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The Power of Voice in Stimulating Morality: Eliciting Taxpayer Preferences Increases Tax Compliance

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Decisions about paying taxes represent one of the most common moral quandaries faced by citizens. In the present research, we argue that taxpayer compliance can be raised by increasing "voice": allowing taxpayers to express non-binding preferences about the way their taxes are used. We first test for effects of preference expression on tax compliance with a tax in a laboratory setting. Here, we find that allowing participants to express non-binding preferences over tax spending priorities leads to a 16% increase in compliance. A follow-up online study tests this treatment with a simulation of paying US federal taxes. Allowing taxpayers to express their preferences on the distribution of government spending reduces the stated take-up rate of a questionable tax loophole by 15%. A third experiment shows that this effect only occurs when taxpayers have voice in spending on preferred tax categories, allocating tax dollars across disliked spending categories increased neither feelings of voice nor likelihood of payment.

Keywords Ethics and morality; Public policy issues

Tax non-compliance is estimated to amount to some \$406 billion in lost funding annually in the United States (IRS, 2016) and £34 billion in the UK (HMRC 2017). Given that tax payment is legally required, non-compliance represents a substantial and surprisingly common moral failing among consumers: while most would hesitate to steal from needy consumers or vandalize a public park, consumer willingness to take tax loopholes or underreport income comes much more easily.

While tax aversion has many causes, we suggest that non-compliance can be understood—and mitigated—by considering two psychological characteristics of the tax process. First, tax aversion is created by the decoupling of tax payments and the public goods obtained in return. This disconnect places distance between payment and benefit, and therefore decreases taxpayers' perceptions of the tax-funded benefits they receive. Data suggest that such under-recognition of benefits is widespread: in one survey, among Americans who claimed they had never used a government social service, 53% had in fact taken out student loans, and 40% had benefited from Medicare (Mettler, 2011). Because perceptions of such benefits are low at the time of payment, consumers feel that tax payment is a poor investment, and thus feel licensed to cheat (Soman & Gourville, 2001; Thaler, 1999).

However, we propose that recoupling alone may not be enough to solve this problem. Rather, we

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argue that taxpayer non-compliance also results in part from the sense that one has no voice in the taxation process. One means by which voice—"the voluntary expression of people's views" (Banerjee & Somanthan, 2001)—can be provided in this context is by allowing individuals to express non-binding preferences about the manner in which their tax dollars are spent. Building on the previous literature, we propose that voice will reduce tendencies toward "exit" strategies (Hirschmann, 1970)—manifest in this domain as non-compliance with the tax system.

In demonstrating the potential of preference expression, the present research offers both practical and theoretic contributions. To recouple payment and benefit, policymakers have publicized information on the allocation of tax dollars across expenditure categories (White House 2014) and have introduced "Personal Tax Statements" (UK Treasury 2012). The success of such interventions is yet to be determined on a large scale. Thus, our test of the role of recoupling in a preference expression context provides initial insight into the conditions under which such efforts will prove successful. Specifically, we explore whether information alone will generate greater levels of tax compliance, or whether an opportunity for preference expression is critical. Furthermore, if our effects hold, the present work suggests a low-cost, psychologically grounded means of addressing the individual moral failures that, in the aggregate, undermine the support of public goods. While our results also suggest that this intervention has limits, we argue that its practical impact may be large enough to warrant broader testing and implementation.

From a theoretical perspective, we build on prior research suggesting that information alone—such as information about penalties for undesirable behaviors—may not be the optimal way to change moral behavior (Alm & Torgler, 2011). In addition, our work extends prior research that focused on satisfaction with tax payment (Lamberton, 2013; Sussman & Olivola, 2011). While increasing satisfaction with payment may have important downstream consequences for civil sentiment, this previous research leaves open the critical question of whether taxes will be paid at all. Thus, we are still in need of evidence that the interventions that change attitudes will change costly moral behavior. Furthermore, we contribute to the literature on transparency in information provision and in voice, showing both the power and contingent nature of both. Although in a much less tragic context, our findings about the limitations of the power of voice

as an empowering mechanism are generally consistent with those of Botti, Orfali, and Iyengar (2009), in that we find that not all preference expressions have positive consequences. Similarly, our findings suggest that the effect of information may be conditional on the context in which it is provided, consistent with recent work in governance (e.g., de Fine Licht, Naurin, Esaiasson, & Gilljam, 2014). We propose that future research should continue to explore the psychology of enhanced voice in the civic domain, to test for the longevity of morally relevant effects over time. Such studies would allow us to learn even more about voice in consequential consumption and spending decisions, and ideally, to identify other means of prompting moral behavior.

Theoretical Background

Tax compliance and tax morale form the basis for a large literature. This research suggests that factors driving tax compliance can be broadly understood to be either extrinsic or intrinsic in nature, with the former mostly centered on tax audit probabilities and penalty rates (Allingham & Sandmo, 1972) and the latter including numerous cultural and normative factors (for recent reviews, see Alm, 2012; Luttmer & Singhal, 2014; OECD, 2012; Torgler, 2007). The more psychological aspects of the tax system have received increasing attention, as standard extrinsic drivers of compliance (such as enforcement rates) do not fully explain moral decision-making in this domain (McCaffery & Slemrod, 2006; Torgler, 2007; Konrad & Qari, 2012; Christian and Alm, 2014). More particularly, recent research has focused on understanding the "interaction climate" between tax authorities and taxpayers (Kirchler, 2007) and examines factors such as tax code complexity (Abeler & Jäger, 2013), enforcement information dissemination (Alm, Jackson, & Mckee, 2009), tax code kink points (Saez, 2010), service provision by tax authorities (Chetty & Saez, 2013; Mckee, Siladke, & Vossler, 2011), tax framing (McCaffery & Baron, 2004), norms and varied means of communication (Cotterill, Moseley, Smith, Stoker, & John, 2011; Hallsworth, List, Metcalfe, & Vlaev, 2014), and effects of tax form design such as the placement of the signature box (Shu, Mazar, Gino, Ariely, & Bazerman, 2012).

Research suggests that providing information about the purpose and benefits of taxation may increase satisfaction with tax payment. For example, Sussman and Olivola (2011) show that asking

individuals to consider the positive uses of their tax payments improves their negative attitudes toward taxation. Such results align with work that shows that making the outcome of a charitable donation more tangible (Cryder & Loewenstein, 2010; Cryder, Loewenstein, & Scheines, 2013) or providing more information on the beneficiary (Bohnet & Frey, 1999) can increase generosity. In essence, such work addresses the well-known mental accounting problem of decoupling between payment and its economic outcomes (Soman & Gourville, 2001; Thaler, 1999).

At the same time, however, there is also a possibility for such informational interventions to fail. Recent work demonstrates that transparency does not always engender trust in government, and indeed, that information about priorities and procedures can, in some cases, create disruption and decrease trust (de Fine Licht, 2011; Worthy, 2015). Rather, only when transparency provides individuals with a belief that outcomes are the results of a “deliberative democratic ideal,” does it appear to lead to a sense that a government is more legitimate (de Fine Licht et al., 2014, p. 111).

Thus, we hypothesize that positive effects of information provision on the decision to pay taxes will be more likely to emerge when such information is accompanied by voice. Voice has been shown to have substantial effects on well-being and loyalty, particularly when individuals are in groups subject to aggregate, shared outcomes (Banerjee & Somanthan, 2001). Classic research on voice argues that when encountering suboptimal experiences (such as the need to pay taxes), individuals tend to react either via “exit,” that is, opting-out of the undesirable system, or through “voice” (Hirschmann, 1970). In the present context, exit is captured via non-compliance; taxpayers simply decide to violate both laws and social norms rather than participate fully in the system. Hirschmann (1970) would suggest that this is rational, as taxpayers have no opportunity to communicate with decision-makers at the time of tax payment. Thus, offering voice should reduce individuals’ likelihood of exiting—and increase their probability of compliance.

But how can voice be implemented both politically and practically? Most prior work on voice in the tax system has relied on voice that directly shapes behavior, often discussed under the heading of “participatory government” (e.g., Avritzer, 2000; Fung & Wright, 2001). For example, Torgler (2005) found in a comparative study of Swiss cantons that more direct democracy—that is, more binding voice among citizens in the decision-making process—had

significant positive effects. In the same spirit, Alm, Jackson, and McKee (1993), Alm, Sanchez, and DeJuan (1999) showed that tax compliance increased when small groups of individuals voted on the outcome in an incentive-compatible referendum setting. A classic case study exists in Porto Alegre, Brazil, where each year 10% of the annual budget is allocated directly by citizens, via participation in neighborhood meetings and thematic assemblies. All of this previous research demonstrating the benefits of preference expression has required either a guarantee or implication of actual direction of expenditures or actions (e.g., Lamberton, 2013)—a change that would require substantial changes in thinking in most current Western political systems.

The present research, while similar in its focus on the importance of voice, suggests that tax compliance can be increased by merely providing the opportunity to express preferences without the need to make taxpayer preferences consequential. We base our belief in the efficacy of simple preference expression in the idea that voice allows individuals to feel that they are *influential* but still *deferential* (Hirschmann, 1970)—they have been “heard,” but are not wresting control from those in power. Indeed, individuals can feel that their ability to participate in a decision-making process is more critical than is the actual outcome to which the process leads (Lind, Lissak, & Conlon, 1983; Tyler, Rasinski, & McGraw, 1985). In other words, whether or not individuals’ voice directly translates into specific actions or outcomes can be less important than the feeling that they were given the opportunity to express their preferences as part of the process. Recent work also suggests that merely having the opportunity to communicate with government entities may foster positive outcomes. For example, Porumbescu (2015) finds that the use of public-sector social media is associated with greater trust in government, suggesting that the opportunity to simply interact may, for at least some individuals, increase civic engagement. Thus, we predict that expressing preferences will increase taxpayer compliance even when such preferences are not binding: as long as taxpayers feel they have a voice, compliance will increase.

Our theory also suggests a boundary condition for these effects. Specifically, we propose that the types of choices presented to taxpayers—which specific budget categories they are allocating their taxes toward—will affect both the sense of voice they feel and, as a result, their tax compliance. This argument is consistent with previous research in the political domain, which argues that when

individuals express their preferences in a way that provides no positive outcome, a “frustration effect,” emerges, such that voice may have no benefit, or even exert a negative influence (Cohen, 1985): if forced to express preferences for non-preferred alternatives, people are less likely to feel that they have real voice. Similarly, Botti et al. (2009), working in the domain of emotionally painful medical decisions, showed that having choice between negative outcomes is not so much empowering as it is frustrating—in such cases, individuals would prefer to cede choice to others. Extending this previous research, we propose that if individuals feel that their tax dollars are supporting goods they do not value, the benefits of preference expression may be reduced.

We test our prediction that preference expression increases consumers’ tax compliance in three studies. In the first experiment, we create an incentive-compatible employment and taxation environment in a lab, where participants choose to pay or withhold assessed taxes. In the second experiment, participants complete an online tax-paying simulation in which they choose whether or not to take a loophole in the federal tax code. In the third experiment, we assess our proposed moderator: whether preferences are expressed across tax categories that taxpayers prefer or dislike.

Experiment 1

In experiment 1, undergraduate lab participants complete a simple task for which they are paid a fixed amount of money. They are then given the opportunity to complete a bonus task, which allows them to earn more money, but informed that this extra “income” is subject to a 30% “lab tax.” Randomly selected participants are given voice in tax payment by allowing them to express their preferences for the use of their tax dollars.

Participants were asked to put their tax dollars in an envelope and bring it to the experimenter at the end of the session. They were also told that one in eight participants would be “audited.” If selected for audit, participants who had not fully complied were subject to their \$3.00 tax plus an additional \$2.00 penalty. This method thus allowed participants a private, auditable form of compliance.

Our account suggests that preference expression should result in a higher rate of tax compliance. Furthermore, to ensure that preference expression does not elicit negative reactions, we capture satisfaction with payment. In addition, we measure

perceived audit probability, to examine whether preference expression raises concerns about heightened monitoring of payment. If individuals given voice feel their behavior will be more carefully scrutinized because of their expressed preferences, they may be both less satisfied with payment and comply simply out of heightened audit fear.

Methods

Experiment 1 followed a two-group between-subjects design in which participants were randomly assigned to either the preference expression or no preference expression condition. Participants were 189 participants who registered online to participate in a university’s lab for experimental research. As part of the online registration process, demographic information was collected (age, gender, marital status, ethnicity, and income). After registering, participants came to a physical lab with the understanding that they would receive a certain \$10 payment for completing the multi-study session. In the course of that session, participants were told that they could also complete a “bonus” task in return for an additional \$10.

Before deciding whether to complete the \$10 “bonus task,” participants were informed about the tax rate and audit probability associated with their payment for that task. In this experiment, both were held constant. All participants read that their tax rate would be 30% (\$3 out of the \$10 bonus), and that this tax rate represented the average amount of labor time devoted to tax payment in the United States (Morrison, 2013). All participants were also told that at the end of the session, an experimenter would collect all the envelopes and audit one out of every eight envelopes. Participants read that if they were audited and the experimenter found less than \$3 in their envelope, they would have to pay the full \$3 plus an additional \$2 penalty out of their \$10.

All participants chose to opt into the bonus task. The bonus task consisted of rating their enjoyment of 12 landscape and home interior pictures on a 9-point scale (1: not at all to 9: very much.) After rating the pictures, participants raised their hand to receive their \$10 payment. To facilitate payment, payment was provided in 10 \$1 bills.

Participants were then randomly assigned to either the preference expression or no preference expression condition. Participants in the no preference expression condition continued directly to the tax payment portion of the study. Participants in the preference expression condition first read:

We will allow you to provide input into how your tax money is used. You will be able to provide your thoughts about how tax money should best be used. The categories you can select as your preferred expenditure of tax money will only benefit you indirectly, but will benefit the broader community of individuals who participate in lab studies at (university). The lab administrators may take your preferences into account as they determine what to do with the lab tax money.

They were then reminded that they would pay \$3 in tax and told that they were able to allocate their money across three categories, which they were told would benefit individuals who participated in experiments and surveys in the lab in the future. The three categories were beverages, snacks, and enhanced incentives for future study participants.

Participants then completed attitudinal measures. First, they reported their beliefs regarding audit likelihood on a 7-point scale (1: I am very unlikely to be audited to 7: I am very likely to be audited). They also reported the sense of value they associated with their taxes on a 7-point scale (1: I do not value the way my lab taxes will be used to 7: I value the way my lab taxes will be used), and their satisfaction with paying their taxes on a 7-point scale (1: I feel dissatisfied with paying the tax to 7: I feel satisfied with paying the tax); the order of these questions was randomized.

Participants were reminded that their tax rate was 30% and were asked to place their tax payment in an envelope on their desk (All envelopes were numbered so that participants' responses could later be connected to their preference expression condition and other responses.) At the end of the lab session, research assistants collected the envelopes, randomly selected one in eight participants for audit, collected tax payment and penalty money consistent with prior instructions, and recorded the amount each participant had placed into their tax envelope. At no point did lab administrators view participants' other data, including responses on other measures or condition assignment. Consistent with the idea that expressed preferences were non-binding, while we did review the data on participant preferences, the tax money was in reality used to fund additional experiments.

Analysis and Results

We analyzed this data in two ways. First, we considered the likelihood of compliance with the

tax. To do this, we classified participants who had put \$3.00 or more into their tax payment envelopes as compliant and those that put less than \$3.00 as non-compliant. A logistic regression using preference expression condition to predict likelihood of compliance suggests that as expected, preference expression lowered the likelihood of non-compliance (Preference expression = 32%; No preference expression = 48.2%; $b = -.19$; Wald $\chi^2 = 4.41$, $p = .03$). Excluding nine respondents who failed to provide complete demographic information, we see that this effect remains robust when controlling for gender, ethnicity, marital status, household income, and age ($b = -.21$, Wald $\chi^2 = 4.05$, $p = .04$; see Appendix S1 for all details); gender is the sole significant covariate, such that women were significantly less likely to be non-compliant than were men ($b = -.34$, Wald $\chi^2 = 11.16$, $p = .0008$).

Second, we analyzed the exact amounts placed into the envelopes using generalized linear modeling with a negative binomial distribution, to accommodate the bi-modal nature of the tax amounts paid. Without any covariates in the model, preference expression has a significant positive effect on the amount of tax paid ($M_{\text{no preference expression}} = \1.58 , $M_{\text{preference expression}} = \2.11 ; Wald $\chi^2 = 4.18$, $p = .04$). Preference expression continues to increase tax payment when including demographic measures as covariates and without the nine individuals who provided partial data (Wald $\chi^2 = 3.94$, $p = .05$). Once again, gender was the sole significant covariate, such that men paid less (i.e., showed lower compliance) than did women ($M_{\text{male}} = \$1.40(1.57)$, $M_{\text{female}} = \$2.14(1.54)$; Wald $\chi^2 = 11.11$, $p = .0009$).

One possible explanation for the effect of preference expression on compliance is that individuals who had been given an opportunity to express their preferences felt they would be under greater scrutiny and thus, be more likely to experience an audit. To test this possibility, we estimate an analysis of variance (ANOVA) using experimental condition as well as the same covariates to estimate participants' perceptions of their likelihood of audit. There is no significant effect of preference expression on perceived likelihood of audit ($M_{\text{preference expression}} = 3.35$ ($SD = 1.63$), $M_{\text{no preference expression}} = 3.56$ ($SD = 1.82$); $F(1, 166) = 0.88$, $p = .35$). Men believe they are significantly less likely to be audited than women. ($M_{\text{male}} = 2.94$ ($SD = 1.70$), $M_{\text{female}} = 3.79$ ($SD = 3.79$), $F(1, 166) = 12.27$, $p = .0006$).

Furthermore, we test for possible mediating mechanisms that would explain the effect of preference expression on compliance. These analyses follow Preacher and Hayes (2008) bootstrapping

methodology, set at 1,000 iterations; a significant mediation is indicated by a 95% confidence interval for the indirect path that does not include zero. All analyses use preference expression condition as well as the same set of covariates as before to predict compliance amount. Perceived likelihood of audit does not mediate the effect of preference expression on compliance (95% CI for indirect effect: $-.09$ to $.04$). However, considered either with or without covariates, participants given the opportunity to express their non-binding preferences over tax use value the uses associated with the taxes more than those not given the opportunity to express such preferences ($M_{\text{preference expression}} = 4.16$ ($SD = 1.75$), $M_{\text{no preference expression}} = 3.49$ ($SD = 1.74$); $F(1, 168) = 5.21$, $p = .02$). Furthermore, bootstrapping analyses specified as before suggest that perceptions of the value of the items funded by the tax payment mediated the effect of preference expression on compliance (95% CI for indirect effect: $.02$ to $.15$). These results suggest that preference expression may operate in part by recoupling, and that this recoupling, in turn, raises compliance. In this data, we see no significant direct relationship between preference expression and satisfaction with tax payment ($M_{\text{preference expression}} = 3.37$ ($SD = 1.69$), $M_{\text{no preference expression}} = 3.25$ ($SD = 1.80$); $F(1, 168) = 0.30$, $p = .59$).

Discussion

Experiment 1 provides evidence that providing individuals with an opportunity to express their preferences about the way tax dollars are used increases tax compliance, using an incentive-compatible method in a controlled environment. These results appeared to be driven at least in part by more positive perceptions of the value that tax dollars would provide, suggesting that preference expression in this context may successfully recouple payment and benefit. We also note that taxpayer preference expression does not appear to be associated with higher concern of audit. Moreover, though asking participants to express their preference about tax dollar expenditure raised average tax payment and required some additional cognitive effort, we did not see changes in satisfaction with tax payment itself.

Of course, this tightly controlled setting is not without its limitations: the sample consisted predominantly of young students, and participants made their compliance decisions about a novel type of taxes used to benefit the laboratory—a tax about which no prior negative attitudes are likely to exist. This blank slate may allow more behavioral

flexibility than would a more baggage-laden situation. It may also be the case that this somewhat atypical procedure created upward pressure on non-compliance simply by requiring participants to return money they had already received. While this effect would have existed across both conditions, it is important to make sure that effects persist in less unfamiliar contexts, where non-compliance requires action rather than inaction. Perhaps most importantly, this experiment necessarily provided information about the way that tax dollars would be used—the categories provided could easily have created a baseline level of recoupling that drove results. While this speaks to the potential of a typical implementation of preference expression, these data do not allow us to see whether it is information or preference expression itself that matters. Experiment 2 was designed to conceptually replicate the results of Experiment 1, while also addressing some of these limitations.

Experiment 2

Experiment 2 tests for preference expression's effects in a population of taxpayers encompassing a wider demographic range than seen in Experiment 1, and also uses a more familiar income tax payment scenario as a decision-making context. In this experiment, we compare the typical tax-paying experience (i.e., no information about current tax expenditures and no preference expression opportunity) to one where consumers have information about the specific ways that taxes are paid but no opportunity to express their preferences as well as to two preference expression conditions: one that includes preference expression but provides no information about current expenditure levels and a second that includes both information about current expenditures and the opportunity to express one's preferences about their preferred allocations in a non-binding way. We focus on a consequential and typical moral decision made by many taxpayers: whether or not to take a questionable loophole when completing their income tax returns.

We again collect information about both satisfaction and perceptions of audit likelihood. Finally, we collect information about anticipated payment date, as well as post-treatment attitudes that have been assessed in prior work on tax morale, to examine whether any of these planned behaviors or attitudes shift in response to preference expression.

Methods

Participants were 267 English-speaking, US citizens who had opted into Amazon's Mechanical Turk panel, and received a \$1.00 payment for completing the experiment. All participants read an informed consent prior to opting-in to the survey. In this informed consent, they were informed that the survey would focus on Federal income tax dollars and were assured of the confidentiality of their responses. They then provided demographic information (see Table 1). The median reported household income was \$43,000/year.¹

The experiment followed a two (preference expression, no preference expression) \times two (informed, uninformed) between-subjects design, resulting in four focal conditions: One group of participants was provided neither information about current tax spending nor an opportunity to express their preference (control group) before proceeding

to a compliance measure. A second group of participants was provided information about existing tax expenditures but was provided no preference expression opportunity (information-only group). A third group was provided an opportunity to express their preference, but was not given information about existing tax expenditure rates (uninformed preference expression group). A fourth group was provided both information about precise existing tax expenditure rates in different categories and was provided an opportunity to express their preferences (informed preference expression group). Participants in the informed conditions viewed current spending information in both list and pie-chart form, included as Appendix A. In addition, within the two preference expression conditions (uninformed preference expression and informed preference expression), the percentage of tax payment over which participants were told they could express their preferences was varied at 10%, 25%, and 50%.

Table 1
Experiment 2 Sample Characteristics by Condition

Sample Characteristic	Experimental Condition				Significance test
	Control (<i>n</i> = 32)	Information only (<i>n</i> = 23)	Preference expression only (<i>n</i> = 104)	Information plus preference expression (<i>n</i> = 98)	
Percent female	52.13%	56.5%	47.12%	53.1%	$\chi^2 = (3) 1.13, p = .77$
Paid tax last year (vs. did not or do not recall)	56.25%	52.17%	59.62%	58.16%	$\chi^2 (6) = 2.71, p = .84$
How taxes filed in the last year					
Filed by themselves electronically	59.38%	69.57%	66.35%	64.29%	$\chi^2 (12) = 11.02, p = .53$
Filed electronically with help of tax preparer	31.25%	8.70%	19.23%	22.45%	
Filed by themselves by mail	6.25%	4.35%	7.69%	8.16%	
Filed by mail with help of tax preparer	3.13%	17.39%	5.77%	5.10%	
Filing status					
Filing status: single	40.63%	65.22%	56.73%	47.96%	$\chi^2 (9) = 13.26, p = .15$
Filing status: married filing jointly	53.13%	30.43%	31.73%	41.84%	
Filing status: married filing separately	0.00%	0.00%	5.77%	1.02%	
Filing status: qualifying widow or widower	6.25%	4.35%	5.77%	9.18%	
Number of dependents	.81 (.97)	.52 (.99)	.75 (1.09)	1.06 (1.37)	$F(3, 252^a) = 1.89, p = 13$
Age	37.31 (13.60)	36.30 (10.85)	33.57 (9.94)	36.24 (12.25)	$F(3, 253) = 1.41, p = .24$
Household income	40,031 (22,200)	34,538 (26,010)	49,333 (34,830)	55,490 (36,772)	$F(3, 248) = 3.38, p = .02^b$

^aOne respondent entered 1,000 dependents; difference is non-significant whether this individual is or is not included in the analysis.

^bSome respondents did not provide income information, where information is available, it is included in the analysis.

Participants in the information-only condition then proceeded directly to the compliance measures. Participants in the uninformed preference expression and informed preference expression conditions read the following:

Imagine that you are told that you can provide input about how [10%/25%/50%] of your tax payment would be spent across categories used by the Federal government. The government would take your opinion into account for this portion of your tax dollars. Below, please provide the allocation you would advise for [10%/25%/50%] of your income tax dollars.

Participants were given as much time as they wished to express their preferences, by allocating funds across the same spending categories. A running total at the bottom of the screen allowed them to see the percentage they had allocated across the categories. They were instructed to ensure that their preferred answers totaled 100%.

Next, we assessed participants' likelihood of compliance, by presenting the following situation: "Imagine that you were aware of a tax loophole that would enable you to avoid paying 10% of your income tax bill. However, you aren't sure you really qualify. Would you take the tax loophole to lower your tax bill?" Participants answered this question by indicating yes or no.

As in Experiment 1, we assessed participants' beliefs about their likelihood of being audited by the IRS on a 7-point scale (1: very unlikely to 7: very likely). We also asked participants on what date they intended to pay their taxes, given a due date of April 15th. Furthermore, we again asked participants to rate their satisfaction with tax payment on a 7-point scale (1: I feel dissatisfied with paying taxes to 7: I feel satisfied with paying taxes). While not focal to our account, we assessed the possible impact of preference expression on factors previously identified as drivers of tax morale and compliance: belief in the redistributive role of taxes, attitudes toward other taxpayers, trust in government, prosocial attitudes, and general tax sentiment. Items are included in Appendix B. Finally, participants rated how seriously they took the survey on a 7-point scale (1: not at all seriously to 7: very seriously).

Analysis and Results

We first analyzed data relevant to the decision to take the tax loophole for the full sample of 267

participants. Note that sample sizes per cell—if considered at the condition-aggregate level—vary because preference expression cells are subdivided into percentage amounts, each of which was targeted to include 20–30 per cell. For example, within the informed preference expression condition were included three sub-conditions: preference expression over 10%, 25%, and 50%, each of which was targeted to include 20–30 participants. The same was the case within the uninformed preference expression conditions. As a result, the uninformed and informed preference expression conditions, collapsed across percentages, contained a larger number of participants than did other conditions. Overall, participants reported taking the survey seriously (range = 3–7; $M = 6.83$, $SD = 0.53$), regardless of condition ($p = .21$). Most background characteristics do not vary significantly across conditions (Table 3). However, contrasts show that household income is significantly lower in the control condition than in the uninformed preference condition ($F(1, 248) = 5.08$, $p = .03$) and informed preference expression condition ($F(1, 248) = 7.22$, $p = .008$). To address the role of demographic differences on compliance, we will first report simple chi-square analyses on the entire sample, followed by analyses controlling for demographics. Participants who did not provide complete demographic information are omitted only from the latter analysis.

We first examine likelihood of compliance (non-loophole adoption) as a function of condition, without any covariates, using the full sample. An overall chi-square test using experimental condition to predict compliance suggests that, as expected, experimental condition has a significant effect on compliance likelihood ($\chi^2(3) = 7.61$, $p = .05$). Follow-up chi-square analyses comparing the experimental groups to one another indicate that first, simply providing information offers no significant difference in compliance relative to the control group (control: 36.4% vs. information only: 30.4%; $\chi^2 = 0.22$, $p = .64$). Second, the two preference expression conditions (uninformed and informed) do not differ significantly from one another (uninformed preference expression: 52.3% vs. informed preference expression: 55.9%; $\chi^2 = 0.27$, $p = .60$), but both preference expression conditions show marginally or significantly greater compliance than the information-only condition (30.4%): information only versus uninformed preference expression (52.9%, $\chi^2 = 3.63$, $p = .06$); information only versus informed preference (55.9%, $\chi^2 = 4.86$, $p = .03$). Finally, both the informed preference expression

group and uninformed preference expression group display somewhat higher levels of compliance relative to the control condition although both effects are just or marginally significant ($p = .05$ and $p = .09$, respectively).

We next estimate a model including the experimental factors and their interaction as we as all available covariates (household income, filing status, gender, age, means of filing, and whether they paid taxes in the prior year) to predict likelihood of compliance. Five participants did not provide all demographic information, yielding a sample size of 262 for this analysis. This analysis yields a significant positive main effect of preference expression on compliance ($b = .19$, Wald $\chi^2 = 3.55$, $p = .05$), but no main effect of information ($b = -.009$, Wald $\chi^2 = 0.00$, $p = .95$), and no interaction of the two factors ($b = .02$, Wald $\chi^2 = 0.01$, $p = .91$). No other covariate effects were significant at conventional levels; see Table S2 for all statistics.

Further analysis tested whether preference expression's effects were sensitive to the proportion of the tax bill about which participants expressed their preferences. For this purpose, both non-preference-expression conditions were coded as offering 0% preference expression, and the full sample is used. A chi-square test reveals a significant effect of percentage on likelihood of taking the loophole ($\chi^2(3) = 8.68$, $p = .03$). More specifically, preference expression regarding 10% of one's tax bill generates a significant change in compliance compared to the information-only condition (52.9% vs. 33.9%; $\chi^2(1) = 4.45$, $p = .03$), as does preference expression over 25% of one's tax dollars (59.46% vs. 33.9%; $\chi^2(1) = 8.31$, $p = .004$). Interestingly, however, the difference between the 0% and 50% conditions is only marginally significant (48.28% vs. 33.9%; Wald $\chi^2(1) = 2.98$, $p = .08$).

As in Experiment 1, we analyzed participants' beliefs about their likelihood of being audited. A 2 (information condition) \times 2 (preference expression condition) ANOVA suggests no significant main effects or interactions between the two factors (information ($F(1, 263) = 0.53$, $p = .47$); ($F(1, 263) = 0.45$, $p = .50$); interaction ($F(1, 263) = 0.19$, $p = .66$). No main or interactive effects of the focal factors become significant when covariates are added (all $p < .08$); as in Experiment 1, we again observe only that men believe they are significantly less likely to be audited than are women ($M_{\text{males}} = 2.06$ (1.07), $M_{\text{females}} = 2.52$ (1.26); $F(1, 245) = 8.02$, $p = .005$).

As in Experiment 1, we again examined the relationship between preference expression and payment satisfaction. A 2 (information condition) \times 2

(preference expression condition) ANOVA reveals no significant main effects or interactions between the two factors on payment satisfaction (information ($F(1, 263) = 0.52$, $p = .47$); preference expression ($F(1, 263) = 0.00$, $p = .98$); interaction ($F(1, 263) = 0.17$, $p = .68$). No effects become significant when all covariates are added to the model (all $p > .48$); we also see no significant effects of any covariates.

Finally, we test for relationships between the experimental conditions and the ancillary battery of planned behavior and attitudinal measures. Although not focal in our theory, these measures allow us to test for other potential effects of preference expression. For the analysis of planned payment date, 10 respondents were removed from the sample for providing nonsensical responses on the relevant open-ended measure (asked to specify the day of the month on which they would pay taxes, they entered a number greater than 31), resulting in a sample of 257 individuals for this analysis. A 2 (information condition) \times 2 (preference expression condition) ANOVA reveals significant main effects of preference expression ($F(1, 253) = 14.34$, $p = .0002$) and information ($F(1, 253) = 8.96$, $p = .003$) and a significant interaction of both factors ($F(1, 253) = 4.65$, $p = .03$) on anticipated payment date. Planned contrasts reveal that when information is provided, preference expression has an accelerating effect on payment date, shifting it from approximately April 29th to March 8 ($F(1, 253) = 15.55$, $p = .0001$). However, information can in fact create delay when it is not accompanied by preference expression, shifting anticipated payment date from approximately March 15th to April 29th.

To accommodate the different scales used in the attitudinal questions, all measures were standardized. We then conducted a factor analysis using a varimax rotation on these standardized measures. Items that loaded at .5 or above and showed no cross-loadings were averaged to create indices for four factors: general anti-tax sentiment, redistributive tax philosophy, prosocial engagement, and trust in government. We then estimate a 2 (information condition) \times 2 (preference expression condition) ANOVA to predict each index (means and standard deviations are in Table 2); where responses were not provided, degrees of freedom vary. This analysis identifies a significant effect of preference expression ($b = -.13$, $F(1, 262) = 5.14$, $p = .02$) and a marginally significant interaction of preference expression and information condition ($b = -.10$, $F(1, 262) = 2.94$, $p = .09$). Contrast analyses further reveal that informed preference

Table 2
Experiment 2 effects of agency on standardized attitude indices

Preference Expression Condition	Attitude Index							
	Redistributive philosophy		Prosocial engagement		Trust in government		Anti-tax sentiment	
	No information	Information	No information	Information	No information	Information	No information	Information
No preference expression	1.19 (.82)	1.48 (.77)	-.10 (1.01)	-.10 (.84)	.14 (.99)	-.17 (.91)	.12 (.78)	.29 (.88)
Preference expression	1.20 (.77)	1.29 (.81)	.08 (.82)	-.02 (.75)	-.01 (.91)	.004 (.90)	.05 (.75)	-.16 (.70) ^{a,b,c}

^aDifferent from control (no preference expression, no information) condition; $p < .1$

^bDifferent from information-only condition; $p < .05$.

^cDifferent from preference expression only condition; $p < .05$

expression significantly lowered general anti-tax sentiment relative to the uninformed preference expression condition ($F(1, 262) = 4.35, p = .04$) and created a marginally significant decrease in anti-tax sentiment relative to the control condition (no information, no preference expression) ($F(1, 262) = 3.45, p = .06$). Preference expression, information, and their interaction had no significant effects on participants' redistributive tax philosophy, prosocial engagement, or trust in government.

Discussion

Thus far, our experiments suggest that allowing individuals to express their preference about the use of their tax dollars prompts higher levels of compliance with tax-paying norms. This second experiment adds the insight that this effect seems to be specific to the provision of the opportunity to express one's preference about the use of tax dollars, and has its strongest effect when it is accompanied by information about existing expenditure. However, information alone does not provide a significant enhancement of compliance likelihood relative to baseline—the addition of voice to

information appears to be important in raising compliance. Furthermore, we find that preference expression may lower general anti-tax sentiment and provide some acceleration of payment intention although we hesitate to lean too heavily on the latter finding without behavioral evidence that these intentions would, in fact, be realized. While we can only conjecture on the reason for the information-only delay, it may be that participants reacted negatively to the information provided. In short, the recoupling in this study, when not accompanied by a preference expression opportunity, might not have been negative enough to completely eliminate one's belief that they would pay in full, but may have lowered enthusiasm about doing so.

These data however also raise some questions. First, the lack of difference between the 0% and 50% conditions is somewhat puzzling, as one might have anticipated that as the amount of control provided by preference elicitation increased, so should compliance tendencies. To test the stability of this finding, we re-ran the present experiment including only the informed preference expression conditions but with larger sample sizes (55–57) in each cell and varying percent at 10% intervals up to 50%.

Table 3
Follow-up Test of Non-compliance by Informed Preference Expression Percentage

Percent preference expression (<i>n</i>)	0% (92)	10% (57)	20% (55)	30% (57)	40% (55)	50% (57)
Percent non-compliance	47.8%	29.8%**	27.3%**	33.3%*	34.6%	50.1%

*Significantly different from 0% condition at $p < .1$; **Significantly different from 0% condition at $p < .05$.

We purposely oversampled the control (0% cell; $n = 92$) given the number of cells against which it would be compared. Once again, chi-square tests show that compliance proportions differ significantly based on percentage allocated ($\chi^2 = 12.83$, $p = .02$); results are in Table 3. The difference in proportion compliance between control and preference expression conditions is significant at 10% ($\chi^2 = 4.78$, $p = .03$) and 20% ($\chi^2 = 6.05$, $p = .01$), but the positive effect of preference elicitation erodes at 30% ($\chi^2 = 3.02$, $p = .08$), and is outside any conventionally meaningful levels at 40% ($\chi^2 = 2.57$, $p = .12$) and 50% ($\chi^2 = .13$, $p = .71$).

In this small study, we also probed a few possible explanations for this finding. Regression analysis showed that two measures changed linearly with percent: the sense of difficulty in deciding where the funds should be spent ($b = .01$, $F(1, 371) = 5.99$, $p = .01$) and the extent to which participants trusted other taxpayers to make good decisions ($b = -.01$, $F(1, 371) = 9.90$, $p = .002$). Neither of these variables were statistically significant mediators of the likelihood to take the tax loophole at any level, either tested individually or simultaneously.

This decay at higher percentage levels, while requiring further study, may help make preference expression more plausible as a practical intervention. We note that even were Experiment 2's non-binding allocations fully enacted by policymakers in the proportions assigned in the experiment, many categories would experience little change. While military spending would decrease substantially (from 24.9% to 18.8%) and education spending increase (from 3.6% to 18.2%) based on our sample's responses, changes of less than 1% would be seen in many other categories, such as international aid, immigration and law enforcement, agriculture and natural disaster relief. Given that effects on compliance were seen when preference expression was applied to as little as 10% of tax dollars and decay after 20%; however, it would not be necessary to elicit preferences at the aggregate level in our data, which extended to 50%. Thus, it may be possible for policymakers to acknowledge and respond to non-binding preferences in many categories without disrupting budgets beyond the levels ordinarily seen due to changes in administrative or political policy.

We also still face a number of open theoretical questions. Chiefly, we have not yet directly measured consumers' sense of voice or established that it explains the relationship between preference expression and tax compliance. To show this relationship more clearly, we designed experiment 3 to

vary in the extent to which preference expression's effects on these mediating variables both should and should not emerge. Specifically, we varied the valence of the categories over which preferences could be expressed. When preferences can be expressed over generally positive categories, we anticipate replication of our prior findings. We will test for the role of voice in explaining these effects. However, when participants only have the option to express their preferences across less-preferred alternatives, we anticipate that participants may not feel a real sense of voice, thus failing to reduce downward pressure on compliance likelihood.

Experiment 3

Experiment 3 was designed to shed further light on our proposed theoretical account. First, experiment 3 explores the role of a critical moderator of our effect: does the effect of preference expression persist when individuals are given the opportunity to express their preference over the use of tax dollars in less-preferred categories? In experiment 1, the uses of tax dollars generally provided benefits that would accrue to lab participants, and in experiment 2, people could allocate tax dollars to budget categories that aligned with their political beliefs, even if the benefits to themselves might be fairly indirect. But does expressing preferences across less attractive options continue to translate into a real sense of voice—or an enhanced likelihood of compliance?

Second, in our prior studies, we manipulated preference expression (vs. no expression), but did not directly assess measure individuals' sense that they in fact had voice as a result of this experience. In experiment 3, we both measure perceived voice and test whether voice mediates the effect of preference expression on tax-paying likelihood. We also collect measures of other potential explanatory constructs. For example, it may be that individuals given a preference expression opportunity feel that they will personally benefit more from the allocated tax dollars, thus converting the tax compliance decision from a moral choice to a rational cost-benefit calculation. It may also be that individuals feel that they have more freedom or choice in paying their taxes when they have expressed their preferences, and this sense of agency increases compliance (Bandura, 2000). Alternately, individuals who have the opportunity to express their preferences may feel that they better understand the tax system, and thus, are more willing to comply, or may feel an emotional benefit from preference expression that raises compliance.

Methods

In this study, 300 Amazon Mechanical Turk (M (age) = 34 years, 54% female) workers were randomly assigned to one of three conditions: a control condition where no preference expression opportunity was provided, a positive voice condition that included an option set designed to be positively viewed, and a negative voice condition that included an option set designed to be negatively viewed. Preference expression was offered over 20% of one's hypothetical tax payment, in line with results from experiment 2. Participants read the following:

Imagine that when you sit down to pay your bills, you find a notice telling you that you have a \$40 tax due to your local municipal or community government. All residents of your community are required to pay these taxes.

In the two preference expression conditions, participants then read:

A portion of the tax you pay will go to support the community's general fund, which takes care of recurring overhead costs.

You get to provide input about where 20% of your tax dollar goes.

Your local government will take your opinion into account when deciding how to spend your tax dollars.

Which local budgets would you send 20% of your payment to, if you had the chance?

Indicate which sections of the community budget you would like to allocate your tax dollars to. This must total to 100% before you can continue.

In the positive preference expression condition, participants could divide their 20% across the following categories: sidewalk improvement, street and bridge repair, water and sewer services, pools, parks and recreation, public safety and security, community holiday events and special event facilitation. In the negative preference expression

condition, participants could divide their 20% across sidewalk removal, resident ticketing, sewer mandatory shutdown, Wal-Mart feasibility study, quiet zone fines, neighborhood border policing, and construction zone enlargement.

All participants then read:

Now imagine that before you send in your tax payment, you receive a receipt clearly stating that your local taxes have been paid for this year.

However, as proof of payment, a copy of last year's check is attached.

And since you haven't sent your payment in yet, you know that you still owe \$40 for this year.

How likely are you to mail your \$40 in for this year's taxes?

Answers to this question, anchored at 1 (very unlikely) and 5 (very likely) constituted our primary dependent measure.

We then collected measures intended to capture our proposed process measures, presented in randomized order and anchored with bipolar statements that translate to scores from 1 to 7, where the left point is represented by the text in parentheses and the right is represented by the text without the parenthetical included: Voice ("I feel that I have (no) a lot of voice in my local tax payment"), understanding of the coupling between payment and benefit ("I (can't) can connect my tax payment to its benefits"), personal benefit ("My tax dollars will (not) pay for things that directly benefit me, personally"; "My tax dollars will (not) pay for things that benefit others" (reverse-coded); $r = .70$); felt agency ("Paying taxes involves (no) choice"; "I have (no) control in the domain of taxes; $r = .55^2$), and a manipulation check related to the valence of the option set ("I (do not) feel positive about the things my local tax dollars do; My tax dollars will (not) pay for things I value; I (do not) like the things that will be done with my tax dollars; 1 = strongly disagree to 5 = strongly agree, $\alpha = .77$). For participants in the preference expression conditions, we also collected affective measures: "While expressing your preferences for the use of tax dollars, to what extent did you feel: conflicted, overwhelmed, happy (reversed)" on a scale

anchored at 1 = (not at all) to 5 = (extremely), $\alpha = .83$. We also collected demographic measures and a measure of individual psychological reactance; these measures had no effect in any analyses and are not reported further.

Analysis and Results

Manipulation check. To confirm that our positive and negative choice sets were appropriately constructed, we estimated an ANOVA using experimental condition to predict the valence manipulation check. This analysis yielded a significant omnibus effect ($F(2, 293) = 5.06, p = .007$). Planned contrasts established both that the positive option set was seen as directing tax dollars to more positively viewed uses than the no-information control condition ($M_{\text{positive}} = 4.46$ vs. $M_{\text{control}} = 4.09$; $F(1, 293) = 4.21, p = .04$), and that the positive option set was seen as more positive than was the negative option set ($M_{\text{negative}} = 3.88$; $F(1, 293) = 9.87, p = .001$). However, there was no significant difference on this measure between the control and negative option set condition ($F(1, 293) = 1.32, p = .25$), suggesting that individuals' baseline inferences may be that their tax dollars are not used in ways that match their preferences.

Emotion. The condition involving preference expression over the positive choice set generated less negative emotion than did the condition allowing preference expression over the negative choice set ($F(1, 189) = 10.55, p = .001$).

Payment likelihood. Estimating the same ANOVA as used for the manipulation check, we observe a main effect of experimental condition ($F(2, 293) = 5.05, p = .007$). Planned contrasts revealed that expressing preference across the positively valenced options led to a significant increase in tax payment likelihood relative to the control ($M_{\text{positive}} = 4.21$ vs. $M_{\text{control}} = 3.59$; $F(1, 293) = 9.71, p = .002$) and relative to expressing preferences across negatively valenced options ($M_{\text{negative}} = 3.78$; $F(1, 293) = 4.60, p = .03$). Mirroring the manipulation check, there was no significant difference in payment likelihood between the control condition and preference expression across negative options, however ($F(1, 293) = 0.90, p = .34$).

Voice. We observed a similar pattern for voice. Within the omnibus test ($F(2, 293) = 3.06, p = .04$), we observe that preference expression in the positive option set significantly increased participants' sense of voice relative to the control condition ($M_{\text{positive}} = 4.16$ vs. $M_{\text{control}} = 3.65$; $F(1, 293) =$

$4.98, p = .02$) and relative to expressing preferences across negatively valenced options ($M_{\text{negative}} = 3.68$; $F(1, 293) = 4.28, p = .04$). Critically, however, preference expression in the negative option set did not translate into a sense of voice to a greater extent than in the control condition ($F(1, 293) = 0.02, p = .89$).

Alternate explanations. We also estimated the same ANOVA to examine whether the experimental treatment had created significant differences in other perceptions, such that they might offer better explanations of changes in payment likelihood. Experimental condition did not yield significant differences in felt agency ($M_{\text{negative}} = 2.95, M_{\text{positive}} = 3.24, M_{\text{control}} = 3.17$; $F(2, 293) = 1.08, p = .34$), or in the degree to which individuals reported understanding the relationship between tax payments and their use ($M_{\text{negative}} = 4.12, M_{\text{positive}} = 4.32, M_{\text{control}} = 3.93$; $F(1, 293) = 1.08, p = .34$), and created a marginally significant difference in the sense that tax payments would lead to their own personal benefit ($M_{\text{negative}} = 3.53, M_{\text{positive}} = 3.88, M_{\text{control}} = 3.82$; $F(2, 293) = 2.53, p = .08$).

Mediation. Finally, we tested to see whether participants' sense of voice explained the relationship between preference expression and the likelihood of tax payment. Given that only the positive choice condition yielded differences in payment likelihood and voice, we used contrast codes to compare the positive preference expression condition (coded 1) to the other two conditions (coded -1). This analysis shows that the confidence interval of the indirect effect of positive preference expression on likelihood of tax payment through perceived voice does not include zero (95% CI: .004 to .08). The other measured constructs—including emotion, understanding, felt agency, and personal benefit—do not show non-zero confidence intervals for indirect effects, whether entered individually or together.

Discussion

Though in a different context, these results conceptually align with those of Botti et al. (2009) study of negative choices in the medical domain: providing voice across less-preferred categories does not offer positive benefits. In our experiment, providing negative options did not lead participants to feel that they had voice, resulting in no increase in their likelihood of tax compliance. Notably, however, expressing preference across less-preferred alternatives also did not decrease payment likelihood relative to baseline, suggesting that when uncertain about taxpayers' preferences, it may still

be safe to offer the opportunity for citizens to express their preferences.

The similarity on our critical measures between the control condition and the negative voice condition indicates that individuals' default beliefs may be that their tax dollars are generally not used in ways that match their preferences, suggesting that providing voice may allow a chance for governments to correct this misperception—if indeed tax dollars are allocated at least in part to categories preferred by citizens. Future research should explore the proportion of acceptable voice options required to be present in order for preference expression to yield positive results. Our theory would predict that the presence of one positive expenditure category could have been enough to create a real sense of voice, as taxpayers could direct all of their dollars toward that single desired expenditure. However, a mixed bundle could also simply remind individuals that many uses of their tax dollars do not match their preferences, thus failing to recouple paying with benefit in a way that raises the value of tax payment. A third option is that taxpayers recognize that many categories that are irrelevant to them personally may benefit others in important ways. Responses to voice about spending for others may depend on idiosyncratic perceptions of connectedness or beliefs about redistribution in general. Finally, in experiment 3, note that we also did not observe that preference expression increased individuals' belief that they understood the uses of their tax dollars; while this result may simply be a reflection of overconfidence or past experience with such taxes, it may suggest that the primary role of voice is not always to recouple payment and benefit. Instead, the mediating effect of perceived voice in explaining changes in the likelihood of tax compliance, consistent with our theoretical account.

General Discussion

In three experiments, we show an effect of preference expression on increasing tax compliance. First, in an incentive-compatible laboratory setting, participants allowed to express their preferences about where their tax dollars would be allocated were less likely to underpay their taxes. Second, in a more representative sample in which participants completed a simulated federal income tax filing, those allowed to express their preferences were less likely to state that they would take a questionable loophole. Experiment 1 suggests that increased

compliance arises in part due to a decrease in decoupling between tax payments and benefits: those given the opportunity to express their preferences reported seeing more value in how their tax dollars would be used, and this increase in value mediated the effect of preference expression on compliance. Importantly from both a conceptual and public policy standpoint, Experiment 2 suggests that while informed preference expression increases tax compliance relative to a control condition that mirrors current tax-paying experiences, information alone does not have the same effects. Experiment 3 shows that these effects have an important boundary condition—expressing preferences for public goods that are not preferred does not enhance compliance. This appears to be because doing so does not enhance individuals' sense of voice.

In addition, we see no negative effects of preference expression on tax satisfaction in our experiment. As such, it appears that providing taxpayers the opportunity to express their preferences may offer a means of encouraging tax compliance using a positive incentive, in line with calls to reduce tax avoidance without reliance on negative affect, fear, or threat (Smith and Stalans, 1991)

In our experiments, preferences were expressed but were not binding, suggesting that the mere opportunity to express preferences changes their tax-paying experience. This treatment differs from the binding voting behaviors examined in Alm et al. (1993, 1999) and Lambertson (2013), providing initial evidence that the actual reallocation of resources may not be necessary to observe changes in compliance.

The magnitude of compliance shifts generated by preference expression in our studies is substantial and somewhat consistent. In the first two experiments, we observe approximately a 16% increase in the proportion of participants who choose the more compliant option offered, whether to pay their tax burden or to reject a questionable loophole. This effect occurs whether audit possibility is concrete and imminent, as in Experiment 1, or distant and uncertain, as in Experiment 2, and regardless of whether real money is at stake, as in experiment 1, or in a hypothetical scenario, as in experiments 2 and 3. In addition, it appears that the effects of preference expression persist above and beyond effects driven by demographics.

Simply providing expenditure information to taxpayers may lead to more informed voting (Beckett & King, 2002), and has been proposed as a means of increasing tax compliance. To the extent that

information recouples payment and benefit, it is possible that such positive effects will emerge. However, our data do not bode well for information that is provided without a means of preference expression. Specifically, our findings suggest that not only may information alone fail to increase compliance, but also it may not generate ancillary benefits created by an opportunity to express one's preference. First, results from experiment 1 suggest that preference expression's effects are in part driven by enhanced appreciation of the value of tax-funded services. Misalignment between actual and preferred spending may explain the delaying effect of information in the absence of preference expression, as seen in Experiment 2. Indeed, in experiment 3, we show that if preference expression is limited to dislike categories of spending, it no longer yields greater compliance; the ability to express preferences outside current spending may be particularly important in such cases. We also note, however, that uninformed preference expression's effect on compliance, relative to a control condition, is not as robust as is the effect of informed preference. Thus, we propose that the power of both preference expression and information is maximized when both are offered at the same time.

Second, in experiment 2, we note that preference expression may lower general anti-tax sentiment among participants. This effect is consistent with work arguing that enhanced citizen engagement can reduce antigovernment sentiment (Ebdon & Franklin, 2006), but suggests that preference expression's effects may be localized to the domain in which voice is provided. If preference expression can prompt broader acceptance of taxes, policymakers may be able to both argue for and allocate new tax dollars in ways that enhance perceived benefits among taxpayers. Such effects convert the tax payment experience from a uni-directional, passive experience to a bi-directional "nudge," whereby consumers reward government for providing voice while also actively communicating their preferences.

These studies are not without limitations and their results should be considered with caution until replicated in a large-scale field trial. Although tax compliance experiments in the lab tend to be considered externally valid (Alm, Bloomquist, & McKee, 2015), their translation to the field remains an important step to inform evidence-based policymaking (Hallsworth et al., 2014). Furthermore, as with any behavioral intervention, the downstream outcomes and permanence of the effects we observe here are important to consider. Part of preference

expression's effect may be determined by the extent to which policymakers are responsive to the preferences expressed by taxpayers. It may be that in some cases, policymakers are simply unable to reallocate funds in ways desired by taxpayers. In many community-based programs where new taxes are levied to fund-specific projects, preference expression would not disturb a pre-existing spending plan. Federal income taxes, however, are committed to a wide variety of non-discretionary spending categories, though as noted in our discussion, the actual spending shifts requested by taxpayers may be relatively small.

Furthermore, prevailing political climates may affect results such as those we report; during the course of this research, for example, the US Presidential administration has changed. The effects of voice opportunities may rise or fall as does trust or antipathy toward leadership; in more skeptical times, stronger interventions may be necessary to motivate tax compliance.

It is also possible that allowing citizens to express their tax preferences would increase compliance in the first year of implementation, but such effects would wear off as citizens became accustomed to their new tax form. Indeed, to the extent that voicing preferences led citizens to expect government to respond rapidly to those preferences, preference expression not mirrored by correspondent outcomes might actually lead to citizen dissatisfaction, consistent with Ulbig (2008)'s conclusion that, "a voice that is perceived to have no influence can be more detrimental than not perceiving a voice at all" (p. 523). Indeed, if taxpayers' hopes that their voice will change spending create unrealistic expectations, they may yet try to exit the tax-paying system via non-payment. In fact, they may feel licensed to engage in even more substantial moral failures, as a means of restoring fairness to a system they feel has broken its implicit promise. Our results, however, suggest that at least in the short term, offering citizens the chance to merely voice their tax preferences in a context where they may be taken into account can substantially impact tax compliance. Furthermore, if taxpayer voice offers a positive experience, it may be less subject to adaptation than would voice over an equivalent purchase (Van Boven, 2005), particularly given the relatively long timeframe between tax payments. If preference expression experiences are even marginally positive and not dissatisfying, they may in fact gain value with time. Given the huge sums of money that governments fail to collect in taxes—and given that the cost is merely small edits to

existing tax forms—simple preference expression opportunities may transform the tax payment process to an experience with fiscal, informational and social rewards for taxpayers and government alike.

Endnotes

¹This is lower than the US 2011 median (\$50,054) but consistent with the slightly lower household income reported in Mechanical Turk samples (see Paolacci, Chandler, and Ipeirotis (2010)).

²Given the relatively low correlation between these constructs, we also analyzed them as individual outcomes. Neither shows a significant effect of experimental condition: Paying tax involves choice ($F(1, 293) = .21, p = .81$); I have control in the domain of taxes ($F(1, 293) = 1.95, p = .14$).

Appendix A

Experiment 2 List and pie-chart format display of current tax allocation.

National Defense: Military personnel salaries and benefits; Ongoing operations, equipment and supplies; Research, development weapons and construction; Atomic energy defense activities; Defense-related FBI activities and additional national defense 24.9%.

Health Care: Medical and Children’s Health Insurance Program; Medicare physician, prescription drug, and other payments; Health research and food safety; Disease control and public health

services; COBRA tax credit and additional health care activities 23.7%.

Job and Family Security: Unemployment Insurance; Food and nutrition assistance; Housing assistance; Earned income, Making Work Pay, and child tax credits; Supplemental Security Income; Federal military and civilian employee retirement and disability; Child care, foster care, and adoption support; Temporary Assistance for Needy Families; Railroad retirement and additional income security 19.1%.

Education and Job Training: Elementary, secondary, and vocational education; Student financial aid for college; Job training and employment services; Employment training for people with disabilities and additional education and Job services 3.6%

Veterans’ Benefits: Income and housing support; Healthcare; Education, training, and additional veterans benefits 4.5%.

Natural Resources, Energy and the Environment: Water and land management; Energy supply and conservation; Environmental protection and other energy and natural resources 2.0%.

Science, Space and Technology Programs: NASA; National Science Foundation and additional science research and laboratories 1.0%.

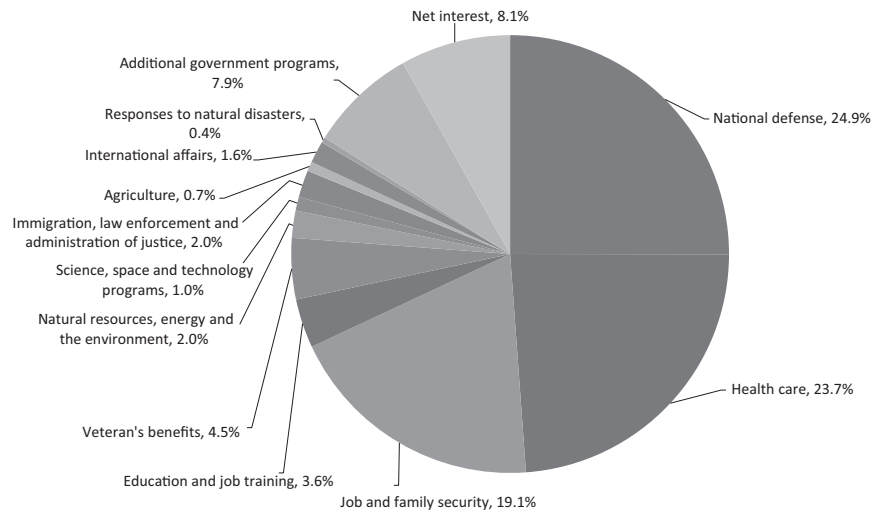
Immigration, Law Enforcement and Administration of Justice 2.0%.

Agriculture 0.7%.

Responses to Natural Disasters 0.4%.

Additional Government Programs 7.9%.

International Affairs: Development and humanitarian assistance; Security assistance; Foreign affairs, embassies, and additional international affairs 1.6%.
Net Interest 8.1%.



Appendix B

Experiment 2 Attitude Indices

Anti-Tax Sentiment ($\alpha = .76$)

All (1 = strongly disagree, 5—strongly agree).

No one should have to pay income taxes in the United States.

The economy cannot grow unless taxes are cut.

People will work harder if their taxes were cut.

I would be comfortable with higher national debt if my taxes were cut.

Trust in Government ($r = .70$)

“I would distrust the tax system” to “I would trust the tax system” (5-point, bipolar scale).

I trust government to do what’s best for citizens (1 = strongly disagree, 5—strongly agree).

Redistributive Beliefs ($\alpha = .75$)

All (1 = strongly disagree, 5—strongly agree).

I am my brother’s keeper.

Individuals with more resources are responsible for helping the poor to improve their standard of living.

We have a moral responsibility to make sure the poor can meet their basic needs.

Government should redistribute wealth by heavy taxes on the rich.

Prosocial Engagement ($\alpha = .75$)

All (1 = strongly disagree, 5—strongly agree).

I feel very connected to those around me.

I care a great deal about my community.

I am interested in volunteering for charitable causes at some points in the next 6 months.

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Supporting Information

Additional supporting information may be found in the online version of this article at the publisher's website:

Table S1. Experiment 1 effect of taxpayer preference expression on noncompliance.

Table S2. Experiment 2 effect of taxpayer preference expression on compliance.

Appendix S1. Methodological detail.