An Experimental Test of the Pigovian Hypothesis

Jason J. Delaney[†]
ecojjdx@langate.gsu.edu

Department of Economics
Andrew Young School of Policy Studies
Georgia State University

PO Box 3992
Atlanta, GA 30302-3992

443-310-5511

December 2009

Abstract

Implementation of Pigovian taxation relies on the presumption that individuals follow self-interested Nash equilibrium predictions of behavior when making decisions. Experimental evidence indicates that, while Nash predictions perform quite well in impersonal exchange, in other environments, subjects behave in ways inconsistent with these equilibria. The predictive power of game-theoretic results with respect to an optimal subsidy in a common-pool resource game (CPR) remains an open question. This paper presents an experiment with training and a simplified decision task, allowing more tractable computerized CPR experiments. In this experiment, subject behavior converges to the Nash prediction, but it takes a number of periods to reach convergence. In addition, this paper provides the first experimental test of theoretical predictions of behavior under an optimal subsidy in CPR games. I find that a Pigovian subsidy effectively moves subject behavior to the pre-subsidy social optimum. Finally, this paper provides evidence of a small and non-persistent effect of information provision in moving subjects toward the social optimum.

JEL Codes: H41, H21

Key Words: laboratory experiment, Pigovian taxation, Pigovian subsidy, optimal tax, optimal subsidy, common-pool resource, CPR experiment, CPR game, learning, Nash equilibrium, information provision

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[†] This research was supported by a Georgia State University Dissertation Grant. I thank James C. Cox, Todd Swarthout, Vjollca Sadiraj, and Sarah Jacobson, as well as the participants in the Experimental Economics Workshop at Georgia State University and the IFREE Graduate Student Workshop at Chapman College for their helpful comments and guidance.

Many of the most important policy questions of our time relate not to privately consumed goods, but to the unintended consequences of consumption of goods, broadly referred to as externalities. Carbon emissions, obesity, and the stability of financial firms—they all have consequences that extend beyond those involved in making the economic decisions. A classic model to describe externalities is that of the common-pool resource (CPR), and a classic solution to the problem of externalities is a Pigovian tax or subsidy. The theoretical implications of consumption of a CPR by self-interested agents are straightforward, but the robustness of those results is less clear. This paper addresses several related issues: First, the literature has presented mixed results with respect to the performance of the self-interested Nash equilibrium in predicting subject behavior. Second, this paper presents an experimental test of the use of a Pigovian subsidy to induce socially optimal behavior. Finally, we ask: given the economic and political costs of introducing such a policy, are there other, nonmonetary ways to induce socially preferred behavior?

This paper introduces a laboratory limited-access CPR experiment designed to test the theory and examine potential policy to achieve improvements in governing common-pool resources. Our experiment offers important contributions to: the public finance literature by testing the theory of Pigovian taxation; the social preferences literature by presenting data on the comparative results of two different policy tools—price-based incentives and informational appeals—; and the field of experimental design in that it presents a simple design that makes common-pool resources more tractable for future experimental analysis.

In general, the literature has had a mixed response with respect to an important question: does self-interested Nash equilibrium predict subject behavior toward an open- or limited-access CPR? In their baseline experiment, Ostrom et al. (1994) (OGW) find that subjects appropriate from a CPR at a suboptimal level—there is congestion—but that subjects' observed choices do not achieve a stable equilibrium. Walker et al. (1990) find that the CPR is dissipated by more than the Nash prediction, while Budescu et al. (1995) find that subjects over-consume, but by less than the Nash prediction. Bru et al. (2003) find that even strategically irrelevant factors affect behavior. Rodriguez-Sickert et al. (2008)

present a CPR game with fines and find that even low fines have high deterrence power, and that a fine which is voted against maintains an effect of enforcing a norm. Velez et al. (2009) find that subjects balance self-interest with conformity when selecting strategies. Cox et al (2009) find that first movers' choices in a common property version of the investment game are more likely to increase the size of the pie—and efficiency—than in the private property version; neither version accords with the Nash prediction.

This lack of consensus in the previous literature is perhaps unsurprising. In environments with pure private goods and institutions of impersonal exchange, Nash equilibrium under the assumption of self-interested agents does an excellent—but not perfect—job of predicting behavior. This is in contrast to the line of research concerning pure public goods, following, among others, Isaac and Walker (1988), and Marwell and Ames (1979). The deviations from the self-interested Nash equilibrium have been so ubiquitous and persistent in public goods games and games of personal exchange that it has led to the flourishing of the other-regarding preferences literature (R. Mark Isaac and James M. Walker, 2003).

One reason that the question of the predictive power of self-interested Nash equilibrium in CPR games remains problematic may be attributed to other-regarding preferences. The effects of these preferences on the outcome depend greatly upon the way in which the utility or consumption of others is incorporated into one's own preferences. In the cases of pure and impure (or "warm-glow") altruism, for example, the optimal Pigovian tax will be the same as in the self-regarding case, but the level of consumption of the CPR will differ from the Nash prediction. Paternalistic altruism, however, implies a higher optimal tax than the one under self-interest, because the social optimum requires less consumption than under the presumption of self-interest (Johansson 1997).

Another reason equilibrium predictions might fail could be the difficulties present in modeling the situation experimentally. In practice, making congestion operational in an experimental setting presents a formidable task, particularly in a framework that allows simple testing of a Pigovian subsidy. This problem derives from the fact that congestion requires a nonlinearity in payoffs such that total social payoff peaks and declines at the congested—and privately optimal—level of consumption This has the side effect of reducing incentives to think hard about it at the margin, because the marginal return to social payoff is closest to zero at the social optimum and the private return is closest to zero at the congested level of consumption. If subjects are confused or frustrated, they may simply (and rationally) decide not to think too hard about it. In one treatment, OGW allow communication, and note that in some of their experiments, a lack of dominance appears to be a problem. When the CPR consumption increased in one period, the group members tried to determine whether greed or error was to blame, and one member noted that a defector would have earned "Just a few darn cents above the rest of us." Because of the payoff structure, determining the optimal strategy can be difficult, which may cause Nash predictions to perform poorly.

The predictive power of Nash equilibria with respect to CPR games directly affects the theoretical efficacy of Pigovian taxation or subsidies as a means to achieving efficiency. One of the earliest and simplest solutions to congestion under an open- or limited-access property regime, the Pigovian hypothesis has, to my knowledge, never been tested experimentally. Pigou (1920) hypothesized that, to offset congestion, an optimal tax or subsidy could be applied to internalize the congestion externality—essentially altering the game so that the socially optimal outcome of the CPR is the Nash equilibrium outcome of the modified system. If the Nash equilibrium strategy profile fails to predict behavior in a CPR game, it is unclear what to expect from a Pigovian subsidy.

Finally, the costs of monitoring and enforcement—be they technical or political—required to implement and maintain a Pigovian scheme are often prohibitive. To the extent that people are motivated by non-monetary factors—other-regarding preferences, conformity and other social norms, or merely cognitive difficulty—it may be possible to reduce deadweight welfare loss through non-monetary means.

In order to try to minimize dominance effects, the present experiment reduces the complexity of the payoff function, provides an intuitive interface and response mode, and provides training and software-assisted payoff calculation. The aim is to reduce the cognitive costs of decision-making to allow a sharper test of the Nash equilibrium prediction in this CPR game. This experiment provides evidence that subjects' choices converge, but that it takes some time to reach the predicted outcome.

To date, there has been incidental evidence with respect to the performance of a Pigovian subsidy in achieving the intended outcome, but there has been no direct test of the theory. This experiment presents an experimental test of the Pigovian hypothesis; the experimental results fit well with the theoretical prediction—Pigou was correct. A second treatment in this paper presents subjects with information on the social optimum as a test for the effect of such information on subjects' behavior. I find a small and non-persistent effect, but further experimental study is warranted to determine the feasibility of information provision as a means of improving efficiency.

The paper is set up as follows: The next section presents the basic model of a limited-access CPR that I use in this experiment. Section 2 presents the experimental design, the hypotheses, and the statistical approach. Section 3 presents the results and a discussion and Section 4 presents some concluding comments.

1. Theory

The basic theory of limited-access common-pool resources is a standard in public finance, and environmental, urban and regional economics. The basic intuition derives from a difference between the marginal private benefit (MPB) or cost (MPC) from consumption, and the marginal social benefit (MSB) or cost (MSC) of consumption—an externality. Assuming MPB > MSB and MPC = MSC, for example, the marginal social cost at equilibrium will be greater than the marginal social benefit, and the socially optimal quantity will be less than the equilibrium quantity. Pigou asserted that there exists a subsidy (or tax), t^*, that will induce the socially optimal quantity choice, and that t^* is simply the difference between the MSB and MPB (or MSC and MPC) at the optimal quantity.

The theory itself is relatively straightforward, but the design of an experimental framework to represent congestion has proven complicated. In general, CPR games, including OGW, represent the CPR using a production function approach with an "outside option", which is a pure private good. A test of the Pigovian hypothesis can be implemented by increasing the opportunity cost of expenditure on the CPR, by increasing the private return to the outside option. In order to avoid potential subjective considerations surrounding subjects' concept of taxation, as well as to avoid negative returns and potential effects due to prospective losses, I test the theory using a subsidy, rather than a tax.

Formally, let $i = (1, \ldots, n)$ index individual agents. Let z_i represent individual i's endowment, x_i represent i's expenditure on the CPR, and Σx_j represent total (combined) expenditure on CPR (including i). Let $\alpha(z_i - x_i)$ represent the payoff from an outside option, $g(x_i, \Sigma x_j)$, the payoff from the CPR, and $\pi(x_i, \Sigma x_j)$ an individual's total payoff. Specify the payoff to the common pool resource by defining $g(x_i, \Sigma x_j) = (\beta - \gamma \Sigma x_j)x_i$, where β is a per-token payoff to the CPR that declines with increasing consumption of the CPR with the γ parameter (for γ = 0, there is no congestion in the good). Under standard economic assumptions, each individual is maximizing $\pi(x_i, \Sigma x_j)$with respect to x_i. In general, with appropriation games, there is an incentive to consume the CPR and an incentive to consume the outside option. The game played in the present experiment has the following payoff function[1]:

$$\pi(x_i, \Sigma x_j) = \alpha(z_i - x_i) + (\beta - \gamma \Sigma x_j)x_i$$

To assist subjects in determining their payoffs, the software provides a payoff calculator that allows subjects to figure out the consequences of hypothetical situations before making a decision. The calculator is discussed further in section 2.

This payoff function presents subjects with a fixed per-token return to the outside option and a declining per-token return to the CPR. In order to introduce a subsidy, I add an additional fixed per-token amount (δ) to the return to the outside option.

Proposition. Define the payoff function for individual i as:

$$\pi(x_i, \Sigma x_j) = (\alpha + \delta)(z_i - x_i) + (\beta - \gamma \Sigma x_j)x_i$$

a) Without a subsidy $(\delta = 0)$, the Nash equilibrium is symmetrical with each player choosing $x_i^* = \frac{\beta - \alpha}{(n+1)\gamma}$.

[1] This is similar, but not identical, to the payoff function used in OGW. In particular, OGW use an approach where each subject earns a share of quasi-linear production in the CPR, in which the framing and the functional form are presented to the subjects. We use a per-token approach, explained as such, which seems more transparent, and requires no facility with exponents to figure out one's own payoff.

b) For ($\delta = 0$), the social optimum occurs when each player chooses $x_i' = \frac{\beta-\alpha}{2n\gamma}$. The socially optimal level of consumption and the Nash equilibrium level of consumption are only identical for n = 1 or β = α.[2]

c) For ($\delta \neq 0$), the strategy at the Nash equilibrium becomes $x_i^* = \frac{\beta-\alpha-\delta}{(n+1)\gamma}$, and the optimal Pigovian subsidy is $\delta^* = (\beta - \alpha)\left(\frac{n-1}{2n}\right)$.[3]

The incentives governing the marginal decision to consume the CPR warrant a brief discussion. Unlike linear VCM games, the marginal per-capita return (MPCR) is not constant in this game. Consider a unit increase in the consumption of the CPR (implying a unit decrease in consumption of the outside option), and where x_i represents the current level of CPR consumption. The MPCR to oneself (which is the previously discussed MPB) from consuming an additional unit of the CPR is $[\beta - \gamma(\Sigma x_j +$ $1 - (\alpha + \delta) - \gamma x_i$. The MPCR to others varies across individuals, proportional with their level of consumption of the CPR, and is equal to $-\gamma x_k$ for each individual, where k indexes other individuals. This is straightforward: each unit of CPR consumption carries a variable benefit, which is $\beta - \gamma(\Sigma x_j + 1)$ for the $(x_i + 1)^{\text{th}}$ unit, an opportunity cost in the form of a forgone return to the outside option, $-(\alpha + \delta)$, and reduces the value of all previous consumption of the CPR by γ, which decreases own-payoff by γx_i, and decreases other payoffs by γx_k for each k in the group. Except for the case where no one else is currently consuming the CPR, one's own consumption of the CPR unambiguously reduces others' payoffs, which implies that the MSB < MPB for $\Sigma x_k \neq 0$.

[2] These represent two trivial cases: the case of individual use, in which there is no externality, and the case of an outside option that dominates the CPR.

[3] For the purposes of this experiment, I am abstracting away from the source of the subsidy and possible distortionary effects in raising the required revenue. It should be noted that the theory postulated by Pigou is not complete in this respect, as it does not posit a budget balancing constraint—the taxes go nowhere and the subsidies come from nowhere. This is typical in tax theory in a partial equilibrium framework, and in practice, it seems unlikely that people are aware of the total effect of every dollar they receive as a subsidy or dollar they pay in taxes. In addition, a number of other mechanisms for achieving efficiency rely on abandonment of budget balancing; the Clarke tax and the Groves-Ledyard mechanism are two important examples

2. Protocol

Because the impact of social norms and framing is of interest, I avoid terminology like "common-pool resource", "extraction", "appropriation", "tax", "subsidy", etc. I follow Andreoni & Petrie (2004) in presenting the decision they face as an "investment" decision in which they will decide how to invest a number of tokens in each period. Subjects are given the choice to invest their tokens in the outside option or the CPR, which are referred to in the experiment as the "RED investment" and the "BLUE investment", respectively.

I implement this model using the following parameterization: (α: per-token baseline RED payoff; β: per-token starting BLUE payoff; γ: per-token BLUE congestion parameter; δ: per-token RED subsidy; z: period endowment; n: group size) = ($0.00, $0.36, $0.01, $0.12, 10, 3). These parameters were chosen for a number of reasons. In particular, they guarantee a unique (and symmetric) interior Nash equilibrium in both the baseline and the subsidy treatments ($x = 9$ and $x = 6$, respectively). They also provide enough distance between the two equilibria for statistical inference. In addition, the differences are economically significant. Under the socially optimal outcome, subjects would earn $26.88; the per-subject payment under the Nash equilibrium outcome is $22.26. The minimum possible payoff is $0.00 for the information treatment and $4.20 for the subsidy treatment. The maximum possible payoff is $54.60 under both conditions. Finally, the group size is such that off-Nash behavior might reasonably be sustained, as implicit collusion is easier with smaller groups. If Nash cannot be rejected, it seems likely that it would predict well for larger groups.

This analysis has relied on continuity and differentiability to determine Nash results. In practice, it is not generally true that a unique Nash equilibrium in the continuous case implies a unique equilibrium in a discrete implementation (Swarthout and Walker 2009). In order to ensure that these continuous results hold for the implementation I use in the experiment, I tested every strategy profile under the parameters and find that there is indeed a unique (and symmetric) interior Nash equilibrium in both the baseline case $[(s_1, s_2, s_3) = (9,9,9)]$ and the subsidy case $[(s_1, s_2, s_3) = (6,6,6)]$.

The experiment was conducted in two sessions at Georgia State University's Experimental Economics Center (ExCEN). In each session there were 24 subjects, randomly separated into 8 groups of 3.[4] Each session lasted about an hour and a half. Individual earnings, including a $5 show-up payment, ranged from $17.98 to $40.60.

The sessions were run with a double-blind protocol. Our primary research questions concern individual behavior under induced preferences, as well as those preferences they might have regarding the welfare of anonymous members of their group. In addition, the information treatment looks at the provision of information without a direct appeal to social norms. There is some experimental evidence that with less than strict anonymity, the domain of other-regarding preferences may expand beyond the group (see, for example, Hoffman et al (1994), Cox and Deck (2005), and Andreoni and Petrie (2004)). Relaxing anonymity to observe CPR consumption decisions in the presence of external subjective norms is another straightforward extension of the present experiment.

Strict anonymity was maintained, but in each round, all subjects were aware of the sum of the decisions made by the other members of their group in each previous round. Groups were randomly assigned, but fixed throughout the experiment. The experiment was computerized, and was run in an experimental lab with dividers in place so that subjects could not easily see one another. Each subject participated in two baseline treatments ("B") and one of two experimental treatments: either "S," the Pigovian subsidy treatment, or "I", the information treatment. All subjects in a given session participated in the same treatments. For each treatment, each individual was asked to make seven "investment" decisions.

In each period, each token invested in the RED investment paid a fixed per-token amount. Each token invested in the BLUE investment paid a per-token amount that depended upon the total number of tokens invested in the BLUE investment by the group. Each session consisted of two treatments,

[4] In the first session, a student asked to leave after subjects had been signed in and placed in groups, but before the experiment began. A graduate student took his place to satisfy the requirements of the software and to allow the other students in his group to participate. I exclude data from that group; inclusion does not affect the qualitative results.

administered in B-A-B format, so that each session consisted of a baseline treatment, an experimental treatment, and a second baseline treatment. Subjects knew the number of periods, but were not made aware ahead of time when treatments would begin or end. Because of the relative complexity of the payoff structure as well as an established downward trend, or "decay", in group contributions, widely documented in public goods games (Isaac and Walker 1988, for example), providing a second baseline allows us to observe, and perhaps account for, any such trends when trying to discern a treatment effect.

In the baseline periods in both sessions, tokens invested in the RED investment provided a per-token payoff of $0.00. Tokens invested in the BLUE investment provided a per-token payoff of $0.35 for a single token. The per-token value of tokens invested in the BLUE investment declined by $0.01 per token invested in BLUE under all experimental conditions, down to a minimum of $0.00 per token. After each period, subjects were informed of the total group investment in the BLUE investment, as well as their period payoff and their total profit.

In the first session, the experimental treatment was the administration of a Pigovian subsidy. During periods 8-14, the RED token payoff was increased to $0.12.

In the second session, the experimental treatment was the provision of information regarding the common pool resource. During periods 8-14, subjects were given the total group payoff in the previous period, the hypothetical group payoff at the social optimum, and an explanation of how to achieve the social optimum in the event that the two are unequal (Figure 1).

> During the last period, your group earned a total of **$2.43**. The maximum your group could have earned was **$3.24**.Your group earned LESS than it could have in that period. To increase your group's total payoff, your group should **REDUCE** its investment in the BLUE investment.

Figure 1. Information Treatment

Each session proceeded as follows: subjects were allowed to read the instructions privately; the instructions were then read aloud, verbatim. (Appendix A) After the instructions were completed, an example was drawn from the instructions and demonstrated by the experimenter on a projection of the computer interface. Subjects then were given a walk-through tutorial of the computer interface (Figure 2), in which they were allowed to select from several sets of parameters and then given the opportunity to practice using the software with a computer playing deterministically as the “rest of the group,” selecting 0 tokens in the BLUE investment in the first round, followed by 1 token in the second round, continuing up through 20 tokens, before restarting at 0 tokens. Subjects were allowed to practice this way as long as they liked. They chose to participate in between 0 and 42 practice rounds.

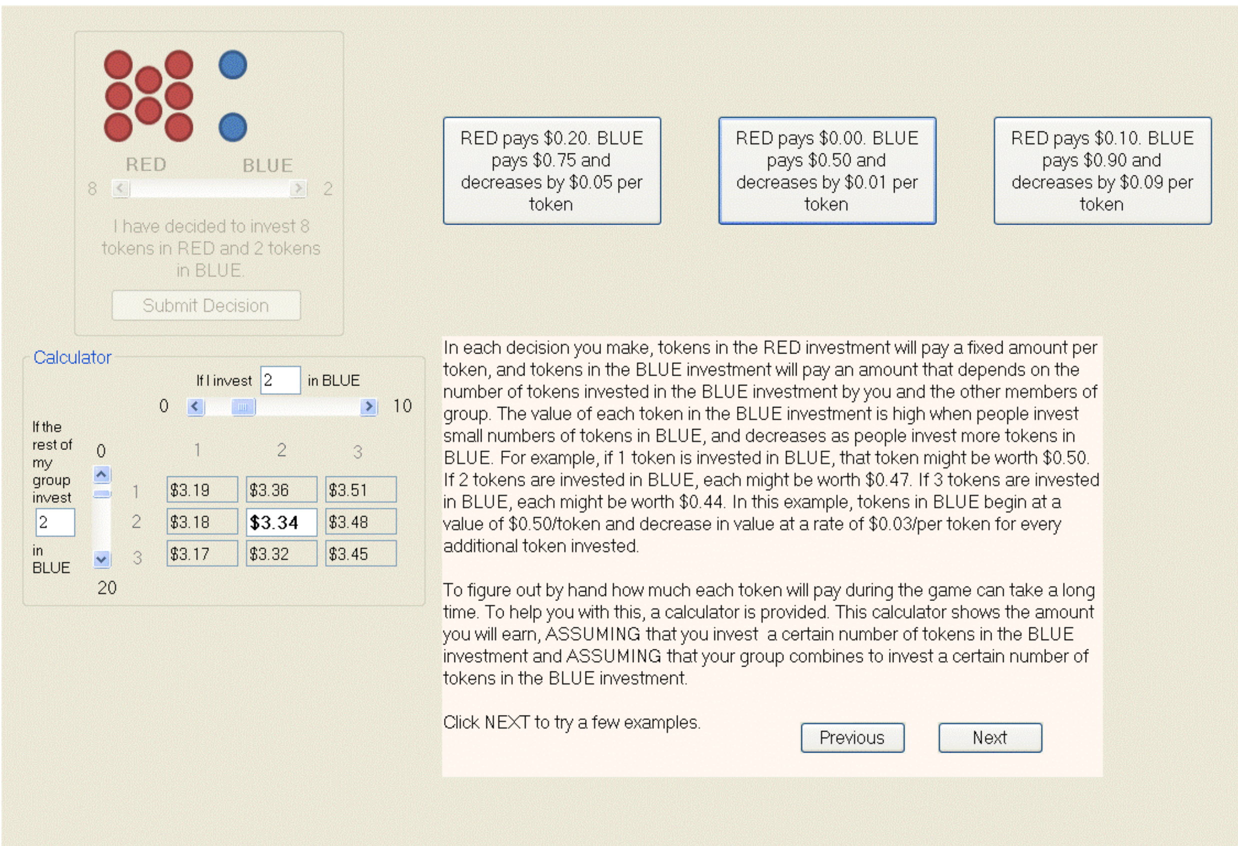

Figure 2. Tutorial Screenshot. See Appendix B for screenshots of the full tutorial.

In addition to the practice rounds, subjects had access to a payoff calculator throughout the tutorial and the experiment. The payoff calculator (Figure 3) allows subjects to choose a hypothetical decision for themselves, a hypothetical combined investment in the BLUE investment for the rest of the group, and provides information on their payoffs under the current parameters, as well as the own-payoff consequences of single-token changes in either direction for themselves or for the group. The practice periods and tutorial were intended to introduce subjects to the decision task, familiarize them with both the task and the interface, and provide them with an opportunity to use the calculator and the interface before making decisions for real payoffs. I collected data on the number of practice rounds each subject chose to use.

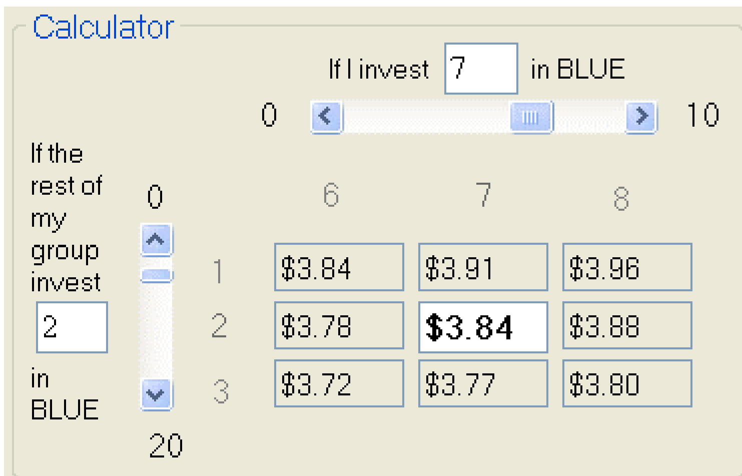

Figure 3. Payoff Calculator

Once the experiment concluded, subjects were asked to fill out a questionnaire while payments were prepared. This questionnaire included basic demographic data, as well as data on education and measures of outlook regarding trust, justice, and human nature.

3. Hypotheses

My primary hypotheses of interest are ($\bar{x}$ indicates mean):

1. The Nash equilibrium outcome is a good predictor of subjects' choices: $\bar{x}_{baseline} = x^*_{NE}$
2. The Pigovian subsidy has the theoretically predicted effect: $\bar{x}_{subsidy} = x'_{no-subsidy}$, where x' is the mean investment in the CPR at the pre-subsidy social optimum.
3. The presentation of information has no effect: $\bar{x}_{information} = \bar{x}_{baseline}$.

If subjects express other-regarding preferences—particularly pure, impure, or paternalistic altruism—we should expect 1 and 2 to fail. In particular, if other-payoff enters positively into the utility function, we should expect $\bar{x}_{baseline} < x^*_{NE}$ and $\bar{x}_{subsidy} < x'_{no-subsidy}$.

If subjects are intending to express other-regarding preferences, but making errors in the attempt, the provision of information on the group payoff in addition to information on their own payoff would allow them to change their investment decisions to more accurately represent their preferences. If they possess an external norm that indicates that, given an opportunity to make the group better off at one's own expense, one *ought* to take such an opportunity, provision of information on the group's total payoff provides both a reminder of the relevance of the choice task to group welfare and information on how to improve group welfare at one's own expense. Finally, if information acts as a coordination point, even self-interested agents might strategically coordinate on a point that would give them higher payoffs with the hope of either sustaining a higher level of earnings or reneging in the future. Consequently, if subjects are either prone to errors, have norms that are not fully internalized, or are prone to strategic coordination, we should expect to see $\bar{x}_{information} < \bar{x}_{baseline}$.

In addition, I test a number of other hypotheses regarding subsets of the data to try to get a more accurate picture of subject behavior. I also consider other questions, including the source and causes of deviations from Nash strategy, as well as concerns regarding censoring, using more parametric estimation techniques.

4. Results

As previously mentioned, in both treatments the first seven rounds were baseline rounds, as were the last seven rounds, with the intervening seven rounds presenting experimental treatments. I report the results discursively; statistical test results are presented in Table 1 and indexed by hypothesis being tested (e.g. H_1, H_2, …). In the table, "Baseline 1" refers to periods 1-7, "Baseline 2" refers to periods 15-21, and "Baseline" without a number refers to the combined results from Baseline 1 and Baseline 2. In addition, unless otherwise specified, the variable of interest in this section is the across-period mean CPR investment decision by a given subject, paired when appropriate. This approach accounts for both individual and group fixed effects.

Hypothesis	Test 1	Test 2	Reject?
H_1: Session 1 Baseline = Session 2 Baseline	Wilcoxon Rank-Sum Test Z = 3.665, p = 0.002	K-S Test[5] (10000 iterations) D = 0.4464, p = 0.017	Reject
H_2: Session 1 Baseline 1 = Session 1 Baseline 2	Wilcoxon matched-pairs sign-rank test Z = -2.575, p = 0.010	K-S Test (10000 iterations) D = 0.5238, p = 0.006	Reject
H_3: Session 2 Baseline 1 = Session 2 Baseline 2	Wilcoxon matched-pairs sign-rank test Z = -3.002, p = 0.003	K-S Test (10000 iterations) D = 0.3333, p = 0.093	Reject
H_4: Session 1 Baseline = 9	Wilcoxon signed-rank test Z=-0.863, p =0.388		Cannot reject
H_5: Session 1 Treatment = 6	Wilcoxon signed-rank test Z=-0.233, p =0.816		Cannot reject
H_6: Session 1 Baseline = Session 1 Treatment	Wilcoxon matched-pairs sign-rank test Z = -3.002, p = 0.000	K-S Test (10000 iterations) D = 0.857, p = 0.000	Reject
H_7: Session 2 Baseline = 9	Wilcoxon signed-rank test Z=-6.714, p =0.000		Reject
H_8: Session 2 Baseline = Session 2 Treatment	Wilcoxon matched-pairs sign-rank test Z = 0.729, p = 0.466	K-S Test (10000 iterations) D = 0.125, p = 0.975	Cannot reject
H_9: Session 2 Baseline 2 = Session 2 Treatment	Wilcoxon matched-pairs sign-rank test Z = 3.211, p = 0.001	K-S Test (10000 iterations) D = 0.25, p = 0.347	Reject
H_{10}: Session 2 Mid-Baseline = Session 2 Treatment	Wilcoxon matched-pairs sign-rank test Z = 1.416, p = 0.157	K-S Test (10000 iterations) D = 0.1667, p = 0.815	Marginal rejection
H_{11}: Session 2 Baseline 2 = Session 2 Treatment (detrended)	Wilcoxon matched-pairs sign-rank test Z = 0.743, p = 0.458	K-S Test (10000 iterations) D = 0.1667, p = 0.820	Cannot reject
H_{12}: Session 2 Mid-Baseline = Session 2 Treatment (detrended)	Wilcoxon matched-pairs sign-rank test Z = 0.972, p = 0.331	K-S Test (10000 iterations) D = 0.1667, p = 0.834	Cannot reject
H_{13}: Session 2 Baseline = Session 2 Period 8	Wilcoxon matched-pairs sign-rank test Z = 2.258, p = 0.024	K-S Test (10000 iterations) D = 0.375, p = 0.047	Reject
H_{14}: Session 2 Mid-Baseline = Session 2 Period 8	Wilcoxon matched-pairs sign-rank test Z = 2.733, p = 0.006	K-S Test (10000 iterations) D = 0.417, p = 0.020	Reject

[5] Where appropriate, I use a boot-strapped Kolmogorov-Smirnov test of equality of distributions for distribution tests, which does not incorporate matching, but has the nice property of being able to test against discrete distributions. (See **Sekhon, Jasjeet S.** Forthcoming. "Multivariate and Propensity Score Matching Software with Automated Balance Optimization: The Matching Package for R." *Journal of Statistical Software*.) I use this test primarily as a robustness check.

H_{15}: Session 2 Baseline = 9 (random-effect tobit model)	Wald test $\chi^2(1)$ = 0.02, p = 0.896		Cannot reject

Table 1. Statistical tests of hypotheses and robustness checks

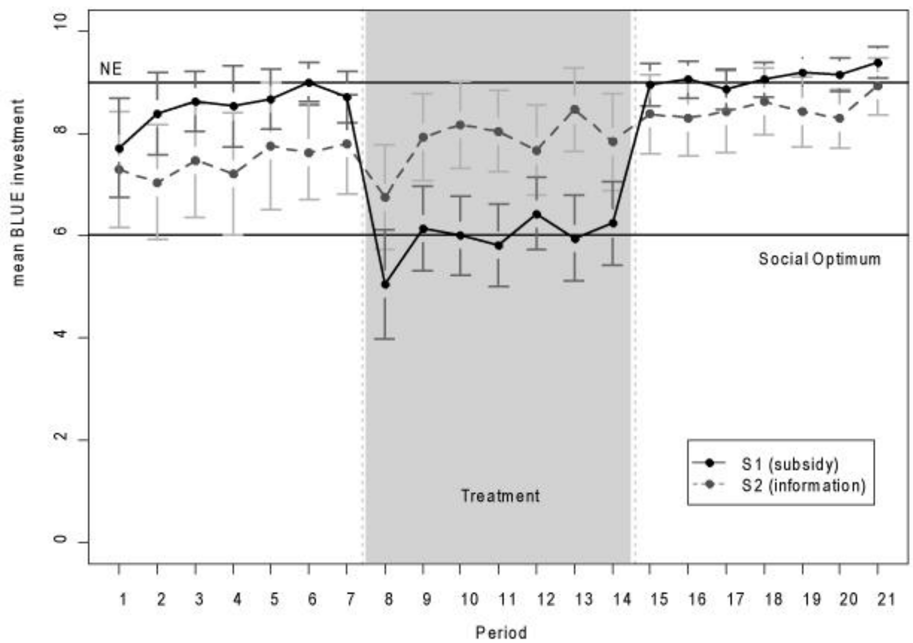

Figure 4. Mean BLUE investment by period by session

(NE line indicates Nash equilibrium prediction without subsidy)

The sessions differ significantly (H_1: p = 0.000, Figure 4). The mean baseline investment in the CPR in Session 1 was 8.803 tokens, while the mean baseline investment in Session 2 was 7.964 tokens. The null that these are equal can be rejected. In addition, there is evidence of either learning or a "decay"-type trend (probably both). In the first session, baseline 1 mean investment in the CPR was 8.517 (SE = 0.126) tokens while the baseline 2 mean investment was 9.088 (SE = 0.063) tokens. Again, we can reject the null of equality (H_2: p = 0.010). In the second session, the baseline 1 mean investment was 7.452 (SE = 0.200) tokens, while the baseline 2 mean investment was 8.476 (SE = 0.125) tokens. Once again, we can reject the null that these observations are drawn from the same distribution. (H_3: p = 0.003).

Figure 5 presents the mean decision by period in the first session. In the first session baseline periods, we cannot reject the null that subjects' behavior accorded with the Nash prediction, on average

(H_4: $p = 0.388$). The subsidy, in addition, seems to have the effect posited by Pigou (H_5: $p = 0.816$). Subjects' mean investment in the CPR was 5.946 (SE = 0.154) tokens, which is not significantly different from the Pigovian prediction of 6 tokens. We can reject the null of no treatment effect; this is robust to using the first, the second, or the combined baseline treatment as a basis for comparison (H_6: $p = 0.000$).

Because of the existence of an underlying time trend, two approaches were used to try to separate the effects of learning and decay from the treatment effect. The first is to use as a basis of comparison only those periods which are most like those of the treatment group in terms of learning and decay—namely, the last three of the first baseline and the first four of the last baseline, which I will refer to as the "mid-baseline." Using the mid-baseline has a few advantages: I expect some of the noise of experimentation and learning has dissipated by period 4, while these periods do not contain the same level of decay as the last three periods.

The second attempt requires the assumption of a linear trend that is stationary throughout the session. Elimination of this trend was done by simple OLS regression of the subjects' investment decisions on the period, and then subtraction of this period-based component to produce a de-trended decision. For the subsidy treatment session, neither method has a qualitative effect on the magnitude or significance of this treatment effect.

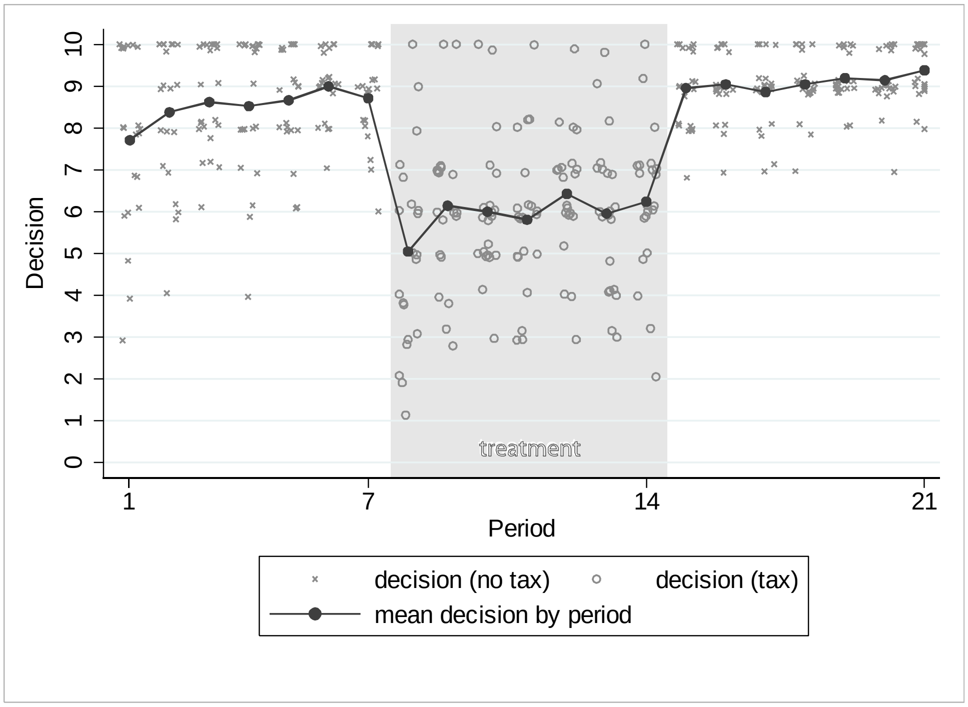

Figure 5. Investment decisions by Period, Session 1

For the second session, we can reject the null that pooled baseline behavior is equal to the Nash prediction (H_7: $p = 0.000$), and subjects' investment decisions appear to be noisier and converge later than do those in the first session (Figure 6). The effect of the information they receive is more difficult to discern. The mean contribution decision during the information treatment was 7.833 (SE = 0.163) tokens, and indeed, we cannot reject the null of equality with the baseline mean (H_8: $p = 0.466$). Considering the noise of the first several periods of this session, however, other tests seem appropriate. Comparing the treatment periods only to the second baseline, for example, produces a paired test that recommends rejecting the null of equality (H_9). Because the treatment precedes this second baseline, it appears that the underlying time-trend may confound the result. Both methods to account for the time-trend in the first session were also used for the second session.

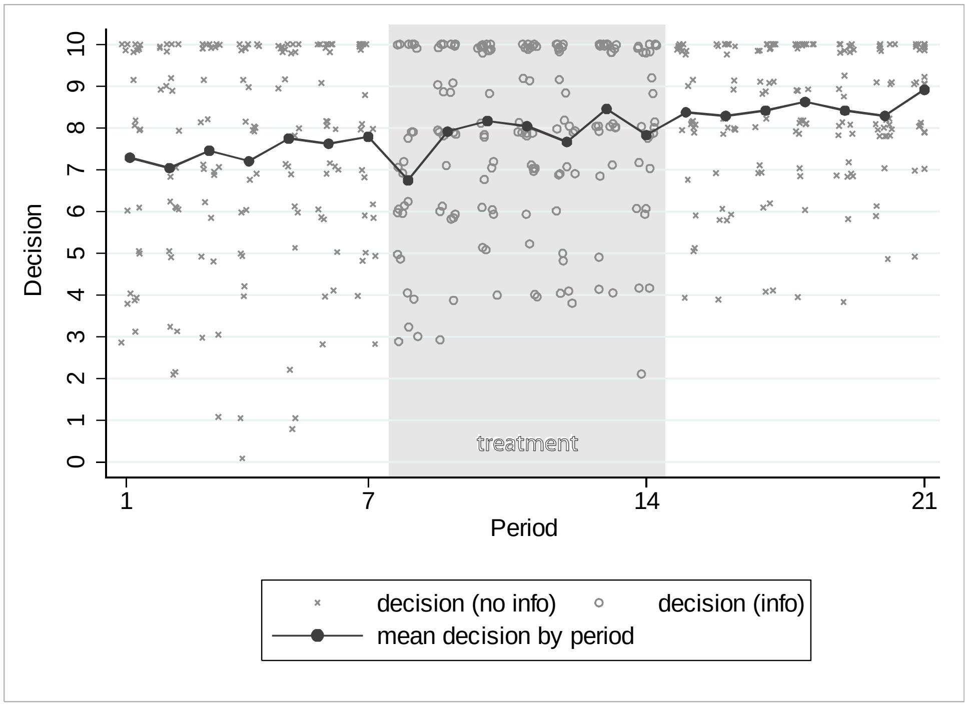

Figure 6. Investment decisions by Period, Session 2

The mean contribution in the mid-baseline periods was 8.125 (SE = 0.162), which is marginally different than that of the treatment group (H_{10}). The mean de-trended decision in the session was 7.127 (SE = 0.117) tokens in the CPR. Use of the de-trended version removes any significant difference between the second baseline and the treated group or the mid-baseline and the treated group (H_{11}, H12).

A sharp decline in contributions is visible in Figure 6 during period 8, the first period of the treatment (mean contribution to the CPR = 6.750, SE = 0.494). Nonparametric tests indicate that this is indeed significantly different from the full baseline, as well as the mid-baseline, and that these results persist even in the de-trended data (H_{13}, H14).

It is unclear that an assumption of a linear trend is a legitimate one, so while the tests for the de-trended data are illustrative, they may not be conclusive. A more sophisticated test for an effect of information can be developed by considering the nature of the treatment: in particular, subjects may see one of three different types of message. For subjects in groups that under-invest in the CPR, they are informed that an increase in their level of investment would increase the payoff to the group. For those in groups at the social optimum, they are informed that their current level of investment is optimal. Finally, for those in groups suffering from congestion in the CPR, subjects are informed that a reduction in investment would lead to an increase in group payoff. It may be the case that the information is having an effect, but that offsetting behavior leads to an inability to reject the null of no effect, because the changes preserve the mean level of investment within subjects.

In practice, of the 168 messages subjects received during the information treatment, 147 informed subjects that a decrease would improve group payoff, 9 informed subjects that an increase would improve group payoff, and 12 informed subjects that they were at the maximum group payoff. Consequently, 12.5% of the messages sent to subjects would not be expected to induce a reduction in CPR investment. Considering the subset of subjects who received a message related to a decrease in CPR investment should allow a better test of a treatment effect. Consider this “sub-treatment” the “Decrease” treatment.

Selection of the counterfactual is important in this case. Those who received the Decrease treatment are similar in known ways. In particular, these are subjects in the Information session, during the middle seven periods, who were members of groups whose combined investment in the previous period exceeded the socially optimal level of investment. Considering all periods in session 2, we cannot reject the null of no effect of the Decrease treatment. Considering only those periods between period 4 and period 17, we can reject the null of no effect at the 10% level. In both cases, these hypothesis tests are unconditional and, as we are using mean levels of investment by subject, we have 24 observations. Using regression methods, we may be able to account for censoring and improve statistical power.

Matched-pair sign-rank test	Session 2 only	Session 2 mid-baseline	Treatment
Mean (SD)	8.070 (1.508)	8.178 (1.638)	7.846 (1.657)
Z	1.001	1.753	
p	0.317	0.080	

Table 2. Tests of the effect of the Decrease treatment.

In this case, again, selection of the counterfactual is important. In order to increase the power of the test, some of the regressions include data from both sessions. Table 3 presents the results for selected regressions. Those observations that are considered counterfactuals from session 1, under the “Full” subset of the data, are those investment decisions for which the group decision in the previous period exceeded the social optimum and the price level was the same as in the information treatment in both the preceding period and the period in which the decision was made. The reported results are robust to modifications in the chosen counterfactual set of observations.

In addition to tests of the average effect of the Decrease treatment over the seven-period treatment, the regressions include specifications using only the first 8 periods of session 2 (the results labeled “One-shot” in the “Data subset” row), which provides a test of the effect of the Decrease treatment on first sight. This “first-sight” effect is always significant at the 10% level. Subjects’ observed choices declined significantly the first time they received the Decrease treatment.

The effect of the Decrease treatment is always negative and generally significant, so this particular form of information provision appears to have a small negative effect on investment in the CPR that spikes in subjects’ first exposure, reducing investment levels on average by a little over a single token, but which does not persist through subsequent periods. It is smaller than the effect of the Pigovian subsidy, but is perhaps surprisingly large, given that there is no direct appeal to social norms nor any communication allowed among subjects. These results represent a roughly 9% increase in subjects’ single-period earnings as a result of the first exposure to the Decrease treatment, indicating that there may be greater efficiency gains possible without requiring a costly intervention such as a tax or subsidy.

The observed difference between sessions may be correlated with use of practice rounds. During the tutorial phase of the experiment, subjects had the opportunity to play with a deterministic computerized “rest of the group” as many times as they liked. The median number of practice rounds for subjects in Session 1 was 3.5, while the median for session 2 was 1.5 (the corresponding means are 9.04 and 5.583). The two distributions are marginally significantly different (the Mann-Whitney test gives a p-

Dependent variable is number of tokens invested in the CPR

information	-0.247*	-0.277*	-0.891*	-0.188	-0.178	-1.303***	-0.194	-0.18	-1.304**	-0.249*	-0.231	-1.186**
	(0.089)	(0.080)	(0.083)	(0.234)	(0.321)	(0.010)	(0.231)	(0.294)	(0.022)	(0.087)	(0.135)	(0.044)
candidate	0.654**	0.916**	0.402	0.426**	0.416*	0.326	0.571**	0.507	0.252	0.720**	0.667*	0.68
	(0.020)	(0.013)	(0.205)	(0.027)	(0.077)	(0.234)	(0.047)	(0.180)	(0.477)	(0.018)	(0.080)	(0.331)
subsidy	-2.323***			-2.494***			-2.519***			-2.477***		
	(0.000)			(0.000)			(0.000)			(0.000)		
period				0.0570***	0.0713***	0.121	0.0595***	0.0743***	0.119	0.0554***	0.0683***	0.0562
				(0.000)	(0.004)	(0.191)	(0.000)	(0.000)	(0.148)	(0.000)	(0.000)	(0.604)
L.decision							0.227***	0.204	0.183	0.159**	0.141	0.228
							(0.006)	(0.120)	(0.308)	(0.029)	(0.194)	(0.262)
L.otherdecision							-0.150***	-0.132***	-0.0711	-0.139***	-0.114***	-0.113
							(0.000)	(0.009)	(0.317)	(0.000)	(0.002)	(0.162)
L2.decision										0.160***	0.184***	0.0741
										(0.000)	(0.001)	(0.186)
L2.otherdecision										-0.0742***	-0.0934***	-0.0455
										(0.000)	(0.005)	(0.552)
Constant	7.830***	7.224***	7.163***	7.367***	6.817***	6.672***	7.798***	7.169***	6.436***	8.021***	7.374***	6.869***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.003)
sigma_u												
sigma_e												
Specification	Fixed-effects OLS, standard errors clustered on group						Fixed-effects OLS, heteroskedasticity-robust standard errors					
Data subset	Full	Session 2	One-shot	Full	Session 2	One-shot	Full	Session 2	One-shot	Full	Session 2	One-shot
Observations	900	480	168	900	480	168	900	480	168	855	456	144
R-squared	0.346	0.03	0.036	0.384	0.097	0.052	0.455	0.176	0.092	0.501	0.228	0.132
Number of id	45	24	24	45	24	24	45	24	24	45	24	24

Robust p-values in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3. Regression results under different specifications. Data subsets: "Full" indicates both sessions are included with the first period omitted, as candidacy for treatment depends on lagged group decisions. "Session 2" indicates only session 2 data is included. "One-shot" indicates that data is drawn from periods 1 – 8 only. Significance of the coefficient on "information" represents a test of the null hypothesis that information about a decrease in CPR investment had no effect.

value of 0.1215, but the total number of subjects is only 45), but in other observable ways, the two sessions appear to draw from the same population.[6]

This seems to be borne out by the progress of subjects' behavior over the course of the experiment. The mean absolute deviation from best response is, in a sense, a measure of the deviation from self-interested behavior, as payoffs are decreasing with this deviation. Figure 7 presents the mean absolute deviation from the best response over time: it is clear that both samples are converging over the course of the experiment—in the limit, to the Nash prediction—but that 21 periods are not enough to ultimately converge within the second session.

If learning is a concern, we might expect the practice rounds to help subjects converge, and indeed there is a marginally significant effect of the number of practice rounds played on the mean absolute deviation from best response (p=0.058, n = 45). For the average subject, in terms of mean absolute deviation from best response, the effect of practice rounds reduces the mean absolute deviation from best response by 0.0354 tokens per round. With an average number of tutorial trials of 7.18 across the full sample, the mean effect of practice rounds reduces the mean absolute deviation from best response by .25 tokens, or a 15% reduction in average absolute deviation.

[6] An early hypothesis for the difference in baseline behavior was a "Friday effect," as the second session was run on a Friday, the first on a Tuesday. This could either be due to a hypothetical change of behavior among subjects on Fridays or to drawing from different sets of students not in class at the time of the experiment—different types of classes might be held on a Tuesday/Thursday schedule, others on Monday/Wednesday/Friday. This second hypothetical cause of a "Friday effect" does not appear to be detectable among observable covariates.

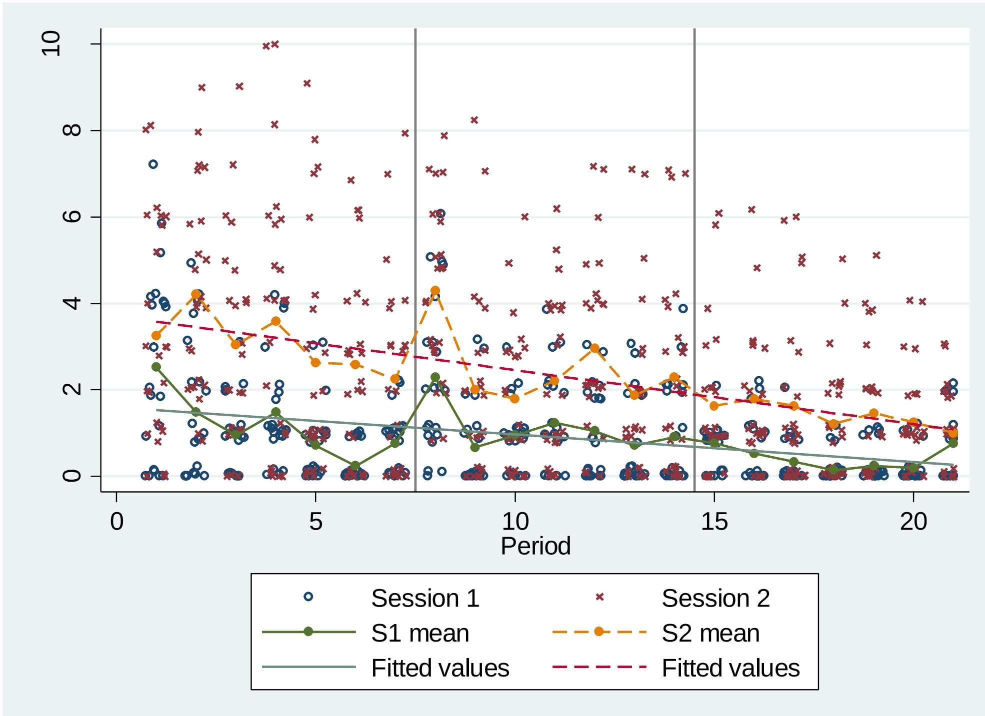

Figure 7. Absolute deviation from best-response by period by session
(with population means and lines of best fit)

5. Conclusion

As population continues to rise, the impact of congestion externalities continues to increase. Common-pool resources are increasingly policy-relevant, and while there is a growing literature on common-pool resource experiments, these goods still have not received the research attention that private and pure public goods have received. The reasons for this are both technical and theoretical—these goods are complicated by their very nature, and the institutions that govern them vary widely. This experiment presents a simplified common-pool resource experiment to subjects and the results indicate that subjects do indeed converge to the Nash prediction under these conditions, but that convergence can take quite a while.

One of the simplest (theoretically, if not practically) policy tools to correct for the congestion externality inherent in common-pool resources is the introduction of a Pigovian tax or subsidy to internalize the externality. I show that such an intervention, if feasible, should have the effect hypothesized by Pigou. Bearing in mind the impracticality or high cost of introducing such a direct intervention, I find a smaller, but significant effect from an information provision treatment. Further study on similar approaches to appeals to social norms should provide more insights into how effective such appeals can be at reducing congestion in common-pool resources. Ferraro (2009), for example, reports a large-scale randomized policy field experiment and finds “pro-social” messages have an effect on water use. The information treatment used here primarily provided information, rather than appealing directly to social norms. Future research should look at the effect of specific appeals to social norms in reducing congestion in the lab.

In addition, extending the experiment to incorporate taxes directly, allowing subjects to see marginal changes in both own- and other-payoff, changing group size, and directly modifying marginal per-capita return on investment would provide useful information on the sensitivity of CPR consumption decisions to these conditions. In particular, experiments using very large groups could be very useful in extending external validity to more closely represent naturally occurring common-pool-resources.

Finally, I find that subjects’ participation in practice rounds has a positive and significant effect on the rate of convergence to the Nash prediction. This, as well as the evidence on the rate of subjects’ convergence to equilibrium, confirms that common-pool resource experiments are complicated, and our inference with respect to subject behavior should allow for a nontrivial amount of time for convergence to equilibrium.

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Appendix A: Subject Instructions

[The instructions, as viewed by subjects, were presented as part of the computer interface and were formatted as a webpage. As a result, there were no page breaks and the spacing and leading were slightly different than in the example below.]

This is an experiment about decision making. You will be paid for participating, and the amount of money you earn depends on the decisions that you and the other participants make. At the end of the session, you will be paid privately and in cash for your decisions.

Privacy and Anonymity

You will never be asked to reveal your identity to anyone during the course of the experiment. Your name will never be associated with any of your decisions. In order to keep your decisions private, please do not reveal your choices to any other participant.

Your Key and Your Payment

All the money that you earn will be yours to keep, and your earnings will be paid to you **IN CASH** at the end of the experiment.

At this time, you will be given a key with a number on it. After you have finished reading the instructions, you will be asked to enter the number on your key into the computer.

IT IS VITALLY IMPORTANT THAT YOU ENTER YOUR KEY NUMBER CORRECTLY,
AS THIS NUMBER WILL BE LINKED TO YOUR PAYMENT.

At the end of the experiment, we will place payment in a locked box which your key will open. We will call you out of the room, one by one, to open your box anonymously, retrieve your earnings, and deposit your key.

This Experiment

In this experiment you will be asked to make a series of decisions about how to invest a set of tokens. You and the other subjects will be randomly assigned into groups and you will not be told each others' identities.

There will be three people in your group—you and two others.

In each period, each of you will have ten (10) tokens to invest. You can invest these in either a ***RED*** investment or a ***BLUE*** investment. The amount of money you earn depends upon how many tokens you decide to invest in the ***RED*** investment or the ***BLUE*** investment, as well as how many tokens others decide to invest in the ***RED*** investment or the ***BLUE*** investment.

In each decision you make, tokens in the ***RED*** investment will pay a fixed amount per token, and tokens in the ***BLUE*** investment will pay an amount that depends on the number of tokens invested in the ***BLUE*** investment by you and the other members of group. The value of each token in the ***BLUE*** investment is high when people invest small numbers of tokens in ***BLUE***, and decreases as people invest more tokens in ***BLUE***. For example, if 1 token is invested in ***BLUE***, that token might be worth $0.50. If 2 tokens are invested in ***BLUE***, each might be worth $0.47. If 3 tokens are invested in ***BLUE***, each might be worth $0.44. In this example, tokens in ***BLUE*** begin at a value of $0.50 per token and decrease in value at a rate of $0.03 per token for every additional token invested. *No token ever pays less than $0.00*, which is to say, you can never lose money from a token. In this example, if more than 17 tokens are invested in ***BLUE***, all tokens invested in ***BLUE*** will have a value of $0.00.

To summarize:

- In each period, you will have ten (10) tokens.
- Your task, in each period, is to decide how many of your tokens to invest in the ***RED*** investment and how many to invest in the ***BLUE*** investment.
- In each period, you will earn a fixed amount for each token you invest in the ***RED*** investment.
- You may earn money for each token you invest in the ***BLUE*** investment—the actual amount you earn for each token you invest depends on your and everyone else in your group's decision to invest in the ***BLUE*** investment.

Earning money in this experiment

You will be asked to make twenty-one (21) investment decisions like the example we have just discussed. At the end of the experiment, whatever money you have earned will be yours to keep.

As an example of how money is earned, assume that:

- tokens invested the ***RED*** investment pay $0.05 per token
- tokens invested in the ***BLUE*** investment begin at a value of $0.50 per token and decrease in value at a rate of $0.03 per token for every additional token invested.

You will make a decision about how to invest your ten tokens.

Example 1: If you invest 6 tokens in ***RED*** and 4 tokens in ***BLUE***, and the other members of your group combine to invest 3 tokens in ***BLUE***, then your earnings will be calculated as follows:

Each token in ***RED*** pays **$0.05**.

There are 7 tokens invested in ***BLUE*** in total, combining your decision with the rest of the group's decisions. Each token in ***BLUE*** begins at $0.50, and then for each token invested after the first one, decreases by $0.03 per token. So each token in ***BLUE*** pays $0.50 – 6 * ($0.03) = $0.50 - $0.18

In this case, each token in ***BLUE*** pays **$0.32**.

You earn $0.05 * 6 = $0.30 for your ***RED*** tokens, $0.32 * 4 = $1.28 for your ***BLUE*** tokens, so your total earnings for the round are:

$0.30 + **$1.28** = **$1.58**.

Example 2: If you decide to invest 2 tokens in ***RED*** and 8 tokens in ***BLUE***, and the other members of your group combine to invest 17 tokens in ***BLUE***, then your earnings will be calculated as follows:

Each token in ***RED*** pays **$0.05**.

There are 25 tokens invested in ***BLUE*** in total, combining your decision with the rest of the group's decisions. Each token in ***BLUE*** begins at $0.50, and then for each token invested after the first one, decreases by $0.03 per token. So each token in ***BLUE*** is worth $0.50 – 24 * ($0.03) = $0.50 - $0.72 = -$0.22.

Because this is less than zero, in this case, each token in ***BLUE*** pays = **$0.00**.

You would earn $0.05 * 2 = $0.10 for your ***RED*** tokens, $0.00 * 8 = $0.00 for your ***BLUE*** tokens, so your total earnings for the round are:

$0.10 + **$0.00** = **$0.10**

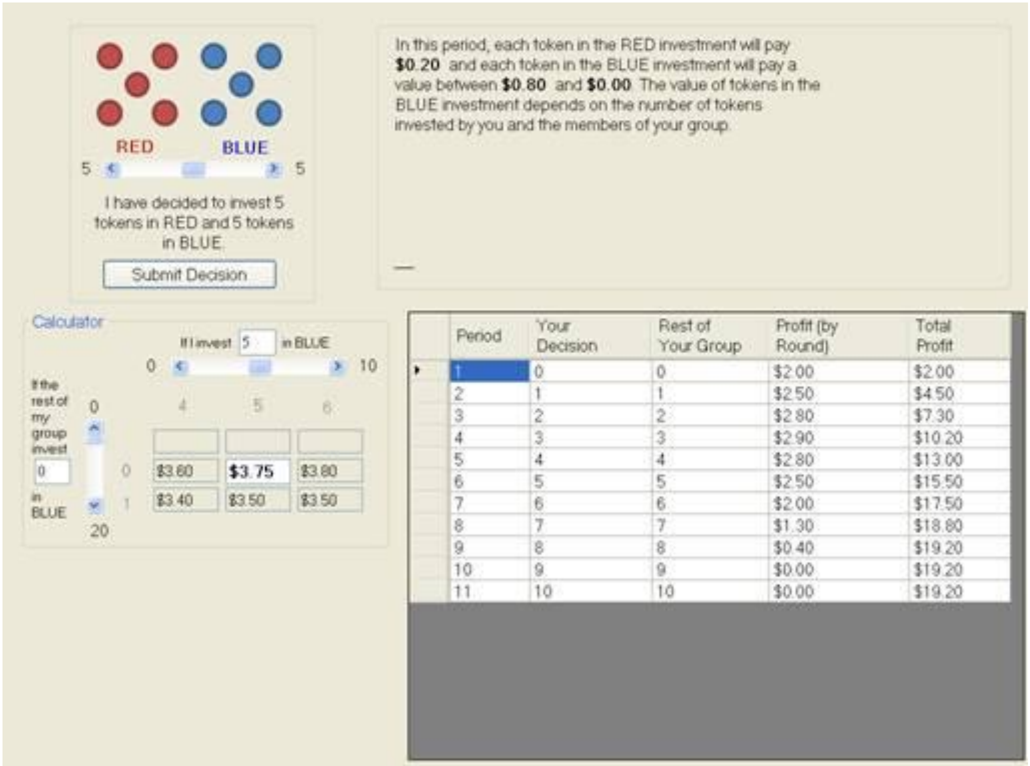
To figure out by hand how much each token will pay during the game can take a long time. To help you with this, a calculator is provided as part of the computer program. This calculator shows the amount you will earn, **assuming** that you invest a certain number of tokens in the ***BLUE*** investment and **assuming** that your group combines to invest a certain number of tokens in the ***BLUE*** investment. You will have an opportunity to practice using the calculator before you make any decisions that will determine your payment.

After each choice, the decision you have made and the decision the other members of your group have made will be tallied, and your earnings will be determined. You will be informed of your earnings for the round. You will then have an opportunity to review the decision you made, the decision made by the other members of your group, and your earnings for the round.

The Computer Interface

In the experiment, you will be making decisions on the computer screen. This section of the instructions will briefly introduce and explain the parts of the program. After you complete the instructions, you will have an opportunity to practice making decisions before any of your decisions will be counted for payment.

The screen you will see will look like the one below.



You will use the slide-bar in the upper left to decide how to invest your tokens. As you move the slider on the slide-bar, the tokens you see will change. In the image above, it says “I have decided to invest 6 tokens in ***RED*** and 4 tokens in ***BLUE***.” Use the slider to make your decision, and then click that button to submit your investment choice for the period.

Below the decision slider is the Calculator. The Calculator will tell you what your earnings for the period will be if you submit your decision, *depending on what the other members of your group decide*. As you move the sliders or enter numbers in the text boxes, the contents of the Calculator will change. In each case, the table will tell you what your earnings for the period will be under different choices by your group members.

In the example above, the Calculator is being used to predict what the profit would be for a decision of 4 tokens in BLUE, assuming that the rest of the group combines to invest 9 tokens in BLUE.

In the upper right corner, you will see messages that change depending on what you are currently doing. While you are making your decision, the message will tell you what the value of the tokens are. While you are reviewing your decision and earnings, the message will tell you what you earned in the round and what your total earnings are.

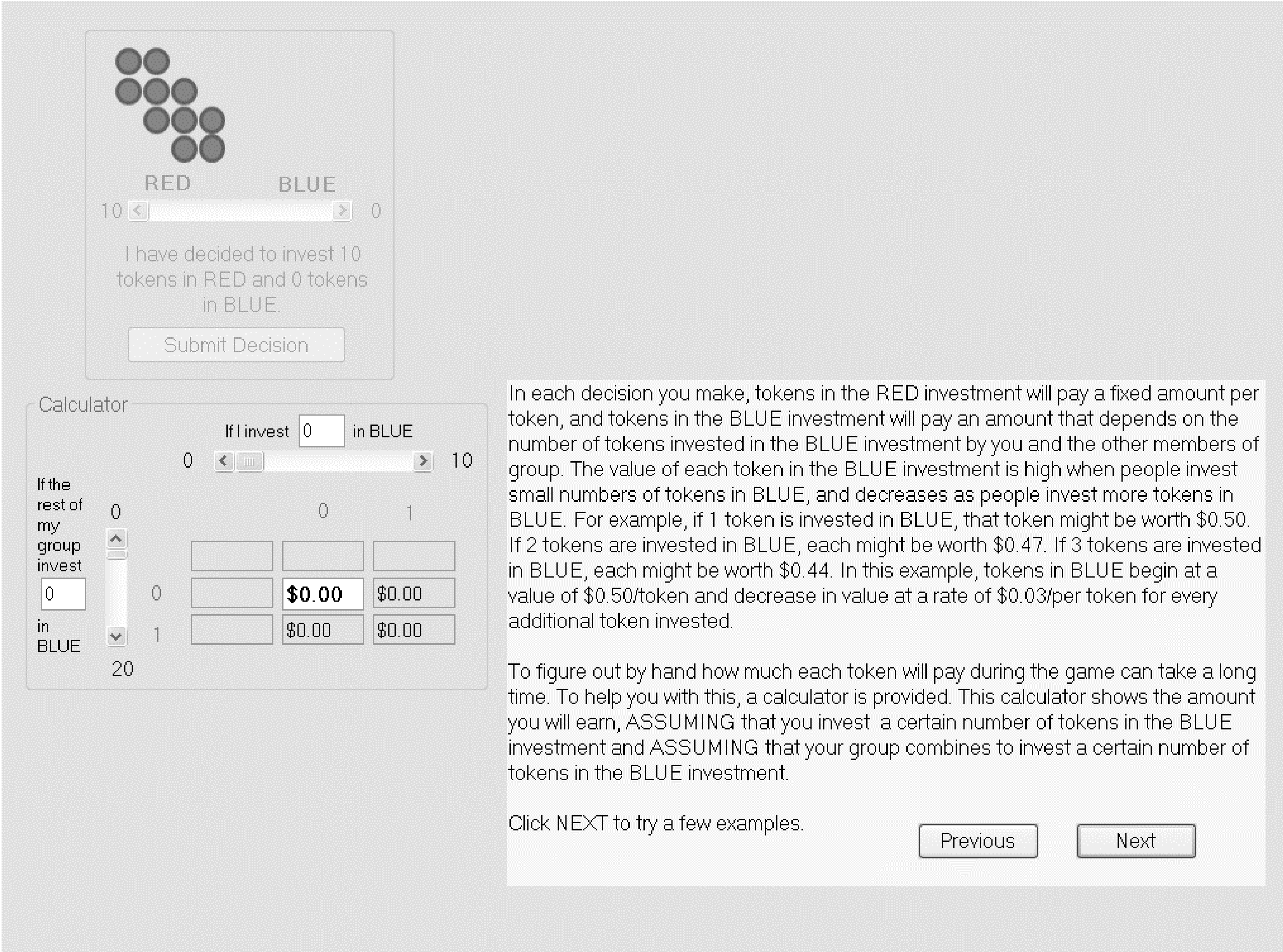
The table at the right of the screen contains the decisions you've made in previous rounds, your earnings for those rounds, as well as your total earnings.

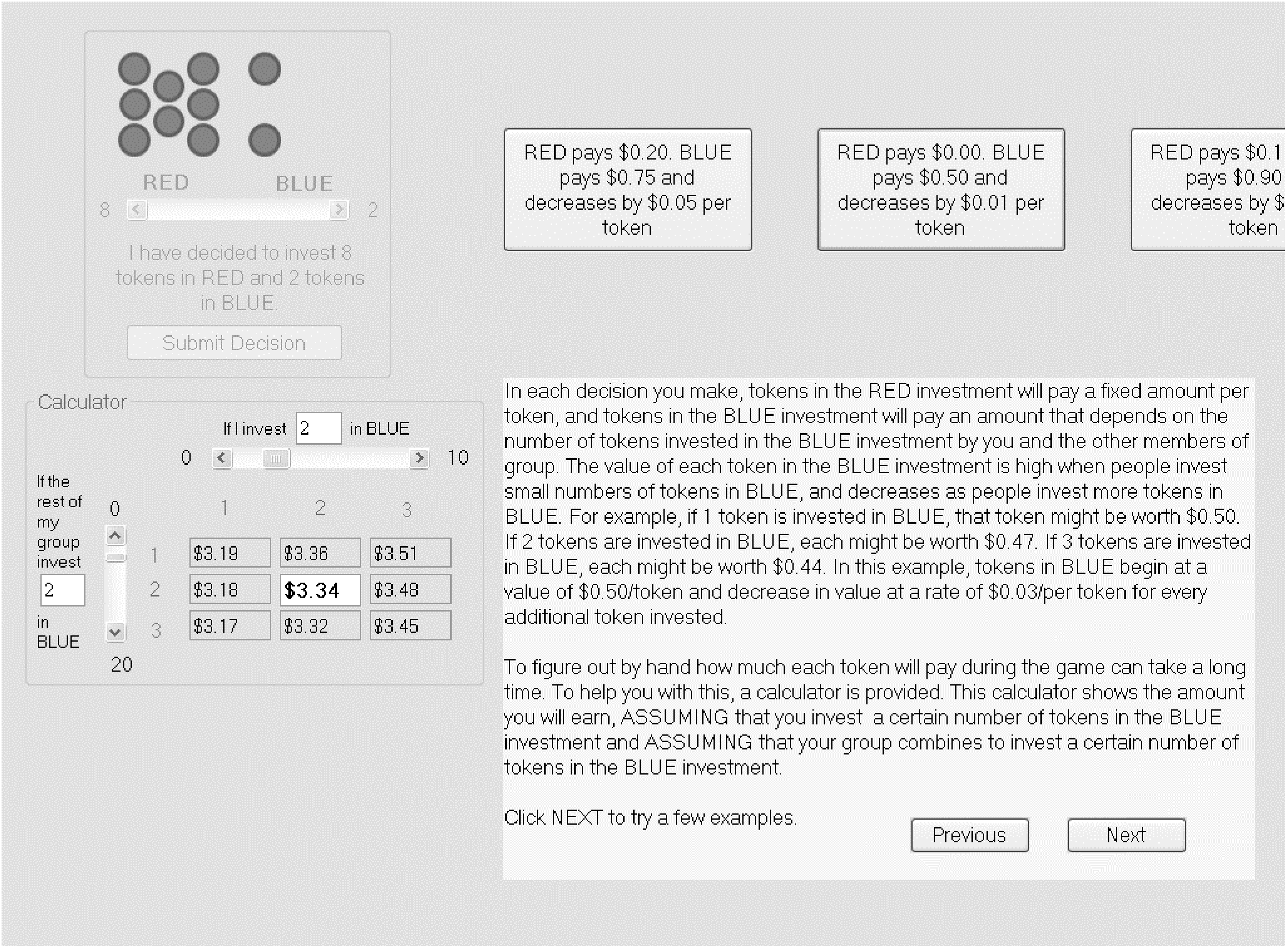
Questions

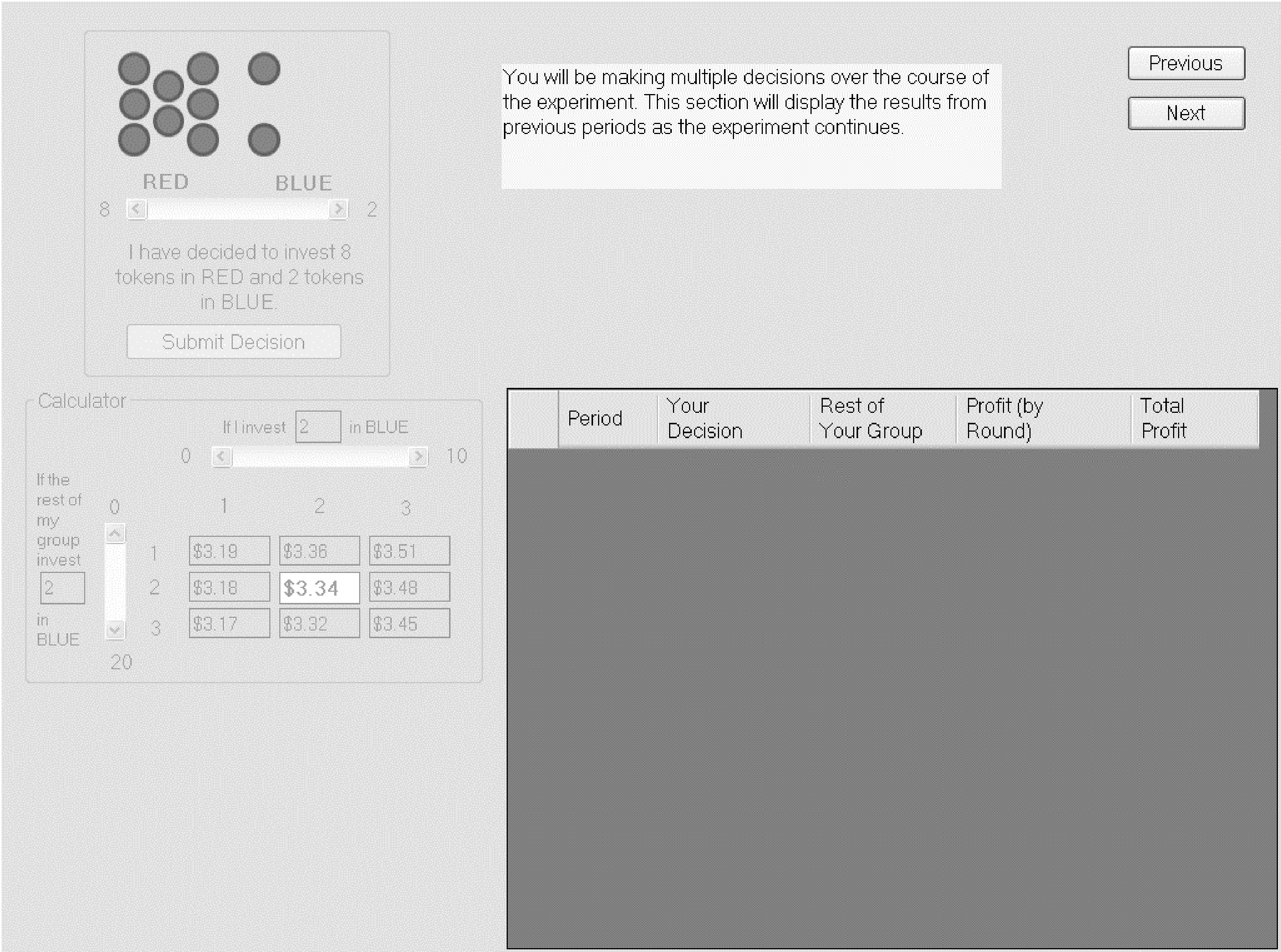
If you have any questions, please raise your hand and the experimenter will come by to answer your question privately.

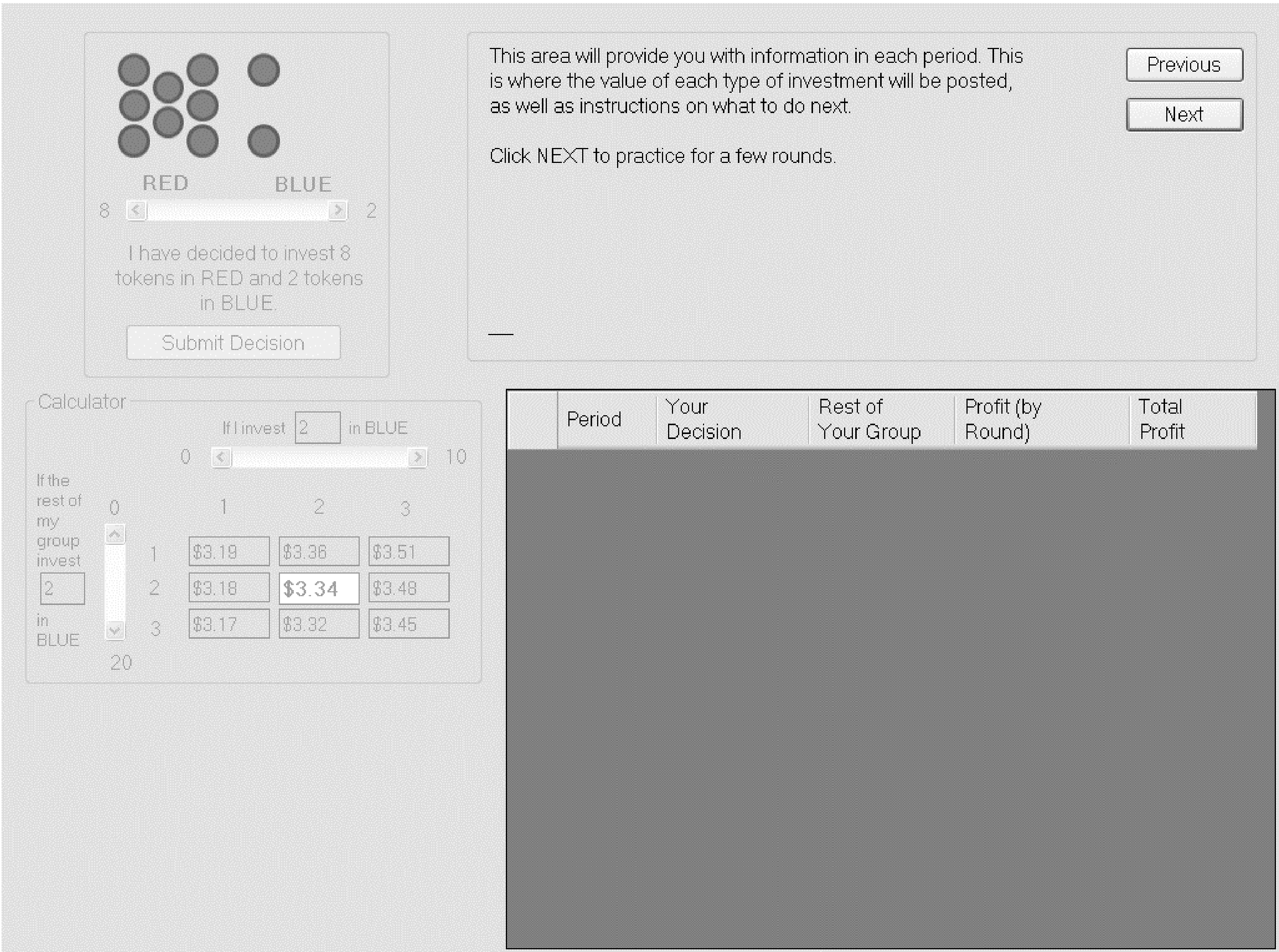
When you are finished reading these instructions, click OK below. Once you have finished reading the instructions, you will have an opportunity to practice using the computer screen.

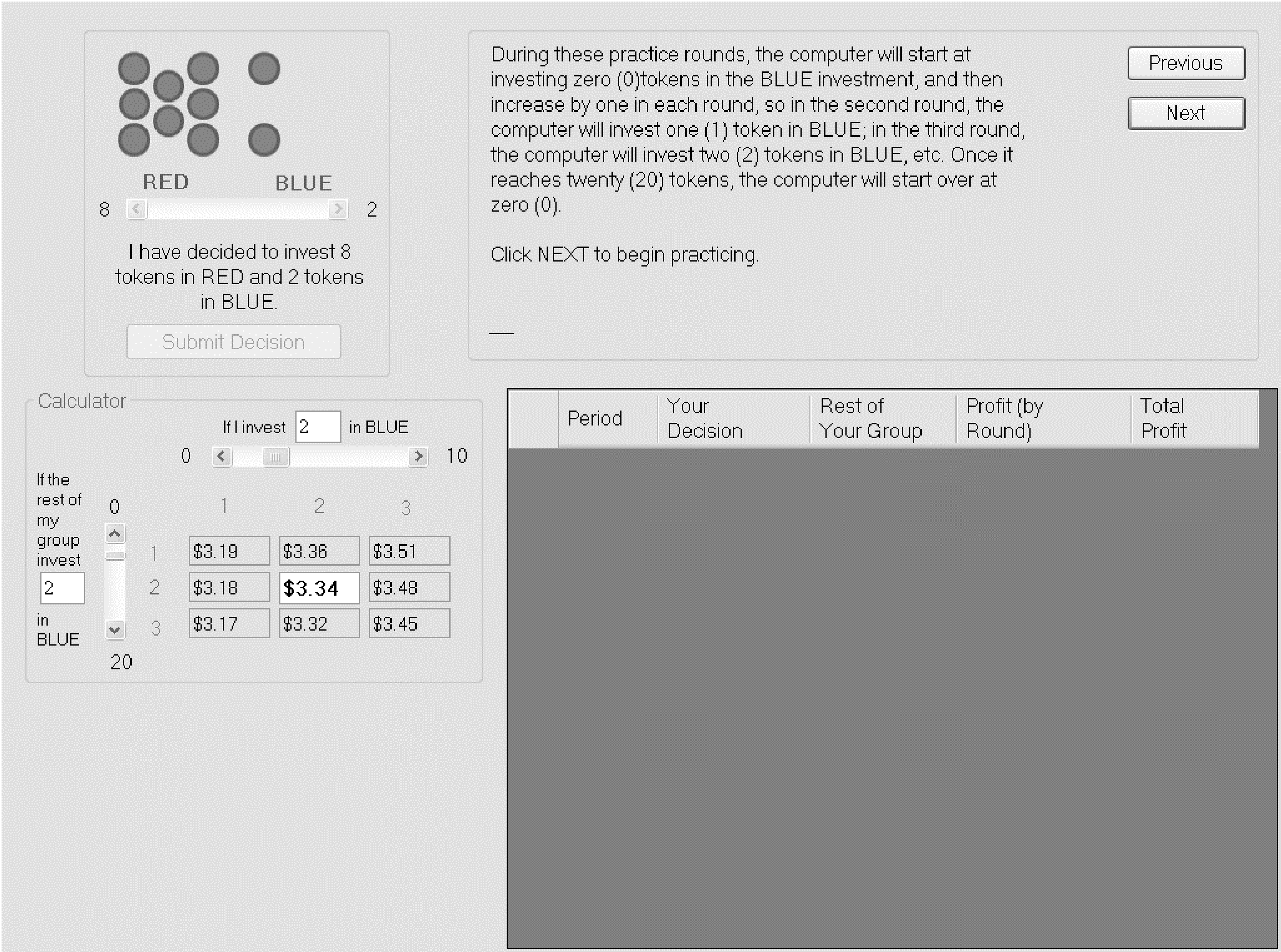
Appendix B: Tutorial Screenshots

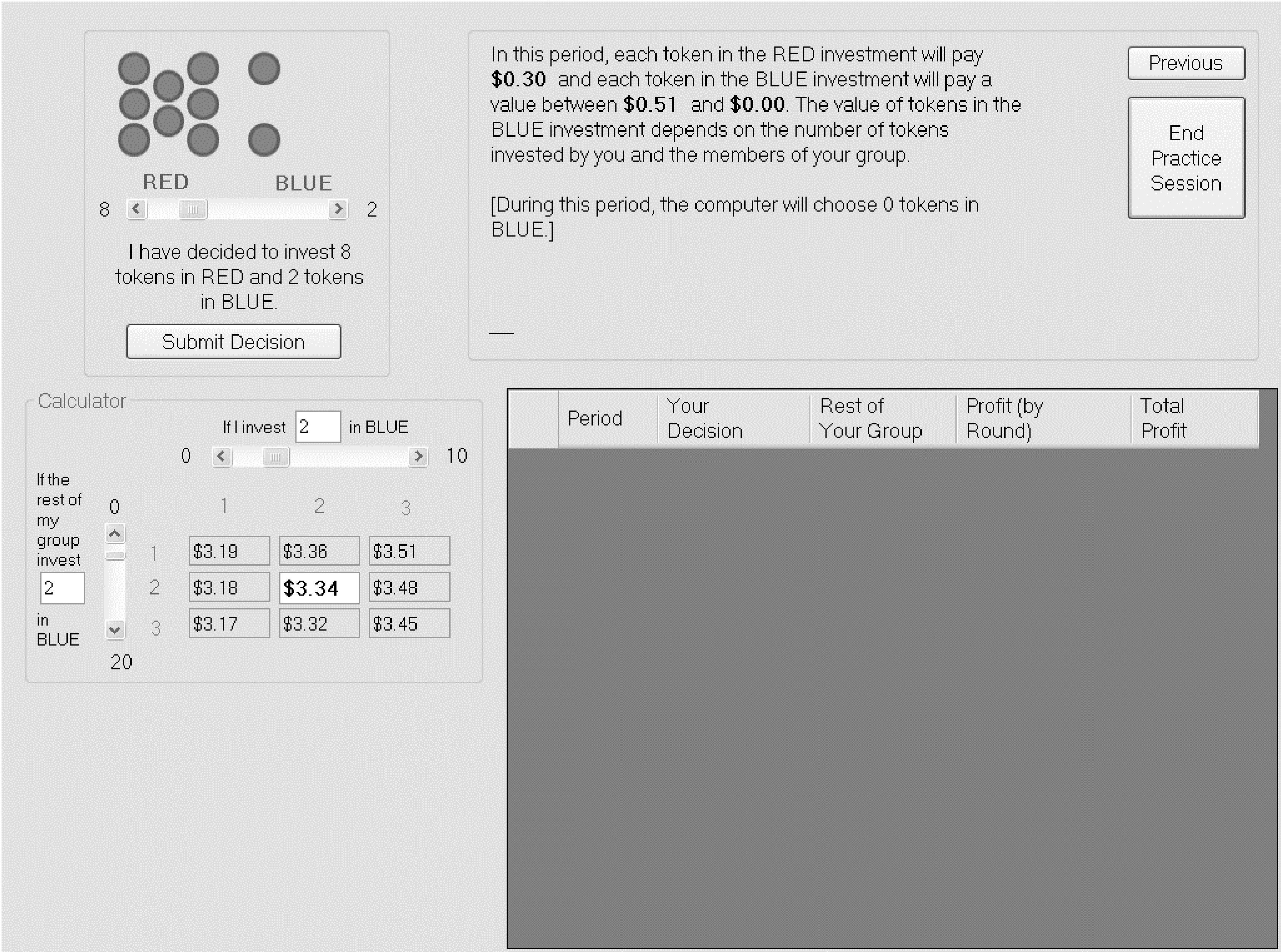

RED
BLUE
10
0
I have decided to invest 10 tokens in RED and 0 tokens in BLUE.
Submit Decision
Calculator
If I invest 0 in BLUE
0
10
If the rest of my group invest
0
in BLUE
0
20
0
1
0
1
$0.00
$0.00
$0.00
$0.00
In each decision you make, tokens in the RED investment will pay a fixed amount per token, and tokens in the BLUE investment will pay an amount that depends on the number of tokens invested in the BLUE investment by you and the other members of group. The value of each token in the BLUE investment is high when people invest small numbers of tokens in BLUE, and decreases as people invest more tokens in BLUE. For example, if 1 token is invested in BLUE, that token might be worth $0.50. If 2 tokens are invested in BLUE, each might be worth $0.47. If 3 tokens are invested in BLUE, each might be worth $0.44. In this example, tokens in BLUE begin at a value of $0.50/token and decrease in value at a rate of $0.03/per token for every additional token invested.
To figure out by hand how much each token will pay during the game can take a long time. To help you with this, a calculator is provided. This calculator shows the amount you will earn, ASSUMING that you invest a certain number of tokens in the BLUE investment and ASSUMING that your group combines to invest a certain number of tokens in the BLUE investment.
Click NEXT to try a few examples.
Previous
Next

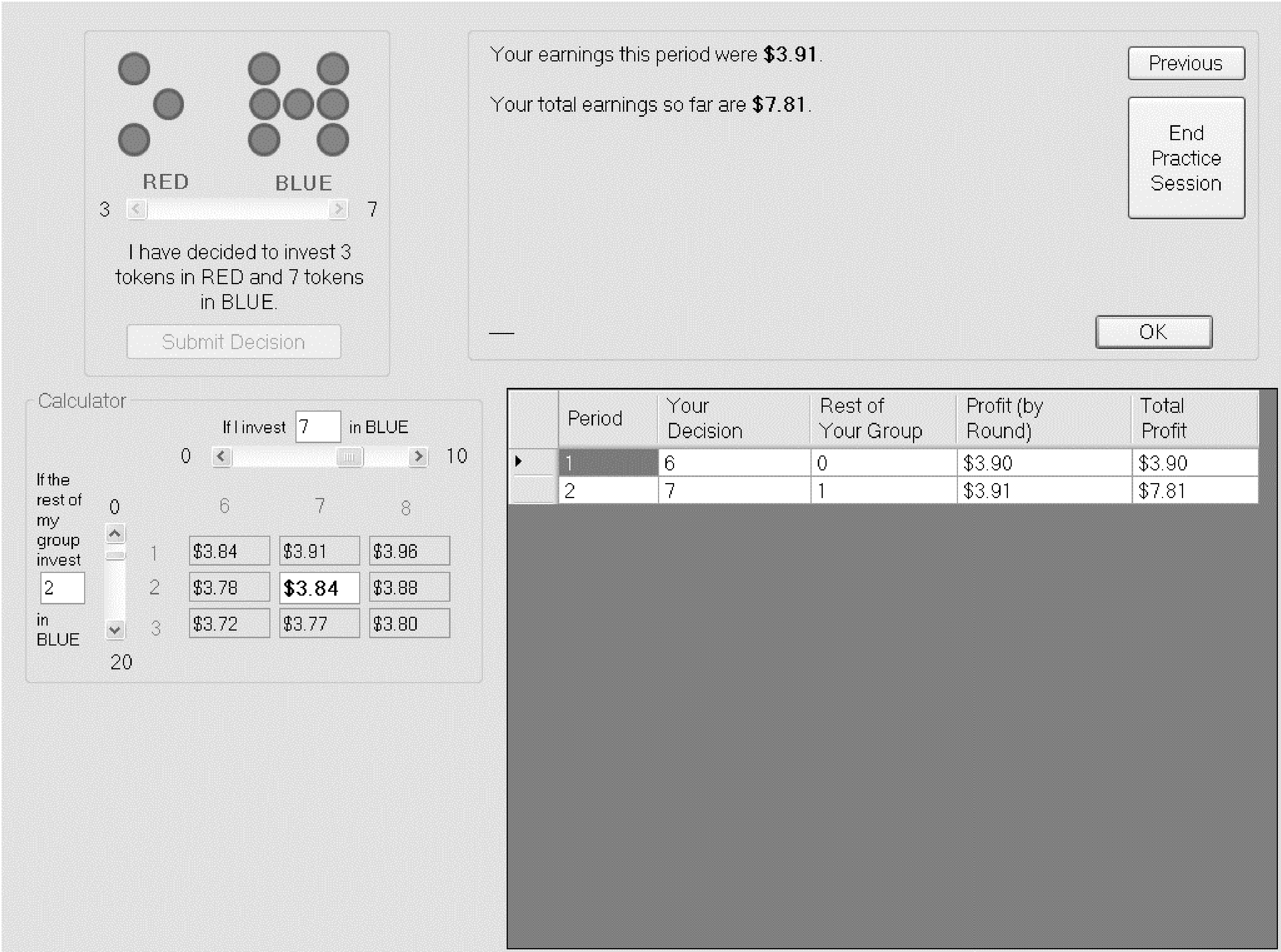

RED
BLUE
8
2
I have decided to invest 8 tokens in RED and 2 tokens in BLUE.
Submit Decision
RED pays $0.20. BLUE pays $0.75 and decreases by $0.05 per token
RED pays $0.00. BLUE pays $0.50 and decreases by $0.01 per token
RED pays $0.1 pays $0.90 decreases by $ token
Calculator
If I invest 2 in BLUE
0
10
If the rest of my group invest 2 in BLUE
0
20
1 2 3
1 $3.19 $3.36 $3.51
2 $3.18 $3.34 $3.48
3 $3.17 $3.32 $3.45
In each decision you make, tokens in the RED investment will pay a fixed amount per token, and tokens in the BLUE investment will pay an amount that depends on the number of tokens invested in the BLUE investment by you and the other members of group. The value of each token in the BLUE investment is high when people invest small numbers of tokens in BLUE, and decreases as people invest more tokens in BLUE. For example, if 1 token is invested in BLUE, that token might be worth $0.50. If 2 tokens are invested in BLUE, each might be worth $0.47. If 3 tokens are invested in BLUE, each might be worth $0.44. In this example, tokens in BLUE begin at a value of $0.50/token and decrease in value at a rate of $0.03/per token for every additional token invested.
To figure out by hand how much each token will pay during the game can take a long time. To help you with this, a calculator is provided. This calculator shows the amount you will earn, ASSUMING that you invest a certain number of tokens in the BLUE investment and ASSUMING that your group combines to invest a certain number of tokens in the BLUE investment.
Click NEXT to try a few examples.
Previous
Next

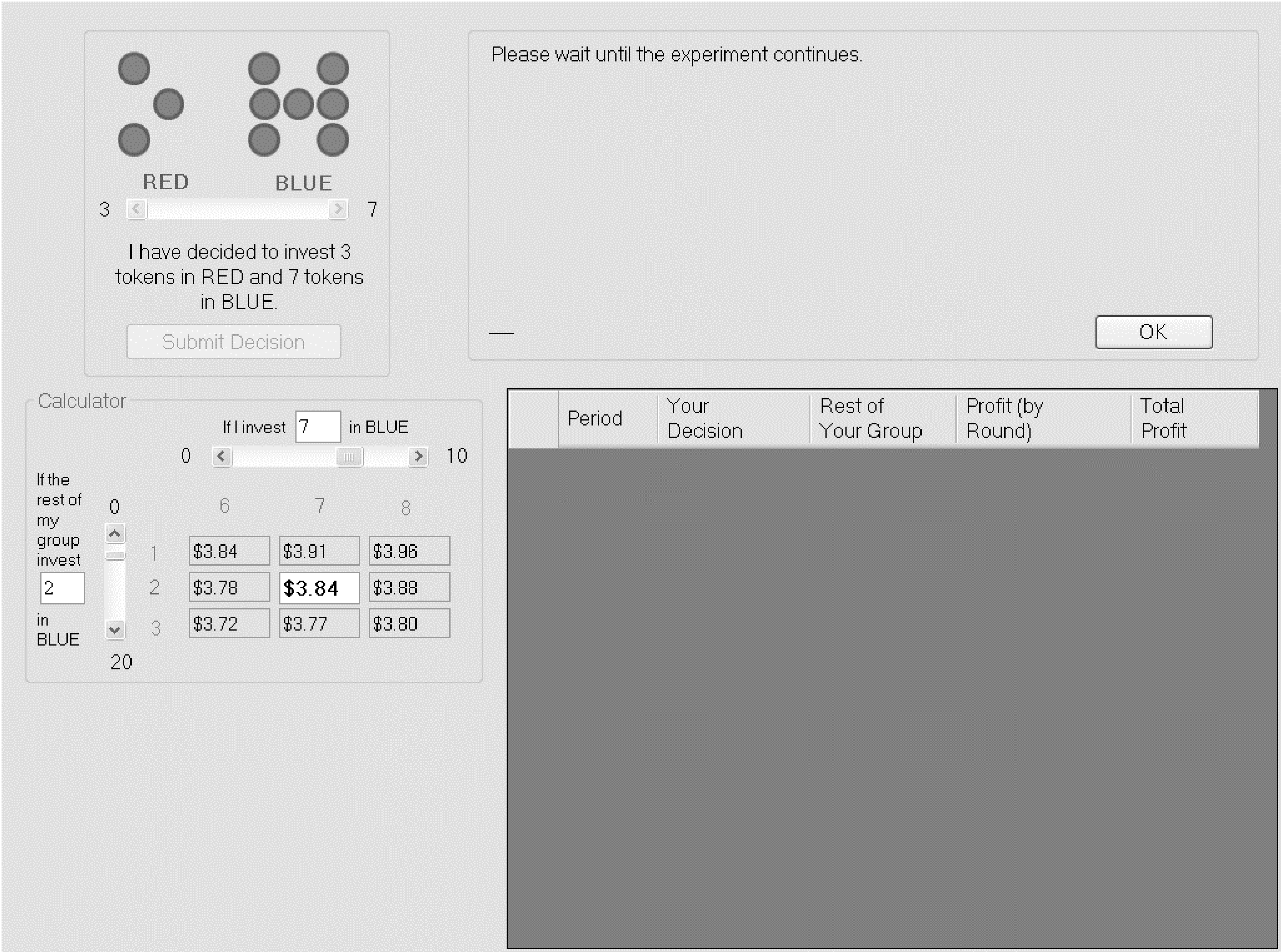

Previous
Next
You will be making multiple decisions over the course of the experiment. This section will display the results from previous periods as the experiment continues.
RED
BLUE
8
2
I have decided to invest 8 tokens in RED and 2 tokens in BLUE.
Submit Decision
Calculator
If I invest 2 in BLUE
0
10
If the rest of my group invest 2 in BLUE
0
20
1
2
3
$3.19
$3.36
$3.51
$3.18
$3.34
$3.48
$3.17
$3.32
$3.45
Period
Your Decision
Rest of Your Group
Profit (by Round)
Total Profit


RED
BLUE
8
2
I have decided to invest 8 tokens in RED and 2 tokens in BLUE.
Submit Decision
This area will provide you with information in each period. This is where the value of each type of investment will be posted, as well as instructions on what to do next.
Click NEXT to practice for a few rounds.
Previous
Next
Calculator
If I invest 2 in BLUE
0
10
If the rest of my group invest 2 in BLUE
0
20
1
2
3
$3.19
$3.36
$3.51
$3.18
$3.34
$3.48
$3.17
$3.32
$3.45
Period
Your Decision
Rest of Your Group
Profit (by Round)
Total Profit


RED
BLUE
8
2
I have decided to invest 8 tokens in RED and 2 tokens in BLUE.
Submit Decision
During these practice rounds, the computer will start at investing zero (0)tokens in the BLUE investment, and then increase by one in each round, so in the second round, the computer will invest one (1) token in BLUE; in the third round, the computer will invest two (2) tokens in BLUE, etc. Once it reaches twenty (20) tokens, the computer will start over at zero (0).
Click NEXT to begin practicing.
Previous
Next
Calculator
If I invest 2 in BLUE
0
10
If the rest of my group invest 2 in BLUE
0
20
1
2
3
$3.19
$3.36
$3.51
$3.18
$3.34
$3.48
$3.17
$3.32
$3.45
Period
Your Decision
Rest of Your Group
Profit (by Round)
Total Profit


RED
BLUE
8
2
I have decided to invest 8 tokens in RED and 2 tokens in BLUE.
Submit Decision
In this period, each token in the RED investment will pay $0.30 and each token in the BLUE investment will pay a value between $0.51 and $0.00. The value of tokens in the BLUE investment depends on the number of tokens invested by you and the members of your group.
[During this period, the computer will choose 0 tokens in BLUE.]
Previous
End Practice Session
Calculator
If I invest 2 in BLUE
0
10
If the rest of my group invest 2 in BLUE
0
20
1
2
3
1 $3.19 $3.36 $3.51
2 $3.18 $3.34 $3.48
3 $3.17 $3.32 $3.45
Period
Your Decision
Rest of Your Group
Profit (by Round)
Total Profit


RED
BLUE
3
7
I have decided to invest 3 tokens in RED and 7 tokens in BLUE.
Submit Decision
Your earnings this period were $3.91.
Your total earnings so far are $7.81.
Previous
End Practice Session
OK
Calculator
If I invest 7 in BLUE
0
10
If the rest of my group invest 2 in BLUE
0
20
6
7
8
1
$3.84
$3.91
$3.96
2
$3.78
$3.84
$3.88
3
$3.72
$3.77
$3.80
Period
Your Decision
Rest of Your Group
Profit (by Round)
Total Profit
1
6
0
$3.90
$3.90
2
7
1
$3.91
$7.81


RED
BLUE
3
7
I have decided to invest 3 tokens in RED and 7 tokens in BLUE.
Submit Decision
Please wait until the experiment continues.
OK
Calculator
If I invest 7 in BLUE
0
10
If the rest of my group invest 2 in BLUE
0
20
6
7
8
1
2
3
$3.84
$3.91
$3.96
$3.78
$3.84
$3.88
$3.72
$3.77
$3.80
Period
Your Decision
Rest of Your Group
Profit (by Round)
Total Profit