The Top-Ten Effect: Consumers' Subjective Categorization of Ranked Lists

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Long lists of ranked items, such as *Bloomberg Businessweek*'s rankings of MBA programs, are ubiquitous in Western culture, and they are often used in consumer decision making. Six studies show that consumers mentally subdivide ranked lists into a smaller set of categories and exaggerate differences between consecutive items adjacent to category boundaries. Further, despite prior work suggesting that people might subjectively produce place-value categories (e.g., single digits, the twenties), this research shows that consumers interpret ranked lists by generating round-number categories ending in zero or five (e.g., top 10, top 25). Thus, for example, consumers will more favorably evaluate improvements in rank that cross round-number-category boundaries (e.g., shifting from rank 11 to rank 10) than improvements in rank that cross place-value-category boundaries (e.g., shifting from rank 10 to rank 9). This phenomenon, labeled the *top-ten effect*, occurs because round numbers are cognitively accessible to consumers due to their prevalent use in everyday communication.

Long lists of ranked items are ubiquitous in Western culture. For example, each December, *Car and Driver* magazine lists the 10 best-performing cars and the 10 worst-performing cars of the year. *Bloomberg Businessweek* magazine publishes biennial rankings, listing the top 25 business schools. *Billboard* magazine publishes a weekly "Hot 100" list, a ranking of the sales and airplay of popular songs. Further, highly publicized ranked lists are not a new phenomenon. In 1930, the *Chicago Tribune* published an article that ranked Chicago's 28 most prominent and influential gangsters, and it placed the notorious gangster Al Capone atop the list, making him "public enemy number one" (Bergreen 1994).

From a consumption standpoint, there is considerable evidence that individuals find ranked lists informative and influential. For example, *Billboard*'s music charts are widely regarded as the "weekly bible" of the music industry (Hatschek 2002). In fact, 15 of the top 30 search-engine queries that drive traffic to the *Billboard* website focus on *Billboard*'s music rankings (according to the ranked list provided by Alexa.com; accessed January 27, 2013). Consumers often choose goods and services based on a product's inclusion in a ranked list (Sorensen 2007) or on its direction of movement on the list (Pope 2009). Not surprisingly, then, firms that offer goods or services that are subject to third-party rankings not only devote significant resources to improve their rank but often make major organizational decisions based on their attained rank (e.g., Fee, Hadlock, and Pierce 2005; Martins 2005).

Given the influence of ranked lists on both the consumers and producers of goods and services, it is important to understand how the information provided on these lists is interpreted by users. Because a ranking is an ordinal scale of measurement, there is no technical reason why an information user should interpret items at adjoining ranks as having equal differences in the ranked attribute. Yet adults in Western cultures tend to assume that adjacent integers on a number line, particularly those below 1,000, are equidistant (Dehaene et al. 2008; Siegler and Opfer 2003; Siegler, Thompson, and Opfer 2009). Even academic researchers have utilized linear functions that imply interval scales when modeling the effects of changes in an organization's rank

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on the outcomes and policies of the ranked organizations (Monks and Ehrenberg 1999a, 1999b). Although studies of numerical approximations and very large numbers have indicated that adult consumers may perceive adjacent numbers to be separated by logarithmic, rather than linear, distance (Banks and Coleman 1981; Dehaene et al. 2008), ranked lists typically involve neither of these. Thus, in the absence of specific information about the ranked items, linear equidistance seems reasonable for the information user to assume.

In this article, we propose the existence of a cognitive bias that overrides the presumption of equidistance between adjacent ranks in the interpretation of ranked lists. This bias, which has important implications for consumer evaluations of items presented in a ranked list, emerges due to our tendency to see complex arrays that are not already categorized, including long lists of numbers, in terms of a smaller set of subjectively generated categories. A consequence of this tendency to subjectively categorize is an exaggeration of the perceived differences between items at adjoining ranks that cross category boundaries. In the first section of this article, we review existing evidence that individuals tend to generate categories when perceiving complex arrays and discuss two types of categories that individuals might create from ranked but uncategorized lists. We then present six studies that provide evidence of a cognitive bias that is consistent with one of these categorization types and document a psychological mechanism underlying this bias.

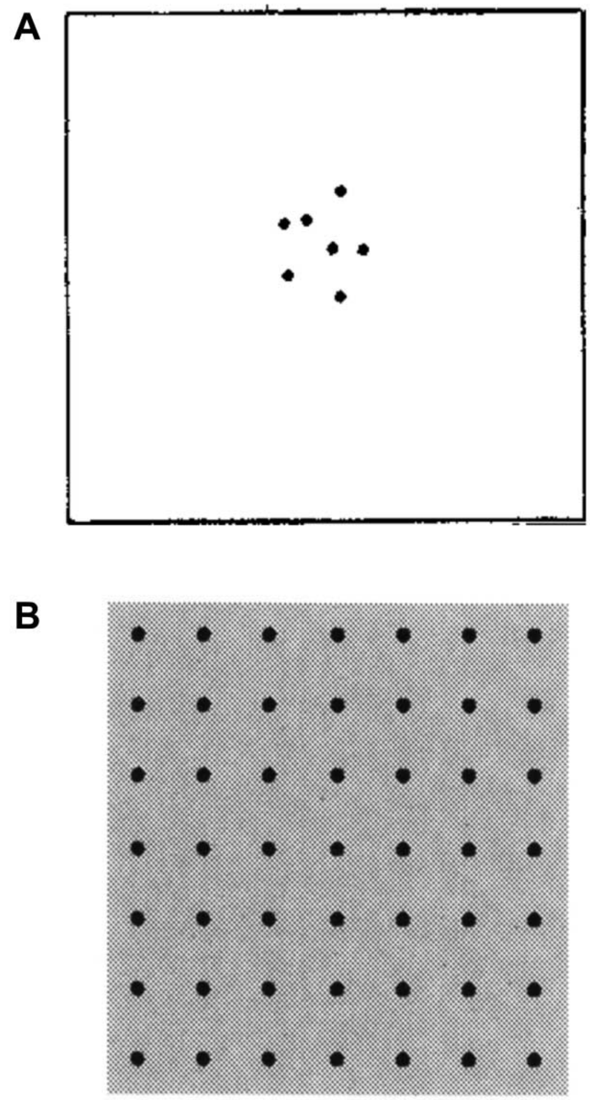
USE OF CATEGORIES TO MAKE SENSE OF COMPLEX ARRAYS

In order to perceive, or intuitively grasp, a complex array of items given the limited human information processing ability, people tend to recode arrays into a smaller and more manageable number of categories, or "chunks" (Miller 1956). For example, because people cannot subitize, or quickly grasp, the numerosity of more than six items (Kaufman et al. 1949), an individual might enumerate the dots in figure 1*A*, by mentally separating the dots into two or three groups (Kaufman et al. 1949; Van Oeffelen and Vos 1982) and might perceive the dots in figure 1*B*, by grouping them into rows or columns (Kubovy and Wagemans 1995). Transforming a complex array into a manageable number of groups fulfills one of the primary purposes of categorization, namely, the attainment of cognitive economy (Rosch 1978).

The tendency to generate subjective categories is not limited to perceptual stimuli such as dot patterns. For example, it has been shown that adults and even children organize sets of numbers into categories (e.g., small/medium/large, odd/even, integer/noninteger, positive/negative, etc.; Laski and Siegler 2007; Shepard, Kilpatric, and Cunningham 1975). Nevertheless, it is not obvious that consumers will generate their own mental categories when interpreting ranked lists. Ranked lists are unique in that they are already highly organized—they contain a defined number of items that have been arranged explicitly on an ordinal scale and grouped together in a list for a specific reason. Given their inherent structure, ranked lists may be relatively less predisposed to unprompted subjective categorization.

FIGURE 1

ARRAYS THAT MAY BE RECODED INTO CATEGORIES



NOTE.—*A*, individuals might enumerate these dots by mentally separating the dots into two or three groups (Kaufman et al. 1949); *B*, individuals might interpret these dots by grouping them into rows or columns (Kubovy and Wagemans 1995).

If categorization does occur, it may affect perceptions of the distance between items in an array. A common introspection, albeit one that has not been robustly tested, is that categorizing dot arrays (e.g., figs. 1*A* and *B*) slightly expands the perceived distance between groups, exaggerating the distances between items of different categories and/or reducing the distances between items within the same category. The Gestalt psychologists have talked about this as "attraction" between the items in a group and "segregation" from the items not in the group (e.g., Koffka 1935, 165–66, 439). Such effects have been empirically documented for spatial categories, such as geographic borders (Maddox et al. 2008; Maki 1982; Mishra and Mishra 2010; Tversky 1992), social categories, such as arbitrary in-groups and out-groups (Allen and Wilder 1979; Locksley, Ortiz, and Hepburn 1980; Otten 2002), and combinations of spatial and social categories, such as neighborhoods associated with different ethnic groups (Maddox et al. 2008). For example, Maki (1982) taught experimental subjects the names of 12 fictitious cities that either did or did not have a "state" border that separated one set of six cities from the other. In subsequent distance-estimation tasks, the state-border subjects judged the two cities on either side of the state border as farther apart than did the subjects who were exposed to those two cities when they were not separated by a state border.

In spite of this prior work, it is not clear whether the tendency toward between-category exaggeration will apply to the evaluations of ranked items located along category boundaries. It is conceivable, for example, that the previously discussed presumption of numerical equidistance might overwhelm the inclination to exaggerate differences between items of different categories, resulting in no category-level effects on evaluations. It is even possible that consumers may exaggerate differences among ranked items within the same category. Although most prior research has highlighted between-category separation, the exaggeration of within-category differences has been found in certain situations where information users attend disproportionately to a single category instead of considering multiple categories. For example, Leclerc, Hsee, and Nunes (2005) found that, when a category of items (e.g., products in a company's product line) is evaluated separately from other categories, experimental subjects appeared to overweight the product rankings within the category. However, when subjects were asked to make comparisons between items in different categories, this "ranking effect" disappeared.

In the present research, we attempt to empirically examine whether consumers form categories from ranked lists and, if so, to assess whether the assumption of evaluative equidistance holds in this domain. Despite the unique features of ranked lists and the viable alternative hypotheses discussed in this section, we propose that consumers will in fact approach the large amount of information in a ranked list by generating multiple mental categories. Further, in line with research showing that categorization overstates the perceived differences between products within a category and a lone product outside of the category (Brenner, Rottenstreich, and Sood 1999), we posit that the subjective categorization of ranked lists will lead to an exaggeration of between-category differences, particularly for adjacently ranked items in different categories.

POSSIBLE CATEGORIES FOR PERCEPTION OF RANKED LISTS

What are the possible categories that might be formed by consumers when interpreting ranked-list information? One possibility is that individuals see ranked lists in terms of categories based on the value of a rank's left-most digit. In support of this possibility is the evidence that multidigit numbers are processed digit by digit from left to right (Poltrock and Schwartz 1984) and that when two-digit numbers with differing tens digits are compared, there are no numerical distance effects from the values of their ones digits (Verguts and De Moor 2005). Further, it has been found that in consumer price-response judgments, a price's left-most digit tends to be perceptually overweighted (e.g., Thomas and Morwitz 2005). In one study, Bizer and Schindler (2005) found that subjects overestimated the quantity of $2.99 items that could be purchased for $73.00 in a way that suggested that they based their mental calculations on the digit 2 in the dollars place and did not fully consider the digits 99 in the cents places. For ranks up to 100, such *place-value categories* would consist of ranks 1–9 (single digits), ranks 10–19 (the teens), ranks 20–29 (the twenties), and so on.

An alternative possibility is that individuals see ranked lists in terms of categories based on round numbers. In our decimal number system, round numbers are those that are multiples of 10 (e.g., 10, 20, 30) and halfway points between these multiples (e.g., 5, 15, 25). Support for this possibility is provided by the extensive evidence for the cognitive salience of round numbers. Incidence counts in large samples of written text in various languages have shown that round numbers are highly overrepresented relative to nonround numbers of similar magnitudes (Coupland 2010; Dehaene and Mehler 1992; Jansen and Pollmann 2001). Further, individuals show a marked tendency to produce round numbers when asked about the quantity of items too numerous to count (e.g., Kaufman et al. 1949), when reporting magnitudes or estimating numbers of occurrences (e.g., De Lusignan et al. 2004; Hornik, Cherian, and Zakay 1994; Tarrant and Manfredo 1993), and even when articulating personal goals (Pope and Simonsohn 2011). For ranks up to 100, such *round-number categories* would consist of ranks 1–5 (i.e., the top 5), ranks 1–10 (i.e., the top 10), ranks 1–15 (i.e., the top 15), ranks 1–20 (i.e., the top 20), and so on.

Consistent with the possibility that consumers form round-number categories would be a tendency for ranked lists themselves to consist of a round number of items. To provide a test for our intuition that this is the case, we conducted an informal survey of the lengths of ranked lists. For all numbers 1–100, we entered "top [number]" (in quotation marks) into a Google search field and counted the number of search results (all searches were implemented on

November 12, 2012). Figure 2 shows that there was a marked tendency for the number of items in a ranked list to end in the digits 0 or 5. In other words, ranked lists do indeed tend to include a round number of items.

If, as we expect, information users create subjective categories to interpret ranked lists and tend to perceptually exaggerate between-category differences, then it would be possible to test whether it is place-value or round-number categories that are typically used. With place-value categories, there would be a category boundary—and thus an exaggerated difference—after ranks ending in 9 (e.g., between rank 9 and rank 10). With round-number categories, there would be a category boundary—and thus an exaggerated difference—following round numbers (e.g., between rank 10 and rank 11). We next describe our investigation to determine if either of these two perceptual deviations from the presumed equidistance of ranked items exists and influences consumer judgments.

STUDY 1

In study 1, we examine how changes to a business school's ranking on the annual *U.S. News & World Report* list might influence the number of applications subsequently received by the school. This approach, a type of natural experiment, allows us to determine whether the place-value-category account or the round-number-category account is better at predicting the number of applicants at top business schools.

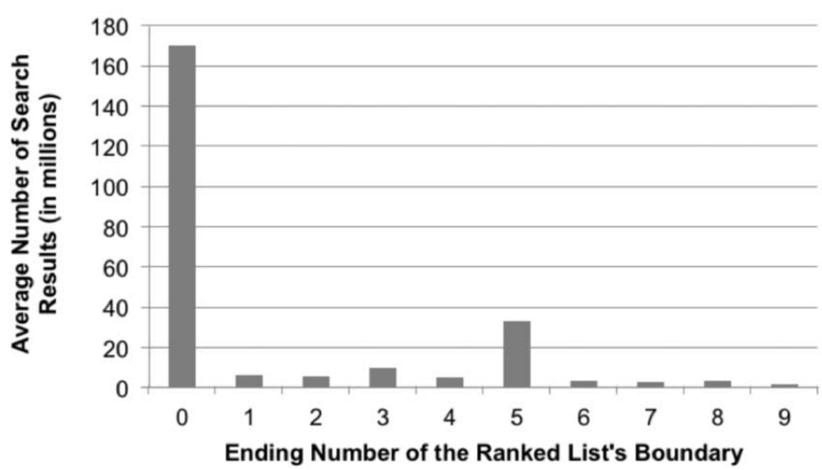
Method

We based this investigation on a longitudinal data set provided to us by the Graduate Management Admissions Council. This data set contained information from 484,922 test takers (39% female) who took the GMAT exam (for admission into business school) between July 1, 2005, and March 31, 2008. Prior to taking the exam, as part of their GMAT registration fee, test takers could select up to five business schools to which their official score report would be sent following the exam. Our data set includes the date that each test taker took the GMAT, along with the identities of the schools that he/she designated. Given that most business schools require an official GMAT score report as part of the application process, we reasoned that the total number of score reports sent to a business school was a valid indicator of the number of completed applications that it might ultimately receive. On average, each test taker designated 4.2 schools, resulting in 2,026,975 score report requests.

We utilized this data set to assess whether a change in the number of applications received by a particular school (approximated by the number of GMAT score reports sent) from one year to the next could be explained by a change in its rank on a third-party list. We focused our analysis on the annual *U.S. News & World Report* list of top 50 business schools, which is typically released online at the end of March each year (in advance of the print version of the magazine that is usually published in early April). Specifically, we examined the number of GMAT score reports sent to each of the 54 schools that made at least one appearance on the 2006, 2007, and 2008 *U.S. News & World Report*

FIGURE 2

NUMBER OF SEARCH RESULTS FOR A RANKED LIST ENDING IN THE NUMBERS 0–9



NOTE.—For all numbers 1–100, we entered "top [number]" (in quotation marks) into a Google search field and counted the number of search results (all searches were implemented on November 12, 2012). There was a marked tendency for the number of items in a ranked list to end in the digits zero or five.

lists (released online on April 1, 2005, March 31, 2006, and March 30, 2007, respectively). A total of 548,289 GMAT score reports were sent to these 54 schools during the 33 months for which we had data.

Using these score reports, we built a database of school-level observations in which a single observation included the following: (1) the school's rank in the *U.S. News & World Report* list released at the beginning of time period T_0 (a 12-month period from the beginning of April until the end of March), (2) the mean number of score reports sent to a particular business school during time period T_0, (3) the school's rank in the *U.S. News & World Report* released at the beginning of T_1 (the 12-month period following T_0), and (4) the mean number of score reports sent to the school during time period T_1. Because we had access to score report requests only from July 1, 2005, the mean number of score reports for certain T_0 observations was imputed from 9 months of data (July 2005 through March 2006). For these observations, we adjusted for seasonality when imputing 12-month means.

Our database consisted of one observation per school for every 2-year period, thus resulting in 108 total observations. For each observation, we also created six dummy variables that allowed us to separately assess the impact of the following types of rank-related shifts: (1) any positive shift in rank, (2) any negative shift in rank, (3) a positive place-value-category shift in rank (moving into the single digits, the teens, the twenties, the thirties, or the forties), (4) a negative place-value-category shift (moving out of the single digits, the teens, the twenties, the thirties, or the forties), (5) a positive round-number-category shift (moving into the top 5, the top 10, the top 15, the top 20, the top 25, the top 30, the top 35, the top 40, or the top 45), and (6) a negative round-number-category shift (moving out of the top 5, the top 10, the top 15, the top 20, the top 25, the top 30, the top 35, the top 40, or the top 45). Our dummy coding reflected the fact that some changes in rankings simultaneously generated multiple types of shifts. For example, a shift in rank from number 11 to number 9 would have been flagged as a positive shift, a positive place-value-category shift, and a positive round-number-category shift.

Results

We conducted a series of regressions to assess the relationship between yearly changes in a school's rank on the *U.S. News & World Report* list and yearly changes in the number of GMAT score reports that were sent to the school (a proxy for completed applications). First, we examined whether the *magnitude* of a school's rank change within the top 50 led more potential students to apply to the school (regression 1). Next, we tested whether the *direction* of a school's rank change led potential students to apply (regression 2) by regressing the first two dummy variables (i.e., any positive shift in rank or any negative shift in rank). As shown in table 1, we found no effect of rank change magnitude nor rank change direction on the year-to-year change in number of GMAT score reports sent to a particular school. Although we expected the magnitude and direction of a school's rank change on the *U.S. News & World Report* list to influence the number of applications a particular school received, our analysis may have obfuscated this result since it did not account for many of the other factors that might also affect applications (e.g., rankings in other media outlets, relative advertising budgets, etc.).

To examine whether place-value-category shifts could explain changes in applications, we regressed the next two dummy variables (i.e., a positive place-value-category shift and a negative place-value-category shift). As shown in regression 3 of table 1, we detected no effect of place-value-category shifts on the year-to-year change in number of GMAT score reports sent to a particular school.

To ascertain whether round-number-category shifts could explain changes in applications, we regressed the final two dummy variables (i.e., a positive round-number-category shift and a negative round-number-category shift). As shown in regression 4 of table 1, there was a significant and direct effect of a positive round-number-category shift on the year-to-year change in number of GMAT score reports sent. However, the hypothesized inverse effect of a negative round-number-category shift on the year-to-year change in number of GMAT score reports sent was not observed.

When all four place-value and round-number dummy variables were included in the same regression (regression 5) or when all six dummy variables were included (regression 6), the presence or absence of a positive round-number-category shift remained the best and only significant predictor of the year-to-year change in number of GMAT score reports sent to a particular school.

In order to determine whether the significant round-number-category shift occurred for both multiples of 10 (e.g., 10, 20, 30, 40) and their halfway points (e.g., 5, 15, 25, 35, 45), we conducted a follow-up analysis. Using dummy variables, we classified each positive round-number-category shift as a 10-ending-category shift if it traversed a round-number category that was a multiple of 10 (i.e., moving into the top 10, the top 20, the top 30, the top 40) or a 5-ending-category shift if it did not. We reran regression 6 after replacing the "positive round-number-category shift" dummy variable with these two new dummy variables. As shown in regression 7 of table 1, the results of this analysis indicate that both positive 10-ending-category shifts and positive 5-ending-category shifts were significant predictors of year-to-year changes in number of GMAT score reports.

Discussion

The results of study 1 provide support for the round-number-category account over the place-value-category account. The finding of an application-rate effect of a positive round-number-category shift is striking given that many factors besides changes in *U.S. News & World Report* rank are likely to affect the number of GMAT score reports sent to a particular business school by its potential applicants. For example, changes to school admission requirements, tuition increases, the arrival or departures of key faculty members

TABLE 1

RELATIONSHIP BETWEEN CHANGES IN A SCHOOL'S RANK AND THE NUMBER OF GMAT SCORE REPORTS SENT TO THE SCHOOL (STUDY 1)

Independent variable	Coefficient	SE	*t*-ratio	*p*-value
Regression 1:				
Change in rank	11.6	19.0	.61	.54
Intercept	94.8	92.1	1.03	.31
Regression 2:				
Positive change in rank	2.6	215.6	0.01	.99
Negative change in rank	−45.4	229.6	−.20	.84
Intercept	71.8	178.4	.40	.69
Regression 3:				
Positive place-value-category change	−8.9	268.6	−0.03	.97
Negative place-value-category change	−254.9	268.6	−.95	.35
Intercept	93.7	97.0	.97	.34
Regression 4:				
Positive round-number-category change	425.4	212.8	2.00	.05
Negative round-number-category change	−124.3	236.8	−.53	.60
Intercept	−3.5	104.0	−.03	.97
Regression 5:				
Positive place-value-category change	−582.5	339.4	−1.72	.09
Negative place-value-category change	−117.5	324.6	−.36	.72
Positive round-number-category change	716.6	273.5	2.62	.01
Negative round-number-category change	−72.2	289.4	−.25	.80
Intercept	10.4	104.6	.10	.92
Regression 6:				
Positive change in rank	−244.0	244.9	−1.00	.32
Negative change in rank	115.1	267.6	.43	.67
ositive place-value-category change	−553.2	340.0	−1.63	.11
Negative place-value-category change	−193.8	332.9	−.58	.56
Positive round-number-category change	883.8	299.7	2.95	.004
Negative round-number-category change	−205.7	317.2	−.65	.52
Intercept	71.8	176.5	.41	.69
Regression 7:				
Positive change in rank	−237.8	244.8	−.97	.33
Negative change in rank	115.1	267.5	.43	.67
Positive place-value-category change	−707.3	369.9	−1.91	.06
Negative place-value-category change	−193.8	332.7	−.58	.56
Positive 10–ending-category change	1124.5	376.7	2.99	.004
Positive 5-ending-category change	688.4	352.3	1.95	.05
Negative round-number-category change	−205.7	317.0	−.65	.52
Intercept	71.8	176.4	.41	.69

NOTE.—The presence or absence of a positive round-number-category shift remained the best and only significant predictor of the year-to-year change in number of GMAT score reports sent to a particular school. Ranks are based on the annual *U.S. News & World Report* list. Number of GMAT reports submitted to a school by potential applicants is used as an indicator of the number of business school applications ultimately received. $R^2_{\text{equation1}} = .004$, $F = .38$, $p = .54$; $R^2_{\text{equation2}} = .001$, $F = .04$, $p = .97$; $R^2_{\text{equation3}} = .009$, $F = .45$, $p = .64$; $R^2_{\text{equation4}} = .05$, $F = 2.47$, $p = .09$; $R^2_{\text{equation5}} = .075$, $F = 2.02$, $p = .10$; $R^2_{\text{equation6}} = .094$, $F = 1.69$, $p = .13$; $R^2_{\text{equation7}} = .105$, $F = 1.61$, $p = .14$.

and administrators, changes in local economic indicators such as unemployment rates, and performance on other business school rankings such as that of *Bloomberg Businessweek* magazine could impact the number of applications received by a particular school. Our results suggest that the influence of positive round-number-category shifts on applicant behavior may be greater than rank magnitude or rank direction shifts.

The finding of an application-increase effect of a positive round-number-category shift raises the question of why an inverse effect (i.e., a decrease in applications) was not found for a negative round-number-category shift. One possible explanation would be that applicants tend to be more aware of positive shifts than negative shifts because positive shifts are advertised on the schools' websites and in their promotional materials.

The evidence from study 1 that rank increases that traverse a round-number-category boundary are associated with application increases whereas other rank increases are not is consistent with the view that individuals form subjective categories to interpret long ranked lists, and it suggests that these categories are based on round numbers. From here on, we will refer to this phenomenon as the *top-ten effect*.

STUDY 2

The results of study 1 provide initial support that individuals who encounter an uncategorized ranked list generate

TABLE 2

TARGET RECALL AS A FUNCTION OF RANK (STUDY 2)

Actual rank	School	Correct recall (*n*)	Incorrect recall (*n*)	Correct recall (%)	Place-value border	Round-number border
9	Virginia	1	54	2	Lower recall	
	Carnegie Mellon	3	42	7		
	Total	4	96	4		
10	Virginia	21	68	24	Higher recall	Higher recall
	Carnegie Mellon	20	84	19		
	Total	41	152	21		
11	Virginia	8	41	16		Higher recall
	Carnegie Mellon	11	33	25		
	Total	19	74	20		

NOTE.—Information users are more likely to recall a target accurately when it is ranked either 10th or 11th versus 9th, which suggests that individuals subjectively generate round-number categories, not place-value categories.

mental partitions at round-number-category boundaries. In order to provide stronger evidence for the subjective categorization process that we have proposed, in study 2 we examine how the recall of an identical target differs when its position on a ranked list is systematically varied. Although we are chiefly interested in understanding the impact of subjective categorization on evaluations, we believe that recall accuracy can help inform us as to whether information users generate round-number categories or place-value categories when reviewing a ranked list.

Although we expect evaluations of adjacent items on opposite sides of subjective partitions to be exaggerated, we anticipate that the recall accuracy of these two items will be similar to one another but higher than other items on the ranked list. Our prediction that recall accuracy will be increased along category boundaries is based on prior research documenting "scalloped" serial position curves in the learning of grouped lists (e.g., Hitch 1996). Even when information users generate subjective categories, we suspect that they will be particularly attentive to items at the borders of these categories. The controlled experimental design of study 2 allows us to directly compare the place-value and round-number-category accounts, since they make different predictions. If information users tend to create place-value categories, then items with ranks that are immediately adjacent to place-value-category boundaries (e.g., 9 and 10) are likely to be recalled better than other ranks (e.g., 11). On the other hand, if information users tend to create round-number categories, items with ranks next to round-number-category boundaries (e.g., 10 and 11) are likely to be recalled better than other ranks (e.g., 9).

Method

One hundred and ninety-three participants (47% female; mean age = 31.8 years) were recruited using an online panel obtained on Amazon Mechanical Turk. Participants were directed to carefully review a list of the top 25 business schools in the United States (according to *Bloomberg Businessweek* magazine) for 30 seconds. The key manipulation (between participants) involved the placement of two schools, the University of Virginia (Darden) and Carnegie Mellon University (Tepper), on the *Businessweek* list. These schools were assigned adjacent ranks on the list, either on opposite sides of a place-number-category boundary (i.e., 9 and 10) or on opposite sides of a round-number-category boundary (i.e., 10 and 11). We also varied the identity of the higher-ranked school. As a result, there were four conditions to which participants were randomly assigned: (Condition 1) 10-Virginia, 11-Carnegie Mellon; (Condition 2) 9-Virginia, 10-Carnegie Mellon; (Condition 3) 10-Carnegie Mellon, 11-Virginia; (Condition 4) 9-Carnegie Mellon, 10-Virginia. Aside from manipulating the ranks of these two business schools, the lists resembled one another across conditions and all participants encountered the same 25 business schools (see app. A for the stimuli).

After reviewing the ranked list for 30 seconds, participants completed a short filler questionnaire. Embedded in this filler questionnaire was an attentional check question, in which participants were instructed to input the total number of business schools that appeared on the *Businessweek* list into an open-ended text box. After completing the filler questionnaire, participants were asked to recall the exact ranks of Carnegie Mellon and Virginia on the *Businessweek* list and enter these ranks in two open-ended text boxes. Question order was counterbalanced. These two questions served as the dependent variables for this study.

Results

One hundred and fifty-four participants (80% of the sample) correctly recalled that the entire *Businessweek* list contained exactly 25 schools. The results reported here include responses from all 193 participants in our sample; however, we conducted a separate analysis using only the subset of participants who correctly answered the attentional check question and found the same pattern of results.

Table 2 denotes the number of participants who accurately recalled the rank for Virginia and/or Carnegie Mellon that they had previously encountered. For those who had seen the *Businessweek* list where Virginia was ranked 9th, only 2% of the participants correctly remembered Virginia's rank. In contrast, a significantly higher proportion of the participants (24%) correctly recalled Virginia's rank when it was ranked 10th ($\chi^2(1) = 12.46$, $p < .001$). Furthermore, 16% of participants correctly recalled Virginia's rank when it was ranked 11th, a proportion higher than the number 9 group ($\chi^2(1) = 6.90$, $p < .01$) but not statistically different from the number 10 group ($\chi^2(1) = .01$, $p > .92$).

Next, we separately analyzed recall rates for Carnegie Mellon. For those who had seen the *Businessweek* list where Carnegie Mellon was ranked 9th, only 7% of the participants correctly remembered Carnegie Mellon's rank. In contrast, a significantly higher proportion of participants (19%) correctly recalled Carnegie Mellon's rank when it was ranked 10th ($\chi^2(1) = 3.80$, $p = .051$). Furthermore, 25% of participants correctly recalled Carnegie Mellon's rank when it was ranked 11th, a proportion higher than the number 9 group ($\chi^2(1) = 5.64$, $p < .02$) but not statistically different from the number 10 group ($\chi^2(1) = .62$, $p > .43$).

Discussion

Taken together, the higher likelihood of accurate recall when an item was ranked 10th or 11th versus 9th is consistent with the round-number-category hypothesis but inconsistent with the place-value-category hypothesis. Based on their recall rates, information users appear to attend more carefully to items with ranks on either side of round-number-category borders. Although information users remember adjacent items on opposite sides of round-number-category boundaries equally well, our field study (study 1) suggests that the evaluative distance between these items may be exaggerated because of this mental partition. In our remaining studies, we seek a deeper understanding of how the evaluation of a target within a ranked list is influenced by this subjective categorization process.

STUDY 3

In study 3, we examine how evaluations of an identical target differ when its position on a ranked list is varied between participants. This experiment is similar to study 1 in that it allows us to compare the place-value-category and round-number-category accounts as possible explanations for the obtained evaluation data pattern, and it provides the opportunity to directly test for the existence of the top-ten effect in evaluative judgments. However, study 3 eliminates potential confounds associated with temporal variation, since its between-participant experimental design allows us to assess whether differences in rank (at the same point in time) rather than changes in rank (between time periods) affect evaluations. Additionally, the controlled experimental design of study 3 provides a more valid test of the proposed subjective categorization process and the expected monotonic relationship between rank and evaluation.

Method

Two hundred and five participants (53% female; mean age = 33.3 years) were recruited using an online panel obtained on Amazon Mechanical Turk. Participants were presented with a ranked list containing 28 names, ranked from top to bottom from number 1 to number 28. They were told that the list contained the names of 28 students in a math class who had been ranked "in order of their performance on the last exam." The key manipulation involved the placement of one student, Charles Pipp (the target), on the ranked list. Participants were randomly assigned to one of five conditions, in which the target was either ranked number 8, 9, 10, 11, or 12. Aside from this manipulation, the lists resembled one another across conditions and all participants encountered the same 28 student names (see app. B for the stimuli).

After reviewing the ranked list that they had been given, participants were asked to evaluate the target's math skills on an unnumbered sliding scale ranging from "extremely weak" to "extremely strong." The scale position of each response was coded into a number ranging from 0 to 100, with higher numbers denoting higher absolute evaluations. Subsequently, participants provided relative evaluations of the target by assessing the target's math skills in comparison to the student ranked immediately above him on the ranked list. Respondents rated the target's relative math skills on an unnumbered sliding scale ranging from "about the same as" to "much worse than" the student ranked just ahead of him on the list. Once again, the scale position of each response was coded into a number ranging from 0 to 100, with higher numbers denoting greater perceived evaluative distance between items of adjacent rank. Importantly, participants did not need to recall the rank of the target when providing their evaluations as the entire ranked list remained on screen during the evaluation task. Finally, to verify that they had correctly observed the target's rank, participants proceeded to another screen where they were asked to recall the target's exact rank on the list of students that they had just seen by entering a number into an open-ended text box.

Results

One participant did not recall the target's rank correctly and was eliminated from subsequent analysis, leaving 204 participants in the sample.

Absolute Evaluations. We first regressed the target's absolute evaluation on its rank and found that rank was a significant inverse predictor of the target's absolute evaluations ($\beta = -.36$, $t(199) = -5.41$, $p < .001$). Thus, the expected monotonic relationship between rank and evaluation was observed.

To test whether absolute target evaluations differed by condition, we conducted a one-way ANOVA and found a significant effect of rank on target evaluation ($F(4, 199) = 7.80$, $p < .001$). All planned contrasts for pairs of consecutive within-round-number-category ranks (i.e., 8 vs. 9, 9 vs. 10, and 11 vs. 12) were nonsignificant (M_8 vs. M_9; $t(199) = .96$, $p > .33$; M_9 vs. M_{10}; $t(199) = .63$, $p > .53$; M_{11} vs. M_{12}; $t(199) = .23$, $p > .81$). However, the planned contrast for the pair of between-round-number-category consecutive ranks (i.e., 10 vs. 11) was significant (M_{10} vs. M_{11}; $t(199) = 2.61$, $p < .01$). Mean absolute target evaluations are shown in figure 3.

Relative Evaluations. Next, we evaluated whether relative target evaluations differed by condition. We conducted a one-way ANOVA and found a significant effect of rank on target evaluation ($F(4, 199) = 2.50$, $p < .05$). Consistent with our theorizing, this effect was driven by participants who saw the target as ranked number 11 (and evaluated it relative to the student ranked number 10). Compared to all other conditions ($M_{\text{non-11}} = 51.9$), this group ($M_{11} = 63.7$) perceived greater evaluative distance between the target and the item immediately above it on the ranked list ($M_{\text{non-11}}$ vs. M_{11}; $t(199) = 2.90$, $p < .01$), which suggests that the ability gap between number 11 and number 10 was perceived to be larger than the gap between the other adjacent ranks. Mean relative target evaluations are shown in figure 4.

Discussion

Consistent with our round-number-category hypothesis, we find evidence in study 3 that between-round-number-category rank changes exert greater impact on an information user's judgments than within-round-number-category changes. Furthermore, we find no evidence that between-place-value-category rank changes exert greater impact on an information user's judgments than within-place-value-category rank changes. These results support the round-number-category account but not the place-value-category account, and thus they provide direct evidence for the existence of the top-ten effect.

STUDY 4

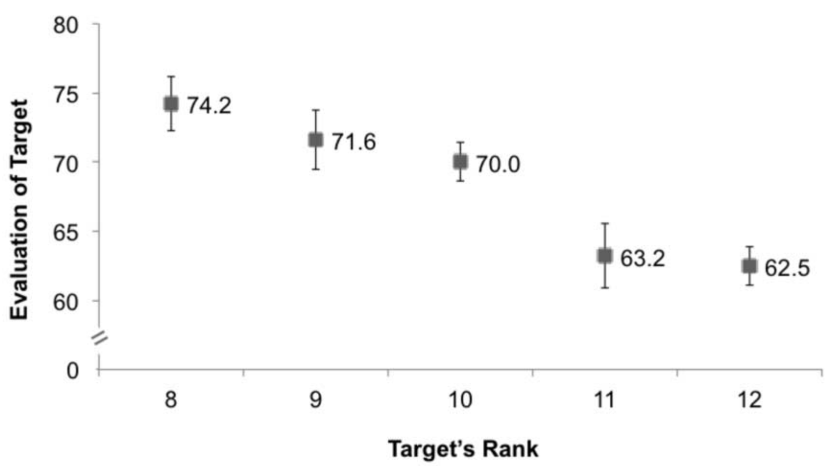
We believe that the top-ten effect occurs because a cognitive bias overrides the presumption of evaluative equidistance for adjacent items in a ranked list that span round-number boundaries. The results of our first three studies suggest that, for ordinal scales (i.e., ranked lists) ranked from best to worst, the equidistance assumption is malleable and therefore presents an opportunity for a cognitive bias to emerge. We wondered whether interval scales, in which adjacent items are by definition evaluatively equidistant, would also be susceptible to the top-ten effect. We test this potential boundary condition in study 4.

Method

One hundred and fifty-four participants (53% female; mean age = 29.3 years) were recruited using an online panel obtained on Amazon Mechanical Turk. Participants were randomly assigned to either an ordinal list or an interval list condition. All participants were asked to review an identical list of 39 New York City restaurants. Participants in the ordinal condition were informed that the restaurants had been ranked from number 1 to number 39 based on patrons' ratings of the quality of each of the restaurants, with higher-

FIGURE 3

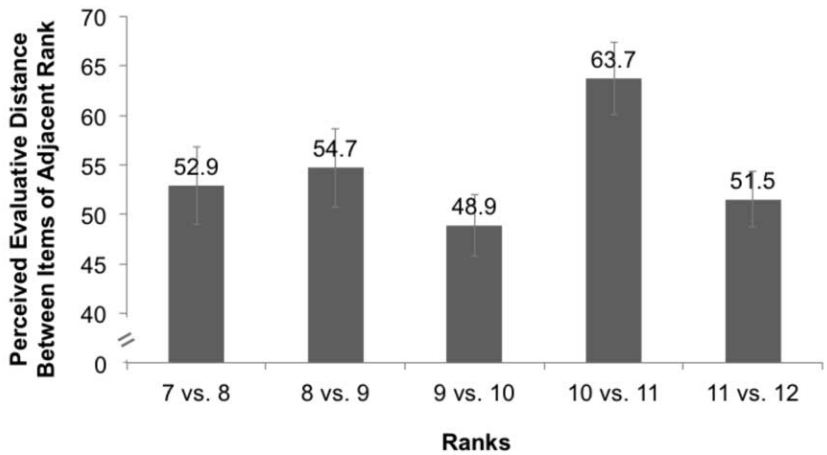
TARGET EVALUATION AS A FUNCTION OF RANK (STUDY 3)



NOTE.—Absolute evaluations of the target indicated a monotonic relationship between rank and evaluation, where more favorable ranks coincided with higher evaluations. The largest gap between evaluations of adjacent items on the ranked list occurred when the items were on opposite sides of a round-number-category border (i.e., 10 vs. 11), not a place-value-category border (i.e., 9 vs. 10).

FIGURE 4

TARGET EVALUATION AS A FUNCTION OF RANK (STUDY 3)



NOTE.—Relative evaluations of adjacent items on opposite sides of round-number categories (e.g., 10 vs. 11) were exaggerated as compared to adjacent items in the same round-number category (e.g., 7 vs. 8, 8 vs. 9, 9 vs. 10, 11 vs. 12). Furthermore, between-place-value-category rank differences (e.g., 9 vs. 10) did not yield an exaggeration of perceived evaluative distance.

quality restaurants listed at the top. Those in the interval condition instead learned that the restaurants were listed based on the number of complaints each restaurant had received during the past year, with higher-quality restaurants (i.e., those with fewer complaints) again appearing at the top (although these quantity-of-complaints numbers are also ratio-scaled, we refer to them as our "interval condition" to emphasize the equidistance aspect). As shown in appendix C, the identities and sequencing of the restaurants were identical in the two conditions. For participants in the ordinal condition, unique ranks were listed from number 1 (Gramercy Tavern) to number 39 (Café Boulud). For those in the interval condition, complaints increased successively in number by one from 1 complaint (Gramercy Tavern) to 39 complaints (Café Boulud).

In this experiment, the target item was a restaurant named Blue Water Grill, which appeared exactly at the midpoint of each list (e.g., number 20 in the ordinal condition, 20 complaints in the interval condition). Participants were directed to attend closely to the target restaurant (it appeared on their list in bold type and was highlighted in yellow) and were given the following description of the restaurant to review: "Blue Water Grill has provided excellent seafood, sushi, scene and jazz to New York City for almost two decades."

As our key dependent variable, all participants were asked to rate Blue Water Grill's relative quality as compared to two restaurants, one inferior and one superior, that were adjacent to it on the list. Specifically, participants compared Blue Water Grill to Jean Georges and Nobu, which, depending on condition, had been ranked number 19 and number 21 or had received 19 and 21 complaints, respectively (see app. C). As in study 3, participants did not need to recall the rank of the target when providing their evaluations as the entire ranked list remained on the screen during the evaluation task. Participants rated Blue Water Grill's quality on an unnumbered sliding scale ranging from "About the same quality as Jean Georges" to "About the same as Nobu." The scale position of each response was coded into a number ranging from 0 to 100, with a value of 50 indicating the target's evaluative equidistance from the adjacent restaurants on the list. Values greater than 50 would be consistent with a round-number-category account (i.e., 20 would be perceived as closer to 19 and farther from 21), whereas values between 0 and 49 would be consistent with a place-value-category account (i.e., 20 would be perceived as farther from 19 and closer to 21). Thus, the design of study 4 allows us to again test for either of these two perceptual deviations from presumed equidistance and to check, within a controlled experiment, whether the top-ten effect applies broadly to other round-number categories (i.e., top 20).

Results

We first evaluated whether relative target evaluations differed by condition. Participants in the ordinal condition ($M = 56.7$) provided more favorable relative evaluations of Blue Water Grill than participants in the interval condition ($M = 49.6$; $t(152) = 2.22$, $p < .03$). As another indication of the difference between conditions, 63% of the participants in the ordinal condition provided ratings above 50, compared to just 45% of participants in the interval condition ($\chi^2(1) = 5.00$, $p < .03$).

Next, we examined the ordinal and interval conditions separately for evidence of round-number categorization, place-value categorization, or evaluative equidistance. Consistent with our earlier studies, participants in the ordinal list condition ($M = 56.7$) provided evaluations that were significantly higher than 50.0, suggesting that they perceived Blue Water Grill, the 20th ranked restaurant, to be closer in quality to the 19th ranked restaurant (Jean Georges) than the 21st ranked restaurant (Nobu; $t(78) = 3.28$, $p < .01$). Additionally, the proportion of participants (63%) who provided ratings above 50 was greater than chance ($\chi^2(1) = 5.58$, $p < .02$).

However, participants in the interval list condition ($M = 49.6$) provided evaluations that did not differ from 50.0, suggesting that they perceived the quality of Blue Water Grill, with 20 complaints, to be exactly in between the restaurant with 19 complaints (Jean George) and the restaurant with 21 complaints (Nobu; $t(74) = .16$, $p > .87$). Furthermore, the proportion of participants (45%) who provided ratings above 50 was no different from chance ($\chi^2(1) = .65$, $p > .41$).

Discussion

Although our field study (study 1) suggests that the top-ten effect is not limited to the number 10, we provide more direct evidence for this point in study 4 by demonstrating the top-ten effect for an item ranked number 20. The design of study 4 also allowed us to demonstrate a boundary condition for the top-ten effect. Unlike ordinal lists, interval lists appear to be less susceptible to round-number categorization. Our results indicate that the top-ten effect may only occur for ranked lists, where distances between adjacent items technically do not have to be the same. Interestingly, we did not find evidence for place-value categorization in interval lists, which we will discuss further in the General Discussion.

We conducted a follow-up study that was similar to study 4 to test another plausible boundary condition of the top-ten effect. Specifically, we wanted to determine whether the effect would hold for ordinal lists that were reverse ordered (i.e., ranked from worst to best) or if another subjective categorization scheme would be evoked. Using the same stimuli and dependent measure as study 4, we ran this follow-up study with 188 participants (35% female; mean age = 28.8 years) recruited through an online panel obtained on Amazon Mechanical Turk. Participants were randomly assigned to an ascending condition that was identical to the ordinal list condition of study 4 or to a new descending condition, in which they encountered the same set of restaurants that were listed from number 39 to number 1 instead (see app. D for the stimuli).

The results of our follow-up study revealed a robust top-ten effect for both ascending and descending lists. Specifically, participants in the ascending condition ($M = 58.2$) provided evaluations that were significantly higher than 50.0, suggesting that they perceived the 20th ranked restaurant to be closer in quality to the 19th ranked restaurant than the 21st ranked restaurant, a result that replicates the ordinal list condition of study 4 ($t(94) = 3.86$, $p < .001$). Moreover, participants in the descending condition ($M = 58.7$) also provided evaluations that were significantly higher than 50.0, suggesting that they too perceived the 20th ranked restaurant to be closer in quality to the 19th ranked restaurant than the 21st ranked restaurant ($t(92) = 4.16$, $p < .001$). There were no between-condition differences ($t(186) = .16$, $p > .87$). Thus, our follow-up study suggests that the top-ten effect occurs for both ascending and descending ordinal lists. Next, in studies 5 and 6, we focus on identifying a mechanism for the top-ten effect in ranked lists.

STUDY 5

In study 5, we attempt to isolate a psychological mechanism underlying the top-ten effect. We hypothesize that the effect occurs because round-number categories (e.g., top 10) are cognitively accessible to consumers because of their prevalence in everyday communication. As a result, consumers create a mental partition between ranks on opposite sides of round-number-category boundaries (e.g., 10 vs. 11). If this is the case, we reasoned that we might be able to eliminate this effect by reducing the accessibility of round-number categories. We implement this by temporarily increasing the accessibility of ranked lists that highlight "sharp numbers" (Dehaene 1997) rather than round ones, such as the "top 101" or the "top 19."

Method

Eighty-one students at Seattle University (43% female; mean age = 20.8 years) participated in a study examining ranked-list inclusion claims in advertising in exchange for course credit. In the first part of the study, participants were shown four advertisements, each of which made a ranked-list inclusion claim. They viewed each ad for at least 5 seconds and then evaluated the advertised product featured in this ad. Our round-number accessibility manipulation was embedded within these claims. Participants in the high accessibility condition were shown four advertisements featuring ranked-list claims ending in round numbers (i.e., top 5, top 20, top 25, top 100). Participants in the low accessibility condition encountered claims that did not end in a round number (i.e., top 6, top 19, top 24, top 101). The products that were featured in these advertisements (i.e., energy drinks, jet skis, smartphones, places to live) did not differ across conditions.

In the second part of the study, all participants were shown an identical ranked list, specifically a *Forbes* magazine list of the 15 best companies to work for (see app. E for the stimuli). Of interest to us were the evaluations of three companies that appeared on this list, namely Quicken Loans (number 10), Zappos.com (number 11), and Mercedes Benz USA (number 12). We had conducted a pretest with 29 other students (41% female; mean age = 21.3 years) and determined that these three companies were considered equally good places to work when their specific rankings on the *Forbes* list were not disclosed ($F(2, 56) = .30$, $p > .58$).

After reviewing the rank-ordered *Forbes* list, participants in the main study then answered two questions in which they evaluated Zappos.com (number 11) relative to the two companies that were adjacent to it on the list. They evaluated Zappos.com (number 11) relative to Quicken Loans (number 10) on a scale ranging from 0 ("Quicken Loans and Zappos.com are probably equally good places to work") to 100 ("Quicken Loans is probably a far superior place to work than Zappos.com"). Then they evaluated Zappos.com (number 11) relative to Mercedes Benz USA (number 12) on a scale ranging from 0 ("Zappos.com and Mercedes Benz USA are probably equally good places to work") to 100 ("Zappos.com is probably a far superior place to work than Mercedes Benz USA"). The order of these two questions was randomized between participants. Based on our pretest results, we expected that these two judgments would yield undifferentiated evaluations unless any category-boundary effects were present.

Finally, to verify that our cognitive accessibility manipulation was successful, participants were asked to reflect on all the ranked lists they had ever encountered (including the ones they had viewed during the experimental session) and indicate the percentage of ranked lists ending in either 0 or 5 (e.g., top 10, top 25, top 100) on a sliding scale ranging from 0% to 100%.

Results

Our round-number-accessibility manipulation was successful. Participants in the high-round-number-accessibility condition estimated that a larger percentage of ranked lists end in zeros or fives ($M = 77.1$) as compared to participants in the low-round-number-accessibility condition ($M = 64.4$; $t(79) = 2.11$, $p < .04$).

To test whether the two relative evaluations differed among participants in the high- versus low-round-number accessibility conditions, we conducted a mixed ANOVA. There was neither a main effect of the company being evaluated ($F(1, 79) = .92$, $p > .34$) nor a main effect of round-number accessibility ($F(1, 79) = .76$, $p > .38$) on evaluations. However, more central to our theorizing, we found a significant interaction between these variables ($F(1, 79) = 4.54$, $p < .04$).

Participants in the high-round-number-accessibility condition exhibited the top-ten effect that we had seen in earlier studies. Among this group, the perceived gap between Zappos.com (ranked number 11) and the number 10 company on the list, Quicken Loans ($M = 52.7$) was more pronounced than the gap between Zappos.com and the number 12 company on the list, Mercedes Benz USA ($M = 41.6$; $F(1, 79) = 4.60$, $p < .04$). However, for those in the low-round-number-accessibility condition, the perceived gap between the adjacent between-round-number-category companies (number 10 and number 11; $M = 41.2$) was not statistically different from the gap between the adjacent within-round-number-category companies (number 11 and number 12; $M = 45.4$; $F(1, 79) = .72$, $p > .40$).

As another comparison, participants in the high-round-number-accessibility condition perceived the gap between the number 10 and number 11 company to be greater ($M = 52.7$) than participants in the low-round-number-accessibility condition ($M = 41.2$; $F(1, 79) = 4.45$, $p < .04$). However, participants in the high-round-number-accessibility condition perceived the gap between the number 11 and number 12 company ($M = 41.6$) no differently than participants in the low-round-number-accessibility condition ($M = 45.4$; $F(1, 79) = .41$, $p > .52$.) The results of study 5 are displayed in figure 5.

Discussion

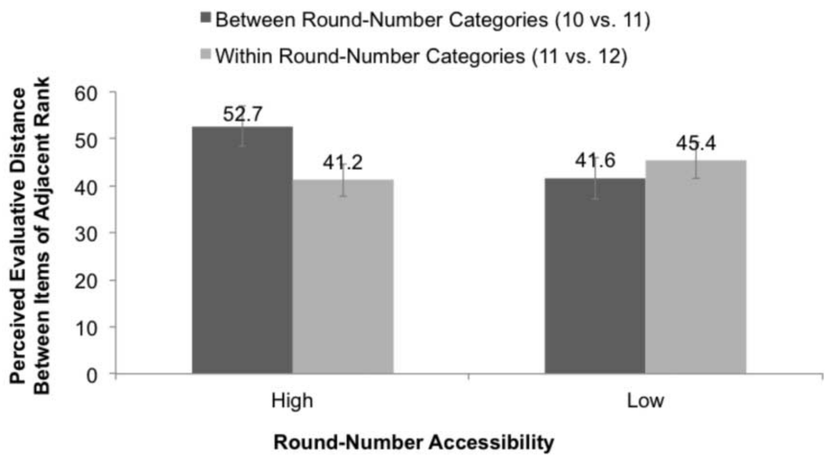
Study 5 provides evidence that it is the high cognitive accessibility of round-number categories (e.g., ranks ending in zero) that leads consumers to use these categories to interpret ranked lists, which in turn creates mental partitions between numbers adjacent to the boundaries between these round-number categories. When round-number categories are made less accessible (by highlighting the prevalence of ranked-list claims that use sharp numbers), these categories are not used, and the mental partitions between them are not observed.

It is worth noting that we consider the top-ten effect a cognitive bias, in that the actual distance between adjacent items that cross a round-number border (e.g., rank 10 vs. rank 11) is not typically greater than the distance between two adjacent items in the same round-number category (e.g., rank 9 vs. rank 10). If instead the top-ten effect were the generalization of a true principle learned from life experience (i.e., that a disproportionate evaluative gap typically exists following ranks with round-number endings) rather than the application of a subjective categorization process, then our study 5 manipulation to disrupt the categorization process should not have destroyed the top-ten effect. Our finding that increasing the accessibility of sharp numbers inhibits the top-ten effect is evidence against a learned-from-life alternative explanation.

In study 5, when participants were primed with four sharp ranked-list claims, the top-ten effect was eliminated in a subsequent evaluation task. At first glance, this result may seem at odds with studies 3 and 4, where the top-ten effect was observed for ranked lists with a sharp number of items (28 and 39, respectively). We suspect that these divergent results occur because the ranked-list claims of study 5 imply a categorization norm, whereas the set sizes of studies 3 and 4 do not. As the manipulation check of study 5 showed, participants who encountered four advertisements with sharp ranked-list claims were less inclined to believe in the pervasiveness of round-number categories. On the other hand, participants in study 3, upon learning about a math class with a sharp number of students (i.e., 28), probably did not update their default (round-number) categorization scheme. Stated differently, we believe that it is the relative cognitive accessibility of round versus sharp *categories*, and not

FIGURE 5

TARGET EVALUATION AS A FUNCTION OF ROUND-NUMBER ACCESSIBILITY (STUDY 5)



NOTE.—Participants in the high round-number accessibility condition exhibited the top-ten effect that we had seen in earlier studies. However, for those in the low-round-number-accessibility condition, the perceived gap between the adjacent between-round-number-category companies was not statistically different from the gap between the adjacent within-round-number-category companies.

merely the accessibility of round versus sharp *numbers*, that dictates whether or not the top-ten effect will arise.

STUDY 6

In study 5, we manipulated the cognitive accessibility of round-number categories using a priming task and found that the top-ten effect occurred only when round (vs. sharp) numbers were made temporarily accessible. However, our other studies suggest that round-number categories may be cognitively accessible even when they have not been explicitly primed, presumably because of their normative or prevalent everyday usage. Indeed, the results of studies 1–4 imply that round-number categories are used to interpret lists even when their temporary accessibility has not been bolstered via an experimental intervention. Nevertheless, in study 6, we aim to provide further evidence that round-number categories are naturally accessible and can produce the top-ten effect, even in the absence of temporary primes.

If the everyday use of round-number categories does in fact contribute to the top-ten effect, then we should not observe this effect in domains where another categorization scheme is normative. Therefore, in study 6, we vary the underlying meaning of the items in a ranked list to show that context-dependent norms moderate the occurrence of the top-ten effect. To accomplish this, we present respondents with an uncategorized list of eight male names. When the names are described as the top male baby names, we expect the natural tendency to use round-number categories as a default to arise. However, when the names are described as the top finishers in an athletic event, we anticipate a different categorization scheme to emerge that is prevalent in athletic events (i.e., medalists vs. nonmedalists), thereby inhibiting the top-ten effect.

Finally, study 6 was designed to provide direct evidence of the categories that consumers form when interpreting ranked lists. In each of the previous studies, use of categories in the interpretation of ranked-list information is inferred from responses to items on the lists. We relied on inferential evidence in these earlier studies because one of the goals of this article was to show that consumers subjectively categorize ranked lists on their own accord, even without being prompted to do so. Given the preponderance of evidence from our earlier studies that consumers subjectively categorize ranked lists, we shift our focus in study 6 to obtaining unambiguous evidence of round-number categorization by explicitly asking participants to subdivide a ranked but uncategorized list of items.

Method

Our sample consisted of 83 students at Seattle University (44% female; mean age = 21.8 years), who completed a pencil-and-paper study on consumer decision making in exchange for course credit. All participants were given an identical list of eight male names, ranked from first to eighth (i.e., #1-Jacob, #2-Benjamin, #3-William, #4-Matthew, #5-Alexander, #6-Caleb, #7-Logan, and #8-Dylan) to review. Although the ranked list that participants encountered was identical for all, we manipulated what the list represented by providing different instructions to two randomly assigned groups. Half of the participants were informed that the list represented "the top 8 male baby names of the past year." The remaining participants learned that the list was of "the top 8 male swimmers in a recent swim meet" (see app. F for the stimuli).

All participants were instructed to subdivide the list into two categories without changing the relative positions of any member on the list, thereby creating a "top" group and a "bottom" group. In order to keep participants from simply splitting the original list in half, which is a likely categorization strategy when sorting brief arrays of items into two groups, participants were not allowed to create a partition between the fourth and fifth item on the list. Finally, we asked participants to provide a one- or two-sentence explanation for their categorization process.

Results

We predicted that the normative tendency to generate round-number categories would prevail for the list of baby names that were ranked in order of popularity, since no other natural bases for categorization would come to mind. Therefore, we expected most consumers who encountered the list of baby names to create a partition between ranks 5 and 6. On the other hand, we expected round-number categories to be relatively less accessible for ranked lists based on individual athletic achievement (e.g., swim performance). Particularly for sports strongly associated with the Olympics, such as swimming, we surmised that a different categorization norm might operate in which medalists versus nonmedalists are placed into different categories. Thus, we hypothesized that a natural partition might exist between the top three performers (i.e., the gold, silver, and bronze medalists) and the rest of the field, creating a mental partition between ranks 3 and 4.

To test our hypothesizing, we examined the breakpoints that participants employed when creating two categories from the original eight-member list. See table 3. In line with our predictions, 65% of participants who encountered the list of baby names (26 out of 40) created a partition between the fifth name and sixth name on the list, which was significantly greater than chance ($\chi^2(1) = 67.23$, $p < .001$) and nearly a significant majority of all responses ($\chi^2(1) = 3.60$, $p = .058$). Furthermore, this proportion was significantly higher than the 12% of participants in the swim performance condition who generated a round-number category (i.e., top 5; $\chi^2(1) = 25.23$, $p < .001$). Our claim that round-number categorization is a normative default is further supported by the varied set of reasons provided by participants when asked to briefly explain the rationale for their categorization scheme. The 26 participants in this condition who suggested creating a partition between ranks 5 and 6 generated a total of nine different reasons for splitting the list as they had done (one participant in this condition did not respond to this question), including idiosyncratic preferences, arbitrary selection, or the length, familiarity, and trendiness of certain names. The lack of a consensus reason for grouping the top 5 baby names together suggests that round-number categorization may be a normative default that consumers do not consciously recognize.

TABLE 3

EXPLICIT CATEGORIZATION OF AN EIGHT-ITEM LIST INTO TWO GROUPS (STUDY 6)

	List of eight male baby names		List of eight male swimmers	
Size of top category	*n*	%	*n*	%
Top 1	1	3	3	7
Top 2	2	5	5	12
Top 3	8	20	28	65
Top 4*	. . .	. . .	. . .	. . .
Top 5	26	65	5	12
Top 6	3	8	2	5
Top 7	0	0	0	0
Total	40	100	43	100

NOTE.—When the eight-item list contained top male baby names, the natural tendency to use round-number categories (i.e., top 5) as a default was observed. However, when the same names were described as the top finishers in an athletic event, a different categorization scheme (i.e., top 3) that is prevalent in athletic events (i.e., medalists vs. nonmedalists) was prevalent.

*Participants were instructed not to divide the list into two equal groups, which is a likely categorization strategy when dividing brief arrays of items into two groups.

Participants who encountered the list of swimmers exhibited an entirely different categorization tendency. Consistent with our theorizing, 65% of participants in this condition chose to create a partition between the top three swimmers and the rest of the field (28 out of 43), a percentage that was greater than chance ($\chi^2(1) = 72.67$, $p < .001$) and a significant majority of all responses ($\chi^2(1) = 3.93$, $p < .05$). Additionally, this proportion was significantly higher than the 20% of participants in the baby name condition who generated a top 3 category ($\chi^2(1) = 17.17$, $p < .001$). Furthermore, the rationale provided by participants in this condition for their categorization scheme was extremely consistent and suggests their awareness of another prevalent norm in this domain. Specifically, of the 28 participants in this condition who suggested creating a partition between ranks 3 and 4, 19 indicated that they had placed medalists and nonmedalists in separate categories (two participants in this condition did not respond to this question). Representative responses from participants in this condition include "The top 3 swimmers would be the ones receiving bronze, silver, and gold medals" and "The top 3 would receive medals, so I put them in one group."

Discussion

The results of this study indicate that, in a context that does not provide a prominent means of separating the elements of a list into categories (such as top baby names), round-number categorization will tend to be used. However, when prevalent everyday usage in a domain suggests other categories (e.g., the top 3 winners of an athletic event), then those categories will tend to be used instead.

Consideration of study 6 along with study 5 indicates that both temporary factors, such as accessibility priming, and long-term factors, such as prevalent everyday usage, can moderate the occurrence of the top-ten effect. Together, these two studies suggest the importance of both factors; in fact, prevalent everyday usage may act to increase the cognitive accessibility of categories and cause those categories to be used in the interpretation of ranked-list arrays.

GENERAL DISCUSSION

In a series of six studies, we demonstrate the existence of a cognitive bias in the interpretation of ranked lists—an exaggeration of the perceived distance between ranks that border round-number categories. We refer to this phenomenon as the top-ten effect. Along with demonstrating the generalizability of this effect, we highlight certain boundary conditions and characterize a psychological mechanism that underlies this effect.

In study 1, we use real-world longitudinal data to show that rank improvements that cross the boundaries of round-number categories have a more pronounced effect on consumer behavior than other types of rank changes. In study 2, we provide experimental evidence for the consumer's use of round-number categories by showing superior recall of ranked items that are adjacent to a round-number-category boundary. In study 3, we confirm the implications of this use of round-number categories by experimentally showing an exaggerated evaluative difference across a round-number-category border but not across a place-value-category border.

In study 4, we show a boundary condition of the top-ten effect—it does not occur with interval-scaled rather than ordinal-scaled numbers; however, a follow-up study indicates that the top-ten effect does emerge for both ascending and descending ordinal lists. In study 5, we support the role of cognitive accessibility in the use of round-number categories by showing that the top-ten effect can be eliminated by experimentally reducing the high cognitive accessibility of round-number categories. In study 6, we use a categorization task to show that, in the absence of context-specific category information, consumers do produce round-number categories but that context-specific category norms can override the tendency to use round-number categories.

Theoretical Contributions

These studies contribute to the literature on categorization by demonstrating that subjective categorization and its perceptual consequences will occur with ranked lists. Despite the fact that ranked lists are already explicitly ordered and highly structured, our work demonstrates that consumers nevertheless mentally subdivide them into a smaller set of subjectively generated categories. Furthermore, we show that the subjective categorization of ranked lists affects perceptions of distance in a manner akin to arrays that come to the perceiver already categorized on the basis of spatial and/or social criteria—in all cases, information users tend to exaggerate the perceived evaluative distance between items of different categories. In the case of ranked lists, we find evidence of exaggerated evaluative gaps between adjacent items at category borders, which indicates that the tendency to perceptually separate categories overrides any presumption of numerical equidistance. It also suggests that the within-category distance exaggeration (the "ranking effect") found by Leclerc et al. (2005) does not occur in the perception of ranked lists. This is consistent with their finding that the ranking effect disappears in between-category evaluation situations, which seem to be likely when consumers use ranked-list information.

Our research also contributes to the literature on numerical cognition by showing that round-number categories (e.g., top 10, top 25), rather than place-value categories (e.g., single digits, the twenties), guide the interpretation of ranked lists. The common use of numeral 9 endings in retail prices indicates the widespread belief (confirmed by research, e.g., Thomas and Morwitz 2005) that there is a major discontinuity in the evaluation of a 0-ending number and the 9-ending number next to it. Our studies indicate that there can also be an exaggerated gap between a 0-ending number and the adjacent 1-ending number. Further, we have shown that the use of round-number categories, which produces this gap, is related to the high cognitive accessibility of round numbers, due in turn to their normative or typical everyday use. Indeed, our work suggests that the top-ten effect may not be seen in contexts where category group sizes other than multiples of 5 and 10 are the group sizes most commonly used.

Furthermore, this research distinguishes between various types of numerical information in terms of their susceptibility to category-based evaluative gaps. Specifically, we demonstrate that interval lists are less susceptible than ordinal lists to round-number categorization (study 4) and that positive round-number-category shifts may produce stronger effects than negative round-number-category shifts (study 1). Our work also indicates that the top-ten effect generalizes to both ascending and descending ranked lists, to ranked lists that have either a sharp or round number of items, and to round numbers that end in the digits 0 or 5, not just the number 10.

Managerial Implications

Knowing that consumers will tend to exaggerate the differences between ranks bordering round-number categories has important implications for companies whose products and services are subject to third-party ranked lists. It suggests that a firm should invest more aggressively in improving its rank if it is just on the outside looking in to a round-number category. For example, an organization ranked number 26 or number 11 could substantially benefit from increasing its rank one or two steps to become number 25 or number 10. In contrast, a similar rank-increasing effort by an organization that is already ranked number 9 or number 24 might not provide cost-effective results.

Applying our finding concerning the role of cognitive accessibility in producing the top-ten effect, it may be possible for sellers of products ranked just on the outside of a round-number category to make sharp numbers prominent so as to reduce consumers' tendencies to see their products as second-rate. For example, it may be possible to temporarily reduce the cognitive accessibility of round-number categories by mentioning ranked lists involving sharp numbers (e.g., "the top 12" items in a category) as we did in study 5 or by cueing sharp numbers in other contexts. As for consumers, if they were made aware of the perceptual effects of subjective categorization, they might be able to use this knowledge to compensate for their tendency to experience this cognitive bias.

Future Directions

Our work raises the question of the conditions under which round-number effects, rather than place-value effects, are likely to occur. Specifically, why do we observe place-value effects in perceiving price information (e.g., Thomas and Morwitz 2005) but round-number effects when responding to information from ranked lists? One possibility is suggested by the finding of study 4 that the top-ten effect did not occur when the sequence of numbers represented a quantity of items rather than ordinal ranks —prices are certainly quantities. However, this possibility raises the question of why a place-value effect was not observed in our study 4 results. Our tentative explanation, which would need to be confirmed in future research, is that the monotonic succession of numbers 1–39 may have evoked ranks to some interval-condition respondents and weakened the study's manipulation sufficiently to prevent the observation of a place-value effect.

A second, somewhat related possibility to account for when round-number rather than place-value effects will occur may have to do with whether a big-picture, "ballpark" estimate is sufficient or if people are thinking more concretely and specifically. Building on construal-level theory (Trope and Liberman 2010), consumers thinking at a big-picture level, from a standpoint of greater psychological distance, may use more abstract, round-number categories. When they are thinking more concretely, with less psychological distance, consumers may focus instead on the concrete numbers in a price and employ place-value categories.

Such a psychological-distance view would suggest that when consumers think about prices in more abstract or general terms, they may use round-number rather than place-value categories. This might explain why consumers often set round-number prices in "pay-what-you-want" situations, such as determining how much to spend at a self-service gasoline pump or deciding how much to tip at a restaurant (Lynn, Flynn, and Helion 2013). Further exploration of such phenomena could be a fruitful topic for future research.

Our research on the top-ten effect may also have implications for other ways that round-number categories can be involved in retail transactions. One possibility is that retailers who use tensile price claims (e.g., Mobley, Bearden, and Teel 1988) may find it worthwhile to advertise a number that just exceeds a round-number category. For example, the claim "save up to 51%" may evoke not just saving "half" but saving "more than half," thus bringing to mind a second round-number category. Similarly, it is possible that, in some situations, package sizes might benefit from being just above round-number amounts.

Future research might attempt to identify whether the top-ten effect will extend to other situations that were not empirically investigated in this article. Our follow-up to study 4 suggests that both ascending (e.g., number 1 to number 39) and descending (e.g., number 39 to number 1) ranked lists exhibit the top-ten effect. But what about lists where larger numerical values are preferable (e.g., quality ratings) rather than unfavorable (e.g., rankings, complaints)? Further work is also required to determine how, if at all, the number of categories generated by information users varies as a function of list size.

Conclusions

The results of the series of studies that we report here highlight that there is not a one-to-one correspondence between the numbers that are seen in ranked lists and the impressions that are perceived. Just as the field of psychophysics has mapped the relations between physical stimuli and subjective impressions, our findings could be considered a starting point for the mapping of numerical ranks to their implications in the minds of consumers. In this sense, our results could be considered an investigation into the psychophysics of rank.

The top-ten effect that we demonstrate in these studies is based on the mental tendencies to use categories and to exaggerate the differences between them. These tendencies are part of the natural human readiness to perceive the world in terms of discrete things (Rock 1983, 25). Our results suggest that it is wise for marketers to be in tune with this aspect of human nature. By researching consumers' categories—the "things" in the environment that are salient to consumers—marketers can become better able to speak to consumers in their own terms.

DATA COLLECTION INFORMATION

The first author supervised the collection of data for all studies and had primary responsibility for data analysis. However, the second author had access to all data sets, and the data were discussed and results reviewed on multiple occasions by both authors. The GMAT data for study 1 were received by the first author in June 2009 after he was awarded a MERInstitute Doctoral Student Fellowship by the Graduate Management Admissions Council. Studies 2–4 and the study 4 follow-up were conducted using paid online participants living in the United States who were recruited using Amazon Mechanical Turk in February 2013, March 2012, July 2013, and September 2013, respectively. Study 5 was conducted online with students at Seattle University in May 2012 in exchange for course credit. The first author's undergraduate research assistant at Seattle University helped organize study 5 data in Microsoft Excel. Study 6 was conducted on pencil and paper with students at Seattle University in May 2013 in exchange for course credit.

APPENDIX A

FIGURE A1

STUDY 2 STIMULI

Condition 1

Rank	School
1	Chicago (Booth)
2	Harvard
3	Pennsylvania (Wharton)
4	Stanford
5	Northwestern (Kellogg)
6	Duke (Fuqua)
7	Cornell (Johnson)
8	Michigan (Ross)
9	MIT (Sloan)
10	Virginia (Darden)
11	Carnegie Mellon (Tepper)
12	Dartmouth (Tuck)
13	UC-Berkeley (Haas)
14	Columbia
15	Indiana (Kelley)
16	NYU (Stern)
17	North Carolina (Kenan-Flagler)
18	UCLA (Anderson)
19	Texas-Austin (McCombs)
20	Notre Dame (Mendoza)
21	Yale
22	Emory (Goizueta)
23	Georgia Tech (Scheller)
24	Maryland (Smith)
25	Vanderbilt (Owen)

Condition 2

Rank	School
1	Chicago (Booth)
2	Harvard
3	Pennsylvania (Wharton)
4	Stanford
5	Northwestern (Kellogg)
6	Duke (Fuqua)
7	Michigan (Ross)
8	MIT (Sloan)
9	Virginia (Darden)
10	Carnegie Mellon (Tepper)
11	Dartmouth (Tuck)
12	UC-Berkeley (Haas)
13	Columbia
14	Indiana (Kelley)
15	Cornell (Johnson)
16	NYU (Stern)
17	North Carolina (Kenan-Flagler)
18	UCLA (Anderson)
19	Texas-Austin (McCombs)
20	Notre Dame (Mendoza)
21	Yale
22	Emory (Goizueta)
23	Georgia Tech (Scheller)
24	Maryland (Smith)
25	Vanderbilt (Owen)

Condition 3

Rank	School
1	Chicago (Booth)
2	Harvard
3	Pennsylvania (Wharton)
4	Stanford
5	Northwestern (Kellogg)
6	Duke (Fuqua)
7	Cornell (Johnson)
8	Michigan (Ross)
9	MIT (Sloan)
10	Carnegie Mellon (Tepper)
11	Virginia (Darden)
12	Dartmouth (Tuck)
13	UC-Berkeley (Haas)
14	Columbia
15	Indiana (Kelley)
16	NYU (Stern)
17	North Carolina (Kenan-Flagler)
18	UCLA (Anderson)
19	Texas-Austin (McCombs)
20	Notre Dame (Mendoza)
21	Yale
22	Emory (Goizueta)
23	Georgia Tech (Scheller)
24	Maryland (Smith)
25	Vanderbilt (Owen)

Condition 4

Rank	School
1	Chicago (Booth)
2	Harvard
3	Pennsylvania (Wharton)
4	Stanford
5	Northwestern (Kellogg)
6	Duke (Fuqua)
7	Michigan (Ross)
8	MIT (Sloan)
9	Carnegie Mellon (Tepper)
10	Virginia (Darden)
11	Dartmouth (Tuck)
12	UC-Berkeley (Haas)
13	Columbia
14	Indiana (Kelley)
15	Cornell (Johnson)
16	NYU (Stern)
17	North Carolina (Kenan-Flagler)
18	UCLA (Anderson)
19	Texas-Austin (McCombs)
20	Notre Dame (Mendoza)
21	Yale
22	Emory (Goizueta)
23	Georgia Tech (Scheller)
24	Maryland (Smith)
25	Vanderbilt (Owen)

APPENDIX B

FIGURE B1

STUDY 3 STIMULI

Below are the names of the 28 students in Ms. Smith's 8th Grade Math Class, ranked in order of their performance on the last exam:

Condition 1	**Condition 2**	**Condition 3**	**Condition 4**	**Condition 5**
1) Lisa Cahill	1) Lisa Cahill	1) Lisa Cahill	1) Lisa Cahill	1) Lisa Cahill
2) Harry Dammer	2) Harry Dammer	2) Harry Dammer	2) Harry Dammer	2) Harry Dammer
3) Margaret Davis	3) Margaret Davis	3) Margaret Davis	3) Margaret Davis	3) Margaret Davis
4) Susanne Foster	4) Susanne Foster	4) Susanne Foster	4) Susanne Foster	4) Susanne Foster
5) John O'Callaghan	5) John O'Callaghan	5) Paul Murphy	5) Patrick Howell	5) May Cassidy
6) Raymond Schroth	6) Mark Scalese	6) John O'Callaghan	6) Paul Murphy	6) Patrick Howell
7) Aparna Venkatesan	7) Raymond Schroth	7) Mark Scalese	7) John O'Callaghan	7) Paul Murphy
8) Charles Pipp	8) Aparna Venkatesan	8) Raymond Schroth	8) Mark Scalese	8) John O'Callaghan
9) Matthew Brooks	9) Charles Pipp	9) Aparna Venkatesan	9) Raymond Schroth	9) Mark Scalese
10) Mary-Antoinette Smith	10) Matthew Brooks	10) Charles Pipp	10) Aparna Venkatesan	10) Raymond Schroth
11) Jim Chamberlain	11) Mary-Antoinette Smith	11) Matthew Brooks	11) Charles Pipp	11) Aparna Venkatesan
12) Christina Astorga	12) Jim Chamberlain	12) Mary-Antoinette Smith	12) Matthew Brooks	12) Charles Pipp
13) May Cassidy	13) Christina Astorga	13) Jim Chamberlain	13) Mary-Antoinette Smith	13) Matthew Brooks
14) James Keenan	14) May Cassidy	14) Christina Astorga	14) Jim Chamberlain	14) Mary-Antoinette Smith
15) Mark Scalese	15) James Keenan	15) May Cassidy	15) Christina Astorga	15) Jim Chamberlain
16) David Hollenbach	16) David Hollenbach	16) James Keenan	16) May Cassidy	16) Christina Astorga
17) Janet Quillian	17) Janet Quillian	17) David Hollenbach	17) James Keenan	17) James Keenan
18) Tony Lowney	18) Tony Lowney	18) Janet Quillian	18) David Hollenbach	18) David Hollenbach
19) Patrick Howell	19) Patrick Howell	19) Tony Lowney	19) Janet Quillian	19) Janet Quillian
20) Paul Murphy	20) Paul Murphy	20) Patrick Howell	20) Tony Lowney	20) Tony Lowney
21) Kyle Outlaw	21) Kyle Outlaw	21) Kyle Outlaw	21) Kyle Outlaw	21) Kyle Outlaw
22) Majorie Allen	22) Majorie Allen	22) Majorie Allen	22) Majorie Allen	22) Majorie Allen
23) Tom Kelly	23) Tom Kelly	23) Tom Kelly	23) Tom Kelly	23) Tom Kelly
24) Bill Denison	24) Bill Denison	24) Bill Denison	24) Bill Denison	24) Bill Denison
25) Mark Ravizza	25) Mark Ravizza	25) Mark Ravizza	25) Mark Ravizza	25) Mark Ravizza
26) Yvonne Harrison	26) Yvonne Harrison	26) Yvonne Harrison	26) Yvonne Harrison	26) Yvonne Harrison
27) Stephen Birdsell	27) Stephen Birdsell	27) Stephen Birdsell	27) Stephen Birdsell	27) Stephen Birdsell
28) Dan Pearson	28) Dan Pearson	28) Dan Pearson	28) Dan Pearson	28) Dan Pearson

APPENDIX C

STUDY 4 STIMULI

Ordinal List Condition (Ascending)

Based on input from restaurant patrons, food critic Michael Levine has ranked the quality of each of the restaurants he visited in the past year from #1 to #39. Higher-ranked restaurants appear near the top.

Please take a minute and review these restaurants closely, paying particular attention to the Blue Water Grill, which was ranked number 20.

1 Gramercy Tavern
2 Union Square Café
3 Le Bernardin
4 Babbo
5 Peter Luger
6 Bouley
7 Gotham Bar & Grill
8 Daniel
9 Atlantic Grill
10 Four Seasons
11 Aquavit
12 Eleven Madison Park
13 Per Se
14 Rosa Mexicano
15 Balthezar
16 The Modern
17 Chanterelle
18 Tabla
19 Jean Georges
 # 20 Blue Water Grill
21 Nobu
22 Artisanal
23 Davidburke/donatella
24 Café Gray
25 Blue Hill
26 One if BY Land
27 Spice Market
28 Il Mulino
29 Café des Artistes
30 Aquagrill
31 Aureole
32 Picholine
33 Palm
34 Saigon Grill
35 L'Inpero
36 Craft
37 Lupa
38 Blue Ribbon
39 Café Boulud

Interval List Condition

Based on input from restaurant patrons, food critic Michael Levine has compiled the total number of formal complaints received by each of the restaurants he has visited in the past year. Restaurants with fewer complaints appear near the top.

Please take a minute and review these restaurants closely, paying particular attention to the Blue Water Grill, which received 20 complaints.

Gramercy Tavern—1 complaint
Union Square Café—2 complaints
Le Bernardin—3 complaints
Babbo—4 complaints
Peter Luger—5 complaints
Bouley—6 complaints
Gotham Bar & Grill—7 complaints
Daniel—8 complaints
Café des Artistes—9 complaints
Aquagrill—10 complaints
Aureole—11 complaints
Eleven Madison Park—12 complaints
Per Se—13 complaints
Rosa Mexicano—14 complaints
Balthezar—15 complaints
The Modern—16 complaints
Chanterelle—17 complaints
Tabla—18 complaints
Jean Georges—19 complaints
Blue Water Grill—20 complaints
Nobu—21 complaints
Artisanal—22 complaints
Davidburke/Donatella—23 complaints
Café Gray—24 complaints
Blue Hill—25 complaints
One if BY Land—26 complaints
Spice Market—27 complaints
Il Mulino—28 complaints
Atlantic Grill—29 complaints
Four Seasons—30 complaints
Aquavit—31 complaints
Picholine—32 complaints
Palm—33 complaints
Saigon Grill—34 complaints
L'Inpero—35 complaints
Craft—36 complaints
Lupa—37 complaints
Blue Ribbon—38 complaints
Café Boulud—39 complaints

APPENDIX D

STUDY 4 FOLLOW-UP STIMULI

Ordinal List Condition (Descending)

Based on input from restaurant patrons, food critic Michael Levine has ranked the quality of each of the restaurants he visited in the past year from #39 to #1. Higher-ranked restaurants appear near the bottom.

Please take a minute and review these restaurants closely, paying particular attention to the Blue Water Grill, which was ranked number 20.

39 Gramercy Tavern
38 Union Square Café
37 Le Bernardin
36 Babbo
35 Peter Luger
34 Bouley
33 Gotham Bar & Grill
32 Daniel
31 Atlantic Grill
30 Four Seasons
29 Aquavit
28 Eleven Madison Park
27 Per Se
26 Rosa Mexicano
25 Balthezar
24 The Modern
23 Chanterelle
22 Tabla
21 Jean Georges
 # 20 Blue Water Grill
19 Nobu
18 Artisanal
17 Davidburke/donatella
16 Café Gray
15 Blue Hill
14 One if BY Land
13 Spice Market
12 Il Mulino
11 Café des Artistes
10 Aquagrill
9 Aureole
8 Picholine
7 Palm
6 Saigon Grill
5 L'Inpero
4 Craft
3 Lupa
2 Blue Ribbon
1 Café Boulud

APPENDIX E

STUDY 5 STIMULI

Rank	Company	Job growth	U.S. employees
1	Google	33%	18,500
2	Boston Consulting Group	10%	1,958
3	SAS Institute	8%	6,046
4	Wegmans Food Markets	5%	41,717
5	Edward Jones	1%	36,937
6	NetApp	30%	6,887
7	Camden Property Trust	−2%	1,678
8	Recreational Equipment (REI)	12%	10,466
9	CHG Healthcare Services	17%	1,312
10	Quicken Loans	20%	3,808
11	Zappos.com	70%	3,003
12	Mercedes-Benz USA	2%	1,680
13	DPR Construction	18%	1,265
14	DreamWorks Animation	8%	2,151
15	NuStar Energy	6%	1,512

APPENDIX F

STUDY 6 STIMULI

Top Baby Names Condition

Below is a list of names of the top 8 male baby names of the past year ranked from first to eighth. We would like you to put these 8 baby names into 2 groups, by using your pen to draw one circle around the top group and another circle around the bottom group.

The only restriction is that the two groups cannot have the same number of names. That is, you are not allowed to draw one circle around the top 4 names and another circle around the bottom 4 names.

#1 Jacob
#2 Benjamin
#3 William
#4 Matthew
#5 Alexander
#6 Caleb
#7 Logan
#8 Dylan

Top Swimmers Condition

Below is a list of names of the top 8 male swimmers in a recent swim meet ranked from first to eighth. We would like you to put these 8 swimmers into 2 groups, by using your pen to draw one circle around the top group and another circle around the bottom group.

The only restriction is that the two groups cannot have the same number of names. That is, you are not allowed to draw one circle around the top 4 names and another circle around the bottom 4 names.

#1 Jacob
#2 Benjamin
#3 William
#4 Matthew
#5 Alexander
#6 Caleb
#7 Logan
#8 Dylan

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