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Are Preferences Rank Stable?
The Persistence of Preference Reversal Asymmetries in a Static Choice Setting

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Abstract: In a static decision making environment, we find considerable evidence that preferences across two goods can be reversed through anchoring. By anchoring subjects counter to their initial preference ordering of tasks, using a simple labor-market experiment, we induce many subjects to take a smaller payment for a task they reveal inferior to another task during a direct ranking of the tasks . Similar to results in the preferences over gambles literature, we find these preference reversals to be asymmetric with respect to the initial preference. The results call into question the fundamental assumption that individuals are endowed with stable, coherent preferences that allow for intransigent orderings over consumption bundles and suggest explanations for the asymmetry in preference reversals previously put forth in the literature may need to be expanded to include preference heterogeneity across subjects and subject self selection.

Key Words: Preference reversals, experiment, coherent preferences

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Preference reversals, in which subjects favor one object in a set when preference is elicited via one method (e.g., choice among alternatives) but favor another object when preference is elicited via another method (e.g., inferred from auction bids), are well-documented. Most evidence of reversals comes from studies of preferences over lotteries with similar expected payoffs, though more evidence is emerging from studies with time differentiated payments or with payments involving issues of equity and fairness across several recipients (see Christian Seidl for a thorough review of preference reversals).

Evidence from studies of choice over gambles, time differentiated payments or interpersonal income distributions may simply expose the tenuousness of extending a static preference theory to richer choice environments rather than problems with the axioms girding static choice theory. A smaller though intriguing trove of evidence is emerging that documents similar reversals in simple, static settings that more directly contradicts the core assumptions of utility theory: Individuals are endowed with stable, coherent preferences that allow for intransigent orderings over consumption bundles regardless of the procedure used to elicit them.

For example, John List (2002) reports that average bid prices for a smaller, nested set of sports cards are higher than for the full set when bids are elicited for each set separately, but average bid prices are higher for the larger set when both sets are evaluated jointly. The bids are elicited from sports card enthusiasts and dealers spending their own money in a real market and the goods in question did not involve any of the complications outlined above. Because of its market setting, many of the critiques lodged by economists against preference reversal studies are clearly avoided (David Grether and Charles Plott 1979).

Dan Ariely and colleagues (2003) report several experiments in which they elicit subjects' willingness to accept compensation to experience unpleasant stimuli (noises, disagreeable drinks and mild pain) using incentive compatible bidding mechanisms. They find considerable arbitrariness in terms of cardinal preference structures but considerable robustness of preference orderings. Ariely et al. conclude that preferences are arbitrary but coherent, which leaves unchallenged the base assumption that preference orderings are stable though cardinal representations of these may not be unique. Other evidence of reversals in static situations has been documented in the psychology literature (see Christopher K. Hsee et al. 1999 and Seidl for summaries), though few of these studies feature market pressures and realities like the List or Ariely et al. studies.

Few studies performed on preference reversals in static situations (hereafter, SR for static reversals) utilize a within subject design, while many studies performed in richer choice contexts (hereafter GR for gamble reversals) feature within subject design. Hence the GR literature reports inconsistency within the same person while SR studies report reversals on the aggregate.[3] Extensive analysis of within subject reversals from the GR literature uncovers an unusual pattern within the anomaly. Reversals tend to occur for only one subset of respondents: those respondents who prefer gambles with high odds and low stakes to gambles of equivalent expected payouts but low odds and high stakes. The typical setup involves allowing respondents to choose between two such lotteries where the low odds, high stakes gamble is called the $-bet and the high odds, low stakes gamble is called the *P*-bet. Those who prefer the *P*-bet in a direct, joint comparison are

[3] The exception from the SR literature is Ariely et al., who collect multiple observations per subject but only report averages across treatment groups. Even if their data were analyzed for within subject reversals, it is unlikely that such inconsistencies would arise, as this was not the primary focus of their research.

much more likely to reverse themselves than those who prefer the $-bet when, later in the experiment, the minimum bids for selling each lottery are elicited.

A common explanation focuses on anchoring and adjustment. When faced with formulating dollar bids that may determine if the subject actually plays the gamble, subjects anchor on the amount of the potential payout because is it also demarcated in dollars. Because the $-bet involves a higher dollar figure than the *P*-bet and payouts for losing the bet are similarly small, the subjects are likely to state a higher price for the $-bet than the *P*-bet, that is subjects anchor their responses to the winning dollar payout for each bet. This increases the likelihood that those who chose the *P*-bet in the choice setting will be reversed and reduces the likelihood that those choosing the $-bet in the choice setting will be reversed. The anchoring implicit in many of the GR studies reinforces the preference ordering of those who initially choose the $-bet and offsets the preference ordering of those who initially choose the *P*-bet and from this pattern emerges the asymmetry. Subsequent experiments that reverse the direction of anchoring (David A. Schkade and Eric J. Johnson 1989) or state winning outcomes in units other than dollars (Paul Slovic et al. 1990) show that this asymmetry in reversals is often reduced, though typically not eliminated, which lends credence to anchoring and adjustment mechanisms as part of the explanation.

In this article, we induce within subject preference reversals in a static choice setting by using anchoring cues prior to eliciting selling prices. The experiment is formulated in a simple labor market context where subjects are asked their preferences for performing several menial tasks (among them, typing and adding) and then later asked to formulate bids representing the minimum willingness to accept to perform the tasks subject to an incentive compatible elicitation mechanism. In the process of reversing preferences, we identify an asymmetry in these static reversals where subjects who prefer typing words to adding numbers are likely to reverse themselves during the price elicitation stage, but subjects who prefer adding numbers rarely reverse themselves. Unlike the asymmetries in gambling reversals, however, reversing the direction of the anchors does not reduce this asymmetry. Furthermore, the plethora of other explanations forwarded for preference reversals do not seem to explain the asymmetry in static reversals we identify.

The remainder of the paper is organized as follows. We first introduce the two experiments and discuss the results. We then compare the results to those of other SR and GR studies and discuss similarities and differences. We finish with some hypotheses of what could be driving this static asymmetry result and a discussion of the implications of the results for microeconomic theory and behavioral economics.

Experiment 1

To test the rank stability of preferences, we develop an experiment that asks subjects to rank order two goods in the presence of external stimuli. Previous experiments have found that value elicitation experiments are more effective if they elicit subject willingness to accept to give up goods rather than the willingness to pay for non-endowed goods (Daniel Kahnemann et al., 1991). To avoid potential endowment effects, we elicit values for a good with which subjects are pre-endowed: their time. The experiment simulates a simple labor market in which we act as an employer and ask subjects to indicate their 'payment demanded' for two simple but potentially time-consuming tasks: addition and typing.

Subjects were recruited to participate in the computer-based experiment via posted flyers and e-mails sent to undergraduate and graduate students of several departments on campus. Subjects were told they would have the opportunity to participate in a computer-based experiment that would guarantee a minimum cash payout of $5 and would offer the possibility of earning more money during the course of the experiment. The expected time commitment for the experiment was less than one hour. The resulting subjects consisted of a mix of undergraduate and graduate students. The experiment is fully automated, and once the subjects begin, they have no contact with anyone other than questions of clarification to the moderator (a very rare occurrence). Seventy-one subjects participated in the first experiment.

The introductory section gets subjects to think about and rank their preferences over five tasks: Typing a list of words, adding pairs of numbers, mopping floors in an office building, working in a fast-food kitchen and telemarketing. On separate screens, with the order randomized, subjects are asked to imagine performing each task for one hour and then rank order the five tasks from most to least enjoyable if they were paid the same amount to perform each task.

We then provide a sample demonstration of two of the five tasks: typing and adding, and elicit a pre-treatment ranking of the two tasks. Subjects are told that later in the experiment they will have the opportunity to perform one of two tasks for pay. The exact tasks to be performed consist of typing a list of 500 words into the computer, or adding together 175 pairs of two-digit numbers and typing the sum into the computer. To familiarize subjects with the tasks and to allow them to form realistic expectations of the full task, each task is demonstrated, in random order, on a small scale. Subjects type a list of 30 words, and add together 10 pairs of numbers. The time to complete each of the demonstrations is recorded, and subjects are given an estimate of the time it will take to complete the full adding or typing task. Subjects can modify this predicted time according to their own expectations. Following the demonstrations, subjects rank the five tasks assuming the tasks paid the same. The lowest ranked (most enjoyable) of the adding and typing tasks determines the *pre-treatment preference.*

Following the pre-treatment preference elicitation, a series of questions establishes the payment demanded for each of the two tasks. The procedure for establishing the minimum payment demanded utilizes a Becker-DeGroot-Marshak (BDM) incentive compatible elicitation method described to subjects as follows: "We are going to ask you some questions to determine the smallest amount of money we would have to pay you to perform the tasks. We will refer to this as your 'wage demanded.' After we have established your wage demanded for each task, the computer will randomly choose <u>one of the two tasks.</u> After the task is chosen, the computer will randomly select a price and announce it to you. ***Please note that the price chosen by the computer is completely random and does not depend on your wage demanded for each task***. Depending on your wage demanded for the chosen task, and the announced price, you will be asked to either a) perform the task for the announced price and receive payment upon completion of the task, or b) not perform the task and receive no payment." No specifics are given to the subject regarding the distribution from which the random numbers are drawn as such information would dilute the effect of anchors we provide later in the bidding elicitation.[4] A series of follow-up screens repeats the procedure, and a quiz ensures subjects understand the procedure. Subjects are told that they have no incentive to reveal an amount other than their true lowest willingness to accept.

For the value elicitation questions, subjects are randomly assigned to one of three anchor-treatment groups: 25 percent receive a no anchor control treatment and 75 percent are randomly split between two anchor treatments. The no anchor treatment group receives an open ended value elicitation question for each task of the form: "We would like to know the lowest amount that we would have to pay you to add 175 pairs of number together. What is the smallest amount we will have to pay you to add 175 pairs of numbers?" A similar question is asked for the 500 word typing task (and the order of the two elicitations is randomized).

The two anchored treatment groups receive an anchoring question prior to the open-ended elicitation for each task. For the adding task, the anchor question is: "Would you be willing to add 175 pairs of numbers for A$?" For the typing task, the anchor question is "Would you be willing to type 500 words for $T?" The anchor amounts ($A and $T) vary depending on the randomly assigned anchor treatment. Table 1 summarizes the two anchor treatments: add low/type high and type low/add high.

Following the value elicitation, the computer randomly determines the task to be offered (a 50/50 chance for each), and the price to be paid for the task. The price is randomly chosen from a uniform distribution over the range $2 to $12. Depending on the payment demanded for each task, the subject either performs the full task for the

[4] Also, Peter Bohm et al. (1997) show that failing to describe the distribution helps alleviate some of the critiques lodged against the BDM procedure and yields bids equivalent to those elicited during a second price auction mechanism.

announced price or does not perform the task and receives no payment for the task. Following the successful completion of the task, the total payment due subjects is calculated and subjects are dismissed to the hallway for a simple game of chance to win an additional $20 (1/16 chance of success).

Discussion of Experiment 1:

Of relevance are two preference rankings: the pre-treatment ranking and the ranking implied through the value elicitation procedure. Prior to the anchoring treatment subjects rank the adding and typing tasks in relation to each other and three other tasks. If preferences are rank stable, the anchoring treatments will have no effect on the subsequent relative ranking of the two tasks as implied by the value elicitation. If in the pre-treatment ranking the subject prefers the typing task, but post-treatment, the subject indicates a willingness to accept less compensation to perform the adding task, then we label this a preference reversal. Similarly, a preference reversal will be observed if the subject prefers the adding task pre-treatment but reports a strictly lower willingness to accept payment for the typing task.

Given the random assignment of subjects to the three treatment groups, and the predetermined anchors for each group, the anchor treatments can be either reinforcing or counter-balancing to the pre-treatment rankings. If the subject has a pretreatment preference for typing and receives the type low/add high anchoring treatment, then the anchor reinforces the pretreatment ranking. If the same subject receives the add low/typ high anchor treatment then the anchors are counter to the pretreatment ranking. If anchoring effects do not exist, we expect the post-treatment preferences for the two tasks to be independent of the anchoring treatment across subjects, and we expect that the anchoring effects will not induce reversals from pre- to post-treatment within subjects.

Experiment 1 Results

We first examine the potential for across subject reversals. If preferences are rank stable across subjects, we expect the proportion of participants preferring each task after the anchoring treatment to be independent of the anchoring treatment. Table 2 presents the number of participants preferring each task post-treatment broken down by treatment. Rows represent the three experimental treatments. Columns represent the post-treatment preferences as revealed through the relative payments demanded for each task[5]. Table entries are the number falling in each preference category over the number offered each treatment.

Figure 1 gives a graphical representation of these results. It is apparent that there are large differences in the relative proportion of those preferring each task by anchoring treatment. Pair-wise tests of differences in proportions support the assertion that anchoring significantly influences the ranking of the adding and typing tasks on average. The proportion of subjects indicating a post-treatment preference for the adding task is significantly higher for subjects receiving the low anchor for addition than for those receiving the low anchor for typing (p=.002). Similarly, the proportion of subjects preferring the typing task, post-treatment, is significantly higher for those that received the low typing anchor than for those receiving the low adding anchor (p=.002). The proportion of indifferent subjects does not vary significantly by treatment. Interestingly,

[5] If the payment demanded for the adding task is strictly less than the payment demanded for the typing task, they are classified as preferring adding, and vice-versa. If the two payments demanded are identical then they are classified as indifferent.

the adding anchor appears to have no statistically significant effect relative to the no anchor treatment.[6]

Reinforcing the findings of Ariely et al., who find that on average a single anchor can affect the location of preferences for a single good (preferences are arbitrary), but not the relative ranking to other goods (i.e., preferences are coherent, even in the presence of multiple anchors), we find that multiple anchors can on average affect the relative ranking of goods. Of potentially more significance would be a demonstration that participants could be induced to reverse rank orderings of two goods internally. To investigate this, we look at the incidence of preference reversals within individuals.

Of the 71 participants in the first experiment, 21 (30 percent) reversed their preferences for the two tasks from pre- to post-treatment. Of those, 19 received a (randomly assigned) anchoring treatment that provided anchors counter to their pre-treatment preferences. Anchoring opposite of the pre-treatment preferences results in a 23 percent (and statistically significant) increase in the probability of a preference reversal relative to the no anchor treatment, while reinforcing anchors do not significantly affect the probability of reversal relative to the no anchor treatment.

Table 3 summarizes Probit results on with-in subject reversals. The dependent variable is a binary indicator for with-in subject preference reversals. Independent variables are indicators for counter-balancing anchors by pre-treatment preference group, and an indicator for a preference reinforcing anchoring treatment[7]. The omitted category is the no anchor treatment. For those with a pre-treatment preference for the typing task,

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[6] The results of all of the pair-wise tests are available from the authors.

[7] We assume that the effect of reinforcing anchors on the probability of preference reversal is the same for those preferring typing and those preferring adding. Because of the small number of subjects preferring adding pre-treatment, it was not possible to separately identify the effects of reinforcing anchors by pre-treatment preference.

counter-balancing anchors induce a statistically significant increase (32 percent) in the probability of preference reversal relative to the no anchor treatment. The effects of reinforcing anchors, or counter-balancing anchors for subjects with a pretreatment preference for the adding task, produced no significant change in the probability of reversal relative to the no anchor treatment. Similar to the preference over gambles literature, it appears possible that an asymmetry exists in the preference reversal over the typing and adding tasks. Those that prefer the adding task pre-treatment appear to have well-defined and invariant preferences and are therefore uninfluenced by anchors opposite their preferences. On the other hand, those that prefer typing pre-treatment seem to have more malleable preferences and are significantly more likely to reverse preferences in the presence of counter-balancing anchors.

<u>A Second Experiment</u>

The asymmetry anomaly is surprising in a static setting where anchoring, a leading explanation for asymmetry in GR, is controlled for within the experimental design. To provide further verification of the asymmetry, we conduct a second experiment with slight modifications from the first. Because the pre-treatment ranking in experiment 1 requires the subject to completely rank five tasks, it is possible that the ranking of the additional three tasks distracts the subject from ranking the two tasks of interest: typing and adding. Further, a possible source of confusion in experiment 1 is the use of the wording 'wage demanded.' It is possible that the responses to the value elicitation questions were based on the expected hourly wage for performing the tasks rather than the piece-meal rate for the two tasks as we anticipated[8]. To circumvent these

[8] Although, when responses to the experiment one are converted to an hourly wage rather than a piece-meal wage, the reversal results are qualitatively identical.

potential problems in experiment 2, we phrase all question in terms of the 'payment demanded,' and we repeatedly remind subjects that we are looking for lump-sum payments and not hourly wages. Further, the pre-treatment ranking from experiment 2 asks subjects to directly rank just the typing and adding tasks in terms of which task would demand a higher payment: "If we were to pay you to either add 175 pairs of numbers, or type 475 words, which task would we have to pay you more to perform?"[9] Finally we introduce reminders to subjects before the pre-treatment ranking, and before the value elicitation questions that their payment demanded responses will indicate to us a preference for one task or the other: "We believe that if you prefer a task, you would be willing to accept a lower payment for that task. That is, we would have to pay you more to perform a less preferred task."

Because we are interested in testing the asymmetry of preference reversals, we modify the anchoring treatments from experiment 1 to be conditional on the pre-treatment ranking. Following the pre-treatment ranking of the two tasks, 70 percent of the sample was assigned to one of two treatment groups while the remaining subjects received no anchoring treatment. Subjects assigned to an anchor treatment receive anchors designed to induce preference reversals, i.e., those who prefer typing (PT) receive a $12 anchor for typing and a $2 anchor for adding while those who prefer adding (PA) receive a $12 anchor for adding and a $2 anchor for typing.

Experiment 2 Results

Fifty-eight subjects participated in experiment 2. Of those, 18 indicated a pre-treatment preference for the adding task (15 were assigned to the anchor group), and 40

[9] We reduced the number of words to be typed from 500 in experiment one to 475 in experiment two based upon average times recorded in experiment one such that both tasks would take about 15 minutes to complete.

indicated a pre-treatment preference for adding (26 assigned to the anchor group). After the anchoring treatment and value elicitations, 15 preference reversals (26 percent of subjects compared to 30 percent in experiment one) were observed. Thirteen reversals were in one of the two anchored groups: 10 from the pre-treatment typing group that was anchored, and 3 from the pre-treatment adding group that was anchored. Of the two unanchored reversals, one preferred typing and one preferred adding pre-treatment.

Tables 4 reports the results of a Probit model from experiment 2 reversals (1=reversal). Independent variables include indicator variables for pre-treatment preference for adding, pre-treatment preference for typing and the same two indicators crossed with anchoring treatment.

The significant coefficient of the anchor treatment in the typing preferred group indicates that those who prefer typing in the pre-treatment can be induced to prefer adding in the value elicitation by anchoring opposite of their initial ordering. The same cannot be said for those that preferred adding in the pre-treatment. The insignificant anchoring effect indicates that those that prefer adding in the pre-treatment are not induced by the counter-balancing anchors to indicate a preference for typing in the post-treatment rankings. Experiment 2 reinforces the findings of experiment 1, that counter-balancing anchors can induce rank reversals for the typing task. However, the result is asymmetric. Those with a pre-treatment preference for the adding task appear to have less manipulable preferences over the two tasks.

Discussion and Conclusion

Complementary to, and perhaps more damning than, the findings of preference reversals in preferences over uncertain gambles, we find the existence of rank reversals in preferences over simple tasks with certain outcomes. Subjects that are anchored counter to their initial preference ordering during a subsequent bidding elicitation show a tendency to reverse those preferences in the direction of the anchors. An asymmetry arises in the static preference reversals, as one self-selected group – those who prefer adding to typing in the joint preference elicitation – are rarely manipulated to reverse their preference via an anchoring mechanism. Several common explanations for such asymmetries in the GR literature exist (anchoring theory, prominence theory, compatibility theory). Subjects facing preference elicitations demarcated in dollars rely upon other dollar demarcated information within the problem, such as the value of payouts in a lottery, but may rely upon other attributes (e.g., probability of winning) when facing other preference elicitation modes (choice). We control for such possibilities in our experiments in a way that no asymmetry should arise, but yet they do.

We hypothesize that instead of the phenomenon arising from heterogeneity of the method in which preferences are elicited, which has been the focus of much of the preference reversal literature, that preference reversals may be a simple issue of sample selection that arises from heterogeneity across individuals. Even when studies feature extensive experimental control, no research design can assign a subject's first rank ordering of preferences. Rather, it is always self-selected, regardless of the mode of preference elicitation. Subsets of subjects immune to preference reversals may simply self-select into the $-bet preference in the GR literature or choose adding in our study.

Alternatively, heterogeneity with regards to intensity of preferences may lead to reversal asymmetries, e.g., those who prefer the $-bet and the adding task may simply have more intense, less malleable preferences for these particular goods and, hence, are less likely to reverse themselves. Our data shows some credence for this explanation. In both experiments, those who rank adding higher than typing pre-treatment provide an average absolute difference in willingness to accept of approximately $4.00 more than those who rank typing above adding, after controlling for anchoring treatments. Among the non-anchored subjects, the average difference between the payment demanded for adding and the payment demanded for typing is $3.91 higher for those preferring adding pre-treatment in experiment 1 and $4.09 for those preferring adding pre-treatment in experiment 2. Both differences are significant at the 90% level indicating mild support for the hypothesis that in the absence of anchoring, those preferring the adding task have more intense preferences for their preferred task than those preferring the typing task.

It is clear, however, that much work remains before we understand the causes and implications of rank instability of preferences. Our ability to use simple anchoring mechanisms to induce within subject preference reversals in an economically meaningful, static, simple choice setting further escalates the need for economists to reconsider the wisdom of treating preferences as stable in either a cardinal or an ordinal sense, even in choice settings that are not encumbered with the intricacies of probabilistic outcomes, time discounting or issues of interpersonal utility.

We restate with amplification many of the concerns raised by others who question the dogma of stable, coherent of preferences (Grether and Plott, Amos Tversky and Richard Thaler, Seidel). How does the core of microeconomic theory operate if preferences are, minimally, highly malleable and, maximally, constructed on the fly as people are bombarded with key economic and institutional stimuli inherent in the decisions that interest economists? A partial list of disturbing questions includes: Which set of preferences is correct – those elicited via direct choice among alternatives or those inferred from transacted prices? Is a market mechanism that expends \$X to match a marginal buyer to a marginal seller more or less efficient than one that expends \$Y < \$X to shape the preferences of a proximate buyer and seller such that they are both at the margin? Which preference elicitation mechanism should be used when gathering information that will shape public policy? Do we adopt a policy if those who gain under that policy can use their windfall to shape the preferences of those who lose such that the policy passes the potential compensation principle?

Further experimental work to investigate the robustness of our results is warranted. For example, unlike the preference reversal over gambles literature (e.g., James C. Cox and David M. Grether, 1996), our experimental work has not introduced repetition of the elicitation mechanisms into the design to see if increased familiarity with the choice and bidding procedures limits either the extent or asymmetry of reversals. We leave this and other design augmentations to future work and would not be surprised if the asymmetry we uncover in static preference reversals follows the course of the asymmetries uncovered in preference reversals in gambles: they remain robust in many settings but, with enough market feedback and experience, they shrink to the boundary of significance.

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Table 1: Anchor Treatments for Experiment 1

		Anchor	
		$A	$T
Treatment	Add low/type high	$2	$12
	Type low/add high	$12	$2

Table 2: Post-treatment Preferences by Treatment

		Post-Treatment Preference		
		Prefer Adding	Indifferent	Prefer Typing
Anchoring Treatment	Low Add ($2)/High Type ($12)	5/28	8/28	15/28
	No Anchor	4/18	6/18	8/18
	High Add ($12)/ Low Type ($2)	15/25	7/25	3/25

Table 3: Probit Results on With-in Subject Preference Reversals, Experiment 1: Dependent variable=1 if preferences reverse, 0 otherwise

	Coefficient (p-value)
Constant	-0.765 (0.020)
Adding Preferred Pre-treatment/ Counterbalancing Anchors	-0.303 (0.652)
Typing Preferred Pre-treatment/ Counterbalancing Anchors	0.861 (0.036)
Reinforcing anchors	-0.517 (0.306)

Log-likelihood=-36.85, Chi-Squared=12.52, Observations=71

Table 4: Probit Results on With-in Subject Preference Reversals, Experiment 2: Dependent variable =1 if preferences reverse, 0 otherwise

	Coefficient (p-value)
Adding Preferred Pre-treatment[a]	-0.431 (0.565)
Typing Preferred Pre-treatment[a]	-1.465 (0.004)
Adding Preferred Pre-Treatment and Counter-Balanced Anchors	-0.411 (0.622)
Typing Preferred Pre-Treatment and Counter-Balanced Anchors	1.172 (0.037)

Log-likelihood=-30.34, Chi-squared=5.63, Observations=58

a. With no constant, default categories are pre-treatment preference for each task with no anchors. No reinforced anchors are offered.

Figure 1: Between Subject Comparison: Experiment 1
Post Treatment Preferences for Tasks (by Treatment)