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OBSERVATIONAL LEARNING:
EVIDENCE FROM A RANDOMIZED NATURAL FIELD EXPERIMENT

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ABSTRACT

We present results about the effects of observing others' choices, called observational learning, on individuals' behavior and subjective well-being in the context of restaurant dining from a randomized natural field experiment. Our experimental design aims to distinguish observational learning effect from saliency effect (because observing others' choices also makes these choices more salient). We find that, depending on specifications, the demand for the top 5 dishes was increased by an average of about 13 to 18 percent when these popularity rankings were revealed to the customers; in contrast, being merely mentioned as some sample dishes did not significantly boost their demand. Moreover, we find that, consistent with theoretical predictions, some modest evidence that observational learning effect was stronger among infrequent customers. We also find that customers' subjective dining experiences were improved when presented with the information about the top choices by other consumers, but not when presented with the names of some sample dishes.

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1 Introduction

Social learning has attracted increasing attention from the economics literature recently. Generally, social learning refers to any mechanism through which individuals learn from others. It includes mechanisms in which individuals learn from each other through formal or casual/word-of-mouth communications. It also includes *observational learning*, as modelled in the seminal work by Banerjee (1992) and Bikhchandani, Hirshleifer and Welch (1992), where individuals' behavior is impacted by their observation of the behavior of others because of the *information* contained therein.[1]

Despite the large theoretical literature on observational learning and its intuitive appeal, to establish empirically that individuals' decisions are affected by the observation of others' actions because of its *informational content* is an not an easy task. The main challenge comes from at least two possible confounding channels. First, observing the choices made by others also makes these choices *salient*. It is thus possible that individuals follow others' choices just because their choices are more salient than alternative choices. Second, if individuals have conformity concerns,[2] then they may also be led to adopt the observed choices of others.

Convincing empirical evidence about the importance of observational learning is not only relevant for the aforementioned theoretical literature in economics, it also has potentially crucial implications regarding policy. Observational learning can take place, as long as the underlying decision problems faced by individuals are similar, regardless of time, space and whether individuals are socially connected; in contrast, social learning via direct communications and personal interactions are more local and can occur only if there is time, spatial and social proximity among individuals. If observational learning is important, then a policy maker who wants to, say, expedite the adoption of an advantageous technology may, as a possible intervention, engage in an information campaign emphasizing the technology's popularity among other group of agents. In contrast, if direct communication is the main channel of social learning, then such an information campaign will not be effective.

This paper aims to provide direct evidence of observational learning (due to the informational content of others' choices) using a randomized natural field experiment in the setting of a medium-scale restaurant chain in Beijing, China.[3] The restaurant has a rather thick menu with many

[1] Bandura (1977) was the pioneering book in psychology that started the research on social and observational learning. Chamley (2004) provides a textbook treatment of the theoretical literature in economics that studies herding, information cascades, and observational learning. Smith and Sørensen (2000) made the distinction between social learning in general and observational learning.

[2] The classic social psychology experiments documenting conformity in individuals' judgment is Asch (1951, 1955). See Bernheim (1994) for more related literature on conformity experiments and an economic model.

[3] See Harrison and List (2004) and List (2007) for surveys and methodological discussions, including categorizations, of the surging literature of field experiments. A website `http://www.fieldexperiments.com` maintained by John List provides a useful categorization and comprehensive and updated list of papers in this literature.

dishes and customers typically have a hard time to make choices. The thick menu means that making some dishes salient could potentially influence the consumers' choices. In the experiment (which involved two levels of randomization, one at the site level and another at the table level), we randomly exposed diners to three different information displays (or lack thereof): in the control tables, we did not give diners any information display; in what we call "ranking treatment," we gave the diners a display listing the "top five" dishes according to *the actual number of dished sold last week*; and in what we call "saliency treatment, " we gave diners a display listing five "sample dishes," which always included the actual top 3 dishes (without being revealed as such) together with two other randomly selected dishes, and were arranged in a random order. We analyze how the information displays affected the choices of customers. We also conducted a short survey at the end of the meal to gather information about bill-payers' basic demographic characteristics, and frequency of patronage to this restaurant chain and their subjective dining satisfaction.

The "ranking" and "saliency" treatments included in our experimental design are aimed to explicitly incorporates the concerns for the separate estimation of pure saliency effect (i.e. saliency without additional informational content) and the pure observational learning effect (i.e., resulting from the informational content). Though we do not directly address the conformity channel in our experimental design, we would like to argue that our choice of the experimental setting in a natural situation of restaurant dining and the fact that information provided to the current customers are those of the customers in the past likely make this channel unimportant.[4]

We find that, in the context of restaurant dining, depending on specifications the demand for the top 5 dishes was increased by an average of about 13 to 18 percent when these popularity rankings were revealed to the customers; in contrast, being merely mentioned as some sample dishes did not significantly boost their demand. Moreover, we find that, consistent with theoretical predictions, some modest evidence that observational learning effect was stronger among infrequent customers. We also find that customers' subjective dining experiences were improved when presented with the information about the top choices by other consumers, but not when presented with the names of some sample dishes.

1.1 Related Literature

In terms of experimental design, our paper is mostly related to Salganik, Dodds and Watts (2006). In an artificial music market, they asked subjects (recruited from visitors to a particular website) to rate a list of eight songs, then the subjects were offered an opportunity to download the song with or without the knowledge of previous participants' choices. Their focus is on how

[4]As is well-known since Asch (1955), social pressure seems to exert the most influence via conformity when individuals are forming opinions in the *presence and visibility* of others. It is our belief and certainly our maintained assumption that individuals are unlikely to order certain dishes that were popular with others due to conformity motives.

social influence may lead to unpredictable outcomes for popular cultural products. Our paper differs from theirs in at least two dimensions. First, the main focus of our paper is to distinguish observational learning effect (as a result of the informational content) from saliency effect. In our experimental setting, the restaurant menu is thick and includes more than 60 hot dishes (together with an additional large number of cold dishes). Diners are unlikely *aware* of all the alternatives, thus saliency is an important issue that might affect their choices.[5] In contrast in their setting, as a result of the small choice set (eight songs), they could not experimentally manipulate the saliency of the songs because all subjects were aware of the complete choice set when making choices. Second, as we mentioned earlier conformity concerns play an important role in the demand of popular cultural products where shared experience is a major component of the utility from consuming such goods. Thus conformity is likely an important part of the social influence they found in their study, which they did not attempt to distinguish. Restaurant dining is a more private experience, and as such conformity is less a concern.

There are also several papers that examined social learning and informational cascades in laboratory settings (for example, see Anderson and Holt 1997 and Çelen and Kariv 2004). Alevy, Haigh and List (2007) compared the behavior of professional traders from Chicago Board of Trade and student subjects in artificial lab experiments similar to those of Anderson and Holt (1997). The experiments in these papers all have simple choice sets where the researchers explain to the subjects prior to the experiment. Thus, again, the saliency of possible alternatives is not subject to manipulation by the researchers.

There is also a large empirical literature attempting to identify and quantify the effect of *social learning generally* on individuals' choices in a variety of contexts. This turns out to be a notoriously difficult empirical exercise due to the identification problems that have been eloquently described by Manski (1993, 2000). The main issue is to distinguish social learning from common unobserved individual characteristics, which Manski (1993) called "the reflection problem" or "correlated effects." The existing empirical literature addresses this issue using a variety of strategies to varying degrees of success.[6] One approach is to examine the different implications of social learning and common unobservable shock. For example, Conley and Udry (2005) showed that pineapple farmers in Ghana imitate the choices of fertilizer quantity of their "information neighbors" (instead of "geographical neighbors") when these neighbors have a good harvest, and move further away from

[5] See Section 2.1 for more discussions of our experimental setting.

[6] An incomplete list of studies of social learning effects includes, in the context of crime (Glaeser, Sacerdote and Scheinkman 1996), contraception (Munshi and Myaux 2000), adoption of seeds, fertilizer and other technologies (Besley and Case 1994, Foster and Rosenzweig 1995, Conley and Udry 2005; Munshi 2000; Duflo, Kremer and Robinson 2005; Kremer and Miguel 2003), welfare program participation (Bertrand, Luttmer and Mullainathan 2000), stock market participation (Hong, Kubik and Stein 2001), labor market outcomes (Bayer, Ross and Topa 2005), retirement saving plan choices (Duflo and Saez 2002, 2003), health insurance plan (Sorensen 2005), consumer demand (Mobius, Niehaus and Rosenblat 2005, Moretti 2007) and voting in sequential primaries (Knight and Schiff 2007).

their decisions when they experience a bad harvest. They argue that this is not due to correlated shock by showing that the choices made on an established crop (maize-cassava intercropping) for which no learning is necessary do not exhibit the same pattern. A second approach is to exploit the panel nature of the data to control for the common unobservables using fixed effects under the assumption that the common unobservables are not time varying. An example of this approach is Sorensen (2005) who examined the health plan choices of University of Californian employees where he showed that after controlling for department fixed effects social effects still exist.[7] A third and more recent approach is via randomized experiment where "treatment" (which differs in different papers) is randomly assigned to individuals and then behavior of others who are more or less connected to the treated individuals is measured. For example, Duflo and Saez (2003) randomly assigned different information sessions about 401k options to individuals and found that their 401k participation decisions have significant effects on their coworkers, consistent with their non-experimental evidence (Duflo and Saez 2002).[8]

Almost none of the existing papers, however, attempts to distinguish observational learning from the general form of social learning.[9] That is, the literature does not try to ask whether one's behavior is impacted by others because he/she observed others' choices only, or whether they communicated and shared information in a more personal manner.

The remainder of the paper is structured as follows. Section 2 describes our experimental design; Section 3 presents the basic features of our data; Section 4 spells out our identification strategy and identifying assumptions; Section 5 describes our experimental findings; and finally Section 6 concludes.

2 Experimental Design

2.1 Choice of Experimental Setting

To distinguish observational learning from other forms of social learning with personal communication, it is most important to conduct the experiment in a setting where, first, except for the information about others' choices that the researchers feed or not feed them, the researchers are more or less sure that the subjects are not involved in any direct communication with others; and, second, there is some commonality in the decision problems of the current subjects and

[7] The estimated impact of coworkers' choices does decrease when he includes department fixed effects, pointing to the presence of common unobservables.

[8] However, in several other settings, randomized field experiments yield results that substantially differ from those that would have been obtained with other econometric methodologies (see, for example, Duflo, Kremer and Robinson 2005, Kremer and Miguel 2004).

[9] Mobius, Niehaus and Rosenblat (2005) is an exception. They attempt to disentangle social learning in strong form (direct information sharing) and weak form (observational learning). Their experimental design, however, includes both forms of social learning and thus they have to rely on a structural model to disentangle them.

others. Restaurant dining provides an almost ideal setting for this purpose. Firstly, when diners make decisions about what dishes to order during an outing, they typically do not interact with other diners (except for those in their table) in a particular session, and unlikely with customers in the past. Thus, we do not have to be concerned with possibilities that personal conversations between current and past customers that might affect the current customers' choices occur out of the experimental session and beyond the researchers' observation. Secondly, diners, though with potentially different tastes, all care about the (common) quality of the dishes.

Restaurant dining also has other advantages for conducting our experiment. First, because of computerization, it is very easy to obtain information of diners' choices; second, it is relatively easy to implement randomized treatments in terms of diners' information set; third, we can observe the effect of treatment on subjects' choices accurately and instantly; fourth, we can survey *on the spot* the effect of treatment on customers' well-being in terms of their subjective dining experience.

Our experiment was conducted in a medium-sized Szechuan restaurant chain *"Mei Zhou Dong Po"* (MZDP in short). MZDP is a chain with 13 separate sites in the city of Beijing, China. Each location has an average of 50 tables, and the same menu with about 60 hot dishes (and many additional cold dishes) are offered in all locations, though the popularity of dishes varies by locations. The restaurant is considered medium both in scale and price level, popular to both leisure and ordinary business dining.

2.2 Experimental Design

In our experiment, diners were randomly assigned into tables with three different information sets. We first describe the three information sets and then explain the actual two-stage randomization which we implemented. The first group of tables were simply the **"control tables"** where nothing was displayed on the tables; the second was **"ranking treatment tables"** where diners were provided with a plaque on the table displaying the names of the five most popular dishes (varying with sites) according to the actual number of dishes sold in the previous week; the third group of tables were called **"saliency treatment tables"** where the diners were provided with a plaque on the table displaying the names of five sample dishes from the menu, which included the names of the actual top 3 dishes at that site (without being revealed as such) together with two other randomly selected dishes. The five sample dishes were sequenced in the plaque in a random order.

The saliency treatment tables are important in order for us to distinguish "observational learning" effect from "saliency" effect, which specifically refers to the phenomenon that the dishes mentioned on the display plaque may be chosen with higher probabilities simply because they are more salient than other dishes on the menu, even though no information about the quality of the dishes is revealed in the display.

We decided to implement a two-stage randomization strategy where the first stage randomization was at the level of restaurant sites, and the second stage is at the table level within a site. More specifically, in the first stage, we randomly selected 5 locations where tables in each location were randomized into "control tables" and "ranking treatment tables," and randomly selected 4 other locations where tables in each site were randomized into "control tables" and "saliency treatment tables."[10] Table 1 summarizes our experimental design.

[Table 1 About Here]

An important component of our experimental design is data collection in the week prior to the introduction of any informational treatments. After randomly selecting the locations for the ranking and saliency treatments, we randomly assigned tables in each of the selected locations into "control" and "treatment" tables, and then collected data of the diners' choices for the week of October 16-22, 2006, before we implemented the ranking and saliency treatments in the week of October 23-30, 2006.[11]

The data collected in this pre-experiment week serves three separate purposes. First, we used the pre-experiment data to come up with the list of five most popular dishes in the pre-experiment week that would be displayed in the ranking treatment tables in the experiment week. The top five dishes differed by locations, and the five sample dishes in the saliency treatment locations also differed by location. Second, the pre-experiment data allows us to conduct some test regarding the quality of randomization. Third, and more importantly, pre-experiment data allows us to implement a triple-difference estimator of the observational learning effect to eliminate possible (unobservable) systematic differences between treatment and control tables.

We would like comment briefly on our choice of the two-stage randomization strategy described above, instead of using a single randomization at the table level alone. A single stage randomization at the table level would lead to the presence of all three information treatments (control, ranking treatment and saliency treatment) in a same location. This is desirable from the statistical point of view, because it would allow us to estimate the difference between the saliency effect and overall ranking treatment effect without having to assume, as we have to for our two-stage randomization strategy, that ranking treatment locations and saliency treatment locations are similar in unobservable dimensions.[12] However, our choice of the two-stage randomization strategy is due to practical considerations. The managers of the restaurant chain expressed the concern that such a single stage randomization with three treatments in a same location would create confusion among waiters and

[10] We did not use all of the 13 sites because we initially had planned on another treatment.

[11] The restaurants implemented our experiments one day longer than we requested. (In fact, after the experiment, they have adopted ranking information display as part of their regular business strategy.) We used all the data from the eight-day period in our analysis presented below, but the results do not change at all if we throw away the last day data.

[12] See Section 4 for more discussion about the identification strategy and how it is related to our experimental design.

waitresses, as well as in record keeping. Moreover, if customers found out the two different displays of dishes, they might raise suspicion about the restaurant's intention. In our analysis, we will do our best to test whether the randomization at the location stage is well implemented.

2.3 Post-Dining Survey

For about 20 percent of randomly selected dining parties in the experimental week, we also administered a short dining survey (the questionnaire is provided in Appendix A) where we collected information from the person who paid for the bill of the whole table. In particular, we collected information about his or her gender, age, cumulative frequency of dining in MZDP, subjective dining experience etc. It typically took less than a minute to answer the post-dining survey. All the tables that filled out the survey were given a box of poker cards and a piece of moon-cake as tokens of appreciation.

The post-dining survey allows us to ask two important questions. First, is observational learning a more pronounced phenomenon among infrequent customers who have less information about the quality of the dishes than among frequent customers? Second, how does observational learning affect customers' subjective dining experience?

At this moment, it is useful to explain a bit about social customs about dining in China. It is socially customary in China to have one person to be in charge of paying for the whole group's bill. Sharing the bill or separate billing is not common.[13] The bill-payer is often the one who "treats" the other members of the dining party and is typically the one who is most responsible for making the dish choices.[14] It is important to emphasize that the potential separation of biller-payers and dish-orderers does not pose any problem for our analysis regarding the effect of observational learning on dish choices and dining experience as long as it is not systematically correlated with our randomization of control and treatment tables. However, the patronage frequency of the bill-payer we collected in the survey may not be the relevant one for the dish-orderer, this measurement error may have played a role in the small (though statistically significant) effect of the patronage frequency on the strength of observational learning effect in Table 9).

3 Data

3.1 Structure of Raw Data

The restaurant records each dining party by a unique bill ID, and a unique number corresponding to each dish ordered in that bill, together with the Chinese name and the price of that dish, are

[13] More accurately, sharing occurs in repeated interactions where people take turns in paying the bill.

[14] This is not true in formal business dining, in which the "host" usually does the ordering and his/her subordinate pays the bill. Due to this concern, we deleted 407 large bills suspected to be formal business dining in our analysis (see footnote 15 for more details).

also recorded. For example, suppose that eight dishes are ordered in bill ID 3135, then in the raw data, there are eight lines with each line recording one dish. Importantly, the first line under each bill ID records the table number where the dining party sits and the total amount spent. The table number is then compared with our prior randomization that assigned each table to treatment or control.

We only include bills in which hot dishes were served because the bill was typically a take out bill (with no table assignment) if it included only cold dishes. We also deleted very large bills which were most likely weddings and company banquets. We used 800 CNY (Chinese Yuan) as the cutoff above which the bill was considered large, after consulting with the restaurant managers.[15] From a total of 13,302 bills in our data set (including both the pre-experiment and experiment weeks), a total of 407 bills were deleted due to these considerations, leaving us a total of 12,895 bills for analysis. As can be seen in Table 1 below, 7,355 bills were from the five ranking treatment locations, and 5,540 were from the four saliency treatment locations.

3.2 Pre-Experiment Data

In the week of October 16-22, 2006, the restaurant provided us its accounting data. We first used the data to come up with the list of top 5 dishes, for each of the five ranking treatment locations, and the top 3 dishes for inclusion in the display in saliency treatment locations.[16] We merged the raw data with our table assignment to treatment and control tables, and Panel A of Table 2 provides the basic summary statistics of the control and treatment tables in the pre-experiment week in both the ranking and saliency treatment locations.

The pre-experiment data shows that our experiment design achieved randomization both at the table and site levels. First, there are a total of 3401 bills from the five ranking treatment locations (for an average of 97 bills per day in each location), and 2671 bills for the four saliency treatment locations (for an average of 95 bills per day in each location). Thus, there is only slight difference in customer volume between the ranking treatment and saliency treatment locations.

Second, notice that in the ranking treatment locations, the average bill amount is about 148 CNY, with little difference between treatment and control tables (p-value is 58% in a formal t-test of equality of means); the average total number of dishes ordered is about 4.6, with the average for the control tables (4.76) being slightly larger than that for the treatment tables (4.49), with the p-value for the equality of these means being 6%. The average total bill amount in the saliency treatment

[15] It is worth emphasizing, though, deletion of the 407 large bills only affects the calculations of the means for dishes ordered and bill amount, but does not at all affect subsequent analysis on the effect of observational learning on customers' choices. Including the 407 large bills would lead to significantly larger means for both dishes ordered and bill amount, inconsistent with what the restaurant managers would consider as being reasonable. The 800 CNY cutoff was suggested by the restaurant managers.

[16] The plaques were printed immediately and sent to their relevant locations, and put on display on October 23, 2006.

locations is about 146 CNY, again with negligible difference between treatment and control tables (p-value is 36%). Similarly, the difference in the saliency treatment locations between control and treatment tables in the pre-experiment week for the number of dishes ordered is also small.

Third, we can also test for the equality of means across ranking treatment and saliency treatment locations. The p-value for the t-test for the equality of means in the average bill amount across the two locations is 69%, and that for the average number of dishes ordered is 22%. Thus, at least in the two dimensions we examined there, we are quite confident that randomization is well implemented at both the site and table levels. [17]

[Table 2 About Here]

3.3 Experiment Week Data

The experiment with different information treatments was conducted in the eight-day period between October 23 and October 30 of 2006. We chose to include both weekdays and weekends in our experimental coverage period because weekends are significantly busier than weekdays; moreover, patrons on weekends include more tourists, which allows us to test the hypothesis that observational learning is more pronounced among infrequent patrons.

A total of 6,823 bills were collected in the experiment week. Noting that the experiment period ("week") actually lasted eight days while the pre-experiment period was seven days, it is quite remarkable that the average daily number of bills was quite similar between the two periods. Panel B of Table 2 provides some basic descriptive statistics of our experimental week data. As is clear from the comparison of Panel A and Panel B, the average bill amount and the average number of dishes ordered per bill do not seem to differ much between the pre-experiment and experiment week.

3.4 Data Structure and Empirical Specification

As we mentioned earlier, the raw data is organized according to each bill and each *ordered* dish. We create a dummy variable for each bill and *each dish on the menu,* which takes value 1 if a dish is ordered in that bill and 0 otherwise. Thus in our analysis below (reported in Tables 3-6), *an observation is a bill/dish combination.* For each observation, the dependent variable of interest is whether the dish is ordered in the bill, and the control variables include whether the dish is a top five dish in that location, whether the associated table is a treatment table, whether a treatment occurred, the total number of dishes ordered in the bill, and the total amount of the bill. In the most complete specification, we also include dish and location dummies. Robust standard errors clustered at the bill ID level are calculated.

[17] That is not to say that there are no differences across locations. As can be seen in Panel A of Table 2, the standard deviations for both the bill amount and the number of dishes ordered differ substantially between the ranking and saliency treatment locations.

Specifically, let P_{db} indicate the dummy with value 1 if dish d is ordered in bill b, and 0 otherwise, where $d \in \{1, ..., N_d\}, b \in \{1, ..., N_b\}$ with N_d and N_b denoting the total number of dishes and the total number of bills respectively. Thus the total number of observations in our regression analysis is $N_d \times N_b$. Of course these observations are dependent within a bill, so we calculate the robust standard errors clustered at the bill ID level. Depending on the specifications, we will write $\Pr(P_{db} = 1)$ either as a linear function or a Probit of a list of covariates.

4 Identification Strategy

Now we explain our identification strategy for our main results about the effect of observational learning on consumer choices.

The first empirical strategy relies only on the data from the experiment period (October 23-30, 2006). We compare the probabilities that top 5 dishes were ordered on the ranking treatment tables and control tables in ranking treatment locations to estimate the effect of "being displayed as a top 5 dish," or the *total ranking treatment effect*. We then compare the probabilities that displayed dishes were ordered on the saliency treatment and control tables in the saliency treatment locations to estimate the saliency effect. The difference in the two estimates provides an unbiased estimate of observational learning effect (net of the saliency effect) under the assumption that genuine randomization was achieved *at both the table level* (within the ranking treatment locations and within the saliency treatment locations) *and at the site level* (that assigned sites into ranking and saliency treatment locations).

To see this, note that genuine randomization at the table level in the ranking treatment locations implies that the comparison of the demand of the top 5 dishes between the treatment and control tables in these locations estimates the sum of both observational learning effect and saliency effect (as being displayed as a top 5 dish provides both information and saliency to that dish). Similarly, genuine randomization at the table level in the saliency treatment locations implies that the comparison of the demand of the displayed sample dishes between the treatment and control tables in these locations estimates the saliency effect alone. Genuine randomization at the site level insures that the saliency effect estimated from the saliency treatment locations is an unbiased estimate of the saliency effect in the ranking treatment locations, thus allowing us to difference the saliency effect estimate from the saliency treatment locations from the total ranking treatment effect estimated from the ranking treatment locations to obtain the estimate of the net observational learning effect. The results from this Difference-in-Difference (DD) approach are presented in Section 5.1.1.

While Table 2 shows that randomization seems to be well implemented at both the table and site levels, there is always the possibility that there are unmeasured differences between the control and treatment tables and between the ranking and saliency treatment locations. The pre-experiment data from October 16-22 allows us to deal with the potential unmeasured differences between the control and treatment tables by implementing an additional layer of differencing that compares the sales of displayed dishes on the same table between the pre-experiment and experiment week. The results from this triple differencing (DDD) approach is presented in Section 5.1.2.

5 Results

In this section, we provide three results from our field experiment. The main result, presented in Subsection 5.1, is the finding of a significant observational learning effect, and a non-significant saliency effect. Almost the same magnitude of observation learning is estimated from the DD and DDD approaches, and from the linear probability and Probit specifications. The knowledge that a particular dish was among the top 5 dishes ordered by others increased the chance of the dish being ordered by an average ranging from 13 to 18 percent, but being merely mentioned as some sample dishes did not significantly boost their demand.

The second and third results use the subsample of bills for which we have post-dining surveys. The second result, presented in Subsection 5.2.2, shows that customers' dining experience improved in the ranking treatment tables, while there was no satisfaction gain in the saliency treatment tables. The third result, presented in Subsection 5.2.3, provides modest support for the theoretical prediction that observational learning is more pronounced among infrequent customers who do not have much information about the quality of the dishes.

5.1 Effect of Observational Learning on Choices

5.1.1 DD Estimation Results Using Only Experiment Period Data

[Table 3 About Here]

Table 3 analyzes data from the five ranking treatment locations during the experiment period of October 23-30. Recall that an observation here is a bill/dish combination and the dependent variable is a 0/1 dummy indicating whether a dish was ordered in the bill. In Table 3 (as well as Tables 4-6 and 8 below), Columns (1)-(3) report linear probability estimates and (4)-(6) report the *marginal effects* from Probit estimates. Within each specification, the columns differ by the covariates included: Column (1) and (4) only include a "Treat" dummy for the table where the bill was served, and a "Top 5" dummy for the dish, and an interaction for "Treat*Top 5" which is 1 only when the dish was a top 5 dish and the bill was served on a ranking treatment table; Columns (2) and (5) also control for the total number of dishes ordered in the bill, and the log of the total bill amount; and Columns (3) and (6) control for dish and location dummies, which absorb unobserved differences among the dishes and the locations (for example the price of the dish). The standard errors are robust standard errors with clustering at the bill ID level, thus they account for both heteroscedasticity and dependence in the dependent variables within a bill.

Clearly specifications reported in Columns (3) and (6) with dish and location dummies are our preferred specifications, but the qualitative and quantitative results are similar across specifications. Let us discuss Column (3) for illustration. First note that, not surprisingly, the "Top 5" dummy coefficient indicates that top 5 dishes were about 13.8 percentage points more likely to be chosen than non-top 5 dishes on *control tables* in the ranking treatment locations. However, the coefficient estimate of "Treat*Top 5" indicates that *top 5 dishes at the treatment tables, where the rankings were displayed,* were ordered with an *additional 2.1 percentage points* relative to non-top 5 dishes. This relationship holds after controlling for dish and location dummies. In order to gauge the magnitude of this effect, however, we must come up with an estimate of the base probability that top 5 dishes were ordered. This is not transparent in Column 3 because we need to include the dish dummy coefficients corresponding to the top 5 dishes in each location. However, examining the coefficient estimates in specifications (1) we can conclude that the base average probability that top 5 dishes were ordered in control tables was about 16.2 percent (11.7 from the "Top 5" dummy coefficient and 4.5 from the constant). Thus, displaying a dish as a top 5 dish increases its demand by about 13 percent. The coefficient estimates for this effect is statistically significant at 1%.[18]

Note that the 13 percent increase in the demand of top 5 dishes in the ranking treatment tables includes potentially *both* the observational learning effect and saliency effect. Next we examine the saliency effect only.

[Table 4 About Here]

Table 4 reports analogous regression results using the experiment period data from the saliency treatment locations. Any effect on the demand of the "displayed " dishes on the treatment tables will be considered as simply the saliency effect. These dishes were displayed with no information about their popularity.

Because the five displayed sample dishes always included the actual top 3 dishes together with two randomly selected dishes (in a randomly mixed order), the displayed dishes were 7.5 percentage points more likely to be chosen than non-displayed dishes at the control tables (from Row 2 of Table 4). However, the coefficient estimate for the interaction term "Treat*Displayed" is small, less than 1 percentage points, and statistically insignificant, in all specifications.

It will be useful to compare the treatment effect on the demand of Top 3 dishes between the ranking treatment locations and the saliency treatment locations. In un-reported regressions, we indeed ran these specifications, and found that the demand of top 3 dishes increase was slightly more pronounced than that we found for top 5 dishes in the ranking treatment locations, and the

[18] It is also worth mentioning that the coefficient estimate of "Treat" is negative and statistically significant 0.1 percentage point. That is, non-top 5 dishes' demand is lower in treatment tables. This reflects a substitution effect in the treatment tables: as customers switch their demand to top 5 dishes, the demand for other non-top 5 dishes in these tables is suppressed.

effect remained statistically significant at 1% level.[19] For the saliency treatment, the demand of top 3 dishes that were merely displayed as sample dishes was not affected at all, with the coefficient estimate for "Treat*Displayed" remaining small and statistically insignificant.

To summarize, our finding in Table 4 indicates that being made salient, i.e., being displayed on a plaque, does not significantly attract consumers to order these displayed dishes, even for those displayed dishes that were in fact top 3 dishes. Thus, at least in our restaurant setting, saliency effect is small or almost zero,.[20] Under our assumption that saliency effect in the saliency treatment locations is an unbiased estimate of the saliency effect in ranking treatment locations (which is true when randomization at the site level is well implemented), then our finding in Table 3 of a significant treatment effect is close to the net observational learning effect. Of course, we can combine the data from the ranking treatment and saliency treatment locations and run single regressions with a triple interaction "Treat*Displayed* Ranking Treatment Locations" to obtain an estimate of the net observational effect. We do not report these regression results here for space, but the magnitude for the coefficient estimate of the above triple interaction is, not surprisingly, similar to those we found in Table 3 for "Treat*Top 5" and remains statistically significant at 1% level.[21]

5.1.2 DDD Estimation Results Using Both Pre-Experiment and Experiment Period Data

Despite our best effort to randomize over the tables within each site, one might still be concerned that potential unmeasured differences between control and treatment tables, e.g., the treatment tables might be more centrally located and thus might have a better view of what others were ordering, may favor the top 5 dishes being displayed. We deal with this potential concern by including the pre-experiment week data using a triple differencing strategy. We calculate the change in the demand of the top 5 dishes in the treatment tables from the pre-experiment week to the experiment week, and use the change in the demand of the top 5 dishes in the control tables between the same periods as a benchmark to measure the temporal unobservable changes in demand within the two week period. This DDD estimation strategy will be valid under a different identifying assumption, namely, under the assumption that the temporal unobservable changes in the demand of the top 5 dishes within the two-week period were identical for the control and treatment tables. Such an assumption is impossible to verify, but it is definitely plausible.

[Table 5 About Here]

In Table 5, we use data for the five ranking treatment locations from both the pre-experiment and experiment weeks. For each bill, even if it occurred in the pre-experiment week, we categorize it

[19] The results are available from the authors upon request.

[20] The finding of negligible saliency effect certainly counters our priors.

[21] These results are available from the authors upon request.

into whether or not the bill was served at a treatment table according to its table's treatment/control assignment in the experiment week. Then we define a new dummy variable "after" to indicate whether or not the bill occurred in the experiment week. Thus, the key coefficient of the triple interaction term "Treat*Top 5*After", which is a "difference-in-difference" estimator of the effect of ranking display on the demand of the displayed top 5 dishes: the first difference is the difference in sales probability of top 5 dishes on the tables selected for treatment and on the tables selected as control, separately for the pre-experiment and experiment weeks; the second difference is the difference of the above first difference between the pre-experiment and experiment weeks. This DD estimator eliminates potential unobservable differences among treatment and control tables and will provide a consistent estimate of the top 5 display effect as long as the unobservable differences among the treatment and control tables are not affected by the information displays, which is a highly plausible assumption.

Focusing again on our preferred specification in Column (3) of Table 5 where we control of dish and location dummies, we note that the coefficient estimate for the triple interaction term "Treat*Top 5*After" in this OLS specification is 3%, which is statistically significant at 1% level and the magnitude is larger than the estimate of the interaction term "Treat*Top 5" in Column (3) of Table 3. The 3 percentage point estimate of the total ranking treatment effect represents an almost 18% increase in the demand for the top 5 dishes.

The results for other specifications are similar. This indicates that, if anything, the sales of top 5 dishes on the tables selected for treatment were not as good as those in control tables in the pre-experiment week, which is indeed reflected in the negative estimate of the term "Treat*Top 5".

Thus, the estimated effect of ranking display on the demand of top 5 dishes using the DD approach and both weeks of data is very similar to those we found using just a single difference with only the experiment week data.

[Table 6 About Here]

We analogously report the estimate of the saliency effect using the DD estimator and both weeks of data. In Table 6, the triple interaction "Treat*Displayed*After" is estimated to be positive, but it is tiny in magnitude and statistically insignificant in all specifications, thus confirming that our previous finding in Table 4 about the insignificant saliency effect on the demand for the displayed dishes (without information about popularity) is not due to systematic differences between control and treatment tables in the saliency treatment locations.

5.1.3 Summary and A Caveat

To summarize, we find that, depending on specifications, the demand for the top 5 dishes was increased by an average of about 13 to 18 percent when these popularity rankings were revealed to the customers; in contrast, being merely mentioned as some randomly selected dishes did not significantly boost the demand for these mentioned dishes. In other words, we find that the saliency effect is positive but very small and statistically insignificant. Thus the demand increase for the top 5 dishes in the ranking treatment was mostly due to observational learning.

Our finding of significant observational learning has to be understood with an important caveat that may lead to biased estimates for the observational learning effect and saliency effect. This caveat is related to the customers' perception of the restaurant's motivation in putting up these information displays. Even though in our field experiment we used the pre-experiment week data to come up with the genuine top 5 dishes according and displayed them in the ranking treatment locations, the consumers might be suspicious of whether such rankings were true rankings, or were fabricated by the restaurant to boost sales of these dishes. Such suspicion might dilute the true observational learning effect on the customer's demand. Of course, customers might also be suspicious of the motives of the restaurant regarding the display of five sample dishes in the saliency treatment locations, such suspicion would lead to a downward bias in our estimate of the saliency effect.[22] While it is impossible to precisely evaluate the degree of downward biases in the ranking and saliency treatment locations, it seems to be plausible that the ranking treatment is likely to be met with more suspicion than the saliency treatment.

5.2 Results Using the Survey Data

As we mentioned earlier, for about 20% of the randomly selected bills, we administered a short post-dining survey filled out by the customer who paid the bill. This survey data is then merged with the detailed choice and expenditure data of the bill using the bill ID. We use this merged data to ask two questions. First, we investigate whether providing information about others' choices (as in the ranking treatment) improved the subjective dining satisfaction. Second, we investigate the hypothesis that infrequent visitors, who had more diffuse priors about the quality of dishes, might be more prone to the influence of others' choices.

5.2.1 Descriptive Statistics of the Survey Data

[Table 7 About Here]

We received 644 and 693 surveys respectively for the ranking and saliency treatment locations during the experiment period of October 23-30. Table 7 provides descriptive statistics of the survey, again broken down by the ranking treatment and saliency treatment locations. From Table 7, the customers of the kinds of locations were quite similar. For the first question about the cumulative times that the bill payer had visited the restaurant chain, 36.3% in the ranking treatment locations (and respectively 38.66% in the saliency treatment locations) reported less than 5 times, which

[22] Such concerns are not new, of course, because they are closely related to "intent to treat" and "compliance" in the policy evaluation and clinical trial evaluation literatures (see, e.g., Heckman and Vytlacil 2001).

we define as *infrequent* visitors. Respectively 40% and 45% of the bill payers in the ranking and saliency treatment locations were female; and the age and education attainment configuration of the customers were also similar across the two locations with about 82% of the customers below 40 years old and close to 70% being two year or four year college graduates. There seemed to be more tourists in the saliency treatment locations with 19% of their customers coming from outside of Beijing, in contrast to 11% in the ranking treatment locations. Finally, overall, about 35% of the customers expressed to be "very satisfied" in both ranking and saliency treatment locations.

5.2.2 Effect of Observational Learning on Subjective Dining Satisfaction

[Table 8 About Here]

Table 8 presents our results about the effect of "top 5" ranking displays on the customers' dining satisfaction, in contrast to that of the "5 sample dishes." Different from Tables 3-6, here an observation is a surveyed bill, instead of a bill/dish combination. The reported standard errors are robust and clustered at the table level (instead of the bill ID level previously). The dependent variable is a dummy that takes value 1 if the customer chose "Very Satisfied" for Question 8 of the survey. The covariates included vary by specifications. Panel A reports that customers seated on treatment tables with ranking information displays were 8.3 to 9 percentage points more likely to summarize their dining experience as being "Very Satisfied" than those seated at control tables in the ranking treatment locations. The coefficient estimates for the "Treat" dummy are statistically significant at least at 5% level for all specifications. In contrast, Panel B reveals that in the saliency treatment locations, those seated at treatment tables which displayed five randomly selected dishes were statistically no more satisfied than those seated at control tables.[23]

Unfortunately we could not distinguish whether those at ranking treatment tables reported "Very Satisfied" with higher frequency because the ranking display made their dish choice *easier* or *better*, even though we do find that in ranking treatment tables those who reported "Very Satisfied" are more likely to order one or more of the top dishes than those who did not report "Very Satisfied."

5.2.3 Is Observation Learning More Important for Infrequent Customers?

[Table 9 About Here]

Now we use the survey data merged with the detailed bill information to ask whether customers who were relatively unfamiliar with the restaurant were more likely to be influenced in their choices

[23] Qualitatively similar results are obtained when we use ordered Probit. We find that customers at treatment tables in the ranking treatment locations were more likely to be "very satisfied" than those at control tables, but no statistically significant effects are found in the saliency treatment locations. Results are available from the authors upon request.

by the knowledge of others' choices. In Table 9, an observation is again a bill/dish combination, but this time we only use the subsample for which we have surveys. We only use data from the ranking treatment locations in the experiment week, and only report the OLS specifications. We first define a dummy variable *"frequent"* which takes value 1 if the survey respondents reported to have visited the restaurant for 6 or more times, and 0 otherwise.[24]

Column (1) of Table 9 just replicates Column (1) of Table 3 using the whole sample, and Column (2) shows the result for the subsample with the same specification. As can be seen, the basic observational learning effect found in Column (1) for the whole sample survives in the subsample, though the statistical significance drops from 1% to 5%. The key result in Table 9 is Column (3) where we add an interaction term "Treat*Top 5*Frequent" to allow for the observational learning effect to differ by whether or not the customer was a frequent visitor to the restaurant. The coefficient estimate is small negative 0.4 percentage point, and is statistically significant at 5% level. Thus we conclude that the data provides modest support that the choices of frequent visitors were less affected by the observation of others' choices, consistent with the theoretical predictions of observational learning models.

6 Conclusion

In this paper we present results about the effects of observational learning on individuals' behavior and subjective well-being in the context of restaurant dining from a randomized natural field experiment. Our experimental design aims to distinguish observational learning effect from saliency effect. We find that the demand for the top 5 dishes was increased by an average of about 13 to 18 percent when these popularity rankings were revealed to the customers; in contrast, being merely mentioned as some sample dishes did not significantly boost their demand. Moreover, we find that, consistent with theoretical predictions, some modest evidence that observational learning effect was stronger among infrequent customers. We also find that customers' subjective dining experiences were improved when presented with the information about the top choices by other consumers, but not when presented with the names of some sample dishes.

[24] We have experimented with alternative ways of creating the "frequent" dummy. We only get modestly significant estimate for "Treat*Top 5*Frequent" interaction if we define "frequent" according to whether the cumulative visits are more or less than 6, even though we always get the same negative sign. One possible reason is that 6 visits are needed in order for a customer to be familiar enough about the menu so as not to be less influenced by the ranking information. Another reason is that using the 6 cutoff yields sufficient numbers of 0 and 1 for the "frequent" dummy in order to get statistical significance.

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A Appendix: Post-Dining Survey Questionnaire

The simple post-dining survey questionnaire includes the following eight questions and it took on average less than one minute to complete.

1. How many times have you dined in this restaurant (including other branches of Mei Zhou Dong Po)?

   a.___first time; b.___ 2-5 times; c. ___ 6-10 times; d. ___ more than 10 times.

2. Your Gender: a.___ male; b.___ female.

3. Your Age: a.___ 20-30; b. ___ 31-40; c. __ 41-50; d. __ 51-60

4. Your Occupation:______

5. What is your level of education? a.___ High school; b.___ 2 year college; c. __ 4 year university; d. __ Graduate degree.

6. Which province were you born?_______

7. Do you work in Beijing? a.___ Yes; b.___ No.

8. Overall, how would you rate the dining experience? a.___ Very satisfied; b.___ Satisfied; c.___ So so; d.___ Not satisfied.

Table 1: Experimental Design

	Ranking Treatment Locations (5 Locations)	Saliency Treatment Locations (4 Locations)
	Pre-Experiment Week (October 16-22, 2006)	
Control Tables	No Display	No Display
Treatment Tables	No Display	No Display
	Experiment Period (October 23-30, 2006)	
Control Tables	No Display	No Display
Treatment Tables	Display a plaque showing ***five most popular dishes*** *	Display a plaque showing ***five sample dishes*****

*: The five most popular dishes are displayed in the order of their rankings, with No. 1 listed first.

**: The five sample dishes always include the actual top 3 dishes (without being revealed as such) and two other randomly selected dishes. They are displayed in random order in the display.

Table 2: Descriptive Statistics in the Pre-Experiment and Experiment Data

	Ranking Treatment Locations (5 Locations)		Saliency Treatment Locations (4 Locations)	
Variables	Mean	Std. Dev.	Mean	Std. Dev.
	Panel A: Pre-Experiment Data (October 16 - 22)			
Total Bill Amount (CNY)	148.4	138.4	145.6	116.7
Treatment Tables	147.2	135.6	143.6	119.5
Control Tables	149.7	141.8	147.7	113.9
Total Number of Dishes Ordered	4.61	3.78	4.59	5.06
Treatment Tables	4.49	3.77	4.45	4.82
Control Tables	4.76	3.78	4.74	5.29
Total Number of Bills	3401		2671	
Treatment Tables	1865		1336	
Control Tables	1536		1335	
	Panel B: Experiment Data (October 23-30)			
Total Bill Amount (CNY)	142.6	133.2	147.9	121
Treatment Tables	139.2	129.1	146.8	118.8
Control Tables	146.9	138	149	123.2
Total Number of Dishes Ordered	4.91	3.77	4.9	4.48
Treatment Tables	4.72	3.75	4.83	4.36
Control Tables	5.15	3.78	4.96	4.59
Total Number of Bills	3954		2869	
Treatment Tables	2182		1418	
Control Tables	1772		1451	

Notes: An observation is a bill.

Table 3: Effect of Ranking Treatment on the Demand of "Top 5" Dishes: Using Experiment Week Data Only (October 23-30)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	Probit	Probit	Probit
Treat	-0.005	-0.001	-0.001	-0.006	-0.002	-0.002
	(0.001)***	(0.001)	(0.001)*	(0.001)***	(0.001)***	(0.000)***
Top 5	0.117	0.117	0.138	0.113	0.11	0.107
	(0.004)***	(0.004)***	(0.006)***	(0.004)***	(0.004)***	(0.007)***
Treat * Top 5	0.018	0.017	0.021	0.0115	0.0102	0.0102
	(0.006)***	(0.006)***	(0.006)***	(0.003)***	(0.003)***	(0.002)***
Total No. of Dishes Ordered		0.014	0.013		0.009	0.008
		(0.000)***	(0.000)***		(0.000)***	(0.000)***
Log of Total Bill Amount		0.001	0.00016		0.00043	-0.0001
		(0.000)***	(0.00012)		(0.00017)**	(0.0001)
Constant	0.045	-0.012	-0.026			
	(0.001)***	(0.001)***	(0.021)			
Dish Dummy	No	No	Yes	No	No	Yes
Location Dummy	No	No	Yes	No	No	Yes
No. of Observations	235052	235052	235052	235052	235052	235052
R-squared	0.02	0.05	0.07			

Notes:

1. An observation is a bill and dish combination. See data section for data construction.
2. For Probits in Columns (4)-(6), the reported coefficients are ***marginal effects*** at the mean.
3. Robust standard errors, culstered at the Bill ID are reported in parentheses.
4. *, ** and *** represents respectively significance at 10%, 5% and 1%.

Table 4: Effect of Saliency Treatment on the Demand of "Displayed" Dishes: Using Experiment Week Data Only (October 23-30)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	Probit	Probit	Probit
Treat	0.001	-0.0003	-0.0005	0.001	-0.0004	-0.0005
	(0.0008)	(0.0004)	(0.0004)	(0.0009)	(0.;0005)	(0.0004)
Displayed	0.0754	0.0758	0.0679	0.0763	0.0746	0.078
	(0.0038)***	(0.0038)***	(0.005)***	(0.0038)***	(0.0038)***	(0.008)***
Treat * Displayed	0.0077	0.0078	0.0076	0.0026	0.0027	0.00215
	(0.0056)	(0.0056)	(0.0056)	(0.0025)	(0.0025)	(0.00211)
Total No. of Dishes Ordered		0.0118	0.0121		0.0087	0.0079
		(0.0001)***	(0.0001)***		(0.0002)***	(0.0002)***
Log of Total Bill Amount		0.0001	-0.0000		-0.0001	-0.0002
		(0.0001)	(0.0001)		(0.0001)	(0.0001)**
Constant	0.0316	-0.0028	-0.0023			
	(0.0006)***	(0.0004)***	(0.0058)			
Dish Dummy	No	No	Yes	No	No	Yes
Location Dummy	No	No	Yes	No	No	Yes
No. of Observations	181868	181868	181868	181868	181868	181868
R-squared	0.01	0.02	0.04			

Notes:

1. An observation is a bill and dish combination. See data section for data construction.
2. For Probits in Columns (4)-(6), the reported coefficients are ***marginal effects*** at the mean.
3. Robust standard errors, culstered at the Bill ID are reported in parentheses.
4. *, ** and *** represents respectively significance at 10%, 5% and 1%.

Table 5: Effect of Ranking Treatment on the Demand of "Top 5" Dishes: Using Data from Both Pre-experiment and Experiment Weeks (October 16-30)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	Probit	Probit	Probit
Treat	-0.0003	0.00127	0.00127	-0.0003	0.0005	0.0005
	(0.0012)	(0.00065)**	(0.0005)**	(0.0014)	(0.0006)	(0.0005)
After	0.00358	-0.0011	-0.00065	0.00378	0.00064	0.0012
	(0.0013)***	(0.0007)	(0.00059)	(0.00138)***	(0.00064)	(0.0005)**
Top 5	0.1174	0.1162	0.1433	0.118	0.111	0.1177
	(0.00467)***	(0.0046)***	(0.0055)***	(0.004)***	(0.004)***	(0.006)***
Treat * After	-0.0048	-0.0021	-0.0022	-0.005	-0.0025	-0.0026
	(0.0017)***	(0.001)**	(0.0008)***	(0.002)***	(0.0008)***	(0.0007)***
Top 5 * After	-0.0003	0.0012	0.00158	-0.002	-0.001	-0.0012
	(0.0065)	(0.0064)	(0.0064)	(0.003)	(0.002)	(0.0021)
Treat * Top 5	-0.0123	-0.0128	-0.0114	-0.0047	-0.0045	-0.0037
	(0.006)**	(0.006)**	(0.006)*	(0.0023)**	(0.0022)**	(0.002)**
Treat * Top 5 * After	0.0302	0.0302	0.0320	0.0174	0.016	0.0149
	(0.00852)***	(0.008)***	(0.0084)***	(0.0044)***	(0.004)***	(0.0039)***
Total No. of Dishes Ordered		0.0142	0.0136		0.0086	0.0074
		(0.0002)***	(0.0001)***		(0.0001)***	(0.0001)***
Log of Total Bill Amount		0.0069	0.0003		0.0002	-0.0001
		(0.0001)***	(0.0001)***		(0.0001)**	(0.0007)*
Constant	0.0414	-0.0127	-0.0765			
	(0.0096)***	(0.0079)***	(0.0238)***			
Dish Dummy	No	No	Yes	No	No	Yes
Location Dummy	No	No	Yes	No	No	Yes
Observations	448371	448371	448371	448371	448371	448371
R-squared	0.02	0.05	0.07			

Notes:

1. An observation is a bill and dish combination. See data section for data construction.
2. For Probits in Columns (4)-(6), the reported coefficients are ***marginal effects*** at the mean.
3. Robust standard errors, culstered at the Bill ID are reported in parentheses.
4. *, ** and *** represents respectively significance at 10%, 5% and 1%.

Table 6: Effect of Saliency Treatment on the Demand of "Displayed" Dishes: Using Data from Both Pre-experiment and Experiment Weeks (October 16-30)

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	Probit	Probit	Probit
Treat	-0.0001	-0.0004	-0.0006	-0.0002	-0.0004	-0.0006
	(0.0007)	(0.0004)	(0.0004)	(0.0009)	(0.0005)	(0.0004)
After	-0.0007	-0.0008	-0.0005	-0.00075	-0.0006	-0.0004
	(0.0008)	(0.0004)**	(0.0004)	(0.0009)	(0.0005)	(0.0004)
Displayed	0.070	0.0708	0.0625	0.0704	0.069	0.0685
	(0.0039)***	(0.0039)***	(0.0047)***	(0.004)***	(0.004)***	(0.006)***
Treat * After	0.0011	0.0001	0.0001	0.0012	-0.00006	0.00009
	(0.0012)	(0.0006)	(0.0006)	(0.0013)	(0.0007)	(0.0006)
Displayed * After	0.005	0.0050	0.0052	0.0027	0.00237	0.0023
	(0.0054)	(0.0055)	(0.0055)	(0.0026)	(0.0025)	(0.0022)
Treat * Displayed	0.0057	0.0058	0.00567	0.0026	0.00247	0.0021
	(0.0057)	(0.0057)	(0.00568)	(0.0027)	(0.00260)	(0.0023)
Treat * Displayed * After	0.00199	0.0020	0.00196	-1.68e-6	0.00018	0.00004
	(0.00795)	(0.0080)	(0.0080)	(0.00353)	(0.00339)	(0.0029)
Total No. of Dishes Ordered		0.0118	0.0122		0.0088	0.008
		(0.001)***	(0.0001)***		(0.0001)***	(0.0001)***
Log of Total Bill Amount		0.0001	-0.00002		-0.00007	-0.00017
		(0.000)**	(0.00005)		(0.00007)	(0.00005)***
Constant	0.0323	-0.0022	-0.0155			
	(0.0006)***	(0.0004)***	(0.00296)***			
Dish Dummy	No	No	Yes	No	No	Yes
Location Dummy	No	No	Yes	No	No	Yes
Observations	346649	346649	346649	346649	346649	346649
R-squared	0.01	0.02	0.04			

Notes:

1. An observation is a bill and dish combination. See data section for data construction.
2. For Probits in Columns (4)-(6), the reported coefficients are ***marginal effects*** at the mean.
3. Robust standard errors, culstered at the Bill ID are reported in parentheses.
4. *, ** and *** represents respectively significance at 10%, 5% and 1%.

Table 7: Descriptive Statistics of the Survey Data

	Ranking Treatment Locations (644 Surveys)	Saliency Treatment Locations (693 Surveys)
Survey Q1: How many times have you dined in this restaurant?		
First time	14.29	18.15
2-5 times	22.54	20.51
6-10 times	13.55	11.88
10+ times	49.63	49.46
Survey Q2: Your gender?		
Male	60.39	55.22
Female	39.61	44.78
Survey Q3. Your age?		
20-30	36.13	39.36
31-40	45.87	42.22
41-50	13.19	14.39
51-60	4.81	4.02
Survey Q5: Do you have a college and higher degree?		
high school	10.09	14.8
2 year college	23.83	28.82
4 year uinversity	45.65	35.76
graduate degree	20.44	20.62
Survey Q7: Do you work in Beijing?		
Yes	88.7	80.92
No	11.3	19.08
Survey Q8: Overall, how would you rate the dining experience?		
Very Satisfied	34.31	35.8
Satisfied	58.95	54.05
So so	6	9.6
Not satisfied	0.74	0.55

Notes: Percentage reported.

Table 8: Effects on Dining Satisfaction: Ranking Treatment vs. Saliency Treatment

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS	OLS	OLS	Probit	Probit	Probit
			Panel A: Ranking Treatment			
Treat	0.0833	0.0898	0.0891	0.0833	0.0906	0.0899
	(0.0428)**	(0.0425)**	(0.0428)**	(0.0428)**	(0.0429)**	(0.0430)**
Obsevations	644	640	640	644	640	640
R-squared	0.0074	0.0190	0.0198			
			Panel B: Saliency Treatment			
Treat	0.0261	0.0271	0.0280	0.0261	0.0244	0.0258
	(0.0370)	(0.0372)	(0.0372)	(0.0370)	(0.0362)	(0.0360)
Obsevations	693	680	680			
R-squared	0.0024	0.0096	0.0118			
Additional Controls:						
Age Interval Dummies	No	Yes	Yes	No	Yes	Yes
College Dummy	No	No	Yes	No	No	Yes
Gender Dummy	No	Yes	Yes	No	Yes	Yes
Tourist Dummy	No	No	Yes	No	No	Yes
Cumulative Visits	No	No	Yes	No	No	Yes
Constant	Yes	Yes	Yes	Yes	Yes	Yes

Notes:

1. An observation is a bill. The dependent variable is a dummy indicating "very satisfied".
2. Data from the experiment week is used in this analysis.
3. For Probits in Columns (4)-(6), the reported coefficients are ***marginal effects*** at the mean.
4. Robust standard errors, culstered at the table level are reported in parentheses.
5. *, ** and *** represents respectively significance at 10%, 5% and 1%.

Table 9: Frequent Customers Respond Less in the Ranking Treatment

	(1)	(2)	(3)
	Whole Sample	**Survey Sample**	**Survey Sample**
Treat	-0.005	-0.006	-0.005
	(0.001)***	(0.0057)	(0.0059)*
Top 5	0.117	0.122	0.119
	(0.004)***	(0.012)***	(0.013)***
Treat * Top 5	0.018	0.019	0.0196
	(0.006)***	(0.009)**	(0.009)**
Treat * Top 5 * Frequent			-0.0004
			(0.0002)**
Constant	0.045	0.043	0.043
	(0.001)***	(0.005)***	(0.005)***
Dish Dummy	No	No	No
Location Dummy	No	No	No
No. of Observations	235052	48843	48843
R-squared	0.021	0.022	0.0223

Notes:

1. An observation is a bill and dish combination. All regressions are OLS regressions.
2. The variable "Frequent" is a dummy that takes value 1 if the answer to post-dining survey Q1 is either a (first time) or b (2-5 times).
3. Robust standard errors, culstered at the Bill ID are reported in parentheses.
4. *, ** and *** represents respectively significance at 10%, 5% and 1%.