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Three Essays on the Search for Economic Efficiency

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THREE ESSAYS ON THE SEARCH
FOR ECONOMIC EFFICIENCY

By

JASON JAMES DELANEY

A Dissertation Submitted in Partial Fulfillment
of the Requirements for the Degree
of
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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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As I completed the following dissertation, I had the unique and irreproducible pleasure of working with some of my favorite people; my only regret is that the solitary work of a graduate student did not afford me more opportunities to work more closely with them. It is my hope that the luxurious existence of the tenure-track assistant professor will give me some opportunities to work with them again—if not then, well, there's always tenure.

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The second paper would never have been written if not for Jane Gravelle's insistence that I do the thing right. She pushed me to concern myself not with the feasible but with the ideal, and then find a way to make it happen—and that lesson will improve all the work I ever do. Jorge Martinez-Vazquez pushed me to improve the dissertation as a whole, and the second essay in particular. His policy focus and his expertise are an inspiration, and his sense of humor is one I always find refreshing.

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ABSTRACT

ESSAYS ON THE SEARCH FOR ECONOMIC EFFICIENCY

By

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July 2010

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The chapters of this dissertation examine efficiency failures in three areas of applied microeconomics: experimental economics, public finance, and game theory. In each case, we look at ways to resolve these failures to promote the public good.

The first chapter, "An Experimental Test of the Pigovian Hypothesis," looks at two different policies designed to reduce congestion in a common-pool resource (CPR). The predictive power of game-theoretic results with respect to an optimal subsidy in a common-pool resource game remains an open question. We present an experiment with training and a simplified decision task, allowing more tractable computerized CPR experiments. We find that subject behavior converges to the Nash prediction over a number of periods. A Pigovian subsidy effectively moves subject behavior to the pre-subsidy social optimum. Finally, we find a significant but non-persistent effect of information provision in moving subjects toward the social optimum.

The second chapter, "Apples to Apples to Oranges," looks at efficiency and equity failures across states resulting from public expenditure. The literature on fiscal equalization and horizontal equity has established that measures of fiscal capacity should be complemented by measures of fiscal need: the ability of a sub-national government to

provide services given an average level of revenue. This chapter introduces an extension of the Representative Expenditure System that uses regression methods and both state and metropolitan statistical area (MSA) level data, allowing for comparability of input costs, service requirements, and levels of need. The regression-based results are robust across state- and MSA-level formulations, although state-level approaches overestimate need for larger, less populous states. All regression-based results diverge from previous workload-based approaches.

The third chapter, “Evading Nash Traps in Two-Player Simultaneous Games,” looks at efficiency failures in two-player simultaneous games. In some important games, Nash equilibrium selects Pareto-inferior equilibrium profiles. Empirically, Nash equilibrium sometimes performs poorly when predicting actual behavior. Previous approaches rely on repetition or external correlation to support efficient outcomes in simultaneous games. This chapter presents two new concepts: “détente” and “no-initiative,” in which players consider their own strategies and other-best-responses. We discuss their efficiency and descriptive properties across a set of simultaneous games.

Chapter I: An Experimental Test of the Pigovian Hypothesis

Introduction

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 whether, given the economic and political costs of introducing such a policy, there are 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 policies 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 making 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 subjects over-consume by more than the Nash prediction, while Budescu et al. (1995) also 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 down nonetheless establishes 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 (Isaac and Walker 2003).

Perhaps theory and behavior diverge due to other-regarding preferences. The effects of these preferences on both predicted behavior and optimal policy depend greatly upon how 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)[1].

Another reason equilibrium predictions might fail could be the difficulties present in modeling the situation experimentally. In practice, creating congestion 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 an overcongested—and privately optimal—level of consumption. This has the side effect of reducing the incentive to think very hard about it at the margin, because the marginal return to social payoff is closest to zero at the social optimum and the marginal private return is closest to zero at the overcongested level of consumption. Because of the payoff structure, determining the optimal strategy can be difficult, which may cause Nash

[1] Briefly, the intuition for pure altruism derives from the assumption that the utility from own-consumption is "larger" than the external utility effect through altruism, and for large populations, the difference in the tax approaches zero. For impure altruism, there is merely an additional utility advantage to reducing own-consumption, but the difference between private and social impact is unchanged. In the case of paternalistic altruism, marginal damages are increased by the extent of the paternalism, and the tax should thus be higher.

predictions to perform poorly. If subjects are confused or frustrated, they may simply (and rationally) decide not to think too hard about it. In one treatment, OGW allow (and record) communication, and note that in some of their experiments, this lack of dominance appears to be a problem. When 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."

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 our 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. We 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 we 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.

Theory

The theory of limited-access common-pool resources is a standard in public finance, and environmental, urban and regional economics. The 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 net MSB and net MPB 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, we 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). 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[2]:

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

To help subjects determine their payoffs, the software provides a payoff calculator that allows subjects to examine 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, we add an additional fixed per-token amount (δ) to the return to the outside option.

Proposition. Define the payoff function for individual i as:

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

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

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.[3]

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 $\delta^* = (\beta - \alpha)\left(\frac{n-1}{2n}\right)$.[4]

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[2] This is similar, but not identical, to the payoff function used in OGW (although the solutions are the same). 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.

[3] 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.

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 $\left[\beta - \gamma\left(\Sigma x_j + 1\right)\right]$ – $(\alpha + \delta)$ – γx_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 $\beta - \gamma\left(\Sigma x_j + 1\right)$ for the $(x_i + 1)^{\text{th}}$ unit, carries 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 (fishing or driving congests own-consumption as well), 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: MSB < MPB for $\Sigma x_k \neq 0$.

[4] For the purposes of this experiment, we are 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

Protocol

Because the impact of social norms and framing seems non-trivial, we avoid terminology like “common-pool resource,” “extraction,” “appropriation,” “tax,” “subsidy,” etc. We follow Andreoni and 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.

We 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 we use in the experiment, we 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.[5] 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 information provision 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 (2006), 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.

[5] 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. We exclude data from that group; inclusion does not affect the qualitative results.

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 and one of two experimental treatments: either the Pigovian subsidy treatment or 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, 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.

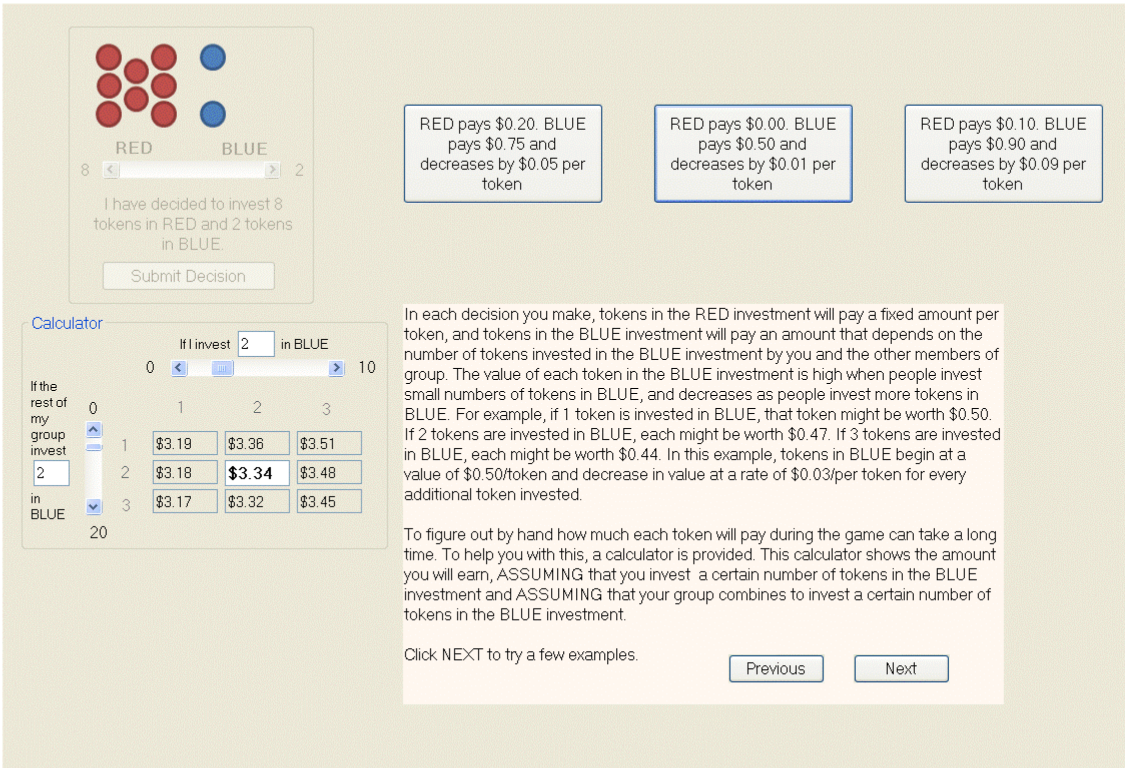

Figure 2. Tutorial screenshot
See Appendix B for screenshots of the full tutorial.

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. We collected data on the number of practice rounds each subject chose to use.

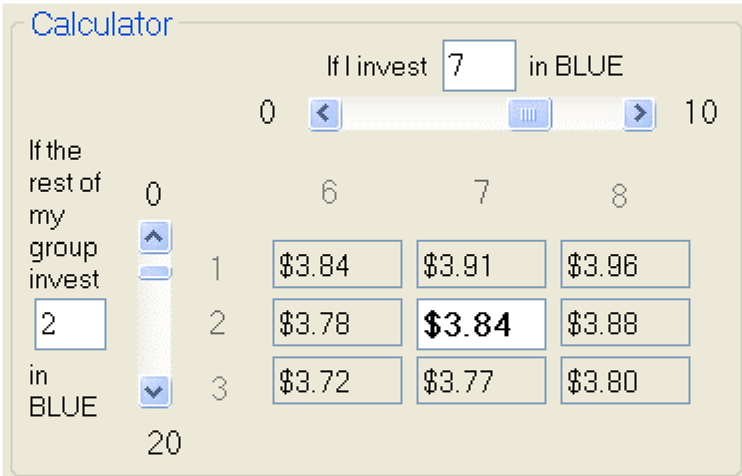

Figure 3. Payoff calculator

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.

Hypotheses

The primary hypotheses of interest are as follows ($\bar{x}$ 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, we test a number of other hypotheses regarding subsets of the data to try to get a more accurate picture of subject behavior. We 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.

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. We 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.

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

Table 1. Statistical tests of hypotheses and robustness checks

Hypothesis	Reject?	Wilcoxon test	K-S Test[6]
H_1: Session 1 Baseline = Session 2 Baseline	Reject	rank-sum Z = 3.665, p = 0.002	D = 0.4464, p = 0.017
H_2: Session 1 Baseline 1 = Session 1 Baseline 2	Reject	matched-pairs sign-rank Z = -2.575, p = 0.010	D = 0.5238, p = 0.006
H_3: Session 2 Baseline 1 = Session 2 Baseline 2	Reject	matched-pairs sign-rank Z = -3.002, p = 0.003	D = 0.3333, p = 0.093
H_4: Session 1 Baseline = 9	Cannot reject	sign-rank Z=-0.863, p =0.388	
H_5: Session 1 Treatment = 6	Cannot reject	sign-rank Z=-0.233, p =0.816	
H_6: Session 1 Baseline = Session 1 Treatment	Reject	matched-pairs sign-rank Z = -3.002, p = 0.000	D = 0.857, p = 0.000
H_7: Session 2 Baseline = 9	Reject	sign-rank Z=-6.714, p =0.000	
H_8: Session 2 Baseline = Session 2 Treatment	Cannot reject	matched-pairs sign-rank Z = 0.729, p = 0.466	D = 0.125, p = 0.975
H_9: Session 2 Baseline 2 = Session 2 Treatment	Reject	matched-pairs sign-rank Z = 3.211, p = 0.001	D = 0.25, p = 0.347
H_{10}: Session 2 Mid-Baseline = Session 2 Treatment	Marginal rejection	matched-pairs sign-rank Z = 1.416, p = 0.157	D = 0.1667, p = 0.815
H_{11}: Session 2 Baseline 2 = Session 2 Treatment (detrended)	Cannot reject	matched-pairs sign-rank Z = 0.743, p = 0.458	D = 0.1667, p = 0.820
H_{12}: Session 2 Mid-Baseline = Session 2 Treatment (detrended)	Cannot reject	matched-pairs sign-rank Z = 0.972, p = 0.331	D = 0.1667, p = 0.834
H_{13}: Session 2 Baseline = Session 2 Period 8	Reject	matched-pairs sign-rank Z = 2.258, p = 0.024	D = 0.375, p = 0.047
H_{14}: Session 2 Mid-Baseline = Session 2 Period 8	Reject	matched-pairs sign-rank Z = 2.733, p = 0.006	D = 0.417, p = 0.020
H_{15}: Session 2 Baseline = 9 (random-effect tobit model)	Cannot reject	Wald test $\chi^2(1) = 0.02$, p = 0.896	

[6] Where appropriate, we use a boot-strapped (10,000 iteration) 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, forthcoming) We use this test as a robustness check.

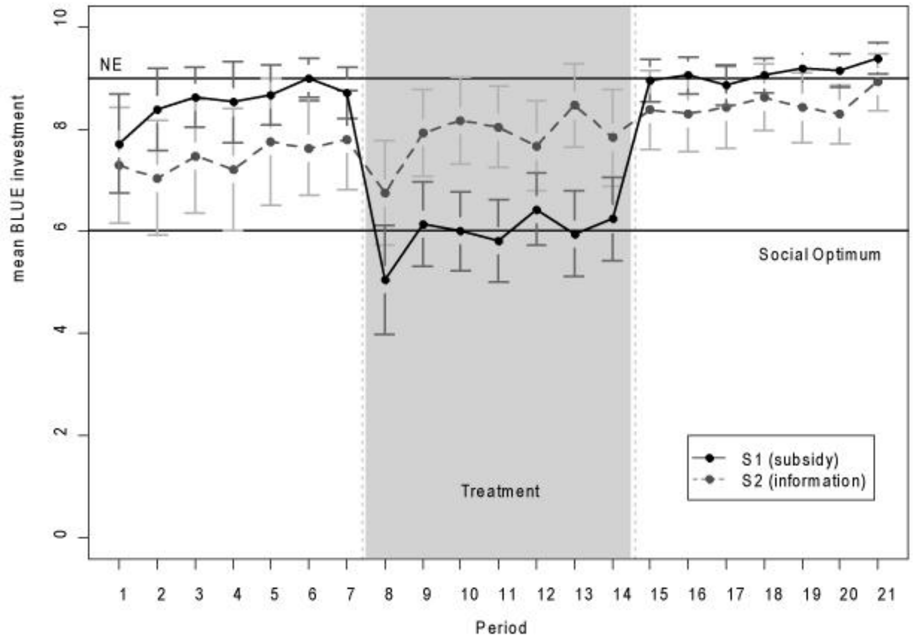

Figure 4. Mean BLUE investment by period by session
(NE line indicates Nash equilibrium prediction without subsidy)

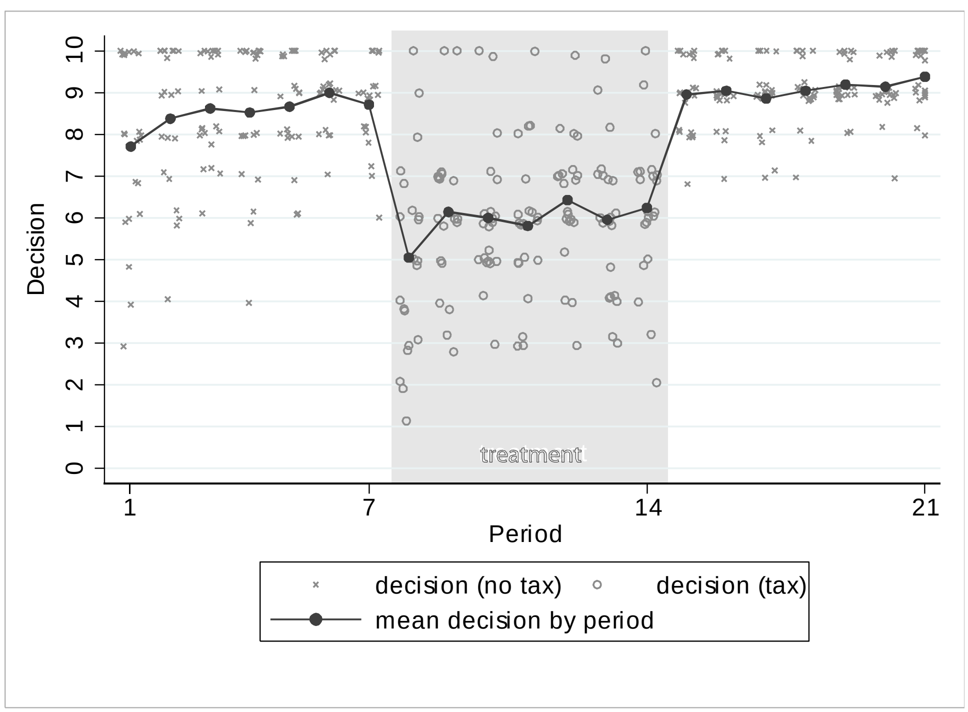

Figure 5. Investment decisions by period, Subsidy Session

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 second baseline, which we will refer to as the "mid-baseline." Using the mid-baseline has a few advantages: we expect some of the noise of experimentation and learning has dissipated by period 5, 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.

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 dispersion of decisions in 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.

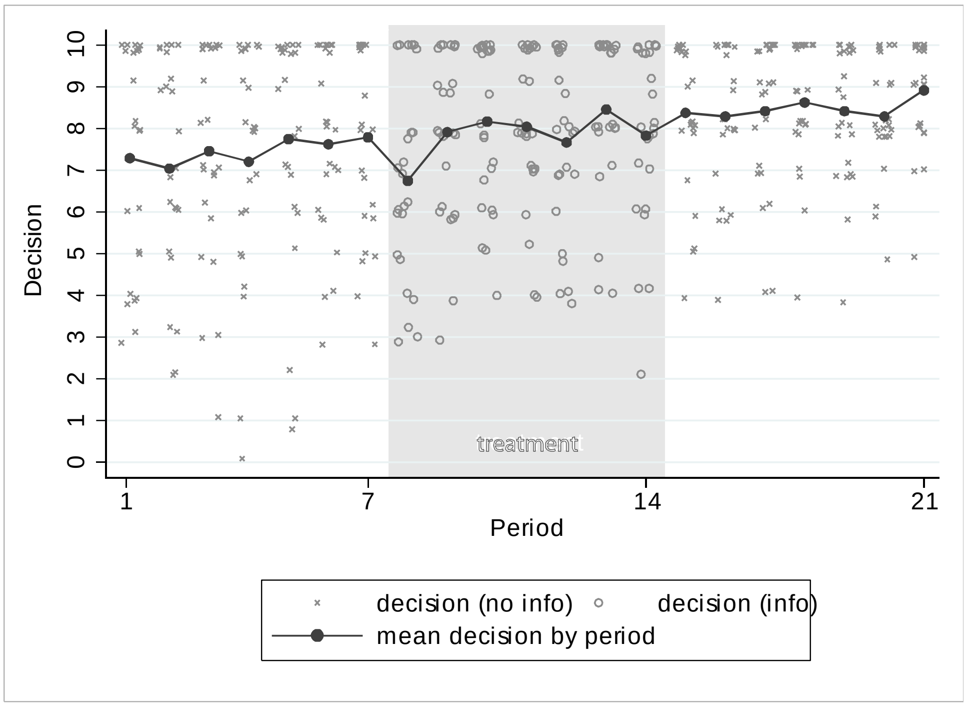

Figure 6. Investment decisions by period, Information Session

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}, H_{12}).

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}, H_{14}).

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 overcongestion 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. First, these are subjects in the Information session. Second, the decisions under the treatment occur during the middle seven periods. Finally, only those who were members of groups whose combined investment in the previous period exceeded the socially optimal level of investment received advice to decrease their investment. For a basis of comparison, we can consider decisions that meet the first and third criteria as “candidates for treatment.”

Considering all periods in session 2, we cannot reject the null of no effect of the Decrease treatment (Table 2). When comparing against the mid-baseline, 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.

Table 2. Tests of the effect of the Decrease treatment

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	

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 “candidates” 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.

Table 3. Regression results under different specifications

	Dependent variable is number of tokens invested in the CPR										
information[a]	-0.247*	-0.277*	-0.891*	-0.188	-0.178	-1.303***	-0.249*	-0.231	-1.186**	-1.257*	-0.422*
	(0.089)	(0.080)	(0.083)	(0.234)	(0.321)	(0.010)	(0.087)	(0.135)	(0.044)	(0.090)	(0.057)
candidate	0.654**	0.916**	0.402	0.426**	0.416*	0.326	0.720**	0.667*	0.68	0.654	1.080***
	(0.020)	(0.013)	(0.205)	(0.027)	(0.077)	(0.234)	(0.018)	(0.080)	(0.331)	(0.487)	(0.000)
subsidy	-2.323***			-2.494***			-2.477***				-2.551***
	(0.000)			(0.000)			(0.000)				(0.000)
period				0.0570***	0.0713***	0.121	0.0554***	0.0683***	0.0562		0.0590***
				(0.000)	(0.004)	(0.191)	(0.000)	(0.000)	(0.604)		(0.000)
Constant	7.830***	7.224***	7.163***	7.367***	6.817***	6.672***	8.021***	7.374***	6.869***	7.725***	8.369***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.003)	(0.000)	(0.000)
Lags?	N	N	N	N	N	N	Y	Y	Y	N	Y
σ^u										2.636***	1.186***
										(0.000)	(0.000)
σ^e										2.274***	1.500***
										(0.000)	(0.000)
Specification	Fixed-effects OLS, standard errors clustered on group						Fixed-effects OLS, heteroskedasticity-robust standard errors			Panel Tobit with bootstrapped standard errors	
Data subset[b]	Full	Session 2	One-shot	Full	Session 2	One-shot	Full	Session 2	One-shot	One-shot	Full
Observations	900	480	168	900	480	168	855	456	144	168	855
R-squared	0.346	0.03	0.036	0.384	0.097	0.052	0.501	0.228	0.132		
Number of id	45	24	24	45	24	24	45	24	24	24	45

[a] Significance of the coefficient on "information" represents a test of the null hypothesis that information about a decrease in CPR investment had no effect.

[b] "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.

p-values in parentheses. *p < 0.10 **p < 0.05 ***p < 0.01

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-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.[7]

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

[7] 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.

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.

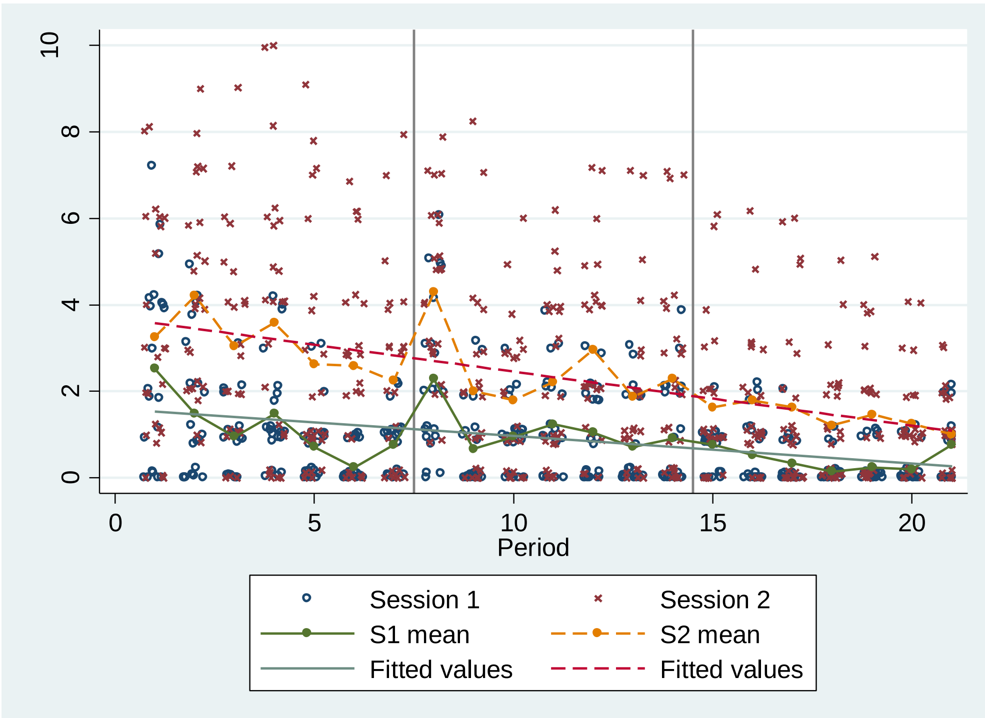

Figure 7. Absolute deviation from best-response by period by session
(with population means and lines of best fit)

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. We 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, we 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 that “pro-social” messages have an effect on water use. The information treatment used here primarily provides 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 useful in extending external validity to more closely represent naturally occurring common-pool-resources.

Finally, we 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.

Chapter II: Apples to Apples to Oranges
Fiscal Need in the United States in a Regression-Based Representative Expenditure Approach

Introduction

Fiscal need is a measure of the ability of a sub-national government (SNG) to provide an average level of services with an average level of revenue. The level of services required of U.S. states has grown over the last fifty years, without reference to the differing abilities of the states to keep up with these requirements.[8] The latter half of the twentieth century witnessed the steady advance of minimum standards of public service provision, motivated both by local public choice and by federal legislation.

Such laws, in general, have the potential to create efficiency gains. The federal government has the unique ability to internalize externalities at the national level, circumventing difficult public choice quandaries that can lead to pollution havens or interstate competition over fair labor standards, for example. While many programs have been designed and mandated at the national level, the fiscal apparatus required to implement them, including the primary source of funding, remains primarily a state and local phenomenon: national standards are not generally funded by the Federal Government (e.g. No Child Left Behind and the Clean Air Act). States face different challenges in complying with these standards. A state with a stiff wind blowing in off the coast may find it easier to comply with clean air standards, while a state with entrenched

[8] Since 1960, the share of GDP devoted to state and local public expenditure has nearly doubled from 11.6% of GDP in 1960 to 22.2% in 2010.

poverty and low levels of adult education may have a difficult time improving eighth-grade test scores.

A common standard with heterogeneous costs and needs leads to spending different amounts of money to provide a mandated level of public services. Implicitly, this results in redistribution of net fiscal burden (NFB) across states. The fiscal equalization literature notes that redistribution represents an opportunity to advance both equity and efficiency through equalization. By the same token, service provision standards without consideration of fiscal need can reduce both efficiency and equity.

The implications for policy have an upside: policy that accounts for this burden-shifting can improve efficiency and equity by eliminating the incentive to move for fiscal reasons. In principle, this means equalizing the NFB for each individual across SNGs. In practice, the policy goal has been to provide SNGs with the *ability* to do so by equalizing "fiscal comfort," or the ability of a SNG to provide an average level of services with an average level of *tax effort*, not revenue.

Measuring fiscal comfort involves measuring two dimensions: revenue-raising ability, or "fiscal capacity," and service-provision ability. Measuring fiscal capacity has proven to be more straightforward than measuring fiscal need for practical, theoretical, and analytical reasons, and as a result most equalization schemes are based on tax equalization.[9] This adjusts revenue as though per-capita expenditure need were constant within a country. When only tax treatment is accounted for in an equalization program, the equalization program may increase efficiency, but there remains an incentive to move to reduce one's NFB, and thus allocative inefficiencies remain (Boadway and Flatters 1982). Tax-based equalization leaves money on the table.

[9] Bird and Vaillancourt (2007) provide a good overview of the types of exceptions found in practice.

Empirical evidence indicates that ignoring fiscal need is economically significant. Wilson (2003) looks at Canadian migration data in response to Canada's capacity-based equalization program and finds significant efficiency gains in addition to the more straightforward equity improvements. Shah (1996) provides evidence on the size of the disparity that arises from excluding fiscal need in Canada's equalization program and finds that incorporating expenditure need in the measure of fiscal comfort leads to significant changes in the existing entitlements, nearly halving the net transfer out of Ontario and nearly doubling the net transfer out of British Columbia.

Given the potential gains from a fiscal-comfort approach, why do existing policies generally ignore fiscal need? There are several reasons. First, the concept of fiscal need can be politically unpalatable. As controversial as property value assessments can be, the idea of measuring a tax base is relatively straightforward. Asserting that higher levels of per-capita expenditure in one area are "necessary" or "just," while it advances equity in practice, may appear to violate the principle of equity.[10] In addition, this policy approach, like others, creates winners and losers relative to the status quo. Any change is likely to be met with resistance from those who lose from the policy change, even if net social welfare is improved.

Second, while the size of the tax base does not directly depend on preferences, the size and structure of public expenditures does, and so differentiating between idiosyncratic preferences for public goods and fiscal need must be done by assumption or

[10] For example, providing higher per-capita funds for schooling to a city with more children in poverty or higher teacher salaries may effectively provide the same level of service, but when making cross-jurisdictional comparisons, funding levels are easier to compare than service levels, and the inequality of funding levels may appear to be inequitable.

by government definition. As is often the case, our estimates must be qualified by these assumptions, or by the adequacy of government guidelines.

Finally, measurement of fiscal capacity runs into problems typical of measurement of stocks and flows of capital, while fiscal need measurement requires a more diverse set of variables: people and their possessions, the stock of existing public infrastructure, crime rates, public health measures—any major determinants of public expenditure.

Despite these challenges, the literature has sought to develop some good measures of fiscal need. The primary approaches to measuring both fiscal capacity and fiscal need were developed by the Advisory Committee on Intergovernmental Relations (ACIR). Mushkin and Rivlin (1962) developed the Representative Tax System (RTS) to measure fiscal capacity, and Rafuse (1990) introduced the complementary Representative Expenditure System (RES) to measure fiscal need. In both cases, the approach uses mean values across SNGs as the benchmark to which all SNGs are compared, and produces absolute levels of capacity or need as well as an index for comparison across SNGs.

The RTS approach has become well-established, but the RES approach has seen less use. Most noteworthy is the contribution of Robert Tannenwald, who produced a series of papers continuing and improving on Rafuse's RES approach (Tannenwald 1999; Tannenwald and Cowan 1997; Tannenwald and Turner 2004; Yilmaz, Hoo, Nagowski, Rueben and Tannenwald 2006). The existing work using the RES method has relied on Rafuse's original workload-based approach which, while informative and parsimonious with respect to data, relies heavily on assumptions in generating its estimates of fiscal need. Delaney (2007) looks at some of the difficulties faced when using workloads and

states as the unit of analysis and finds that the estimates are sensitive to variables selected for inclusion. Boex and Martinez-Vazquez (2007) note that "the technically most sophisticated techniques (notably local expenditure needs computed using a regression-based [RES])…quite possibly provide the best possible measures." (p. 329). If this is the best measure, why is it not more widely implemented? Bird and Vaillancourt (2007) provide some insight:

> Almost all who have studied the RTS-RES approach agree on two points: first, it is formally the most satisfactory way to meet the normative objectives of the theoretical equalization model, and, second, that it is difficult and costly to obtain the necessary data…, especially for expenditures. (284)

This is certainly the case in many of the countries Bird and Vaillancourt consider. Data in the United States is readily accessible, however, and these data make it possible to explore the differences between workload- and regression-based approaches, as well as the practical data requirements for improving regression-based estimates.

In addition, these representative approaches rely on the assumption that observed patterns of revenue and expenditure accurately capture decisions made by autonomous local governments in raising revenue and providing services to meet the needs of their constituents. To the extent that observed patterns instead represent structural inefficiencies from central control, discrimination across segments of society, or factor immobility, for example, both the RTS and RES would fail to correct for these historical problems. This is unlikely to be the case in the United States, but any implementation of an RTS-RES approach should consider these possible problems.

This paper represents several contributions to the literature. First, we introduce a "hybrid-regression" method of determining fiscal need. Using this method, we make use of data from U.S. economic sub-national units, which we define as MSAs (or CMSAs,

where relevant) or the surrounding rural areas (hereafter referred to as a group as "sub-state areas"). We produce per-capita need measures by sub-state area. Next we use this data to generate measures of fiscal need for SNGs, which include the fifty states and the District of Columbia. We thus contribute to the public finance and urban and regional economic literatures by producing the first MSA-level and sub-state-level measures of fiscal need for the United States as well as the first regression-based measures of fiscal need for states (including D.C.).[11] Finally, we produce estimates using other regression-based methods with different levels of data aggregation and different levels of data availability for comparison.

We find that estimates of need depend on the approach used to estimate need levels. These estimates differ greatly from previous workload-based approaches, consistent with previous comparisons of regression- and workload-based estimates (Boex and Martinez-Vazquez 2007). The preferred estimates are relatively robust across regression-based approaches, maintain a U-shaped trend with respect to population density, and accord more closely with actual expenditure than previous estimates.

Sub-state-level estimates also indicate that while the District of Columbia is an outlier among states, it is not unique among cities (Delaney 2007). Comparing measures developed with state- and sub-state-level data reveal high correlation. However, the use of state-level data significantly and systematically overestimates need in larger, less populated states, relative to the more disaggregated approach.

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[11] Previous regression-based RES estimates exist for the provinces of Canada (Shah 1996) as well as the local governments within the state of Georgia, USA, in 1960 (Boex and Martinez-Vazquez 2007).

Cost and Comparability in States and Cities

The RES method runs into several complications, which we refer to as comparability and cost. Figure 8 presents actual per-capita direct expenditures in the United States and illustrates the comparability issue: when developing relative measures and transforming them into absolutes, one must establish comparable jurisdictional units. The District of Columbia, Alaska and Hawaii provide obvious examples of idiosyncratic SNGs, although the same critique applies to many interstate comparisons. Looking at data from the 2002 Census of State and Local Government Finances, public expenditure varies across states, with direct per-capita expenditures in 2002 ranging from $4,746 in Arkansas to $10,802 in Alaska (Figure 8). In principle, actual expenditures should be positively correlated with expenditure need, but measuring disparity is difficult without accounting for heterogeneity.

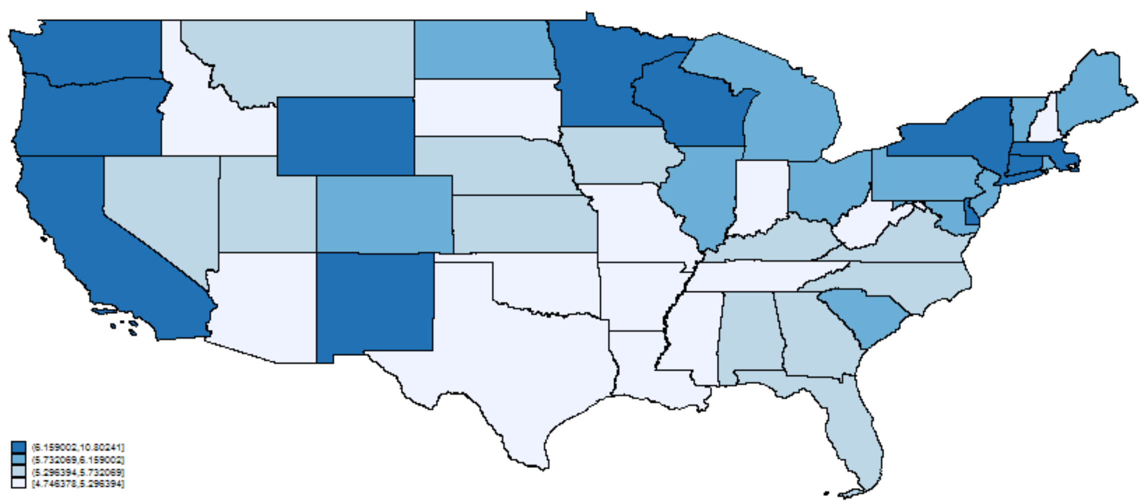

Figure 8. Per-capita state-and-local direct expenditure by state in 2002 ($1,000)

States vary widely across a number of dimensions, including land area, population, urbanization, land rents, industrial characteristics, input costs, prices of final goods, and age distribution. To the extent that expenditure need might vary in ways

correlated with these characteristics, treating the state as the unit of observation may lead to problems in the measurement of fiscal need.

The use of aggregated data reduces the cost of data acquisition but systematically changes inference. First, aggregation of data reduces information in an important way: if there are two areas, one with high wage or wealth inequality and the other with the same mean and median wage and wealth but a lower level of inequality, aggregation could make these two look similar and understate variation in fiscal need. In addition, to the extent that data availability exists at smaller units of observation, we can use more observations of disaggregated data to improve our estimation of fiscal need.

Most importantly, however, there is a sound theoretical reason in this case, in particular, to prefer MSA-level data. Labor markets tend to be urban labor markets, and as Arnott and McMillan (2006) note:

> Although well-known studies of local labor markets have used US states as their unit of analysis, few believe that Los Angeles and San Francisco are in the same labor market, let alone the agricultural areas of the San Joaquin Valley. Indeed, the San Francisco labor market may be more similar and more closely tied to labor markets in Boston and Seattle than to rural areas in northern California. (386)

To the extent that this is the case, the use of state wage levels may fail to account for input costs.

The large literature on human capital externalities confirms a concern with explicitly accounting for labor costs. Recent results indicate that labor cost differences may, in fact, be driven by differences in productivity (Glaeser and Maré 2001), especially for white-collar workers (Gould 2007), for whom voluntary mobility may be highest. Comparing cities that are similar to one another (or part of the same *national* labor

market) rather than state-level wage data may better account for input costs in the provision of public goods.

In addition, the determinants of need for public services may differ across heterogeneous areas, both in type and in quantity. Rural police expenditures, for example, might depend more on land area that police forces must cover than on the incidence of crime, while in urban areas, population density and poverty might be primary determinants of police expenditures. Similarly, transportation costs in education and hospital provision would likely be much greater in rural areas than in towns or cities. We can use data to try to account for this. Using the sub-state-level as the unit of analysis and allowing the effect of a given determinant on expenditure need to vary across "types" of places provides comparability of input costs and of the basket of public goods required in an area.

As is visible in Figure 9, actual public expenditures have a pronounced U-shape with respect to population density (here and elsewhere, the traditional RES results are taken from Yilmaz et al. 2006). This relationship is robust to the exclusion of outliers and to the scale under consideration—it holds for both states and sub-state areas. The optimal jurisdiction size literature provides some insight as to why this might be the case. Because of this robust and pronounced relationship, we categorize types of places ("clusters") by using an index of urbanization in our preferred approach. This allows for flexibility in the determinants of need across clusters. While this has intuitive and theoretical appeal, the soundness of this approach is ultimately an empirical question. If there is no systematic effect, a Chow test will prevent rejection of the null of equality of coefficients across clusters. In addition, we present results in which this flexibility is

removed in case favoring density over other factors in this way negatively affects our results.

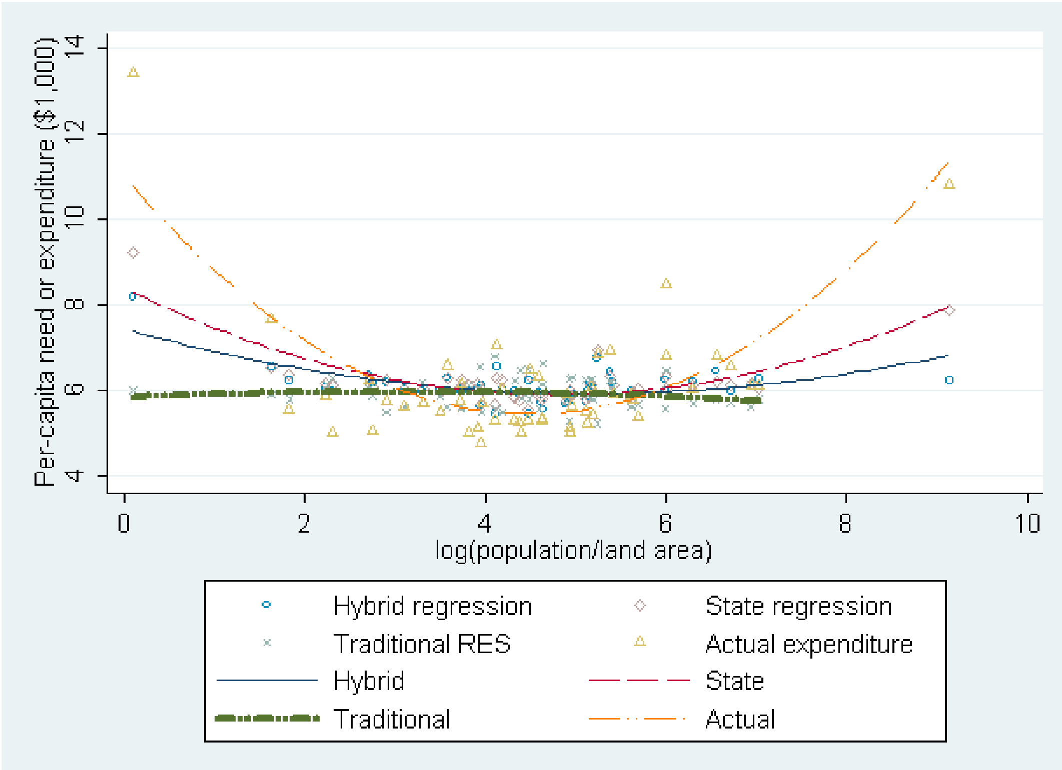

Figure 9. Measures of state expenditure need by state population density
Traditional RES data taken from Yilmaz et al. 2006.

A recurring critique of regression-based estimates of net fiscal benefit is that they suffer relative to other approaches because they are less transparent (McLarty 1997). It is not entirely clear that this is a disadvantage. In any case, as Shah (1997) notes, the potential for abuse is no greater than that of a RTS-based equalization program, in which assessment of tax bases and selection of tax rates are both explicitly determined by SNGs in many cases.

Several Methods

We look at seven different approaches; 4 at the state level and 3 at the sub-state level. This paper advocates for a “hybrid-regression” method, a method that shares features with two other sub-state-level-based regression approaches, the MSA-regression and the cluster-regression, both of which we will detail further below. The hybrid-regression method provides flexibility in both baskets of public services and input costs and makes it possible to correct for systematic revenue constraints in some areas. This preference over a pooled approach is supported by the data—using a Chow test, we can reject the null that public expenditure determination is homogenous across clusters at the $p < 0.001$ level—and the estimates are systematically different from those using state-level data.

Before looking at the relationship between estimates generated by the different methods, their theoretical differences warrant a brief discussion. In all regression-based approaches, we use data to separate expenditures into those parts determined by revenue-raising capacity, by idiosyncratic preference (by constituents or their government), and by fiscal need. We can then hold revenue and preference constant across economic units to come up with a measure of fiscal need by isolating expenditure related to need determinants.

Previous literature has used different approaches to account for heterogeneity. Shah (1996) fixes revenue measures at the national average (across Canadian provinces) and fixes coefficients across all provinces. Our state-based regression method essentially replicates this approach for the United States, using states and the District as units of analysis. Using states as the unit of analysis produces regression-based measures of

expenditure need directly comparable to workload-based measures outlined in Rafuse (1990) and succeeding papers, including a previous analysis of 2002 expenditure data (Yilmaz et al. 2006). The MSA-regression method uses sub-state areas as units of analysis, but replicates Shah's approach as well, with revenue measures and need coefficients fixed across provinces.

The cluster-regression and hybrid-regression methods rely on an index of urbanization to sort sub-state areas into clusters, but treat these clusters differently. The cluster-regression method treats each cluster as a separate population of data, allowing coefficients to vary across clusters and setting the average level of revenue at the within-cluster mean. The hybrid-regression method allows coefficients to vary across clusters, providing flexibility in the baskets of services and input costs across sub-state areas, but sets the average level of revenue at the national mean revenue.

These methods vary by the unit of analysis, by the constraints placed upon the basket of services and the scope of potential implied redistribution. The state-regression method relies on political place definitions, while the MSA-, cluster-, and hybrid-regression methods all use sub-state areas, which are perhaps more defensible economic units of analysis. The state- and MSA-regression methods hold the basket of services and input costs fixed across all sub-state areas, while the cluster- and hybrid-regression methods allow places that are substantially different in urbanization to have substantially different determinants of need. Finally, all methods use some average level of revenue as a baseline, and so the measures involve some implicit "redistribution." Total expenditure is the same, but the RES approach redistributes expenditure based on need. The cluster-based approach constrains redistribution to happen within clusters, while the state-,

MSA-, and hybrid-regression methods all allow redistribution to happen across the entire population, which would be preferable if cities or rural areas are needier than average in systematic ways, but are constrained in their ability to raise revenue. Table 4 illustrates the relevant differences across all the different approaches.

Table 4. Seven approaches to estimating fiscal need in the United States

Approach	Workload-based	Regression-based					
	Traditional	State	MSA	Cluster	Hybrid	Single	Barebones
Regressor selection process	Structural	State-level	Sub-state-level			State-level	Population and land area only
Capacity/preference indicator means	national mean state value	national mean state value	national mean sub-state value	Within-cluster mean sub-state value	national mean sub-state value	national mean state value	national mean state value
Regression coefficients restricted to equality	N/A	Across all states	Across all sub-states	Within cluster	Within cluster	Across all states	Across all states
Expenditure broken down by category?	Y	Y	Y	Y	Y	N	N
Total units of observation	51	51	323	323	323	51	51
Total number of regressions	0	7	7	63	63	1	1

Two other regression-based methods are presented. Throughout the RES literature, expenditure is broken down by major category before it is analyzed. In practice, this means that a system of at least seven equations (seven by nine clusters for sixty-three, sometimes) is estimated for the regression-based methods. The single-regression method replicates the state-regression method, but instead of using spending by category, it relies on a single regression of expenditure on capacity and determinants

of need to produce its results.[12] The barebones-regression method replicates the single-regression method, but uses only population and land area as determinants of need, to provide a true minimum performance improvement from a regression-based approach.

In addition to these four regression-based methods, we compare regression-based results to those generated by the traditional workload-based approach, in which determinants of need are selected beforehand and the index is constructed ex ante (here and elsewhere, traditional results are taken from Yilmaz et al. 2006). Some other data about the "correct" percentage of expenditure on elementary and secondary education, for example, is used, and a structural formula is determined with pre-assigned weights. This is a much less data-intensive but much more constrained approach and is sensitive to incorrect specification. In practice, this may be particularly useful for establishing a prescriptive expenditure norm, and its relationship with the regression-based approach may indicate the distance between the standards of service that are currently observed and those embodied in the structural formula.

In all cases, expenditure and capacity data is taken from the 2002 Census of State and Local Government Finances. Determinants of expenditure need and political preference are taken from the 2000 Census, from the 2000 FBI Uniform Crime Reports, and from the 1997 Economic Census.

While the approaches differ somewhat, the process itself is straightforward. We use a modified version of that laid out in Shah (1996) for the regression-based methods:

Step 1. Disaggregate expenditures into major functional categories.

[12] This may introduce aggregation problems, but represents the simplest case in terms of expenditure data collection, and illustrates the scope of such aggregation problems.

Step 2. For each expenditure category, select determinants of expenditure from several categories: those that determine fiscal capacity, those that exhibit an idiosyncratic preference for public services, and those that indicate a need for public services.

Step 3. Estimate influence on spending levels of both capacity/preference and need/cost indicators through regression analysis.

Step 4. Holding capacity/preference indicators constant at the mean, evaluate regression results for individual need/cost levels to construct per-capita expenditure need.

For the cluster-based approaches (cluster- and hybrid-regression methods), we modify the procedure as follows:

Step 2a′. Using an index of urbanization, sort sub-state areas into subgroups, or "clusters."

Step 3′. For each cluster, estimate influence on spending levels of both capacity/preference and need/cost indicators through regression analysis.

Step 4′. Holding capacity/preference indicators constant at an average level (cluster-level mean for cluster-regression, national sub-state-area mean for hybrid-regression), evaluate regression results for individual need/cost levels to construct per-capita expenditure need.

Step 5. Disaggregate expenditure need down to the county level, and aggregate back up to the state level.

What Determines Public Expenditures?

Following Rafuse (1990), total direct expenditure is broken down into seven categories:

- Elementary & Secondary Education (29.8% of total direct expenditure)
- Public Welfare (16.9% of total direct expenditure)
- Higher Education (10.1% of total direct expenditure)
- Health & Hospitals (8.9% of total direct expenditure)
- Highways (7.5% of total direct expenditure)
- Police (3.2% of total direct expenditure)
- and All Other Expenditures (29.7% of total direct expenditure)

For each expenditure category, determinants of need and of capacity are selected. We will report those selected for the cluster, MSA, and hybrid estimates; the set considered is large and available upon request. For the state regression-based estimates, the same procedure was used to select variables, as if more disaggregated data were unavailable and information on the relevance of a particular variable could only be inferred from state-level data. All variables are in per-capita terms (or percentages of the population) unless otherwise specified. In addition, all capacity measures are combined state and local unless otherwise specified. For sub-state areas, state and local measures are combined on a per-capita basis.

When constructing these indices, endogeneity bears particular consideration. The most important form of endogeneity derives from the fact that a number of the regressors might be justly considered either determinants of need or measures of idiosyncratic

preference.[13] While this may be a concern, we can examine the potential effect of the difficulty. Using the subset of disputable variables, we can consider them as purely determinants of need or purely elements of political preference. While these variables describe some of the variance, they do not seem to be driving the main results: the correlation between the sub-state-level regression-based expenditure need with and without the assumption that these variables represent preferences is .9838.

In light of the small magnitude of the likely effect, except for the case of the political party affiliation of the state Governor, all variables considered in these models are treated as determinants of either capacity or need.[14] In practice, it would fall to policymakers to decide on a case-by-case basis which variables are indicators of need and which indicate preference before doing any such analysis with the end goal of implementing a regression-based equalization scheme.

In addition to the problem of discriminating between need and preference variables, public expenditures and tax revenue are highly endogenous—most SNGs are required by law to maintain balanced budgets, and the data support revenues as the primary determinant of expenditures. Fortunately, for our purposes, the RES method for measuring fiscal need has the advantage of relying little on specific variable-by-variable causal arguments for its results. This reduced-form approach means that causal inference relying on the coefficients is inadvisable, but that the indices constructed will satisfy the

[13] For example, migrants from the Midwest may place a demand on highway expenditures because they are acclimated to an area with readily accessible highways, bring cars when they move, and thus demand highway expenditures in order to enjoy the use of their cars. Alternatively, they may be more likely than most to move to areas where manufacturing is an important sector of the economy. Manufacturing generally occurs in relatively diffuse places, and so the distribution of housing relative to the workplace may require longer commutes. Whether this is a “desire” for longer commutes or a “need” for longer commutes can be debated.

[14] The set of variables considered does not include many that clearly reflect idiosyncratic preferences. The inclusion of more elaborate measures of voter preference, including, for example, religious affiliation or outcomes of referenda, would be a useful avenue for future research on public expenditure determination.

goals of the method: providing a measure of the amount of public funds required to provide an average level of services within an area that is characterized by a particular set of observable variables.

For elementary and secondary education, the determinants of capacity are general own-source revenue, property taxes, individual income taxes, and state debt at the end of the year.[15] Determinants of need include the population between 5 and 17 years of age, the population attending private high school, the population with at least a bachelor's degree, and the number of households receiving some form of public assistance.

For public welfare expenditures, determinants of capacity include general own-source revenue, long-term debt outstanding, and federal intergovernmental revenue for public welfare. The determinants of need are the number of households receiving some form of public assistance, the native population born in the state of current residence, the population living with total income below the poverty line, the percentage of married families, and the population under 18 years of age.

For higher education, the capacity variables include charges for higher education, federal intergovernmental revenue for education, and total cash securities. The political party of the governor is included as a measure of preference for education expenditure. The determinants of need are the population with no schooling and the population that commutes to work via carpool.

Health and hospital capacity is determined by federal intergovernmental revenue for health and hospitals and total hospital charges. Need determinants include the log of the median rent in a given area and full-time government employment in 1997.

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[15] Here and elsewhere, it is possible that "double-counting" might occur. The method attempts to use these variables as proxies for overall capacity, so if including both own-source revenue and individual income taxes improves explanatory power without introducing multicollinearity problems, we include both.

The capacity variables for highway expenditure are federal intergovernmental revenue for highways, and interest earnings on investments. The determinants of need include land area and population density (defined as log (population/land area)), as well as the following per-capita measures: high school graduates, rural population living on farmland, and the population that migrated from the Midwest.

The sole capacity variable for expenditure on police and protection is general own-source revenue. The determinants of need include population, population density, and land area, as well as the percentage of families that are married, the percentage of households with less than $15,000/year in income, and a standardized index of crime calculated using the number of reported assaults, armed robberies, auto thefts, burglaries, larcenies, murders, rapes, and robberies in the year 2000. Per-capita need determinants for police expenditure include the native population born in state of current residence, the over-65 population in poverty, the population commuting to work via bicycle, and the urban population.

“Other Expenditures” have four measures of capacity: general own-source revenue, federal intergovernmental revenue for other expenditures, tobacco tax revenue and general debt interest. Determinants of expenditure include land area, per-capita income, the population commuting to work by bike or on foot, the number of households in urban areas and the number of households receiving public assistance.

Given the potential effects of unobservable characteristics within each area, these regressions are run as a system of equations using a seemingly-unrelated regression (SUR) framework. Table 5 shows regression equations for the pooled MSA-level model. The regression equations have quite a good fit, with a system adjusted R^2 of .83. While

not our preferred model, this regression equation is reported to provide a sense of the magnitude and direction of the effects of the determinants of need and measures of fiscal capacity.

Because of its flexibility with respect to entrenched revenue constraints and baskets of services across clusters, the preferred model is the hybrid-regression model. Using a Chow test, we can reject the null hypothesis that the coefficients are equal across clusters at the $p < 0.000$ level, which is to say that our intuition that public expenditures are indeed determined differently across areas with different levels of "urbanization" turns out to be well-founded, and thus that a cluster-based regression approach is preferable to the pooled approach. Regression equations for all regression-based approaches are available on request.

Comparing Apples to Apples to Oranges

As previously discussed, we consider a number of approaches to measuring fiscal need. Table 6 presents the top five, median, and bottom five states and sub-state areas by estimate of fiscal need. As our primary focus is on the relative performance of different approaches to estimating expenditure need, we relegate the full reporting of point estimates of expenditure need to Appendices C (by state) and D (by sub-state area). In this section, we will discuss the ways in which estimates differ across states, and how these results vary by approach.

Table 5. Regression results from pooled MSA-level SUR

System's adjusted R^2 = 0.8318. p-values <0.001 unless listed in parentheses below coefficient

PUBWELF	= CONST	+ HHPA	+ LT18	+ MARRIED	+ NATIVE	+ POOR	+ OSREV	+ LTD	+ FIPW
$R^2 = 0.76$	0.688	7.567	-1.823	0.688	0.183	-1.506	0.028	0.019	0.844
							(0.008)		

POLICE	= CONST	+ AREA	+ BIKE	+ CRIME	+ MARRIED	+ NATIVE	+ POORHH	+ POOR>65	+ POP	+ POPDEN	+ URBAN	+ OSREV
$R^2 = 0.68$	2.841	0	3.918	0.013	-0.227	-0.057	-0.367	2.841	0.005	-0.011	0.108	0.029
		(0.035)			(0.002)							

HIGHWAY	= CONST	+ AREA	+ FARM	+ HS	+ MIGMW	+ FIHW	+ INTREV	+ POPDEN
$R^2 = 0.67$	0.091	-0.001	1.041	0.547	0.321	0.645	0.438	-0.033
		(0.001)	(0.005)					

HIEDUC	= CONST	+ RGOV	+ CARPOOL	+ NOSCHOOL	+ CASHSEC	+ FIHE	+ HIEDCHG
$R^2 = 0.71$	0.12	-0.018	2.159	1.319	0.004	0.547	1.327
		(0.019)		(0.040)			

HEALTH	= CONST	+ GOVEMP	+ RENT	+ FIHH	+ HHCHG
$R^2 = 0.94$	-2.485	3.97	0.303	0.445	0.975
				(0.041)	

ESEDUC	= CONST	+ COLLEGE	+ HHPA	+ POP517	+ PRIV	+ OSREV	+ PROPTAX	+ STDEBT	+ YTAX
$R^2 = 0.74$	-0.567	-0.567	3.834	6.2	-21.872	0.102	0.226	0.715	0.119
		(0.028)							

OTHER	= CONST	+ AREA	+ BIKEWALK	+ HHPA	+ HURBAN	+ INCOME	+ DEBTINT	+ FIOTHER	+ OSREV	+ TOBACCO
$R^2 = 0.85$	-1.382	0.003	7.552	9.787	1.076	0	1.461	1.425	0.299	17.345
										(0.008)

Table 5 continued. Variable Definitions

Variable Name	Description
AREA	Land Area (1,000 sq miles)
BIKE	Population that bikes to work (%)
BIKEWALK	Population commuting to work via bike or on foot (%)
CARPOOL	Population commuting to work via carpool (%)
CASHSEC	Total cash securities held ($ per capita)
COLLEGE	Population with college degree or higher (%)
CRIME	Crime index
DEBTINT	General debt interest ($ per capita)
FARM	Population living on a farm (%)
FIHE	Federal Intergovernmental Transfers for higher education ($ per capita)
FIHH	Federal Intergovernmental Transfers for health and hospitals ($ per capita)
FIHW	Federal Intergovernmental Transfers for highways ($ per capita)
FIOTHER	Federal Intergovernmental Transfers for other categories ($ per capita)
FIPW	Federal intergovernmental transfers for public welfare ($ per-capita)
GOVEMP	Population employed full-time by the government in 1997 (%)
HHCHG	Total charges received for hospitals ($ per capita)
HHPA	Households receiving public assistance (per-capita)
HIEDCHG	Total charges received for Higher Education ($ per capita)
HS	Population with a high-school diploma (%)
HURBAN	Housing stock in urban areas (per-capita)
INCOME	Per-capita income
INTREV	Total State & Local Interest Revenue ($ per capita)
LT18	Population below age 18 (%)
LTD	Long term debt outstanding ($ per capita)
MARRIED	Families that are married (%)
MIGMW	Population that migrated from the Midwest (%)
NATIVE	Population that is native-born living in state of birth (%)
NOSCHOOL	Population with no schooling (%)
OSREV	General own-source revenue ($ per capita)
POOR	Population living below poverty line (%)
POOR>65	Population below poverty line over age 65 (% of total population)
POORHH	Households with income less than $15,000 (%)
POP	Population (millions)
POP517	Population between 5 and 17 years of age (%)
POPDEN	Population density (log(Population/Land Area))
PRIV	Population attending private high school (%)
PROPTAX	Revenue from property taxes ($ per capita)
RENT	Log Rent (=log(median earnings * median rent as % of income))
RGOV	1 if State Governor is Republican
STDEBT	State Debt outstanding at end of year ($ per capita)
TOBACCO	Revenue from tobacco taxes ($ per capita)
URBAN	Population living in Urbanized Areas or Urban Clusters (%)
YTAX	Revenue from State & Local Income Tax ($ per capita)

Table 6. Top 5, Median, and Bottom 5 States and sub-state areas by fiscal need

State	Hybrid		State		Traditional		Actual		Difference	
	Need	Rank	Need	Rank	Need	Rank	Need	Rank	Need	Rank
Alaska	$8,177	1	$9,226	1	$5,995	21	$13,418	1	$5,241	1
Hawaii	$6,767	2	$6,938	3	$5,216	50	$6,828	7	$61	15
Wyoming	$6,564	3	$6,534	4	$5,894	23	$7,677	4	$1,113	4
Minnesota	$6,560	4	$6,274	11	$5,553	45	$7,052	5	$492	8
Connecticut	$6,448	5	$6,199	15	$5,772	31	$6,789	9	$341	10
⋮		⋮								
Oklahoma	$6,012	26	$6,163	17	$6,059	19	$5,109	45	-$903	47
⋮		⋮								
South Carolina	$5,682	47	$5,747	48	$5,745	33	$5,903	21	$221	13
Arkansas	$5,631	48	$5,689	49	$6,539	3	$4,746	51	-$885	46
Louisiana	$5,548	49	$5,781	45	$6,631	2	$5,287	40	-$261	28
Alabama	$5,448	50	$5,558	51	$6,492	4	$5,308	38	-$140	21
Mississippi	$5,442	51	$5,672	50	$6,800	1	$5,296	39	-$146	22

Sub-State Area	Hybrid		Actual		Difference	
	Need	Rank	Need	Rank	Amount	Rank
Alaska - Rural	$7,857	1	$13,694	1	$5,837	1
Anchorage, AK MSA	$7,768	2	$13,029	2	$5,261	2
Massachusetts - Rural	$7,096	3	$10,010	3	$2,914	3
Fresno, CA MSA	$6,983	4	$6,821	37	-$162	170
Visalia--Tulare--Porterville, CA MSA	$6,925	5	$7,850	7	$925	36
⋮		⋮				
Wilmington, NC MSA	$5,692	162	$7,795	8	$2,103	5
⋮		⋮				
Mississippi - Rural	$5,042	319	$5,104	238	$62	131
Houma, LA MSA	$5,024	320	$6,132	91	$1,108	26
Lafayette, LA MSA	$4,991	321	$4,693	283	-$298	201
Bryan--College Station, TX MSA	$4,963	322	$4,388	313	-$575	250
Auburn--Opelika, AL MSA	$4,682	323	$4,686	285	$4	137

Following Boex and Martinez-Vazquez (2007), we present correlation coefficients across measures of need in Table 7. Our results are similar to those of Boex and Martinez-Vazquez, who also find a correlation of -0.11 between actual expenditure and the traditional RES approach, and positive correlations between actual expenditure and their regression RES results. In addition, the estimates generated by our preferred

approach, the hybrid-regression method, are highly correlated with the other regression-based approaches, implying that these results are relatively robust to different specifications. We also find a high correlation between the hybrid- and state-regression estimates, despite the loss of information from aggregation and the use of a different set of regressors.

Table 7. Correlation coefficients for different measures of expenditure need

	Actual Expenditure	Hybrid	MSA	Cluster	State	Single	Trad.	Adj. R^2
Hybrid	0.7860							.8600
MSA	0.7892	0.9667						.8318
Cluster	0.7529	0.9059	0.9406					.8600
State	0.9088	0.8811	0.9128	0.8222				.9356
Single	0.4586	0.6995	0.7556	0.7149	0.7021			.9740
Traditional	-0.1192	-0.2845	-0.2583	-0.2067	-0.1975	0.0864		---
Barebones	0.5036	0.6394	0.6594	0.5529	0.6819	0.6672	0.0196	.7329

The traditional RES approach is negatively correlated or uncorrelated with all the regression-based approaches. The barebones approach (which uses only area and population) displays a surprisingly high correlation with our preferred approach, the hybrid-regression method—which bodes well for our ability to account for expenditure need with poor data: at least in the United States, population and area are important

determinants of public expenditure. Using units of observation less varied in size and population may change these results, although the similarity between state-based and sub-state estimates indicates that the results are robust to scale.

Figure 10 presents kernel density estimates of the distributions of the most important measures of expenditure need: the hybrid results, the state-based results, and the traditional RES method. All three approaches reduce the variance of the expenditure estimates, which both accords well with theory and should occur of necessity; recall not only that the goal is to isolate the difference in expenditure that derives from differences in need across areas, but also that much of this effect is attained by holding capacity measures constant at the national or cluster-based average level. In addition, the variance in estimates among the hybrid approach is lower than that of the state-based approach ($p<0.02$ using an F-test). The finer data resolution available without aggregation allows us to account for more expenditure heterogeneity across states, and more finely isolate that portion of expenditure which is related to need.

Figure 9 (page 38) plots the same measures of expenditure need against state population density. The marked U-shape of actual expenditures with respect to population density is greatly muted in the regression-based approaches, but is still present. While Alaska and the District of Columbia appear to be outliers, the underlying U-shape is robust to their exclusion. The traditional RES approach, however, appears to show no meaningful relationship between expenditure need and population density.

Figure 11 presents hybrid regression and MSA regression estimates of expenditure need, along with actual expenditure, against sub-state area population

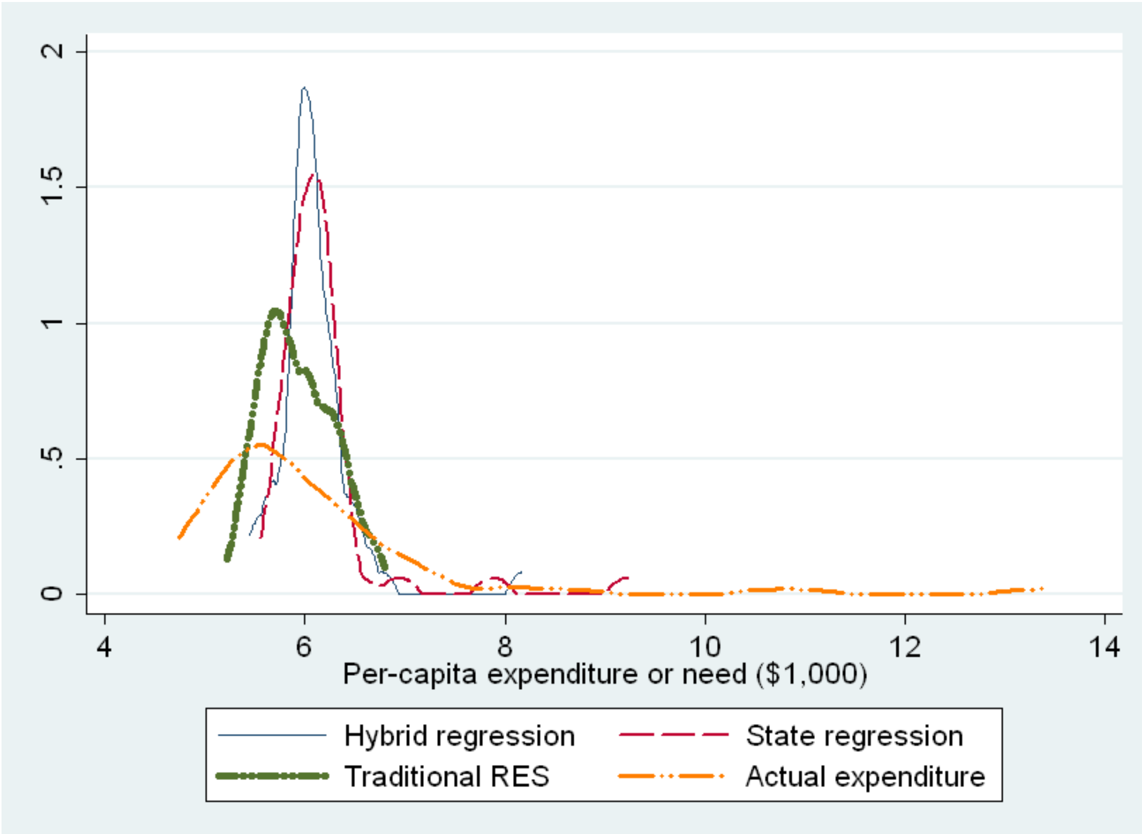

Figure 10. Kernel density estimates of hybrid, state-based, and traditional RES estimates against actual expenditures

density. The U-shape of actual expenditure and expenditure need with respect to population density is again robust to the exclusion of outliers. One point of note is that while the District of Columbia is an outlier when compared to states across a number of dimensions, it is no longer an outlier when considering sub-state-level data, either in terms of actual expenditure or expenditure need. The outliers in the sub-state-level analysis are rural Alaska, the Anchorage MSA, and rural Massachusetts, all of which spend more than $10,000 per capita.[16]

[16] This is likely due to Alaska's oil-revenue redistribution policies and rural Massachusetts' citizens' high income, which may increase both the demand for and the cost of public services.

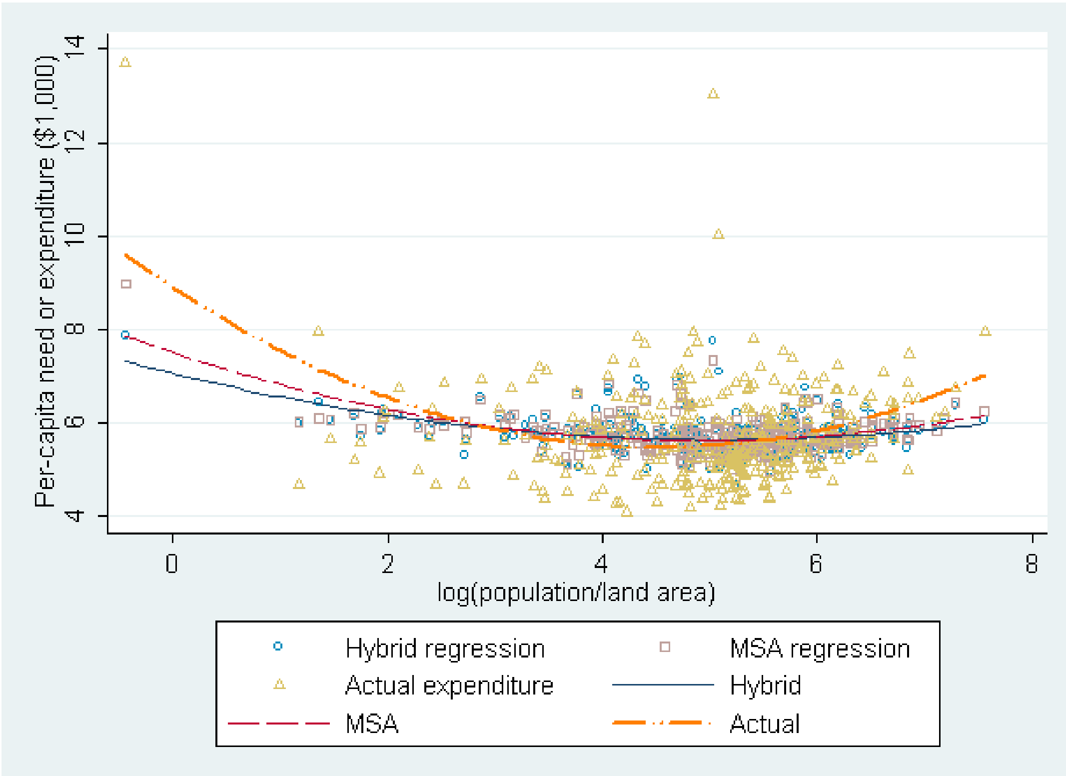

Figure 11. Measures of sub-state-level expenditure need by sub-state population density

Figure 12 presents the geographical distribution of expenditure need under the hybrid approach. Figure 13 illustrates the comparative results of the state-based approach and the hybrid approach. Due to an aggregation effect, we hypothesize that the state-based approach would be expected to overestimate expenditure need for areas that are relatively large and less populous. Table 8 presents a simple OLS regression of the difference between the state and hybrid results on land area and population density and confirms that there is a systematic difference in the estimates generated by these two regression-based methods. This difference is economically significant: our results imply that aggregation at the state level represents a 15% overestimate of expenditure need for the District of Columbia and a 4% underestimate of need for Minnesota, with the rest of the states arrayed in between.

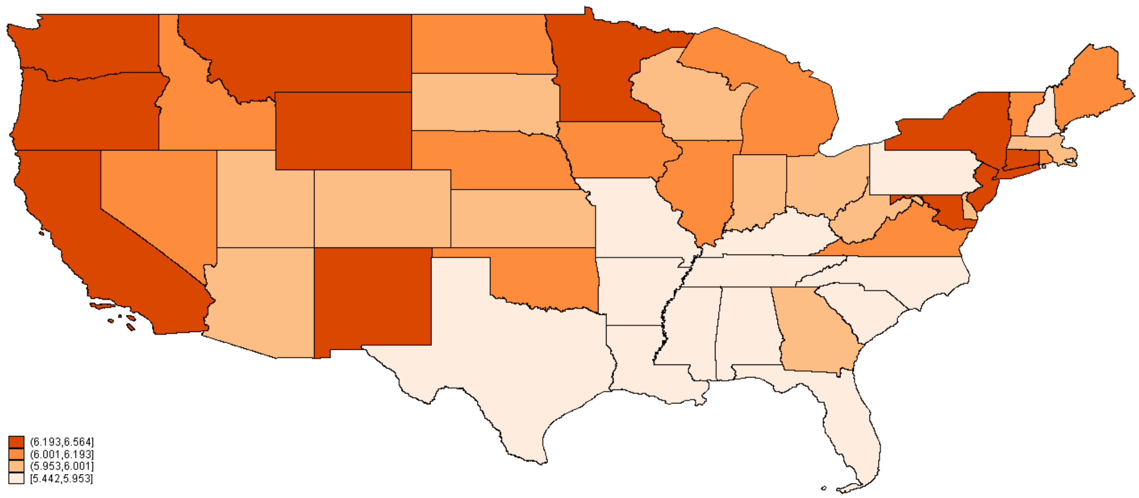

Figure 12. Hybrid-regression RES results across States
(darker implies greater expenditure need)

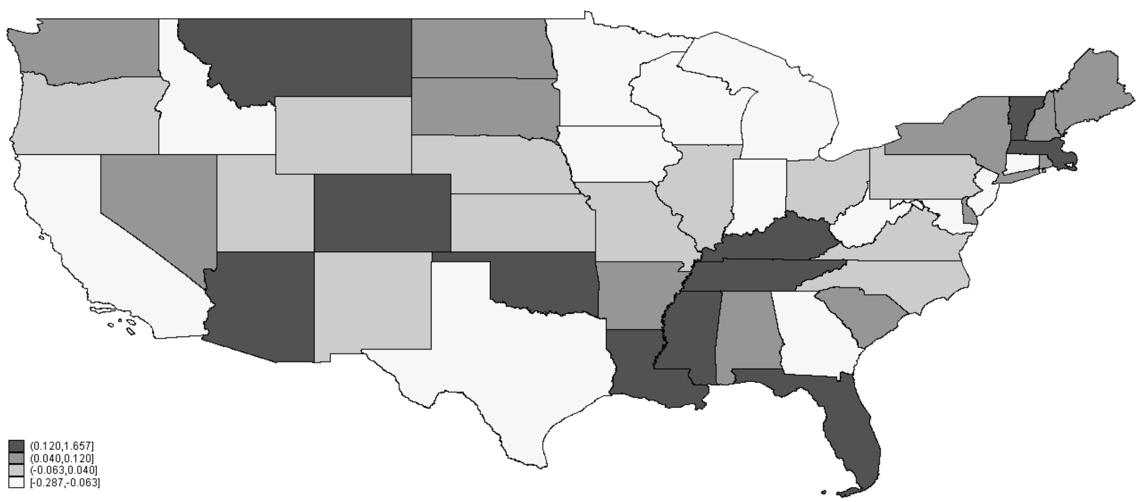

Figure 13. State-based regression vs. Hybrid-regression
(Dark implies State-based expenditure need estimate is higher than Hybrid)

Table 8. The difference in regression-based expenditure need estimates dependent on land area and population density

	Difference between State & Hybrid regression RES estimates (State – Hybrid)
land area	9.52e-07
	(0.033)
log(population)	-0.109
	(0.004)
Constant	1.645
	(0.004)
Observations	51
R^2	0.24

p-values in parentheses

Figure 14 shows the difference between the traditional RES results and the hybrid regression estimates, and the traditional RES results clearly overestimate expenditure need for the South. There are a number of possible reasons for this, but the most likely seems to be the approach to adjusting for input costs. With no input cost adjustment, for example, estimates would tend to be much higher wherever wages were lower, because the poverty rate would tend to be higher in those areas—expenditure need and input costs are both correlated with the prevailing wage.

Accounting for input costs should mitigate that effect to some extent, but if we consider two areas with similar median wages, one of which has high wage inequality, and one of which has low wage inequality, then accounting for median wages will not account for the higher cost of providing public services (from the relatively higher wage half of the population) and the higher need for public services (for the lower wage half) in the area with greater wage inequality. Comparing expenditures using a regression-based approach—particularly one that attempts to compare areas with similar labor markets to one another—should account for not only a median-wage effect, but to some extent for a

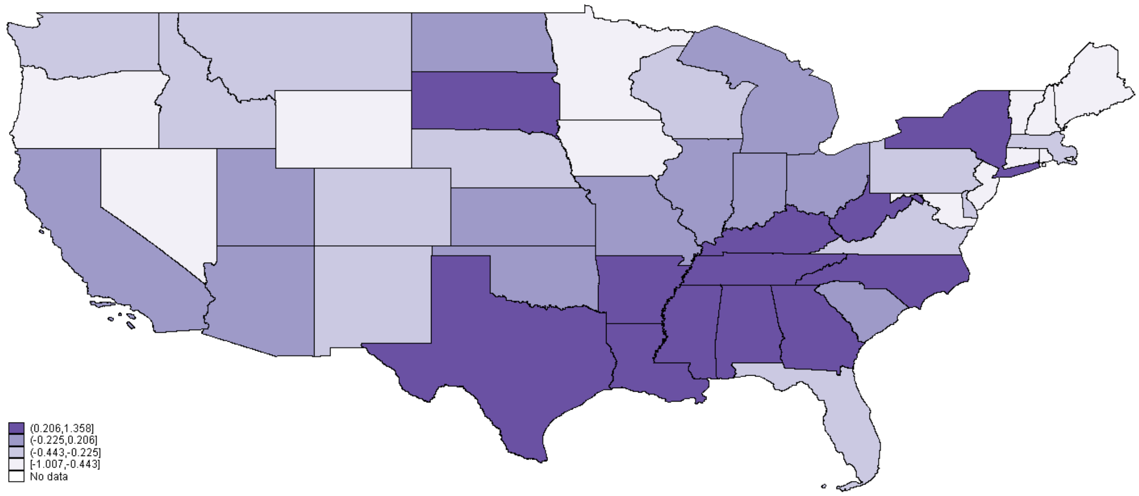

Figure 14. Traditional estimates of fiscal need vs. Hybrid estimates
(Dark implies Traditional projects higher required per-capita expenditure)

full distributional wage effect. We hypothesize that the difference between the traditional approach and our regression-based results is linked to wage-inequality across states.

Another possible source of the difference between the sub-state approaches and any state-based approach could arise from within-state heterogeneity. Heterogeneity should increase the effect of aggregation on a state’s expenditure need estimates. Because size may be a proxy for heterogeneity, an alternative hypothesis to account for the disparity between our results and previous results might be an aggregation effect, although the similarity between regression results across different scales indicates that this does not fully explain the disparity.

Figure 15 presents the difference between the hybrid regression estimates and actual expenditure. When compared to overall hybrid estimates (see the final column in Table 6), it becomes clear that states with actual expenditures higher than estimated expenditure need are states with high estimates of expenditure need to begin with, in

general. In other words, states with high levels of need overspend, and states with low levels of need do not spend enough.

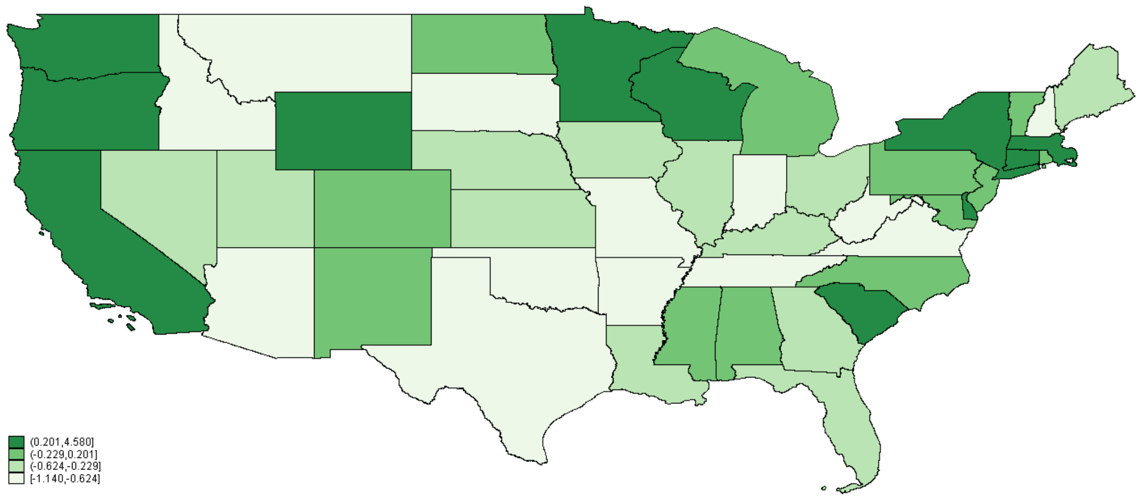

Figure 15. Hybrid estimate of fiscal need vs. Actual per-capita expenditure
(Dark implies over-expenditure, Light implies under-expenditure)

In general we should expect places that spend more to have a higher level of expenditure need, both based on the principle that policymakers are doing their jobs and on some insight from basic political economy. The fact that actual expenditure might not only be correlated with our estimates of need, but that policymakers might overshoot in both directions makes sense as well. States with the highest levels of expenditure need might have the largest percentage of constituents who might directly benefit from greater public expenditure, while those with the lowest levels of expenditure need are more likely to have net contributors to the public good outnumbering net beneficiaries. We hypothesize then that the population receiving a NFB greater than 0 is larger in those states with higher levels of public spending.

Conclusion

We have presented a data-intensive approach to estimating expenditure need in the United States in the hope of producing a benchmark to which other approaches might be compared. Our findings echo those of Boex and Martinez-Vazquez (2002) in that the regression-based approaches appear to be (very slightly) negatively correlated with those achieved by traditional representative expenditure approaches.

The evidence appears to suggest that these regression-based approaches are preferable in a few different dimensions. First, these estimates are less sensitive to specification in both unit of analysis and selection of regressors than are more traditional methods that rely on structural assumptions. Second, regression-based estimates accord more closely with actual expenditure, both in relation to population density and to geographical distribution. As previously discussed, this represents an advantage for regression-based approaches, as both the RTS and RES approaches rely on the assumption that, on average, sub-national government tax and expenditure packages do not raise problems in and of themselves. Finally, the use of data to decide upon the determinants of need seems consistent with this assumption as well, as regression-based approaches use underlying relationships within actual expenditure, demographic, and characteristic data to apportion expenditure need.

In addition to presenting a comparison of traditional and regression-based approaches to estimating expenditure need, we have presented a comparison of the effects of using state-level data relative to sub-state-level data. Sub-state-level data provide the advantage of allowing determinants of need to differ across clusters of places, which allows for better estimation of expenditure need. In addition, it allows for better

comparison across similar labor markets, so that the need to explicitly account for input costs is mitigated or eliminated. While the estimates are relatively robust to the choice between state-based and sub-state-level approaches, the state-based approaches provide less variance in estimates, and thus capture less of the expenditure need variation across places, and systematically overestimate need in larger, less densely populated places. This last insight confirms the intuition that more heavily urbanized areas, with greater wage and wealth inequality, are likely to have systematic underestimation of expenditure need under a state-based approach.

Chapter III: Evading Nash Traps in Two-Player Simultaneous Games: Two New Concepts

Introduction

When people play strategic games with strangers, the problem they face is not a simple one—this is fundamentally different from mere constrained optimization. As von Neumann and Morgenstern argue:

> This is certainly no maximum problem, but a peculiar and disconcerting mix of several conflicting maximum problems…. [Other players' actions] cannot, from [the player's] point of view be described by statistical assumptions. This is because the others are guided, just as he himself, by rational principles—whatever that may mean—and no modus procedendi can be correct which does not attempt to understand those principles and the interactions of the conflicting interests of all participants. (Von Neumann and Morgenstern 1947)

Under such conditions, how can we expect people to behave? Nash equilibrium is perhaps the most widely applied tool of analysis in attempting to understand and address this question. In many cases, it performs quite well; the existence results presented in Nash's original paper recommend it highly, and its refinements make up the foundation for study in both strategic- and extensive-form games (1951). Sometimes, however, the Nash prediction is Pareto-inferior to another strategy profile—perhaps the clearest case of this is the Prisoner's Dilemma. If mutually preferable profiles can be supported, all players will be made better off. Previous literature has relied on external coordinating devices or repetition to achieve efficiency (Aumann 1974, Friedman 1971). This paper introduces strategic concepts that are at least as efficient as Nash predictions.

Under certain institutions, Nash equilibrium performs well in *predicting* behavior. This is an important result: if Nash equilibrium predicts behavior, then we may be able to design an institution to achieve a particular outcome that has desirable properties. In one-

shot simultaneous games, however, the experimental evidence on the predictive power of the Nash concept has been mixed. Frank et al. (1993) find that participants in a Prisoners' Dilemma game with nonbinding communication chose dominated strategies between 39% (economics undergraduates) and 60% (other majors) of the time. Stahl and Wilson (1995) find that 42.8% of responses in one-shot 3x3 games with pure-strategy NE are non-NE. Cooper et al. (1996) find that 22% of subjects' responses are dominated in one-shot Prisoner's Dilemma games.

In some games—particularly those in which strategic interactions can lead to Pareto improvements—subjects systematically deviate from game-theoretic predictions, with a non-trivial proportion playing dominated strategies. The centipede game (Figure 16) is perhaps the most oft-cited extensive-form game of this kind—McKelvey and Palfrey (1992) find that 85-99% of first movers in their centipede games choose the non-Nash strategy and 15-31% of last movers play the dominated strategy. The Traveler's Dilemma (Figure 17) is an important illustrative example among the class of simultaneous games (Basu 1994). Capra et al. (1999) report on a set of repeated Traveler's Dilemma games and find that only under extreme conditions do decisions approach the Nash prediction. Becker et al. (2005) report that 20% of their respondents choose the dominated strategy in a one-shot Traveler's Dilemma.

While Nash equilibrium may not always predict behavior well, the desirability, simplicity, and plausibility of axiomatic representations of Nash equilibrium attest to its normative power. In equilibrium, by definition, one's decisions are robust to a unilateral deviation: the strategy played is the strategy one ought to play, given others' strategies. The Nash prediction is also the strategy one ought to play to maintain consistency with

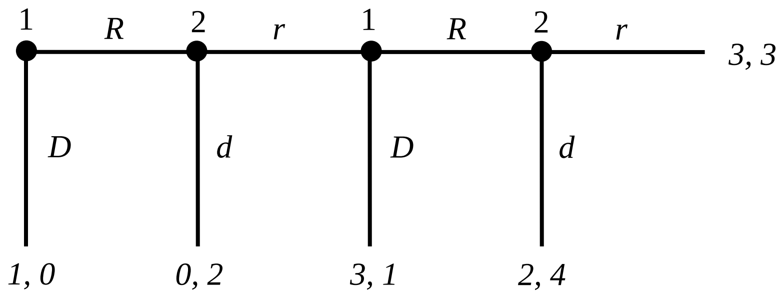

Figure 16. Four-stage centipede game

	2	3	4	5	…	97	98	99	100
2	2, 2	4, 0	4, 0	4, 0		4, 0	4, 0	4, 0	4, 0
3	0, 4	3, 3	5, 1	5, 1	⋮	5, 1	5, 1	5, 1	5, 1
4	0, 4	1, 5	4, 4	6, 2		6, 2	6, 2	6, 2	6, 2
5	0, 4	1, 5	2, 6	5, 5		7, 3	7, 3	7, 3	7, 3
⋮			…				…		
97	0, 4	1, 5	2, 6	3, 7		97, 97	99, 95	99, 95	99, 95
98	0, 4	1, 5	2, 6	3, 7	⋮	95, 99	98, 98	100, 96	100, 96
99	0, 4	1, 5	2, 6	3, 7		95, 99	96, 100	99, 99	101,97
100	0, 4	1, 5	2, 6	3, 7		95, 99	96, 100	97, 101	100, 100

Figure 17. Traveler's Dilemma

apparently plausible axioms of rationality: a long line of research has sought an internally consistent generalized set of predictions by transforming the problem into a Bayesian decision problem. This approach has yielded advances in our understanding of, among other things, the relationship between belief and equilibrium, the epistemic conditions for Nash equilibrium, and rationalizability and admissibility (Harsanyi 1976, Aumann and

Brandenburger 1995, Tan and Werlang 1988, Asheim and Dufwenberg 2003, Pearce 1984, Bernheim 1984).

In practice, however, it is unclear that Nash predictions recommend those strategies one *ought* to play, except in two cases: in the first, other agents' actions are given, in which case the problem becomes one of constrained optimization, begging the question posed by von Neumann and Morgenstern. In the second, one highly values consistency with the set of axioms of rationality upon which the equilibrium hinges. In these games, however, consistency with a particular set of axioms provides no utility.

Following Vernon Smith's taxonomy of a microeconomic system (1982), Nash predictions and agents' choices might differ because of a divergence in the workings of the environment, the institution, or of subject behavior. In light of the simplicity of the institution in many strategic-form games, the most likely culprits—and the avenues of approach taken by researchers—are the other two. In the environment, subjects' preferences may differ from those the experimenter intended to induce. The ways in which they differ may vary, from a concern for others' payoffs to other (perhaps exotic) utility functions that account for the sign of a payoff or order statistics (Becker 1974, Rabin 1993, Cox et al. 2008, Kahneman and Tversky 1979, Quiggin 1982). Apparent failures of Nash predictions under these circumstances might instead be a result of poor tests of the theory—if preferences are misspecified or unknown by researchers, then the predictive power of a theory that requires preferences to make its predictions becomes very difficult to test.

On the other hand, preferences may be correctly specified and induced, in the sense that subjects rank outcomes in the intended fashion. Even under these conditions, a

long line of research posits (and confirms) that people diverge from rational behavior in a number of ways. Subjects may, for example, iterate toward a maximum, but not achieve the limit; they may systematically edit information to reduce cognitive costs; they may misapprehend probability, minimize regret, or choose with error (Hey and Orme 1994, Stahl and Wilson 1995, Nagel 1995, McKelvey and Palfrey 1995, Holt and Laury 2002, Halpern and Pass 2008).

The present paper follows in the behavior-focused line in the introduction of two new strategic concepts—the détente concept and the no-initiative concept—in simultaneous games. We will argue that détente and no-initiative are often preferable on grounds both normative, in that agents can improve efficiency, and positive, in that these concepts describe observed behavior.

The paper is organized as follows: Section 2 presents a discussion of internal correlation, and some possible psychological and normative bases for these concepts, as well as some properties of strategic concepts. Section 3 presents the definitions of two strategic concepts that satisfy these properties, Section 4 discusses some implications in commonly studied two-player simultaneous games, and Section 5 concludes with a discussion of these concepts and some thoughts on future developments.

Theory of Mind, the Categorical Imperative, and Agents

From the outset, game theory has relied on the intuition that the player faced with a game is aware that there is another player in similar circumstances; there is another *person* playing the game. This separates choice in a game from choice under uncertainty. It is a unique, albeit ordinary, situation, as people have well-developed faculties for

constructing propositions about others' inferences when in circumstances similar to our own: we predict how people will act.

In many cases, a unilateral deviation undermines the justification for the Nash strategy for other players in the game. Under any of a number of deviations, agents may be better off playing a different strategy, and where no unilateral deviation may be profitable for the deviator, multilateral deviations may lead to economically significant efficiency improvements. While external correlation devices or repetition may offer one means of increasing efficiency, the similarity between agents—and resulting self-reflection—may itself provide a source of strategic correlation of reasoning. Considering the evolution of humans within groups, it may be ecologically rational to take advantage of this correlation, even if the underlying presumptions are not themselves traditionally rational. Consider two examples: agents' reasoning may be correlated due to common internal models of the other, or it may be correlated due to common internalized social norms.

The philosophical and psychological literature has developed the concept of "theory of mind" (Baron-Cohen 1997, Carruthers and Smith 1996). In essence, individuals possess a model of others which allows them to postulate behavior.[17] In practice, this modeling process usually includes quite a bit of sensory evidence about a specific other; in the absence of other evidence, agents may regard their own thought process as a good predictor of others' thought processes, using this as a coordinating device. As Aumann (1987) argues, "The player is not really conditioning on his choice,

[17] The question of whether this process is the result of a simulation module or a working internal theory of mind—which might matter in a more comprehensive model of strategic behavior—is the topic of no small debate with the psychological literature, although a number of people, including Carruthers (1996) and Mitchell (2005) have argued for a synthesis of the two. For more, see Carruthers and Smith (1996).

but on the substantive information that leads him to make this choice." This "substantive information" should include not only a description of the game, but also a working model of other players. Nash equilibrium relies on models of others as myopically self-interested and individually rational to establish its results. Rationalizability relies on models of others as plausible—dominated strategies are never played or posited to be played. In analyzing a game, agents may realize that the Nash equilibrium solution is inferior, and that a mutual deviation provides mutual benefit. Alternatively, they may not be aware of the Nash equilibrium concept, and may instead search under some other criterion. If this process itself is correlated, then agents may make a separate but correlated decision to approach the problem differently.[18]

Another rationale for not playing a Nash strategy profile is the belief that one *should* abandon that behavior. Ethicists throughout history—Kant, Jesus, Bentham—have argued that self-interest should be replaced with something: other-regarding preferences or even merely "enlightened" self-interest, wherein we escape traps by moving past myopic self-interest and trusting that others will do the same—particularly if it is to our mutual benefit. This trust may be innate—subjects have been selected from a social species—or acquired—subjects are active, living members of a complex, functioning society of interdependent people.[19]

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[18] This is clearly different from an external device used to select particular strategies, but it changes expected payoffs to particular strategies. Instead, agents may use their model of the other's mind to provide sufficient belief for abandoning a particularly inferior Nash equilibrium profile in favor of another approach. A different approach than the one taken in this paper might examine the ways in which prior belief might be shifted due to internal correlation.

[19] Widespread rejection in ultimatum games, such as is visible in Henrich et al. 2001, is consistent with the hypothesis that pro-social norms are reinforced with varying degrees of severity across cultures. It may be the case that Pareto-inferior Nash deviations serve as costly signals to support a norm that encourages Pareto-improving Nash deviations in other simultaneous games.

In some games, people *should* violate Nash predictions, as groups of violators can sometimes be made better off, and in practice, people *do* violate such predictions. These types of agents are not classically rational, but a population of such agents might achieve higher levels of utility. We turn now to a proposal of what such agents might look like.

Modeling agents

When evaluating a profile, agents consider only counterfactual profiles that satisfy “feasibility under opponent rationality.” When considering deviations from a given strategy profile, agents presume that other agents are at least as insightful and flexible as they are. They consider only those strategy profiles resulting from bi- or multilateral strategic changes, rather than considering a strategy profile involving a unilateral strategic change. Consequently, agents only consider those counterfactual profiles in which opponents are playing best response strategies. In this paper, we constrain players’ consideration to bilateral strategic changes.

Agents are limited in their depth of reasoning—they engage in finite (but nonzero) steps of inference when evaluating alternative strategies.[20] These agents, then, are boundedly rational. The experimental evidence recommends the number of iterations to be 1-2 (Nagel 1995). In this paper, we constrain agents to consider two iterations—their own strategic change and their opponent’s best response to the strategic change.

Stemming from the limited depth of reasoning, agents use a neighborhood heuristic—if iteration leads to inferior outcomes, agents cut off the iteration process and “settle.” The possible existence of search costs implies that strategy profiles that offer

[20] Nagel (1995) estimates that the optimal and model level of steps is about 2: best reply to best reply to a uniform distribution. Stahl and Wilson (1995) consider a number of levels of inference and reject a “perfect foresight” type model.

local maxima when compared to 1-2 iterations might be preferred to an uncertain, perhaps nonexistent, improvement requiring further iteration. One way to consider Nash equilibrium is that it represents a maximal neighborhood heuristic (as many iterations as necessary to achieve a steady state). This particular heuristic lies at the heart of the strategic concepts contained herein. Détente strategic profiles and no-initiative strategic profiles are in equilibrium with respect to a "move-countermove" neighborhood heuristic.

The distinction between the détente concept and the no-initiative concept is related to a distinction between two potential properties of the agents. The first is "best response focus," in which agents consider only best responses to opponents' strategies when evaluating a strategy profile. This carries intuitive appeal as potentially satisfying ecological rationality. It seems likely that naturally occurring payoffs accruing to a particular strategy might be positively correlated, which is to say: some actions in life are "good ideas" and others are "bad ideas" across large subsets of other players' actions. Détente profiles satisfy best response focus.

The alternative property is "flexibility." Under flexibility, agents are not bound to play best response if another strategy would yield a higher payoff, given previous assumptions; agents compare all possible strategies when assessing a strategy profile. This is satisfying with respect to profile selection, as a profile that is robust to flexibility passes a particularly strict robustness test. It also reduces the multiplicity of predicted outcomes. No-initiative profiles satisfy flexibility.

Properties of strategic concepts

A useful strategic concept, for both descriptive and normative purposes, should allow dominated strategies to be played if efficiency gains can be achieved. Evidence

from experiments indicates that any concept that eliminates all dominant strategies is going to eliminate played strategies: Becker et al. (2005) present evidence that experienced players selected dominated strategies 20% of the time in the Traveler's Dilemma. As discussed earlier, Frank et al. (1993) find that participants in a Prisoners' Dilemma game with nonbinding communication chose dominated strategies between 39% (economics undergraduates) and 60% (other majors) of the time. Both the détente concept and the no-initiative concept allow dominated strategies.

The efficiency of strategic concepts is also of primary interest. If the concept is to be supported on normative grounds, it should offer agents a chance to improve on Nash equilibrium. In the next section, we show that in 2x2 normal-form games that contain both a pure-strategy Nash equilibrium and a détente strategic profile, a détente strategic profile is always weakly preferred by some player to a pure-strategy Nash equilibrium. We also show that any two-player game that contains both a pure-strategy Nash equilibrium and a no-initiative strategic profile, all players will weakly prefer all no-initiative strategic profiles to all pure-strategy Nash equilibria.

Finally, one of the goals of the introduction of these strategic concepts is their descriptive power. A descriptive strategic concept should coincide with subject behavior, particularly in cases where Nash equilibrium fails. After dealing with the formal definitions of the concepts and some of their properties, we provide some evidence of their descriptive power from existing experimental results.

Détente and No-Initiative Strategic Concepts

Let us be given a strategic form game $\Gamma = (N, S, u)$. For our purposes, we will confine ourselves to the two-player case. *N* is the set of all *players* $i \in \{1,2\}$, $s_i \in S_i$ is player i's strategy, S_i is the *strategy set* for player i, $S = S_1 \times S_2$ is the set of strategy profiles $s = (s_1, s_2)$, $s_{-i} =$ the other player's strategy, and $u = (u_1, u_2)$ is the set of payoff functions $u_i: S \rightarrow \mathbb{R}$, player i's payoff to profile s. Let $s_i^*(s_{-i})$ or $s_i^*(s)$ represent player i's *best response* to the strategy s_{-i} chosen by the other player, that is:

$$u_i(s_i^*, s_{-i}) \geq u_i(s_i', s_{-i}) \quad \forall s_i' \neq s_i^*;\ s_i', s_i^* \in S_i$$

Define the *détente alternative profile* for player i to the strategy profile s, $\hat{s}_i(s)$ as the strategy profile consisting of i's best response to s_{-i} and the counter-response:

$\hat{s}_i(s) = \left(s_i^*(s_{-i}), s_{-i}^*\left(s_i^*(s_{-i})\right)\right)$

Define i's *initiative-response set* of strategy profiles with respect to a given strategy profile as the set of profiles in which i's opponent is playing a best-response to i's strategy: $S_i^*(s) \subseteq S = \bigcup_{s_i' \in S_i} (s_i', s_{-i}^*(s_i'))$

Definition 1. A strategy profile $s = (s_1, s_2) \in S$ is a *Nash equilibrium* (NE) if no unilateral deviation in strategy by any single player is profitable for that player, that is:

$$\forall i, s_i' \in S_i, s_i' \neq s_i: u_i(s_i, s_{-i}) \geq u_i\left(s_i', s_{-i}\right)$$

Definition 2. A strategy profile $s = (s_1, s_2) \in S$ is a *strict Nash equilibrium* (SNE) if any unilateral deviation in strategy by any single player would reduce the payoff for that player, that is:

$$\forall i, s_i' \in S_i, s_i' \neq s_i: u_i(s_i, s_{-i}) > u_i\left(s_i', s_{-i}\right)$$

Definition 3. A strategy profile $s = (s_1, s_2) \in S$ is *détente strategic* (DS) if the payoff to each player from s is greater or equal to that of the détente alternative profile:

$$\forall i: u_i(s_i, s_{-i}) \geq u_i\left(s_i^*(s_{-i}), s_{-i}^*\left(s_i^*(s_{-i})\right)\right)$$

Definition 4. A strategy profile $s = (s_1, s_2) \in S$ is *no-initiative strategic*[21] (NIS) if the payoff to each player from the strategy profile is greater than or equal to that from each profile in his initiative-response set for the strategy profile:

$$\forall i, s_i' \in S_i: u_i(s_i, s_{-i}) \geq u_i\left(s_i', s_{-i}^*(s_i')\right)$$

To illustrate these concepts, let us consider the 3x3 game in Figure 18. This game has a unique pure-strategy Nash equilibrium profile: $s = (2{,}2), u = (1{,}1)$. In addition it has four détente strategic profiles and three no-initiative strategic profiles. Looking at $s = (4{,}4)$, we can see that it does indeed satisfy the détente strategic definition: The row player gets a payoff of 3 from the strategy profile: $u_R(4{,}4) = 3$. If he considers his best response, $s_R^*(s_C = 4) = 3$, and the counter-response, $s_C^*(s_R = 3) = 2$, we can see that his utility from the détente alternative profile is $u_R(3{,}2) = 0 < u_R(4{,}4)$, so he has no incentive to deviate. This is a symmetric game, so the reasoning for the column player is identical, and thus $s = (4{,}4)$ is détente strategic.

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[21] The intuition behind the nomenclature I'm using here is that détente equilibrium assumes something akin to a Mexican standoff—players can only respond by employing a best reply (firing the pistol in the Mexican standoff), but because the consequences thereafter would be utility-reducing for all parties involved, no one wants to pull the trigger first. If both sides are aware of the tension, the détente concept "solves" this problem by easing the tension, achieving a détente.

In the case of no-initiative equilibria, players can choose not only (to continue the metaphor) to unilaterally fire, but also to unilaterally put down their guns (or do a little dance, sing "The Yellow Rose of Texas," or anything else in their action set if they think they can win their opponents over), and so a profile is no-initiative when no player has an incentive to take the initiative *in any sense*, not just in the "shooting first" sense.

The strategy profile (4,4) is also no-initiative strategic. The profile (3,3) on the other hand, while it is DS, is not no-initiative strategic. We can see this by looking at a posited change from s = 3 for the row player to s = 4. For both players, the best response

	2	3	4
2	1,1#*	4,0	3,0
3	0,4	1,1#	4,2#!
4	0,3	2,4#!	3,3#!

*: Nash equilibrium, #: Détente profile, !: No-initiative profile

Figure 18. Nash equilibrium, détente strategic, and no-initiative strategic profiles in a two-player game

to a strategy of 3 is to choose a strategy of 2, but by allowing the players to be flexible, we can see that the row player's payoff to (3,3) is 1. If the row player considers a change to $s_R = 4$, and he predicts that the column player will foresee this and play a best response: $s_C^*(4) = 3$, then we can see that his utility from the new profile is $u_R(4,3) = 2 > u_R(3,3)$, and so the row player has an incentive to deviate from the strategy profile (3,3): this profile is not NIS. There is no strategic change from (4,3), however, that will provide an improved payoff for either player, if they believe their opponent will play a best response to their altered strategy: this is a NIS profile.

Nash equilibrium, the détente concept, and the no-initiative concept all rely on the idea that a given strategy profile provides at least as great a payoff to each individual than that of any other profile in a particular subset of S. In particular, a NE profile provides at least as great a payoff than all those strategy profiles in which i's strategy varies, but the other player's strategy is held constant. A DS profile provides at least as great a payoff to each player i than that strategy profile in which i's strategy is a best response to his

opponent's strategy, and his opponent's modified strategy represents the posited counter-response to i's best response. A NIS profile provides at least as great a payoff as all those strategy profiles in which i's strategy is varied, and his opponent's strategy is the posited counter-response to the modified strategy profile.

The differences, then, stem from the counterfactuals that players consider when making a decision. The détente concept shares characteristics with k-step thinking models, in that agents are posited to have insight into the behavior of their opponents with limited powers of induction; in this case, agents are symmetric in that they possess the same depth of inference. One possible way to consider agents is that they all believe that their opponents have one additional level of inference.

These concepts are all similar in that they rely on agents seeking to make themselves better off. The following results formally establish relationships between these strategic concepts and Nash equilibrium. The results are presented here; the proofs can be found in Appendix F.

Result 1: Every strict Nash equilibrium profile is détente strategic.

If the strategy profile is a strict Nash equilibrium, then the current strategy s_i is the unique best response to the set of other players' strategies, s_{-i}, for each player i, so clearly each player weakly prefers the profile to itself.

Result 2: Every no-initiative strategic profile is détente strategic.

A strategy profile is no-initiative strategic if there exists no alternative strategy $s_i^{'}$ for any player such that the counter-response to the modified strategy profile $(s_i^{'}, s_{-i})$ provides a greater payoff to the player. The détente concept requires that this be true only of alternative strategies that are themselves best responses to a given set of opponents' strategies.

Result 3(a): There are profiles that are DS but neither NE nor NIS.

3(b): There are profiles that are NE but not NIS, and NIS but not NE.

These results are visible in Figure 18.

Result 4: In any 2x2 game with both a pure-strategy Nash equilibrium profile and a détente strategic profile, at least one player weakly prefers the détente strategic profile to some pure-strategy Nash equilibrium profile.

If there is both a DS profile and a NE profile in a 2x2 game, there must be some NE profile such that the NE profile does not represent an actual Pareto improvement over the DS profile. The next result, however, illustrates the efficiency of NIS profiles relative to NE profiles.

Result 5: In any game with both a pure-strategy Nash equilibrium profile and a no-initiative strategic profile, both players weakly prefer all no-initiative strategic profiles to all pure-strategy Nash equilibrium profiles.

This result has particular appeal, in that it indicates that in two-player games, NIS profiles are at least as efficient as pure-strategy Nash equilibrium profiles.

Détente and No-Initiative in Two-Player Games

Conflict games

There are 57 2x2 purely ordinal "conflict games" in which there is no mutually preferred outcome and no indifference (Brams 1994). Of these games, 41 have a unique pure-strategy Nash equilibrium. In 35 of these 41, the unique NE profile is both DS and NIS. Figure 19 displays the three purely ordinal conflict games with a unique NE and a unique NIS profile which diverge. Worth noting is that in all three cases, the NIS profile requires that the row player play a dominated strategy, and that if players can successfully coordinate in the face of domination, then a Pareto improvement can be achieved—the Nash trap can be evaded.

	L	R
U	$2,3^{\#*}$	4,1
D	1,2	$3,4^{\#!}$

	L	R
U	$2,2^{\#*}$	4,1
D	1,3	$3,4^{\#!}$

	L	R
U	$2,3^{\#*}$	4,2
D	1,1	$3,4^{\#!}$

*: Nash equilibrium, #: Détente profile, !: No-initiative profile

Figure 19. Purely ordinal conflict games with different NE and NIS profiles

Social dilemmas

Figure 20 presents results for the Prisoner's Dilemma and an abbreviated version of the Traveler's Dilemma (Basu 1994). In both games, there is a unique pure-strategy Nash equilibrium (indeed, these games are dominance solvable). In the Prisoner's Dilemma, both the Nash equilibrium profile of (Confess, Confess) and the Pareto optimal profile, (Deny, Deny), are détente strategic and no-initiative strategic. One drawback of détente is that it is, in some cases, not very restrictive. Much like Nash equilibrium, détente suffers from multiplicity. In both the Prisoner's and Traveler's Dilemma's, for example, every available strategy falls into a détente strategy profile. One approach to improve descriptive power is to refine the détente concept. No-initiative serves as a restriction on détente in this fashion. As the Traveler's Dilemma is expanded, the distance between the NE and the set of NIS strategy profiles grows.[22]

In the full (99x99) Traveler's Dilemma, the set of NIS profiles includes six strategy profiles, four symmetric, and two asymmetric: {(97,97), (98,98), (99,99), (99,100), (100,99), (100,100)}. Becker et al. (2005) present behavior of game theorists in the Traveler's Dilemma. Of the 51 entries they received, 45 played pure strategies; their reported subject behavior is displayed in Table 9. While their procedure and sample render the results perhaps illustrative at best, only ~6% of their subjects played the Nash strategy, while nearly 20% played the dominated strategy s_{100}, and 55% of their subjects chose strategies within NIS profiles. Becker et al. choose to model s_{100} players as "irrational cooperators," but the concepts of détente and no-initiative support s_{100} players in fully half the no-initiative profiles.

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[22] The characteristic shape remains the same: the NIS profiles make a "kite" shape in the lower right corner, while DS profiles represent a 7x7 swath along the diagonal from (4, 4) down to (s_{max}, s_{max}).

	Confess	Deny
Confess	$2,2^{\#!*}$	4,1
Deny	1,4	$3,3^{\#!}$

*: Nash equilibrium, #: Détente profile, !: No-initiative profile

	2	3	4	5	6	7	8	9	10	11
2	$2,2^{\#*}$	4,0	4,0	4,0	4,0	4,0	4,0	4,0	4,0	4,0
3	0,4	$3,3^{\#}$	5,1	5,1	5,1	5,1	5,1	5,1	5,1	5,1
4	0,4	1,5	$4,4^{\#}$	$6,2^{\#}$	$6,2^{\#}$	$6,2^{\#}$	$6,2^{\#}$	$6,2^{\#}$	$6,2^{\#}$	6,2
5	0,4	1,5	$2,6^{\#}$	$5,5^{\#}$	$7,3^{\#}$	$7,3^{\#}$	$7,3^{\#}$	$7,3^{\#}$	$7,3^{\#}$	$7,3^{\#}$
6	0,4	1,5	$2,6^{\#}$	$3,7^{\#}$	$6,6^{\#}$	$8,4^{\#}$	$8,4^{\#}$	$8,4^{\#}$	$8,4^{\#}$	$8,4^{\#}$
7	0,4	1,5	$2,6^{\#}$	$3,7^{\#}$	$4,8^{\#}$	$7,7^{\#}$	$9,5^{\#}$	$9,5^{\#}$	$9,5^{\#}$	$9,5^{\#}$
8	0,4	1,5	$2,6^{\#}$	$3,7^{\#}$	$4,8^{\#}$	$5,9^{\#}$	$8,8^{\#!}$	$10,6^{\#}$	$10,6^{\#}$	$10,6^{\#}$
9	0,4	1,5	$2,6^{\#}$	$3,7^{\#}$	$4,8^{\#}$	$5,9^{\#}$	$6,10^{\#}$	$9,9^{\#!}$	$11,7^{\#}$	$11,7^{\#}$
10	0,4	1,5	$2,6^{\#}$	$3,7^{\#}$	$4,8^{\#}$	$5,9^{\#}$	$6,10^{\#}$	$7,11^{\#}$	$10,10^{\#!}$	$12,8^{\#!}$
11	0,4	1,5	2,6	$3,7^{\#}$	$4,8^{\#}$	$5,9^{\#}$	$6,10^{\#}$	$7,11^{\#}$	$8,12^{\#!}$	$11,11^{\#!}$

*: Nash equilibrium, #: Détente profile, !: No-initiative profile

Figure 20. The Prisoner’s Dilemma and an abbreviated Traveler’s Dilemma

Table 9. Subject behavior in the Traveler’s Dilemma
Taken from Becker et al. (2005), Table 1.

Strategy	Entries	Strategy	Entries	Strategy	Entries
2	3	88	1	96	3
4	1	90	1	97	6
31	1	93	1	98	9
49	1	94	2	99	3
70	1	95	2	100	10

Furthermore, Becker et al. elicited beliefs and over 50% of the 47 belief respondents believed (correctly) that subjects would play a strategy of 96 or greater. In addition, game theorists exhibited a pronounced lack of classical rationality: only 36% of subjects played a best response to their stated belief, and as mentioned above, nearly 20% of subjects chose s_{100}, the only dominated strategy.

Constant-sum games

In constant-sum games, the concepts of détente and no-initiative can offer no efficiency gains. In some cases, they provide little predictive power, although in many of these cases pure-strategy NE profiles are either multiple or nonexistent as well. In Matching Pennies, for example, as well as the game presented to subjects in O'Neill (1987), every strategy profile is both détente and no-initiative, while no pure-strategy profile is a Nash equilibrium. In the 13 basic 2x2 constant-sum games, every game has at least one profile that is NIS. In 3 cases, there is a unique profile that is DS and NIS—in all three cases this is also the unique pure-strategy NE profile. In 5 cases, 2 of the 4 profiles are DS and NIS. As one of the primary benefits of these concepts is Pareto improvement, and all strategy profiles are equally efficient in any zero-sum game, it is perhaps unsurprising that the predictive power of these concepts is limited in these games.

Concluding Remarks

This paper has introduced two strategic concepts, détente and no-initiative, that characterize strategy profiles in normal-form games that might be reached by particular types of agents. These concepts represent a formal justification for the consideration of

strategy profiles that improve on Nash equilibrium profiles in terms of efficiency, and, we argue, predictive power. These concepts allow play of dominated strategies, which excludes them from the set of “solutions” as defined by von Neumann and Morgenstern, but may more accurately represent observed behavior.

As Aumann (1974) showed, it is possible to achieve efficiency gains through external correlation. By the similar nature of agents playing games, it may be possible to coordinate on Nash deviations to achieve efficiency gains through either an internal model of the other person, or shared external norms—including the willingness to play a dominated strategy if it is possible to improve efficiency.

The détente concept is attractive in that it is roughly a generalization of Nash equilibrium, allowing for the possibility of an additional level of sophistication on the part of agents. As a consequence, however, it restricts the set of strategy profiles for consideration even less than does the Nash concept. The no-initiative concept is a refinement of détente that allows for more flexible fictional play, and consequently reduces the set of strategy profiles considerably. In addition, in two-player games, the no-initiative concept always recommends strategy profiles at least as efficient as any existing pure-strategy Nash equilibrium profiles.

In social dilemmas, these concepts provide significant predictive power over Nash equilibrium; in other games, the evidence is mixed. In many cases, the predictions align. The predictive power of détente and no-initiative is diminished in some constant-sum games, but the lack of the opportunity for efficiency gains limits their usefulness in constant-sum games in any case.

This discussion has focused exclusively on pure strategies in two-player simultaneous games. Further research should expand these concepts to mixed strategies, n-player games, and sequential play. It may also be the case that some people play in ways predicted by Nash while others play strategies that are détente or no-initiative. Further analysis of experimental results may improve our ability to discriminate between the two.

Appendix A. Subject Instructions for the Pigovian Subsidy Experiment

[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:

<u>Each token in ***RED*** pays **$0.05**.</u>

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.

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

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

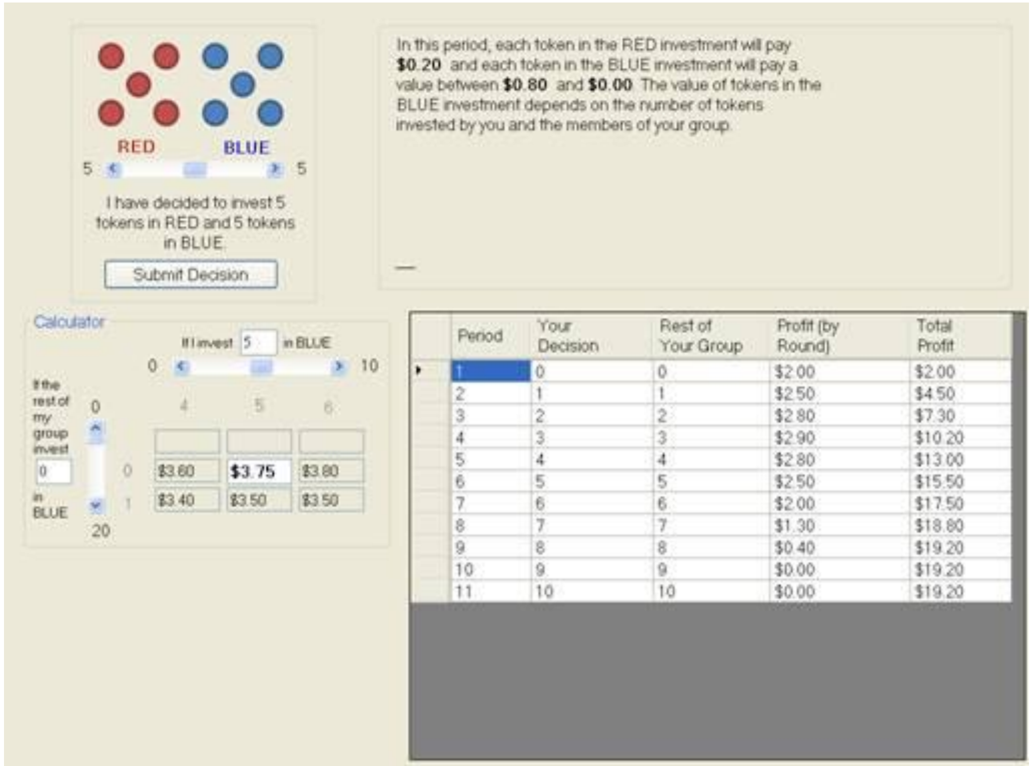
to practice using the calculator before you make any decisions that will determine your payment.

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.

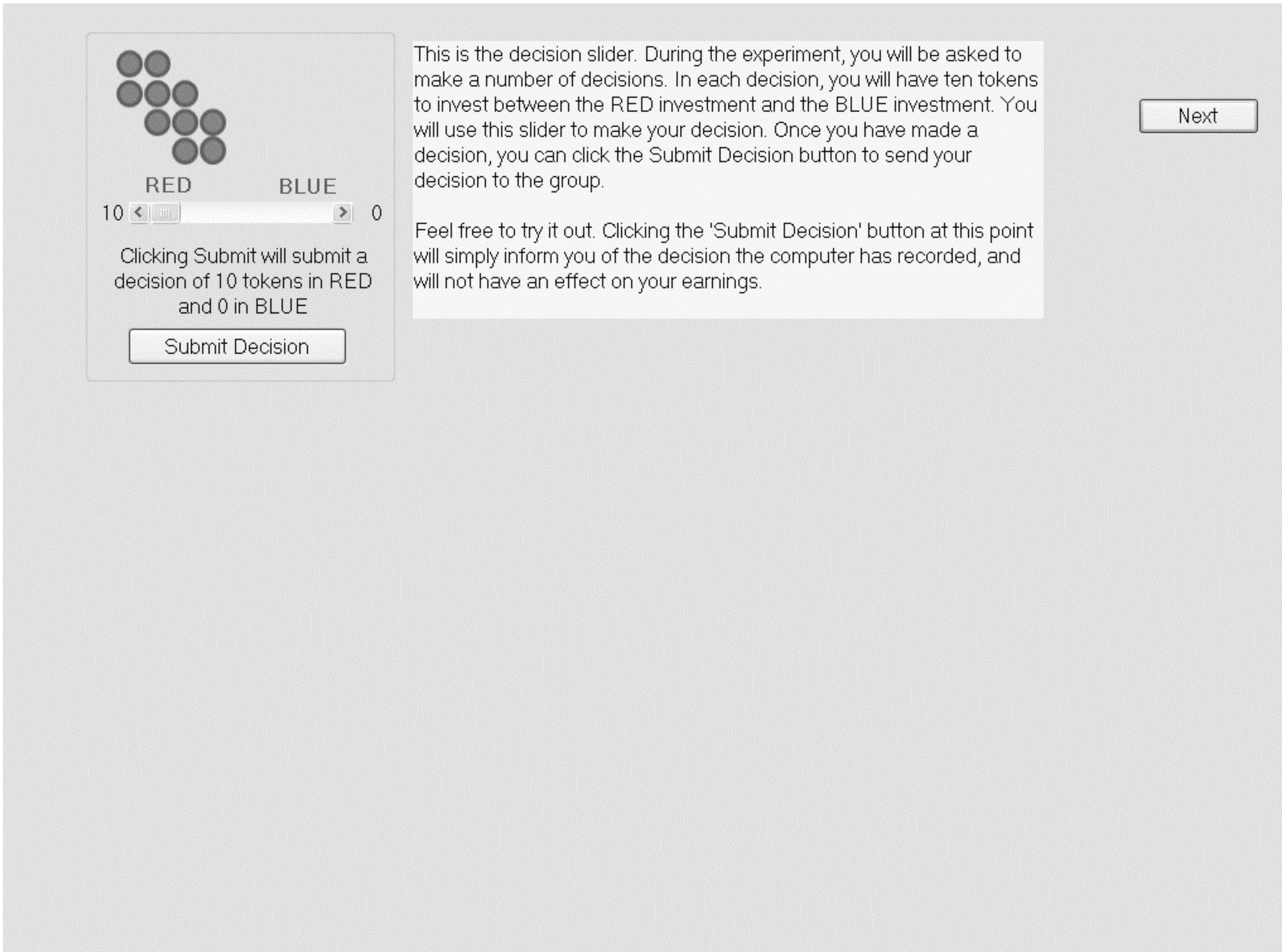
The table at the right of the screen contains the decisions you've made in previous rounds, your earnings for those rounds, as well as your total earnings.

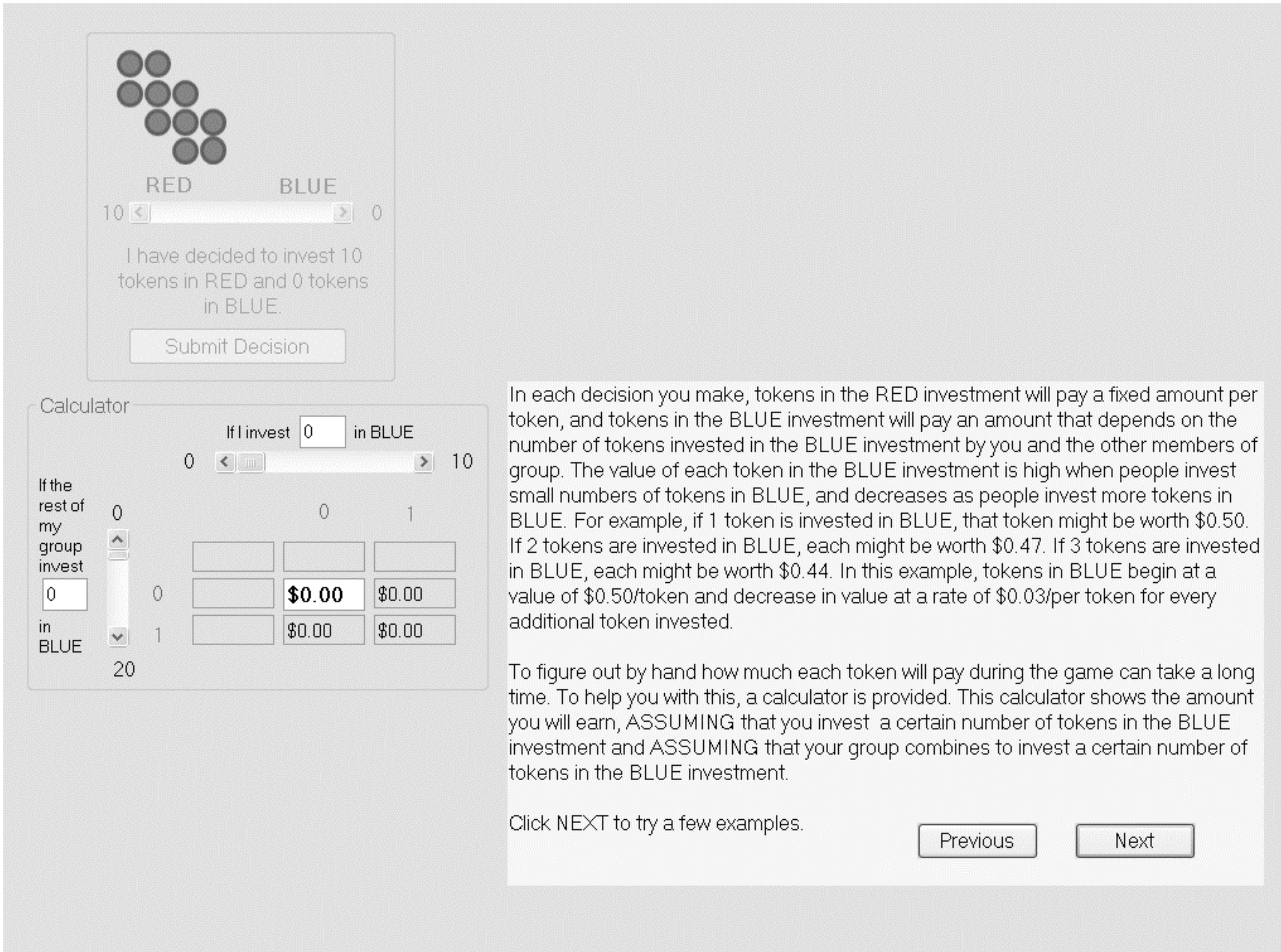
Questions

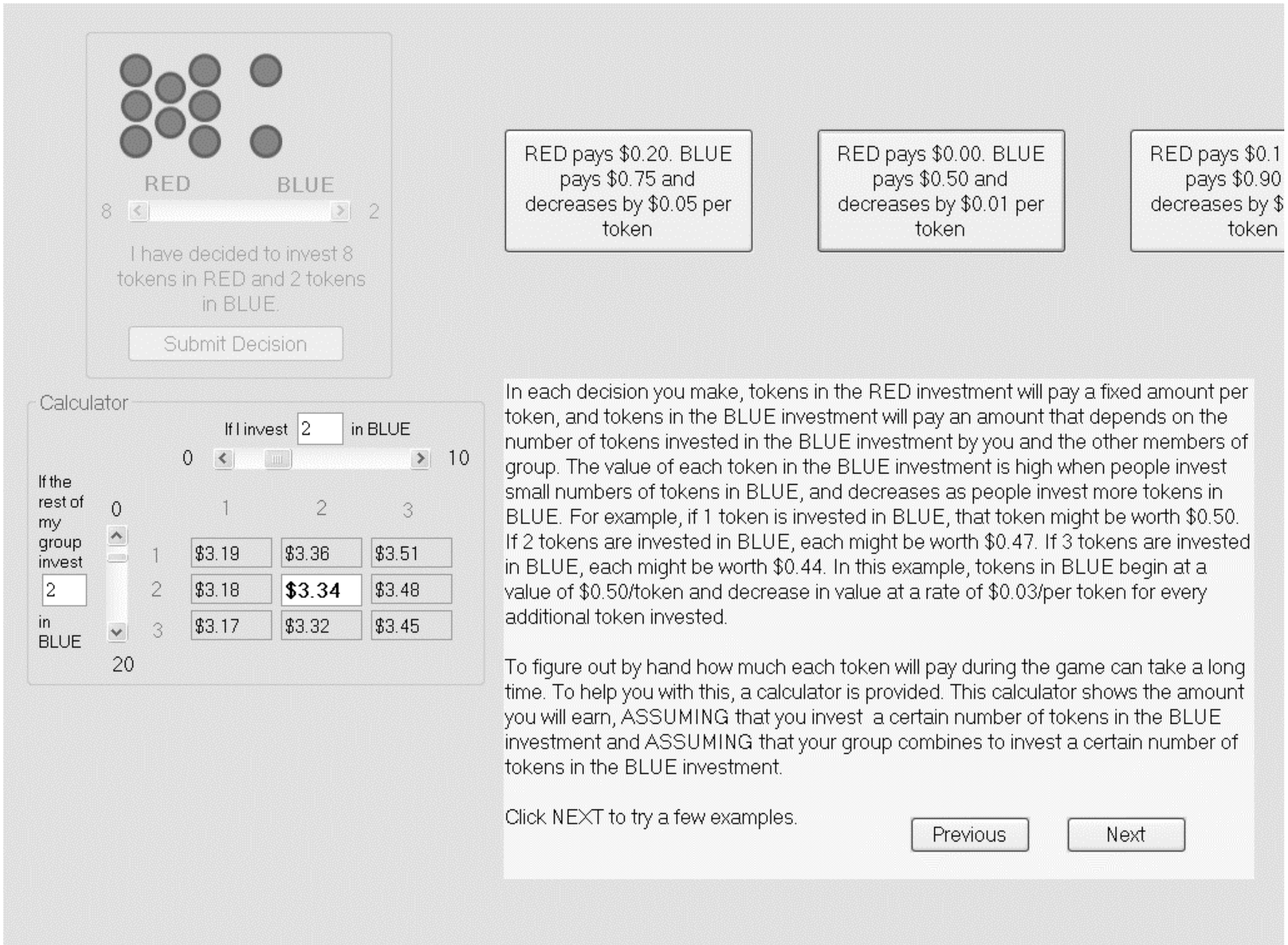
If you have any questions, please raise your hand and the experimenter will come by to answer your question privately.

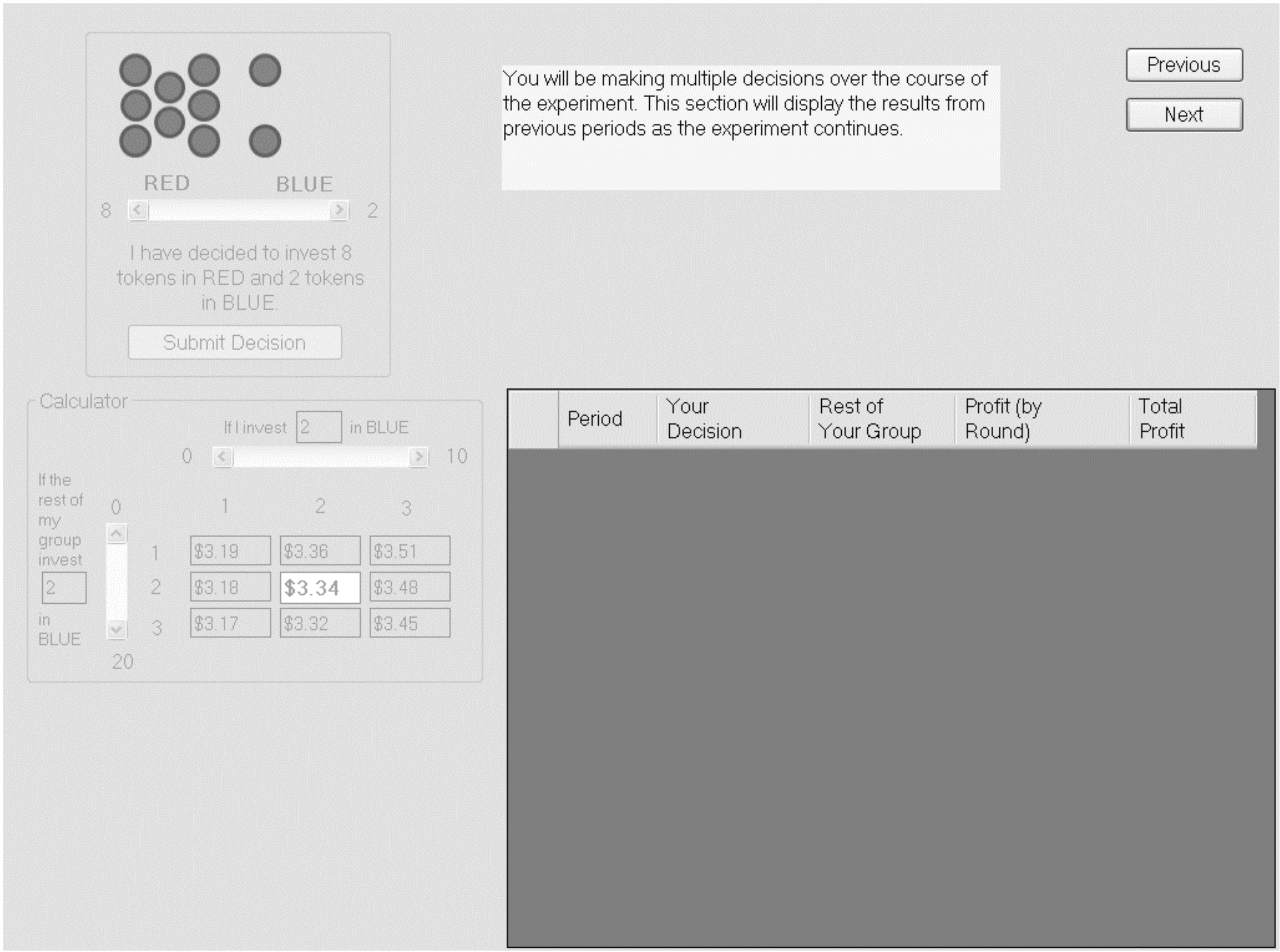
When you are finished reading these instructions, click OK below. Once you have finished reading the instructions, you will have an opportunity to practice using the computer screen.

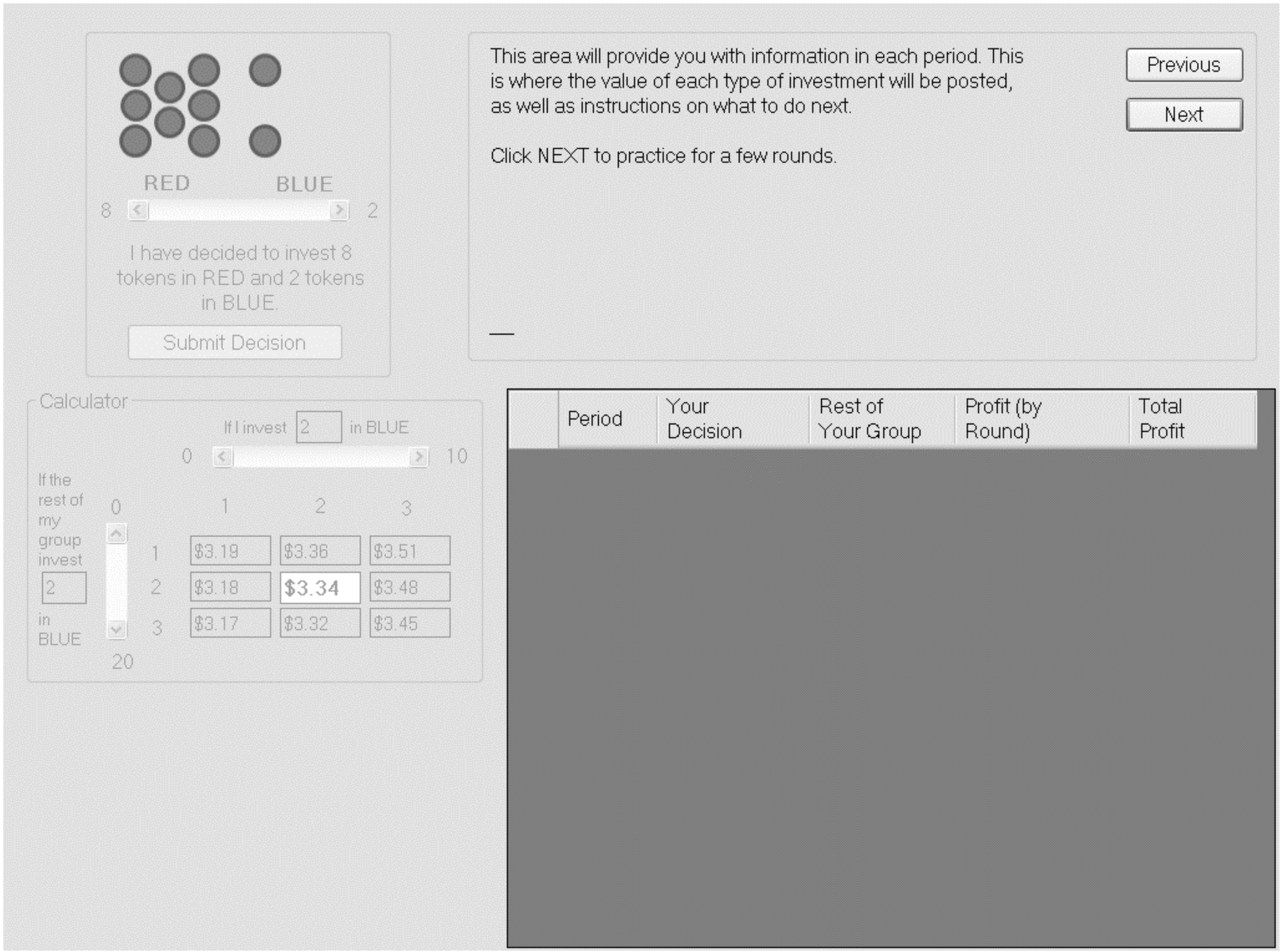
Appendix B. Tutorial Screenshots

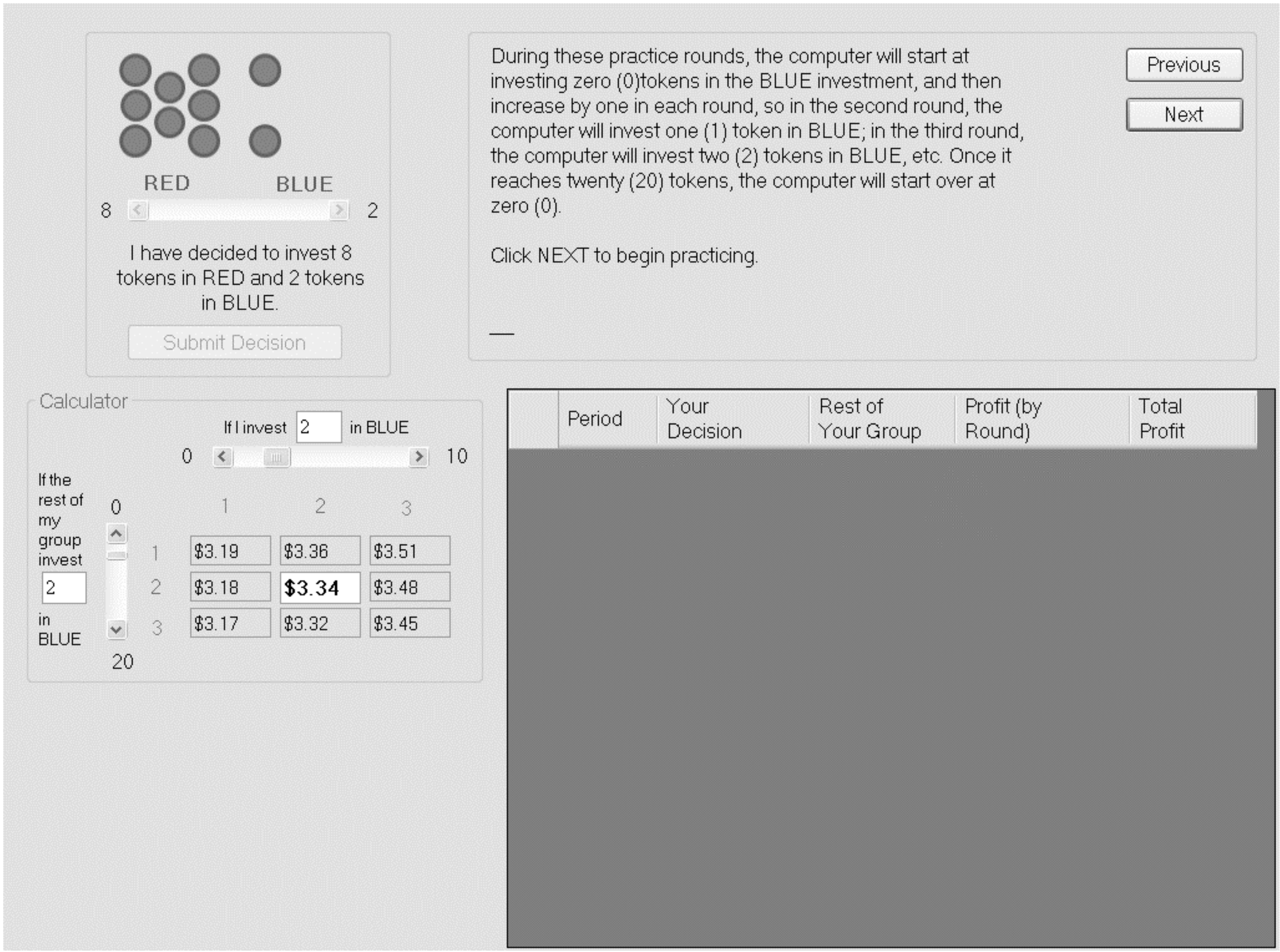


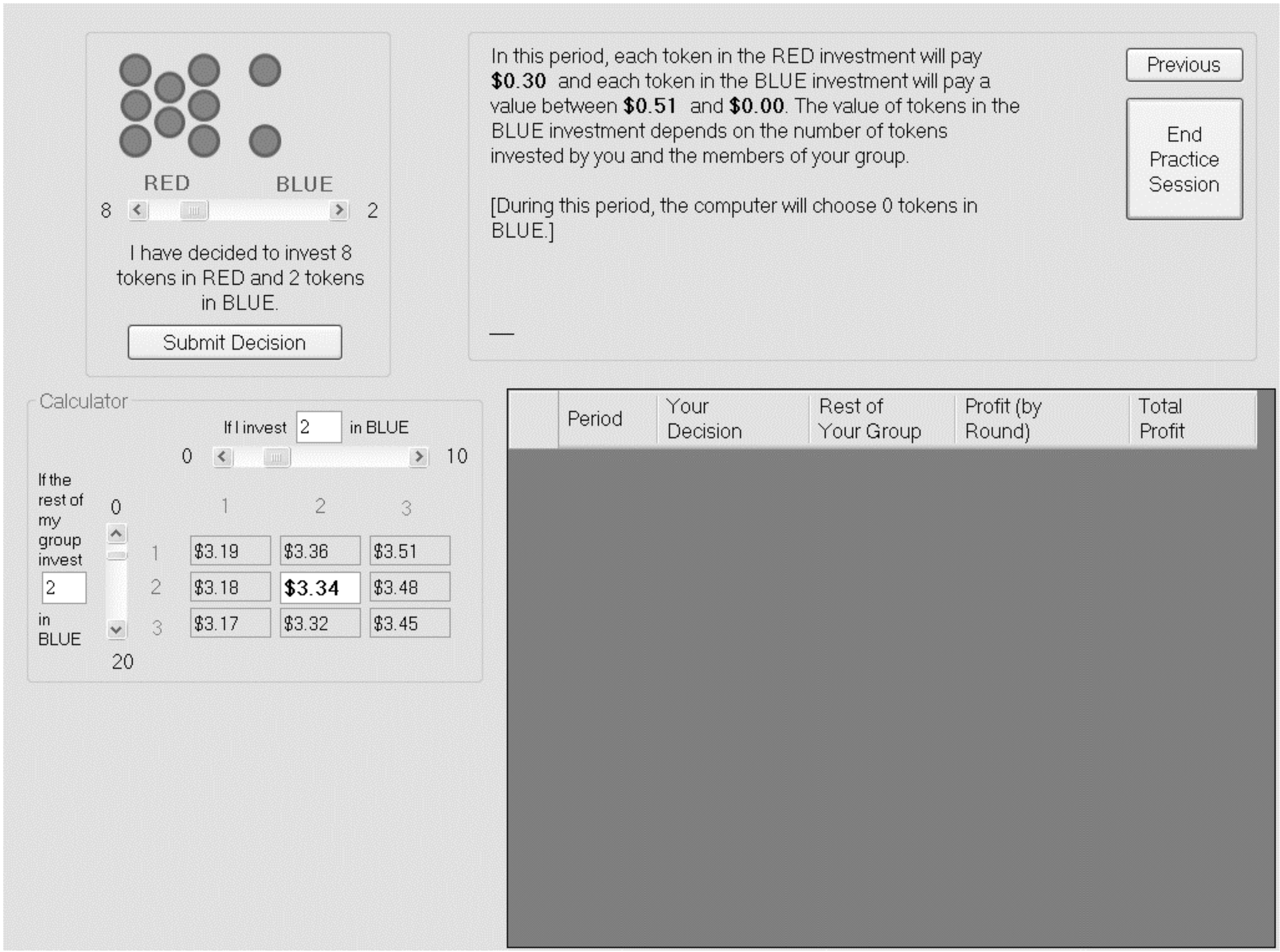

RED
BLUE
10
0
I have decided to invest 10 tokens in RED and 0 tokens in BLUE.
Submit Decision
Calculator
If I invest 0 in BLUE
0
10
If the rest of my group invest 0 in BLUE
0
20
0
1
0
1
$0.00
$0.00
$0.00
$0.00
In each decision you make, tokens in the RED investment will pay a fixed amount per token, 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

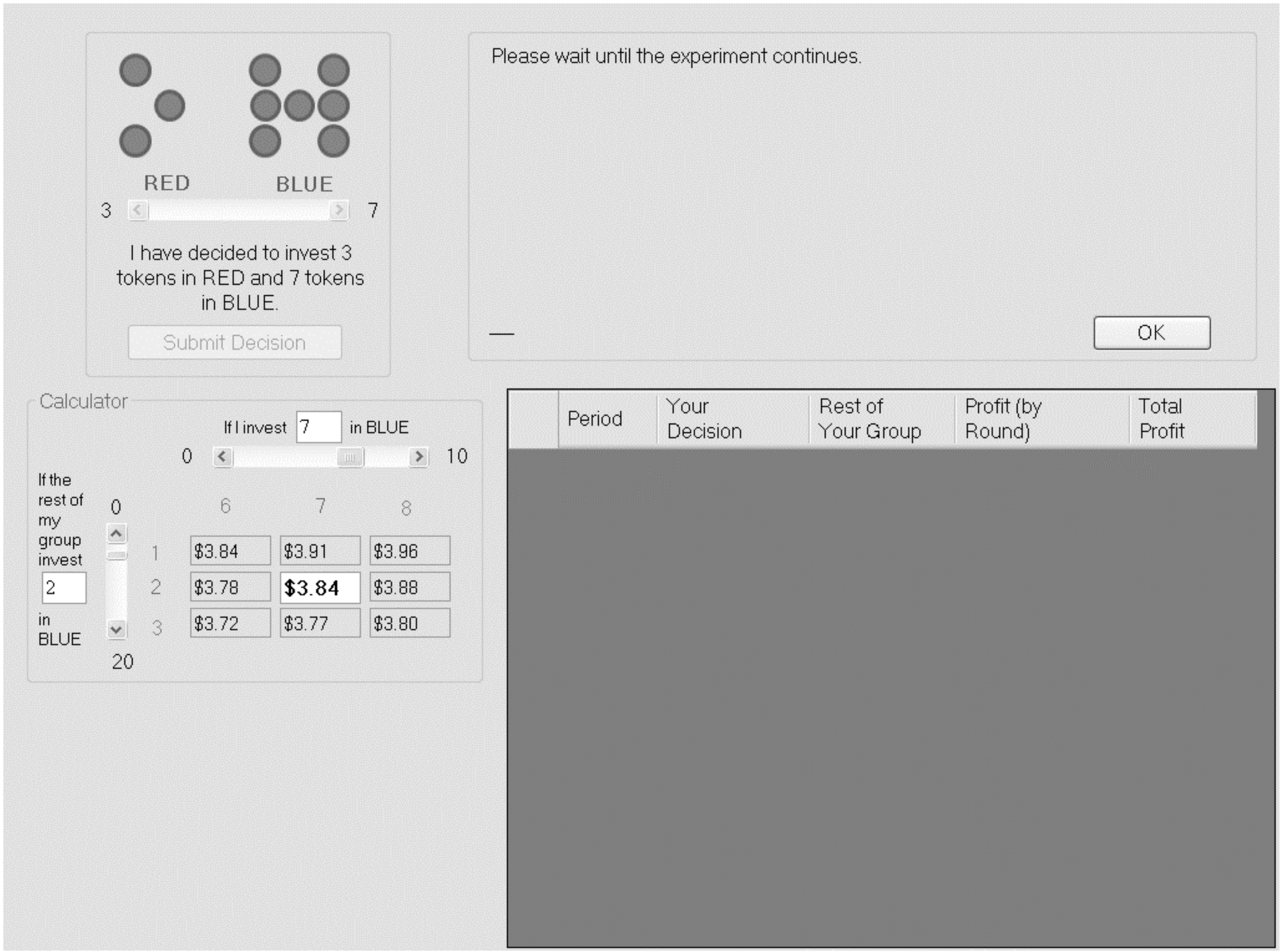

RED
BLUE
8
2
I have decided to invest 8 tokens in RED and 2 tokens in BLUE.
Submit Decision
Calculator
If I invest 2 in BLUE
0
10
If the rest of my group invest 2 in BLUE
0
20
1 2 3
1 $3.19 $3.36 $3.51
2 $3.18 $3.34 $3.48
3 $3.17 $3.32 $3.45
You will be making multiple decisions over the course of the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

Appendix C. Estimates of Per-Capita Expenditure Need by State

State	Hybrid		State		Barebones		Traditional		Actual	
	Need	Rank	Need	Rank	Need	Rank	Need	Rank	Need	Rank
Alaska	$8,177	1	$9,226	1	$7,305	1	$5,995	21	$13,418	1
Hawaii	$6,767	2	$6,938	3	$6,079	33	$5,216	50	$6,828	7
Wyoming	$6,564	3	$6,534	4	$6,313	6	$5,894	23	$7,677	4
Minnesota	$6,560	4	$6,274	11	$6,130	17	$5,553	45	$7,052	5
Connecticut	$6,448	5	$6,199	15	$5,906	42	$5,772	31	$6,789	9
California	$6,434	6	$6,335	6	$5,998	51	$6,211	13	$6,933	6
New Mexico	$6,364	7	$6,303	9	$6,257	3	$6,052	20	$6,225	14
Oregon	$6,282	8	$6,277	10	$6,155	11	$5,605	40	$6,576	10
New Jersey	$6,280	9	$6,043	30	$5,817	48	$5,797	29	$6,094	16
New York	$6,247	10	$6,327	7	$5,877	50	$6,460	5	$8,486	3
District of Columbia	$6,223	11	$7,880	2	$5,992	35	--	--	$10,802	2
Montana	$6,212	12	$6,365	5	$6,414	2	$5,798	28	$5,550	32
Washington	$6,211	13	$6,310	8	$6,091	23	$5,791	30	$6,468	12
Maryland	$6,199	14	$6,123	23	$6,001	43	$5,688	35	$6,024	20
Illinois	$6,193	15	$6,140	19	$6,014	44	$6,126	17	$5,887	22
Michigan	$6,188	16	$6,125	22	$6,068	41	$6,255	10	$6,079	18
Nevada	$6,174	17	$6,251	12	$6,206	5	$5,489	48	$5,732	27
Idaho	$6,174	18	$6,096	26	$6,254	8	$5,880	24	$5,034	46
Iowa	$6,102	19	$5,998	32	$6,168	18	$5,491	47	$5,683	29
Rhode Island	$6,100	20	$6,152	18	$6,069	37	$5,603	41	$6,091	17
North Dakota	$6,061	21	$6,179	16	$6,212	13	$6,113	18	$5,866	24
Virginia	$6,049	22	$5,988	34	$6,066	40	$5,764	32	$5,400	35
Nebraska	$6,041	23	$6,058	27	$6,206	14	$5,619	37	$5,631	30
Maine	$6,037	24	$6,098	25	$6,108	22	$5,593	42	$5,734	26
Vermont	$6,030	25	$6,227	14	$6,100	29	$5,493	46	$6,041	19
Oklahoma	$6,012	26	$6,163	17	$6,157	16	$6,059	19	$5,109	45
Ohio	$6,001	27	$5,945	37	$6,078	46	$5,814	27	$5,772	25

State	Hybrid		State		Barebones		Traditional		Actual	
	Need	Rank	Need	Rank	Need	Rank	Need	Rank	Need	Rank
Delaware	$5,999	28	$6,045	29	$6,032	34	$5,557	44	$6,797	8
South Dakota	$5,994	29	$6,114	24	$6,252	10	$6,291	8	$4,990	48
Massachusetts	$5,992	30	$6,134	21	$6,020	45	$5,709	34	$6,562	11
Utah	$5,982	31	$6,021	31	$6,137	12	$6,181	14	$5,708	28
Arizona	$5,981	32	$6,139	20	$6,114	7	$6,128	16	$5,004	47
Georgia	$5,979	33	$5,885	41	$6,099	36	$6,297	7	$5,617	31
West Virginia	$5,978	34	$5,820	44	$6,111	26	$6,227	12	$5,282	41
Kansas	$5,971	35	$5,948	36	$6,195	15	$5,846	25	$5,498	34
Indiana	$5,961	36	$5,891	39	$6,081	38	$5,908	22	$5,228	43
Colorado	$5,958	37	$6,245	13	$6,126	9	$5,610	38	$6,159	15
Wisconsin	$5,957	38	$5,886	40	$6,113	25	$5,566	43	$6,328	13
Missouri	$5,953	39	$5,975	35	$6,132	20	$5,816	26	$4,985	50
New Hampshire	$5,908	40	$5,997	33	$6,031	30	$5,282	49	$5,116	44
Florida	$5,894	41	$6,053	28	$6,068	49	$5,666	36	$5,360	36
Pennsylvania	$5,889	42	$5,892	38	$6,060	47	$5,609	39	$5,883	23
Texas	$5,884	43	$5,766	47	$6,166	4	$6,456	6	$5,260	42
Tennessee	$5,744	44	$5,865	43	$6,105	32	$6,271	9	$4,987	49
North Carolina	$5,737	45	$5,774	46	$6,102	39	$6,248	11	$5,510	33
Kentucky	$5,733	46	$5,882	42	$6,125	28	$6,141	15	$5,314	37
South Carolina	$5,682	47	$5,747	48	$6,096	31	$5,745	33	$5,903	21
Arkansas	$5,631	48	$5,689	49	$6,154	19	$6,539	3	$4,746	51
Louisiana	$5,548	49	$5,781	45	$6,102	27	$6,631	2	$5,287	40
Alabama	$5,448	50	$5,558	51	$6,115	24	$6,492	4	$5,308	38
Mississippi	$5,442	51	$5,672	50	$6,162	21	$6,800	1	$5,296	39

Appendix D. Estimates of Per-Capita Expenditure Need by Sub-State Area

Name	Hybrid		Actual		Difference	
	Need	Rank	Need	Rank	Amount	Rank
Alaska - Rural	$7,857	1	$13,694	1	$5,837	1
Anchorage, AK MSA	$7,768	2	$13,029	2	$5,261	2
Massachusetts - Rural	$7,096	3	$10,010	3	$2,914	3
Fresno, CA MSA	$6,983	4	$6,821	37	-$162	170
Visalia--Tulare--Porterville, CA MSA	$6,925	5	$7,850	7	$925	36
Merced, CA MSA	$6,893	6	$7,042	24	$149	120
Bakersfield, CA MSA	$6,783	7	$7,680	10	$897	40
McAllen--Edinburg--Mission, TX MSA	$6,758	8	$5,343	195	-$1,415	320
Hawaii - Rural	$6,751	9	$7,014	25	$263	101
Laredo, TX MSA	$6,644	10	$5,894	118	-$750	271
Redding, CA MSA	$6,604	11	$6,663	51	$59	132
California - Rural	$6,558	12	$6,944	30	$386	89
Brownsville--Harlingen--San Benito, TX MSA	$6,506	13	$5,234	211	-$1,272	314
Stockton--Lodi, CA MSA	$6,477	14	$6,841	36	$364	92
Wyoming - Rural	$6,435	15	$7,951	5	$1,516	12
Chico--Paradise, CA MSA	$6,421	16	$6,338	71	-$83	154
Minneapolis--St. Paul, MN--WI MSA	$6,412	17	$7,222	17	$810	48
Yuba City, CA MSA	$6,389	18	$6,508	59	$119	123
Honolulu, HI MSA	$6,364	19	$6,757	40	$393	87
Hartford, CT MSA	$6,340	20	$6,427	66	$87	126
New Mexico - Rural	$6,307	21	$6,265	78	-$42	147
Modesto, CA MSA	$6,305	22	$6,712	47	$407	83
Yakima, WA MSA	$6,304	23	$5,942	113	-$362	216
Sacramento--Yolo, CA CMSA	$6,302	24	$7,012	26	$710	54
Rochester, MN MSA	$6,225	25	$6,875	33	$650	60
Utah - Rural	$6,219	26	$6,121	93	-$98	159

Name	Hybrid		Actual		Difference	
	Need	Rank	Need	Rank	Amount	Rank
Rochester, NY MSA	$6,205	27	$7,374	14	$1,169	24
Richland--Kennewick--Pasco, WA MSA	$6,190	28	$6,183	87	-$7	140
Colorado - Rural	$6,185	29	$6,741	41	$556	65
El Paso, TX MSA	$6,176	30	$5,248	208	-$928	294
Los Angeles--Riverside--Orange County, CA CMSA	$6,149	31	$6,722	44	$573	63
Arizona - Rural	$6,145	32	$4,672	291	-$1,473	321
Minnesota - Rural	$6,131	33	$6,732	42	$601	61
New London--Norwich, CT--RI MSA	$6,120	34	$7,533	11	$1,413	17
Flagstaff, AZ--UT MSA	$6,117	35	$5,214	216	-$903	291
Eugene--Springfield, OR MSA	$6,110	36	$6,521	57	$411	82
Duluth--Superior, MN--WI MSA	$6,102	37	$7,138	20	$1,036	30
Santa Fe, NM MSA	$6,090	38	$6,627	52	$537	66
Washington - Rural	$6,083	39	$6,310	73	$227	107
Grand Forks, ND--MN MSA	$6,058	40	$6,937	31	$879	42
Montana - Rural	$6,055	41	$5,672	142	-$383	218
New York--Northern New Jersey--Long Island, NY--NJ--CT--PA CMSA	$6,053	42	$7,963	4	$1,910	6
Rocky Mount, NC MSA	$6,045	43	$6,423	67	$378	90
Portland--Salem, OR--WA CMSA	$6,019	44	$6,689	48	$670	59
Detroit--Ann Arbor--Flint, MI CMSA	$6,018	45	$6,420	68	$402	84
Fort Collins--Loveland, CO MSA	$6,017	46	$5,436	178	-$581	252
Casper, WY MSA	$6,006	47	$6,846	35	$840	45
Nevada - Rural	$6,001	48	$4,679	288	-$1,322	317
Chicago--Gary--Kenosha, IL--IN--WI CMSA	$5,992	49	$6,237	81	$245	105
San Francisco--Oakland--San Jose, CA CMSA	$5,989	50	$7,480	13	$1,491	15
Springfield, MA MSA	$5,980	51	$6,432	63	$452	75
Oregon - Rural	$5,975	52	$6,291	74	$316	97
Miami--Fort Lauderdale, FL CMSA	$5,959	53	$6,117	95	$158	116
Washington--Baltimore, DC--MD--VA--WV CMSA	$5,951	54	$6,431	64	$480	70
Reno, NV MSA	$5,951	55	$5,828	121	-$123	165

Name	Hybrid		Actual		Difference	
	Need	Rank	Need	Rank	Amount	Rank
Pueblo, CO MSA	$5,950	56	$4,957	257	-$993	300
Spokane, WA MSA	$5,949	57	$5,778	128	-$171	173
Bismarck, ND MSA	$5,946	58	$5,741	132	-$205	181
San Luis Obispo--Atascadero--Paso Robles, CA MSA	$5,944	59	$6,194	84	$250	103
Idaho - Rural	$5,938	60	$4,969	254	-$969	298
Indianapolis, IN MSA	$5,922	61	$5,716	135	-$206	182
Medford--Ashland, OR MSA	$5,918	62	$5,911	116	-$7	140
Portland, ME MSA	$5,909	63	$5,967	111	$58	133
Norfolk--Virginia Beach--Newport News, VA--NC MSA	$5,902	64	$5,784	126	-$118	164
Lawton, OK MSA	$5,897	65	$5,469	175	-$428	227
Corvallis, OR MSA	$5,897	66	$6,050	103	$153	118
Albuquerque, NM MSA	$5,897	67	$6,151	88	$254	102
Bangor, ME MSA	$5,895	68	$5,835	120	-$60	150
Green Bay, WI MSA	$5,894	69	$6,964	28	$1,070	28
West Palm Beach--Boca Raton, FL MSA	$5,891	70	$5,738	134	-$153	168
Cincinnati--Hamilton, OH--KY--IN CMSA	$5,884	71	$5,744	131	-$140	167
Lima, OH MSA	$5,881	72	$5,372	190	-$509	239
Omaha, NE--IA MSA	$5,880	73	$5,628	155	-$252	195
Iowa - Rural	$5,879	74	$5,496	173	-$383	218
Burlington, VT MSA	$5,878	75	$6,065	99	$187	112
Mansfield, OH MSA	$5,878	76	$5,309	200	-$569	249
Kalamazoo--Battle Creek, MI MSA	$5,876	77	$5,652	150	-$224	187
Las Vegas, NV--AZ MSA	$5,876	78	$5,741	133	-$135	166
Boise City, ID MSA	$5,874	79	$5,059	242	-$815	281
Janesville--Beloit, WI MSA	$5,873	80	$6,312	72	$439	77
Columbus, OH MSA	$5,871	81	$6,191	85	$320	95
Salinas, CA MSA	$5,870	82	$7,491	12	$1,621	10
Utica--Rome, NY MSA	$5,865	83	$7,102	21	$1,237	22
Bellingham, WA MSA	$5,861	84	$5,799	124	-$62	151

Name	Hybrid		Actual		Difference	
	Need	Rank	Need	Rank	Amount	Rank
Michigan - Rural	$5,861	85	$5,609	160	-$252	195
Las Cruces, NM MSA	$5,859	86	$6,010	107	$151	119
Binghamton, NY MSA	$5,859	87	$6,975	27	$1,116	25
Kansas City, MO--KS MSA	$5,858	88	$5,627	157	-$231	189
Seattle--Tacoma--Bremerton, WA CMSA	$5,858	89	$6,714	46	$856	44
Jamestown, NY MSA	$5,855	90	$7,714	9	$1,859	7
Des Moines, IA MSA	$5,853	91	$6,058	100	$205	111
South Dakota - Rural	$5,843	92	$4,923	262	-$920	293
Kokomo, IN MSA	$5,840	93	$5,244	209	-$596	256
Davenport--Moline--Rock Island, IA--IL MSA	$5,840	94	$5,506	172	-$334	206
Lansing--East Lansing, MI MSA	$5,837	95	$5,977	110	$140	121
Elmira, NY MSA	$5,833	96	$6,820	38	$987	34
Enid, OK MSA	$5,830	97	$4,575	301	-$1,255	313
Yuma, AZ MSA	$5,830	98	$4,642	295	-$1,188	311
Cheyenne, WY MSA	$5,826	99	$7,193	19	$1,367	18
Florida - Rural	$5,826	100	$4,431	311	-$1,395	319
Philadelphia--Wilmington--Atlantic City, PA--NJ--DE--MD CMSA	$5,825	101	$6,530	56	$705	56
Providence--Fall River--Warwick, RI--MA MSA	$5,824	102	$6,864	34	$1,040	29
Springfield, IL MSA	$5,819	103	$5,296	203	-$523	241
Tulsa, OK MSA	$5,819	104	$5,256	205	-$563	246
St. Louis, MO--IL MSA	$5,811	105	$5,209	217	-$602	257
Illinois - Rural	$5,809	106	$4,997	252	-$812	280
Alexandria, LA MSA	$5,808	107	$5,195	223	-$613	258
West Virginia - Rural	$5,807	108	$5,310	199	-$497	238
Rapid City, SD MSA	$5,807	109	$5,288	204	-$519	240
Sioux City, IA--NE MSA	$5,806	110	$5,749	129	-$57	149
Cleveland--Akron, OH CMSA	$5,805	111	$6,276	77	$471	72
Buffalo--Niagara Falls, NY MSA	$5,804	112	$7,076	22	$1,272	20
Grand Junction, CO MSA	$5,787	113	$5,195	222	-$592	254

Name	Hybrid		Actual		Difference	
	Need	Rank	Need	Rank	Amount	Rank
Tucson, AZ MSA	$5,780	114	$4,792	275	-$988	299
Georgia - Rural	$5,776	115	$5,520	170	-$256	197
Saginaw--Bay City--Midland, MI MSA	$5,771	116	$5,656	148	-$115	162
Dayton--Springfield, OH MSA	$5,766	117	$5,652	149	-$114	161
Pittsburgh, PA MSA	$5,765	118	$6,188	86	$423	80
Joplin, MO MSA	$5,765	119	$4,206	322	-$1,559	323
Barnstable--Yarmouth, MA MSA	$5,764	120	$6,665	50	$901	39
Rockford, IL MSA	$5,763	121	$5,513	171	-$250	193
Great Falls, MT MSA	$5,761	122	$5,230	213	-$531	243
Memphis, TN--AR--MS MSA	$5,757	123	$5,664	144	-$93	157
Longview--Marshall, TX MSA	$5,757	124	$4,653	294	-$1,104	307
Columbia, SC MSA	$5,757	125	$6,069	98	$312	98
Cedar Rapids, IA MSA	$5,756	126	$6,230	82	$474	71
Odessa--Midland, TX MSA	$5,755	127	$6,052	102	$297	99
Glens Falls, NY MSA	$5,753	128	$7,273	16	$1,520	11
San Antonio, TX MSA	$5,751	129	$5,326	198	-$425	226
San Diego, CA MSA	$5,750	130	$6,579	54	$829	46
Lewiston--Auburn, ME MSA	$5,749	131	$5,330	197	-$419	225
Lincoln, NE MSA	$5,743	132	$5,628	154	-$115	162
Pine Bluff, AR MSA	$5,741	133	$4,676	290	-$1,065	302
Danville, VA MSA	$5,737	134	$4,661	292	-$1,076	303
Madison, WI MSA	$5,736	135	$6,434	61	$698	57
Oklahoma - Rural	$5,736	136	$4,875	269	-$861	286
Fargo--Moorhead, ND--MN MSA	$5,732	137	$6,542	55	$810	48
North Dakota - Rural	$5,730	138	$5,564	163	-$166	172
Peoria--Pekin, IL MSA	$5,730	139	$5,113	235	-$617	259
Greenville, NC MSA	$5,729	140	$5,370	191	-$359	215
Wausau, WI MSA	$5,729	141	$6,469	60	$740	52
Dallas--Fort Worth, TX CMSA	$5,727	142	$5,379	187	-$348	211

Name	Hybrid		Actual		Difference	
	Need	Rank	Need	Rank	Amount	Rank
Topeka, KS MSA	$5,727	143	$5,405	183	-$322	204
Maryland - Rural	$5,724	144	$5,806	123	$82	127
Atlanta, GA MSA	$5,723	145	$5,660	147	-$63	152
Billings, MT MSA	$5,722	146	$5,477	174	-$245	192
Pocatello, ID MSA	$5,722	147	$5,563	165	-$159	169
Benton Harbor, MI MSA	$5,719	148	$5,252	206	-$467	231
Toledo, OH MSA	$5,718	149	$5,784	125	$66	130
Iowa City, IA MSA	$5,713	150	$5,176	226	-$537	245
Vermont - Rural	$5,712	151	$6,030	105	$318	96
Grand Rapids--Muskegon--Holland, MI MSA	$5,708	152	$5,703	137	-$5	138
Syracuse, NY MSA	$5,708	153	$7,202	18	$1,494	14
Macon, GA MSA	$5,707	154	$6,718	45	$1,011	32
Nebraska - Rural	$5,706	155	$5,701	138	-$5	138
Oklahoma City, OK MSA	$5,706	156	$5,308	201	-$398	222
Augusta--Aiken, GA--SC MSA	$5,705	157	$5,061	241	-$644	262
Panama City, FL MSA	$5,700	158	$5,815	122	$115	124
Maine - Rural	$5,699	159	$5,609	159	-$90	156
Wisconsin - Rural	$5,698	160	$6,092	96	$394	86
Wichita, KS MSA	$5,696	161	$5,202	220	-$494	237
Wilmington, NC MSA	$5,692	162	$7,795	8	$2,103	5
Boston--Worcester--Lawrence, MA--NH--ME--CT CMSA	$5,690	163	$6,150	89	$460	73
Muncie, IN MSA	$5,689	164	$4,560	303	-$1,129	309
Columbus, GA--AL MSA	$5,689	165	$4,894	266	-$795	277
Richmond--Petersburg, VA MSA	$5,688	166	$5,374	189	-$314	202
Abilene, TX MSA	$5,686	167	$4,385	314	-$1,301	315
St. Cloud, MN MSA	$5,684	168	$6,390	70	$706	55
Colorado Springs, CO MSA	$5,678	169	$5,748	130	$70	128
South Bend, IN MSA	$5,675	170	$5,402	184	-$273	199
Missoula, MT MSA	$5,675	171	$5,152	230	-$523	241

Name	Hybrid		Actual		Difference	
	Need	Rank	Need	Rank	Amount	Rank
Cumberland, MD--WV MSA	$5,675	172	$5,438	177	-$237	191
Elkhart--Goshen, IN MSA	$5,667	173	$4,924	261	-$743	270
New York - Rural	$5,665	174	$7,346	15	$1,681	9
Roanoke, VA MSA	$5,660	175	$5,173	227	-$487	236
Wichita Falls, TX MSA	$5,657	176	$4,309	318	-$1,348	318
Pittsfield, MA MSA	$5,656	177	$6,433	62	$777	50
Kansas – Rural	$5,654	178	$5,605	161	-$49	148
Albany--Schenectady--Troy, NY MSA	$5,649	179	$6,916	32	$1,267	21
Albany, GA MSA	$5,648	180	$6,023	106	$375	91
Champaign--Urbana, IL MSA	$5,644	181	$5,302	202	-$342	208
Canton--Massillon, OH MSA	$5,641	182	$5,047	245	-$594	255
Charleston, WV MSA	$5,635	183	$5,384	186	-$251	194
Savannah, GA MSA	$5,633	184	$5,663	145	$30	135
Sheboygan, WI MSA	$5,632	185	$6,211	83	$579	62
Milwaukee--Racine, WI CMSA	$5,630	186	$6,514	58	$884	41
Beaumont--Port Arthur, TX MSA	$5,627	187	$4,788	276	-$839	284
Lakeland--Winter Haven, FL MSA	$5,625	188	$4,431	310	-$1,194	312
Amarillo, TX MSA	$5,622	189	$4,689	284	-$933	296
Decatur, IL MSA	$5,620	190	$4,956	258	-$664	264
Salt Lake City--Ogden, UT MSA	$5,619	191	$5,641	153	$22	136
Corpus Christi, TX MSA	$5,618	192	$4,921	263	-$697	269
Pensacola, FL MSA	$5,613	193	$4,684	287	-$929	295
Raleigh--Durham--Chapel Hill, NC MSA	$5,613	194	$5,450	176	-$163	171
Eau Claire, WI MSA	$5,609	195	$6,055	101	$446	76
Johnstown, PA MSA	$5,605	196	$5,251	207	-$354	213
Waterloo--Cedar Falls, IA MSA	$5,603	197	$5,564	164	-$39	146
Evansville--Henderson, IN--KY MSA	$5,602	198	$4,958	256	-$644	262
Myrtle Beach, SC MSA	$5,602	199	$6,686	49	$1,084	27
Virginia - Rural	$5,601	200	$4,284	320	-$1,317	316

Name	Hybrid		Actual		Difference	
	Need	Rank	Need	Rank	Amount	Rank
Youngstown--Warren, OH MSA	$5,599	201	$5,014	249	-$585	253
Dubuque, IA MSA	$5,598	202	$5,379	188	-$219	186
Phoenix--Mesa, AZ MSA	$5,597	203	$5,166	228	-$431	228
Biloxi--Gulfport--Pascagoula, MS MSA	$5,597	204	$6,285	75	$688	58
Nashville, TN MSA	$5,595	205	$5,122	233	-$473	233
Delaware - Rural	$5,589	206	$6,588	53	$999	33
Shreveport--Bossier City, LA MSA	$5,587	207	$5,192	224	-$395	221
Victoria, TX MSA	$5,587	208	$6,075	97	$488	68
Terre Haute, IN MSA	$5,584	209	$4,487	309	-$1,097	305
Huntington--Ashland, WV--KY--OH MSA	$5,577	210	$5,231	212	-$346	209
Lynchburg, VA MSA	$5,575	211	$4,678	289	-$897	290
Sharon, PA MSA	$5,574	212	$5,134	231	-$440	229
San Angelo, TX MSA	$5,569	213	$4,096	323	-$1,473	321
Dover, DE MSA	$5,567	214	$6,765	39	$1,198	23
Charlotte--Gastonia--Rock Hill, NC--SC MSA	$5,566	215	$6,281	76	$715	53
Fort Wayne, IN MSA	$5,562	216	$4,869	270	-$693	268
Jacksonville, FL MSA	$5,560	217	$5,086	239	-$474	234
Lake Charles, LA MSA	$5,559	218	$5,680	140	$121	122
Indiana - Rural	$5,551	219	$4,878	268	-$673	266
Bloomington--Normal, IL MSA	$5,545	220	$5,521	169	-$24	143
Springfield, MO MSA	$5,541	221	$4,777	277	-$764	273
New Hampshire - Rural	$5,539	222	$5,347	193	-$192	177
St. Joseph, MO MSA	$5,538	223	$4,686	286	-$852	285
Jackson, TN MSA	$5,537	224	$7,947	6	$2,410	4
Santa Barbara--Santa Maria--Lompoc, CA MSA	$5,530	225	$7,044	23	$1,514	13
Charleston--North Charleston, SC MSA	$5,530	226	$5,932	114	$402	84
Pennsylvania - Rural	$5,527	227	$4,960	255	-$567	248
Reading, PA MSA	$5,522	228	$5,869	119	$347	93
Austin--San Marcos, TX MSA	$5,522	229	$5,426	181	-$96	158

Name	Hybrid		Actual		Difference	
	Need	Rank	Need	Rank	Amount	Rank
Denver--Boulder--Greeley, CO CMSA	$5,521	230	$6,265	79	$744	51
Clarksville--Hopkinsville, TN--KY MSA	$5,518	231	$4,694	282	-$824	282
Ohio - Rural	$5,518	232	$5,332	196	-$186	176
Killeen--Temple, TX MSA	$5,514	233	$4,640	297	-$874	289
Melbourne--Titusville--Palm Bay, FL MSA	$5,512	234	$4,744	278	-$768	274
Missouri - Rural	$5,511	235	$4,390	312	-$1,121	308
Fort Walton Beach, FL MSA	$5,509	236	$4,605	299	-$904	292
Sherman--Denison, TX MSA	$5,507	237	$4,642	296	-$865	288
Chattanooga, TN--GA MSA	$5,505	238	$5,661	146	$156	117
La Crosse, WI--MN MSA	$5,503	239	$6,410	69	$907	38
Fort Pierce--Port St. Lucie, FL MSA	$5,498	240	$4,823	273	-$675	267
Appleton--Oshkosh--Neenah, WI MSA	$5,496	241	$5,979	108	$483	69
Williamsport, PA MSA	$5,495	242	$5,428	180	-$67	153
Harrisburg--Lebanon--Carlisle, PA MSA	$5,489	243	$5,922	115	$433	79
Fort Smith, AR--OK MSA	$5,486	244	$4,816	274	-$670	265
Allentown--Bethlehem--Easton, PA MSA	$5,483	245	$5,977	109	$494	67
Decatur, AL MSA	$5,482	246	$5,551	167	$69	129
Louisville, KY--IN MSA	$5,477	247	$5,650	151	$173	113
Johnson City--Kingsport--Bristol, TN--VA MSA	$5,469	248	$4,370	316	-$1,099	306
Charlottesville, VA MSA	$5,467	249	$4,712	280	-$755	272
Parkersburg--Marietta, WV--OH MSA	$5,457	250	$5,070	240	-$387	220
Tampa--St. Petersburg--Clearwater, FL MSA	$5,452	251	$4,993	253	-$459	230
Houston--Galveston--Brazoria, TX CMSA	$5,451	252	$5,664	143	$213	110
Fayetteville, NC MSA	$5,445	253	$5,902	117	$457	74
Naples, FL MSA	$5,444	254	$5,244	210	-$200	180
State College, PA MSA	$5,444	255	$4,489	308	-$955	297
Jacksonville, NC MSA	$5,443	256	$4,357	317	-$1,086	304
Lubbock, TX MSA	$5,439	257	$5,208	219	-$231	189
Erie, PA MSA	$5,437	258	$5,548	168	$111	125

Name	Hybrid		Actual		Difference	
	Need	Rank	Need	Rank	Amount	Rank
Altoona, PA MSA	$5,437	259	$4,655	293	-$782	276
Knoxville, TN MSA	$5,437	260	$4,858	271	-$579	251
Kentucky - Rural	$5,436	261	$5,122	232	-$314	202
Greensboro--Winston-Salem--High Point, NC MSA	$5,427	262	$5,018	248	-$409	224
Orlando, FL MSA	$5,424	263	$5,596	162	$172	114
Monroe, LA MSA	$5,423	264	$5,700	139	$277	100
Asheville, NC MSA	$5,421	265	$5,228	215	-$193	178
Lancaster, PA MSA	$5,416	266	$5,209	218	-$207	183
Texarkana, TX--Texarkana, AR MSA	$5,416	267	$4,583	300	-$833	283
Baton Rouge, LA MSA	$5,415	268	$5,057	243	-$358	214
Sarasota--Bradenton, FL MSA	$5,411	269	$5,627	156	$216	108
North Carolina - Rural	$5,409	270	$5,200	221	-$209	184
Jackson, MI MSA	$5,407	271	$5,643	152	$236	106
Columbia, MO MSA	$5,406	272	$5,008	251	-$398	222
Birmingham, AL MSA	$5,406	273	$5,966	112	$560	64
Tallahassee, FL MSA	$5,398	274	$5,049	244	-$349	212
Arkansas - Rural	$5,395	275	$4,532	305	-$863	287
Jackson, MS MSA	$5,389	276	$5,110	237	-$279	200
Lawrence, KS MSA	$5,376	277	$5,415	182	$39	134
Sioux Falls, SD MSA	$5,374	278	$5,028	247	-$346	209
Owensboro, KY MSA	$5,373	279	$5,622	158	$249	104
Provo--Orem, UT MSA	$5,370	280	$5,350	192	-$20	142
Lexington, KY MSA	$5,369	281	$5,345	194	-$24	143
Gainesville, FL MSA	$5,361	282	$4,724	279	-$637	261
New Orleans, LA MSA	$5,356	283	$5,677	141	$321	94
Ocala, FL MSA	$5,351	284	$4,216	321	-$1,135	310
South Carolina - Rural	$5,342	285	$5,779	127	$437	78
Fayetteville--Springdale--Rogers, AR MSA	$5,330	286	$4,850	272	-$480	235
Waco, TX MSA	$5,323	287	$5,713	136	$390	88

Name	Hybrid		Actual		Difference	
	Need	Rank	Need	Rank	Amount	Rank
Fort Myers--Cape Coral, FL MSA	$5,321	288	$6,137	90	$816	47
Punta Gorda, FL MSA	$5,318	289	$4,546	304	-$772	275
Texas - Rural	$5,315	290	$4,698	281	-$617	259
Huntsville, AL MSA	$5,311	291	$6,245	80	$934	35
Wheeling, WV--OH MSA	$5,310	292	$5,111	236	-$199	179
Tennessee - Rural	$5,303	293	$4,305	319	-$998	301
Steubenville--Weirton, OH--WV MSA	$5,301	294	$4,935	259	-$366	217
Lafayette, IN MSA	$5,300	295	$4,501	307	-$799	278
Hattiesburg, MS MSA	$5,300	296	$6,723	43	$1,423	16
Little Rock--North Little Rock, AR MSA	$5,292	297	$5,114	234	-$178	175
Jonesboro, AR MSA	$5,266	298	$4,932	260	-$334	206
Daytona Beach, FL MSA	$5,264	299	$5,178	225	-$86	155
Mobile, AL MSA	$5,260	300	$5,229	214	-$31	145
Greenville--Spartanburg--Anderson, SC MSA	$5,259	301	$6,121	94	$862	43
Tuscaloosa, AL MSA	$5,259	302	$6,953	29	$1,694	8
Hickory--Morganton--Lenoir, NC MSA	$5,258	303	$5,154	229	-$104	160
Sumter, SC MSA	$5,232	304	$5,008	250	-$224	187
Florence, SC MSA	$5,232	305	$5,394	185	$162	115
York, PA MSA	$5,216	306	$5,430	179	$214	109
Bloomington, IN MSA	$5,215	307	$4,885	267	-$330	205
Scranton--Wilkes-Barre--Hazleton, PA MSA	$5,212	308	$5,038	246	-$174	174
Tyler, TX MSA	$5,190	309	$4,382	315	-$808	279
Goldsboro, NC MSA	$5,181	310	$4,916	265	-$265	198
Gadsden, AL MSA	$5,165	311	$4,633	298	-$532	244
Anniston, AL MSA	$5,147	312	$5,560	166	$413	81
Florence, AL MSA	$5,130	313	$6,045	104	$915	37
Louisiana - Rural	$5,128	314	$4,919	264	-$209	184
Athens, GA MSA	$5,108	315	$6,126	92	$1,018	31
Alabama - Rural	$5,094	316	$4,528	306	-$566	247

Name	Hybrid		Actual		Difference	
	Need	Rank	Need	Rank	Amount	Rank
Dothan, AL MSA	$5,084	317	$6,428	65	$1,344	19
Montgomery, AL MSA	$5,044	318	$4,575	302	-$469	232
Mississippi – Rural	$5,042	319	$5,104	238	$62	131
Houma, LA MSA	$5,024	320	$6,132	91	$1,108	26
Lafayette, LA MSA	$4,991	321	$4,693	283	-$298	201
Bryan--College Station, TX MSA	$4,963	322	$4,388	313	-$575	250
Auburn--Opelika, AL MSA	$4,682	323	$4,686	285	$4	137

Appendix E. Workload and Expenditure Need Calculations Under the ACIR Approach

The ACIR method requires demographic data at the state level for all states. In particular, the following data are required:

From the U.S. Census Bureau:

- Total population
- Population age cohorts:
    - 5 and 13
    - 14 - 17
    - 18 - 24
    - 25 - 34
    - 34 and over
- Private school attendance
- K-8
- 9-12 (High School)
- Population living in poverty
- Population living below 1.5 times poverty line
- Population under 18 living in poverty
- Population between 16-64 with work disability

From the Federal Highway Administration:

- Vehicle miles traveled
- Lane-miles of streets and road

From the Federal Bureau of Investigation's Uniform Crime Reports:

- Number of murders and non-negligent manslaughters

Because the workloads are the basic factor by which the RES apportions national expenditures by expenditure category and state, these statistics provide the main variation in expenditures. In particular, because expenditure need is presented in per-capita terms, and because poverty, population by age, and crime make up such a large portion of the workloads, the RES method largely provides a measure of poverty, youth, and the murder rate. The workloads are determined for seven different categories of expenditure: (1) elementary and secondary education, (2) higher education, (3) public welfare, (4) health and hospitals, (5) highways, (6) police and corrections, and (7) all other expenditures.

The ACIR report places weights on demographic statistics in calculating the workloads for each of these categories of expenditures, but does not present these weights clearly. The following equations are adjusted so that in every case, the sum of workloads across states equals one.[23]

$$ESeduc^i = 0.324\left(\frac{p^i_{5-13} - prvsch^i_{K-8}}{p^{US}_{5-13} - prvsch^{US}_{K-8}}\right) + 0.54\left(\frac{p^i_{14-17} - prvsch^i_{HS}}{p^{US}_{14-17} - prvsch^{US}_{HS}}\right) + 0.135\left(\frac{ppvty^i_{<18}}{ppvty^{US}_{<18}}\right)$$

$$HIeduc^i = 0.046\left(\frac{p^i_{14-17}}{p^{US}_{14-17}}\right) + 0.781\left(\frac{p^i_{18-24}}{p^{US}_{18-24}}\right) + 0.148\left(\frac{p^i_{25-34}}{p^{US}_{25-34}}\right) + 0.029\left(\frac{p^i_{>34}}{p^{US}_{>34}}\right)$$

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[23] Note: these workloads will not match exactly the ones used in either the ACIR study or the GAO study (which are unreported), because neither study explicitly normalizes the workloads. Nonetheless, these equations are just normalized versions of the ACIR workloads, and the results that arise are comparable to those reported in the ACIR study.

In these equations, superscript "i" indexes states, and "US" refers to the national total. Numerical subscripts refer to age ranges and the subscripts "K-8" and "HS" refer to school grade. the rest of the abbreviations are as follows: ESeduc = Elementary and Secondary education workload; HIeduc = Higher education workload; pubwelf = Public welfare workload; highways = Highways workload; police = Police and Corrections workload; allother = All other expenditures workload; p = population; prvsch = private school enrollment; ppvty (1.5*pvrty) = population living below (1.5 times) the poverty line; pwrkdsabl = population with work disabilities; VMT = vehicle miles traveled; lanemiles = lane-miles of road and highway; murders = # of murders.

$$pubwelf^i = \frac{ppvty^i}{ppvty^{US}}$$

$$health^i = \frac{1}{3}\left(\frac{p^i}{p^{US}}\right) + \frac{1}{3}\left(\frac{pwrkdsabl^i_{16-64}}{pwrdsabl^{US}_{16-64}}\right) + \frac{1}{3}\left(\frac{ppvty^i_{1.5*pvty}}{ppvty^{US}_{1.5*pvty}}\right)$$

$$highways^i = 0.825\left(\frac{VMT^i}{VMT^{US}}\right) + 0.175\left(\frac{lanemiles^i}{lanemiles^{US}}\right)$$

$$police^i = \frac{1}{3}\left(\frac{p^i}{p^{US}}\right) + \frac{1}{3}\left(\frac{p^i_{18-24}}{p^{US}_{18-24}}\right) + \frac{1}{3}\left(\frac{murders^i}{murders^{US}}\right)$$

$$allother^i = \frac{p^i}{p^{US}}$$

In the ACIR and GAO studies, these workloads are adjusted for variations in the cost of labor inputs across states and variations of the relative importance of labor inputs across categories of expenditure. Thus, there is a matrix of input-cost indices across states and categories of expenditure that modifies the representative expenditure level.[24]

The ACIR study combined data on the mean annual earnings of 45-64 year-old males who worked 40 or more weeks in 1979, by years of educational attainment, across states, with data on the portion of national expenditure by category attributable to payroll.

[24] An example of such a matrix is available in Tannenwald and Turner (2004) pp. 87-90.

Appendix F: Proofs of Results in "Evading Nash Traps in Two-Player Simultaneous Games"

Result 1: Every strict Nash equilibrium profile is détente strategic.

Given that (s_i, s_{-i}) is a SNE,

$\forall i, s_i' \in S_i, s_i' \neq s_i : u_i(s_i, s_{-i}) > u_i(s_i', s_{-i})$	Def. of SNE
$\forall i: s_i = s_i^*(s_{-i})$ is unique	Def. of best response
$s_{-i} = s_{-i}^*(s_i)$ is unique	Def. of counter-response
$s_{-i} = s_{-i}^*\big(s_i^*(s_{-i})\big)$	Substitution
$\forall i: (s_i, s_{-i}) = \Big(s_i^*(s_{-i}), s_{-i}^*\big(s_i^*(s_{-i})\big)\Big)$	Substitution
$\forall i: u_i(s_i, s_{-i}) = u_i\Big(s_i^*(s_{-i}), s_{-i}^*\big(s_i^*(s_{-i})\big)\Big)$	Def. of function
$\Rightarrow (s_i, s_{-i})$ is DS	Def. of DS

Result 2: Every no-initiative strategic profile is a détente strategic profile.

A strategy profile is no-initiative strategic if there exists no alternative strategy $s_i^{'}$ for any player such that the counter-response to the modified strategy profile $(s_i^{'}, s_{-i})$ provides a greater payoff to the player. The détente concept requires that this be true only of alternative strategies that are themselves best-responses to a given set of opponents' strategies:

Given that (s_i, s_{-i}) is NIS,

$\forall i, s_i' \in S, s' \in S_i^*(s): u_i(s_i, s_{-i}) \geq u_i(s')$ Def. of NIS

$\left(s_i^*(s_{-i}), s_{-i}^*\left(s_i^*(s_{-i})\right)\right) \in S_i^*(s)$ Def. of initiative-response set

$\forall i: u_i(s_i, s_{-i}) \geq u_i\left(s_i^*(s_{-i}), s_{-i}^*\left(s_i^*(s_{-i})\right)\right)$ Substitution

$\Rightarrow (s_i, s_{-i})$ is a DE Def. of DS

Result 3a: There are profiles that are DS but neither NE nor NIS.

Given that (s_1, s_2) is DS,

$u_1(s_1, s_2) \geq u_1\left(s_1^*(s_2), s_2^*\left(s_1^*(s_2)\right)\right)$ Def. of DS

$u_2(s_1, s_2) \geq u_2\left(s_1^*\left(s_2^*(s_1^*)\right), s_2^*(s_1^*)\right)$ Def. of DS

Assume $s_1^*(s_2) = s_1' \neq s_1, s_2^*(s_1') = s_2' \neq s_2$

$u_1(s_1', s_2) > u_1(s_1, s_2) > u_1(s_1', s_2') \Rightarrow (s_1, s_2)$ is not NE, but is DS.

Assume $\exists s_2'' \neq s_2^*(s_1), s_1'' = s_1^*(s_2''), u_2(s_1, s_2) < u_2(s_1'', s_2'')$

$u_2(s_1, s_2) < u_2(s_1'', s_2'') \Rightarrow u_2(s_1, s_2) < u_2(s_1^*(s_2''), s_2'')$

$\Rightarrow (s_1, s_2)$ is not NIS.

Result 3b: There are profiles that are NE but not NIS, and profiles that are NIS but not NE.

Given that (s_1, s_2) is NE,

Assume $\exists s_2'' \neq s_2^*(s_1), s_1'' = s_1^*(s_2''), u_2(s_1, s_2) < u_2(s_1'', s_2'')$

$u_2(s_1, s_2) < u_2(s_1'', s_2'') \Rightarrow u_2(s_1, s_2) < u_2(s_1^*(s_2''), s_2'')$

$\Rightarrow (s_1, s_2)$ is not NIS.

Given that (s_1, s_2) is NIS,

$u_2(s_1, s_2) \geq u_2(s_1^*(s_2'), s_2') \; \forall s_2' \in S_2$ Def. of NIS

Assume $s_2^*(s_1) = s_2'' \neq s_2, s_1'' = s_1^*(s_2'')$

$u_2(s_1, s_2'') > u_2(s_1, s_2) > u_2(s_1'', s_2'') \Rightarrow (s_1, s_2)$ is NIS, but not NE.

Result 4: In any 2x2 game with both a pure-strategy Nash equilibrium profile and a détente strategic profile, at least one player weakly prefers the détente strategic profile to some pure-strategy Nash equilibrium profile. For purposes of the proof, Figure 21 presents a general form for a 2x2 normal-form game.

	L	R
U	(x_1, y_1)	(x_3, y_3)
D	(x_2, y_2)	(x_4, y_4)

Figure 21. General normal-form 2x2 game.

Assume (D, R) is a DE profile:

Assume there is a pure-strategy NE profile.

Case 1: $D = s_R^*(R), R = s_C^*(D)$, meaning (D, R) is both DE and a NE. Clearly, $u_i(D, R) = u_i(D, R)$.

Case 2: $D = s_R^*(L), L = s_C^*(D)$, making (D, L) a NE

$D = s_R^*(L), L = s_C^*(D)$ Definition of Nash

$u_C(D, R) \geq u_C\left(s_R^*(s_C^*(D)), s_C^*(D)\right)$ Definition of DE

$u_C(D, R) \geq u_C(D, L)$ Substitution

Case 3: $U = s_R^*(L), L = s_C^*(U)$, meaning (U, L) is a NE.

$U = s_R^*(L), L = s_C^*(U)$ Definition of Nash

3a: (U, L) is a SNE ⇒ (U, L) is a DE. Result 1

3b: (U, L) is a weak NE and not DE. Assume

$u_R(U, L) > u_R(D, L) \Rightarrow D \neq s_R^*(L)$

and $u_C(U, L) = u_C(U, R) \Rightarrow R = s_C^*(U)$.

Either

3b1: $u_C(U, L) < u_C\left(s_R^*(s_C^*(U)), s_C^*(U)\right)$ for some détente alternative profile

$u_C(U, L) < u_C(U, L)$ is false by identity

$u_C(U, L) < u_C(U, R)$is false by assumption

3b1': $D = s_R^*(R) and\ u_C(U, L) < u_C(D, R)$

OR

3b2: $u_R(U, L) < u_R(s_R^*(L), s_C^*\left(s_R^*(L)\right))$ for some détente alternative profile

$u_R(U,L) < u_R(U,L)$is false by identity

3b2': $u_R(U,L) < u_R(U,R)$

$u_R(D,R) < u_R(U,L) \Rightarrow u_R(D,R) < u_R(U,R)$ Assumption, transitivity

$\Rightarrow U = s_R^*(R)$

But $u_R(D,R) \geq u_R\left(s_R^*(R), s_C^*\big(s_R^*(R)\big)\right) \Rightarrow u_R(D,R) \geq u_R\big(U, s_C^*(U)\big)$ $=u_R(U,R)$

Therefore $u_R(D,R) \geq u_R(U,L)$.

Result 5: In any 2-player game with both a pure-strategy Nash equilibrium profile and a no-initiative strategic profile, both players weakly prefer the no-initiative strategic profile to all pure-strategy Nash equilibrium profiles.

Assume (s_1, s_2) is a NIS profile, and assume there is a pure-strategy NE: $(s_1'', s_2'') \in S$.

Player 1:

$u_1(s_1,s_2) \geq u_1\big(s_1', s_2^*(s_1')\big)\ \forall s_1' \in S_1$ Def. of NIS

$u_1(s_1,s_2) \geq u_1\big(s_1'', s_2^*(s_1'')\big)$ Substitution

$u_1(s_1,s_2) \geq u_1(s_1'', s_2'')$ Def. of best response

Player 2:

$u_2(s_1,s_2) \geq u_2(s_1^*(s_2'), s_2')\ \forall s_2' \in S_2$ Def. of NIS

$u_2(s_1,s_2) \geq u_2(s_1^*(s_2''), s_2'')$ Substitution

$u_2(s_1,s_2) \geq u_2(s_1'', s_2'')$ Def. of best response

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Vita

Jason James Delaney was born November 2, 1980 in Allentown, Pennsylvania. He received a Bachelor of Arts in English from The Pennsylvania State University in 2002. He worked as an Editorial Assistant and a Production Coordinator for Lippincott Williams and Wilkins between 2003 and 2005. In 2005, he returned to school to begin his graduate work. He received his Masters of Arts in 2010 from Georgia State University. He expects to receive his Doctor of Philosophy degree in Economics from Georgia State University in December 2010. He has accepted a tenure-track faculty position as an Assistant Professor of Economics at the University of Arkansas at Little Rock in Little Rock, Arkansas, to begin August 2010.

During his tenure at Georgia State University, Mr. Delaney served as a research assistant to Professor Paul Ferraro, Professor Erdal Tekin, and Professor James Cox. As an assistant to James Cox, he served as an affiliated graduate student in the Experimental Economics Center (ExCEN). He served as a teaching assistant for Professor Shelby Frost (for Principles of Microeconomics). In Spring 2010, he taught “The Global Economy” as sole instructor.

Mr. Delaney received several grants and awards while attending Georgia State University: he received the Georgia State University Dissertation Grant in 2009, the Jack Blicksilver Scholarship in Economics in 2008, the Harold Ball Award for Economics in 2007, and the Mark E. Schaefer Graduate Fellowship in 2007. In addition, he served as an intern at the Congressional Research Service in Washington, D.C., between May and

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Mr. Delaney’s primary research interests are in applied microeconomics. His areas of specialization are experimental economics, public economics, urban economics and individual choice. His current research programs look at institutions and group behavior, public expenditure policy, and preferences under risk and uncertainty. He published a paper “The Representative Expenditure System and the District of Columbia's Fiscal Need” in the October 2007 issue of *State Tax Notes*. The research funds for “An Experimental Test of the Pigovian Hypothesis” were provided through a Georgia State University Dissertation Grant.

Mr. Delaney has presented his research at seminars at Georgia State University, at IFREE, and at conferences held by the American Economic Association and the Southern Economic Association. He has served as a referee for the journal *Public Finance Review*.

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