The Relative Income Effect: An Experiment[1]

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Abstract

John Stuart Mill claimed that "men do not desire merely to be rich, but richer than other men." Do people desire to be richer than others? Or do people desire favorable comparisons to others more generally, and being richer is merely a proxy? We conduct an online experiment in which we measure subjective well-being before and after an exogenous shock that reveals to subjects how many experimental points they and another subject receive, and whether points are monetized. We find that subjects like receiving monetized significantly more than non-monetized points but dislike being "poorer" than others in monetized and non-monetized points equally, suggesting relative money is valued only for the relative points it represents. We find no evidence that subjects like being "richer" than others. Women have a strong(er) distaste for being "richer" and "poorer" (than do men); conservatives have a strong(er) distaste for being "poorer" (than do progressives).

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1. Introduction

John Stuart Mill is credited with the claim that "men do not desire merely to be rich, but richer than other men."[2] This claim raises two fundamental questions. Do people actually desire to be richer than others? And if so, why? Empirical evidence regarding the first question is mixed. While subjective well-being (SWB) scholars have assembled substantial empirical evidence from large observational datasets of a negative relationship between others' income and one's own SWB, identification is often confounded. For example, some studies have found that cost-of-living explains the negative relationship, while others have estimated a positive relationship. A simple answer to the second question is that people care about relative consumption--that is, they want to consume more than others. Another plausible explanation that has not been explored in the SWB literature, though, is that people want to compare favorably to others in a more general sense, and being richer than others proxies for this ineffable relativity. In this case, being richer may only matter in the absence of other measures of relativity.

In this paper, we attempt to address these two questions using an experiment. Specifically, we measure subjects' SWB before and after an exogenous relative-rewards shock in which subjects learn the number of experimental points they and another subject receive (2 or 10), and whether each experimental point is monetized (worth $1) or non-monetized (worth $0). This design enables us to compare the SWB-change of two subjects receiving the same experimental points who differ only in the points they learn another subject receives. Comparison of the impact of monetized and non-monetized points allows us to identify the impact of others' money on one's SWB and to determine whether non-monetized points generate the same observed relativity. To illustrate the nuance of this approach, consider a less nuanced version wherein a subject learns that she receives $2 and another subject receives $10. SWB may change because the subject learns that she is receiving $8 less than the other subject, but it may also be affected by learning that: (i) she is receiving $2; (ii) the other subject is receiving $10; (iii) there is a metric on which she is in worse relative standing than the other subject by 8 units; or (iv) she is receiving $4 less than she expected when agreeing to participate in the experiment. Our design attempts to rule out explanations (i) - (iv).

First, we find that it is not SWB-improving to learn that the other subject receives a smaller--in comparison to an equal--number of monetized points, suggesting that subjects do not prefer to be "richer" than other subjects *ceteris paribus*. Second, we find that while it is SWB-diminishing to learn that the other subject receives a larger--in comparison to an equal--number of monetized points, it is statistically indistinguishably SWB-diminishing when points are non-monetized. This suggests that others' money impacts own SWB only insofar as it proxies for others' points; that is, subjects only care about being "poorer" because it means they receive fewer points than others, not because they receive less money than others. In contrast, subjects do seem to value receiving money themselves, and not just for the points that money represents: while it is SWB-improving to receive non-monetized points, it is significantly more SWB-improving to receive monetized points.

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[2] Pigou (1920) attributes this quotation to Mill (Luttmer, 2005), but its authorship is contested by Mill scholars (see Rees (1956)).

2. Literature Review

SWB scholars have found evidence of a negative relationship between others' income and one's own SWB that is attributed to a "relative income effect" (RIE): income comparisons cause SWB to decrease with others' income, *ceteris paribus*. The RIE has important implications, for example, regarding the potential benefits of economic growth. Some SWB researchers believe that the RIE helps explain the Easterlin Paradox: the empirical observation that over time in many countries, average national SWB does not increase with real per capita GDP (Easterlin, 1974; 2010; Easterlin et al., 2013). Luttmer (2005) reports a negative relationship between regional median income and SWB that is at least as big as the positive relationship between own income and SWB, implying that shared economic growth would not be associated with improved SWB.

Identification of the RIE, though, is often confounded. First, observational studies do not generally account for selection (e.g., into neighborhoods or occupations). Indeed, the studies with the most credible exogenous relative-income shocks--the Moving to Opportunities for Fair Housing demonstration and the Dutch Postcode Lottery--do not find a negative effect of neighbors' income on one's SWB (Ludwig et al., 2012; Kuhn et al., 2011). Second, *ceteris paribus* is easily violated in observational studies. With additional controls, Ifcher et al. (2018) find that the negative relationship between others' income and own SWB can be explained by cost-of-living. Further, the sign of the others-income-own-SWB relationship is positive in some contexts. A positive relationship has been identified in immediate neighborhoods and has been attributed to local public goods (Ifcher et al., 2018; Brodeur & Fleche, 2015; Deaton & Stone, 2013; Ludwig et al., 2012; Clark et al., 2009) or to altruism (Kingdon & Knight, 2007). A positive relationship has also been identified in periods of rapid economic growth and has been attributed to "the tunnel effect," whereby others' income serves as a signal of one's future income (Hirschman & Rothschild, 1973; Senik, 2008; 2004).

These violations of *ceteris paribus* illustrate that others' income can impact SWB through channels other than the RIE. They do not, however, prove or disprove the existence of the RIE itself, as the sign of the relationship between others' income and own SWB represents a net effect of an indeterminate set of channels. For example, Clark et al. (2009) find that, controlling for own income, neighborhood median income and own SWB are positively correlated, which the authors attribute to local public goods. At the same time, controlling for own income and neighborhood median income, they find that one's income-rank within one's neighborhood is positively correlated with own SWB, which they attribute to the RIE.

These confounds render compelling the control afforded by an experimental analysis. To our knowledge, the only such experiment with real-money rewards is McBride (2010), who attempts to identify RIE in a single-player, multiple-round, penny-matching game played against a computer. In each round, subjects are informed of the computer's randomized probability of choosing heads or tails (there are five possible types) and then choose heads or tails. Subjects learn their own payment in that round and, depending on treatment, either: (i) no further

information, (ii) the average payment of all other subjects, or (iii) the average payment of subjects by probability-type. Subjects then report their satisfaction with the results of that round. Satisfaction with a round's results significantly decreases with the average payment of other subjects in (ii) and only with the average payment of subjects with the same probability-type in (iii). While these results appear consistent with the RIE, there are features of the experiment that confound identification. The measure of SWB is not general but is specific to satisfaction with a particular round's results. This focuses attention inorganically and may result in obscuring the effects on general SWB of factors other than the results, like procedural considerations. Also, because subjects have agency, their satisfaction (dissatisfaction) may not be due to income per se but to feelings of relative success or self-congratulations (failure or self-criticism).[3]

3. Experimental Design

3.1. Discovery and pilot studies

Like the main online experiment, the goal of the discovery study was to identify the SWB-impact of an exogenous relative-rewards shock. A more general ambition was to determine whether the control afforded by laboratory experimentation could be leveraged to study SWB (as measured in large observational datasets and increasingly in national accounts) and its determinants, especially income. The discovery study was necessary to determine SWB's sensitivity to money-rewards of the size paid in experiments. This required testing the many different measures of SWB available in the literature, and varying payment-sizes. As such the discovery study, while similar in methodology to the online experiment, includes much longer SWB surveys and more rewards-shocks. Details and discussion of the discovery study are presented in Appendix B.

While the discovery study enabled us to refine the SWB survey and revelation mechanism, the pilot study enabled us to test the revised methodology and help estimate the number of subjects that were needed for the online experiment. Appendix B also includes details and discussion of the pilot study.[4]

3.2. Online experiment

To identify the RIE, we conducted an online experiment in December 2017. Prospective subjects were recruited on Amazon Mechanical Turk (mTurk). They were informed that participation would take less than 20 minutes, that they would be paid for their participation

[3] It warrants mention that other experiments have found evidence supportive of the RIE using choices over hypothetical scenarios (Johansson-Stenman et al., 2002; Solnick & Hemenway, 1998; Zeckhauser, 1991). Also supportive of the RIE, Smith et al. (1989) document higher SWB with a given hypothetical wage when it is from a distribution with a lower mean. Further, there are three economic experiments in which relative real-money payments vary and SWB is measured. The correlation between others' payments and subjects' SWB is consistent with the RIE in Bosman & van Winden (2002) and Konow & Earley (2008) and inconsistent with the RIE in Charness & Grosskopf (2001). That said, these experiments are not designed to test the RIE so attempts to attribute the correlations to the RIE are beset by endogeneity and other identification issues.

[4] Columns (1) and (2) of Appendix Table 1 present the results of pooling the pilot study and online experiments, using the methods presented in Section 4.2 below; results are materially unchanged.

(minimum, average, and maximum payments of $3.00, $7.00, and $11.00), and that the payment-amount would be determined randomly. Each recruited subject received a link to Qualtrics, where she received an exogenous relative-rewards shock (revelation) and completed pre- and post-revelation SWB surveys, allowing for measurement of the SWB-effect of the revelation. Subjects entered a survey-completion code generated by Qualtrics into mTurk to receive their payments. 996 subjects completed the experiment in 10 minutes on average and were paid a minimum, average, and maximum of $3.00, $7.27, and $11.00.[5] Screenshots are included in Appendix A.

3.2.1. Pre- and post-revelation SWB surveys

The pre- and post-revelation SWB surveys each included the Mood Short Form (MSF), which enables a quick measurement of subjects' experiential SWB (Peterson & Sauber, 1983).[6] The MSF includes the following four items, with a five-point Likert response-scale for each item ("Strongly disagree" =1, "Disagree" =2, "Neither agree nor disagree" =3, "Agree" =4, and "Strongly agree" =5):

- "Currently I am in a good mood."
- "As I answer these questions, I feel very cheerful."
- "For some reason I am not very comfortable."
- "At this moment I feel 'edgy' or irritable."

MSF scores are calculated by summing the four responses with the response-scale reverse-coded for the last two items. Possible scores range from 4 (worst possible mood) to 20 (best possible mood).

In an attempt to reduce the propensity of subjects to anchor their post- to pre-revelation MSF responses, items from the Basic Psychological Need Satisfaction Scale (BPNSS) were included in both surveys. The BPNSS is a 21-item instrument that measures needs for competence, autonomy, and relatedness (Deci & Ryan, 2000; Gagné, 2003). Responders evaluate statements (e.g., "Often, I do not feel very confident," and "People in my life care about me") on a seven-point scale in which "Not at all true" =1, "Somewhat true" =4, and "Very true" =7. The pre- and post-revelation SWB surveys included 11 and 10 BPNSS items, respectively.

To further reduce the propensity to anchor, the order of all items in the pre-revelation SWB survey was randomized by subject. The post-revelation items were randomized in two strata:

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[5] Of 1,180 individuals who clicked the Qualtrics link, 128 did not complete the survey, and another 56 completed the survey but either entered an incorrect survey-completion code or none at all. These individuals could not be paid and are not included as subjects in our analyses.

[6] Three dimensions of SWB have been identified in the literature. Experiential SWB measures moods and affects experienced currently or in the recent past. Evaluative SWB measures how people assess their lives as a whole or particular domains of their lives (e.g., finances or family). Eudaimonic SWB measures the extent to which people have purpose or meaning in their lives. For a thorough survey of the three dimensions of SWB, and their shared versus distinct correlates, refer to the National Academy of Sciences report (Stone & Mackie, 2013).

the first contained the four MSF items randomized by subject, and the second contained the 10 BPNSS items also randomized by subject. The stratified randomization ensured that the post-revelation MSF items were presented immediately after the revelation, thus reducing concerns that the impact of the revelation may have worn off.

3.2.2. Revelation Mechanism

The revelation mechanism was designed to provide an unconfounded relative-rewards shock. Prior to the pre-revelation SWB survey, in an attempt to standardize expectations, the instructions fully informed subjects about the revelation mechanism. Each subject was informed that:

- She would be paid a $1 reward for completing the study.[7]
- She would be randomly assigned to a two-person group.
- The other subject in the group (hereafter Participant X) could be any other subject in the study.
- She would never learn Participant X's identity and vice versa.
- She and Participant X would be allotted 2 or 10 experimental points each, creating four possible allocations of points:
    - She receives 2 points, and Participant X receives 2 points.
    - She receives 2 points, and Participant X receives 10 points.
    - She receives 10 points, and Participant X receives 2 points.
    - She receives 10 points, and Participant X receives 10 points.

Below, we refer to these allocations as low-low (LL), low-high (LH), high-low (HL), and high-high (HH), respectively.

Subjects were then randomly informed of being in one of two treatments. Each subject in the "points-money (pts$) treatment" was informed that each experimental point was worth $1. Each subject in the "points (pts) treatment" was informed that she and Participant X would receive a $6 payment regardless of the number of experimental points. Subjects were presented with neither the allotment nor the treatment terminology. Also, subjects in the pts$-treatment did not know about the pts-treatment, and vice versa.

After completing the pre-revelation SWB survey, subjects were reminded, as appropriate, that either "the number of points you and Participant X receive will not affect your bonus payments," or "you and Participant X will receive a bonus payment of a dollar for each point you receive." Subjects were also reminded that they were randomly assigned to one of the four possible allocations of points. To reveal the allocation of points, subjects had to press an "OK" button. They were then informed of the number of points they and Participant X received.

[7] In mTurk, a flat payment that all subjects receive is called a "reward;" and a payment that can vary by subject is called a "bonus." It should be noted that our payments are sizeable for mTurk, where workers' median hourly rewards have been estimated to be $2, and only 4% of workers earn more than $7.25 per hour (Hara et al., 2018).

3.2.3. Screening questions, questionnaire, and end of experiment

The experiment also included four screening items to test subjects' attentiveness:

- After reading the instructions and before answering the pre-revelation SWB survey, subjects were asked what year it was; there were five possible responses: "1990," "2017," "2000," "2018," and "2019."
- After completing the pre-revelation SWB survey, subjects were presented with the following statement: "Currently, the year is 2025." The response scale was the same five-point Likert scale used with the MSF items.
- In the post-revelation SWB survey (after completing the MSF items and before completing the BPNSS items), subjects were asked the following two items:
    - "Please indicate the value of the bonus you will receive (in addition to the $1 reward you will receive for completing this study)." Possible responses were "$2," "$6," and "$10."
    - "Please indicate the value of the bonus Participant X will receive (in addition to the $1 reward Participant X will receive for completing this study)." Possible responses were "$2," "$6," and "$10."

After the post-revelation SWB survey, subjects completed a 15-item questionnaire (including demographic characteristics and political views), entered their survey-completion codes, and were paid.

3.3. Own- and relative-rewards shocks

Our experiment yields 8 distinct cells from a 4 (allocations of 2 or 10 pts to the subject and 2 or 10 pts to Participant X) X 2 (pts$- versus pts-treatment) design: $LL_{pts\$}$, $LH_{pts\$}$, $HL_{pts\$}$, $HH_{pts\$}$, LL_{pts}, LH_{pts}, HL_{pts}, and HH_{pts} (see Table 1). The design enables identification of both relative-rewards and own-rewards shocks by comparing cells that hold all other factors constant.

For example, let's compare the HL and HH cells by treatment. Each subject in the $HL_{pts\$}$ cell was told that she would receive 10 pts worth $1 each and that Participant X would receive 2 pts worth $1 each, whereas each subject in the $HH_{pts\$}$ cell was told that she and Participant X would each receive 10 pts worth $1 each. In these two cells, each subject's realized pts- and $-rewards are constant (10 pts and $10). Further, as the instructions disclosed all possible revelations, each subject's expected pts- and $-rewards should be 6 pts and $6 for both herself and Participant X. Thus, the $HL_{pts\$}$ and $HH_{pts\$}$ cells differ only in Participant X's realized pts- and $-rewards (2 pts and $2 versus 10 pts and $10). By comparing the MSF-change (post- minus pre-revelation) of $HL_{pts\$}$ and $HH_{pts\$}$ subjects, we can identify *ceteris paribus* the differential impact on a subject's mood of Participant X receiving 2 pts and $2 rather than 10 pts and $10: $\Delta MSF_{HL,pts\$} - \Delta MSF_{HH,pts\$}$. We refer to this as the impact of an *advantageous* relative-pts-$ shock. In relative-rewards-shock comparisons, "advantageous" indicates that the subject receives greater rewards than Participant X, rather than the same rewards as Participant X.

Let's now consider the analogous cells of the pts-treatment: because pts have no value and the realized $-rewards are constant in the HL_{pts} and HH_{pts} cells, $\Delta MSF_{HL,pts}$ - $\Delta MSF_{HH,pts}$ identifies *ceteris paribus* the differential impact on a subject's mood of Participant X receiving 2 pts rather than 10 pts. We refer to this as the impact of an advantageous relative-pts shock. Therefore, we can capture the impact of Participant X receiving $2 rather than $10 by subtracting the impact of an advantageous relative-pts shock from the impact of an advantageous relative-pts-$ shock: $(\Delta MSF_{HL,\$} - \Delta MSF_{HH,\$}) = (\Delta MSF_{HL,pts\$} - \Delta MSF_{HH,pts\$}) - (\Delta MSF_{HL,pts} - \Delta MSF_{HH,pts})$. We refer to this difference as the impact of an advantageous relative-$ shock.

In relative-rewards-shock comparisons, "disadvantageous" indicates that the subject receives lesser rewards than Participant X, rather than the same rewards as Participant X. By comparing LH and LL cells, we can identify the impact of disadvantageous relative-pts-$ ($\Delta MSF_{LH,pts\$} - \Delta MSF_{LL,pts\$}$), relative-pts ($\Delta MSF_{LH,pts} - \Delta MSF_{LL,pts}$), and relative-$ (($\Delta MSF_{LH,\$} - \Delta MSF_{LL,\$}$) = ($\Delta MSF_{LH,pts\$} - \Delta MSF_{LL,pts\$}$) - ($\Delta MSF_{LH,pts} - \Delta MSF_{LL,pts}$)) shocks.

Similarly, we can identify the impact of an *advantageous* own-pts-$ shock by comparing $HH_{pts\$}$ to $LL_{pts\$}$ cells: $\Delta MSF_{HH,pts\$} - \Delta MSF_{LL,pts\$}$. Subjects in the $HH_{pts\$}$ ($LL_{pts\$}$) cells are told that they and Participant X will each receive 10 pts (2 pts) worth $1 each. In both cells, subjects and Participant X receive the same rewards, thus the RIE should not explain the impact of the advantageous own-pts-$ shock. Therefore, in own-rewards-shock comparisons, "advantageous" indicates that the subject receives greater rewards than expected, rather than lesser rewards than expected (recall that expected rewards are 6 pts and $6 for all subjects).[8]

Comparing the analogous cells of the pts-treatment, we can identify the impact of an advantageous own-pts shock by comparing HH_{pts} and LL_{pts} cells: $\Delta MSF_{HH,pts} - \Delta MSF_{LL,pts}$. Because pts have no value and the realized $-rewards are constant, subjects in these cells differ only in the pts-rewards that they and Participant X receive (10 pts each versus 2 pts each). Therefore, we can capture the impact of the subject receiving $10 rather than $2 by subtracting the impact of an advantageous own-pts shock from the impact of an advantageous own-pts-$ shock: $(\Delta MSF_{HH,\$} - \Delta MSF_{LL,\$}) = (\Delta MSF_{HH,pts\$} - \Delta MSF_{LL,pts\$}) - (\Delta MSF_{HH,pts} - \Delta MSF_{LL,pts})$. We refer to this difference as the impact of an advantageous own-$ shock. In Panel A of Table 2, we provide a complete list of the relative- and own-rewards shocks and, in Panel B, the formulae we use to identify their impacts.[9]

3.4. Experimental predictions

3.4.1. Relative-rewards-shock predictions

[8] This interpretation of the impact of an advantageous own-pts-$ shock is potentially confounded if Participant X's rewards impact subjects' SWB through channels other than the RIE. This is because, while subjects' rewards relative to Participant X are the same in the $HH_{pts\$}$ and $LL_{pts\$}$ cells, Participant X's absolute rewards vary.

[9] The impact of disadvantageous own-rewards shocks are not listed, as they can be calculated by multiplying the impact of the corresponding advantageous own-rewards shocks by negative one.

Making no further assumptions, the RIE yields two unambiguous predictions about the impact of relative-rewards shocks:

- Positive impact of the advantageous relative-pts-$ shock:
    - $\Delta MSF_{HL,pts\$} - \Delta MSF_{HH,pts\$} > 0$
- Negative impact of the disadvantageous relative-pts-$ shock:
    - $\Delta MSF_{LH,pts\$} - \Delta MSF_{LL,pts\$} < 0$

These predictions can be inferred from the definition of the RIE, as both comparisons hold constant the subject's monetized pts and vary Participant X's, with smaller (larger)--in comparison to an equal--number of monetized pts for Participant X associated with higher (lower) values of ΔMSF.

Predicting the impact of the other relative-rewards shocks requires assumptions about MSF-scores' responsiveness to non-monetized pts. We will consider two alternative assumptions: (a) *strict RIE*, whereby the RIE applies to monetized but not non-monetized pts, versus (b) *generalized RIE*, whereby the RIE applies equally to monetized and non-monetized pts. Under strict RIE, the impacts of both advantageous and disadvantageous relative-pts shocks are predicted to be zero, and this yields the prediction that $-shocks will have the same impact as their corresponding pts-$-shocks. Specifically:

- Zero impact of the advantageous relative-pts shock:
    - $\Delta MSF_{HL,pts} - \Delta MSF_{HH,pts} = 0$
- Zero impact of the disadvantageous relative-pts shock:
    - $\Delta MSF_{LH,pts} - \Delta MSF_{LL,pts} = 0$
- Equal impacts of the advantageous relative-$ and advantageous relative-pts-$ shocks:
    - $\Delta MSF_{HL,\$} - \Delta MSF_{HH,\$} = \Delta MSF_{HL,pts\$} - \Delta MSF_{HH,pts\$}$
- Equal impacts of the disadvantageous relative-$ and disadvantageous relative-pts-$ shocks:
    - $\Delta MSF_{LH,\$} - \Delta MSF_{LL,\$} = \Delta MSF_{LH,pts\$} - \Delta MSF_{LL,pts\$}$

Under generalized RIE, the impacts of the advantageous and disadvantageous relative-pts shocks are predicted to equal the impacts of the corresponding pts-$ shocks, and this yields the prediction that the relative-$ shocks will have no impact.

- Equal impacts of the advantageous relative-pts and advantageous relative-pts-$ shocks:
    - $\Delta MSF_{HL,pts} - \Delta MSF_{HH,pts} = \Delta MSF_{HL,pts\$} - \Delta MSF_{HH,pts\$}$
- Equal impacts of the disadvantageous relative-pts and disadvantageous relative-pts-$ shocks:
    - $\Delta MSF_{LH,pts} - \Delta MSF_{LL,pts} = \Delta MSF_{LH,pts\$} - \Delta MSF_{LL,pts\$}$
- Zero impact of the advantageous relative-$ shock:
    - $\Delta MSF_{HL,\$} - \Delta MSF_{HH,\$} = 0$
- Zero impact of the disadvantageous relative-$ shock:

- $\Delta MSF_{LH,\$} - \Delta MSF_{LL,\$} = 0$

3.4.2. Own-rewards-shock predictions

Assuming that subjects prefer receiving more to fewer monetized pts, and/or that, relative to expectations, subjects prefer gains of monetized pts to losses, the following prediction obtains:

- Positive impact of the advantageous own-pts-$ shock:
  - $\Delta MSF_{HH,pts\$} - \Delta MSF_{LL,pts\$} > 0$

Additional predictions about own-rewards shocks require additional assumptions. If we assume that, analogous to the strict RIE, MSF-scores do not respond to own non-monetized pts, then an advantageous own-pts shock is predicted to have no impact, and an advantageous own-$ shock will have the same impact as an advantageous own-pts-$ shock:

- Zero impact of the advantageous own-pts shock:
  - $\Delta MSF_{HH,pts} - \Delta MSF_{LL,pts} = 0$
- Equal impacts of the advantageous own-$ and advantageous own-pts-$ shocks:
  - $\Delta MSF_{HH,\$} - \Delta MSF_{LL,\$} = \Delta MSF_{HH,pts\$} - \Delta MSF_{LL,pts\$}$

If we assume that, analogous to the generalized RIE, MSF-scores respond equally to own non-monetized and monetized pts, then an advantageous own-pts shock is predicted to have the same impact as an advantageous own-pts-$ shock, and an advantageous own-$ shock will have no impact:

- Equal impacts of the advantageous own-pts and advantageous own-pts-$ shocks:
  - $\Delta MSF_{HH,pts} - \Delta MSF_{LL,pts} = \Delta MSF_{HH,pts\$} - \Delta MSF_{LL,pts\$}$
- Zero impact of the advantageous own-$ shock:
  - $\Delta MSF_{HH,\$} - \Delta MSF_{LL,\$} = 0$

4. Results

Of the 996 subjects who completed the experiment, 136 completed at least one of the screening items incorrectly and were dropped from the analysis.[10] Table 3 presents the demographic characteristics of the sample. Table 4 presents the pre- and post-revelation MSF-scores and ΔMSF for each cell.

[10] 2 subjects did not indicate that the year was "2017;" 9 subjects did not "Disagree" or "Strongly Disagree" that the current year was 2025; 53 (45) subjects indicated the incorrect payment for themselves (Participant X) in the pts$-treatment; and 56 (64) subjects indicated the incorrect payment for themselves (Participant X) in the pts-treatment. The main results, using the methods presented in Section 4.2 below, hold if we do not drop these subjects (see columns (3) and (4) of Appendix Table 1). It can be argued that keeping these subjects in the analysis may be preferred, as it may be more reflective of a general population in which some individuals do not pay close attention to their own and/or others' income; thank you to Alan Kirman for pointing this out.

4.1. Difference-of-means tests[11]

In Table 5, we present the estimated impacts of all relative- and own-rewards shocks.[12] Considering the latter, we observe that the impact of advantageous own-pts-$, own-pts, and own-$ shocks are positive, statistically significant, and economically meaningful. For example, the impact of the advantageous own-$ shock is 2.17 MSF-points. In other words, controlling for the impact of own pts, subjects who receive $10 rather than $2 experience a mood-improvement of roughly 14% of the pre-revelation sample-mean MSF-score (= 2.17/15.25). The impact of the advantageous own-pts shock being positive and significant indicates that subjects' mood is impacted by receiving non-monetized pts. The impact of the advantageous own-$ shock being positive and significant indicates that the impact on subjects' mood of monetized pts exceeds the impact of receiving equivalent non-monetized pts.

Turning to the advantageous relative-rewards shocks, there is no support for the RIE. Table 5 shows that the impacts of the advantageous relative-pts-$, relative-pts, and relative-$ shocks are all negative. This suggests that subjects do not prefer to be "richer" than other subjects. The estimated impacts of the disadvantageous relative-rewards shocks, though, favor the generalized version of the RIE. The impacts of the relative-pts-$ and relative-pts shocks are negative and significant. Further, they are statistically indistinguishable from each other, yielding an insignificant impact of the disadvantageous relative-$ shocks. This suggests that being "poorer" than other subjects in monetized points is only as mood-diminishing as is being "poorer" in non-monetized points.

Comparing the impacts of the own-rewards and relative-rewards shocks reveals two interesting asymmetries. First, people seem to value their own money--but not others' money--more than the points the money represents. Second, people have a distaste for being behind others but not a taste for being ahead.

4.2. Regression and subgroup analyses

To analyze the MSF-effects of relative-rewards shocks while controlling for demographic characteristics, we estimate the following equation:

$$(1)\ \Delta MSF = \beta_0 + \beta_1 I_{2pts\$} + \beta_2 I_{10pts\$} + \beta_3 I_{10pts} + \beta_4 I_{X10pts} + \beta_5 I_{2pts\$,X10pts\$} + \beta_6 I_{10pts\$,X10pts\$} + \beta_7 I_{10pts,X10pts} + \delta Y + \varepsilon$$

The indicator variable $I_{2pts\$}$ ($I_{10pts\$}$) equals one if the subject receives 2 pts (10 pts) worth $1 each. The indicator variable I_{10pts} (I_{X10pts}) equals one if the subject (Participant X) receives 10 pts. The indicator variable $I_{2pts\$,X10pts\$}$ ($I_{10pts\$,X10pts\$}$) equals one if the subject receives 2 pts (10 pts) worth $1 each and Participant X receives 10 pts worth $1 each. The indicator variable

[11] All p-values reported in this section are from two-tail difference-of-means tests.

[12] Columns (5) and (6) of Appendix Table 1 show, using the methods from Section 4.2 below, that the relative- and own-rewards shocks did not impact BPNSS scores.

$I_{10pts,X10pts}$ equals one if the subject receives 10 pts and Participant X receives 10 pts. The vector Y contains demographic characteristics.[13] Robust standard errors are calculated.

The constant term β_0 captures the MSF-change experienced by subjects in the LL_{pts} cell. Regression coefficients can be used to recover the impacts of relative- and own-rewards shocks; the formulae are presented in Panel C of Table 2 and the corresponding estimates are reported in Table 6. Column (1) of Table 6 excludes vector Y and replicates the corresponding results in Table 5. Column (2) includes vector Y, yielding results similar to column (1).

Turning to subgroup analyses, we investigate differences that are suggested by the literature. For example, Alesina et al. (2004) find that conservatives' SWB is unaffected by--while progressives' SWB decreases with--income inequality in the US; this may be due to conservatives being more likely to attribute success to hard work and talent, and progressives to luck (Frank, 2016). Kamas & Preston (2015) identify gender-differences in social preferences, with women more likely to be inequity averse and men more likely to be social-surplus maximizers. Ifcher et al. (2018) and Alesina et al. (2004) find that the negative income-inequality-SWB relationship is significantly stronger for high- than low-income Americans, with no significant relationship for low-income subgroups in some specifications. Kuziemko et al. (2014) find that "last-place aversion" is particularly strong for conservatives and individuals with low income. Subgroup analyses by political orientation, gender, and income are presented in columns (3) - (8) of Table 6.

The most notable difference by political orientation is that the disadvantageous relative-pts-$ shock is significantly MSF-diminishing for conservatives and has no significant MSF-effect for liberal/progressives. Further, the MSF-effects for conservatives and liberal/progressives are marginally significantly different (-2.260 versus -0.106, p-value = 0.056). Because the MSF-effects of the disadvantageous relative-pts shocks are similar by political orientation, the MSF-effects of the disadvantageous relative-$ shocks are marginally significantly different for conservatives and liberal/progressives (-2.101 versus 0.608, p-value = 0.107).[14] In fact, the disadvantageous relative-$ shock for conservatives is the only relative-$ shock with even a marginally significant impact. In sum, an interesting asymmetry is revealed by political orientation: being "richer" does not affect conservatives and liberal/progressives differently, but being "poorer" does. This finding is consistent with Kuziemko et al. (2014) and may be explained by conservatives' strong last-place aversion.

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[13] Demographic characteristics include gender (female, male, other, prefer not to answer), age (bottom-quartile (< 29 years) and top-quartile (> 42 years) indicator-variables), race (American Indian/Alaska Native, Asian, Black/African American, Hispanic, Native Hawaiian/other Pacific Islander, White, other, prefer not to answer), religion (Atheist/Agnostic, Buddhist, Christian, Hindu, Jewish, Muslim, other, prefer not to answer), political leaning (conservative, liberal/progressive, moderate, prefer not to answer), household income ($0-$25K, $25K-$50K, $50K-$75K, $75K-$100K, $100K-$125K, $125K-$150K, $150K+, prefer not to answer), education (some high school, completed high school, some college, completed college, some grad/professional school, completed grad/professional school, prefer not to answer), and employment status (employed, full-time; employed, part-time; not employed, looking for work; not employed, not looking for work; retired; student; prefer not to answer).

[14] In Table 6, a number of p-values of two-sided tests are between 0.10 and 0.11, so we decided to identify estimated coefficients with p-values < 0.11 as marginally significant to highlight these.

Subgroup analyses by gender reveal that the impact of the advantageous relative-pts-$, advantageous relative-pts, and disadvantageous relative-pts-$ shocks are significantly MSF-diminishing for women but not men. Further, the MSF-effect of the advantageous relative-pts shock is significantly greater in magnitude for women than men (-1.435 versus -0.296, p-value = 0.031), and the MSF-effect of the disadvantageous relative-pts-$ shock is marginally significantly greater in magnitude for women than men (-1.748 versus -0.293, p-value = 0.104). In sum, there is evidence that being both "richer" and "poorer" is MSF-diminishing for women but not for men. This finding is consistent with Kamas & Preston (2015) and may be explained by women being more inequity-averse than men. There is also evidence of gender differences in the MSF-effects of own-pts versus own-$ shocks: while the impact of the advantageous own-pts-$ shock is similar for men and women, the impact of the advantageous own-pts shock is marginally significantly more MSF-improving for women than men (1.901 versus 0.915, p-value = 0.078), and thus the impact of the advantageous own-$ shock is marginally significantly less MSF-improving for women than men (1.236 versus 2.710, p-value = 0.105). These are the only own-rewards shocks that differ even marginally significantly by subgroup.

Lastly, the only notable difference by income-subgroups is that the disadvantageous relative-pts-$ shock is significantly MSF-diminishing for individuals with household income in the top-quartile and not for those in the bottom 3 quartiles.[15] This finding is consistent with Ifcher et al. (2018) and Alesina et al. (2004), but not Kuziemko et al. (2014).

5. Discussion

In an experiment designed to test for the RIE, we find no support for an interpretation of the RIE that applies strictly to income; this is because we find that the impact of being "richer" or "poorer" in experimental points is the same when points are worth money and when they are worthless. This cannot be explained by subjects' indifference toward money and points in the experiment, as receiving more money than expected makes subjects significantly happier than receiving equivalently more points than expected. Further, we find only partial support for an interpretation of the RIE that applies generally to income and non-income relative concerns alike; this is because we find evidence of a distaste for being "poorer" than others, but no evidence of a taste for being "richer" than others.

Our attempt to disentangle the RIE's income from non-income relativity concerns is novel. Further, we do so with an extreme counterfactual: non-income relativity is manipulated using a worthless but cardinal "good." How does this relate to evidence of the RIE from observational studies? As noted in the literature review, the SWB-impact of factors like cost-of-living may be misattributed to others' income in the absence of proper controls. Our findings suggest that,

[15] The advantageous relative-pts shock does not significantly impact the MSF of individuals with household income in the top quartile, but it is significantly MSF-diminishing for those in the bottom 3 quartiles. While this may seem like an important difference, the similar magnitudes of the point-estimates (-0.776 versus -0.855, p-value = 0.909) suggest that it is likely due to variant sample sizes (n = 200 versus 646).

even if the RIE is identified, income itself may not be driving it. Because of its numerical and/or material salience, income may be used as a proxy for ineffable factors in social comparisons that, if defined and concretized, would weaken relative income's relationship to SWB. For example, suppose SWB's only determinant is feeling like a productive member of society, and that people use income as a proxy for this in the absence of a more direct measure. If a more direct measure of productivity became available, the relationship between relative income and SWB would be eliminated. Because the RIE literature has not considered the role of money per se, prior evidence regarding the RIE implicitly measures the impact of something akin to our relative-pts-$, rather than relative-$, shocks.[16]

Another novel contribution of our research is our use of SWB-change as the outcome of interest rather than observed choice. A benefit of this approach is that it allows us to capture preference-magnitudes more so than would a revealed-preference approach. For example, our approach suggests a preference for $HH_{pts\$}$ over $LL_{pts\$}$ that is of significantly greater magnitude than the preference for HH_{pts} over LL_{pts}; if subjects instead faced choices of HH versus LL, estimation of how much more they prefer HH in the pts$- than pts-treatment would rely on binary data and be less precise.

Our approach, of course, begs the question of whether SWB-change is predictive of choice, and whether a revealed-preference approach would result in the same patterns we observe. In Benjamin et al. (2014b), medical students report their choice-rankings of residency programs, along with their anticipated-SWB rankings and the features they expect each program to have. The preferences over program-features implied by choice-rankings differ substantially from those implied by anticipated-SWB rankings, with the preferences implied by anticipated-evaluative-SWB rankings closer to those implied by choice than are the preferences implied by anticipated-experiential-SWB rankings. While this is relevant in that it addresses the relationship between SWB and revealed-preference, the SWB is only considered anticipatorily and not contemporaneously. Further research is necessary to directly address whether our SWB-change approach is compatible with a revealed-preference approach. If so, it could substitute for revealed-preference in other contexts, like identifying preferences for earned versus unearned income, or estimating parameters related to loss aversion, time-, risk-, or competitive-preferences. If not, it could potentially complement revealed-preference. For example, it may be that our approach, being emotionally based, reflects System-1 thinking while revealed-preference, being deliberative, reflects System-2 thinking, and that preferences may be best understood using a combination of the two approaches (Kahneman, 2011).

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[16] Other evidence of money's protean nature comes from the experimental psychology literature that shows that priming the concept of money increases self-sufficiency (Vohs et al., 2006), tolerance for physical pain (Zhou et al., 2009), work and productivity (Mogilner, 2010); and decreases the savoring of experiences (Quoidbach et al., 2010), willingness to volunteer or donate (Pfeffer & DeVoe, 2008; Chatterjee et al., 2013), socializing and happiness (Mogilner, 2010), and anxiety and fear of death (Zaleskiewicz et al., 2013).

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Table 1: 4X2 treatment-design, with own, Participant X's, and relative rewards by cell

Cell	pts-$-Treatment			pts-Treatment		
	Self	Participant X	Relative rewards	Self	Participant X	Relative rewards
LL	$2	$2	0	$6	$6	0
	2 pts	2 pts	0	2 pts	2 pts	0
LH	$2	$10	-$8	$6	$6	0
	2 pts	10 pts	-8 pts	2 pts	10 pts	-8 pts
HL	$10	$2	+$8	$6	$6	0
	10 pts	2 pts	+8 pts	10 pts	2 pts	+8 pts
HH	$10	$10	0	$6	$6	0
	10 pts	10 pts	0	10 pts	10 pts	0

Notes: As the instructions disclosed all possible revelations, the subject's expected pts- and $-rewards are 6 pts and $6 for both herself and Participant X for all cells. Relative rewards are own minus Participant X's rewards.

Table 2: Relative- and own-rewards shocks and formulae used for estimation

A. Shocks	B. Difference-of-means estimators	C. Regression estimators
Advantageous relative-pts-$	$\Delta MSF_{HL,pts\$} - \Delta MSF_{HH,pts\$}$	$\mathbf{-(\beta_4 + \beta_6 + \beta_7)} = (\beta_0 + \beta_2 + \beta_3) - (\beta_0 + \beta_2 + \beta_3 + \beta_4 + \beta_6 + \beta_7)$
Advantageous relative-pts	$\Delta MSF_{HL,pts} - \Delta MSF_{HH,pts}$	$\mathbf{-(\beta_4 + \beta_7)} = (\beta_0 + \beta_3) - (\beta_0 + \beta_3 + \beta_4 + \beta_7)$
Advantageous relative-$	$(\Delta MSF_{HL,pts\$} - \Delta MSF_{HH,pts\$}) - (\Delta MSF_{HL,pts} - \Delta MSF_{HH,pts})$	$\mathbf{-\beta_6} = -(\beta_4 + \beta_6 + \beta_7) + (\beta_4 + \beta_7)$
Disadvantageous relative-pts-$	$\Delta MSF_{LH,pts\$} - \Delta MSF_{LL,pts\$}$	$\mathbf{\beta_4 + \beta_5} = (\beta_0 + \beta_1 + \beta_4 + \beta_5) - (\beta_0 + \beta_1)$
Disadvantageous relative-pts	$\Delta MSF_{LH,pts} - \Delta MSF_{LL,pts}$	$\mathbf{\beta_4} = (\beta_0 + \beta_4) - \beta_0$
Disadvantageous relative-$	$(\Delta MSF_{LH,pts\$} - \Delta MSF_{LL,pts\$}) - (\Delta MSF_{LH,pts} - \Delta MSF_{LL,pts})$	$\mathbf{\beta_5} = (\beta_4 + \beta_5) - \beta_4$
Advantageous own-pts-$	$\Delta MSF_{HH,pts\$} - \Delta MSF_{LL,pts\$}$	$\mathbf{-\beta_1 + \beta_2 + \beta_3 + \beta_4 + \beta_6 + \beta_7} = (\beta_0 + \beta_2 + \beta_3 + \beta_4 + \beta_6 + \beta_7) - (\beta_0 + \beta_1)$
Advantageous own-pts	$\Delta MSF_{HH,pts} - \Delta MSF_{LL,pts}$	$\mathbf{\beta_3 + \beta_4 + \beta_7} = (\beta_0 + \beta_3 + \beta_4 + \beta_7) - \beta_0$
Advantageous own-$	$(\Delta MSF_{HH,pts\$} - \Delta MSF_{LL,pts\$}) - (\Delta MSF_{HH,pts} - \Delta MSF_{LL,pts})$	$\mathbf{-\beta_1 + \beta_2 + \beta_6} = (-\beta_1 + \beta_2 + \beta_3 + \beta_4 + \beta_6 + \beta_7) - (\beta_3 + \beta_4 + \beta_7)$

Table 3: Demographic characteristics, n = 860

	mean
Gender	
Female	0.45
Male	0.54
Other / prefer not to answer	0.01
Age	36.48
Are you a citizen or permanent resident of the United States?	
Yes	1.00
No	0.00
Prefer not to answer	0.00
Rate your English	
Native	0.98
Fluent	0.02
Proficient	0.00
What race/ethnicity do you identify yourself as?	
American Indian and Alaska Native	0.00
Asian	0.08
Black or African	0.07
Hispanic	0.05
White	0.77
Other / prefer not to answer	0.02
What religion do you consider yourself?	
Atheist / agnostic	0.47
Buddhist	0.02
Christian	0.42
Hindu	0.00
Jewish	0.02
Muslim	0.00
Other / prefer not to answer	0.07
How would you characterize your political views?	
Conservative	0.20
Liberal / progressive	0.50
Moderate	0.28
Prefer not to answer	0.02
What is the total (gross) income last year of your household?	
$0-25,000	0.18
$25,000-50,000	0.30
$50,000-75,000	0.24
$75,000-100,000	0.15
$100,000-125,000	0.05
$125,000-150,000	0.03
$150,000+	0.04
Prefer not to answer	0.02
What is your highest level of education?	
Some high school	0.00
Completed high school	0.10
Some college	0.28
Completed college	0.44
Some grad / professional school	0.04
Completed grad / professional school	0.14
Prefer not to answer	0.00
Please indicate your employment status:	
Employed, full-time	0.67
Employed, part-time	0.15
Not employed, looking for work	0.06
Not employed, not looking for work	0.06
Retired	0.02
Student	0.03
Prefer not to answer	0.02

Table 4: Pre- and post-revelation MSF-scores and ΔMSF by cell

Cell	MSF-score	pts-$-Treatment	pts-Treatment
LL	pre-revelation	15.081 ***	15.495 ***
		(0.347)	(0.291)
	post-revelation	13.97 ***	15.495 ***
		(0.397)	(0.329)
	change	-1.11 ***	0.000
		(0.275)	(0.176)
	observations	99	103
LH	pre-revelation	14.92 ***	15.055 ***
		(0.334)	(0.327)
	post-revelation	12.77 ***	14.312 ***
		(0.416)	(0.358)
	change	-2.16 ***	-0.743 ***
		(0.304)	(0.249)
	observations	103	109
HL	pre-revelation	15.40 ***	16.056 ***
		(0.280)	(0.311)
	post-revelation	17.29 ***	16.539 ***
		(0.270)	(0.290)
	change	1.89 ***	0.483 ***
		(0.223)	(0.153)
	observations	123	89
HH	pre-revelation	15.03 ***	15.139 ***
		(0.302)	(0.350)
	post-revelation	17.29 ***	16.337 ***
		(0.254)	(0.321)
	change	2.26 ***	1.198 ***
		(0.203)	(0.170)
	observations	133	101

Notes: Robust standard errors in parenthesis. *, **, *** represents p-values < 0.1, 0.05, and 0.01, respectively.

Table 5: Estimated impacts of relative- and own-rewards shocks on ΔMSF

Shocks	Formulae		
Adv own-pts-$	$\Delta MSF_{HH,pts\$} - \Delta MSF_{LL,pts\$}$	3.367	***
		(0.342)	
Adv own-pts	$\Delta MSF_{HH,pts} - \Delta MSF_{LL,pts}$	1.198	***
		(0.245)	
Adv own-$	$(\Delta MSF_{HH,pts\$} - \Delta MSF_{LL,pts\$}) - (\Delta MSF_{HH,pts} - \Delta MSF_{LL,pts})$	2.169	***
		(0.421)	
Adv rel-pts-$	$\Delta MSF_{HL,pts\$} - \Delta MSF_{HH,pts\$}$	-0.361	
		(0.302)	
Adv rel-pts	$\Delta MSF_{HL,pts} - \Delta MSF_{HH,pts}$	-0.714	**
		(0.228)	
Adv rel-$	$(\Delta MSF_{HL,pts\$} - \Delta MSF_{HH,pts\$}) - (\Delta MSF_{HL,pts} - \Delta MSF_{HH,pts})$	-0.354	
		(0.378)	
Disadv rel-pts-$	$\Delta MSF_{LH,pts\$} - \Delta MSF_{LL,pts\$}$	-1.044	**
		(0.422)	
Disadv rel-pts	$\Delta MSF_{LH,pts} - \Delta MSF_{LL,pts}$	-0.743	**
		(0.305)	
Disadv rel-$	$(\Delta MSF_{LH,pts\$} - \Delta MSF_{LL,pts\$}) - (\Delta MSF_{LH,pts} - \Delta MSF_{LL,pts})$	0.301	
		(0.521)	

Notes: Robust standard errors in parenthesis. *, **, *** represents p-values < 0.1, 0.05, and 0.01, respectively.

Table 6: Regression estimates of the impacts of relative- and own-rewards shocks on ΔMSF

Shocks	All (1)	All (2)	Conservative (3)	Liberal / Progressive (4)	Female (5)	Male (6)	Income in top quartile (7)	Income in bottom 3 quartiles (8)
Panel A: Impacts of advantageous relative-rewards shocks								
Adv rel-pts-$	-0.361	-0.431	-0.780	-0.554	-0.991 **	-0.076	-0.369	-0.311
	(0.302)	(0.313)	(0.671)	(0.496)	(0.490)	(0.449)	(0.673)	(0.346)
Adv rel-pts	-0.715 ***	-0.800 ***	-0.953 ***	-0.463	**-1.435** ***	**-0.296**	-0.776	-0.855 ***
	(0.228)	(0.242)	(0.553)	(0.339)	(0.437)	(0.295)	(0.631)	(0.285)
Adv rel-$	0.354	0.370	0.173	-0.091	0.445	0.220	0.406	0.544
	(0.378)	(0.395)	(0.841)	(0.602)	(0.659)	(0.549)	(0.943)	(0.448)
Panel B: Impacts of disadvantageous relative-rewards shocks								
Disadv rel-pts-$	-1.044 **	-1.026 **	*-2.620* **	*-0.106*	*-1.748* ***	*-0.293*	-1.975 **	-0.788
	(0.422)	(0.432)	(1.176)	(0.590)	(0.664)	(0.598)	(0.925)	(0.503)
Disadv rel-pts	-0.743 **	-0.665 **	-0.518	-0.714 *	-0.686	-0.500	-0.708	-0.520
	(0.305)	(0.318)	(0.538)	(0.405)	(0.511)	(0.413)	(0.607)	(0.399)
Disadv rel-$	-0.301	-0.361	*-2.101* *	*0.608*	-1.061	0.207	-1.267	-0.268
	(0.521)	(0.537)	(1.294)	(0.718)	(0.822)	(0.702)	(1.120)	(0.647)
Panel C: Impacts of advantageous own-rewards shocks								
Adv own-pts-$	3.367 ***	3.392 ***	2.934 ***	4.136 ***	3.136 ***	3.625 ***	2.820 ***	3.548 ***
	(0.342)	(0.353)	(0.660)	(0.562)	(0.484)	(0.542)	(0.711)	(0.415)
Adv own-pts	1.198 ***	1.294 ***	1.208 **	1.167 ***	*1.901* ***	*0.915* ***	0.898 *	1.369 ***
	(0.245)	(0.256)	(0.508)	(0.350)	(0.445)	(0.337)	(0.543)	(0.327)
Adv own-$	2.169 ***	2.098 ***	1.726 **	2.969 ***	*1.236* *	*2.710* ***	1.922 **	2.179 ***
	(0.421)	(0.433)	(0.845)	(0.643)	(0.650)	(0.637)	(0.886)	(0.529)
Vector Y included	No	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	860	860	175	434	391	463	200	646

Notes: Robust standard errors are reported in parenthesis. *, **, *** indicate p-value < 0.11, 0.05, and 0.01, respectively. Coefficients that are underlined and italicized (bolded) are significantly different from each other with p-value < 0.11 (0.05). Vector Y includes controls for gender, age, race, religion, political leaning, household income, education, and employment status.

Appendix Table 1: Regression estimates of the impacts of relative- and own-rewards shocks on ΔMSF when including dropped subjects and when including pilot-study subjects, and on post-revelation 10-item BPNSS score

	Include subjects from pilot study	Include subjects from pilot study	Include dropped subjects	Include dropped subjects	Post-revelation BPNSS score (10 items)	Post-revelation BPNSS score (10 items)
Shocks	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Impacts of advantageous relative-rewards shocks						
Adv rel-pts-$	-0.400	-0.429	-0.118	-0.118	0.092	0.114
	(0.269)	(0.313)	(0.278)	(0.284)	(0.124)	(0.127)
Adv rel-pts	-0.595 ***	-0.801 ***	-0.622 ***	-0.677 ***	0.115	0.118
	(0.200)	(0.242)	(0.235)	(0.251)	(0.131)	(0.124)
Adv rel-$	0.195	0.371	0.504	0.559	-0.023	-0.004
	(0.335)	(0.395)	(0.364)	(0.375)	(0.180)	(0.176)
Panel B: Impacts of disadvantageous relative-rewards shocks						
Disadv rel-pts-$	-1.019 ***	-1.026 **	-0.990 **	-0.939 **	-0.152	-0.186
	(0.388)	(0.432)	(0.396)	(0.400)	(0.158)	(0.154)
Disadv rel-pts	-0.497 *	-0.663 **	-0.631 **	-0.484 *	-0.066	-0.085
	(0.256)	(0.318)	(0.291)	(0.300)	(0.148)	(0.148)
Disadv rel-$	-0.522	-0.363	-0.359	-0.454	-0.087	-0.101
	(0.465)	(0.537)	(0.491)	(0.500)	(0.216)	(0.212)
Panel C: Impacts of advantageous own-rewards shocks						
Adv own-pts-$	3.187 ***	3.392 ***	3.100 ***	3.137 ***	-0.015	-0.048
	(0.315)	(0.353)	(0.314)	(0.321)	(0.149)	(0.148)
Adv own-pts	1.104 ***	1.297 ***	1.285 ***	1.366 ***	0.182	0.201
	(0.213)	(0.257)	(0.236)	(0.248)	(0.141)	(0.138)
Adv own-$	2.083 ***	2.096 ***	1.814 ***	1.771 ***	-0.197	-0.248
	(0.380)	(0.434)	(0.393)	(0.400)	(0.205)	(0.202)
Vector Y included	No	Yes	No	Yes	No	Yes
Observations	1,054	1,054	996	996	860	860

Notes: Robust standard errors are reported in parenthesis. *, **, *** indicate p-value < 0.11, 0.05, and 0.01, respectively. Vector Y includes controls for gender, age, race, religion, political leaning, household income, education, and employment status.