Using competition to stimulate regulatory compliance: a tournament-based dynamic targeting mechanism

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Abstract: This article develops a tournament-based dynamic targeting mechanism for achieving regulatory enforcement leverage. In contrast to existing models which rely on a representative agent, we model a game among a regulated group of agents, possibly heterogeneous in their levels of a regulated activity, that compete through their compliance decisions to avoid being targeted for future audits. The empirical properties of the dynamic tournament are established using economics experiments. In particular, we test comparative statics, highlight the importance of inducing competition through comparisons with a (non-competitive) standards-based targeting mechanism, and demonstrate enforcement leverage through comparisons with simple random audits. The experiments suggest that the dynamic tournament induces incentives consistent with theory, and overall we find that (introducing) competition in the regulatory enforcement arena may have important advantages.

Keywords: dynamic tournament; contests, competition; regulatory enforcement; targeting; self-reporting

JEL classification: C91; C92; L51; Q58

1. Introduction

Limited resources are typically available to enforce the compliance of regulatory standards, and this mandates that the regulator uses all available information to target for audit likely offenders. One potentially important source of information is the observed differences in the behavior of agents. Within the environmental, health, and tax arenas, this heterogeneous behavior may be tied to mandatory reports regarding toxic releases, workplace injuries, and tax liabilities. When such information from regulated "peers" is available to use for targeting audit efforts, this naturally creates competition amongst players not to stand out in some undesirable way and draw scrutiny. A second source of information is the agent's own compliance history. The regulatory compliance literature has largely focused on this, developing models where agents are placed into two (or more) groups based on their history of compliance relative to a regulatory standard (e.g., Landsberger and Meilijson, 1982; Greenberg, 1984; Harrington, 1988; Harford, 1991; Raymond, 1999; Friesen, 2003; Stafford, 2008; Liu and Neilson, 2009). Agents with poor compliance history are placed in a targeted group that is associated with higher expected costs (e.g., higher audit probability), and agents found to be compliant are transitioned to the non-targeted group (or remain non-targeted if already so).

In this paper, we develop a dynamic tournament model that characterizes a setting where the regulator incorporates both peer-evaluation and compliance history to target enforcement effort.[1] Agents in the dynamic tournament, through compliance efforts, compete to avoid being targeted for (costly) future audits. This competition hinges on agents' relative compliance as

[1] To our knowledge, with the exception of concurrent work by Liu and Neilson (2013), both of these features have not been simultaneously modeled. In contrast to their work, we assume agents in different groups compete in separate tournaments, rather than in a single tournament. This simplifies the model dramatically (importantly, there is an analytical solution), and significantly increases the compliance effort induced by competition. Just as crucial, given that a key characteristic of targeting models is that different groups face different compliance incentives (e.g. audit probabilities), a single-tournament becomes a competition that only those in a particular group are likely to win. This characterization does not appear to fit the settings we endeavor to model.

revealed by audits. We assume that agents placed in the targeted and non-targeted groups engage in separate tournaments. That is, those in the targeted group compete to be transitioned to the non-targeted group, and agents in the non-targeted group compete to avoid being moved to the targeted group. Harrington's (1988) seminal work began as a way to explain how the exercise of discretion in regulatory enforcement could achieve high levels of regulatory compliance despite the appearance of few inspections and low fines for violations. Our model, and complementary economics experiments, extend that argument to suggest targeted enforcement that promotes competition may be an effective way to achieve enforcement leverage.

A key feature in our model is that agents can be heterogeneous in the level of the regulated activity (e.g. pollution), which demonstrates that dynamic targeting mechanisms can be applied to groups of dissimilar agents. Existing models instead examine a representative firm that is simply solving a dynamic optimization problem with complete information about the rules governing inspections. In our model, the number of audits conducted at a given time is fixed, which introduces inspection capacity constraints. This is a departure from existing models that fix audit rates but do not limit the sizes of the targeted and non-targeted groups.

Similar to Harford (1991), we consider a continuous choice setting and uncertainty in the audit process. These features are endemic to field settings. Following Harrington (1988), most dynamic targeting models and all related experimental studies focus on a binary choice setting wherein firms choose to comply with a regulation or not, and audits perfectly reveal violations. In such settings targeting is only relevant to a firm if it complies when targeted but does not when not targeted; otherwise, if a firm always complies then there is no value to being in the non-targeted group. Consequently, as Friesen (2003) points out, optimal behavior by the regulator is to never inspect firms in the non-targeted group and to place firms in the targeted group at random.

In addition to these theoretical issues, experimental evidence suggests the leverage achieved by previous dynamic targeting models may be less than predicted. Cason and Gangadharan (2006) test Harrington's (1988) model and find that increasing the probability of being transitioned to the non-targeted group (when found compliant) increases the proportion of agents in compliance. However, the effect is not as large as theory suggests. Clark, Friesen and Muller (2004) test Harrington's (1988) and Friesen's (2003) dynamic targeting models and find that compliance rates are no higher than with simple random audits.

Though competitive incentives may (be perceived to) exist in common regulatory settings, the opaqueness of most enforcement processes make it difficult to identify the effects of competition using naturally-occurring data. As such, congruent with previous empirical studies in this area, we turn to the experimental laboratory to gain insight on the performance of the dynamic tournament mechanism. There are several existing experimental studies based on dynamic tournament models, but none are tied to regulation. The closest to our investigation are multi-stage elimination contests, where workers' effort choices in one stage affect payoffs in subsequent stages (e.g. Parco et al., 2005; Amaldoss and Rapoport, 2009; Shremata 2010; Altmann, Falk and Wibral, 2012). In contrast to these tournaments, rather than the contest prize being a fixed amount, the level of "effort" (i.e. disclosure) in our setting determines not only who "wins" (i.e. who is in the non-targeted group) but payoffs upon winning as effort determines the expected penalties for misreporting. Instead of eliminating losers from further competition, both winners and losers continue to compete with the possibility of transitioning back and forth between winner and loser groups. Finally, as the costly effort choice is intertwined with a compliance benchmark, attitudes towards cheating may lead to unique behaviors.

The experiment results confirm the comparative statics of the proposed dynamic tournament, and further that the mechanism results in significant leverage over random audits. These basic findings are consistent with Gilpatric, Vossler and McKee (2011), who provide favorable theoretical and experimental evidence on the ability of relative evaluation mechanisms such as tournaments to motivate regulatory compliance, but in a static setting. The standards-based dynamic targeting mechanism also achieves significant leverage. However, the theoretical incentives of this mechanism appear to translate poorly to behavior, as the predicted effects of changing the transition probability (through a change in the group-specific standards) or audit costs are not observed in the data. Thus, our findings overall stress caution in the use of the standards-based dynamic targeting mechanisms as policy instruments, and highlight the importance of (inducing) competition in regulatory settings.

2. Regulatory Enforcement Models

Prior to formalizing the enforcement mechanisms a discussion of the underlying regulatory environment these models seek to characterize is warranted. The literature on targeted enforcement has developed to reflect the observation that many regulations are enforced by infrequent inspections coupled with low fines for violations. In keeping with this line of work, we consider a setting where a resource-constrained agency wishes to maximize compliance through its choice of enforcement mechanism. Our emphasis is on showing how the tournament mechanism we develop may increase compliance relative to a random process, which is commonly referred to as "leverage". Of course the problem faced by a regulator is broader, entailing a choice of resources dedicated to enforcement, which must weigh the costs to firms as well the benefits generated (including revenues from taxes or fines). Our framework may in some circumstances appear to be unfair or otherwise lead to undesirable outcomes. For example, it is possible for the mechanisms to induce over-compliance, i.e., firms reporting more than their true output. However, if this is undesirable the regulator's optimization problem would never yield such an outcome as this would entail supra-optimal resources dedicated to enforcement.[2]

We frame our theory in the context of a regulation requiring disclosure of an activity, which we will label as output. In an environmental setting, this could reflect the mandatory disclosure of emissions through the Toxics Release Inventory. In a public finance setting, this could characterize tax compliance under a voluntary reporting system. Although we frame our model in terms of disclosure, it can apply with minor modifications to regulated actions. For example, a regulation may limit emissions, with fines for exceeding the limit. A dynamic regulation tournament would then be based on a comparison of audited firms' detected emissions.

The basic components of our models follow the static models of Gilpatric, Vossler and McKee (2011). Actual output, x, is exogenously determined, and firms choose how much to disclose. Firms may be heterogeneous in terms of their output. Disclosure of output, q, is a continuous choice that is assumed to have a constant marginal cost, α, which could result from a tax, but also could incorporate other costs such as those emanating from a negative market reaction. An audited firm pays a fixed cost of being inspected, γ, which can represent costs associated with accommodating inspectors, and documentation requirements. Further, there is a marginal penalty on output determined by the audit to have been underreported, denoted β. This

[2] Over-compliance is possible in the models presented here if and only if errors in the audit process make it possible for an inspection to reveal more output than the firm believes it has emitted.

penalty is assumed to be at least as high as the unit cost of disclosed output. The penalty represents any regulatory fines imposed, but may also entail other costs to the firm of being found non-compliant with the disclosure requirement. When an audit occurs the output detected is stochastic, which can represent errors in the audit process, or firm uncertainty over actual output. An audit reveals output of $x + \varepsilon$, with ε being drawn from the distribution $F(\varepsilon)$, which is assumed to have positive density $f(\varepsilon)$ on the interval $[a, b]$. We impose little structure on the distribution of audit errors. If audit errors are one-sided (meaning an audit cannot reveal output in excess of units actually emitted) then $a < 0$ and $b = 0$. If audits yield an unbiased estimate of output then $E[\varepsilon] = 0$. We will assume $a > -x$ so that an audit cannot reveal negative output.

2.1 Disclosure in a static model with random auditing

Suppose a firm is audited at random with probability p which is independent of whether other firms are audited. Employing an enforcement framework similar to that developed in Evans, Gilpatric and Liu (2009), firm i chooses the optimal output to disclose to minimize expected costs, which are denoted π_i

(1) $$min_{q_i} \pi_i(q_i) = \alpha q_i + p\left\{\gamma + \beta \int_{q_i - x_i}^{b} (x_i + \varepsilon - q_i) f(\varepsilon) d\varepsilon\right\}.$$

So long as an interior solution exists the optimum q_i^* is *independent* of actual output. For notational convenience, define $z_i \equiv q_i - x_i$ as the reporting deviation, so that a negative z_i represents underreporting. The reporting choice can then be restated as

(1') $$\min_{z_i} \pi_i(z_i) = \alpha(x_i + z_i) + p\left\{\gamma + \beta \int_{z_i}^{b} (\varepsilon - z_i) f(\varepsilon) d\varepsilon\right\}.$$

The optimal reporting deviation, z_i^* is implicitly defined by

(2) $$\frac{\alpha}{p\beta} = \int_{z_i^*}^{b} f(\varepsilon) d\varepsilon = 1 - F(z_i^*).$$

Under random audits an interior solution exists for z_i^* on the interval $[a, b]$ if $0 < \frac{\alpha}{p\beta} < 1$, with z_i^* defined by (2) above. For $\alpha > p\beta$ it is not optimal to report any output, so a corner solution at $q_i = 0$ obtains. At an interior solution, the firm's optimal report is decreasing in the reporting cost; increasing in the probability of audit; and increasing in the penalty on revealed but unreported units (these results follow directly from the fact that F is an increasing function of z_i). The solution is independent of the fixed cost of being audited. Note that a firm's minimized cost, $\pi_i(z_i^*)$, is an increasing concave function of the audit probability p.[3]

2.2 Disclosure with targeted enforcement in a dynamic Markov tournament

We assume *N* firms operate in a regulated industry. In each period the regulator places each firm into one of two groups, G_1 (the "non-targeted" group) and G_2 (the "targeted group"). We assume that the audit probability is higher in the targeted group, although in general the targeted group could face higher expected compliance costs through several channels, including higher fixed audit costs and higher marginal penalties. The regulator and firms play an indefinite game with common discount factor δ. Firms' reports in each period will, if audited, determine both their penalties (as above) and whether they remain in their current group through a rank-order tournament among firms in each group. Firms in G_1 are indexed by *i*, firms in G_2 are indexed by *j*, time periods are index by *t*, and groups are indexed by *l*.

We assume that the error distribution is identical for firms across groups. When his holds, firms that are heterogeneous in output will nevertheless be strategically symmetric competitors. The fact that this mechanism applies to firms that are heterogeneous in output is an important

[3] The concavity of the firm's minimized cost follows the usual logic. If the firm did not change reporting as *p* increased, expected costs would increase linearly; by re-optimizing with a higher report as the audit probability increases the firm's expected cost increases at a decreasing rate.

feature of our model because it applies targeted enforcement to a setting that explicitly accommodates firm heterogeneity. Because only the *difference* between a firm's report and its true emissions affects a firm's marginal payoffs, the mechanism renders the true output of each firm strategically irrelevant. Since firms are strategically symmetric competitors we will identify the symmetric equilibrium of the game where the reporting deviations of all firms are equal.

Let n_l be the number of firms, m_l be the number of firms selected for audit, and $\rho_l = \frac{m_l}{n_l}$ denote the audit probability in group *l*. Being selected for audit exposes the firm to possible fines, and places the firm in the tournament that determines whether it is transitioned. The τ firms in G_1 that are audited and found to have reported the least relative to the audit outcome (irrespective of whether they are found to have reported less than the audit outcome) are transitioned to G_2. That is, the τ firms in G_1 for which $\varepsilon_i - z_i$ is largest are transitioned to G_2. The τ firms in G_2 that are audited and found to have reported the most relative to the audit outcome (irrespective of whether they are found to have reported more than the audit outcome), i.e. the firms for which $\varepsilon_j - z_j$ is smallest, are transitioned to G_1. Of course $\tau < m_l$, and firms choose their reports before it is revealed which will be selected for audit.[4]

Let the probability that a firm in G_1, selected for audit, ranks among the bottom τ firms in the resulting tournament (and therefore gets transitioned to G_2) be represented by $Q_i(z_i, z_{-i})$, and the probability that a firm in G_2 which is selected for audit ranks among the top τ firms (and therefore gets transitioned to G_1) be represented by $R_j(z_j, z_{-j})$. In each period, two standard symmetric rank-order tournaments of the type widely studied beginning with Lazear and Rosen

[4] The structure of the tournament implies that one or more firms "losing" in G_1 and being transitioned to G_2 could possibly have been found to have reported more truthfully than those "winning" in G_2 and being transitioned to G_1. In equilibrium this would be unlikely and would occur only due to the randomness of audit outcomes because the equilibrium choice of disclosure in G_1 is lower than in G_2. It is also true that, since τ firms must transition each direction, it is possible that a firm in G_1 can be transitioned despite being found to have complied or over-reported.

(1981) and Nalebuff and Stiglitz (1983) take place, one among the m_1 firms from G_1 who are selected, and one among the m_2 firms from G_2 who are selected. These tournaments differ in that the G_1 contest is a competition to avoid ranking at the bottom while the G_2 contest is a competition to rank at the top. Applying a result from Nalebuff and Stiglitz (1983), the probability that a firm in G_1 who chooses report z_i when the other firms in G_1 choose z_{-i} ranks in exactly the *k*th position up from the bottom (e.g. *k*=1 denotes ranking last) is the following

(3) $$Q_{ik}(z, z_{-i}) = \int \frac{(m_1-1)!}{(m_1-k)!(k-1)!} f(\varepsilon_i)\big(F(\varepsilon_i + z_i - z_{-i})\big)^{k-1}\big(1 - F(\varepsilon_i + z_i - z_{-i})\big)^{m_1-k} d\varepsilon_i.$$

The probability that *i* ranks among the bottom τ is then

$$Q_i(z, z_{-i}) = \sum_{k=1}^{\tau} Q_{ik}(z_i, z_{-i}).$$

For identifying the symmetric Nash equilibrium of the tournament we require the marginal effect of disclosure on the probability of ranking among the bottom τ evaluated when $z_i = z_{-i}$. The marginal effect on the probability of ranking in position *k* is

(4) $$\frac{\partial Q_{ik}(z_i,z_{-i})}{\partial z_i}\Big|_{z_i=z_{-i}} = \int \frac{(m_1-1)!}{(m_1-k)!(k-1)!}\big(f(\varepsilon_i)\big)^2\left\{\big(1-F(\varepsilon_i)\big)^{m_1-k-1}\big(F(\varepsilon_i)\big)^{k-2}\right\}\big\{(k-1)\big(1-F(\varepsilon_i)\big) - (m_1-k)F(\varepsilon_i)\big\} d\varepsilon_i.$$

The effect of disclosure in symmetric equilibrium on the probability of ranking among the bottom τ is then

(5) $$\frac{\partial Q_i(z_i,z_{-i})}{\partial z_i}\Big|_{z_i=z_{-i}} = \sum_{k=1}^{\tau} \frac{\partial Q_{ik}(z_i,z_{-i})}{\partial z_i}\Big|_{z_i=z_{-i}}.$$

The G_2 tournament is directly analogous except that reporting higher output increases the probability of a firm's ranking among the top τ in the group.

The dynamic game follows a Markov chain process with a transition matrix identifying the probabilities of transitioning between targeted and non-targeted states. The Markov transition matrix representing the probability that a firm will be in G_1 or G_2 in period t+1 conditional on his group assignment in period *t*, is as follows:

	To Group	
From Group	G_1	G_2
G_1	$1-\rho_1 Q_i$	$\rho_1 Q_i$
G_2	$\rho_2 R_j$	$1-\rho_2 R_j$

Let π_{lt} be the expected cost in the current period for a firm in group *l* at time *t*. Following from the random audit model, this is equal to $\pi_{lt}=\alpha(x_i+z_i)+\rho_l\left\{\gamma+\beta\int_{z_i}^{b}(\varepsilon-z_i)f(\varepsilon)d\varepsilon\right\}$. Further, let V_{lt} be the expected present value of future costs for a firm in group *l* at time t. Then for the two groups we have

(6) $V_{1t}=\pi_{1t}+\delta(1-\rho_{1t}Q_{it})V_{1,t+1}+\delta\rho_{1t}Q_{it}V_{2,t+1}$

(7) $V_{2t}=\pi_{2t}+\delta\rho_{2t}R_{jt}V_{1,t+1}+\delta\big(1-\rho_{2t}R_{jt}\big)V_{2,t+1}.$

Firms choose disclosure in each period to minimize the expected present value of future costs. The expected present value of future costs is the sum of expected costs in the current period and the discounted expected present value of costs starting from the next period, accounting for the probabilities associated with the two possible states the firm may find itself in the following period. The dynamic Markov tournament thus adds "leverage" to the stage-game (simple random audit). A firm in G_1 at any point in time minimizes V_{1t} and a firm in G_2 at any point in time minimizes V_{2t}. Applying the ergodic theorem for Markov chains, the optimal strategy for a firm is stationary, i.e. conditioned only the firm's current state (group), not on the period in the game (Kohlas, 1982; Harrington, 1988). Given stationarity (which allows us to drop the t subscript) we obtain the following first-order necessary conditions:

(8) $$G_1: \frac{\partial \pi_i}{\partial z_i} = -\delta(V_2 - V_1)\rho_1 \frac{\partial Q_i}{\partial z_i}$$

(9) $$G_2: \frac{\partial \pi_j}{\partial z_j} = \delta(V_2 - V_1)\rho_2 \frac{\partial R_j}{\partial z_j}$$

where $(V_2 - V_1) = \frac{(\pi_2 - \pi_1)}{1-\delta(1-\rho_2 R_j - \rho_1 Q_i)}$.

Imposing symmetric behavior among all firms in each group yields the conditions identifying the stationary symmetric equilibrium[5]

(10) $$G_1: \frac{\partial \pi_i}{\partial z_i} = -\delta(V_2 - V_1)\rho_1 \frac{\partial Q_i}{\partial z_i}|_{z_i = z_{-i}}$$

(11) $$G_2: \frac{\partial \pi_j}{\partial z_j} = \delta(V_2 - V_1)\rho_2 \frac{\partial R_j}{\partial z_j}|_{z_j = z_{-j}}$$

where $(V_2 - V_1) = \frac{(\pi_2 - \pi_1)}{1-\delta\left(1-\rho_2\left(\frac{\tau}{m_2}\right)-\rho_1\left(\frac{\tau}{m_1}\right)\right)}$.

This set of equations implicitly defines the equilibrium of the dynamic game entailing symmetric behavior by firms (all firms follow identical strategies conditional on their group).[6] Note that $(V_2 - V_1) > 0$, as the present value of expected costs is higher when currently targeted than when non-targeted (i.e., $\pi_2 - \pi_1 > 0$).[7] Importantly, $\pi_2 - \pi_1$ is determined by the differential audit probabilities which yields different expected enforcement costs, which in turn are dependent on the equilibrium level of disclosure in each group. As discussed earlier, although the equilibrium of the game is symmetric with all firms in each group choosing a common reporting

[5] Collusion in repeated tournament games can be a concern (see, for example, Ishiguro, 2004, and Gurtler, 2009). However, a trigger strategy to support collusion in equilibrium typically requires players to be able to observe the choice (effort) of other contestants. In our context we assume that firms observe each other's *reports*, but the underlying output of each firm, *x*, is unobservable, so competitors do *not* observe each other's strategic choice *z*, the reporting deviation.

[6] The satisfaction of second-order conditions and the existence of pure strategy equilibrium in any rank-order tournament requires sufficient variance of the random component of firms' output. Nalebuff and Stiglitz (1983) discuss this in detail. Consistent with the literature, we assume this condition is met.

[7] It cannot be the case that in equilibrium $\pi_1 > \pi_2$ because if that were true firms would be better off when in the targeted state, which is inconsistent with choosing to report more due to the competition to avoid being targeted. The equilibrium requires that each firm is minimizing the present value of its expected costs at each point in time. For any candidate equilibrium (z_1, z_2) such that $\pi_1 > \pi_2$ a firm would reduce its expected costs by deviating.

deviation, z, because firms may have heterogeneous output x_i, they are likewise heterogeneous in their reported output, $q_i = z + x_i$.

PROPOSITION 1: Equilibrium disclosure by firms in both groups exceeds the level that is optimal in the static random audit model for identical values of α and β, and given equivalent audit probabilities.

PROOF: see online supplement.

This result establishes that leverage arises from the dynamic enforcement mechanism. A firm minimizing its cost in the current period given its group and consequent audit probability would set $\frac{\partial \pi}{\partial z} = 0$. The equations above show there is a gain from leverage in dynamic enforcement because $\frac{\partial \pi_i}{\partial z_i} > 0$ and $\frac{\partial \pi_j}{\partial z_j} > 0$. That is, firms report more than the amount that would minimize their cost in the stage game. Furthermore, note that the magnitude of the gain depends on the value of $(V_2 - V_1)$, the difference in the present value of expected costs to the firm in equilibrium when in G_2 versus G_1. This is the prize at stake in the contest, and its magnitude depends here on the difference in inspection probabilities between the two groups and the equilibrium transition probabilities, which leads to the comparative static results below.

PROPOSITION 2: The equilibrium report of firms in both groups increases with the fixed audit cost, γ, and decreases with the number of firms transitioned between groups each period, τ .

PROOF: see online supplement.

An increase in the probability of audit in a group increases the disclosure of firms in that group directly through the same mechanism that applies in the static random audit case. An increase in ρ_2 (holding ρ_1 constant) increases the value of being in G_1 and thus increases the leverage present in the mechanism, whereas an increase in ρ_1 (holding ρ_2 constant) reduces the leverage effect.

Suppose the regulator's objective is to minimize the sum of enforcement costs and the social costs it believes arise from inaccurate reporting. We can show that the tournament mechanism will better serve this objective relative to random audits. Let $\bar{z}$ represent the level of reporting the regulator seeks to induce. If audits are unbiased such that the expected outcome of an audit is the firm's true output, and if the regulator seeks to induce truthful reporting, then $\bar{z} = 0$. However, $\bar{z}$ may take on other values if, for example, one believes firms are uncertain about their true output and the variance in audit outcomes thus reflects uncertainty in actual output. In that case the regulator's objective may entail $\bar{z} > 0$ if it considers the social cost of any output going unreported to be very large. The regulator is assumed to have a loss function associated with deviations in reports from this ideal, represented as $L(z)$ with $L'(z) < 0$ for $z < \bar{z}$, $L'(z) \geq 0$ for $z \geq \bar{z}$, and $L''(z) > 0$. Let c represent the cost of an audit. Revenues from enforcement are a transfer and thus not present in the objective.

The regulator's optimization problem, assuming parameters other than the audit probabilities are exogenously determined, and accounting for the share of firms in each group in each period, is:

$$(12) \qquad \min_{\rho_1,\rho_2} \frac{n_1}{N}\{L(z_1) + \rho_1 c\} + \frac{n_2}{N}\{L(z_2) + \rho_2 c\}.$$

Note that the random audit mechanism represents a special case within this problem which can obtained by setting $\rho_1 = \rho_2$.

PROPOSITION 3: Suppose the regulator's objective is to minimize the sum of enforcement costs and the social costs associated with deviations from a target level of reporting. Then, the optimal tournament mechanism yields lower cost than a random audit mechanism.

PROOF: see online supplement.

Although there always exists some combination of audit probabilities such that a tournament better serves the regulator's objective, we note that it is not necessarily the case that the tournament mechanism increases the average expected costs each period across the industry when holding constant the regulator's enforcement budget.

2.3 Disclosure under dynamic standards targeted enforcement

Here we develop a dynamic targeting model where each firm is regulated independently so that transitions are determined solely by a firm's disclosure choice relative to a standard. This model is an adaptation of Harford's (1991) model to the disclosure choice setting, which we include in our experimental design as another reference for comparison with the tournament mechanism. The stage game is exactly the same as for the dynamic tournament. The only difference in the mechanism is that transitions from $G_1$to G_2 occur if a firm is audited and found in violation of a standard, and transitions from G_2 to G_1 occur if a firm is audited and found to have met the standard (or exceeded it). In the disclosure choice setting a natural standard is "the truth" in which case a firm is in violation if an audit reveals greater output than disclosed by the firm, $x + \varepsilon > q$, or equivalently, the audit error exceeds the output over-reported by the firm, $\varepsilon > z$. However, this need not be the case, and fixing the standard in this fashion constrains the regulator. In particular, the level of the standard relative to actual output has important consequences because it affects the equilibrium transition probabilities and, consequently, equilibrium disclosure.

We assume the standard can be chosen by the regulator and that it is possible to apply group-specific standards. We denote the distance of the standard between the report and audit outcome in each group, respectively, by s_1 and s_2. Thus a firm in G_1 that is audited will be transitioned to G_2 only if $\varepsilon > z + s_1$, i.e. if the firm is found to have *underreported by more than* s_1. Similarly a firm in G_2 that is audited will be transitioned to G_1 only if $\varepsilon < z + s_2$, i.e. if the firm is found to have *underreported by no more than* s_2. Note that the standards can be negative or positive, i.e. the position of the standard may be "looser" or "tighter" than the truth.

With this notation, an audited firm in G_1 is transitioned to G_2 with probability $\int_{z_1+s_1}^{b} f(\varepsilon)d\varepsilon = \big(1 - F(z_1 + s_1)\big)$ and a firm in G_2 that is audited is transitioned to G_1 with probability $\int_{a}^{z_2+s_2} f(\varepsilon)d\varepsilon = F(z_2 + s_2)$. This yields the following transition matrix:

	To Group	
From Group	G_1	G_2
G_1	$1 - \rho_1\big(1 - F(z_1 + s_1)\big)$	$\rho_1\big(1 - F(z_1 + s_1)\big)$
G_2	$\rho_2 F(z_2 + s_2)$	$1 - \rho_2 F(z_2 + s_2)$

As before, let V_{lt} be the expected present value of total costs for a firm in group *l* at time *t*. Then we have

$$V_{1t} = \pi_{1t} + \delta\left(1 - \rho_{1t}\big(1 - F(z_{1t} + s_1)\big)\right)V_{1,t+1} + \delta\rho_{1t}\big(1 - F(z_{1t} + s_1)\big)V_{2,t+1}$$

$$V_{2t} = \pi_{2t} + \delta\rho_{2t}F(z_{2t} + s_2)V_{1,t+1} + \delta\big(1 - \rho_{2t}F(z_{2t} + s_2)\big)V_{2,t+1}.$$

A firm in G_1 at any point in time minimizes V_{1t} and a firm in G_2 at any point in time minimizes V_{2t}. Given stationarity we obtain the following first order conditions:

$$G_1: \frac{\partial \pi_i}{\partial z_i} = -\delta(V_2 - V_1)\rho_1\big(-f(z_1 + s_1)\big)$$

$$G_2: \frac{\partial \pi_j}{\partial z_j} = \delta(V_2 - V_1)\rho_2 f(z_2 + s_2)$$

where $(V_2 - V_1) = \frac{(\pi_2 - \pi_1)}{1-\delta\big(1-\rho_2 F(z_2+s_2)-\rho_1(1-F(z_1+s_1))\big)}$.

There are two important differences of these equations defining equilibrium behavior relative to those derived for the dynamic tournament. First, the marginal effect of disclosure on the probability of being transitioned is not determined by the tournament equilibrium but instead directly by the density of the audit error distribution. Second, in the dynamic tournament model the equilibrium transition probability in each group is simply the number of transitions divided by group size. In the dynamic standards model the equilibrium transition probabilities depend on equilibrium disclosure levels as well as the standards.

In general, between the tournament and standards mechanism the relative strength of the leverage incentive arising from the dynamics is ambiguous. [8] Where the two dynamic mechanisms differ dramatically is with regard to the variation in the number of audits conducted each period. In the tournament the number of audits is fixed, whereas in the standard mechanism there is considerable variation in even the *expected* number of audits in a period (because the number of firms in each group varies), and of course even greater variation in the actual number of audits because an independent draw determines whether each firm is audited. In our view it is extremely unlikely that a regulator's decisions regarding whether each firm is audited would be

[8] It can be shown, for example, that equilibrium disclosure will be relatively greater in the tournament mechanism, *ceteris paribus*, when the following conditions hold: (1) The standard for transition is identical in both groups, i.e. $s_1 = s_2$; (2) only one player is transitioned each direction in the tournament mechanism, i.e. $\tau = 1$; and (3) audit errors are uniformly distributed. The first two conditions imply greater state-persistence for the tournament, and the last condition equates across mechanisms the marginal effect of disclosure on the equilibrium transition probability.

independent due to the difficulty of managing the resulting variability in auditing resources required over time. It is precisely the interdependence of audits that motivates the tournament model, and which may make it more representative of the reality of targeted enforcement.

3. Experimental Design

The first objective of the laboratory experiments is to test the main comparative statics of the dynamic tournament theory. The second objective is to empirically test the dynamic tournament mechanism relative to alternative mechanisms, which allows us to draw comparisons with related experimental work in this area.

Each session involves 20 participants, which are randomly and anonymously matched into separate cohorts of 10 players for the entire experiment. The participants play four dynamic "games", where each game consists of a sequence of decision periods under the same treatment conditions. The first and second games involve one treatment, whereas the third and fourth games involve a second treatment. At the beginning of each game, $n_1 = 5$ players in each cohort are randomly assigned to G_1 ("Group A" in the experiment) and $n_2 = 5$ to G_2 ("Group B"). In each decision period, players receive endowment E and have baseline output of 20. The decision task for each player is to choose a level of disclosure ("reported output"), at a per-unit tax ("reporting cost") of $1 in experiment currency, by selecting a whole number between 0 and 40, inclusive. After all choices are made, players are randomly selected for audit ("inspection").

The probability of audit, or in the case of the dynamic tournament the fixed proportion of audited players, differs across the two groups. For players selected for audit, they pay a fixed audit cost ("inspection cost"). The audit is unbiased and reveals a level of output ("estimated output") by drawing an *i.i.d.* random number from the uniform distribution with supports [0, 40].

A penalty of \$2 is levied on any output *estimated* by the audit to have been undisclosed.

Similar to related dynamic regulation experiments (Clark, Friesen and Muller 2004; Cason and Gangadharan 2006), the number of periods in a game is determined randomly prior to the session. The possible game lengths are consistent with the distribution implied by a 90% continuation probability – lengths of 5, 8, 12 and 15. A cohort faces each game length exactly once, although the game-length orders vary across cohorts. To capture in the lab setting the incentives of an indefinitely-repeated game (or infinitely-repeated with discounting), participants are informed of the 90% continuation probability but the number of periods in a game is not pre-disclosed.

The feedback given at the end of the decision period includes: (1) whether the player was audited, and if so revealed output; (2) all relevant earnings calculations; (3) the reported output of all ten players in their regulated cohort and whether they were audited; (4) whether the game will continue an additional period; and, for the targeting mechanisms, (5) which player(s) will be transitioned to the alternate group (if the game continues). Providing information on the reports of others reflects naturally-occurring public information disclosure programs.

Tables 1 and 2 summarize the 16 experimental sessions. As illustrated in Table 1 there are six (parallel) treatments for each of the two targeting mechanisms and four treatments for the random audit mechanism. Variable across treatments are audit probabilities (40% or 60% for G_1 and 60% or 80% for G_2), fixed audit cost (25 or 50), and for the targeting mechanisms the (equilibrium) transition probabilities (20% or 40%). To achieve these transition probabilities with the dynamic tournament, either one or two of the members in each group (of five) are transitioned. For the dynamic standards treatments, group-specific standards, s_1 and s_2, are chosen such that – conditional on the audit probability and audit cost parameters – the desired transition probability is achieved in expectation. Transition probabilities are zero for the random audit mechanism, and as such the random audit treatment R3 serves as the basis of comparison for dynamic tournament treatments T3 and T5 (S3 and S5 for dynamic standards), and R4 for T4 and T6 (S4 and S6).

In each session, the first treatment is paired selectively with a second treatment, as illustrated in Table 2. Given the complexity of the experiment, and the time needed to go through instructions, the second treatment was included in order to gather some additional data while economizing on participants. To minimize both cognitive burden as well as to allow for identification of a specific treatment effect, with few exceptions, only one main element of the design changes across treatments within-session (e.g. the mechanism or one treatment parameter changes). As this second treatment data is likely confounded by order effects, we use this data as a robustness check, and focus on the specific within-session tests dictated by the treatment pairs.

Table 2 further presents the group-specific Nash equilibrium predictions of disclosed output, q.[9] Note that particular standards were chosen such that the corresponding dynamic tournament and dynamic standards treatments have approximately equal predictions.[10] This is deliberate, in order to place the mechanisms on theoretically equal footing. Note that, to avoid odd-looking standards, the actual standards in the design differ very slightly from those that make the two mechanisms theoretically equivalent in equilibrium. When testing for equal mean disclosure between the two dynamic mechanisms, we account for the expected differences in disclosure due to the slight differences in theoretical predictions.

Finally, given the large range of predicted outcomes, it was desirable to vary endowments

[9] Theoretical predictions were generated using Matlab code, which is available from the authors upon request.
[10] This was achieved by solving the tournament equilibrium based on the first order conditions displayed in equations (10) – (11). Then, using the standards mechanism first-order conditions we plugged in the tournament equilibrium disclosure levels and then solved for s_1 and s_2.

and experimental-to-U.S.-dollar exchange rates across treatments. These parameters were chosen to equate the group-specific payoffs, under equilibrium play, across treatments as well as to insure meaningful differences in expected payoffs across players in non-targeted and targeted groups (approximately $0.55 per period or $22 for a 40-period session).

3.1 Testable hypotheses

The chosen parameters generate a wide range of predictions, with meaningful differences between key treatment pairs, and predicted under-compliance, approximate compliance, and over-compliance. The main testable hypotheses are summarized below:

Hypothesis 1. Dynamic audits: increasing the fixed audit cost increases disclosure; Random audit: no audit cost effect.

Hypothesis 2. Increasing the audit probabilities leads to higher disclosure.

Hypothesis 3. Dynamic audits: increasing the transition probability decreases disclosure.

Hypothesis 4. Disclosure is higher in the targeted group, G_2.

Hypothesis 5. Dynamic audits lead to higher disclosure than random audits.

Hypothesis 6. The tournament and standards mechanisms lead to identical disclosures.

The first five hypotheses follow from the theory, whereas Hypothesis 6 follows from our specific parameterizations. The design allows all hypotheses except for Hypothesis 4 to be tested based on between-subjects comparisons. The group effect is based on differences in disclosure between the targeted and non-targeted groups, and given that many players switch groups within the dynamic game, identification of this group effect is predicated on within-subjects comparisons unless one focuses solely on the first period of the dynamic game. As a robustness check, the transition effect, leverage effect, and mechanism equivalence hypotheses are testable by comparing behavior across the two treatments encountered within session.

3.2 Participant pool and procedures

Three-hundred and twenty undergraduate students enrolled at The University of Tennessee, Knoxville, participated in the study. Sessions were conducted during the fall of 2010, in the UT Experimental Economics Laboratory. There are 16 sessions, and 20 unique players participated in each. This allowed us to conduct two replications of two treatments (and up to eight dynamic games) within each session, which afforded additional anonymity, as well as variation in game length for each treatment.[11] Registration and scheduling was accomplished using the Online Recruiting System for Experimental Economics (ORSEE) developed by Greiner (2004). The participants were drawn from a large pool of students, similar to the overall undergraduate student body in terms of age, gender, and the distribution of academic majors. Participant earnings were denominated in experimental dollars, and exchanged for U.S. dollars at the end of the session at a common and known exchange rate. The experiment lasted approximately 1 hour and 45 minutes with average earnings of approximately $35.

The experiment was implemented using software programmed with z-Tree (Fischbacher, 2007). Written instructions were provided to each participant, and were read aloud by the same author. To help facilitate learning, participants were asked to work through a series of calculations questions (using pencil and paper) and were paid for providing correct answers. The questions involved making a hypothetical disclosure choice and then determining earnings under three possible audit outcomes. Further, participants had to determine whether they would be transitioned to the other group based on their disclosure choice and audit outcome. Experiment

[11] Overall, there are four replications (four unique groups of 10 players) for most treatments, with the exception of treatments T5, R1, R4 and S5 (2 replications), and treatments T3, R2, R3 and S3 (6 replications).

moderators privately checked the calculations and re-explained procedures in the case of wrong answers.[12] Prior to each of the two treatments, there were two corresponding practice periods. At the conclusion of the experiment, a short questionnaire was administered to assess how well instructions were understood and to elicit basic information on demographics.[13]

4. Results

4.1 Within-mechanism comparisons

Figures 1 – 3 present mean disclosed output levels corresponding with "first treatment" data for the dynamic tournament, dynamic standards and random audit mechanisms, respectively. In particular, shown are the group-specific means across both games for each treatment. Simple visual examination of the data reveals the main treatment effects. Overall, with few exceptions, disclosed output is notably higher for the targeted group in all treatments for all mechanisms. Turning to the dynamic tournament (Figure 1), mean disclosed output is above the Nash equilibrium for the non-targeted group, but the level of over-reporting tends to be the same across treatments. For the targeted group, disclosed output is reasonably close to Nash in all treatments.[14] As disclosed output parallels theoretical predictions, this suggests behavior consistent with the theoretical comparative statics.

For the dynamic standards (Figure 2), patterns in the data are less prominent. There is very little difference in reported output across treatments S3 – S6, although there are prominent differences in the theoretical predictions. This suggests invariance to changes in the standards as

[12] Over 95 percent of participants answered the transition questions and performed the calculations correctly.
[13] Representative instructions, including calculation questions, are included in the online supplement.
[14] When averaged across the targeted and non-targeted groups, there is statistically significant over-reporting the dynamic regulation tournament, for each of our six treatments. This parallels the findings from multi-stage elimination tournaments, where observed effort is higher than predicted by theory, and evidence suggests this is driven by players having a positive utility from the act of winning (Dechenaux, Kovenock and Sheremeta, 2012). There may be related behavioral drivers in our experiment.

well as audit cost. The systematically lower disclosed output for treatments S1 and S2 are suggestive of an audit probability effect.

Finally, for the random audit (Figure 3) there is over-reporting for both groups in all treatments. This is a finding common in regulatory compliance experiments with random audit mechanisms (e.g. Alm, McClelland and Schulze, 1992), and may be driven by risk aversion or distaste for evasion. There is a pronounced audit probability effect given the higher levels of disclosed output in R3 and R4 as compared to R1 and R2. The similarity between R1 and R2, and between R3 and R4 suggests there is little to no effect of audit cost.

Table 3 presents linear regression estimates of treatment main effects. In particular, since each of the treatment variables – audit cost, audit probability, and for dynamic mechanisms the transition probability – have two possible levels in the design, this is controlled for with three indicator variables. Differences across the targeted and non-targeted groups are controlled for with an additional indicator variable. The model is estimated using pooled "first treatment" panel data from all mechanisms and all paid decision periods, and all treatment main-effects (as well as the overall mean) are allowed to vary across mechanisms. We compute cluster-robust standard errors for the regression coefficients, and likewise compute heteroskedasticity-autocorrelation robust t and F statistics. The standard errors are clustered by cohort (i.e. group of 10 matched players). This allows for within-player serial correlation, as well as contemporaneous and serial correlation across players matched within a session.[15, 16]

[15] Analysis is carried out using Stata version 13, which incorporates a degree of freedom correction when computing cluster-robust standard errors and the uses limiting distributions based on the number of clusters. As shown in studies such as Hansen (2007), these adjustments can lead to accurate inferences even when there is a modest number of clusters and time periods.

[16] Note that this covariance estimator is also a consistent estimator in the presence of individual and cohort-level random effects. It is not possible to include subject fixed effects given issues of perfect collinearity. However, since subjects are randomly assigned to treatments, subject-specific unobservables should be uncorrelated with treatment-specific control variables.

We first focus on testing comparative statics predictions, associated with Hypotheses 1 through 4. The regression confirms the theoretical comparative statics for the dynamic tournament: there are statistically significant and correctly-signed audit cost, audit probability, transition probability and group effects. As a stronger test, we test for equivalence in the magnitudes of estimated and theoretical predictions of treatment main effects.[17] These tests reveal a statistical difference for the group effect, which is about half as large as predicted. The small effect could stem from participants receiving feedback about both groups in their experiment.

For the dynamic standards, many results are inconsistent with theory. Most notable, there is no statistical evidence of either an audit cost or transition probability effect (i.e., an effect of changing the group-specific standards, holding audit cost and audit probabilities fixed). The comparative statics for random audits are consistent with theory, although the magnitudes are off for all but the audit cost effect. Similar to the dynamic tournament, for the random audit and dynamic standards mechanisms, the group effects are smaller than predicted. The main within-mechanism results are summarized below.

Result 1. All comparative statics are confirmed for the dynamic tournament and random audit. For the dynamic tournament, the audit cost, audit probability and transition probability effects are consistent with theory in terms of their magnitude.

Result 2. In contrast to theory, for the dynamic standards mechanism, disclosed output is invariant to either a *ceteris paribus* change in the audit cost or a *ceteris paribus* change in the transition probability.

4.2 Between-mechanism comparisons

[17] In order to generate theoretical predictions of treatment effects, we estimate parallel regressions that instead use the Nash equilibrium prediction (rather than the participants' disclosure choice) as the dependent variable. Thus, this procedure generates what the regression coefficients would be if participants behaved according to theory.

To evaluate Hypotheses 5 and 6, Table 4 presents additional test results related to mechanism equivalence based on the estimated regression model. We first test for joint equality of treatment effects across mechanisms. These tests reveal systematic differences in treatment effects between the dynamic tournament and other mechanisms, but equality between the dynamic standards and random audit mechanisms. Indeed, for the dynamic standards and random audit mechanisms, the audit probability effect (4.08 versus 4.26) and the group effect (5.34 versus 5.04) only differ by approximately 5%. The audit cost effects are statistically equal to zero for both mechanisms and there is no transition effect for the dynamic standards mechanism. In a second set of tests, we test for equality in mean disclosed output across mechanisms. For these tests, we evaluate the mechanism-specific models at the covariate means. We find strong statistical evidence of a leverage effect as both dynamic mechanisms achieve higher disclosed output than random audits (which is evident when examining Figures 1 – 3). Relative to theoretical predictions, we find the average leverage effect to be about 20% smaller than theory predicts. Disclosed output is not statistically different between the two dynamic mechanisms.

Result 3. Both dynamic targeting mechanisms achieve significant leverage.

Result 4. The dynamic tournament and dynamic standards mechanisms lead to equivalent mean disclosed output, but not equivalent treatment-effects.

4.3 Additional results

Within-session comparisons across treatments. Table B1 in the online supplement presents tests of equal disclosed output across the two treatments encountered within session.[18] These tests are based on a regression model that allows disclosed output to fully vary across groups, treatments, mechanisms, and order of treatment. In other words, four means are

[18] Due to time considerations, not all periods for the second treatment were completed in all sessions. Data from incomplete games are not included in the analysis.

estimated from a session given there are two groups and two treatments. The tests of equivalence between the dynamic mechanisms are mixed, whereas the leverage effect is significant and in the expected direction in all possible comparisons. The most enlightening of these are tests of the transition effect. For the dynamic tournament, we find that (group-specific) disclosed output increases statistically when the transition probability decreases (T4 to T6 in Session 8) and decreases when the transition probability increases (T6 to T4 in Session 10). In contrast, we find that decreasing the transition probability (S4 to S6 in Session 14) has a null effect for the dynamic standards mechanism, and that increasing the transition probability (S6 to S4 in Session 16) actually *increases* disclosed output for G_1. This result further illustrates the theory's inability to predict behavior under the dynamic standards mechanism.

Analysis of variances. As a final line of analysis we estimate the treatment main-effects model but using instead a measure of within-group variation as the dependent variable. In particular, we use the squared deviation between the participant's disclosed output in a particular period and mean disclosed output for her group in that period. This model is presented in Table B2 in the Reviewer Appendix. For both dynamic mechanisms we find that increasing the audit probability decreases the variance and that those in the targeted group have a higher variance. Testing for equality of variance across mechanisms, we find that the dynamic standards mechanism has a statistically different, and higher, variance than both the dynamic tournament (t = 2.16; p = 0.03) and the random audit (t = 2.33; p = 0.02). The dynamic tournament and random audit and have equal variance (t = 0.50; p = 0.62). The finding of higher variance in the dynamic standards mechanism provides further evidence that the incentives of this mechanism were less transparent, leading to noisy and less predictable outcomes.

5. Discussion

In this paper we have demonstrated theoretically how competition can motivate disclosure of a regulated activity in an indefinite game among firms in an industry where targeting based on past compliance behavior creates competition to avoid this extra scrutiny. Such competition naturally arises in a targeting framework where the total number of audits that can be conducted at a given time is fixed. In modeling the regulatory competition among firms, relative to existing targeting models, we relax the assumption of a representative firm, and explicitly model a regulated industry (or some other established group of firms) where firms are potentially heterogeneous in their levels of the regulated activity (e.g., pollution). Similar to existing targeting models, our dynamic tournament achieves significant enforcement leverage relative to random audits. Decreasing the number of firms transitioned to the targeted (non-targeted) group after each inspection period increases this leverage.

Turning to the experimental results on the dynamic regulation tournament, the basic implication of the theory – that targeting leads to significant enforcement leverage – is strongly confirmed by experiment data. The dynamic tournament exhibits strong audit cost, audit probability, and transition effects. In fact, the magnitudes of these treatment effects are similar to theoretical predictions with the exception that the leverage effect is empirically smaller than predicted. In contrast, the effects of changing enforcement parameters are largely subdued for the dynamic standards mechanism. Specifically, there are no statistically discernible effects of audit cost or changes in the equilibrium transition probabilities. These findings echo those from related experiments on dynamic targeting mechanisms, which also suggest that changes in parameters lead to weak or null effects. We find a higher variation in disclosed output with the dynamic standards mechanism, which suggests a lower degree of transparency in incentives.

Although it remains an open question as to why the dynamic tournament appears to have more transparent incentives, we can offer speculation. The dynamic standards mechanism requires players to solve a complicated individual optimization problem, in an environment where it is difficult to learn how to optimize by trial and error. As such, this is likely to lead players to inadequately consider the dynamic implications of their choices, and adopt coarse heuristics that are only loosely tied to regulatory intensity. In contrast, the dynamic tournament forces players to respond to others, and thus the choices of others has value. Competition therefore is more likely to serve as a coordination device, similar to the "invisible hand" in a market setting, and motivate players to develop and adopt more refined strategies.

The audit probabilities we explored experimentally are much higher than what is typical in field regulatory settings, and this design choice in combination with our assumption of unbiased audit errors led in some cases to theoretical and empirical over-compliance. From our choice of parameters we do not intend to imply that over-compliance is desirable. Given the high audit probabilities, as well as other features of our design which may not reflect field conditions, there is of course a need for caution in extrapolating our lab results. However, the lack of treatment effects for the dynamic standards mechanism in this setting suggests that changes in policy parameters are unlikely to have the desired effect in a field setting characterized by more modest regulatory intensity. At a minimum, even without possible field confounds, the incentives induced by this mechanism appear poorly understood.

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Table 1. Selected experiment parameters

Treatment	Audit cost (γ)	Audit probability for G_1 (ρ_1)	Audit probability for G_2 (ρ_2)	Transitions per group	Standards
T1	25	0.4	0.6	1	N/A
T2	50	0.4	0.6	1	N/A
T3	25	0.6	0.8	2	N/A
T4	50	0.6	0.8	2	N/A
T5	25	0.6	0.8	1	N/A
T6	50	0.6	0.8	1	N/A
S1	25	0.4	0.6	N/A	$s_1 = 10$, $s_2 = -15$
S2	50	0.4	0.6	N/A	$s_1 = 10$, $s_2 = -15$
S3	25	0.6	0.8	N/A	$s_1 = 0$, $s_2 = 0$
S4	50	0.6	0.8	N/A	$s_1 = 0$, $s_2 = 0$
S5	25	0.6	0.8	N/A	$s_1 = 10$, $s_2 = -15$
S6	50	0.6	0.8	N/A	$s_1 = 10$, $s_2 = -15$
R1	25	0.4	0.6	N/A	N/A
R2	50	0.4	0.6	N/A	N/A
R3	25	0.6	0.8	N/A	N/A
R4	50	0.6	0.8	N/A	N/A

Notes: R≡random audit; T≡dynamic tournament; S≡dynamic standards.

Table 2. Session summary and Theoretical Predictions

Session	First treatment	Nash equilibrium (First treatment)		Second treatment
		Group 1 disclosed output (q_1^*)	Group 2 disclosed output (q_2^*)	
1	T1	4.0	20.7	S1
2	T2	9.9	26.5	R2
3	T3	11.8	20.1	S3
4	T4	14.6	22.9	T6
5	T5	16.1	24.4	R3
6	T6	21.6	30.0	T4
7	S1	5.6	22.4	T1
8	S2	11.0	27.6	R2
9	S3	11.6	20.0	T3
10	S4	14.3	22.6	S6
11	S5	16.2	24.4	R3
12	S6	20.8	29.2	S4
13	R1	0.0	6.7	S2
14	R2	0.0	6.7	T2
15	R3	6.7	15	S3
16	R4	6.7	15	T3

Notes: R≡random audit; T≡dynamic tournament; S≡dynamic standards.

Table 3. Treatment main-effects model

Dependent variable: disclosed output			
	Coefficient estimates (cluster-robust standard errors)		
Variable	Tournament	Standards	Random Audit
=1 for High Audit Cost	4.00* (0.79)	0.29† (1.13)	-0.51 (0.83)
=1 for High Audit Probability	6.96* (1.09)	4.26* (1.63)	4.08*† (0.83)
=1 for High Transition Probability	-4.73* (0.78)	0.77† (1.36)	–
=1 for Targeted Group	6.28*† (0.99)	5.04*† (0.92)	5.34*† (1.07)
Intercept	13.89*† (1.38)	17.58*† (1.32)	9.27*† (1.34)
R^2	0.8543		
n	6400 [20 periods x 320 participants]		

Notes: * indicates coefficient is statistically different than zero at the 5% significance level. † indicates coefficient is statistically different than theoretical prediction at the 5% significance level. The standard errors are clustered by cohort (i.e., independent groups of 10 matched players).

Table 4. Between-mechanism comparisons

	Hypothesis	
	Equal treatment main-effects	Equal mean disclosure
Tournament = Standards	$F = 6.62$; $p < 0.01$	$t = -0.77$; $p = 0.45$
Tournament = Random Audit	$F = 12.21$; $p < 0.01$	$t = 14.20$; $p < 0.01$
Standards = Random Audit	$F = 0.50$; $p = 0.74$	$t = 12.78$; $p < 0.01$

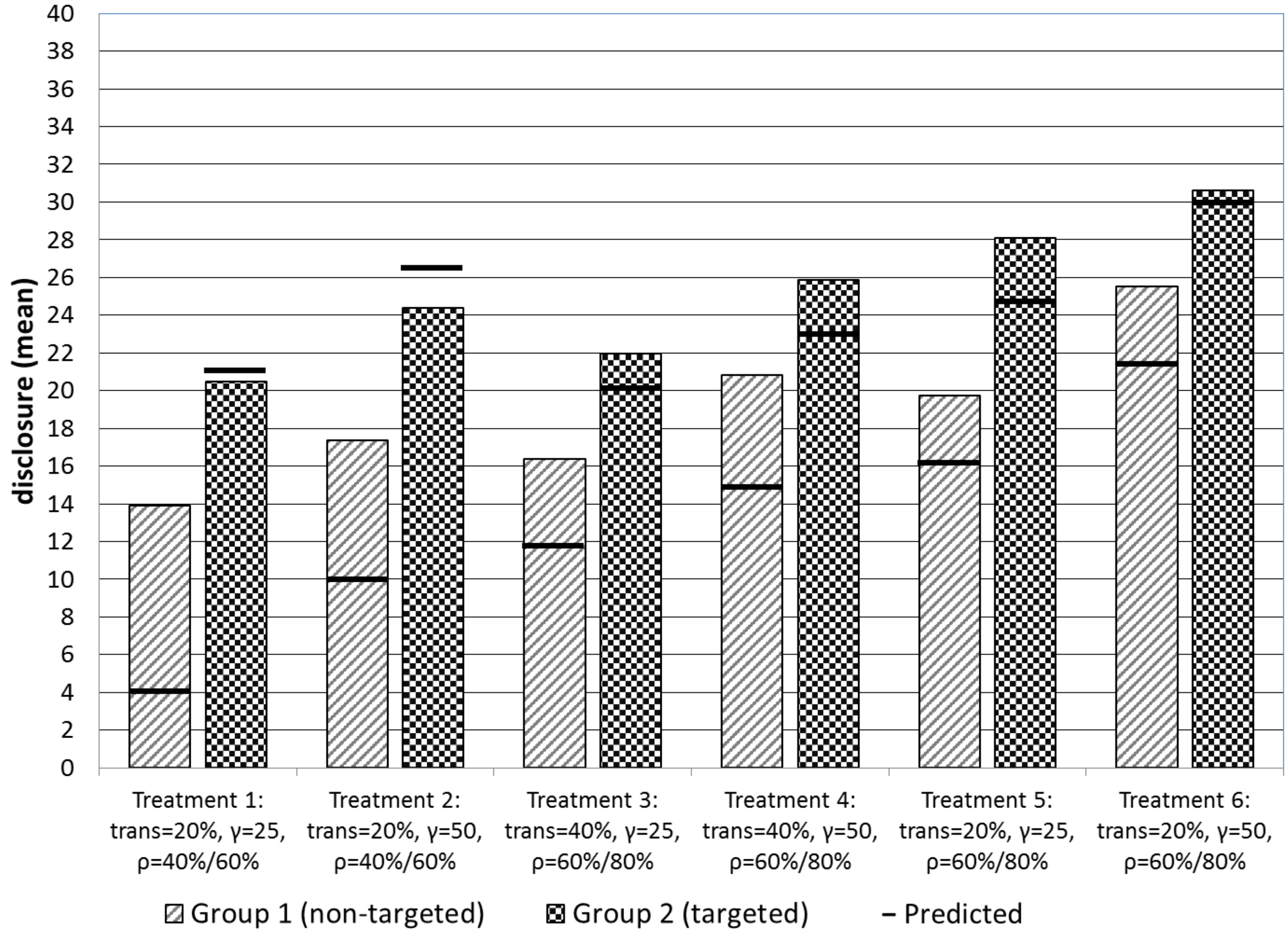

Figure 1. Mean disclosed output by group for dynamic tournament mechanism

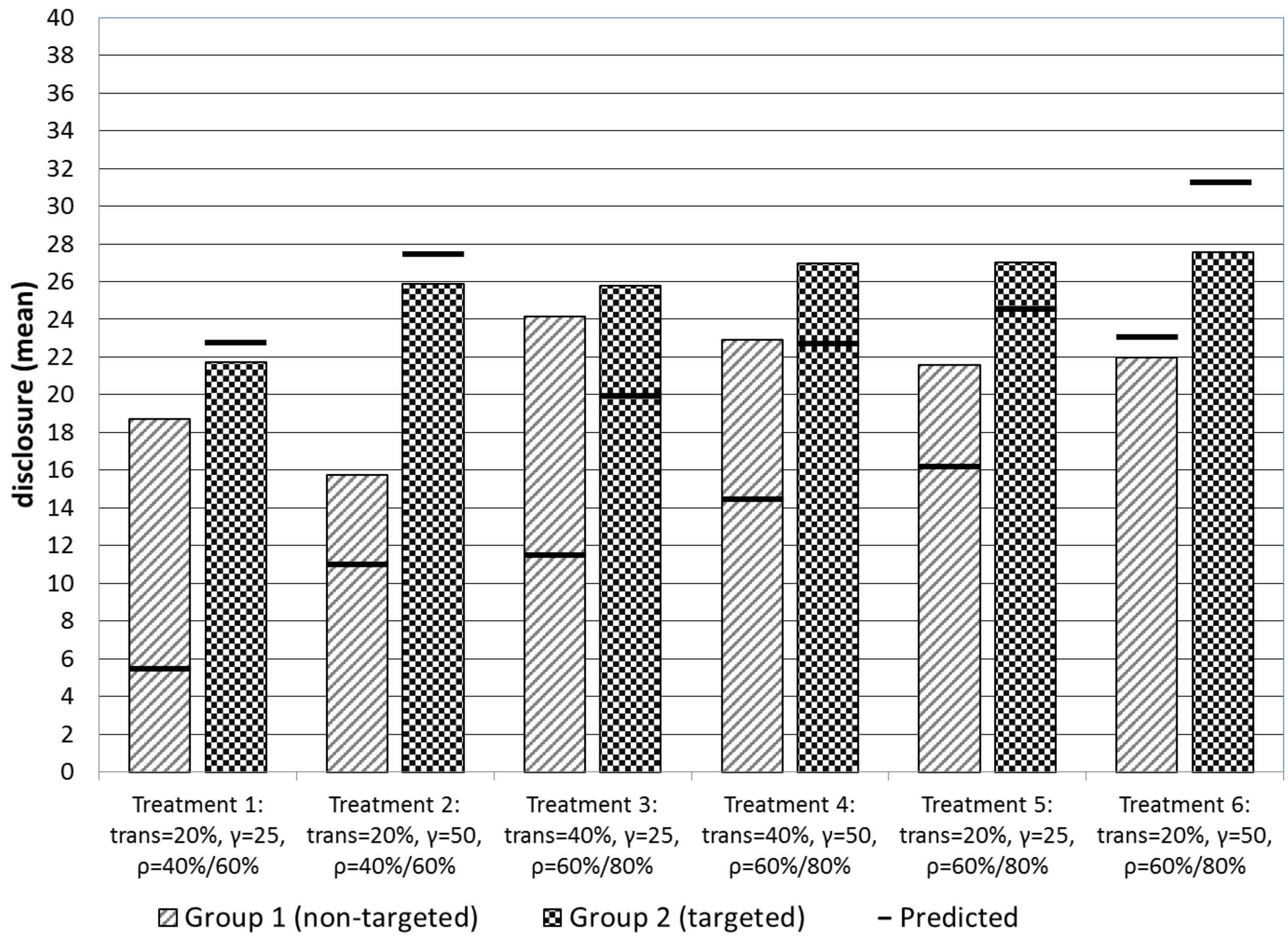

Figure 2. Mean disclosed output by group for dynamic standards mechanism

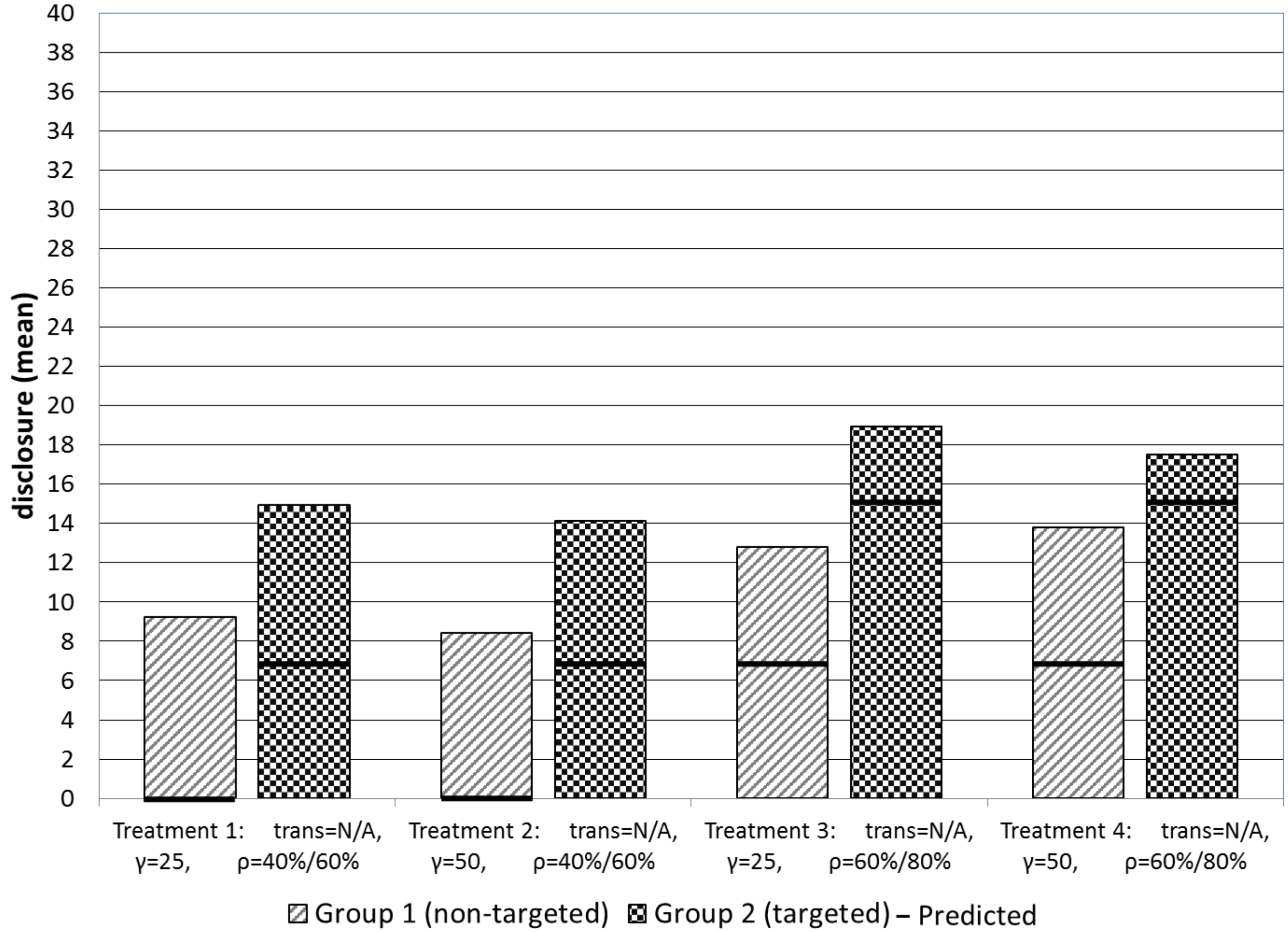

Figure 3. Mean disclosed output by group for random audit mechanism

Online Supplement

Title: *Using competition to stimulate regulatory compliance: a tournament-based dynamic targeting mechanism*

Authors: Scott M. Gilpatric, Christian Vossler and Lirong Liu

Date: June 2015

Appendix A. Theory Supplement

Proof of Proposition 1: Under a simple random audit mechanism the optimal level of disclosure for a firm in group *l* minimizes expected costs in the current period, which is identified by the condition that $\frac{\partial \pi_l}{\partial z_l} = 0$. Equations (10) and (11) establish that at the equilibrium level of disclosure in the dynamic tournament $\frac{\partial \pi_l}{\partial z_l} > 0$. That is, firms choose *z* such that current costs are increasing, implying that the equilibrium level is above that which minimizes costs in the present period.

Proof of Proposition 2:

From equations (10) and (11) the equilibrium report of firms in both groups is increasing with $(V_2 - V_1)$. As stated, $(V_2 - V_1) = \frac{(\pi_2 - \pi_1)}{1-\delta\left(1-\rho_2\left(\frac{\tau}{m_2}\right)-\rho_1\left(\frac{\tau}{m_1}\right)\right)}$. Consider first the effect of an increase in τ. The denominator of the expression is increasing in τ. As discussed in the text, the numerator, $(\pi_2 - \pi_1)$, is positive. The numerator does not contain τ, but π_2 and π_1 vary with the endogenous choices z_2 and z_1, respectively. Holding z_2 and z_1 constant, an increase in τ decreases $(V_2 - V_1)$. This decrease in the tournament prize has an ambiguous effect on $(\pi_2 - \pi_1)$ through the choices of z_2 and z_1, and thus may increase or decrease the numerator of the expression. To see that it must nevertheless be the case that an increase in τ decreases $(V_2 - V_1)$, consider the two possible cases. Case 1: Choices z_2 and z_1 respond to a decrease in $(V_2 - V_1)$ such that $(\pi_2 - \pi_1)$ decreases. In this case the numerator of the expression decreases while the denominator increases, and $(V_2 - V_1)$ thus decreases with τ. Case 2: Choices z_2 and z_1 respond to a decrease in $(V_2 - V_1)$ such that $(\pi_2 - \pi_1)$ increases. In this case both the numerator and denominator of the expression are increasing with τ. However, we can prove by contradiction that the total effect of τ on $(V_2 - V_1)$ cannot be positive. Suppose it were positive. Since by supposition of Case 2 an increase in $(V_2 - V_1)$ decreases $(\pi_2 - \pi_1)$, then the effects of an increase in τ on both the numerator and denominator of the expression would be in the direction of decreasing $(V_2 - V_1)$, which is contradiction. Therefore, an increase in τ must decrease $(V_2 - V_1)$.

Regarding the effect of γ, note that z_2 is chosen to minimize V_2 and z_1 is chosen to minimize V_1. Therefore, by the envelope theorem, the effect of γ on $(V_2 - V_1)$ depends only on the direct effect. This spread is increasing with γ, i.e., $\frac{\partial(V_2 - V_1)}{\partial\gamma} = \frac{\rho_2 - \rho_1}{1-\delta\left(1-\rho_2\left(\frac{\tau}{m_2}\right)-\rho_1\left(\frac{\tau}{m_1}\right)\right)} > 0$.

<u>Proof of Proposition 3</u>:

The regulator constrained to employ a random audit mechanism must choose the audit probability $p = \rho_1 = \rho_2$ to solve the following problem:

(A1) $\quad \min_p L(z) + pc.$

Denote the solution to this problem p_R and the associated induced reporting by firms to be z_R. These are implicitly defined by the first order condition:

(A2) $\quad L'(z_R)\frac{\partial z_R}{\partial p}|_{p_R} + c = 0.$

Note that from the FOC of the random audit mechanism, equation (2), $F(z_R) = 1 - \frac{\alpha}{p\beta}$, and $\frac{\partial z_R}{\partial p} > 0$; therefore, $L'(z_R) < 0$. That this, the optimal level of enforcement yields $z_R < \bar{z}$.

Now consider the case where the regulator can choose the audit probabilities for each group, ρ_1 and ρ_2, within the tournament framework. Note that here we are treating these audit probabilities as continuous rather than discrete, abstracting from the fact that the number of audits must be an integer (as *N* grows the audit probability choice approaches a continuum). We will show that this mechanism can always yield a better outcome for the regulator than the random audit mechanism.

The first-order necessary conditions for the regulator's problem are

(A3) $\frac{n_1}{N}\left\{L'(z_1)\frac{\partial z_1}{\partial \rho_1}+c\right\}+\frac{n_2}{N}\left\{L'(z_2)\frac{\partial z_2}{\partial \rho_1}\right\}=0$, and

(A4) $\frac{n_1}{N}\left\{L'(z_1)\frac{\partial z_1}{\partial \rho_2}\right\}+\frac{n_2}{N}\left\{L'(z_2)\frac{\partial z_2}{\partial \rho_2}+c\right\}=0.$

As noted in the text, by setting $\rho_1=\rho_2=p_R$ the tournament collapses to a random audit mechanism (because there is no difference between the groups), and the same outcome is obtainable. However, the auditor can obtain a better outcome by setting the audit probability for the non-targeted group such that $\rho_1<p_R$. Note that only a left-side derivative $\frac{\partial z_1}{\partial \rho_1}$ exists at $\rho_1=\rho_2=p_R$ because z_1 is not defined for $\rho_1>\rho_2$ (by construction, G_1 is the non-targeted group and the audit probability in this group cannot exceed that in the targeted group, G_2). At a point where $\rho_1=\rho_2=p_R$ the marginal effect of ρ_1 on reporting by firms in G_1 must be less than the effect of p on reporting by firms in the random audit mechanism: $\frac{\partial z_1}{\partial \rho_1}|_{\rho_1=\rho_2=p_R}<\frac{\partial z_R}{\partial p}|_{p_R}$. This holds because the direct effect of an increase in the audit probability on reporting in $\frac{\partial z_1}{\partial \rho_1}$ is identical to $\frac{\partial z_R}{\partial p}|_{p_R}$, but this is offset by the reduction in the competitive leverage effect that occurs as ρ_1 increases, causing (V_2-V_1) to decline. More formally, z_1 in equilibrium satisfies the FOC from equation (10): $\frac{\partial \pi_i}{\partial z_i}=-\delta(V_2-V_1)\rho_1\frac{\partial Q_i}{\partial z_i}|_{z_i=z_{-i}}$. For $\rho_1=\rho_2=p_R$ the right side of this expression is zero, so (10) reduces to the FOC for optimization in the random audit mechanism, which is equivalent to $\frac{\partial \pi_i}{\partial z_i} = 0$, and therefore $z_1 = z_R$. A decrease in ρ_1 from this point will decrease the value of z that solves $\frac{\partial \pi_i}{\partial z_i} = 0$ exactly as in the random audit mechanism. However, a decrease in ρ_1 decreases $V_1$and increases $(V_2 - V_1)$, so that the z_1 that solves $\frac{\partial \pi_i}{\partial z_i} = -\delta(V_2 - V_1)\rho_1 \frac{\partial Q_i}{\partial z_i}|_{z_i = z_{-i}}$ must be larger than that which solves $\frac{\partial \pi_i}{\partial z_i} = 0$, thus reducing the effect of a decrease in ρ_1 compared to the impact in the random audit mechanism . Therefore, $\frac{\partial z_1}{\partial \rho_1}|_{\rho_1 = \rho_2 = p_R} < \frac{\partial z_R}{\partial p}|_{p_R}$.

By similar logic, the reduction in leverage also implies, $\frac{\partial z_2}{\partial \rho_1}|_{\rho_1 = \rho_2 = p_R}$<0.

Under the conditions just described it follows that $\left\{L'(z_1)\frac{\partial z_1}{\partial \rho_1} + k\right\} > 0$ and $\left\{L'(z_2)\frac{\partial z_2}{\partial \rho_1}\right\}$>0. Therefore at such a point the left-hand-side of (A3) is strictly positive, implying that the regulator's costs are rising with ρ_1 as it approaches $\rho_1 = \rho_2 = p_R$. Therefore setting $\rho_1 = \rho_2 = p_R$ cannot be optimal, and there must exist a combination of audit probabilities in the tournament mechanism that yields lower overall costs.

Appendix B. Additional Econometric Analysis

Table B1. Within-subjects tests (comparison of first and second treatment data)

Hypothesis	First treatment in session	**Difference in disclosure (std. err.)** Pooled	G_1	G_2
Transition effect				
T6 = T4	T4	4.69** (0.23)	3.47** (0.50)	5.91** (0.47)
T6 = T4	T6	2.60** (1.01)	2.32 (2.12)	2.88** (0.25)
S6 = S4	S4	0.19 (0.15)	-1.05** (0.24)	1.90* (1.12)
S6 = S4	S6	-0.23 (1.73)	-3.65 (2.71)	3.11* (1.88)
Leverage effect				
T2 = R2	R2	6.12** (0.29)	5.97** (1.40)	6.26** (1.92)
T3 = R4	R4	7.18** (0.09)	7.21** (1.01)	7.16** (2.08)
T2 = R2	T2	8.72** (0.50)	9.22** (2.65)	8.22** (1.99)
T5 = R3	T5	7.88** (1.66)	8.42** (1.86)	7.33** (2.34)
S1 = R1	R1	8.68** (0.74)	9.06** (1.86)	8.65** (0.86))
S3 = R3	R3	4.29** (0.14)	4.93** (0.56)	4.61** (1.04)
S2 = R2	S2	7.14** (0.25)	5.43** (0.78)	9.80** (0.56)
S5 = R3	S5	2.36** (0.10)	1.61** (0.75)	4.44** (1.28)
Mechanism equivalence				
T1 = S1	T1	0.31 (1.53)	-1.22 (3.76)	2.36** (0.19)
T1 = S1	S1	-1.50 (2.35)	-1.57 (2.20)	-0.56 (2.38)
T3 = S3	T3	-3.15** (0.48)	-3.90** (0.41)	-3.48** (0.63)
T3 = S3	S3	3.01** (0.13)	1.65** (0.21)	3.88** (0.03)

Notes: *, ** denote difference is statistically significant at the 10% and 5% significance levels, respectively. The tests for mechanism equivalence take into account the slight differences in theoretical predictions across the two targeting mechanisms.

Table B2. Treatment main-effects model: Variance

Dependent variable: squared deviation from mean disclosed output			
	Coefficient estimates (cluster-robust standard errors)		
Variable	Tournament	Standards	Random audit
=1 for High Audit Cost	3.68 (6.94)	-1.83 (9.54)	11.03 (8.45)
=1 for High Audit Probability	-49.30* (9.40)	-25.75* (12.31)	-0.87 (8.45)
=1 for High Transition Probability	5.09 (5.99)	-7.03 (10.01)	–
=1 for Targeted Group	24.92* (5.77)	21.21* (8.38)	-11.04 (7.86)
Intercept	73.06* (7.77)	79.78* (10.98)	53.80* (8.07)
R^2	0.3340		
n	6400 [20 periods x 320 participants]		

Notes: * indicates coefficient is statistically different than zero at the 5% significance level. The standard errors are clustered by cohort (i.e., independent groups of 10 matched players).

Appendix C. Experiment instructions for Session 9 (treatment S3 followed by T3)

INTRODUCTION

This experiment is a study of group and individual decision making. The amount of money you earn depends on the decisions that you make and thus you should read the instructions carefully. The money you earn will be paid privately to you, in cash, at the end of the experiment. A research foundation has provided the funds for this study.

You will make decisions privately, that is, without consulting others. Please do not attempt to communicate with other participants in the room during the experiment. If you have a question as we read through the instructions or at any time during the experiment, please raise your hand and an experiment moderator will answer it.

The experiment is broken up into many decision “periods”. With the exception of your decisions in practice periods, you will be paid based on your decision in each and every period. In other words, each decision you make is important in determining the amount of money you earn.

There will be two parts to the experiment. The instructions below are for the first part. After this part is finished there will be additional instructions.

Your earnings in the experiment are denominated in experimental dollars, which will be exchanged at a rate of 20 to $1 U.S. at the end of the experiment.

Overview

For this experiment you are randomly matched with nine other players in this room (i.e. there are ten players in your experiment). At the beginning of the experiment you will be placed into either Group A or Group B. Each group will have five players in it.

There are four parts to each decision period:

- You make a decision of how much “output” to report. This is your only decision.
- Players in your group are randomly selected to have their reports inspected. Those inspected face additional costs. Based on this inspection, one or more players may be moved into the other group.
- The computer calculates your earnings.
- The computer determines whether the current game will continue for an additional period, or whether we will start a new game.

Your reporting decision

Your actual output in each decision period is 20 units. Your sole decision is to choose how much output to report. Your **reported output** can be any amount between, and including, 0 and 40.

For *each* unit of reported output, you pay a cost of $1. We refer to the total amount as your reporting cost.

<u>Inspections</u>

In each period, players in **Group A** have a **60%** chance of being inspected.
In each period, players in **Group B** have an **80%** chance of being inspected.

Whether or not you are inspected is determined randomly according to these chances. Your chance of being inspected is <u>not</u> affected by your report or the reports of other players.

***If* you are inspected: Inspection Cost**

If you are inspected you pay an **inspection cost** of **$25**. This cost does <u>not</u> depend on your reported output.

***If* you are inspected: Penalty**

If you are inspected you *may* pay a **penalty**, which <u>does</u> depend on your reported output.

The computer makes an estimate of your output. In particular, **estimated output** is a randomly determined amount between 0 and 40. Any number between 0 and 40 has an equal chance of being selected. On average, estimated output is equal to your actual output of 20 units. The computer separately determines the estimated output of each inspected player so these estimates can differ.

If the estimated output is *greater than* your reported output, you pay **$2** for *each* unit of output you are estimated to have *under*-reported. Otherwise, you do not pay a penalty. So, for example, if you report 20 units and the Inspector estimates your output to be 25 units, you would pay $2 multiplied by 5 units or $10. Alternatively, if you report 20 units and the Inspector estimates your output to be 15 units, you would not pay any penalty.

***If* you are inspected: Group assignment**

If you are in Group A, you will be moved to Group B if your reported output is *less than* your estimated output. In other words, the more you report, the less likely it is that you will be moved to the other group. Notice that those in Group B face a higher chance of inspection.

If you are in Group B, you will be moved to Group A if your reported output is *more than* your estimated output. In other words, the more you report, the more likely it is that you will be moved to the other group. Notice that those in Group A face a lower chance of inspection.

Based on these rules for how players are moved between groups, notice that the reporting decisions of others have <u>no</u> effect on whether or not you change groups.

Note: if you are <u>not</u> inspected, then you do not face additional costs, nor will you be assigned to a different group based on your report.

Your earnings

In each period are given an initial earnings of **$60**, and your overall earnings for the decision period depend upon how much you report (reporting cost) and – *if* you are inspected – an inspection cost and possibly a penalty.

Thus, after you have submitted your report, three things can happen: (1) You are not inspected; (2) You are inspected and your estimated output is *less than* your reported output; or (3) You are inspected and your estimated output is *greater than* your reported output. We summarize below how your earnings will be calculated under each scenario.

Your earnings (You are not inspected)
Since you are not inspected, there is no inspection cost and no penalty is possible.
Your earnings for the period are your initial earnings minus your reporting cost.
In particular:

	$60	(Initial earnings)
–	Reported output x $**1**	(Reporting cost)
–	**$0**	(Inspection cost)
–	**$0**	(Penalty)
=	Period Earnings	

Your earnings (You are inspected and your estimated output is *less than* your reported output)
Since your estimated output is *less* than your reported output you do not pay a penalty.
Your earnings for the period are your initial earnings minus your reporting cost and inspection cost.
In particular:

	$60	(Initial earnings)
–	Reported output x $**1**	(Reporting cost)
–	**$25**	(Inspection cost)
–	**$0**	(Penalty)
=	Period Earnings	

Your earnings (You are inspected and your estimated output is *greater than* your reported output)
Since your estimated output is greater than your reported output you pay a penalty of **$2** for each unit you are estimated to have *under*-reported. Your earnings for the period are your initial earnings minus your reporting cost, inspection cost and penalty. In particular:

	$60	(Initial earnings)
–	Reported output x $**1**	(Reporting cost)
–	**$25**	(Inspection cost)
–	[Estimated output – reported output] x **$2**	(Penalty)
=	Period Earnings	

Continuing the game

The length of the game is uncertain. In particular, at the end of the first (paid) decision period, the computer will determine whether the game will continue at least one additional period. There is a 90% chance that the game will continue at least one additional period. This chance of continuing does not change during the experiment; i.e., regardless of whether the game has already lasted two or twenty periods, there is still a 90% it will last at least one additional period.

We will play the game twice. So when the first game has ended, we will then start a new game. The second game will follow the same rules. Importantly, just like prior to the first game, everyone will be randomly placed into one of the two groups at the start of the second game.

Results

After everyone in the session has made their decisions, you will see several results screens. The first screen will display your reported output, whether you were inspected, and your earnings.

On the second screen, you will be told whether or not you will move to the other group.

On the third screen, you will see the reported output of all ten players in your experiment, whether they were inspected, and their group assignment.

On the last results screen, you will be notified whether the current game will continue an additional period.

Important note: it is possible for your earnings to be negative in a particular period. This negative amount is actually subtracted from your overall earnings, i.e. negative earnings do not simply count as $0 earnings. Although you can lose money for a particular period, in our experience with similar experiments there should be ample opportunities for you to overcome the loss through your decisions (and associated large positive earnings) in other periods.

Questions of understanding

To assess your understanding of the experiment, we would like for you to work through some examples. If all of your calculations/answers are correct we will give you $2 U.S. in addition to what you earn in the experiment. We will give you $1 if you make only one mistake.

First, as in the experiment, please choose your reported output: ______________.

Now, use your reported output above in answering all of the following questions.

Scenario A. You do not get inspected.

(a) Please calculate what your earnings would be based on your choice of reported output and write this in the space below.

(b) If you were in Group A, and the current game continued another period, would you be moved to Group B for the next period? (circle one) YES NO

Scenario B. You get inspected and your estimated output is 10.

(a) Please calculate what your earnings would be based on your choice of reported output and write this in the space below.

(b) If you were in Group A, and the current game continued another period, would you be moved to Group B for the next period? (circle one) YES NO

Scenario C. You get inspected and your estimated output is 30.

(a) Please calculate what your earnings would be based on your choice of reported output and write this in the space below.

(b) If you were in Group A, and the current game continued another period, would you be moved to Group B for the next period? (circle one) YES NO

Please raise your hand when you are finished or if you have a question.

ADDITIONAL INSTRUCTIONS

In this second part of the experiment you will be randomly matched with nine other players in this room (i.e. there are ten players in your experiment). There will be different players in your experiment than in the first part.

The second part of this experiment will be exactly like the first part, with the following important changes:

Inspections

Exactly 3 out of 5 players in **Group A** will be inspected each period (a **60%** chance).
Exactly 4 out of 5 players in **Group B** will be inspected each period (an **80%** chance).

Whether or not you are inspected is determined randomly according to these chances. Your chance of being inspected is not affected by your report or the reports of other players.

***If* you are inspected: Group assignment**

Out of the 3 players inspected from Group A, the two players who reported the least relative to their estimated output will be moved to Group B. In other words, the more you report, the less likely it is that you will be moved to the other group. Notice that those in Group B face a higher chance of inspection.

Out of the 4 players inspected from Group B, the two players who reported the most relative to their estimated output will be moved to Group A. In other words, the more you report, the more likely it is that you will be moved to the other group. Notice that those in Group A face a lower chance of inspection.

Based on these rules for how players are moved between groups, notice that the reporting decisions of others now do have an effect on whether or not you change groups.

Since exactly two players are moved from each group, the size of Group A and Group B will remain at 5 throughout the experiment.

With these exceptions, the instructions from the first part of the experiment apply.

We will play two practice periods under the new rules and then proceed to play two (paid) games.

Before we proceed, are there any questions?