The Rise of Cooperation in Correlated Matching Prisoners Dilemma: An Experiment

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Abstract

Recently, there has been a Renaissance for multi-level selection models to explain the persistence of unselfish behavior in social dilemmas, in which assortative/correlated matching plays an important role. In the current study of a multi-round prisoners' dilemma experiment, we introduce two correlated matching procedures that match subjects with similar action histories together. We discover significant treatment effects, compared to the control procedure of random matching. Particularly with the weighted history matching procedure we find bifurcations regarding group outcomes. Some groups converge to the all-defection equilibrium even more pronouncedly than the control groups do, while other groups generate much higher rate of cooperation, which is also associated with higher relative reward for a typical cooperative action. All in all, the data show that cooperation does have a much better chance to persist in a correlated/assortative-matching environment, as predicted in the literature.

Keywords: Prisoners' dilemma, cooperation, experiment, unselfish behavior, evolution, assortative matching, correlated matching, multi-level selection

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1. Introduction

Cooperation in social dilemma situations like the prisoners' dilemma (PD), both in human societies and primitive species, has been a puzzle for philosophers, social scientists, and biologists alike. Assuming rationality and sufficient sophistication, cooperation can be sustained as a result of reciprocal actions in the context of 2-person repeated PD games, as argued in Trivers (1971), Axelrod and Hamilton (1981), Kreps et al. (1982), which in some cases can be evolutionarily stable as shown by Fudenberg and Maskin (1990) and Binmore and Samuelson (1992). The same idea can be extended so as to sustain cooperation with various sorts of contagious punishments even if a finite number of individuals are randomly matched to play the game, as discussed in Milgrom et al. (1990), Kandori (1992), Ellison (1994). The focus on ostracism by Hirshleifer and Rasmussen (1989) and that on opting-out with a pool of myopic defectors by Ghosh and Ray (1996) are further extensions of the idea that reciprocity sustains cooperation.

However, reciprocity cannot explain why people do things that are good for someone else, the group, or society without expecting direct personal reward from their actions. To explain this seemingly unselfish behavior, recent theoretical development has focused on the general principle of correlated, or assortative, matching within the population. The basic idea is that such unselfish behavior can only have a survival chance if its adoption entails advantages for its hosts over the selfish behavior at least in some contexts. These advantages can be subsumed under the notion of higher-than-chance likelihood to be matched with another unselfish type. Eshel and Cavalli-Sforza (1982) and Bergstrom (2003) among others have abstract models to explicitly calculate the fitness implication of such assortative meeting. Sober and Wilson (1998) discuss all those models that are based on the "general principle of group/multilevel selection": more or less localized or isolated interactions for more or less extended generations before dispersion can generate assortative matching effects.

They show how this could sustain a positive level of unselfish genes with scores of examples from nature. Bowles and Gintis (2002) survey some recent work in support of the multi-level selection model. Also, viscosity models by Pollock (1989) and Nowak and May (1992) can be reinterpreted as models of multi-level selection.

In the economics literature, Frank (1988) for example echoes the correlated matching theme to explain cooperation in PD of one-shot nature. He argues that the unselfish and cooperative act as such leaves traces on its hosts that can be observed by potential trading partners over one's lifespan. This signal of honesty must not be easily imitated. An assortative procedure that matches people similar in this signal to play one-shot PD will have the cooperative people more likely to be matched together, and similarly for the selfish ones. In other words, acting is also investing in the chance of encountering people of similar action histories.[1] Robson (1990), Amann and Yang (1998), and Vogt (2000) have extended this theoretical approach by introducing mutant types into the problem who are more sophisticated at recognizing others' types. This can endogenously induce correlated matching among those who play cooperation. Also, Bergstrom and Stark (1993) discuss several models of multilevel selection, i.e. assortative matching, based on family interactions.

Can people who cooperate really recognize other cooperators? Are there other mechanisms that achieve correlated matching in a similar manner? Can we expect more cooperation consequently, given those mechanisms? Can experiments shed light on these questions?

There are some experimental studies related to correlated matching in a PD environment. Camerer and Knez (2000) show that a successful group experience of solving a coordination problem can affect the willingness to cooperate in the one-shot PD. Despite their strong presentation effect, the experience can be interpreted as a catalyst to identify same-minded peers. Frank et al. (1993), as confirmed and refined by Brosig (2002), show that pre-play communication leads to better assessment of the cooperative types and a very high, yet difficult to explain, rate of cooperation even in one-shot PD. Yamagishi et al. (2003) show evidence that humans better recognize faces of cheaters than those of cooperators. Orbell and Dawes (1993), featuring the option of autarky and the false consensus effect, show that more cooperation may result among those who ventured to enter the PD game with another partner in an open-view six-person group without communication.

Actually, for experimental purposes, the problem of cooperation via correlated matching is more complex than it appears in a theoretical model. The reason is that theoretical models assume either extremely sophisticated or extremely simple individuals, while neither is the case with real subjects. In other words, real subjects are more sophisticated than the basic evolutionary models assume. This makes it a nontrivial issue as to the characterization of *similar* individuals. As the working assumption, we identify the type of an individual with his history of actions, as if saying, “You are what you do”.

The novelty of the current experimental study is that we separate recognition from action in subjects’ behavior. More precisely, unlike in the studies cited above, subjects are not given the opportunity, which is uncontrollable for the purpose of the experiment, to assess the type of their matched partners before making the trade decision. In contrast, we set objective criteria by which they are endogenously matched with one another, according to the similarity of their action histories within the cohort. We introduce two such correlated matching procedures in a multi-round PD experiment. We are interested in the questions of whether subjects respond to the matching procedure and whether and how more cooperation results or subject behavior changes, compared to the control procedure of random matching. If indeed “acting is investing” as envisioned by Frank

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[1] See Frank, 1994, for a discussion of its relationship with the multilevel selection idea.

(1988), can those who sow also reap the fruit, i.e. those who cooperate more encounter more cooperative actions in general?

It turns out that the answers depend both on the specific correlated-matching procedure and on the group composition. If the matching-relevant history is limited to only one round, we have little treatment effect. In the weighted-history matching procedure involving history up to five rounds, some groups achieve high levels of convergent behavior with or without positive numbers of subjects sticking to cooperation. In some other groups, high volatility persists even late in the game. In any case, the total amount of cooperation as well as the likelihood for cooperators to meet one another shows a more extreme distribution here, compared both to the control and the one-period correlation treatments. This bifurcation result also extends to the reward for a typical cooperative action. Moreover, even the reward ratio between cooperation and defection is significantly higher if we only focus on the later rounds, close to or higher than 1.0 in all the more cooperative groups. All in all, the data show that cooperation does have a much better chance to persist in a correlated/assortative-matching environment, as predicted by the multilevel selection theory in the literature.

Before presenting the details of the paper, it is important to note that there are other kinds of experiments on cooperative behavior without the option of strategic reciprocity that support the same multilevel selection idea. For example, Bolton et al. (2001) show in the Nowak and Sigmund (1998) framework of one-way cooperative giving that mere information about the receiver’s last-round action alone suffices to induce a significant increase in cooperation, which is also the object of Wedekind and Milinski (2000), and Seinen and Schram (2000). Although their matching procedure is random, the individual value of reputation is close to the basic idea behind Frank (1988) that *acting* (cooperatively) *is investing* (into social reputation or score for a good matching position). Fehr and Gächter (2000, 2002) show, in public goods experiments, how cooperation can proliferate even in complete-stranger settings when costly punishment is feasible. Gunnthorsdottir et al. (2001) show that sorting the groups by past contributions can in fact sustain high-level contributions among the cooperators, even though they are unaware of this sorting mechanism. Page et al. (2005) also obtain higher levels of contribution by allowing subjects to determine the new group formation based on past actions.

2. Game, Matching Procedures, and Design of Experiment

The Payoffs In this study, we consider the following basic prisoners' dilemma under different matching schemes. Table 1 shows the payoffs in NT (1 USD = ca. 34NT) for the row players. C and D stand for the strategies of cooperation and defection respectively. The payoff numbers are chosen so that the incentive to play defection is strong.

Table 1: Payoff Matrix

	C	D
C	8	1
D	12	3

The Matching Procedures Besides the common procedure of random matching (RM), we introduce two novel procedures, the *one-period correlated matching* (OP) and the *weighted-history correlated matching* (WH). In the OP procedure, subjects whose actions in a given round are the same are randomly paired with one another in the next period, as long as there is such a person available. The WH procedure extends the OP one so as to pair subjects according to some well-defined weighted history of previous actions. For the current study, we track only five rounds back and the history is weighted using the Fibonacci numbers[2]. Each subject starts with a sorting score T(t)=0 at the start of the game, for all t $\leqq$1. At each particular round t, his sorting score is defined as T(t) =

5a(t-1) + 3a(t-2) + 2a(t-3) + 1a(t-4) + 1a(t-5), where a(*s*) is 0 if his action in period *s* is defection, and 1 otherwise. In each round, all subjects are sorted after their current T-values. The relative order among those with the same T-value is randomly determined. And starting from the bottom, they are matched pair-wise in that order. An additional test ensures that subjects understand the matching rule correctly. See the corresponding instruction in the appendix. Subjects do not know their match-partners' T-values, nor are they informed about the distribution of T-value in the group. This uncertainty reflects the feature in Frank's model of imperfect signal recognition. Note that we chose not to use the simpler linear weighting of history, because discounting makes the recent actions more salient, which in turn may help get people to stick to a stable course of action.

Theoretical and Experimental Considerations Assuming perfect rationality, any finite repetition of the PD game with any of the above three matching procedures entails the unique subgame perfect equilibrium (SPE) of defection throughout. With infinite repetition, always-defection is still an SPE for all matching procedures. Kandori (1992) and Ellison (1994) show that the so-called contagious punishment scheme can sustain always-cooperation as an SPE under some conditions in the RM procedure. However, all-cooperation can never be an SPE in the OP and WH matching situations. In that case, each subject would be tempted to deviate to defection unilaterally, since contagious punishment would not be sustainable in equilibrium. The one he exploited would have no incentive to play defection, as required by contagious punishment. At least with the WH procedure, punishing the next partner would imply a rematch with the previous defector for many more rounds for sure, in exchange for the high likelihood of meeting someone in the previous all-cooperation class of players. In fact, with the correlated matching procedures, there cannot even exist a *stationary* equilibrium with positive

[2] Note that Fibonacci numbers F(n) has the property that F(n)/F(n+1) → "Golden ratio".

numbers of cooperators if subjects are aware of the stationary distribution. To be more precise, in any stationary equilibrium, the payoffs for the groups of always-cooperators and always-defectors must be close, so that nobody can profit by unilaterally changing his action(s). Yet, due to the correlated matching scheme, cooperators roughly expect the symmetric cooperation payoff while defectors get the symmetric defection payoff, if the population size is not too small. It is conceivable that a fleshed-out behavior model may lead to spatial chaos in the mode of Novak and May (1992).

For experimental purposes, there are other more salient issues at hand. First, we have to have a finite horizon in any experiment, but we still can find significant and persistent cooperation even late in the game (15-20%), as discussed by Andreoni and Miller (1993), Cooper et al. (1996), Friedman (1996) among many others for the RM procedure. Second, subjects in general only have very limited ability to do backwards induction (e.g. Selten and Stöcker, 1986), rendering the SPE based on complicated backwards payoff calculations unfit as the prediction to be tested in experiment. However, the lack of refined sophistication is not necessarily an obstacle for the rise of cooperation in human society. As suggested by recent literature on the evolution of cooperative behavior via multi-level selection, the correlated matching designs here are meant to create a device that can potentially increase the inherent value of cooperation. However, as all-defection is always equilibrium, it is still a big question whether and how this potential can be materialized.

As for our specific correlated matching procedures, since subjects are not informed about the current action distribution during the game, it is indeed conceivable that the play can converge to some stationary state with positive numbers of cooperators present. And we may speculate that the weighted-history procedure is better at enabling such a stationary state than the one-period one, since it takes a person only one round to get back to the “elite” class of “cooperators” as identified by the matching method in the latter, while five rounds are needed for the same task under the former.

Working Hypothesis. In the correlated matching treatments, we expect a higher chance that cooperative actions meet one another. At least in some groups we expect a higher total rate of cooperation.

The Experiment Design We have one treatment for each of the matching procedures RM, OP, and WH. Each treatment has been conducted with 5 groups of 14 subjects each. So, a total of 210 students of various majors from Chengchi University in Taiwan participated, recruited via announcement on bulletin boards and flyers with standard slogans to appeal for participation. There is a show-up fee of 50 NT.

General instructions are handed out and read, and questions answered. Each subject is randomly assigned to a terminal with an ID card in the spacious computer room of the statistics department. At the beginning of each game, subjects get the specific instruction about the game to be played, i.e. about both the payoffs and the matching methods as well as the number of rounds. After nobody has any further question, the game is played. The total duration of the session is less than 90 minutes. Subjects are then separately paid off and dispatched. Anonymity is ensured throughout. The sequences of games played in the specific treatments are summarized below. Subjects have no information about what is to come after the current game they are in.

Table 2: Treatment summary

Treatment	**Game 1**	**Game 2**	**Game 3**
1: OP	RM-5	OP-25	RM-5
2: RM	RM-5	RM-25	RM-5
3: WH	RM-5	WH-25	RM-5

The number indicates the number of rounds played in that game. Game 1 and 3 are 5-round random matching PD games with no feedback. Only at the very end of the experiment will subjects get informed of the results in those games and their total account balances will be adjusted accordingly. With this design, we want to test whether all groups are statistically from the same sample pool initially. Moreover, we avoid any dynamic effect before game 2 by the no-feedback feature. Game 2 is a 25-round PD game with feedback following the matching procedures of OP, RM, and WH in treatments 1, 2, and 3 accordingly. In game 2, each subject's view window carries his own play and payoff history as well as the current balance of account. Note that subjects in each session played two more games after this, which does not affect the subsequent data analysis. Average payoff for participants during the whole session is about 350 NT while the hourly wage for student jobs is around 120 NT in general.

3. Data Analysis

Game 1 is designed to check the initial inclination to cooperation in the population and to make sure that the groups are indeed random samples from the same population. Game 3 is designed to check what changes with added experience during the experiment. Table 3 below summarizes these data.

Table 3: Group data on the 5-round, no-feedback RM games

	Trt1(OP)			**Trt2(RM)**			**Trt3(WH)**	
group	**p1(d)**	**p3(d)**	**group**	**p1(d)**	**p3(d)**	**group**	**p1(d)**	**p3(d)**
1	**0.600**	**0.843**	**6**	**0.714**	**0.786**	**11**	**0.629**	**0.743**
2	**0.757**	**0.900**	**7**	**0.557**	**0.686**	**12**	**0.743**	**0.914**
3	**0.800**	**0.886**	**8**	**0.657**	**0.900**	**13**	**0.686**	**0.814**
4	**0.657**	**0.929**	**9**	**0.629**	**0.914**	**14**	**0.529**	**0.743**
5	**0.729**	**0.657**	**10**	**0.643**	**0.771**	**15**	**0.571**	**0.829**
avg.	**0.709**	**0.843**	**avg.**	**0.640**	**0.811**	**avg.**	**0.631**	**0.809**
std	**0.080**	**0.108**	**std**	**0.057**	**0.096**	**std**	**0.095**	**0.075**

Observation 1. The *Kruskal-Wallis rank sum test confirms that there is no significant sampling bias regarding the aggregate rate of cooperation* p1(c) *across all three*

treatments, p = 0.221, in game 1. Also, cooperation in game 3 is significantly lower, p=0.000, than that in game 1. In fact, the 36% average rate of cooperation in game 1 from subjects without experience and 18% in game 3 are compatible with data in the literature for RM prisoners' dilemma experiments.

Note that, because the results of using Mann-Whitney and Wilcoxon rank sum tests in this paper coincide with those of the Kruskal-Wallis (KW) test, all equal median tests in this paper refer to the KW test, unless stated otherwise. A logistic regression shows that individual inclination to play cooperation in game 3 is positively affected by his/her game 1 and game 2 cooperative actions and how often he/she switches actions in game 2.[3]

We now turn to game 2 to investigate the treatment effects. Throughout the paper, we will use data from rounds 6-23 only, to avoid any potential effects of the initial learning and end-game behavior often reported in the literature. Note that, in the correlated treatments, OP and WH, there indeed seems to be a decrease in cooperation in the last two rounds, indicating likely strategic intention associated with the cooperative action over the course.

Table 4 summarizes the most relevant group aggregate data. First, the rate of cooperation p2(c), though slightly higher in WH, is not significantly different across treatments, p=0.677. Exactly the same is true for the likelihood p2(cc|c) that a cooperator expects to meet another cooperator in the same round (p=.228), the average payoff av-r for the whole group (p=.733), and the empirical reward ratio, r-ratio,

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[3] $$\text{logit } p3(d) = -0.4809 + 1.6213\,p1(d) + 2.3695\,p2(d) - 0.1003\,p2(sw) + \text{noise}$$
$$(0.3034)\quad (0.3437)\qquad\qquad (0.3653)\qquad\qquad (0.0204)$$

Log-Likelihood = -426.071; Concordant = 74.8%; Hosmer-Lemeshow (H-L) goodness-of-fit test p =0.493; p2(sw) denotes the individual frequency of action switches in game 2. Note that we have experimented with different models with different combinations of potentially relevant variables. Yet, the associated H-L goodness-of-fit test failed for other models.

between cooperation and defection.

Table 4: Group level summary over rounds 6-23

Treatment	Group	p2(c)	p2(cc|c)	av-r	r-ratio
OP	**1**	0.183	0.261	4.183	0.630
	2	0.230	0.345	4.452	0.717
	3	0.246	0.355	4.548	0.718
	4	0.190	0.292	4.222	0.676
	5	0.306	0.468	4.853	0.836
	avg.	**0.231**	0.344	**4.452**	**0.714**
	std	**0.049**	0.079	**0.272**	**0.077**
RM	**6**	0.214	0.074	4.468	0.288
	7	0.258	0.400	4.599	0.779
	8	0.139	0.171	3.925	0.523
	9	0.179	0.222	4.171	0.565
	10	0.218	0.218	4.433	0.509
	avg.	**0.202**	0.217	**4.319**	**0.533**
	std	**0.045**	0.118	**0.270**	**0.175**
WH	**11**	0.409	0.816	5.194	1.617
	12	0.083	0.095	3.567	0.446
	13	0.333	0.476	5.016	0.809
	14	0.405	0.588	5.357	0.927
	15	0.135	0.176	3.897	0.538
	avg.	**0.273**	0.430	**4.606**	**0.867**
	std	**0.154**	0.297	**0.815**	**0.463**
KW test	**p-value**	**0.677**	**0.228**	**0.733**	**0.185**

Note: Total number of actions in each group: 252. p2(c): proportion of cooperation; p2(cc|c): chance for a c-playing subject to meet another c; av-r: average reward.

Conspicuously, however, the weighted-history treatment displays much higher standard deviations with respect to all variables in Table 4. In fact, groups 11, 13 and 14 display the highest rates of cooperation, the highest chances that a cooperative action encounters another cooperative one, and the highest average payoffs for the group and for cooperation within the group, among all 15 groups, while groups 12 and 15 rank among the lowest three groups regarding all the above variables except for average payoff for cooperation. In fact, this bifurcation effect of WH is also evident when we look at the data in rounds 6-14 and 15-23 separately.[4] Table 5 below summarizes results of all equal median and equal dispersion tests relevant.

Observation 2. *All rounds together (6~23), WH has a significantly greater dispersion than RM in rate of cooperation (*p2(c)*) and average reward (*av-r *).*

Observation 3. *Within rounds 15~23, WH has a significantly higher median than RM, in reward ratio (*r-ratio*) with p-value 0.016, and a greater dispersion in* p2(c) *and* av-r. *Within rounds 6~14, WH has a greater dispersion than RM in all variables.*

These also indicate that the advantage of cooperation in WH compared to RM can increase over time, after an extended period of learning and adaptation to the environment. In fact, comparing data from 6~14 with that from 15~23, we can see that the average payoff for cooperation and the r-ratio greatly increase even in groups 12 and 15 where the total number of cooperation decision is lower than in the worst groups in RM.

Table 5: Tests of treatment effect for group-aggregate variables

	Test of Equal Median			Test of Equal Dispersion								
				OP vs. RM			OP vs. WH			RM vs. WH		
	6~23	6~14	15~23	6~23	6~14	15~23	6~23	6~14	15~23	6~23	6~14	15~23
p2(c)	.677	.577	.706	1	.828	.671	.011	.011	.034	.011	.011	.034
p2(cc|c)	.228	.632	.063	.203	.519	.396	.011	.011	.090	.203	.053	1
av-r	.733	.645	.733	.671	.830	.671	.011	.011	.034	.011	.011	.034
r-ratio	.185	.590	.027	.203	.519	.396	.034	.011	1	.396	.090	1

Note: The tests of equal median and dispersion are Kruskal-Wallis and Ansari-Bradley[5] tests, while ▯ and ▮ indicate 10% and 5% significance, respectively.

[4] Data can be found in the discussion paper version, Yang, Yue, and Yu (2004).

[5] Ansari-Bradley test and Siegel-Tukey test are two frequently used non-parametric tests for dispersion. Since the results of Ansari-Bradley and Siegel-Tukey tests are very similar, we only show the p-values of the Anasari-Bradley test. For detailed discussion of nonparametric tests, we refer to Daniel (1990).

Observation 4. *WH has significantly greater dispersion than OP in all relevant variables, which, however, fades away in later rounds 15~23 for reward ratio,* r-ratio.

Observation 5. *The only differences between OP and the control treatment RM are regarding the medians for the conditional likelihood that a cooperative action meets an equal* p2(cc|c), *and for reward ratio* r-ratio *in rounds 15~23, with p-values 0.028, and 0.047, respectively.*

These indicate that one-period protocol does induce some behavior difference compared to RM, in the later rounds. But this effect is by far not as strong as that of the weighted-history protocol.

Observation 6. *In both OP and WH,* p2(cc|c) *and* r-ratio *display increase from 6~14 to 15~23, to the significance level 10% two-way, or 5% one-way, with the Wilcoxon signed rank test.*

This shows that the group aggregate behavior is still evolving in the OP and WH treatment, but not so much in the control treatment RM.

On the group-aggregate level, we have found significant differences across treatments. The legitimate question is whether we can observe consistent and compatible treatment effects if the data are aggregated differently. Next, we will show that the observed treatment effects are indeed robust when we focus on distributions of individual characteristics.

Table 6: Distribution of individual c-frequency in Game 2 (rounds 15~23)

OP					**RM**					**WH**				
1	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**	**15**
0	0	0	0	0	0	0	0	0	0	0	0	0	0	0
0	0	0	0	0	0	0	0	0	0	0	0	0	1	0
0	0	0	0	0	0	1	0	0	0	0	0	0	1	0
0	0	0	0	1	0	1	0	0	0	0	0	0	2	0
0	0	1	0	2	0	1	0	0	0	1	0	1	3	0
1	1	1	0	2	0	1	0	0	1	1	0	2	3	0
1	1	1	0	3	1	1	0	0	1	1	0	2	3	0
1	1	2	0	3	1	2	0	0	2	1	0	2	4	0
2	2	3	1	3	2	2	1	1	3	8	0	2	4	1
2	3	3	2	4	2	3	1	1	3	9	1	4	4	2
2	4	4	2	5	3	3	1	3	3	9	1	5	6	2
3	4	5	3	5	4	4	2	3	4	9	1	6	6	2
4	6	6	7	7	7	6	3	6	4	9	3	9	9	3
4	7	9	9	9	9	9	3	9	9	9	3	9	9	3

Note: Each column has 14 entries for the 14 players of each group. Max.= 9.

Table 6, for example, reveals that group 11 has apparently converged to a stationary state of 6 cooperators and 8 defectors, with nobody deviating more than once in a total of 9 rounds (15~23). From Table 6 we can also see that both groups 13 and 14 have exactly one pair locked-in in the always-cooperation mode. No other groups come close, though the presence of four always-cooperators in RM suggests that there are always some people who might not have understood the game or are just unconditional cooperators for whatever reason. Conversely, no players in groups 12 and 15 chose cooperation more than 3 times in the last 9 rounds, indicating a convergence to the all-defection equilibrium. They also display less overall inclination to cooperation than even groups in the random matching case.

Table 7: Derived variables for Game 2 (15~23)

	OP					RM					WH				
Group	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**11**	**12**	**13**	**14**	**15**
H6 mean	2.83	4.33	5	4	5.5	4.5	4.5	1.83	3.83	4.33	8.33	1.5	5.83	6.33	2.17
L6 mean	0.17	0.17	0.33	0	0.83	0	0.67	0	0	0.17	0.33	0	0.5	1.67	0
H6–L6	2.67	4.17	4.67	4	4.67	4.5	3.83	1.83	3.83	4.17	8.5	1.5	5.33	4.67	2.17
#c ≧ 50%	0	2	3	2	4	2	2	0	2	1	6	0	4	4	0
#c = 0	5	5	4	8	3	6	2	8	8	5	4	9	4	1	8

Note: Definitions of variables in Table 7 can be found in the following observations.

To illustrate where the above observed aggregate treatment effects may have originated, we define some further statistical variables. Their values for all groups and all three time-intervals we discuss are summarized in Table 7 for rounds 15~23. Table 8 summarizes the tests.

Table 8: Tests of treatment effect for derived variables

	Test of Equal Median			Test of Equal Dispersion								
				OP vs. RM			OP vs. WH			RM vs. WH		
	6~23	*6~14*	*15~23*	*6~23*	*6~14*	*15~23*	*6~23*	*6~14*	*15~23*	*6~23*	*6~14*	*15~23*
H6	.677	.493	.674	.667	.128	.667	.011	.032	.011	.011	.007	.032
L6	.210	.201	.578	.430	.397	.676	.070	.624	.250	.933	.676	.549
H6–L6	.686	.228	.580	.830	.364	.825	.053	.090	.025	.034	.010	.032
#c ≥ 50%	.808	.165	.544	.397	.621	.196	.065	.027	.099	.038	.056	.018
#c = 0	.381	.206	.815	.919	.770	.768	.210	.435	.432	.728	.712	.708

Note: The tests of equal median are Kruskal-Wallis tests. The tests of dispersion are Ansari-Bradley tests for the continuous variables and bootstrap simulation tests for the discrete variables. Also, light grey and dark grey indicate 10% and 5% significance, respectively.

Observation 7. *With respect to the mean of the 6 highest-ranking cooperators (*H6-mean*), that of the 6 lowest ranking cooperators (*L6-mean*), and their difference* H6-L6*, there is no significant difference in the treatment median.*

Observation 8. *WH has significantly higher dispersion than OP and RM in* H6-mean *and* H6-L6*.*

Observation 9. *With the exception of OP vs. WH for rounds 6~23, there is no significant difference in dispersion for* L6-mean *across the treatments.*

Observation 10. *Regarding the statistical variable of the number of subjects in a group who played cooperation at least 50% of the time,* #c ≥ 50%*, WH displays significantly higher dispersion than both OP and RM in all three time frames.*

Observation 11. *Regarding the number of subjects in a group who consistently chose defection,* #c = 0*, there is no significant difference across treatments whatsoever.*

Observations 7 through 11 combined indicate that the afore-mentioned bifurcation effect of the WH treatment, groups 11, 13, 14 vs. groups 12, 15, mainly stems from the bifurcation of the number of most cooperative players. Regarding the number of least cooperative subjects, there seems to be parity across treatments, except for OP vs. WH for timeframe 6~23. In the latter case, groups 13, 14 show higher means for their least cooperative subjects than the groups in OP, while groups 11, 12, 15 show lower means that are similar to the groups in RM. However, this significance does not hold if we consider rounds 6~14 and 15~23 separately. In any case, this oddity may be from the fact that groups 13 and 14, like most groups in OP, still display high volatility in subject behavior, while subject behavior is more settled in groups 11, 12, 15 and the RM groups, especially among those who have decided to stick to defection. This is also evident from the following observation.

So far, we have discussed treatment effects based on various kinds of group-level variables. Now we will show more evidence to further confirm that cooperation has a much better chance to survive in correlated matching environments. Figures 2, 3, and 4 show the scatter plots for the joint distribution of average individual T-value and payoff in the RM, OP, and WH treatments, respectively. Each graph contains 70 dots for the same number of subjects in each treatment. The correlation coefficient between T-value and payoff in the One-Period treatment is 0.1412 and not significantly different from 0. It is negatively correlated in the Random Matching treatment ($r = -0.5717$), but positively correlated in the Weighted-History one (r = 0.7705), both significantly differently from 0.

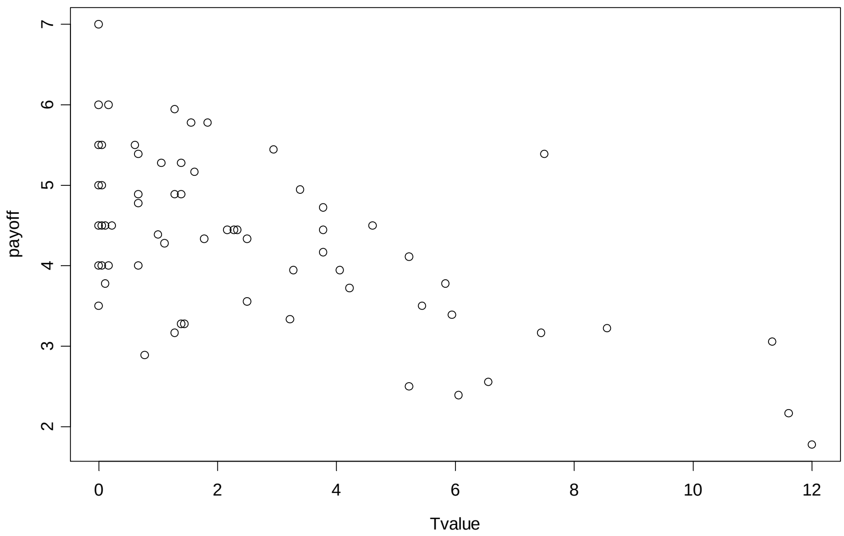

Figure 1: T vs. payoff for RM (r=–0.5717)

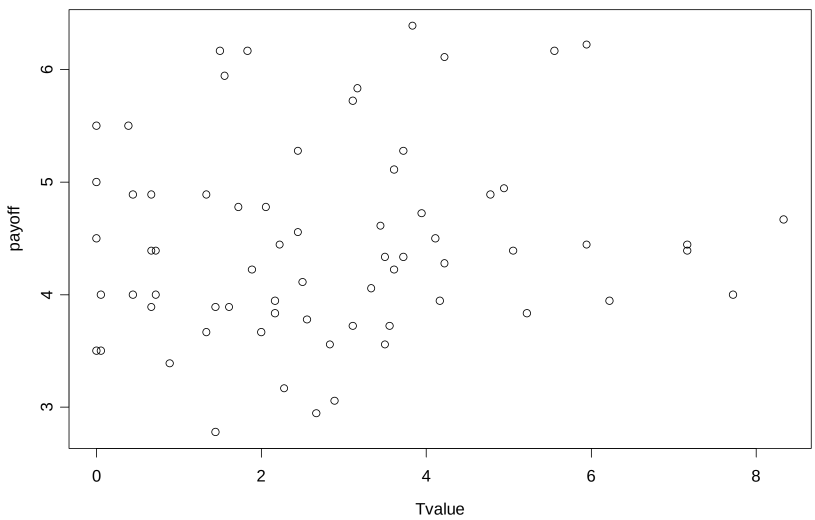

Figure 2: T vs. payoff for OP (r=0.1412)

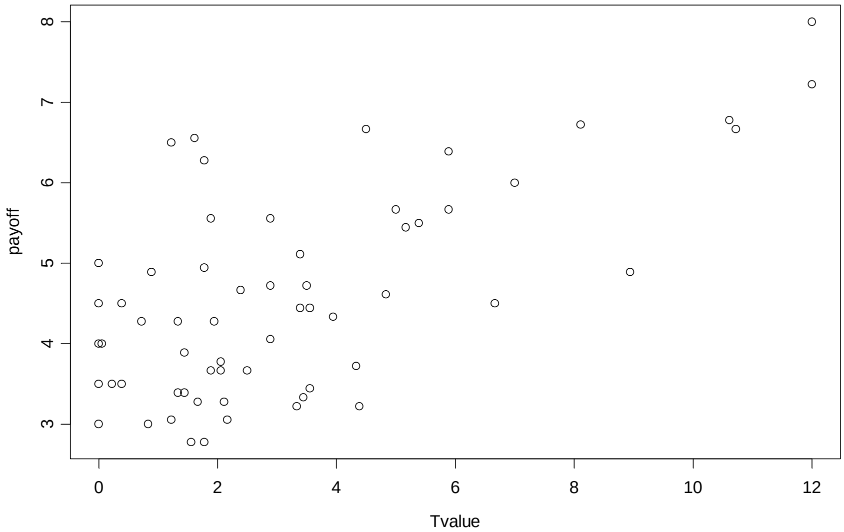

Figure 3: T vs. payoff for WH (r=0.7705)

It is evident that the negative correlation of RM, which reflects the very nature of the predicted failure of cooperation in PD, disappears with the introduction of both the OP and the WH treatments. This observation accentuates the insight we gained with the variable r-ratio in Table 4 with statistical significance stated in Observation 3. It implies that, particularly with the WH matching device, cooperation has a good chance to prevail.

The final part of the current analysis focuses on the likelihood of cooperation aspect of the T-related behavior patterns. Figure 4 shows that the rate of cooperation displays an increasing tendency with T for all treatments.

Surprisingly, it appears that there is not much difference in this regard across the treatments. This makes the variable T-value seem to be compatible with the interpretation as an inertia variable: subjects' inclination towards cooperation is similar, i.e. positively correlated, with their average behavior in the recent history of play. Starting with 15 group dummies and iteratively eliminating dummies of p-values greater than 0.05, it turns out that the following logistic regression outcome fits the data quite well.

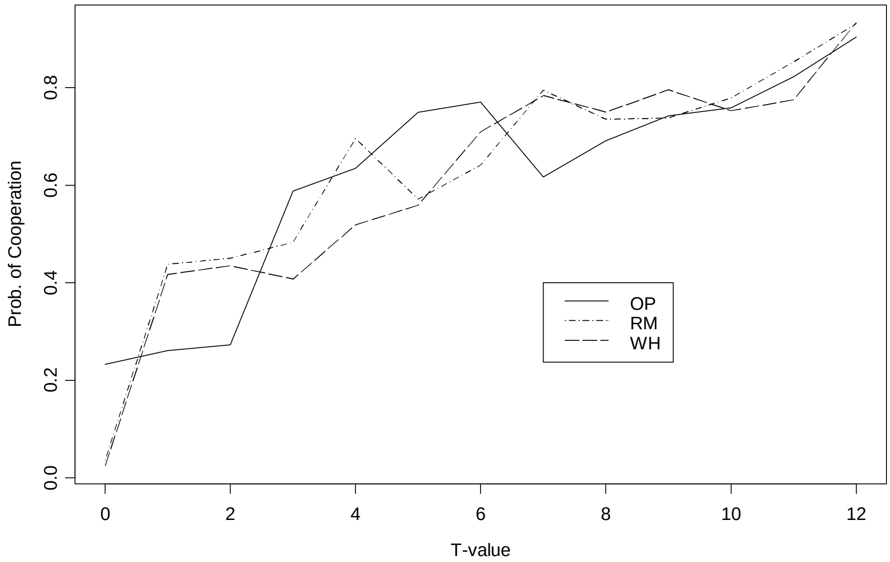

Figure 4: T-dependent probability of cooperation

$$\underset{}{\text{logit}(p) = -2.3999} + \underset{(.0889)}{.8277T} - \underset{(.0855)}{.1520T^2} + \underset{(.0201)}{.0096T^3} + \underset{(.0012)}{.2766\,Trt\,3} - \underset{(.1090)}{.7458\,\text{Gr}12\&15} \qquad (1)$$

(.0889) (.0855) (.0201) (.0012) (.1090) (.1762)

(-2LL = -1585.92, Concordance = 76.3%, Hosmer-Lemeshow p-value = .587)

Note that we initially ended up with significant dummies left for groups 12, 14, and 15 in this manner. The Hosmer-Lemeshow goodness-of-fit test[6] of this particular model failed, however. Note also that we unsuccessfully experimented with the model conditioned on whether subject behavior was C or D for the same T-value. After trial and error, model (1) ended up as the only one with all-around satisfactory statistical properties. So, the bifurcation effect of the WH treatment resurfaces here. Figure 5 displays the fitted

[6] Two frequently used goodness-of-fit are Pearson and Deviance tests. However, as the number of groups increases, these two tests are more likely to falsely reject the null hypothesis. Another (Pearson-like) goodness-of-fit test, proposed by Hosmer and Lemeshow (1980), is used more often in practice, as in this paper. Note that the Hosmer-Lemeshow (H-L) test is to group residuals based on the values of the estimated probabilities. The default number of groups in H-L test is 10, as in this paper.

curves.

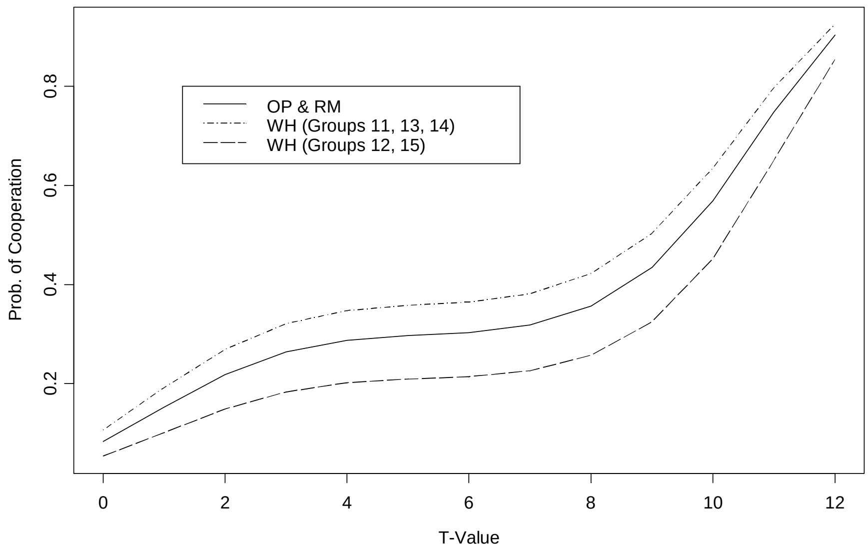

Figure 5: Fitted curves for the joint logistic model

Summary: Piecing together the evidence, we see that the One-Period matching procedure has a slight effect of increasing cooperation, which can even get more pronounced with time. But subjects seem to be still very actively changing their actions, so our current design is not particularly conducive to founded speculation as to where all this is leading.

However, the Weighted-History procedure shows much clearer effect compared to the control treatment. There is a clear bifurcation of groups under WH. In some groups, 12 and 15, the behavior seems to converge to the all-defection equilibrium, even more accentuated than in the groups in the Random-Matching treatment. In others, 11, 13, and 14, we observe strong performance for cooperative actions, particularly evident in the later rounds (15~23). Group 11 is notable for an extreme, within-group bifurcation of persistent defectors and cooperators. On the other hand, groups 13 and 14 are similar to the OP groups with still a lot of subjects changing behavior in the later rounds, but with apparently better reward overall. Most notably, with an average of 0.8 and 1.07, both OP and WH show a significantly better reward ratio between cooperation and defection than RM, in the later rounds. Even in the defective groups 12 and 15, the r-ratios are very high compared to the RM groups. These are all evidence that the assortative matching devices we investigate in this study are indeed effective as the theory predicted.

4. Concluding Remarks

Think of the groups as different tribes in competition for hunting territory. And, think of their military power as proportionate to the group's total reward accruing from those repeated PD games. Those tribes that manage to end up being more cooperative within the tribe, like our groups 11, 13, and 14, will have a better chance to be the ones left in, say, a war of attrition. Alternatively, if the reward is interpreted as representative of the reproductive power common to biology models, these groups also will have more offspring to participate in the next generation group formations and we will have higher chances of having similarly composed groups with the hope of similarly better group total reward.[7] In any case, with the correlated, particularly the Weighted-History, matching device, society as a whole has a better chance to have more cooperation persistent in the long run. Figure 3 in contrast to Figure 1 very well illustrates this phenomenon that it often pays to be consistently cooperative in the weighted-history environment.

Now that we have confirmed the theoretical prediction that correlated/assortative matching can indeed break the curse of defection in Random-Matching prisoners' dilemma, the next step is to characterize the process and outcomes under these new matching devices. Our data indicate that we may expect a bifurcation in aggregate group outcomes: Some groups may converge to the all-defection equilibrium even faster than in the random-matching environment, while cooperation can thrive among a sub-group

[7] See e.g. Sobel and Wilson (1998, pp.63, pp.65) for this line of argument and for further references.

of subjects in the other groups. The latter can be further divided into two types. Some groups have the more cooperative subjects sticking to cooperation quite soon in the experiment, while most of them in the other groups are still not fully committed to cooperation even late in the game, quite similar to the typical behavior in the One-Period treatment. It is not clear whether this phenomenon is only transient, i.e. reflecting the natural learning process towards a more stable behavior pattern, or whether it already reflects some stable pattern of the subjects who explicitly want to "exploit" fellow cooperators every now and then. However, the potential danger of fatigue or boredom due to long experimental sessions limits our options to directly answer the last question by experiment. We hope to develop learning models in the future to be able to simulate the long-run outcomes with data from sessions of limited duration. Also, it is relevant to manipulate the information the subjects receive, in order to check the robustness of the results in this study.

Note that this bifurcation result is also consistent with the conditional cooperation hypothesis by e.g. Keser and van Winden (2000) that many cooperators are only willing to cooperate if they expect others to do the same, an idea also to be found in Rabin (1993). Amann and Yang (1998) call them "cautious cooperators" in an evolutionary model. Our study shows the experimental evidence that correlated matching devices can induce those cautious cooperators to actually cooperate in the end.

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