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Equilibrium selection in static and dynamic entry games ☆

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ABSTRACT

We experimentally assess the predictive power of two equilibrium selection principles for binary N-player entry games with strategic complementarities. In static entry games, we test the theory of *global games* which posits that players play games of complete information *as if* they were playing a related global game of incomplete information. By contrast, in dynamic n-period entry games, the efficient *subgame perfect* equilibrium prediction is for all to enter whenever the payoff relevant state variable exceeds a certain threshold. The subgame perfect entry threshold of the dynamic game will generally differ from the global game threshold of the static version of the same game. Nevertheless, our experimental findings suggest that entry thresholds are similar between static and dynamic versions of the same game. An implication is that the modeling of entry games with strategic complementarities as static, one-shot games – ignoring the dynamic element of such interactions – may not be unreasonable.

1. Introduction

Many games of complete information with strategic complementarities have multiple equilibria. An important class of such games may be labeled as "entry games". In an entry game an individual has two possible actions, 'enter' and 'not enter'. The payoff to a player who does not enter is a fixed amount, F, while the payoff to entering, $\pi(e) = G(m, Y)$, depends in a monotonic way on the number of people who enter, m, and on a payoff-relevant parameter, Y, of the game. In this class of games there exists a value of Y, $\underline{Y}$ such that for $Y < \underline{Y}$ the dominant strategy is 'not enter'. There exists another value $\overline{Y} > \underline{Y}$, such that for $Y > \overline{Y}$ the dominant strategy is 'enter'. For intermediate values $Y \in [\underline{Y}, \overline{Y}]$, there are two equilibria in pure strategies, all 'enter' or all 'not enter'.

Financial markets provide important examples of such 'entry' games. For instance, this game form may represent situations of speculative attack on a currency (Obstfeld, 1996; Morris and Shin, 1998), with $\overline{Y}$ representing a state in which the fundamentals are such that the currency is certain to be devalued if a single speculator sells the currency short, so that a short sale is certain to be profitable, while $\underline{Y}$ represents a state in which the State holds sufficient reserves that it will certainly withstand any feasible attack by all currency speculators. For other states, $\underline{Y} < Y < \overline{Y}$ the State can withstand short

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sales from a fraction, $f(Y)$ of currency speculators. If less than $f(Y)$ agents 'enter' all those who enter lose money while if more than $f(Y)$ 'enter' all those who enter make a profit. The same game form can be used to characterize the situation faced by a group of lenders, all of whom have debts that have come due from a particular borrower and find that the borrower has insufficient cash on hand to pay off more than some fraction of the outstanding debt (Morris and Shin, 2006). Given the borrower's cash holdings, Y, if a sufficient fraction $f(Y)$ of lenders agree to roll over the debt, the borrower need not default, but if too few lenders agree to a roll-over, a default occurs.

Viewed as static games of complete information, standard game theory yields no prediction of how these entry games will be played when there are multiple equilibria. However, Carlsson and van Damme (1993) propose an equilibrium refinement for 2×2 entry games that is based on the assumption that when players face the uncertainty associated with the multiplicity of equilibria in these games they effectively transform the game ***in their own minds*** to a related game of incomplete information. The related game is called a 'global game'. As shown by Carlsson and van Damme for the case of 2×2 games of complete information, in the related global game the required equilibrium strategies for all possible realizations are *cut-off* strategies. In the global game a player always chooses one action upon receiving a signal that comes from one portion of the probability distribution of signals and the other action when the realized signal comes from outside this set. This result has been generalized by Morris and Shin (2003) to a class of $2 \times N$ symmetric games. Unlike the original game of complete information with its multiplicity of equilibria, the global game of incomplete information has a unique Bayesian perfect equilibrium. Therefore, if all players play the game of complete information as though there were a common understanding that all players will play that game *as if* they were playing the related global game, the coordination problem is resolved.

The theory of global games offers an equilibrium selection criterion for a *static* entry game of complete information.[1] Heinemann et al. (2004) have shown that the static theory of global games does capture important characteristics of behavior in static entry games. However, many entry games of interest are inherently dynamic, as individuals must decide not only whether to enter but also *when to enter*. Dynamic entry games have subgame perfect equilibria that differ from the global game equilibrium of the corresponding static game. Therefore, it seems natural to ask whether modeling phenomena such as bank runs and speculative currency attacks as static global games misses something important in the way people actually behave in these dynamic entry games. Foreshadowing our results, we find that aggregate behavior in dynamic entry games of complete information does not differ systematically from aggregate behavior in corresponding static entry games.

2. Related literature

The paper most closely related to this one is the experimental study of Heinemann et al. (2004) testing Morris and Shin's (1998) model of speculative attacks. In the Heinemann et al. design, subjects play sequences of static entry games under several treatment conditions that are chosen to test the comparative static implications of the theory. Their main treatment variable is whether there is common, complete information or private, noisy information about the state of fundamentals. Their principal finding is that there is little difference in observed behavior under the different information treatment conditions. They further find that behavior responded to variations in the threshold fundamental in the direction predicted by the theory of global games. As we elaborate upon below, their design provides only an indirect test of the hypothesis that the global game provides an equilibrium refinement of the game of complete information.

Gale (1995) considers an N-player, dynamic "monotone" entry game that is similar to the dynamic game we study. In Gale's game, a player's decision to enter ("invest") can take place at discrete periods in time, $t = 1, 2, \ldots,$ is irreversible, and provides a flow of benefits that depends on the number of other players who have already entered by date t. There is a critical number of players, n^*, who must enter before entry is profitable. As the number of players, N, increases so does n^*. Gale shows that there is a subgame perfect equilibrium in which everyone delays entry until $n^* - 1$ periods have elapsed. This equilibrium is supported by the off-equilibrium belief that if anyone were to enter before $n^* - 1$ periods have elapsed, no one else will enter prior to period $n^* - 1$ so early entry will be unprofitable. Gale's game differs from the one we consider as it has a different payoff function and is indefinitely repeated.

Dasgupta et al. (2012) analyze a dynamic entry game in which each player receives a sequence of signals about the true value of a fundamental, payoff relevant state variable. If the fundamental exceeds a critical value and all players enter, then all receive a positive payoff. If the fundamental is favorable, but not all enter, those who do enter receive a negative payoff. A player may enter at any time, but entry is irreversible. Each entrant secures a payoff that is decreasing in absolute value with the time of entry. As time passes, information becomes more nearly precise. They show that for any fundamental above the critical value, the probability that everyone will enter approaches 1 as time passes. Furthermore, they show that when information is precise, if the fundamental exceeds the value at which all enter is the Pareto Optimal pattern of actions,

[1] Dynamic models of global games that are based upon learning over time of better estimates of the true value of Y yield multiple equilibria. See Angeletos et al. (2007), and Costain (2007) for versions of such dynamic global games, and Costain et al. (2007) for an experimental study. We abstract from this type of statistical uncertainty and consider dynamic entry games of *complete* information as this facilitates our comparison with static entry games of complete information. We show below that, under certain conditions, the symmetric subgame perfect equilibrium of the dynamic game of complete information is unique and will generally differ from the unique global game equilibrium prediction for the corresponding static game of complete information.

everyone enters immediately. This result is analogous to the characterization of the subgame perfect equilibrium of our version of the dynamic entry game.

3. Experimental design

In this section we describe our experimental design including a description of the entry game and our experimental treatments. As the theoretical predictions are based on details of our experimental design, we provide those predictions later in Section 4.

3.1. Overview

Our experimental design builds upon and complements an earlier experiment by Heinemann et al. (2004). In their experiment, subjects play a sequence of entry games under one of two information conditions: complete information or incomplete information. Only one information condition was used in a given experimental session. In each of their sessions, all N subjects played the same sequence of N-person entry games. Let A denote the strategy "don't enter" and B the strategy "enter". In all treatments studied by Heinemann et al. (2004), each subject i has a payoff function of the form:

$$\pi_i(\mathrm{A}) = F > 0$$
$$\pi_i(\mathrm{B}) = \begin{cases} Y & \text{if } \#\mathrm{B} \geqslant f(Y) \\ 0 & \text{if } \#\mathrm{B} < f(Y) \end{cases} \tag{1}$$

Here, F is a fixed payoff that is independent of the actions chosen by the other $N-1$ players, #B denotes the number of the N players including i who choose action B, Y is a random payoff parameter drawn from a known, uniform distribution, that completely characterizes each game, and $f(Y)$ is a monotonically decreasing "hurdle" function of Y that is fixed across all games in a session.

In Heinemann et al.'s (2004) study, subjects are repeatedly presented with lists of entry games having different values for Y. In their complete information game treatment, every subject in a session receives the same list. In their incomplete information treatment, random Y values (games) are drawn as before, but subjects are not given a list of different values of Y. Instead, for each value of Y drawn, each subject i receives a private signal X_i that is a random draw from a commonly known, uniform distribution centered on the unknown value of Y and having known support $[Y-\lambda, Y+\lambda]$. Each individual's signal of Y is drawn independently of the signals of others. In both treatments, a subject is asked to specify an *action* A (no entry), or B (entry) for each game on the list. After all subjects submitted their action list, the outcomes of the games on that list were presented to them. This procedure was repeated for 16 rounds. Heinemann et al. find that in both information treatments the pattern of action choices of most subjects corresponds to a "cut-off" strategy in which entry is chosen only if the commonly observed Y value or the private signal X exceeds a certain threshold. The estimated mean thresholds are smaller under the complete information treatment than under the incomplete information treatment. However, in both treatments, the estimated thresholds were consistently below the global game equilibrium threshold. Heinemann et al. found that the estimated mean thresholds varied in response to changes in the payoff function in the direction predicted by the global game equilibrium and that the variance in individual estimated thresholds decreased with experience.

Our research strategy is as follows: We use the same entry game as Heinemann et al., with the payoff function re-scaled to reflect the fact that only 10 subjects play in the entry games in our experiment. The equilibrium of the global game is defined in terms of a cut point strategy for all possible values of the true state Y, given a measure of noise in the signal, λ. We consider only Heinemann's complete information case where $\lambda \to 0$, as this will facilitate comparison with our dynamic game of complete information. Heinemann et al. infer cut-point strategies from action choices. We propose a direct test by eliciting cut-point strategies from subjects and then seeing whether or not those same subjects make action choices that correspond to their elicited strategies. A second modification addresses the fact that subjects tend to change their behavior with experience. Unlike Heinemann and Nagel, in our design, we attempt to reduce the likelihood of group efforts at coordination by running two games (groups of 10 subjects) in a session simultaneously and randomly reassigning individuals to one or the other group after the play of each game. These treatments serve as a robustness check of the findings of Heinemann and Nagel.

Our primary research interest differs from Heinemann et al. and involves a comparison of the way subjects play static entry games of complete information with corresponding dynamic entry games. Therefore, our design incorporates treatments of *dynamic* entry games with payoffs that correspond to those used in the static entry game sessions. In these dynamic game treatments, each of the subjects chooses not only whether to enter but also *when* to enter. Once a subject chooses to enter, that decision is irreversible. As explained below, the dynamic entry games have subgame perfect equilibria in which everyone enters whenever it is not a dominant strategy to stay out. This contrasts with the global game equilibrium of the corresponding static game in which entry occurs only if the value of the state variable, Y, is sufficiently above the region for which staying out is a dominant strategy.

While the dynamic entry game with no explicit cost of delay has multiple subgame perfect equilibria, with the addition of delay costs there is a unique subgame perfect equilibrium in which everyone enters immediately whenever Y is outside the region where staying out is a dominant strategy. Therefore, we consider both a dynamic game treatment where there is no cost of delay (to facilitate comparison with our static game treatment), and a second dynamic game treatment with delay costs.

3.2. Specific details

The specific details of our experimental design are as follows. The experiment was conducted using Fischbacher's (2007) z-Tree software using networked PC workstations in the Pittsburgh Experimental Economics Laboratory. Each experimental session consisted of 20 subjects with no prior experience in any of our treatments. Subjects were recruited from the student population of the University of Pittsburgh and Carnegie-Mellon University. Each session begins with the reading aloud of the written instructions followed by play of a series of either 60 static or 30 dynamic entry games.[2]

At the start of each new entry game our computer program randomly assigns the 20 subjects to one of two groups of size $N = 10$. Subjects are informed of their random assignment to either "group 1" or to "group 2" at the start of each entry game, but the composition of the members of each group is anonymous, and no communication is possible among group members. Subjects are instructed that they will participate in a series of games where their payoff function is as described in (1) with the threshold number of players needed for entry to yield a positive payoff equal to

$$f(Y) = 10(80 - Y)/60$$

In the experiment, we use $\hat{f}(Y)$, which denotes the round-up of $f(Y)$ to the nearest integer in the set $\{1, 2, \ldots, 10\}$. While this formula and the round-up rule *are* shown and explained to subjects, we also provide subjects with tables using this formula for ease of reference – see the instructions for the details. The Y values characterizing each "game" are random draws from a uniform distribution over the interval $[10, 90]$. The distribution and support of the Y values, the payoff function (1), and the hurdle function $f(Y)$ are all public information in all sessions of our experiment, as provided in the written instructions and written on a chalkboard for all to see.[3]

There are four treatment variables in our design. The first treatment variable is the strategy space. In treatments labeled 'C', the strategy space consists of two possible actions, $\{A, B\}$, corresponding to "not enter" and "enter". In this treatment the randomly drawn Y value is announced publicly prior to the choice of a strategy. In treatments labeled 'G', the Y value is not drawn until all 10 subjects have submitted their "cut-point" strategies. The strategy space in G treatments consists of the set of integers, I, where $\{10 \leqslant I \leqslant 90\}$. These correspond to cut-point strategies such that if the randomly drawn value $Y \geqslant I$, the action that will be automatically chosen for the subject (by the computer program) is "enter" (choice B) otherwise if $Y < I$, the action that will be automatically chosen for the subject is "not enter" (choice A).

The second treatment variable concerns the value of the fixed payoff F earned by all participants who choose not to enter (choice A). Variations in this parameter shift the theoretical equilibrium cut-point strategy in the global game in which the noise of the signal goes to zero and also affect the subgame perfect equilibria of the dynamic game as explained below. We consider two values for F, $F = 20$ and $F = 50$; we use only one of these two values for F in each experimental session.

The third and most important treatment variable is whether the game is 'static' or 'dynamic'. In the static treatment, a game consists of a single decision round. In the dynamic game treatment, each game consists of $n = 10$ decision rounds, $r = 1, 2, \ldots, 10$; players remain in a fixed group of 10 over all 10 rounds of a dynamic game. Following the first round of a dynamic game, all individuals are informed at the start of each new round about the number of individuals in their group of 10 who have previously entered in that game. Each individual who has not already entered (chosen B) then decides whether to enter in that round (choice B) or to stay out (choice A). Entry is irreversible. Thus, to preserve the right to enter at a later round of a dynamic game, a subject would have to choose not to enter (choice A). The dynamic game ends after 10 rounds.

As noted earlier, the payoff to a player who chooses not to enter (choice A) is always fixed at F (20 or 50). The payoff to a player who enters (chooses B) in a game is determined by the number of players in their group of 10 who have entered by the *end* of the static or dynamic game (round $r = 10$) and by the value of the state variable, Y. If the number of entrants, #B, meets or exceeds $f(Y)$, rounded to the nearest integer, $\hat{f}(Y)$, then all those who have chosen to enter receive a payoff of $Y - d(r_i - 1)$ where d is a fixed entry delay cost and r_i is the round number of the (dynamic) game in which player i first chose to enter (made irreversible choice B); in the static game, there is only one round, so $r_i = 1$ for any entrant and successful entrants earn Y. If the number of entrants is less than $\hat{f}(Y)$, those who have chosen to enter receive $0 - d(r_i - 1)$. Again, in the one round static game, unsuccessful entrants earn 0.

The fourth and final treatment variable pertains to the dynamic, $F = 20$ game treatment only and involves the cost of delayed entry, d. In our baseline dynamic game treatment, there is no entry delay cost, i.e., $d = 0$. In our dynamic game with delay cost treatment, we set $d = 1/2$, so that subjects are charged a cost of delaying their decision to enter (choice B) beyond the very first round 1, at the rate of 1/2 point per round delayed; that is, as in the static game, there is no cost for an immediate choice of entry in round 1 of the dynamic game but each additional round of delay beyond round 1 reduces a subjects' payoff by 1/2 point (up to a maximum loss of −4.5 points if they delayed entry until round 10). If they chose not

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[2] Sample instructions are available in the online supplementary content or from the authors on request.

[3] Except for scaling to accommodate the smaller number of subjects, the payoff function is identical to that used by Heinemann et al. (2004).

to enter in all 10 rounds (always chose A), there is no delay cost charged and subjects earned the fixed payoff for non-entry, $F = 20$.

The timing of events in a static game is as follows. At the start of each static game, subjects are randomly assigned to one of two, 10-player groups and a value of Y is drawn at random. The Y number chosen is the same for both groups. In the C treatment, this Y number is announced to the subjects on their computer screens. Each subject then uses their mouse to click on their choice, either not enter (A) or enter (B). After all subjects submitted their choices, the game is over. Each subject is then reminded of the Y number and their own choice (A or B). They are further informed of their own payoff for the game, the number in their group of 10 who "entered" (chose B) in that game and the payoff earned by those who "entered" (Y or 0); the payoff to non-entry is a known constant, F. In the G treatment, the Y number is not announced until *after* the game has been played. Instead, each subject chooses a cut-off strategy – an integer from the set $\{10 \leqslant I \leqslant 90\}$ – for the game and enters it when prompted by their computer terminal. Once all subjects had submitted their cut-off numbers, the computer program automatically selected an action, A or B for each subject, based on the value of Y chosen for that game and the subject's cut-off strategy. Subjects were then informed of the Y number, reminded of their cut-off value and shown the action chosen for them by the computer program based on their cut-off value. As in the C treatment, they were then informed of their individual payoff for the game, the number in their group of 10 who had (via their threshold) chosen to enter and the payoff earned by those who chose to enter (Y or 0). Before the next static game began, subjects were again randomly assigned to one of the two groups of size 10.

In all 'static' game sessions we used a within-subjects design where subjects played half of the 60 games under condition C and the other half under condition G. The order of play of the two conditions C and G was varied from session to session. In each session, the changeover from condition G to C or from condition C to G was *not* announced in advance; instead, following the first 30 entry games, the session was briefly paused while subjects were given new 'continuation' instructions on the rules of play for the remaining 30 entry games (see the experimental instructions for further details).

All dynamic game sessions both with and without delay costs were conducted only under the C treatment strategy space. In each dynamic game session, 30, 10-round dynamic games are played. The timing of events in a dynamic game is as follows. At the start of each dynamic game, two groups of 10 subjects were randomly formed. Then, as in the static-C treatment, a value of Y was randomly drawn at random and announced to all group members. In the dynamic treatment, this value of Y remains fixed over all 10 rounds of the dynamic game, as does the composition of the group. Subjects then decide whether to enter or not (B or A) by using their mouse to click on their action choice. In treatments with and without delay costs, a decision to enter in the first round was costless. In the delay cost dynamic game treatment a delay cost of 1/2 cent per round was assessed in rounds $2, 3, \ldots, 10$ for those who delayed entry beyond the first round. At the end of each round of the dynamic game, subjects were told the cumulative number in their group of size 10 (including themselves) who had chosen to enter in all prior rounds. If the 10th round had not yet been played, the dynamic game continued with another round.

Subjects were informed that a decision to enter (a B choice) once made, was *irreversible* and that they could only preserve the right to enter in a later round of the game (or never enter at all) by using their mouse to click on choice A (not enter) in each round in which they wanted to delay entry. Thus, even in the treatment without delay costs, there was some psychic cost to having to repeatedly click a mouse button for choice A (delay entering). Subjects who had chosen to enter in any prior round of the dynamic game (by clicking on choice B) did not have to make any further choice for the remaining rounds of that dynamic game. The payoff function for entry in the dynamic game sessions is the same one used in the static game, with two differences. First, in the dynamic game, the payoff depended on the number who had entered by the end of the final, 10th round; this number was compared with $\hat{f}(Y)$ to determine the payoff to those who chose to enter (payoffs were either Y or 0), as in the static game. Second, in the dynamic game with delay costs, payoffs were reduced if a player chose to enter after the first round.

Subjects earned their payoff in "points" from all games played in a session. In the static treatment, points were converted into dollars at the rate of 1 point = 1/2 cent, while in the dynamic treatment which had half as many games, the conversion factor was 1 point = 1 cent. In addition, subjects in all sessions received a \$5 show-up payment. Total payments for the 60 games played in the static-game sessions averaged \$19.95 for a 75 minute session. The dynamic game sessions also lasted for 75 minutes. Total payments for the 30 games played in the dynamic-game sessions without delay costs averaged \$20.21 per player; with delay costs, the average was \$19.82 per player.

4. Theoretical predictions

We assume that subjects are rational and risk neutral with regard to uncertain monetary amounts.[4] In the static game, we have the following predictions. First, if $Y < \underline{Y} = F$, the maximal payoff from entering, Y, is dominated by the payoff from not entering, F, so "not enter" (choice A) is a player's dominant strategy.[5] Similarly, if $Y \geqslant \overline{Y} = \hat{f}^{-1}(1) > F$, a decision to enter by a *single* individual (the 1 in the inverse mapping, $\hat{f}^{-1}$) guarantees a payoff of $Y > F$ so "enter" (choice B) is

[4] The predictions that follow may be different if agents are risk averse with respect to uncertain money amounts or boundedly rational in their payoff calculations.

[5] If $Y = \underline{Y} = F$, risk neutral individuals should be indifferent between choosing enter or not enter.

Table 1
Experimental design and predicted entry thresholds.

Treatment conditions			Number of sessions	Equilibrium concept GG or SGP, predicted entry threshold for Y
Game type: static or dynamic	Strategy space, no. games	F		
Static	C 30 games, G 30 games	20	3	GG, 42
Static	G 30 games, C 30 games	20	3	GG, 42
Dynamic	C 30 games, 10 rounds/game	20	6	SGP, 21
Dynamic	C + delay costs, 30 games, 10 rounds/game	20	6	SGP, 21
Static	C 30 games, G 30 games	50	2	GG, 62
Static	G 30 games, C 30 games	50	2	GG, 62
Dynamic	C 30 games, 10 rounds/game	50	4	SGP, 51

a player's dominant strategy. Given our specification for f and our rounding rule, we have $\overline{Y} = 72$. The lower bound, $\underline{Y}$ varies directly with the treatment variable, F: $\underline{Y} = 20$ or $\underline{Y} = 50$. For $\underline{Y} < Y < \overline{Y}$ there are two equilibria in pure strategies: all-enter and all-not-enter. This multiplicity is resolved in the global game approach of Morris and Shin (1998) by assuming that players do not know the true value of the state variable Y; instead each player i receives a private signal X_i of the Y value that is drawn randomly from a uniform distribution over $[Y - \lambda, Y + \lambda]$, where λ represents a small amount of noise. Morris and Shin show that this private information game has a unique threshold X^* below which players choose not to enter and above which players choose to enter. Heinemann (2000) further shows that in the limit, as the noise tends to zero, the threshold signal X^* converges to a threshold value, Y^*. As we do not consider the case of noisy signals – subjects in our experiment are either informed in advance of the value of Y or learn it after all have submitted their cut-off thresholds – this limiting threshold value Y^* is the relevant global game prediction for our static game. In our parameterization, Y^* is the solution to $Y^*[10 - \hat{f}(Y^*) + 1] = 10F$. Intuitively, Y^* is the value of Y for which players are indifferent between entering and not entering. For the treatment where $F = 20$, we have $Y^* = 42$ and for the treatment where $F = 50$, we have $Y^* = 62$.

In the dynamic game it remains a dominant strategy for players to not enter if $Y < \underline{Y} = F$ and to enter if $Y \geqslant \overline{Y} = \hat{f}^{-1}(1) = 72$. Unlike the static game, in the dynamic game, the $N = 10$ individuals face a game involving $n = 10$ rounds of play. A player's decision to enter prior to the last (10th) round of the dynamic game has an influence on the decisions of other players in future rounds. This feature of the dynamic game implies that subgame perfection is an appropriate solution concept.

In the dynamic game a player knows at each round how many players have already entered. A player's strategy must specify what action to take for every possible history of the game. We focus here on subgame perfect equilibria, and in particular on subgame perfect equilibria where one or more subjects choose to enter whenever $Y > F$. One such equilibrium is associated with everyone adopting the following strategy. If $Y \leqslant F$, do not enter. Otherwise, enter in round k if the number of others who have entered before round k is at least some critical number, $c(Y, k)$, where $c(Y, k) = \hat{f}(Y) - 1$ when $k = 10$ and $c(Y, k - 1) = c(Y, k)$ when $k < 10$. This strategy says: enter in round 10 if entry will assure you of a payoff of Y; enter in round 9 if your entry will induce sufficient entry in round 10 to yield you a payoff of Y; more generally, enter in round k, if your entry will induce sufficient entry in the remaining rounds to yield you a payoff of Y. Since the maximum value of $\hat{f}(Y) = 10$ in this equilibrium $c(Y, 1) \leqslant 0$ so everyone enters in round 1. Of course, absent any cost to delaying entry, there are many other subgame perfect equilibria in which everyone enters (or no one enters). But when there is a small cost to delayed entry, as in our dynamic game with delay cost treatment, then by iterated elimination of dominated strategies, the *only* subgame perfect equilibrium when $Y > F$ is the one where everyone enters immediately and without delay in round 1. This is also the Pareto efficient equilibrium of this game. We chose to study the dynamic game *without* delay costs as our static game treatment does not have any delay costs and we do not want to introduce more than a single change with each new treatment. However, one can also think of the static game as a version of the dynamic game *with* delay costs, where the delay costs are infinite.

Summarizing, the global game refinement predicts an entry threshold of 42 in the static game with $F = 20$ and an entry threshold of 62 in the static game with $F = 50$. In the dynamic game, where players can condition on the prior decisions of others, there exists a subgame perfect equilibrium where every player enters immediately in round 1 whenever $Y > F$. This Pareto efficient equilibrium is the *unique* subgame perfect equilibrium when there are delay costs to entry.

5. Empirical findings

We report results from 26 sessions involving 20 subjects each – a total of 520 subjects. Our experimental design, treatment conditions, the number of sessions conducted of each treatment and our equilibrium predictions are reported in Table 1.

Our main experimental finding is that there is no significant difference in entry behavior between static and dynamic entry games with the same value of F, regardless of whether subjects' entry decisions are determined by choice or according to elicited thresholds in the static treatment or whether or not there are costs associated with delayed entry in the dynamic treatment. In particular we summarize behavior in these games in terms of entry thresholds. Prior to describing these entry thresholds we first provide some evidence on subject rationality and efficiency in our experimental sessions.

Table 2
Dominated strategies and efficiency.

Sess. No.	Treatment conditions	F	Percent of subjects who *never* play a dominated strategy* C/G/All	Average frequency of the play of dominated strategies in instances where there were dominated strategies C/G/All	Payoff efficiency relative to equilibrium prediction† C/G/All	Equilib. concept-predicted entry threshold
1	Static C, G	20	0.85/0.80/0.80	0.02/0.07/0.04	0.99/1.03/1.01	GG-42
2	Static G, C	20	0.95/0.60/0.55	0.01/0.13/0.07	1.04/0.94/0.99	GG-42
3	Static C, G	20	1.00/0.60/0.60	0.00/0.08/0.03	1.01/0.98/0.99	GG-42
4	Static G, C	20	0.95/0.80/0.75	0.00/0.05/0.02	1.19/0.88/1.04	GG-42
5	Static C, G	20	1.00/0.80/0.80	0.00/0.04/0.02	0.96/0.87/0.91	GG-42
6	Static G, C	20	1.00/0.60/0.60	0.00/0.06/0.02	0.96/0.84/0.91	GG-42
7	Dyn C	20	0.95	0.03	0.91	SGP-21
8	Dyn C	20	1.00	0.00	0.94	SGP-21
9	Dyn C	20	0.80	0.04	0.96	SGP-21
10	Dyn C	20	1.00	0.00	0.97	SGP-21
11	Dyn C	20	0.95	0.03	0.90	SGP-21
12	Dyn C	20	0.95	0.03	0.90	SGP-21
13	Dyn C + DelayCost	20	0.95	0.03	0.92	SGP-21
14	Dyn C + DelayCost	20	0.90	0.03	0.92	SGP-21
15	Dyn C + DelayCost	20	0.95	0.03	0.92	SGP-21
16	Dyn C + DelayCost	20	0.90	0.03	0.98	SGP-21
17	Dyn C + DelayCost	20	0.65	0.04	0.89	SGP-21
18	Dyn C + DelayCost	20	0.90	0.03	0.95	SGP-21
19	Static C, G	50	0.75/0.70/0.65	0.03/0.05/0.04	0.99/0.97/0.98	GG-62
20	Static G, C	50	0.90/0.75/0.70	0.01/0.09/0.04	1.00/0.95/0.97	GG-62
21	Static C, G	50	0.95/0.80/0.75	0.01/0.03/0.02	1.01/0.99/1.00	GG-62
22	Static G, C	50	0.80/0.50/0.35	0.02/0.08/0.05	1.01/0.94/0.98	GG-62
23	Dyn C	50	0.90	0.01	0.99	SGP-51
24	Dyn C	50	0.70	0.02	0.98	SGP-51
25	Dyn C	50	0.80	0.01	0.99	SGP-51
26	Dyn C	50	1.00	0.00	0.99	SGP-51
Sessions 1–26 overall Avgs:			0.80	0.03	0.96	

* Dominated strategies are entering (choice B) when $Y < F$ or not entering (choice A) when $Y > \overline{Y}$. For the dynamic treatment, entry decisions are assessed as of the final (10th) round of the game.
† Payoff efficiency calculations include losses due to delay costs in the dynamic treatment with delay costs.

5.1. Dominated strategies and efficiency

A basic test of rationality is whether subjects avoided play of dominated strategies. Table 2, column 4 reports the percentage of subjects in each session who never once played a dominated strategy over the 30–60 games of the treatment they were in. For the static game treatments, these frequencies are divided up according to the 30 C games, the 30 G games and all 60 games. For example, the first session frequencies of 0.85/0.80/0.80 mean that 17/16/16 of the 20 subjects never once played a dominated strategy in the C/G/ and both treatments (All). Table 2 column 5 reports the frequency with which subjects played dominated strategies in instances where dominated strategies could be played. Again, these frequencies are disaggregated for the static treatment sessions. The frequencies shown in columns 4–5 of Table 2 provide support for the notion that most subjects avoided the play of dominated strategies.

Finding 1. 80% of subjects in our experiment *never* played a dominated strategy. In instances where it was possible to play dominated strategies ($Y < \underline{Y} = F$, $Y > \overline{Y} = 72$) the frequency of play of dominated strategies is low, averaging just 3%.

Finding 1 is consistent with Heinemann et al.'s (2004) finding that subjects in their design largely avoided the play of dominated strategies.[6] We note that the play of dominated strategies was always more frequent in the G treatment than in the comparable C treatment of each static game session.

Table 2 column 6 reports on a measure of *payoff efficiency*, specifically, how subjects' payoffs compare with those they could have earned if all had played according to the threshold strategies specified by the equilibrium solution concept for the class of entry games examined in each session. For instance, session 1 involved play of a static entry game with $F = 20$. Under the global game (GG) solution concept, the threshold equilibrium prediction is that subjects enter if $Y \geqslant 42$ and don't enter otherwise. Our payoff efficiency measure takes the sum of all subjects' payoffs for all games played in the session and divides that number by the sum of the payoffs that all subjects would have earned had they faced the exact

[6] While Heinemann et al. (2004) report a somewhat higher frequency of play of dominated strategies (8%), this frequency is still quite low. The difference may be owing to experimental design differences: Heinemann et al. elicit entry decisions for 10 different situations (Y values) in each of 16 repetitions whereas we elicit only a single entry decision (for a single Y value) in each of 30–60 repetitions. With more decisions to make in a single repetition (10 rather than 1) the play of dominated strategies might become more frequent.

same sequence of randomly drawn Y numbers and played according to the global game equilibrium strategy. For the static game sessions, these efficiency ratios are again disaggregated by treatment. For example, in the static game session 1, the payoff efficiency ratios of 0.99/1.03/1.01 indicate that subjects earned 99% of the payoffs they could have gotten by playing the GG-42 equilibrium cut point strategy in the C treatment, 103% of such equilibrium payoffs in the G treatment and 101% of equilibrium payoffs in both treatments (over all). For the dynamic game sessions, the solution concept is subgame perfection, SGP, and the entry threshold is the lower, Pareto efficient one where all enter whenever $Y > F$. Based on data in column 6 of Table 2 we have:

Finding 2. Over all sessions, subjects earned payoffs that averaged 96% of the payoffs they could have earned by following either the global game (static treatment) or the subgame perfect (dynamic treatment) equilibrium threshold strategies.

We further observe that, using nonparametric two-sided Wilcoxon–Mann–Whitney tests on the session level payoff efficiency averages reported in Table 2 (we use the observations for "all" games of each session), there is only a marginally significant difference in payoff efficiency relative to equilibrium prediction between the six static and six dynamic $F = 20$ sessions without delay costs, i.e., efficiency is slightly higher in sessions 1–6 versus sessions 7–12 ($p = 0.07$); however this might be attributed to the different equilibrium thresholds we use for the two types of games. There is no significant difference in payoff efficiency relative to equilibrium prediction between the six static and six dynamic $F = 20$ sessions with delay costs, i.e., sessions 1–6 versus sessions 13–18 ($p = 0.19$). There is also no difference in payoff efficiency relative to equilibrium predictions between the six sessions of the two dynamic $F = 20$ treatments, i.e., sessions 7–12 versus sessions 13–18 ($p = 0.87$). Finally, there is no difference in payoff efficiency relative to equilibrium predictions between the four static and four dynamic $F = 50$ sessions, i.e., sessions 19–22 versus 23–25 ($p = 0.36$). Indeed, efficiency is very high across all treatments and Finding 2 suggests that both equilibrium refinements may be characterizing subject behavior rather well. However, as we shall see in the next two sections, the payoff efficiency evidence does not indicate that subjects were actually playing according to the prescribed equilibrium threshold strategies.

5.2. Estimated entry thresholds across treatments

We now turn to the main focus of our analysis, the entry thresholds characterizing subject behavior across the static and dynamic game treatments of our experiment. For the static-G treatment, we have elicited entry thresholds for each subject; we will later (in Section 4.4) address the consistency of those elicited thresholds with choice behavior in the static-C treatment. As for the static-C treatment, as well as for all our dynamic treatment sessions, empirical entry thresholds have to be estimated from observed entry decisions. Here we follow Heinemann et al. (2004) and *estimate* a logit response model in which the binary entry decision depends on a constant and the Y value, using all data (binary entry decisions and associated Y values) from a given treatment of a session. That is, we use maximum likelihood estimation to find the coefficient estimates $\hat{a}$ and $\hat{b}$, that are a best fit to the logit response function:

$$\Pr(\mathrm{B}|Y) = \left[1 + \exp(-a - bY)\right]^{-1} \tag{2}$$

The entry threshold can be viewed as the critical value, Y^*, for which a representative player is indifferent between entering and not entering, which obtains when $\Pr(\mathrm{B}|Y) = 0.5$. Using that probability in (2) and the maximum likelihood estimates of a and b, we obtain the estimated entry threshold $\hat{Y}^* = -\hat{a}/\hat{b}$. The standard deviation of a logistic distribution with mean $-\hat{a}/\hat{b}$, and scale parameter $1/\hat{b}$ is given by $\frac{\pi}{\hat{b}\sqrt{3}}$, which we take as a measure of the coordination of subjects around the estimated entry threshold. We estimate the entry threshold and its standard deviation for all static-C and dynamic treatment sessions as well as for the static-G treatment sessions; in the latter case, we can compare the logit-estimated thresholds and standard deviations with the actual mean and standard deviation of the elicited entry thresholds. For the dynamic game treatment, we use each player's entry decision as of the final, 10th round of each game.

For each of our 26 sessions, Table 3 reports the logit estimates of a, b, the ratio $-a/b$, and the associated standard deviations. Note that we again divide the static game sessions up according to whether it was a C or G treatment. Static game sessions involved play of 30 games of each treatment, just as dynamic game sessions involved the play of 30 dynamic games. Thus, each of the 36 estimates in Table 3 is based on data from 30 games played by 20 subjects (consisting of 600 individual observations).

Based on the estimated entry thresholds, $-\hat{a}/\hat{b}$, as shown in Table 3 we report the following findings:

Finding 3. Estimated entry thresholds in the static-C treatment sessions are indistinguishable from estimated entry thresholds in the corresponding dynamic treatment sessions (same F value) without delay costs.

Using a nonparametric Wilcoxon–Mann–Whitney test, we cannot reject the null hypothesis of no difference between the six, session-level estimated entry thresholds for the $F = 20$ static-C treatment and the six, session-level estimated thresholds for the $F = 20$ dynamic treatment without delay costs, i.e., the estimated thresholds for sessions 1–6 versus sessions 7–12 as reported in column 7 of Table 3 ($p = 0.63$). The same conclusion holds for the four session-level estimated entry thresholds

Table 3
Logit coefficient estimates, implied entry threshold and standard deviation.

Sess. No.	Session characteristics			C treatment estimates				G treatment estimates				Equilib. concept-pred.
	F	Game type	Treat. order	$\hat{a}$	$\hat{b}$	$-\hat{a}/\hat{b}$	$\frac{\pi}{\hat{b}\sqrt{3}}$	$\hat{a}$	$\hat{b}$	$-\hat{a}/\hat{b}$	$\frac{\pi}{\hat{b}\sqrt{3}}$	
1	20	Static	C, G	−4.63	0.15	31.65	12.40	−4.10	0.13	31.30	13.85	GG-42
2	20	Static	G, C	−6.77	0.24	28.57	7.65	−2.26	0.09	25.39	20.38	GG-42
3	20	Static	C, G	−7.90	0.24	32.92	7.56	−3.85	0.15	25.67	12.09	GG-42
4	20	Static	G, C	−8.60	0.38	22.63	4.77	−4.70	0.12	39.17	15.11	GG-42
5	20	Static	C, G	−10.67	0.28	37.84	6.43	−4.97	0.15	34.12	12.44	GG-42
6	20	Static	G, C	−7.97	0.29	27.36	6.22	−3.31	0.11	30.52	16.72	GG-42
7	20	Dynamic	C	−6.63	0.21	31.89	8.72	n/a	n/a	n/a	n/a	SGP-21
8	20	Dynamic	C	−11.18	0.45	24.77	4.02	n/a	n/a	n/a	n/a	SGP-21
9	20	Dynamic	C	−6.88	0.30	22.77	6.00	n/a	n/a	n/a	n/a	SGP-21
10	20	Dynamic	C	−6.28	0.24	26.17	7.56	n/a	n/a	n/a	n/a	SGP-21
11	20	Dynamic	C	−11.47	0.32	36.19	5.72	n/a	n/a	n/a	n/a	SGP-21
12	20	Dynamic	C	−9.47	0.32	29.22	5.59	n/a	n/a	n/a	n/a	SGP-21
13	20	Dynamic + DC	C	−10.60	0.43	24.72	4.23	n/a	n/a	n/a	n/a	SGP-21
14	20	Dynamic + DC	C	−9.37	0.39	23.92	4.63	n/a	n/a	n/a	n/a	SGP-21
15	20	Dynamic + DC	C	−11.18	0.36	31.28	5.07	n/a	n/a	n/a	n/a	SGP-21
16	20	Dynamic + DC	C	−8.76	0.48	18.28	3.79	n/a	n/a	n/a	n/a	SGP-21
17	20	Dynamic + DC	C	−6.29	0.19	33.64	9.71	n/a	n/a	n/a	n/a	SGP-21
18	20	Dynamic + DC	C	−10.19	0.44	23.01	4.10	n/a	n/a	n/a	n/a	SGP-21
19	50	Static	C, G	−9.54	0.18	54.47	10.35	−7.87	0.13	59.99	13.83	GG-62
20	50	Static	G, C	−18.89	0.36	52.36	5.03	−5.03	0.09	55.21	19.92	GG-62
21	50	Static	C, G	−16.97	0.34	51.46	5.41	−11.42	0.22	51.71	8.21	GG-62
22	50	Static	G, C	−14.40	0.29	49.65	6.36	−7.08	0.13	54.46	14.52	GG-62
23	50	Dynamic	C	−41.33	0.83	49.95	2.19	n/a	n/a	n/a	n/a	SGP-51
24	50	Dynamic	C	−15.31	0.31	48.90	5.79	n/a	n/a	n/a	n/a	SGP-51
25	50	Dynamic	C	−19.60	0.38	51.58	4.82	n/a	n/a	n/a	n/a	SGP-51
26	50	Dynamic	C	−49.81	0.97	51.35	1.87	n/a	n/a	n/a	n/a	SGP-51

for the $F = 50$ static-C treatment and the four session-level estimated entry thresholds for the $F = 50$ dynamic treatment without delay costs, i.e., sessions 19–22 versus sessions 23–26 ($p = 0.25$).

Finding 4. The addition of delay costs to the dynamic, $F = 20$ game does not lead to any significant difference in estimated entry thresholds relative to the static-C or dynamic game without delay costs.

Again using a nonparametric Wilcoxon–Mann–Whitney test, we find we cannot reject the null hypothesis of no difference between the six estimated entry thresholds for the dynamic $F = 20$ game *with* delay costs and (1) the six estimated entry thresholds for the $F = 20$ static-C game, i.e., sessions 13–18 versus sessions 1–6 ($p = 0.26$), or (2) the six estimated entry thresholds for the $F = 20$ dynamic game *without* delay costs, i.e., sessions 13–18 versus sessions 7–12 ($p = 0.34$). Thus, despite the fact that delay costs make the efficient equilibrium of the dynamic game the *unique* subgame perfect equilibrium, there is little difference in behavior relative to static or dynamic versions of the $F = 20$ game without delay costs.

Finding 5. Estimated entry thresholds in the $F = 20$ static-G treatment are indistinguishable from estimated entry thresholds in (1) the $F = 20$ static-C treatment and (2) the $F = 20$ dynamic, no delay cost treatment but they are significantly greater than in (3) the $F = 20$ dynamic, delay cost treatment. Estimated entry thresholds in the $F = 50$ static-G treatment are significantly greater than in (1) the $F = 50$, static-C treatment and (2) the $F = 50$ dynamic, no delay cost treatment.

For the $F = 20$ static-G versus static-C treatment comparison we use a two-sided Wilcoxon signed rank test for matched pairs (as the data come from a single, within-subject sample involving two treatments) and we find we cannot reject the null hypothesis of no difference using the six session-level estimated entry thresholds for each treatment ($p = 0.60$). For the comparisons between the $F = 20$ static-G and dynamic-C treatments we used a two-sided Wilcoxon–Mann–Whitney test and found we could not reject the null of no difference in estimated entry thresholds between the static-G treatment and the dynamic game without delay cost treatment ($p = 0.42$) but we found that entry thresholds in the static-G treatment were significantly greater than in the dynamic $F = 20$ treatment with delay costs ($p = 0.08$).

For the $F = 50$ static-G versus static-C comparison, a two-sided Wilcoxon signed rank test leads us to reject the null in favor of the alternative that entry thresholds are higher in the $F = 50$ static-G treatment ($p = 0.07$). For the comparison between the $F = 50$ static-G and dynamic-G treatment without delay costs a Wilcoxon–Mann–Whitney test leads us to reject the null in favor of the alternative that estimated entry thresholds are higher in the static-G treatment than in the dynamic no delay cost treatment ($p = 0.02$).

While the static-G treatment involves a different methodology for gathering entry decision data and is thus not strictly comparable to the static-C and dynamic treatments, we find only mixed evidence of a difference in behavior across these two elicitation methods, particularly for the $F = 20$ treatment.

Table 4
Linear regression examining treatment effects: coefficient estimates (standard error).

Variable	Entry threshold, $-a/b$	Standard deviation, $\pi/(b\sqrt{3})$
Intercept	30.16***	7.51***
	(2.18)	(1.09)
δ^G	0.87	7.59***
	(3.52)	(1.79)
δ^{50}	21.82***	−0.72
	(2.38)	(1.61)
δ^D	−1.66	−1.24
	(3.01)	(1.29)
δ^{DC}	−2.69	−1.01
	(3.15)	(1.16)
$\delta^G \times \delta^{50}$	2.49	−0.26
	(3.70)	(3.24)
$\delta^{50} \times \delta^D$	0.12	−1.88
	(3.21)	(1.99)
R^2	0.88	0.73

Statistically significant at the: 0.01 level, ***; 0.05 level, **; 0.10 level, *.

For further evidence confirming the findings above, we report the results of two simple OLS regressions involving the 36 logit-estimated entry thresholds (the values of $-\hat{a}/\hat{b}$ as reported in Table 3) and the 36 estimated standard deviations ($\frac{\pi}{\hat{b}\sqrt{3}}$, also from Table 3) on several dummy variables: $\delta^G = 1$ if the G, "cut-point strategy" treatment was used, 0 otherwise; $\delta^{50} = 1$ if $F = 50$, 0 otherwise, $\delta^D = 1$ if the entry game was dynamic, 0 otherwise, $\delta^{DC} = 1$ if there were delay costs, 0 otherwise as well as two multiplicative dummies, $\delta^G \times \delta^{50}$ and $\delta^{50} \times \delta^D$. The OLS regression results – with standard errors (in parentheses) that have been corrected for clustering of estimates within (static-treatment) sessions using a Huber–White sandwich estimator – are presented in Table 4.

For the regression involving the estimated entry threshold (column 2 of Table 4) we see that the change in the value of F from 20 to 50 significantly increases this threshold, as predicted, from an estimated mean of about 30 to one of about 52. However, all other treatment variable conditions appear to be irrelevant for the determination of the entry threshold, as the coefficient estimates on the dummy variables δ^D, δ^G, δ^{DC}, $\delta^G \times \delta^{50}$ and $\delta^{50} \times \delta^D$ are not significantly different from zero.

For the regression involving the standard deviation as the dependent variable (column 3 of Table 4), we find that the only treatment condition with a significant effect on the standard deviation of entry decisions is whether subjects submitted cut-off strategies or made action choices as indicated by the significant coefficient estimate on the δ^G dummy variable. In particular, we observe that the standard deviation of entry decisions is significantly larger in the static-G treatment where we elicited cut-off strategies than in the static-C treatment where actions were chosen. Unlike the C treatment, in the G treatment the Y value is *not* known at the time a strategy is submitted. This difference in the timing with which the Y value is revealed may lead subjects to think more (less) carefully in the C (G) treatment, where their choice is less (more) hypothetical and we posit that this difference can account for the significantly higher variance in entry choices (cut-off values) in the G treatment relative to the C treatment.

Finally, we test for differences between the logit-estimated entry thresholds in Table 3 and the global game or subgame perfect equilibrium predictions. Using a simple t-test, we consider whether the six independent estimated entry thresholds for the static, $F = 20$ game are statistically different from the global game prediction. For both the C and G treatments, we may reject the null of no difference from the global game threshold of 42 (two-sided t-test, $p = 0.00$ in the C treatment, $p = 0.00$ in the G treatment). For the $F = 20$ dynamic game, we use a *one*-sided version of the t-test to determine whether the six estimated entry thresholds for the dynamic $F = 20$ game with or without delay costs are greater than the subgame perfect equilibrium threshold prediction of 21 (it would not be rational for these thresholds to be lower than 21). We again find that we can reject the null of no difference in favor of the alternative that entry thresholds are greater than 21 for the dynamic treatment without delay costs ($p = 0.01$) or with delay costs ($p = 0.05$). We conclude that in the $F = 20$ case, entry thresholds in the static game lie *below* the global game threshold while in the dynamic game they lie *above* the subgame perfect equilibrium prediction, which is consistent with our earlier finding of no significant difference in the distribution of entry thresholds between these two treatments.

For the $F = 50$ static game sessions, a t-test indicates that we may also reject the null of no difference between the four estimated session-level entry thresholds and the predicted global game threshold of 62 for both the C and G treatments (two-sided t-test, $p = 0.01$ for the C treatment, $p = 0.03$ for the G treatment). However, for the $F = 50$ dynamic game, a one-sided t-test indicates that we *cannot* reject the null of no difference between the four session level estimated entry thresholds and the subgame perfect (and efficient) equilibrium threshold prediction of 51 (one-sided test, $p = 0.79$). Given that we earlier found no difference in the distribution of entry thresholds between the static-C treatment and the dynamic treatment of the $F = 50$ game, it would seem that entry thresholds in the static-C treatment should also not differ significantly from the efficient equilibrium threshold of 51 for the dynamic game. Indeed, a t-test confirms that we cannot reject the null hypothesis of no difference between the estimated entry thresholds in the static-C treatment and a hypothesized entry threshold of 51 (one-sided test, $p = 0.20$).[7] As noted earlier, observed and estimated entry thresholds in the static game where $F = 50$ are a smaller proportion of the distance $Y^* - F$ than are entry thresholds in the static game where $F = 20$. This difference most likely reflects the greater likelihood that entry will succeed in the $F = 50$ treatment as compared with the $F = 20$ treatment, as in the $F = 20$ case, more subjects must choose entry on average for entry to yield a non-zero payoff than in the $F = 50$ case. Summarizing, these last findings with regard to our equilibrium predictions we have:

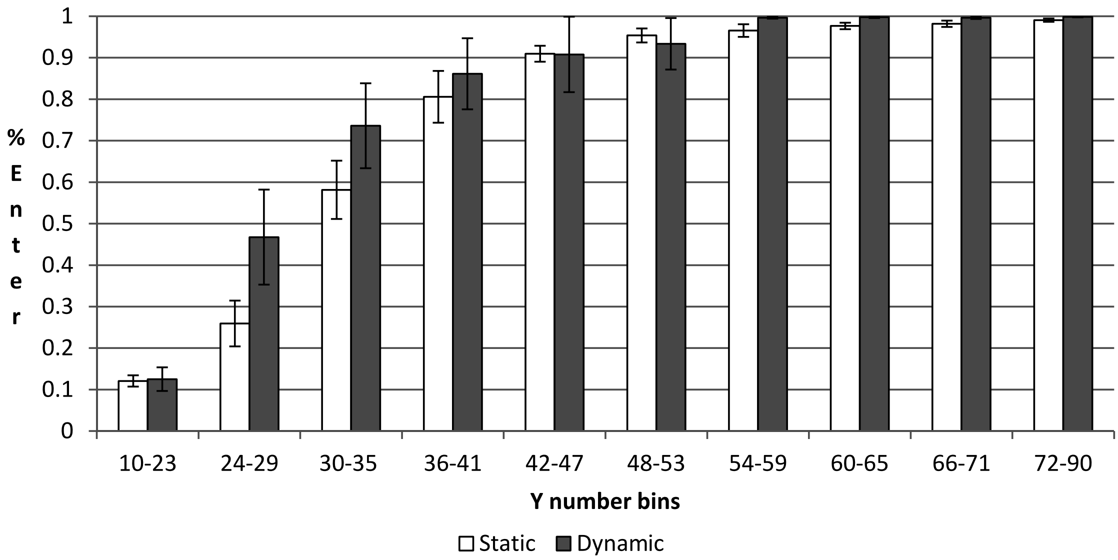
Finding 6. Estimated entry thresholds in both the $F = 20$ and $F = 50$ *static* game treatments are significantly below the global game predictions. A comparison of the distribution of entry thresholds between *static-C* game and *dynamic* game treatments with the *same F value* reveal no significant differences.

Finding 7. In the $F = 50$ treatment, the efficient strategy of entering if $Y > F$ and not entering otherwise can be used to characterize behavior in *both* the static-C and dynamic game treatments.

5.3. Distribution of entry frequencies

In addition to considering whether there are differences in estimated entry thresholds between treatments, it is also of interest to examine whether the *distribution* of entry frequencies over Y numbers differs between the static and dynamic treatments. Figs. 1(a)–1(d) show the mean entry frequencies in all static and dynamic $F = 20$ sessions disaggregated by Y number bins. Here we used the same bins as are used in the experimental design to determine the number of entrants needed for successful entry. Specifically for each Y-bin we report the mean entry frequency along with associated standard errors, indicated by the error bars in these figures, which provide an indication of the precision of these means across sessions.

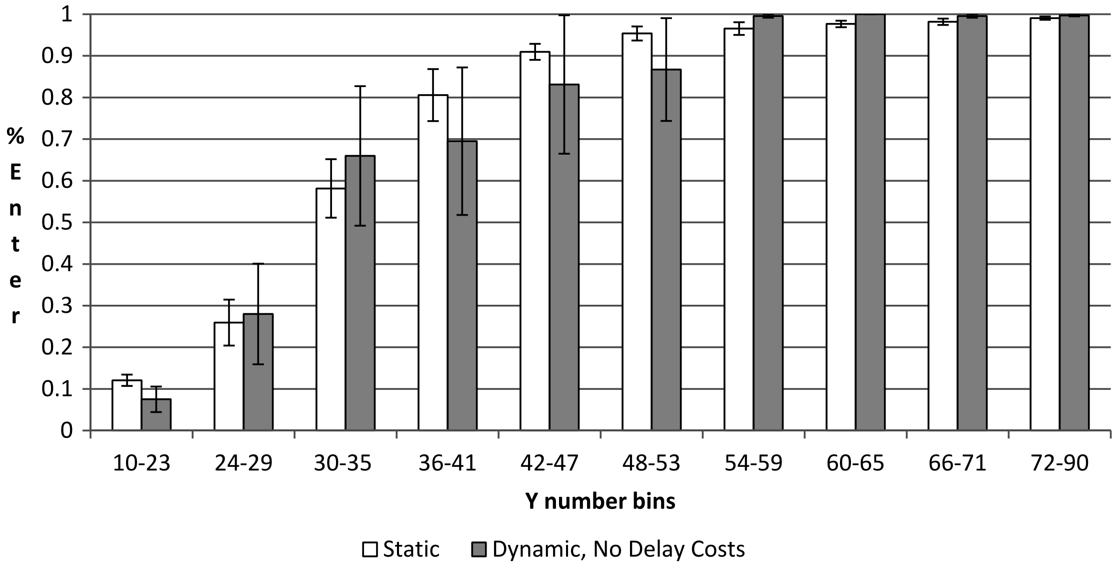
Fig. 1(a) compares mean entry frequencies using pooled data from the six static $F = 20$ game sessions with pooled data from the twelve $F = 20$ dynamic game sessions (both with and without delay costs). T-tests confirm the impression that whenever the standard error bars in these figures do not overlap we can reject the null hypothesis of no difference in favor of the alternative that entry frequencies are statistically higher in the one treatment (one-sided test $p < 0.10$ in all such instances). We observe that entry frequencies in the dynamic game are significantly higher in 5 of the 10 bins. However, in 4 of those 5 bins – the last four Y bins involving Y numbers between 54 and 90 – the differences in entry frequencies are not economically important. For instance, in the 54–59 bin the mean entry frequency in the static sessions is 96.5% versus 99.9% in the dynamic sessions; both frequencies guarantee that entry is successful in this bin. As for the other Y-bin where



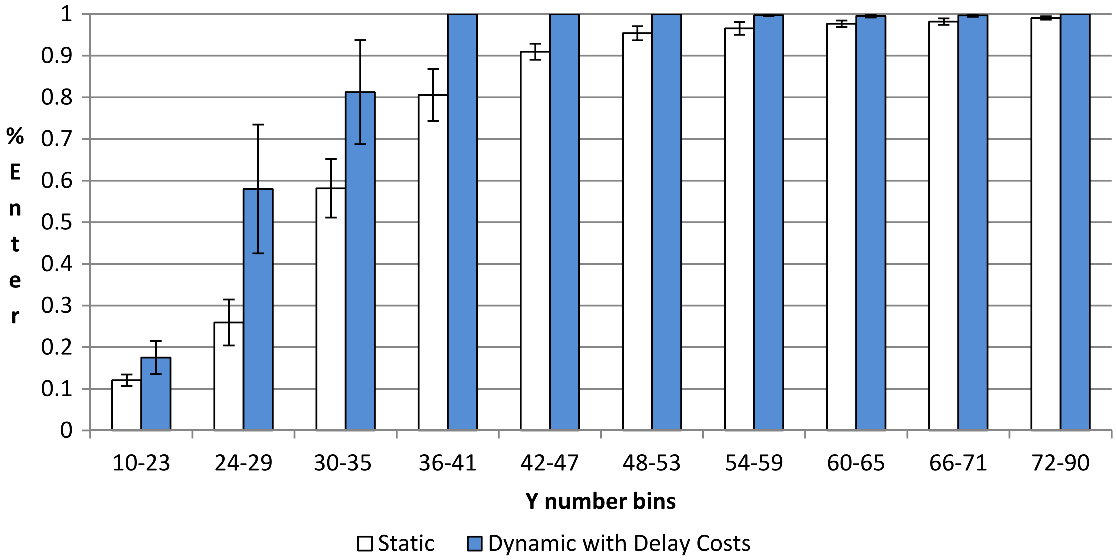
(a)

Fig. 1. Mean entry frequencies by Y number bins: (a) from 6 static and all 12 dynamic game sessions, with 1 standard error band; (b) from 6 static and 6 dynamic game sessions *without* delay costs, with 1 standard error band; (c) from 6 static and 6 dynamic game sessions *with* delay costs, with 1 standard error band; (d) from 6 dynamic game sessions *without* delay costs and 6 dynamic game sessions *with* delay costs, with 1 standard error band.

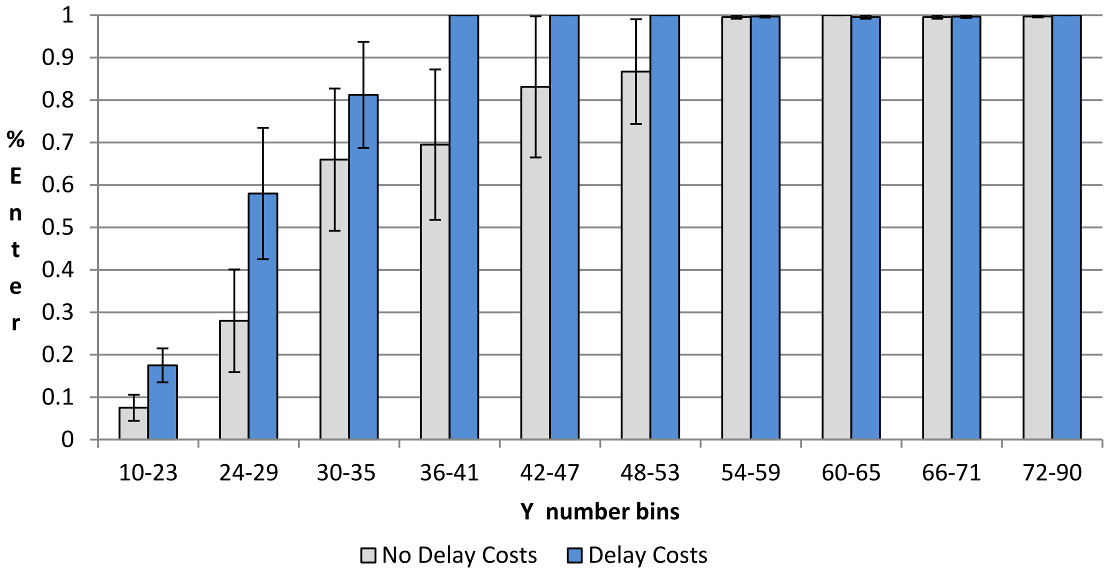
[7] We *can* reject the null of no difference between the estimated entry thresholds in the static-G treatment and a hypothesized entry threshold of 51 in favor of the alternative that the static G-treatment thresholds are greater than 51 (one-sided test, $p = 0.04$). However, the timing of information concerning the Y-value in the G treatment is different from that in the static-C and dynamic game treatments, so the more natural comparison to make (as in the text above) is between the static-C and dynamic treatments with the same value of F.



(b)



(c)



(d)

Fig. 1. (*continued*)

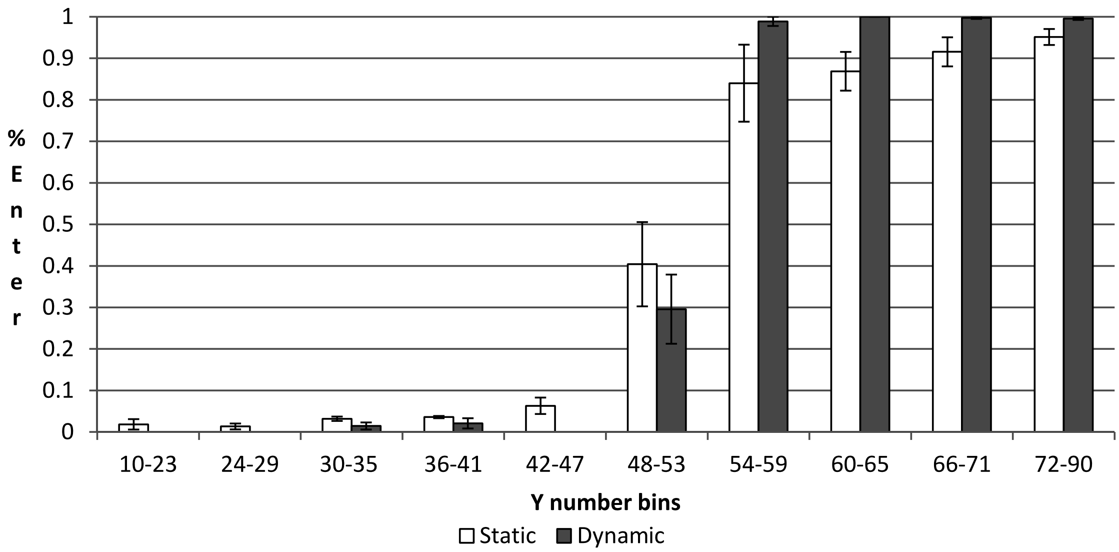


Fig. 2. Mean entry frequencies by Y number bins from 4 static and 4 dynamic game sessions where $F = 50$, with 1 standard error band.

entry frequencies in the dynamic game are statistically higher than in the static game – the bin for Y values between 24 and 29 – the entry frequencies in that bin are always less than 50%, which lies well below the 80% entry frequency needed for successful entry via the hurdle function $\hat{f}$; thus again, the difference is not economically significant.

Figs. 1(b)–1(d) provide comparisons of mean entry frequencies across bins between two of the three different treatments of the $F = 20$ entry game. Once we remove the six sessions of the $F = 20$ dynamic game with delay costs, we observe in Fig. 1(b) that there is little difference in entry frequencies by Y number bin between the six static and six dynamic without delay cost sessions, except again for the last four bins, where entry frequencies are significantly higher in the dynamic treatment. The $F = 20$ dynamic game *with delay costs* does lead to significant increases in entry frequencies across nearly all bins by comparison with either the static or the dynamic game without delay costs as seen in Figs. 1(c) and 1(d).

Fig. 2 shows the mean frequency of entry in the $F = 50$ static and dynamic game sessions disaggregated by Y number bins. Here again, whenever the standard error bars in the figure do not overlap, t-tests allow us to reject the null hypothesis of no difference in favor of the alternative that entry frequencies are statistically higher in one treatment (one-sided test $p < 0.10$ in all such instances). We observe no significant difference in entry frequencies between the static and dynamic game treatments when Y is in the critical, 48–53 bin. However, for values of $Y > 53$, entry is significantly higher in the dynamic than in the static treatment of the $F = 50$ entry game.

We emphasize again that while many differences in entry frequencies across Y-bins are statistically significant, these differences are not *economically* important. If we consider the important question of whether the mean entry frequency equaled or exceeded the relevant hurdle value, $\hat{f}(Y)$, we find no difference between the static-C or dynamic game treatments for any of the 10 Y-bins. And, indeed as we have already seen in the estimates reported in Table 3, the small but statistically significant differences in entry frequencies observed in Figs. 1(a)–1(d) and 2 do not lead to significant differences in the overall estimated entry threshold, $\hat{Y}^*$, across treatments. We summarize these findings as follows:

Finding 8. There are differences in the distribution of entry frequencies over Y values between the static and dynamic entry games with the same F value. Specifically entry game frequencies are often significantly higher in dynamic games with delay costs or for large values of Y. However these entry frequency differences do not affect whether entry is successful or not on average, in any Y-bin across all treatments.

5.4. Consistency of cut-point strategies with action choices in the static game

We now compare the elicited entry thresholds in the static-G treatments with choice behavior in the static-C treatments. We begin by reporting in Table 5 the mean and standard deviations of the threshold strategies elicited in all sessions involving the static-G treatment. For comparison purposes we also report the estimated entry thresholds and standard deviations (from Table 3) for these same static-G sessions. Specifically, for each static game session, we report in column 4 of Table 5 the mean elicited cut-off value provided by all 20 subjects in the 30 static-G games played in the session and in column 5 we provide the associated standard deviations. The average elicited cut-off value for the static-G treatment is 32.41 when $F = 20$ and 55.67 when $F = 50$. These overall averages, along with the individual session-level averages, lie *below* the global game prediction of 42 and 62 for the $F = 20$, $F = 50$ treatments, respectively. Indeed, using the session-level averages, a one-sample, one-sided Wilcoxon signed rank test reveals that we may reject the null hypothesis that the cut-off is 42 for the $F = 20$ treatment and that the cut-off is 62 for the $F = 50$ treatments in favor of the alternative that both cut-offs are lower ($p = 0.03$, $F = 20$, $p = 0.07$, $F = 50$). Also, contrary to the global game prediction, we find using

Table 5
Elicited entry thresholds, static-G treatment sessions and elicited entry thresholds.

Sess. No.	Treatment order	F	Mean elicited entry threshold games 1–30	St. dev. of elicited entry threshold games 1–30	Mean elicited entry threshold games 26–30	St. dev. of elicited entry threshold games 26–30	Estimated entry threshold from Table 3	Estimated std. deviation from Table 3
1	C, G	20	31.24	12.93	30.91	9.19	31.30	13.85
2	G, C	20	29.25	16.81	29.12	11.78	25.39	20.38
3	C, G	20	27.97	12.10	28.35	8.97	25.67	12.09
4	G, C	20	40.77	15.46	39.50	10.97	39.17	15.11
5	C, G	20	33.78	11.74	34.76	8.92	34.12	12.44
6	G, C	20	31.45	13.99	32.20	12.40	30.52	16.72
1–6	Averages	20	32.41	13.84	32.47	10.37	31.03	15.10
19	C, G	50	61.80	14.46	62.20	12.43	59.99	13.83
20	G. C	50	55.32	15.36	53.58	11.79	55.21	19.92
21	C, G	50	50.20	8.49	49.89	8.96	51.71	8.21
22	G, C	50	55.36	11.98	53.14	8.32	54.46	14.52
19–22	Averages	50	55.67	12.57	54.70	12.43	55.34	14.12

the same test on session-level data that there is significant variance in the distributions of these elicited thresholds, i.e., the standard deviations reported in Table 5, column 5 are far from zero. Columns 6–7 of Table 5 report the same mean and standard deviations of elicited entry thresholds but using data from just the final 5 games of each session. Notice that while there is little difference in the mean elicited thresholds over the last 5 games as compared with all 30 games, there is typically a reduction in the standard deviation. This observation suggests that subjects were converging on entry thresholds that are well approximated by the 30-game mean elicited entry thresholds.

Finding 9. Estimated entry thresholds and standard deviations for the static-G treatments are a good approximation to the mean elicited entry thresholds and standard deviations for the static-G treatments.

Support for Finding 9 comes from Table 5, where we observe little difference between the mean elicited entry thresholds (column 4) and the estimated entry thresholds (column 8) or between the standard deviations in elicited entry thresholds (column 5) and the standard deviations in the estimated entry thresholds (column 9). For the $F = 20$ treatment, we have enough observations (6) to conduct a meaningful two-sided, Wilcoxon matched-pairs signed ranks test of the null hypothesis that there is no difference between the elicited and estimated measures as reported in Table 5. The tests confirm that we cannot reject the null hypothesis of no difference between the mean elicited entry thresholds (column 4) and the estimated entry thresholds (column 8) or between the standard deviations in elicited entry thresholds (column 5) and the standard deviations in the estimated entry thresholds (column 9) for the $F = 20$ treatment ($p > 0.10$ for both tests). This finding provides us with some assurance that the logit-estimated thresholds and associated standard deviations reported in Table 3 are a good approximation to the average cut-off values that were elicited from subjects in our static-G treatment. We further observe that:

Finding 10. In both the $F = 20$ and $F = 50$ treatments the mean elicited entry thresholds in the static-G treatment are very similar to the logit-estimated thresholds in the corresponding static-C treatment.

When the six estimated entry thresholds for the static-C treatment of the $F = 20$ static game (Table 3, column 7) are compared with the corresponding six mean elicited thresholds for the static-G treatment of the same game (Table 5, column 4), we find no significant difference (Wilcoxon matched-pairs signed-ranks test (2-sided test, $p = 0.75$). For the $F = 50$ treatment, we do not have enough independent observations (just 4) to do a meaningful test of differences between static-C and static-G treatments. Nevertheless, it appears that for the $F = 50$ static game, the differences between the four estimated entry thresholds of the static-C treatment (Table 3, column 7) and the four mean elicited thresholds of the static-G treatment (Table 5, column 4) are quite small.

The next two findings address in further detail the consistency of behavior between the static-C and the static-G treatments and whether there is a change in strategy when the treatment changes. It could well be that the change in the static game treatment from C to G or G to C is used by subjects as an opportunity to change their strategy with the possible aim of increased efficiency.[8] Thus we first explore whether subjects changed their strategy in moving from the static-C treatment to the static-G treatment or when the reverse order was in effect. In static game sessions where the treatment order was C, G, we calculated the mean elicited entry threshold for each subject in the first 5 games (repetitions) of the G treatment and used that threshold to predict each subject's behavior in the last 15 games of the C treatment that preceded the switch to

[8] For instance, Van Huyck et al. (1991) found evidence that subjects changed strategies with a change in the treatment conditions of an experimental coordination game.

the G treatment (i.e., using the same sequence of Y values they faced in those last 15 games of the C treatment).[9] Similarly, in static sessions where the treatment order was G, C, we calculated the mean elicited entry threshold for each subject in the last 5 games of the G treatment and used that threshold to predict each subject's behavior in the first 15 games of the C treatment that followed the G treatment. The result is a consistency metric for each of the 200 subjects who participated in one of our 10 static game sessions. A consistency metric value of 1 corresponds to perfect consistency between a subject's mean elicited strategy threshold in the static-G treatment and the subject's action choices in the 15 immediate prior or 15 immediate subsequent games of the static-C treatment and a value of 0 corresponds to complete inconsistency.

We regress these 200 individual observations for the consistency metric on a constant and two dummies: $\delta^{50} = 1$ if F was 50, 0 otherwise and $\delta^{G,C} = 1$ if the treatment order was 30 rounds of the static-G treatment (strategy method) followed by 30 rounds of the static-C treatment and 0 for the reverse order.

The OLS regression results, with standard errors (in parentheses) that have been corrected for clustering of the consistency metric within sessions using a Huber–White sandwich estimator, are as follows:

$$\begin{array}{lcccc} \text{Consistency} = & \underset{(0.023)}{0.868} & - \underset{(0.024)}{0.001\delta^{G,C}} & + \underset{(0.023)}{0.058\delta^{50}} \\ & & & R^2 = 0.06 \end{array}$$

The regression results reveal that mean consistency in the baseline $F = 20$ static game is high, at around 87%; that is, 87% of the time, subjects' action choices in the C treatment of the $F = 20$ static game were consistent with their cut-off strategy choice in the G treatment of the $F = 20$ static game close to the time that the switch in treatments took place. This finding provides direct evidence of high (though not perfect) consistency between subjects' action choices and their use of threshold strategies; by contrast, Heinemann et al. (2004) merely asserted that such a relationship held in their study. Note further that the coefficient on the order dummy, $\delta^{G,C}$, is not significantly different from zero, suggesting that the two different treatment orders did not affect the consistency between subjects' elicited thresholds (G treatment) and their prior or subsequent action choices (C treatment). Finally, the regression also reveals that there is a statistically significant increase in the consistency metric in the $F = 50$ static game as compared with the $F = 20$ static game; mean consistency rises from 87% in the $F = 20$ static game to about 93% in the $F = 50$ static game.

Finding 11. The consistency between elicited cut-point strategies and action choices averages 87% in the $F = 20$ treatment and 93% in the $F = 50$ treatment. The consistency measure is unaffected by whether strategies were elicited in the first or second half of static game sessions, i.e., there is no order effect.

While these consistency levels may seem high, note that they are calculated for all values of Y in $[10, 90]$. We next ask whether our consistency metric is uniform across all Y values or whether it varies with certain values of Y.

Figs. 3(a)–3(b) plot weighted average values of the consistency measure from all sessions of the $F = 20$ and the $F = 50$ sessions grouped according to the 10 non-overlapping "bin" values for the actual Y-numbers subjects faced in the experiment.

Notice that for Y-values that lie either below F or above the hypothesized global game thresholds (42 when $F = 20$ and 62 when $F = 50$), the average consistency metric is generally very high, often close to 100%. However, there is a sharp fall-off in this consistency measure in the neighborhood of the empirically observed thresholds, bins 24–29, 30–35 and 36–41 in the $F = 20$ sessions and bins 48–53 and 54–59 in the $F = 50$ sessions. In the $F = 20$ case, when Y is between 30–35, subjects' strategies from the G-treatment part of the session predict their actions in the C treatment part of the session only 52% of the time. This finding leads us to conclude that there is only mixed support for an important implication of the global game solution – that individuals approach the play of a sequence of coordination games, which differ only in a payoff-relevant variable, by adopting a unique threshold strategy. This finding is not inconsistent with the finding that inferred or estimated entry thresholds in the $F = 20$ static-C and static-G game treatments are not significantly different from one another as the variance of entry thresholds is significantly greater in the G treatment as compared with the C treatment (see, e.g., Table 4). And indeed, we earlier reported (Finding 5) that both the mean entry threshold and its variance in the $F = 50$ static-G treatment were significantly higher than in the corresponding $F = 50$ static-C treatment. The observation that subjects are varying their cut-off thresholds to a greater degree than the variation in their entry choices is what appears to account for the inconsistencies observed in Figs. 3(a)–3(b). Summarizing, we have:

Finding 12. Individual's chosen cut-point strategies are a poor predictor of their actions when the payoff relevant state variable, Y, lies between F and the global game equilibrium threshold.

[9] We used data from 15 games of the C treatment so as to obtain sufficient variation in the Y values to assess the consistency in behavior between the C and G treatments.

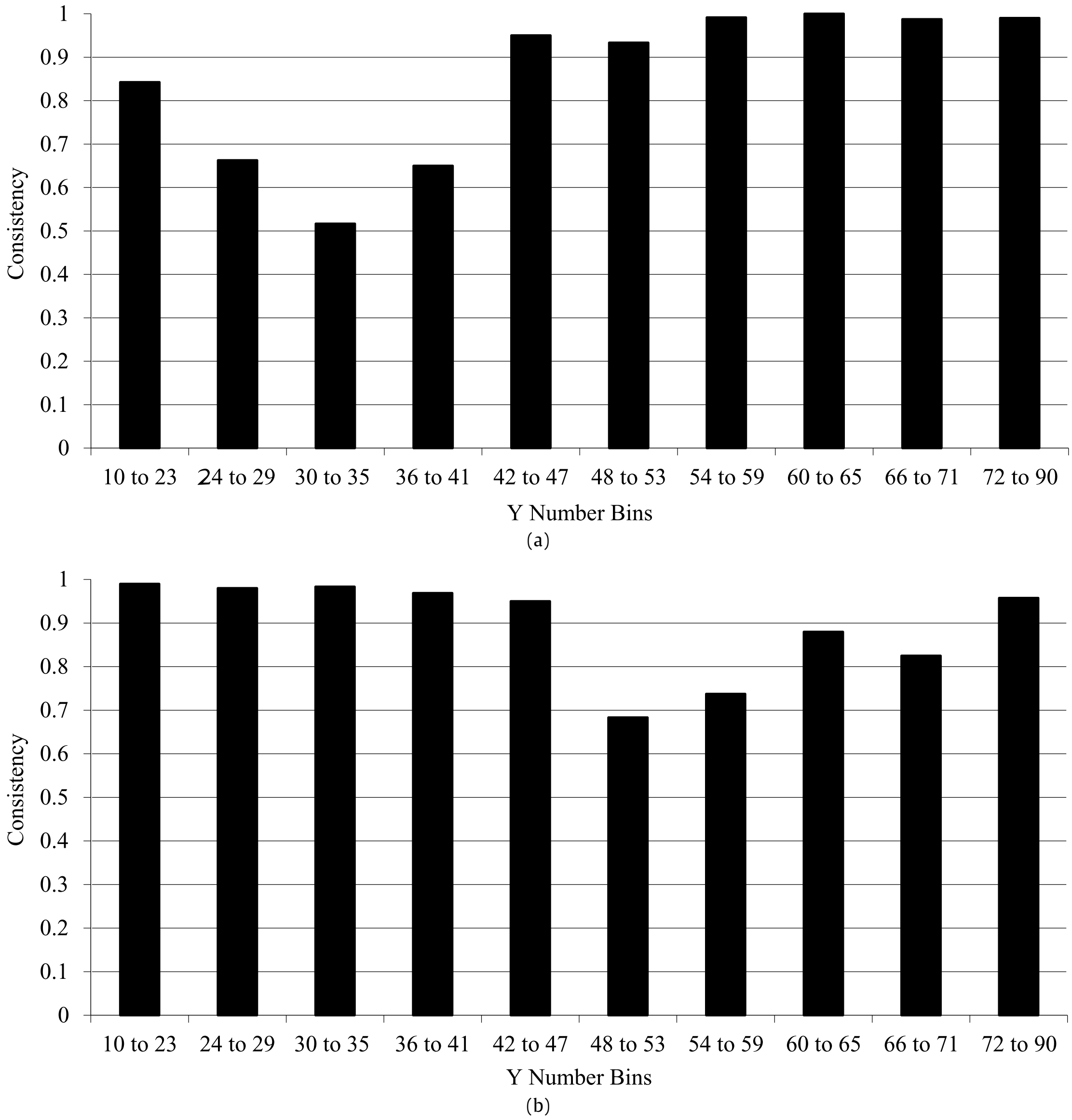


Fig. 3. Average accuracy of predicted entry decisions using elicited cut-points: pooled data (a) from six static game sessions where $F = 20$ and (b) from four static game sessions where $F = 50$.

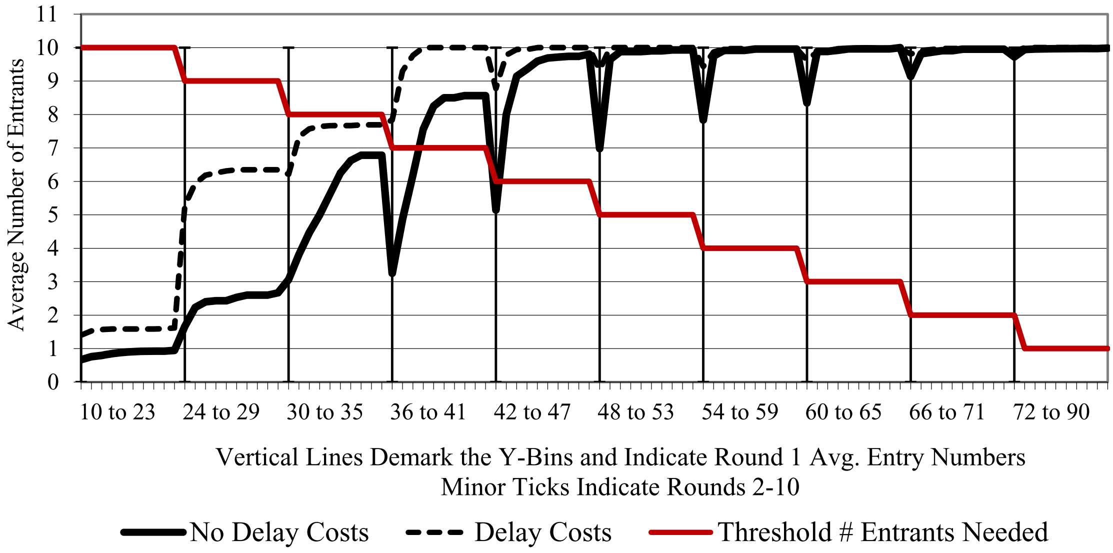
5.5. *Timing of entry decisions in the dynamic game*

Finally, we focus attention in this section on the *dynamic* game treatment and examine the timing of entry decisions. We have the following finding:

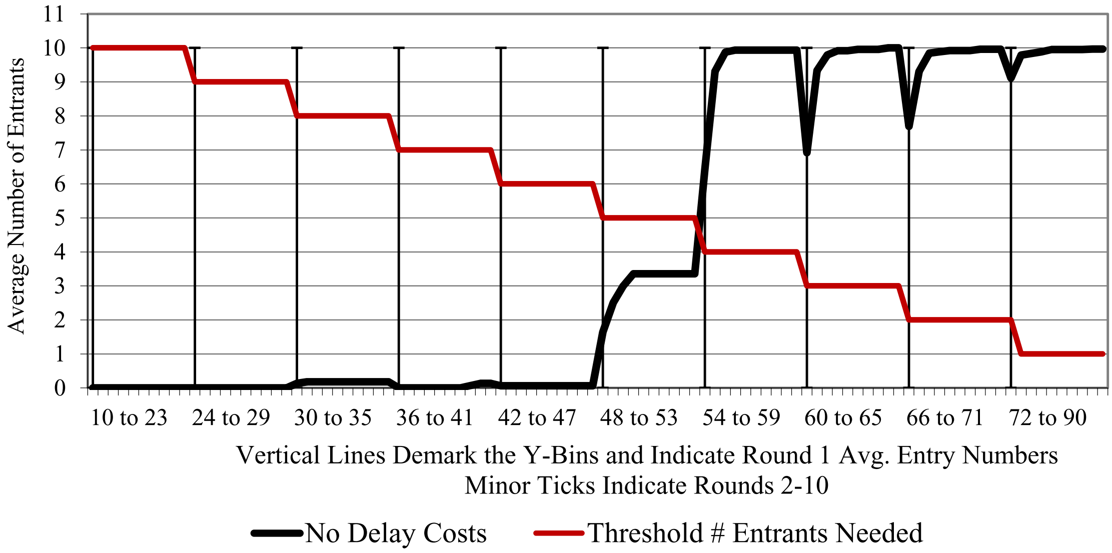
Finding 13. There is evidence against the efficient symmetric, subgame perfect equilibrium prediction of immediate entry for all values of $Y > F$, especially in the $F = 20$ dynamic game treatment.

For the dynamic game, the efficient, symmetric subgame perfect equilibrium calls for immediate entry in round 1 by all 10 individuals whenever $Y > F$. As noted earlier, this is also the unique symmetric subgame perfect equilibrium in the case where there are delay costs. However, as Figs. 4(a)–4(b) reveal, this prediction of immediate entry finds only conditional support, when Y is sufficiently greater than F. Figs. 4(a)–4(b) show the mean number of the 10 subjects who choose to enter at each of the 10 rounds of the dynamic game (indicated by tick marks within each Y-bin).[10] These time paths are constructed using pooled data from all sessions of the (1) $F = 20$ treatment with delay costs, (2) the $F = 20$ treatment without delay costs (both shown together in Fig. 4(a)) and (3) the $F = 50$ treatment without delay costs (shown in Fig. 4(b)).

[10] The 10th tick for each bin, representing the 10th round should not be connected to first tick of the next bin representing the 1st round; the connection is unavoidable using our graphical software.



(a)



(b)

Fig. 4. Average number of entrants in rounds 1–10 of all dynamic (a) $F = 20$ and (b) $F = 50$ game sessions without delay costs or with delay costs, grouped according to Y-bins. The threshold number of entrants needed for entry to yield a positive payoff is also indicated.

Figs. 4(a)–4(b) reveal that decisions to enter or not enter are, on average, immediate (occur in round 1) only when Y is below the value of F or well above the global game threshold of 42 in the $F = 20$ case and 62 in the $F = 50$ case. For intermediate values of Y, there is clear evidence that subjects are *conditioning* their entry decisions on the number of subjects who have previously entered, that is, some subjects are playing a "wait-and-see" strategy that is inconsistent with the subgame perfect equilibrium prediction. Consider for instance, the Y number bin 30–35 in Fig. 4(a) for the $F = 20$ dynamic game. When there are no delay costs, an average of just 3 of 10 players enter immediately in round 1 (marked by the vertical line in the figure). With delay costs a higher average, 6 of 10 players enter immediately in round 1. However, by the 10th and final round, an average of just 6.8 of 10 players have entered in the no delay cost treatment while an average of 7.7 of 10 players have entered in the delay cost treatment. The lack of immediate entry in round 1 by all players in this bin and other instances where $Y > F$ is clear evidence against the backward induction logic of the subgame perfect equilibrium prediction. Fig. 4(b) reveals that the incidence of "wait-and-see" before entering behavior is less pronounced in the dynamic $F = 50$ game, as it is less risky for individual subjects to immediately enter in the first round of such games when $Y > 50$; in such cases, *at most* 5 of the 10 group members must choose to enter for an entry decision to yield the larger, Y payoff. However, entry hesitation nevertheless persists in the $F = 50$ treatment. For example, consider the Y number bin 54–59 in Fig. 4(b): an average of just 6.4 of 10 players choose to enter in the first round of games with those Y values; in that bin, only 4 out of the 10 players must choose to enter for entry to succeed. While the average number of entrants in the 54–59 bin does climb to 9.9 of 10, this only by the 10th and final round of the game.

The pooled data from all sessions used to construct Figs. 4(a)–4(b) masks some important session-level heterogeneity. For instance, in Fig. 4(a) for the dynamic $F = 20$ treatment, it appears that entry is unsuccessful on average, for Y numbers

Table 6
Frequency of successful entry, dynamic $F = 20$ sessions.

Entry success frequencies, dynamic with no delay cost sessions

Bins	All sessions 7–12			Session 7		Session 8		Session 9		Session 10		Session 11		Session 12	
	1st	Subsq	Overall	1st	Subsq	1st	Subsq	1st	Subsq	1st	Subsq	1st	Subsq	1st	Subsq
10–23	0.00	0.00	0.000	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
24–29	0.25	0.00	0.067	0.00	0.00	0.50	0.00	nobs	nobs	nobs	nobs	0.00	0.00	0.50	0.00
30–35	0.75	0.55	0.625	0.00	0.50	1.00	1.00	1.00	nobs	1.00	0.50	0.50	0.00	1.00	0.75
36–41	0.90	0.75	0.833	1.00	1.00	1.00	nobs	nobs	nobs	1.00	0.50	0.50	0.50	1.00	1.00
42–47	0.92	1.00	0.742	0.50	1.00	1.00	nobs	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
48–53	1.00	1.00	1.000	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
54–59	1.00	1.00	1.000	1.00	nobs	1.00	1.00	1.00	nobs	1.00	1.00	1.00	1.00	1.00	1.00
60–65	1.00	1.00	1.000	1.00	1.00	1.00	nobs	1.00	1.00	1.00	nobs	1.00	1.00	1.00	nobs
66–71	1.00	1.00	1.000	1.00	1.00	1.00	1.00	1.00	nobs	1.00	1.00	1.00	1.00	1.00	1.00
72–90	1.00	1.00	1.00	1.00	1.00	1.00	1.00	0.00	1.00	1.00	1.00	1.00	1.00	1.000	1.00

Entry success frequencies, dynamic with delay cost sessions

Bins	All sessions 13–18			Session 13		Session 14		Session 15		Session 16		Session 17		Session 18	
	1st	Subsq	Overall	1st	Subsq	1st	Subsq	1st	Subsq	1st	Subsq	1st	Subsq	1st	Subsq
10–23	0.08	0.05	0.06	0.00	0.00	0.00	0.08	0.00	0.00	0.00	1.00	0.00	0.00	0.50	0.00
24–29	0.75	0.41	0.47	1.00	0.00	1.00	0.33	0.00	0.00	1.00	1.00	nobs	nobs	0.00	0.50
30–35	0.83	0.63	0.69	1.00	1.00	1.00	1.00	0.50	0.25	1.00	1.00	0.50	0.00	1.00	1.00
36–41	1.00	1.00	0.80	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
42–47	1.00	1.00	1.00	1.00	1.00	1.00	nobs	1.00	1.00	nobs	nobs	1.00	1.00	1.00	1.00
48–53	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	nobs	1.00	1.00
54–59	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
60–65	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00
66–71	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	nobs	nobs
72–90	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00	1.00

Notes: 1st = first round in which the Y value was in the given bin, Subsq = all subsequent rounds the Y value was in the given bin, Overall = overall success frequency for that Y bin, nobs = No observations available.

less than 36, as the average number of entrants by round 10 is less than hurdle number needed for entry success for Y values between 10 and 35. However, this inference is not correct. Table 6 reports the frequency of successful entry in the twelve $F = 20$ dynamic games (similar session-level heterogeneity obtains for the $F = 50$ dynamic game). Entry success means that at least the hurdle number of subjects in each group chose to enter (so entrants received positive payoffs), and these entry success frequencies are further disaggregated according to the 10 Y number bins and whether it was the first (1st) dynamic game for which a Y number was chosen in that particular bin versus all subsequent games ("Subsq") that the chosen Y number was in that same bin. For all 6 sessions of a treatment we also report the overall success rate for each Y number bin.

Table 6 makes it clear that there are session-level differences. For instance, in the dynamic with delay cost treatment, when the Y number lies between 30–35, we observe that in 4 of 6 sessions, entry is always successful, while in the other two sessions (15 and 17), entry is initially successful just half the time (the two groups of size 10 split in coordinating on successful entry) so that subsequently, entry in the 30–35 bin was not very successful in those two sessions. However, overall when the Y number was in the 30–35 bin, entry was successful 69 (62.5) percent of the time in dynamic $F = 20$ treatment with delay costs (without delay costs).

Finally, we address whether delay costs affected the *timing* of entry decisions in the dynamic game relative to dynamic game treatment without delay costs.

Finding 14. The addition of a small delay cost serves to hasten the round in which individuals choose to enter in the dynamic game.

Here we again focus on the six sessions of the $F = 20$ dynamic treatment with and without delay costs. Fig. 5 shows the mean period of entry among all those choosing entry by Y-number bins. We observe that for all bins, the mean period of entry is always greater for the dynamic game *without* delay costs than for the dynamic game *with* delay costs; t-tests confirm the impression from the non-overlapping standard error bars that such differences are statistically significant. However, the typical difference consists of an average of 1 period of delay or less. Further, while delay costs do hasten the period of entry in the dynamic game, we have already seen that such costs do not affect whether entry is successful or not or the aggregate estimated entry threshold, i.e., the Y^* number that characterizes play of the dynamic game. Finding 14 thus indicates that subjects were paying attention to delay costs and seeking to minimize them whenever their entry threshold was reached.

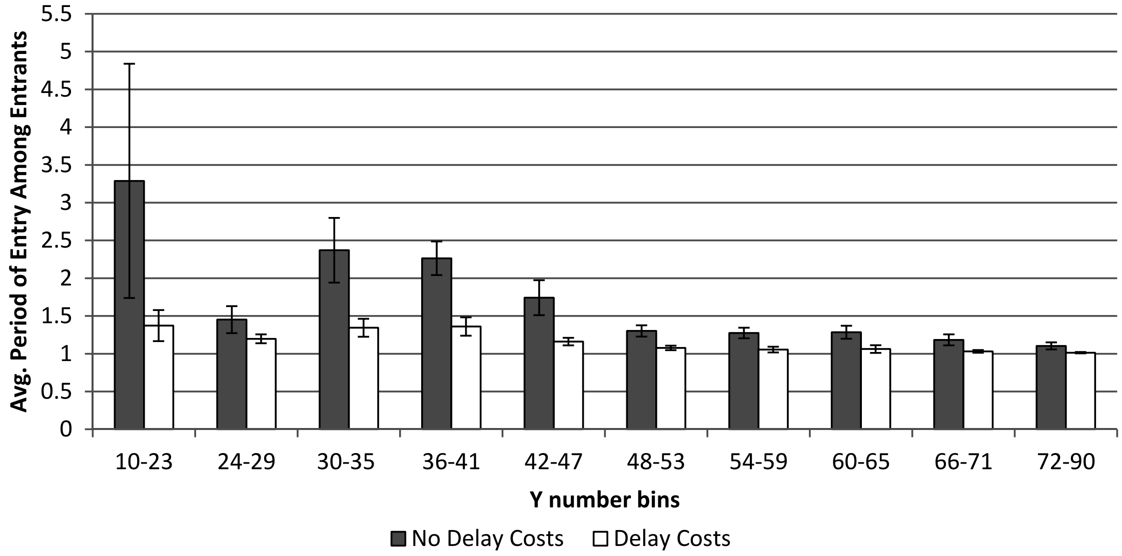


Fig. 5. Mean period of entry in all $F = 20$ dynamic game sessions without delay costs and in all $F = 20$ dynamic game sessions with delay costs.

6. Summary and concluding remarks

Games with strategic complementarities give rise to a multiplicity of equilibria. An important class of such games are binary choice, 'entry' games in which the payoff to a player who 'enters' is a monotonic increasing function of the number of other players who enter. These games have been used to model speculative attacks, bank runs, and other situations in which actions by a critical mass of players can produce a regime change. The theory of global games has been proposed as a model of how people resolve the coordination that is inherent in such games. That theory implies that an individual will play all entry games whose payoff functions differ only with respect to the value of a threshold parameter using the same cut-point strategy and that in equilibrium all individuals will choose the same strategy. We test these implications by directly eliciting the strategies subjects choose to play in all entry games that vary only with respect to the threshold parameter, Y. We then compare those strategies with the actual action choices the same subjects make in a series of entry games in which the value of Y varies from one game to another. We find only mixed support for the theoretical predictions. In support of the proposed refinement, we find that individual actions are generally consistent with the actions that are implied by the strategies they chose when asked to choose a cut-point strategy. Further, the lack of a treatment order effect suggests that individuals are employing a cut-point strategy when selecting which action to take in these entry games. However, inconsistent with the theory, there is substantial variance among individuals in their elicited cut-point strategies. Perhaps most importantly, elicited or estimated entry thresholds in the static game are significantly below the global game equilibrium prediction, while the mean estimated dynamic game entry threshold is above the subgame perfect equilibrium prediction in the $F = 20$ case, but it is not significantly different from it in the $F = 50$ case.

Most phenomena that are modeled as static entry games have an inherently dynamic property in that individuals do not have to move simultaneously and when they do act, they may possess information about how close the system is to the threshold that would make entry individually profitable. Theoretically, the equilibrium of the dynamic version of the entry game we study is quite different from the equilibrium of the global game. Surprisingly, we find that despite the large, predicted difference in the play of the static and dynamic games, the actual pattern of behavior in these different games is statistically indistinguishable, even when we consider the case of delay costs in the dynamic game or vary the payoff to non-entry, F. This finding suggests that the modeling of N-player entry games with strategic complementarities as static, one-shot games – ignoring the dynamic element of those interactions – may not be leaving out empirically important determinants of behavior observed in such environments. However, some caution is warranted as we may not have explored game parameterizations for which there may be significant differences in behavior between dynamic and static versions of the same entry game. We leave that exercise to future research.

Supplementary material

The online version of this article contains additional supplementary material.
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