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Everyone Wants a Chance: Initial Positions and Fairness in Ultimatum Games*

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Abstract

Fairness emerges as a relevant factor in redistributive preferences in surveys and experiments. We study experimentally the impact of varying the probability with which players are assigned to initial positions in Ultimatum Games (UGs). In the baseline case players have equal opportunities of being assigned the proposer position – arguably the more advantaged one in UGs. Chances become increasingly unequal across three treatments. We also manipulate the inter-temporal allocation of opportunities over rounds. We find that: (1) The more initial chances are distributed unequally, the lower the acceptance rates of a given offer; consequently, offers increase; (2) Being assigned a mere 1% chance of occupying the proposer role compared to none, significantly increases acceptance rates and decreases offers; (3) Players accept even extreme amounts of unequal chances within each round in exchange for overall equality of opportunities across rounds. Procedural fairness – both static and dynamic - has clear relevance for individuals.

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1 Introduction

"[...]*All are equal, all are free, and all deserve a chance to pursue their full measure of happiness." (from Barack Obama's swearing-in speech after the 2008 US elections).*

"There is a symbolic utility to us of certainty itself. The difference between probability .9 and 1.0 is greater than between .8 and .9, though this difference between differences disappears when each is embedded in larger otherwise identical probabilistic gambles– this disappearance marks the difference as symbolic." (Nozick 1994, p. 34).

The idea that initial positions have to be assigned equitably in the competition for scarce resources is one of the cornerstones of systems of distributive justice in contemporary societies. It is functional to guaranteeing equal opportunities to citizens in the pursuit of their goals, along with the requirement that the rules of the competition confer no advantage to participants (Diamond, 1967; Rawls, 1999; Arneson, 1989; Cohen, 1989; Roemer, 1998).

In practice, large amounts of resources are spent to 'level the playing field' in many developed countries. A clear example is public spending on primary and secondary education [1]. Fairness in the allocation of initial positions also bears on preferences for redistribution. Corneo and Gruner (2002) find that people believing that *"coming from a wealthy family is important for getting ahead in life"* are also more supportive of income redistribution. Their sample includes 12 countries from both Western economies and ex-socialist countries, so this view seems widespread. This evidence may be construed as proof that people perceiving initial positions as unfairly distributed also see the final income allocation as unfair and thus demand more income redistribution[2]. Since US citizens generally believe that their society grants more opportunities for climbing the economic ladder than their European counterparts (Gilens, 1999), fairness considerations have been deemed to be

[1] According to OECD data (2002), governments in OECD countries allocate substantial public spending to primary and secondary education, the average being 3.5% of GDP in 1999. Interestingly, US expenditures are in line with the OECD average and with continental European countries, whilst they differ markedly with respect to other public expenditure provisions (Alesina and Glaeser, 2004). This can be interpreted as a general expression of political support for policies equalizing initial positions. See also Lipset (1997).

[2] Other factors affecting the fairness of the competition for final positions have also been shown to be relevant. For instance, both Fong (2001) and Alesina and La Ferrara (2005) show that US citizens believing that hard work and effort are important for getting ahead in life are less supportive of income redistribution.

at the root of the much lower redistribution taking place in the US vis-à-vis Europe (Alesina and Angeletos, 2005; Benabou and Tirole, 2006).

In spite of the widespread support for the 'level playing field' idea, many people nonetheless judge their society to fall short of this principle. 77% of the participants in the 2006 International Social Survey Programme responded that the government should spend "more" or "much more" on education - even if that implied increased taxation - in a sample including 33 countries. Education comes second after health as the item for which respondents want most to increase public spending, ahead of pensions, law enforcement, the environment, unemployment benefits, culture and arts, and defence[3].

A first basic requirement of equality of opportunities is *formal* in that nobody should be prevented from entering a competition for final positions, offices, or powers, and everyone should be judged on the merits relevant for that position. However, many societies fall short of this objective. This is the case when people are discriminated against for their belonging to ascriptively defined social groups, such as apartheid systems, or the caste system in India[4]. A stronger requirement has been put forward that societies should provide not only formal but also *substantial* equality of opportunities. That is the notion that all citizens should be materially put in the condition to compete for positions regardless of circumstances for which they cannot be held responsible, such as their social background or genetic endowment (Williams, 1962; Rawls, 1999). However, most societies fall short of this ideal by a greater or smaller margin (see e.g. Bowles *et al.*, 2008). This may be put down to either the conceptual difficulty in providing a sound notion of opportunity (Fleurbaey, 1995; Dworkin, 2000: 87), or to the practical difficulties associated with its implementation (see Romer, 1998, for a method in this respect, and Fleurbaey, 2002, for a critic). However, in other cases lack of political willpower may be to blame, especially as this objective may conflict with society's other objectives, for instance efficiency (Roemer, 1998).

Not only is luck relevant to the assignment of social and economic groups, but also it plays a role in several other dimensions of one's career. For instance, job selection procedures may involve some degrees of discretionality by the selectors, so the outcome of the selection may partly depend on the

[3]Even in the US, the country traditionally seen as the "land of opportunity", 29% of respondents to the 1998 Gallup Social Audit declared that *"all Americans do not have an equal opportunity to succeed"* (Gallup, 1999). Corneo and Fong (2008) find this measure of availability of opportunity to be a strong (negative) predictor of individual preferences for redistribution.

[4]As recently as 2006, India's prime minister Mr Manmohan Singh likened discrimination against the "untouchables" in India to an apartheid system. See report in http://www.guardian.co.uk/world/2006/dec/28/india.mainsection. See also Deshpande (2011) and Hoff and Pandey (2006).

luck that a person has in meeting the selectors' favour. Such arbitrary elements in judgements may be difficult to eliminate completely.

It is thus undoubtedly important for economic policy to understand how deviations from the ideal of a fully level playing field or a fully unbiased judgement are perceived by individuals. How negative is individuals' evaluation of the lack of equal opportunities, and how *more* negative does it become as we move *farther* away from the ideal? What value do individuals attach to a purely symbolic opportunity, such as one attributing one very small *chance* of gaining an advantaged position, in comparison to a situation of absolute discrimination? Do equal opportunities need to be granted at *any* instance of social interaction? Or are people happy to accept equal opportunities *on average* across time, for instance being advantaged half the time and disadvantaged the other half?

The empirical investigation on the topic suffers from its almost exclusive reliance on survey questions where, by necessity, the notion of opportunity is often vague and prone to subjective interpretation. In this paper we seek to shed some light on these issues adopting an experimental approach. Experiments have the advantage of allowing the researcher to manipulate the degree of fairness in assigning initial positions, and to measure rigorously individuals' reactions to deviations from the benchmark situation of perfect fairness. Many experiments have already dealt with the issue of procedural fairness showing its relevance to individuals (see section 2). We are, to the best of our knowledge, the first to tackle the issue of fairness in the assignment of initial positions proper.

We take the Ultimatum Game (UG henceforth) as our basic interaction. This game has been used extensively to examine individuals' assessment of the fairness of a payoff allocation involving two players. For the purpose of our investigation what matters is that interaction in the UG takes place from *asymmetric* positions. The *proposer* in a UG has a first-mover advantage over the responder in that she can dictate the shares of the final allocations. This position of advantage is normally conducive to a larger share of the payoffs accruing to proposers, who on average obtain more than 60% of the pie (see e.g. Oosterbeek *et al.*, 2004). Guth and Tiez (1986) show that when subjects are asked to bid on the two positions of a UG before bargaining, they offer twice as much to occupy the proposer's role than the receiver's role. Arguably, the proposer's position is more desirable than the responder's.

The main novelty of our experimental design is to make the access to the two UG roles subject to a lottery, and to manipulate the distribution of probability of these lotteries. The baseline case is that both players have *equal opportunities*, as the lottery assigns both individuals a 50% chance of acquiring the proposer role. In the other treatments, the initial lottery is biased in favour of one of the two players. We consider three treatments in which one of the two players is *favoured* with respect to the other in that she has, respectively, 80%, 99%, and 100% probability of becoming the proposer, while the *unfavoured* player only has the residual probability. In this way we are able to assess the impact of increasing disparity in the distribution of initial chances on individual perception of allocation fairness. We also study another dimension of procedural fairness, relative to the distribution of opportunities over time. In what we call the fixed role condition (FRC), an unfavoured player remains disadvantaged throughout the 20 interactions of the experiment. Under the variable role condition (VRC), positions are reassigned before each round. We claim that VRC provides for equality of opportunity in a *dynamic* rather than in a *static* sense. We believe our study to be the first to tackle the issue of the inter-temporal distribution of opportunities.

It is worth noting that the initial lottery represents a purely procedural addition to the bargaining stage that brings about no strategic consequences. Individuals who are *consequentialist* (Machina, 1989; Hammond, 1988) should regard such initial stage as irrelevant. Self-interested people should therefore attach no value to them. People having *other-regarding* consequentialist preferences, as those modelled in Fehr and Schmidt's (1999) (FS henceforth) and Bolton and Ockenfels's (2000) (BO henceforth) theories, should find such an initial stage irrelevant, too. The same holds for theories of intention-based reciprocity, and theories emphasizing concerns for efficiency (Charness and Rabin, 2002). The fairness of the initial lottery should instead be relevant for *procedural* individuals. Models of procedural fairness have been put forward (Bolton, Brandts and Ockenfels, 2005 - BBO henceforth; Trautmann, 2009), where preferences are defined over the *procedures* bringing about outcomes, rather than on mere outcomes. However, these models are defined over lotteries that directly determine final outcomes, so they remain silent on procedures determining the initial positions of an interaction. Section 8 shows how a simple modification of the FS model can account for most of our findings.

We illustrate the main hypotheses and the experimental protocol in sections 2 and 3. In sections 4 and 5 we show the results for FRCTs and the VRCTs, respectively. Section 6 focuses on the first round of interactions while 7 contrasts FRC with VRC and analyses the dynamic evolution of play. Section 8 presents our theoretical model. Section 9 concludes the paper.

2 Review of the literature

The UG is an allocation problem among two players. The first-mover (the proposer) makes an offer to the second-mover (the receiver) about how to divide a given pie. The receiver can then accept the offer, in which case both players receive what dictated by the proposal, or reject it, in which case neither receives anything. The subgame perfect Nash equilibrium (SPNE) of this simple interaction is the most unequal allocation possible: the proposer should demand all but the smallest possible amount the pie, and the receiver should accept this offer. It is well-known that experimental evidence consistently and conspicuously deviates from this prediction (Oosterbeek *et al.,* 2004).

Several experiments have varied the way initial positions are assigned in UGs - or in Dictator Games (DGs) [5]. A first class of experiments conditions role assignment to a contest, based e.g. on general knowledge quizzes, ability tests, or simple tasks. In the pioneering study by Hoffman *et al.* (1994), best-ranked (worst-ranked) players were assigned the proposer (receiver) role. This resulted in proposers demanding significantly more for themselves, and UG receivers being willing to accept such higher claims. This result has been construed in terms of the enhanced legitimacy that being assigned to the advantaged position has when this has been "earned" in a fair contest. Hence the proposers' entitlement to a larger share. A similar impact of earning one's initial endowment has been confirmed in several other studies[6].

A second class of experiments makes players' initial endowments unequal. In Armantier (2006) either proposers or receivers were assigned a larger endowment than their counterparts. In the initial rounds of interactions, subjects tried to compensate for this imbalance, with proposers (receivers) having larger endowments offering more (accepting more) in comparison with the baseline case of endowment equality[7]. A similar willingness to compensate arbitrary differences in initial endowments has been found in Becker and Miller (2009). Guth and Tietz (1986) let players alter their initial endowments through their own choice when participating in an auction to determine their roles. Proposers paid on average twice as much as receivers to acquire their role. In the subsequent bargaining phase, the mode of the distribution shifted away from the 50-50 split, and moved closer to a 2/3-1/3 split. This

[5] A Dictator Game is the same as a UG apart from the removal of the receiver's action space. In other words, the receiver of a DG is a passive player who takes home all of what the dictator has left to her. This interaction was first studied by Forsythe *et al.* (1994) to tease out whether proposer's behaviour in UGs was due to fairness or self-interested calculation. The fact that offers still remain positive in DGs but are lower than in UGs is evidence that both motivations are present in individuals' decision-making.

[6] See e.g. Konow (2000), Cherry *et al.* (2002), Esarey *et al.* (2006), Cappelen *et al.* (2007), Durante and Putterman (2008), Schurter and Wilson (2009), for (modified versions of) dictator games.

[7] This trend was however reversed in the final rounds of the game. It has been conjectured that this may be due to something alike a "moral credit": Players tend to be more pro-social in initial rounds than in final ones.

means that, taking into account the initial costs sustained in the auction, the resulting payoff distribution came close to a 50-50 split. The lesson from these experiments seems to be that players perceive the disparity in their initial endowment as arbitrary, and act in ways to compensate it.

In a third class of games, it is the final outside options which are modified. Buchan *et al.* (2006) compare a standard UG with a "buyer power" condition where the proposer has a greater-than-zero outside option in case of rejection by the receiver. They find that such treatment condition has *opposite* results in the US - where receivers lower, albeit slightly, their acceptance thresholds - and Japan - where receivers significantly increase theirs. Binmore *et al.* (1991) study repeated UGs where a player has a much larger outside option than the other player. They find that the exploitation of such "power" is not universal but is limited to specific bargaining schemes. Similarly, Suleiman (1996) and Handgraaf *et al.* (2008) consider UGs where receivers have varying powers to modify the pair's payoffs after rejecting the proposal. A rejection by the receiver entails that only a percentage $(1-\delta)$ of the pie is destroyed, while a percentage δ is preserved. So, receivers's power is maximal (minimal) when $\delta = 0$ ($\delta = 1$),which corresponds to a standard UG (DG). Both studies consider treatments where δ is set at values in the interior of the interval as well as at the two extremes. Suleiman (1996) finds, as expected, an overall negative relationship between receivers' power and proposers' offers. No firm inference on receivers' behaviour can be drawn due to the paucity of low offers in some of the treatments. Handgraaf *et al.* (2008) find a significant variation in proposers' behaviour triggered by a 10% change in δ. Moreover, when given the possibility to choose how much power they want to have, receivers show a strong preference to have more power than less. It is worth noting that both studies find significant effects for relatively small variations of their manipulation parameter δ, and some reversal in the otherwise monotonic trends linking δ to players' behaviour. We find similar instances of both effects in our setting, too.

All of these studies find relevant effects on final payoffs of modifying the way initial positions, initial endowments, or final outside options, are assigned. This is remarkable because the strategic nature of the bargaining problem is left unaltered by such modifications - namely, the SPNE remains the same as in the standard UG (or DG). Equity theory (Homans, 1958; Walster *et al.,* 1978) - the idea that allocations should be distributed in proportion to one's relative effort, abilities, or investment, compared to others' - has been called upon to account for some of these results. In contexts where proposers 'earn' their position of advantage, their increased demand is legitimate. In cases where greater power is arbitrarily assigned, a substantial portion of players try to compensate for such imbalance, sometimes at the cost of destroying the whole pie.

All the studies reviewed above more or less explicitly affect the fairness of the game procedures, either introducing a competition for initial positions (first class of games) or by modifying the game's *material* aspect, that is, initial endowments, and outside options (second and third class). Only recently has the focus been extended to studying the impact of unequal *opportunities to achieve* monetary payoffs, rather than inequality in the payoffs themselves. BBO show that individuals are indeed sensitive to the way chances are distributed in a social interaction. They contrast a treatment where UG proposers can choose between an equal share of the pie and an unequal allocation, to a treatment in which proposers can choose between an unbiased lottery - giving equal chances to win the whole pie - and the same unequal allocation as in the previous treatment. They notice that responders' behaviour when offered the unequal allocation is virtually the same in these two scenarios. Thus they conclude that procedural fairness is a substitute for outcome fairness. This result has been replicated in several other experiments using modified DGs, although it appears that equality of opportunity is not a *full* substitute for equality of outcomes (Karni *et al.*, 2008; Becker and Miller, 2009; Krawczyk and Le Lec, 2010). In a second study, BBO report how rejection rates in a modified UG with random computer-generated proposals is significantly higher when the lottery is strongly biased in favour of the proposer than when it is unbiased. Hence, they conclude that receivers seem to dislike situations where they had significantly fewer chances than proposers, even when the final outcome is the same.

Other studies use modified versions of DGs to investigate how variations in initial opportunities affect redistributive choices. In Krawczyk (2010), participants are matched in groups of four, and are assigned different probabilities of winning (PoW) monetary prizes. After being informed of their own and others' PoW, players are asked to propose a tax rate which will be applied to the individuals' earnings. This acts as a redistributive scheme, which ranges from the two extremes of either leaving the prize assignment unchanged (in case of a 0% tax rate), or bringing about equal final allocations (if a 100% tax rate is selected). Each player has an even probability of having her proposed tax rate selected. PoWs are manipulated so that each player makes decisions in groups with either a high or a low dispersion of PoW. The theoretical prediction is that procedural players should react to higher dispersion in PoW by increasing their proposed tax rates. However, this is not the case. Demanded tax rates do not react to changes in the dispersion of PoWs. The only aspect in which procedures seems to matter lies in the fact that players react to the varying dispersion in PoW *more* in the "merit" treatment - where incomes are determined by the relative performance in a contest - *relative to* the "luck" treatment - where incomes are determined randomly.

Cappelen *et al.* (2010) also study the impact of varying initial positions, where these are determined by individual choice rather than luck. In an initial stage players choose between participating in a lottery and opting for a safe alternative payment. Players are then matched in twos and asked to put forward a tax rate to be applied to their pair. This has an even probability of being selected. Even in this setting players have an even chance of acting as "dictators" for their pair. The important aspect for our investigation is that players are faced with differing values of the safe alternative to the lottery. A player having a small safe alternative may be thought of as having fewer opportunities than a player having a high safe option. However, even in this case, such a manipulation of initial opportunity does not bring about the expected results: Players do not in fact react to the variation in their respective initial opportunities.

The results coming from these two papers seemingly imply that distribution of initial opportunity does not matter much to people - at least in an experimental context - whereas considerations of merit (in Krawczyk's (2010) paper), or the actual choice of how much risk one wants to take (in Cappelen *et al*'s (2010) paper) are much more relevant for "pro-social" individuals. We believe, though, that the negative result emerging from these two papers may be due to the choice of a within-subject design, which may dilute the differences across conditions. It is also possible that the information over initial opportunities was not salient enough to subjects, partly because of the existence of other experimental manipulations, partly because the parameter range variation was not extreme.

In order to eliminate these possible causes of concern and magnify the differences across decisions, we revert to a between-subject approach, and we consider extreme values in the possible dispersion in initial opportunities. In fact, our baseline case consists of players having even chances, whereas at the other extreme a treatment assigns all chances to one player and no chances to the other. Alike Suleiman (1996) and Handgraaf *et al.* (2008), we manipulate fairness parameters from the extremes to the interior of the relevant interval in order to examine in detail the impact of fairness variations. Similarly to BBO we use receivers' rejection rates of a given offer as a way to measure the degree to which a certain procedure is deemed as unfair. The existing models of procedural fairness predict that changes in initial chances should make no difference to players' behaviour. The reason is that they only consider procedures falling within the players' strategy sets, or affecting final outcomes. On the contrary, our conjecture is that individuals may be sensitive to the distribution of relative chances even when these determine initial positions.

3 Experimental design and hypotheses

The game tree of the stage game is displayed in Figure 1. 10 GBP are at stake in every round. Two players, named Player 1 and Player 2, are matched to play an extended version of a UG. Players simultaneously make a proposal, which is a division of the pie, to their counterpart. Formally, a proposal by player i is a division $(x_i, 10 - x_i)$ where x_i is the amount player i demands for herself and $10 - x_i$ is the residual being offered to her counterpart, $i \in \{1, 2\}$. Players do not know the counterpart's proposal. After the proposals $x_1$and x_2 have been submitted, one of the two is selected at random. The key aspect of the design is that treatments differ according to the probability with which proposal 2 (1) is randomly selected. This is given by the probability p $(1 - p)$. Such probability has a maximum (minimum) of $p = 0.5$ for Player 2 $(1 - p = 0.5$ for Player 1) in the 50% treatment, it goes down (up) to $p = 0.2$ $(1 - p = 0.8)$ in the 20% treatment, it goes further down (up) to $p = 0.01$ $(1-p = 0.99)$ in the 1% treatment, and finally reaches a minimum of $p = 0$ $(1 - p = 1)$ in the 0% treatment. In the 0% treatment we dispense Player 2 from submitting a proposal, as this would have no possibility of being selected. Both Suleiman (1996) and Handgraaf *et al.* (1998) follow a similar strategy in *not* asking players to perform an action when this has a 0% probability of being relevant to the game.

At the top node of the decision tree, people are informed of whether they are Player 1 or 2, so they know from the outset whether they are favoured or not. For this reason we have also named Player 1 and Player 2 "(1–p)-player" and "p-player", respectively. After Nature makes the draw and selects the proposal, the interaction is exactly like a standard UG. Suppose it is *i's* proposal that is selected. Then player i becomes the proposer of the UG in the bottom part of the game tree, and player j becomes the receiver. Player i is informed that her offer has been selected, but does not receive any information about j's offer. Conversely, x_i is communicated to player j, who has to either accept or reject the proposal. Should player j accept, payoffs are x_i and $1-x_i$ for player i and player j respectively. Should player j reject, both players' payoff is 0. All random draws were made by the computer.

Apart from the 50% baseline case where opportunities to become the proposer are even, in the other three treatments Player 1 always has higher opportunities to be selected as the proposer of the game. We thus refer to Player 1 (2) as the favoured (unfavoured*)* player. Note that for a consequentialist agent all treatments are strategy equivalent. The NE for rational payoff-maximisers is, as in standard UGs, the proposer obtaining the highest possible allocation consistent with making the responder willing to accept and the responder obtaining the residual. That is, the only SPNE is $(10 - \varepsilon, \varepsilon)$[8],

[8] Since players were allowed to make offers up to the second decimal digit, $\varepsilon = 0.01$ in

for all p.

Subjects played the game described above anonymously for 20 rounds with random re-matching at the beginning of each round. Payoffs were given by the outcomes of two randomly-selected rounds out of the 20. Random payments were done partly to limit income effects as the play went on, partly to minimise the profitability of dynamic strategic behaviour such as rejecting with higher frequency in the early stages of the game to induce counterparts to offer more at later stages. We preferred to pay subjects for two rounds instead of just one because we feared that a payment based on only one round, coupled with the relatively low show-up fee (5 GBP), may have discouraged receivers from rejecting unfair proposals. After each round each pair was informed of the outcome of the interaction. No information about the outcome of the other pairs' interactions was instead released. The experiment instructions are reported at the end of the paper.

We want to test the following hypotheses. First, a reasonable assumption, in line with BBO's finding and the survey results reported in 1, is that the higher the fairness of the initial lottery - i.e. the less unbiased the distribution of initial chances, the more acceptable a certain earnings allocation. We thus posit our first hypothesis, which we call the "Monotonic Fairness Hypothesis":

H1a: The more biased the initial lottery, the lower responders' acceptance rate.

If proposers correctly anticipate this behaviour, then we can also posit that:

H1b: The more biased the initial lottery, the lower proposers' demands.

Whilst BBO compare only two extremely different lotteries in their second study, our setting enables us to study a finer range of distribution of chances within lotteries. We are particularly interested in testing for Nozick's prediction that individuals are highly sensitive to the symbolic value of actions. Nozick argues that individuals attach value to the possibility of expressing their own individuality through actions, where this power of expression magnifies the utility intrinsic to the action. As Nozick (1994: 27.28) puts it: "Having a symbolic meaning, the actions are treated as having the utility of what they symbolically mean [...] Since symbolic actions often are expressive actions, another view of them would be this: the symbolic connection of an action to a situation enables the action to be expressive of some attitude, belief, value, emotions or whatever. Expressiveness not utility is

our experiments.

what flows back." Furthermore, Nozick's citation reported in the introduction implies that the individual's value metric over the probability space may not be linear, and may suffer "discontinuities" in the origin of the space, i.e. when we move from full certainty to even limited uncertainty. Accordingly, our conjecture is that the act of making a proposal with a sheer 1% chance of it being relevant, may symbolize, for the unfavoured player, expressive value independently of the intrinsic expected utility coming from having this option. This power of "voice" (Anand, 1991; see also discussion in section 9) may give the individual what Nozick calls an "expressiveness", that is, a source of "value" that goes beyond the mere utility associated with the act itself.

If this were true, we should expect a significant difference in behaviour between the treatment where a player has a mere 1% chance of acquiring the proposer's role to that in which she has no chance. We thus posit what we call the "Symbolic Opportunity Hypothesis":

H2a: Responders' acceptance rate decreases significantly in the 0% treatment in comparison to the 1% treatment.

H2b: Proposers demand significantly less in the 0% treatment in comparison to the 1% treatment.

Hence, the second hypotheses is consistent with the first hypothesis, claiming that even allowing a purely symbolic chance of acquiring the advantaged position in the UG has a significant effect in changing the fairness judgement of the resulting allocations.

We are also interested in testing for the impact of varying the allocation of opportunities over time. As mentioned in the introduction, the FRC setting entails a relevant procedural change vis-à-vis the VRC setting in that in FRC a player keeps the same role as either favoured or unfavoured throughout the whole 20 rounds. This is determined prior to all interactions taking place. Conversely, in VRC treatments players' positions are reassigned before each round. In both cases players are informed of the way roles are assigned prior to the beginning of the interactions. So, a player being assigned the unfavoured role in a FRC setting can see her fate as being sealed throughout the whole 20 rounds, whereas in the VRC setting a player who is unfavoured in the current round knows that she may be assigned the favoured role in each of the future rounds. Arguably, FRCTs are *less* fair from the procedural point of view than VRC treatments, because there is no possibility of reversing the initial role assignment[9]. Consistently with the above considerations, we

[9] One may object that opportunities are fairly distributed in FRC treatments because *before* the initial role assignment each player had an even chance of being assigned the advantaged position. Nevertheless, we believe that this observation does not actually affect

expect that procedural individuals will be sensitive to this difference, and will find the same allocation more acceptable when this has been generated within an overall fairer setting rather than a less fair one. We thus posit our third set of hypotheses, which we call "Dynamic Opportunities Hypothesis":

H3a: Receivers' acceptance rate decreases significantly in the FRC setting vis-à-vis the VRC setting.

H3b: Proposers demand less in the FRC setting vis-à-vis the VRC setting.

Finally, our setting also enables us to test the presence of learning, i.e. convergence, or nearing, the SPNE of the game. In spite of the initial negative results in Slonim and Roth (1996), subsequent studies have found some evidence that players behaviour's in repeated UGs tend to converge to the SPNE (List and Cherry, 2000; Armantier, 2006). We then test whether:

H4a: Receivers' acceptance rate increases over time.

H3b: Proposers demand more over time.

Experiments were conducted with a sample of 426 Warwick University undergraduate students, with an average of 60 students per treatment. Only subjects who had not been attending courses in Game Theory were allowed to participate. We ran three sessions per treatment. Due to varying show-up rates, the number of subjects per session was not constant across sessions but varied from a minimum of 16 to a maximum of 24 subjects, with an average of around 20 subjects per session. The analysis we present in the next sections is however robust to controlling for the number of subjects participating in each session. Each subject only participated in one session. We took care to balance the composition of the sessions in terms of gender and number of people enrolled in Economics and Psychology courses with respect to the total. Each session was organised according to the following procedures. Subjects were paid a show-up fee of 5 GBP upon their entering the experimental room, and were randomly assigned to a computer in the room. After instructions were administered, written comprehension quizzes were carried out. Subjects were asked to re-try answering the quizzes until they gave the correct answer. Subjects were then involved in the 20 interactions of the stage game. At the end of the decisions subjects completed a short questionnaire asking demographic and attitudinal questions, and finally received their payoffs. The whole session lasted around an hour. The average earnings - in addition to the show-up fee - was GBP8.22 . The game was conducted using the z-tree software (Fischbacher, 1999).

our statement. Because in FRC the outcome of the initial - and only - role assignment is extremely unequal - one player is favoured (unfavoured) for all 20 rounds. Conversely, in VRC treatments the outcome of the 20 different role assignments is undoubtedly *less* unequal, as the probability that a player ends up as favoured a certain number of times is given by a binomial distribution with parameters $(20; 1/2)$.The most likely outcome is thus that each player is assigned the favoured role half of the times.

4 Results for FRCTs

In the following two sections we analyse the baseline *50%* treatment in conjunction with FRCTs.

4.1 Analysis of Receivers' Behavior in FRC

4.1.1 Descriptive Analysis

Table 1 reports descriptive statistics for the relevant features of proposers and responders' behaviour in each treatment. First, we note that overall acceptance rate in our *0%_FRC* treatment is in line with what found in standard UGs, though at somewhat lower levels. In the meta-analysis by Oosterbeek *et al.* (2004), the weighted average acceptance rate from 66 UG studies is 84.25%, whereas it equals 77.58% in our study. Our *0%_FRC* is the treatment in our experiment that is closest to standard UGs, so we have some assurance that the behavioural patterns we observe in our study are not due to specific idiosyncrasies of our sample. Comparing the 50% treatment (Table 1a) and the FRCTs (Tables1 b, c, d) brings out the existence of a monotonic pattern consistent with *H1* and *H2.* First, as the bias of the initial lottery increases, both the mean and the median values of rejected demands decreases (see Tables 1a-d, Columns 1). This means that as the initial lottery becomes more biased, responders request larger shares of the pie to accept the offer.

Second, the acceptance rates of low offers decreases as the bias of the initial lottery increases (see Tables 1a-d, Columns 3). The acceptance rate for a treatment is the proportion of offers that were accepted over the total number of offers that were made. We consider a demand as "high" when the proposer demanded at least 80% of the pie for herself, thus leaving a share equal to or less than 20% to the responder. This is a somewhat arbitrary value but it is often taken as a reference point in the UG literature. It is worth noting that the drop in the acceptance rate for high demands is particularly pronounced between *FRC 1%* and *FRC 0%,* consistently with *H2a.* This monotonic pattern does not emerge in overall acceptance rate, but this is because the magnitudes of proposals varied considerably across treatments (see Tables 1a-d, Columns 2). The econometric analysis of the next section controls for this aspect.

Finally, Figure 2 offers a graphical representation of responders and proposers' behavior in each treatment by reporting histograms of proposals as well as acceptance rates for different classes of proposals. Here proposals have been grouped in intervals of length equal to 0.5 for all demands greater than five, whereas all demands smaller than five have been grouped in one category. It is evident that acceptance rates tend to decrease within each class as the initial lottery becomes more biased.

4.1.2 Econometric Analysis

We pool all observations coming from FRCTs and the *50%* treatment together. We model the repeated nature of the data with a random-effects model. Given the dichotomic nature of the receiver's variable, we fit the following logit models:

$$\begin{aligned} ACCEPTANCE_{i,t} &= \alpha_i + \tau CHANCE + \gamma_{i,t} OFFER + \\ &\quad + \delta_{i,t} FAVOURED + \theta_t ROUND_t + \lambda_i Z_i + u_i + \varepsilon_{i,t} \end{aligned} \qquad (1)$$

$$\begin{aligned} ACCEPTANCE_{i,t} &= \alpha_i + \beta_j TREATMENT_j + \gamma_{i,t} OFFER + \\ &\quad + \delta_{i,t} FAVOURED + \theta_t ROUND_t + \lambda_i Z_i + u_i + \varepsilon_{i,t} \end{aligned} \qquad (2)$$

The dependent variable is the dichotomic variable $ACCEPTANCE$ - where 1 (0) denotes acceptance (rejection) of a given proposal. In model 1 the key variable to test for *H1a* is *CHANCE*. This variable takes the value of p, that is, the probability for an unfavoured player to become the proposer. Hence, *CHANCE* offers a continuous measure of the bias in the initial lottery. Furthermore, we control for the size of the offer assigned to the receiver through the variable *OFFER*. The probability of acceptance is likely to be highly correlated with the size of the offer, which in turn may vary considerably across treatments (see next section). The dummy variable *FAVOURED* identifies the interactions where a subject being in the favoured role was later drawn as receiver of the game. Obviously this only applies to the *1%* and the *20%* treatments. It may be the case that subjects initially drawn as favoured formed higher earnings expectations for that particular round than if they were drawn as unfavoured. Or maybe fairness concerns due to their awareness of being overall more likely to occupy the proposer role made them more lenient to accept offers when occupying the receiver role. Having a higher initial 'reference point' in terms of expected earnings may affect the likelihood with which they subsequently accepted offers. Z_i is a set of individual characteristics that may affect propensity to accept. Z_i includes a subject's age and gender, and two dummy variables identifying UK citizenship and enrolment in economic degrees. Introducing Z_i considerably reduces the number of observations due to missing questionnaire answers, so we shall report regression results with and without Z_i. $ROUND$ dummies are also included to control for time trend effects of effects associated with specific rounds. Finally, u_i and $\varepsilon_{i,t}$ are individual-specific and observation-specific error terms. The indexes i and t denote the individual and the round of the interaction, respectively. Hence, $i = 1 \ldots N$, and $t = 1 \ldots 20$. Note that all regressors apart from the dummy variable identifying attendance of economic degree are exogenous. Hence, in regressions not including Z_i the individual-specific effects α_i must be uncorrelated with the other regressors, thus ensuring that a between estimator is consistent.

Model (2) keeps the same specification as (1) apart from replacing *CHANCE* with dummy variables identifying individual treatments. We introduce three dummy variables identifying each treatment, leaving the *50%* treatment as the baseline. The index j of the variable $TREATMENT$ denotes observations coming from a given treatment. This enables us to study the differential effects of pairs of treatments on propensity to accept, thus testing directly *H2a* as well as performing a more stringent test of *H1a.*

Regression results are reported in Table 2. Column 1 reports results for model (1) without including demographic controls Z_i. *CHANCE* has a strong and positive effect, thus supporting *H1a*. For instance, the predicted probability of accepting an offer equal to 20% of the pie shifts from 0.95 when $CHANCE = 0.5$.to 0.75 for $CHANCE = 0.2$, and to 0.48 for $CHANCE = 0$. Thus, receivers clearly reacted to procedural fairness in the initial lottery, and were more likely to accept offers when these came after a less unbiased initial lottery. $OFFER$ has a positive and strong effect, as expected. $FAVOURED$ also has a positive sign and is significant at the 5% level. Favoured subjects were more likely to accept offers when drawn as receivers than unfavoured subjects. As conjectured above, this may be due to fairness considerations[10].

The specification in the second column adds the demographic and individual characteristics measures Z_i. $CHANCE$ keeps a strong and positive effect in this specification, too, thus confirming the validity of H1a. $OFFER$ and $FAVOURED$ also show positive and significant effects. Among the demographic variables, it is interesting to note that attending Economics degrees significantly increases probability of acceptance - this is so at the 5% level. Hence, similarly to other experiments, Economics students show patterns of behaviour closer to 'Homo Economicus' (Marwell and Ames, 1981). Moreover, $GENDER$ also exerts significant effects. Women are significantly more inclined to accept offers than men. This is in line with Eckel and Grossman

[10]Note that this result is mainly driven by behaviour in *20%_FRC* because there were only four observations in which a favoured subject was drawn as receiver in *1%_FRC.* Qualitatively similar results to those illustrated hold when these four observations are omitted.

(2001). UK students were significantly more likely to accept offers, *ceteris paribus,* than foreigners. Age did not exert significant effects.

Table 2, Column 3, reports the results for model (2) without including individual characteristics Z_i. Figure 3 reports the probabilities of acceptance in each treatment for various offers, as predicted by this model. $ROUND$ has been set to equal the last interaction, and $FAVOURED$ is set at the mean value of the sample. The horizontal axis reports point values for offers ranging from 5% to 35% of the pie. The estimated probability of acceptance for each treatment is reported on the vertical axis. The diagram shows that for any offer, as the bias of the initial lottery decreases, the probability of acceptance increases. Probabilities are close to 0 (1) for the lowest (highest) offer considered. For the other intermediate offer values, sizable differences emerge across treatments. For instance, for offers equal to 20% of the pie, the predicted probability of acceptance is equal to 0.92 in the baseline case, drops to 0.85 in the 20%_FRC and to 0.69 in the 1%_FRC, and drops to a mere 0.17 in the 0%_*FRC.* For offers equal to 15% of the pie, the predicted probability of acceptance is equal to 0.69 in the baseline case, drops to 0.51 in the 20%_FRC and to 0.28 in the 1%_FRC. Finally, it goes down to 0.03 in the 0%_*FRC.* Clearly, differences are particularly pronounced comparing the 0%_FRC vis-à-vis other treatments, but are relevant in other treatments, too.

Table 3 reports the z-statistics and standard errors of two-tailed Wald tests over the null hypothesis Ho: $\beta_k - \beta_l = 0$ against H$_1$: $\beta_k - \beta_l \neq 0$, for each pair of $TREATMENT$ coefficients. β_k (β_l) is the row (column) entry. This is done for model (3), but results for model (4) are virtually identical. Note that a positive (negative) sign for the z-statistic means that probability of acceptance was higher (lower) in treatment k (row entry) than in treatment l (column entry). Table 3 supports hypothesis *HP2a* of a symbolic value of opportunity. The difference between $\beta_{0\%\_FRC}$ and $\beta_{1\%\_FRC}$ is negative and significant ($P - value = 0.017$). Receivers in 1%_FRC had, *ceteris paribus,* a significantly higher probability of accepting a given offer than receivers in the 0%_FRC Hence, an assignment of even minimal opportunities seems to matter a great deal for receivers of FRCTs. As for all other comparisons, the difference between $\beta_{0\%\_FRC}$ and the other two treatments coefficients is negative and significantly different to 0 at less than the 1% level. The difference between $\beta_{1\%\_FRC}$ and both $\beta_{50\%}$ is also significantly different from 0 ($P = 0.049$). The differences between $\beta_{1\%\_FRC}$ and $\beta_{20\%\_FRC}$ and $\beta_{20\%\_FRC}$ and $\beta_{50\%}$ are not large enough to reach significance level, but it is noteworthy that all the signs in the Table are negative and thus in line with *HP1a.* Overall, receivers of FRCTs seem to react to the increased bias of the initial lottery, with a large portion of the differences being driven by the sizable reduction in the frequency of acceptance in $\beta_{0\%\_FRC}$ compared to all other treatments and $\beta_{1\%\_FRC}$ in particular.

Very similar results in the estimation of the β coefficients obtain using Model (4) of Table 2. It is noteworthy that individual characteristics bring about the same effects as those stressed for Model (2). The only difference is that $FAVOURED$ drops out of the significance region. We can thus conclude:

Conclusion 1 *Descriptive, graphical and econometric analysis supports HP1a.*

Proof: Descriptive statistics and graphical analyses show the existence of a negative relationship between the bias of the initial lottery and (a) the mean and median value of rejected demands; (b) the acceptance rate of high demands. Econometric analysis shows a strong and positive effect of the variable $CHANCE$ over probability of acceptance. Pairwise comparisons between treatment dummy coefficients all have the correct sign and are statistically significant in four out of six cases.

Conclusion 2 *Descriptive, graphical and econometric analysis supports HP2a.*

Proof: Descriptive statistics and graphical analyses show (a) lower mean and median values of rejected demands; (b) lower acceptance rate of high demands, in the *0%_FRC* compared to the *1%_FRC.* Econometric analysis shows a significant difference at the 5% level between $\beta_{0\%\_FRC}$ and $\beta_{1\%\_FRC}$.

4.2 Proposers Behavior in FRC

4.2.1 Descriptive Analysis

Similar patterns to those observed above for receivers' behaviour are found for proposers' behaviour. Again, the average proposal in the *0%_FRC* treatment is seemingly not too dissimilar from what found in other UGs. Oosterbeek *et al.* (2004) report average offers equal to 59.5% of the pie from 75 UG experiments, whereas this is equal to 62.8% in our study. As the initial lottery becomes more biased, both the mean and median proposals of favoured proposers decrease (see Tables 1a-d, Columns 4), with the only exception of *50%* and *FRC_20%* where the median remains constant as we move from the former to the latter. The drop in proposals is particularly pronounced between *1%_FRC* and *0%_FRC,* consistently with *H2b.* Moreover, the frequency of high demands decreases with the bias of the initial lottery.

Furthermore, the same monotonic pattern emerges between *20%_FRC* and *1%_FRC* with respect to non-favoured proposers (see Tables 1a-d, Columns 5). Distinguishing between favoured and unfavoured proposers in these two treatments is necessary, as the difference in initial opportunities may influence their behaviour. It is interesting to note that unfavoured proposers demanded more than their favoured counterparts in both *20%_FRC* and *1%_FRC.* This difference is statistically significant in the *20%_FRC* according to a Mann-Whitney test at a significance level of less than 1% ($z = -5.620$; $P < 0.001; n = 620$), whereas it is not distinguishable from 0 in *1%_FRC* ($z = 0.19$; $P = 0.85; n_1 = 640$). Perhaps unfavoured proposers tried to compensate for having been discriminated against in the initial lottery by demanding a larger share of the pie. This would be consistent with attempting to minimise the expected earnings between favoured and unfavoured proposers. We return to this point below. However, the difference between favoured and unfavoured proposers is not statistically significant in *1%_FRC.* Moreover, Figure 2 shows that the distribution of proposals tends to become more skewed towards the left as the chances of the non-favored player decrease.

4.2.2 Econometric Analysis

Even in this case we model the longitudinal characteristic of the data using a random effects model. We consider the following models, analogous to models (1) and (2) of section 4.1.2:

$$\begin{aligned} DEMAND_{i,t} &= \alpha_i + \tau CHANCE + \delta_{i,t} UNFAVOURED + \\ &\quad + \theta_t ROUND_t + \lambda_i Z_i + u_i + \varepsilon_{i,t} \end{aligned} \tag{3}$$

$$\begin{aligned} DEMAND_{i,t} &= \alpha_i + \beta_j TREATMENT_j + \delta_{i,t} UNFAVOURED + \\ &\quad + \theta_t ROUND_t + \lambda_i Z_i + u_i + \varepsilon_{i,t} \end{aligned} \tag{4}$$

The dependent variable $DEMAND$ is how much proposers demanded for themselves. In model (3), the main independent variable is *CHANCE* (see section 4.1.2). Moreover, all controls included in model 1, suitably adjusted, are included here, too. In particular, *UNFAVOURED* identifies subjects who were in Player 2 role. The error structure includes an individual-specific error term u_i, and an observation-specific error term $\varepsilon_{i,t}$. To prevent the risk of error heteroschedasticity, we use robust estimates of the variance-covariance matrix of the estimator, with errors clustered on individuals (Froot, 1989). Clustering makes it possible to treat errors as independent across decisions from different individuals, and arbitrarily correlated for decisions made by the same individual[11]. Model (4) substitutes treatment dummies for the *CHANCE* variable in model (3).

The results of the regression are reported in Table 4. Columns (1) and (2) show that $CHANCE$ has a positive and strongly significant effect, thus supporting *HP1b*. Hence proposers' behaviour, too, reacted markedly to the degree of unbiasedness in the initial lottery. The higher the unbiasedness of the initial lottery, the higher proposers' demands. A weak effect for $UNFAVOURED$ also emerges. Unfavoured players, knowing they had a low probability of being selected as proposers, may have been less attentive in their choices, or they may have applied different patterns of behaviour compared to favoured players. For instance, they might have sought to compensate for their overall disadvantaged position by demanding more than favoured players. In line with what observed for receivers, Economics students demanded significantly more than students from other degrees. The other demographic variables are not significant predictors of $DEMAND$.

Specification introduces treatment dummies. Table 5 reports in each cell *(k,l)* the z-statistics and the standard error for Wald tests over the null hypothesis Ho: $\beta_k - \beta_l = 0$ against H$_1$: $\beta_k - \beta_l \neq 0$,where β_k is the row entry and β_l the column entry. Table 5 supports *HP2b* relative to the symbolic value of opportunity. The difference between $\beta_{FRC\_0\%}$ and $\beta_{FRC\_1\%}$ is negative and significant ($P = 0.024$). Table 5 also supports hypothesis *HP1b* postulating a monotonic relationship between the fairness of the initial lottery and proposers' demands. All the coefficient signs are in accord with this hypothesis, apart from $\beta_{20\%\_FRC}$-$\beta_{50\%}$ where the sign is positive but the coefficient is indistinguishable from 0. In all of the other five comparisons, the lower the initial opportunity for the unfavoured player, the lower on average the proposals in that treatment. The difference across treatments is significant at the 1% level in three out of the six comparisons, and significant at the 5% level in two other comparisons. Specification 4 brings about similar results to Specification 2 as far as β coefficients are concerned, and confirms the same individual characteristics effects found in Specification (2).

Figure 2 also allows us to examine whether offers made by proposers were those maximising expected earnings, given the acceptance rates for each treatment. The dotted line marked with asterisks shows the expected earnings for each proposal category, given by the product of the acceptance rate for that category and the lowest extreme of the interval. One can notice that in two cases out of four, the mode of the proposal lies in the same category as the payoff maximising proposal. In the *0%* _FRC the maximum

[11] Note that clustering on sessions instead of individuals would be inappropriate because a necessary conditions for the validity of cluter-robust standard errors is that the number of clusters tend to infinity. Given the relatively small number of sessions, this assumption would not be satisfied. See Woolridge (2002).

is in the adjacent category and is very close to the payoff maximising category. Only in the 1%_FRC can a sizable difference be detected. Apart from this case, proposers' behaviour seems to have converged towards the income-maximising proposal.

On the grounds of this analysis, we conclude:

Conclusion 3 *Descriptive, graphical and econometric analysis supports HP1b.*

Proof: Descriptive statistics and graphical analyses show the existence of a negative relationship between the bias of the initial lottery and the mean and median proposals, the only exception being 50% and $20\%\_FRC$ where proposals are very close. Econometric analysis shows a strong and positive effect of the variable *CHANCE* over demands as the lottery bias decreases. Differences between treatment dummy coefficients all have the correct sign and are statistically significant in five out of six cases.

Conclusion 4 *Descriptive, graphical and econometric analysis supports HP2a.*

Proof: Descriptive statistics and graphical analyses show lower mean and median demands in the *0%_FRC* than in the *1%_FRC.* Econometric analysis shows a significant difference at the 5% level between $\beta_{0\%\_FRC}$ and $\beta_{1\%\_FRC}$.

5 The Variable Role Condition

As illustrated in section 3, VRCTs have the same structure as FRCTs except for the fact that in VRC a random assignment to the favoured or unfavoured role occurs in each round, whereas in FRC players' roles are fixed by one random draw prior to play. In this section we analyse jointly VRCTs and the *50%* treatment.

5.1 Analysis of Receivers' Behavior in VRC

5.1.1 Descriptive Analysis

Table 1 shows that the monotonic pattern linking bias in the initial lottery and rejection rates still holds moving from *0%_ VRC* to *20%_ VRC,* but is reversed between *20%_ VRC* and *50%.* Mean and median values of rejected demands increase from *0%_ VRC* through *20%_ VRC,* but subsequently drop when moving from *20%_ VRC* to *50%.* Hence, receivers' hostility decreases between *0%_ VRC* up to *20%_ VRC,* but then it rises again (see Tables

1a,e-g, Columns 1). A similar trend can be detected with respect to the acceptance rate of 'low' offers (see Tables 1a, e-g, Columns 3).

Figure 2 shows a noticeable drop in acceptance rates for high demands in *0%_ VRC,* whereas acceptance rates remain high (i.e. higher than 80%) for a larger class of proposals in the *20%_ VRC* than in other treatments. Figure 2 also bears out that the mode of proposals did not generally coincide with the class with higher expected earnings, although the latter are generally situated in an adjacent class.

5.1.2 Econometric Analysis

We fit models analogous to (1) and (2) to analyse receivers' behaviour in VRCTs (see section 4.1.2). The results are reported in Table 6. The main result of specification (1) is that, contrary to what occurred for FRCTs, the variable $CHANCE$ is no longer significant (Table 6, column 1). Clearly the inversion of the monotonic trend between *20%_ VRC* and *50%* already observed in the descriptive statistics is what prevents this variable to be a significant predictor of acceptance rates. Specification (2) shows instead a sizable increase of the coefficient relative to *CHANCE*, which is now weakly significant (Table 6, column 2). However, we interpret this result with caution. We think this result is mostly due to the treatment *20%_ VRC* being particularly affected by missed observations in individual characteristics. The elimination of the observations in this treatment clearly attenuates the weight of the very treatment causing the break in monotonicity[12].

As for the individual controls, $ECONOMICS$ is not significant this time, but the sign is consistent with what found in section 4.1. $GENDER$ is again a significant predictor of receivers' behaviour, with women more likely to accept offers. No significant difference emerges between UK students and foreigners, and AGE is also insignificant. To check whether *HP1a* may hold limitedly to a portion of the relevant interval, we add a squared term for the variable $CHANCE$. The resulting specifications (3) and (4) show that both the linear term and the quadratic term have indeed significant effects, particularly in specification (3) (see Table 6, columns 3 and 4). Probability of acceptance reaches a maximum for $CHANCE = 0.27$ in specification 3 and 0.30 in specification 4. Hence, $HP1a$ appears to be supported within a limited region of the interval.

[12] In fact, using specification (1) on the same observations available for specification (2) has the effect of increasing the coefficient of CHANCE to 2.15, which is nearly two thirds of the overall increase of CHANCE. Therefore, we conjecture that about two thirds of the increase in the CHANCE coefficient is due to sample omissions and only one third to the introduction of demographic controls.

Specification (5) replaces $CHANCE$ with treatment dummies, as per model 2. Figure 4 depicts the predicted probability as per specification (5), estimated in the last round of interaction at the mean value for $FAVOURED$ (see Table 6, column 5). The main feature of this diagram is that the three treatments $0\%\_VRC$, $1\%\_VRC$, $20\%\_VRC$, do follow a monotonic trend. For instance, for offers equal to 10% of the pie, the predicted probability of acceptance is equal to 0.16 in the 0%_VRCT, and it rises to 0.40 in the $1\%\_VRC$, and to 0.69 in $20\%\_VRC$. However, rather than following the monotonic trend, the probability of acceptance in *50%* drops to 0.37, even further below than the predicted probability in $1\%\_VRC$. For offers equal to 15% of the pie, the predicted probability of acceptance is equal to 0.45 in the 0%_VRCT, and it rises to 0.74 in the $1\%\_VRC$, and 0.90 in $20\%\_VRC$. Even in this case, the probability of acceptance in *50%* drops to 0.91, below the $1\%\_VRC$ level of predicted probability.

Table 7 reports the results of Wald tests for the null hypothesis H_0: $\beta_k - \beta_l = 0$ against the hypothesis H_1: $\beta_k - \beta_l \neq 0$. *HP1a* appears to be supported limitedly to the three treatments *0%_ VRC, 1%_ VRC,* and *20%_ VRC.* All the signs of the z-statistics are negative, and thus consistent with the idea that less unbiasedness in the initial lotteries is associated with higher acceptance rates. The difference is weakly significant between *1%_ VRC* and *20%_ VRC* (P = 0.078), and strongly significant between *0%_ VRC* and *20%_ VRC* (*P<0.01*). This analysis confirms that responders in *50%* did not behave in accordance to *HP1a*. The signs are contrary to the hypothesis for both $\beta_{50\%} - \beta_{1\%\_VRC}$ and $\beta_{50\%} - \beta_{20\%\_VRC}$, and the test even results in a weakly significant difference in the latter case ($P = 0.051$). Finally, the test returns a weakly negative significant difference for $\beta_{50\%} - \beta_{0\%\_VRC}$ (P = 0.084). As far as *HP2a* of a symbolic value of opportunity is concerned, Table 7 shows support for the hypothesis, albeit weakly. The difference between $\beta_{0\%\_VRC}$ and $\beta_{1\%\_VRC}$ is negative and significant at the 10% level ($P = 0.063$). Even in VRCTs, giving players a mere 1% of having their proposal selected raises the probability of acceptance in comparison to giving no chance at all, though less markedly than in FRCTs. Specification 6 brings about qualitatively similar results to specification 5 as far as treatment dummies are concerned, and to specifications (2) and (4) with respect to demographic variables (see Table 6, column 6).

On the grounds of this analysis, we conclude:

Conclusion 5 *Descriptive, graphical and econometric analysis supports HP1a only limitedly to the* $0\%\_VRC$ *through* 20%_ *VRCTs. The monotonic pattern breaks between* $20\%\_VRC$ *and* 50%.

Proof: Descriptive statistics and graphical analyses show the existence of a negative relationship between the bias of the initial lottery and (a) the mean and median value of rejected demands; (b) the acceptance rate of high demands, between $0\%\_VRC$ and $20\%\_VRC$. The opposite occurs between $20\%\_VRC$ and 50%. Econometric analysis shows no effect for the variable *CHANCE* over probability of acceptance, but a strong effect when a linear and a quadratic term are both included. The estimate suggests a maximum around $p = 0.30$. All pairwise comparisons between $0\%\_VRC$ through $20\%\_VRC$ have the correct signs and are statistically significant (weakly in two cases). Conversely, the difference $\beta_{50\%} - \beta_{20\%\_VRC}$ has the wrong sign and is (weakly) statistically significant.

Conclusion 6 *Descriptive, graphical and econometric analysis supports HP2a.*

Proof: Descriptive statistics and graphical analyses show (a) lower mean and median values of rejected demands; (b) lower acceptance rate of high demands, in the *0%_ VRC* than in the *1%_ VRC.* Econometric analysis shows a significant difference at the 10% level between $\beta_{0\%\_VRC}$ and $\beta_{1\%\_VRC}$.

5.2 Proposers Behavior in VRC

5.2.1 Descriptive Analysis

A pattern similar to what observed for receivers holds for proposers in VRCTs (see Tables 1a,e-g, Columns 4). As far as being favoured in the lottery is concerned, a striking difference between VRCTs and FRCTs is given by the fact that unfavoured proposers demand *less* than favoured proposers. This is the case for both *20%_ VRC* and *1%_ VRC.* Although the difference is statistically significant in the first case ($z = 1.884$; $P = 0.06; n = 600$) but not in the latter ($z = 1.240$; $P = 0.22; n_1 = 560$). Hence, the intuition that unfavoured proposers may demand *more* to compensate for their being disadvantaged in their chance to access the proposer role clearly does not hold in this case. This hints at the possibility that players may have considered the VRC procedure less unfair than the FRC procedure. We return to this point in section 7.2. Likewise, Figure 2 shows that the distribution of proposals is clearly more skewed towards the right in the *20%_ VRC* than in the remaining treatments.

5.2.2 Econometric Analysis

Table 8 reports the results of regressions using models (3) and (4) applied to VRCTs and *50%* (see section 4.2.2). This analysis confirms the patterns observed for responders behaviour. $CHANCE$ is not significant in either specification (1) or (2) (see Table 8, columns 1-2), but the inclusion of a quadratic term makes both coefficients significant predictors of proposers' behaviour (see Table 8, columns 3-4). The models predicts the maximum to be reached at $CHANCE = 0.27$ (specification 3) and $CHANCE = 0.29$ (specification 4). Hence, *HP1b* seems to hold for a region of similar size to what found for responders' behaviour. The analysis of Wald tests over differences in β confirms the validity of *HP1b* limitedly to the three treatments *0%_ VRC, 1%_ VRC,* and *20%_ VRC*. The difference between $\beta_{20\%\_VRC}$ and $\beta_{1\%\_VRC}$ is not large enough to reach significance levels ($P = 0.22$), but $\beta_{0\%\_VRC}$ is significantly smaller than both $\beta_{20\%\_VRC}$ ($P < 0.01$) and $\beta_{1\%\_VRC}$ ($P < 0.01$). *HP2b* relative to a symbolic value of opportunity for UG proposers is thus strongly supported in VRCTs. Conversely, $\beta_{20\%\_VRC}$ is significantly greater than $\beta_{50\%}$ ($P < 0.01$), thus reverting the previous trend, whereas the hypothesis that $\beta_{1\%\_VRC}$ is the same as $\beta_{50\%}$ cannot be rejected ($P = 0.15$) and $\beta_{0\%\_VRC}$ is significantly smaller than $\beta_{50\%}$ ($P < 0.01$).

Even in this case, specification (6) controlling for individual characteristics brings about qualitatively similar results to specification (5) (see Table 8, columns 5-6). It is noteworthy that unfavoured proposers demand significantly *less* than favoured ones, this result being strongly significant in specifications (1), (3), and (5), and weakly significant in Specification (6). This result is in contrast with what found in FRCTs. No effect for individual characteristics can be detected. We thus conclude:

Conclusion 7 *Descriptive, graphical and econometric analysis supports H1a only limitedly to the* $0\%\_VRC$ *through* $20\%\_$ *VRCTs. The monotonic pattern breaks between* $20\%\_VRC$ *and* 50%.

Proof: Descriptive statistics and graphical analyses show the existence of a negative relationship between the bias of the initial lottery and the mean and median proposals limitedly to the $0\%\_VRC$ through 20%_VRCTs, but the trend is reversed between 50% and $20\%\_VRC$. Econometric analysis shows a strong effects for the linear and the quadratic term of *CHANCE,* with a maximum being reached around $p = 0.30$. All pairwise comparisons between $0\%\_VRC$ through 20%_VRCTs have the correct signs and are statistically significant in two out of three cases. Demands are significantly higher in $20\%\_VRC$ than 50%.

Conclusion 8 *Descriptive, graphical and econometric analysis supports H2a and H2b.*

Proof: Descriptive statistics and graphical analyses show lower mean and median demands in the *0%_ VRC* than in the *1%_ VRC.* Econometric analysis shows a significant difference at the 1% level between $\beta_{0\%\_VRC}$ and $\beta_{1\%\_VRC}$.

6 Results for First Round

The random effects model used in the foregoing sections is a popular method to analyse experimental data coming from repeated interactions (see e.g. Dickinson, 2000; Armantier, 2006; Gächter and Thöni, 2010). However, it is plausible that as interactions went on, subjects adapted their strategies to the feedback received at the end of each interaction. In particular, it is quite likely that proposers updated their beliefs over responders' minimum acceptable offer on the basis of their past experience, and modified their demands accordingly[13]. It is then interesting to investigate the priors that agents had *before* such feedback were received. Is the monotonic behaviour that we observed over the whole experiment something that subjects - proposers in particular - anticipated right from the start? To answer this question we analyse subjects' behaviour in the first round of interactions.

Table 10 reports descriptive statistics for the first round of interactions only. Although paucity of observations prevent us from drawing firm conclusions, patterns are striking similar to what we observed across the whole 20 rounds. In fact, proposals and rejected demands in the first round of FRCTs present an *identical* pattern to that emerging in the whole 20 rounds as far as mean values are concerned, whereas there is one deviation from this pattern for median values. This means that in the first round receivers participating in more biased initial lotteries gave proposers a harsher treatment than receivers participating in less biased initial lotteries, and that proposers correctly anticipated this. However, acceptance rates of small offers do not match the pattern observed across the 20 rounds. In VRCTs the pattern in the first round is the same as that observed over the whole 20 rounds, except that the *VRC_1%* treatment lies above the *VRC_20%* in terms of mean and median of rejected demands, and mean and median of proposals. Figure 5 depicts mean proposals in each round. It is noteworthy that FRCTs follow a monotonic pattern consistent with *H1b*, and a sizable gap between *FRC_0%* and *FRC_1%* consistent with *H2b* exists.

We also use econometric specifications analogous to those applied earlier. The removal of the time component calls for estimation with a logit model for acceptance and an OLS estimator for proposals. Moreover we drop controls for individual characteristics Z_i to save degrees of freedom and avoid missing observations. The econometric specifications we use are thus as follows:

[13] Indeed, in unreported analyses some variables defined over a subject's history in the game (such as the number of demand having been rejected to a proposer) turn out as strong predictors of future behaviour for proposers. Not surprisingly, past rejections have a negative impact on subjects' demands.

$$ACCEPTANCE_{i,1} = \alpha_i + \tau CHANCE + \gamma_{i,1} OFFER + \\ +\delta_{i,1} FAVOURED + u_i + \varepsilon_{i,1} \quad (5)$$

$$ACCEPTANCE_{i,1} = \alpha_i + \beta_j TREATMENT_j + \gamma_{i,1} OFFER + \\ +\delta_{i,1} FAVOURED + u_i + \varepsilon_{i,1} \quad (6)$$

$$DEMAND_{i,1} = \alpha_i + \tau_1 CHANCE + \\ +\delta_{i,1} UNFAVOURED + u_i + \varepsilon_{i,1} \quad (7)$$

$$DEMAND_{i,1} = \alpha_i + \beta_j TREATMENT_j + \\ +\delta_{i,1} UNFAVOURED + u_i + \varepsilon_{i,t} \quad (8)$$

Clearly the sharp decrease in the number of observations considerably reduces the power of statistical tests in comparison to regressions conducted on the whole sample. All the same, we find clear similarities to the patterns detected over the whole 20 rounds. This is clearer in proposers' behaviour. As shown in Table 14 (column 1), the variable *CHANCE* has a positive and significant effect in FRCTs. The pairwise comparisons of treatment coefficients from Model 2 (see Table 14, column 2), shows that the sign of coefficient differences is always negative, consistently with *HP1b* (See Table 15). That is, the higher the bias of the initial lottery, the lower the proposals. In two cases such differences are small, but in four out of the six comparisons the differences are statistically significant. This is so when comparing *20%_FRC* with *1%_FRC* ($P = 0.098$), *20%_FRC* with *0%_FRC* ($P = 0.027$), *50%* vis-à-vis *1%_FRC* ($P = 0.076$) and *50%* vis-à-vis *0%_FRC* ($P = 0.019$). *HP2b* is instead not supported in this case.

In the VRCT, *HP1b* cannot be supported because proposals are highest in *1%_ VRC,* so *CHANCE* is no longer significant (see Table 14, column 3). Nevertheless, proposals in *0%_ VRC* are considerably lower than in all other treatments (see Table 14, column 4, and Table 16). The difference is strongly significant with respect to *1%_ VRC* ($P = 0.009$), thus clearly supporting *HP2b* (See Table 16). Differences are also significant comparing *0%_ VRC* to *20%_ VRC* ($P = 0.025$) and *0%_ VRC* to *50%* ($P = 0.012$). However, beyond this point the differences are not consistent with a monotonic pattern, and they are in fact far from the conventional region of hypothesis rejection.

As already pointed out when commenting descriptive statistics, proposers' behaviour mirrors responders' actions. The pattern of signs in the acceptance Tables mirrors that in the proposals Tables. This means that variations in proposers' behaviour across treatments exactly matched responders' actions. It seems that proposers were able to anticipate those treatments where probabilities of acceptance were lower raising their offers to receivers correspondingly. We cannot say whether this is due to fairness considerations by proposers or to their willingness to maximise income, though research on this specific issue argues that both motives are present (Cox, 2004). More in detail, signs of coefficient pairs always change monotonically in FRCTs, though differences are never significant (see Table 12). A binomial test suggests that this pattern cannot be considered as random. The null hypothesis of finding a positive and a negative sign with equal probability over the 6 tests conducted in Table 12 yields a P-value of $B(6, 0.5) = 0,016$ and is thus rejected. Although this test is not as stringent as that carried out above, this evidence supports *H1a* in FRCTs.

In VRCTs, we observe a clear difference in *0%_ VRC* compared to other treatments, with difference being significant in *0%_ VRC* vis-à-vis *1%_ VRC* ($P = 0.003$). This strongly supports *HP2b.* Differences are also significant in *0%_ VRC* vis-à-vis *20%* ($P = 0.042$), *0%_ VRC* vis-à-vis *50%* ($P = 0.018$). In all other treatments, differences appear small and are not significant. We conclude:

Conclusion 9 *Descriptive analysis supports H1a in FRCTs. Descriptive, graphical, and econometric analysis supports H1b in FRCTs.*

Proof: In FRCTs mean and median values of rejected demands decrease as the initial lottery becomes more biased (see Table 10). Moreover, a binomial test over the hypothesis that z-statistics of pairwise tests in FRCTs have equal probability of being positive or negative is rejected at less than the 5% level. This further supports *H1a*.

As for *H1b,* in FRCTs demands decrease monotonically as the bias of the initial lottery decreases (See Table 10 and Figure 5). The variable *CHANCE* has a positive sign and is statistically significant at the 5% level (see Table 14, column 1). The z-statistics of pairwise tests are again all negative, and in four out of the six cases the differences are statistically significant.

Conclusion 10 *Descriptive, graphical, and econometric analyses support H2a and H2b in VRCTs. Descriptive and graphical analyses supports H2a and H2b in FRCTs.*

Proof: Descriptive statistics show lower mean and median values for rejected demands in the *0%_ VRC* than in the *1%_ VRC,* as well as lower demands (see Table 10 and Figure 5). Pairwise tests over the null hypothesis that $\beta_{0\%\_VRC} - \beta_{1\%\_VRC} = 0$ strongly reject this hypothesis in both the analysis of demands and acceptances, thus supporting both *H2a* and *H2b*.

Table 10 and Figure 5 also indicate lower mean and median values for rejected demands in the *0%_FRC* than in the *1%_FRC,* as well as lower demands. Finally, we put forward:

Conclusion 11 *There exists a one-to-one correspondence between proposers and responders behaviour across each treatment within VRC and FRCTs. As the probability of rejection increases, demands decrease.*

Proof: See comparison of Tables 12 and 15, and of Tables 13 and 16.

7 Comparing VRC and FRC

7.1 Comparison across all rounds

Let us focus on comparing FRCTs and VRCTs. If subjects were *consequentialist* (see section 1), they would be indifferent between FRCTs and VRCTs because their decision node takes place after Nature has moved, thus it bears no consequence on their future strategies. Conversely, were subjects procedural, they may take into account Nature's moves prior to their choices. As posited in *H3a* and *H3b,* we believe that procedural individuals see VRCTs as implying a more fair procedure than FRCTs, thus we expect both acceptance rates and demands to be higher in VRCTs compared to FRCTs.

We first note that descriptive statistics from Table 1 support *H3a* and *H3b*. For each pair of treatments - e.g. *20%_FRC* vis-à-vis *20%_ VRC,* the mean and median value of rejected demands, and the acceptance rate of high demands, are all lower in FRCTs than VRCTs. Moreover, demands are higher in VRC than FRC in each pair of treatments.

As for econometric analysis, we fit models analogous to (2) and (4) for pooled observations coming from FRCTs and VRCTs (see sections 4.1.2 and 4.2.2). This enables us to test directly for differences in the coefficients associated with different treatments. The result of the analysis are reported in Tables 17 and 21. Tables 18 through 20 (Tables 22 through 24) report in each cell the coefficient value and the standard error for $\beta_k - \beta_l$, as estimated in model (2) (model (4)) applied to the whole data. Row (column) entries denote FRCTs (VRCTs). We only report the result of Wald tests on the null H_0: $\beta_k - \beta_l = 0$ against the hypothesis H_1: $\beta_k - \beta_l \neq 0$ for the three pairs of treatments on the "diagonal" of all possible pairings. Acceptance rates are ceteris paribus significantly lower in FRCTs than in VRCTs in all three comparisons. As shown in Table 18, the difference is significant at less than the 5% level for *20%_FRC* vis-à-vis *20%_ VRC* ($P = 0.012$) and for *1%_FRC* vis-à-vis *1%_ VRC* ($P = 0.033$), and strongly significant for *0%_FRC* vis-à-vis *0%_ VRC* ($P < 0.01$). Tests reported in Table 22 also confirm that demands were significantly lower in FRCTs than VRCTs, by a factor of 0.48 GBP in 2*0%_FRC* vis-à-vis *20%_ VRC* ($P < 0.01$), £0.42 in *1%_FRC* vis-à-vis *1%_ VRC* ($P < 0.01$), and £0.26 in *0%_FRC* vis-à-vis *0%_ VRC* ($P < 0.095$)[14]. We thus conclude:

Conclusion 12 *Descriptive and econometric analyses support H3a and H3b.*

Proof: See Table 1, Table 18 and 22.

7.2 Learning and strategies evolution

We turn to analyse the difference between VRC and FRC by looking at the evolution of strategies over time. We speculate that the break of monotonicity we observe in VRCTs is due to the establishment of a "convention" assigning favoured players a dominant role in the allocation of the pie. The existence of a trend for some of the VRCTs is quite clear in Figure 5, which plots the evolution over rounds of mean proposals from favored proposers. Secondly, we fit a model similar to the one used in the previous section. We replace all round dummies by a single $ROUND$ variable that simply equals the number of the round to which an observation refers. It thus captures the existence of trends in the evolution of proposals and acceptance rates over time. We also add interaction terms of this variable with a dummy variable identifying FRCTs (*FRC_X_ROUND*) and VRCTs (*VRC_X_ROUND*).

These models enable us to assess the existence of different trends in the evolution of strategies between FRCTs and VRCTs. First we focus on receivers' behaviour. The coefficient of $ROUND$ refers to the baseline 50% condition. The sign is positive and is significant at the 5% level (See Table 17, column 2). As for FRCTs and VRCTs, the coefficient estimate is only weakly significant for the FRC ($\gamma = 0.032$; z=1.94; $P = 0.052$; N= 4260), whereas it is strongly significant for the VRC ($\gamma = 0.064$; z=3.31; $P < 0.01$; N= 4260). Notwithstanding, the coefficient difference between VRC and

[14]We have restricted the analysis to favoured proposers only. Including unfavoured proposers would weaken the results, although the general pattern would be unaltered. As already mentioned in sections 4 and 5, unfavoured proposers behaviour was dyammetrical opposite in FRCTs and VRCTs, and this has the effect of weakening the regression results conducted over the unrestricted set of observations.

FRC is not significant ($\gamma = 0.032$; $z = 1.26$; $P = 0.208$; $N = 4260$). It has to be noted, though, that acceptance rates start off at a considerably lower level in FRC from the beginning ($\gamma = 1.409$; z=2.73, $P = 0.006$; $N = 4260$). So, the model predicts the probability of accepting an offer of 20% of the pie is in round 20 equal to 69% in the average FRCT while it is 90% in the average VRCT.

Although the sign of the coefficient is positive, 14.6% of the proposals (31 out of 231) are rejected even in the last round. We are thus very far away from NE behaviour. A Wilcoxon signed-rank test strongly rejects the hypothesis that acceptance rate is equal to 1 in the last round (z= -5.568; P <0.0001). It is interesting to ask why responders 'softened up' over time. We doubt this is due to subjects learning to play the NE of the game. Should this be the case, learning should be faster in the FRC than the VRC - because FRC responders play more often in this role than VRC ones - but, if anything, the opposite happens. As we argue below, we believe that the higher intertemporal fairness of VRC made it possible for a convention to arise in VRC, which made responders more lenient in VRC than FRC. All the same, we do observe some evidence of increased willingness to accept in FRC, too. We believe this is due to the fact that the "value" of punishing a greedy proposer is higher in the first rounds than in later rounds. This may be due to either an "altruistic punishment" motive - if punishment "redeems" a greedy proposer to making fairer offers, the value of punishment in early rounds is higher than punishing in later rounds - or to a purely selfish motive - since there is a positive, albeit small, probability of meeting the same proposer in future rounds, the value of early punishment is again higher.

Different is the case of proposals. Here no trend can be detected in neither the *50%* treatment (see Table 21, column 2), nor in FRCTs ($\gamma = -2.5X10^{-3}$; z=0.63, $P = 0.526$), whereas a steep trend occurs in VRCTs ($\gamma = 0.02$; z=5.47, $P < 0.01$). The difference between FRCTs and VRCTs is strongly significant ($\gamma = -0.021$; z=-3.55, $P < 0.01$). It is worth noting that offers in the last round are still much lower than the SPNE of the game (See Figure 5). Rather than "learning" the NE of the game, we are inclined to believe that subjects used information relative to receivers' acceptance thresholds to their own advantage. We summarise these results as follows:

Conclusion 13 *Support for convergence towards the SPNE of the game is limited.*

We find evidence of convergence in the direction of the SPNE of the game only in VRCTs, where both acceptance rates and demands increase over time. Weak evidence of convergence is also found in both FRC and *50%* treatments but limitedly to acceptance rates. Conclusions are similar even if we analyse single treatments separately[15]. Both acceptance rates and demands are very distant from the game SPNE in the last round of the game.

Proof: See Table 17 and 21.

7.2.1 A social norm emerging in VRCTs?

To understand the extent to which the two sets of treatments differed at the beginning and at the end of the interaction we have fitted the same econometric specifications illustrated in section 7.1 to the first and last five rounds only, and then run Wald tests over the null hypothesis that coefficients differ between FRC and VRC in corresponding treatments. Overall, the picture that emerges is one of very small differences, if any, at the beginning of the interactions, and of large differences at the end. Differences are weak for proposals in *FRC_1%* and *VRC_1%* ($P = 0.055$), and non-existing in the other two cases (see Table 23). Conversely, in the last five rounds proposers in VRCTs demand significantly more than FRC proposers in all treatments, the differences being strongly significant in the *20%* and the *0%* treatment, and significant in the *1%* treatment ($P = 0.012$). Acceptance rates are already higher in the first five rounds in VRCTs compared to FRCTs, particularly in the *1%* treatment ($P < 0.01$), but also in the *20%* treatment ($P = 0.091$), whereas no statistical difference emerges in the *0%* treatment (see Table 19). Perhaps receivers in the 1%_FRC treatment perceived the unfairness of the procedure even more than participants in the 0%_ FRC. All the same, differences grew even larger at the end of the interactions. Responders were significantly more likely to accept a given offer in the *0%* treatment ($P < 0.01$), in the *1%* treatments ($P = 0.024$) and in the *20%* treatment ($P = 0.035$).

One can conclude that the pronounced differences we observe between

[15] Fitting to individual treatments the same model for $PROPOSAL$ as the one considered in this section, yields the following values for the coefficient of PERIOD: $\gamma = 0.011 (p = 0.236)$ in 50%, $\gamma = .008 (p = 0.124)$ in 20%_FRC, $\gamma = -.006 (p = 0.555)$ in 1%_FRC,c $\gamma = -.006 (p = 0.349)$ in 0%_VRC, $\gamma = 0.030$ ($p < 0.01$) in 20%_VRC,

$\gamma = 0.007$ ($p = 0.407$) in 1%_VRC, $\gamma = .036$ ($p < 0.01$) in 0%_VRC.As for $ACCEPTANCE$, we find the following values: $\gamma = 0.037 (p = 0.501)$ in 50%, $\gamma = 0.059 (p = 0.086)$ in 20%_FRC, $\gamma = 0.083 (p < 0.01)$ in 1%_FRC, $\gamma = -.0242 (p = 0.356)$ in 0%_FRC, $\gamma = 0.057 (p = 0.252)$ in 20%_VRC, $\gamma = 0.035 (p = 0.256)$ in 1%_VRC, $\gamma = 0.165 (p < 0.01)$ in 0%_VRC.

Hence, 0%_VRC is the only treatment in which both proposals and acceptance rates increased significantly over time. Proposals grew strongly in 20%_VRC but this time the coefficient term for acceptance was not significant at conventional levels. However, this is partly due to the drop in observations brought about by the introduction of demographic controls. If we remove these, the PERIOD coefficient is positive and strongly significant for 20%_VRC, too.

VRC and FRC over the whole 20 rounds are relatively small at the beginning of the interactions, but grow larger as the number of interactions increases. This is particularly the case for proposers' behaviour. It is thus clear that they are not the result of the beliefs held by players prior to the start of the interactions. It is instead plausible, although our design does not enable us to prove it, that what we observe is the result of the establishment of a convention in VRC. Hargreaves-Heap and Vaourofakis (2002; HHV henceforth) define a convention as the situation in which players use an exogenously given characteristic to solve a co-ordination problem. In their experiments, such characteristic was the assignment of players to one of two colours and the interaction was the Hawk-Dove game. They found that as time went by players being assigned one colour played "Hawk" with higher frequency whereas those assigned the other colour played Dove with higher frequency. This obviously resulted in increased efficiency than the control case where no colour was assigned. Colour assignment was entirely random, and, in principle, completely irrelevant to the strategic interaction. The establishment of such convention took time, but was rather stable towards the end of the interaction.

We believe something similar occurred in our VRCTs. Players used the assignment to the favoured role in the random draw as a characteristic enabling them to demand a larger share of the pie - thus acting more "hawkishly" - whereas players being assigned to the unfavoured role accepted with higher frequency such demands - thus acting more like "doves" - in comparison to FRCTs. In other words, players used an exogenously given characteristic of the interaction - in a sense analogous to the colour assignment - to "co-ordinate" over an equilibrium assigning around three quarters of the pie to the proposer. Admittedly, in our game such characteristic is not irrelevant to the interaction, as it has a direct influence in determining the odds with which a proposal is selected. All the same, it may be deemed as sharing similar properties to a convention in HHV game. The second important difference between HHV and our experiments lies in that their colour assignment was invariant throughout the whole of the interaction, whilst in our VRCTs the random assignment occurred at each round. However, we believe that the nature of the HHV result carries over to our case as well. It was exactly this feature of the interaction that explains why the convention got established in VRCTs and not in FRCTs. Only on the backdrop of overall inter-temporal fairness did VRC receivers accept the higher inequality of the distribution in comparison to FRCTs. This is hardly surprising. It is easier to stick to a disadvantageous convention today knowing the same convention may turn advantageous tomorrow, rather than sticking to a convention in which one is always on the receiving end.

We believe this account also helps explain the reversal in monotonicity that we observe in VRCTs with respect to the *50%* treatment. If subjects were only concerned with procedural fairness, and if there existed a (negative) monotonic relationship between overall procedural fairness and acceptance rates, then we should clearly observe higher or equal acceptance rates in 50% compared to 20%_VRC. The opposite occurs instead. We tentatively conjecture that the existence of a characteristic able to differentiate players at each round has made it possible that players reduced their overall level of conflictuality. Perhaps surprisingly, the existence of a characteristic randomly *differentiating* players in terms of their luck made it more salient that subjects would be favoured on average half of the times and be unfavoured the other half. This may have induced them to find the asymmetric distribution of power in UG more acceptable, thus reducing their conflictuality rate.

In fact, the random assignment of roles to individual leads to surprising consequences in terms of overall efficiency and allocation inequality. Figure 6 offers a graphical representation of the share of total surplus that (a) has gone lost because of receivers' rejection (black segment of the bar), (b) has been accrued to proposers (grey segment of the bar), (c) has been accrued to receivers (white segment of the bar), for each of the treatments. It is worth noting that the two treatments where losses are *lowest* are *20%_ VRC* and *0%_ VRC*. Acceptance rates over the whole 20 rounds are exactly the same in these two treatments (85%). *20%_FRC* comes third, with an acceptance rate of 84%, and *50%* is only fourth in this ranking, with an acceptance rate of 81%. In the last five rounds of the game (See Figure 6b), all three VRCTs present lower conflictuality rates than *50%*. Such differences are not huge but are sizable. A Mann-Whitney test conducted on the dicotomic variable *ACCEPT* between *50%* and *20%_ VRC* over the whole 20 rounds rejects the null that observations come from the same distribution, though only weakly ($z = -1.657$, p=0.0976, N=1220). Exactly the same result holds comparing the distribution of ACCEPT between *50%* and *0%_ VRC*. What is also interesting to note is that overall inequality between proposers and receivers is considerably lower in *0%_ VRC* than in both *20%_ VRC and 50%*. Running a Mann-Whitney test on receivers payoffs yields a strongly significant difference favouring *0%_ VRC* compared to *20%_ VRC* (z = -10.238, p<0.01, N=1200) and *50%* (z = -5.896, p<0.01, N=1200). In fact, only *0%_FRC* guarantees receivers a higher share (z = 3.111, p= 0.0019, N=1220).

8 A post-hoc model

8.1 Existing models of other-regarding preferences

In this section we try to move towards an explanation connecting the concept of opportunity with allocation inequality. Our results show the existence of a generally increasing monotonic pattern in the relationship between fair allocation of opportunities on the one hand, and demands and probability of acceptance on the other- with the exception of the non-linearity found in VRCTs (see section 5). Moreover, we find a "discontinuous" jump between the 0% and the 1% treatments, which is strong and robust in both FRCTs and VRCTs (see sections 4 and 5).

Before introducing our own explanation, let us briefly discuss why traditional models fail to explain our result. There are three main class of models that departs from the traditional rational choice theory and offer explanations for the anomalies observed in laboratory experiments. The first group of models assume that individuals maximize well defined preferences, but permit preferences to depend on the payoffs of other players. FS and BO among others have followed this approach and have suggested specific functional form for interdependent preferences. These utility function, in general, could be written in the following form:

$$u_i(x) = x_i + \lambda_{ij}(x_i - x_j)x_j \tag{9}$$

$u_i(x)$ is agent i's utility given an allocation x where x_i and x_j denote the shares of i and j respectively. The function λ_{ij} is the source of interdependence - it connects i's utility with j's share. In this formulation, it is easy to see that irrespective of the opportunity level, the responder's behaviour in a UG always remains the same. Hence, proposers' behaviour will also remain unaffected by the opportunity level as well as the *ex-post* inequality in allocation. A second approach seeks to introduce fairness concerns directly into the utility function, by making individuals' utility dependent on the characteristics of the lotteries they face in their decisional process. For instance, Trautmann (2009) applies FS model of inequality aversion to *expected* payoffs rather than final payoffs. In similar fashion, BBO extend the original BO model by defining the "fairest" available allocation in the game as the closest possible - in expected value - to the equal divide, after taking into account players' strategy sets and the set of lotteries available for choice. Individuals condition their social motivations to the distance between the actual allocation and the fairest allocation possible. In this way, preferences over procedures may be accommodated directly into utility functions. A third stream of argument is based on reciprocity and permits the preference over outcomes to depend on the context in which the outcome was reached (see e.g. Rabin, 1993).

The reason why these approaches fail to account for our results is ultimately that they make individuals' utilities depend either on final allocations, or on lotteries that are directly defined over such allocations. However, in our experiments the only source of variation across treatments is given by lotteries determining the game initial positions, rather than final allocations. As such, these lotteries cannot but be irrelevant for these models[16]. Furthermore, such models are 'continuous' in nature and are thus unsuitable to explain the discontinuous jump that we observe between no opportunity and positive opportunity settings. Finally, although the context dependence of the third class of models sounds promising in our setup, the source of unfairness in our setting is due to external lotteries rather than human action. Therefore, intention-based reciprocity cannot be held responsible for our findings.

8.2 Envy factor dependent on 'Opportunity'

Here, we demonstrate that a simple combination of inequality aversion model with Nozick's idea of symbolic utility can rationalise our findings. It would be proper to mention at this stage that our aim is no broader than that. We neither claim this to be the only possible explanation nor we propose symbolic utility to be the basis of a new social utility model. The analysis we sketch here is rudimentary and leaves many important questions unanswered. We propose a simple extension of the inequality aversion model introduced by FS. In a two-person society, FS utility function for an agent can be represented

[16]A possible strategy for models of the second class to accommodate lotteries applied to initial positions, rather than to final outcomes, may be the following. Let us call Λ_I (Λ_F) lotteries defined over the initial (final) positions of the game. We argued that the main limitation of models of the second class was that no payoff can directly be attached to Λ_I. However, subjects may attach their *subjective* expected payoff to Λ_I,either taking into account their previous experience in the game, or their *a priori* beliefs over others' behaviour. For instance, individuals may use their subjective distribution of a receiver's probability of rejection of a given offer to attribute an expected value to being selected as proposer of the game. Likewise, individuals may use their subjective distribution of probability of receiving a certain offer from a proposer, and determine the expected payoffs on the basis of their own minimum acceptable offers. In this way, expected payoffs may be indirectly attributed to all the events associated with Λ_I. Such expected payoffs may thus be used in a similar fashion as the "objective" payoffs that can be derived from Λ_F. The main problem of this strategy is obviously that the subjective expected payoffs so determined may (widely) differ across individuals, thus making it difficult for an equilibrium to become established.

as follows:

$$u_i(x) = x_i - \alpha_i \max(x_j - x_i, 0) - \beta_i \max(x_i - x_j, 0) \tag{10}$$

where x_i and x_j are share of agent i and j respectively. $\alpha_i \geq 0$ is the envy factor and $0 \leq \beta_i \leq \alpha_i$ is the altruism factor. Our experimental results, in particular, rejection rate across different opportunity level suggests that 'opportunity' enters into the utility function through the envy factor. That is, α_i is not a constant but depends on p, where p is a measure of opportunity. We have already argued that in our settings it is simply the probability with which a proposal from an unfavoured player is selected as a proposal. Thus p can vary between 0 and 0.5. Moreover, one can expect that the higher the difference in opportunity levels between the two players, the stronger the envy factor. That is $\alpha_i(p)$ is a decreasing function in p. Following FS, we assume that the proposer does not know the exact value of the envy factor of the responder, but knows that it is distributed according to some distribution function $F_p(\alpha)$. The equivalent of decreasing function $\alpha_i(p)$ in this setup is as follows. If $p_1 > p_2$ then F_{p_2} first-order stochastically dominates F_{p_1}.

To keep our model simple we assume $\beta_i = 0$ and we use our toy model to explain two main observations: *i*) *ceteris paribus*, a decrease in inequality of opportunity (that is, an increase in p) increases the probability of acceptance of a proposal and *ii*) a decrease in inequality of opportunity increases the inequality of the allocation.

Suppose j is the proposer and i is the responder. Since, x_i and x_j denote the share of agent i and j respectively, we have $x_i + x_j = 1$. First note that in equilibrium, $x_j \geq 0.5$. Otherwise j can increase her utility because any $x_j < 0.5$ will be accepted by i. Thus $(x_j - x_i) \geq 0$ and agent i accepts a proposal if and only if $[x_i - \alpha_i(x_j - x_i)] \geq 0$. Equivalently, an offer will be accepted if and only if $\alpha_i \leq \frac{x_i}{1-2x_i}$. Hence, the probability with which x_i is accepted is $F_p\left(\frac{x_i}{1-2x_i}\right)$. If $p_1 > p_2$ then F_{p_2} first-order stochastically dominates F_{p_1}, implying $F_{p_1}\left(\frac{x_i}{1-2x_i}\right) \geq F_{p_2}\left(\frac{x_i}{1-2x_i}\right)$. That is probability of acceptance increases with p.

Now, the expected payoff of the proposer is $\left[(1-x_i)F_p\left(\frac{x_i}{1-2x_i}\right)\right]$. Thus agent j chooses x_i which maximizes $\left[(1-x_i)F_p\left(\frac{x_i}{1-2x_i}\right)\right]$. The first order condition is as follows,

$$\frac{(1-x_i)}{(1-2x_i)^2} = \frac{F_p\left(\frac{x_i}{1-2x_i}\right)}{f_p\left(\frac{x_i}{1-2x_i}\right)} \tag{11}$$

where f_p is the density function. To show our next result on allocation inequality, we need to make further assumptions on F. We assume that f is non increasing. Moreover we only consider one particular type of first order stochastic dominance which comes from a shift in the support. For example, consider the following family of exponential distribution,

$$f_p(t) = \begin{cases} \lambda e^{-\lambda(t-a(p))} & \text{if } t \geq a(p) \\ 0 & \text{otherwise} \end{cases} \tag{12}$$

where $a(p)$ is a decreasing function of p. One can check that if $p_1 > p_2$ then $F_{p_1}(t) \geq F_{p_2}(t)$ for all t. $p_1 > p_2$ also implies $a(p_1) < a(p_2)$ and hence for all t, $f_{p_1}(t) \leq f_{p_2}(t)$. Therefore, for all x_i

$$\frac{F_{p_1}\left(\frac{x_i}{1-2x_i}\right)}{f_{p_1}\left(\frac{x_i}{1-2x_i}\right)} \geq \frac{F_{p2}\left(\frac{x_i}{1-2x_i}\right)}{f_{p2}\left(\frac{x_i}{1-2x_i}\right)} \tag{13}$$

Note that the left-hand side of Equation 11 is an increasing function of x_i and starts above the right hand side (at $x_i = 0$). Let $x_i(p)$ be the equilibrium share of the pie that j offers to i, where

$$x_i(p) = \{\min x_i | x_i \text{ satisfies Equation 11}\} \tag{14}$$

Thus for all $x_i < x_i(p_1)$, we have $\frac{(1-x_i)}{(1-2x_i)^2} > \frac{F_{p1}\left(\frac{x_i}{1-2x_i}\right)}{f_{p1}\left(\frac{x_i}{1-2x_i}\right)}$. By Equation 13, $\frac{(1-x_i)}{(1-2x_i)^2} > \frac{F_{p2}\left(\frac{x_i}{1-2x_i}\right)}{f_{p2}\left(\frac{x_i}{1-2x_i}\right)}$ for all $x_i < x_i(p_1)$. Therefore $x_i(p_2) \geq x_i(p_1)$. That is, inequality of allocation increases with a decrease in inequality of opportunity. From the data (see sections 4 and 5) we observe a jump from $p = 0$ to $p = 0.01$. This can be accommodated in our post-hoc model if we assume that there is a jump from F_0 to F_p for any $p > 0$. This is consistent with what we called the Symbolic Opportunity Hypothesis. As illustrated in section 3, our application of Nozick's symbolic utility argument to our case is that the purely symbolic opportunity of submitting a proposal in the game suffices to give "expressive" (or symbolic) utility to the agent that adds to the intrinsic utility. Such symbolic act suffices to make the overall assessment of procedures as more fair than when this act is not possible, thus making receivers more lenient to accept offers, *ceteris paribus.* This may thus explain the discontinuity between F_0 and $F_{0.1}$.

9 Conclusions

The following conclusions can be drawn from the present study. First, we find robust support for the Monotonic Fairness Hypothesis in FRCTs. The greater the inequality in the distribution of initial opportunities, the lower the acceptance rates of a given offer. Consequently, average offers increase. In VRCTs we instead find an inverted-U pattern. The trend in acceptance rates and proposals increases between 0% and 20%, but then suffers a reversal, as we find higher acceptance and inequality rates in the *20%_ VRC* treatment than in the equal opportunity treatment. Overall, it is striking that manipulations of initial opportunities which should leave a consequentialist player indifferent seem to matter a great deal to individuals. Although several other studies have emphasised the relevance of procedural fairness, we believe our study to be the first to investigate the issue, systematically varying the degree of initial opportunity over the whole range of the scale. Another exception is Suleiman (1996), who analyses behavioural patterns associated with changes in a "structural" parameter over the whole extension of the relevant interval. Nevertheless, in his study procedural changes are mixed with payoff changes (see section 2), so cannot be deemed to be singling out the purely procedural aspect of fairness. We discuss below possible reasons for the break in monotonicity in VRCTs.

Second, we find clear support for the Symbolic Opportunity Hypothesis. In both FRCTs and VRCTs, receivers act significantly more leniently after having been previously assigned a mere 1% initial chance of acting as proposers compared to having no chance. We believe that ours is the first study finding such a clear variation in behaviour associated with such a small modification of chances in an experimental context. Our results are reminiscent of those by Suleiman (1996), and Handgraaf *et al.* (2004), who find some significant changes in behavioural patterns between the "corner" and the interior of the interval scale. However, their findings have nothing to do with the attribution of symbolic chances to unfavoured players, but rather to a sense of "responsibility" by favoured players (see in particular Handgraaf *et al.,* 2004).

In this fashion, our study validates experimentally other pieces of empirical and survey evidence regarding the importance of "voice" for people. Frey and Stutzer (2005) find support for the thesis that the mere right to participate in the political process - rather than actual participation - increases individual satisfaction - a phenomenon they refer to as "procedural utility". Anand (2001) reports survey evidence supporting the importance people place on having the right to have their opinion heard - or appropriately represented - in collective decision processes. The relevance of this right to voice may be caused by the desire to express one's position, or to obtain respect for one's worth.

As a matter of fact, in our experiments we are not able to disentangle whether such a result is due to the purely procedural aspect of having a say in the collective decision problem, or to the actual allocation of a 1% chance of acquiring the advantaged position. Discriminating between these two interpretations would call for the running of a treatment where subjects have a 0% chance of having their proposal submitted, but are nonetheless asked to submit a proposal[17]. In other words, the symbolic value of opportunity may matter for individuals in a purely representative sense, even if it is not attached to any chance - as small as it may be - of influencing the assignment of positions. This is certainly an interesting topic worth studying in the continuation of our research.

Third, we also find support for the Dynamic Opportunities Hypothesis. Acceptance rates are significantly higher in VRCTs than FRCTs, and as a consequence, proposers' demands are also higher. As argued in section 3, this is consistent with our claim that subjects see VRCTs as a fairer procedure by which to allocate initial opportunities. It appears that players are prepared to accept even extreme levels of opportunity inequality within each round, in exchange for overall equality of opportunity across the whole series of interactions.

What we did not expect is that each VRC treatment ended up with even lower conflictuality rates - and thus greater efficiency - than the baseline case of equal opportunities. This result is arguably worth more investigation. It may be the case that VRCTs simply made more salient to subjects the possibility of achieving some *form* of fairness, albeit in a dynamic rather than in a static sense, thus inducing subjects to become more lenient over proposed allocations. Perhaps paradoxically, the treatment that allows perfect fairness both in a static and in a dynamic sense is less conducive than VRCTs to elicit such a perception, and consequently conflictuality is higher.

However, our findings may point to something more substantial. One hypothesis that is worth examining in future research is whether creating "spheres" of relative advantage of opportunity is conducive to a public endorsement as potent as that of granting equal opportunities under all circumstances. For instance, affirmative action has been criticised as a measure for redressing unfairly distributed initial opportunities on the grounds that it grants unfair advantage to some groups of normally disadvantaged people. It has been argued that a truly meritocratic society should not grant a specific advantage to someone in *any* domain, even if this person happens to be generally disadvantaged. Our results stress that, on the contrary, people may perceive favourably the allocation of preferential advantage to some people in

[17]We thank Tim Salmon for this suggestion.

some domains, even when the degree of favouritism is very high. Admittedly, this occurs within a context of *overall* equality of opportunity, thus it may not generalise to other contexts. Obviously we do not need to be reminded of the wide gulf existing between our experiments and real-life social relations, especially when a topic as important as that of opportunities is concerned. However, at the very least we believe that our research points to the possibility that individuals' endorsement of the fairness of opportunity distribution over time may not be as clear-cut as one may at first have thought.

Fourth, we find strong evidence of convergence towards the SPNE only in VRCTs, whereas in FRCTs and the 50% treatment, this occurs only for receivers but not for proposers. We have argued that this is not due to "learning". If that was the case convergence should have been faster in FRCTs, where there is less variation across roles. Rather, in section 7.2 we conjectured that it may be due to the emergence of a specific social norm of fairness emerging in VRCTs, whose characteristics have already been discussed above. We also found that most of the patterns emerging over the whole period of interactions were already present in the first round. Proposers were able to anticipate correctly the variation of receivers' behaviour across treatments.

To sum up, we believe that our study confirms and substantially extends recent survey and experimental evidence regarding the relevance of procedural fairness for individuals. The major innovation of our study has been to focus on the fairness in the assignment of *initial positions* in a standard problem of division of resources. This is a topic of paramount importance for our societies. However, it had been so far neglected in experimental studies, which had focussed on procedural fairness related to *final allocations*. Our results show that the most basic results gained so far in the context of final positions carry over to the case of initial positions. More specifically, subjects are sensitive to the fairness in the assignment of initial positions. This calls for an extension of existing theoretical models, which in their current form fail to attach any relevance to the way initial positions are assigned. We have offered a theoretical extension that goes in this direction, although others are possible. However, some of our results - such as the symbolic value of opportunities, and the relevance of the dynamic allocation of opportunity - are entirely novel.

We have also shown and debated how some of our findings escape a straightforward theoretical rationalisation, thus indicating further avenues of research. More generally, we believe that an open question in the literature is the assessment of the relative importance of procedural fairness vis-à-vis individual merit, responsibility, or needs, in acquiring certain positions. We have already stated that existing studies seem to imply that individual merit or responsibility prevail over procedural fairness when the two are considered jointly. For instance, Anand (2001) reports situations - such as health care decisions - where random lotteries are deemed as unfair by survey respondents. Other experimental studies seem to agree with this hypothesis (Krawczyk, 2010; Schurter and Wilson, 2009; Cappelen *et al.*, 2010). However, we believe that the available evidence is still too limited to draw any firm conclusion. More in-depth research on the role and the interplay among the various components of an individual's sense of justice is, to be sure, needed to shed light on these issues.

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A Instructions

Welcome to this research project. A team of researchers is looking at the way in which people make decisions. If you pay close attention to the instructions then you could make a significant amount of money. The research team that is here today includes myself, Gianluca Grimalda, and my assistants.

Before starting with the explanation of the decisions you are going to make, please pay attention to some important information and recommendations.

In this project you are going to be asked to make decisions with other people who are currently in this room. Your choices, and the choices of others, will be matched with the help of a computer programme as we proceed. It is important for you to note that all interactions are entirely anonymous. Firstly, we will not know anything about your choices and your payment. We will just record your choices through the ID number that you have just drawn, and the payments will be made using that number as identification. It is therefore important that you do not lose the card you have drawn, because that is the only document that enables you to be paid. You may collect your payments at the end of this session. You will be required to sign a receipt, but there is no need for you to print your name. University administration does require that you write in your student number when signing this receipt. However, your student number will be held confidentially by our research group, and we will not make any attempt to link your student number to the decisions you have made.

At the end of your decisions, while we prepare your payments, we would ask that you complete a short questionnaire. You are required to state your Student ID number. Even in this case, your responses to this questionnaire will be held under confidentiality rules by our research group.

Secondly, the decisions you are going to make involve interacting with other people who are present in this room. However, you will not have to talk or communicate directly in any way with anybody in this room. Instead, your decisions will be processed through a computer programme that networks all of the computers in this room. In this way, nobody will be able to identify with whom s/he is actually making decisions. The interaction will proceed as follows: You will receive some messages on the screen in front of you. This will either include some information on the state of the decisions, or prompt you to make certain choices. Once you are sure about your choice, you have to press the button OK, which will take you to the next stages of the decisions. At times, you will be asked to wait for further instructions, because it may take a bit of time before the programme processes all your decisions.

If you are not clear on this or on other issues, please raise your hand.

You will be involved in 20 different interactions with other people in this room. In each interaction, you will be paired with another person, and the two of you will be making a decision together. Our programme will draw at random the pairs at the beginning of each interaction. This means that with very high probability you will be paired with a different partner at each interaction.

As you will see, the decisions involve money. In each decision there will be £10 at stake. Unfortunately, we will not be able to pay you for each decision you make, but only for TWO interactions out of the 20. These will be drawn at random at the end of this session, and everyone will be paid according to the outcome of those 2 rounds. In this way, you are required to pay maximum attention to each decision you are going to make, because only at the end of the session we will learn which ones determine your payments.

We are now going to look at the simple rules that will govern each of the interactions:

- *[All treatments]:* An amount worth £10 is to be divided between you and the person you have been paired with.
- *[1%, 20%, 50%]:* Both of you are asked to make a proposal. *[0%]:* One of the two people is drawn at random, and both people are informed about whether s/he has been selected or not. The person who has been selected is asked to make a proposal. *[All treatments]*: The proposal is any amount X less than or equal to £10 that the 'proposer' wants to keep for him/herself. The proposer may use any number up to the second decimal digit. The residual amount (10-X) is to be assigned to the other person in the group (the 'receiver').
- *[1%, 20%, 50%]:* Once you and the other person in your group have submitted your proposals, one of them is drawn at random. *[1%, 20%]:* The random selection works as follows. Half of the people in this room are favoured with respect to the others in having their proposals selected. In particular, half of the people in this room have a *[1-p]%* probability that their proposals will be selected within their groups, whereas the others have a *[p]%* probability. *[{p= 0.01, 0.2}] [50%]:* There is a 50-50 probability that

either proposal is extracted.

- *[0%]:* Each group is composed of a 'proposer' and a 'receiver'. Whether you will act as a proposer or as a receiver is determined by a random draw that will occur *[0%_FRC]:* before the first round. Your role will remain the same throughout the 20 rounds. *[0%_VRC]:* before each round. *[1%, 20%]:* Each group will be made up of a person with a *[1-p]%* probability and another person with a *[p]%* probability of their proposals being selected. *[{p= 0.01, 0.2}]* You will be informed about which probability your proposal has of being selected *[1%_FRC, 20%_FRC]:* before the first round, and this probability will remain the same throughout all the remaining rounds. *[1%_VRC, 20%_VRC]:* before submitting it.
- *[All treatments]:* The person whose proposal has been selected (the 'proposer') is asked to wait for the decision of the other person in the group. The person whose proposal has not been selected (the 'receiver'), is informed of the share allocated to him/her by the proposal of the other person. She is then asked to either ACCEPT or REJECT this proposal.
- *[All treatments]:* If the receiver accepts this proposal, then everyone gets the share determined by this proposal. If the receiver rejects this proposal, then both people in the group get £0 each.
- *[All treatments]:* At the end of each interaction, a new random draw will take place to determine your next partner. *[For FRCTs only]:* This will be a person from the half of the people in this room with a probability different from yours of their proposals being selected. *[All treatments]:* It is therefore very unlikely you will be paired with the same person again. Moreover, all decisions are independent. What you do in a round does not influence the next rounds and is not influenced by the previous rounds.

Examples and comprehension test follow.

Figure 1: Game tree of the stage game

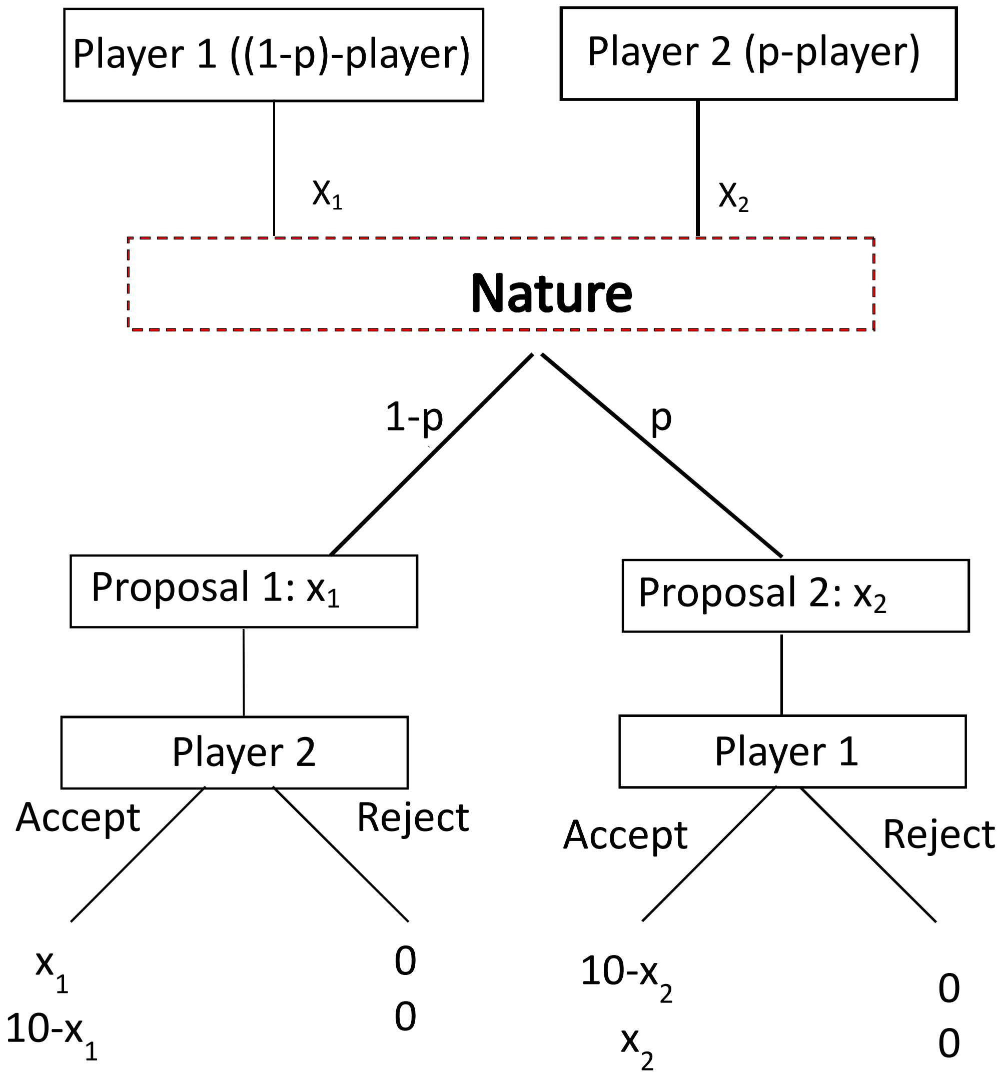


Table 1: Descriptive Statistics of Responses and Demands per Treatment

Table 1a: Eq-Opp.

	Responses			Demands	
	(1)	(2)	(3)	(4)	(5)
	RD	AR (All)	AR (Low)	FAV	UNF
Mean	7.85	81.45%	53.7%	6.97	
St. Dev	0.85	0.39	0.50	1.07	
Median	8			7.00	
Obs	115	620	134	1240	

Table 1b:FRC 20%

	Responses			Demands	
	(1)	(2)	(3)	(4)	(5)
	RD	AR (All)	AR (Low)	FAV	UNF
Mean	7.61	84.03%	52.7%	6.89	7.17
St. Dev	0.75	0.37	0.50	0.82	1.09
Median	7.6			7.00	7.20
Obs	99	620	91	620	620

Table 1e: VRC 20%

	Responses			Demands	
	(1)	(2)	(3)	(4)	(5)
	RD	AR (All)	AR (Low)	FAV	UNF
Mean	8.39	85%	66.6%	7.37	7.13
St. Dev	0.75	0.36	0 .47	0.98	1.40
Median	8.4			7.50	7.50
Obs	90	600	225	600	600

Table 1c:FRC 1%

	Responses			Demands	
	(1)	(2)	(3)	(4)	(5)
	RD	AR (All)	AR (Low)	FAV	UNF
Mean	7.47	78.91%	46.9%	6.77	6.62
St. Dev	0.71	0.41	0 .50	0.88	1.77
Median	7.5			6.99	6.75
Obs	135	640	66	640	640

Table 1f: VRC 1%

	Responses			Demands	
	(1)	(2)	(3)	(4)	(5)
	RD	AR (All)	AR (Low)	FAV	UNF
Mean	7.97	81.07%	51.4%	7.20	6.87
St. Dev	0.78	0.39	0 .50	0.90	1.79
Median	8			7.00	7.00
Obs	106	560	134	560	560

Table 1d:FRC 0%

	Responses			Demands	
	(1)	(2)	(3)	(4)	(5)
	RD	AR (All)	AR (Low)	FAV	UNF
Mean	7.04	77.58%	21.7%	6.28	
St. Dev	0.71	0.42	0.42	0.91	
Median	7			6.17	
Obs	139	620	23	620	

Table 1g: VRC 0%

	Responses			Demands	
	(1)	(2)	(3)	(4)	(5)
	RD	AR (All)	AR (Low)	FAV	UNF
Mean	7.56	85%	47.1%	6.56	
St. Dev	0.91	0.36	0.50	1.07	
Median	7.33			6.50	
Obs	90	600	70	600	

Notes: RD= Rejected demands; AR (All) =Acceptance Rate with respect to all offers; AR (Low) =Acceptance Rate with respect to low offers (less or equal to 20% of the pie); FAV=Favoured; UNF=Unfavoured.

Figure 2: Histograms – All rounds

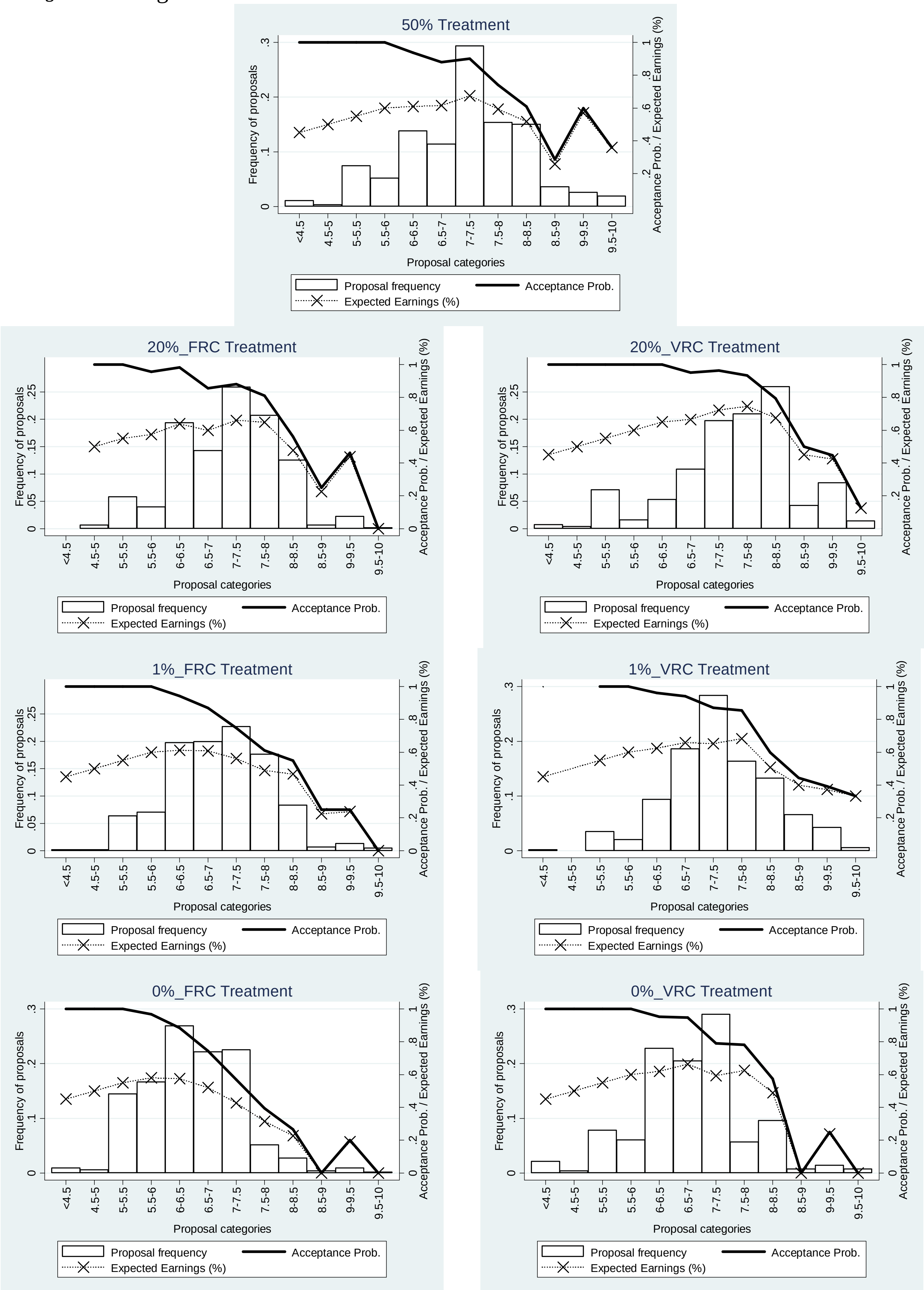


Table 2: Regression Analysis of Logit model for probability of acceptance – *50%* treatment & FRC treatments

DEP VAR	ACCEPT			
	(1)	(2)	(3)	(4)
CHANCE	5.872***	6.695***		
	(1.552)	(1.878)		
FRC_20%			-0.782	-1.438
			(0.898)	(1.025)
FRC_1%			-1.735**	-1.788*
			(0.882)	(1.003)
FRC_0%			-4.114***	-4.819***
			(0.919)	(1.068)
OFFER	3.425***	3.525***	3.438***	3.545***
	(0.214)	(0.237)	(0.214)	(0.238)
FAVOURED	1.870**	1.666*	1.061	1.173
	(0.897)	(0.934)	(1.062)	(1.069)
ECONOMICS		1.757**		1.858**
		(0.786)		(0.765)
YEAR		-0.0654		-0.0405
		(0.203)		(0.196)
GENDER		1.512**		1.438**
		(0.737)		(0.717)
UK		1.646**		1.559**
		(0.752)		(0.726)
Constant	-7.422***	118.8	-4.751***	72.50
	(0.831)	(402.5)	(0.779)	(388.7)
ROUND DUMMIES	YES	YES	YES	YES
Observations	2,500	2,165	2,500	2,165
Number of id	189	159	189	159
Chi2	264.7	227.5	265.8	228.8
Percentage of correct predicted outcomes	81.9%	82.7%	81.7%	82.5%

Notes: Predicted outcomes are computed from the model predicted probability of acceptance by assigning a predicted outcome of acceptance (rejection) whenever the predicted probability is greater (smaller or equal) to 0.5. So a predicted outcome is correct when it matches the actual decision of the subject, i.e. when the subject accepted (rejected) an offer and the model predicted a probability greater (smaller or equal) than 0.5.

Figure 3: Predicted probability of acceptance according to specification (3) of Table 2

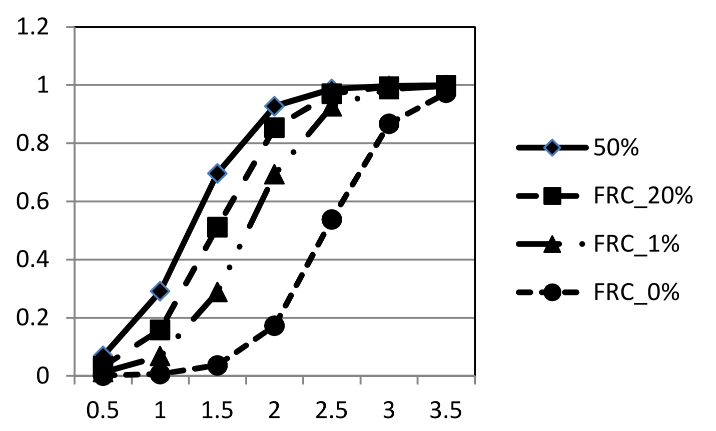


Table 3: Results of Wald test relative to specification (3) of Table 2

	50%	20%	1%
20%	-0,87		
	(0,898)		
1%	-1,97**	-0,97	
	(0,882)	*(0,979)*	
0%	-4,48***	-3,28***	-2,39**
	(0,918)	*(1,016)*	*(0,994)*

Note: The Table reports z-statistics and its standard errors relative to Wald tests of Ho: β_k-β_l=0 against H_1: β_k-$\beta_l \neq 0$, where β_k and β_l are the coefficients of Treatment dummies determined in specification 3 of Table 2. Rejections of H_0 at the 10% / 5% / 1% is denoted by one, two and three stars respectively.

Table 4: Regression Analysis of proposals –*50%* treatment & FRC treatments

DEP VAR	DEMAND			
	(1)	(2)	(3)	(4)
CHANCE	0.995***	1.072***		
	(0.274)	(0.341)		
FRC_20%			0.0384	-0.0937
			(0.157)	(0.185)
FRC_1%			-0.298*	-0.274
			(0.154)	(0.190)
FRC_0%			-0.684***	-0.810***
			(0.159)	(0.179)
UNFAVOURED	0.258*	0.273*	0.0649	0.0910
	(0.154)	(0.148)	(0.165)	(0.162)
ECONOMICS		0.499***		0.527***
		(0.143)		(0.151)
YEAR		0.0663		0.0665
		(0.0492)		(0.0476)
GENDER		0.0174		0.0330
		(0.132)		(0.128)
UK		0.152		0.161
		(0.140)		(0.140)
Constant	6.357***	-125.7	6.786***	-125.7
	(0.110)	(97.74)	(0.117)	(94.56)
ROUND DUMMIES	YES	YES	YES	YES
Observations	4,380	3,740	4,380	3,740
Number of id	219	187	219	187
R2 Between	0.0510	0.143	0.0817	0.174
R2 Within	0.00855	0.0117	0.00855	0.0117
R2 Overall	0.0339	0.0906	0.0522	0.110

Table 5: Results of Wald test relative to specification (3) of Table 4.

FRC DEMANDS ALL ROUNDS			
	50%	20%	1%
20%	0,03		
	(0,156)		
1%	-0.2***	-0.3**	
	(0.154)	*(0.164)*	
0%	-0.6***	-0.7***	-0.3**
	(0.159)	*(0.172)*	*(0.17)*

Notes: See Table 3.

Table 6: Regression Analysis of Logit model for probability of acceptance – *50%* treatment & VRC treatments

DEP VAR	ACCEPT					
	(1)	(2)	(3)	(4)	(5)	(6)
CHANCE	1.245	2.615*	15.11***	14.33**		
	(1.199)	(1.587)	(4.837)	(6.787)		
CHANCE SQUARED			-27.85***	-23.83*		
			(9.391)	(13.37)		
VRC_20%					1.274*	0.691
					(0.654)	(0.942)
VRC_1%					0.108	-0.776
					(0.654)	(0.844)
VRC_0%					-1.134*	-1.589*
					(0.657)	(0.902)
OFFER	2.845***	3.043***	2.861***	3.049***	2.880***	3.061***
	(0.188)	(0.253)	(0.188)	(0.252)	(0.189)	(0.253)
FAVOURED	0.263	0.402	0.0704	0.248	0.0746	0.250
	(0.443)	(0.659)	(0.454)	(0.672)	(0.454)	(0.672)
ECO		1.365**		1.401**		1.400**
		(0.666)		(0.658)		(0.654)
YEAR		0.368		0.279		0.257
		(0.229)		(0.230)		(0.230)
GENDER		1.301**		1.185*		1.127*
		(0.659)		(0.651)		(0.650)
UK		0.124		0.132		0.160
		(0.648)		(0.638)		(0.635)
Constant	-4.839***	-738.8	-5.378***	-561.6	-4.836***	-517.7
	(0.662)	(454.8)	(0.688)	(456.7)	(0.716)	(456.4)
ROUND DUMMIES	YES	YES	YES	YES	YES	YES
Observations	2380	1610	2380	1610	2380	1610
N_g	238	161	238	161	238	161
chi2	236.1	153.1	239.8	154.3	241.3	154.9
Percentage of correctly predicted outcomes	84.2%	85.6%	84.6%	86%	84.3%	85.7%

Notes: See Table 2

Figure 4: Predicted probability of acceptance according to specification (3) of Table 6

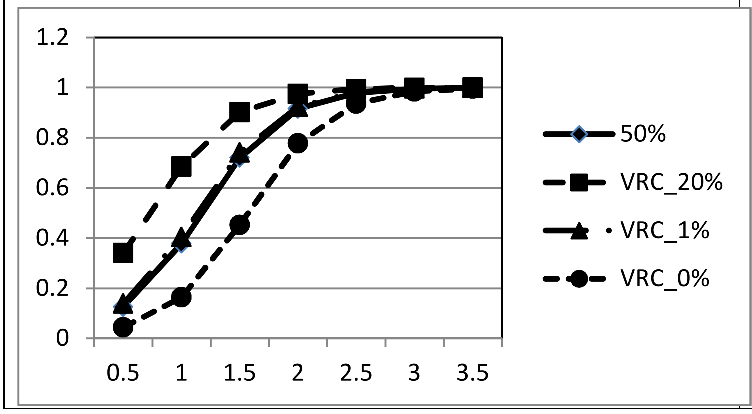


Table 7: Results of Wald test relative to specification (3) of Table 6

VRC ACCEPTANCES ALL ROUNDS			
	50%	20%	1%
20%	1,95*		
	(0,653)		
1%	0,17	-1,7*	
	(0,653)	*(0,661)*	
0%	-1,73*	-3,56***	-1,86*
	(0,657)	*(0,676)*	*(0,669)*

Notes: See Table 3.

Table 8: Regression Analysis of proposals –*50%* treatment & VRC treatments

DEP VAR	DEMAND					
	(1)	(2)	(3)	(4)	(5)	(6)
CHANCE	0.277	0.485	4.711***	3.545**		
	(0.266)	(0.345)	(1.196)	(1.552)		
CHANCE SQUARED			-8.889***	-6.214**		
			(2.281)	(3.073)		
FRC_20%					0.434***	0.234
					(0.151)	(0.215)
FRC_1%					0.221	0.0676
					(0.154)	(0.192)
FRC_0%					-0.415***	-0.523***
					(0.143)	(0.177)
UNFAVOURED	-0.264***	-0.137	-0.279***	-0.144	-0.292***	-0.157*
	(0.0956)	(0.0908)	(0.0961)	(0.0911)	(0.0957)	(0.0908)
ECONOMICS		0.134		0.156		0.162
		(0.144)		(0.146)		(0.145)
AGE		0.0499		0.0276		0.0111
		(0.0597)		(0.0604)		(0.0592)
GENDER		0.0808		0.0522		0.00372
		(0.141)		(0.137)		(0.136)
UK		-0.0641		-0.0622		-0.0453
		(0.142)		(0.136)		(0.134)
Constant	6.714***	-92.76	6.574***	-48.42	6.710***	-15.40
	(0.113)	(118.8)	(0.119)	(120.1)	(0.124)	(117.7)
ROUND DUMMIES	YES	YES	YES	YES	YES	YES
Observations	4160	2824	4160	2824	4160	2824
Number of id	238	161	238	161	238	161
R2 Between	0.00591	0.0143	0.0330	0.0389	0.0820	0.0911
R2 Within	0.0381	0.0360	0.0382	0.0360	0.0382	0.0360
R2 Overall	0.00593	0.0169	0.0289	0.0279	0.0493	0.0499

Table 9: Results of Wald test relative to specification (3) of Table 8

	VRC DEMANDS ALL ROUNDS		
	50%	20%	1%
20%	0,43***		
	(0,15)		
1%	0,22	-0,21	
	(0,153)	*(0,174)*	
0%	-0,41***	-0,84***	-0,63***
	(0,142)	*(0,152)*	*(0,155)*

Notes: See Table 3**.**

Table 10: Descriptive Statistics of Responses and Demands per Treatment – Round 1

Table 10a: Eq-Opp.

	Responses			Demands	
	(1)	(2)	(3)	(4)	(5)
	RD	AR (All)	AR (Low)	FAV	UNF
Mean	7.9	83.9%	77.8%	6,83	
St. Dev	0.82	0.37	0.44	1,15	
Median	7.5			7	
Obs	5	31	9	62	

Table 10b:FRC 20%

	Responses			Demands	
	(1)	(2)	(3)	(4)	(5)
	RD	AR (All)	AR (Low)	FAV	UNF
Mean	7.7	83.9%	57.1%	6,66	7,01
St. Dev	1.40	0.37	0.53	,94	1,27
Median	8			6,5	7
Obs	5	31	7	31	31

Table 10c:FRC 1%

	Responses			Demands	
	(1)	(2)	(3)	(4)	(5)
	RD	AR (All)	AR (Low)	FAV	UNF
Mean	7,48	81.2%	50%	6,59	6,30
St. Dev	0,55	0.40	0.58	,97	1,92
Median	7,5			6,625	6
Obs	6	32	4	32	32

Table 10d:FRC 0%

	Responses			Demands	
	(1)	(2)	(3)	(4)	(5)
	RD	AR (All)	AR (Low)	FAV	UNF
Mean	7,33	80.6%	66.7%	6,21	
St. Dev	0,98	0.401	0.58	1,19	
Median	7,25			6	
Obs	6	31	3	31	

Table 10e: VRC 20%

	Responses			Demands	
	(1)	(2)	(3)	(4)	(5)
	RD	AR (All)	AR (Low)	FAV	UNF
Mean	8.87	73.3%	22.2%	6,77	6,86
St. Dev	0,74	0.45	.44	1,08	1,55
Median	9			6,5	6,78
Obs	8	30	9	30	30

Table 10f: VRC 1%

	Responses			Demands	
	(1)	(2)	(3)	(4)	(5)
	RD	AR (All)	AR (Low)	FAV	UNF
Mean	8,48	85.7%	42.9%	6,98	6,89
St. Dev	0.41	0.36	53.4	1,07	2,16
Median	8,46			7	7,09
Obs	4	28	7	28	28

Table 10g: VRC 0%

	Responses			Demands	
	(1)	(2)	(3)	(4)	(5)
	RD	AR (All)	AR (Low)	FAV	UNF
Mean	7,64	76.7%	0	6,10	
St. Dev	1,38	0.43		1,37	
Median	7			6	
Obs	7	30	2	30	

Notes: See Table 1

Table 11: Regression Analysis of Logit model for probability of acceptance in Round 1

DEP VAR: ACCEPT	(1) FRC & 50%	(2) FRC & 50%	(3) VRC & 50%	(4) VRC & 50%
CHANCE	1.952		2.089	
	(1.498)		(1.697)	
FRC_20%		-0.394		
		(0.828)		
FRC_1%		-0.785		
		(0.773)		
FRC_0%		-1.150		
		(0.889)		
VRC_20%				-0.982
				(1.013)
VRC_1%				0.00710
				(0.992)
VRC_0%				-2.875**
				(1.214)
OFFER	1.121***	1.123***	1.637***	2.083***
	(0.275)	(0.280)	(0.308)	(0.398)
FAVOURED	0.533	0.395	0.666	1.325
	(0.794)	(0.870)	(0.854)	(1.070)
Constant	-2.144**	-1.223	-3.360***	-3.170***
	(0.938)	(0.831)	(0.896)	(1.022)
Observations	125	125	119	119
Chi2	16.95	17.19	29.90	31.62
Percentage of correctly predicted outcomes	83%	82.4%	87.4%	88.2%

Notes: See Table 2

Table 12: Results of Wald test relative to specification (2) of Table 11

FRC ACCEPTANCES ROUND 1	50%	20%	1%
20%	-0,48		
	(0,828)		
1%	-1,01	-0,53	
	(-1,01)	*(0,743)*	
0%	-1,29	-0,87	-0,49
	(0,889)	*(0,866)*	*(0,743)*

Notes: See Table 3.

Table 13 Results of Wald test relative to specification (4) of Table 11

VRC ACCEPTANCE ROUND 1	50%	20%	1%
20%	-0,97		
	(1,012)		
1%	0,01	1,22	
	(0,992)	*(0,812)*	
0%	-2,37**	-2,04**	-2,98***
	(1,213)	*(0,929)*	*(0,965)*

Notes: See Table 3.

Table 14: Regression Analysis of Demands for Round 1

	(1)	(2)	(3)	(4)
DEP VAR: DEMAND	FRC & 50%	FRC& 50%	VRC& 50%	VRC& 50%
CHANCE	1.051**		0.556	
	(0.406)		(0.453)	
FRC_20%		-0.00498		
		(0.225)		
FRC_1%		-0.401*		
		(0.225)		
FRC_0%		-0.611**		
		(0.259)		
VRC_20%				-0.0128
				(0.246)
VRC_1%				0.107
				(0.251)
VRC_0%				-0.731**
				(0.289)
UNFAVOURED	0.165	0.0222	0.248	-0.00397
	(0.223)	(0.239)	(0.265)	(0.282)
Constant	6.373***	6.830***	6.563***	6.830***
	(0.127)	(0.147)	(0.156)	(0.147)
Observations	219	219	208	208
R-squared	0.026	0.035	0.009	0.037

Table 15: Results of Wald test relative to specification (2) of Table 14

FRC DEMANDS ROUND 1			
	50%	20%	1%
20%	-0,005		
	(0,224)		
1%	-0,4*	-0,39*	
	(0,224)	*(0,238)*	
0%	-0,61**	-0,6**	-0,21
	(0,019)	*(0,272)*	*(0,272)*

Notes: See Table 3.

Table 16: Results of Wald test relative to specification (4) of Table 13

VRC DEMANDS ROUND 1			
	50%	20%	1%
20%	-0,01		
	(0,245)		
1%	0,11	0,12	
	(0,25)	*(0,284)*	
0%	-0,73**	-0,71**	-0,83***
	(0,288)	*(0,317)*	*(0,321)*

Notes: See Table 3.

Figure 5: Evolution of mean proposals over rounds – FRC and VRC treatments

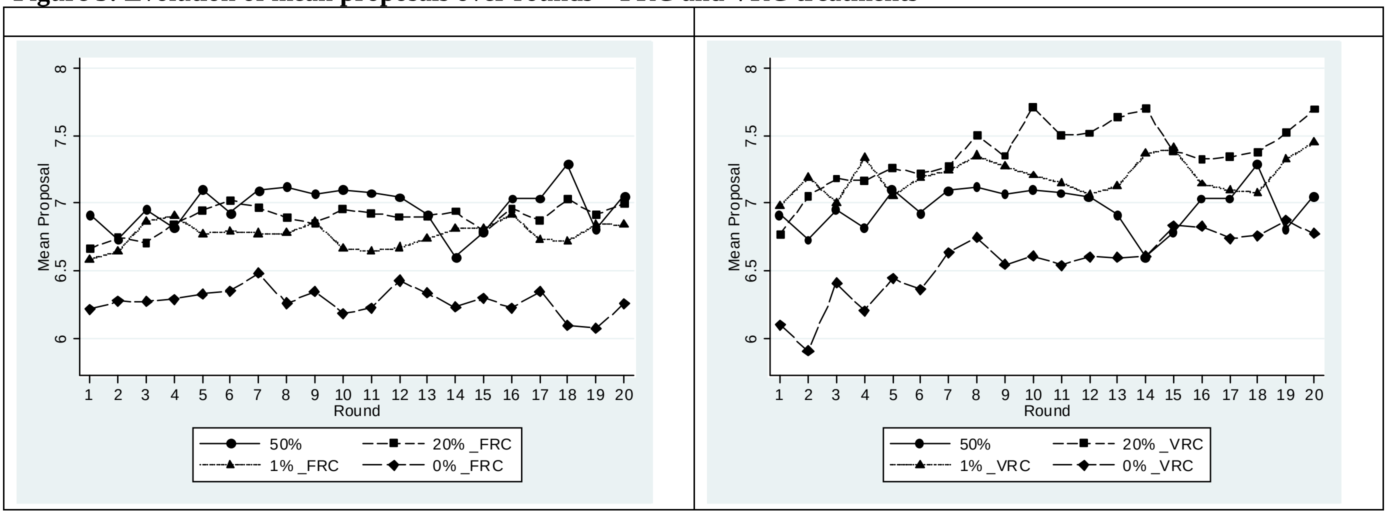


Table 17: Regression Analysis of Logit model for probability of acceptance – All Treatments

DEPENDENTE VARIABLE	ACCEPT			
	(1)	(2)	(3)	(4)
FRC_20%	-0.396		-0.150	0.233
	(0.689)		(0.786)	(1.246)
FRC_1%	-1.492**		-1.685**	-2.095
	(0.754)		(0.778)	(1.282)
FRC_0%	-3.613***		-2.262***	-5.817***
	(0.778)		(0.826)	(1.462)
VRC_20%	1.315**		1.335*	2.865**
	(0.669)		(0.788)	(1.244)
VRC_1%	0.135		0.703	0.941
	(0.673)		(0.757)	(1.215)
VRC_0%	-1.181*		-1.506**	-0.430
	(0.673)		(0.752)	(1.225)
OFFER	3.057***	3.000***	2.667***	4.983***
	(0.145)	(0.143)	(0.309)	(0.648)
FAVOURED	0.243	0.688*	-0.0240	0.500
	(0.413)	(0.400)	(0.757)	(1.011)
ROUND		0.0642**		
		(0.0315)		
FRC TREATMENTS		-1.363**		
		(0.690)		
VRC TREATMENTS		0.0458		
		(0.674)		
ROUND_X_FRC		-0.0323		
		(0.0355)		
ROUND_X_VRC		-0.000420		
		(0.0367)		
Constant	-4.740***	-5.083***	-4.318***	-7.433***
	(0.624)	(0.648)	(0.808)	(1.575)
ROUND DUMMIES	YES	NO	YES	YES
Observations	4,260	4,260	1,065	1,065
chi2	452.7	442.0	75.49	60.66
Percentage of correctly predicted outcomes	83.26%	82.61%	81.78%	85.53%

Notes: See Table 2

Table 18: Results of Wald test relative to specification (1), Table 17

		VRC ACCEPTANCE ALL ROUNDS		
		20%	1%	0%
FRC ACCEPTANCE	20%	-1.71** *(0.679)*		
	1%		-1.63** *(0.76)*	
	0%			-2.43*** *(0.773)*

Notes: See Table 3.

Table 19: Results of Wald test relative to specification (3), Table 17

		VRC ACCEPTANCE FIRST 5 ROUNDS		
		20%	1%	0%
FRC ACCEPTANCE FIRST 5 ROUNDS	20%	-1.69* *(0.930)*		
	1%		-2.95*** *(0.847)*	
	0%			-0.96 *(0.793)*

Notes: See Table 3.

Table 20: Results of Wald test relative to specification (4), Table 17

		VRC ACCEPTANCE LAST 5 ROUNDS		
		20%	1%	0%
FRC ACCEPTANCE LAST 5 ROUNDS	20%	-2.11** *(1.250)*		
	1%		-2.26** *(1.343)*	
	0%			- 3.74*** *(1.440)*

Notes: See Table 3.

Table 21: Regression Analysis of Demands – All Treatments

DEP VAR	PROPOSAL			
	(1)	(2)	(3)	(4)
FRC_20%	-0.0700		-0.0897	-0.114
	(0.158)		(0.195)	(0.150)
FRC_1%	-0.193		-0.116	-0.259
	(0.155)		(0.199)	(0.159)
FRC_0%	-0.684***		-0.594***	-0.866***
	(0.159)		(0.205)	(0.155)
VRC_20%	0.414***		0.231	0.410***
	(0.147)		(0.176)	(0.155)
VRC_1%	0.223		0.272	0.161
	(0.144)		(0.179)	(0.145)
VRC_0%	-0.415***		-0.649***	-0.275*
	(0.143)		(0.188)	(0.144)
ROUND		0.00819		
		(0.00682)		
FRC TREATMENTS		-0.255		
		(0.165)		
VRC TREATMENTS		-0.0912		
		(0.161)		
ROUND_X_FRC		-0.00562		
		(0.00793)		
ROUND_X_VRC		0.0154*		
		(0.00807)		
Constant	6.729***	6.876***	6.756***	7.083***
	(0.111)	(0.137)	(0.128)	(0.103)
ROUND DUMMIES	YES	YES	YES	YES
Observations	4,880	4,880	1,220	1,220
R-squared	0.108	0.0385	0.0819	0.142

Table 22: Results of Wald test relative to specification (1) – Table 21

		VRC PROPOSALS ALL ROUNDS		
		20%	1%	0%
FRC PROPOSAL ALL ROUNDS	20%	-2.96***		
		(0.163)		
	1%		-2.63***	
			(0.158)	
	0%			- 1.67*
				(0.161)

Notes: See Table 3.

Table 23: Results of Wald test relative to specification (3), Table 21

		VRC PROPOSAL FIRST 5 ROUNDS		
		20%	1%	0%
FRC PROPOSAL FIRST 5 ROUNDS	20%	-1.64		
		(0.195)		
	1%		-1.92*	
			(0.202)	
	0%			0.25
				(0.215)

Notes: See Table 3.

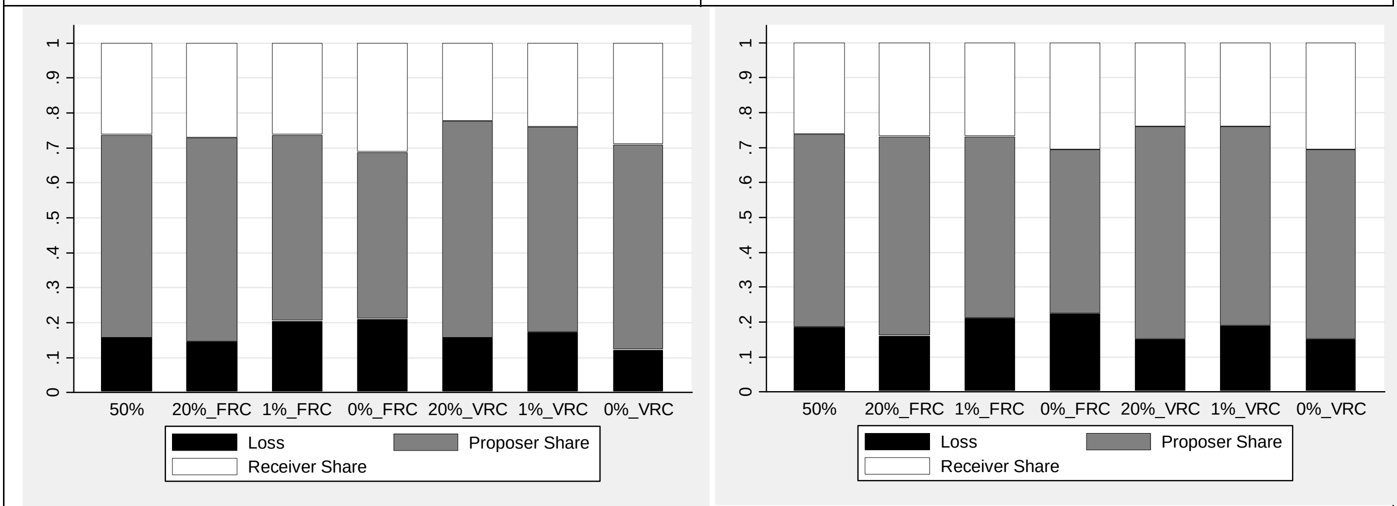
Table 24: Results of Wald test relative to specification (4), Table 21

		VRC PROPOSAL LAST 5 ROUNDS		
		20%	1%	0%
FRC PROPOSAL LAST 5 ROUNDS	20%	-3.14***		
		(0.167)		
	1%		-2.52 **	
			(0.167)	
	0%			-3.67***
				(0.161)

Notes: See Table 3.

Figure 6: Allocation per Treatments: All rounds and last five rounds

Panel a: All rounds | Panel b: Last five rounds



Examples:

Reference to a journal publication:

Van der Geer, J., Hanraads, J.A.J., Lupton, R.A., 2010. The art of writing a scientific article. J. Sci. Commun. 163, 51–59.

Reference to a book:

Strunk Jr., W., White, E.B., 2000. The Elements of Style, fourth ed. Longman, New York.

Reference to a chapter in an edited book:

Mettam, G.R., Adams, L.B., 2009. How to prepare an electronic version of your article, in: Jones, B.S., Smith , R.Z. (Eds.), Introduction to the Electronic Age. E-Publishing Inc., New York, pp. 281–304.