Observational Learning: Evidence from a Randomized Natural Field Experiment

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March 10, 2008

Abstract

We report results from a randomized natural field experiment conducted in a restaurant dining setting to distinguish the observational learning effect from the saliency effect. We find that, when customers are given ranking information of the five most popular dishes, the demand for those dishes increases by 13 to 20 percent. We do not find a significant saliency effect. We also find modest evidence that the observational learning effects are stronger among infrequent customers, and that dining satisfaction is increased when customers are presented with the information of the top five dishes, but not when presented with only names of some sample dishes. (*JEL* D83, C93)

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Social learning has attracted increasing attention in the economics literature. The general concept of social learning encompasses many mechanisms through which individuals may learn from others. In particular, it includes the mechanism in which individuals learn from each other through direct (formal or informal) communications; it also includes the mechanism of observational learning where the behavior of individuals is influenced by their observation of other people's choices because of the information contained therein.[1]

Convincing empirical evidence about the importance of observational learning is not only relevant for the theoretical literature in economics, it also has policy implications. The key difference between direct communications and observational learning as channels of social learning lies in whether temporal, spatial and social proximity among individuals is important for learning to occur. Observational learning can take place as long as the underlying decision problems faced by individuals are similar; in contrast, learning from others via direct communications requires individuals to be close in time, space and social distance. As a result, if a policy maker wants to, say, expedite the adoption of an advantageous technology, an information campaign about the technology's popularity among other groups of agents will be effective if observational learning is important, but will not be effective if instead direct communication is the main channel of social learning.

Despite the intuitive appeal of observational learning, to empirically establish that an individual's decisions are affected by the observation of others' choices because of its informational content is complicated by at least two plausible confounding mechanisms. The first is the *saliency effect*. The term "saliency" is widely used in the perceptive and cognitive psychology literature to refer to any aspect of a stimulus that, for whatever reason, stands out from the rest.[2] Observing others' choices could make those choices more salient than the alternatives. When consumers are not aware of their entire choice set, the differential salience of the elements in the choice set may affect the decision-maker's choices.[3] As a result, a consumer may follow others' choices because they are more salient.[4] The second confounding mechanism is the *conformity effect*, that is, individuals may adopt the observed choices of others because they want to conform.[5]

[1] Albert Bandura (1977) wrote the pioneering book in psychology that started the research on social and observational learning. Abhijit Banerjee (1992) and Sushil Bikhchandani, David Hirshleifer and Ivo Welch (1992) are the seminal works in the economics literature on observational learning. Christophe P. Chamley (2004) provides a textbook treatment of the topic.

[2] See, for example, Douglas L. Medin and Brian H. Ross (1997).

[3] See Jing Li (2006) and David S. Ahn and Haluk Ergin (2007) for decision-theoretical models of unawareness.

[4] Note that saliency effect is also an informational effect. The key difference between observational learning and saliency effect is that the information is about the characteristics of the choices in the former, while it is about the choice set itself in the latter.

[5] The classic social psychology experiments documenting conformity in individuals' judgment are reported in Solomon E. Asch (1951, 1955). Morton Deutsch and Harold B. Gerard (1955) attempted to distinguish conformity and informational social influences on individual judgment. Robert B. Cialdini and Noah J. Goldstein (2004) reviewed the social influence literature in psychology. B. Douglas Bernheim (1994) discussed many conformity experiments and offered an economic model of conformity.

The goal of this paper is to provide direct evidence of observational learning using a randomized natural field experiment in a restaurant dining setting in Beijing, China.[6] The restaurant we choose for our experiment has a thick menu with about 60 hot dishes. In our experimental design, detailed in Section II, we randomly expose diners to one of three information conditions: in the control tables, we do not give diners any additional information about the dishes other than what is contained in the menu; in "ranking treatment" tables, diners are provided with a display with the names of the "top five" dishes sorted by the actual number of plates sold in the previous week; and in "saliency treatment" tables, diners are provided with a plaque simply listing the names of five "sample dishes." We analyze how the information conditions affect the choices of customers. The three information conditions allow us to separately estimate the saliency effect and the observational learning effect. Note that our experimental design does not directly address the conformity channel. We would like to argue that the choice of restaurant dining as our experimental setting and the fact that information provided to the current customers is about the choices of past customers likely make this channel less important.[7, 8]

To understand why Chinese restaurant dining is an almost ideal setting for our experiments, it is useful to start with a brief introduction about social customs about dining in China. The most important and distinctive feature of dining in China is that typically diners at a table share dishes, and one person is in charge of making the dish selections as well as paying for the whole group's bill.[9] Sharing the bill or separate billing is not common.[10] This fact is crucial because it allows us to use each dining party, or the table, as the unit of our analysis.

We choose restaurant dining as the setting for our experiment for two main reasons. First, in order to distinguish observational learning from learning via direct communications, it is crucial that we be generally confident that the subjects are not involved in any direct communications with others. In a particular outing, diners typically choose dishes without communicating with other diners other than those at their own table. They are certainly unable to communicate with past customers; thus we do not have to be concerned about direct communication occurring beyond our observation.[11] Second, for observational learning to take place, it is important that there be some

[6] See Glenn W. Harrison and John A. 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 List provides a useful categorization as well as comprehensive and updated list of papers in this literature.

[7] As is well-known since Asch (1955), conformity pressure seems to exert the most influence when individuals are forming opinions in the *presence and visibility* of others.

[8] It is our maintained assumption that individuals are unlikely to order certain dishes that were popular with other customers in the past due to conformity motives. We do not yet know of a clean design to separate observational learning from the conformity effect.

[9] The exception is formal business dining, in which the "host" usually does the ordering and his/her subordinate pays the bill. We deleted large bills suspected to be formal business dining in our analysis for this reason (see footnote 22 for more details).

[10] The social norm is that friends take turns paying the bills for the group in repeated interactions.

[11] Notice that the conversations that may occur *within* a table are not an issue because we are using the table as the unit of our analysis, which is justified by the Chinese dining custom we explained above.

commonality in the decision problems of the subjects and others; diners, though with potentially different tastes, all care about the common quality of the dishes. Restaurant dining also offers other advantages. First, because of computerization, it is very easy to obtain information about diners' choices; second, it is relatively easy to implement randomized treatments in terms of diners' information set; third, we can observe the treatment effects of information displays on subjects' choices accurately and instantly; fourth, we can survey on the spot the effect of information displays on customers' subjective dining experience.[12]

Our experiment was conducted in a Szechuan restaurant chain *"Mei Zhou Dong Po"* (MZDP). MZDP is a restaurant chain with 13 sites in Beijing. Each location has an average of 50 tables; all locations have identical menus with about 60 hot dishes (and many additional cold dishes); however, the popularity of dishes varies by locations. The restaurant is medium-scale both in quality and price, popular for both leisure and ordinary business dining.

We find that, depending on the specifications, the demand for the top 5 dishes is increased by an average of about 13 to 20 percent when the top 5 popularity rankings are revealed to the customers; in contrast, being merely mentioned as some sample dishes does not significantly boost their demand. Moreover, we find some modest evidence that the observational learning effect is stronger among infrequent customers, and that customers' subjective dining experiences are improved when presented with the information about the top choices by other consumers, but not when presented with the names of some sample dishes.

The remainder of the paper is structured as follows. Section I summarizes the related literature; Section II describes our experimental design; Section III describes the data and two identification strategies; Section IV presents the results; and Section V concludes.

I. Related Literature

Our paper is mostly related to a paper by Matthew J. Salganik, Peter S. Dodds and Duncan J. Watts (2006). In an artificial music market, subjects (recruited from visitors to a particular website) are shown a menu of forty-eight songs sorted either randomly or by the number of downloads, and they are asked to listen to, rate and download as many songs as they wished. Their focus is on how social influence may lead to unpredictable outcomes for popular cultural products. Our paper differs from theirs in at least two respects. First, their experimental design does not distinguish the informational content of the download rankings from the saliency effect. Second, conformity effects are likely more severe in their setting. Social influence is an important determinant of the demand for popular cultural products because shared experience is a major component of the utility from consuming such goods; in contrast, restaurant dining is a more private experience. Another related study is by Catherine Tucker and Juanjuan Zhang (2007). They use a field experiment on the internet-portal for wedding service vendors to examine whether popularity rankings for the vendors in terms of past clicks affect customers' clicking behavior. They are mostly interested in whether

[12] We indeed conducted a short survey at the end of the meals (see Section II for details).

the internet leads to a "long tail" effect, i.e., more customers buy the low-volume products; but they also attempted to distinguish observational learning from the saliency effect. A major difference from our paper, however, is that in their experimental design, different information conditions were implemented in different wedding service categories; thus they had to rely on comparisons across different service categories to isolate the observational learning effect.

There are also several papers that examined social learning and informational cascades in laboratory settings (for example, Lisa R. Anderson and Charles A. Holt 1997; Boğaçhan Çelen and Shachar Kariv 2004). Jonathan E. Alevy, Michael S. Haigh and List (2007) compare the behavior of professional traders from the Chicago Board of Trade and student subjects in artificial laboratory experiments similar to those of Anderson and Holt (1997). These experiments all have very simple choice sets; thus the saliency of 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 Charles 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 different strategies with varying degrees of success.[13] One approach is to examine the different implications of social learning and common unobservable shock. For example, Conley and Udry (2005) show 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 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 (2006) who examine the health plan choices of University of California employees where he showed that social effects still exist after controlling for department fixed effects. 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 assign different information sessions about 401k options to individuals and find that their 401k participation de-

[13] An incomplete list of studies of social learning effects includes, in the context of crime (Edward L. Glaeser, Bruce Sacerdote and José Scheinkman 1996), contraception (Kaivan Munshi and Jacques Myaux 2006), adoption of seeds, fertilizer and other technologies (Timothy Besley and Anne Case 1994; Andrew D. Foster and Mark R. Rosenzweig 1995; Timothy G. Conley and Christopher Udry 2007; Michael Kremer and Ted Miguel 2003), labor market outcomes (Patrick Bayer, Stephen L. Ross and Giorgio Topa 2005), retirement saving plan choices (Esther Duflo and Emmanuel Saez 2002), health insurance plan (Alan T. Sorensen 2006), consumer demand (Markus M. Mobius, Paul Niehaus and Tanya S. Rosenblat 2005; Enrico Moretti 2008) and voting in sequential primaries (Brian Knight and Nathan Schiff 2007).

cisions have significant effects on their coworkers, consistent with their non-experimental evidence (Duflo and Saez 2002).[14] None of the above papers, however, attempts to distinguish observational learning from other forms of social learning. 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.[15]

II. Experimental Design

Experimental Design. In our experiment, diners are randomly assigned to tables with three different information conditions. We first describe these information conditions and then explain the two-stage randomization that we implement in the field experiment.

The first group of tables are the "*control*" tables where no additional information about the dishes other than the regular menu is displayed on the tables. The second group are the "*ranking treatment*" tables where we place a 19 cm by 12 cm plastic plaque on the table displaying the names of the five most popular dishes sorted by the actual number of plates sold in the previous week in that location, with the No. 1 dish listed on top. The actual numbers of plates sold are not displayed. Note that the top 5 rankings may differ by locations. The third group of tables are called the "*saliency treatment*" tables where we place a same-size plastic plaque on the table displaying the names of five sample dishes from the menu, sequenced in a random order.[16] In order to assess whether the saliency effect differs by the popularity of dishes, we choose to include the names of the actual top 3 dishes at that site (without being revealed as such) together with two other randomly selected dishes.[17]

We implemented a two-stage randomization strategy where the first-stage randomization was at the level of restaurant sites, and the second stage was at the level of tables within a site. Specifically, in the first stage, we randomly selected 5 locations where tables in each location were subsequently randomized into control tables and ranking treatment tables; and we randomly selected 4 other locations where tables in each location were subsequently randomized into control tables and saliency treatment tables.[18]

It is useful to comment on our choice of the two-stage randomization described above, instead of a single-stage randomization at the table level. The key issue for a single-stage randomization at the table level is that it will lead to the presence of all three information conditions (control, ranking, and

[14] 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, e.g., Kremer and Miguel 2004).

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

[16] See an online appendix for images of these plastic plaque displays. The information display stayed on the tables during the whole experiment period.

[17] We do not find that the small saliency effect is any stronger for the top 3 dishes. See Section IV.A for detailed discussions.

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

	Ranking Treatment Locations (5 Locations)	Saliency Treatment Locations (4 Locations)
	Panel A: Pre-experiment Period	
Control Tables	No Display	No Display
Treatment Tables	No Display	No Display
	Panel B: Experiment Period	
Control Tables	No Display	No Display
Treatment Tables	Display a plaque showing the names of the five most popular dishes	Display a plaque showing the names of five sample dishes

Table 1: Experimental Design.

Notes: The names of the five most popular dishes were displayed in the order of their rankings with No. 1 listed first; the actual numbers of plates sold in the previous week were not displayed. The five sample dishes always included the actual top 3 dishes (without being revealed as such) and two other randomly selected dishes. They were displayed in random order.

saliency treatments) in the same location. This is desirable from the statistical point of view because it permits the estimation of the difference between the overall ranking treatment effect and the saliency effect without having to assume that ranking and saliency treatment locations are similar in unobservable dimensions, an assumption that is necessary for the two-stage randomization strategy (see Section III for more discussions). However, the presence of three information conditions in a single location leads to serious practical difficulties, as the managers of the restaurant chain expressed the concern that this would create confusion among waiters and waitresses, as well as in record keeping. We are also concerned that customers may raise suspicions about the restaurant's intention if they found out about the two different displays in the same location. We adopt the two-stage randomization strategy due to these practical reasons.

Pre-Experiment Period Data Collection. 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 (the first-stage randomization), we randomly assigned tables in each of the selected locations into control and treatment tables (the second-stage randomization). We then collected data on the diners' choices for each location for the week of October 16-22, 2006 (which we will call the "pre-experiment period" from now on), before we implemented the ranking and saliency treatments in the week of October 23-30, 2006 (which we will call the "experiment period" hereafter).[19]

The data collected during the pre-experiment period serves three separate purposes. First, we

[19] The restaurants implemented our experiments one day longer than we requested. (In fact, after the experiment, they 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 discard the data from the last day.

use the pre-experiment period data from each of the locations 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 displays in the four saliency treatment locations in the experiment period.[20] Second, the pre-experiment data allows us to conduct tests regarding the quality of randomization. Third, the pre-experiment data allows us to implement a triple-difference estimation strategy of the observational learning effect to eliminate possible systematic unobservable differences between treatment and control tables. Table 1 summarizes our experimental design.

Post-Dining Survey. We randomly selected about 20 percent of the dining parties in the experiment period to administer a short post-dining survey, where we collected information about the persons who paid the bill for the whole table.[21] In the survey we collected information about his or her basic demographics, cumulative times of dining visits to MZDP and subjective dining experience.

III. Data and Identification Strategies

A. *Data*

For each dining party, the restaurant records a unique bill ID and includes information on the unique identifier of the table where the dining party sat, the unique number for each of the dishes ordered in that bill, as well as their prices, and the total amount spent. The table identifiers were compared with our randomization that assigned each table to treatment or control. We only included bills with non-missing table assignments because otherwise the bill was typically a take-out bill. We also deleted very large bills which were most likely weddings and company banquets.[22] This left us a total of 12,895 bills for analysis. As can be seen in Table 2 below, 7,355 bills were from the five ranking treatment locations, and 5,540 were from the four saliency treatment locations.

B. *Descriptive Statistics*

Panel A of Table 2 provides the basic summary statistics of the control and treatment tables in the pre-experiment period in both the ranking and saliency treatment locations. It shows that our

[20] The displays were immediately printed and sent to their respective locations, and put on display the next day.

[21] The questionnaire for the post-dining survey is available in the online appendix. It typically took less than a minute to answer all questions. After the completion of the questionnaires, the tables that were surveyed were given a box of poker cards and a piece of moon-cake as tokens of appreciation.

[22] After consulting with the restaurant managers, we used 800 CNY (Chinese Yuan) as the cutoff above which the bill was considered large. From a total of 13,302 bills in our data set (including both the pre-experiment and experiment periods), a total of 407 bills were deleted due to these considerations. The deletion of these large bills only affects the calculations of the means for dishes ordered and bill amount, but does not at all affect subsequent analysis of the effect of observational learning on customers' choices. Including these large bills would lead to significantly larger means for both dishes ordered and the bill amount, inconsistent with what the restaurant managers would consider as being reasonable.

	Ranking Treatment Locations (5 Locations)		Saliency Treatment Locations (4 Locations)	
Variables	Mean	Std. Dev.	Mean	Std. Dev.
Panel A: Pre-Experiment Period Data				
Total Bill Amount (CNY):				
All Tables	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:				
All Tables	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:				
All Tables	3401		2671	
Treatment Tables	1865		1336	
Control Tables	1536		1335	
Panel B: Experiment Period Data				
Total Bill Amount (CNY):				
All Tables	142.6	133.2	147.9	121.0
Treatment Tables	139.2	129.1	146.8	118.8
Control Tables	146.9	138.0	149.0	123.2
Total Number of Dishes Ordered:				
All Tables	4.91	3.77	4.90	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:				
All Tables	3954		2869	
Treatment Tables	2182		1418	
Control Tables	1772		1451	

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

Note: An observation is a bill.

experiment achieved reasonable randomization at both the table and site levels, at least in several important observable dimensions.

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 business volumes between the ranking treatment and saliency treatment locations.

Second, in the ranking treatment locations, the average bill amount is about 148 CNY (Chinese Yuan), with little difference between treatment and control tables (the p-value is 0.58 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 0.06. The average total bill amount in the saliency treatment locations is about 146 CNY, again with a negligible difference between treatment and control tables (the p-value is 0.36). Similarly, the difference in the number of dishes ordered between control and treatment tables in the pre-experiment period is also small in the saliency treatment locations

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 0.69, and that for the average number of dishes ordered is 0.22. Thus, at least in the two dimensions we examined, we are quite confident that randomization is well implemented at both the site and table levels.[23]

Panel B of Table 2 provides some basic descriptive statistics of our experiment period data, which consists of a total of 6,823 bills. The average daily number of bills per location and the average bill amount, as well and the average number of dishes ordered per bill, do not seem to differ much between the pre-experiment and experiment periods.

C. *Empirical Specifications and Identification Strategies*

The raw data is organized by bill ID, and records only dishes that were ordered. For every bill we include in our analysis, we create a dummy variable for *each dish* on the menu which takes a value of 1 if at least one plate of that dish was recorded on that bill and 0 otherwise. Thus in our main 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. We report results from both linear probability (OLS) and Probit specifications. Robust standard errors clustered at the bill ID level are calculated.

[23] 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.

We use two identification strategies to estimate the effect of observational learning on consumer choices. The first empirical strategy uses only the data from the experiment period. We compare the probabilities that top 5 dishes were ordered by 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 (Table 3). We also compare the probabilities that displayed dishes were ordered by the saliency treatment tables and control tables in the saliency treatment locations to estimate the saliency effect (Table 4). The difference between the two estimates provides an unbiased estimate of the pure observational learning effect under the assumption that genuine randomization was achieved at both the site level and at the table level. We call this the Difference-in-Difference (DD) estimation strategy.

The second empirical strategy uses data from both the pre-experiment and experiment periods. Even though Table 2 shows that randomization seems to be well implemented at both the table and site levels in several observable dimensions, there is always the possibility that there are systematic unobserved differences between the control and treatment tables and between the ranking and saliency treatment locations. We use the pre-experiment data to deal with the potential unmeasured differences between the control and treatment tables by implementing one additional layer of differencing that compares the sales of displayed dishes on the same table between the pre-experiment and experiment periods. We call this the trip differencing (DDD) estimation strategy and the results are reported in Tables 5 and 6 below.

IV. Results

A. *The Effect of Observational Learning on Choices*

In this subsection, we present our main finding that there is a significant observational learning effect, but a non-significant saliency effect. We present the estimation results from the DD and DDD empirical strategies separately below, but almost the same magnitude of observation learning is estimated from the two strategies and from the linear probability and Probit specifications. Specifically, we find that 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 20 percent; but being merely mentioned as some sample dishes did not significantly boost their demand.

DD Estimation Results. Table 3 analyzes data from the five ranking treatment locations during the experiment period. 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 below), Columns (1)-(2) report linear probability estimates and (3)-(4) report the *marginal effects* from Probit estimates; Column (1) and (3) 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 (4) also control for the total number of dishes ordered in the bill, and the log of

Variables	(1) OLS	(2) OLS	(3) Probit	(4) Probit
Treat	-0.005	-0.001	-0.006	-0.002
	(0.001)***	(0.0005)*	(0.001)***	(0.000)***
Top 5	0.117	0.138	0.113	0.107
	(0.004)***	(0.006)***	(0.004)***	(0.007)***
Treat * Top 5	0.018	0.021	0.0115	0.0102
	(0.006)***	(0.006)***	(0.003)***	(0.002)***
Total Number of Dishes Ordered		0.013		0.008
		(0.000)***		(0.000)***
Log of Total Bill Amount		0.00016		-0.0001
		(0.00012)		(0.0001)
Constant	0.045	-0.026		
	(0.001)***	(0.021)		
Dish Dummy	No	Yes	No	Yes
Location Dummy	No	Yes	No	Yes
Number of Observations	235052	235052	235052	235052
R^2	0.02	0.07	0.04	0.13

Table 3: The Effect of Ranking Treatment on the Demand of "Top 5" Dishes: Using Experiment Period Data Only.

Notes: An observation in this analysis is a bill-dish combination. See Section II for its construction. For Probits in Columns (3) and (4), the reported coefficients are the *marginal effects* at the means. Robust standard errors clustered at the Bill ID level are reported in parentheses; *, **, *** respectively denotes significance at 10%, 5% and 1%.

the total bill amount, as well as dish and location dummies which absorb unobserved differences among the dishes and the locations (for example the price of the dish). We report robust standard errors clustered at the bill ID level, thus accounting for both heteroscedasticity and dependence within a bill.

Specifications reported in Columns (2) and (4) with dish and location dummies are our preferred specifications, but the qualitative and quantitative results are similar across specifications.[24] Let us discuss Column (2) 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 by *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

[24] We prefer these specifications because the popularity ranking may reflect both information aggregation about unobserved quality of dishes and the similarity of tastes. Including dish dummies would soak up the component of popularity due to the similarity of tastes.

Variables	(1) OLS	(2) OLS	(3) Probit	(4) Probit
Treat	0.001	-0.0005	0.001	-0.0005
	(0.001)	(0.0004)	(0.0009)	(0.0004)
Displayed	0.0754	0.0679	0.0763	0.078
	(0.0038)***	(0.005)***	(0.0038)***	(0.008)***
Treat * Displayed	0.0077	0.0076	0.0026	0.0022
	(0.0056)	(0.0056)	(0.0025)	(0.0021)
Total Number of Dishes Ordered		0.0121		0.0079
		(0.0001)***		(0.0002)***
Log of Total Bill Amount		-0.0000		-0.0002
		(0.0001)		(0.0001)**
Constant	0.0316	-0.0023		
	(0.0006)***	(0.0058)		
Dish Dummy	No	Yes	No	Yes
Location Dummy	No	Yes	No	Yes
Number of Observations	181868	181868	181868	181868
R^2	0.01	0.04	0.02	0.11

Table 4: The Effect of Saliency Treatment on the Demand of "Displayed" Dishes: Using Experiment Period Data Only.

Notes: An observation in this analysis is a bill-dish combination. See Section II for its construction. For Probits in Columns (3) and (4), the reported coefficients are the *marginal effects* at the means. Robust standard errors clustered at the Bill ID level are reported in parentheses; *, **, *** respectively denotes significance at 10%, 5% and 1%.

of this effect, however, we must derive an estimate of the base probability that top 5 dishes were ordered by control tables. This is not transparent in Column (2) because we included the dish dummies in this specification. 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 (2.1/16.2 ≈ 13%). The coefficient estimates for this effect are statistically significant at 1%.[25]

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 reports analogous regression results using the experiment period data from the saliency treatment locations. Any effect on the demand of the "displayed " dishes at the

[25] 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 reduced.

treatment tables will be considered as simply the saliency effect. These dishes were displayed with no information about their popularity.

Note that, 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 (Row 2 of Table 4). However, the estimate of the key coefficient of interest for the interaction term "Treat*Displayed", which measures the saliency effect, is small in magnitude (less than 1 percentage points) and statistically insignificant in all specifications. Thus we did not find any statistically significant saliency effect.

One possible concern is that the saliency effect was small because the sample dishes being displayed at the saliency treatment tables are not popular to start with. In our experimental design, we deliberately included the actual top 3 dishes in each restaurant location in the five sample dishes we displayed in the saliency treatment tables. This feature of the experimental design allows us to evaluate whether the saliency effect varies by the initial popularity of the dishes. In particular, we can compare the treatment effects on the demand of Top 3 dishes between the ranking treatment locations and the saliency treatment locations. In the ranking treatment locations, we find that the demand increase for top 3 dishes when ranking information was displayed was somewhat more pronounced than that for top 5 dishes on average; specifically, the estimated coefficient of "Treat*Top 3" in an OLS specification identical to Column (2) in Table 3 is 0.032 with a standard error of 0.008 (and a p-value of close to 0). In the saliency treatment locations, we find that the estimated saliency effect for top 3 dishes that were merely displayed as sample dishes remains small and statistically insignificant; specifically, the estimated coefficient of "Treat*Displayed (Top 3)" in an OLS specification identical to Column (2) in Table 4 is 0.01 with a standard error of 0.008 (and a p-value of 0.19).[26]

To summarize, our finding in Table 4 indicates that being made salient, i.e., being displayed on a plaque, does not significantly persuade 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, the saliency effect is small or almost zero. Under our assumption that the 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 complete regressions with a triple interaction "Treat*Displayed* Ranking Treatment Locations" to obtain an estimate of the net observational learning effect. To save space we do not report these regression results here 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 the 1% level.

[26] All of the un-reported regression results can be found in the online appendix.

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

In Table 5, we use data for the five ranking treatment locations from both the pre-experiment and experiment periods. For each bill, even if it occurred in the pre-experiment period, we categorize it into whether the bill was served at a treatment table according to its table's treatment/control assignment in the experiment period. Then we define a new dummy variable "after" to indicate whether the bill occurred in the experiment period. Thus, the key coefficient of the triple interaction term "Treat*Top 5*After" provides 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 for control, separately for the pre-experiment and experiment periods; the second difference is the difference of the above first difference between the pre-experiment and experiment periods. 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. In the third differencing we subtract the saliency effect from the total ranking treatment effect to obtain an estimate of the pure observational learning effect.

Focusing again on our preferred specification in Column (2) of Table 5 where we control for dish and location dummies, we note that the coefficient estimate for the triple interaction term "Treat*Top 5*After" in this OLS specification is 3.2 percentage points; it is statistically significant at the 1% level, and the magnitude is larger than the estimate of the interaction term "Treat*Top 5" in Column (2) of Table 3. The 3.2 percentage point estimate of the total ranking treatment effect represents an almost 20 percent ($3.2/16.2 \approx 20\%$) 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 period, 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 this approach is very similar to that we found using just a single differencing with only the experiment period

Variables	(1) OLS	(2) OLS	(3) Probit	(4) Probit
Treat	-0.0003	0.00127	-0.0003	0.0005
	(0.0012)	(0.0005)**	(0.0014)	(0.0005)
After	-0.00358	-0.00065	0.00378	0.0012
	(0.0013)***	(0.00059)	(0.00138)***	(0.0005)**
Top 5	0.1174	0.1433	0.118	0.1177
	(0.0047)***	(0.0055)***	(0.004)***	(0.006)***
Treat * After	-0.0048	-0.0022	-0.005	-0.0026
	(0.0017)***	(0.0008)***	(0.002)	(0.0007)***
Top 5 * After	-0.0003	0.00158	-0.002	-0.0012
	(0.0065)	(0.0064)	(0.003)	(0.0021)
Treat * Top 5	-0.0123	-0.0114	-0.0047	-0.0037
	(0.006)**	(0.006)*	(0.0023)**	(0.002)**
Treat * Top 5 * After	0.0302	0.0320	0.0174	0.0149
	(0.0085)***	(0.0084)***	(0.0044)***	(0.0039)***
Total Number of Dishes Ordered		0.0136		0.0074
		(0.0001)***		(0.0001)***
Log of Total Bill Amount		0.0003		-0.0001
		(0.0001)***		(0.00008)
Constant	0.0414	-0.0765		
	(0.0096)***	(0.0238)***		
Dish Dummy	No	Yes	No	Yes
Location Dummy	No	Yes	No	Yes
Number of Observations	448371	448371	448371	448371
R^2	0.02	0.07	0.03	0.13

Table 5: The Effect of Ranking Treatment on the Demand of "Top 5" Dishes: Using Data from Both Pre-Experiment and Experiment Periods.

Notes: An observation in this analysis is a bill-dish combination. See Section II for its construction. For Probits in Columns (3) and (4), the reported coefficients are the *marginal effects* at the means. Robust standard errors clustered at the Bill ID level are reported in parentheses; *, **, *** respectively denotes significance at 10%, 5% and 1%.

Variables	(1) OLS	(2) OLS	(3) Probit	(4) Probit
Treat	-0.001 (0.007)	-0.0006 (0.0004)	-0.0002 (0.0009)	-0.0006 (0.0004)
After	-0.0007 (0.0008)	-0.0005 (0.0004)	-0.00075 (0.0009)	-0.0004 (0.0004)
Displayed	0.070 (0.0039)***	0.0625 (0.0047)***	0.0704 (0.004)***	0.0685 (0.006)***
Treat * After	0.0011 (0.0012)	0.0001 (0.0006)	-0.0012 (0.0013)	0.00009 (0.0006)
Displayed * After	0.005 (0.0054)	0.0052 (0.0055)	0.0027 (0.0026)	0.0023 (0.0022)
Treat * Displayed	0.0057 (0.0057)	0.0057 (0.0057)	0.0026 (0.0027)	0.0021 (0.0023)
Treat * Displayed * After	0.00199 (0.00795)	0.00196 (0.008)	-1.68e-6 (0.00353)	0.00004 (0.0029)
Total Number of Dishes Ordered		0.0122 (0.0001)***		0.008 (0.0001)***
Log of Total Bill Amount		-0.00002 (0.00005)		-0.00017 (0.00005)**
Constant	0.0323 (0.0006)***	-0.0155 (0.00296)***		
Dish Dummy	No	Yes	No	Yes
Location Dummy	No	Yes	No	Yes
Number of Observations	346649	346649	346649	346649
R^2	0.01	0.04	0.02	0.10

Table 6: The Effect of Saliency Treatment on the Demand of "Displayed" Dishes: Using Data from Both Pre-Experiment and Experiment Periods.

Notes: An observation in this analysis is a bill-dish combination. See Section II for its construction. For Probits in Columns (3) and (4), the reported coefficients are the *marginal effects* at the means. Robust standard errors clustered at the Bill ID level are reported in parentheses; *, **, *** respectively denotes significance at 10%, 5% and 1%.

data.

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. Taking the difference between the total ranking treatment effect (the coefficient estimate of "Treat*Top 5*After" in Table 5) and the saliency effect (the coefficient estimate of "Treat*Displayed*After" in Table 6) would give us the DDD estimate of the pure observational learning effect. Again, in unreported regressions, we found the estimate of pure observational learning effect to be substantial and statistically significant.

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 (Table 3) to 20 percent (Table 5) when the 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. The 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 period data to come up with the genuine top 5 dishes 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 may 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.[27] 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.

B. *Additional Results Using the Post-Dining Survey Data*

We now report some additional results using the data from the post-dining surveys, which were collected from about 20% of the bills randomly selected in the experiment period. We merged this survey data with the dining choice data using the bill ID. We received 644 and 693 surveys

[27] 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., James Heckman and Edward Vytlacil 2001).

	(1) OLS	(2) OLS	(3) Probit	(4) Probit
Panel A: Ranking Treatment Locations				
Treat	0.0833	0.0891	0.0833	0.0899
	(0.0428)**	(0.0428)**	(0.0428)**	(0.0430)**
Number of Observations	644	640	644	640
R^2	0.0074	0.0198	0.0082	0.0213
Panel B: Saliency Treatment Locations				
Treat	0.0261	0.0280	0.0261	0.0258
	(0.0370)	(0.0372)	(0.0370)	(0.0360)
Number of Observations	693	680	693	680
R^2	0.0024	0.0118	0.0031	0.0189
Additional Controls	No	Yes	No	Yes

Table 7: The Effects on Dining Satisfaction: Ranking Treatment vs. Saliency Treatment.
Notes: An observation is a *bill*. The dependent variable is a dummy that takes value 1 if the customer reported "Very Satisfied" in the post-dining survey. Only data from the experiment period is used in this analysis. The additiona controls include dummies of age intervals, college, gender, tourist, and cumulative number of visits. For Probits in Columns (3) and (4), the reported coeffcients are the *marginal effects* at the mean. Robust standard errors clustered at the table level are ported in parenthesis. *, ** and ** deotes significance at 10%, 5% and 1% respectively.

respectively for the ranking and saliency treatment locations.[28] We ask two questions. First, does providing information about others' choices (as in the ranking treatment) improve the subjective dining experience? Second, are infrequent visitors, who had more diffuse priors about the quality of dishes, more susceptible to the influence of others' choices?

Effect of Observational Learning on Subjective Dining Satisfaction. Table 7 presents our results about the effect of "top 5" ranking displays on the customers' dining satisfaction, in contrast to that of the "five 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 a value of 1 if the customer reported "Very Satisfied" in the post-dining survey. The covariates included vary by specifications. Panel A reports that customers seated at 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 the 5% level for all specifications. In contrast, Panel B reveals that in the saliency treatment locations, those seated at treatment tables where we displayed five randomly selected

[28] The descriptive statistics of the survey data are available in an online appendix.

	(1) Whole Sample	(2) Survey Sample	(3) 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)***
Number of Observations	235052	48843	48843
R^2	0.021	0.022	0.0223

Table 8: Frequent Customers Respond Less in the Ranking Treatment.
Notes: An observation is a *bill and dish combination.* All regressions are OLS without dish and location dummies. The variable "Frequent" is a dummy variable that takes value 1 if the customer reported having dined in the restaurant chain 6 or more times. Robust standard errors clustered at the Bill ID level are ported in parenthesis. *, ** and ** deotes significance at 10%, 5% and 1% respectively.

dishes were statistically no more satisfied than those seated at control tables.[29]

There are at least three potential mechanisms for ranking displays to improve the subjective dining satisfaction. First, the information displays may lead diners to make *better* dish choices; second, they could make the diners' dinner choices *easier;* and third, the diners may be more satisfied because they ordered the same dishes as others due to *conformity* concerns. Unfortunately we could not satisfactorily distinguish these three channels from one another. But it is useful to point out some facts that may suggest that the first channel is more important. First, 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"; second, other researchers have found that choices made under conformity pressure are likely to lead to less satisfaction *ex post.*[30]

Is Observational Learning More Important for Infrequent Customers? 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 by the knowledge

[29] 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.

[30] Dan Ariely and Jonathan Levav (2000) examined the satisfaction of consumers who chose what beer to order sequentially in a group setting. They found that those who ordered later were less satisfied with what they ordered.

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 a value of 1 if the survey respondents reported to have visited the restaurant 6 or more times, and 0 otherwise.[31]

Column (1) of Table 8 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 8 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.[32]

V. *Conclusion*

In this paper we present results from a randomized natural field experiment about the effects of observational learning on individuals' behavior and subjective well-being in the context of restaurant dining. Our experimental design aims to distinguish the observational learning effect from the saliency effect. We find that the demand for the top 5 dishes increases by an average of about 13 to 20 percent, depending on the empirical specifications, when customers are given ranking information of the five most popular dishes; in contrast, being merely mentioned as some sample dishes does not significantly boost their demand. We also find modest evidence that the observational learning effect is stronger among infrequent customers. Moreover, we find that customers' subjective dining experiences improve when they are presented with the information about the top choices by other consumers, but not when presented with the names of some sample dishes.

Our result provides convincing evidence that consumers do learn from the information contained in the choices of others, thus providing empirical support for the theoretical models of herding and information cascades (Banerjee 1992; Bikhchandani, Hirshleifer and Welch 1992). Our result also establishes that observational learning is an important component of social learning; thus it suggests

[31] 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 visit cutoff yields sufficient numbers of 0 and 1 for the "frequent" dummy in order to get statistical significance.

[32] We also ran regressions analogous to Column (3) in Table 8 using survey data from the saliency treatment locations. We found that the point estimate for the coefficient for "Treat*Displayed*Frequent" is negative but almost negligible in magnitude, and is statistically insignificant (with a p-value of 0.89).

that policy makers may affect individuals' decisions through information campaigns that release popularity information about relevant alternatives from other groups of agents. It also provides a partial explanation for the commonly observed practice of popularity information displays in the internet business.

Finally, there are several interesting directions for future research. For example, how do we separate conformity motives from observational learning? Are observational learning effects persistent? How would the effect of observational learning change when profit-maximizing sellers, not third parties, are providing the popularity information? The latter two questions could be potentially addressed if we can track customers exposed to differential information conditions through customer loyalty cards.

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