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Nudges that hurt those already hurting – distributional and unintended effects of salience nudges*

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ABSTRACT

Nudges are becoming increasingly popular policy tools. Yet, distributional effects of nudges are largely unknown. We first design an economic laboratory experiment to examine the incidence of an opportunity cost reminder nudge (a salience nudge) designed to curb spending, while accounting for heterogeneity in emotional responses – specifically the pain of paying. Pain of paying is optimal for 'unconflicted' consumers, but too low for 'spendthrifts' and too high for 'tightwads', causing sub-optimal spending. Our empirical results imply the nudge increases pain of paying for tightwads, thereby reducing spending by tightwads, who already spend too little, while it entirely fails to reduce the spending of those who would have benefited from a spending reduction (spendthrifts). Overall, the nudge therefore might reduce consumer welfare. We next examine if the adverse impact of the opportunity cost reminder nudge is explained by a general tendency for all nudges to exacerbate peoples' underlying spending preferences. We specifically test whether a salience nudge designed to boost spending correspondingly adversely affects spendthrifts? We unexpectedly find that subjects perceive the spending booster nudge as a "spending reminder", which again, reduces spending by tightwads only, while not affecting spending by the other consumer types. Our results highlight two important aspects of salience nudges – given the complexity of consumer emotions and information processing, salience nudges can have undesired welfare effects, and the direction of their impact may be the opposite of what was intended.

1. Introduction

Although many emotions impact decision-making (e.g. Loewenstein, 2000; Schwarz, 2000; Bosman and Van Winden, 2002; Fehr and Gächter, 2002; Sanfey et al., 2003; Knutson et al., 2007; Pfister and Böhm, 2008; Coricelli et al., 2010; Cubitt et al., 2011; Jordan et al., 2015), the pain felt when spending money may be particularly important to consumer behavior (Loewenstein and O' Donoghue, 2006). Pain of paying may help in making responsible economic decisions, in that it acts as a proxy for opportunity costs (Prelec and Loewenstein, 1998; Loewenstein and O' Donoghue, 2006; Rick, 2013).[1]

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[1] The original pain of paying theory proposed by Prelec and Loewenstein (1998) was derived from mental accounting theory, which posits direct or immediate utility from transactions, in addition to the indirect utility of the transaction that is derived from consumption itself (Thaler, 1985, 1999). Hoelzl et al. (2009) and Kamleitner et al. (2010) find that consumer attitudes toward loan repayment are consistent with the pain of paying model.

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However, for some people, the pain of paying is suboptimal. Rick et al. (2008) develop a scale that identifies subgroups of people who experience either too little pain, leading to too much spending for the individual's own liking ('spendthrifts'), or too much pain, leading to too little spending ('tightwads'). People with helpful levels of pain of paying are referred to as 'unconflicted'.[2] Rick et al. (2011) tested, and found, that spendthrifts and tightwads are indeed more unsatisfied with their own spending behavior, than are unconflicted consumers.

Suboptimal spending has shaped public policy. For instance, spendthriftiness has been explicitly targeted by 'spendthrift trusts', institutionalized to protect a beneficiary from spending too much. Some states even allow self-settled trusts, where a beneficiary protects herself from overspending (Hirsch, 1995). In addition, both tax deductions on future spending and default nudges for retirement savings are policies designed to reduce spending today. Of particular interest to this study, though, are policies of the 'one-size-fits-all' -type: specifically nudges that increase the salience of costs to spending today, i.e., opportunity cost reminder nudges. Nudges are becoming increasingly popular as part of public policy and opportunity cost reminder nudges in particular have been adopted and studied, for example, in the context of paying delinquent taxes and fines (Hallsworth et al., 2014; Haynes et al., 2013), repaying loans (Cadena and Schoar, 2011), meeting savings goals or commitments (Karlan et al., 2016), and taking prescription medicines (Pop-Eleches et al., 2011).[3] Such nudges are typically justified by the assumption that consumers focus too little of their limited attention on opportunity costs, causing them to over spend. All of these policies focus on increasing welfare by reducing over consumption, with the goal of increasing the welfare of people who overspend. If such policies target the whole population, they may decrease welfare for those who already underspend.

Sunstein (2016) states undesired distributional effects might weaken the argument for a nudge (p.179). Roberts (2018) goes further and argues it is the government's ethical responsibility to consider distributional effects of nudges. Despite their importance, distributional effects of salience nudges are largely unknown. This might be partly due to the standard assumption in social sciences that the impact on consumers of information is non-negative – more information is typically thought to benefit consumers by allowing them to better align decisions with preferences. However, theoretical and empirical studies show how people may benefit from less information if the information causes negative emotions (e.g. Köszegi, 2003; Dana et al., 2007; Oster et al., 2013; Grossman, 2014; Onwezen and van der Weele, 2016; Thunström et al., 2016; Gigerenzer and Garcia-Retamero, 2017; Grossman and van der Weele, 2017). Also, informational nudges have been referred to as "emotional taxes", due to the negative emotions (e.g. pain, anxiety, fear, guilt) they may evoke (e.g. Glaeser, 2006; Loewenstein and O'Donoghue, 2006). Allcott and Kessler (2015) examined welfare effects of home energy conservation reports, suggesting such reports may impose moral costs on consumers. They found considerable heterogeneity in consumer welfare impact from the reports, ranging from positive to negative. Like any tax, emotions may be helpful in steering consumption to optimal levels. However, if emotions evoked by nudges are suboptimal, consumers will fail to maximize utility.[4]

In this study, our primary objective is to examine the impact on spending of an opportunity cost reminder nudge. The intent of the opportunity cost reminder nudge is to focus consumers' attention on the opportunity cost of their spending. If the nudge focuses consumer attention on their true opportunity cost, it benefits tightwads and spendthrifts alike. However, if the nudge instead focuses consumer attention on their pain of paying (the imperfect proxy for their true opportunity cost), it may instead decrease their welfare by further distorting spending.

It is an open question how opportunity cost reminder nudges impact consumer spending in an incentivized context, where consumers make actual spending decisions. Based on hypothetical consumer choices, Frederick et al. (2009) find that an opportunity cost reminder nudge reduces spending intentions by spendthrifts, suggesting the nudge focuses consumer attention on the true opportunity cost of their spending, rather than exacerbate emotions from payments. In an incentivized setting, Thomas et al. (2011) find that cash payments are more painful than credit card payments, and find cash payments exacerbate pain of paying for tightwads, but not for others.[5] These studies imply that certain instruments (nudges or modes of payment) may exacerbate intrinsic pain of paying, thereby distorting spending, while others might be helpful. Another implication is that responses to instruments that impact spending might be context dependent. Context dependency is well documented along the dimension of hypothetical versus real choices (see Ajzen et al., 2004). This dimension might be

[2] Note that tightwaddism/spendthriftiness is distinctly different from self-control. As discussed in Rick et al. (2008), tightwads and spendthrifts may have similar levels of self-control, since both groups have problems with self-regulation. Both tightwads and spendthrifts believe they would benefit from revising their spending, but lack sufficient self-regulation to overcome their suboptimal spending.

[3] See Ericson (2014) for a detailed model, and Gilbert and Zivin (2014), Grubb and Osborne (2014) and Grubb (2015) for an empirical and theoretical examination in the context of household electricity and mobile phone usage. The lack of attention to one's opportunity costs is also a critical feature of the tax salience literature which often argues that obscuring opportunity costs can increase consumer spending, and by extension, tax revenue (Chetty et al., 2009). Further, opportunity cost reminders (often also referred to as a 'budget reminders') were recommended for contingent valuation studies by the NOAA Panel on Contingent Valuation (Arrow et al., 1993), and has since become common practice in contingent valuation studies.

[4] Previous studies have examined heterogeneity in responses to other informational nudges designed to redirect consumer behavior. Alcott (2011) finds the response to information on positional energy usage, relative to that of one's neighbors, depends on a consumer's original position. Beshears et al. (2015) find heterogeneity in responses to peer information on retirement savings, over nonunionized and unionized recipients. Ho et al. (2016) find that informational nudges on green electricity have a stronger effect on intrinsically pro-social consumers. Our study differs from this literature by focusing on heterogeneity in the impact of a nudge over consumer groups who might be adversely affected.

[5] Fusaro (2013) and Khan et al. (2015) provide further evidence that usage of, and attitudes toward different modes of payment are consistent with the predictions with regards to pain of paying (for further details, see also Prelec and Loewenstein, 1998; Soman, 2001; and Raghubir and Srivastava, 2008).

particularly important when consumers are conflicted about their behavior, as is the case for tightwads and spendthrifts. Both of these groups under- and overspend, respectively, compared to some inner benchmark – they themselves are unsatisfied with their actual spending levels (Rick et al., 2008). Dittmar and Drury (2000) and Rick et al. (2011) provide empirical support of tightwads and spendthrifts feeling conflicted (unsatisfied) about their spending behavior. Specifically, what they think they "should" do (base decisions on their true opportunity cost) differs from what they actually do (base decisions on an imperfect proxy for the opportunity cost, namely suboptimal pain of paying levels). Given stated behavior (e.g., purchase intentions) is costless, stated behavior may be particularly affected by what one thinks one "should" do, making it an unreliable predictor of actual behavior. It is therefore important to examine the impact of a spending reminder nudge in an incentivized setting that measures actual behavior.

To perform our analysis, we design a laboratory experiment to examine how consumers with different levels of pain from paying adjust their consumption from a nudge that reminds people of opportunity costs (an 'opportunity cost reminder'). Subjects are endowed with money and a generic product, and bid on the option to upgrade to a premium (locally produced) product. Subjects either receive an opportunity cost reminder or not. We define subjects as tightwads, unconflicted and spendthrifts using the scale developed by Rick et al. (2008).

We find that the opportunity cost reminder only reduces spending by consumers who already feel too much pain from spending (tightwads), while doing nothing to reduce spending by those who feel too little pain from spending (spendthrifts). Hence, our results suggest the nudge only focuses attention on pain of paying for those pre-disposed to feel too much pain of paying, while it entirely fails to target those who would benefit from increased pain of paying (the spendthrifts). The nudge therefore might have undesired consumer welfare effects. Our results suggest that a nudge designed to curb spending may adversely affect those who already are inclined to spend too little, while they do nothing to help those who would benefit from spending reductions.[6]

A natural follow-up question is if nudges designed to boost spending similarly only increase spending by those who are already inclined to spend too much?[7] To answer this question, our secondary research objective is to determine the impact of a nudge designed to encourage spending – a "spending booster nudge". For instance, a nudge aimed to boost spending can be part of expansionary fiscal policy, i.e., a substitute or complement to other policies, such as tax cuts and tax rebates. As a means to encourage spending, we focus subjects on the benefits of 'buying local'. Surprisingly, we find that the spending booster nudge impacts spending in *the same direction* as does the spending reminder nudges – it *reduces* spending by tightwads, while we find no evidence of increased spending by any other consumer group, including spendthrifts. Our finding suggests the spending booster nudge in our experiment predominantly focuses consumer attention on the pain of spending, rather than the benefits of their spending. These results relate to information noticing and learning (Hanna et al., 2014). Marreiros et al. (2017) find similar effects from information messages on companies' privacy practices. Both a positive and a negative message about companies' privacy practices reduce consumer willingness to self-disclose private information, compared to a neutral information condition – consumers react to the mere mention of privacy issues. Further, Wilson et al. (2015) find a salience nudge to encourage low-calorie milk choices fails to focus consumer attention on low-calorie milk, it instead focuses consumer attention on milk in general and increases average consumption of *both* low- and high-calorie milk.

Our results highlight two important features of salience nudges that focus consumer attention on spending. First, consumers' responses to these nudges may exacerbate consumption distortions, rather than reduce such distortions. Our study underscores the importance of examining distributional effects of salience nudges. For instance, although an opportunity cost reminder nudge might reduce spending, it might entirely fail to target those who benefit from spending reductions, thereby reducing overall consumer welfare. Second, given the complexity of consumer information processing, both the incidence and the direction of the impact of the salience nudge can be the opposite of what was intended. This is shown particularly by the response to the spending booster nudge in our experiment.

The structure of the paper is as follows. In Section 2, we describe the experimental design and data collection. Section 3 presents the empirical results, and Section 4 concludes.

2. Experimental design and data

Below, we design an experiment entailing spending on locally produced honey to empirically test the impact of nudges designed to impact spending on consumers with varying levels of pain of paying.

In total, we recruited 410 subjects from the general student and staff population at the University of Wyoming to participate in an economic laboratory experiment. Experimental sessions took place during the day in a behavioral laboratory and subjects were paid $25 for their participation.

[6] We examined the impact of another type of spending reminder nudge, that might have less of an emotional impact given it does not focus on costs per se – an 'arbitrage reminder' (Cherry et al., 2003). The 'arbitrage reminder' informs consumers that people are often inconsistent in how they value risky choices, given their stated preferences, and that such inconsistencies may be exploited in markets by arbitrage, and this nudge has been found to reduce spending in economic experiments (Cherry et al., 2003; Cherry and Shogren, 2007). We find the incidence of the arbitrage reminder is similar to that of the opportunity cost reminder nudge, but results are statistically weak and mixed (we describe the arbitrage reminder and the results in Appendix C). Our weak and mixed results for the arbitrage reminder suggests, as expected, that it has less impact on pain of paying than does the opportunity cost reminder nudge. It, however, also implies the arbitrage reminder nudge has less impact on spending behavior in general.

[7] We thank an Editor and an anonymous Reviewer for suggesting this informative addition to our analysis.

A wide range of products have been used to elicit WTP (or willingness to accept, WTA) in laboratory studies, such as pens, mugs and candy bars (Kahneman et al., 1990; Shogren et al., 1994; Shogren et al., 2001), tortilla chips (Rousu et al., 2007; Lusk et al., 2001), potatoes (Rousu et al., 2007; Lacey and Huffman, 2016), vegetable oil (Chern et al., 2003; Rousu et al., 2007), meat (Dickinson and Bailey, 2002), lunch sandwiches (Shogren et al., 1994), cookies (Lusk et al., 2004), pork chops (Melton et al., 1996), salmon (Chern et al., 2003), milk (Bernard and Bernard, 2009), honey (Wu et al., 2015), coffee and paper (Sapci et al., 2016), cheese (de-Magistris and Gracia, 2017). Our study is most closely related to Wu et al., who estimate the WTP for locally, domestic and international honey, and find that demand is impacted by consumer knowledge of health risks associated with international honey.

When choosing an experimental product for our study, we used three criteria. First, the product is produced both locally and non-locally, while remaining the same in all other dimensions. The second and third criteria were that both the locally and non-locally produced versions of the product would be accessible for purchase by the research group in the local market, and that the product would be easy to store. Honey meets all three criteria. We could transfer both the local and non-local honey to identical, generic containers, such that there was no difference in appearance between the two honey types. Further, honey has a long shelf life, such that it was easy to store between experimental sessions.

In our experiment, all subjects were physically endowed with an eight-ounce jar of honey of unknown origin. They were then given the option to pay to switch their endowed jar for an eight-ounce jar of locally produced Wyoming honey. Subjects' willingness to pay to switch to a local honey jar was extracted by their participation in a Becker–DeGroot–Marschak (BDM) auction (Becker et al., 1964). The auction price was subtracted from the $25 participation fee for auction winners at the end of the experiment.

To meet our primary research objective, we recruited 341 (of our total of 410) subjects to participate in an experiment where we varied the presence of an opportunity cost salience nudge. The opportunity cost reminder nudge consisted of the following message: '*Remember that the less you spend in this study, the more money you will have for other purchases*'.

Upon starting their participation in the experiment, subjects were randomized into treatments entailing the opportunity cost reminder nudge, and no nudge at all. In all treatments, the time subjects spent deciding on their marginal willingness to pay for the locally produced honey (their BDM auction bid) was recorded. The timing of the experiment was as follows:

1 Subjects were given their jar of honey of unknown origin and verbally, and in writing, given the experimental instructions, which explained the mechanisms of the BDM auction and the importance of bidding their true value of the locally produced honey (see Appendix A).
2 Subjects participated in the study via a link to the survey tool Qualtrics. In the beginning of the study, they participated in a hypothetical practice BDM auction of a Snickers bar, to further ensure their understanding of the BDM auction mechanism.
3 Subjects were randomized into different treatments. They were all given the following message: "*You are participating in an experimental auction of local honey. I.e. you will be asked to state your maximum willingness to pay to switch out your endowed honey of unknown origin for an equal size jar of local honey*."
4 Subjects were given either the opportunity cost reminder nudge or no nudge at all.
5 Subjects participated in the BDM auction and stated their bids to switch their endowed honey for an equal amount of locally produced honey. They could place bids ranging from $0 to $10.
6 Subjects in all treatments proceeded to answer questions used to create the pain of paying (spendthrift) scale, and a set of control variables (e.g., gender, income, importance assigned to locally produced food, importance assigned to food being cheap, how often they buy honey, how often they eat honey).
7 The monitor's bid was drawn from a jar and ranged from $0.01 to $9.99. Specifically, the jar contained 10 pieces of paper, ranging from 0 to 9. The monitor's bid was determined by three consecutive draws from the jar (with replacement) – one draw for the dollar amount of the monitor's bid, one for the amount in ten cents, and one for the amount in one cents. Subjects were informed that the monitor had to draw a positive bid.
8 Subjects who had stated a bid higher than or equal to the monitor's bid switched their endowed jar of honey for an equal amount of locally produced honey, and the monitor's bid was subtracted from their $25 participation fee. Subjects who had stated a bid lower than the monitor's bid kept their endowed honey and the full $25 participation fee.[8]

[8] We varied the availability of information on differences between locally produced honey and honey of unknown origin, specifically the following information: *Honey may be produced from domestic or foreign beehives. In the United States, the Environmental Protection Agency (EPA) regulates the pesticides and antibiotics that can be used to support bee health. In a foreign country, there may be similar regulations, or there may be looser or no regulations on chemical additives.*

The honey you are endowed with is of unknown origin, and may have been produced in the US (following US regulations on chemical use), or in a foreign country, with either similar regulations or looser or no regulations on hive chemical inputs. One third of honey found on the international market comes from China. The United States has banned imports of Chinese honey due to concerns about illegal antibiotic and heavy metals found in Chinese honey. Still, Chinese honey has the potential to make it into the American honey supply due to fraudulent activities in the international sweetener market. It is therefore not clear honey of unknown origin on the US market did not originate from China or some other foreign country with chemical regulations that deviate from US regulations.

The local honey is guaranteed to be produced in the US and therefore adhere to US regulations on chemical use.

In addition to either exogenously exposing subjects to this information or not, we had an intermediate information condition – a condition where subjects could seek information. As many as 92 percent of subjects in this condition did seek out the information. None of these information conditions impacted spending in our experiment (see Appendix B). The dummy variable "Informed" sums subject (exogenous or endogenous) exposure to the above information. Generally, the Grocery Manufacturers Association (GMA) of America estimates that food fraud may affect as much as 10 percent of all food

Table 1
Summary statistics.

	N	Mean	Std. Dev.	Min	Max
TW-ST	410	13.68	4.58	4	26
Tightwad	410	0.34	0.48	0	1
Unconflicted	410	0.50	0.50	0	1
Spendthrift	410	0.16	0.37	0	1
Practice Bid	410	0.97	1.46	0	10
Bid	410	2.06	2.26	0	10
Time to Bid	410	32.28	22.98	0	159.4
Local Importance	410	4.19	1.58	1	7
Wyo. Importance	410	3.61	1.68	1	7
Price Importance	410	4.96	1.24	1	7
Gender	410	0.57	0.50	0	1
Income Category	410	1.23	0.84	1	9
Age Category	410	2.14	1.31	1	10
How often Buy	397	2.35	1.20	0	5
Informed	410	0.58	0.49	0	1

The spending booster nudge treatments were performed (69 subjects, out of the total 410) to further examine the distributional impact of nudges designed to impacts spending, and followed the same steps as the opportunity cost reminder nudge/no nudge design above.

Given the focus on spending on locally produced honey in our experiment, we designed the spending booster nudge to make salient the benefit to buying local honey. In a review of studies on consumer preferences for local food, Feldmann and Hamm (2015) finds consumers are willing to pay a premium for local production. Carpio and Isengildina-Massa (2009) find the main reason for buying local is to support the local economy. Darby et al. (2008) find that the willingness to support local production is independent of other attributes, such as freshness and farm size, and that consumers perceive state borders as the geographical boundaries of "local." We therefore worded the spending booster nudge to make salient the benefit of buying local honey in terms of the support it provides to in-state honey producers. Further, we strived to keep the spending booster nudge as parallel as possible, in length and design, to the opportunity cost reminder nudge. The booster nudge therefore consisted of the following message: '*Remember that the more you spend in this study, the more you benefit Wyoming honey producers. Your spending therefore helps the local economy*.'

Table 1 presents descriptive statistics on subjects and variables used in our analysis. To calculate pain of paying (spendthriftiness), we follow Rick et al. (2008). Based on four survey questions pertaining to spending, we calculate a continuous tightwad-spendthrift scale (hereafter referred to as the TW-ST scale). The scale can take on values from 4 to 26. The mean value in our sample is 14, which is the same as in Rick et al. (2008). Based on subjects' TW-ST scale values, they were classified as "Tightwad" (values of 11 and below), "Unconflicted" (values of 12–18) or "Spendthrift" (values of 19 and above). Our sample consists of 34 percent tightwads, 50 percent unconflicted, and 16 percent spendthrifts.

The variable "Bid" represents subjects' BDM auction bids to exchange their honey of unknown origin for an equal size (eight-ounce) jar of locally produced honey. We find that the average premium subjects are willing to pay for local honey is $2.09. This can be compared to the results in Wu et al. (2015), who find consumers are willing to pay a premium of $0.65-$2.48 per eight-ounces of honey. Despite important differences between our study and theirs, e.g., information provided to subjects, nudges (which are not part of the Wu et al. study), and subject pools, the average premium subjects are willing to pay in our study is within that same range.

The variable "Practice Bid" represents the dollar bid subjects stated in the practice auction of the Snickers bar (see Step 2 in the experiment description above). "Time to Bid" gives how many seconds subjects took to decide on their maximum marginal willingness to pay for local honey, i.e. their auction bid.

The variables "Local Importance", "Wyo. Importance" and "Price Importance" are all based on Likert scales ranging from 1 (not at all important) to 7 (extremely important), that represent subjects' rating of how important it is to them when/if they buy honey that the honey is locally produced, Wyoming produced, or cheap. The variable "How often Buy" takes a value between 0 (never) and 5 (once a month), and measures how often someone in the subject's household buys honey (of any origin). (For subjects who stated that they "do not know", this variable was coded as "missing". In our main analysis, there are 13 such missing observations (out of 410 possible)). The variable "Informed" takes a value 1 if subjects were exposed to information on the specific difference between locally produced honey and honey of unknown origin (see footnote 8).

Subjects were asked to state their age and annual pre-tax income by choosing the appropriate interval for these variables. The variable "Age Category" is a continuous variable based on 10 five-year-wide age categories (starting with "15 to 19 years" and ending with "60–64 years"). No subject reported an age higher than 64. Similarly, the "Income Category" variable

eaten in the developed world, and 20 percent of food eaten in the developing world. Further, honey is one of the most common ingredients associated with food fraud (Moore et al., 2012; Johnson, 2014). For instance, when testing honey for food fraud, the European Commission found that 13 percent of their honey samples were non-compliant with the EU Honey Directive (001/110/EC) and another 13 percent were "might be" non-compliant. An important part of non-compliances was misdeclaration of geographical origin (Aries et al., 2016).

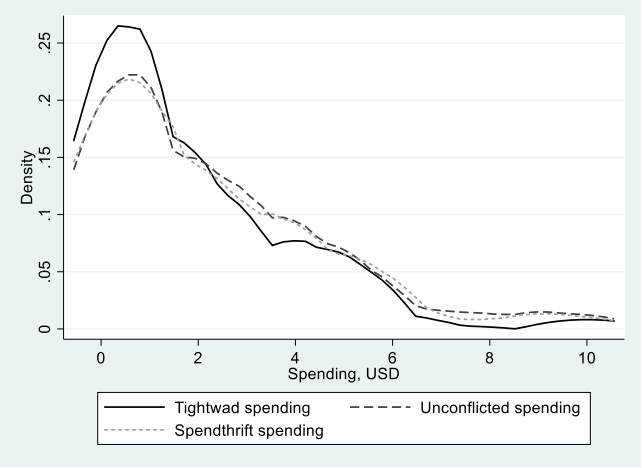

Fig. 1. Kernel density distributions on spending by consumer type.

is treated as a continuous variable based on eight $25,000-wide income categories, ranging from "$0-$25,000" to "$175,001-$200,000". No subject reported an income higher than $200,000. "Gender" takes the value one if female, zero otherwise.

3. Results

First, we examine if spending differs over consumer pain-of-paying types. Fig. 1 shows Kernel density functions of spending (experiment bids, in USD) by tightwads, unconflicted and spendthrifts. The density distributions are similar over subgroups, although the share of tightwads spending nothing in our experiment (29.17 percent) appears to be higher than the corresponding share for unconflicted (25.09 percent). However, based on a Chi-square test, we cannot reject the null hypothesis that these shares are the same (p-value = 0.323).

We next examine whether spending differs over pain-of-paying levels (both over the TW-ST scale itself, and over the subgroups defined by the scale – tightwads, unconflicted and spendthrifts), while accounting for the truncation in our spending data. We use a Tobit regression censored at the bids' lower and upper limits of $0 and $10, respectively (see Table 2). We find, at best, mixed evidence that average spending is impacted by the TW-ST scale. Column (1) shows that when we regress the TW-ST scale on auction bids, the estimated coefficient is of the expected positive sign, and weakly statistically significant. Column (3) shows that this is no longer statistically significant after controlling for bidder characteristics in the regression. Only when subjects are divided into the pain level categories (tightwads, spendthrifts and unconflicted consumers), tightwads are found to have significantly lower bid amounts (at the one percent level) than unconflicted consumers (the reference group), see Column 2. However, when we include control variables, the estimated coefficient is smaller and no longer statistically significant at the 10 percent level. Although unexpected, these results need not undermine the TW-ST scale – the scale is based on people's perceived over-/under spending, relative to some inner benchmark, which may not transfer into differences in observable higher/lower spending levels. In theory, a spendthrift may even spend less than a tightwad. However, the similarity of spending levels may also be an artifact of our experimental design. Specifically, subjects are provided windfall money in the experiment, which may loosen up spending by tightwads more than it does spending by other groups, thereby reducing differences in spending levels over consumer groups. This possibility does not affect our interpretation of the effects of nudges, which we discuss in a moment.

The control variables in Table 2, columns (3) and (4), show that when we pool all data, (i) bids are higher when subjects attach more importance to local production, (ii) bids are lower when subjects attach more importance to low prices, (iii) bids are higher when subjects also bid more in the practice auction, and (iv) age, income, gender and how often subjects' buy honey are not statistically significant predictors of bidding behavior.[9]

Table 3 gives an overview of average spending over nudges (no nudge, opportunity cost (OC) reminder and spending booster) by all consumers, as well as broken down by pain-of-paying consumer type. Wilcoxon Mann-Whitney and t-tests of equality of the spending levels in Table 3 imply (i) none of the nudges impacts *average* spending, (ii) tightwads spend less than unconflicted – both the Wilcoxon Mann-Whitney test, p-value = 0.027, and a one-sided t-test, p-value = 0.005,

[9] The variables "How often Buy" and "How often Eat" are highly correlated. The bivariate correlation (Pearson's r) is 0.71 (p-value=0.000), such that including both variables as explanatory variables would cause multicollinearity. We therefore only include "How often Buy", since we believe this variable is most likely to impact bids – in addition to capturing preferences for honey, this variable may affect experiment bids through knowledge of the outside price of honey (i.e., the price consumers face in markets outside of the behavioral laboratory). Also, our data implies the nudges impacted not only spending, but also "Time to Bid". We therefore do not include "Time to Bid" as an explanatory variable.

Table 2
Relationship between TW-ST measures and bids.

	(1) Bid	(2) Bid	(3) Bid	(4) Bid
TW-ST	0.054*		0.005	
	(0.031)		(0.028)	
Tightwad		−0.836***		−0.200
		(0.324)		(0.292)
Spendthrift		−0.437		−0.258
		(0.423)		(0.405)
Practice Bid			0.326**	0.321**
			(0.151)	(0.151)
Local Importance			0.516***	0.519***
			(0.140)	(0.139)
Wyo. Importance			0.201	0.193
			(0.131)	(0.131)
Price Importance			−0.332***	−0.333***
			(0.119)	(0.118)
Age Category			−0.011	−0.014
			(0.085)	(0.084)
Income Category			0.084	0.090
			(0.120)	(0.116)
Gender			0.163	0.150
			(0.279)	(0.278)
How often Buy			−0.083	−0.080
			(0.107)	(0.107)
Informed			−0.014	−0.016
			(0.269)	(0.270)
Constant	0.871**	1.964***	−0.437	−0.210
	(0.440)	(0.215)	(1.042)	(0.934)
N	410	410	397	397

Robust standard errors in parentheses. Models are estimated by tobit regression, censored at the minimum and maximum bids of $0 and $10. "Tightwad" refers to consumers scoring from 4 to 11 on the TW-ST scale, "Unconflicted" (baseline) refers to those scoring from 12 to 18 on the scale, and "Spendthrift" to those scoring 19 to 26. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3
Mean bids over treatments and consumer types.

Group	Treatment	N	Bid	Std. Dev.
All	All treatments	410	2.058	2.258
subjects	No nudge	165	2.189	2.195
	OC reminder	176	1.990	2.317
	Spending booster	69	1.919	2.271
Tightwads	All treatments	141	1.685	1.897
	No nudge	56	2.135	2.115
	OC reminder	61	1.387	1.650
	Spending booster	24	1.394	1.813
Unconflicted	All treatments	203	2.328	2.482
	No nudge	83	2.293	2.250
	OC reminder	86	2.353	2.636
	Spending booster	34	2.347	2.689
Spendthrifts	All treatments	66	2.026	2.160
	No nudge	26	1.974	2.255
	OC reminder	29	2.180	2.324
	Spending booster	11	1.740	1.525

reject the null hypothesis that spending by unconflicted and tightwads is the same, (iii) tightwads reduce their spending as a result of *both* nudges compared to when not being nudged. Specifically, both a Wilcoxon Mann-Whitney test (p-value = 0.064) and a two-sided t-test (p-value = 0.034) reject the null hypothesis that tightwad spending is the same over no nudge and the opportunity cost reminder nudge. Further, we weakly reject the null hypothesis that tightwad spending is the same over no nudge and the spending booster nudge (Wilcoxon Mann-Whitney test, p-value = 0.139; one-sided t-test, p-value = 0.069). Note that the "weak" rejection of the null hypothesis is likely highly impacted by the limited power of the statistical test, with only 24 observations for the spending booster nudge), and (iv) neither spendthrifts nor unconflicted significantly change their spending as a result of *any* nudge, compared to when not being nudged.

Table 4
Treatment effect of spending salience nudges.

	(1)	(2)	(3)	(4)
	Bid	Bid	Bid	Bid
OC Reminder	−0.309 (0.323)	−1.182 (0.967)	−0.289 (0.294)	−1.449* (0.869)
OC Reminder X TW-ST		0.064 (0.069)		0.084 (0.0)
Spending Booster	−0.458 (0.438)	−2.481** (1.183)	−0.275 (0.446)	−2.808*** (1.074)
Spending Booster X TW-ST		0.150* (0.085)		0.184** (0.075)
TW-ST		−0.001 (0.047)		−0.064 (0.045)
Practice Bid			0.317** (0.151)	0.326** (0.148)
Local Importance			0.530*** (0.140)	0.533*** (0.136)
Wyo. Importance			0.194 (0.131)	0.190 (0.128)
Price Importance			−0.328*** (0.119)	−0.330*** (0.118)
Age Category			−0.011 (0.084)	−0.008 (0.082)
Income Category			0.073 (0.117)	0.069 (0.121)
Gender			0.159 (0.279)	0.157 (0.279)
How often Buy			0.094 (0.106)	0.083 (0.107)
Informed			−0.078	−0.105
			(0.309)	(0.309)
Constant	1.813*** (0.220)	1.833*** (0.700)	−0.265 (0.929)	0.653 (1.134)
N	410	410	397	397

Baseline: No nudge. Robust standard errors in parentheses. Models are estimated on subsamples of the TW-ST scale, by tobit regression, censored at the minimum and maximum bids of $0 and $10. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5
Treatment effect of the salience nudge by subgroup of the TW-ST scale.

	(1) Tight	(2) Unconfl	(3) Spend	(4) Tight	(5) Unconfl	(6) Spend
OC Reminder	−0.892* (0.474)	0.019 (0.488)	0.012 (0.822)	−1.188*** (0.420)	0.122 (0.454)	−0.054 (0.823)
Spending Booster	−1.035 (0.660)	−0.135 (0.694)	−0.157 (0.824)	−1.228* (0.681)	−0.120 (0.670)	0.676 (0.952)
Practice Bid				0.781*** (0.183)	0.373** (0.185)	−0.190 (0.191)
Local Importance				0.768*** (0.165)	0.440** (0.221)	0.484* (0.248)
Wyo. Importance				−0.108 0.159)	0.231 0.192)	0.271 (0.232)
Price Importance				−0.485*** (0.166)	−0.397** (0.166)	−0.042 (0.293)
Age Category				−0.091 (0.103)	0.080 (0.161)	−0.160 (0.202)
Income Category				0.002 (0.203)	0.391** (0.184)	−0.313 (0.217)
Gender				0.063 (0.417)	0.421 (0.424)	−0.412 (0.672)
How often Buy				0.240 (0.174)	−0.014 (0.167)	−0.051 (0.276)
Informed				−0.341	−0.246	0.397
				(0.460)	(0.447)	(0.665)
Constant	1.788*** (0.359)	1.934*** (0.315)	1.578*** (0.542)	0.839 (1.251)	−0.117 (1.455)	−0.287 (1.988)
N	141	203	66	138	196	63

Baseline: No nudge. Robust standard errors in parentheses. Models are estimated on subsamples of the TW-ST scale, by tobit regression, censored at the minimum and maximum bids of $0 and $10. "Tight" refers to the Tightwad group scoring from 4 to 11 on the scale, "Unconfl" refers to the Unconflicted group scoring from 12 to 18 on the scale, and "Spend" refers to the Spendthrift group scoring 19 to 26 on the scale. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

To test the impact of nudges on spending while properly accounting for the truncation of our bid data and the effect of covariates, we apply a Tobit regression models to analyze our data (see Table 4). The results from Table 4 show that the qualitative results from simple mean comparisons (as shown in Table 3) hold up well.

Table 4 shows that the estimated coefficient of the spending reminder nudge is consistently negative over different model specifications, but generally not statistically significant at the 10 percent level. We therefore cannot reject the null hypothesis that the nudge has no impact on *average* spending levels. The interaction term "OC Reminder X TW-ST" is positive, which is in line with the result in Table 2 suggesting tightwads reduce their spending the most as a result of the nudge, but it is not statistically significant. Columns (2) and (4) suggest the booster nudge does reduce spending, particularly for tightwads, as implied by the estimated coefficient for the interaction term between the booster nudge and the TW-ST. Note from columns (1) and (3) that if we do not control for the heterogeneity in responses to the nudge over spendthriftiness, we cannot reject the null hypothesis that the booster nudge has no impact on spending.

We proceed by further examining the impact of the nudges over consumers with different levels of pain of paying, see Table 5. We find that the spending of subgroups is significantly affected by both the opportunity cost reminder and the booster nudge. Table 5, column (4), shows that the opportunity cost reminder nudge reduces bids in the experiment by tightwads, by around $1.20, compared to if they are not nudged at all. This result is highly statistically significant. Estimated coefficients of the impact of the opportunity cost reminder nudge for unconflicted consumers and spendthrifts are small in magnitude and statistically insignificant. Hence, the nudge *reduces* spending by those who would typically benefit from

increasing their spending (tightwads), while it fails to target spending by those who may benefit from reducing spending (spendthrifts). Our finding that spendthrifts and unconflicted in our sample do not respond to the opportunity cost reminder nudge likely also explains the statistically insignificant estimated coefficient of "OC Reminder X TW-ST" in Table 4.

Table 5, column (4), further shows that the booster nudge may have a similar impact over sub-groups – it too reduces spending by tightwads by around $1.20, although the estimated coefficient is only weakly statistically significant. Columns (5) and (6) in Table 5 further shows that the size of the estimated coefficient of the booster nudge increases over unconflicted and spendthrifts, as implied by the estimated coefficient for "Spending Booster X TW-ST" in Table 4. However, the estimated coefficients are not statistically significant, i.e., when we break down the data over consumer groups, we cannot detect an effect of the booster nudge on spending by unconflicted or spendthrifts.

Table 5 shows that the impact of the control variables on spending differs in some interesting respects between pain level groups. Both tightwads and unconflicted consumers reduce their spending if the importance of products being cheap increases. However, we cannot reject the null hypothesis that bids by spendthrifts are unaffected by the stated importance they assign to products being cheap. This may result from spendthrifts' stated behavior being impacted by what they think they "should do" (be concerned about prices), while their revealed behavior suggests they might be less concerned with value for money. Further, income has the expected positive impact on spending by the unconflicted group. However, we cannot reject the null hypothesis that income has no impact on spending by tightwads and spendthrifts. This finding suggests the actual budget (e.g., true opportunity cost) is a less important determinant of spending for tightwads and spendthrifts, in line with the idea that their spending is instead largely determined by emotions (pain of paying), which do not perfectly reflect their true budget, or opportunity cost.

Our results on the impact of the opportunity cost reminder stand in contrast to previous findings. Frederick et al. (2009) find that an opportunity cost reminder has the greatest impact on spending by spendthrifts, and has the desired impact of reducing their spending. Their study differs from this study in important ways, though, which may explain the opposing results – as discussed earlier, their measure of spending is based on stated (hypothetical) spending intentions. Further, they implement an opportunity cost reminder in a different way, and the products offered for purchase differ over the studies.[10]

4. Discussion

Standard economic theory does not account for the impact of emotions on decision-making. However, there is vast evidence that emotions impact decision-making, and that nudges evoke emotions. Salience nudges have therefore been referred to as "emotional taxes" (Glaeser, 2006; Loewenstein and O'Donoghue, 2006). In this paper, we examine the incidence of an opportunity cost reminder nudge, aimed to reduce spending. We account for the previous finding that emotions (pain of paying) impacts spending in the population (Rick et al., 2008; 2011) and that there is heterogeneity in emotional responses to the nudge. To examine the distributional effects of the nudge, we design an economic laboratory experiment. We find that the opportunity cost reminder nudge entirely fails to impact spending by those who would benefit from reducing their spending (spendthrifts), while it significantly reduces spending by those who already spend too little (tightwads). Hence, the overall impact of the opportunity cost reminder nudge is a likely reduction in consumer welfare.

Given these findings, we proceed by exploring if the impact of spending nudges might be determined by consumers' inherent preferences for spending, as given by their suboptimal pain of paying. In other words, if tightwads are the ones responding to a nudge designed to reduce spending, will spendthrifts be the ones primarily responding to a nudge that encourages spending (specifically, a nudge that makes salient the benefits to spending in our experiment, by benefiting the local economy)? We find, however, that this spending booster nudge acts similar to the opportunity cost reminder – it negatively affects spending by tightwads, while having no impact on spending on the other groups. It therefore seems the spending booster nudge primarily made salient the pain associated with spending for tightwads and not, as intended, the benefits from spending. This result underscores that consumer responses to salient nudges might be the opposite of what was intended, in line with Marreiros et al. (2017) and Wilson et al. (2015). Marreiros et al. (2017) examine the impact on consumers' willingness to disclose identifiable information of positive and negative information on companies' privacy practices. They find both types of information reduces self-disclosed private information – the mentioning of privacy issues dominates if the information is positive or negative. Similarly, Wilson et al. (2015) find a salience nudge designed to encourage low-calorie milk choices, instead focuses consumer attention on milk in general and increases average consumption of both low- and high-calorie milk.

Our study is preceded by Frederick et al. (2009), who also examine the impact of an opportunity cost reminder nudge over consumer types, although in a hypothetical context. They find the opportunity cost reminder has the desired impact, i.e., it reduces purchase intentions for spendthrifts only. Our study constitutes an important complement to theirs and shows

[10] Specifically, Frederick et al. (2009) asked subjects to make a hypothetical choice between an expensive and a cheap stereo. To examine the impact of an opportunity cost reminder on subjects' choice of stereo, they added the wording "leaving you $300 in cash" at the description of the cheap stereo. The share of spendthrifts who chose the cheap stereo increased significantly when this wording was added to the stereo alternative descriptions. The share of tightwads choosing the cheap stereo was also higher in the condition with higher salience of opportunity costs, but this effect for tightwads was not statistically significant.

the impact of opportunity cost reminder nudges might be context dependent, e.g., over hypothetical versus incentivized contexts.

In order to measure the magnitude of the welfare impact of the nudges one would need to know the distribution of pain of paying in the population. It may be informative to know, however, that a large share of subjects in our study are negatively affected by the nudges. Not only did none of the pain level sub-groups benefit from the nudges, but tightwads, i.e., those harmed by both nudges, typically outnumber spendthrifts. For instance, even if the opportunity cost reminder nudge would have raised welfare for spendthrifts, the relative size of the tightwad population could reverse the aggregate welfare gain. In our study, 34 percent of subjects are tightwads, 50 percent are unconflicted and 16 percent of subjects are spendthrifts. In Rick et al. (2008), where the subject pool consisted of 13,000 subjects from the general population rather than 410 students and university staff, the corresponding numbers were 24 percent tightwads, 60 percent unconflicted and 15 percent spendthrifts. Hence, tightwads seem to comprise a larger share of the population than spendthrifts.

Over spending might have graver future consequences than under spending, as implied by studies on regret from spending (O' Guinn and Faber, 1989; Horváth, 2015). This might explain why most policies focus on reducing over spending, rather than increasing spending by those who spend too little. However, our study shows that salience nudges designed to reduce over-spending might entirely backfire, and only target those who are harmed by reducing their spending. Our study also shows that consumers might react to pieces of the information provided by the salience nudge in unintended ways. Our results therefore illustrate the need for careful analyses of the impact of salience nudges, before such policies are implemented. We also encourage future research to examine both average and distributional effects of other policies designed to impact spending/savings. This might be particularly important for policies that we know might substantially impact spending/savings, such as default nudges, employer matching, and tax deductions (see e.g., Madrian and Shea, 2001; Rutledge et al., 2014; Ferman, 2015; Blumenstock et al., 2018). Do these policies also primarily impact consumers who already save too much, or do they better target spending by spendthrifts?

Our results also has implications for contingent valuation studies that use opportunity cost reminders intended to bring respondents closer to their true willingness to pay (Arrow et al., 1993). Our findings suggest that such reminders and related "cheap talk" scripts (see e.g. Cummings and Taylor, 1999) may distort stated preference valuation estimates among particular subgroups, rather than bring them closer to their true value. Further, our results have implications for how we may interpret standard measures of consumer values of product attributes. Revealed preferences are often used as a basis for demand analysis – marginal values for product attributes based on actual choices are generally uncontroversial. However, our study shows that measures of values of product characteristics derived from actual choices may be muddled by suboptimal levels of pain of paying. Spend thrifty consumers may be willing to pay a premium for products simply as a result of their sub optimally low pain of paying (i.e. independent of their true value for the product), while tightwads may be willing to pay little for a product, as a result of their excess pain of paying.

Although not a main objective of our study, our results also contribute to the impact of "buy local" campaigns. While many studies examine consumer preferences for locally produced food (see Feldman and Hamm, 2015, for a review), the impact of campaigns that encourage consumers to buy local is largely unknown. The few studies that aim to address this issue all rely on consumer attitudes and survey data (Cameron and Elliott, 1998; Saffu and Walker, 2006; Hughes and Isengildina-Massa, 2015). Our results imply such campaigns might fail to strengthen preferences for local production. We encourage future studies to examine the impact of such campaigns, including the distributional effects over consumer types, based on revealed preference data.

Finally, this study is limited in several important ways. First, our study uses a self-reported measure of pain of paying (the TW-ST scale). An alternative way of measuring pain of paying would be to observe actual pain (e.g. using fMRI scans, as in Knutsson et al., 2007). Second, we only examine marginal spending on a specific good (locally produced honey). We encourage future research to examine the impact of pain of paying on other types of consumption. This may be particularly important since some of our results differ from other exploratory studies on pain of paying (Rick et al., 2008; Frederick et al., 2009). We particularly encourage future research to examine the impact of spending nudges on larger purchases, including overall spending and saving by consumer types. Third, our spending booster nudge may not have been a strong enough motivator for our experimental subjects to increase spending. If the nudge fails to increase the urgency felt by consumers to support the local economy, consumers may be more likely to respond to the "booster nudge" as a spending reminder only. Further, we only vary market contexts in terms of the availability of product quality information. Other dimensions may also be important, both in determining spending over levels of pain from paying and the welfare impact of salience nudges, e.g. transactions costs, and market characteristics that increase spending, such as low tax salience and modes of payment that encourage spending.

Declarations of interest

None.

Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2018.07.005.

Appendix A. Experimental instructions

[Provided in writing and read to subjects by the experiment monitor]

- The objective of this study is to understand how you make decisions about honey.
- At the beginning of the study, you will be given a 8 ounce jar of honey of unknown origin (i.e. honey that could have been produced anywhere in the world). You will then be asked to participate in an auction.
- In the auction, you will be asked to bid on the option to exchange your jar of honey of unknown origin for an 8 ounce jar of local honey.
- The amount of money you will receive for participating in this study will depend on your bid in the auction, and the auction price. Your final payment will consist of a $25 participation fee, minus the price you end up paying if you win the auction. If you lose the auction, then nothing will be subtracted: you will get the full $25 participation fee.
- In addition to your monetary payment: if you win the auction, you also walk away with a jar of locally produced honey. If you lose the auction, you also get to walk away with a jar of honey of unknown origin.
- The total amount will be paid to you privately, in cash, at the end of the study today.
- The experiment will take approximately 30 minutes.
- Please do not talk, exclaim, or try to communicate with other participants during the study. Please put away all outside materials (such as book bags, notebooks, cell phones) before starting the study. Please also turn off your cell phones.
- All the information collected in this study is confidential and will be used only for statistical purposes.
- If you decide to leave before the study is over, you will still receive your participation fee of $25.

Auction procedure

The auction in this study differs in two ways from "standard" auctions you might be familiar with:

(1) Unlike standard auctions, your bid will not be compared with those of others who participate in the auction. Rather, your bid will be compared with a bid that is randomly drawn by the study monitor (a number randomly drawn from a jar, at the end this session). If your bid is greater than or equal to the monitor's random bid, then you win the auction; if your bid is lower, then you lose the auction.
(2) Unlike standard auctions, the price you pay if you win the auction is not your winning bid, but rather the monitor's losing bid.

For example, suppose that you were endowed a green chair and asked to state your bid to change the green chair for another chair (the "red chair"). Also, suppose we paid you a large fee of $1000 to participate in this study.

Now, suppose you bid $100 to change your green chair for the red chair.

Suppose now that the monitor drew $81.95.

You will then have won the auction, because your $100 bid is greater than $81.95. You will therefore get to exchange your green chair for a red one, and your final payment will be $1000 minus $81.95, not minus your bid of $100!

If instead the monitor's random bid is $110, then the monitor will have won the auction, because your $100 bid in that auction is less than $110. You will therefore *not* get to exchange your green chair for the red chair. And your final payment will be $1000, with nothing subtracted.

Tips for how to bid

The best bidding strategy in an auction of this type is to bid your *true value* of the product you are bidding on. Let us explain why:

Assume that you have decided that you would be willing to pay up to $200 for the auctioned good: the red chair. I.e. $200 is your true value of switching to the red chair. Which strategy is best for you: to bid higher than $200, lower than $200, or to bid exactly $200? The answer is to bid exactly $200.

If you were to bid less than $200 (say $100), then the auction might be won by the monitor (if its random bid was $150, say), and you will not be able to buy the red chair even though you were truly willing to pay more than $150. Therefore bidding less than your true valuation is not a good strategy.

If you were to bid more than $200 dollars (say $300), you might end up winning the auction but having to pay a price higher than what you really value the red chair at (if the monitor's random bid was $225, say). Therefore bidding more than your true valuation is not a good strategy.

The best strategy in this type of auction is to always bid exactly the maximum amount that you would be willing to pay, i.e. your true value of $200 in our example. You do not risk paying more for the good than you want to, and you will never pay more than the monitor's bid anyway.

Final notes

- If you do not want to exchange your jar of honey of unknown origin for the local honey offered in the auction, then you should just bid zero. Because the monitor's random bid is always positive (though possibly very small), you are then certain to lose the auction, and you will just get to keep your jar of conventional honey.
- By local honey, we mean honey produced in Wyoming or Colorado.

Table B1
Treatment effects by subgroup of the TW-ST scale, over market contexts.

	(1) Tight	(2) Unconfl	(3) Spend	(4) Tight	(5) Unconfl	(6) Spend
OC Reminder, Market 1	−1.594* (0.835)	0.314 (0.919)	1.441 (1.721)	−1.137 (0.737)	0.720 (0.933)	2.643 (2.029)
No nudge, Market 2	−1.242 (0.796)	0.460 (0.706)	1.299 (1.734)	−1.098 (0.668)	0.464 (0.720)	2.495 (1.883)
OC Reminder, Market 2	−1.774** (0.781)	0.802 (0.780)	0.823 (1.648)	−1.976*** (0.688)	0.935 (0.799)	2.100 (1.730)
No nudge, Market 3	−0.423 (1.126)	0.837 (0.899)	0.329 (1.552)	0.418 (0.913)	1.254 (0.880)	2.285 (1.698)
OC Reminder, Market 3	−1.327 (0.899)	−0.138 (0.849)	−0.677 (1.785)	−1.425** (0.719)	−0.021 (0.765)	0.913 (1.815)
Spending Booster, Market 1	−1.736** (0.842)	0.280 (0.825)	0.457 (1.509)	−1.422* (0.720)	0.675 (0.827)	2.381 (1.706)
Practice Bid				0.801*** (0.184)	0.374** (0.179)	−0.051 (0.214)
Local Importance				0.771*** (0.161)	0.433** (0.217)	0.427* (0.239)
Wyo. Importance				−0.099 (0.149)	0.227 (0.188)	0.451*(0.228)
Price Importance				−0.507*** (0.170)	−0.403** (0.163)	0.097 (0.294)
Age Category				−0.054 (0.106)	0.087 (0.157)	−0.251 (0.217)
Income Category				0.032 (0.221)	0.393** (0.167)	−0.294 (0.188)
Gender				0.101 (0.396)	0.385 (0.415)	−0.594 (0.651)
How often Buy				0.203 (0.169)	−0.037 (0.173)	−0.130 (0.289)
Constant	2.501*** (0.659)	1.524*** (0.549)	0.973 (1.388)	1.084 (1.299)	−0.647 (1.448)	−2.721 (2.795)
N	141	203	66	138	196	63

Baseline: No nudge, Market 1. Robust standard errors in parentheses. Models are estimated on subsamples of the TW-ST scale, by tobit regression, censored at the minimum and maximum bids of $0 and $10. "Tight" refers to the Tightwad group scoring from 4 to 11 on the scale, "Unconfl" refers to the Unconflicted group scoring from 12 to 18 on the scale, and "Spend" refers to the Spendthrift group scoring 19 to 26 on the scale. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix B. The impact of nudges over information conditions

In our experiment, we varied availability of information stating the difference between locally produced honey and honey of unknown origin. We define the information availability that subjects were subjected to as "market contexts". Specifically, subjects who participated in the 'Market 1' treatments were given information on the difference between honey of unknown origin and locally produced honey (similar to Wu et al., 2015), while information was optional to subjects in the 'Market 2' treatments who were given the option to opt in to the information. Subjects in the 'Market 2' treatments were given the following message: "*First, you are offered short information on the characteristics that differ between the conventional honey and the local honey. If you wish this information, please click "reveal information" below, otherwise proceed to the next question.*" We find information exposure in Market 2 is very similar to that in Market 1, since 92 percent of subjects in the 'Market 2' treatments opted into information. Subjects exposed to information was hence all subjects in the two Market 1 treatments and all subjects in the Market 2 treatments who opted in. We also measured if these subjects read the information to which they were exposed, and 98 percent of subjects who had the information also indicated that they read the information. Subjects in the 'Market 3' treatments could not learn the information.

More specifically, our 410 subjects participated in one of the following seven treatment groups:

- 'OC Reminder, Market 1' – subjects were given the opportunity cost reminder and then given information.
- 'OC Reminder, Market 2' – subjects were given the opportunity cost reminder and then offered to opt in to information.
- 'OC Reminder, Market 3' – subjects were given the opportunity cost reminder and given no information.
- 'No Nudge, Market 1' – subjects were not reminded of their spending, but given information.
- 'No Nudge, Market 2' – subjects were not reminded of their spending and then offered to opt in to information.
- 'No Nudge, Market 3' – subjects where not reminded of their spending and given no information.
- 'Spending booster, Market 1' – subjects were given the spending booster and then given information.

Table B below shows that if we break down the data over market contexts, per the above, results remain consistent with those presented in Table 5. Market 1 (i.e., full information on differences between local and non-local honey), no nudge, is baseline. Column (4) shows that the estimated interaction term coefficient of the opportunity cost reminder is negative for all markets, and generally statistically significant, with the exception of Market 1. In no market context are the estimated nudge coefficients statistically significant for the other pain level groups, i.e., in none of the market contexts adopted in this study does the opportunity cost reminder nudge seem to impact unconflicted or those in need of reduced spending, i.e., spendthrifts. Again, the opportunity cost reminder nudge seems to only impact spending of those who already spend too little.

Although only evaluated over one market condition (Market 1), we include the boost nudge in the below analysis, given it allows us to test the robustness of the impact of the boost nudge over different model specifications. The estimated coefficient for the boost nudge over consumer types is in line with our previous finding – this nudge reduces spending by tightwads. Note that the estimated coefficient for the boost nudge in Table B is close to significant at the 5 percent level ($p\text{-}value = 0.051$). The estimated impact of the control variables also remain the stable, compared to the results reported in Table 5.

Table C1
Treatment effect of the money pump by subgroup of the TW-ST scale.

	(1) Tight	(2) Unconfl	(3) Spend	(4) Tight	(5) Unconfl	(6) Spend
Money Pump	−0.183 (0.606)	−0.540 (0.443)	0.210 (0.786)	−0.676 (0.546)	−0.275 (0.407)	0.368 (0.828)
Practice Bid				1.105*** (0.340)	0.460** (0.210)	0.133 (0.276)
Local Importance				0.717*** (0.222)	−0.152 (0.215)	0.437 (0.342)
Wyo. Importance				0.148 (0.215)	0.328* (0.189)	0.480 (0.336)
Price Importance				−0.166 (0.210)	−0.673*** (0.164)	−0.482 (0.383)
Age Category				0.102 (0.178)	−0.006 (0.189)	−0.376 (0.501)
Income Category				−0.812 (0.607)	0.462** (0.179)	−0.806 (0.784)
Gender				0.484 (0.560)	−0.272 (0.445)	−0.028 (0.760)
How often Buy				0.243 (0.271)	−0.273 (0.166)	−0.141 (0.289)
Informed				−0.432 (0.657)	−0.018 (0.491)	0.616 (0.681)
Constant	1.685*** (0.391)	1.996*** (0.299)	1.515*** (0.568)	−1.399 (1.944)	4.559*** (1.550)	0.752 (2.273)
N	107	167	66	105	163	64

Baseline: No nudge. Robust standard errors in parentheses. Models are estimated on subsamples of the TW-ST scale, by tobit regression, censored at the minimum and maximum bids of $0 and $10. "Tight" refers to the Tightwad group scoring from 4 to 11 on the scale, "Unconfl" refers to the Unconflicted group scoring from 12 to 18 on the scale, and "Spend" refers to the Spendthrift group scoring 19 to 26 on the scale. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix C. The impact on spending from a money pump nudge

The 'Money pump' was designed by Cherry et al. (2003) and informs consumers that people are often inconsistent in how they value risky choices, given their stated preferences, and that such consumer inconsistencies may be exploited in markets by arbitrage. This nudge therefore does not draw people's attention to opportunity costs per se, but reminds them of the potential economic losses from irrational decision-making. It has been found to cause lower levels of spending in economic experiments (Cherry et al., 2003; Cherry and Shogren, 2007).

The money pump explained

Following the design in Cherry et al. (2003), subjects in the "Money pump" treatments were treated with the following information and exercise, prior to submitting their bid for the local honey:

You are participating in an experimental auction of local honey. I.e. you will be asked to state your maximum willingness to pay to switch out your endowed honey of unknown origin for an equal size jar of local honey.

Before you state your maximum willingness to pay for switching to the local honey, we want to get a sense for your preferences over lotteries. Specifically, you will be asked to state your preferences and values for two lotteries.

Consider the following two lotteries, with the odds and potential outcomes shown below.

Lottery 1: odds 33/36 for outcome $5.25 and odds 3/36 for outcome -$1.50 (i.e. a loss).
Lottery 2: odds 9/36 for outcome $24.00 and 27/36 for outcome -$1.75 (i.e. a loss).

Which lottery do you prefer to gamble in?

- ○ *Lottery 1*
- ○ *Lottery 2*

Consider the same two lotteries again.

Lottery 1: odds 33/36 for outcome $5.25 and odds 3/36 for outcome -$1.50 (i.e. a loss).
Lottery 2: odds 9/36 for outcome $24.00 and 27/36 for outcome -$1.75 (i.e. a loss).

Please indicate the maximum amount (i.e. reservation price) you would be willing to pay for a ticket to participate in each of these lotteries [Subjects needed to state their reservation prices].

My reservation price for a ticket to lottery 1: $_____
My reservation price for a ticket to lottery 1: $_____

People are often inconsistent in their preferences and values for risky choices - and this can cause them to lose money. The example above illustrates a common mistake; people often prefer the first lottery because it is less risky, but they name a higher monetary value for the second lottery because of the larger potential winnings. However, this inconsistency exposes them to arbitrage.

To see this, suppose you valued Lottery 1 at $1, and Lottery 2 at $2, but said you prefer to play in Lottery 1. A person should be able to sell you a ticket for Lottery 2 for $2, which was your stated value. But you said you prefer to play Lottery 1, so that

same person should be able to trade you a ticket for Lottery 1 for the Lottery 2 ticket you just bought. You also said a ticket for Lottery 1 was worth $1 to you, so someone should be able to buy it from you for $1.01. Now you have a hole in your pocket – you just lost 99 cents.

People sometimes want to revise their valuation of the lottery tickets once this has been pointed out.

You are now offered the opportunity to revise your preferred lottery and reservation prices, if you so wish. Therefore, please consider again the same lotteries:

Lottery 1: odds 33/36 for outcome $5.25 and odds 3/36 for outcome -$1.50 (i.e. a loss).
Lottery 2: odds 9/36 for outcome $24.00 and 27/36 for outcome -$1.75 (i.e. a loss).

Which lottery do you prefer to gamble in?

- ○ *Lottery 1*
- ○ *Lottery 2*

Lottery 1: odds 33/36 for outcome $5.25 and odds 3/36 for outcome -$1.50 (i.e. a loss).
Lottery 2: odds 9/36 for outcome $24.00 and 27/36 for outcome -$1.75 (i.e. a loss).

Please indicate the maximum amount (i.e. reservation price) you would be willing to pay for a ticket to participate in each of these lotteries [Subjects needed to state their reservation prices].

My reservation price for a ticket to lottery 1: $_____
My reservation price for a ticket to lottery 1: $_____

Table C below shows the impact of the money pump on spending over the different pain level consumer types. Baseline is no nudge. Focusing on columns (4), (5) and (6), we note that the sign and magnitude of the estimated Money pump coefficients share similarities with those of both the opportunity cost reminder nudge and the boost nudge – in particular, like both the opportunity cost reminder and the boost nudge, the coefficient is negative for tightwads (and like the boost nudge, it is positive for spendthrifts). However, none of the estimates of the impact of the Money pump are statistically significant.

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