AUDIT STATE DEPENDENT TAXPAYER COMPLIANCE: THEORY AND EVIDENCE FROM COLOMBIA

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ABSTRACT

We develop and analyze a dynamic model of individual taxpayer compliance choice that predicts "audit state dependent taxpayer compliance", by distinguishing between naïve and myopic behavior versus sophisticated and forward-looking behavior. We then test experimentally the audit state dependent model by reporting the results from the first tax compliance experiment run in Colombia. Consistent with previous studies as well as theoretical predictions, we find that subjects' compliance rates increase in the audit probability and in the fine rate. We also find more novel results, both theoretically and empirically: fine rates should be increased after an audit to discourage otherwise-increased underreporting, and "nudging" myopic individuals toward reporting a constant rather than a fluctuating proportion of income would benefit both the taxpayer and the tax authority.

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1. INTRODUCTION

How should government design policies to improve tax compliance? Most answers to this question are based upon, or at least consistent with, the standard economics-of-crime model first applied to tax compliance by Allingham and Sandmo (1972), which assumes naïve and static behavior on the part of an individual who balances the expected costs of detected cheating with the expected benefits of successful cheating. Most answers also require estimating the impact of various policy tools on individual compliance decisions, a task made challenging by the absence of detailed and reliable information on individual compliance choices. By its very nature, people have an incentive to hide information on their evasion behavior, and this concealment makes empirical work quite difficult. In the United States, researchers have found increasingly creative ways to estimate the factors that motivate compliance by using naturally occurring field data (including administrative data), controlled field experiments, and laboratory experiments. These same empirical approaches have sometimes been applied elsewhere as well, but the extent of empirical work in other countries is far more limited.[1]

In this paper, we first develop a dynamic theoretical model of the individual compliance decision that makes it possible to identify "audit state dependent" compliance. Our model distinguishes between compliance conditional on no previous audits and compliance conditional on previous audits, which makes it possible to discriminate between the implications of naïve and myopic behavior versus sophisticated and forward-looking behavior in our empirical analysis. Previous work applying static models to data analysis has not been able to identify either audit state dependent compliance or myopic versus sophisticated responses to changes in audit rates. We then report the results of the first experiment to examine taxpayer compliance decisions in Colombia. The experiment includes treatments that

[1] For a recent and comprehensive survey of the tax compliance literature, see Alm (2018). For earlier discussions of much of this work, see Cowell (1990), Andreoni, Erard, and Feinstein (1998), Slemrod and Yitzhaki (2002), Kirchler (2007), Alm (2012), and Hashimzade, Myles, and Tran-Nam (2013).

incorporate changes in the full range of fiscal parameters (e.g., audit rates, fine rates, and tax rates), as well as a treatment in which tax revenue is donated to a charity (e.g., a public good). The subject pool includes full-time students, part-time students, workers, and unemployed individuals, which makes it possible to examine subject pool effects. With these experimental data, in combination with our theoretical model, we are able to estimate for the first time the determinants of audit state dependent taxpayer compliance behavior, and to do so for a country (Colombia) with no previous history of experimental studies of tax compliance. The similarity of our experimental design to the tax compliance enforcement in most countries, as well as to the designs in many previous experimental studies raises the possibility that application of our dynamic model to data, rather than the usual static model applied in much of the literature, could lead to different conclusions about taxpayer responses to policy innovations.

Colombia has a long and rich tradition of sophisticated tax policy analyses and tax reforms, going back to several studies in the 1960s (Taylor and Richman, 1965; Bird, 1970; Gillis, 1971), continuing into the 21st century (Bird, Poterba, and Slemrod, 2005), and extending to the tax reform recently enacted by the Government of Colombia in December 2016. However, these reforms have to our knowledge been enacted with no estimates of taxpayer compliance behavioral responses to policy innovations in Colombia, estimates that would make any tax reform efforts more meaningful. We provide these estimates here.

There are of course reasons for caution in attempting to use our results to explain behavior outside the laboratory. Even so, the laboratory offers an opportunity to investigate in a controlled environment individual responses to audit, penalty, and tax rate changes, as well as to other policies for which field data do not readily exist, while controlling for economic and demographic characteristics of the responders. Further, unlike much empirical work that relies on imperfect proxies for evasion, the measures that we use from the laboratory are accurate and unambiguous measures of individual noncompliance, derived in a setting that controls explicitly for extraneous influences on individual behavior.

The estimation results confirm some (although not all) of the theoretical predictions, as well as some (although not all) of the empirical results from other studies. Not surprisingly, we find that reported income increases with an increase in the audit probability. However, we also find the reported income decreases following the event of an audit. Previous work applying static modeling has not been able to examine such distinctions, and concluded simply that taxpayer reporting increases with the audit probability. We explain why such audit state dependent compliance has new implications for tax policy design. We also find that reported income increases with the fine rate, but we find mixed evidence about the effects of changes in the tax rate. Further, we find that compliance depends on the use of tax payments; that is, taxpayer reporting is greater when aggregate tax payments are donated to a charity. Finally, we estimate the effects of all of these fiscal variables on two other variables of interest, earned income and tax revenues. In all of our results, we see no evidence of differences in behavior between subject types, regardless of whether they are students, workers, or unemployed.

Our paper makes several contributions. Our theoretical model of audit state dependent compliance is the first to differentiate between naïve/myopic behavior and sophisticated/forward-looking behavior. Unlike most experimental papers, our theoretical model applied in data analysis is fully consistent with the experimental design. Finally, we are the first paper to use experimental methods to examine compliance behavior in Colombia, and to use these data to examine subject pool effects there.

2. THEORETICAL BACKGROUND

We present here a summary discussion of the static and dynamic theoretical models that are developed more fully in Appendix 1.

2.1. A Simple Static Model of the Individual Compliance Decision

The standard theory of tax compliance is based on the work of Allingham and Sandmo (1972). Consider an individual who earns income w and must choose the fraction R to report to the tax authorities. At time of the decision, let the cumulative amount of unreported income from previous time periods be x. The individual pays taxes at tax rate τ on each peso of reported income. Unreported income is not taxed, but the individual may be audited with a fixed audit rate p at which point the individual must pay taxes on unreported income [$x + (1 - R)w$] plus pay a fine at rate f on each peso of unreported taxes.

The standard approach then assumes that the individual chooses the fraction R of income w in order to maximize the (instantaneous) expected utility of the evasion gamble $U(\cdot)$, with concave utility over after-tax and after-penalty income π.[2] Appendix 1 presents the analysis for a general concave utility function; here we show the illustrative special case of CARA utility, or

$$
\begin{aligned}
U(y) &= \frac{1-\exp(-\lambda y)}{\lambda}, \quad \lambda \neq 0 \\
&= y, \qquad\qquad\quad \lambda = 0
\end{aligned}
$$

where λ captures curvature of the utility of money.

Without any loss of generality, write accumulated unreported income as a multiple of current income, $x = zw$ to simplify notation. The decision problem of our individual is then

[2] For simplicity, we do not include a public good that is financed by individual tax payments. It is straightforward to add this feature to our theoretical model (see Appendix A.1), and our experimental design allows for a public good.

$$V(z, R^*(z)) = \max_{R\in[0,1]} pU(\pi(R,z,1)) + (1-p)U(\pi(R,z,0))$$

where

$$\pi(R,z,s) = w[1-\tau R - s\tau(1+f)(z+1-R)] \tag{1}$$

$$s = \begin{Bmatrix} 0, & \textit{if not audited} \\ 1, & \textit{if audited} \end{Bmatrix}$$

The individual weighs the uncertain benefits of successful underreporting against the risky prospect of detection and punishment, and the individual pays taxes because of the fear of getting caught and penalized for income that goes unreported.

Optimization of expected utility *EU* with respect to *R* in decision problem (1) proceeds using standard methods. The optimal $R \in [0,1]$ is defined as

$$R = \max\{0, \min\{1, 1+z-\frac{1}{\lambda\tau(1+f)w}(\ln(\frac{1}{p}-1)-\ln f)\}\}. \tag{2}$$

Comparative statics results when the solution is from the interior (0,1) are easily derived. This "portfolio" approach gives the plausible result that the proportion of income reported, R is increasing in both audit probability and fine rate. Indeed, the central point of this approach is that an individual pays taxes because – and *only* because – of this fear of detection and punishment.

Further – and novel – results are that optimal compliance rate, R is an increasing function of the accumulated unreported income (measured in terms of the multiple z) at the time of decision-making. As a result, the compliance rate is expected to decrease after an audit because the accumulated unreported income is zero following an audit. The effect of the tax rate τ and the amount of income w on the compliance rate depends on the sign of $[\frac{1}{p}-1-f]$. The income and tax rate effects are predicted to be positive in our experiment as $p(1+f)<1$ in all treatments.

For the special case of risk neutrality (or $\lambda = 0$), the decision on how much to report does not depend on income, accumulated unreported income, the tax rate, or whether an audit took place. The optimal compliance rate is either 0 or 1, depending on the sign of $[\frac{1}{p} - 1 - f]$. As demonstrated later, the prediction for a risk neutral individual in our experiment is to *always* report 0 income.

2.2. A Dynamic Model of the Individual Compliance Decision: Audit State Dependent Compliance

The simple static model might be a good model for some myopic taxpayers and for some inexperienced experimental subjects. However, one might argue against it on grounds of learned behavior or hired expertise to make such decisions. In this subsection we look at rational individuals who are forward looking and who take into account how current compliance choices affect future exposure to audits and fines. To allow for a dynamic process of individual choice, we denote the time-variant variables with a subscript *t* for the period. In each period *t*, the individual earns income w and must choose the fraction R_t to report.[3] The individual plays the game T times. To derive optimal R_t, we use the standard approach of backward induction.[4]

If audited, the individual is responsible for current unreported income as well as for the unreported income during the two most recent periods. So, at the beginning of the final round T, there are only three possible exclusive states for the individual: state 00 (not audited in either of the two preceding periods), state 10 (audited in period $t-2$ but not in period $t-1$) and state 1 (audited in period $t-1$). If we let $z_t w$ denote the accumulated unreported income at the time of decision-making, then we have

[3] For simplicity, income w is assumed to be constant over time in the theoretical derivation. Our experimental design allows income to vary, and this variation is included in the empirical estimation.
[4] Again, for simplicity we do not here introduce a public good that is financed by individual tax payments, although it is included in the model in Appendix 1.

$$
\begin{aligned}
z_t &= 2 - R_{t-1} - R_{t-2}, \quad \textit{if state}_t = 00 \\
&= 1 - R_{t-1}, \qquad\qquad \textit{if state}_t = 10 \\
&= 0, \qquad\qquad\qquad\quad \textit{if state}_t = 1
\end{aligned}
$$

In round T, the decision-maker maximizes expected utility of the round, or

$$V(z_T, R_T^*(z_T)) = \max_{R\in[0,1]} \left[pU(\pi(R, z_T, 1)) + (1-p)U(\pi(R, z_T, 0)) \right]. \tag{3}$$

Going backwards, at any $t < T$ the optimal reported fraction of income one round later $R_{t+1}^*(\cdot)$, as well as the value of the continuation of the game $V(z_{t+1}, R_{t+1}^*(z_{t+1}))$, are known. The individual choice of R_t affects the instantaneous expected utility as well as the continuation value of the game via z_{t+1}

$$
\begin{aligned}
z_{t+1} &= z_t + 1 - R_t, \text{ if not audited at } t \\
&= 0, \qquad\qquad\quad \text{if audited at } t
\end{aligned}
$$

The optimal choice of R_t is determined as the solution to[5]

$$V(z_t, R_t^*(z_t)) = \max_{R\in[0,1]} \begin{bmatrix} p[U(\pi(R, z_t, 1)) + \delta V(0, R_{t+1}^*(0)] + \\ (1-p)[U(\pi(R, z_t, 0)) + \delta V(z_{t+1}, R_{t+1}^*(z_{t+1}))] \end{bmatrix}. \tag{4}$$

As in the static model, there will be lower compliance after an audit because of the instantaneous utility. Adding the value of the continuation of the game in the decision problem moderates differences in compliance rates across states.[6] The signs of the estimated effects of the tax rate, the audit rate, and the fine rate are similar to the signs from the static model as both models work

[5] In our experiment $\delta \in (0,1)$ may capture the subject's belief of the likelihood that the game continues. If so, one might argue that the likelihood of continuation of the game decreases with the round number; that is, the subject may believe at the end of the first round that the game is more likely to continue than at the end of round 15. See Appendix Table A.1 in Appendix 1 for problem (4) with δ being replaced by δ'. We find that compliance rates are smaller in state 1 (i.e., following an audit), and we also find that, for each given state, compliance rates decrease in *t*. See the columns in the middle in Appendix Table A.1. In the econometric model we add *Round* to the list of the regressors, and we predict a negative sign for the estimate.

[6] See Appendix Table A.1 in Appendix 1 for optimal subject compliance rates. For $\delta = 0$, there are two optimal levels of compliance rates: the low compliance (0.10) and the full compliance (1). For $\delta = 0.9$, the optimal compliance rates take values between 0.81 and 0.88 (ignoring the end rounds).

with $U(\cdot)$ but the absolute values differ in large part because future effects (via $\delta V(\cdot)$) are accounted for in the dynamic problem (4) but not in the myopic problem (1).

To illustrate the effects of policy changes in the dynamic model, Figure 1 shows optimal reported fractions of income for an individual with discount factor $\delta = 0.9$, CARA coefficient λ = 0.026, and income *w*=240 (the median income in the experiment). To make patterns visually accessible, we use dotted lines for state 00 (e.g., an individual who is not audited at *t*-1 and who is not audited at *t*-2), and solid lines for state 1 (e.g., an individual who is audited at *t*-1). We use parameter values of 10 percent for the audit rate *p*, 60 percent for the fine rate *f*, and 30 percent for the tax rate *τ*, which are the values of these policy parameters in the baseline experimental treatment discussed in section 3. The reference compliance rates are 0.81 (state 1) and 0.88 (state 00) for these baseline parameter values.

Several patterns are visible. First, the solid lines in Figure 1 are always below the dotted lines, indicating that the compliance rate is lower following an audit (i.e., state 1), for any combination of (*τ, p, f*). The intuition here is that, although audits across rounds are independent events, the amount of accumulated unreported income in state 00 is expected to be larger than zero income, which is the accumulated unreported income following an audit (or state 1).

Second, the compliance rates in Figure 1 are increasing in the audit rate, the fine rate, and the tax rate. As shown by the three right-most points in the left panel of Figure 1, doubling the audit rate (from 0.1 to 0.2) increases the compliance rate by 11 percentage points, up from 0.81 to 0.92 in state 1, and by 7 percentage points in state 00. However, when the audit probability is decreased from 0.1 to 0.01 (i.e. by 0.89 percentage points), the optimal compliance rates (or the two left-most points in the left panel of Figure 1) decrease by 33 percentage points in state 1 and by 26 percentage points in state 00. These results suggest diminishing sensitivity of the compliance rate to the audit probability, calling attention to the limitations of the effectiveness of this instrument in boosting compliance. Similarly, the middle panel of Figure 1 shows the effects of changes in the fine rate on compliance. The effect of doubling the fine rate from 0.6 to 1.2 is similar to the doubling of audit rate from 0.1 to 0.2, an implication of which is that for strictly convex auditing cost technologies policymakers might want to consider an (inexpensive) increase in the fine rate rather than an (expensive) increase in the audit rate as a policy tool to boost compliance.

Third, the effects of changes in the tax rate on compliance are shown in the right panel of Figure 1. Given the parameters in our experiment together with the CARA specification, we find that compliance is increasing and concave in the tax rate. More generally, the effects of changes in the tax rate are ambiguous.[7]

Our dynamic model also illustrates the cost of naïve and static (or myopic) decision-making. With a low audit probability, state 00 is the most likely state, so consider individual compliance of myopic decision-makers in state 00. A myopic individual will make choices for decision problem (4) with $\delta = 0$, which is equivalent to the static problem (1). Because unreported income at time t (in state 00) will be added to unreported income at time t+1 and t+2 if the state remains 00, we expect the optimal compliance rate to periodically vary with t even when our myopic individual is not audited. However, by concavity of $U(\cdot)$ the individual is better off reporting the same intermediate level of income in all rounds in state 00. For example, using expression (2) with w=240 and $\lambda = 0.026,$ we find that it is optimal for the myopic individual to alternate between reporting a fraction of income 0.10 followed by full compliance for two subsequent rounds when in state 00. Such a pattern of compliance for myopic decision-making generates a sequence of expected utilities that

[7] See Appendix 1.

alternate between 38.2 and a subsequent level of 37.3.[8] In this example, our individual's optimal compliance following two rounds of full compliance is the same as his optimal compliance following an audit. In contrast, the expected utility from reporting, say, a constant fraction 0.76 of income is 38.1 (assuming that the state remains 00). It is straightforward to show that the stream of tax revenue from reporting a constant 0.76 fraction of income is also greater than the revenue stream from the reporting sequence {0.1, 1, 1}. These results suggest that policies that nudge the individual away from myopic compliance decisions benefit the individual while also raising tax revenues; that is, there is a cost both to the individual and to the tax authority of myopic decisions.

2.3. Some Testable Implications of the Static and Dynamic Models

Both the static model and the dynamic model suggest alternative ways by which the optimal compliance decision of an individual varies with the underlying parameters. Specifically, the optimal fraction of income R_t that an individual chooses to report in period t depends upon $[p, f, \tau, w, z_t]$, of which the main fiscal parameters are the audit rate p, the fine rate f, and the tax rate τ. This function can be written generically as $R_t=\phi(p, f, \tau, w, z_t)$.[9] There are also other factors that may affect the compliance decision. For example, for subjects who are not forward looking (or δ=0), we expect no round effect on the compliance rates; however, for subjects whose belief in the game continuing decreases as the experiment progresses, we expect compliance rates to decrease with the number of rounds. Finally, although our subjects are paid for every round, with the CARA specification we expect no wealth effects.

Stated as hypotheses, the main theoretical implications for our experiment are as follows:

[8] See also the results reported in the right-most column in Appendix Table A.1.a for a myopic decision maker and the third column for a forward-looking decision maker.
[9] As robustness checks, we use alternative specifications for $\phi(\cdot)$ in our empirical work.

H1: For any given state of the world, an individual's compliance rate is higher (lower) with a higher (lower) audit, fine, or tax rate.

H2: An individual's compliance rate is lower in state 1 (immediately following an audit) than in state 00.

H3: An individual's compliance rate is higher the larger is accumulated unreported income.

The next section discusses our experimental design to test these hypotheses.

3. EXPERIMENTAL DESIGN

3.1. General Design Features

Our experimental setting implements the fundamental elements of the voluntary reporting systems of Colombia's individual income tax, as well as of Colombia's social insurance program. Participants earn income, and they must choose how much income to report to the tax authority. The participant pays taxes only on reported income, and not on any unreported income. However, the individual faces a fixed probability of audit. If the individual is audited, then any unreported income in the current period and in the previous two periods are discovered, and the individual must pay taxes plus a penalty on all unreported income. Subjects are fully and accurately informed about the various features of the experimental setting (e.g., tax rates, audit rates, and penalty rates). This process is repeated over a number of rounds, each representing a tax period. At the completion of the experiment, all participants are paid in cash their laboratory market earnings converted to Colombian pesos.

Participants are all adults, recruited from several major Colombian universities, from Colombian workers (both employed and self-employed), and from the unemployed. No participant had prior experience in this experimental setting. Methods adhere to all guidelines concerning the ethical treatment of human subjects.[10]

Upon arrival at the laboratory, participants are assigned to a computer station with each station being situated in an isolation carrel. The experimental lab consists of networked computers, a server, and software designed for this series of experiments. Basic instructions are provided via a hardcopy and also via a series of screen images; a representative set of instructions is provided in Appendix 3. There is no interaction between the participants and the person running the experiment beyond the initial seating of the participants at terminals and the reading of the consent document. Decisions are made privately, and participants are not allowed to talk with one another during the session. Participants also do not sign and return consent forms to further increase anonymity. Participants are informed that all responses are anonymous, that no individual identification will be collected, and that the only record of participation will be the receipt signed to receive payment at the end of the session. Participants are also told that payments will be made in private at the end of the session. Taken together, these experimental procedures effectively eliminate both subject-to-subject interaction and subject-to-experimenter interaction.

The detailed steps of the experiment can be briefly described. At the beginning of the session, a participant receives an endowment of 500 experimental coins, equivalent to 5000 Colombian pesos. Then at the beginning of each round of the session, participants are presented with a simple task that requires them to add numbers; their performance on this task determines their earned income. Subjects are then presented with a screen that provides the details of the treatment in effect. They are informed with certainty of the tax rate, the audit rate, and the fine rate. Each subject must then choose the

[10] All Colombian subjects provided written consent for their participation in the laboratory experiment, and the Institutional Review Board at Georgia State University approved the analysis of the experimental data.

amount of income to report to the tax authority. The computer automatically calculates the resulting tax liability. Participants are able to experiment with different reports during the time allowed for filing, and they can observe the potential changes in their reported take home income.

The process of determining who among the filers is audited is generated by a computerized draw. After each subject reports income, the participant is presented on his or her computer screen with an animated (computerized) representation of a bucket from which a draw is made. In this bucket there is a fixed number of balls (either blue or white), with a white ball signifying no audit, a blue ball denoting an audit, and the number of blue balls relative to the number of white balls determining the audit rate. Each taxpayer is audited independently. The balls "bounce" in this bucket, and then a door opens and a ball exits the bucket through this door, with the color of the ball indicating whether the individual is audited. The audit applies to the current period declarations of taxable income and to the two previous periods. The computer automatically deducts taxes paid and penalties (if any are owed) from each audited participant's account. After the audit process has been completed, each subject is presented with a new screen that provides the earnings and audit outcome summary for the round.

Our experimental setting is very contextual in order to promote "parallelism" to the naturally occurring world (Smith, 1982; Plott, 1987). Our experimental interface and instructions use tax language throughout, the participants decide how much income to report in the same manner as on the typical tax form (e.g., entering income on a tax form). There is a time limit on the subjects in making the reporting decision.

Participants are not told the exact duration of the experimental session, which is predetermined to last for 20 real rounds. Including instructions, 3 practice rounds, and the 20 real rounds, sessions take on average 70 minutes to complete. Each subject's payoff is realized at the end of each round, and the subject is paid the sum of payoffs in all 20 real rounds at the end of the experiment.[11] Each session has either 15 or 16 subjects. In all, 122 subjects participated in these sessions. Salient payoffs averaged 35,390 Colombian pesos (about $17), which is more than 10 times the minimum hourly wage in effect at the time of the experiment.

3.2. Experimental Treatments

Recall that our main objective is to estimate the impact of independent changes in the main fiscal parameters (p, f, τ) on individual compliance choices. We do this by conducting treatments in which each of these fiscal parameters is individually varied. In addition, we conduct one session in which the subjects' total tax payments are aggregated and donated to a Colombian nonprofit organization, *Fundación Cardioinfantil*, a medical institution that helps children with heart disease. This session is designed to examine the impact of public good provision on tax compliance decisions. Table 1 summarizes the experimental treatments.

To establish a baseline, Treatment 1 (T1) sets the three fiscal parameters at baseline levels: p=10 percent, f=60 percent, and τ=30 percent. Note that a 60 percent fine rate means that the audited individual must pay unreported taxes plus an additional fine of 60 percent, so that the "effective" fine rate is 1.60. The other treatments vary one parameter at a time, holding constant the others at their baseline values. In Treatment 2 (T2), the fine rate is increased to 120 percent (or an effective fine rate of 2.20); in Treatment 3 (T3), the fine rate is lowered to 30 percent (or an effective fine rate of 1.30). In Treatments 4 and 5, the tax rate is varied, increasing to 45 percent in T4 and decreasing to 10 percent in T5. In Treatments 6 and 7, the audit rate is varied, to 20 percent in T6 and to 1 percent in

[11] This "pay all sequentially" payoff protocol has been found to elicit risk preferences that are not significantly different from those for the "one task" protocol (Cox, Sadiraj, and Schmidt 2015), which is the gold standard of incentive compatible payoff protocols.

T7. Finally, Treatment 8 (T8) sets the fiscal parameters at their baseline (T1) levels, but donates all subject taxes to the nonprofit organization.

3.3. Expected Value Calculations

A risk-neutral individual will make choices so as to maximize the expected value of the compliance gamble. Accordingly, it is useful to calculate the expected value in the treatments. These calculations also provide support for the hypotheses from the earlier discussion.

For example, in the baseline session (T1), the expected value EV equals $0.7w$ when the individual reports fully and honestly (or R=100 percent). In contrast, the expected value from reporting zero income is $0.87w$.[12] In the baseline session, the optimal risk neutral strategy is therefore to report zero income. More generally, optimal individual decisions for any linear payoff function will tend to exhibit all-or-none behavior; only very large changes in parameter values alter this outcome. As we demonstrate below, our experimental results are largely consistent with all-or-none behavior. They are also largely consistent with lower compliance following an audit.

4. RESULTS

4.1. Aggregate Treatment Effects on Compliance

Table 2 shows the main summary statistics (median, mean, standard deviation) for the compliance rate in each of the eight treatments. Note that, in order to ensure independence, we first generate a new variable that takes the value of the mean of the compliance rates of each subject over

[12] To illustrate this calculation, recall that the effective fine rate is 1.60 (=1+0.60) because the individual must pay unreported taxes plus a fine of 60 percent. In state 1, expected income is $0.952w$ because the individual pays taxes plus a fine on w if audited; in state 10 expected income is $0.904w$ because the individual pays taxes plus a fine on $2w$ if audited; in state 00 expected income is $0.856w$ because the individual pays taxes plus a fine on $3w$ if audited. The expected value of strategy "always report 0 income" is $0.87w$ as our tax game is an ergodic Markov chain with steady-state probabilities 0.1 (state 1), 0.09 (state 10) and 0.81 (state 00).

20 rounds. The summary statistics reported in Table 2 are therefore based on one data point per subject. The overall mean *Compliance Rate* (calculated as the ratio of reported income to earned income) is 72.0 percent; the overall median *Compliance Rate* is 86.0 percent. Six subjects reported zero income in very round, and 30 subjects reported full income in every round. Full and zero compliance account for 57.8 percent and 17.5 percent of 2437 observations, respectively. Following an audit, full compliance decreases to 49.5 percent while zero compliance increases to 22.2 percent. The demographic characteristics of the subjects are reported in Table 3, including dummy variables for whether the subject was *Born in Bogota*, *Lives in Bogota*, lives with a *Parent Household Head*, *Owns a home*, is a Full-time *Student*, and is a *Female*. Consider each of the treatments.

Fine Rates. Higher fine rates are hypothesized to induce higher compliance rates, and the data are consistent with this hypothesis (H1). As one moves from the lower to the higher fine rates, the mean compliance rates increase, from 0.705 (T3: Lower Fine Rate, f=0.30), to 0.732 (T1: Baseline, f =0.60), and to 0.797 (T2: Higher Fine Rate, f =1.20). There is no clear pattern on zero compliance, but full compliance increases from 46 percent to 57 percent to 63 percent as the fine rate increases from 0.3 to 0.6 to 1.2. However, neither the data from the Higher Fine Rate (T2) nor the data from the Lower Fine Rate (T3) treatments are statistically different from the Baseline treatment T1 at conventional levels of significance. We consider later whether the sample size accounts for this result when we conduct analysis at the individual level.[13]

Figure 2 shows mean compliance rates across audit states and treatments. A visual inspection of the middle panel in Figure 2 indicates that compliance rates in state 00 are increasing in fine rates, which is consistent with hypothesis H1. However, for state 1, we see a decrease by 5 percentage

[13] For example, to detect the (0.06) effect of doubling the fine rate from the baseline level of 60 percent to 120 percent (i.e., the Higher Fine Rate treatment effect of T2), we need 174 observations per group, assuming conventional levels for power (80 percent) and significance level (5 percent).

points in the compliance rate (from 72 percent to 67 percent) when the fine rate is doubled, in contradiction with H1. With this one exception, data are consistent with H1. Compliance rates in the Baseline state 1 (67 percent) and the Higher Fine Rate state 1 (71 percent) are smaller than in state 00, consistent with hypothesis H2, and compliance rates across states are similar in the Lower Fine Rate treatment.

Tax Rates. The impact of a higher or lower tax rate on the compliance rate depends on whether one examines the mean or the median statistic. If one examines the mean compliance rate, compliance actually increases with a higher tax rate (T4 versus T5); if one examines the median compliance rate, compliance declines significantly with a higher tax rate. In trying to understand these patterns of responses, it is helpful to look more closely at the individual data.[14] Zero compliance falls from 26 percent to 19 percent to 14 percent as the tax rate increases from 10 percent to 30 percent to 45 percent, but the effect on full compliance is not monotonic. On average, Figure 2 (the right panel) shows that, consistent with H1, compliance rates are increasing in tax rate in both audit states. Furthermore, at each level of tax rate, compliance rates are smaller in state 1, which is consistent with hypothesis H2.

Audit Rates. Data show monotonic effects on extensive margins. Zero compliance increases from 12 percent to 19 percent to 42 percent as the audit rate decreases from 20 percent to 10 percent to 1 percent while full compliance decreases from 63 percent to 57 percent to 30 percent. Figure 2 (the left panel) shows that when audit probability is 1 percent as in T7 (or the Lower Audit Rate treatment), subjects report on average 40.6 percent of their true earned income. Doubling the audit rate from 10 percent in the Baseline T1 to 20 percent in the Higher Audit Rate treatment T6 increases the compliance rate by 13 percentage points, from 73.2 percent (T1) to 85.9 percent (T6). Comparing

[14] Note that theory predicts compliance increasing in tax rate for CARA preferences but for general preferences the effect is ambiguous (see Appendix 1).

compliance rate data from the two treatments to the Baseline (T1) data, we find that only in the Lower Audit Rate treatment T7 is the compliance rate significantly lower (p-value=0.013, Kruskal-Wallis test).[15]

Note that for a risk neutral subject the expected cost (i.e., the product of the fine rate and the audit rate) of underreporting affects the optimal tax compliance rate, but the decomposition between the fine and audit rates is irrelevant. For such subjects we should therefore expect the compliance rate to be the same in the Higher Fine Rate and in the Higher Audit Rate treatments. As seen in Table 2, the median of compliance rates seems to be slightly higher in the former (up to 0.93 from 0.86), but the means are comparable (0.80 and 0.81). A closer look at Figure 2, however, suggests that a finer level of behavior decomposition between the fine and audit rates might be relevant. The distribution of compliance rates across audit states differs between the Higher Audit Rate treatment (0.61 and 0.87) and the Higher Fine Rate treatment (0.71 and 0.84).[16]

With respect to hypothesis testing, consider the left panel in Figure 2. Data are consistent with hypothesis H1 in two out of three cases; the one case of rejection comes from state 1 data when audit rates are 0.1 and 0.2 (the solid line). Compliance rates are smaller in state 1 in both the Baseline (audit rate 0.1) and the Higher Audit Rate (0.2) treatments, which is consistent with hypothesis H2. The observed decrease in compliance after an audit is consistent with the audit state dependent taxpayer compliance predicted by the dynamic model. This is a phenomenon not discussed in the previous literature that applied static models. In the Section 5 conclusions we discuss its possible importance for tax policy.

[15] This result is also consistent with p-values reported by the t-test, the median test, or the Kolmogorov-Smirnov test.

[16] We get similar figures on average compliance rates because the steady-state probabilities are different between the two treatments. In the Higher Audit Rate treatment, the system is 20 percent of the time in state 1 and 64 percent in state 00; in the Higher Fine Rate treatment, the steady state distribution is 10 percent (state 1) and 81 percent (state 00). For the optimal compliance rates shown in Figure 1, the expected compliance rate is 0.94 in the Higher Fine Rate treatment as well as in the Higher Audit Rate treatment.

Donation Effect. The mean compliance rate is highest across all treatments in the Donation treatment T8, where tax payments were totaled across all subjects and donated to a charity, a result that is consistent with altruistic preferences.[17] Out of 15 subjects in this treatment, 7 subjects always reported full income. In contrast, in the Baseline treatment only 3 subjects reported full income in very round. Overall zero compliance decreases from 19 percent to 9 percent, while full compliance increases from 57 percent to 75 percent when tax payments are donated.

4.2. Tobit Estimates of Individual Data

We look next at the individual data to see whether more can be said about the effects of specific variations in the parameters. The individual data also allow us to examine the impact of demographic characteristics, including subject pool effects.

The common empirical approach used with data like ours involves estimation of a Tobit model, and we utilize this Tobit approach in several specifications. Table 4 presents Tobit regressions with random effects with an upper bound at 1 and a lower bound at 0 for the dependent variable, the observed *Compliance Rate* (defined as Reported Income/True Earned Income) for each individual in each round of the treatment. Estimates reported in the left-most three columns use data from all subjects. Estimates reported in the right-most three columns do not include data from subjects who either reported 0 or full income in every round, since responses to any changes in the environment are less informative for individuals who do not change their behavior.

As indicated in right-most three columns of Table 4, the impacts of the policy parameters are generally consistent with the theory. For example, a higher penalty rate increases the compliance rate, as does a higher audit rate. Figure 3 shows estimated unconditional (left figures) and conditional (right

[17] See Appendix 1 for additional discussion.

figures) elasticities calculated at the baseline treatment values for the policy parameters (audit rate=0.1, fine rate=0.6, and tax rate=0.3) and at means of the other variables (*Accumulated Unreported Income Rate*, *Income*, *Round*). The top panel show results for all 122 subjects whereas the bottom panel shows results for the 86 selected subjects. The estimated effect of the fine rate on compliance rate has a significant effect on selected subjects, with an estimated elasticity of 0.41 and an estimated coefficient of 0.58. For the audit rate, the estimated elasticity 0.66 (0.51 for the selected subjects) is consistent with most other empirical estimates derived either from field data or laboratory data (Alm, 2018). The tax rate estimate is not statistically different from 0 in any specification. Of some note, the use of the tax payments (*Donation*) has a positive and statistically significant impact on compliance, although the significance disappears for selected subjects (Table 4, top row).

The audit rate has a highly significant positive effect on compliance in all models reported in Table 4. The audited last round dummy variable has a highly significant negative coefficient: the compliance rate is estimated to decrease by 32 percent following an audit event. As explained earlier in the discussion of Figure 2, the observed decrease in compliance after an audit is a result predicted by the dynamic model, a result that has not been reported in much of the previous literature.[18] This has possible implications for tax policy that are discussed in the Section 5.

As reported in Table 4, demographic variables are largely insignificant, including dummy variables for whether the subject is *Born in Bogota*, is a Full-Time *Student*, owns a home (*Own House*), lives with a *Parent Household Head*, lives in Bogota (*Lives in Bogota*). Importantly, we find no evidence of subject pool effects; that is, there is no significant difference in behavior between

[18] Note that this result is consistent with the so-called "bomb-crater effect", in which an audited individual reduces his or her immediate post-audit compliance before recovering somewhat in succeeding rounds. This effect is often found in laboratory experiments (Mittone, 2006; Maciejovsky, Kirchler, and Schwarzenberger, 2007; Kastlunger et al., 2009). However, with some exceptions (Mendoza, Wielhouwer, and Kirchler, 2017), field data generally find little or no evidence of a bomb-crater effect (Erard, 1992; Advani, Elming, and Shaw, 2015). These studies generally provide no theoretical explanation for the observed behavior.

students and other demographic groups, consistent with the results of Alm, Bloomquist, and McKee (2015). *Female* subjects seem more compliant than male subjects, a result that has generally been found in other laboratory experiments (Alm, Jackson, and McKee, 1992). The positive estimated coefficient of *Accumulated Unreported Income Rate* is consistent with the theoretical predictions stated in section 2. The estimated elasticity for the basic model is 0.07 for all subjects and slightly larger 0.12 for selected subjects, indicating in both bases relatively inelastic compliance rates. The compliance rate drops significantly (-0.3) following an audit, which is also consistent with our theoretical analysis. We find no wealth effect from *Accumulated Payoff*, which provides some support for the CARA specification, although the *Income* estimate (which is expected to be positive for the exponential Bernoulli utility) questions the CARA specification. Negative *Round* effects are expected for the dynamic model with δ decreasing with t, but no effects are expected either for the static model or for the dynamic model with fixed δ (unless subjects know that the last round is round 20, in which case less compliance is expected during the end rounds). While we see a significant negative estimate for the *Round* variable, the effect disappears when demographics are added.

4.3. Robustness Tests: Hurdle Estimates of Individual Data

As suggested by Cragg (1971), it is possible to examine the individual as making two related but distinct compliance decisions: the first is the decision on whether to report all or some of the true earned income, and the second is the decision on how much of true earned income to report (conditional upon reporting some income). We also estimate this "hurdle" model of Cragg (1971) as an alternative to the Tobit results in Table 4. These results are reported and discussed in detail in Appendix 2. The hurdle results are largely consistent with the Tobit results.

4.4. Empirical Validity of Myopic Behavior

As discussed in section 2.2, compliance rates of a myopic individual in state 00 are varying between lower and higher values in a periodic way. A flat pattern is predicted for a forward-looking individual. The estimated coefficient for the *Accumulated Unreported Income Rate* is positively significant for all model specifications (see Table 4), which is consistent with myopic behavior. Data from all audit states are used in the regression, so to get some insights on the empirical validity of theoretical predictions for behavior in audit state 00 we look closely at data from that state. For each subject, we created a new variable, the difference in compliance rate (dcr) of a subject's observed choices in two subsequent rounds when in state 00. For periodic compliance rates, the values of the difference variable are positive, 0, or negative. For the example in section 2.2, the difference variable has period 3; the pattern is a repetition of $\{0.9, 0, -0.9\}$. For each subject we conduct a *t*-test on the absolute value of the difference variable ($|dcr|$). If the *t*-test does not reject the hypothesis that the mean of $|dcr|$ is larger than 0.1, then we classify the subject as "myopic". Using this criterion, we find that 44.2 percent (or 54 out of 122) of the subjects' compliance patterns in state 00 reveal myopic behavior.[19] The percentage figure increases to 62.8 percent (54 subjects out of 86) if we do not include 36 subjects whose compliance rate was the same for all 20 rounds. A subject who reported full (or 0) income in all 20 rounds will not be classified as myopic by this criterion. We cannot classify these 36 subjects because a very risk averse subject (myopic or sophisticated) would always fully comply, while a risk lover or risk neutral subject (myopic or sophisticated) would always report zero income.

4.5. Additional Treatment Effects on Tax Revenues and Earned Income

[19] In the last column of Table 2 we report percentages of myopic subjects across treatments.

We also investigate the effect of treatments on tax revenues and on earned income by creating two variables, total tax revenue (*TaxRev*) and true earned income (*I*). For any given treatment and round, the value of true earned income is the total amount of income earned in that round by all subjects in the session. Similarly, *TaxRev* is the total amount paid in taxes in a given treatment and round by all subjects in the session. Note that the *TaxRev* does not include the amount paid in penalties (tax plus fine times unreported income) in case the subject is audited. Because the Baseline and Higher Tax Rate treatments each had one more subject than the other treatments, we divide the values of these two variables by the number of subjects in the session. There are 20 observations of each variable for each of the 8 treatments, giving 160 total observations. Using OLS regression analysis, we estimate the impacts of the tax rate, the fine rate, the audit rate, and donations on each of these dependent variables, along with the demographic variable *Female* in an additional specification for each dependent variable. These results are shown in Table 5.

Results in Table 5 indicate that the tax rate has a negative effect on true earned income *I*. The estimated tax rate effect is -97EC or -72EC, depending on the specification. (True earned income per subject in the Baseline treatment T1 was 235 EC.) The estimated elasticity is -0.13 or -0.09, revealing an inelastic income response to the tax rate. The effect of the tax rate on *TaxRev* is positive, with estimates for the marginal effect and the elasticity of 136EC and 0.86, respectively for the model with *Female* dummy variable. Both the penalty rate and the audit rate have positive effects on true earned income as well as on tax revenues; the estimated elasticities are 0.25 for the penalty rate and 0.35 for the audit rate. To get some further insight into the effects of the audit rate and the penalty rate, we examined the individual data. We find suggestive evidence that individuals increased their work effort in an attempt to make up for lost earnings after having to pay large penalties. Figure 4 shows scatter plots of true earned income on the penalty rate when the last round payoff was positive (left) or negative (right).

With respect to the Donation treatment (T8), tax revenues per capita are higher when subjects are informed that their taxes are being donated to a charity. This is consistent with a positive demand for giving to charities. Note that round estimates suggest that subjects get better in performing the income-earning task as the experiment proceeds. Also, the larger the fraction of females in a session the smaller is true income per capita but the larger is *TaxRev*. The negative income estimate comes from females earning on average less (218EC) than males (240EC). Even so, females pay more in taxes (49.86EC versus 47.76EC) because compliance rates for females versus males (0.754 for females versus 0.697 for males) are sufficiently large to compensate for lower income.

5. CONCLUSIONS

Our experimental results are similar to some but not all of the results reported in the previous literature. We find evidence that taxpayers are more compliant when facing higher fine rates. We find conflicting results on the effects of higher tax rates on compliance, although there is suggestive evidence that compliance increases with an increase in the tax rate. Income reporting increases as the audit probability increases, as typically found in previous work. Compliance also depends upon the use of tax payments; that is, taxpayer reporting tends to be larger when taxes are donated to a charity, a common result. We find no subject pool effects; that is, we find no significant difference in behavior between students, workers, and the unemployed.

Of some note, our dynamic model of compliance predicts an idiosyncratic effect of tax return auditing that is supported by the experimental data. This audit state dependent taxpayer compliance predicts that income reporting decreases right after an audit if the audit probability and fine rate are constant over time. In order to counter such a bounce in underreporting, tax authorities may want to consider applying conditionally higher fine rates that apply to recently audited taxpayers.

Also of some note, we are able to use the dynamic model to distinguish between naïve and myopic behavior versus sophisticated and forward-looking behavior in tax compliance. Applying this distinction in analysis of the data, we find that about half of our subjects are myopic. Compliance rates for myopic individuals in the most likely scenario (no audit for adjacent reporting periods) fluctuate between high and low fraction of income reporting. Forward-looking behavior requires a nearly constant fraction of income reporting. An implication for tax policy is that nudging myopic individuals towards constant reporting would increase their expected utilities and would also increase tax revenue, an outcome that benefits both taxpayers and the tax administration.

There are certainly reasons for caution in the use of and generalization from these estimates. They are based upon somewhat artificial laboratory experiments, they are derived from subjects whose experience with real-world taxation is uncertain, and they are generated from small samples.

Still, it seems likely that the results can contribute to understanding of the compliance puzzle, especially in a country like Colombia where previous estimates of individual behavioral responses simply do not exist. The laboratory experiment provides accurate information on individual compliance choices for Colombian subjects. Further, there is much accumulating evidence that suggests that the "external validity" of tax compliance experiments is in fact significant (Alm, Bloomquist, and McKee, 2015). Most importantly, there is now a large literature that argues convincingly that experimental methods can contribute significantly to policy debates, as long as some conditions are met: the payoffs to subjects must be salient; better subject decisions yield higher subject payoffs; decision costs must be commensurate with the payoffs; and the experimental setting must capture the essential properties of the naturally occurring environment that is the subject of investigation (Smith, 1982; Plott, 1987). These conditions are met here.

How can the Government of Colombia use these results in its ongoing efforts to improve compliance? One obvious strategy is consistent with the "Enforcement Paradigm" of Alm and Torgler (2011): Increase audits and fines. However, our results also suggest additional strategies, such as ensuring that individuals can see the uses to which their taxes are put. These latter strategies are consistent with additional and emerging paradigms of tax administration, or the "Trust Paradigm" and the "Service Paradigm" (Alm and Torgler, 2011). Other underutilized strategies, such as providing information designed to nudge individuals away from myopic decision-making, can also be beneficial when tax auditing protocols have dynamic features. In short, the Government of Colombia – just like governments elsewhere – should pursue a range of approaches in its efforts to promote compliance, approaches that are consistent with the "full house" of behaviors that our results demonstrate.

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Table 1. Experimental Design

Session	Session Name	Tax Rate	Fine Rate	Audit Rate	Donation?
T1	Baseline	30%	60%	10%	No
T2	Higher Fine Rate	30%	120%	10%	No
T3	Lower Fine Rate	30%	30%	10%	No
T4	Higher Tax Rate	45%	60%	10%	No
T5	Lower Tax Rate	10%	60%	10%	No
T6	Higher Audit Rate	30%	60%	20%	No
T7	Lower Audit Rate	30%	60%	1%	No
T8	Donation of Taxes	30%	60%	10%	Yes

Table 2. Compliance Rates – Summary Statistics by Treatment

Session	Percent Zero Compliance Observations	Percent Full Compliance Observations	Median Compliance Rate	Mean Compliance Rate	Compliance Rate Standard Deviation	Number of Subjects	Percent Myopic Subjects
T1	19	57	0.838	0.732	0.310	16	38 (50)
T2	4	63	0.925	0.797	0.251	15	60 (69)
T3	14	46	0.807	0.705	0.339	15	60 (82)
T4	14	66	0.832	0.775	0.257	16	69 (92)
T5	26	62	0.977	0.706	0.392	15	20 (43)
T6	12	63	0.859	0.810	0.200	15	33 (45)
T7	42	30	0.316	0.406	0.375	15	40 (50)
T8	9	75	0.950	0.830	0.242	15	33 (63)

Notes: Figures in parenthesis in the last column do not include 30 subjects who reported full income always and also do not include 6 subjects who always reported 0 income.

Table 3. Demographic Characteristics of Subjects by Treatment

	Demographic Characteristic (Proportion of Subjects)					
Session	Female	Student	Own House	Parent As Household Head	Lives in Bogota	Born in Bogota
T1	0.563	0.563	0.813	0.938	0.875	0.813
T2	0.333	0.533	0.600	0.667	0.867	0.600
T3	0.533	0.800	0.667	0.733	0.600	0.600
T4	0.625	0.375	0.688	0.625	0.813	0.750
T5	0.333	0.533	0.667	0.667	0.733	0.733
T6	0.333	0.533	0.667	0.733	0.933	0.867
T7	0.333	0.667	0.667	0.600	0.600	0.800
T8	0.267	0.667	0.867	0.867	0.733	0.800
Total	0.418	0.582	0.705	0.730	0.770	0.746

Table 4. Random-effects Tobit Regression Results for Compliance Rate

Explanatory Variable	**Data from All 122 Subjects**			**Data from 86 Subjects**		
	(1)	**(2)**	**(3)**	**(1)**	**(2)**	**(3)**
Donation [D]	0.950** (0.398)	0.915** (0.383)	1.115*** (0.379)	0.126 (0.284)	0.120 (0.274)	0.125 (0.281)
Tax Rate	0.436 (1.512)	0.383 (1.454)	-0.435 (1.415)	0.685 (1.037)	0.592 (1.002)	0.375 (1.031)
Penalty Rate	0.542 (0.555)	0.546 (0.537)	0.398 (0.534)	0.581* (0.330)	0.585* (0.319)	0.568* (0.333)
Audit Rate	8.311*** (2.743)	8.285*** (2.655)	7.785*** (2.603)	4.355*** (1.681)	4.437*** (1.624)	4.512*** (1.617)
Accumulated Unreported Income Rate	0.229*** (0.041)	0.175*** (0.042)	0.180*** (0.042)	0.220*** (0.041)	0.163*** (0.042)	0.168*** (0.042)
Income	-0.073 (0.056)	-0.090 (0.056)	-0.089 (0.056)	-0.057 (0.055)	-0.074 (0.054)	-0.075 (0.055)
Round	-0.019*** (0.004)	-0.017*** (0.004)	0.001 (0.013)	-0.020*** (0.004)	-0.017*** (0.004)	-0.001 (0.013)
Audited Last Round [D]		-0.321*** (0.073)	-0.329*** (0.073)		-0.336*** (0.075)	-0.342*** (0.075)
Accumulated Payoff			-0.010 (0.007)			-0.009 (0.007)
Female [D]			0.516** (0.253)			-0.045 (0.169)
Student [D]			-0.119 (0.270)			-0.124 (0.178)
Own House			0.058 (0.313)			0.049 (0.207)
Parent Household Head [D]			0.425 (0.366)			0.327 (0.266)
Lives in Bogota [D]			-0.561 (0.383)			-0.174 (0.260)
Born in Bogota [D]			0.415 (0.344)			-0.197 (0.234)
Constant	0.185 (0.665)	0.261 (0.642)	0.340 (0.699)	0.117 (0.451)	0.206 (0.437)	0.428 (0.473)
Observations	2,437			1,717		
Number of Subjects	122			86		
Censored observations (left, uncensored, right)	(427, 602, 1408)			(307, 602, 808)		

Notes: The dependent variable is *Compliance Rate* (=Reported Income/Full Income), with a lower bound of 0 and an upper bound of 1. In "Data from 86 Subjects", information from 30 subjects who reported full income in every round and information from 6 subjects who reported 0 income in every round are not included. The unit of measurement for *Income* (earned points in the current round) as well as *Accumulated Payoff* (sum of earnings at the beginning of the current round) is 100 EC; dummy variables are denoted with [D]; *Accumulated Unreported Income Rat*e is the ratio between the total unreported income during the previous two rounds and income in the current round. Standard errors are in brackets; *** p<0.01, ** p<0.05, *p<0.1.

Table 5. OLS Estimates of Tax Revenues and True Earned Income

	Tax Revenues (per capita)		**True Earned Income (per capita)**	
Explanatory Variable	(1)	(2)	(1)	(2)
Donation [D]	9.658***	14.518***	1.155	-4.648
	(1.402)	(1.696)	(3.614)	(4.599)
Tax Rate	156.946***	135.983***	-96.724***	-71.694***
	(5.250)	(6.747)	(13.530)	(18.295)
Penalty Rate	14.315***	20.070***	27.246***	20.375***
	(1.979)	(2.250)	(5.101)	(6.100)
Audit Rate	165.718***	167.430***	143.345***	141.300***
	(9.730)	(9.167)	(25.078)	(24.855)
Round	-0.058	-0.054	1.211***	1.205***
	(0.080)	(0.076)	(0.207)	(0.205)
Female [D]		26.404***		-31.528**
		(5.787)		(15.691)
Constant	-24.173***	-33.454***	214.769***	225.851***
	(2.422)	(3.055)	(6.242)	(8.284)
Observations	160	160	160	160
R-squared	0.894	0.906	0.485	0.499

Notes: Dummy variables are denoted with [D]. Standard errors are in parentheses; *** p<0.01, ** p<0.05, * p<0.1.

Figure 1. Optimal Reported Fraction of Income across Treatments and Audit States

State 1 (audited at t-1) and State 00 (not audited at t-1 nor at t-2)

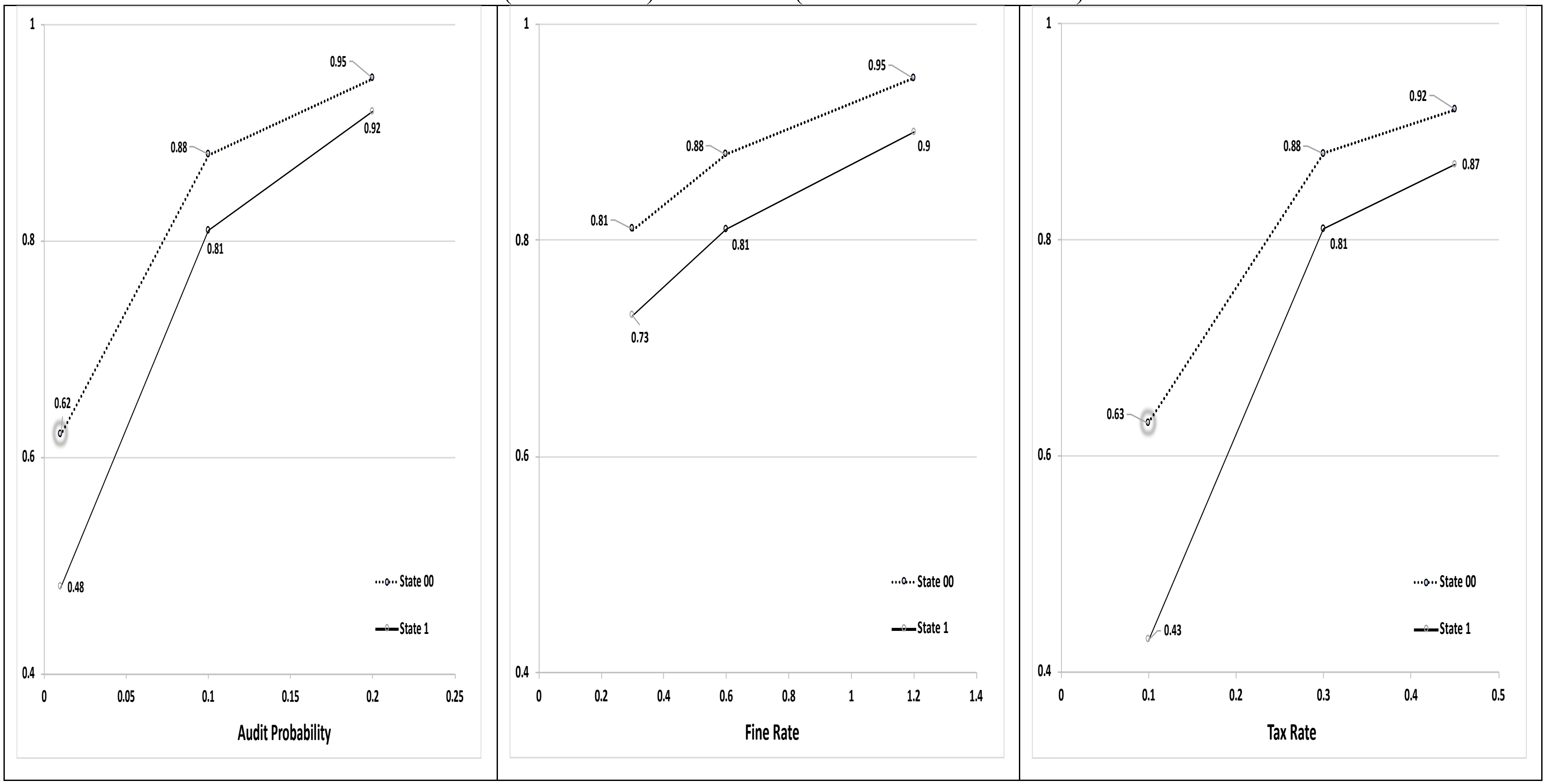


Figure 2. Observed Reported Fraction of Income across Treatments and Audit States

State 1 (audited at t-1) and State 00 (not audited at t-1 nor at t-2)

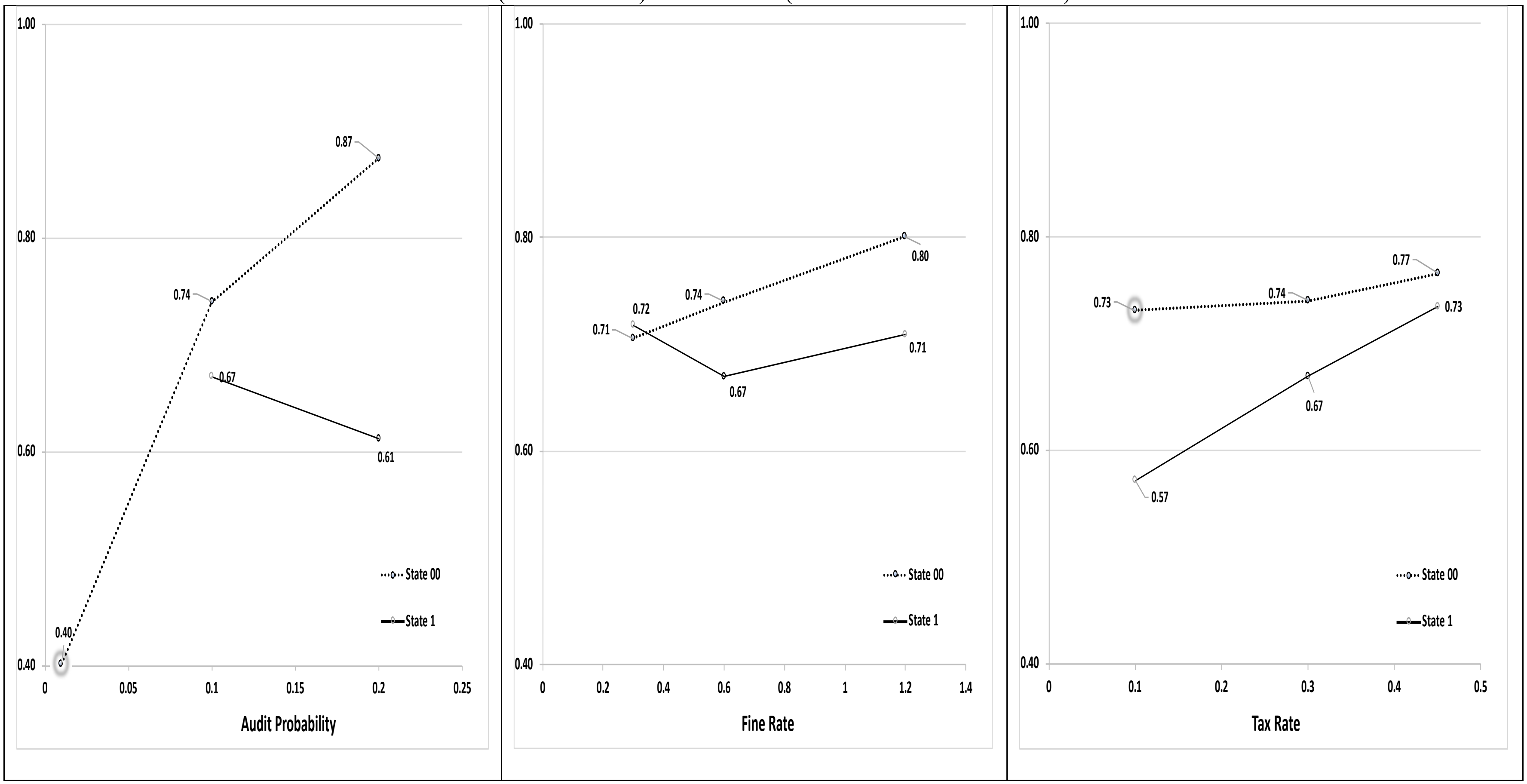


Figure 3. Estimated Elasticities for Model (1) in Table 3

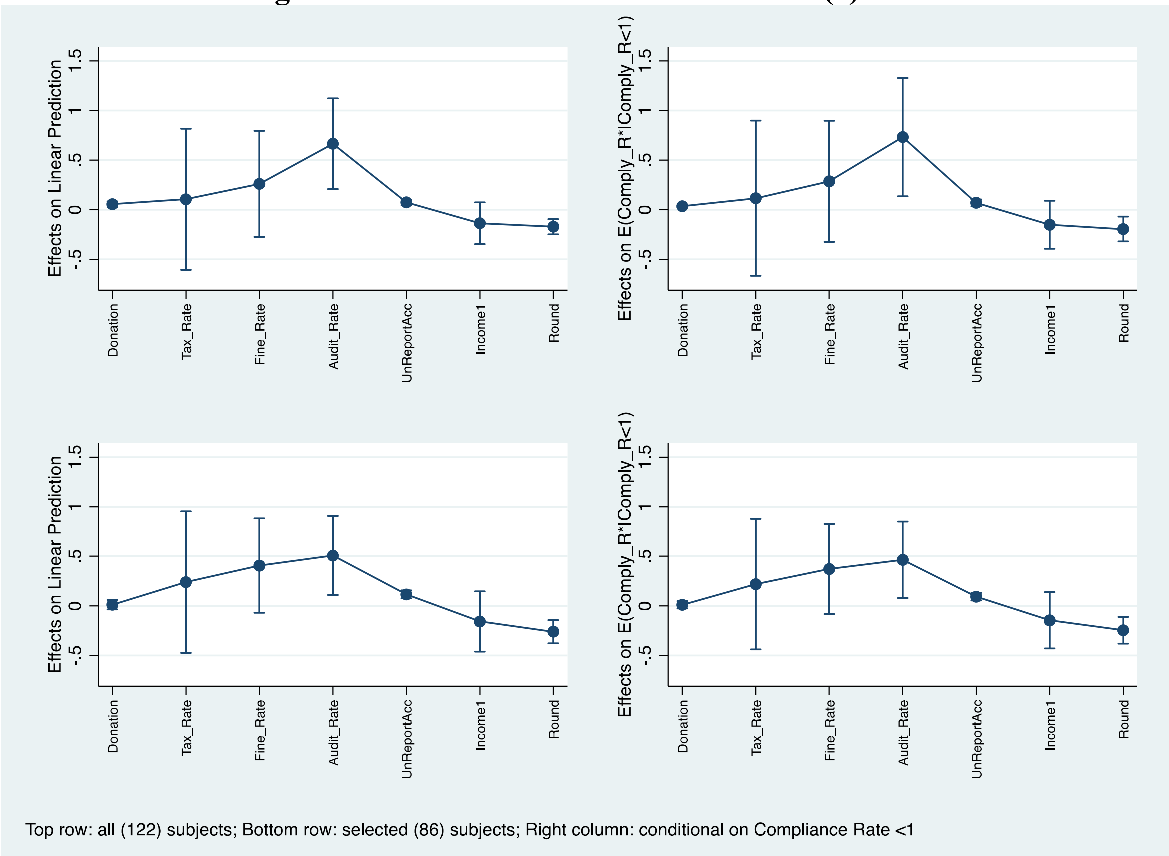
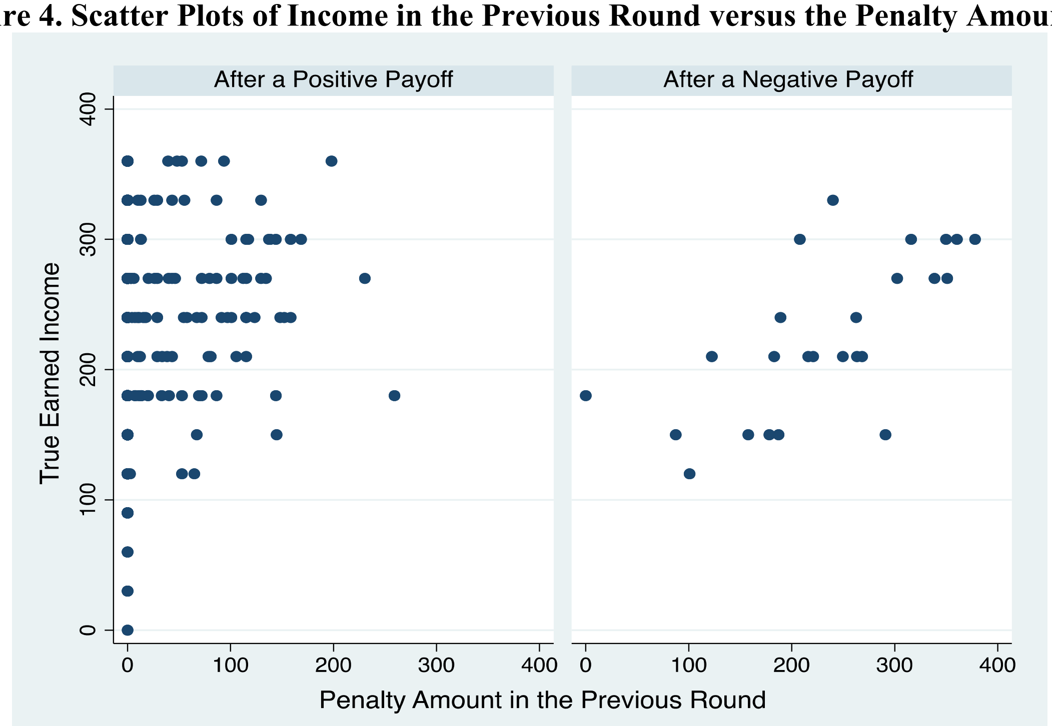


Figure 4. Scatter Plots of Income in the Previous Round versus the Penalty Amount



APPENDIX 1: THEORETICAL ANALYSIS

Static Model.

We show that the optimal compliance rate for decision problem (1):

(1) increases with the accumulated unreported income;
(2) increases with the fine rate;
(3) increases with the audit rate;
(4) increases with the tax rate for CARA and DARA preferences; and
(5) increases with income for CARA and IARA preferences.

Also, we show that the optimal compliance rate is smaller than the optimal compliance rate when tax payments are donated to a public good, and we show that the optimal compliance rate may be affected by wealth effects, depending on preferences.

Proof. To simplify notation write the accumulated unreported income, x as $x = zw$. The decision problem (1) of our individual is

$$\max_{R\in[0,1]} V = \left[(1-p)U(\pi(R,z,0)) + pU(\pi(R,z,1))\right] \quad where$$

$$\pi(R,z,s) = w\left[1-\tau R - s\tau(1+f)(1-R+z)\right], s \in \{0,1\}$$

By concavity of $U(.)$, solution of the first order condition maximizes V. Differentiating V with respect to R, we get

$$F = (1-p)U'(\pi_o)\frac{\partial \pi_o}{\partial R} + pU'(\pi_1)\frac{\partial \pi_1}{\partial R} = \tau w\left[-(1-p)U'(\pi_o) + pfU'(\pi_1)\right]$$

The optimal R^* is 0 if $F(.)<0$ on [0,1]. It is 1 if $F(.)>0$ on [0,1]. Otherwise we have an interior optimal compliance rate R^*, the one that solves $F(.)=0$ (*).

Comparative Statics. To get effects of parameters on optimal interior solution, R^* we use the implicit function theorem. Optimal R^*:

1. increases with accumulated unreported income multiple as $sign(\frac{\partial R^*}{\partial z}) = sign(F_z)$, and

$$F_z = -\tau^2 w^2 (1+f)pfU''(\pi_1) > 0$$

   where the inequality follows from concavity of $U(.)$;

2. increases with the fine rate f as $sign(\frac{\partial R^*}{\partial f}) = sign(F_f)$, where

$$F_f = -\tau^2 w^2 (z+1-R)pfU''(\pi_1) + \tau wpU'(\pi_1) > 0$$

3. increases with the audit rate p as $sign(\frac{\partial R^*}{\partial p}) = sign(F_p)$, where

$$F_p = \tau w\left[U'(\pi_o) + fU'(\pi_1)\right] > 0$$

4. increases with the tax rate τ for CARA and DARA preferences as the $sign(\frac{\partial R^*}{\partial \tau}) = sign(F_\tau)$, and $F_\tau = \tau w^2 \left[(1-p)RU''(\pi_o) - \left[R + (1+f)(z+1-R)\right] pfU''(\pi_1)\right]$

   The first term within the square bracket is negative while the second term is positive, so the total effect depends on which term is largest in absolute value. Using (*), $(1-p)/pf = U'(\pi_1)/U'(\pi_o)$ and the Arrow-Pratt measure of absolute risk aversion, $A(y) = -U''(y)/U''(y)$ we get
$$sign(F_\tau) = sign\left[-A(\pi_o) + \left[1 + (1+f)(\frac{z+1}{R} - 1)\right] A(\pi_1)\right]$$
   For CARA preferences or DARA preferences compliance rate increases with the tax rate.

5. increases with income *w* for CARA and IARA preferences. Verify that
$$F_w = \tau w\left[-(1-p)(1-\tau R)U''(\pi_o) + pf\left[1 - \tau R - \tau(1+f)(z+1-R)\right]U''(\pi_1)\right]$$
   The first term is always positive. The sign of the second term is the negative of the sign of the expression within the inside square bracket, the sign of which is ambiguous. If negative (e.g. large *z*, or large *f*), then the compliance rate increases with income *w*; if not, then the total effect is ambiguous. Furthermore, similar steps as in (5) result in
$$sign(F_w) = sign\left[A(\pi_o) - \left[1 - \frac{\tau(1+f)(z+1-R)}{1-\tau R}\right] A(\pi_1)\right]$$
   For CARA or IARA preferences then the optimal compliance rate increases with income.

Donation Effects. If tax payments are used to finance a public good *P* that benefits others, then the utility function is $\varphi(\pi, P)$, which is a concave and increasing function of both own income and the public good level. For a separable specification, $\varphi(\pi, P) = f(U(\pi), v(P))$, we have that at R^*, $\partial V(\varphi(.))/\partial R > 0 = \partial V(U(.))/\partial R$, and therefore the optimal *R* that solves $\partial V(\varphi(.))/\partial R = 0$ is larger than R^*.

Wealth Effects. Subjects are paid for all rounds in our experiment, which could elicit wealth effects (Cox, Sadiraj, and Schmidt, 2015). Here we allow the decision problem (1) for subjects to depend on the accumulated payoff. For DARA preferences, we predict wealth to have a negative effect on the compliance rate; for IARA preferences, we predict a positive effect; and for CARA preferences, we predict no effect, which is our working specification in the main text. The decision problem (1) becomes
$$\max_{R\in[0,1]} V = \left[(1-p)U(\pi(R,z,0)) + pU(\pi(R,z,1))\right] \quad where$$
$$\pi(R,z,s) = W + w\left[1 - \tau R - s\tau(1+f)(1-R+z)\right], s \in \{0,1\}$$
where *W* is accumulated payoff. Optimal R^* decreases with wealth (accumulated payoff) as $sign(\frac{\partial R^*}{\partial W}) = sign(F_W)$. Using expression (*) and the definition of (the coefficient of Arrow-Pratt) absolute risk aversion we get

$$F_W = \tau w\left[-(1-p)U''(\pi_o) + pfU''(\pi_1)\right] = \tau wpfU'(\pi_1)\left[A(\pi_0) - A(\pi_1)\right]$$

Hence, for CARA preferences W has no effect on the compliance rate. For DARA preferences the effect is negative, while for IARA preferences the effect is positive.

Dynamic Model.

Audit State Dependent Compliance: Appendix Table A.1a reports optimal compliance rates as the solutions to problem (3) and (4) with T=30, for an individual with CARA preferences with parameter 0.026 who earned 240 EC and participated in the Baseline treatment. The left two columns show state dependent compliance rates when the discount factor is 0.9 and is time invariant. The compliance rates are stable with the exception of the end game rounds. A time trend is visible in compliance rates in the middle two columns that correspond to the case with discount factor 0.9^t, that depends on how long the game has been on. The right two columns show compliance rates for the myopic case with discount factor 0. In all scenarios, the compliance rate is smaller in state 1.

Treatment Effects. To get some insights on treatment effects on compliance rates, we report in Appendix Table A.1b optimal solutions for rounds 11-13 for all seven treatments. Note several results. First, compliance rates in state 1 (following an audit event) are always smaller than in the state 00. Second, compliance rates are stable for fixed and 0 discount factor but they decrease when "patience" decreases with time (the middle columns). Third, compliance rates are lowest for myopic individuals, whose behavior is captured by decision problem (1). For such individuals, compliance rates across states are either very low or very high. We expect to see clusters of 1 (full compliance) since the cumulative probability of state 00 is 0.81 (with the exception of the Lower Audit Rate treatment T7). Fourth, all fiscal parameters have positive effects on compliance rates. For example, the state 1 compliance rate (first column) increases from 0.73 to 0.81 to 0.9 as the fine rate increases from 0.3 to 0.6 to 1.2 (first column). The state 00 compliance rate (the second column) increases from 0 to 0.10 to 0.37 when the audit rate increases from 0.01 to 0.10 to 0.20.

Table A.1: Optimal Compliance Rates

a. Baseline

	Discount factor 0.9		Discount factor 0.9^t		Discount factor 0	
Round*	state 1	state 00	state 1	state 00	state 1	state 00
4	0.81	0.87	0.73	0.83	0.10	0.10
5	0.81	0.88	0.71	0.82	0.10	1
6	0.81	0.87	0.68	0.81	0.10	1
7	0.81	0.88	0.66	0.80	0.10	0.10
⋮						
26	0.81	0.88	0.28	0.72	0.10	1
27	0.78	0.85	0.27	0.70	0.10	1
28	0.90	0.96	0.25	0.72	0.10	0.10
29	0.79	0.83	0.24	0.71	0.10	1
30	0.10	0.31	0.10	0.67	0.10	1

b. Across Treatments

		Discount factor 0.9		Discount factor 0.9^t		Discount factor 0	
Treatment	Round	state 1	state00	state 1	state00	state 1	state00
Baseline	11	0.81	0.88	0.56	0.77	0.10	1
	12	0.81	0.87	0.54	0.76	0.10	1
	13	0.81	0.88	0.52	0.76	0.10	0.10
Higher Fine Rate	11	0.90	0.94	0.72	0.88	0.51	1
	12	0.90	0.95	0.71	0.87	0.51	1
	13	0.90	0.94	0.69	0.87	0.51	0.51
Lower Fine Rate	11	0.73	0.81	0.43	0.65	0	0.60
	12	0.73	0.81	0.40	0.65	0	1
	13	0.73	0.81	0.37	0.64	0	0
Higher Tax Rate	11	0.87	0.92	0.71	0.84	0.40	1
	12	0.87	0.92	0.69	0.85	0.40	1
	13	0.87	0.91	0.68	0.83	0.40	0.40
Lower Tax Rate	11	0.43	0.63	0	0.30	0	0
	12	0.43	0.63	0	0.29	0	0.29
	13	0.43	0.62	0	0.27	0	0
Higher Audit Rate	11	0.92	0.95	0.70	0.85	0.37	1
	12	0.92	0.95	0.68	0.85	0.37	1
	13	0.92	0.95	0.67	0.84	0.37	0.37
Lower Audit Rate	11	0.48	0.62	0.16	0.51	0	0.30
	12	0.48	0.62	0.13	0.50	0	1
	13	0.48	0.62	0.10	0.49	0	0

Notes: T=30; lambda=0.026; Tax Rate =0.3; Fine Rate =0.6; Audit Rate=0.1. *Figures in rounds 8 to 25 are the same as in rounds 5 and 6 for a constant discount factor, but decrease for a time-varying discount factor, 0.9^t.

<u>APPENDIX 2: DOUBLE HURDLE PANEL ESTIMATES</u>

Observations with full compliance account for 58 percent of our data, and full compliance remains the mode choice even in state 1 (following audit), as can be seen in Appendix Figure A.1. Estimation of treatment effects requires that we address this aspect of our data. Out of 122 subjects, 30 subjects reported full income in every round. One way to think about this is to categorize subjects as "truthful" types (e.g., subjects are averse to misreporting, they think of paying taxes as a moral duty) or not; this is the first hurdle. In the main text we dealt with this problem by getting Tobit estimates with and without these subjects. Here we report double hurdle models that allow for estimating "truthful" types of subjects, while at the same time subjects who cleared the "truth" hurdle are allowed to report true income at any time if they wish to do so (e.g., when previously unreported income becomes large). The first hurdle is a probit equation, and the Tobit equation is estimated only with subjects who clear the first hurdle. The double hurdle model allows for differences in the variables that affect the different decisions of whether to report all or some of true earned income versus the decision on how much to report conditional upon clearing the first hurdle (e.g., deciding to misreport). The estimation approach we use here is a double-hurdle panel model (Cragg, 1971; Engel and Moffatt, 2014).

More precisely, let y_i denote the observed compliance rate of individual i (defined as Reported Income/True Earned Income) and y_i^* denote the continuous latent compliance rate that is observed only if the subject has elected to misreport income. The model specification is:

$$y_i = D_i y_{it}^*$$

where

$$D_i = \begin{cases} 1 & if \quad \beta Z_i + \delta_i < 1 \\ 0 & if \quad \beta Z_i + \delta_i \geq 1 \end{cases}; \quad \delta_i \sim N(0,1)$$

$$y_{it}^* = \begin{cases} \alpha X_{it} + u_i + \varepsilon_{it}, & if \quad \alpha X_{it} + u_i + \varepsilon_{it} < 1 \\ 1, & if \quad \alpha X_{it} + u_i + \varepsilon_{it} \geq 1 \end{cases}; \quad \varepsilon_{it} \sim N(0,\sigma^2),\ u_i \sim N(0,\sigma_u^2)$$

where Z_i and X_{it} are explanatory variables for the first and the second equations. The first equation (for D_i) does not depend on task t because a "truthful" type would never report less than full income under no circumstances. As a result, in the list of the regressors for the first stage probit equation we include only variables that are fixed across rounds, such as the tax rate, the audit rate, the fine rate, and gender. Subjects who report true income in round will fail the hurdle. The second equation (y_{it}) is a random effects Tobit regression, which allows for choices to depend on the task that differs across rounds because of the variation on unreported income from previous rounds. Therefore, we add time-varying variables (such as unreported income rate, true earned income, being audited last) in the list of regressors.

Appendix Table A.2 reports average marginal effects for the double hurdle panel model, estimated using all individual data by round and by treatment. The columns labeled *Hurdle* present the marginal effect on the decision to report *True Earned Income*; the columns labeled *Linear* report the results for the subject's *Compliance Rate* conditional upon the subject misreporting income.

The impacts of the policy parameters are again largely consistent with the theory and with the Tobit estimation results of Table 4 in the main text. For the basic model specification (1), knowing that the tax payment goes to charity decreases the probability of clearing the hurdle by 23 percent while a one-unit change in the tax rate increases it by almost 87 percent. The signs are consistent with Tobit estimates in the first column of Table 4 (which can be seen as the first hurdle that applies to all subjects). Focusing on the linear estimates in Appendix Table A.2, or the estimates of individual behavior conditional upon clearing the "hurdle" of choosing some noncompliance, compliance increases with an increase in the audit rate, an increase in the penalty rate, and with an increase in accumulated unreported income; compliance decreases with income. The use of the tax payments (or *Donation*) and the *Tax Rate* are not statistically significant determinants of the individual compliance rate, once the hurdle has been passed. *Female* decreases the probability of clearing the hurdle by 19 percent, but has no effect on compliance rate conditional on passing. These estimates are comparable to the Tobit estimates for selected subjects (the 30 subjects not included there would have failed the hurdle).

Appendix Figure A.1 Histograms of Compliance Rates after an Audit (State 1)

Table A.2 Panel Double Hurdle Model: Average Marginal Effects for Compliance Rate

Explanatory Variable	Specification 1		Specification 2		Specification 3	
	Hurdle	Linear	Hurdle	Linear	Hurdle	Linear
Donation [D]	-0.227*** (0.086)	-0.055 (0.135)	-0.223** (0.089)	-0.068 (0.133)	-0.259*** (0.092)	-0.016 (0.138)
Tax Rate	0.869* (0.498)	1.233 (0.834)	0.822* (0.488)	0.961 (0.610)	0.835* (0.445)	0.926 (0.732)
Penalty Rate	0.216 (0.259)	0.527*** (0.185)	0.205 (0.249)	0.502*** (0.174)	0.110 (0.216)	0.442** (0.200)
Audit Rate	-0.678 (0.920)	3.500*** (0.885)	-0.714 (0.927)	3.651*** (0.841)	-0.903 (0.943)	3.333** (1.408)
Accumulated Unreported Income Rate		0.097*** (0.022)		0.088*** (0.024)		-0.016 (0.138)
Income		-0.080** (0.032)		-0.055* (0.032)		0.926 (0.732)
Audited Last Round [D]				-0.209*** (0.045)		-0.264*** (0.042)
Accumulated Payoff				-0.004*** (0.001)		-0.004*** (0.001)
Female [D]					-0.190** (0.077)	0.016 (0.130)
Observations	2437		2437		2437	
Number of Subjects	122		122		122	

Notes: The dependent variable is *Compliance Rate* (=Reported Income/Full Income), with a lower bound of 0 and an upper bound of 1. The first hurdle is full compliance. The unit of measurement for *Income* (earned points in the current round) as well as *Accumulated Payoff* (sum of earnings at the beginning of the current round) is 100 EC; dummy variables are denoted with [D]; *Accumulated Unreported Income Rat*e is the ratio between the total unreported income during the previous two rounds and income in the current round. Standard errors are in brackets; *** p<0.01, ** p<0.05, *p<0.1.

Appendix 3: Subject Instructions for Treatment 1

Welcome and thank you for participating in today's experiment.

Introduction
This is an experiment in the economics of decision-making. Your earnings will be determined by your answers and decisions and the chance of audit*, as described in the following instructions.

* In this context, "audit" means that the system can choose you in order to verify if you are behaving correctly and telling the truth.

IT IS IMPORTANT THAT YOU READ THESE INTRUCTIONS CAREFULLY.

This experiment is structured in a way that only you can know your earnings. All of the money that you earn will be paid to you privately and in cash immediately at the end of today's experiment.

> *If you have any questions, RAISE YOUR HAND and an experimenter will approach you and answer your questions in private. Please feel free to ask as many questions as you like.*

A. Time
This experiment will last no more than two hours.

B. Show up fee
You will be paid an amount of 25,000 pesos (USD 13) for showing up for today's experiment. This is in addition to what you will earn from the decisions you make during today's experiment. You will not receive any payment if you decide to leave the experiment before it finishes.

C. Earning money during the experiment
You earn money in Experimental Coins (EC) in each decision period. This amount will be displayed on your computer screen at the completion of each decision period. At the end of today's experiment, your total accumulated earnings in Experimental Coins will be converted into Colombian Pesos at the below mentioned conversion rate.

Conversion Rate: 1 Experimental Coin (EC) = 10 Pesos

How to earn money in the experiment?

D. Task and decision-making process
Beginning
Each subject will be given an endowment of 500 EC at the beginning of the experiment.
In this experiment you will have to pass through an indefinite number of decision-making periods. Each of the periods has the following sequence of events.

Event 1

During each period, each subject is given a work task of adding numbers together in order to earn income. You will be given 90 seconds to conduct the task. You will earn 30 Experimental Coins for each correct answer to an addition question. This income you earn will be displayed on your screen at the completion of the task.

Event 2

Your Earned Income is what you earn in Event 1. You will have to make the choice of how much of this Earned Income to report for tax purposes using the sliding scale on your computer screen. There is an income tax at rate 30% that you need to pay on the income you report. As you move the slide to determine how much income you will report, you can see the consequences of your choice in terms of your net income if you are audited or not.

You can choose to report all of your income, part of your income, or none of the income earned from the work task in this experiment. Since the income tax rate is 30%, the amount of tax you will owe is equal to:

30%* Reported Income

Event 3

Once you choose the amount of your Earned Income to report, a random audit will be performed. The probability that you will be randomly selected for audit is 10% in each and every period, which means that you would be selected for audit about 10 times in 100 periods. If you are chosen for the random audit in some period, <u>your earned income in that period and in the preceding two (2) periods</u> will be disclosed for audit. If the audited individual's reported income is <u>less than</u> the earned income in the current period or any of the preceding two (2) periods, then the individual pays, <u>in addition to the tax of</u> 30% of the earned income, a penalty of 60% of the unpaid tax in the current and two (2) preceding periods.

You pay a tax penalty only if you are audited and if your reported income is less than the earned income in the current period or any of the 2 preceding periods.

Section E below shows your total payoff in each decision period resulting from Events 1 to 3 explained above.

E. Earnings in each decision period

<u>Scenario I: If you are **no**t audited</u>

Total earnings equals Earned Income minus Tax Payment on Reported Income.

<u>Scenario II: If you **are** audited</u>

Total earnings equals Earned Income minus Tax Payment on Earned Income minus Penalty paid on unreported income in the current period and the preceding 2 periods.

(Note: The penalty is equal to zero if your Reported Income is equal to your Earned Income in every period.)

F. Practice Periods and Real Periods

Practice Periods: There will be 3 Practice Periods in the experiment. In the Practice Periods, the audit results are not randomly selected. You will be audited in the last Practice Period but not in any of the others. No money can be made or lost in the practice periods; they are only for practice.

Real Periods: There will be an undetermined number of Real Periods in the experiment. Real money, paid in cash at the end of the experiment, can be made and lost in the Real Periods. The audit probability is 10% in every Real Period.