When and Why? A Critical Survey on

Coordination Failure in the Laboratory[1]

Giovanna Devetag and Andreas Ortmann

Correspondence:
Giovanna Devetag
Department of Law and Management, University of Perugia
Via Pascoli, 20, 06123 Perugia, Italy
tel.: (39) 075 5855271, fax: (39) 075 5855257
e-mail: devetag@unipg.it, mgiovanna.devetag@libero.it

Andreas Ortmann
CERGE-EI, Charles University and Czech Academy of Sciences
P.O.BOX 882, Politickych veznu 7, 111 21 Prague, Czech Republic
tel.: (420) 242 30 117, fax: (420) 242 11 374, 242 27 143
e-mail: aortmann@cerge-ei.cz, aortmann@yahoo.com

Abstract

Coordination games with Pareto-ranked equilibria have attracted major theoretical attention over the past two decades. Two early path-breaking sets of experimental studies were widely interpreted as suggesting that coordination failure is a common phenomenon in the laboratory. We identify the major determinants that seem to affect the incidence, and/or emergence, of coordination failure in the lab and review critically the existing experimental studies on coordination games with Pareto-ranked equilibria since that early evidence emerged. We conclude that there are many ways to engineer coordination successes.

1. INTRODUCTION

> "Several basic conclusions have emerged from this research: Coordination failure is common …" (Camerer, 2003, p. 403)

Coordination games with Pareto-ranked equilibria, or "payoff-asymmetric" coordination games (Camerer, 2003, section 7.4), have attracted major theoretical attention over the past two decades (e.g., Bryant, 1983; Cooper and John, 1988; Carlsson and Van Damme, 1993; Cooper, 1999; Frankel, Morris and Pauzner, 2003). Two path-breaking and frequently cited early sets of experimental studies (namely, Van Huyck, Battalio and Beil [from here on VHBB], 1990, 1991, and Cooper, DeJong, Forsythe and Ross [from here on CDFR], 1990, 1992) have been interpreted as suggesting that coordination failure is a common phenomenon in the laboratory. Coordination failure describes either of these events: Failure to coordinate on any one of the multiple equilibria (sometimes called "disequilibrium outcome"), or failure to coordinate on the payoff dominant equilibrium. The latter meaning has been used by VHBB (1990, 1991) who pointed out that this meaning was the convention that was developing then in the literature on macroeconomic coordination games. We follow that convention below.

The claim that coordination failure might be a common phenomenon prompted a steady flow of robustness tests. In this article we review critically order-statistic games like those in VHBB (1990) and VHBB (1991) and stag-hunt games like those in CDFR (1992). We are well aware that these labels are somewhat misleading, as both are coordination games with Pareto-ranked equilibria, and stag-hunt games[2] can also be discussed as a special kind of order-statistic games (e.g., Camerer, Ho and Chong, 2004, p. 870). Mainly for historic reasons – namely, the two sets of experimental studies that initiated the experimental literature on coordination games with Pareto-ranked equilibria,

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[2] Stag-hunt game have motivated the global games literature (e.g., Carlsson and Van Damme, 1993; Morris and Shin, 2003; Heinemann, Nagel and Ockenfels, 2004).

and the rather different experimental paradigms used to implement them – do we stick to these labels.

Our research strategy consists in a qualitative review[3] of the available evidence that is informed by an attempt to classify the major classes of structural, cognitive, and behavioral determinants that seem to affect coordination failure in the lab[4]. Because of its self-evident importance for coordination games outside of the laboratory, we also consider briefly the issue of the external validity of the currently available set of laboratory coordination game studies.

The remainder of the manuscript is organized as follows. In section 2 we review order-statistic games and stag-hunt games by way of some classic examples. In section 3 we review critically laboratory evidence of coordination failures and successes, paying particular attention to the reasons for particular outcomes. In section 4 we summarize what the evidence has taught us about how to engineer coordination successes. Section 5 concludes.

2. THE CLASSES OF GAMES UNDER CONSIDERATION

Order-statistic games. The payoff function of a generic order-statistic game can be represented as follows:

(1) $\Pi_i = f(OS - |e_i - OS|)$

where OS stands for the order statistic (which could be the median or the minimum – the weak link --, or something else), e_i denotes player i's choice of an ordered set of numbers which is meant to represent an ordered set of efforts[5], $|e_i - OS|$ denotes the (symmetric)

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[3]A meta-study (e.g., Croson and Marks, 2000, or Zelmer, 2003, for public good experiments) that quantitatively evaluates the impact of various factors on coordination (failure) is not (yet) possible since design and implementation details have not reached the volume that would make such an endeavor possible. A major part of the problem is that few authors in the literature under consideration have followed the advice of Davis and Holt (1993, p. 520) not to change too many things at once.

[4] Space constraints forced significant selection on us. We decided, for example, to focus on published and forthcoming studies. The present article is a heavily distilled version of Devetag and Ortmann (2006).

[5] This widely used terminology reflects the frequent interpretation of order-statistic games as problems of joint production. Strictly speaking, the same effort is exerted in selecting any of the action choices in an order-statistic experiment. Likewise, this terminology is poorly reflecting interpretations of order-statistic games as models of macro-economic activities. As a referee put it: "Keynes and macroeconomists think coordination failures arise from failures of price adjustment mechanisms (e.g. lots of people are unemployed/homes unsold because wages/home prices are sticky downward) - mechanisms that are not really present in any of these effort-based, game-theoretic interpretations."

deviation cost, and f is some scalar function of these terms. Obviously, the terms can be arbitrarily modified by setting the coefficients of the two terms on the RHS not equal to 1, or by squaring the second term, or by defining the deviation costs asymmetrically, etc.

VHBB (1990, 1991) used the following earnings tables for their two path-breaking studies.[6]

Earnings table for the "Median game" (Table Γ in VHBB, 1991)

		Median value of X chosen						
		7	6	5	4	3	2	1
Your choice of X	7	1.30	1.15	0.90	0.55	0.10	-0.45	-1.10
	6	1.25	1.20	1.05	0.8	0.45	0.00	-0.55
	5	1.10	1.15	1.10	0.95	0.70	0.35	-0.10
	4	0.85	1.00	1.05	1.00	0.85	0.60	0.25
	3	0.50	0.75	0.90	0.95	0.90	0.75	0.50
	2	0.05	0.40	0.65	0.80	0.85	0.80	0.65
	1	-0.5	-0.05	0.3	0.55	0.70	0.75	0.70

Earnings table for the "Minimum game" (Table A in VHBB, 1990)

		Smallest value of X chosen						
		7	6	5	4	3	2	1
Your choice of X	7	1.30	1.10	0.90	0.70	0.50	0.30	0.10
	6	-	1.20	1.00	0.80	0.60	0.40	0.20
	5	-	-	1.10	0.90	0.70	0.50	0.30
	4	-	-	-	1.00	0.80	0.60	0.40
	3	-	-	-	-	0.90	0.70	0.50
	2	-	-	-	-	-	0.80	0.60
	1	-	-	-	-	-	-	0.70

Note that the payoff-dominant, or efficient equilibrium is in the upper left corner for both the Minimum game and the Median game while the secure action induces an equilibrium (the secure equilibrium from here on) in the lower right corner for the Minimum game and two rows up from the bottom in the Median game. Both games feature seven (identical) Pareto-ranked pure-strategy equilibria on the main diagonal. There is a tension between the secure action – the lowest action in the Minimum game, and the third lowest in the Median game - and the action required for the efficient equilibrium.

Importantly, the payoffs in the triangular area above the main-diagonal are not the same: For the Minimum game deviation costs are linear and feature positive payoffs only, whereas for the Median game they are highly non-linear, leading to negative payoffs in

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[6]The results of these studies are among the most celebrated in the literature on coordination failure (e.g., Ochs, 1995; Camerer, 2003; or scholar.google).

the upper right corner and lower left corner. This nonlinearity (and the negative payoffs that it induces) counteracts, and possibly neutralizes, the more forgiving Median statistic. The different types and strengths of the deviation costs confound the comparison of effects of the order statistic and any comparison of the results of Median game and Minimum game experiments that does not account for it. In fact, the labeling of the games is unfortunate because it distracts from the consequences of the different parameterizations.[7]

Stag-hunt games. This class of games, like order-statistic games, feature (typically two) pure-strategy equilibria that are Pareto-ranked. Payoffs result from the strategic interaction of two players with two action choices each. CDFR (1992) contained the paradigmatic example of this class of games, $sg(1,x,y,z) = 1,000g(1,0,0.8,0.8)$, where g is normalized to 1, s is a scalar function here taking on the value 1000, $x<z$, $y<1$, and $x,y,z \, \varepsilon \, [0,1)$:

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                     Other player's choice
                     2        1
Your    2         1,000       0
Choice  1            800    800
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Like the order-statistic games discussed earlier, the payoff-dominant equilibrium is in the upper left corner while the secure equilibrium is in the lower right corner: There is thus a tension between the risky action (required for the efficient equilibrium) and the secure action. It is an unattractive feature of the concept of security that it will always select Choice 1 and therefore, quite possibly, select secure but unattractive equilibria. A more persuasive solution concept is risk dominance.[8] For certain values of y and z, efficient and risk-dominant equilibrium might coincide (in the upper left corner). Essentially, this is the case when the secure action choice is not attractive enough.

Classes of determinants of coordination outcomes. Prominent structural determinants of coordination failure are the specific forms the payoff matrix takes (namely such characteristics as the *attractiveness of the secure, or maximin, strategy and the riskiness of the other action choices)*, which are partially defined by the type and strength of deviation costs (i.e., the penalty incurred by a player who does not best respond to other players' choices), as well as the *coordination requirements determined by the order*

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[7] This confound was to some extent addressed in several later studies (e.g., Cachon and Camerer, 1996; Van Huyck, Battalio, and Rankin, 2001; Goeree and Holt, 2005).
[8] A risk-dominant equilibrium has a greater Nash product of deviation losses relative to the other equilibrium (e.g., Harsanyi and Selten, 1988). For example, the product of the deviation losses attached to the Pareto-inferior pure-strategy equilibrium is $(800-0)(800-0) = 640,000 > (1,000-800)(1,000-800) = 40,000$. Note that as the secure strategy becomes less attractive, risk-dominant and efficient equilibrium might coincide.

statistic[9], the *group size*, and the opportunities for *shared experience, interaction, and informational feedback*. These structural factors may be usefully labeled exogenous risk characteristics because they are fully under the control of the experimenter.

Cognitive and behavioral determinants are those not fully under the control of the experimenter. By cognitive determinants we mean issues such as how subjects understand the payoff matrices that they are given, or the effects that potentially *negative payoffs* might have on subjects. In light of the well-documented sensitivity of outcomes to initial conditions, to be discussed below, in some games (e.g., the "Median" games in VHBB, 1991) but not others (e.g., the "Minimum" games in VHBB, 1990), these issues seem of obvious importance as they add to the exogenous risk characteristics endogenous ones that VHBB (1990, 1991) called "strategic uncertainty": One's own behavior is a function of the structural characteristics of the environment – including the opportunities for shared experience, interaction, and informational feedback provided by the experimenter -- but also of one's individual characteristics (e.g., one's own beliefs about the cognition and risk attitudes of the other players, other players' beliefs, cognition, and behavior, and higher-order beliefs).

We now turn our attention to what we know <u>empirically</u> about the structural, cognitive, and behavioral determinants.

3. LABORATORY EVIDENCE OF COORDINATION FAILURES AND SUCCESSES

3.1. *Attractiveness of the secure strategy and riskiness of the other action(s)*

Order statistic games. Was efficiency psychologically salient in VHBB (1990, 1991) or were competing concepts such as security, or risk dominance, more salient?

The key result of VHBB (1990) is the stable and speedy unraveling of action choices to the worst of the seven Pareto-ranked strict equilibria. Between 14 – 16 participants played the stage game repeatedly (10 times in treatment A, and 5 times in treatment A'), receiving only information about the minimum (and hence, by inference, their own payoffs) after each stage. The outcome was essentially the same even after payoff efficient precedents emerged in a treatment (B) that was inserted between treatments A and A' for four out of six sessions. Several other experimenters – in baseline treatments for various modifications reported in those papers -- replicated this unraveling result with the same payoff matrix but the number of subjects varying from 6 – 14 (e.g., Cachon and Camerer, 1996; Bornstein, Gneezy and Nagel, 2002; Blume and Ortmann, 2007; Chaudhuri, Schotter and Sopher, 2005). Other experimenters – also in baseline treatments for various modifications reported in those papers -- chose structurally similar payoff matrices (e.g., linear deviation costs, no negative payoffs) with slightly more or less action choices (e.g., Berninghaus and Ehrhart, 1998; Knez and Camerer, 1994; Weber,

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[9] Some of these determinants do not apply to both classes of games under consideration. For example, since stag-hunt games are a special kind of order-statistic game (minimum), the coordination requirement issue is moot for them.

Camerer, Rottenstreich and Knez, 2001; Brandts and Cooper, 2004, 2005, 2005a) and also replicated this result.

The key result of VHBB (1991) is the influential role that the initial action choices played. For the baseline treatment neither the unique payoff dominant equilibrium nor the unique secure equilibrium emerged when 9 participants played the stage game repeatedly (10 times) receiving only information about the median after each stage. Furthermore, the initial median constituted a strong precedent from which subjects had trouble extracting themselves. Blume and Ortmann (2007), in their baseline treatments, replicated this result by using the same payoff matrix, the same number of subjects, and the same feedback conditions. So did Cachon and Camerer (1996). So did VHBB (1993).

Because of their remarkable results on coordination failure, VHBB (1990, 1991) drew considerable attention and a steady flow of attempts to test their robustness.[10]

Every choice between a secure and a (set of) riskier actions is ultimately a function of (the perception of) expected values of the available choices. The higher the expected value of the secure action (relative to the riskier action(s)) the more likely it is to undermine the risky actions, and vice versa. (Of course, the expected value is also a function of the order statistic, group size, etc.)

Brandts and Cooper (2005a) address this issue head-on. Studying coordination in a minimum effort game with five effort levels, and keeping the payoff associated with the minimum constant, they vary the payoff associated with the efficient equilibrium (an idea already explored in Knez and Camerer, 1994), and observe higher incidence of coordination success as the efficient equilibrium becomes significantly more attractive.

Other authors have explored the robustness of coordination by manipulating experimentally the type and strengths of the deviation costs. Keeping the action space roughly comparable to that in the classic VHBB (1990) study, Berninghaus and Ehrhart (1998) introduced longer time horizons (scaling down the per-round payoffs accordingly), so as to lower the opportunity cost of exploration. They showed that number of rounds had the hypothesized effect although they did not bring about complete convergence to the Pareto-efficient outcome, and although there was no difference in the distribution of initial choices, as one might expect.

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[10] VHBB (1990, 1991) themselves conducted a number of important robustness tests. Among their key insights are the importance of the number of participants, the matching protocol, the feedback conditions, and the deviation cost. In VHBB (1990), for example, the authors demonstrated (in the already mentioned treatment B) that setting the coefficient on the deviation cost equal to zero lead to quick convergence to efficiency. They also demonstrated that two participants when matched repeatedly and with the same person (but not with randomly drawn others), were able to coordinate on the efficient outcome.

Van Huyck, Battalio and Rankin (2001) explore the consequences of a finer action grid (as well as the impact of the order statistic and the number of players). Letting their subjects choose among 101 actions (and letting them run through twice the numbers of rounds), Van Huyck et al. (2001) find that local exploration is "skewed in the direction of efficiency" (p. 14). It is possible, and likely – in light of the observed perfect correlation between "creeping up" and time in some of the treatments and the results by Berninghaus and Ehrhart (1998) --, that this result is due to both the refined action space as well as the increased number of rounds.[11] The refined action space, in conjunction with the somewhat larger number of rounds, may also have been responsible for a similar drift toward efficiency in Van Huyck, Cook and Battalio (1997). Goeree and Holt (2005), studying systematically the effects of what we call deviation costs – they call it "effort costs" -- , also use a refined action space taking as point of departure the payoff function used in VHBB (1990). In experiments for two-player minimum and both three-player minimum and median effort games with random matching, they document significantly higher frequency of coordination on the payoff-dominant equilibrium for lower deviation costs. The very refined action space they used complicates comparison to other experiments.

Stag-hunt games. Both CDFR (1990) and CDFR (1992) were concerned with stag-hunt games of the sg(1,x,y,z) variety where x < y = z. CDFR (1990), however, embedded the stag-hunt games (in games 3 – 8 the 2 x 2 principal minor was the same across all games) into a larger 3 x 3 matrix that featured – apart from two Pareto-ranked equilibria of the embedded stag-hunt games (the "augmented stag-hunt game") – a cooperative (Pareto-dominant in games 3 – 6 but not games 7 - 8) outcome that was induced by a dominated strategy. The key question was whether the Pareto-dominant equilibrium would always be selected. The answer to this question was not in the affirmative. By and far, dominated strategies that could induce Pareto-dominant equilibria were not selected.

Following up on related work published in CDFR (1989)[12], CDFR (1992) also explored whether the results in CDFR (1990) were robust to the use of both one-way and two-way communication, for both the augmented stag-hunt game and the particular parameterization of the stag-hunt game, 1,000g(1,0,0.8,0.8), discussed earlier. Coordination failure turned out to be endemic in the no-communication baseline conditions (and still significant with one-way communication); coordination failure was eliminated by two-way communication between players. We return to the issue of communication below.

It is important to mention that these coordination failure results came about under a matching protocol that differed sharply from the one used by VHBB (1990, 1991) and other multi-player studies afterwards. Specifically, while VHBB and others nearly always

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[11] We can not tell for sure because only order statistic and number of players were systematically varied but not action space and number of rounds.

[12] In that paper, CDFR investigated the role of communication in the battle of the sexes game and concluded that one- way communication helped coordination while two-way communication hurt as subjects had no way to sort out contradictory signals.

used multi-player, finitely repeated coordination games, CDFR (1989, 1990, 1992) used two-player sequences of one-shot games typically resulting from a random matching or rotation matching (Kamecke, 1997) protocol. As already shown in VHBB (1990), this choice of interaction pattern can make an efficiency-reducing difference. (More on this in section 3.3. in our discussion of Clark and Sefton, 2001; see also Schmidt et al., 2003, especially pp. 295 - 297.)

In the following years, several authors followed up on the CDFR results. Overall, it is interesting to note, and very likely a consequence of the predominant matching protocol, that many authors working in this area focused on the structure of the payoff matrix (e.g., Battalio, Samuelson and Van Huyck [BSVH], 2001, and Clark, Kay and Sefton [CKS], 2001)[13] rather than implementation details that had shown to be of importance in order statistic games.

Schmidt et al. (2003), in an article closely related to BSVH (2001), systematically vary measures of payoff-dominance and risk-dominance (using for the latter a formulation proposed in Selten 1995) and find – both for random matching and fixed matching protocols -- that players react to changes in risk-dominance but not payoff-dominance. This result is at odds with both the results in BSVH (2001) and CKS (2001). Importantly, and in contradiction to the message the title of their paper suggests, subjects selected "the payoff dominant strategy more often than not." (Schmidt et al. 2003, p. 298), with this statement applying to almost all treatments (the random repeated match and fixed repeated match protocols, and the one-shot random matching protocol, for all games the four games employed); see specifically Table 3 on p. 296 in that article.

Arguably the most intriguing article in this area – because its results seem dramatically at odds with claims that coordination failure is common -- is Rankin, Van Huyck and Battalio [RVHB] (2000). The authors use a scaled-up version of g(1,0,x,x) where x is, for each round, drawn randomly from the unit-interval and then, ever so slightly, perturbed. Taking the cue from Kreps's argument (1990, pp. 169 - 174) that experience with precisely the same game in precisely the same situation is hardly a way to instill trust in the generalizability of laboratory results, RVHB had their subjects play a sequence of 75

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[13] BSVH (2001) used a random matching protocol (allowing for repeated interaction) to explore through between-subject design three variants of the stag hunt game that differed in the optimization premia, R, reflected in the ratio of the payoffs of the risk-dominant equilibrium (40:20:12 = 2R:R:.6R). As hypothesized, BSVH find the premium affects systematically the responsiveness of beliefs and behavior (which converges quicker the larger is the optimization premium), and also the initial choices. CKS (2001) use two versions of the stag-hunt game, g(1,0,0.8, 0.8) and g(1,0,0.9,0.7), both scaled up by s = 1,000. The first one replicates the CDFR (1992) design and is also similar to treatment 2R in BSVH, the second is similar to treatments R and .6R in BSVH albeit for another reason (the Aumann conjecture). In the absence of pre-play communication, CKS find no difference in outcomes between these two versions of the stag-hunt game when they match subjects randomly for ten rounds. This result seems to contradict the result in BSVH (2001).

such games, in addition scrambling the action labels so that the payoff dominant equilibrium and the secure equilibrium would not show up in the same cell throughout the 75 rounds. The intriguing result of this experiment was the high percentage of efficient play both when x <0.5 (making the secure strategy less attractive and making payoff dominant and risk dominant equilibrium coincide) and, even more intriguing, when x>0.5 (making the secure strategy more attractive and positioning the payoff dominant and the risk dominant equilibrium at opposite ends of the main diagonal). Specifically, for the first 10 periods 65% (85%) of choices corresponded to the efficient action when x>0.5 (x<0.5). For the last 10 periods, about 90% (almost 100%) of the choices corresponded to the efficient action when x>0.5 (x<0.5). Thus, payoff dominance clearly carried the day. RVHB point out that their set-up inhibits learning from experience and focuses subjects on the exploration of deductive principles. In addition, in about half of the rounds subjects faced a situation in which payoff-dominance and risk-dominance selected the same equilibrium. It probably also helped that subjects were told in the instructions that "you will remain grouped with the same seven other participants for the next 75 rounds." This formulation is likely to have translated in most subjects' minds into, "I'm going to see each of the seven other participants about 10+ times", a trust-building insight of sorts.

3.2. *Coordination requirement: order statistic and group size*

The coordination requirement in order statistic games is related both to the particular order statistic used to calculate payoffs and to the group size. The intuition suggests that, all other things being held constant, in the minimum effort game it is riskier to pick the efficient action in large groups than in small groups.

Van Huyck, Battalio and Rankin (2001) directly tested the claim that order statistic and group size are substitutes by experimentally crossing two group sizes (5 and 7) and two order statistics (2 and 4) in a 2 x 2 design that also featured a dramatically increased action space (101 actions) and a relatively large number of periods. The authors carefully analyze initial, adaptive, and terminal behavior. Among the many interesting results – contradicting the Berninghaus and Ehrhart (1998) results about the initial values – is the finding that "some of the behavior predicted to emerge in the session has already been incorporated into initial behavior" (p. 9). Specifically, the variation in order statistic and group size influenced behavior in the first round, with subjects reacting more strongly to differences in the order statistic than in group size (see Table 2, p. 8).

3.3. *Shared experience, interaction, and other informational issues*

A precedent results from shared experience (Lewis, 1969) and creates expectations on the part of the participants about what happens next. Precedents are created when players interact repeatedly with the same players, as in VHBB (1991), or the two-player fixed matching treatments of VHBB (1990). Shared experience can also be induced, ex ante, via precedents established in other contexts. The possibility of observing the actions of other players, or the possibility to inform other players of one's intentions through costly

or costless pre-play communication is among the other informational issues that affect the outcomes of coordination games.

Order statistic games. VHBB label precedents from other games "weak precedents" to distinguish them from the "strong precedent" established in a previous round of the same game (e.g., VHBB, 1990, 1991; Knez and Camerer, 2000; Weber, 2005; Devetag, 2005; or Brandts and Cooper, 2005a). This terminology is not always descriptive. Weber (2005), building on Knez and Camerer (2000, experiment 2), has demonstrated that -- if trust is built slowly and new participants are made aware of the group's history – efficient precedents can spill over from n-person weak-link experiments to (n+1)-person weak-link experiments (but see also Knez and Camerer, 2000, experiment 1, who demonstrate precedents can indeed be fragile expectational assets.)

The effect of information has been studied in a number of experiments. Somewhat surprisingly perhaps, the effects of providing subjects with post-play information about the distribution of choices do not allow a conclusion yet (although the number of participants in these experiments suggests tentatively that the number of participants interacts in a predictable manner with post-play information about the distribution of players): the results in Berninghaus and Ehrhart (2001) and Brandts and Cooper (2005) suggest post-play information is efficiency-enhancing ; the results in Devetag (2005) and the full information treatment in VHBB (1990).suggest no effects.

Other studies investigate the role of pre-play communication which can be costly or costless ("cheap talk"). VHBB (1993) -- by auctioning off the right to play – used costly (but tacit) information to overcome coordination failure completely. Turning from costly to costless messages, Blume and Ortmann (2007), using the key earnings tables from VHBB (1990, 1991) to facilitate comparison, test the effect of cheap talk both in the Minimum and Median game. They find that costless messages with minimal information content, when added to games with Pareto-ranked equilibria, can facilitate both quick convergence to, and participants' initial coordination on, the Pareto-dominant equilibrium. Cheap talk is thus a substitute for other efficiency- enhancing design and implementation characteristics such as a more forgiving order statistic, smaller group size, or step size, or a refined actions space. See also Burton and Sefton (2004) for similar results in a closely related class of games. Chaudhuri, Schotter and Sopher (2005), using the key earnings table from VHBB (1990), find that cheap talk of sorts[14] is efficiency-enhancing in intergenerational minimum effort game experiments. Importantly, their results suggest that the quality of advice given is positively related to the probability of coordination success.[15]

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[14] Advice – even though it consists of free-from messages – is costly in that payoffs are a function of both one's own earnings as well as that of one's successor. So, while there is no "out-of-pocket" costs involved in giving advice, one's advice does matter in the same sense as the cheap talk in Blume & Ortmann (2007) might matter.

[15] In Blume and Ortmann (2007), costless minimal information content pre-play messages take the specific form of "I intend to play action … ". Chaudhuri, Schotter and Sopher (2005) and also Brandts and Cooper (2005a) present a radical departure from this

Stag-hunt games. While a number of papers have studied the effect of changes in the payoff matrix (e.g., Friedman, 1996; Straub, 1995; BSVH, 2001; CKS, 2001; Schmidt et al., 2003; RVHB, 2000), relatively few authors have studied the effect of the kind of design and implementation details that we have documented in our discussion of order statistic games. As our discussion of RVHB (2000) indicates, this state of affairs seems deplorable because these issues may be more important than structural characteristics of the payoff matrix. Shared experience is surely one such issue.

Interestingly, the impact of pre-play communication in terms of costly signals has not been studied in the context of stag-hunt games. Aumann (1990) conjectured that the impact of costless communication, or cheap talk, would significantly depend on the structure of the payoff matrix. Specifically, in g(1,0,0.9,0.7) messages expressing the intent to shoot for the payoff-dominant equilibrium would not be credible because it is in a player's interest to entice the other player to do so. In contrast, in g(1, 0,0.8,0.8) such an expression would not be self-serving. CKS (2001) provide evidence in support of this conjecture when comparing no communication and two-way communication. Charness (2000) also provides evidence in favor of the Aumann conjecture.

Concentrating on a set-up not afflicted by such issues of credibility, Duffy and Feltovich (2002, 2005) study the impact of words and deeds and lies on behavior in prominent strategic situations, including the stag-hunt game. If cheap talk is credible (i.e., not undermined by the kind of parameterizations that motivated the Aumann conjecture), then words indeed speak louder than deeds. While subjects are quite honest to start with, the possibility of being caught lying improves the already high coordination even more.

Relatedly, and in an interesting twist on Van Huyck, Gillette and Battalio (1992), Bangun et al. (2006) study the effects of external assignments. The former authors had found significant effects of external assignments, but they found them in three-action scenarios with Pareto-ranked equilibria that did not have the tension between payoff-dominant and risk-dominant outcomes. Bangun et al., (2006) took Game 2 of Rydval and Ortmann (2005), g(8,1,5,5), and found – in contrast to the results of Chaudhuri, Schotter and Sopher (2005) – that recommendations by the experimenter to play the risky strategy induce the efficient equilibrium under both "common knowledge" and "almost common knowledge".

Among the few papers that have explored implementation issues in the stag-hunt scenarios, Clark and Sefton (2001) investigate the role of interaction structure. Their experiment involves the play of a stag-hunt game either as a sequence of one-shot games implying a random matching protocol, or as a repeated game with a fixed matching protocol (recall VHBB 1990 for a similar exercise). The latter may influence behavior in a variety of ways, the most obvious of which is the possibility to use precedent. However, an additional, more subtle way in which a fixed matching protocol may alter behavior is

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template allowing far-ranging communication that they analyzed ex post for content. The evidence in the latter papers suggests that the content of communication matters.

through the possibility of costly signaling that it offers players. This type of signaling is costly insofar as it implies the possibility of having zero payoff rounds initially. In order to distinguish between the two phenomena, Clark and Sefton investigate first round behavior, in which only the impact of signaling should be observed. Their data show that, indeed, in the first round of play the frequencies of choice of the risky action were 0.3 in the random matching and 0.6 in the fixed matching protocol, a highly significant difference. Moreover, the fixed matching protocol reduced the instances of disequilibrium outcomes and increased the overall proportion of risky choices across rounds.

3.4. *Negative payoffs*

Order-statistic games. Although an affine transformation of payoffs does not change the structure of equilibria in a coordination game, there is some evidence (albeit by no means undisputed, see e.g., List, 2004; Plott and Zeiler, 2005) that framing outcomes as gains or losses is not neutral with respect to behavior. Drawing on VHBB (1991, 1993), Cachon and Camerer (1996) investigate loss avoidance as a selection principle: if people follow loss avoidance, they should avoid playing strategies that result in certain losses if strategies leading to potential gains are available. They find that loss avoidance functions as a selection principle in the median as well as the minimum effort game, inducing coordination on the Pareto-dominant equilibrium. Here, too, no studies exist (yet) that investigate the role of negative payoffs in a systematic way, though it would seem to be called for given the likelihood that the initial choices in the classic Median and Minimum game (e.g., VHBB, 1990, 1991; Cachon and Camerer, 1996; Blume and Ortmann, 2007) were at least partially affected by the differential presence of negative payoffs. A reasonable conjecture would be that the prominent negative payoffs in the upper right, and lower left, corner of the Median game earnings table of VHBB (1991) did affect people's choices, and were responsible for the clustering of initial choices slightly above the secure action.

Stag-hunt games. Rydval and Ortmann (2005), and Feltovich, Iwasaki and Oda (2005) tested experimentally the Cachon-Camerer conjecture that loss avoidance might also work its magic in stag-hunt games. Both their results seem to suggest that loss avoidance may indeed be a (weak) selection principle in stag-hunt games, especially if losses are certain for a chosen action.

4. DISCUSSION

What we have learned since VHBB (1990, 1991) and CDFR (1990, 1992)[16]:
- Lower attractiveness of the secure action relative to the risky action required for the efficient equilibrium is efficiency-enhancing (e.g., Brandts and Cooper, 2004).
- Low (zero) deviation costs are efficiency enhancing (e.g., VHBB, 1990; Goeree and Holt, 2005; BSVH, 2001).

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[16] All statements below are ceteris paribus.

- Lower costs of experimentation such as increasing the number of rounds while keeping the overall earnings roughly the same, or refining the actions space, or some combination thereof, are efficiency-enhancing (e.g., Berninghaus and Ehrhart, 1998; Van Huyck et al., 2001).

- Less stringent coordination requirements (i.e., a smaller group size or a less stringent order statistic) are efficiency-enhancing (e.g., VHBB, 1990; Van Huyck et al., 2001).

- Fixed matching protocols are efficiency enhancing (e.g., VHBB, 1990; Clark and Sefton, 2001; Schmidt et al., 2003).

- Repeated encounters are efficiency enhancing even under random matching schemes if the experimental design and implementation focuses subjects on deductive principles (e.g., Rankin, Van Huyck, and Battalio, 2000; see also Schmidt et al., 2003).

- Providing full informational feedback seems efficiency enhancing in "small" groups (e.g., Berninghaus and Ehrhart, 2001; Brandts and Cooper, 2005; but see VHBB 1990 and Devetag, 2005).

- The possibility of observation of action choices, especially if paired with previous expressions of intent, is efficiency-enhancing (Duffy and Feltovich, 2002, 2005).

- Slowly growing groups that have managed to establish efficient precedents, is efficiency enhancing (Weber, 2005).

- Costly pre-play communication is efficiency-enhancing (e.g. VHBB, 1993).

- Costless pre-play communication is efficiency-enhancing (e.g., CDFR, 1992; Van Huyck, Gillette, and Battalio, 1992; Blume and Ortmann, 2005; Duffy and Feltovich, 2002, 2005; Bangun, Chaudhuri, Prak and Zhou, 2006).

- More meaningful communication, and common knowledge of information, are efficiency- enhancing (Chaudhuri, Schotter and Sopher 2005; see also Bangun, Chaudhuri, Prak and Zhou, 2006.)

- Loss avoidance may be efficiency-enhancing if losses are certain for a chosen action (e.g., Cachon and Camerer, 1996; Rydval and Ortmann, 2005; Feltovich, Iwasaki and Oda, 2005).

5. CONCLUSION

> "The ultimate goal of the laboratory honing of simple models is to explain behavior in the economy." (Camerer, Ho and Chong 2004, p. 892)

We have qualitatively reviewed the evidence on coordination failure in the laboratory While two initial sets of experiments (VHBB, 1990, 1991; CDFR, 1990, 1992) seemed to suggest that coordination failure is almost inevitable, the sum total of subsequent attempts to understand the robustness of these results suggests myriad ways to engineer coordination successes in the lab.

Much of what we know about the incidence, and/or emergence, of coordination successes (and failures) in the lab seems related to what we have called structural determinants. We know surprisingly little about the impact of cognitive and behavioral determinants in order-statistic and stag-hunt games. Since coordination is ultimately about trust (and

since the literature on trust seems to mainly be concerned with behavioral and cognitive issues) the current state of affairs of research on coordination failures seems odd. Even elementary behavioral determinants such as the effects of risk attitudes have hardly been studied directly (see Heinemann et al., 2004a, for an important and intriguing exception) although their potential impact has been indirectly acknowledged by some researchers analyzing stag-hunt games (e.g., the laudable but problematic early attempts by CDFR 1990, 1992 to control for risk preferences through the Roth – Malouf procedure in the stag hunt game) and demonstrated by a recent study by Holt and Laury (2002; see also Harrison et al., 2005; Holt and Laury, 2005). Surprisingly, the impact of group composition along dimensions such as cultural homogeneity also remains a blind spot (see Dufwenberg and Gneezy, 2005, Cooper 2006, and Engelmann and Normann 2007, for isolated exceptions).

Moreover, cognitive determinants (e.g., how subjects interpret and represent - or maybe (mis)represent – the payoff matrix) need to be investigated.[17] There is tantalizing evidence (e.g., Cowan, 2001; Devetag and Warglien, 2005; Wilcox, 1993) that the complexity of the matrix, and for that matter the task itself, systematically affects people's choice of strategies and heuristics. We conjecture that, for example, the difference in results between Bangun et al. (2006) and Chaudhuri et al. (2005) is likely to reflect the complexities of the tasks involved. Somewhat surprisingly, there exist up to now no studies that use easily available tools such as MouseLab that have been used successfully in other contexts (e.g., Johnson et al., 2002) to study information acquisition and choice patterns. Also in need of investigation is the impact of precedent formation and transfer and the effects of both the quantity and quality of information.

Clearly, the question of how wide spread and pervasive coordination failure is in the lab, and in the wild, can hardly be answered conclusively by summarizing the extant experimental literature the way we have done. Ideally, one would start with an identification of a widely agreed-upon set of key determinants that would reflect the essential determinants in real-world situations of which order-statistic and stag-hunt games are claimed to be models of. Such a set of key determinants should span an agreed-upon parameter space which likewise ought to be informed by real-world settings

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[17] The classic studies of VHBB (1990, 1991) used 7 action choices. Most later studies followed that pattern, at least approximately (e.g., Weber et al., 2001). An important exception is Van Huyck, Battalio and Rankin (2001) who give subjects 101 action choices (and hence a 101x101 earnings table) in an attempt to reduce the costs of experimentation. It seems a reasonable conjecture that a 2 x 2 earnings table (as used in typical stag-hunt games discussed below) or a 4 x 4 matrix with simple integer entries (as in Weber et al., 2001) is easier to understand than a 7 x 7 or 101 x 101 matrix. Realizing the problem, Van Huyck, Battalio, and Rankin (2001) address the issue by comparing percentages of initial choices in their earlier experiments but this comparison is confounded by the use of a new technology.

(something which is a standard practice in macro economics but a practice essentially non-existent in micro-economics[18].)

Notwithstanding frequent appeals to real-world problems (e.g., Knez and Camerer, 1994, Camerer and Knez, 1996; Knez and Simester, 2001; Weber et al., 2001; Weber, 2005; Brandts and Cooper, 2004, 2005, 2005a; Chaudhuri, Schotter, and Sopher, 2005; Cooper, 2006), the coordination literature has not been much concerned with external validity, and definitely not with issues of calibration. Of course, not every experiment has to be calibrated. Much can be learned from experiments such as Rankin, Van Huyck and Battalio (2000) because they ask fundamental questions about what it is that we test in the laboratory.

The evidence that we have reviewed above suggests myriad ways to engineer efficient outcomes in the lab. Most of these ways also seem to increase external validity (e.g., various forms of communication or repetition of slightly payoff perturbed games), at least for organizational contexts broadly construed.[19] The potential of these efficiency-enhancing strategies to increase external validity for macro economic problems (and hence our ability to engineer efficient outcomes in the wild) is more difficult to assess.[20]

We conclude that, while coordination failures are common in the lab, they are by no means ubiquitous. More fundamentally, we are still far way away from an understanding of how common coordination failures are in the wild. We consider an answer to that question a interesting challenge with high payoffs .

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[18] A laudable recent exception in the gift exchange literature is List (2006) who tackles the fundamental issue of the external validity of the laboratory evidence and the issue of calibration e.g., in the transition from treatment R to RF). See also Gneezy and List (2006).

[19] We make that statement with some hesitation, partially reflecting the reservations of an referee: "Let's take a 7x7 min game with say 14-16 subjects (as in the original VHBB set-up) and introduce one or another of the devices that helps to overcome coordination failure in that game – pick any of the ones you mention on p. 13. First, do any of these actually work with the minimum game with this number of subjects? – if you make the cost of deviation essentially zero – but then you have taken all the tension out of the game. Possibly yes if you start with 2-3 players and add 1 or 2 at a time – but then what kind of structure does this correspond to in the real world? Possibly yes if you give them a limited set of communication options that foster confidence on playing the Pareto optimal outcome – but does this still hold true if they employ unstructured communication so they have to develop these self-assuring messages on their own without the help of the experimenter?" Good questions all.

[20] For starters there is the important discussion of what constitutes a large number in laboratory settings (e.g., Selten, 1973; Huck, Norman, and Oechssler, 2004.)

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