax electricity bill is profit.[22] An 11 percentage point margin and $1,457 average bill implies a $1,297 annual per customer cost. Applying our estimates of negotiation effects on electricity costs, a 4 percentage point reduction off an average bill yields a $1,399 average bill and an 8 percentage point profit margin. In sum, retail

22. All margin figures correspond to earnings before interest, taxes, depreciation, and amortization.

profit margins fall from 11 to 8 percentage points, or by 27%, when customers negotiate. In this sense, our estimated negotiated price discounts are economically meaningful.

On the customer side, an average annual savings of $40 from less than 20 minutes bargaining represents three hours of minimum-wage work.[23] Further recall that we obtained price quotes for a two-person household living in an apartment. A customer with children or with older and less energy-efficient appliances is likely to consume more electricity and leave more money on the table if they fail to negotiate. Although we did not vary expected consumption levels in our experiment, we note in Online Appendix B that negotiating leads to reductions in variable rates. Variable-rate discounting implies that our estimate of customer savings from negotiation is likely a lower bound on savings for large electricity users.

IV.B. Incumbent and Entrant Pricing

Our discussion in Section II highlights that the customers of incumbents and entrants have different propensities to search and bargain, with incumbents serving more captive types. Armstrong and Vickers (2019) study Bertrand pricing in a duopoly with homogeneous products when firms have exogenously different shares of captive customers who fail to compare rival prices. Their model setup aligns well with our context in that incumbents are endowed with captive types from the outset of retail deregulation, and firms compete on prices and sell a homogeneous product. Our setting and unique price data allow us to assess predictions from their model empirically.

A central insight from Armstrong and Vickers (2019) is that discrimination against captive customers can emerge in equilibrium. In particular, the unique asymmetric mixed-strategy Nash equilibrium of their model sees firms with larger shares of captive types offer price distributions that have higher prices than firms with few captive types (in the sense of first-order stochastic dominance). Figure III plots cumulative distributions for incumbents' and entrants' call-in prices from our data.[24]

23. Savings are calculated based on a 2017 federal minimum wage of $18.29, or $13.10 per hour after tax assuming a 38-hour workweek and no tax-free threshold (Australian Bureau of Statistics 2021).

24. We do not include negotiated prices in comparing incumbents' and entrants' price distributions because Armstrong and Vickers (2019) do not consider

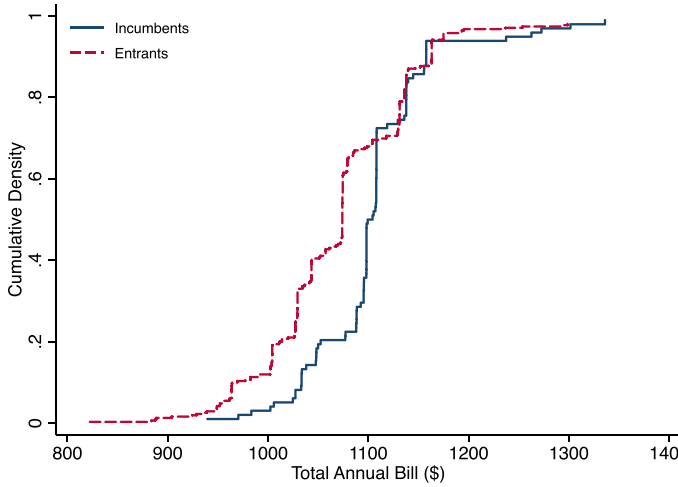

FIGURE III

Cumulative Distribution of Annual Bills among Incumbents and Entrants

Contracts assume 300 kWh/month usage for all customers in the experimental sample. Presenting all standardized call-in price offers collected from three incumbent retailers and nine entrants (407 offers total).

Retailers in our market implement their price distributions through public posting and private negotiation. Another quote, this time from a Big 3 retailer submission to the ACCC (2018) inquiry notes that privately negotiated offers:[25] "are usually at a lower price because they only apply to a limited number of customers. For commercial reasons, it is not viable for retailers to make these offers available to all customers." The quote speaks to the underlying tension in the mixed-strategy Nash equilibrium of Varian (1980)–type models like the one from Armstrong and Vickers (2019): retailers want to charge high prices to exploit captive types and offer low prices to compete for noncaptive types. Under uniform pricing, mixed

price negotiation. Moreover, in the spirit of Sorensen (2000), we have confirmed from auxiliary regressions that there is nonnegligible residual dispersion in call-in prices for both incumbents and entrants after accounting for dispersion explained by our experimental conditions and all the regressors in equation (1), including retailer, actor, and date dummy variables. This result is consistent with firms engaging in some degree of randomization in making call-in price offers, which aligns with equilibrium pricing in Armstrong and Vickers (2019).

25. Origin Energy (2018), 2.

strategies balance these competing tensions in these models. In practice, however, retailers can offer different prices to captive and noncaptive customers by privately negotiating with the latter group. Such pricing behavior gives rise to a form of third-degree price discrimination among captive and noncaptive types. In this market context, we undertake a bargaining field experiment to study how retailers compete through private negotiation for noncaptive customers and price discriminate among them.[26]

V. Bargaining, Information Revelation, and Discrimination

In this section, we leverage the second key aspect of our audit study: it imposes structure on real-world high-stakes negotiations to allow us to examine how different sources of incomplete information affect bargaining outcomes at the individual level. Sobel and Takahashi's (1983) model of bargaining under incomplete information, which we tailor to our experimental design and research context, is the lens through which we conduct our analysis. The model yields four testable implications which we bring to the data, where we focus on variation induced by our experimental variations in customer experience (switcher versus new-to-market) and reference prices (low price versus high price). Last, we study whether retailers discriminate in negotiations based on low-income subsidy eligibility, which we view as a novel and policy-relevant application of IO audit studies in markets with private negotiation and government subsidies.

V.A. Model

We consider a two-stage one-sided seller-offer bargaining game between a customer C and a retailer R.[27] Before bargaining begins, C is endowed with a reference contract that

26. As Armstrong and Vickers (2019) note, little is known about the existence and theoretical properties of asymmetric equilibria in oligopoly models with more than two firms, uniform pricing, and customers who consider different subsets of firms.

27. Alternatively, we could consider an incomplete-information bargaining model with two-sided offers as in Cramton (1984) or Rubinstein (1985). We use a model with one-sided offers to align with the stark difference between retailers and customers in negotiating experience. Retailers negotiate with hundreds of customers every day. In contrast, customers rarely negotiate and likely do so at yearly frequencies in line with typical contract durations. In our model, the customer thus does not have bargaining power through the ability to make

yields a (normalized) value of $v^0 = 0$ at a price p^0. Assuming quasi-linear utility, the reference contract yields utility $v^0 - p^0 = -p^0$.[28] C is also endowed with a value v from choosing R's contract instead of the reference contract.[29] Last, C has a time discount factor $\delta_C \in (0, 1)$.

R can provide electricity to C at a (normalized) cost of 0 and has a discount factor $\delta_R \in (0, 1)$. p^0 and v are independently drawn by Nature N from continuous distributions $F(v)$ and $G(p^0)$ with full support over $[v_{min}, v_{max}]$ and $[p^0_{min}, p^0_{max}]$.[30] At the start of the game, p^0 and v are privately known only to C. Their respective distributions are, however, common knowledge, as are all other model parameters.[31]

Bargaining proceeds in two stages, which aligns with our experimental design in Figure I and the base model setup of Sobel and Takahashi (1983). In the first stage (e.g., the call-in stage of the experiment), R makes an offer p_1, which C either accepts ($a_1 = 1$) or rejects ($a_1 = 0$). If C accepts, the game ends with C realizing utility $v - p_1$ and R earning profit p_1. If C rejects, the game moves to a second stage (e.g., the negotiation stage). At the beginning of the second stage, p^0 ceases to be C's private information and becomes commonly known. This can happen if C truthfully reveals p^0 and R is able to immediately and costly verify C's announcement as a legitimate market offer.[32] That is, R becomes aware

counteroffers. They do, however, have bargaining leverage through their awareness of a reference contract offer and value from switching retailers.

28. As in our experiment, we interpret p^0 as the annual cost of electricity for C from the reference contract.

29. v can be interpreted as a utility shock associated with actually accessing the reference price p^0 (e.g., additional effort of signing up for p^0 even if C is aware of it), potential disutility costs C faces in switching to R, or idiosyncratic brand preferences.

30. Fudenberg and Tirole (1983) consider a one-sided incomplete-information bargaining model similar to Sobel and Takahashi (1983) except buyers' private values are discrete. The models yield similar intuition regarding sellers' and buyers' sequential bargaining strategies and beliefs. The continuous-type case is simpler to work with in developing testable hypotheses for our experiment.

31. We could alternatively assume that δ_C is the source of private information as in Rubinstein (1985). Given the importance of search and switching costs in our market, assuming that a customer's reference price p^0 and incremental utility from choosing a retailer v is the main source of private information that retailers anticipate in making price offers better aligns with our institutional setting.

32. We abstract from cheap talk in our bargaining game (as in Farrell and Gibbons 1989) by assuming R can verify p^0 as a legitimate market offer. In our experimental design, we calibrated low price and high price to indeed be legitimate

that C is aware of the p^0 reference price. Knowing this, R makes a second stage offer p_2, which C either accepts ($a_2 = 1$) or rejects ($a_2 = 0$). If C rejects, they take the reference contract and realize discounted utility $-\delta_C p^0$ and R earns zero profit.[33] If C accepts, they realize utility $\delta_C(v - p_2)$ and R earns profit $\delta_R p_2$. Figure IV presents the extensive form of the model and payoffs.

1. *Equilibrium.* The sequential equilibrium (Kreps and Wilson 1982) of the game consists of R price offers p_1^* and p_2^* and C acceptance rules a_1^* and a_2^* such that (i) the price offers and acceptance rules are best responses to their opponent's strategies, and (ii) R updates, via Bayes's rule, its belief about v in the second stage given C's first-stage rejection decision and revelation of p^0 at the start of the second stage. We denote the retailer's updated second-stage belief in Figure IV by $\widetilde{F}(v|p^0, a_1)$.

To help build intuition for our hypotheses regarding the effect of customer type and reference prices on R's price offers, we consider the simplest specification of $F(v)$ and $G(p^0)$ as independent uniform distributions with means $\bar{v}$ and $\bar{p}^0$. These assumptions yield closed-form solutions that directly map model predictions regarding potential differences in levels of p_1^* and p_2^* across our experimental variations to mean differences in v and p^0. In Online Appendix C, we solve the unique equilibrium and derive testable hypotheses.[34]

market offers at the time of our audit study. In our experimental context, it is well known that retailers closely track posted and negotiated contracts in the market (ACCC 2018), in line with our assumption that retailers can verify p^0. We thus interpret $G(p^0)$ as R's prior belief over the distribution of verifiable reference price offers from which a customer draws a reference price before entering a negotiation.

33. We abstract from continuation values at the end of the game that would exist in a larger dynamic sequential search-and-bargaining process involving multiple retailers. Likewise, we can think of the p^0 draw from $G(p^0)$ at the start of the game as corresponding to prior search-and-negotiation in such a dynamic process (e.g., it could be a private state variable for C on entering the negotiation with R). Modeling a market equilibrium involving multilateral contracting, sequential search, and sequential bargaining with incomplete information between customers and retailers is beyond the scope of our article.

34. Sobel and Takahashi (1983) prove equilibrium uniqueness in a model where the buyer is privately informed about p^0 but where v is 0. Uniqueness follows if $F(p^0)$ is in the class of distributions that implies $p^0F(p^0)$ is decreasing. In other words, if the seller has decreasing marginal revenue.

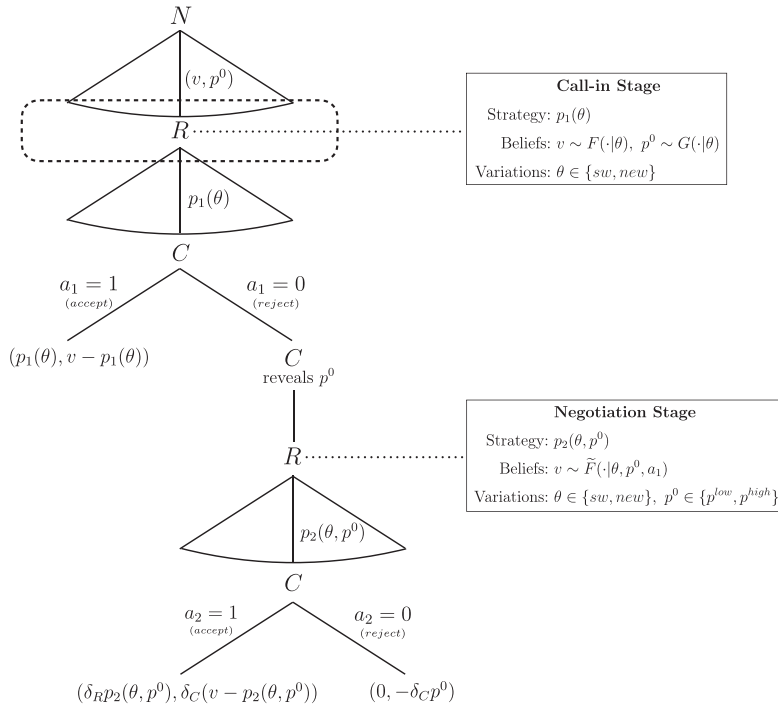

FIGURE IV

Model Extensive Form and Experimental Variations

V.B. *Hypotheses*

1. *Call-In Price.* Figure IV also illustrates how our switcher versus new-to-market and low-price versus high-price experimental variations can affect the retailer's equilibrium call-in and negotiated price offers. To this end, we introduce one additional piece of notation, θ, to represent signals that a retailer potentially conditions on before making an initial call-in price offer. These signals might include, for example, the gender or accent of our callers.[35]

35. Bertrand and Duflo (2017) overview research on gender and racial discrimination in negotiations. More broadly, θ comes from a large set of signals that customers can send in free-form communication with retailers before an initial price offer. In this larger game, we exogenously vary θ to include customer experience (new-to-market or switcher) and subsidy status (eligible or not eligible) variations. In effect, our experiment reveals retailers' price offers down the game tree conditional on these signals.

They also include our callers' exogenously varied switcher or new-to-market status, which we assume that retailers can verify.[36]

Let $\bar{p}^0(\theta)$ be the mean reference price. Holding other factors fixed (as our audit study does), retailers may expect switchers to enter negotiations with lower reference prices than new-to-market customers: $\bar{p}^0(sw) < \bar{p}^0(new)$. There are at least three potential underlying channels for a difference in these means:

i. Consideration sets: switchers are more familiar with the market than new-to-market customers who just moved there. Switchers could thus have more retailers in their consideration sets, leading to lower expected reference prices.
ii. Time constraints: switchers have less time pressure in our experimental negotiations to shop around for low electricity prices than do new-to-market customers who just need to "turn the lights on," among various other things, when moving to a new city.
iii. Selection on search: switchers actively choose to call up and negotiate, which may cause a retailer to update its initial belief $F(p^0|\theta)$ about a switcher's willingness to search for prices. In contrast, new-to-market customers do not actively choose to call up; they need power regardless of their willingness to search. There could thus exist a selection-on-search effect that leads retailers to expect switchers to enter negotiations with lower reference prices than new-to-market customers.[37]

These channels point to switchers entering negotiations with lower expected reference prices, implying stronger expected bargaining leverage during the call-in and negotiation stages.

Likewise, let $\bar{v}(\theta)$ be the mean private value for the customer from choosing the retailer over their reference contract. We

36. Retailers might anticipate that new-to-market customers lie and claim to be switchers to obtain lower price offers. However, if a customer states that they are a switcher and eventually accepts a retailer's offer, their current retailer is revealed during the retailer switching process. Lying new-to-market customers are identified at this later (unmodeled) part of the switching process since they do not have a current retailer. Retailers' ability to verify types in this way undermines the potential for new-to-market types to act strategically and pool with switcher types in practice.

37. We thank a referee for highlighting this channel and for emphasizing the potential role of switching costs, which we discuss momentarily.

expect $\bar{v}(sw) < \bar{v}(new)$, that is, holding other factors fixed, retailers expect switchers to enter negotiations with lower private values than new customers. From our discussion of switching costs in Section II.A, there are two potential channels:

i. Exit fees: switchers face exit fees from terminating their current contract and signing up with a new retailer, typically between $20 and $22. New-to-market customers do not face these fees.
ii. Transactions costs: switchers needed to coordinate back-and-forth with their new and old retailers in making a switch. New-to-market customers only have to deal with their new retailer in signing up.

In sum, differences between switchers' and new-to-market customers' mean reference price and private value (due to switching costs) lead the retailer to offer lower initial call-in prices to switchers.[38] Our first hypothesis summarizes how these two sources of private information both generate price discrimination against new-to-market customers in the call-in stage:

Hypothesis 1 (initial price discrimination): Switchers are offered lower prices than new-to-market customers during the call-in stage.

$$p_1^*(sw) < p_1^*(new)$$

2. *Negotiated Price.* Turning to the negotiation stage, while the retailer sets p_1^* considering all possible second-stage reference prices revealed by the customer, our experiment exogenously puts

38. Dynamic harvesting motives, as recently studied by Allen and Li (2020), could motivate retailers to instead offer lower prices to new-to-market customers in the call-in phase. This pricing behavior can arise if retailers believe switchers who engage in active search are never captive types (as in Armstrong and Vickers 2019). In contrast, retailers may perceive that new-to-market customers, who are forced to search and sign up with a retailer, are a mixture of captive and noncaptive types. Under these beliefs, retailers may end up competing aggressively for new-to-market customers who, with some probability, are captive types that yield large net present value revenue streams due to their captivity in later periods. Such dynamic pricing incentives work against us finding discrimination against new-to-market customers, particularly in the call-in phase. Harvesting motives are less relevant during the negotiation stage, however, as switchers and new-to-market customers reveal they have searched and are aware of a reference price and hence are unlikely to be captive types. We thank a referee and Paul Heidhues for pointing out this potential channel.

the retailer in two particular second-stage negotiation subgames with $p^0 \in \{p^{low}, p^{high}\}$. We highlight this in the bottom half of the extensive form in Figure IV. At this point in the game, the customer has revealed their reference price p^0 but the retailer still does not know the customer's private value v. The retailer has updated its belief about v, $\widetilde{F}(v|\theta, p^0, a_1)$, given the customer's type $\theta \in \{sw, new\}$, reference price $p^0 \in \{p^{low}, p^{high}\}$, and decision to reject the call-in price, $a_1 = 0$.

The equilibrium of the model yields two testable implications regarding the retailer's negotiated price p_2^*. The first is that retailers offer lower negotiated prices to customers who are aware of lower reference prices:

Hypothesis 2 (leverage effect of reference prices): Conditional on type θ, customers with lower reference prices are offered lower negotiated prices.

$$p_2^*(\theta, p^{low}) < p_2^*(\theta, p^{high})$$

Our final hypothesis concerns how negotiated prices will vary with a customer's type. While p^0 is revealed in the negotiation stage, v remains privately known only to the customer. The retailer has updated their belief about v. Conditional on p^0, the retailer continues to anticipate that switchers face higher switching costs than new-to-market customers. In equilibrium, this causes the retailer to offer lower negotiated prices to switchers. We summarize this in our third hypothesis:

Hypothesis 3 (switching cost–based discrimination): Conditional on reference price p^0, switchers will be offered lower negotiated prices than new-to-market customers.

$$p_2^*(sw, p^0) < p_2^*(new, p^0)$$

V.C. Testing the Hypotheses

Building on our baseline regression, we use the following specification to test the three hypotheses:

$$\begin{aligned}\log(\text{Bill}_{ij}) = {} & \beta_0 + \beta_1 \text{Call-In}_{ij} + \beta_2 \text{Call-In}_{ij} \times \text{Switcher}_i \\ & + \beta_3 \text{Negotiated}_{ij} + \beta_4 \text{Negotiated}_{ij} \times \text{Switcher}_i \\ & + \beta_5 \text{Negotiated}_{ij} \times \text{LowResP}_i \\ & + \beta_6 \text{Negotiated}_{ij} \times \text{Switcher}_i \times \text{LowResP}_i \\ & + \gamma_1 Ncall_k + \gamma_2 Ncall_k^2 + \alpha_k + \rho_j + \delta_t + \epsilon_{ij}, \end{aligned} \tag{2}$$

where Switcher_i equals 1 if caller i states that they are a switcher during the call-in stage, and LowResP_i equals 1 if the caller reveals they are aware of a reference contract with a low price during the negotiation stage. All other variables are defined as in equation (1). As with our baseline results, we report two sets of estimates with $\log(\text{Bill}_{ij})$ and Bill_{ij} as dependent variables to quantify price discrimination in negotiations in terms of percentage changes and dollar amounts.

The base group in equation (2) during the call-in stage is new-to-market customers; during the negotiation stage, the base group is new-to-market customers with high reference prices. The coefficients on regressors involving Switcher_i or LowResP_i thus capture relative negotiated discounts for switchers or customers with low reference prices. Exploiting our audit study's ability to exogenously vary Switcher_i and LowResP_i and standardize all other customer and contract characteristics, we can use OLS estimates of β_2, β_4, and β_5 to test our model's predictions regarding information revelation, bargaining, and discrimination:

H1 H_0: $\beta_2 = 0$ versus H_1: $\beta_2 \neq 0$
H2 H_0: $\beta_5 = 0$ versus H_1: $\beta_5 \neq 0$
H3 H_0: $\beta_4 = 0$ versus H_1: $\beta_4 \neq 0$.

Table II presents our test results in terms of percentage discounts (top panel) and dollar amounts (bottom panel). Column (1) reproduces our baseline estimates, and column (4) is our preferred specification. All three hypotheses are confirmed by our field experiment: new-to-market customers are immediately discriminated against during the call-in stage (H1), customers with lower reference prices obtain lower negotiated price offers (H2), and discrimination against new-to-market customers (conditional on the reference price) persists through to the negotiation stage (H3).

1. *Initial Price Discrimination.* Quantitatively, there are various findings of note from our preferred specification in column (4). Switchers, who immediately obtain a 1.4 percentage point discount off posted prices during the call-in phase, drive our baseline call-in price discounts. New-to-market customers, in contrast, are discriminated against and are offered a price that is no better, statistically speaking, than a retailer's posted price at the start of negotiations.

TABLE II

INFORMATION REVELATION AND NEGOTIATED PRICE OFFERS

	(1)	(2)	(3)	(4)	(5)
Panel A: Log bill					
Call-in	−0.014***	−0.008*	−0.010**	−0.008*	−0.008*
	(0.003)	(0.005)	(0.004)	(0.005)	(0.005)
Call-in × switcher		−0.014**	−0.009*	−0.014**	−0.014**
		(0.007)	(0.005)	(0.007)	(0.007)
Negotiated	−0.040***	−0.032***	−0.033***	−0.025***	−0.022***
	(0.004)	(0.005)	(0.005)	(0.006)	(0.007)
Negotiated × switcher		−0.019**		−0.019**	−0.025**
		(0.008)		(0.008)	(0.010)
Negotiated × low res. price			−0.014**	−0.015**	−0.020**
			(0.006)	(0.006)	(0.008)
Negotiated × switcher × low res. price					0.013
					(0.012)
R-squared	0.490	0.507	0.506	0.510	0.510
Observations	1,221	1,221	1,221	1,221	1,221
Panel B: Bill					
Call-in	−13.24***	−6.17	−8.54*	−6.15	−6.09
	(3.71)	(5.47)	(4.92)	(5.48)	(5.49)
Call-in × switcher		−15.99**	−10.62*	−16.03**	−16.18**
		(7.90)	(6.30)	(7.88)	(7.91)
Negotiated	−39.83***	−30.43***	−32.60***	−22.45***	−19.28**
	(4.15)	(6.07)	(5.69)	(7.31)	(8.52)
Negotiated × switcher		−21.25**		−22.07***	−28.87**
		(8.48)		(8.43)	(11.75)
Negotiated × low res. price			−15.74**	−16.58**	−23.06**
			(6.44)	(6.42)	(9.30)
Negotiated × switcher × low res. price					14.67
					(12.51)
R-squared	0.466	0.485	0.483	0.488	0.488
Observations	1,221	1,221	1,221	1,221	1,221

Notes. The dependent variable is the logarithm (Panel A) or level (Panel B) of an annual electricity bill assuming 300 kWh/month usage. The omitted category is the retailer's posted price. Standard errors are clustered to allow for arbitrary covariance at the retailer and contract type level. All regressions include actor, retailer, and date fixed effects, the number of calls made by an actor up to that point, and its square. *10%,** 5%,*** 1% significance level.

Translating these estimates into behavior and dollar amounts, switchers generate $16.03 in annual electricity cost savings by calling retailers to make a switch instead of switching online (the other common channel through which customers switch retailers). These savings from bargaining are economically meaningful compared with potential financial gains from search and switching online. In particular, if the representative electricity customer from our field experiment switches from their retailer to another based on posted rates online, they save $58.34 on annual electricity costs on average. If they

switch online to the retailer with the lowest posted price they save $178.01.[39] Our experimental estimates of the gains from calling-in among switchers of $16.03 is nonnegligible at 27% and 9% of these financial gains from switching based on posted prices.[40]

2. *Leverage Effect of Reference Prices.* In the second stage of negotiations, the leverage effect from reference prices is also economically significant. Customers aware of the high reference price obtain a 2.5 percentage point discount, an 80% increase compared to the call-in stage discount for switchers. Customers citing the low reference price realize an even larger 4.0 percentage point discount, a statistically significant increase compared to discounts offered to customers citing the high reference price. As expected, being informed about lower reference prices from the market matters in negotiations.

In terms of behavior and financial gains, our representative customer generates $22.45 in annual electricity cost savings from bargaining if they are aware of the reference price on the government price comparison platform. This benchmark is policy relevant: it quantifies the magnitude of expected gains from search using the government-facilitated platform. A customer who goes into the negotiation aware of the potential gains from negotiating obtains on average a $22.45 + $16.58 = $39.03 total discount from bargaining. This degree of price awareness aligns with the set-up of Varian (1980)–type search models, namely, that customers learn the lowest price offer in a market on sinking a search cost. This is thus a relevant benchmark from theories of price search and an upper bound on the magnitude of search-based gains in negotiations for our representative customer.

Compared with returns from online search, the incremental returns from bargaining with high and low reference prices are economically meaningful. Respectively, these negotiated savings are 13% and 22% of the $178.01 financial gain from searching

39. Our experimental design allows us to construct benchmark estimated gains from search based on posted prices because all of our switcher customers state they are currently with the same retailer.

40. Calling in does not lead to meaningful time savings for customers. It takes similar amounts of time to switch online or calling in, approximately 10 minutes.

and switching to the lowest-priced retailer based on posted prices.[41]

3. *Switching Cost–Based Discrimination.* Table II further reveals that switching costs have a statistically and economically significant effect on negotiated price offers. Switchers are offered an additional 1.9 percentage point negotiated discount relative to new-to-market customers, conditional on the reference price. In total, a switcher who is aware of a low reference price obtains a maximal 5.9% negotiated discount.

In dollar amounts, the switcher premium in negotiations is $22.07, which has a quantitatively similar effect on negotiated price discounts as being informed about the high reference price. This experimental estimate corresponds closely to the $20 to $22 early contract termination fees that most retailers charge. Together, our experimental results and these external exit fees are consistent with retailers attempting to compensate customers for switching fees when competing for their business through private negotiation.

Last, it is natural to ask whether initial discrimination against new customers in the call-in stage is driven by them being less likely to be informed about competitors' prices versus having lower switching costs. Clearly, and as expected, reference prices have a large effect on bargaining outcomes. If price discrimination by retailers during the call-in stage is driven purely by expected differences in awareness about reference prices, switchers and new-to-market customers should ultimately obtain similar second-stage negotiated price offers. That our β_4 estimate is large and statistically significantly different from zero and not statistically different from our estimate of β_2 in both panels of Table II points to switching costs as being an important driver of initial price discrimination against new-to-market customers.

V.D. Subsidy-Based Discrimination

Our last experimental condition, subsidy status, varies whether a household is eligible or not eligible for a low-income electricity bill subsidy. Recall that the state government offers

41. We note, however, that this interpretation of magnitudes is conditional on a customer engaging in search and bargaining. There may exist significant and unmeasured sunk costs which limit this behavior (as documented by Byrne and de Roos 2022) and offset individual gains from search and bargaining.

17.5% discounts off total annual electricity bills for disadvantaged groups and concerns of targeted discrimination based on subsidy status exist. We show how our audit study can be adapted to test whether such discrimination exists, a potential mechanism for incomplete subsidy pass-through not previously tested in observational studies.

From our experimental design in Figure I, the actors immediately reveal subsidy status before retailers make their call-in price offer. Given the exogenous variation in subsidy status from our experiment and standardization of customer characteristics, we can use a regression to test for the effect of subsidy status on call-in and negotiated prices:

$$\begin{aligned}\log(\text{Bill}_{ij}) =& \beta_0 + \beta_1 \text{Call-In}_{ij} + \beta_2 \text{Call-In}_{ij} \times \text{Subsidy}_i \\ &+ \beta_3 \text{Negotiated}_{ij} + \beta_4 \text{Negotiated}_{ij} \times \text{Subsidy}_i \\ &+ \gamma_1 Ncall_k + \gamma_2 Ncall_k^2 + \alpha_k + \rho_j + \delta_t + \epsilon_{ij}, \end{aligned} \tag{3}$$

where Subsidy_i equals 1 if an actor states that they are a subsidy recipient. All other variables are as before. We also report results where Bill_{ij} is the dependent variable.

How can we test for targeted discrimination against subsidy recipients? If the government's concerns about rent-seeking and targeted discrimination are real, this causes subsidy recipients' call-in prices to be higher than those of nonrecipients. However, theoretical predictions and empirical findings from Gulati, McAusland, and Sallee (2017) emphasize that per unit subsidies can reduce the gains from search, leading to a reduction in search effort among subsidy recipients. From Figure IV, if retailers update their prior belief $F(p^0|\theta)$ when $\theta = eligible$ according to this intuition, then $\bar{p}^0(eligible) > \bar{p}^0(not\ eligible)$. That is, retailers would expect subsidy recipients to enter negotiations with higher expected reference prices, causing them to offer high call-in prices to subsidy recipients. This search channel thus confounds the use of β_2 to test for targeted discrimination.

However, in the negotiation stage, the customer reveals their reference price and the search channel no longer affects the retailer's price offer. Remaining, however, is the possibility of subsidy-based price discrimination, which would imply $\beta_4 > 0$. We summarize this in our final hypothesis:

Hypothesis 4 (targeted discrimination against subsidy recipients): Subsidy-eligible customers are offered higher negotiated

TABLE III

SUBSIDY ELIGIBILITY AND TARGETED DISCRIMINATION IN NEGOTIATIONS

	Log bill (1)	Bill (2)
Call-in	−0.013***	−11.56**
	(0.005)	(5.26)
Call-in × subsidy	−0.003	−3.46
	(0.005)	(6.30)
Negotiated	−0.040***	−39.61***
	(0.005)	(5.30)
Negotiated × subsidy	−0.000	−0.45
	(0.006)	(6.77)
R-squared	0.501	0.479
Observations	1,221	1,221

Notes. The dependent variable is the log (column (1)) or level (column (2)) of an annual electricity bill assuming 300 kWh/month usage. The omitted category is the retailer's posted price. Standard errors are clustered to allow for arbitrary covariance at the retailer and contract type level. All regressions include actor, retailer, and date fixed effects, the number of calls made by an actor up to that point, and its square. *10%,** 5%,*** 1% significance level.

prices than ineligible customers:

$$p_2^*(eligible, p^0) > p_2^*(not\ eligible, p^0).$$

Empirically, we can test the hypothesis using the estimate of β_4 from equation (3):

H4 H_0: $\beta_4 = 0$ versus H_1: $\beta_4 \neq 0$.

Table III presents our test results. Despite public concern over the issue, we find little evidence of targeted discrimination against subsidy recipients. In unreported results, we also find no evidence of targeted discrimination among switchers and new-to-market customers, nor among those with high or low reference prices.[42]

42. We emphasize that these findings pertain strictly to the existence of targeted discrimination as a potential mechanism for incomplete subsidy pass-through among noncaptive customers who engage in bargaining. While ACCC (2018) establishes that low-income customers pay higher prices, without data on prices and consumption for captive and noncaptive customers, we cannot estimate market-level subsidy pass-through.

VI. Conclusion

Price posting and private negotiation characterize pricing in many markets, yet there is little research into these pricing structures because of limited access to negotiated price data. Using a field experiment that borrows from audit studies to study labor market discrimination, we have provided a novel empirical analysis of price posting, negotiation, and discrimination in an oligopoly.

At the market level, we documented substantial dispersion between posted and negotiated prices and asymmetric pricing structures among incumbents and entrants that underline customer segmentation based on willingness to search and bargain. At the individual level, we examined how incomplete information affects sequential bargaining between customers and retailers. Combining a novel experimental design with institutional features of our research context, we found evidence of price discrimination in negotiations based on retailers' perceptions of customer informedness of reference prices and switching costs. Last, we highlighted how audit studies can deliver simple yet powerful tests of targeted discrimination against subsidy recipients as a potential mechanism for incomplete subsidy pass-through.

Our study contributes methodologically to a small but growing area of research on bargaining under incomplete information. Our audit study–based approach allows future researchers to access individually negotiated price data from various markets. Moreover, by experimentally imposing different bargaining structures on real-world high-stakes negotiations, researchers can leverage a wealth of economic theory to examine how different sources of private information and bargaining structures affect bargaining outcomes.[43] Our empirical results establish two fundamental sources of consumer inertia—search frictions and switching costs—as microfoundations for bargaining leverage in markets with individual buyer-seller negotiation and incomplete information. In this way, our results inform new models of oligopoly pricing that build from bilateral bargaining between privately informed individual buyers and sellers (e.g., Loertscher and Marx 2022), but that currently abstract from underlying sources of bargaining power and leverage.

43. Researchers can also potentially use audit studies to investigate behavioral phenomena such as anchoring or fairness on bargaining outcomes in the field.

Our results also speak to potential regressivity in pricing structures involving public price posting and private negotiation. To what extent do vulnerable customers benefit or suffer in such markets? Our results suggest that the answer depends on which types of customers engage in search and negotiation. For example, are low-income, elderly, or disadvantaged customers less able to search or haggle, perhaps due to cultural, technological, or language constraints?

Although audit studies are not well suited to comment on the distribution of customer types in the population—focusing instead on revealing prices by customer type—other research has shown that vulnerable customers are often less likely to search or search effectively. Byrne and Martin (2021) provide an overview of empirical evidence of this phenomenon across industries and evidence that lower-income customers are significantly less likely to engage in search and bargaining in our specific market. Office of Gas and Electricity Markets (2019) documents a similar pattern in the United Kingdom's deregulated retail electricity market. The evidence from both studies implies that pricing structures like those we uncover in this article are regressive.

This regressivity is particularly important when the observed pricing structures result from policy decisions. In our context, the government deregulated retail electricity markets to promote competition. Deregulating a geographically based monopoly where all customers pay the same regulated price typically leads to price dispersion (Borenstein and Rose 1994). But is the resulting price dispersion one that society wants? Deregulating retail electricity may lead to customer segmentation based on time costs, with high-income customers choosing not to search and paying higher prices. However, in stark contrast, deregulation may instead drive segmentation based on confusion, unfamiliarity, and discomfort in navigating a decentralized market to the detriment of low-income households. There may thus be a trade-off between a single regulated price and the dispersed prices achieved under competition with potentially regressive price discrimination. This tension speaks more generally to efficiency and equity tensions in market design which Akbarpour, Dworczak, and Kominers (2021) recently explore theoretically.

Policy makers may be able to mitigate this potential regressivity in pricing structures. Our sequential bargaining results suggest that informing low-income customers about low reference prices can strengthen their bargaining leverage in negotiations.

However, the market we studied had a government price search platform for more than two years when we ran our experiment. Despite this, low-income customers continued to pay relatively higher prices, so simply making price data publicly available may not be enough to address regressivity in pricing.[44] To the extent that low-income customers cannot overcome search and bargaining costs, governments may need to consider alternative market designs if they wish to avoid regressive pricing structures. One example is centralized exchanges where retailers periodically bid to be the lowest-cost supplier among contract bids. Customers are then defaulted to switch to the lowest-cost contract each time the exchange runs. Offering low-income customers the opportunity to opt out of a decentralized market with price posting and negotiation by opting into a centralized exchange with automatic switching may be more effective than a more laissez-faire approach in moving low-income customers to more competitive prices in markets with search frictions, price posting, and negotiation.

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SUPPLEMENTARY MATERIAL

An Online Appendix for this article can be found at the *Quarterly Journal of Economics* online.

DATA AVAILABILITY

Data and code replicating the tables and figures in this article can be found in Byrne, Martin, and Nah (2022) in the Harvard Dataverse, https://doi.org/10.7910/DVN/KRHAWJ.

44. The government is now paying subsidy recipients to search on the platform. In particular, https://compare.energy.vic.gov.au offers a $250 "Power Saving Bonus" to low-income subsidy recipients simply for searching for the first time on the platform. This bonus offer is revealing of the hurdle low-income customers face in engaging in the market. Such hurdles might include, for example, fixed costs to start searching on platforms as identified by Byrne and de Roos (2022).

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