Default Options in Economic Games:
Experiments Demonstrating a 'Default Pull' Effect and Arbitrarily Constructed Preferences

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Introduction

By now, a large literature has used simple economic games, like the standard one-shot dictator game (see Forsythe et al., 1994), to analyze so-called social preferences. In this game, a "dictator" chooses how to divide an experimental endowment of money between herself and an anonymous, passive "receiver". If players are selfish, then rational choice predicts they should keep the entire endowment for themselves. Numerous experiments, however, have shown that dictators give away on average more than 20% (Camerer, 2003), even when efforts are made to ensure recipient anonymity (thereby eliminating any concern for reputation, future interaction, or punishment). Various attempts have been made to reconcile this behavior with rational choice by assuming people have social preferences that in some way incorporate concern for others' payoffs (see, e.g., Fehr and Schmidt, 1999).

Because these games use real-money endowments, typically without time pressures or complicated contexts, it would seem that a player's actions reflect a clear and reasoned preference. In the present paper, we challenge this notion by presenting evidence of a strong "default pull" effect: Choices are affected by the presence of a subtle default option, even when the default itself is not chosen. We demonstrate this default pull effect in a standard dictator game with seemingly innocuous default options that appear to be a feature of the software presenting the game; i.e., the first in an apparently randomized list of allocation options is checked off by default. Such defaults obviously do not convey normative information about what to choose and, in fact, were often unnoticed by our participants. Although virtually every participant in our experiment denied that the default could have affected their choices, they were more inclined to give away the endowment (on average over 20%) when presented with a generous default option than when presented with a

selfish one. Unlike many previous findings showing that people stick with the default (see Thaler and Sunstein, 2008), the default pull effect sometimes involves participants staying with the default (e.g. when the default is sharing the endowment equally or keeping everything) and sometimes involves moving substantially away from the default (e.g. when the default involves giving everything away). Our findings add to our understanding of default biases by showing that even in a simple environment with clear incentives, people can be unconsciously affected by the presence of a default option.

Before moving to a description of the experiment, we briefly review findings from the literature on default biases, dictator games, and anchoring effects, and then differentiate our default pull from them.

Default Biases

Johnson et al. (1993) examines recent changes made in the insurance laws of Pennsylvania and New Jersey and illustrates how simple framing manipulations (changing default settings) can lead consumers to make inconsistent choices when evaluating the premiums and benefits associated with insurance purchases. Both states introduced a new insurance plan granting the option of lower insurance rates with limited suing rights (tort), but New Jersey set this new plan as the default while Pennsylvania featured the alternative full tort option (higher rates and full suing rights) as the default. 75% of Pennsylvanians retained the full right to sue, while only 20% of New Jersey purchasers actively chose to acquire the same rights. With limited tort as the default, Pennsylvanians could have paid over $200 million dollars less for auto insurance. In some cases, it is likely that defaults carry normative value; purchasing insurance can be a relatively complex task and people may often assume that the default is best for the standard, average consumer. Similarly, Thaler and Sunstein (2008) discuss the ramifications of default options in the context of the Medicare prescription drug program. Participants eligible for both, but who failed to sign up for a plan, were assigned to default plans set by the government. While they could freely

switch to a plan that would better suit their medical needs, many failed to do so.

Korobkin (1998) demonstrates a similar default effect with legal contract default rules. Law student participants provided advice to clients in hypothetical contract negotiation scenarios with the content of the legal default terms manipulated between experimental groups. The results suggest that the choice of legal default terms affects not only the final terms contracting parties will agree upon, but also their actual preferences for these terms. The study shows the effects of status quo and default biases, suggesting that if parties perceive legal default terms as part of the status quo, then their preferences will shift in favor of those terms more than they would if other terms were the legal defaults. Default options have the power not only to influence an agent's actions, but also an agent's underlying preferences.

Abadie and Gay (2004) examine default options within the opt-in/opt-out structure of organ donations. The study compares opt-in systems of informed consent, in which one must demonstrate explicit consent to being a donor, with opt-out systems of presumed consent, in which a person is classified as a potential donor when there is no explicit opposition. The survey results show that people retained the provided default option, but that switching the default option from explicit to presumed consent would substantially increase donation rates. Clearly, default options wield powerful influence over decisions of considerable significance.

Dictator Games

Dictator games are notoriously sensitive to descriptions and context, and studies often present the game in a context-laden environment. For example, when simply expanding the dictator's choice set to allow taking from the receiver's initial endowment, players rarely choose to transfer money and most choose to take from the receiver (Bardsley et al., 2005; List, 2007). An immediate concern is that such studies are susceptible to endowment effects, for the language used implies the assignment of a property right to one of the players (e.g. "you have 10 dollars," or "player Y has 10 dollars"). Further, the experimental

design of such games often implies some normatively appropriate behavior that can have a significant influence on dictators' decisions. Krupka and Weber (2008) showed that dictators behave more equitably when the endowment is initially split equally between the two players ("bully" game) than when the entire endowment is given initially to the dictator ("standard" game). The results show that dictators believe it is more socially acceptable to keep the majority of the initial endowment in the standard game, but it is not acceptable when framed in terms of taking from the receiver's initial endowment, such as in the bully game. To address these concerns, we went through meticulous efforts to construct a purely abstract environment by allowing dictators to merely select the payoff double of their choosing, thereby eradicating the normative implications associated with initial endowments.

The positive amount consistently given away in dictator games makes it difficult to describe dictator preferences in terms of only outcomes. Some studies attempt to rationalize such systematic behavioral inconsistencies by attributing them to the fact that motivation for giving may not be adequately captured by monetary payoffs alone. Hayashi (2008) finds evidence that people who are not willing to create unequal distributions of wealth in their favor are nonetheless willing to permit such randomly generated inequality to persist. He finds that participants appear to exploit omission bias in a self-serving manner, failing to act only when omission is in their self-interest. Dana et al. (2006) finds that people have an "illusory preference" for fairness: They dislike appearing unfair to themselves or others when the causal link between their action and outcome is transparent; however, when transparency is relaxed they tend to act more selfishly, exploiting the uncertainty, or "moral wiggle room" between the distributional fairness of their action and its outcome. Another reason is that people may be compelled to give when in fact they truly prefer a purely self-interested outcome; and, vice-versa, they may be compelled to take more than they would normally if presented with the costless opportunity. In Dana (2006), people are responding to some consistent desire (don't appear unfair) and thus, the behavior is able

to be rationalized. Our study involves an exploration of a broader hypothesis than many of the aforementioned studies: We think that people are affected by normatively irrelevant contextual factors, regardless of whether such factors are part of the individual's payoff function.

Default Pull Effect

In this section, we aim to disentangle the default pull effect from traditional default, status quo, and anchoring-adjustment effects. Thaler and Sunstein (2008) emphasize that people tend to stick with the default, suggesting that default effects are defined discretely (e.g. opt-in or opt-out) rather than continuously. However, our results showed that participants sometimes retained the default and sometimes moved substantially away from it. Although almost every participant denied the possibility of being affected by the default when surveyed, the data strongly suggest otherwise.

It is important to note that the default pull is not anchoring in the classical sense. Tversky and Kahneman (1974) describe anchoring as a process by which "people make estimates by starting from an initial value that is adjusted to yield the final answer" (p. 1178)., and anchors are typically used when people are uncertain about an objectively correct answer to a question. The dictator game, however, does not involve any decision under uncertainty because there is no "right" answer; rather, it is a matter of choice involving personal preference, and preferences are subjective and not typically thought of as being anchored (Simmons et al., 2009; see discussion on self-generated anchors). A dictator game outcome should be solely a function of individual subjective preference; anchors should be irrelevant. Many anchoring studies present a clearly arbitrary and uninformative anchor, such as the number generated by a wheel of fortune (Tversky and Kahneman, 1974) or the first three digits of the participant's social security number (Ariely et al., 2003). Our experiment, however, features relevant default options that, unlike anchors, can be retained.

In general, default options often carry some normative value. They are typically

suggestions that are meant to actively influence choice with the individual's best interest in mind (e.g. default funds, default insurance programs; see Thaler and Sunstein, 2008). Some anchors, particularly when relevant to the estimated value, may appear to carry normative value. Our default settings, on the other hand, carry no normative value. We avoided this in a few ways: First, we did not explicitly inform participants of the default options, nor were they actively endorsed or suggested in some way. Second, each default was preselected as a filled-in bubble on a simple computer interface, thereby appearing to be a feature of the game software. Third, the scrambling of the payoff doubles suggested that the top preselected option was randomly generated.

Experiment

Participants

Participants (n = 50) were all students at the University of Pennsylvania, all of who participated voluntarily in response to an online advertisement for paid decision experiments. All participants gave informed consent prior to the experiment. Four sessions were run, the first with 12 participants, the second with 18, the third with 8, and the fourth with 12 participants.

Proceedures

We presented each participant with a series of four dictator-type games, three of which had different "default" conditions, or preselected options of wealth allocation between themselves and another anonymous player. Default options were designated as such by being the first and only "preselected" allocation on the list of thirteen wealth allocations. The fourth "no-default" condition simply had no preselected payoff double. We arrayed the payoff doubles along a straightforward, minimalist interface, with the checked off default allocation appearing at the top of the list. By doing this, we avoided the inevitable normative implications of context-laden settings. We ran four sessions with 50 participants in total. In every session, each participant received the four-game series in a different order

(4! gives us 24 possible combinations).

Participants were situated at computer terminals for the entirety of the experiment and were given visual as well as oral instructions. No interaction other than via computer took place. First, participants were randomly assigned identification numbers that designated their role; odd-numbered participants were determined to be dictators (Player X), and even-numbered were receivers (Player Y), however specific role assignments were withheld until after the experiment. We used the strategy method to elicit choices; that is, participants made all decisions under the assumption that they were a dictator (Player X) and after all decisions were made, specific role assignments were revealed. Participants were then instructed that they would play a simple economic game on decision-making with another anonymous and random person in the room and that their payment for participation in the experiment would be determined in part by their and/or others' decisions. Regardless of role assignment, players were informed that each would receive an equal base pay.

Each participant completed a series of four consecutive dictator games. The series was scrambled so that each player received a different ordering of the games. We ran all 4! = 24 possible combinations of these four rounds. In each round, thirteen wealth allocations were presented as payoff doubles in a multiple choice format arrayed along the computer interface, and participants were instructed to ensure the allocation of their choosing was selected before clicking "submit" (see Fig. 1 below). The very first allocation at the top of the list was the default setting in three of the four conditions and was designated as such simply by being preselected, or bubbled-in, on the screen (e.g. 10,0 in Fig. 1). The other twelve allocations were all randomized. Ten of these twelve allocations consisted of all the other possible whole-number allocations adding up to ten, and the remaining two allocations were "distractors," which did not add up to ten. The "distractors" are Pareto-dominated, and thus, should not be chosen, but we included them in an effort to promote independence (along with the scrambled orderings) so that each player would treat each round as a new choice. In the fourth "no-default" condition, there was no pre-selected

option on the screen, so participants simply chose from among the thirteen allocations.

Figure 1: Interface

After participants completed all four rounds, a screen appeared with their calculated monetary compensation (at an exchange rate of $0.25 per point). We revealed how dictator and receiver roles were determined, after which all participants filled out a brief survey with questions relevant to their performance during the experiment.

Results

Dictator Choices

Our results indicated that participants were affected by the default wealth allocation presented in the first game, and that initial effect persisted throughout all four rounds. Statistical analyses compared the mean allocation (and average payout) to the receiver (Y) across all four rounds in each condition. Condition A denotes the group of participants

whose first game had a (10,0) default, Condition B saw a (5,5) default, Condition C saw a (0,10) default, and Condition D saw no-default. The mean allocation to Player Y in Condition A was 1.44, in Condition B was 3.08, in Condition C was 3.58, and in Condition D was 1.56.

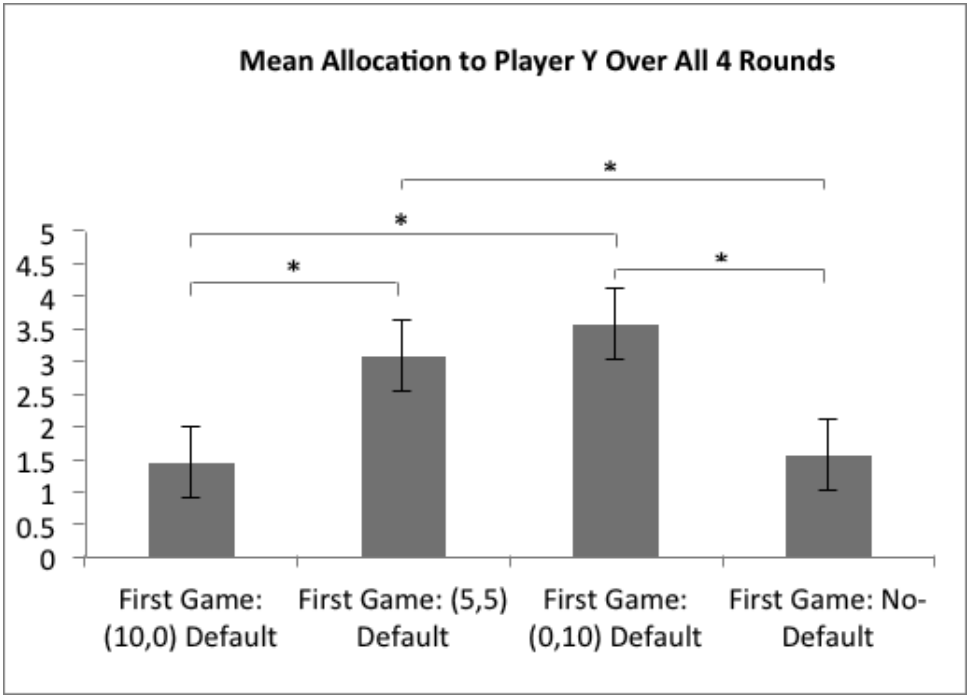

Figure 2

Figure 2 shows the mean allocation of dollars given to the receiver over all four rounds, according to the first-round default allocation. We found differences in the mean allocations between Condition A and Condition B that were significant ($t=3.487$, $df=98$, $p<.001$) and differences between Condition A and Condition C that were significant ($t=4.512$, $df=102$, $p<.0001$). Differences in the mean allocations between Condition B and Condition D were also significant ($t=3.200$, $df=98$, $p=.0019$), and differences between Condition C and Condition D were significant ($t=4.218$, $df=98$, $p<.0001$). However, differences in the mean allocations between Condition A and Condition D, and differences between Condition B and Condition C were not significant ($t=.283$, $df=98$, $p=.777$) and ($t=.950$,

df=98, p=.344), respectively. Furthermore, an ANOVA test returned p<.001, indicating that the means of all four conditions were not equal. Since there are statistically significant differences in the mean allocations to the receiver across all four rounds, and not in just the first round, we interpret this as evidence that the default allocation that participants saw in the first game affected their responses across all four rounds of the game. In addition, we observed that participants allocated fairly consistently over all four rounds, which can be seen visually in Figure 3.1.

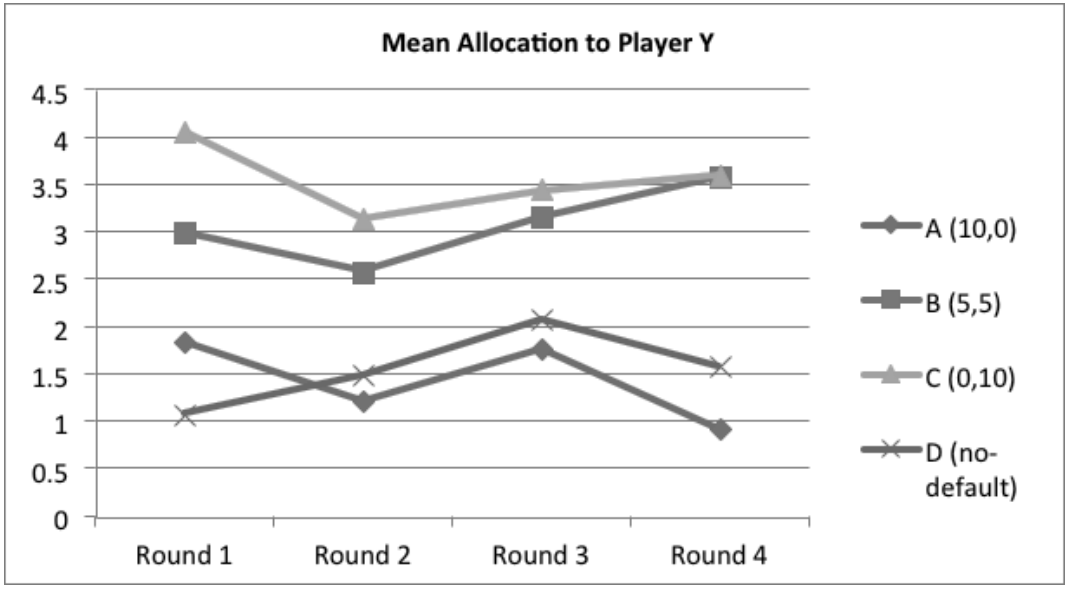

Figure 3.1

Fluctuations in the flatness of each condition across all four rounds can essentially be attributed to random variation, as approximately one participant in each condition distinctly skews the data by varying his/her responses significantly more than the others. Figure 3.2 shows much smoother curves by removing just five data points: two from round 1 and 2 for the (0,10) default condition, and three from round 2 and 3 for the (10, 0), (5,5), and no-default conditions.

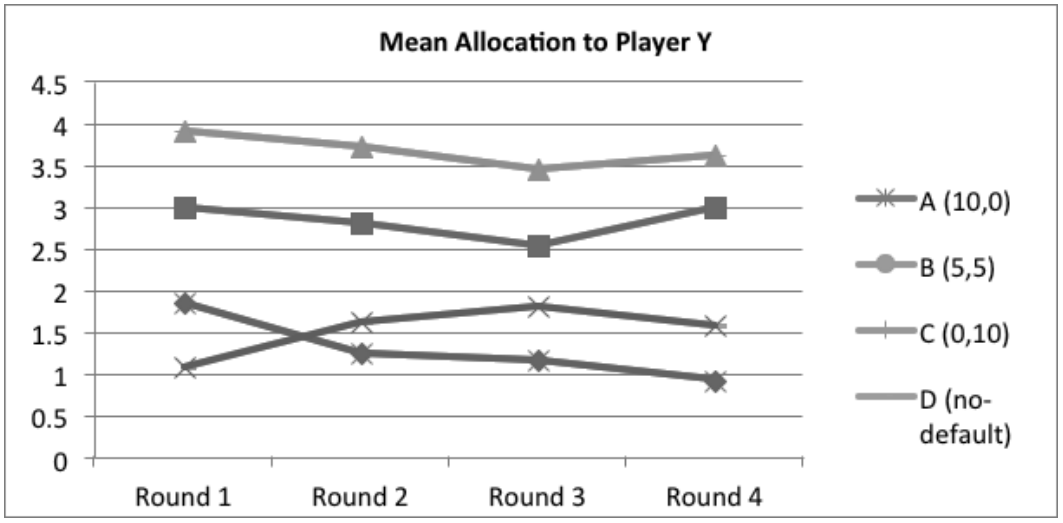

Figure 3.2

Strong positive correlations between round 1 and round 2 (r =.76), round 1 and round 3 (r =.47), and round 1 and round 4 (r =.60) further highlight the fact that there is a strong effect of the participants' round 1 decision on subsequent rounds. Furthermore, from Figures 2 and 3.1-3.2 we gather that Conditions A and D have similar mean allocations across all four rounds, and Conditions B and C have similar mean allocations, with participants in Conditions B and C allocating nearly $2 more on average than those in Conditions A and D. Our results further demonstrate that participants in these conditions were not simply sticking with the default allocation. Rather, they actively shifted from the default, but still remained within close proximity in terms of wealth distribution. This is where our study parses default bias from our observed "default-pull" effect. Figures 4.1-4.4 help demonstrate this observation:

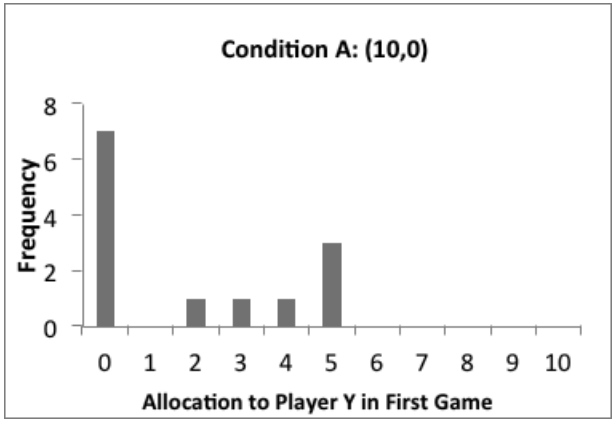

Figure 4.1

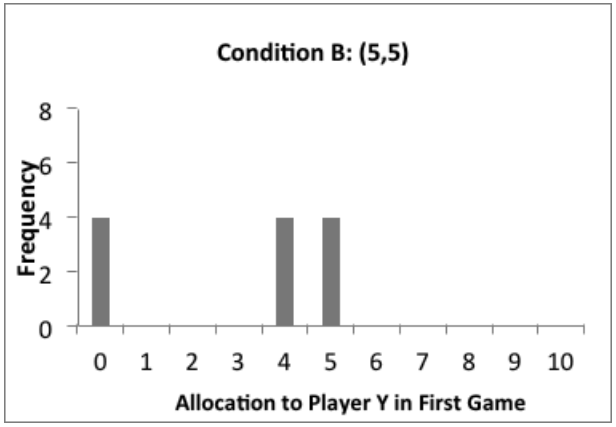

Figure 4.2

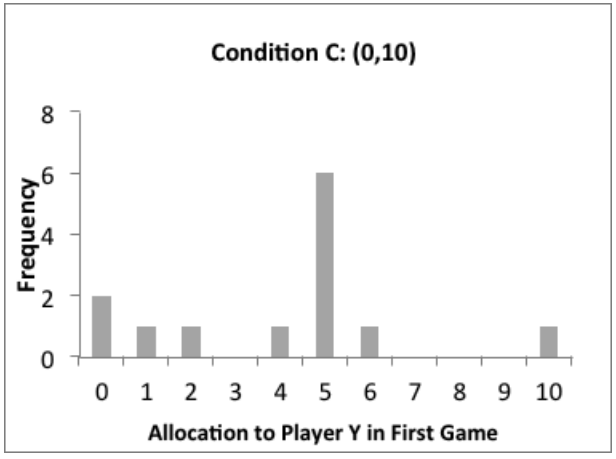

Figure 4.3

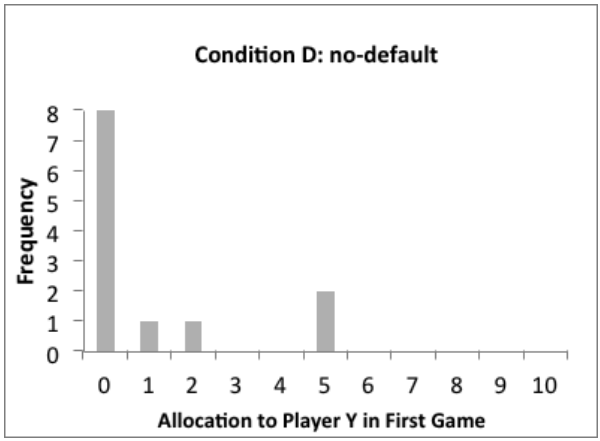

Figure 4.5

As can be seen from the above graphs, in Conditions A and D the modal response was (10,0), whereas in Conditions B and C the modal response was (5,5). In Condition B, just as many participants chose (6,4) as they did (5,5), while in no other condition did more than one participant choose (6,4) in the first game. This shows that in Condition B, those who actively strayed from (5,5) nonetheless often remained very close to the default. In Condition C, almost half the participants chose (5,5) in the first game, demonstrating that while they would likely not stick with the "hyper-fair" (0,10) default (only one participant did), they still were much more fair than in Conditions A and D.

Survey Responses

General responses from the survey are shown in Figures 5.1 and 5.2. The most intriguing effect we found from the surveys was that only approximately 5% of participants believe the default allocation affected their choices in that particular game, even while the data indicate that the first default affected not only the first round game, but also the following rounds. This result of two people who believe the default affected that round can essentially be attributed to chance (note: the responses from one participant who answered "no" for the first question of "did you notice the default settings?" and "yes" to "did the default affect your decision in that particular game?" were discarded from the survey data

analysis because those answers are inconsistent). This means that participants were unwilling to admit that the default affected them and/or that the default had a subconscious influence on their decisions. However, participants were willing to acknowledge their degree of selfishness. When asked how generous they felt they were in their allocations on a five-point scale, only 11 percent of participants said "generous" or "very generous" (see figure 5.2). This is somewhat inconsistent with the data given that this group was fairly generous in comparison to other dictator studies, with a total mean allocation of $2.42 to Player Y across all conditions. Thus, people believe that they are, for the most part, selfish in their decisions, but are unwilling to acknowledge that their degree of selfishness might have been influenced by the default options.

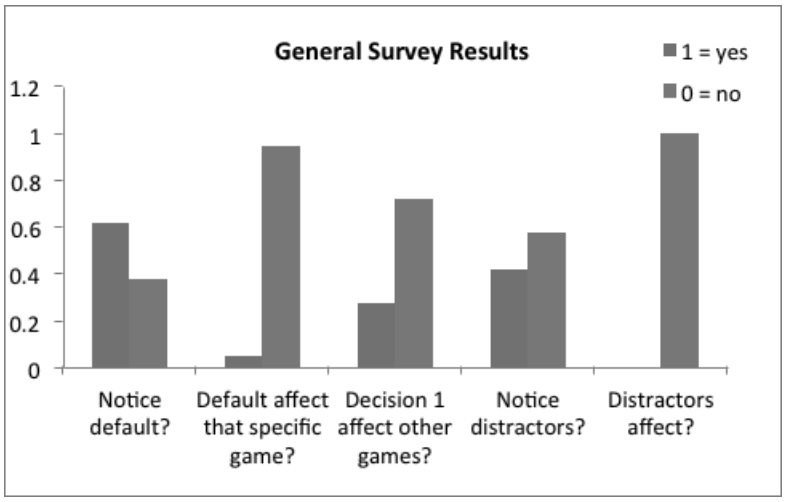

Figure 5.1

Discussion

Like many studies in the field of behavioral economics, we find results inconsistent with models that assume people's preferences are defined solely over payoffs, outcomes, or consequences. But our findings go further. According to a purely consequentialist model, default rules should be theoretically extraneous features on choice; however, we find that presenting participants with default wealth allocations significantly affects decision-making

in dictator games. Our findings show participants giving over $3 on average to a passive and anonymous receiver in two of the default conditions. This begs the question: Can we interpret a significantly positive mean allocation—as much as $3.58 in the (0,10) default condition—as a simple preference for a fair or efficient outcome? Our findings suggest otherwise

Studies with similar results inconsistent with preferences attempt to rationalize this irrational behavior, attributing such inconsistencies to self-serving biases, such as self-serving omission bias. Hayashi finds that people who do not actively choose unequal wealth distributions are nonetheless willing to let randomly generated inequality persist when it is self-serving. However, our results suggest that such inconsistencies are not able to be rationalized and are thus not necessarily attributable to self-serving biases, for we see effects when the default option represents both equal and "hyper-fair" allocations, such as (5,5) and (0,10). We find the greatest effects with the (0,10) and (5,5) default conditions, and these effects are in the other direction, with people giving significantly more on average. The self-serving bias theory would predict our default setting of (10,0), which is essentially a randomly generated purely self-interested and self-serving option, to result in more selfish behavior; it would also suggest that an initial "hyper-fair" allocation will essentially be disregarded and treated as a standard game, but we observe something different. Moreover, we find no significant differences between the (10,0) default condition and the no-default condition; participants essentially treated each of these games the same.

While we do not see the purely self-interested default of (10,0) affecting outcomes, we do see the default (5,5) and (0,10)—the more generous default allocations—affecting outcomes, but not in a manner that is consistent with the traditional views of default or status quo bias. Our results show that participants do not simply stick with the default or "do nothing" (i.e. leave initial wealth allocations as they were), but rather chose allocations with distributions in the general direction of the initial default allocation. Thus, it appears that the default has a somewhat mysterious and anomalous effect different from a simple

bias toward the present state; similar to a subtle anchoring effect, we call this the "default-pull" effect. Rather than defining default bias by the traditional bright-line test of either doing nothing or doing something, our "default-pull" concept defines the present state along a continuous variable, a continuum that shows how people can be biased toward a similar (but different) state as the default.

One way to view our overall results is that although people may not necessarily consciously care about the causal path that leads to an outcome, causal paths nonetheless affect their choices. As illustrated by our survey responses, of the people who noticed the default setting, about 95% said that it did not affect their choices. Most people did not admit to the possibility of the default setting having any influence on their decision. One participant said, "I noticed that there was a pre-selected option. This did not affect my decision because originally I was going to allocate some points to the other player already." It is unclear whether this is a statement of true preferences or simply an ex-post rationalization of the "default-pull" effect. Another participant said, ""Yes, in the first round of the game there was a default setting…it did have some leverage on my decision making, but not significantly." Our data tells a different story, however, showing a significant inconsistency between the participants' subjective responses (which could very well be cheap talk) and their actual actions. This discrepancy strikingly reveals the behavioral component of our study. One participant who was presented with the default condition of (0,10) in round 1 tellingly wrote, "Yes, [I did notice the default] and was trying to make choices so that the probability was close to 50-50," which is an interesting answer that is consistent with our aggregate data for the (0,10) condition.

While our findings are robust, we hope that they serve to spur more theoretical and empirical research on the dynamics of human decision-making. For instance, a follow-up experiment could be conducted to see if participants, knowing that they are a Player Y, would pay a premium to have the Player X with whom they are matched see a default allocation of (5,5) or (0,10) in the first round. The participant would be shown the data from our

experiment, which indicates a Player Y should pay a premium of up to $2 to have Player X placed in (5,5) or (0,10) default conditions. Thus, there are many more questions to be explored, and some future research should be aimed at discovering the causes and origins of such a "default-pull" effect. And, of course, while disentangling behavioral anomalies is a pursuit in and of itself, the ultimate goal of our research is to enhance the predictive power of economic models.

Some critics argue that selfish preferences do not necessarily mean maximizing monetary payoffs, for some may derive utility from other-regarding behavior. However, this assertion assumes that people will show consistent preferences for some particular outcome, which is not always the case. Ariely et al. (2003) find that people's initial preferences are not well-defined, even arbitrary, but become articulated in the process of making a decision (Ariely et al., 2003). Participants' absolute valuations of goods are arbitrary, but relative valuations of different amounts of the good appear orderly, as if supported by demand curves derived from fundamental preferences. This phenomenon they coin "coherent arbitrariness" is consistent with the large body of literature on revealed and constructed preferences, which says that initial preferences or valuations are malleable but then become precisely defined relative to the initial arbitrary point.

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