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**Tom-Reiel Heggedal
BI Norwegian Business School**

**Leif Helland
BI Norwegian Business School**

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Platform Selection in the Lab ^{*}

Tom-Reiel Heggedal[†] Leif Helland[‡]

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Abstract

Emerging literature explores experimental platform selection games. These games converge rapidly on the superior platform under a wide range of conditions. We replicate the remarkable results of Hossain and Morgan (2009) in which such a game tips almost perfectly to the superior platform. Next, we show that platform coordination fails when seemingly innocent increases in out-of-equilibrium payoffs are introduced. The inflated payoffs keep the best reply structure unchanged and do not influence players' security levels. Our design allows control for the explanatory force of risk dominance. We find that equilibrium selection theory is unable to account for coordination failure while observed behavior is consistent with non-rational learning. Furthermore, and contrary to the literature, we find that efficiency suffers when an inferior platform is granted initial monopoly.

JEL: C72, C92, D43

Keywords: Equilibrium Selection, Laboratory Experiment, Network Effects, Platform Selection.

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[†]Department of Economics, BI Norwegian Business School, tom-reiel.heggedal@bi.no (corresponding author).

[‡]Department of Economics, BI Norwegian Business School, leif.helland@bi.no.

1. Introduction

Arguably, the most important unresolved question in economic theory is that of equilibrium selection. Despite the indeterminacy of theory, coordination failures are held responsible for substantial social costs in a wide range of important applications.¹ We focus on coordination failures in platform selection games.² These games are characterized by the presence of network effects and Pareto-ranked equilibria. Protracted investigation by case historians has not produced agreement about the severity of coordination problems in platform selection games.³

The emerging literature exploring experimental platform selection games (EPSGs) is, however, more conclusive (Hossain and Morgan 2009, 2011, Hossain et al 2011). Its main finding is that behavior converges rapidly on the superior (i.e. payoff dominant) equilibrium under a wide range of conditions.⁴ In particular, granting initial monopoly to an inferior platform does not affect subsequent coordination on a superior entrant.⁵ The one exception to the strong efficiency results in the literature is that conflicts between payoff- and risk dominance seem capable of pushing behavior away from the superior platform.⁶

We add to the EPSGs literature in several important ways. First, we show that systematic coordination failures can be generated in platform selection experiments, but that such failures are not caused by conflicts between payoff- and risk dominance. Second, we find strong incumbency (i.e. first-mover) effects at work. In particular, periods of inferior incumbency significantly reduce subsequent coordination on a superior entrant. Finally, we show that non-rational learning rules – not equilibrium selection theory – explain both coordination and coordination failures in platform selection. Specifically, inflated out-of-equilibrium payoffs tend to drive behavior away from the superior platform through payoff reinforcement learning. This happens regardless of the inflated out-of-equilibrium payoffs' impact on risk dominance.

Taken together our findings qualify the main message of the EPSG-literature; efficiency cannot be taken for granted in platform selection games, not even when payoff- and risk dominance are aligned. Inferior incumbency tends to generate inefficiency, and seemingly innocent payoff changes impact forcefully on coordination through behavioral rules.

In EPSGs, positive and negative network effects are at work. There are two types of players and two platforms. The more players of the opposite type and the fewer of own type that choose a platform, the more profitable that platform choice is. Platform users face a major challenge in choosing platforms since coordinating on the superior platform requires mutually consistent beliefs and actions. Such consistency cannot be taken for granted.

¹A non exhaustive list of applications where coordination failures are held culpable include: selection of Condorcet losers in elections (Myerson & Weber 1993, Forsythe et al. 1993); persistency of money illusions (Fehr & Tyran 2007); bank runs (Diamond & Dybvik 1983, Garratt & Keister 2009); currency attacks (Obstfeld 1996, Morris & Shin 1998, 1999); setting of industry standards (Farrell & Saloner 1985); emergence of fiat money (Kiotaki & Wright 1989, Cooper & John 1988); team production (Van Huyck et al. 2008, Brandts & Cooper 2006); and lack of economic development (Rodrick 1996).

²We take "coordination" to mean play consistent with the payoff dominant equilibrium. Coordination "failure", or "breakdown", is understood as the failure to play the payoff dominant equilibrium.

³The prime example is the claim that the QWERTY keyboard is inferior, but still prevalent due to path dependence (David 1985). The claim that QWERTY is inferior is strongly contested by, among others, Liebowitz & Margolis (1990, 1994). For a review of the QWERTY history and several other case histories see Farrell & Klempner (2007) and Gretz (2010).

⁴These include coexistence of tipping and non-tipping equilibria; as well as vertically and horizontally differentiated platforms.

⁵The main finding from EPSGs contrast with the frequent coordination failures commonly observed in the lab for other games in which equilibria are Pareto-ranked, notably order statistic games and stag hunt games (see van Huyck et al. 2008 and Devetag and Ortman 2007 for reviews).

⁶When criteria conflict, market shares of the superior platform hovers around 50 - 60 percent in Hossain et al. (2011:Figure 6, N-treatments), compared to rapid convergence to close to 100 percent in all other treatments of their study.

To fix ideas, consider an island fable (close to the EPSGs we consider). Two sellers and two buyers (two pairs of player "types") interact. Trading can take place on two different islands ("platforms"). One island is far away, the other one is close. Players decide simultaneously which island to trade on. Intuitively, if all players locate on the same island, this is an equilibrium. A deviant would be located on the other island alone and would get no trade. Since there are two islands, there are two such equilibria. Travelling to the far off island is more costly, so the equilibria are Pareto-ranked.

Despite their stylized nature, these games highlight strategic tensions that are present in real world markets. An illustration is the choice of "green" car technology, should I choose an electrical platform or a hydrogen based one? The network of complementary services (e.g., filling/charging stations, repair shops, and secondhand market) is important for consumers. Further, the extent of a network depends on the number of consumers using the technology. The flip side is that the profitability of investing in a platform depends on the expected size of the network. The risk of ending up with a small network may prevent the adoption of a technology, even if a widespread change to that technology is preferred.

An issue of particular relevance for the problems considered in this paper is that the level of both producer and consumer rents may depend on the technology of the platform. Consider a situation in which the fixed costs of a hydrogen filling station (vehicle) are higher than those of the electrical alternative. Now, holding all else equal, monopoly (monopsony) rents will be higher on the electric platform. Importantly, however, high rents are an out-of-equilibrium phenomenon in platform selection games.

In the remainder of the paper, we explore the robustness of EPSG efficiency. First, we present a design that permits replication, facilitates a controlled test of equilibrium selection theory, and allows for the exploration of incumbency advantages. Second, in the results section, we replicate the remarkable coordination result of Hossain and Morgan (2009) (hereafter HM). We then show that coordination is wiped out when out-of-equilibrium payoffs are manipulated in seemingly innocent ways. Subsequently, we demonstrate that incumbency effects are present and that non-rational learning rules, not the theory of equilibrium selection, explains our data. The paper ends with a brief conclusion.

2. Design

The centre piece of our design is a controlled inflation of out-of-equilibrium payoffs (i.e. provision of high rents) in each of the two original payoff matrices used by HM. This leaves us with two pairs of matrices, each pair consisting of an original and an inflated matrix. In each pair we inflate in a way that preserves the best reply structure and security levels of the original matrix. Thus, the inflated out-of-equilibrium payoffs should not lead to coordination breakdown according to standard theory.⁷ In the first pair of matrices (1/1*) the superior platform remains risk dominant after inflation. In the second pair of matrices (2/2*) the superior platform becomes risk dominated after inflation. According to the theory of equilibrium selection, coordination should not fail in the inflated matrix of the first pair, while it may fail in the second.

Our experiment consists of 6 sessions. A session consists of 3 consecutive sets, and each set consists of 15 periods. In the first 5 periods of a set, subjects are constrained to choose a monopoly platform ("the incumbent"). In the last 10 periods of a set, subjects are free to choose between the incumbent and an alternative platform ("the entrant"). Thus, a session has a total of 45 periods. Unique subjects were used in different sessions, and markets of 4 subjects were

⁷By standard theory we understand maximizing behavior from pure self-regard and common knowledge rationality. We make a distinction between standard theory and (the more general) theory of equilibrium selection.

randomly formed at the beginning of each set from a total of either 16 or 20 subjects. The probability of identical markets forming in different sets of a session was marginal.

At the start of a session, subjects were randomly assigned one of two types. Types were kept throughout the session. There were two pairs of opposite types in each market.

The matrices and the incumbents were varied systematically over the sets in a session. We utilized four matrices (Table 1). In the matrices, numbers provide the payoffs of player i from her own platform choice, conditioned on the platform choices of opposite and identical player types. Numbers are player i 's payoffs from choosing a platform (A , B). Access costs of platforms (A , B) are provided below each matrix. Matrices 1 and 2 are identical to the ones used in HM. Matrices 1* and 2* differ only by the inflated out-of-equilibrium payoff in square brackets. Matrices 2 and 2* are identical to the ones used in Hossain et al. (2011).⁸

Table 1: *Payoff matrices for player i*

Matrices 1 [1*]		# of players of same type choosing same platform as me			
		0		1	
# of players of	0	3	6	3	6
opposite type choosing	1	9	10	6	7
same platform as me	2	12	13[24]	11	12
My platform choice:		A	B	A	B
Access fees ($A=2;B=5$)					
Matrices 2 [2*]		# of players of same type choosing same platform as me			
		0		1	
# of players of	0	4	4	4	4
opposite type choosing	1	8	11	6	8
same platform as me	2	11[22]	13	10	12
My platform choice:		A	B	A	B
Access fees ($A=2;B=3$)					

In each of the four matrices in Table 1 there are three equilibria in pure strategies: i) all four players choose platform A , ii) all four players choose platform B , and iii) pairs of opposite player types choose opposite platforms. We refer to the equilibria in i) and ii) as "tipping," while the equilibrium in iii) is referred to as "non-tipping."

In matrices 1 and 1* platform A is the superior platform. In matrices 2 and 2* platform B is the superior platform. The tipping equilibria payoff dominates the non-tipping equilibrium in all matrices.

Harsanyi and Selten (1988) (hereafter HS) is the seminal contribution in equilibrium selection theory.^{9,10}The HS theory has a firm micro foundation (a set of axioms) for 2×2 games only. If

⁸Matrix 2* is the one underlying Hossain et al. (2011: Figure 6, N-treatments).

⁹A number of papers analyze selection in sequential games (see e.g., Ochs and Park 2010; Heal and Kunreuther 2010), where early movers may induce others to follow, inducing the market to tip to an equilibrium (which is said to be selected). The focus in this paper is selection in finitely repeated stage games, where stage-game actions are taken simultaneously.

¹⁰Equilibrium selection theories apply to stationary environments. In the experiment, the stage game is repeated a finite and known number of times in a partner matching. Given that a theory of equilibrium selection provides a clear recommendation for the one-shot game, finite repetition should not challenge this recommendation (by backward induction). Thus, it makes sense to apply stationary theories of equilibrium selection in the experiment.

an equilibrium is both payoff dominant and risk dominant, selection criteria do not conflict, and the equilibrium should be selected. In n -person games, equilibrium selection is a contested issue, and several competing definitions of risk dominance exist (see Carlsson and van Damme 1993 for a discussion).¹¹ For n -person games, HS advocate the use of a tracing procedure, assuming correlated beliefs.¹² In what follows we use the HS definition of risk dominance.

Using the HS tracing procedure with correlated beliefs we find that the superior platform risk dominates the inferior platform in matrices 1, 1*, and 2, while the inferior platform risk dominates the superior platform in matrix 2*. The non-tipping equilibria are always risk dominated. Hence, from equilibrium selection theory one should not expect coordination failure in matrices 1, 1* or 2, since there is no conflict between selection criteria. In matrix 2*, selection criteria are not aligned, and coordination may fail. If the pattern of actual behavior is to coordinate on the superior platform in matrices 1, 2, and 1* but not in matrix 2*, equilibrium selection theory provides a consistent story.

Note that there is also an equilibrium in mixed strategies in each of the four matrices. In this equilibrium, all players randomize with identical distributions over platforms A and B . In going from matrix 1 (2) to matrix 1* (2*), the probability weight on platform A (B) in the mixed-strategy equilibrium is increased. Thus, our inflated out-of-equilibrium payoffs make coordination on the superior platform more likely, *given* that the mixed-strategy equilibrium is being played.¹³ The mixed-strategy equilibrium is payoff dominated by all pure strategy equilibria in the original matrices (1 and 2). In the inflated matrices (1* and 2*), the two tipping equilibria continue to payoff dominate the mixed-strategy equilibrium. However, for one pair of opposite players (but not for the other pair) the mixed strategy equilibrium payoff-dominates the non-tipping equilibrium in the inflated matrices.¹⁴ This should, if anything, make play of the mixed-strategy equilibrium more attractive in the inflated matrices.

Finally, note that the security levels (i.e., maximin) are unaffected by the inflation. Therefore, payoff distances between security levels and equilibria are also kept constant. This payoff distance constitutes an alternative conception of the "riskiness" of an equilibrium; see Van Huyck et al. (1991).

To sum up, inflating the payoffs does not impact on pure strategy equilibria or security levels, and it changes the mixed strategy equilibrium in a direction indicating more efficient coordination *if* the mix is played. According to equilibrium selection theory, coordination should not break down in matrices 1, 1* and 2, while it may well break down in matrix 2*.

The six sessions of our experiment are described in Table 2. Sessions I, II, and III replicate HM, while sessions I*, II*, and III* extend HM.¹⁵

¹¹For example Güth (1992) develops theory that provides an extension of the HS theory to the n -person case, while Peski (2010) develops a concept of "generalized risk dominance".

¹²Combining correlated beliefs with the tracing procedure can lead to counter intuitive results (see Güth 2002 for examples).

¹³The probability of player i choosing (the superior) platform A is 0.374 in matrix 1, and 0.491 in matrix 1*. The probability of player i choosing (the inferior) platform A is 0.561 in matrix 2, and 0.458 in matrix 2*.

¹⁴The expected gain (net of access fees) from playing the mixed-strategy equilibrium for player i is 4.49 (5.31) in matrix 1 (1*), and 5.45 (6.15) in matrix 2 (2*).

¹⁵HM ran an additional treatment to check for ordering effects. We do not perform such checks since HM found no effects of ordering.

Table 2: *Sessions* (Matrix; Incumbent)

	Set 1	Set 2	Set 3	N	Date
Session I	(1; B)	(2; A)	(1; B)	16	9 November 2011
Session I*	(1*; B)	(2*; A)	(1*; B)	20	9 November 2011
Session II	(2; A)	(1; B)	(2; A)	16	24 January 2012
Session II*	(2*; A)	(1*; B)	(2*; A)	20	25 January 2012
Session III	(1; A)	(2; A)	(1; A)	16	10 November 2011
Session III*	(1*; A)	(2*; A)	(1*; A)	20	10 November 2011

As can be seen, in sessions I and I* the incumbent is always inferior; this is also the case in sessions II and II* (which simply run I and I* in reversed order), while in sessions III and III* the incumbent is always cheap.

To facilitate replication, the original z-tree program files¹⁶ and the original instructions from HM were used in sessions I, II, and III. Files and instructions were modified only: i) to account for the changed matrices in sessions I*, II*, and III*, and ii) to account for the new exchange rate of experimental points (from points to Norwegian kroner (NOK) rather than to Hong Kong dollars).

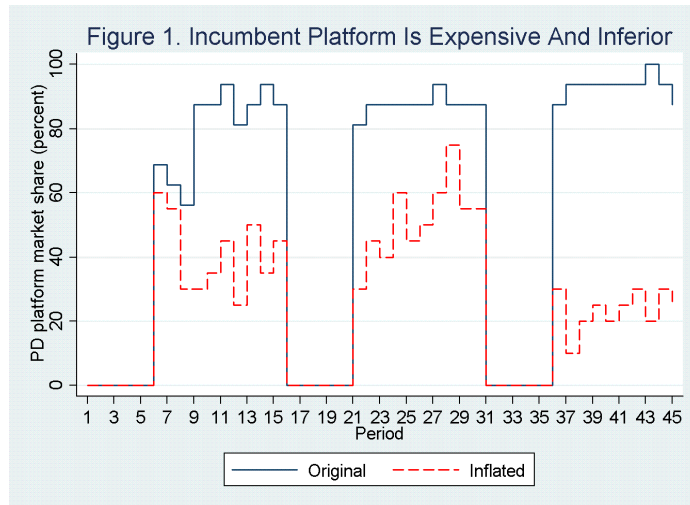
A total of 108 subjects were recruited by e-mail from the pool of BA students at BI Norwegian Business School. Subjects were used for a maximum of 90 minutes at expected earnings of approximately 200 NOK. Actual sessions lasted on average 75 minutes, with average earnings of 196 NOK (which is slightly above the hourly rate for research assistants at the institution). At the end of the experiment, points earned were converted at a rate of 0.6 NOK per point. Subjects were paid their earnings privately in NOK on exit.

All sessions were executed in the research lab of BI Norwegian Business School. On arrival, subjects drew a ticket with a number corresponding to their cubicle in the lab (in order to break up social groups). Once seated, instructions were read aloud to achieve public knowledge about the rules of the game, payoffs, and exchange rates. Subsequently, the session was conducted with a strict no-communication rule enforced. All interactions were performed through the PC network and anonymity was preserved throughout the experiment.

3. Results

Figure 1 shows results from three sets based on matrices $1/1^*$, in which the incumbent is expensive and inferior (sets 1 and 3: sessions I/I*; set 2: sessions II/II*). The y -axis measures the average percentage of subjects choosing the superior platform (i.e., the market share of the superior platform). The solid line shows behavior with original payoffs (from HM), while the stapled line shows behavior with inflated payoffs.

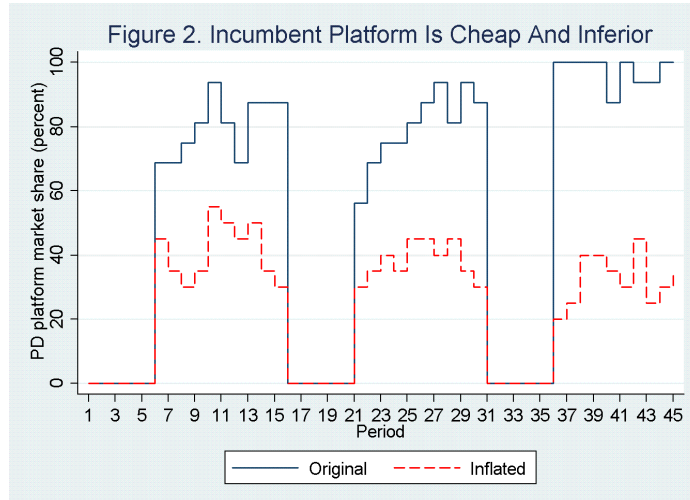
¹⁶Fischbacher (2007).



The solid line in Figure 1 tightly tracks the result in HM.¹⁷ After some initial problems, markets coordinate on the superior platform. The coordination is not perfect, but after period 8, the market share of the superior platform stays above 80 % (excluding the monopoly phases where the inferior platform was the only choice). However, the result changes dramatically when the inflated payoff is introduced. The dotted line shows that the markets fail to coordinate. In fact, the market share of the superior platform is less than 30 % for the last 10 periods. This is certainly surprising, since in this case no clear mechanism exist by which the inflated out-of-equilibrium payoffs should influence equilibrium selection (for both inflated and original matrices the superior platform is risk dominant).

Figure 2 shows results from three sets based on matrices $2/2^*$, in which the incumbent is cheap and inferior (sets 1 and 3: sessions II/II*; set 2: sessions III/III*). As before, the market share of the superior platform is measured on the y -axis. Again, the solid line shows behavior with original payoffs, while the stapled line shows it with inflated payoffs.

¹⁷See HM Figure 1.



Once more, the solid line in Figure 2 reproduces the result in HM.¹⁸ Coordination on the superior platform breaks down once the inflated payoff is introduced. In this case the superior platform is not risk dominant, and this could in principle explain breakdown.

Taken together, Figures 1 and 2 show that coordination is fragile. A change in an out-of-equilibrium payoff that is inconsequential according to standard theory destroys market coordination, and coordination breaks down whether the change impacts risk dominance or not.¹⁹ Also, the incumbent platform may retain a substantial market share even if the entrant platform is superior.

Table 3 displays average market share for the superior platform per market in set 3 ("share"), and displays the average share of markets that were coordinated on the superior platform in set 3 ("coord"). The monopoly phase (periods 31–35) was excluded prior to calculating the averages in Table 3. Data are broken down on sessions and on original versus inflated payoffs. We use these data to perform Mann-Whitney U-tests (two-sample ranksum tests). Markets are selected as observational units since strategic interaction takes place within, but presumably (given our matching protocol and information partition) not across markets. Thus, market-level data should be independent. The final set is selected based on the assumption that behavior has had time to settle down.

¹⁸See HM Figure 2.

¹⁹Güth's definition returns the same results as the HS procedure for matrices 1, 1*. For matrices 2 and 2* the superior and inferior equilibria are risk neutral. With Peski's definition, the superior platform risk dominates in the original matrices, while this method is silent with respect to risk dominance for the inflated matrices. In sum neither Güth's nor Peski's definitions account better for observed behavior than the HS theory.

Table 3: *Set 3, per market data*

	Session I		Session II		Session III	
Market	share	coord	share	coord	share	coord
1	0.975	.900	1.000	1.000	.975	.900
2	0.825	.300	1.000	1.000	1.000	1.000
3	0.975	.900	0.900	0.700	1.000	1.000
4	0.950	.800	1.000	1.000	1.000	1.000

	Session I*		Session II*		Session III*	
Market	share	coord	share	coord	share	coord
1	0.400	0.000	0.100	0.000	0.775	0.300
2	0.175	0.000	0.100	0.000	0.525	0.200
3	0.150	0.000	0.700	0.200	0.400	0.000
4	0.400	0.000	0.150	0.000	0.525	0.200
5	0.050	0.000	0.575	0.000	0.325	0.000

First, we test the differences in the market shares of the superior platform between the original and the inflated treatments. The distributions in the two treatments differ significantly in all sessions (Session I vs I* $p = 0.014$; Session II vs II* $p = 0.012$; Session III vs III* $p = 0.012$). So far, we have discussed market shares of the superior platform as a measure of coordination. Alternatively we may look at the average share of markets that were coordinated on the superior platform. From Table 3, we see that coordination on the superior platform rarely occurs in the inflated treatments. In fact, there were no instances of such coordination at all in Session I*.

Second, we test whether costs of the platforms matter for the market share of the superior platform. We test the differences of distributions in Sessions I vs II (incumbent is inferior and expensive vs inferior and cheap). We find no significant differences in these tests (Session I vs II $p = 0.137$). Hence, our data support the same result on costs as HM's data: costs of the platforms do not seem to matter for coordination.

Third, we test whether equilibrium selection theory can account for variation in the degree of coordination failure. Comparing differences in distributions in Sessions I* vs II* and II* vs III* (aligned vs non-aligned selection criteria) we find no significant differences (Session I* vs II* $p=0.833$; Session II* vs III* $p=0.344$), indicating that selection theory does *not* account for variation in coordination failure.

Fourth, we investigate whether an initial monopoly for the inferior platform matters for the market share of the superior platform. We test the differences in distributions in Session I vs III and I* vs III* (superior vs inferior incumbent). In both cases, there are significant differences (Session I vs III $p = 0.034$; Session I* vs III* $p = 0.044$). Since the incumbent platform is different in Sessions I and III across both the cost and the Pareto dimensions, we cannot directly determine what drives these differences. However, above we indicated that costs do not seem to matter. Thus, initial monopoly seems to impact on coordination.

In sum: coordination breaks down when out-of-equilibrium payoffs are inflated. Standard theory does not seem to explain this breakdown, nor does the theory of equilibrium selection. What about non-rational learning theories that take into account the history dependence of play envisaged in our figures?²⁰ To explore this possibility, we test for the effect of fictitious play

²⁰The observed history dependence of play makes non-rational stationary theories – such as Quantal Response Equilibrium (McKelvey and Palfrey 1995) and other stationary concepts (such as those investigated in Selten and Chmura 2008) – less suited for our purposes. The constancy over time problem in both labeling symmetries (Schelling 1960), and payoff asymmetries (Crawford et al. 2008; Goeree and Holt 2001) in our games, also makes coordination by focal points insufficient for explaining our data.

(Brown 1951; Fudenberg and Levine 1998) and of simple payoff reinforcement learning (Roth and Erev 1995; Erev and Roth 1998) on the probability of making Pareto optimal choices.²¹

Before we run regressions trying to explain the dynamics of play, we address variations in initial behavior. From Figures 1 and 2, we see that for matrices 1, 1*, and 2 the initial market share of the superior platform is 60-65%, while it is only 45% for matrix 2*. A similar gap is present in the full data set underlying the regressions presented below.²² We approach this gap in the usual way, by formulating a level- k model (see for instance Crawford et al. 2012).²³

A level-0 player flips a coin to determine which platform to choose. If a player believes her opponents to be level-0 players, her best response is to choose the superior platform in matrices 1, 1*, and 2, while the inferior platform is her best response in matrix 2*.²⁴ Accordingly, the best reply for a level-1 player to level-0 opponents is to choose the superior platform, unless she is playing matrix 2*, for which her best reply is the inferior platform. It is easily seen that for all $k \geq 2$, a level- k player's optimal response to opponents with level $k - 1$ is identical to her optimal response in the $k = 1$ case.

We conclude that the initial choices are consistent with a simple level- k model: the initial market share of the superior platform is well above 50% in matrices 1, 1*, and 2, while it is well below 50% in matrix 2*. We now address the dynamics of play.

The logistical regressions in Table 4 include only periods where subjects had a choice (i.e., the first 5 periods in each set have been deleted, and periods are re-numbered from 1 to 30). Our dependent variable is a dummy coded 1 if the subject chose the superior platform, and 0 otherwise. We regress this dummy on six explanatory variables. The variable reinforcement lies in the interval $(0, 1)$, and maps a subject's probability of choosing the superior platform from that subject's historical success when making this choice.²⁵ By construction, this choice probability increases (decreases) at a diminishing rate with past payoff success in choosing the superior (inferior) platform.²⁶ The fictitious play variable is a dummy coded 1 (0) if choosing the superior (inferior) platform in a given period maximizes individual expected payoffs for that period, given beliefs corresponding to the historical frequency of choices made by the three other players in the group. Our regressions include a dummy for lagged choice of the superior platform (to capture habitual behavior); a trend variable (periods); as well as a dummy taking the value 1 if matrix 2/2* is used and 0 otherwise, and lastly a dummy taking the value 1 for inferior incumbent platform and 0 otherwise.

We run a split sample analysis for original matrices (1 and 2) and inflated matrices (1* and 2*) in order to capture the interaction between out-of-equilibrium payoffs and other explanatory variables. The regressions in the first two columns of Table 4 have fixed effects for groups, while the two last regressions are without fixed group effects. The fixed effects estimator has the best *a priori* justification, since the dynamics modeled (reinforcement and fictitious play) should be expected to vary systematically over groups within a set. However, the specification without

²¹We work with the pure forms of learning (belief and reinforcement) rather than with the (heavily) parameterized version of Camerer and Ho (1999), which is also able to produce in-between forms of learning. In simple, normal form experiments, reinforcement learning seems able to explain most of the behavioral dynamics.

²²First-period mean market share of the superior platform in the full data set is 0.75 ($N = 32$) for matrix 1; 0.55 ($N = 40$) for matrix 1*; 0.69 ($N = 20$) for matrix 2; and 0.45 ($N = 20$) for matrix 2*.

²³Hossain and Morgan (2011) point to some interesting relationships, in platform competition games, between level- k thinking and the risk dominance concept of the HS theory.

²⁴These choices follow directly from the mixed-strategy equilibrium described in note 13.

²⁵Payoffs are reinforced by their (net) payoff deviations relative to the (net) minimum payoff of the relevant matrix, which happens to be 1 for all matrices.

²⁶We use equal initial weights on the two choices (Pareto optimal platform and non-Pareto optimal platform), making platform choices random in the first period. The "strength of attraction" is set to the numeric value 10 (see Roth and Erev 1995; Erev and Roth 1998 for details). The choice probability is fairly insensitive to the numerical value of the strength of attraction.

group fixed effects allows for incumbent features that are constant within, but not across, groups. Skipping fixed group effects therefore allows us to make use of data for the ten groups in which subjects chose the superior platform in all periods of a session.

In calculating the (history-dependent) reinforcement and fictitious play variables we assume that subject learning in one matrix (1 and 1*, respectively) is fully relevant for behavior in the other matrix (2 and 2*, respectively).²⁷ To check for the soundness of this assumption in data, we ran specifications including dummies for sets (not reported). Main results are unaffected by such controls, indicating that robust cross-matrix learning is taking place.²⁸

Table 4: *Logistical regressions. Dependent: Choice of optimal platform (standard errors).*

	Original [†]		Inflated [†]		Original [‡]		Inflated [‡]	
Reinforcement	8.79	***	1.83	***	8.47	***	1.78	***
	(1.48)		(0.49)		(1.04)		(0.43)	
Fictitious play			-0.31	**			0.13	
			(0.15)				(0.13)	
Superior choice _{t-1}	0.32		0.80	***	1.09	***	1.17	***
	(0.26)		(0.12)		(0.25)		(0.11)	
Period	0.12	**	0.03		-0.01		0.00	
	(0.04)		(0.02)		(0.02)		(0.01)	
Matrix 2/2*					0.03		-0.19	
					(0.22)		(0.12)	
Inferior incumbent					-0.61	*	-0.45	***
					(0.32)		(0.14)	
Constant					-3.73	***	-1.48	***
					(0.64)		(0.29)	
# Groups	36		45		36		45	
# Groups used	26		45		36		45	
# Observations	1440		1800		1440		1800	
# Observations used	996		1740		1392		1740	
Log Likelihood	-241.08	***	-887.78	***	-335.56	***	-1052.61	***

Legend: [†]Fixed group effects; [‡]No fixed effects; Significant at: *** 1% level; ** 5% level; * 10% level

Note first that fictitious play is a constant with original matrices. In this case, the fictitious dummy favors choosing the superior platform in the initial period. Choice of the superior platform is also frequent enough to ensure that the dummy keeps the value 1 in all subsequent periods, and for all subjects. In the treatment with inflated matrices, the fictitious play dummy varies over time and subjects. Fictitious play has a marginal effect on the choice of platform, is contrary to expectations in the fixed-effects specification, and insignificant without fixed effects. There is a positive effect of habit on platform choice for inflated matrices. For original matrices this effect is not robust to econometric specification. There is no robust time trend in the data after control for learning dynamics. As in the aggregate analysis, there is no significant effect attributable to equilibrium selection theory. That is, the effect of the dummy for matrix 2/2* is insignificant

²⁷Experimental evidence indicates that learning takes place even over fairly dissimilar games (see Cooper and Kagel 2008, and the general discussion in Samuelson 2005).

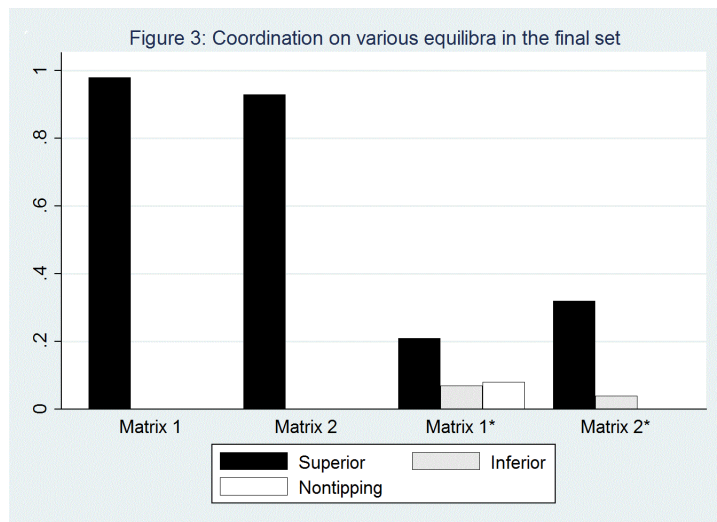
²⁸We also ran a series of other specifications, including linear probability models, with and without fixed and random group effects, and logistical regressions with random group effects. Our results are remarkably robust to these alternative specifications.

for both original matrices (aligned vs aligned selection criteria) and inflated matrices (aligned vs non-aligned selection criteria).

Choosing the superior platform is less likely if the incumbent platform is inferior. This finding is fairly robust even when regressions are restricted to the last few periods of the experiment, indicating persistency in the incumbency effect.²⁹ However, the incumbency effect is less certain for original matrices. Returning to the estimates in Table 4, with other independent variables at their mean, moving from a superior to an inferior incumbent reduces the probability of choosing the superior platform by 11 percentage points in the inflated matrices. The corresponding number for original matrices is a 9-percentage-point reduction.

The effect of reinforcement is highly robust over specifications, and reinforcement of the superior platform choice is stronger in the original than in the inflated matrices. The effect of reinforcement on coordination is substantial. Keeping other independent variables at their mean, while letting reinforcement go from 0 to 1, increases the probability of choosing the superior platform by 96 percentage points for the original matrices, and by 41 percentage points for the inflated matrices. In sum: simple payoff reinforcement of the superior platform choice leads to coordination with original matrices and to coordination failure with inflated matrices.

Lastly, we ask to what extent behavior is consistent with *some* equilibrium for the different matrices. Again, we focus attention on the last set. Figure 3 displays the average fraction of markets playing each of the three pure strategy equilibria, conditioned on matrices.



As seen above, markets facing original matrices coordinate almost perfectly on the superior platform (on average 93 - 98 percent). When these markets fail to coordinate, they are invariably out of equilibrium. In markets facing inflated matrices, play is rarely consistent with any of the pure strategy equilibria. When play is in equilibrium, it usually means coordination on the superior platform. The two inferior equilibria are rarely, if ever, played. In sum: when markets fail to coordinate on the superior platform, they fail to be in equilibrium at all.^{30,31}

²⁹Running the regressions in columns two and three of Table 4, but restricting observations to the last 10 periods in set 3, or the last 5 periods in sets 1 and 3, does not change results qualitatively. A joint test with inflated and original matrices in the same regression, and restricting observations to the last 5 periods of set 3, returns a significant coefficient on inferior incumbency. Results are available from authors upon request.

³⁰One may wonder if out-of-equilibrium behavior stabilizes in the inflated matrices. This, however, does not seem to be the case. With inflated matrices the frequency of subjects changing decisions from one period to the next is 27 percent in the final set, compared to only 4 percent with original matrices.

³¹The coordination failures we observe are not "QWERTY" outcomes; behavior does not stabilize on an inferior

4. Conclusion

An emerging literature finds remarkable efficiency in EPSGs under a broad set of conditions. In particular, inferior incumbency does not reduce the market share of the superior challenger, and coordination does not fail if the superior platform is also risk dominant.

We question the generality of these findings. For some payoff matrices EPSGs coordinate superbly, for other matrices coordination fails miserably. Equilibrium selection theory does not account for observed failures. When coordination fails, it fails regardless of whether the superior platform is risk dominant or not. In addition, incumbency effects are present regardless of whether markets coordinate or fail. Thus, path dependence impacts significantly on market efficiency.

We show that observed behavior is consistent with initial level- k reasoning and subsequent payoff reinforcement learning. Interpreting inflated payoffs as high rents our results suggest that EPSGs coordinate well in the absence of high rents, and lousily in their presence. This holds despite of high rents being an out-of-equilibrium payoff in these games.

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