Customer Discrimination in the Workplace: Evidence from Online Sales*

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Abstract

Many workers are evaluated on their ability to engage with customers. We measure the impact of gender-based customer discrimination on the productivity of online sales agents in Sub-Saharan Africa. Using a novel framework that randomly varies the gender of names presented to customers without changing worker behavior, we find the assignment of a female-sounding name leads to 50 percent fewer purchases. Customers also lag in responding, are less expressive, and avoid discussing purchases. We show similar results for customers around the world and across workers. Removing customer bias, we find women would be more productive than their male co-workers.

JEL Classification: J16; O12
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1 Introduction

Many workers are evaluated on their ability to engage with customers. If customers have explicit or implicit biases against workers of a certain gender or race, then workers of this identity will perform less well. While each instance of customer bias may be unremarkable or unobserved, the accumulation may take a toll, shaping a worker's observable productivity (Basford et al., 2014; DeSouza et al., 2017; Nordell, 2021). Given that firm hiring, pay, and promotion decisions all depend on worker productivity, quantifying the magnitude of discriminatory customer behavior is crucial for understanding an important source of identity-based discrimination and how it shapes workers' labor market outcomes (Becker, 1957). It is also a relevant input for firms that may want to identify and promote the most talented workers rather than those least subject to bias, and for regulators who may wish to identify whether policies like performance-based pay promote discriminatory outcomes. While recent papers have shown how on-the-job discrimination can affect worker outcomes (Glover et al., 2017; Sarsons, 2022; Egan et al., 2022), we know very little about the magnitude or impacts of customer discrimination in the workplace.

Identifying the impact and scale of customer discrimination is challenging. First, measuring productivity in many jobs is prohibitively challenging or subjective. Second, even when measurable, many factors affect worker productivity—e.g., the worker's own skills, customers' behavior and preferences, and the workplace environment—which makes it difficult to isolate the impact of any single determinant. Third, identifying discrimination typically involves testing whether wages or hiring outcomes are different across individuals *who are equally productive*. In doing so, these tests fail to account for discrimination that directly impacts productivity, such as consumer behavior. Consequently, they may erroneously conclude there is no discrimination at all.

This paper directly addresses these challenges by running the first randomized field experiment on customer discrimination for workers within a firm. We do so by partnering with an online travel agency whose offices are scattered across Sub-Saharan Africa. The company sells flights and hotels and hires local sales agents to assist their customers. Specifically, we study over 2000 customers from 70 countries (87% from Africa, 13% abroad) as they chat with online sales agents who answer their questions and help them make purchases. This context allows us to precisely measure worker productivity through sales records and document rich patterns of customer engagement, including bargaining and harassment, through chat transcripts.

We apply a novel framework for estimating the causal effect of customer-based discrimination. First, the names of workers—and implied genders—were randomized daily, providing plausible variation in customer beliefs about the gender of the agent they were chatting with.[1] Customers could only infer agents' gender from their names, as they did not receive any other information about the agent. Second, workers were unaware of their assigned name due to a web plugin that masked the assigned name from their view. This step ensured

[1] Changing the names of workers appears to be relatively common in online sales settings (e.g., LiveAgent).

that agents' behavior was not directly affected by their name assignment. Consequently, any change in consumer behavior towards sales agents could only occur if consumers responded to the randomly assigned names.

This research design is unique and overcomes challenges with two common experimental methods to study discrimination: audit and correspondence studies. Audit studies—in which actors, who are as similar as possible except on one dimension, engage in a task like applying to the same job—struggle to control for all other differences between the actors. They may also be subject to "demand effects" because actors are aware of their treatment status and do not have real incentives to perform well. Correspondence studies—in which fictitious applications with different sounding names are sent to a possible discriminator like employers—can only measure indirect outcomes such as job application callback rates rather than actual job hiring (Bertrand and Duflo, 2017). In our setting, the daily name randomization eliminates omitted variable concerns, the name masking alleviates concerns about demand effects, and by studying real workplace interactions we can collect the ultimate outcome measures of interest—e.g., the likelihood of purchase—and specifics of the interaction—e.g., customer engagement with the sales agent.

The results are striking: we find that randomly assigned female names reduce the likelihood that customers make any purchase, the number of purchases, and the value of the purchases. Specifically, the likelihood of any purchase decreases by 3.8 percentage points, or 50% relative to the baseline purchase rate (7.6%). We observe similarly large reductions in the total number of purchases, the total value of purchases, and the average purchased price conditional on any purchase. To confirm that our treatment effects result from the implied gender of agents' assigned names, we show that customers are aware of agents' names as they are frequently mentioned in conversation. We further show that these results are not driven by our choice of specification, nor by other factors correlated with names.

Multiple mechanisms could explain why customers discriminate against female workers. We consider several: general customer disinterest in working with female agents, differential bargaining, or overtly negative interactions. Data from the evolution of agent-customer chat interactions suggest that customer disinterest is the most likely channel. Initially, we find that consumers respond more slowly to female agents, only responding after receiving additional messages from the agent. Conditional on ever responding, customers are less expressive to workers with female names and are less likely to transition from initial topics of conversation (e.g. a price inquiry) to discuss making a purchase. Consistent with this continued lack of engagement over time, we observe similar proportional reductions in purchases for customers who never respond as those who stay for longer conversations. This finding is at odds with simple predictions of statistical discrimination, as discrimination based on incorrect beliefs about worker quality should attenuate with more experience with the workers.

Additionally, the data do not support other possible mechanisms. We find no evidence that consumers differentially bargain when agents receive female-sounding names and no differences in hostile or harassing behavior, although any form of harassment is rare in this context. Together, the results appear to be most consistent with literature in sociology that highlights the importance of subtle and often unconscious gender bias in the workplace (Basford et al., 2014; DeSouza et al., 2017; Nordell, 2021), rather than more overt forms of discrimination such as bargaining (Castillo et al., 2013; Card et al., 2016; Rousille, 2021) or harassment (Georgieva, 2018; Folke and Rickne, 2020; Dupas et al., 2021).

What are the implications of these results for the labor market? While field experiments studying discrimination can measure the magnitude of discrimination by firms or customers, they are commonly criticized for not being able to assess whether this discrimination ultimately matters for workers (Heckman, 1998). The seminal work of Becker (1957) shows that workers may sort away from discriminating firms or customers in equilibrium, and hence what matters is not whether some discrimination exists in the market but whether it is actually experienced in equilibrium. A unique feature of our experiment is that we can quantify the amount of discrimination experienced by employed workers. If workers could perfectly sort away from discrimination, we should not see any women in these jobs that feature substantial customer discrimination. Surprisingly, the opposite is true. The vast majority of workers in this role are female, indicating important amounts of discrimination faced in equilibrium.

Why would the firm hire female workers who are being subjected to customer bias, and why would workers choose to stay? Comparing male and female agents in a non-experimental sample, we find they are equally productive on average (inclusive of customer bias), and hence the firm is not losing out by hiring female workers at similar wages to men. These results are consistent with the market outcome of a non-discriminatory firm that has internalized discriminatory customer preferences. It is only by running our experiment that we observe that female employees are more productive than their male counterparts absent customer bias. From their side, these female workers may choose to stay in these jobs for two reasons. First, they may be constrained in their outside options, with other occupations exhibiting worse discrimination. Second, they may have comparative advantage or derive non-pecuniary benefits from this occupation. Either way, this experiment points to important constraints that prevent workers from sorting away from discrimination.

By running a novel experiment within a firm, we view this paper as a proof of concept that customer discrimination can meaningfully affect worker productivity in ways that often go unnoticed by firms and econometricians alike. However, as a result of conducting our experiment in a real-world setting, the results may be a product of the type of job, the customers, or the workers that comprise our sample. Nevertheless, we view our results as being more broadly representative for the following reasons. First, the customer sales jobs we study are typical in the service industry: the workers engage in straightforward, non-technical sales and customer assistance for an established firm. Second, we find similar rates of gender-based discrimination from customers in different parts of the world, both inside and outside Africa, suggesting common effects across different customer bases. Finally, we provide evidence that our results do not depend on worker characteristics: the effects for all workers are negative, and we cannot reject they are equal.

This paper makes three contributions. First, we provide the first causal evidence that customer discrimination lowers the measured productivity of female employees in the workplace by a meaningful margin, and in ways they cannot avoid. Studies on the impact of customer discrimination are scarce and find mixed evidence (Kahn and Sherer, 1988; Nardinelli and Simon, 1990; Holzer and Ihlanfeldt, 1998; Leonard et al., 2010; Combes et al., 2016; Bar and Zussman, 2017).[2] Recent work by Kline et al. (2022); Hurst et al. (2021) finds evidence that employer discrimination and racial wage gaps are higher in customer facing roles, suggesting a role for customer bias. We build on this literature in two key ways. First, we use an experimental design to isolate customer discrimination rather than methodologies that may not fully control for differences in unobservable characteristics of workers and customers. Second, we can measure worker productivity rather than relying on wages or hiring outcomes. This provides new insights in how customer discrimination affects workers and for how tests of discrimination may be biased by assuming fixed worker productivity.[3]

Our results also contribute to a growing literature that shows how discrimination can directly impact productivity (Parsons et al., 2011; Hengel, 2022) and how this discrimination cascades to affect other outcomes such as hiring, pay, and promotion decisions (Hull et al., 2023; Glover et al., 2017).[4] Hull et al. (2023) provides an analytical framework to describe "systemic discrimination," specifically how discriminatory outcomes do not just arise from direct interactions but also from responses to past or expected future discrimination by other actors. We find that discrimination by customers leads the firm to only retain women who are more productive than their male counterparts after accounting for bias.[5] While the firm may not be aware of this bias, the ultimate outcome is discriminatory because women have to be more productive to earn the same wage. This is consistent with Glover et al. (2017), which finds that manager bias makes black workers less productive on the job, and this discrimination ultimately causes the firm to only hire relatively more productive black workers. Customer discrimination is a likely more difficult problem for a firm to solve as this result does not occur as a result of internal firm policies but external (customer) forces.

Third, this paper contributes to a large literature investigating explanations for differences in earnings and employment between men and women in the labor market. Classic decompositions of gender earnings gaps split explanations into productivity-based factors (e.g., human capital, experience), discrimination, labor demand, and other drivers (Blau and Kahn, 2017; Caliendo et al., 2017; Gallen et al., 2017; Sin et al., 2020). Our results demonstrate that underlying customer-based discrimination can contribute to productivity differences,

[2] This paper is also related to a literature on how customer discrimination affects goods sellers in marketplaces or product markets through audit or correspondence-type studies (List, 2004; Doleac and Stein, 2013; Ayres et al., 2015; Kricheli-Katz and Regev, 2016). While this literature has found more evidence of discrimination, the implications for how these results translate to workers and the labor market is unclear. We study customer discrimination in a real labor market setting among individuals who have selected into this job based on their comparative advantage and test how this discrimination relates to labor market sorting.

[3] Nardinelli and Simon (1990) is an exception. They define the productivity of baseball players to be their entertainment value, observed through the price of baseball cards.

[4] This paper also contributes to literatures on bias in student evaluations of teachers (MacNell et al., 2015; Mengel et al., 2019) or by audience members for academic economists (Dupas et al., 2021). Recent work has also shown the importance of patient racial preferences for doctor race (Alsan et al., 2019).

[5] This is consistent with the Hull et al. (2023) definition of future-in-present discrimination.

which could lead decompositions of the gender wage gap to underappreciate the importance of discrimination and overestimate the role of productivity.[6] Our work also complements important work investigating the effects of discrimination conditional on equal worker productivity (Goldin and Rouse, 2000; Sarsons, 2022). In contrast to these papers, our results imply that policies that seek to equalize outcomes for men and women of the same productivity (e.g. blind auditions as in Goldin and Rouse (2000), or equal pay for equal work policies), will not eliminate the gender gap and create a level playing field.

Identifying the role of customer discrimination is relevant for any country with a booming service sector where customer-facing roles abound. As the share of women in the service industry continues to increase exponentially in Sub-Saharan Africa, finding ways to prevent discrimination in customer facing roles is particularly important. It is also particularly relevant in this context because barriers to female labor force participation are meaningful.[7] The gender inequality index is high in Sub-Saharan Africa (UNDP, 2022) and issues of gender inequity in the workplace have become a central policy goal for governments across the continent and international institutions alike (World Bank, 2011; O'Donnell et al., 2020). Policymakers must be aware of this source of discrimination when considering different labor market policies.

The paper is organized as follows: section 2 details the context, the company we work with, and the data, section 3 describes the empirical strategy, section 4 presents the results, section 5 discusses the labor market implications, section 6 addresses external validity, and section 7 concludes.

2 Context

2.1 Service sector in Sub-Saharan Africa

We study consumers' discriminatory behavior when engaging with customer sales representatives. These workers engage with customers to answer queries and make sales. This is a common job profile in the service industry: most major companies across the world have sales departments with customer-service agents who assist customers from all over

[6] Blau and Kahn (2017) note that decompositions of discrimination may be understated if "some of the explanatory variables such as experience, occupation, industry, or union status have themselves been influenced by discrimination—either directly through the discriminatory actions of employers, coworkers, or customers, or indirectly through feedback effects."

[7] A variety of other barriers for female employment in low-income countries have been explored. For literature on norms and bargaining dynamics within the household see (Dean and Jayachandran, 2019; Bursztyn et al., 2020; Heath and Tan, 2020; Field et al., 2021; Lowe and McKelway, 2021; McKelway, 2021a,b); workplace attributes (Subramanian, 2021), safety during commutes (Borker, 2021), market demand (Hardy and Kagy, 2020), discrimination in the workplace (Duflo, 2012; Jayachandran, 2015; Sin et al., 2020; Delecourt and Ng, 2021). Delecourt and Ng (2021) uses an audit-study approach to show that customers do not discriminate against female-led small business vegetable sellers in India. Our study differs in three ways. First, our research design allows us to overcome some of the limitations of audit studies by fully controlling for all agent characteristics. Second, our experiment uses real workers in jobs they will continue to have after the experiment is over, which factors in the selection processes for hiring workers and worker incentives. Third, our experiment sheds light on the implications of customer discrimination in the labor market as a whole.

the world. The industry is also dominated by women.[8] In Sub-Saharan Africa, where we work, the number of customer-facing roles are increasingly common across the continent as service sector jobs increases. For example, the share of working-age individuals employed in services throughout Sub-Saharan Africa rose 12% from 2011 to 2019 (World Bank, 2022). Women largely drove these trends: the share of working-age women employed in services increased by 16% over the same period—currently at 39.7%.

This trend will likely persist as internet connectivity spreads across the continent and service-sector jobs increasingly interface with customers online, both within and outside Africa. In 2010, only 8.3% of the population in Africa had internet access. By 2017, internet access had increased to 22.3% (World Bank, 2022). Online shopping, in particular, has increased 18% annually between 2014 and 2017 (UNCTAD, 2018) and estimates suggest that almost 50% of digital buyers in Africa are female (Statista, 2019). The COVID-19 pandemic has likely accelerated these trends as consumers increasingly head online. Reflecting this growth and importance: in 2020, the value of African e-commerce was estimated at 20 billion USD—a 42% increase over 2019 (IFC, 2021).

2.2 Company and study details

We evaluate an experiment at an online travel agency with offices located across Sub-Saharan Africa—we work primarily with an East African field office. For confidentiality reasons, we cannot provide any identifiable information about the company. The company sells flights and hotels primarily for trips to different parts of the continent (the average price of a flight/hotel conditional on making a sale is 140 euros). Customers make purchases on the platform, or engage with local sales agents to ask questions, make complaints, and assist with purchases (using phone calls and online chat interfaces).[9]

In this study, we analyze over 2,000 customer interactions from 70 countries (87% of customers come from Africa, and 13% come from abroad). The company enlisted six agents in the experiment, all of whom were female. In the company overall, approximately two-thirds of the sales agents are women, and these women account for 83% of chats. These sales agents are full-time company employees, and receive an annual wage.[10] Their job description resembles a characteristic customer sales role insofar as sales representatives engage with customers and search for products in their company's catalogue to match customers' requests.

The company provides sales agents with a chat interface to interact with customers. Customers can initiate interactions with sales agents by clicking on a chat button at the bottom of the webpage. Clicking the chat button reveals a chat window displaying the agent's first name and a short greeting message. Thus, agents always send the first message; either the agent or the customer can send subsequent messages as the conversation evolves.

[8] Zippia (2021) estimates that 70% of customer service industry jobs in the US are held by women.
[9] 78% of conversations have an initial chat purpose of general inquiry, price inquiry, or explicitly about making a booking.
[10] We do not have access to other information about agents' demographics or wages within the company.

The company was keen to partner with the research team to investigate whether they could optimize this chat/sales interface. This particular test aimed to identify how customer behavior changed with respect to agents' identities—specifically when agents were assigned male- versus female-sounding names. To this end, the company needed to (1) randomize whether the name appearing in the chat implied a male or female identity and (2) ensure agents were unaware of their assigned names. The randomization was correctly implemented: a software program pulled one name per sales agent per day from an existing list (with replacement) and assigned it to the agents' chat interface. A local field team compiled the list of names by drawing 1,198 names from local school yearbooks and assigned each name an implied gender, ethnicity, and whether it is a common English first name.[11] To limit the customers' inference of other dimensions of agents' identities besides gender, the interface only included agents' (randomly assigned) first names.

Next, to ensure that agents could not see the names that were assigned to them, a web plugin was designed to omit the agent's name from the agent-facing interface. The company installed the plugin on each agent's internet browser with oversight from our field team. The plugin symbol was removed from the list of visible extensions—appearing as a light grey square when all browser extensions were listed. The plugin worked in the following way. Consider a day when agent *James* (real name) was assigned the name *Steve*. Whenever the customer typed "Steve" into the chat, James would only see "Agent" in his chat window. In contrast, the customer would still see "Steve." This name masking included any references to the agent's assigned name in the chat transcript.[12]

The experiment was launched in January 2019 and the name assignment continued until October 2019. The company had full discretion over which agents participated in the study. They included five agents based in an East African office, and an agent from a West African branch.[13] The company informed agents that it was interested in learning more about how customers respond to different agents and may change agents' display name in the chat. No further information about the nature or the objectives of the experiment was provided by the company, including the focus on gender. Agents did not ask any follow up questions throughout the duration of the experiment, and the company made no additional requests of agents (in terms of protocols/procedures to follow). It is unlikely that knowledge of the experiment would have affected agents' behavior as the study was never discussed in any subsequent team meetings. Even if agents thought the experiment was about gender, it is unclear how this would affect their behavior as the name assignment changed daily without their knowledge.

[11] Of the 1,198 names, there are 1,196 unique full names, and 579 unique first names. Of these, 267 were female-sounding and 322 male-sounding. Table A1 lists 20 example names, by gender and whether they are English-sounding or not (59% of male names were non-English sounding, while 47% of female names were non-English sounding). We show the difference in name characteristics across gender does not affect our estimation of customer gender bias in section 4. We do not have access to local data sources like birth certificate records or a census to validate how common these names are for men or women.

[12] The vast majority of interactions occurred in English, limiting concerns about gendered identifiers. Other gendered identifiers like "Sir" or "Miss" are rarely used in 1.3% of chats, and we show the results are unaffected by excluding any days when this occurs.

[13] The company selected all agents that were consistently working at the time at these two offices.

2.3 Data

The analysis relies on two sources of administrative data. The first dataset records every purchase made by customers, including the sale amount. The second dataset contains the agent-customer chat interactions: the full chat transcript, a timestamp for each message, and the customer's country.[14] The sales data were matched to the chats using date and customers' IP address.

To measure overall purchases, we include purchases directly made by customers and purchases made by agents on behalf of customers. When customers purchase a product themselves, the sale is recorded in administrative sales records. When agents input customer details and purchase products on their behalf, we can only capture the sale by reading through the chat records and flagging instances when agents send final purchase confirmation details to customers. Customers then pay separately or at the time of receiving the order. When agents make purchases, we cannot measure purchase values. We therefore only measure the total value of purchases using the administrative sales records.[15]

From the chat transcripts, we create objective and subjective outcome measures.[16] Objective measures do not require human interpretation—for instance, whether a purchase occurred. Subjective measures represent outcomes that require human interpretation of the chat content, and include our team's assessment of the primary and secondary purpose of the chat, the overall tone, whether customers bargained with agents (e.g., asking for a discount), or whether customers harassed agents. Enumerators familiar with the cultural context hand-coded these subjective outcomes; 20% of the observations were double coded to ensure consistent measurement. Enumerators did not know the gender assignment of the agent.

Agents' jobs involved several sales-related activities, including assisting customers via online chat and phone. Each agent worked the chat interface six weekdays per month on average. Table 1 shows that on days when agents responded to chats, they spent 2.6 hours on the online sales interface with customers, engaging in approximately 8 unique chat conversations per day. Additionally, the average chat lasted 22 minutes and contained 73 words. The sales agents did not all work during the full study period for institutional reasons, although they all worked a majority of the time.

We restrict our sample in two ways. First, we limit our sample to normal working days to avoid time periods associated with testing and developing new chat and purchase features at the company. Second, we restrict our sample to observations with five or fewer previous purchases as some users may have accessed the site using non-unique locations—e.g., public

[14] We only know customers' approximate locations—we do not have access to any customer demographic data. However, estimates from other sources suggest that in 2019 nearly 50% of digital buyers in Africa were female (Statista, 2019).

[15] Our sales measure also includes the small set of chats (3%) that were transferred to the phone to complete a sale. There are no discernable patterns among the set of chats that are transferred: they are equally balanced across female and male-sounding agent names, and they do not appear to be transferred for one particular reason over another.

[16] The variables are further described in Table A2.

areas or businesses—and hence their purchase records were not well-linked.[17]

3 Empirical strategy

The design of this study overcomes two major challenges to identifying the causal effect of customer-based gender discrimination. First, daily randomization of agents' names ensured customers were randomly exposed to female- or male-sounding names. This separates unobserved factors that correlate with gender from customers' perceptions of gender. Second, agents were not aware of the name consumers see—any revelation of the agent's name during the chat was masked automatically by a computer program and was not seen by the agent.[18] Therefore, agents' behavior cannot directly respond to the randomized name—only to customers' responses to these names. Together, these elements allow us to test for customer-based gender discrimination.

Treatment assignment occurred as follows. Agents were randomly assigned 'male' or 'female' each day (with replacement) with equal probability.[19] Given the selected gender, a specific name from the name database was randomly selected. This procedure occurred every day of the study period. The number of agents working varied daily. Some days, only one agent worked; other days, multiple agents operated the chat. Customers were allocated programmatically to agents; neither agents nor customers had any choice over who they were matched with.

Using this randomization, we estimate the effect of customer discrimination on worker productivity. Our main specifications take the following form:

$$y_{iam} = \beta\, \mathbb{1}[\text{Assigned female}]_{ia} + \gamma_{am} + \varepsilon_{iam}$$

where y_{iam} is the outcome of interest for customer i, working with agent a, in month m. The indicator $\mathbb{1}[\text{Assigned female}]_{ia}$ is 1 if agent a (matched to customer i) is assigned female on that date. The term γ_{am} represents agent by month-of-sample fixed effects. We augment this regression specification to estimate individual-agent treatment effects and heterogeneity across customer characteristics.[20,21]

Agents worked different times, in different locations, and some only worked part of the sample period, implying that agents themselves are not explicitly randomly assigned to customers. While not strictly necessary, since treatment assignment is uncorrelated with cus-

[17] Only 2% of observations come from IP Addresses with five or more purchases.

[18] See subsection 2.2 for details.

[19] Due to variation in the number of customers arriving on a given day and sampling variability in treatment assignment, 47.6% of customer-agent interactions occurred with a female sounding name.

[20] Note the regression can analogously be run at the (grouped) agent-day level after collapsing and reweighting to exactly match our customer (microdata) approach (Angrist and Pischke, 2008). The focus of the paper on customer behavior and the parsimony of the current approach motivates the customer-level analysis.

[21] We cannot estimate heterogeneity by customer gender as we do not observe customer demographics. We don't view this as a limitation. This paper identifies the *absolute* customer bias workers face in their actual job, which is the key parameter of interest when considering the impact of customer discrimination. We cannot test for *relative* customer bias depending on customer characteristics which has been a focus of other papers (Leonard et al., 2010; Combes et al., 2016; Bar and Zussman, 2017).

tomer type, our main analysis restricts comparisons between similar customers using agent by month-of-sample fixed effects. Specifically, the research design compares (1) a consumer who chats with agent a in month m on a day when the agent was assigned female to (2) a consumer chatting with the same agent in the same month when the agent was assigned male. We show our main results are robust to a range of alternative model choices detailed in subsection 4.3.[22]

Customers may have multiple interactions with agents on the same day if they are disconnected or return to ask additional questions. We account for this possibility in two ways. First, we two-way cluster our standard errors at the agent-day (the level of randomization) and customer-day levels. Second, we assign the customer the treatment status of their first chat of the day. This circumvents the possibility that customers can affect their treatment status by returning to chat with an agent of a different gender.

We validate the randomization procedure in Table 1. In column (3) of this table, we regress observable customer characteristics (e.g., number of past purchases) and agent characteristics (e.g., number of daily chats) on an indicator for whether the agent received a female name, using our main specification.[23] Female assignment does not correlate with any customer or agent characteristics at the 10% level. We fail to reject the joint null hypothesis that each of these effects is zero ($p = 0.59$). The table includes an additional row that identifies whether the customer mentioned the agent's actual name. This event occurs very rarely (mean is $<$0.01), validating the name assignment procedure, and likely results from agents' names coincidentally matching a topic in the chat.

4 Results

4.1 Main purchase outcome

The experiment aims to identify the impact of gender on consumer behavior. This strategy requires that consumers pay attention to agents' assigned names. We confirm that customers notice agents' names by measuring how often consumers use agents' assigned names in chats. This test provides a lower bound for consumers' awareness of agents' names—and likely the names' implied genders. In our study sample, customers use agents' assigned names in 7% of all chats and 11% of chats in which consumers ever respond to agents' initial messages. We interpret this as a relatively high share of customer awareness as many chats are brief and mentioning a person's name is unnecessary. Thus, agent names are indeed salient in chat interactions and could affect customers' behavior.

Table 2 presents the estimated effects of female-name assignment on outcomes related to customer purchases. We measure purchases within 24 or 48 hours of the chat to capture behavior plausibly related to the chat interactions rather than unrelated interactions that

[22] Including agent and month fixed effects separately does not necessarily produce clear comparisons because not all of the agents worked every month.

[23] The number of daily chats by an agent in a day could potentially be affected by name assignment if labor supply or hours worked changes. In practice, since customer allocation is done programmatically there seems to be little room for endogenous response along this margin or more simply, labor supply may be unaffected.

happen later.[24] We measure purchases in two ways: making any purchase and the number of distinct purchases.

We find that consumers assigned to agents with female names are less likely to purchase products on the website. Column (1) shows that female-name assignment decreases the probability that any purchase occurs (within 48 hours) by 3.8 percentage points ($p = 0.003$). The likelihood that a chat results in any purchase in the control group (male-sounding names) is only 7.6%.[25] Thus, the point estimate implies a 50% reduction in the likelihood of making a sale. Column (2) shows that consumers also purchase 0.038 fewer total products ($p = 0.005$) when interacting with female-sounding names. Columns (3-4) repeat the same outcomes but use a 24-hour window after the chat. The results are very similar.[26]

Female name assignment also translates into lost revenue. Table A3 shows customers assigned to agents with female names reduced the total value of their purchases by 60% (column 1), stemming from fewer purchases (discussed previously) and a 36% reduction in the average purchased price conditional on any purchase (column 2).[27] The reduction in average purchased price conditional on any purchase is another interesting potential indication of discrimination, although this result could be driven by either high value purchasers being less likely to purchase or similar customers purchasing lower price products.

These results highlight the importance of customer-side discrimination in productivity differences between women and men in the workplace (for consumer-facing roles). Prior research on the gender wage gap suggests women receive lower pay partly because they are less productive (Blau and Kahn, 2017; Caliendo et al., 2017; Gallen et al., 2017; Sin et al., 2020). We show that discriminatory behavior—on the part of consumers—can drive these productivity differences. In our context, for women and men to have similar productivity levels, women would need to overcome significant barriers created by consumers' behavior. These results also suggest that piece-rate wage structures—i.e., rewarding employees for their output levels—could further workplace inequality.

4.2 Mechanisms

Why do customers discriminate? We use the richness of our data to explore three potential mechanisms that may explain why purchases fall when consumers chat with female agents. First, customers may be hesitant to engage with female sales agents. For instance, customers may dislike working with women or believe women are less efficient at helping with pur-

[24] Statistical power is also likely to be higher in the period directly after these events.

[25] We calculate control group means accounting for agent-month fixed effect cells $c \in \mathscr{C}$ as $\sum_{c \in \mathscr{C}}(E[Y|C = c, D = 0)w_c$ for weights w_c, cell C, and treatment status D. We do so to correspond to the OLS estimand, $\beta^{OLS} = \sum_{c \in \mathscr{C}}(E[Y|C = c, D = 1] - E[Y|C = c, D = 0)w_c$.

[26] We also test for dynamic effects of female name assignment. We do not find evidence for this; the p-value of the joint test of the assignment to a female name in the previous two working days does not reject the null hypothesis of no effect either individually or jointly ($p = .377$).

[27] The total-value and price measures only include purchases by customers and not purchases by agents on behalf of customers. Interpreting these measures is consequently more challenging because the mode of purchase is potentially endogenous to treatment. In practice, we find treatment status affects purchases by agents on behalf of customers and purchases made directly by customers in the same magnitude and direction (see Table A7). This means we likely *underestimate* the coefficient on total price.

chases. Second, recent work suggests women are more likely to face harassment and verbal abuse on the job (Georgieva, 2018; Folke and Rickne, 2020; Dupas et al., 2021) and may face different bargaining processes (Ashraf, 2009; Castillo et al., 2013; Card et al., 2016; Vesterlund, 2018; Rousille, 2021).

Engagement The data suggests that the first mechanism is driving the results: customers are hesitant to engage with female agents. We investigate this along the extensive and intensive margin of the conversation. On the extensive margin, we investigate whether the customer engages with an agent at all, as some consumers may be hesitant to chat with female agents or may entirely avoid female agents. On the intensive margin, we investigate what customers discuss, and the tones they use to express themselves. Note that we use these two measures as imperfect proxies because customer engagement is impossible to measure directly. This means we are likely to miss some changes in customer engagement and we do not expect that the magnitude of our measured effect on this mechanism will be able to explain the entirety of the main sales effect. Columns (1-2) in Panel A of Table 3 show the effect of female-name assignment on *extensive margin* consumer interactions. Mechanically, agents always send the first message; the conversations begin there. In column (1), female assignment leads to a negative albeit statistically insignificant effect on the likelihood the customer ever responds ($p = 0.322$). However, agents can send multiple messages to customers to encourage their response, which means that measuring a binary variable of any response by the customer may not fully capture a lack of engagement. Column (2) shows that female-assigned agents send more messages before receiving a response ($p = 0.034$), consistent with lower customer engagement (higher hesitance). We further investigate customer hesitancy along the *intensive margin* in two ways. First, we analyze the conversations' tones. While specific tones are likely imperfect proxies for genuine emotions, whether a customer expresses any tone may reflect a customer's level of engagement with the agent. To this end, we construct a measure for any non-neutral tone detected in the conversation. Column (3) in Panel A of Table 3 indicates a 3.1 percentage point reduction in the probability of any tone used when customers engage with female-assigned agents, a 35% reduction relative to the control-group mean ($p = 0.032$).

Second, we investigate whether the topic or purpose of the chat changes when customers believe they are talking to a female agent. The results are presented in Panel B of Table 3. As outlined in subsection 2.3, we hand-coded every chat transcript and categorized the primary and secondary purpose of the chat into 4 groups (general inquiry, price inquiry, making a booking, or other).[28] Column (3) shows that female-name assignment does not affect the probability that the primary purpose of the chat is to make a booking.[29] However, column (4) indicates the probability that the secondary purpose of the chat transitions to discussing making a booking falls by 5.7 percentage points (22%, $p = 0.07$). Column (5) shows this effect is much more pronounced among customers who were coming to talk to agents about

[28] Other includes less frequent events like confirming, cancelling, changing a booking, reporting a complaint, and random/unknown reasons.

[29] The treatment status also does not affect the likelihood of other initial topics (see Table A4).

a general or price inquiries than other topics: the likelihood of transitioning to making a booking falls by 10 (14.5) percentage points for customers who initially had a general (price) inquiry. Instead of discussing a booking, conversations with female-name agents simply never transition to another topic; column (6) shows conversations with agents assigned female names have similar-sized reductions in the likelihood of any secondary conversational purpose. Together, the results provide evidence for initial hesitance, less expression, and a lower likelihood of transitioning towards the ultimate goal of purchasing by customers. All of these responses point to underlying biases that prevent customers from engaging a female agent in the same way they would a male agent.

Finally, we test whether the consistent lack of customer engagement over the course of the conversation translates to similarly consistent reductions in purchase records.[30] Table A5 presents regressions of sales for customers split by whether the customer never responds and terciles of conversation length based on number of words for responders. Even if the customer never responds, we find a 31% reduction in purchases when assigned a female named agent; customers who converse the longest reduce their purchases by 53%.[31] These rates are fairly comparable across groups, suggesting hesitancy to engage with females even for customers who spend less time working with the sales agents. The results also suggest that engaging and becoming more familiar with the agents does not help attenuate the discrimination. This result provides some evidence in favor of taste-based and against statistical discrimination as we might expect customers to discriminate less as the conversation progresses and they learn more about women's ability.[32]

Other Mechanisms We also investigate whether customers are more abusive or more likely to bargain with women. The results in column (4-6) of Panel A in Table 3 suggest these mechanisms do not explain the differences in sales in our setting. Column (4) measures whether any language is classified as harassment within the chat. The data contain few instances of harassment: 0.3% of conversations for the male-assigned (control) sample indicate harassment. The rate in the female-assigned sample is practically identical to the male-assigned sample and does not differ statistically. Using a broader definition of any negative wording, we find no evidence for this mechanism either (column 5). Finally, column (6) tests whether customers bargain more often with female sales agents. While 14% of chats exhibit some bargaining behavior—for example, asking for discounts on the listed price—we find no significant effect of female-name assignment on the likelihood of bargaining. This null result rules out meaningful changes in the amount of bargaining faced by women. Therefore, in this context, differential bargaining does not appear to drive the observed productivity differences.

Together, our results show that customers interact differently with women and men in

[30] Conversation length is endogenous to treatment assignment, so we view these results as suggestive.

[31] Note we have less statistical power for never responders given lower purchase rates.

[32] Though disentangling whether taste or statistically based discrimination drives these impacts is not the intent of the study because it does not affect the policy response. Unlike taste-based employer discrimination, customer discrimination, regardless of whether it is taste based or statistical, will not be competed away through more market competition (Becker, 1957; Bartlett and Gulati, 2016).

ways that can meaningfully reduce productivity. This result is especially consequential for the service industry, where customer-facing roles abound. Our investigation of the mechanisms behind this behavior suggests that consumers engage less with female agents—along extensive (any engagement) and intensive (tone and topic) margins.[33] This speaks to an active literature in sociology that explores the impact of subtle and unconscious bias in the workplace. This body of work highlights that while more blatant forms of sexism may be on the decline, the incidence of more subtle forms of prejudice may be rising (Basford et al., 2014; DeSouza et al., 2017).

4.3 Robustness and interpretational confounds

Robustness Our main results are robust to various analysis choices. First, the results are almost identical when we aggregate to the customer-day level (Table A6). Second, they are robust to how we define our outcomes (Table A7). Specifically, the effects are remarkably similar when looking at purchases from chat-based records (via agents, column 3) or from administrative records (via customers, columns 4-5). Third, our results are also unaffected if we exclude any of the relatively few days when customers use a gendered identifier (e.g. Sir or Miss), which could potentially reveal the treatment assignment to the agent (columns 6-7).

Finally, Table A8 shows the results are robust to concerns about correlated treatments and the specification we use. One potential concern with varying name assignment is that we are measuring a factor correlated with gender. The most salient other feature in this context is ethnicity, which a priori is unlikely to be important since only first names are shown. To further support this, column (2) shows that our results are unaffected by directly controlling for name ethnicity as fixed effects, alleviating this concern.[34] Regarding specification, the results are quantitatively and qualitatively similar when we include additional customer controls for past purchases, customer location, and customer chat history (column 3), add day-of-week (column 4) or week fixed effects (column 5), include agent and month fixed effects separately (column 6), only have date fixed effects (column 7), and remove all fixed effects and controls (column 8). The effect of female status leads to proportional reductions of 41-58% across all specifications.

Interpretational confounds Agents may have certain gender-specific language that could appear strange to consumers when assigned the opposite gendered name. For example, a male agent may use specific language that could confuse a customer who assumes they are speaking with a woman because of their female-sounding name—and this may reduce the chance of a sale. In practice, this is unlikely given the short and straightforward conversations we study. Still, could customers be responding to a "mismatch" between the gender

[33] We are unable to test homophily (a customer's preference to interact with an agent of the same gender) because we lack sufficient customer information.

[34] Ethnicity is coded by a field team based on full name. We assign name ethnicity based on the full name, although only first names were shown. There are 17 ethnicities in the data.

implied by agents' assigned name and the agents' actual gender? It is unlikely that this confound explains our results because every agent in our experimental sample is female, and could only potentially 'confuse' a customer with their language when they are assigned a male-sounding name. However, because we find that being assigned a female name reduces the likelihood of a sale, any 'confusing' behavior from a male-sounding name may attenuate our estimates. Furthermore, our results are unlikely to be different for a sample of male agents as we find customers have similar proportional reductions in their purchases even if they never respond or have very short conversations and hence little interaction with the agents (see Table A5).

5 Labor Market Implications

In the previous sections, we found customers are less likely to make purchases when working with agents with female-sounding names. While discrimination that reduces female worker productivity in this occupation suggests negative impacts on worker well-being, how this observed bias affects workers ultimately depends on the labor market (Becker, 1957). If workers can sort away from the customer bias in this occupation into another equivalent job without customer contact, for example, there may be no consequences for worker welfare. In this firm, as is common in the customer service industry, the vast majority of workers in this role are female. Two thirds of workers at this firm are female, and women handle 85% of chat conversations. This indicates that equilibrium labor market sorting is not reallocating workers away from this observed, important source of discrimination.

Why might this be? Sources for the lack of sorting could arise either from the firm side or from the worker side. It's possible that firms are unaware of these productivity differences and simply hire female workers who are lower productivity as a result of customer bias. It's equally possible that female workers face other constraints to taking outside occupations.

To try to understand whether female workers at this firm are less productive than their male counterparts as a result of customer bias, we compare the results from our experimental research design to a simpler non-experimental comparison of male and female agents. The non-experimental results measure correlations between chat purchases and agents' actual gender using chats with over 7,000 customers *outside* the experimental sample.[35] We include office by month-of-sample fixed effects in the non-experimental regression models to limit comparisons between male and female workers working in the same location, with similar customers, over the same period.

Table 4 shows correlations between agents' actual gender and sales in Panel A and experimental estimates of female name assignment in Panel B. In Panel A, we find no economically or statistically significant differences between male and female agents across any of the purchase outcomes. In Panel B, we reproduce our main experimental estimates for comparison—we find female name assignment leads to statistically significant reductions in

[35] The experimental sample is not representative of all sales at the company. Therefore, this exercise is only suggestive.

sales across all purchase outcomes. To compare the effect of female-name assignment in the experimental sample to the effect of being female in the correlational sample, we use seemingly unrelated regression. A test of equality across the two 'female' coefficients rejects the null hypothesis at the 5% level for any purchase and number of purchases within 48 hours and at the 10% level for outcomes within 24 hours.[36]

The difference between the experimental and correlational estimates sheds light on gender-based selection into this occupation. In particular, it suggests women in these jobs may be more productive than their male counterparts in the absence of customer discrimination. This is consistent with an equilibrium outcome in which males and females are paid similar wages, with female employees being taxed by customer bias.

Consequently, it does not appear that the firm faces a trade-off when hiring male or female workers, but female workers experience significant discrimination from customers in this job. Given that they do not sort away, there may be similar or more discrimination in other jobs they are able to obtain, or potentially non-pecuniary benefits and comparative advantage women experience in their current jobs may deter them from leaving. Together, these results highlight important amounts of market-level discrimination and suggest that simple comparisons of job or industry composition may not be good indications of bias faced by workers.

6 External Validity

These results are important in their own right, as they provide a proof of concept that customers can be a meaningful source of labor-market discrimination. Nevertheless, we can leverage our data to explore whether the impacts of customer discrimination are moderated by the context where we work.

Customers Are the results specific to certain types of customers we work with? We can test this in a number of ways. First, we investigate whether the results are specific to customers purchasing from Africa versus abroad.[37] Second, we explore whether customers discriminate along another salient margin: whether the name of the agent they are talking to is English-sounding or not. Table 5 presents these results for the full sample (columns 1-2), customers purchasing from Africa (columns 3-4), and for customers purchasing from abroad (columns 5-6). Focusing on columns (1, 3, 5), which estimate the effect of female-name assignment across customer locations, we see that our results are remarkably consistent. If anything, the results are slightly larger for customers from abroad, who reduce their purchases by 5.2 percentage points, relative to those from Africa, who reduce their purchases by 3.5 percentage points (though the impacts are not statistically different from one another, $p = 0.51$).[38]

[36] Additionally, the rates of sales for agents assigned to a female name in the experimental sample are comparable to sales by female agents in the non-experimental sample (e.g., .042 vs .049 in column 2, respectively).

[37] The majority of customers abroad are in North America or Europe.

[38] We also find quantitatively similar results when splitting across East and West Africa as well.

Next, we investigate customer bias along other margins in columns (2, 4, 6). The most prominent other dimension in this setting is related to race, which we investigate based on whether the names are common English sounding names. African customers are no less likely to purchase products for agents with non-English names (column 4, $p = 0.449$), while customers outside Africa, who may be less familiar and more biased towards these names, reduce their purchases by 4.2 percentage points (column 6, $p = 0.097$). We reject the effects are equal across these customers ($p = 0.07$); therefore, this type of race-based customer discrimination is differential and limited to certain types of customers. This stands in contrast to the gender-based discrimination we observe, which we detect across all customers.

Workers While this paper tests customer discrimination across thousands of customers, we have relatively few workers engaging with customers. This would be a concern for external validity if we thought the results were unique to the specific set of workers we engage with. Given that all agents in our sample are assigned to both treatment statuses, we can directly test for heterogeneity across workers. Table A9 shows we cannot reject that the treatment effects are the same across all agents, in levels ($p = 0.69$) and when calculated proportionally as a fraction of the agent's sales when assigned a male name ($p = 0.58$).[39] Additionally, the agent with the smallest proportional reduction still experiences 25% fewer customer purchases when assigned a female name (relative to their sales rate when not assigned a female name), demonstrating that this customer discrimination is economically significant for all workers.

Alternatively, our external validity may be threatened if the name-masking procedure affects workers' productivity such that they are unable to express their identity as they otherwise would. This concern does not threaten our identification of the effect of customer bias since it is equally true when assigning male or female-sounding names, but it may create interactions that are less reflective of reality. In practice, this seems unlikely as female workers appear to sell similar amounts when assigned a female name in the experiment compared with female workers who are not in the experiment (see section 5 for more details).

Jobs Finally, our results may be specific to the types of customer sales roles we focus on. Yet, the customer sales jobs at the firm we work with are comparable to the majority of jobs in this industry. Most companies have customer service departments, and hire sales agents to engage customers over the phone or online, answering their queries and assisting with sales. Amazon alone employs customer service associates in more than 130 locations in over 40 countries around the world. These sales agents must know the products on offer and be able to satisfy the demands of customers who may come from different parts of the world. Also, the products being sold are non-technical and not experience-based (i.e. the sales agents are not expected to have tried them out or to provide different recommendations based on their experience), which is common among these jobs.

[39] This table includes 5 rather than 6 agents given that one agent has few observations and we cannot consistently estimate their individual-specific treatment effect.

7 Conclusion

This paper demonstrates that customer-based discrimination negatively affects female worker productivity. When sales agents randomly receive female-sounding names, the probability a customer makes a purchase falls by 50%. Consumers also purchase fewer total products, and the total value of their purchases decreases. We find supporting evidence that customer disinterest is driving these effects. Customers lag in responding to female agents and are less likely to transition from their initial inquiry into a discussion about purchasing. In contrast, we do not find evidence that harassment or differential bargaining are important in this context.

Interestingly, these experimental results reveal significant levels of customer discrimination in a market where actual sales rates are equal between men and women. The lack of observed productivity differences between genders would naturally arise if customer discrimination was internalized by the firm: the firm only retains women who can overcome customer discrimination, such that their observed productivity is the same as men's. It is only by running our experiment that we can observe that female employees are more productive than their male counterparts absent customer bias.

Identifying the existence and extent of customer discrimination in a real-world setting is particularly relevant for two reasons. First, seminal work by Becker (1957) suggests that customer-based discrimination will not be competed away in equilibrium because firms internalize these preferences—exactly the dynamic we observe. Secondly, hypotheses that workers may be able to sort away from industries in which they face customer discrimination, thereby limiting its impact, do not appear to hold in our setting (Heckman, 1998). More broadly, if women are unable to avoid customer discrimination through sorting, this may present a barrier to female labor force participation. Women who would otherwise be productive employees will not be hired due to customer preference. Therefore, the customer discrimination we document potentially presents an important and persistent labor market distortion. This is especially relevant in the context where we work: significant gender disparities exist in formal-sector employment across Sub-Saharan Africa, where less than 15 percent of women work full-time for an employer (World Bank, 2013; Klugman and Twigg, 2016).

From a policy perspective, the most direct approach to tackling this problem is to change customer norms around women in the workplace. Governments may use programs that increase the representation of women in positions of power, exposing the general population to women as authority figures.[40] Similarly, if firms believe they could capture future benefits by sensitizing customers (perhaps through market power) or choose to because of pro-social intentions, they may themselves seek to change customer norms. Nevertheless, norm change is likely to be a difficult and slow process, and since many customer services positions are already predominantly female, simply hiring female sales representatives is unlikely to lead to norm change.

[40] This is similar to Beaman et al. (2009) who show that prior exposure to a female politician improves positive perceptions of female leaders.

Absent norm change, a second-best approach may be to limit the consequences of customer-based discrimination on female employees. This consideration is particularly relevant for industries which tie employee pay to productivity/output (e.g., number of sales) through piece-rate wages. Our results suggest that such individual-based incentivized pay schemes may increase the impact of customer discrimination on worker pay.[41] Second, some companies have found that obscuring identities makes the job easier for their customer service representatives (Chan, 2022). In our setting, discrimination's effects might be eliminated by agents using gender-neutral names (or avoiding names altogether). While such measures could reduce inequality, they also potentially perpetuate the bias that creates these inequalities in the first place.

[41] For example, one way to limit this source of discrimination is for employers to 'pool' performance-based bonuses—a common practice for sharing tips in the restaurant industry.

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Tables

Table 1: Placebo tests for female assignment

	N	Var. Mean	Female
Customer mention agent true name	2655	.00	-.00128
			(.00234)
Customer amount of past chats	2655	.38	-.0667
			(.0439)
Customer amount of past purchases	2655	.29	-.0166
			(.0458)
Agent first message length	2655	5.47	-.00066
			(.0041)
Agent chats (daily)	337	7.76	-.106
			(.596)
Agent worked previous day (daily)	337	.54	-.0827
			(.0583)
Agent hours worked (daily)	337	2.57	-.0291
			(.172)
Joint *p*-value			.59

This table shows customer and agent outcome means in column (2) and correlation between female name assignment and outcomes in column (3). The number of chats, hours worked by agents, and whether worked on previous day are at the day level, while the other variables are at the chat level. Controls include agent-month fixed effects. Female indicator determined in customer's first chat of the day. Standard errors in parentheses and clustered at agent-day level. Joint *p*-value tests equality of all coefficients with zero. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2: Effect of female assignment on purchase outcomes

	Purchases (48h)		Purchases (24h)	
	(1) Any	(2) Total	(3) Any	(4) Total
Female	-.038***	-.038***	-.036***	-.035***
	(.013)	(.014)	(.011)	(.012)
Control Mean (wt)	.076	.081	.070	.073
N	2655	2655	2655	2655

This table shows the effect of female name assignment on purchase outcomes. Any represents any purchase and Total represents number of purchases. Any purchases and total purchases combine purchases by customer and by agent. Purchases are measured within 24 or 48 hours of the start of the chat. Female indicator determined in customer's first chat of the day. Controls include agent-month fixed effects. The control group mean is reweighted by fixed effect cells to match the implied agent-month fixed effect OLS weights. Standard errors two-way clustered at the agent-day and customer-day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3: Effect of female name assignment on chat response, outcomes, and purpose

	Extensive margin		Intensive margin			
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Chat response and outcomes						
	Ever respond	Msgs to response	Any tone	Harass	Any negativity	Bargaining
Female	-0.021	0.009**	-0.031**	0.000	0.004	0.006
	(0.021)	(0.004)	(0.014)	(0.003)	(0.004)	(0.018)
Control Mean (wt)	0.676	1.009	0.089	0.003	0.008	0.141
Observations	2,655	2,655	1,745	1,745	1,745	1,745
Panel B: Chat purpose						
			Initial purpose: Booking	Secondary purpose: Booking	Secondary purpose: Booking	Secondary purpose: Any
Female			-0.009	-0.057*		
			(0.025)	(0.031)		
Female X Initial general inquiry					-0.100**	-0.084*
					(0.045)	(0.045)
Female X Initial price inquiry					-0.145*	-0.108*
					(0.076)	(0.060)
Female X Initial make booking					-0.015	-0.026
					(0.020)	(0.042)
Female X Initial other purpose					-0.023	-0.029
					(0.026)	(0.034)
Control Mean (wt)			0.150	0.253	0.378	0.471
Observations			1,745	1,745	1,745	1,745

This table shows the effect of female name assignment on chat responses and outcomes. Columns (1-2) report extensive margin chat responses, while columns (3-6) report intensive margin chat behavior (conditional on any customer response). Panel A shows chat reponses and chat outcomes. Ever respond in column (1) is a 1 if the customer ever responded. Msgs to response in column (2) is the number of messages sent by agent before customer first response. Column (3) measures any non-neutral chat tone, column (4) measures any harassment of the agent, column (5) measures any negative words or phrases, and column (6) measures any bargaining. Panel B shows information on primary and secondary chat purposes. Column (3) measures whether the visitor's initial purpose of the chat was to make a booking. Columns (4-5) indicate whether the visitor's secondary purpose of the chat was to make a booking. Column (6) measures whether the conversation preceded to have any secondary purpose. Female indicator determined in customer's first chat of the day. Variables of the form Female X Initial ... are an interaction between the female indicator and an indicator for the initial purpose of the chat being one of the following: general inquiry, price inquiry, make a booking, or other purpose. Controls include agent-month fixed effects and also initial chat purpose for the heterogeneity analysis (Panel B, columns 5-6). The control group mean is reweighted by fixed effect cells to match the implied agent-month fixed effect OLS weights. Standard errors two-way clustered at the agent-day and customer-day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: Correlational relationship between female agent and administrative sales outcomes

	Purchases (48h)		Purchases (24h)	
	Any	Total	Any	Total
	(1)	(2)	(3)	(4)
Panel A: Correlational estimates				
Female	-0.003	-0.004	-0.002	-0.002
	(0.011)	(0.012)	(0.011)	(0.012)
Control Mean (wt)	0.049	0.053	0.046	0.048
Observations	8,867	8,867	8,867	8,867
Panel B: Experimental estimates				
Female	-0.038***	-0.038***	-0.036***	-0.035***
	(0.013)	(0.014)	(0.011)	(0.012)
Control Mean (wt)	0.075	0.080	0.070	0.072
Observations	2,655	2,655	2,655	2,655
Equality b/w Exp/Non-exp (p)	0.035	0.053	0.033	0.049

This table shows correlational and causal effects of female agent on sales outcomes from administrative records. Panel A shows correlational estimates using the non-experimental sample, while Panel B shows causal estimates from the experimental sample. Any represents any sale, Total represents number of sales, and Total price is the cumulative price of all sales in EUR. Sales are measured within 24 or 48 hours of the start of the chat. Female indicator determined in customer's first chat of the day. Controls include office-month fixed effects in Panel A, and agent-month fixed effects in Panel B. The control group mean is reweighted by fixed effects cells to match the implied OLS weights. For the correlational estimates, the control mean is reweighted by office-month, while in the experimental estimates it is reweighted by agent-month. Standard errors two-way clustered at the agent-day and customer-day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: Effect of female and non-English name assignment on purchase outcomes

	Any purchase (48h)					
	(1)	(2)	(3)	(4)	(5)	(6)
Female	-.038***	-.037***	-.035**	-.034**	-.052**	-.053**
	(.013)	(.013)	(.014)	(.015)	(.026)	(.026)
Non-English name		.0044		.011		-.042*
		(.013)		(.015)		(.025)
Control Mean (wt)	.076	.076	.075	.075	.078	.078
F=Non-E (p)		.00		.01		.74
F=F C4,6 (p)						.51
Non-E=Non-E C4,6 (p)						.07
Sample	Full	Full	Africa	Africa	Not Africa	Not Africa
N	2655	2655	2321	2321	322	322

This table shows the effect of female and non-English name assignment on purchase outcomes, overall and by region. The outcome measures any purchase within 48 hours of the start of the chat. Female indicator determined in customer's first chat of the day. Non-english name indicator determined in customer's first chat of the day. Controls include agent-month fixed effects. The control group mean is reweighted by fixed effect cells to match the implied agent-month fixed effect OLS weights. F=Non-E (p) refers to the p-value from a test of equality between the Female and Non-English coefficients within the same model. F=F C4,6 (p) refers to the p-value from a test of equality between the Female coefficients across columns 4 and 6. Non-E=Non-E C4,6 (p) refers to the p-value from a test of equality between the Non-English coefficients across columns 4 and 6. Standard errors two-way clustered at the agent-day and customer-day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Online Appendix

Tables

Table A1: Example names used in name assignment, by gender and non-English status

Female, English	Male, English	Female, Non-English	Male, Non-English
Emmily	Gabriel	Sekinat	Mohammad
Vivian	Kelvin	Khadijah	Dinah
Abigail	Elias	Genevieve	Jediel
Judith	Stanley	Habeeb	Hamza
Laura	Thomas	Chioma	Ebiason
Helen	Harrison	Fridah	Gideon
Joy	Sammy	Linet	Abuzarin
Eunice	Philip	Sherifa	Abayomi
Jayne	Isaiah	Consolata	Ezekiel
Marian	Samuel	Peace	Taiwo
Carolyne	Anthony	Bayu	Farouk
Josephine	Charles	Oluwafunmibi	Erastus
Lizzy	Denis	Zaida	Mories
Sharon	Simon	Aderonke	Umaru
Annmarie	Henry	Nduta	Brightone
Bianca	Dennis	Sadiya	Bashir
Clare	Lawrence	Nafisha	Alphonce
Deborah	Dick	Flavian	Halilu
Susan	Antony	Habu	Abdulrahman
Stephanie	Edwin	Staline	Adewale

This table shows a random set of names drawn from the dictionary of possible names to be assigned. Twenty names are presented for both male and female, and for both English and non-English coded names.

Table A2: Outcome Variable Descriptions

Purchases	
Any Purchases	Whether customer made any purchase 24 or 48 hours after the chat
Total Purchases	The total number of purchases that were made by the customer 24 or 48 hours after the chat
Total Price	The cumulative price of all purchases in EUR that were made by the customer 24 or 48 hours after the chat
Chats	
Ever Respond	$= 1$ if the customer ever responded
Messages to Response	Number of messages sent by agent before customer first response.
Tone	
	We employed research assistants based in Sub-Saharan Africa to read through all of the chats and categorize them by overall tone of the conversation and flag any instances of harassment, negativity, or bargaining.
Any	Measures any non-neutral chat tone. Chats were coded neutral or non-neutral tone (including angry, sad, happy, ecstatic, impatient)
Harassment	Measures any harassment of the agent
Any negativity	Measures whether any negative words or phrases were used by the customer.
Bargaining	Measures any bargaining with the agent. This includes asking for discounts, or better prices.
Purpose	
Initial / Secondary Purpose	Each chat was hand coded to capture the initial and secondary purpose (if any) the customer had when initiating the conversation with the chat agent. The initial purpose is defined as the first issue the customer raises with the agent while the secondary purpose is any subsequent topic after the primary issue was resolved.
General Inquiry	Customer requested general information about the platform or general service availability.
Make a Booking	Customer asked for help making a specific booking.
Price Inquiry	Customer asked about the price for a specific hotel or for a general category of hotels in an area.
Confirm Booking	Customer asked to confirm that their booking request had been received and processed.
Other Purpose	Captures other less common reasons including complaints, date changes, and cancellations.

Table A3: Effect of female assignment on purchase prices

	Purchase prices (48h)		Purchase prices (24h)	
	(1)	(2)	(3)	(4)
Female	-3.2***	-50**	-3.2***	-58***
	(1.2)	(21)	(1.1)	(20)
Control Mean (wt)	5.3	140	4.9	146
Sample	All	Sales	All	Sales
N	2655	68	2655	58

This table shows the effect of female name assignment on purchase prices. Price is the cumulative price of all purchases in EUR. Prices are based only on customer purchases (via admin-based records). Purchases are measured within 24 or 48 hours of the start of the chat. Odd columns include the full sample, while even columns only include the sample with any customer purchase within the relevant time frame. Female indicator determined in customer's first chat of the day. Controls include agent-month fixed effects. The control group mean is reweighted by fixed effect cells to match the implied agent-month fixed effect OLS weights. Standard errors two-way clustered at the agent-day and customer-day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A4: Effect of female assignment on initial chat purpose

	Initial chat purpose			
	(1) General Inquiry	(2) Make a Booking	(3) Price Inquiry	(4) Other
Female	.02	-.009	.0016	-.013
	(.03)	(.025)	(.016)	(.019)
Control Mean (wt)	.458	.222	.101	.219
N	1745	1745	1745	1745

This table shows the effect of female name assignment on initial chat purpose. The outcome variables are binary variables that are 1 if the initial purpose is about a general inquiry (column 1), about making a specific booking (column 2), an inquiry about a price (column 3), and other less common initial conversation purposes including complaints, confirmations, changes, cancellations, or unknown reasons (column 4). Female indicator determined in customer's first chat of the day. Controls include agent-month fixed effects. The control group mean is reweighted by fixed effect cells to match the implied agent-month fixed effect OLS weights. Standard errors two-way clustered at the agent-day and customer-day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A5: Effect of female assignment on purchase outcomes, split by conversation length

	No response	Word length		
	(1)	(2) Tercile 1	(3) Tercile 2	(4) Tercile 3
Female	-.011	-.022	-.043**	-.087***
	(.013)	(.023)	(.019)	(.028)
Control Mean (wt)	.036	.057	.052	.165
Proportional effect	-.31	-.39	-.83	-.53
N	906	571	582	590

This table shows the effect of female name assignment on any purchase within 48 hours, split by conversational length. Column (1) includes conversations in which the customer never responds. Column (2-4) splits conversations into terciles by the conversational length (in words), for the set of conversations when customers ever respond. Female indicator determined in customer's first chat of the day. Controls include agent-month fixed effects. The control group mean is reweighted by fixed effect cells to match the implied agent-month fixed effect OLS weights. The proportional effect is the ratio of the treatment effect to the control group mean. Standard errors two-way clustered at the agent-day and customer-day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A6: Effect of female assignment on purchase outcomes (customer-day level)

	Purchases (48h)		Purchases (24h)	
	(1)	(2)	(3)	(4)
	Any	Total	Any	Total
Female	-.04***	-.041***	-.038***	-.037***
	(.013)	(.014)	(.011)	(.013)
Control Mean (wt)	.077	.083	.072	.074
N	2172	2172	2172	2172

This table shows the effect of female name assignment on purchase outcomes. The data is aggregated to the customer-day level. Any represents any purchase and Total represents number of purchases. Any purchases and total purchases combine purchases by customer and by agent. Purchases are measured within 24 or 48 hours of the start of the chat. Female indicator determined in customer's first chat of the day. Controls include agent-month fixed effects. The control group mean is reweighted by fixed effect cells to match the implied agent-month fixed effect OLS weights. Standard errors clustered at the agent-day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: Effect of female assignment on purchases, by outcome source and non-gender reveal sample

	Purchases: All		Purchases: Chat-based	Purchases: Admin-based		Purchases: Non-gender ID	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	(48h)	(24h)	All	(48h)	(24h)	(48h)	(24h)
Female	-.038***	-.036***	-.018***	-.019*	-.017**	-.04***	-.038***
	(.013)	(.011)	(.0066)	(.0099)	(.0087)	(.013)	(.012)
Control Mean (wt)	.076	.070	.033	.043	.037	.078	.073
N	2655	2655	2655	2655	2655	2274	2274

This table shows the effect of female name assignment on purchase outcomes across purchase types and samples. All purchases refers to purchases from any source. Chat-based purchases refers to any purchases that were captured by the hand-coded chat data. Admin-based purchases refers to any purchase that were captured from sales data. Non-gender ID sample only includes observations from days when a customer did not use a gendered identifier in any chat to that agent. Gendered identifiers include: sir, maam, ma'am, brother, sister, miss. Purchases are measured within 48 or 24 hours of the start of the chat. Female indicator determined in customer's first chat of the day. Controls include agent-month fixed effects. The control group mean is reweighted by fixed effect cells to match the implied agent-month fixed effect OLS weights. Standard errors two-way clustered at the agent-day and customer-day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A8: Effect of female assignment on purchase outcomes, robustness to alternative specifications

	Any purchases (48h)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	-.038***	-.044***	-.039***	-.035***	-.033***	-.031***	-.029**	-.025**
	(.013)	(.012)	(.013)	(.012)	(.011)	(.011)	(.012)	(.012)
Control Mean (wt)	.076	.076	.076	.076	.076	.066	.056	.062
Proportional effect	-.50	-.58	-.51	-.46	-.43	-.47	-.52	-.41
Agent-month FE	X	X	X	X	X			
Name ethnicity FE		X						
Customer controls			X					
DOW FE				X				
Week FE					X			
Agent FE						X		
Month FE						X		
Date FE							X	
N	2655	2654	2634	2655	2654	2655	2648	2655

This table shows the effect of female name assignment on any purchase within 48 hours in various specifications. Female indicator determined in customer's first chat of the day. Fixed effects and customer controls, for past purchases, customer location, and customer chat history, are included based on the column notes. Name ethnicity FE indicates fixed effects assigned based on the assigned full name. The control group mean is reweighted by fixed effect cells to match the implied fixed effect-only OLS weights: columns (1-4) reweight by agent-month, column (5) by agent, column (6) by date, and column (7) does not reweight. Standard errors two-way clustered at the agent-day and customer-day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table A9: Effect of female assignment on any sales by agent (48 hours)

	Purchases (48h)
	(1)
	Any
Female * Agent 1	-.02
	(.013)
Female * Agent 2	-.022
	(.038)
Female * Agent 3	-.014
	(.033)
Female * Agent 4	-.039***
	(.011)
Female * Agent 5	-.048**
	(.02)
Control Mean (wt)	.076
Joint p-value	.69
Proportional p-value	.58
N	2636

This table shows the effect of female name assignment on any purchase within 48 hours of the start of the chat by agent. Female indicator determined in customer's first chat of the day. Joint p-value tests equality of all coefficients. Proportional p-value tests equality of all effects proportional to the agent-specific control group mean. Controls include agent-month fixed effects. The control group mean is reweighted by fixed effect cells to match the implied agent-month fixed effect OLS weights. Standard errors two-way clustered at the agent-day and customer-day level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.