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Economic Theory

A stress test of fairness measures in models of social utility*

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Summary. Current social utility models posit fairness as a motive for certain types of strategic behavior. The models differ, however, with respect to how fairness is measured. Distribution models measure fairness in terms of relative payoff comparisons. Reciprocal-kindness models measure fairness in terms of gifts given and gifts received. Reference points play an important role in both measures, but the reference points in reciprocal-kindness models are conditioned on the actions available to players, whereas those in distributive models are not. Data from an ultimatum game experiment that stress tests the kindness measure is consistent with the distributive measure. Data from an experiment that stress tests the distributive measure is inconsistent with the distributive measure, but moves in the direction opposite that implied by the kindness measure. A measure that combines relative payoff comparisons with a reference point conditioned on feasible actions provides a first approximation to our data.

Keywords and Phrases: Fairness, Reciprocity, Social utility, Experiments, Bargaining, Ultimatum game.

JEL Classification Numbers: C78, C91, D63.

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> That seems blamable that falls short of the ordinary degree of proper beneficence which experience teaches us to expect of everybody; and, on the contrary, that seems praise-worthy which goes beyond it.
>
> Adam Smith (1790)

1 Social utility models and the measure of fairness issue

One of the long-standing challenges to incorporating fairness into formal economic analysis has been formulating, in a precise way, the proper measure of fairness. We investigate this issue in the context of two recent modeling approaches. The domain of both is *social utility*. Specifically, both approaches treat fairness as a motive that trades-off with material self-interest. The trade-offs are then captured by utility functions. The approaches diverge with respect to how fairness is measured. *Distribution* models (e.g., Bolton, 1991; Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000) posit that people care about how their payoff compares to that of others. *Reciprocal-kindness* models (e.g., Rabin, 1993; Dufwenberg and Kirchsteiger, forthcoming) posit that preferences over outcomes depend directly on the action of the opponent: 'people reward or punish those they believe choose a helping or hurting action.' Both kinds of models provide explanations for well-known anomalies such as rejecting money in the ultimatum game and cooperating in the prisoner's dilemma. Because of the substantially different fairness measures, however, the explanations sometimes lead us to anticipate sharply different behavior, as we will see below.[1]

Much of the literature devoted to comparing distribution and kindness models has centered on what might be termed the 'attribution hypothesis.' Kindness-based fairness, with its focus on reciprocating help or hurt, implies that fairness motivated behavior should cease when the source of the actions taken are a disinterested third party or an act of nature. Distribution models imply that people attempt to satisfy their preference for relative payoffs independent of the source of action. While the empirical literature on this subject is still rapidly expanding, the results so far imply that the issue is more complicated than was at first thought.[2]

[1] The fairness models are proximate in nature; that is, they aim to explain *how* peoples' motives lead to the behavior we observe, as opposed to explaining *why* people have the motives they do. Other proximate approaches to explaining these anomalies include learning (Gale, Binmore and Samuelson, 1995; Roth and Erev, 1995) and spite-altruism (Levine, 1998). There is a burgeoning literature on the *why*-question, most of it associated with evolutionary biology. See for example, Ellingsen (1997), Gale et al. (1995), Güth (1995), Huck and Oechssler (1999), Nowak and Sigmund (1998), Güth and Ockenfels (2000), Nowak et al. (2000), Koçkesen et al. (2000), Sethi and Somanathan (2001), among others.

[2] A well-known result often interpreted as supporting evidence for reciprocal-kindness models is that rejections in ultimatum games go down when the offer is chosen by random draw (Blount, 1995). Bolton, Brandts and Ockenfels (2003) provide evidence for an interpretation of this phenomenon based on the idea that a random offer game evokes norms of *procedural* fairness (see also Bolton and Ockenfels, 2003). Charness (1996) applied Blount's random offer-method to the gift-exchange game, basically a sequential prisoner's dilemma game. He found that while distributive fairness models "can readily account for most of the data", they cannot capture that second mover cooperation depends to some degree on whether the first move is determined by nature. In the context of a sequential dilemma game, Bolton, Brandts and Ockenfels (1998) found only secondary and statistically insignificant evidence for

Attribution, while an important issue, is nevertheless not as decisive in the measurement matter as it may at first appear. To see this, we need to delve more deeply into the fairness measures behind distribution and kindness models. These measures differ in two respects. First is metrics: distribution models rely on a measure that is explicitly relative in nature, whereas the kindness measure is more absolute – essentially a measure of gifts given versus gifts received. Second, while both measures make use of a reference point to determine what is, alternatively kind or fair, the kindness-based reference point is directly conditioned on feasible strategy/payoff opportunities, as well as beliefs about others' actions or about others' beliefs about actions. The distribution reference points are invariant to these considerations (which is not to say that strategy spaces and beliefs play no role in distribution models – they play a role in strategic behavior given utilities and reference points).

It is possible to conceive of a model with a relative metric – hence distribution-based – but with a reference point sensitive to the specifics of the strategy space. Such a model could in principle be sensitive to attribution. Likewise, one can conceive of a model based on an absolute metric – hence kindness-based – but with a reference point that is less sensitive, or sensitive in a different way, to the strategy space and to attribution than are present kindness models. The point is that characteristics like attribution speak only indirectly to the issue of the proper measure of fairness. To directly gauge the measure, we need look directly at the specific mechanics behind the measures.

A related line of work manipulates strategy spaces in ways that are strategically irrelevant but potentially fairness-relevant (Andreoni et al., 2002; Binmore et al., 2002; Bolton et al., 2000; Brandts and Solà, 2001; Falk et al., 1999; Yang et al., 1999). These studies provide clear evidence that fairness is, at least sometimes, sensitive to such manipulations. But, as several of these studies emphasize, with respect to the measurement issue, the results need be interpreted with care. Specifically, sensitivity to the strategy space is not in-and-of-itself evidence in favor of either type of measure. Below we exhibit a manipulation of the strategy space that influences behavior to move in a direction opposite that predicted by present kindness models (present distribution models incorrectly predict no change).

Rather than focus on specific models from the literature, we develop two simple *archetype* models, one distribution-based and one kindness-based. This allows us to focus in a bare-bones-of-it way on the measurement issues we wish to address, without getting bogged down in specifics unique to any single model.[3] It also

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attribution-based reciprocity (also see Brandts and Charness, forthcoming; Güth et al., 2001). Charness and Rabin (2002) distinguish *negative reciprocity* games, games where other-regarding behavior has a punishment flavor to it (such as ultimatum and other bargaining games), from *positive reciprocity* games, games where other-regarding behavior has a reward flavor to it (such as dictator and trust games). Attribution-based behavior was found to be significant only in negative reciprocity games (also see Offerman, 2002). Some studies find that the response to fair or unfair behavior is substantially invariant (Blount, 1995; Dufwenberg et al., 2001) or only weakly sensitive (Charness, 1996) to whether the action is taken by a disinterested third party or by the actual game partner.

[3] Other issues separate these models. For example, while Fehr and Schmidt (1999) assume that people strive for *egalitarian* outcomes, Bolton and Ockenfels (2000) propose a model of *self-centered* fairness in which people only care about their own monetary and relative payoffs. Also, Fehr and Schmidt

allows us to deal explicitly with certain technical problems. For instance, the games we examine are, like most similar games reported in the literature, played in the extensive form. Technically, Rabin (1993), the best known model of reciprocal-kindness, applies exclusively to normal form play. The principle concepts have nevertheless been – mostly informally – applied in many papers, to games played in the extensive form.[4] The archetypes allow us to be specific about this application, important if we are to explicitly examine the mechanics of fairness measures.[5]

The experiments we present have a bioassay design. Bioassay, common to the medical and the engineering literatures, is essentially stress testing. In a bioassay test, one estimates the probability of a response (e.g., death or product failure) as a function of some stress (e.g., dose or pressure). Our stress test involves variations on the ultimatum bargaining game. In study 1, we estimate the probability of a materially advantageous offer being rejected (interpreted as fairness-based behavior) while varying the distribution-measure stress. In study 2, we estimate the same probability while varying the kindness-measure stress. In this way, we are able to examine the influence of each type of fairness measure.[6]

The ultimatum game is a benchmark test case for any social utility theory. That said, it is a single test case, one of negative reciprocity, and so our results must be qualified accordingly.[7] The technique presented here can clearly be extended to other types of games.

2 Study 1. Stress testing the kindness measure of fairness

We frame the discussion of archetype preferences in the context of the games used in the study 1 experiment (Fig. 1). The UG game is a standard (mini-) ultimatum game in which the proposer chooses between offering an equal split of a 4-dollar pie and a split that favors self by a substantial amount. The responder views the offer and then either accepts or rejects. The UG$^*x$ games are all variations of UG,

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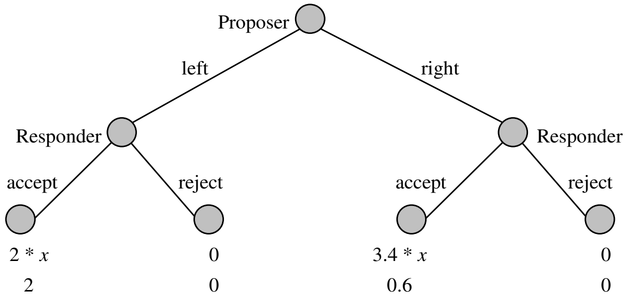
assume that payoff comparisons are measured as payoff *differences*, while Bolton and Ockenfels measure with payoff *shares* of the pie size. These more subtle distinctions are often irrelevant, as they are for the present study. There are games, however, where these differences lead to distinct predictions (see Charness and Rabin, 2002; Engelmann and Strobel, 2003; Falk et al., 1999; Bolton and Ockenfels, 1998 and forthcoming).

[4] One example of a theory of sequential reciprocity is Dufwenberg and Kirchsteiger (forthcoming).

[5] Another approach is to play the games in the normal form. Although theoretically this should have no effect, the form of play is known to at least sometimes influence behavior (e.g., Schotter et al., 1994). The reason for this is not well understood. There are pros and cons to studying both forms. In the context of the present study, playing in the normal form would optimize fidelity with the set-up of specific models, whereas playing in the extensive form optimizes comparability with the majority of past studies. It seems a good idea to explore the robustness of concepts with respect to both forms of play. For studies done in the normal form, as well as further discussion on this issue, see Bolton, Brandts and Ockenfels (1998, 2003).

[6] Bolton and Zwick (1995) is an example of an economics experiment with a bioassay design; also see Fong and Bolton (1997). The latter includes references to the general bioassay literature.

[7] It seems likely that the mechanics of other-regarding behavior within same-category games – negative or positive reciprocity games – are similar; whether the mechanics are similar across categories appears less certain (also see footnote 2).



VARIATION	LEFT		RIGHT	
	ACCEPT	REJECT	ACCEPT	REJECT
UG*0.2	0.4	0	0.6	0
	2	0	0.6	0
UG*0.6	1.2	0	2	0
	2	0	0.6	0
UG*1	2	0	3.4	0
	2	0	0.6	0
UG*1.2	2.4	0	4	0
	2	0	0.6	0

Figure 1. Ultimatum game variations from study 1

each obtained by multiplying the *proposer's* payoffs by the amount x. So UG$^*0.2$ multiplies the original payoffs of \$2 and \$3.40 by 0.2. (The resulting payoffs are rounded to the nearest tenth). In all cases, the standard subgame perfect equilibrium has the proposer (P) offering RIGHT and the responder (R) playing ACCEPT.

Since both types of models (as well as the evidence presented in Sect. 4) suggest that understanding what responders will accept or reject is the key to understanding proposer behavior, we discuss the archetype preferences in terms of responder behavior.

We begin with kindness-based fairness. The responder takes the measure of kindness after observing the proposer's offer. Let w_R and w_P be, respectively, the responder's and the proposer's monetary payoff from the game. Archetype kindness-based fairness preferences for the responder are given by:

$$u_R = \ w_R + r_R\,(k_P \cdot k_R\,) \tag{1}$$

where

a) The measure of reciprocal kindness, $r_R(\cdot)$, is a strictly increasing function of the product of the proposer's kindness towards the responder $k_P = (w_R - b_{\Omega_R})/m_{S_R}$ and the responder's kindness towards the proposer $k_R = (w_P - b_{\Omega_P})/m_{S_P}$.
b) The reference point b_{Ω_Z} is a weighted average taken over Ω_Z, the set of feasible payoffs for player Z if all offers are accepted with probability one (all weights fixed and positive).

c) The normalization factor $m_{S_Z} = \max S_Z - \min S_Z$, where S_Z is the set of *all* feasible payoffs for player Z, and $\min S_Z < \max S_Z$.[8]

By (1.b), the proposer is relatively unfair if he offers the smaller of the two amounts that are available in our games to the responder ($k_P < 0$). Since the value of reciprocal kindness is determined by the product of kindness functions, the responder tends to respond with rejecting the unkind offer yielding $k_R < 0$ and hence $k_P \cdot k_R > 0$. Generally, by (1.a), kindness-based fairness implies that the responder will tend to increase the amount of kindness he shows in response to an increase in the amount of kindness shown by the proposer. Taken together, (1.b) and (1.c) imply that the kindness functions k_Z, and so r_Z, are invariant to affine transformations of S_Z. Rabin's (1993) kindness functions are also invariant in this way. Distribution-based fairness has an analogous invariance property, as we will see in a moment.

The archetype (1) captures two important features of the fairness measures used by reciprocal-kindness models. First, fairness is measured in terms of gifts given and gifts received, where the size of the gift is measured against a reference point. Second, the measure of kindness is conditioned on *volition* – the actions open to the players. The archetype does not, however, afford a role for beliefs about what the other player will play or what the other player believes one's self will play. We omit this factor for two reasons. First, in Rabin (1993), beliefs affect the measure of kindness only when some strategies are believed played with zero probability. Given the incomplete information nature of the lab environment and the nature of the game, strictly zero probability beliefs seem unlikely. Second, obtaining reliable measures of player beliefs presents substantial challenges.[9] It seems sensible to first see how far we can go in explaining the data without relying on this factor.

The distribution models of both Fehr and Schmidt (1999) and Bolton and Ockenfels (2000) define the relative payoff reference point as fixed at an equal split of the payoff pie. The responder's social utility function can then be represented as

$$v_R = \; w_R \; + \; d_R \left(1/2 \; - \; \frac{w_R}{w_P + w_R} \right) \tag{2}$$

where

a) The measure of distributive fairness, $d_R(\cdot)$, is a strictly concave function with a maximum at the equal split – that is at $d_R(0)$.
b) $d_R = d_R(0)$ if $w_P + w_R = 0$.

[8] By (1.b), b_{Ω_P} is equivalent to the payoff that the proposer receives if the responder accepts the offer. Also, in all our games and subgames we have $\min S_Z = 0$. Hence, analogous to Rabin's (1993) model, the kindness of the responder is either $k_R = 0$ if the offer is accepted or $k_R = -1$ if rejected.

[9] Eliciting beliefs before the game is played can change the way the game is played (e.g., Croson, 2000). Eliciting beliefs after the game is played runs the risk that beliefs are formed in a way that justifies own actions depending on outcomes (see the large social psychology literature on 'cognitive dissonance' starting with Festinger, 1957). Other potential biases of reported beliefs about others' behavior include the triangle and the false consensus effect (e.g., Selten and Ockenfels, 1998). Moreover, the elicitation process itself poses problems. Common methods that elicit beliefs in an incentive compatible way, such as the quadratic scoring rule (e.g., Offerman et al., 1996), inevitably affect monetary and relative payoffs that in turn affect the fairness judgment in models of social utility.

The archetype (2) captures two important features of the fairness measures used by distribution models. First, fairness is measured in terms of relative payoff comparisons. Second, the measure of kindness is invariant with respect of characteristics on the strategy space such as volition. The archetype omits certain nuances that separate distribution models. For instance, Fehr and Schmidt explicitly assume an asymmetry property, relative payoffs being more important to a person when receiving a relatively small payoff than a relatively large one.[10]

As with the kindness measure r_Z, the distributive measure d_Z is invariant to positive affine transformations, although this time with respect to the complete payoff space. Both invariance properties follow quite naturally, given the particular measure of fairness.

From Figure 1, the responder's payoff space is fixed across all the games displayed, and the proposer's payoff space varies across any two games by a scale factor. It follows that if the responder's utility is characterized by kindness-based fairness (i.e., Eq. 1), then his utility for any given outcome (e.g., (LEFT, ACCEPT) or (RIGHT, REJECT)) is independent of the game. So, by kindness-based fairness, the responder's rejection behavior is independent of the game.

As to distribution-based fairness, inspection of the UG$^*x$ games in Figure 1 shows that, for proposal RIGHT, rejection rates should increase in x, and for proposal LEFT, rejection rates should be non-increasing through $x = 1$ and should then increase for $x = 1.2$. An asymmetry property, such as supposed by Fehr and Schmidt, would lead us to expect that the change in rejection rates will be more pronounced for RIGHT than for LEFT.

In sum, the kindness measure of fairness implies that rejection rates should be independent of the game in Figure 1, whereas the relative payoff model implies they should change in the manner stated. So study 1 stress-tests the kindness measure of fairness by holding this measure constant while varying the distributive measure.[11]

2.1 Sessions and laboratory protocol

A total of 120 subjects participated in the experiments reported in this paper, with 60 participating in study 1. All subjects were recruited through advertisements posted around the Penn State campus. The chance to earn cash was the only incentive offered. Study 1 consisted of three sessions of 20 subjects each. No subject participated in more than one session.

The entire laboratory protocol is reprinted in Appendix A; here we summarize: Within each session, subjects were assigned fixed roles, half proposers and

[10] Bolton and Ockenfels do not restrict their model in this way. While this kind of asymmetry appears to be an intuitively reasonable characteristic of fairness preferences, the assumption that *everybody* judges fairness this way is inconsistent with some empirical evidence such as in the centipede game (see the discussion in Bolton and Ockenfels, 2000, p. 180).

[11] Falk et al. (1999) report the results of related ultimatum game experiments. While most of their observations appear to be compatible with our results, there are important differences in the objectives of the papers that are reflected in the different experimental designs. For instance, in Falk et al. both proposers' *and* responders' monetary payoff spaces are changed across games (something that admits additional explanation for changes in rejection rates), all games are played in normal form, and there was no opportunity for learning.

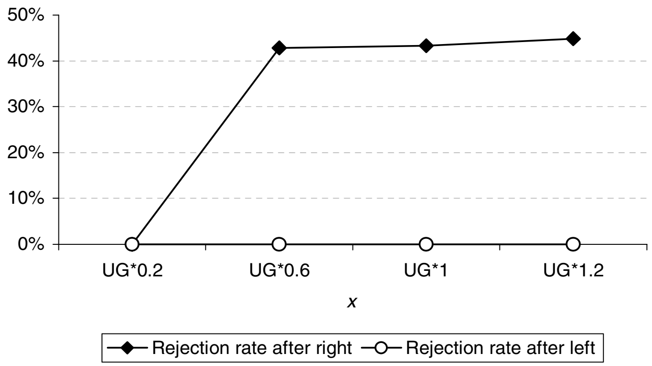


Figure 2. Average rejection rates for UG$^*x$

half responders. Each proposer played one game with each responder in random sequence. Games were played via computer, and matching was anonymous. In order to exclude dynamic effects due to strategic reputation building, no pair was ever matched twice (Appendix B shows the exact matching order). We bundled two types of games in each session. One was always UG^{*1}. The other, one of the three games listed in Figure 1, varied with the session. This permits us to make both within and between subject statistical comparisons. The common link game (UG^{*1}) provides a comparison of UG behavior across sessions and rounds controlling for possible contagion effects. Each session consisted of 10 rounds. We always started with UG^{*1} and then alternated games so that each game is played 5 times, thereby allowing for learning. Alternating games enabled us to separate learning and convergence effects from motivational effects.[12] All games were played in extensive form. Subjects knew the outcome of the games they played but not the outcome of any other games. At the end of the session, all games played were paid according to the payoff matrix in Figure 1 plus a $7 show-up fee.

2.2 Study 1 results

The complete data set appears in Appendix B. We focus here on rejection behavior. First mover behavior is taken up in Section 4.

Figure 2 shows rejection rates averaged across subjects and rounds. Since there are no statistically significant differences in rejection rates in UG^{*1} across the three sessions (the corresponding χ^2-test yields $p = 0.659$), we pooled the UG^{*1} data.

Rejection rates after RIGHT increase with x, although the increase is far stronger from $x = 0.2$, where the rejection rate is 0, to $x = 0.6$, where the rate is 43 percent; thereafter rejection rates increase but mildly, ultimately to 45 percent for $x = 1.2$. Within subject comparisons of rejection rates after RIGHT between UG^{*1} and UG$^*x$ yield significance on the 5 percent level for $x = 0.2$ (two-sided Wilcoxon Signed

[12] Suppose game A and game B had each been played for 5 rounds consecutively beginning with A, and suppose that play in game B was closer to equilibrium play. Then it would not be clear whether this was because subjects learned to play equilibrium over time or because social utility dictates this effect.

Table 1. Responder behavior in UG*x after an offer of RIGHT

Probit estimates (and two-sided p-values) for rejection behavior after RIGHT Dependent variable = "1" for rejection		
Independent variables	Model 1	Model 2
CONSTANT	-4.311 (0.000)	-4.377 (0.000)
x	3.723 (0.000)	3.776 (0.000)
ROUND	-0.077 (0.370)	
ODDROUND (= ROUND if odd, "0" else)		0.062(0.536)
EVENROUND (= ROUND if even, "0" else)		0.084 (0.333)
RHO (random effects)	0.913 (0.000)	0.915 (0.000)
Number of observations	184	184
Log-likelihood	-63.182	-63.130

Ranks Test), but not for $x = 0.6$ and 1.2. Across all games, LEFT was rejected just once, suggesting that, favorable inequality is generally more acceptable than unfavorable inequality.

Table 1 reports maximum likelihood probit estimates concerning responder behavior after RIGHT, controlling for individual heterogeneity and round effects. The highly significant coefficient for x indicates that the higher the multiplier of the proposers' payoffs, the more likely are responders to reject RIGHT. Recall that x alters neither the absolute payoffs of the responders, nor the kindness of the proposers' offers, so that we can conclude that rejections are driven by relative payoffs.[13] Also, individual subject differences in the basic tendency to reject are clearly present, as indicated by RHO.

Figure 2 raises the question of why the increase in rejection rates is so much more pronounced from $x = 0.2$ to $x = 0.6$ than it is from $x = 0.6$ to $x = 1.2$. One potential explanation has to do with the fact that the RIGHT proposal for $x = 0.2$ constitutes an equal division offer, whereas for all the other games, it constitutes a strongly unequal offer. For UG*0.2, a RIGHT proposal offers the responder half the pie (consistent with the distributive models, this proposal is accepted by all). In contrast, for UG*0.6, a RIGHT proposal offers the responder just 23 percent of the pie; for UG*1.2 it is 13 percent. Plausibly, for those who care about fairness, 23 percent of the pie is highly objectionable but not much less objectionable than 13 percent. We note, however, that Güth et al. (2001) observed that even very small deviations from the equal split result in strong resistance on the responder side, while the equal split is never rejected. The authors give a focal point interpretation for this effect. Translated to our experiment, this would suggest that even a slight movement away from the equal division in $x = 0.2$ would lead rejection rates to move up to a relatively high level. Whether such a phenomenon is consistent with fairness models depends on how the models are parameterized; in particular, it depends on the marginal rate of substitution between absolute and relative payoff.

[13] x also affects the efficiency of an offer, i.e., the pie size to be distributed. Recent evidence (Charness and Rabin, 2002) suggests that some subjects might be driven by efficiency considerations among other motives. Efficiency considerations alone, of course, cannot explain rejections since they always yield an efficiency loss. But efficiency considerations also cannot capture our comparative statics since rejection rates are *positively* correlated with the associated efficiency loss (see also Bolton and Ockenfels, 2003).

By Table 1, the only influence on rejection frequencies that is negligible and not significant is the round of play. If we suppose that people learn exclusively through reinforcement to their own payoffs (i.e., Roth and Erev, 1995), Model 1 is appropriate (recall that the responder's absolute payoffs are independent of x). If we additionally assume that initial behavior differs across games, that is, varies with x, Model 2 tests for any change of rejection behavior over time *within* games since a given game is either played in even or in odd rounds. In neither case is there a statistically or economically meaningful round effect. Note, however, that since games are alternated, the strong game effect as captured by the variable x implies that behavior systematically alternates over time.

A similar conclusion holds if we look more closely at the individual level. For instance, we count 29 cases out of 78 cases in which the same responder reacts differently to the same offer in the same game.[14] Out of these 29 cases 15 occur in the standard ultimatum game, UG* 1, when the unfair split is offered. However, these changes of behavior seem to be unsystematic: seven responders reject more often in the second half of the experiment, seven reject less often, and one does not change his frequencies. Controlling for individual experience we find that the average number of unfair offers responders experience in the first half of the experiment is the same for those who reduce as for those who increase their tendency to reject in the second half of the experiment. These changes of rejection strategies over time are plausibly due to noise or indifference. Thus, for our study, learning is at best only a minor influence on responder behavior (this is not true for proposer behavior; see Sect. 4).[15]

To summarize the main findings from study 1, the data moves much as we would expect from the distribution-based preferences. The offer of LEFT is almost never rejected, independent of the variation of UG*x played. Rejection rates for the offer of RIGHT are zero for $x = 0.2$ and substantially higher, though only weakly increasing, for $x > 0.2$.

3 Study 2. Stress testing the distributive measure of fairness

We now stress test distribution-based fairness by holding the distributive measure of fairness invariant while manipulating the kindness measure. The games for the test, variations of the same UG game used for study 1, are displayed in Figure 3. In these games, the LEFT pie division is manipulated by redistributing the pie by an amount y, holding the pie size fixed and the RIGHT division fixed at (3.4, 0.6).

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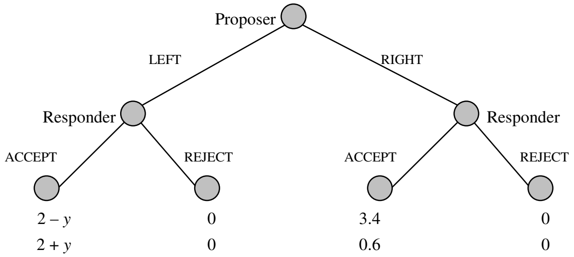
[14] In total, 30 responders played two games that each had two subgames, so that behavior changes can potentially be observed in 120 cases. However, in 42 of these cases, the responder always faced the same offer, so that a change of behavior can actually only be observed in 78 cases.

[15] Because much less subjects change their rejection behavior over time in the three other games, other than UG* 1, meaningful statistical analyses of individual rejection dynamics are not possible there. Previous studies of games similar in nature to the ones we study here also report no or small learning effects on the part of responders; e.g., Bolton and Zwick (1995), Kagel et al. (1996), Duffy and Feltovich (1999), and Abbink et al. (2001). However, adaptation may have a better chance over an extended number of repetitions. Cooper et al. (forthcoming) find a small learning effect on the part of UG responders over 50 rounds (as compared to our five rounds per game). They affirm that "no experiment to date has reliably observed learning on the part of [ultimatum game] responders."



variation	LEFT		RIGHT	
UG + y	ACCEPT	REJECT	ACCEPT	REJECT
UG–1.6	3.6	0	3.4	0
	0.4	0	0.6	0
UG+0	2	0	3.4	0
	2	0	0.6	0
UG+0.8	1.2	0	3.4	0
	2.8	0	0.6	0
UG+1.6	0.4	0	3.4	0
	3.6	0	0.6	0

Figure 3. Ultimatum game variations from study 2

Distribution-based fairness predicts that the rejection rates of the RIGHT offer will be the same across games (compare with Eq. 2). Rejections of the LEFT offer should decrease from $y = -1.6$ to 0. After this, the response is ambiguous since the absolute payoff increases while the relative payoff decreases.

Intuitively, reciprocal kindness-based fairness implies that rejection of RIGHT should increase in y because offering 0.6 becomes less kind, and accepting such an offer becomes more kind, as the value of the alternative offer increases. By the same reasoning, rejection of the LEFT proposal should fall as y increases. Straightforward calculation, using Eq. (1) and Figure 3, formally verifies these predictions.

3.1 Laboratory protocol

Save for changes in the game payoffs, the protocol was identical to that for study 1 (see Sect. 2.1). The number of subjects per session, the subject pool demographics, and all procedures including matching, game, and payoff procedures were the same as for study 1. The common link game is now UG+0 (equivalent to UG*1 in study 1).

3.2 Study 2 results

Figure 4 shows average rejection rates in study 2. As in study 1, there are no statistically significant differences between rejection rates in the standard ultimatum game (UG+0) across the three sessions (the corresponding χ^2-test yields $p = 0.393$), so that we can pool the UG+0 data in Figure 4.[16]

[16] The average rejection rate is 43 percent in UG*1 and 45 percent in UG+0. The numbers are comparable to other studies of ultimatum game behavior that use mini-games; e.g., Bolton et al. (2000) with 41 percent, and Falk et al. (1999) with 45 percent.

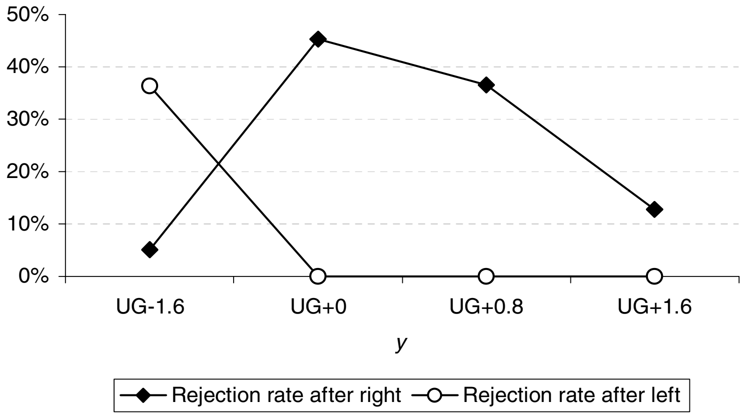


Figure 4. Average rejection rates for UG+y

Table 2. Responder behavior in UG+y after an offer of RIGHT

Probit estimates (and two-sided p-values) for rejection behavior after RIGHT Dependent variable = "1" for rejection		
Independent variables	Model 1	Model 2
CONSTANT	-0.233 (0.675)	-0.397 (0.597)
y	0.370 (0.100)	0.391 (0.157)
$\lvert y \rvert$	-1.141 (0.000)	-0.965 (0.048)
ROUND	-0.004 (0.956)	
ODDROUND (= ROUND if odd, "0" else)		0.046 (0.783)
EVENROUND (= ROUND if even, "0" else)		0.021 (0.780)
RHO (random effects)	0.678 (0.000)	0.689 (0.000)
Number of observations	178	178
Log-likelihood	-72.107	-71.9

The figure shows rejection rates after RIGHT decreasing in the absolute value of y. This is inconsistent with the predictions of both models. Within subject comparisons of rejection rates after RIGHT between UG+0 and UG+y, applying a two-sided Wilcoxon rank test, yields $p = 0.044$ for $y = -1.6$ (10 observations), 0.109 for $y = 0.8$ (8 observations) and 0.144 for $y = 1.6$ (8 observations).[17] LEFT is only rejected for $y = -1.6$.

Table 2 displays a random effects probit analysis of the responder behavior after RIGHT.

[17] Less than ten observations occur because some responders exclusively faced RIGHT or exclusively faced LEFT offers so that comparison of rejection rates are not possible.

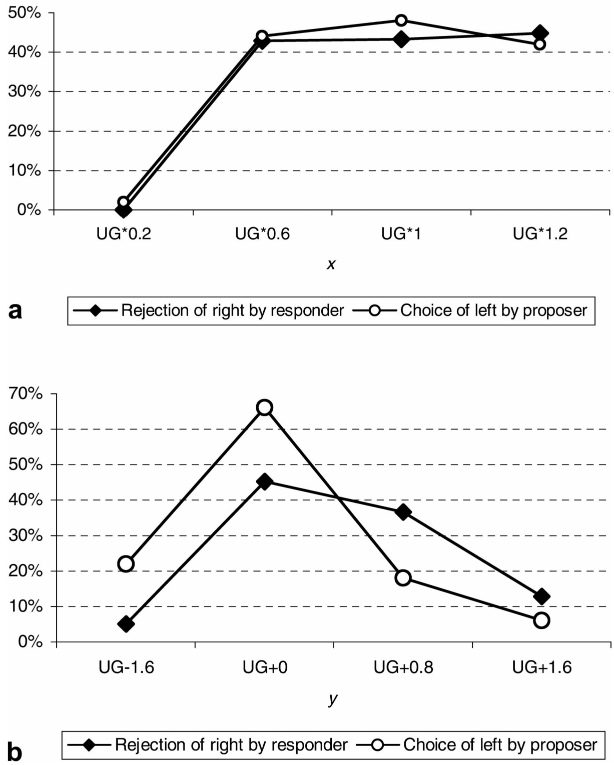


Figure 5a,b. Proposer behavior in response to rejection behavior

The only strongly significant influence on the rejections of RIGHT is $|y|$.[18] This result clearly contradicts the measures proposed by both distributive and kindness fairness. A reasonable intuition for why rejections are maximal for $y = 0$ and decrease as y moves away from zero is that, with the removal of the 2-2 proposal, judgment of what is the fair outcome shifts to some other proposal – plausibly to the proposal that is closest to an equal split. Then, since for any $y \neq 0$ the equal split is not feasible, unequal distributions become increasingly in $|y|$ acceptable. It also explains why LEFT is only rejected for $y = -1.6$. Only this case leads the LEFT-offer to be more unfavorable, inequality-wise, than the alternative offer.

To summarize the main findings from study 2: Consistent with both models, rejection rates for offers of LEFT are nonincreasing in y. The rise-and-fall pattern of rejection rates for an offer of LEFT violates the distribution model's prediction of no change. But the fall in rates also violates the kindness model's prediction of steady increase.

[18] As before, the coefficient for RHO indicates significant individual heterogeneity and, while the influence of the round variables is negligible (and there is also no indication for learning when analyzing individual responder histories), the $|y|$-effect implies that behavior is alternated over time. We also note that if one removes y from either model, all remaining coefficients and significance levels are practically unchanged.

4 First mover behavior

In this section we briefly show that first movers strategically respond to rejection rates. Figure 5 shows rejection rates after RIGHT by responders together with the percentage of LEFT-choices by the proposers.

The graphs illustrate that (average) proposer behavior closely follows (average) responder behavior. In particular, virtually all proposers choose RIGHT in UG*0.2 (LEFT was chosen only once). Combined with responder behavior – in which there were no rejections – 98 percent of all UG*0.2 plays are in (standard) subgame perfect equilibrium. This is in line with the prediction made by the distribution models.[19]

The within subject comparisons of the LEFT-rate confirms the effects illustrated in Figure 5.[20] However, there is a lot of variance in the first mover behavior across sessions[21] and time. In Table 3, we use probit analyses to examine proposer learning, controlling for individual heterogeneity.

Proposer behavior systematically mirrors responder behavior. While rejection rates of RIGHT increase in x and decrease in $|y|$, the rates of choosing RIGHT decrease in x and increase in $|y|$.[22] In addition, the proposers' rate of choosing RIGHT (but not the responders' rejection rate; see Table 2) depends on y, presumably reflecting that the proposers' absolute payoffs associated with choosing RIGHT are increasing in y (while the responders' absolute payoffs associated with accepting RIGHT are independent of y).

In contrast to responders, proposers adjust their behavior over time (as observed in earlier studies, see e.g., Roth and Erev, 1995). In particular, our studies reveal that proposers *learn* from their experience within the *same* game, as indicated by the coefficient for SAME, but also *transfer* their experience from one game to the *other* game, as indicated by the coefficient for OTHER. In both studies, the coefficients for SAME are somewhat larger than for OTHER. While within each study, these differences are not significant, pooling the data across studies and modeling as in Table 3, the differences are significant at the one percent level. This suggests that experience in the same game is more valuable information than experience in the other game (as opposed to the alternative hypothesis that the more recent experience

[19] If one accepts that distributive fairness preferences adequately capture individual motivations in UG*0.2, equilibrium strategies and equilibrium beliefs about other players' strategies are perfectly controlled at the individual level, since (RIGHT, ACCEPT) is the unique subgame perfect equilibrium path in these models. Following Weibull (2002), UG*0.2 is then an appropriate test of whether subgame perfection is a descriptively meaningful solution concept of simple ultimatum games. Our results indicate that this is the case.

[20] The two-sided Wilcoxon rank test yields $p = 0.004$ for UG*0.2, 0.054 for UG*0.6, 0.454 for UG1.2, 0.088 for UG−1.6, 0.005 for UG+0.8, and 0.010 for UG+1.6.

[21] The average percentages of RIGHT choices in the standard ultimatum game are 52, 38 and 66 percent in study 1 and 48, 22 and 32 percent in study 2. For both studies, we can reject the null hypothesis that first mover behavior is the same in the ultimatum game across sessions at the 5 percent level.

[22] This interpretation of the probit analysis is somewhat incomplete since it does not take into account that the choice of the proposer depends on the rejection rates after both LEFT and RIGHT. However, rejection rates after LEFT are (almost) always zero with the one exception of UG−1.6. The fact that rejection rates in UG−1.6 are not only low after RIGHT but also large after LEFT is likely to have strengthened the effect that the LEFT-choice is rarely made.

Table 3. Proposer behavior

Probit estimates (and two-sided p-values) for Proposer behavior
Dependent variable = "1" for RIGHT

Independent variables	UG$^*x$		UG+y	
CONSTANT	2.627	(0.000)	−0.148	(0.556)
x	−2.077	(0.000)		
y			0.745	(0.000)
$\|y\|$			1.371	(0.000)
OTHER*	−0.765	(0.015)	−0.999	(0.025)
SAME**	−1.014	(0.001)	−1.014	(0.001)
RHO (random effects)	0.487	(0.000)	0.530	(0.000)
Number of observations	300		300	
Log-likelihood	−158.7		−136.9	

* OTHER = percentage of rejections in the *other* game in previous rounds if RIGHT was chosen.
** SAME = percentage of rejections in the *same* game in previous rounds if RIGHT was chosen.

– that is gained in the other game – has a bigger impact). This makes sense given the social utility theories and given the responder behavior observed in our studies. If preferences are stable across games, the history of the same game is almost always valuable information about the rejection-behavior of a given population in this game (with the exception of UG$^*0.2$ where acceptance maximizes both absolute and relative payoffs and therefore, theoretically and empirically, has no information value about how fair the responder population is). The history of the other game, however, is only 'sometimes' valuable information. As an example observe that distributive fairness models predict that if a responder rejects RIGHT in UG$^*0.6$ she will also reject RIGHT in UG^{*1}, since the offer implies a worse relative but the same absolute payoff, but not the other way around.

5 Summary and one observation

In study 1 we found that varying the relative payoffs, while holding the responder's monetary payoffs fixed, influences the acceptability of offers in a way that distributive fairness predicts. In study 2, we found that varying the relative payoffs of one offer, while holding both the monetary and relative payoffs of the other offer fixed, influences the acceptability of the other offer in a way that neither the distribution nor kindness archetypes predict. To put it succinctly, our data indicates that both relative shares and volition (the available actions) matter.

A natural way to approach our results would be to retain the relative metric of distribution models but to condition the reference point on the offers available in the bargaining game as kindness models do. Return to Figures 2 and 4 and observe that, for each game, the offer for which rejections are zero, or close to zero, is the offer for which the settlement proportions are closest to 50–50. A reasonable

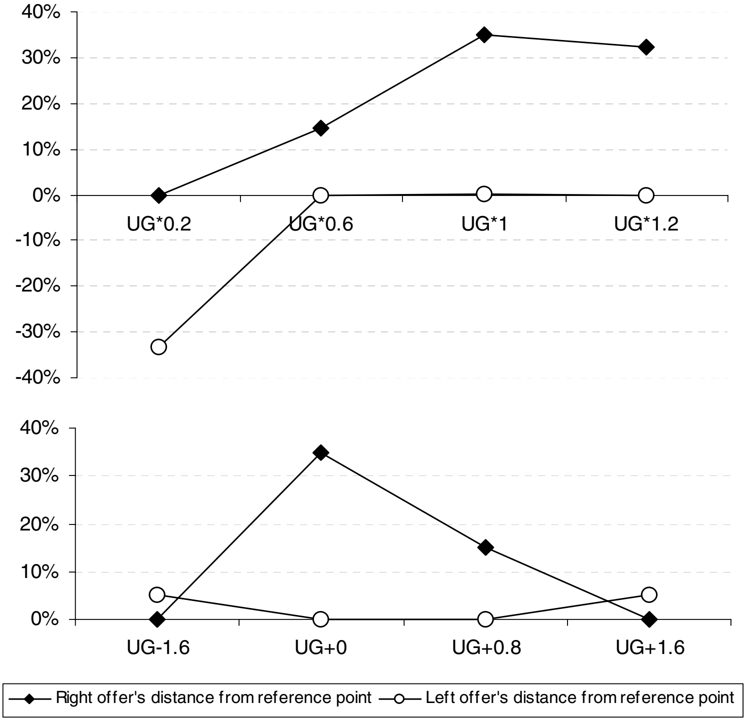


Figure 6. Offer distance from the feasible offer closest to an equal split. The distance is measured as in Eq. (2). So positive values indicate the relative share of the offer is *smaller* than the reference point, and negative values indicate larger

conjecture is that, absent a feasible equal division, the offer closest to equal division substitutes as the reference point for a fair settlement.

Figure 6 shows the distance of each offer from the substitute reference point, where the substitute reference point is tabulated as the smaller share.[23] The graphs track the pattern of rejection rates in Figures 2 and 4 rather closely, even though the impact of absolute payoffs has been neglected (see Eq. (2)).[24]

Over all, this rather simple measure, built on the assumption the reference point is the feasible offer closest to 50–50, provides a reasonable first approximation to the trend in rejection rates we observe in our data. A good deal of work would have to be done to flesh out a model that could be formally taken to the ultimatum

[23] So if the offer closest to 50–50 is a $60 - 40$ split, the substitute reference point is tabulated as 0.4. The distance between an offer of 25–75 and the substitute reference point is calculated as suggested by Eq. (2): $0.4 - 0.75 = -0.35$.

[24] Among the offers closest to an equal split, few (five offers, or less than 2 percent) are rejected. All cases occur in UG$+1.6$ and UG-1.6, the games in which the possible offers are almost an identical distance from 50–50. Also, Figure 6 suggests that – when neglecting absolute payoffs – we should expect positive rejection rates after LEFT in UG$^*0.2$ and UG$+1.6$, while in fact rejection rates are zero. There is, however, just one observation for a LEFT offer in UG$^*0.2$ and only three observations in UG$+1.6$. Further, in all 4 cases, the absolute payoff associated with a LEFT offer is rather large which might diminish the tendency to reject.

game data and to data for other games. But judging from the present data set, the approach is worth investigating.

Appendix A: Laboratory protocol

This section contains a description of all procedures, as well as verbal and written instructions for all sessions of the experiment. The monitor read all verbal instructions directly from the protocol. The only monitor-subject communication not included in the protocol are the answers to individual subject questions (answers were given in private).

1. Terminals are setup so that odd numbers (Player FM's) are on one side of the room, while even numbers (Player SM's) are on the other.
2. As subjects arrive, each gets a randomly assigned cubicle number.
3. Twenty – must be an even number – subjects enter. They sit at cubicles that match numbers. Monitor announces: Please take the sheet marked 'Instructions' out of your folder and read them carefully (see Instruction below). At the end, the instructions will ask you to read and sign the two copies of the Consent form. Please do this, and then wait quietly for the next step.
4. When all are done reading the instructions, the monitor reads them aloud. Human Subject Consent forms are collected.
5. Monitor reads paragraph about practice games from instruction. Subjects play practice games. After about 5 minutes from start of practice games, monitors go around to see if there are any questions.
6. Games are played sequentially with monitor announcing each new game (see below). After games are complete fill out check out forms. Pay subjects at cubicles and have them leave when finished.

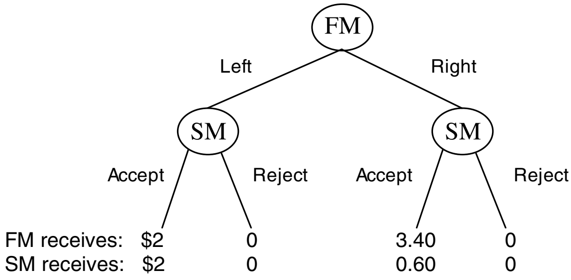
Prior to the beginning of each game announce:

New game is queued from computer and monitor announces: That completes Game $X-1$, we are ready to begin Game X. Before you begin play, please fill out the game record. Fill in the game #, your role, and the potential payoffs. You will be matched with someone you have not previously been matched with. Ok, you may begin.

Written instructions given to subjects:

The purpose of this study is to assess how people make decisions in simple economic situations. If at any time you have questions or problems, raise your hand and a monitor will be happy to assist you. From now until the end of the session, unauthorized communication of any nature with other participants is prohibited.

You will be paid a \$7 cash show-up fee. During the session you will participate in a series of games. Each game gives you the opportunity to earn additional cash.



Description of a game. The games you will play all involve bargaining between two players, a First Mover (FM) and a Second Mover (SM). A bargaining game is illustrated in the game tree diagram above. The First Mover offers either Left or Right. After viewing the First Mover's choice, the Second Mover either *Accepts* or *Rejects*. The two selections together determine the payoff of each bargainer. For the game in the diagram:

First mover offers	&	Second mover chooses	→	First mover receives	Second mover receives
Left	&	Accept	→	$2	$2
Left	&	Reject	→	$0	$0
Right	&	Accept	→	$3.40	$0.60
Right	&	Reject	→	$0	$0

Conduct of the session. The games will be played sequentially, one after another. You will have the same role, First Mover or Second Mover, for all games. You will be told your role at the beginning of the first game. The payoffs at the bottom of the game tree may differ or may be the same from one game to another. The payoffs at the bottom of the game tree will be displayed on your computer screen at the beginning of each game. The sequence of payoffs is predetermined, and does not depend on anyone's actions during the session.

Pairing procedure. You will play each game with a different participant in this room. You will never play with the same participant twice. All pairings are anonymous: Your identity will not be revealed to the person you are playing with either before, during or after the game.

Game history. Your folder includes a blank "Game Record." At the beginning of each game, write the game number and potential payoffs in the space provided. A history of the play of each game, the move of each player and the associated payoff, will appear in a box on your computer screen. You may reference any of this information any time during the session. There is a piece of scratch paper in your folder for any additional notes you might wish to make.

Money earnings. At the end of the session, you will be paid the $7 show-up fee plus your earnings from all the games. In order to collect your earnings you must stay until the end of the session, a maximum of 60 minutes. You will be paid in cash. Earnings are confidential information: Only you and the monitor will be told the amount that you make.

Practice games. When the monitor gives the OK, you should play some practice games. Your bargaining partner for the practice games will be the computer. It has been programmed to choose its moves at random. The practice games will allow you to experience the game from both the First Mover's and Second Mover's perspective. Practice until you feel comfortable with the game and its rules.

Consent forms. If you wish to participate in this study, please read and sign the accompanying consent form.

Appendix B: Data

Study 1							Study 2						
Session #	Round	Game (x)	Proposer #	Responder #	Right = 1	Reject = 1	Session #	Round	Game (y)	Proposer #	Responder #	Right = 1	Reject = 1
1	1	1	1	2	0	0	4	1	0	1	2	1	1
1	2	0,2	1	4	1	0	4	2	−1,6	1	4	1	1
1	3	1	1	6	0	0	4	3	0	1	6	0	0
1	4	0,2	1	8	1	0	4	4	−1,6	1	8	1	0
1	5	1	1	10	1	0	4	5	0	1	10	1	0
1	6	0,2	1	12	1	0	4	6	−1,6	1	12	1	0
1	7	1	1	14	1	1	4	7	0	1	14	1	1
1	8	0,2	1	16	1	0	4	8	−1,6	1	16	1	0
1	9	1	1	18	0	0	4	9	0	1	18	0	0
1	10	0,2	1	20	1	0	4	10	−1,6	1	20	1	0
1	1	1	3	4	0	0	4	1	0	3	4	1	0
1	2	0,2	3	6	1	0	4	2	−1,6	3	6	0	0
1	3	1	3	8	0	0	4	3	0	3	8	1	1
1	4	0,2	3	10	1	0	4	4	−1,6	3	10	0	0
1	5	1	3	12	0	0	4	5	0	3	12	1	1
1	6	0,2	3	14	1	0	4	6	−1,6	3	14	1	0
1	7	1	3	16	0	0	4	7	0	3	16	1	1
1	8	0,2	3	18	1	0	4	8	−1,6	3	18	1	0
1	9	1	3	20	0	0	4	9	0	3	20	1	0
1	10	0,2	3	2	1	0	4	10	−1,6	3	2	1	0
1	1	1	5	6	1	0	4	1	0	5	6	1	0
1	2	0,2	5	8	1	0	4	2	−1,6	5	8	1	0
1	3	1	5	10	1	0	4	3	0	5	10	1	0
1	4	0,2	5	12	1	0	4	4	−1,6	5	12	1	0
1	5	1	5	14	1	1	4	5	0	5	14	1	1
1	6	0,2	5	16	1	0	4	6	−1,6	5	16	1	1
1	7	1	5	18	1	1	4	7	0	5	18	1	0
1	8	0,2	5	20	1	0	4	8	−1,6	5	20	1	0
1	9	1	5	2	1	0	4	9	0	5	2	1	1
1	10	0,2	5	4	1	0	4	10	−1,6	5	4	1	0
1	1	1	7	8	1	0	4	1	0	7	8	0	0
1	2	0,2	7	10	1	0	4	2	−1,6	7	10	1	0
1	3	1	7	12	1	1	4	3	0	7	12	0	0
1	4	0,2	7	14	1	0	4	4	−1,6	7	14	1	0

1	5	1	7	16	1	0	4	5	0	7	16	0	0
1	6	0,2	7	18	1	0	4	6	−1,6	7	18	1	0
1	7	1	7	20	1	1	4	7	0	7	20	0	0
1	8	0,2	7	2	1	0	4	8	−1,6	7	2	1	0
1	9	1	7	4	1	0	4	9	0	7	4	0	0
1	10	0,2	7	6	1	0	4	10	−1,6	7	6	1	0
1	1	1	9	10	0	0	4	1	0	9	10	0	0
1	2	0,2	9	12	1	0	4	2	−1,6	9	12	0	1
1	3	1	9	14	0	0	4	3	0	9	14	0	0
1	4	0,2	9	16	1	0	4	4	−1,6	9	16	1	0
1	5	1	9	18	1	1	4	5	0	9	18	0	0
1	6	0,2	9	20	1	0	4	6	−1,6	9	20	1	0
1	7	1	9	2	0	0	4	7	0	9	2	0	0
1	8	0,2	9	4	1	0	4	8	−1,6	9	4	1	0
1	9	1	9	6	0	0	4	9	0	9	6	0	0
1	10	0,2	9	8	1	0	4	10	−1,6	9	8	1	0
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1	7	1	11	4	1	0	4	7	0	11	4	0	0
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1	5	1	13	2	0	0	4	5	0	13	2	1	1
1	6	0,2	13	4	1	0	4	6	−1,6	13	4	1	0
1	7	1	13	6	0	0	4	7	0	13	6	1	0
1	8	0,2	13	8	1	0	4	8	−1,6	13	8	0	1
1	9	1	13	10	0	0	4	9	0	13	10	0	0
1	10	0,2	13	12	1	0	4	10	−1,6	13	12	1	0
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1	7	1	17	10	0	0	4	7	0	17	10	1	0
1	8	0,2	17	12	1	0	4	8	−1,6	17	12	0	1
1	9	1	17	14	1	1	4	9	0	17	14	1	1

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1	2	0,2	19	2	1	0	4	2	−1,6	19	2	1	0
1	3	1	19	4	0	0	4	3	0	19	4	0	0
1	4	0,2	19	6	1	0	4	4	−1,6	19	6	1	0
1	5	1	19	8	1	0	4	5	0	19	8	1	1
1	6	0,2	19	10	1	0	4	6	−1,6	19	10	0	0
1	7	1	19	12	1	1	4	7	0	19	12	1	1
1	8	0,2	19	14	1	0	4	8	−1,6	19	14	0	1
1	9	1	19	16	0	0	4	9	0	19	16	0	0
1	10	0,2	19	18	1	0	4	10	−1,6	19	18	0	0
2	1	1	1	2	1	0	5	1	0	1	2	0	0
2	2	0,6	1	4	1	0	5	2	0,8	1	4	1	1
2	3	1	1	6	1	0	5	3	0	1	6	0	0
2	4	0,6	1	8	1	1	5	4	0,8	1	8	0	0
2	5	1	1	10	1	1	5	5	0	1	10	0	0
2	6	0,6	1	12	1	0	5	6	0,8	1	12	1	0
2	7	1	1	14	0	0	5	7	0	1	14	1	0
2	8	0,6	1	16	1	1	5	8	0,8	1	16	1	0
2	9	1	1	18	0	0	5	9	0	1	18	1	0
2	10	0,6	1	20	1	1	5	10	0,8	1	20	1	0
2	1	1	3	4	0	0	5	1	0	3	4	0	0
2	2	0,6	3	6	1	0	5	2	0,8	3	6	0	0
2	3	1	3	8	1	1	5	3	0	3	8	0	0
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