Does experience always pay? Welfare and Distribution Effects in Games with Heterogeneously Experienced Players

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Abstract:

Empirical research, intuition and theory suggest that experience pays. Further, two constant-sum game experiments report that experienced participants earn significantly more than inexperienced participants in head-to-head competition. This paper tests the robustness of the experience advantage in four non constant-sum games. In contrast to the constant-sum game results, we find that in all four non constant-sum games experienced and inexperienced players earn statistically similar amounts. We also find that groups with one experienced player rather than none earn statistically more in one game, less in one game and similar amounts in the remaining two games. We discuss several possible explanations for the ineffectiveness of experienced players to either redistribute income in their favor or to increase the income of the groups they are in.

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Does experience always pay? Welfare and Distribution Effects in Games with Heterogeneously Experienced Players

There are many intuitive reasons and empirical studies suggesting markets ubiquitously reward experience. For instance, employers frequently seek experienced applicants and many job postings indicate that experience is required. Further, we feel derelict if we fail to mention an applicant's experience in a letter of recommendation. Similarly, we harm our own job prospects if we downplay our experience.

Empirical evidence shows significant correlation between experience and compensation (see Robert Willis 1986 and Theresa Devine and Nicholas Kiefer 1991 for surveys). Moreover, on-the-job-training, apprenticeships and internships provide experience at significant costs to both individuals and organizations: Sherwin Rosen (1972: p327) states, "(w)orkers demand learning opportunities and are willing to pay for them since their marketable skill or knowledge and subsequent income are increased." Experience can provide institutional details, know-how and relationship understandings: Edward Lazear (1976: p548) states, "(t)hus, it is expected that the growth rate of wages will be related …. also to the amount of time spent on the job …"

As another avenue for the value of experience, learning theories (particularly beliefs based models - see Drew Fudenberg and David Levine 1998 for a review) model people as learning about other participant's behavior. Since people with more experience have more opportunities to learn, more experienced people should be able to more accurately predict, ceteris paribus, market outcomes or other people's behavior and thus earn at least as much as players with less experience. Consistent with this proposition, Martin Dufwenberg et al. 2005 and Robert Slonim 2005 using economic experiments find experienced participants earn significantly more than inexperienced participants when they compete head-to-head. Thus, theory, intuition, and empirical and experimental evidence suggest that experience positively affects compensation. This paper examines the boundaries of the benefits of experience.

The experiments studying the benefits of experience in head-to-head competition with players with heterogeneous amounts of experience to date have examined the income distribution using only constant-sum situations.[1] This paper tests the robustness of the value of experience across non constant-sum games because most economic environments having participants with heterogeneous amounts of experience are not constant-sum (e.g., professional, financial and labor markets). In non constant-sum games, one reason experienced participants may not earn more than inexperienced participants is that experienced participants may not (only) wish to capture value, i.e. to distribute income in their favor, which is the only way to increase one's income in constant-sum environments, but may (also) wish to create value, i.e., increase the population's aggregate welfare, which involves risk in non constant-sum environments.

This paper experimentally examines income distribution and welfare in four well-studied non constant-sum games. We chose the specific games to provide a theoretically diverse set of strategic situations to test the robustness of heterogeneous experience on aggregate welfare and income distribution. They include variants of public goods and coordination games. One game, similar to the public goods game first studied by James Andreoni (1988), has a unique, dominant strategy. Another game, similar to the coordination game first studied by John Van Huyck et al. (1990), has multiple equilibrium strategies. And two games, similar to the traveler's dilemma game first studied by Monica Capra et al. (1999), have a unique equilibrium strategy that is not dominant.

In all these games, attempts to capture value or create value result in different behaviors. Capturing value involves costs to other participants while creating value involves risks to oneself.

[1] Dufwenberg et al. (2005) examine asset markets that pay subjects a randomly determined dividend times the number of shares held at the end of each period. Since the total number of shares is fixed, the payoff to all subjects is the dividend times a constant. Thus, their experiment is constant sum in that subjects influence the distribution of payoffs but cannot influence the total payoff.

For instance, in the public goods game capturing and creating value involve non-cooperative and cooperative behaviors, respectively. However, creating value for an experienced player may also involve teaching inexperienced players by example how to behave altruistically.

This paper tests several hypotheses. First, given the empirical evidence, given the experimental evidence in constant-sum games and given the abundance of anecdotal evidence suggesting employers value experience, we hypothesize that experienced players will earn more than inexperienced players across the games. *Ex-ante*, one may argue that this hypothesis generalizes too much regarding the advantage of experience. However, we can also arrive at this hypothesis if we assume free disposal of knowledge: if players can ignore experience (e.g., when it is detrimental to their income), then experienced players cannot receive a worse outcome than the outcome they would have received had they been inexperienced.[2] We loosely state the first hypothesis for a game (or supergame) G as follows:

(H1): $\pi_j[G] > \pi_k[G] \ \ if \ \ e_j > e_k = 0$

where $\pi_j[G]$ and $\pi_k[G]$ are j's and k's payoff playing G with each other, and e_j and e_k are the number of times j and k have previously played G (i.e., e_j is player j's experience with G).

The first hypothesis may be rejected, as mentioned above, if experienced participants do not attempt to capture value by distributing income in their favor but attempt to create value by increasing aggregate welfare. Thus, the second hypothesis tests whether groups with a mixture of experienced and inexperienced participants earn more than purely inexperienced groups:

(H2): $\sum_{Ji=1}^{Ji=n} \pi_{Ji}[G] > \sum_{Ki=1}^{Ki=n} \pi_{Ki}[G] \ \ if \ \ e_{K1} = ... = e_{Kn} = 0 \ and \ \sum_{Ji=1}^{Ji=n} e_{ji} > 0$

[2] Specifically, we assume a player can ignore her knowledge from experience when interacting with inexperienced players since these players will not know what the experience was. If there are multiple experienced players with common experience, however, then free knowledge disposal may no longer be a reasonable assumption. We control for this concern by including at most only one experienced player in mixed experience groups.

where $\sum_{Ji=1}^{Ji=n}\pi_{Ji}[G]$ and $\sum_{Ki=1}^{Ki=n}\pi_{Ki}[G]$ are group J's and K's total income where at least one member of group J has experience and all members of group K are completely inexperienced. Note that if experience adds monetary value, then either H1, H2 or both must be supported.

To test H1 and H2, we employ the standard *repeated supergame design* in all sessions: subjects repeatedly play a game with the same group within each supergame, but are never paired together in more than one supergame. In the control, identical to past repeated supergame experiments, subjects participate in every round of every supergame and thus always have the same amount of experience. The treatment mixes experienced and inexperienced participants in the same group such that experienced players play in every supergame and inexperienced players play in only one supergame. In both the treatment and control, at the beginning of each game the number of games every subject has played was common knowledge.

We find that in all four non constant-sum games experienced and inexperienced players earn similar amounts in head-to-head competition. We find that groups with one experienced player rather than none earn statistically more in one game, less in one game and similar amounts in the remaining two games. A number of plausible explanations for experienced players' inability to either earn more than less experienced players or to increase the group's welfare are considered, including differences across the structure of the four games (discussed in section 1.C), that some more experienced players are attempting to create value while others are trying to capture value (resulting in the two types adding up to cause no effect in the aggregate data), and idiosyncratic elements of the experimental design.

Section I presents the experimental design and discusses differences across the games that suggest experienced players may be more able to increase the group payoff in some of the games. Section II shows the main results and Section III discusses several factors that may explain why experience does not pay in non constant-sum games. Section IV concludes.

I. Experimental Design[3]

A. The Games

We briefly describe the games since they are well documented in the literature. For each game, we state the Nash Equilibrium (assuming players maximize their monetary payoff) and group welfare maximizing behavior. We also provide a few results from past studies and our control (described below) since the control closely parallels protocols from past studies. These results show that the current subject population behaves similarly to previous subject populations.

PG G*ame 1:* Public Goods (Andreoni 1988). Three players are each endowed with 100 points and must simultaneously choose a contribution c_i from 0, 1, 2, …, to 100. Subject i's payoff $\Pi(i)$ equals the amount not contributed plus 2/3 the sum of the contribution of the group; $\Pi(i) = 100 - c_i + (2/3)\Sigma_{j=1}^{3}(c_j)$. Each 100 points has an exchange rate of $0.40. The unique Nash Equilibrium requires all players to contribute 0. The group's total payoff is maximized when all players contribute 100. Past results (e.g., Andreoni 1988, Rachel Croson 1996) find that the mean first contribution is significantly greater than 0, but depends on conditions (e.g., communications). Research shows that contributions tend to collapse toward the Nash Equilibrium in the final round of the finitely repeated game. We find in our control that the mean first round contribution is 50.1 and the mean last round contribution is less than 10.

CO G*ame 2*: Weak Link Coordination (Van Huyck et al. 1990). Three players simultaneously choose an integer from 1 to 7. Player i's payoff $\Pi(i)$ is $60 + 20 * MIN - 10 * n_i$, where n_i is player i's choice and MIN equals the minimum choice in the group. Each 100 points has an exchange rate of $0.40. There are seven equilibrium outcomes each requiring players to choose the same number. Payoffs are maximized when all players choose seven. Roberto Weber et al. (2001) show

[3] Instructions/protocols and the Z-tree code for specific games are available from the authors.

that behavior depends on group size. With three players per group, Marc Knez and Colin Camerer (1994) report that the median first choice is five and group minimum choices equal to 1, 2, 3, 4, 5, 6 and 7 in the fifth repetition are 37, 15, 15, 11, 0, 4 and 18 percent, respectively. In our control, the median first choice is six and the percent of group minimum choices from 1 to 7 in the fifth repetition are 10, 20, 10, 10, 30, 10 and 10 percent.

TD G*ames 3 & 4:* Traveler's Dilemma (Capra et al. 1999 with R=$0.50). We examine two versions of this game, TD-D and TD-S (discussed below). In both versions, two players, i and k, simultaneously make claims n_i and n_k on the closed interval $0.80 to $2.00. Player i's and k's payoffs, $\Pi(i)$ and $\Pi(k)$, equal the minimum of the claims plus a reward R = $0.50 if the player's claim is the lower of the two, or minus a penalty -R = -$0.50 if the player's claim is the greater of the two. No penalty or reward occurs if the claims are equal. The unique Nash Equilibrium has both players claim $0.80. The sum of payoffs is maximized when both players claim $2.00. Capra et al. (with R = $0.50) report an average first choice of 134 and we find an average first choice of 143 in our control. Both Capra et al. (1999) (with R=$0.50) and our control find that claims converge toward the unique Nash Equilibrium.

B. Control and Treatment Data

We collected data beginning with the TD-D, followed by the PG, CO and then TD-S game. Nearly 400 subjects participated across 26 sessions. Subjects could participate in more than one game, but not in more than one session of any game.

The design follows Slonim (2005). In each session, subjects play one game sequentially many times. We call each play of a game a *round*. We group rounds together sequentially and refer to them as *Supergames* (called sets in the instructions). Figure 1 shows the supergame and round design for the control and treatment PG and CO games. In the control and treatment, subjects play every round within a Supergame with the same players and across Supergames subjects are always paired with different people. In the control, subjects make choices in every round of every Supergame, replicating the standard supergame design in which players always have the same amount of experience as each other. Since players always have the same amount of experience, we refer to control sessions as SAME.

In the PG and CO treatment, we randomly assign subjects to one of five roles. Role 1 players, which we refer to as Insiders, play in all rounds of every supergame. Role 2, 3, 4 and 5 players, which we refer to as 1st, 2nd, 3rd and 4th Outsiders, respectively, play in every round of exactly one supergame, either the 1st, 2nd, 3rd, or 4th. Within supergames, there is always one Insider and two Outsiders. Since players in all but the first supergame have mixed amounts of experience, we refer to treatment sessions as MIX. Table 1 summarizes the control and treatment conditions for each game. The PG and CO games have four supergames with five rounds, while both TD games have three supergames with four rounds.

In all sessions, we read instructions before assigning roles so that all subjects had the same amount of time between the instructions and playing the games. At the beginning of every round we reminded subjects of the round number and how many games each group member had played. At the completion of each round, we told subjects what they earned. We also told subjects the choice each group member made in a way that subjects could link specific choices to the amount of experience other players had.[4]

We examine two versions of the TD game. The first version has identical matching protocols to those in the PG and CO games; subjects are paired with the same partner for every round within a supergame and re-paired with a different partner for each subsequent supergame. We refer to

[4] PG and CO games, behavior in MIX and SAME may differ because in MIX the more experienced player breaks the asymmetry of the three players having the identical amount of experience as in MIX. To address this concern, in MIX we color coded players so Insiders are green and Outsiders are red, and in SAME similarly one player is uniquely colored green and the other two players red for all supergames. In these games, players thus saw the amount of experience and a color for the other players they were playing with. If the more experienced player rather than, or in addition to, being a focal player affects behavior, then we'll see different behavior between SAME and MIX. However, we did not find any focal effects player, so we do not discuss this issue further.

this version as TD-S to indicate the same partner within supergames. In the second version, TD-D, players are re-paired with different partners in every round of every supergame. To maintain the supergame structure, we instructed subjects that Role 1 players are first paired with Role 2 players, then Role 3 players and finally Role 4 players, that Role 2 players are first paired with Role 1 players, then Role 4 players and finally Role 3 players, and so on. Further, we told subjects they are always paired with different players from the group of players they are assigned to.

The bottom of Table 1 shows the number of sessions and subjects in SAME and MIX and the number of Insiders (who play all supergames) and Outsiders (who play just one supergame) in MIX. Table 1 also shows the number of groups per supergame in SAME and the number of groups in the last supergame of MIX. Groups in the first supergame of SAME are independent since they have not interacted with any other group. Groups in the last supergame of MIX are also independent since it is the first and only time Outsiders play and since Insiders have not played with anyone who is in another group, nor played with anyone who played with anyone else who is in another group. The appendix provides additional experimental design information and instructions for the Traveler's Dilemma game for the MIX treatment.

C. Comparison of games for creating value

Comparing the games across several dimensions suggests that in some games it may be more or less difficult for experienced players to create value. One important dimension is whether play is repeated with the same partner. Since subjects do not play repeatedly with the same partner in the TD-D game, we expect experienced players in this game will be least capable of creating value since the experienced player cannot directly benefit in future interactions.[5] A second dimension is the number of players. We expect that having fewer inexperienced players, ceteris paribus, will

[5] We can also compare the number of games played with the same partner. However, in the current design the number of games with the same partner is nearly identical (4 in TD and 5 in PG and CO), so we do not expect this will matter here.

make it easier to create value (particularly if increasing the surplus requires joint action) since the experienced player needs to influence the play of fewer inexperienced players and since the inexperienced players can be less concerned about how other inexperienced players will behave. Thus we expect that creating value will be more likely in TD-S (with just two players) than in PG and CO (with three players). Based on these two dimensions we expect that experienced players will be least likely to create value in TD-D and most likely to create value in the TD-S.

A third dimension is the ability of one player to affect the other player's payoffs independent of the other players' actions. Specifically, we expect that the greater one player has to unilaterally create value for the group, ceteris paribus, the greater the ability the experienced player will have to create value. On this dimension the PG game has an advantage for experienced players to increase the surplus since one player can always affect the group payoff regardless of the choices of the other players (i.e., a one point increase in contribution always increases other player's payoffs by 2/3) whereas in the TD and CO games one player's choice does not always affect other player's payoffs. For instance, in the CO game if one player chooses 1, then the choice of either of the other two players has no effect on the payoff of the third player and in the TD game if one player chooses X then the choice of X+1 to 200 of the second player has no effect on the payoff of the first player. However, the effect one player's choice can have on another player's payoff can be larger in TD and CO than PG. For instance, the maximum change one player's choice can have on another player's payoff (who does not change behavior) in PG is 67 while in TD and CO it is 200 and 120 respectively.[6] As a proportion of the equilibrium payoffs in PG and TD and the Pareto dominated equilibrium in CO, the maximum change can be 0.67 (67/100), 2.5 (200/80) and 1.7 (120/70), respectively. So while one player can always have an influence on another player's payoff in PG but not in TD and CO, larger effects can occur in the CO or TD games. Thus, given

[6] The following illustrates these maximum possible changes. In PG player one changes from 0 to 100. In TD player one changes from 80 to 200 and player two plays 199.99. In CO player one changes from 1 to 7 and the other players play 7.

these conflicts on the dimension of one player's ability to affect other player's payoffs we do not expect that experienced players will be more or less likely to create value.

The final dimension we examine is the relative benefit to cost for a player to try to create value. We expect that the greater the benefit to successfully changing other players behavior relative to the cost if unsuccessful, the more likely experienced players will try to create value. To examine the potential gains, we compare an individual's payoff change when the group's actions move from the equilibrium of the one-shot game (and the Pareto dominated equilibrium in CO) to all players playing the group payoff maximizing choice. In TD, PG and CO the actions change from 80 to 200, 0 to 100 and 1 to 7 and payoffs increase from 80 to 200, 100 to 200 and 70 to 130 respectively. To examine the costs, we compare an individual's payoff change when the group's actions move from the equilibrium of the one-shot game (and all players playing the Pareto dominated equilibrium in the CO game) to the one player individually deviating to play the group payoff maximizing choice. In this case in TD, PG and CO payoffs would decrease from 80 to 30, 100 to 67 and 70 to 10 respectively. The benefit to cost for the TD, PG and CO games is thus 1.4 (120/50), 3 (100/33) and 1 (60/60) respectively. On this benefit to cost dimension we expect that experienced players will have the most incentive, and thus be most likely, to create value in the PG game.

In sum, we expect that whether there is repetition of play with the same players is lexi-graphically the most important dimension; if there is no repetition then the remaining dimensions are irrelevant and experienced players will be least likely to create value. We also expect that the number of other players is the next most important dimension (at least when there are just one or two other players) since an ability to create value must be unambiguously communicated to inexperienced players. Finally, all else equal, we expect a greater benefit to cost ratio will provide greater incentives for experienced players to create value. Based on these dimensions we expect the most to least likely game for experienced players to create value are the TD-S, PG, CO and TD-D with the largest differences between TD-S and the other three games and between TD-D and the other three games.

II Results

Section A presents the income distribution results and Section B shows aggregate welfare results. We test income distribution effects (hypothesis H1) using mixed experience groups when experienced players have the most experience (see Figure 1, H1). We test aggregate welfare effects (hypothesis H2) by comparing mixed experience groups with the most experience to purely inexperienced groups in the control (see Figure 1, H2).[7]

Table 2 shows a snapshot of subject choices. It shows mean choices in each round of the first and last supergame by treatment and player role (Table 1 shows sample sizes). Table 2 shows that mean claims and contributions in the Traveler's Dilemma and Public Goods games, respectively, fall from the first to the last round within each supergame for all player roles and that the mean contribution in the Coordination game is fairly steady ranging from 5.2 to 5.9 in most rounds. The most striking observation regarding these mean choices is how little they differ across player roles both within and across supergames.[8] Section C will further sort out choices to identify which motivation (e.g., maximizing own or group payoffs, playing Nash, etc.) explains subject choices; first, we address the main question – does more experience provide an advantage?

A. Experience-Inexperience Distribution Effects (Hypothesis H1)

Table 3 shows the effect of experience on the income distribution. Panel A presents the mean payoff per round of experienced and inexperienced players in the final supergame of MIX. It

[7] Including the intermediary supergames in the analysis does not change the conclusions but complicates the analysis due to repeated (dependent) measures on the experienced subjects in MIX and on all subjects in SAME).
[8] Table 2 shows that the average contribution in the Traveler's Dilemma game is much higher across all player roles, rounds and supergames when players have the same partners within each supergame than when they have different partners within each round.

shows that experienced and inexperienced players earn similar amounts across all four games. Specifically, experienced players earn 2% and 6% more in the TD-D and TD-S games, and 1% and 3% *less* in the PG and CO games. To put these effects into perspective, we use the means and standard errors reported in Panel A to form confidence intervals of +/- two standard errors around the mean effect of experience (mean earnings of experienced players minus mean earnings of inexperienced players). With 95 percent confidence we find that the distributional change in earnings from having more experience ranges from -18% to 23%, from -14% to 27%, from -27% to 25% and from -34% to 28% in the TD-D, TD-S, PG and CO games, respectively. In comparison, Slonim (2005) found that more experienced players won 58.3% of the games while less experienced players won only 20.3% of the games, for an increase in earning of 280% (58.3/20.3). Thus, the maximum possible gains within the two standard error confidence interval for each game (from 23% to 28%) are at most only 1/10 the size of the effect observed in a constant sum game.[9]

Despite no mean effect of experience on earnings, it is possible that more of the experienced players earned more than their less experienced partners within each pairing. To test whether significantly more of the experienced players earn more than inexperienced players in each pairing within each game, we compare each experienced player's average earnings in the final supergame to the average earnings of his inexperienced partners.[10] For each game, Panel B shows that experienced players are as likely to earn more than their inexperienced counterparts as they are to earn less. Even when aggregating across all games, we find that only 46 percent (36/78) of

[9] Comparisons across games could also vary given the range of theoretically possible outcomes across the games. In theory, in the constant sum game Slonim examined, experienced players could win every game for an infinitely larger percent gain. In the Public Goods game, in theory, the experienced player could contribute 0 while the less experienced players contribute 100, in which case the experienced players would earn 175% (233/133) of the less experienced players' earnings. In the Traveler's Dilemma and Coordination games, the experienced players could in theory earn 433% (130/30) and 700% (70/10) of the less experienced players' earnings. Thus, although in theory the relative earnings of more experienced players is higher than in the non-constant game, there is still considerable increases (from 175% to 700%) in earnings that experienced players could have achieved.

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[10] Specifically, we compare experienced player's average earnings to the average earnings of his two inexperienced partners in the PG and CO games, to the average earnings of his one inexperienced partner in the TD-S game, and to the average earnings that each of his four inexperienced partners received while paired with him in the TD-D game.

experienced players earn more than their inexperienced partners while 40 percent (31/78) earn less (two-tail binomial test, p>.20). In comparison, Dufwenberg et al (2005) results suggest that at least 2/3 (20/30) of experienced players earned more than inexpereineced players and Sloinm (2005) finds that 80% (8/10) earned more than inexperienced players in constant sum games.

Panel C investigates whether the advantage of experience may have been present but short-lived by testing whether experienced players earn more than inexperienced players in the first round (i.e., the first encounter) of the final supergame. The results from this comparison are even less compelling for a distributional experience advantage; over all games only 38 percent (30/78) of experienced players earn more than their inexperienced partners in the first round while 42 percent (33/78) earn less (two-tail binomial test, p>.20). To put this lack of earnings redistribution into perspective, Slonim (2005) reports that in the first round when one experienced player played against two experienced players, the experienced players won 85 percent of the games. Thus, we find no statistical support for hypothesis H1 that experienced players earn more than inexperienced players, and even the magnitude of the maximum possible effects across all four games pales in comparison to those found in constant sum games.

B. Experience-Inexperience Welfare Effects (Hypothesis H2)

Table 4 compares the welfare of mixed experience groups to purely inexperienced groups (Panel A) and to purely experienced groups (Panel B).[11] Table 4 reports mean group earnings per round per player and any significant difference in earnings across experience groups.

Panel A shows mixed experience groups earn 11% and 5% *less* than purely inexperienced groups in the TD-D and CO games and 21% and 3% more in the TD-S and PG games. The mixed experience groups earn significantly *less* than the purely inexperienced groups in the TD-D game (two-tail t=1.96, p=.05), significantly more in the TD-S game (two-tail t= 2.21, p=.032) and there

[11] The experimental design insures that all groups in Panel A are independent.

is no significant difference in the PG or CO games (p>.20).[12] Since no robust pattern emerges, with earnings equally likely to be higher in the mixed or purely inexperienced groups, we reject in general the hypothesis H2 that having an experienced member in an otherwise inexperienced group increase the group's aggregate welfare.

The results, however, are consistent with the games providing different opportunities for experienced players to create value. The TD-S game, the only game where having an experienced player significantly increased the group's payoff, is the game we expected that experienced players would be most likely to create value since there is only one inexperienced player. On the other hand, the TD-D game, the game where having an experienced player significantly decreased the group payoff, is the game we expected experienced players would be least likely to create value since experienced and inexperienced players only play together for one game. Thus, the ability of experienced players to create value for the group does not occur across all games, and depends on the conditions of the game in a predictable manner: fewer players helps experienced players increase value and lack of repetition retards experienced players ability to create value.

We also examine whether purely experienced groups earn more than mixed experience groups, since the value of experience may only be realized for groups if enough participants have experience. Panel B shows that mixed and purely experienced groups earnings differ by only 0.1% in the PG game. In the TD-D and TD-S games, while not significant (two-tail t-tests, p>.10), mixed experience groups actually earn 7% and 14% *more* than purely experienced groups. In the CO game, however, we find that purely experienced groups earn 18% more than mixed experience groups, but the difference is marginally insignificant (two-tail t-test, p=.13).

In sum, we find no significant payoff distribution in favor of experienced players across the games (rejecting H1) nor in general any greater welfare in groups with an experienced player

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[12] We again investigated whether the mixture advantage may have been short-lived by testing whether the mixed experience groups earn more than the purely inexperienced groups in the first round (i.e., the first encounter) of the final supergame. The results from this analysis (not shown) are almost identical to those reported for the supergame.

compared to groups with purely inexperienced participants (rejecting H2), yet when there is only one inexperienced player in a repetitive game environment experienced players successfully increase the group welfare.

III. Why don't experienced players earn more than inexperienced players in these non-constant sum games?

We now examine why there are no payoff distribution effects between inexperienced and experienced players. Section A discusses the possibility that experienced players have heterogeneous objectives that may cancel out in the aggregate data. Section B examines whether inexperienced player behavior was sensitive to experience. Section C examines other possible explanations idiosyncratic to the experimental design including the amount of experience and (lack of) selection.

A. Conflicting objectives

In a constant-sum game the structure of the game clearly defines how one 'wins' or how to take a bigger piece of the pie. Experience can thus teach particular paths of play to best achieve a specific goal. Non-constant sum games are qualitatively more complex. Mechanism design and bargaining are replete with examples of the difficulty of reconciling individual incentives to enhance payoffs at the expense of others (capturing value), and means of fostering group efficiency (creating value). Neither is on face a better way of improving payoffs with respect to inexperienced play. Moreover, inexperienced players imagining or interpreting the play of an experienced player may have difficulty attributing which goal a particular action serves. Thus, the strategic environment of a non-constant sum game may simply be sufficiently different from a constant-sum game so that the role of experience is fundamentally altered.[13]

B. Behavior did not depend on opponent's experience:

While experienced players may or may not have been attempting to achieve different objectives, from an actuarial perspective the mixed experience groups' welfare may differ from purely inexperienced groups only if either inexperienced players' behavior depends on the experience of other group members or if experienced players learn to behave differently than when they were inexperienced. To examine these hypotheses, recall that Table 2 shows mean choices for each game by round in the first and last supergame for all subject roles.

First, we find that inexperienced players' behavior playing with either inexperienced or experienced players differs insignificantly across all four games for the first choice they make (t-tests: p>.15 in all games).[14,15] For instance, in the PG game, an inexperienced player's mean first choice is 50.6 if both opponents are inexperienced (SAME) and is 57.1 if one opponent is experienced and one is inexperienced (two-tail test, t=0.73, p>.20). Thus, an inexperienced player's first choice does not depend on the experience of his group members.

Second, we find experienced players' first choice in a supergame changes significantly (in absolute and percentage terms) from the first supergame to the last supergame in MIX in TD-S (18.8 points higher claims) and in PG games (13.6 points higher contributions), but not

[13] Thus, one explanation for the ineffectiveness of experienced players to earn a higher income may be an aggregation problem: some experienced players may have been trying to increase their own payoff while others were trying to maximize their group's payoffs. Alternatively, experienced players may have been homogeneous in their objectives, for instance, to equalize pay between themselves and the inexperienced players (consistent with other regarding utility models (e.g., Bolton and Ockenfels 2000, Fehr and Schmidt 1999, and Rabin and Charness 2002. In an earlier draft we investigated whether we could classify experienced player types by objectives. In this exercise, we assumed experienced players had expectations of inexperienced players' behavior that matched their actual behavior, and given these beliefs examined whether experienced players actions were closer to a variety of objectives including maximizing own pay, maximizing group pay, Nash equilibrium and equalizing pay across all players. In this exercise we found that most experienced players' behavior was consistent with attempting to equalize pay across all participants. We come to the same conclusion if we assume experienced players expected inexperienced players to make random choices. However, an alternative explanation for experienced players making choices that equalize pay is simply that they were making choices similar to inexpereienced players choices (a behavior that we cannot reject – see).

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[14] Slonim (2005) reports the same result in a constant-sum game (the beauty contest game).

[15] We also conducted non-parametric tests for all t-tests we report in this section; in all cases the non-parametric test results indicate less significance than the already insignificant t-test results.

significantly in TD-D or CO games. The change in TD-S and PG games suggests that players obtain experience that they then use – perhaps attempting to signal greater cooperation to the less-experienced players who are playing for the first time. However, we find that in all games, either the percentage or absolute change in the first choice from the first to the last supergame does not depend on whether the experienced players' new opponents are experienced (SAME) or inexperienced (MIX). For instance, in the Public Goods game the mean contribution in the first round increased by 11.5 points in SAME and 13.6 in MIX (two-tail t-test, $p>.20$). This result contrasts with Slonim's (2005) examination of a constant-sum game where experienced player's behavior depends significantly on opponents' level of experience. Thus, we do not find evidence that experienced player's first round behavior, nor inexperienced player's first round behavior, depends on their opponents' level of experience.

Although more experienced players' first round choices in the last supergame did not depend on their opponent's experience, do the less experienced players respond more to the more experienced (than other less experienced) player choices in the subsequent (second) round? In other words, did more experienced players miss an opportunity to influence the less experienced players? To address this question, Table 5 compares the mean absolute change in choices made from the first to second round by inexperienced players when all other players are inexperienced (SAME sessions, first supergame) and when another player is more experienced (MIX sessions, last supergame). Across all games, Table 5 shows that the mean absolute change for inexperienced players is not significantly greater when another player is more experienced than when all are players are equally inexperienced. For instance, in TD-S inexperienced players' mean claims changed 24.9 from the first to second round in the first supergame if opponents had played just one round whereas the inexperienced players' mean claims changed 16.3 if opponents had already played two previous supergames (two tail t-test: $t=1.20$, $p > .20$). Thus, the more experienced players' lack of behaving differently when they first encounter less experience players was not costly in the sense that the less experienced players were not significantly more responsive to these more experienced players than they were when all other players were equally inexperienced.[16] Thus, it further seems that in all these games it was quite difficult for the more experienced players to use their experience advantage to increase to their advantage.

C. Other Explanations

Design Issue 1: Not enough experience: Players may not have had enough experience to learn to distribute welfare to themselves or to improve their group's welfare. However, conditions were ideal for learning: Hillel Einhorn (1980) wrote that ideal learning conditions include immediate and clear feedback linking actions to consequences. Further, the amount of experience cannot be the only issue since less experience was sufficient in Slonim's (2005) constant-sum game for experienced players to earn nearly three times more than inexperienced players.

Design Issue 2: Selection: Experience without any selection pressure may not be sufficient to learn how to earn more in non constant-sum games. The random assignment of roles in the experimental design, and hence the random choice of who becomes experienced, although advantageous to empirically control for selection, might undermine a key component of experience in naturally occurring markets. With market pressure, only the more economically talented individuals may persist, and these individuals may be more likely to earn a higher amount. Further, if newcomers know that the longevity of established participants is at least partially due to their skills, and not because of a random draw, then newcomers might be more willing to learn from experienced players, and experienced players might in turn behave in ways to

[16] We also examined whether the response of the less experienced players when playing with more experienced rather than other less experienced players differed in terms of best responses, directional changes and imitation. However, we find no significant difference in these types of responses of the less experienced players due to the experience of the other players, thus adding further evidence that partner experience did not differentially affect changes in choices.

improve group welfare. Future work can explore this question by allowing only successful players to persist (see also John Kagel and Dan Levin (1986) for selection in common value auctions).

Design Issue 3: Proportion of experienced to inexperienced players: It is also possible that the proportion of experienced to inexperienced players is too low for experienced players to influence their group's welfare. For instance, if there are relatively more experienced players in MIX of the CO game, then play may more closely resemble the purely experienced groups that coordinate more often and earn more than mixed or purely inexperienced groups (see Weber et al. 2001 for related work). However, it is unlikely that having a greater proportion of experienced players will increase payoffs across all games since it is not uncommon in some non constant-sum repeated supergames that welfare falls with all players gaining experience (e.g., finitely repeated PD games, Reinhard Selten and Rolf Stoecker 1986; finitely repeated Trust games, Jim Engle-Warnick and Slonim 2004). Thus, a greater proportion of experienced players in MIX may have been able to move behavior closer to purely experienced group behavior, but these groups do not necessarily earn more than purely inexperienced groups.

IV. Conclusion

Extensive empirical evidence and experiments examining constant sum-games find experienced participants earn significantly more than inexperienced players in head-to-head competition. In contrast, the four non constant-sum games examined in this paper show that inexperienced and experienced players earn similar amounts. Further, the mixed experience groups only earned significantly more than purely inexperienced groups in one of the four games. Thus, there is little support across four non constant-sum games for the hypotheses that experience adds value by income redistribution towards more experienced participants or the hypothesis that experience adds value by increasing aggregate welfare. We thus find no general support for an experience advantage across non constant-sum games. The results thus provide a potential lower bound on the value of experience: in non-constant sum games, having one experienced player in groups of otherwise inexperienced players does not increase the payoffs to the more experienced players relative to the less experienced players nor does it increase the group's payoff relative to purely inexperienced groups.

The one exception where having an experienced player increased the group payoff may be quite important to begin to identify when experience will have a positive effect on welfare. In this game (TD-S), there was only one inexperienced player. With only one inexperienced player, perhaps the inexperienced player had a simpler task to learn from the experienced player than when there was another inexperienced player to add noise. We think a fruitful direction for future research will be to design experiments to better parse why group welfare in this game alone improved with the presence of an experienced player. There are many other differences between the TD-S and the other games besides the number of inexperienced player that need to be studied.

Why do our results with non constant-sum games differ from the two previous constant sum game experiments? Suppose that experienced players objective is to equalize pay, then in principle they could attempt this behavior in constant-sum games. However, the specific structure of the games studied by Slonim and Dufwenberg et al make it quite difficult for subjects to (unilaterally) equalize pay, even knowing the empirical distribution. In particular, in the Beauty Contest Game, payoffs are highly discontinuous; even small errors in guessing the choices of others can lead to large payoff differences. Thus, if the level of pay inequality is largely fixed, it is not surprising to observe subjects maximizing their own pay (this is similar to the predictions and results for ERC and Inequity Aversion models that competition can lead to "selfish" maximization). This hypothesis suggests that in a constant-sum game that more easily allows for payoff equalization, e.g. a bargaining game, we might expect to see results more similar to our current results.

The current results raise an important question for learning models;[17] what do they predict for possible monetary advantages to experience? In the constant-sum games studied in Dufwenberg et al. and Slonim, where players learn to distribute income in their favor, the results lend support to a variety of "sophisticated" learning research approaches (e.g., Camerer et al. 2002). However, in the non-constant sum games studied here, where subjects are unable to translate their experience into greater monetary value, it does not appear much sophistication is gained lending support for less sophisticated learning models (e.g., Ido Erev and Alvin Roth 1998). More research is needed to understand the value of what players are learning.

Finally, the current results, in combination with the constant sum game experiments, provide some light on a methodological issue regarding using subjects multiple times in experiments. In the constant sum games, experienced subjects had a significant effect on behavior and behaved significantly differently than purely inexperienced players, suggesting that experienced participants presence should be at least monitored and controlled for in the data analysis. While this should also be a standard practice with non-constant sum game experiments, it is less clear in non-constant games that experienced players will affect individual or aggregate behavior.

[17] The learning literature addresses *how people learn* in order to examine how context affects the likelihood and speed that markets reach equilibrium, which equilibrium is more likely to emerge when there is more than one, and which path(s) of play (on and off the equilibrium path) may occur in the short, intermediate and long run. For a few examples taking different approaches, see Erev and Roth (1998), Camerer and Ho (1999) and Camerer et al. (2002). However, this research does not address the economic value of what is being learned, which remains an open question that the current paper begins to address.

Appendix: Additional Experimental Design Information

Each session consists of repeated play of one game, in either the control or treatment, except the PG and CO games. The PG and CO games were run in the same session with half of the sessions starting with PG games and half starting with CO games.[18] For all games, all choices were anonymous; subjects could not identify during the session, or later, who their partners were.

Subjects were paid in cash at the end of each session and received a $5 show up fee plus the sum of the money they made in every round. Outsiders received an additional lump-sum payment of $6.00 since they did not get to participate in as many games as Insiders. Outsiders had to wait until all supergames were completed before being compensated, and were not allowed to talk or communicate in any way. The TD sessions lasted on average 60 minutes and the PG-CO sessions lasted on average 80 minutes.

For each game, instructions include identical examples in SAME and MIX sessions. Quizzes were also used in PG and CO sessions. Post experiment surveys indicate that over 95% of subjects stated the instructions were clear and easy to understand. Over 98 percent of subjects are undergraduates with almost 2/3 male and 2/3 majoring in engineering and science.

The following instructions are for the Traveler's Dilemma for the MIX treatment condition.

[18] In each PG-CO session, both games were played in either the control condition only or in the treatment condition only. Subjects knew another game would be played after the first game was complete, but did not know what the game would be, and they were instructed that all players' identities were completely anonymous and that they would never know whether anyone they were interacting with in the first game was the same or different than anyone they would interact with in the second game.

Instructions (Traveler's Dilemma Same Partners – MIX Treatment)

We would like to thank everyone for coming today. We are going to play a game several times. Please pay careful attention as I read these instructions so that you understand how you can earn as much money as possible.

Getting paid: At the end of the experiment we will pay each of you, in cash, $5 for coming. In addition to this $5, we will also pay you, in cash, any money that you may earn while playing the games.

No talking: During the experiment, we require that you do not talk or communicate with anyone. We ask that you do not communicate with anyone because, as part of the scientific method of conducting research on decision-making, we do not want you to influence what other subjects are thinking about, nor do we want other subjects to influence what you are thinking about. If you have any questions at any time, please raise your hand and we will come over to answer them.

Identification Number: In a few minutes, we will give each of you a unique identification number. This is how we will identify you during the game.

Today's Experiment: The experiment consists of a number of periods. In each period, you will be matched with one other person to play a game. At the beginning of each period, you will choose a number or "claim" between 80 and 200 cents. Claims will be made by writing the claim on a decision sheet identical to the **Example Decision Sheet** attached to these instructions. Your claim amount may be any amount between and including 80 and 200 cents. That is, your claim may include fractions of cents. The person you will be matched with will also make a claim between and including 80 and 200 cents.

Earnings each period: If the claims are equal, then you and the other person each receive the amount claimed. If the claims are not equal, then each of you receives the lower of the two claims. In addition, the person who makes the lower claim earns a reward of 50 cents, and the person with the higher claim pays a penalty of 50 cents. Thus, you will earn an amount that equals the lower of the two claims, plus a 50 cent reward if you are the person making the lower claim, or minus a 50 cent penalty if you are the person making the higher claim. There is no penalty or reward if the two claims are exactly equal, in which case each person receives what they claimed.

Example: Suppose that your claim is X and the other's claim is Y.
If X = Y, the minimum is X or Y, so you get X, and the other gets Y
If X > Y, the minimum is Y, so you get Y - 50 cents and the other gets Y + 50 cents
If X < Y, the minimum is X, so you get X + 50 cents and the other gets X – 50 cents

Making your claim: Now, each of you should examine the **Example Decision Sheet**. This sheet is the last page attached to these instructions. When you get your actual Decision Sheet, your ID will be written in the top-right part of this sheet, where it currently says Z-4. Now, please look at the columns of your Decision Sheet. Going from left to right, you will see columns for the "period," "your claim," "other's claim," "minimum claim," "penalty or reward" (if any), and "your earnings." You begin by writing your claim in the appropriate column. As mentioned above, this claim must be greater than or equal to 80 cents, and less than or equal to 200 cents, and the claim may be any amount in this range; that is, fraction of cents are allowed. Use decimals to separate fractions of cents. For example, wxy.z cents indicates wxy cents and a fraction z/10 of a cent. Similarly, xy.z cents indicates xy cents and a fraction z/10 of a cent.

Recording the results for each period: After you make and record your claim for each period, we will collect all decision sheets. We will write the claim of the person with whom you are matched with on your sheet. We will also write the minimum claim, your penalty or reward (if any), and your earnings in the relevant columns of your decision sheet and return it to you. Thus, for each period, you only need to write your claim and we will fill in the rest of the decision sheet. When we begin each period, we will let you know which period we are on. Do not fill in any claim except for the current period, which we will always announce.

Getting Paid: Before you leave, you will be paid in cash. In addition to how much you earn each period based on the claims you have made, everyone will also get $5 for showing up. So, if you earn $3.50 for the claims, you will receive $8.50 in total, if you earn $12.00 for the claims, you will receive $17.00 in total, if you earn $17.35 for the claims, you will receive a total of $22.35, etc.

Are there any questions so far regarding making claims or how your earnings are determined each period?

How we will identify you: We will give each of you an index card that has your unique and private ID. Your ID will begin with either the letter A, B, C or D and be followed by a number. For example, it may be D-1, A-3, C-4, B-1, etc. When you are given your ID, we will also give you your decision sheet that should have your ID written in the top-right corner. We will use your ID to identify you for who you get matched with and to pay you at the end of the experiment.

Anonymity: Your ID is private and you should not let anyone know it. Thus, no other participants will know what claims you are making and you will not know what claims anyone else is making other than the person you are matched with. For the persons you are matched with, you will not know who they are and they will not know who you are.

Roles and sets: In a moment we are going to **randomly** assign you to be in role A, B, C or D, depending on whether your ID begins with the letter A, B, C or D. There will be exactly four people in role A, four in role B, eight in role C and eight in role D. We are going to always play the game in sets of four periods, and there will be three sets.

Set 1: In the first set, players in role A will play the game with players in role B, and players in roles C and D will not play. Specifically, every player in role A will randomly be matched to play the game four times with exactly one player in role B (and so every player in role B will be randomly matched to play the game with one player in role A).

Set 2: In the second set, players from roles A and B will play the game with players from role C, and players in role D will not play. Specifically, every player in role A and B will be randomly matched to play the game four times with exactly one player in role C (and so every player in role C will be randomly matched to play the game with one player in role A or B).

Set 3: In the third set, players from roles A and B will play the game with players from role D, and players in role C will not play. Specifically, every player in role A and B will be randomly matched to play the game four times with exactly one player in role D (and so every player in role D will be randomly matched to play the game with one player in role A or B).

Thus, players in the role of A or B will play in all three sets of four periods, role player C will play in only the second set of four periods and role player D will play in only the third set of four periods. Within each set, you will play the game with the same player, and across sets you will play the game with different people.

To compensate each of you who we select to be in roles C and D, since you will only have the opportunity to play in one of the three sets of four games, and since you will have to wait while the other games are being played, we will give you a little extra money. If you are randomly selected to be in either role C or D, we will give you an extra six dollars. Thus, if you are in roles C or D and you earn $3.50 for the periods you play, you will get paid $14.50, ($3.50 plus $5 show up plus $6 for being in roles C or D), and if you earn $4.90 for the periods you play, you will get paid $15.90.

Selecting Roles: To select which role you will be in, we have made a set of index cards. In a moment we are going to shuffle them and hand each of you one of them. We use this procedure so that everyone has an equal chance at being in each role.

Selecting Matchings for each period: To determine which players are matched each period, we randomly assigned pairings prior to your arrival, based on the IDs that we will randomly assign to you. We will not tell you who you are paired with each period, but remember that you will be paired with a different person each set and the same person within each set.

Are there any questions on how roles will be randomly assigned, who will play in which set of four periods, how often you will be matched with the same person, or any other questions.

Summary: Before we randomly determine roles and begin playing, let us summarize the procedures and pay. In the first set of four periods, only role player A and B will play the game. In the second set, role players A, B and C will play, and in the third set, role players A, B and D will play. Within each set, you will always be paired with the same person for each game. You will never play the same person in more than one set. You will make decisions one period at a time by writing your claim on your decision sheet for the current period. We will then collect the decision sheets and write on them the claim of the person you are matched with, plus the minimum claim, the penalty of 50 cents if your claim is higher, the reward of 50 cents if your claim is lower, and the amount you have earned for that period. You will never find out the claims of anyone other than the person you are matched with. We will then return the decision sheet to you and move on to the next period.

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Table 1: Experimental Design

	Traveler's Dilemma (TD-D)	Traveler's Dilemma (TD-S)	Public Goods (PG)	Coordi nation (CO)	
a. Game Information					
Group Size	2	2	3	3	
Number of Players with More Experience per Group (Mixture Sessions)	1	1	1	1	
Rounds per Supergame	4	4	5	5	
Number of Supergames	3	3	4	4	
Same Partner within Supergame	No	Yes	Yes	Yes	
Computer or Paper and Pencil	Paper-Pencil	Paper-Pencil	Computer (Z-tree [a])	Computer (Z-tree [a])	
b. Participant Information					Totals
Control SAME Sessions					
Sessions	2	2	3	2	9
Subjects	32	40	51	30	153
Groups per supergame	*16*	*20*	*17*	*10*	*63*
Treatment MIX Sessions					
Sessions	3	5	4	5	17
Subjects	70	115	54	75	314
Insiders (Play all Supergames)	23	38	7	10	78
Outsiders (Play in only 1 Supergame)	47	77	47	65	236
Groups in last Supergame	*23*	*38*	*7*	*10*	*78*
All Sessions	5	7	7	7	26
All Subjects	102	155	105	105	467
Percent Mix Subjects	69%	74%	51%	71%	67%

[a] We thank Urs Fischbacher for allowing us to use Z-tree software for our PG and CO games.

Table 2: Mean (standard error) Choices by Round in the First and Last Supergames

Travelers Dilemma with Different Partners (TD-D)

	First Supergame				Last Supergame			
Round	1	2	3	4	1	2	3	4
Control (SAME)	143.4	130.6	127.0	121.5	115.7	104.1	101.4	101.9
	(8.3)	(8.4)	(7.4)	(7.4)	(6.3)	(4.5)	(5.1)	(5.3)
Treatment (MIXED)	133.2	135.6	130.6	127.7	119.7	123.8	111.9	117.2
More Experienced	(10.7)	(10.0)	(8.9)	(10.3)	(10.8)	(10.8)	(10.1)	(10.7)
Treatment (MIXED)					125.7	123.4	113.6	100.8
Less Experienced					(11.2)	(11.5)	(10.5)	(9.8\7)

Travelers Dilemma with Same Partners (TD-S)

	First Supergame				Last Supergame			
Round	1	2	3	4	1	2	3	4
Control (SAME)	161.0	146.1	144.1	127.2	164.1	152.6	143.8	130.2
	(6.9)	(7.9)	(7.9)	(7.9)	(6.9)	(7.7)	(8.9)	(8.8)
Treatment (MIXED)	156.0	155.9	152.4	138.4	174.8	162.9	161.8	148.8
More Experienced	(7.3)	(6.9)	(7.9)	(7.3)	(6.2)	(7.3)	(8.0)	(8.6)
Treatment (MIXED)					172.3	168.3	165.1	154.5
Less Experienced					(7.5)	(7.1)	(7.7)	(8.3)

Public Goods (PG)

	First Supergame					Last Supergame				
Round	1	2	3	4	5	1	2	3	4	5
Control (SAME)	50.6	52.2	51.1	47.6	42.9	62.1	65.3	63.7	53.3	21.4
	(5.0)	(5.0)	(5.3)	(5.6)	(5.7)	(4.7)	(4.9)	(5.1)	(5.6)	(5.2)
Treatment (MIXED)	45.7	45.7	47.1	49.3	18.6	59.3	53.1	62.1	61.3	37.1
More Experienced	(7.6)	(7.8)	(9.9)	(6.8)	(9.2)	(8.5)	(11.5)	(11.3)	(13.4)	(16.6)
Treatment (MIXED)						57.1	57.9	54.4	54.6	35.7
Less Experienced						(7.5)	(8.3)	(9.2)	(9.8)	(11.3)

Coordination (CO)

	First Supergame					Last Supergame				
Round	1	2	3	4	5	1	2	3	4	5
Control (SAME)	5.8	5.7	5.6	5.4	4.8	5.9	5.8	5.9	5.6	5.3
	(1.5)	(1.6)	(1.5)	(1.7)	(2.1)	(1.4)	(1.6)	(1.7)	(2.0)	(2.2)
Treatment (MIXED)	5.8	5.7	5.3	5.4	5.4	5.9	5.8	5.9	5.2	5.0
More Experienced	(1.1)	(1.0)	(2.0)	(1.9)	(2.0)	(1.7)	(1.5)	(1.3)	(1.8)	(1.9)
Treatment (MIXED)						5.2	5.3	5.6	5.6	5.3
Less Experienced						(1.9)	(1.7)	(1.7)	(1.8)	(2.3)

Table 3: Do More Experienced Players Earn More?
Final Supergame Income Distributions by Experience

	Traveler's Dilemma (TD-D)	Traveler's Dilemma (TD-S)	Public Goods (PG)	Coordi-nation (CO)	
a. Mean (Standard Error) Pay Per Round in Final Supergame					
More Experienced (M. Exp.) Players	\$0.958 (\$0.055)	\$1.566 (\$0.079)	151.1 (13.3)	88.6 (7.6)	
Less Experienced (L. Exp.) Players	\$0.936 (\$0.042)	\$1.474 (\$0.071)	153.3 (6.6)	91.3 (6.4)	
b. By Groups in Final Supergame All Rounds by Pairs	**TD-D**	**TD-S**	**PG**	**CO**	Totals
M. Exp. Pay > L. Exp. Pay	9[a]	20	4	3	36
M. Exp. Pay = L. Exp. Pay	6[a]	5	0	0	11
M. Exp. Pay < L. Exp. Pay	8[a]	13	3	7	31
Binomial Test (two-tailed): M. Exp. Pay ≠ L. Exp.	p>.20	p=.148	p>.20	p>.20	p>.20
c. By Groups in Final Supergame First Round by Pairs	**TD-D**	**TD-S**	**PG**	**CO**	Totals
M. Exp. Pay > L. Exp. Pay	9[a]	14	3	3	30
M. Exp. Pay = L. Exp. Pay	5[a]	9	1	1	15
M. Exp. Pay < L. Exp. Pay	9[a]	15	3	6	33
Binomial Test (two-tailed): M. Exp. Pay ≠ L. Exp.	p>.20	p>.20	p>.20	p>.20	p>.20
Number of Groups	23	38	7	10	78

[a] For the Traveler's Dilemma Game with Different partners (TD-D), we compare the average pay each more experienced player received during the supergame to the average pay his/her opponents received while paired with him/her.
Amounts shown for TD games shown in dollars and amounts shown for PG and CO games shown in points as subjects saw on their decision screens. Each 100 points in the PG and TD game equals \$0.40.

Table 4: Do More Experienced Players Improve Group Welfare?
Comparison of Inexperience, Mixed Experience and Experienced Group

	Traveler's Dilemma (TD-D)	Traveler's Dilemma (TD-S)	Public Goods (PG)	Coordi-nation (CO)
a. Mean (Standard Error) Pay per Round per Player				
Inexperienced Players (First Supergame)	$1.081 ($0.044) [a]	$1.259 ($0.094)	148.9 (5.6)	95.2 (6.1)
Mixture (Last Supergame)	$0.947 ($0.052) [a]	$1.520 ($0.070)	152.8 (10.5)	90.4 (6.3)
Mixture Groups versus Inexperienced Group Earnings? (two tailed t-test: Mixture Pay ≠ Inexperienced Pay)	**t=1.96** **p=.05** [a]	**t=2.21** **p=.032**	t=0.36 p>.20	t=0.55 p>.20
b. Mean (Standard Error) Pay per Round per Player				
Experienced Players (Last Supergame)	$0.884 ($0.017)[a]	$1.331 ($0.103)	153.1 (5.2)	106.5 (8.1)
Mixture (Last Supergame)	$0.947 ($0.052) [a]	$1.520 ($0.070)	152.8 (10.5)	90.4 (6.3)
Mixture Group versus Experienced Group Earnings? (two-tailed t-test: Experienced Pay ≠ Mixture Pay)	t=1.30 p=.200 [a]	t=1.55 p=.128	t=0.03 p>.20	t=1.53 p=.134
c.. Number of Groups				
Inexperienced Groups (Control Sessions)	32 [a]	20	17	10
Experienced Groups (Control Sessions)	32 [a]	20	17	10
Mixture Groups (Mixture Sessions)	23	38	7	10

[a] The analysis for standard errors for the TD-D data assumes each subject is an independent observation although subjects in fact switched partners within supergame. Note though, that since comparisons of means are not significantly different with this strong assumption of independence, addressing the non-independence in the analysis would increase the standard errors and make the differences even less significant than currently reported.
Amounts shown for TD games shown in dollars and amounts shown for PG and CO games shown in points as subjects saw on their decision screens. Each 100 points in the PG and TD game equals $0.40.

Table 5: Mean (Standard Error) Absolute Change From Round 1 to Round 2

	Traveler's Dilemma (TD-D)	Traveler's Dilemma (TD-S)	Public Goods (PG)	Coordi-nation (CO)
When Decision-Makers are Inexperienced				
All inexperienced Players in the Control First Supergame	18.9 (5.7)	24.9 (5.7)	17.5 (2.9)	1.00 (.28)
Less Experienced Players in MIX Last Supergame	18.3 (6.5)	16.3 (4.3)	11.4 (2.4)	0.90 (..25)
All inexperienced (Control) vs. Less Experienced (MX) (two tailed t-test: Control Change ≠ Less Experienced Change)	t=0.07 p=.940	t=1.20 p=.234	t=1.04 p=.280	t=0.25 p=.852
b. When Decision-Makers are Experienced				
All experienced Players in the Control Last Supergame	22.5 (5.6)	31.6 (5.7)	13.3 (2.7)	0.67 (.19)
More Experienced Players in MIX Last Supergame	7.4 (3.8)	24.6 (5.2)	20.4 (8.3)	0.34 (.15)
All Experienced (Control) vs. More Experienced (MX) (two tailed t-test: Control Change ≠ More Experienced Change)	**t=2.04** **p=.044**	t=0.91 p=.360	t=0.91 p=.372	t=1.08 p=.293
Number of Subjects				
Inexperienced Players in the control SAME	32	40	51	10
More experienced Players in the treatment MIX	23	38	7	10
Less experienced Players in the treatment MIX	23	38	14	20

Figure 1: Experimental Design

		Control SAME	Treatment MIX				
Supergame (SG)	Round (RD)	All Players	Role 1 (Insider)	Role 2 1st Outsiders	Role 3 2nd Outsiders	Role 4 3rd Outsiders	Role 5 4th Outsider
SG1	R1 R2 R3 R4 R5	**Play SG1 (H2)**	**Play SG1**	**Play SG1**	Don't Play	Don't Play	Don't Play
SG2	R1 R2 R3 R4 R5	**Play SG2**	**Play SG2**	Don't Play	**Play SG2**	Don't Play	Don't Play
SG3	R1 R2 R3 R4 R5	**Play SG3**	**Play SG3**	Don't Play	Don't Play	**Play SG3**	Don't Play
SG4	R1 R2 R3 R4 R5	**Play SG4**	**Play SG4 (H1, H2)**	Don't Play	Don't Play	Don't Play	**Play SG4 (H1)**

Supergames: four in PG and CO games, three in TD-S and TD-D games
Rounds per supergame: Five in PG and CO games and four in TD-D and TD-S games
H1, H2 indicates data used to test hypothesis H1 and H2.