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"Bonus or Rebate?: The Impact of Income Framing on Spending and Saving".

Bonus or Rebate?: The Impact of Income Framing on Spending and Saving

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ABSTRACT

All income increases a person's absolute wealth, but consumption decisions may be based more heavily on perceived changes in wealth. Change is computed by comparing a current state with a former state, and we predicted that people would be more likely to spend income framed as a gain from a current wealth state than income framed as a return to a prior state. Four experiments confirmed this prediction on people's memory for spending of a government tax rebate (Experiment 1), on unobtrusive self-report measures of spending an unexpected windfall (Experiments 2 and 3), and on actual spending on items for sale in a laboratory experiment (Experiment 4). These results can be explained, at least in part, by the reference points implied in the framing of income (follow-ups to Experiments 1 and 4). Discussion focuses on implications for the consumption of other commodities, assessments of risk, and government tax policies. Copyright © 2006 John Wiley & Sons, Ltd.

KEY WORDS framing; memory; spending; income

INTRODUCTION

In September 2001, US President George W. Bush signed the "Economic Growth and Tax Relief Reconciliation Act" in an effort to stimulate a flagging economy. Among other things, this Tax Relief Act returned a record-setting \$38 billion to American taxpayers in the form of \$300–\$600 "tax rebates," with the explicit expectation that people would spend their rebate checks and hence stimulate economic recovery. Research suggests, however, that only 22% of households in one survey planned to spend their rebate check and the vast majority intended to save it (Shapiro & Slemrod, 2003a). These figures are consistent with the historically negligible short-term impact of tax rebates on the overall economy (Shapiro & Slemrod, 2003b).

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Although most Americans have notorious difficulty saving sufficiently for retirement out of their regular income, Americans appeared to experience little difficulty saving this particular piece of income.

In this paper we suggest that whether people save or spend any particular piece of income, such as a tax windfall, depends critically on how they perceive changes in their overall wealth state, rather than simply how they perceive their absolute wealth state. Traditional economic theory makes no distinction between the two, and assumes that the marginal propensity to consume (MPC) is largely a function of a person's absolute wealth (Keynes, 1936)—the greater a person's overall wealth, the more he or she will actually spend. Although the percentage of spent income is predicted to fluctuate systematically over one's life to maintain a consistent level of consumption (Modigliani, 1986), and overall spending is predicted to change only for permanent rather than temporary changes in income (Friedman, 1957), income is assumed to be fungible (i.e., without labels or accounts) and hence consumed without regard for its source or contextual circumstance. If a person's current MPC is 0.6, then 60 cents of each dollar earned will be spent and 40 cents will be saved.

Both psychologists and psychologically-minded economists, however, have questioned this assumption (Arkes et al., 1994; Fogel, 2004; Heath & Soll, 1996; Kahneman & Tversky, 1984; Shefrin & Thaler, 1990; Thaler, 1990; Warneryd, 1999). Few stimuli in the environment, after all, can be evaluated absolutely but instead must be evaluated in comparison to some standard or reference point. People are tall, attractive, or happy, for example, only in comparison to others who are shorter, uglier, or sadder. As Kahneman and Tversky put it, "our perceptual apparatus is attuned to the evaluation of changes or differences rather than to the evaluation of absolute magnitudes" (Kahneman & Tversky, 1979). This suggests that decisions about whether to spend income may depend more on *changes* in wealth rather than on absolute wealth (Fogel, 2004; Thaler & Johnson, 1990), and are therefore influenced by comparisons with existing standards or reference points. When considering one's income, the default reference point is the *status quo*, or one's current wealth, and changes in wealth therefore are perceived as a relative gain or loss from that reference point (Kahneman & Tversky, 1979).

Although income is by definition an objective gain from the *status quo*, we suggest that it may not always be perceived as a subjective gain to the consumer. In particular, we argue that income can be described (or "framed") as either a gain (a bonus) or a returned loss (a rebate). As described in the opening example, governments occasionally issue "tax rebates" to their constituents in an effort to stimulate consumer spending and thereby create a short-term economic stimulus. Businesses provide "cash-back incentives" to increase sales. And some employers give end-of-the-year bonuses to reward good performance. Despite being objectively identical, a "bonus" describes a positive change from the *status quo* whereas a "rebate" describes a return to a previous wealth state. To the extent that income is evaluated comparatively rather than absolutely, people may therefore feel they have more available income to spend when it is described as a bonus or a gain from one's current wealth than when it is described as a rebate or a return to a previous wealth state. This led us to predict that people will spend income described as a bonus more readily than identical income described as a rebate.

Such results would present a clear violation of the fungibility assumption of traditional economic theory, but such results are certainly not without empirical precedent. People predict that they will be more likely to gamble money, for instance, when they have just experienced a gain in wealth than when they have just experienced a loss (the "house-money" effect; Thaler & Johnson, 1990). People are also more likely to spend unexpected or unearned income (i.e., windfalls) than to spend expected or earned income (Arkes, et al., 1994). And people predict that they would be more willing to spend 20 minutes of their time to save \$5 on a \$15 purchase than to save \$5 on a \$250 purchase (Kahneman & Tversky, 1984).

More generally, our predictions are consistent with research on mental accounting that suggests people partition their income into different mental accounts, each with their own marginal propensity to consume (Thaler, 1999). In the following experiments, we predict that people are likely to spend a windfall described as a gain from one's current wealth state (as a bonus or gain) differently than income described as a return to

a previous wealth state (as a rebate or returned loss). Notice, however, that theories of mental accounting do not provide an *a priori* prediction for which description of income is more likely to lead to spending and which is more likely to lead to saving—it provides a meta-theory to explain violations of fungibility. Our specific predictions that people are more likely to spend income described as a gain from the *status quo* (as a bonus) than as a return to a previous wealth state (as a rebate) are instead based on the broad implications of the initial “editing phase” of Kahneman and Tversky’s Prospect Theory (1979), in which more complicated prospects or outcomes are coded in simpler representations, such as a gain of \$100 or a loss of \$100. Such edited prospects are determined largely by the change in a particular state than by its absolute state. A bonus, we reasoned, is generally perceived as a gain from one’s current wealth and therefore as available income to be spent. A returned loss or rebate, in contrast, is likely to be perceived as a gain from a previous loss and therefore edited into a return to a previous wealth state. A returned loss or rebate may therefore be perceived as no overall gain in one’s wealth state, and therefore more likely to be saved than income described as a gain or a bonus.

We tested our major hypothesis about the framing of income in three ways. First, Experiment 1 examined whether people’s self-reported memory for spending and saving of the US Government’s 2001 tax rebate could be influenced by the framing of that particular tax rebate. Second, Experiments 2 and 3 investigated self-reported spending and saving in a more controlled context by giving participants a \$50 financial gain framed as either a gain from the *status quo* (e.g., as a bonus) or as a return to the *status quo* (e.g., as a rebate). Third, Experiment 4 investigated actual spending by allowing participants to spend their income—again framed as a gain or a returned loss—on items for sale in the laboratory. These experiments attempt to provide convergent evidence for our hypotheses by utilizing a wide diversity of experimental paradigms and dependent variables, from recalled spending in Experiment 1, to reported spending in Experiments 2 and 3, to actual spending in Experiment 4. Because it is well-known that money from different financial or mental accounts may be spent differently (Heath & Soll, 1996; Thaler, 1999), great care was taken in each of the experiments to ensure that all income was described as coming from the same financial account or source—from the US Government in Experiment 1, and from a laboratory fund in Experiments 2–4. This was intended to isolate the impact of the framing of income from the well-known effects of mental accounting or mental budgeting.

In all experiments, we expected that income described as a positive departure from the *status quo* (a “bonus”) would be spent more readily than income described as a return to the *status quo* (a “rebate”). Given that income is distributed by both individual entities on a small scale as well as by large governments on a grand scale, these predicted results—if confirmed—would have important implications for both individual decision makers as well as broad-based public policy interventions such as tax rebates (Epley & Gneezy, in press).

EXPERIMENT 1—RECALLED SPENDING

The US tax rebate of 2001 provided each tax paying American household with a check for either \$300, \$500, or \$600, depending on the household size and reported annual income. Although there are many reasons why people may choose to spend or save any particular windfall such as this, our predictions suggest that the way people code these rebates may have a significant impact on whether they are likely to be saved or spent. In particular, we predict that people are more likely to save these kinds of windfalls than they might otherwise be to the extent that the windfalls are coded as returns to a previous wealth state rather than gains from a current wealth state.

Although we were obviously unable to test whether differing descriptions would influence actual spending and saving of this particular tax rebate, we *were* able to investigate an important corollary. In particular, Experiment 1 investigated the extent to which two descriptions of this tax rebate—one that described the

checks as either returned income or as additional income—influenced participants' *memory* for spending and saving of this rebate. Although decisions about whether to spend or save income are superficially distinct from one's memory for spending and saving, the reconstructive process of memory (Schacter, Norman, & Koutstaal, 1998) operates in much the same way as the construction of preferences that precedes decision and choice (Slovic, 1995). If the framing of income guides the construction of preferences for actual spending or saving, then it should operate similarly in reconstructing memories of spending and saving. The results of Experiment 1 would therefore demonstrate an important generality of our theory about the impact of income framing. In addition, understanding memory distortion is of practical importance in its own right to the extent that public policy analysts base their assessments of government interventions (such as tax rebates) on recalled or anticipated behavior rather than actual behavior.

Method

Approximately 6 months after receiving their rebate checks, 60 Boston-area residents were greeted by an experimenter in public places and agreed to participate in exchange for candy. All participants were first asked whether they recalled receiving a check—either \$300, \$500, or \$600—from the 2001 Tax Relief Act, and all did. Participants then read one of two descriptions of the 2001 Tax Relief Act at the top of a questionnaire. Those randomly assigned to the rebate condition read that “proponents of this tax cut argued that the government collected more tax revenue than was needed to cover its expenses, resulting in a tax surplus” that should be returned “as withheld income.” Those assigned to the bonus condition, in contrast, read that “proponents of this tax cut argued that the costs of running the government were lower than expected, resulting in a budget surplus” that should be returned “as bonus income.” All participants were then asked to indicate the amount their household received (\$300, \$500, or \$600), and what percentage of this money they recalled spending and what percentage they recalled saving. The order of these last two items was counterbalanced but did not influence results and is not discussed further.

Results

Two participants did not report spending or saving 100% of their check and were excluded from the following analyses. Including them, however, does not alter the following analyses in any meaningful way.

As predicted, Table 1 shows that participants in the bonus condition recalled spending dramatically more of their rebate check than participants in the rebate condition, $t(58) = 5.40$, $p < 0.0001$, $d = 1.42$. Interestingly, this low reported spending rate ($M = 25\%$) in the rebate condition is similar to the overall intended spending rates ($M = 22\%$) found in national surveys of the 2001 Tax Relief Act (Shapiro & Slemrod, 2003b), whereas the high recalled spending rate ($M = 87\%$) in the bonus condition is a dramatic departure.

Table 1. Recalled percentage of the 2001 government tax rebate check spent and saved by participants in the bonus and rebate framing conditions (Experiment 1 and Experiment 1 follow-up)

	Framing condition		<i>t</i> , <i>p</i>
	“Bonus”	“Rebate”	
Experiment 1			
Percentage spent	87%	25%	5.40, <0.0001
Percentage saved	13%	75%	
Experiment 1 Follow-up			
Percentage spent	77%	41%	4.54, <0.0001
Percentage saved	23%	59%	
Perceived check as “extra income”	3.78	2.94	1.90, 0.06
Perceived check as “returned income”	3.94	5.34	3.13, 0.002

We have suggested that these differences occurred because participants in the bonus condition construed the tax rebate as a positive gain from the *status quo*, and hence as extra income that could be spent, whereas participants in the rebate condition experienced the tax rebate as a return to a previous wealth state, and hence as returned income that implied no overall increase in their wealth state. Experiment 1 did not test this account directly, but we examined this explanation more carefully in a follow-up experiment approximately 6 months after the completion of Experiment 4 with 76 travelers in New York City's Grand Central Station. This follow-up experiment utilized a procedure very similar to Experiment 1, in which participants were asked if they recalled receiving a rebate check, and those that did were then randomly assigned to either the rebate or bonus conditions used in Experiment 1. However, after indicating what percentage of their tax rebate they recalled spending and saving, these participants were also asked the extent to which they perceived the rebate as a gain from their current wealth—as “‘extra’ money that you received in addition to whatever you would normally earn”—and the extent to which they perceived the rebate as a return to a previous wealth state—as “‘money belonging to your original income that was temporarily withheld, and which is now being given back to you.’” Ratings for both of these measures were made on 11-point scales ranging from 0 (not at all) to 10 (completely).

As in Experiment 1, participants in the bonus condition in this follow-up experiment recalled spending significantly more of their tax rebate ($M = 77\%$) than those in the rebate condition ($M = 41\%$), $t(98) = 4.54$, $p < 0.05$, $d = 0.92$. More important, Table 1 shows that participants in the bonus condition were also more likely to report perceiving the check as an objective gain—as “‘extra income’”—and less likely to perceive it as a return to a previous wealth state, compared to participants in the rebate condition, F for interaction $(1, 98) = 7.28$, $p < 0.001$, $\eta^2 = 0.16$.

To examine whether these differences in the construal of the tax rebate mediated the impact of framing on recalled spending and saving, we followed the mediational analysis procedure outlined by Baron and Kenny (1986). In this analysis, framing condition (bonus versus rebate frame) served as the independent variable, percentage of rebate recalled as saved served as the dependent variable, and the difference between the extent to which participants considered the rebate as returned versus additional income served as the mediator. As can be seen in Figure 1, the framing condition was significantly related to recalled savings, and including the construal mediator in this regression significantly reduced the effect of the framing condition on recalled saving, Sobel's $z = 2.36$, $p = 0.02$. These results suggest that the construal of the rebate significantly mediated the effect of framing on recalled saving. Note, however, that the effect of experimental condition on recalled spending remained significant even when including the proposed mediator in the model,

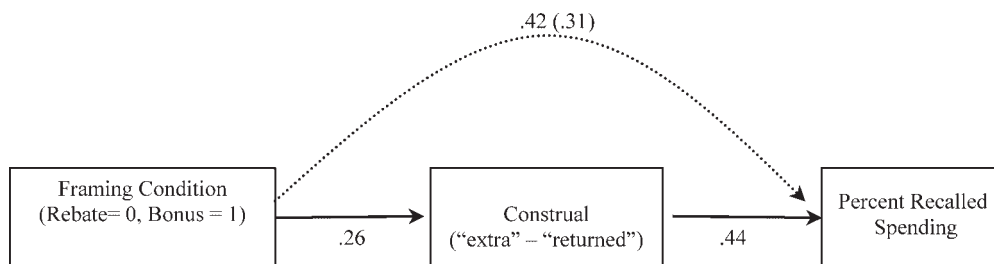


Figure 1. Path analysis for the relationship between framing condition, construal of tax rebate check, and percentage recalled spending (Experiment 1 follow-up). *Note:* Coefficients are standardized betas. The mediator “construal” is the simple difference between the amount participants perceived the rebate check to be “‘extra money received in addition to whatever income you would normally make” and the amount participants perceived the check to be “‘returned money that is now being given back to you.” The coefficient in parentheses is the direct relationship between the independent variable (Framing condition) and the dependent variable (Percent recalled spending), controlling for the proposed mediator (Construal). All $ps < 0.01$.

demonstrating that our measures of construal only partially mediated the impact on recalled saving. Participants in the bonus condition recalled spending more of this income, at least in part, because the bonus frame implied an objective gain in one's overall wealth that the rebate frame did not.

These results are important because they demonstrate generality in the impact of income framing on how people treat income. The theory we are proposing to explain how income framing may alter consumption influences how people construe or perceive their income, and therefore applies equally well to the constructive process of memory that guides recalled behavior as it does to the constructive process of preference creation that guides actual behavior. The follow-up to Experiment 1 also provides evidence that the perceived change from one's current wealth state implied in the description of income—either as a gain from one's current wealth state or as a return to a previous wealth state—may be an important determinant of how a person perceives or construes his or her income. Finally, Experiment 1 suggests at least one major public policy campaign aimed at altering consumption—namely a broad-based tax rebate—may be open to the influence of income framing. If the results obtained on recalled spending in Experiment 1 were matched by similar changes in actual spending, the public policy ramifications would be substantial indeed. Of course, Experiment 1 investigated only recalled spending rather than actual spending, and thus the implications of these results alone are quite limited. The following experiments expand on these results by measuring, in a variety of different ways, the impact of income framing on the actual spending and saving of a financial windfall.

EXPERIMENT 2—REPORTED SPENDING

As anyone with a watchful spouse can attest, having someone monitor the way income is spent can significantly alter how that income is actually spent. People may spend income in ways that they think observers will find appropriate, acceptable, or desirable, rather than spending their income as they might privately. This creates a tension in how to examine the influence of income framing on spending and saving without unintentionally altering that spending and saving. The following three experiments therefore represent a range of methods for monitoring spending and saving, ranging from no known monitoring (Experiment 2), to more moderate monitoring (Experiment 3), to complete monitoring (Experiment 4).

Experiment 2 was designed to measure spending and saving in a context relatively free from the potentially contaminating influence of such monitoring by providing people with a \$50 windfall described as either a gain from their current level of wealth (a “bonus”) or a return to a previous wealth state (a “rebate”) and unexpectedly asking people to report on their spending and saving of that income 1 week later. As with the tax rebate examined in Experiment 1, participants in Experiment 2 were free to spend this windfall on anything, and in any way, they chose. Although objectively identical, we predicted that those given \$50 described as a gain from one's current wealth state (as a bonus) would report spending more of their windfall than those given \$50 described as a return to a previous wealth state (as a rebate).

Method

Forty-seven undergraduates at Harvard University were recruited for an experiment investigating “how undergraduates allocate financial resources.” Upon arrival to the laboratory, participants learned that they were going to receive a \$50 check as part of the experiment. All participants were told that this check was coming from a laboratory that—like most—was partially funded by students' tuition dollars through the university's operating budget. Participants in the rebate condition were then told that “you are receiving this *tuition rebate* because our lab has a surplus of funds,” that “we will contact you in one week to ask you some questions about your *tuition rebate*,” and that they should ask the experimenter “if they have any

Table 2. Self-reported spending and saving of a \$50 windfall by participants in the bonus and rebate conditions (Experiments 2 and 3)

	Framing condition		<i>t, p</i>
	“Bonus”	“Rebate”	
Experiment 2—Recalled behavior			
Dollars spent	\$22.04	\$9.55	2.16, 0.02
Dollars saved	\$27.96	\$40.45	
Experiment 3—Reported behavior			
Dollars spent	\$31.46	\$7.41	3.96, <0.0001
Dollars saved	\$18.54	\$42.59	

questions about this *tuition rebate*.” All instructions were identical for participants in the bonus condition, except that all three instances of “tuition rebate” were replaced with “bonus income.” Participants then received their check and left.

One week after this initial session, participants were contacted by e-mail and asked to indicate how much of the \$50 they had saved and how much they had spent. No framing manipulation was used in this follow-up e-mail at any point. All participants in the initial session responded to the e-mail and completed these dependent measures.

Results

Table 2 shows that participants reported spending more of the \$50 windfall when it was described as a bonus than when it was described as a rebate, $t(45) = 2.16, p < 0.05, d = 0.64$. In fact, 73% of participants in the rebate condition reported spending none of their \$50 check, compared to only 36% in the bonus condition, $\chi^2(1) = 6.34, p < 0.05$. These results support our hypothesis that the subjective perception of changes in wealth is an important determinant in financial decision making. The ecological validity of this study is high because participants were given real money (rather than making hypothetical predictions of spending and saving), received no restrictions on the use of their money in any way, and were not forewarned about the dependent measures that would be asked in the follow-up questionnaire. Indeed, the experimental situation was designed to be as similar as possible to the act of receiving any rebate or bonus check in daily life.

EXPERIMENT 3—ACCOUNTED SPENDING

The ecologically desirable aspects of Experiment 2 came at the cost of having to rely on recalled spending rather than actual spending. It is at least possible that participants simply reported spending different amounts of money, or recalled spending different amounts of money as in Experiment 1, without any differences in actual spending. Experiment 3 was therefore a more tightly controlled version of Experiment 2 designed to obtain a potentially more accurate estimate of actual spending while still providing participants the opportunity to spend their income in any way, and on whatever items in daily life they wished.

In particular, participants in Experiment 3 received the same framing manipulation as in Experiment 2, but instead of recalling spending and saving 1 week later, these participants received a small accounting slip onto which they were asked to record any use of the unexpected windfall. These slips were mailed back at the end of 1 week and all activities listed on the accounting slip were coded as either instances of spending (e.g., making a purchase) or saving (e.g., placing the money in one’s desk drawer). As in Experiments 1 and 2, we predicted that those who received income described as a gain from the *status quo* (i.e., as a bonus) would spend more than those who receive income described as a return to the *status quo* (i.e., as a rebate).

Method

Forty-four Harvard undergraduates were recruited for an experiment investigating “how undergraduates allocate financial resources.” These participants experienced the same procedure as in Experiment 2 except that they received a wallet-sized accounting slip and a return envelope along with their \$50 “bonus” or “rebate” check. This slip included three column headings—“date,” “event,” and “amount”—with a series of lines beneath. No other information was on the accounting slip. Participants were instructed to write a new entry every time they utilized the money in any way (including depositing it in a bank). To ensure that these instructions were clear, the experimenter completed the first line on the accounting slip to document receipt of the check. Participants were instructed to return the accounting slip in the addressed envelope in 1 week or when the money had been entirely spent, whichever came first.

Results

Nearly all participants (83%) returned their accounting slip in the specified time, and the remaining did so within the following week after an e-mail reminder from the experimenter. Excluding participants who returned their slips late does not alter the following analyses in any meaningful way, and we therefore retained all participants for the sake of completeness.

Two raters unaware of the participants’ condition coded any unspent or deposited portion of the \$50 check as saved, and all remaining purchases were coded as spent. This task was quite simple, and ratings were in complete agreement. Consistent with our predictions, the bottom half of Table 2 shows that participants spent more of their \$50 windfall when it was described as a bonus than when it was described as a rebate, $t(42) = 3.96, p < 0.0001, d = 1.22$. Overall, 75% of participants saved their \$50 check in the rebate condition compared to only 21% in the bonus condition, $\chi^2(1, N = 44) = 12.91, p < 0.001$.

These follow-up results provide further evidence that income framing can substantially influence self-reported spending and saving. These results are not as prone to potential memory distortions of behavior as in Experiment 2, as participants were asked to record their behavior at the time it happened rather than recalling it afterwards. Notice that these results are also not open to an alternative interpretation based on a reframing of behavior at the time participants reported their behavior. In Experiments 1 and 2, participants were asked to indicate what percentage of income they spent versus saved. It is possible—and virtually certain for at least some—that participants were not recalling specific instances of past behavior but reconstructing their behavior at the time of judgment. Participants in Experiment 3, however, were not asked to report on instances specifically coded as spending or saving *per se*, but instead to simply report their behavior whenever they used their unexpected income for any purpose. Spending and saving was instead coded by independent raters, rendering any reframing of behavior by participants at the time of judgment relatively unlikely.

EXPERIMENT 4—RECORDED SPENDING

Experiments 2 and 3 investigated the impact of income framing on spending and saving under conditions that maximized, as much as possible, the ecological validity of behavior. The benefits of this approach are that participants did not know their behavior was going to be measured (Experiment 2), did not know exactly how their behavior was going to be coded (Experiment 3), and were not constrained on what they could actually purchase with their income (both Experiments 2 and 3). This concern for ecological validity, although desirable for some purposes, required reliance on self-reported behavior rather than on actual behavior. It is at least possible that the differences observed in the preceding experiments were produced by biases in self-reporting of spending and saving rather than on differences in actual spending and saving. Experiment 4 remedies this issue by designing a laboratory procedure to monitor actual spending directly rather than reported spending.

In addition, Experiment 4 was designed to expand on the previous experiments by altering the method by which participants received their unexpected income. Participants in the preceding experiments received their income in the form of a personal check, which meant that spending this income required a visit to the bank before any spending was possible. A check seemed desirable in order to mimic the contexts in which people normally receive rebates and bonuses, but it is at least possible that the method of payment may interact with the framing of income. To increase generalizability, Experiment 4 paid participants in the equivalent of cash by providing them with an account that could be spent immediately on items in the laboratory, rather than in the form of a check that must be cashed before it could be spent.

Finally, Experiment 4 investigates a potential confound in Experiments 2 and 3. In particular, although all participants believed that their unexpected income was coming from the university's operating budget funded by tuition dollars, the critical words in the framing manipulation were either a "tuition rebate" or "bonus income." This manipulation therefore confounded "rebate" and "bonus" with "tuition" and "income." It is at least conceivable that this latter manipulation may have also influenced participants' spending, and these terms were therefore dropped in the following studies and replaced by "rebate money" or "bonus money" to isolate the key framing manipulation.

Method

Twenty-five Harvard undergraduates received a \$25 windfall framed either as "bonus money" or "rebate money" using a procedure otherwise similar to Experiments 2 and 3. Participants were again informed that the experiment was investigating "how undergraduates allocate financial resources," but instead of receiving a check for the full \$25 amount, participants were instead told that they could choose to spend any amount of their income on items available in the "lab store." Participants were then shown an array of 15 different items displayed in a bookcase, labeled with their sale prices. These items consisting primarily of university memorabilia (e.g., mugs, pens, ID holders) and snack foods (e.g., soda, potato chips), priced between \$0.60 and \$13. Participants were told that these items were being sold at a 20% discount from the Harvard COOP (the main university bookstore), that they could spend as much of their \$25 income as they wished, and that any unspent portion would be given to them as a personal check. Participants then indicated how much they wished to spend, purchased their items, and received a personal check from the experimenter for any unspent amount.

Result

As predicted, Table 3 shows that participants spent more of their income when it was described as "bonus money" than when it was described as "rebate money," $t(23) = 3.08, p < 0.01, d = 1.28$. Participants were generally inclined to save their money, but this was especially true in the rebate condition—79% of participants in the rebate condition saved all of their \$25 income compared to only 16% in the bonus condition, $\chi^2(1, N = 25) = 7.04, p < 0.001$.

These results are consistent with our major hypothesis, and demonstrate that income framing can influence actual spending and saving in a more restricted laboratory context involving a cash-like payment rather than

Table 3. Actual spending and saving of a \$25 windfall by participants in the bonus and rebate conditions (Experiment 4)

	Framing condition		<i>t, p</i>
	"Bonus"	"Rebate"	
Behavior			
Dollars spent	\$11.16	\$2.43	3.08, 0.005
Dollars saved	\$13.84	\$22.57	

Table 4. Actual spending and saving, and answers to subsequent construal items, among participants in the bonus and rebate conditions (Experiment 4 follow-up)

	Framing condition		<i>t</i> , <i>p</i>
	“Bonus”	“Rebate”	
Behavior—Actual spending			
Dollars spent	\$7.76	\$1.63	3.85, <0.001
Dollars saved	\$17.24	\$23.37	
Evaluations			
Perceived as “extra income”	8.25	6.68	2.48, 0.02
Perceived as “returned income”	2.65	4.21	1.88, 0.08
Money seemed like large versus small amount	6.60	5.84	1.17, 0.25
Objects seemed correctly priced	5.40	5.21	0.26, 0.79
Objects seemed like a good value	4.85	5.16	0.41, 0.68
Amount of spending this month compared to average	6.42	7.05	0.93, 0.35
Have recently been spending money wisely	5.25	5.85	0.39, 0.70

Note: Responses to all items listed under “evaluations” were made on 0–10 point scales, with the midpoint labeled as an appropriate neutral response, 0 labeled as an appropriate low or negative response (e.g., money seemed like a small amount, or objects seemed like a poor value), and 10 labeled as an appropriate high or positive response (e.g., money seemed like a large amount, or objects seemed like a good value).

as a personal check. These results do not, however, address the psychological mechanisms underlying this difference. To do so, we conducted a follow-up experiment with 39 Harvard undergraduates to examine both our proposed mechanism as well as several alternative interpretations. This procedure was identical to Experiment 4, except that participants were asked a series of additional questions listed in Table 4. Responses to these additional items were all made on 11-point scales ranging from 0 (not at all) to 10 (completely).

As can also be seen in Table 3, participants in the bonus condition of this follow-up experiment again spent more of their income than participants in the rebate condition, $t(37) = 3.85$, $p < 0.001$, $d = 1.27$. We propose that this spending difference occurred because the income framing led “bonus” participants to construe their income as extra income that can be spent (because it implies an objective gain from the *status quo*), but led “rebate” participants to construe their income as returned income and therefore no additional income to be spent (because it implies a return to a previous wealth state). To test this possibility, participants were asked to indicate the extent to which they perceived the rebate as a gain from their current wealth—as “extra” money that you received in addition to whatever you would normally make this month—and the extent to which they perceived the rebate as a return to a previous wealth state—as “money belonging to your original income that was temporarily withheld, and which is now being given back to you.” Consistent with our predictions and the follow-up to Experiment 1, Table 4 shows that participants in the bonus condition were significantly more likely to report viewing the income as a gain from their current wealth than participants in the rebate condition. Participants in the bonus condition were marginally less likely, however, to report viewing the money as a return to a previous wealth state—as “returned money that is now being given back to you,” F for interaction $(1, 37) = 6.94$, $p < 0.01$, $\eta^2 = 0.16$.

To examine whether these differences in the construal of the \$25 windfall mediated the effect of experimental condition on recalled spending and saving, we followed the mediational analysis procedure outlined by Baron and Kenny (1986). In this analysis, experimental condition (bonus versus rebate frame) served as the independent variable, percentage of windfall saved served as the dependent variable, and the difference between the extent to which participants considered the rebate as returned versus additional income served as the mediator. As can be seen in Figure 2, the experimental condition was significantly related to

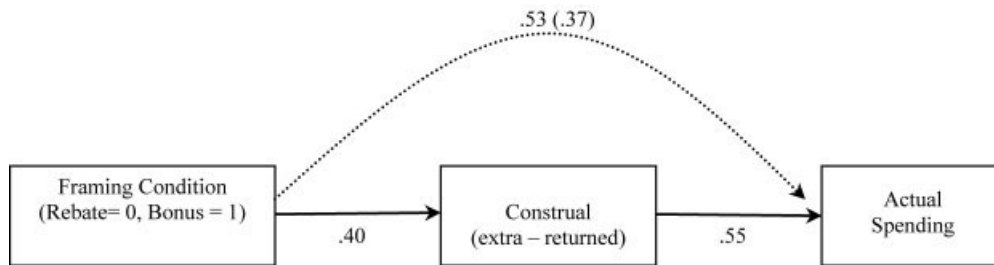


Figure 2. Path analysis for the relationship between framing condition, construal of \$25 laboratory windfall, and actual spending (Experiment 4 follow-up). *Note:* Coefficients are standardized betas. The mediator “construal” is the simple difference between the amount participants perceived the rebate check to be “extra money received in addition to whatever income you would normally make” and the amount participants perceived the check to be “returned money that is now being given back to you.” The coefficient in parentheses is the direct relationship between the independent variable (Framing condition) and the dependent variable (Actual spending), controlling for the proposed mediator (Construal). All $ps < 0.01$.

recalled savings, and including the construal mediator in this regression significantly reduced the effect of experimental condition on recalled saving, Sobel’s $z = 2.20$, $p = 0.03$. These results demonstrate that the construal of the rebate significantly mediated the effect of framing on recalled saving. As in the follow-up to Experiment 1, the effect of experimental condition on behavior remained significant even when the construal mediator was included in the regression model, demonstrating that our measure of construal was a partial mediator.

The remaining questions in this follow-up experiment examined two additional mechanisms that could potentially explain the impact of income framing on participants’ spending and saving. First, it is possible that participants in the rebate condition were also less likely to spend income because a returned loss is perceived as more valuable, in a subjective sense, than an additional gain (making the rebate seem subjectively larger), or making the objects in the array seem relatively overpriced given the income’s value. Such a result would be consistent with the asymmetrical gain/loss weighting function from the evaluation phase of Prospect Theory (Kahneman & Tversky, 1979), in which a returned loss should be seen as more valuable than a simple gain. Such a result would also be consistent with the results of Gregory, Lichtenstein, & MacGregor (1993), who found that participants predicted—in a hypothetical scenario—that they would be willing to pay more money for public policies framed as returned losses (e.g., restoring a wetland) than as gains (e.g., creating a wetland). However, Table 4 shows that this prediction was not supported. There were no significant differences in the extent to which the amount of money seemed like a large or a small amount, in the extent to which the objects seemed appropriately priced, or in the extent to which the objects seemed like a good value, all $ps > 0.25$. We suspect this occurred because the objective value of the income and the objective discount on the items for purchase were so transparent that they allowed for little ambiguity in the subjective value of these goods. This mechanism may play a more significant role in cases when the objective value of a good is more ambiguous.

Second, it is also possible that returning money to participants in the form of a rebate highlighted past expenditures that a bonus does not. One has to have spent money, after all, in order to receive a rebate. Highlighting this earlier expenditure may have made participants feel like they have been spending too much money recently, or have been spending their money unwisely and therefore need to spend less (or spend more). However, Table 4 shows that these hypotheses were also not supported. There were no differences in the amount participants reported spending this month compared to the average month, nor in the extent to which participants reported that they have been spending their money wisely, all $ps > 0.35$.

GENERAL DISCUSSION

Few monetary decisions are more basic than whether to spend or save one's income, and these experiments suggest that subtle framings may have a substantial and dramatic effect on such decisions. In a series of experiments, income received either from the US government or from a laboratory fund was saved more readily when it was described as returned income than as bonus income. This occurred in both field (Experiment 1) and varying laboratory contexts (Experiments 2, 3, and 4), influenced both actual spending (Experiment 4) as well as memory for spending (Experiment 1), using both cash payments (Experiment 4) as well as checks (Experiments 1, 2, and 3). In each experiment, the unexpected income came from the same objective account (government taxes or a previous tuition expense), involved objectively identical dollars across experimental conditions, and utilized dollar figures that would be unlikely to be seen as trivial to our participants.

It is worth noting that the magnitude of observed effects in each of these experiments was substantial. The effect size (d) of the basic framing manipulation ranged across all of the studies from a low of 0.6 in Experiment 2, to a high of 1.42 in Experiment 1. The accepted categorization for a medium effect size is 0.5 and for a large effect size is 0.8, meaning that all but one experiment produced a large effect size, and four of the six produced an effect size larger than 1. Income framing in this context not only has a significant effect on behavior, it had a sizeable effect as well.

The main focus of this research was on the existence of this income framing effect, but these experiments also investigated several explanations for its existence. We suggested an explanation based on comparisons with salient reference points, such that income described as a positive departure from the *status quo* would be perceived as a gain, whereas income described as a return to the *status quo* would be perceived as a return to a previous wealth state, and therefore as no overall gain in one's general wealth. Consistent with this account, participants in follow-ups to Experiments 1 and 4 were more likely to perceive their income as a gain (i.e., as "extra income") when it was described as a bonus than when it was described as a rebate. What is more, the extent to which participants perceived their unexpected income as "extra income" versus "returned income" predicted overall spending, and accounted for a significant percentage of the variability from the framing manipulation. These two experiments also assessed two plausible alternatives to this mechanism, one from the gain/loss asymmetry from Prospect Theory and the other based on memory priming, but found no support for either.

These results bear an obvious resemblance to research on mental accounting and mental budgeting, which demonstrates that the source of a financial account can influence consumers' spending and saving (Heath & Soll, 1996; Thaler, 1999). A person may not be inclined, for instance, to book an extravagant vacation using funds from a relative's inheritance, but may feel more than justified doing so with one's casino earnings. Notice, however, that the income or windfalls received in all of the present experiments came from exactly the same source or financial accounts. All income in Experiment 1 came from the Federal Government, whereas all income in Experiment 2–4 came indirectly from students' tuition. Previous demonstrations of difference in the marginal propensity to consume income from different financial accounts do not therefore provide a compelling explanation of the present experiments.

This does not mean, however, that additional mechanisms are not operating in the framing effects we demonstrated. Indeed, the mediational measures we included in follow-ups to Experiments 1 and 4 failed to account for all of the variance associated with the income framing effect, which could be either because of insensitive mediational measures or because of additional unmeasured mediators. It is possible, for instance, that unexpected income described as a positive departure from the *status quo* may be coded as more frivolous income or as a gift, and therefore be spent more readily. People report being more likely, for example, to purchase a vacation when they receive additional money as a gift than when they receive additional money from their employer (Henderson & Peterson, 1992).

It is also possible that income described as a rebate was more readily coded as part of one's regular income than income described as a bonus. Even though the income in the present experiments was an unexpected

windfall for everyone, it may have retained this windfall status more easily when described as a bonus than when described as a rebate, and hence be spent more readily (Thaler, 1999). Of course, these speculative mechanisms would not be inconsistent with the account we have offered, but would supplement them. Given the magnitude of effects observed in the present experiments, we find the possibility of multiple mechanisms governing these large framing effects quite likely, and the opportunity for future research investigating the interplay and unique contributions of mechanisms quite promising.

Implications

The results of these experiments join a growing body of evidence demonstrating the importance of incorporating basic psychological principles into economic behavior and decision making (e.g., Ariely, Loewenstein, & Prelec, 2003; Camerer, 1999; Kahneman, 2003; Thaler & Benartzi, 2004). One of psychology's most basic insights is that the evaluation of objects is based heavily on descriptions of objects rather than simply on objective features of the objects themselves (Tversky & Koehler, 1994). Different descriptions of the same objective events can therefore yield very different judgments, sometimes—as in this case—contradicting very basic assumptions of traditional economic models.

Although this manuscript has focused exclusively on income framing, analogous framing effects are likely to arise in other contexts as well. Consider, for example, the increased concern that would seem to arise from a 10% increase in the likelihood of a national terrorist attack compared to a 10% return to last month's level of risk. Or differences in the likelihood of changing one's diet after gaining 10 pounds over the last month compared to regaining ten pounds. Or the increased likelihood of "wasting" one's time if a regularly-scheduled meeting is canceled adding an "extra" hour to one's normal work day compared to a 1-time meeting being canceled that simply "returned" an hour to one's work day. From calculating risk to consuming time, the ability to frame events as a change from one's current state versus a return to a previous state seems common, and the empirical extensions of the current findings may therefore be quite broad.

Finally, we believe these experiments may have important economic policy implications. The tax rebates that were part of the 2001 "Economic Growth and Tax Relief Reconciliation Act" described in the introduction represent a non-trivial economic strategy—a \$38 billion strategy, in fact—to influence everyday spending and improve the national economy. Although returning tax dollars is nowhere near as common an economic strategy as collecting tax dollars, rebates are a well-known tool used by governments the world over (and businesses as well) to influence economic activity. Our research suggests that the framing of these windfalls may have a dramatic effect on their overall economic impact.

Our research suggests that at least some of the common rhetoric surrounding tax rebates may ironically reduce their effectiveness as a short-term economic stimulus. Many clarion calls for decreased government spending and economic growth herald the need to "return the tax-payer's money," and President Bush described the rebates of 2001 as a budgetary surplus that "should be returned to the taxpayers who earned it" because "it's the people's money and government ought to be passing it back after it's met priorities" ("Transcript . . .", 2001). This kind of rhetoric, and the framing of this rebate as returned income that it naturally produced (Epley & Gneezy, in press), might help to account for the high rate of savings following these particular tax rebates (Shapiro & Slemrod, 2003a).

Although the impact of such large-scale public policies are obviously multiply determined, the experiments reported here suggest the natural and marketed framing of these tax rebates as returned income may diminish the likelihood that they are spent compared to other financial windfalls, and therefore diminish the likelihood of realizing their full potential as a short-term economic stimulus. The effect sizes observed in the present experiments suggest that altering the framing of these public policies to encourage a different kind of construal could have a profound impact on constituents' behavior. Given the amount of money involved in these public policy initiatives and the impact that even fairly small effects could have on the

population at large, greater care and attention devoted to how decision makers code windfalls would seem to be time well spent.

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