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Where do fairness preferences come from? Norm transmission in a teen friendship network

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People's preferences about the fair distribution of resources vary within and between different populations, and this affects many economic and political outcomes. We argue that a source of these differences is the social transmission of fairness norms from peers during adolescence. We ran an experiment on transmission of fairness norms in a friendship network of 11-15 year olds. Observing others' choices affects young people's fairness norms, as expressed in both their own choices and the attitudes they express. Our results show how young people can adopt redistributive norms via the social influence of their peer group. We also examine how the strength of social influence varies with friendship status and network position.

Introduction

As well as their own self-interest, humans care about fairness in economic exchanges and the distribution of benefits. This affects behaviour in bargaining (Güth, Schmittberger, and Schwarze 1982), the labour market (Akerlof 1982), consumer good markets (Kahneman, Knetsch, and Thaler 1986) and politics (Alesina and Giuliano 2009). What is more, different ideas of distributive fairness circulate in society. Among others, the egalitarian conception is that fairness means equal shares for all, while the meritocratic concept is that goods are distributed in proportion to merit or desert. These ideas have different implications for economic organization: in a fully egalitarian society, everyone would get an equal share, while a meritocracy might distribute returns proportionally to marginal product. Not surprisingly, support for these views varies. The poor are more egalitarian than the rich (Alesina and La Ferrara 2005), and there is less support for redistribution in the US than in Europe (Alesina, Glaeser, and Sacerdote 2001; Almås, Cappelen, and Tungodden 2016). This could affect the actual level of redistribution in different societies, for example via voting for left- or right-wing parties (Frohlich et al. 1984; Corneo and Grüner 2002). To understand what stabilizes these differences, we need to understand where fairness preferences come from. While some variation might be genetic (Wallace et al. 2007), ideas of fairness could also be socially transmitted (Krupka and Weber 2009). Specifically, social preferences might be affected by *social norms* (Bicchieri 2005; Krupka and Weber 2013; Kimbrough and Vostroknutov 2016). If different groups hold different norms, that could explain some of the systematic variation in fairness preferences.

Individuals acquire norms over the life course, and, as we argue below, early life is likely to be a critical stage for this. In this paper, using a "framed field" experiment on 11-15 year olds at a UK school, we test whether young people can pick up fairness norms from each other. Each participant allocated a pot of money between two anonymous others, the size of the pot being determined by the two's performance in a maths task. He or she could choose to split the money equally, or proportionally with performance, or any other way. Then, participants observed the allocation of one of their peers, either a friend, or a peer group member who was not a friend. Lastly, they made another allocation decision just like the first. With this data, we test whether the observed peer's allocation decision affects participants' second decision. We also check whether it affects their (expressed) opinion about the fairness of different hypothetical allocations.

Here is what our results show. First, indeed young people can pick up fairness norms, simply by observing a peer's behaviour; this affects not only their own behaviour, but also what they say is fair, and we argue below that the latter is important. Second, this social transmission is not about learning a new norm. Our participants already understand the two key norms available, and an absolute majority chooses either a pure equal split or an exactly proportional allocation. Where they differ, and what they learn from each other, is which norm to apply in an unfamiliar situation. Third, we use information about our participants' network of friends, to test hypotheses about how norms spread. Surprisingly, friends are not more influential than non-friends, and popular kids are not especially influential either. The transmission of norms is what evolutionary theorists call "unbiased" – with one possible exception, discussed below. Lastly, although we do not test for long-run effects of norm transmission (and we wouldn't expect them from this experiment), we do relate participants' behaviour to characteristics that matter in the long run: more academically able children are less egalitarian.

Literature review

Researchers have learnt much about the development of fairness in childhood. As they get older, children become fairer in their reasoning (Piaget 1965; Kohlberg 1984; Damon 1980) and behaviour (Hook and Cook 1979; Moore and Macgillivray 2004; Fehr, Bernhard, and Rockenbach 2008). Several studies have examined fairness preferences among adolescents: some show an increase in fairness (Sutter and Kocher 2007; Harbaugh, Krause, and Liday 2003), others do not (Gummerum, Hanoch, and Keller 2008; Harbaugh, Krause, and Vesterlund 2007; Martinsson et al. 2011). There is also evidence for a move from strict egalitarianism towards meritocratic considerations (Almås et al. 2010). In any case, it is clear that children acquire fairness concepts at an early age (Sloane, Baillargeon, and Premack 2012; Blake, McAuliffe, and Warneken 2014).

The differences in individuals' preferences for redistribution, however, may be less about understanding a given idea of fairness, than about applying it. For example, most of us would split a pizza equally among friends, and most of us would accept that exam grades should be rewards for performance. But we may disagree about whether equality, meritocracy or another concept should be applied in a particular context – say, the distribution of resources in society as a whole. The philosopher Michael Walzer (1983) argues that there are "spheres of justice" in which different norms may apply. The political argument between left and right is about how far the meritocratic norms of the market sphere should apply in areas such as health or education, rather than about context-free preferences over distribution *per se*. In experiments, framing treatments, like calling an interaction the "Wall Street game" or the "community game", can have large effects on subjects' play (Liberman, Samuels, and Ross 2004; Ellingsen et al. 2012); this can be understood as changing their beliefs about which norm is appropriate in the experimental context.

If so, then to understand differences in people's views, we may want to look not at the development of fairness concepts in early life, but at the formation of beliefs about how these concepts apply. Work in psychology suggests that adolescence is an important period for this. During adolescence, people develop a sense of "moral identity": that is, as well as understanding moral concepts, they come to see themselves as committed to certain moral values (Blasi 1993), and develop political orientations (Erikson 1968). This is part of a general process of consolidating identity, a view of oneself and one's attributes (Erikson 1968; Roberts and DelVecchio 2000). At the same time, adolescence is a period of "social re-orientation", when peer influence becomes increasingly important relative to family members (Nelson et al. 2005). Young people develop a sense of self-worth and self-esteem by emulating the valued behaviours of their peers, and by following the social norms of a valued (e.g., high status) peer group (Abrams and Hogg 1990; Brechwald and Prinstein 2011). Violators of group norms may even be rejected or excluded by peers (Juvonen and Galvan 2008). Since social norms are so powerful in regulating young people's behaviour (Prentice 2008), it is not surprising that adolescents are like their friends in their beliefs, behaviours, and attitudes (Kandel 1978; Tolson and Urberg 1993; Tedin 1980). At the neural level, this is a period when brain areas associated with executive functioning mature swiftly, and abilities such as decision-making and social perspective-taking improve. The importance of the peer group, coupled with the process of experience-dependent pruning of synapses in these brain areas, suggests that social influence during adolescence may have a significant and enduring effect (Blakemore and Choudhury 2006). In other words, an adolescent's interaction with the social world may influence the development of specific brain areas associated with social cognitive processing, and this occurs via the retention of synapses into specialized, efficient networks in these brain areas. So, people's norms of fairness may be laid down during adolescence, having been acquired from friends and other peers.

Our experiment tests how and whether this norm acquisition happens. We put young people in a situation where they have to make distributive choices. The situation is unfamiliar, but has analogies with others within their experience (for example, we let outcomes depend on a maths test, and real-world outcomes like university places actually are allocated based on test performance). This is the same problem people face when forming a view on the fairness of government policy, firm wage-setting, price increases and bargaining offers: "what norm applies here?" We then allow them to observe a peer's distributive choice – either a friend or a non-friend – before they make a second choice. We test how observing their peer's choice affects their own fairness norm.

We define norms broadly as informal rules of behaviour which are jointly held and sustained by social groups (Elster 1989; Krupka and Weber 2013). The question then arises of how to measure norm transmission between individuals. One obvious way would be to look at whether the second individual follows the norm. This approach is not enough, though, because the fact that someone obeys a norm does not guarantee that it has been sustainably transmitted. To see why, consider Fowler's (2005) model of a Prisoner's Dilemma with third party punishment. There, the population cycles between three states: cooperating and punishing non-cooperators; cooperating but not punishing; and defecting. In a world like that, we would be wrong to judge that a norm of cooperation had been transmitted between two generations, just from observing that the second generation played "cooperate". We also need to know if the second generation punishes defectors; otherwise, the norm has not been stably transmitted, and future generations will play "defect".

Thus, we ideally should measure not just conformity with the norm, but all the other behaviours that sustain it. People do this in three central ways (cf. Tankard and Paluck 2016). They can obey a norm in public, encouraging others by their example; they can verbally endorse the norm; and they can sanction others who violate it. For short, they can *practice*, *preach*, and *punish*. All of these behaviours could be both dependent and independent variables: does one person's preaching and practice of a norm, and punishment of norm-violators, cause others to preach, practice and punish? This gives nine potential causal arrows. Here we test just two: whether, after being exposed to peers who follow a specific norm, subjects follow the norm themselves, and verbally endorse it.

This account should make clear why we do not view participants' opinions as "cheap talk". Instead, they are an important way in which people may affect others' normative beliefs. For this reason, we break with standard practice in economics, and make expressed opinions a key dependent variable, alongside behaviour. Measuring opinions has another advantage: it helps us to differentiate norm transmission from simple imitation of behaviour. Imitation is a general-purpose heuristic humans may use in unfamiliar situations (N. E. Miller and Dollard 1941), and observing it on its own may not tell us much about norm transmission processes specifically.

Of other papers on norm transmission, Krupka and Weber (2009) examine the causal arrows from preaching and practice, to practice, using dictator games. Gächter, Nosenzo, and Sefton (2013) do the same in a gift-exchange experiment. Schotter and Sopher (2007) are most similar to our approach: in an intergenerational ultimatum game, each generation is observed by the next generation, and may also provide advice ("preach") to it; they explicitly model advice as part of a convention (see also Schotter and Sopher 2003). Gächter, Gerhards, and Nosenzo (2017) similarly examine the effect of peer behaviour on both own behaviour and beliefs about appropriate behaviour.

Some recent papers measure norms with second-order expectations. That is, they ask what people believe that others believe is appropriate behaviour (or for "descriptive norms", the actual behaviour that others expect), using either a two stage elicitation process (Bicchieri 2005; Charness and Dufwenberg 2006; Bicchieri and Xiao 2009; Dufwenberg, Gächter, and Hennig-Schmidt 2011), or coordination games (Krupka and Weber 2013). This approach is broadly sympathetic to our own: it posits that people care about what others think, a mechanism which can explain how preaching, practice and punishment sustain norms. However, we wish to maintain a distinction between norms – jointly held informal rules of social behaviour – and the particular psychological mechanisms that support them. For example, it is possible that a norm in a community may affect an individual's behaviour by causing them to *internalize* the norm, i.e. to wish to follow it irrespective of whether others want him to; as Adam Smith put it, man "desires, not only praise, but praiseworthiness; or to be that thing which, though it should be praised by nobody, is, however, the natural and proper object of praise." Put another way, we wish to distinguish between causes for behaviour and consciously held motivations for behaviour. I may hold a norm as a result of its being held in my community, without being consciously motivated by that fact. As a result, we do not use the second-order approach here, but simply test whether subjects verbally endorse a norm after exposure to it. Note that we do not deny that second-order beliefs may be an important mechanism of norm transmission. We have not investigated this mechanism here, but it would be interesting to do so in future work.

Studying norms of distributive fairness has several advantages. First, as mentioned, preferences over distribution matter to important areas of political and economic life, like bargaining, the labour relationship, pricing and taxation. Second, there are rival conceptions of what is fair, including libertarian, meritocratic and egalitarian ones. So, we can show that social groups push people to choose one specific fairness norm among many, rather than just encouraging prosocial behaviour in general. Lastly, these norms are relevant and meaningful to our participants. Young people experience both egalitarian and meritocratic distributions in different contexts. For example, school marks and sports scores are awarded for performance, while basic resources like food are more likely to be shared equally. To capture subjects' fairness behaviour, we let them allocate resources between third parties. This elicits preferences over distributions with no element of self-interest. The total size of the pie to be divided depends on performance in a test, allowing us to examine the choice between egalitarianism and meritocracy. Similar choice situations have been used by several researchers (Konow 2000; Frohlich, Oppenheimer, and Kurki 2004; Cappelen et al. 2007; Barr et al. 2015). Some studies relate redistributive choices to real world characteristics, suggesting that the paradigm has external validity: Fisman, Jakiela, and Kariv (2015) link distributive preferences in a dictator game to support for the Democratic party; Fisman et al. (2015) and Barr et al. (2015) show that poor and rich people differ in support for meritocracy; Almås, Cappelen, and Tungodden (2016) compare Scandinavian and American subjects and find Scandinavians are more egalitarian.

Teenagers have different groups of friends, and occupy different positions in an interlinked social network. There are two ways network position can affect norm transmission and social influence. First, there is a *mechanical* effect of being at a certain position. For example, someone with many friends will influence many individuals; if these friends themselves have many friends, then he will influence many people at one remove. Similarly, pairs of individuals ("dyads") who share many mutual friends will have many channels of potential influence between them. We call these effects mechanical because they hold even if, in any single interaction, each individual has the same influence on others. Graph-theoretic models of networks explore these effects (Ballester, Calvó-Armengol, and Zenou 2006; Jackson 2010).

Second, network position may have *psychological* effects. For example, a person with many friends may be especially likely to influence a peer, even in a one-shot interaction in which network structure plays no role. This could happen because people pay more attention to others who are popular (Lippitt, Polansky, and Rosen 1952; Cialdini and Trost 1998), or because popular people are, say, more likable or more dominant. Similarly, two individuals who are close in the network may be more likely to influence each other in a one-shot interaction, because they like each other more, have similar personalities, or are motivated to follow an in-group-specific norm (Terry and Hogg 1996; Bernhard, Fehr, and Fischbacher 2006). In the literature on cultural evolution, effects like this are known as biased transmission (Henrich 2001), and are important for predicting the stable distribution of cultural traits. For example, if people copy "successful" individuals, then high-payoff behaviours are more likely to spread than low-payoff behaviours, including costly altruism (Henrich and Boyd 2001). On the other hand, if people copy others who are similar to them, then this can help to preserve different behaviours in the population (Bisin and Verdier 2010). Biased transmission also raises issues for network models. For instance, Calvó-Armengol, Patacchini, and Zenou (2009)'s model of peer effects in education assumes that all peers have the same direct influence, i.e. that there are no psychological effects of network position. Here we can test this directly: while we capture the friendship network, our experiment holds mechanical network effects constant, by matching subjects in pairs for a single interaction. In particular, we randomly match some subjects with friends, and others with non-friends, and compare the strength of norm transmission between these two groups. We also observe how some other network measures affect norm transmission, including popularity and a dyad's number of shared mutual friends. Similar research designs on networks include Goeree et al. (2010), Belot and Van De Ven (2011) and Brañas-Garza, Durán, and Espinosa (2012).

To sum up how we advance the literature: we look at the social transmission of rival fairness norms, rather than contrasting normative and self-interested behaviour; we study a population of adolescents, at a life stage which may be important for the internalization of norms; we use both behaviour and normative opinions as a dependent variable; and we capture the friendship network, and observe how this affects norm transmission.

Experimental design

The basic setup is as follows. We randomly match participants in pairs, with either friends or non-friends. Participants decide how to share a pot of money, whose size is determined by the performance of two other anonymous participants on an academic test, between those two anonymous others. They observe their match's decision. Then they make a second similar choice, sharing money between two more anonymous others. Lastly, they judge the fairness of different hypothetical distributions. We test whether the match's (first) choice affects their second choice, and their fairness judgments.

Our participants were 524 young people aged between 11 and 15, 272 boys and 252 girls, recruited from a high school in the English county of Suffolk. The experiment consisted of two sessions, two weeks apart. Members of each year group (years 7-10 in the English system) were tested simultaneously under "exam conditions" in a large exam hall. Participants were given a booklet, containing experiment instructions, which were also read out, and the experimental questions themselves, which they completed in ink.

In the first session, participants completed an adding-to-10 maths task as in Mazar, Amir, and Ariely (2008), and a die rolling experiment (not discussed here). They also wrote down their top ten friends. We used this information to construct the friendship network. The school is in a rural area, and is central to pupils' social lives, so we capture much of this network: 88% of friends named were other participants in our experiment.

Before the second session, we used the information on friendships to put participants into two treatments: *Friends* and *Non-friends*. To do this, we started with the friendship networks of boys and girls in each year group, and separated all participants into non-overlapping pairs of friends, using the Blossom algorithm (Edmonds 1991, details in appendix). We then randomly allocated half of these pairs to be in the *Friends* treatment, and half in the *Non-friends* treatment. Pairs in the *Non-friends* treatment were randomly rematched within each gender/year subgroup of the treatment, until none was matched with a friend. Thus, both friends and non-friends were matched with someone of the same gender and year. We call each person's pair his or her "partner".

The second session was the core of the experiment. First, each participant marked two maths tasks from anonymous students ("targets") in the same year. They then had to calculate the total cash amount available: this was 50 pence times the total number of correct answers. Then, each participant allocated this cash between the two targets, in any way they wished. For example, if one student got 7 answers right and the other got 3, they had to allocate 50p × (7 + 3) = £5. They could give both students £2.50, an egalitarian choice; they could make a meritocratic choice by giving each individual 50 pence per correct answer (£3.50 and £1.50); or they could make any other allocation, such as giving all the money to one individual, or mixing between meritocracy and egalitarianism. We made it clear to participants that they could allocate the money "however you prefer". So, subjects made a pure choice of distribution between anonymous third parties, with no element of self-interest. We call this decision 1.

Next, participants swapped booklets with their partner, who was sitting directly behind or in front of them: communication was forbidden, but importantly, they were able to see who their partner was. They were asked to check the partner's calculations. This ensured that they would observe their partner's allocation, without us telling them to do so explicitly, which might have induced demand effects. To ensure that decisions were easy to compare, participants and their partners marked the maths tasks of the same two targets in decision 1.

Then, participants marked the maths tasks of two other anonymous individuals, and allocated cash to these individuals, exactly as in decision 1. We call this decision 2. Finally, they completed a questionnaire, including questions about the perceived fairness of some hypothetical allocations, and the self-other inclusion scale (A. Aron, Aron, and Smollan 1992). Participants were paid based on one randomly chosen allocation from the set of people who had marked them, by cash in envelopes distributed by the school a few days after the experiment.

Results

Some pupils were absent at session 2. Others were present at session 2 but had missed session 1. So that everybody could participate, unmatched pupils were rematched in pairs ad hoc. Table 1 shows the number of subjects correctly matched with their designated partner, by treatment. Throughout, we use *Friends* and *Non-friends* to refer to the pairs in these two treatments, rather than the actual friendship status of the pairs. We also use the designated treatment in all regressions, to ensure that treatment status is exogenous (but in regressions on partner characteristics, we use those of the real partner).

Table 1: Matching status

	Friends	Non-friends
Correctly matched	220	224
Incorrectly matched	22	17
Did not participate	18	23

Table 2 shows balance tests for individuals in the *Friend* and *Non-Friend* treatments. *Maths score* is the score an individual achieved in the maths test (as marked by his or her peers). *Indegree* gives the total number of other pupils who named an individual as their friend; *Eigenvector centrality* is another network measure, see below. *Egalitarian time 1* shows whether individuals made an egalitarian decision (see below) at time 1. These variables are not significantly different between treatments, with the exception of eigenvector centrality (results are unaffected if we control for this). The last four variables check that our treatment was effective in altering friendship status of the pairs. *Self-other inclusion* gives the score on the "inclusion of other in the self" scale, *Actual friend* is a dummy variable for whether the individual named his or her (actual rather than designated) partner as a friend, *Mutual friends* is a dummy for whether both individuals named each other as friends, *Close mutual friend* is a dummy for whether both individuals named each other in their top 3 friends, and *N shared friends* gives the number of third parties who were (mutual) friends with both individuals. These variables are significantly higher in the Friends treatment, as they should be.

Table 2: Balance tests

	Friends	Non-friends	P value
Age	13.67	13.72	0.66
Female	0.48	0.46	0.68
Maths score	7.70	7.66	0.94
Indegree	7.14	7.21	0.82
Eigenvector centrality	0.05	0.08	0.00
Egalitarian time 1	0.12	0.08	0.18
Self-other inclusion	5.07	2.42	0.00
Actual friend	0.76	0.03	0.00
Mutual friends	0.74	0.00	0.00
Close mutual friends	0.41	0.00	0.00
N shared friends	1.35	0.08	0.00

Distributive preferences

We create our dependent variable as follows. When participant i allocates money in decision $t \in \{1, 2\}$ between targets A and B, we derive a *meritocracy score* as:

$$m_i^t = 2 \times \frac{pay_A - pay_B}{score_A - score_B}$$

Here, pay_J is the amount in pounds allocated to target J and $score_J$ is target J's score in the maths test. m_i^t will be 1 if the subject paid each person 50p for each of his or her correct answers, i.e. pay was proportional to performance. It will be 0 if the subject paid A and B the same amount. The meritocracy score would be undefined if targets A and B got the same score, since then it is impossible to distinguish egalitarianism from meritocracy. To avoid this, we made certain that A and B always had substantially different scores. Using the meritocracy score, we put each allocation into five mutually exclusive *fairness categories*, shown in Table 3.

Table 3: Fairness categories

Meritocracy score	Interpretation	Fairness category
$m_i^t < 0$	Giving more to the low scorer	Underdog
$m_i^t = 0$	Giving the same to both	Egalitarian
$0 < m_i^t < 1$	Between equality and meritocracy	Mixed
$m_i^t = 1$	Payment in proportion to answers	Meritocratic
$m_i^t > 1$	Giving disproportionately to the high scorer	Winner takes all (WTA)

The two most salient categories are *egalitarianism*, in which subjects gave the same to both A and B, and *meritocracy*, in which A and B were paid for their own performance on the maths test. We view these as corresponding to pure norms. The other categories are more ambiguous. *Underdog* decisions involved giving more to the lower performer. This could be justified as compensation for the low mark, but it could also reflect a mistaken, random or perverse choice. *Mixed* decisions could involve a trade off between egalitarian and meritocratic motivations. *Winner takes all* (WTA) decisions gave more than his or her "fair share" to the better performer on the test. This might happen if participants treated the money as a prize, for example.

Figure 1 shows the empirical cumulation distribution function of the meritocracy score at time 1, and the proportions in each fairness category. Egalitarianism and meritocracy were chosen by about 70% of subjects, with an absolute majority of meritocrats. This is the first evidence suggesting that subjects thought in terms of these two norms.

More evidence is shown in Figure 2, which plots the distribution of subjects' fairness categories in decision 2 against their fairness categories in decision 1. Meritocratic and egalitarian decisions were highly consistent: subjects who made one of these choices at time 1 were likely to make the same choice at time 2. By contrast, underdog, mixed and WTA decisions were less consistent. Within each of these categories at time 1, absolute majorities changed to either egalitarian or meritocratic decisions at time 2.

Lastly, subjects' free-form explanations of their decisions, from the final questionnaire, show that they were more likely to see egalitarian and meritocratic decisions as corresponding to fairness norms. (Example explanations: "because I find it fair"; "because I used the calculation table on how much they each got"; "I based it on the amount they got right"; "I am a communist.") Figure 3 shows the proportions using the word "fair", and the words "earn" or "deserve", for each fairness category in decision 1. Pure egalitarians and meritocrats were slightly more likely to mention fairness than other categories (chi-squared $p = 0.091$). Meritocrats were much more likely to mention desert or earnings than egalitarians ($p = 0.005$).[1]

[1] These calculations exclude "underdog" decision-makers. They were most likely to mention fairness and desert, but manual inspection of their explanations suggests that many of them filled in their decision wrong.

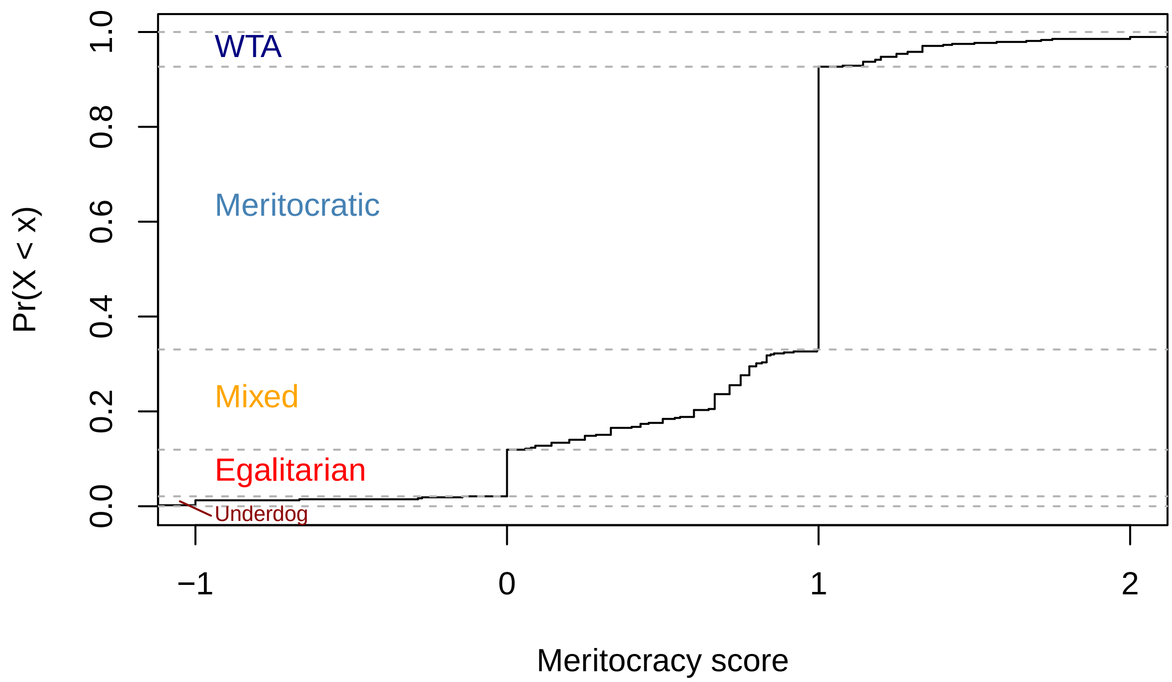

Figure 1: Cumulative distribution of meritocracy score, decision 1

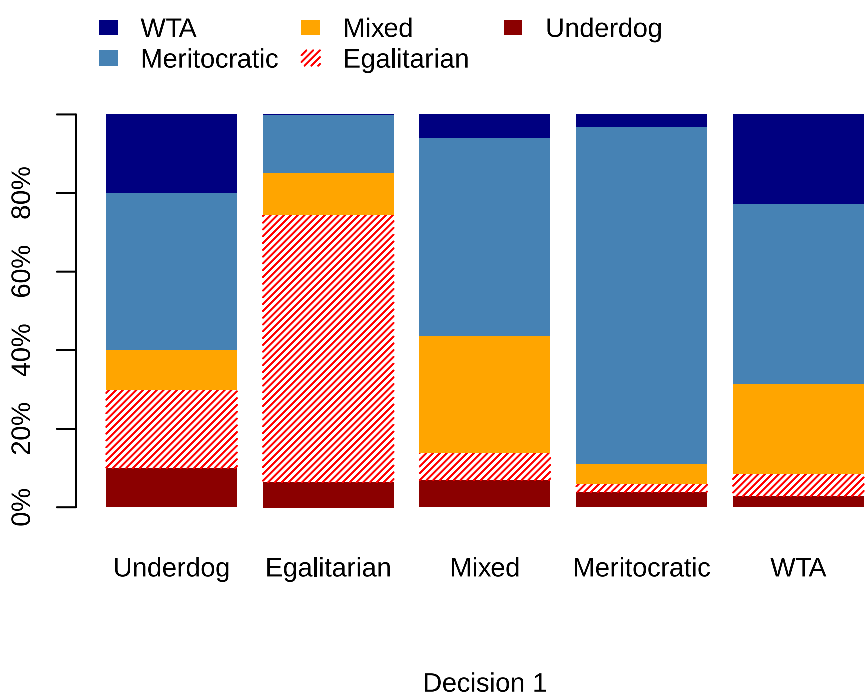

Figure 2: Distribution of fairness categories in decision 2, by category in decision 1

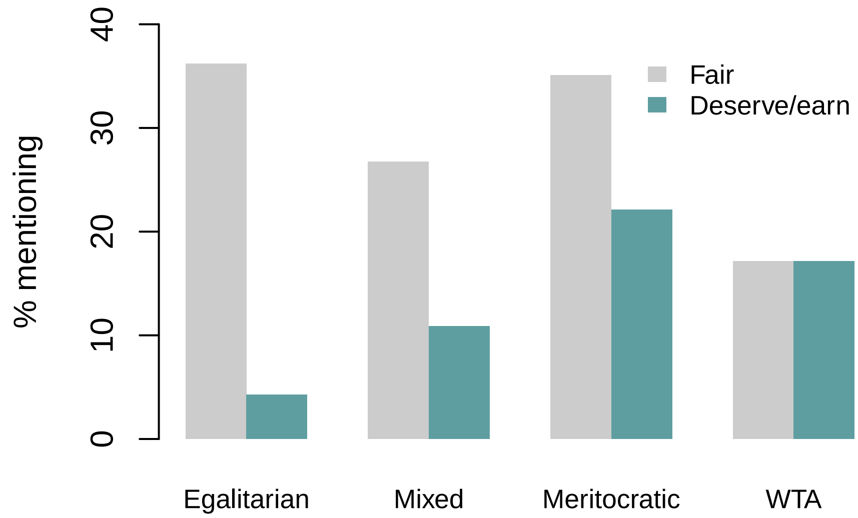

Figure 3: Per cent mentioning fairness and desert in explanations, by decision 1 fairness category. Underdog decisions omitted

Intuitively, these results suggest that subjects only recognize meritocracy and egalitarianism as norms. We can formalize these intuitions as follows. Subjects believe that there is a true norm $m \in \{0, 1\}$. Let m_i be individual i's meritocracy score as defined above, dropping the time superscript temporarily. Suppose that i chooses m_i to maximize:

$$-(1-\mu_i)(|m_i| + \frac{q}{2}(m_i)^2) - \mu_i(|m_i - 1| + \frac{q}{2}(m_i - 1)^2)$$

Here μ_i represents i's belief that $m = 1$. Then the meritocratic norm, choosing $m_i = 1$, is appropriate. Otherwise the egalitarian norm, choosing $m_i = 0$, is appropriate. Subjects anticipate losses from disobeying the true norm. q is a parameter representing the strength of quadratic versus linear losses. It measures the willingness to compromise between norms. If q is low subjects will tend to choose only behaviour that exactly fits one norm; if q is high they will be more likely to make a mixed decision, choosing $m_i \in (0, 1)$.

Solving for the maximum in m_i reveals three kinds of behaviour. If $\mu_i < \frac{1}{2+q}$ then i behaves as an egalitarian, setting $m_i = 0$. If $\mu_i > 1 - \frac{1}{2+q}$, then i behaves as a meritocrat, setting $m_i = 1$. If μ_i is between these values, then i sets $m_i \in (0, 1)$ and falls into the "mixed" category.

Suppose that all subjects start with a prior belief that meritocracy is correct with probability α. Before decision 1, with some independently drawn probability s, each subject receives a private signal about the true norm, $a_i \in \{0, 1\}$, where $a_i = m$ with probability $\pi \gg 0.5$. With probability $1 - s$, the subject receives no signal and does not update their prior. After possibly observing their private signal, subjects make their first decision m_i^1. Subjects then observe their partner $p(i)$'s first decision, $m_{p(i)}^1$, and make their second decision, m_i^2.

This simple model captures three ideas. First, there are only two possible norms; we rule out values of m other than 0 and 1. Second, subjects care about following norms, and are not sure which norm is "correct". Third, they think that some of their peers may know the right norm. If so, then while most people's behaviour is not informative, subjects who follow a rare norm can persuade their peers to copy them. Specifically, suppose that α is high (people are confident meritocracy is the right norm); s is low (not many people are thought to have a signal of the right norm); and π is close to 1 (the signal is usually correct). Then the following facts hold:

1. In the first decision, subjects who observe $a_i = 0$ choose $m_i^1 = 0$. All other subjects choose $m_i^1 = 1$.

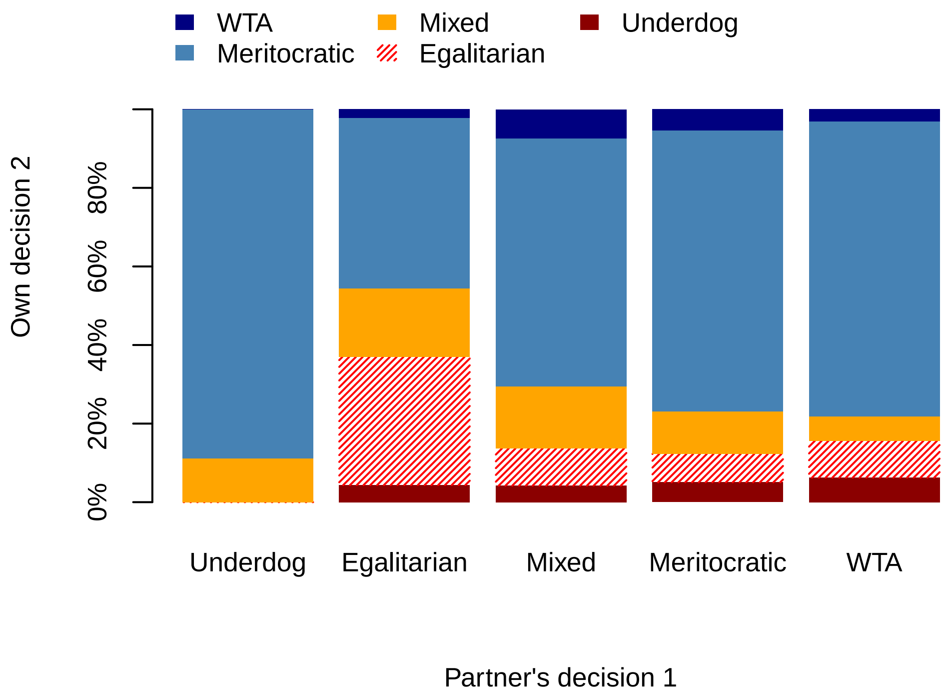

Figure 4: Distribution of fairness categories in decision 2, by partner's category in decision 1

2. In the second decision, some subjects who observe $m^1_{p(i)} = 0$ change their decision to egalitarian, $m^2_i = 0$. All other subjects do not change their decision and play $m^2_i = m^1_i$.

Fact 1 stylizes the observation from Figure 1 that the distribution of meritocracy scores has mass points around pure egalitarianism and pure meritocracy. As we will see, fact 2 also holds: only the minority who follow the pure egalitarian norm are influential.[2] By contrast, in a model in which there are many possible norms and everyone gets a noisy signal of the true norm, there would be no mass points at $m_i \in \{0, 1\}$, and all partners' decisions would be equally influential. Likewise, if subjects just wanted to take similar decisions to their peers, we would expect all decisions to be equally influential.

Effects on behaviour

Figure 4 shows our main result. It categorizes subjects' norm at time 2 by their partner's norm at time 1. Subjects appear to have been influenced by their partner's choices. In particular, subjects whose partner made an egalitarian choice were more likely to then do the same themselves. They were not influenced by partners who made WTA, mixed or underdog choices (see multinomial regressions in the appendix).

We now run regressions of subjects' second decisions on their partners' first decisions. Our central specification is:

$$Egalitarian_{i,2} = \alpha + \beta Egalitarian_{p(i),1} + \gamma Friends_i + \delta Friends_i \times Egalitarian_{p(i),1} + X_i\eta + \epsilon_i$$

where $Egalitarian_{i,t}$ is a dummy variable taking the value 1 if subject i's time t decision followed the egalitarian norm, $m^t_i = 0$; $p(i)$ indexes i's partner; $Friends_i$ is a dummy variable for the *Friends* treatment; and X_i is a vector of controls, always including a full set of year-gender fixed effects.[3] The coefficient

[2] Letting values of q vary between subjects would lead to some subjects making mixed decisions in period 1, as we observe, and would still get the result that only pure egalitarians affected their partner's decisions.

[3] Our original analysis plan, registered at https://osf.io/ckv59/, specified similar regressions using meritocracy scores. We discuss this in the appendix, where we also report robustness checks.

β will identify the effect of the partner's egalitarian decision, if the error term ϵ_i is not correlated with $Egalitarian_{p(i),1}$. One risk is that individuals' propensity to make an egalitarian choice is correlated with their partners' propensity to do so. This is especially likely in the *Friends* treatment, if friends hold similar fairness norms. To mitigate this problem, we can include the subject's own time 1 decision, $Egalitarian_{i,1}$, as a control. However, endogeneity is still possible, for example if friends' norms are similar, but time 1 decisions are noisy. Below, for a further check on identification, we use permutation tests. To guard against non- independence within matched pairs of participants, we cluster standard errors by pair.

The other coefficient of interest is δ. This gives the extra effect of the partner's time 1 decision in the *Friends* treatment. The control variables X always include dummy variables for year-gender combinations.

Our hypotheses are as follows:

Hypothesis 1: The partner's decision influences the subject's time 2 decision, for both friends and non-friends: $\beta > 0$ and $\beta + \delta > 0$.

Hypothesis 2: Friends are more influential than non-friends: $\delta > 0$.

We estimate probability linear models according to the specification above. Table 4 shows the results. Column 1 runs a simple bivariate regression to estimate the overall effect of partner egalitarianism across treatments. Partner's egalitarianism significantly increases the subject's egalitarianism. Column 2 interacts this with the *Friends* treatment dummy. The interaction is negative and insignificant.

Column 3 includes a dummy variable for subjects' own egalitarianism in decision 1. The R^2 increases substantially, confirming the consistency in behaviour that we have already noted. The coefficient on *Partner egalitarian* is about halved. This suggests that indeed, some of the correlation between pairs is driven by underlying similarities. However, the coefficient remains significantly positive. Column 4 includes decision 1 egalitarianism and the interaction with the *Friends* treatment, which remains insignificant.[4]

As a further check on exogeneity, we ran permutation tests. We repeatedly matched subjects with randomly chosen "partners" (friends or non-friends, according to the treatment, who were not the subject's actual partner); we reran our regressions, and recorded the key estimated coefficients. This gives the distribution of coefficients under the null hypothesis that the real partner's decision had no effect. We then check whether coefficients from the true regression are in the 5% tails of this distribution. Results are reported in the Appendix. The overall effect of partner decisions remains significant, and so does the effect in the *Non-friends* treatment. The effect of partner decisions in the *Friends* treatment is insignificant, and the null distribution of coefficients is shifted right from zero. Again, this suggests that some of the effect of partner decisions in the *Friends* treatment may be driven by similarity between friends. The effect in *Friends* is also not significantly different from the effect in *Non-friends*, however.

To summarize, Hypothesis 1 is supported: our participants were affected by observing the decisions of their partners. There is no evidence for Hypothesis 2, that friends were more influential than non-friends. It could still be true that friends have more influence than non-friends in daily life, via the "mechanical" effect that subjects interact more with their friends than with others. What we do not observe is a stronger "psychological" influence, within a single interaction.

Having tested our basic hypotheses, we now explore some further moderators of norm transmission. (Apart from the analysis of self-other inclusion, these regressions were not pre-specified in the analysis plan.) The next four columns start from the specification of column 3, and interact *Partner egalitarian* with other variables.

First, we examine different measures of the subject's relationship with the partner. Column 5 interacts *Partner egalitarian* with our questionnaire measure of self-other inclusion. This is another way to measure friendship: it captures the subject's (perceived) *emotional* closeness to the partner. The interaction term is negative: emotionally close friends are not more influential.

[4] This insignificance might be driven by the fact that not all those in the *Friends* treatment were matched with their intended partner. To check this, we reran these regressions, replacing the *Friends* treatment dummy with a dummy variable which was 1 if subjects were truly matched with a mutual friend. Interactions with partner egalitarianism remained small and insignificant.

Table 4: Probability linear models of egalitarian choice in decision 2

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	0.06	0.06	−0.00	0.00	0.03	0.00	−0.00	−0.00
	(0.04)	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)
Partner egalitarian	0.23**	0.26*	0.14*	0.16	0.28+	0.10	0.10	0.13*
	(0.08)	(0.12)	(0.06)	(0.12)	(0.15)	(0.07)	(0.11)	(0.07)
Friends		0.00		−0.01				
		(0.03)		(0.02)				
Partner egal. x Friends		−0.06		−0.05				
		(0.17)		(0.13)				
Own decision 1 egalitarian			0.61***	0.61***	0.59***	0.59***	0.61***	0.61***
			(0.07)	(0.07)	(0.08)	(0.08)	(0.07)	(0.07)
Self-other inclusion					−0.01+			
					(0.00)			
Partner egal. x s-o inclusion					−0.04			
					(0.03)			
N shared friends						−0.01		
						(0.00)		
P. egal. x N shared friends						0.07*		
						(0.03)		
Partner indegree							0.00	
							(0.00)	
P. egal. x P. indegree							0.01	
							(0.02)	
Partner EV centrality								−0.01
								(0.06)
P. egal. x EV centrality								0.02
								(0.38)
R^2	0.08	0.08	0.43	0.44	0.42	0.44	0.43	0.43
Adj. R^2	0.06	0.06	0.42	0.42	0.40	0.43	0.42	0.42
Num. obs.	459	459	454	454	444	454	451	454
RMSE	0.29	0.29	0.23	0.23	0.23	0.23	0.23	0.23

Standard errors clustered by pair. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{+}p < 0.1$. Not shown: gender/year dummies.

Column 6 shows the interaction with *N shared friends*, the number of (mutual) friends that the subject and partner have in common. This measures the *social* closeness between the subject and the partner. People who have many shared friends are likely to interact more often, and they may care about conforming with their friends. The effect is positive, significant and large: one extra shared friend increases the raw effect of *Partner egalitarian* by more than half. We also tried the number of 3-or-more-person "cliques" that included both the subject and the partner (a clique is a group all of whose members are mutual friends with each other); this also interacts significantly and positively with *Partner egalitarian* (not shown).

Columns 7 and 8 focus on the social status of the partner. Column 7 uses *Partner indegree*, the number of people who named the partner as a friend. This is a simple measure of the partner's popularity. The final column uses the *eigenvector centrality* of the partner. Eigenvector centrality is a measure used in social network analysis. It reflects how many (mutual) friends a person has, how many friends those friends have, and so on. This provides a subtler measure of social influence. Again, neither of these variables interacts significantly with *Partner egalitarian*.

Overall, we find little evidence of biased transmission according to social status, or according to friendship status. Our participants seem to learn equally from friends and non-friends, and from popular and unpopular peers. The one exception is that those with many shared friends seem to influence each other more. If true,

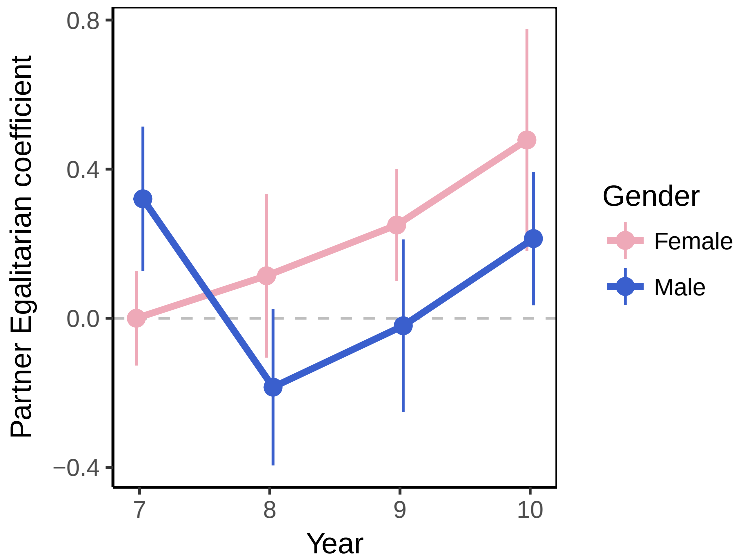

Figure 5: Coefficients for Partner Egalitarian by year and gender

this would suggest that young people are choosing norms so as to fit in with cliques of friends, with whom they expect to have many interactions.

Lastly, we examine how norm transmission changes with age. Figure 5 shows coefficients for *Partner egalitarian* (controlling for own egalitarian decision at time 1), estimated separately for each year-gender combination. The three way interaction between year, gender and *Partner egalitarian* is highly significant (F test, $p < 0.001$). The genders show different age patterns. Girls show constantly increasing effects over the four years, while boys' pattern is U-shaped. Again, this result should be treated cautiously: the friendship network is strongly segregated by age and gender, and peer effects might be different in different networks. However, given that females enter puberty before males (Petersen et al. 1988), and develop mature friendships earlier (Thorbecke and Grotevant 1982), results are consistent with our claim that peer effects are particularly strong during adolescence, with the exception of the high coefficient of the youngest boys.

Effects on attitudes

So far we have shown that subjects' behaviour influences their partners' behaviour. We now test whether it influences the opinions their partners express. As we have argued, this helps us to differentiate true norm internalization from simple imitation, and it also checks the aspect of normative behaviour which we call *preaching*, i.e., expressing opinions about what is normative. To keep the experiment simple, we asked our subjects directly for their opinion, rather than having them communicate it to each other. This is relevant if subjects who told us an action was fair would say the same to their peers, which we find plausible.

We showed participants four hypothetical scenarios in which somebody else had allocated the money. Each scenario corresponded to one fairness type, as shown in Table 5. They rated each scenario from 1 (very unfair) to 5 (very fair).

Table 5: Hypothetical scenarios

Scenario	Score A	Score B	Pay A	Pay A
Meritocratic	5	9	£2.50	£4.50
Egalitarian	5	9	£3.50	£3.50
WTA	5	9	£1.00	£6.00
Underdog	5	9	£6.00	£1.00

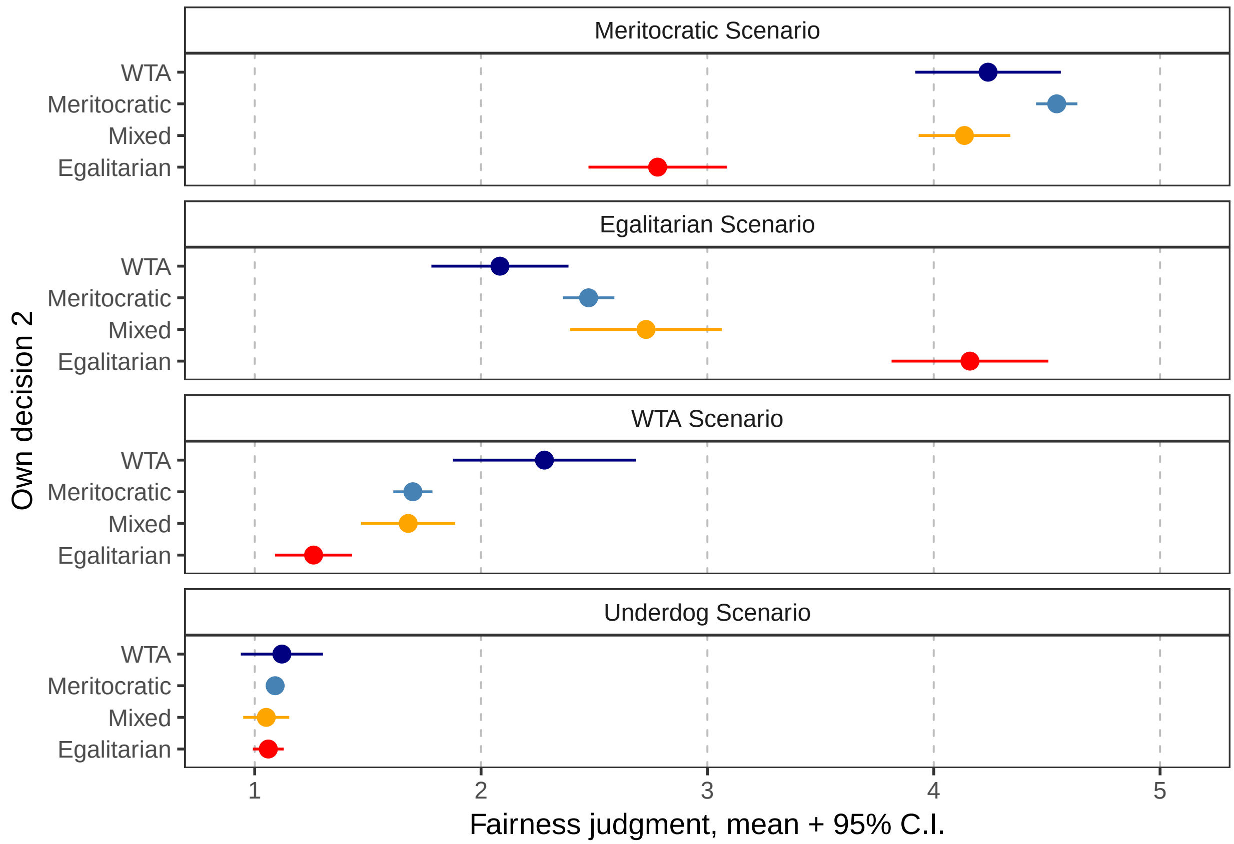

Figure 6: Questionnaire fairness judgments for scenarios, by own behavioural fairness category

We first check whether people's opinions are consistent with their behaviour. Figure 6 shows mean fairness judgments for each scenario, split by the fairness category of the subject's decision at time 2.[5] Opinions do indeed fit behaviour: egalitarian decision-makers are most likely to judge the egalitarian scenario as fair, meritocratic decision-makers are most likely to judge the meritocratic scenario as fair, and those who made a mixed decision are in between. Note that we cannot adjudicate whether fairness judgments cause behaviour. It is possible that people copy the partner's behaviour, and adjust their judgments accordingly so as to reduce cognitive dissonance (Festinger 1957; Konow 2000). We do not see this as a problem, because it would still result in a real change of opinion.

Subjects' opinions also show that most individuals recognized both egalitarian and meritocratic norms. 61% of subjects rated both meritocratic and egalitarian scenarios as strictly fairer than the other two. Of subjects who rated the meritocratic scenario as fairest, 58% rated the egalitarian scenario strictly above the underdog and WTA scenarios; of those who rated the egalitarian scenario as fairest, 83% rated the meritocratic scenario strictly above the other two.

Next, we turn to the effect of partner's decisions. Figure 7 shows mean fairness judgments for each scenario, split by the *partner's* fairness category at time 1. There appears to be an effect of partner decisions. When their partner's decision was egalitarian, subjects judged the meritocratic scenario to be less fair (top panel) and the egalitarian scenario to be slightly fairer (second panel).

To test this formally, we generate an *egalitarian attitude* score for each subject, as their fairness judgment of the egalitarian scenario, minus their fairness judgment of the meritocratic scenario. We then regress this score on their partner's time 1 egalitarianism. Table 6 shows the results. Column 1 shows the relationship between the partner's egalitarianism, and the subject's egalitarian attitude. Egalitarian partner decisions led

[5]We leave out Underdog partner decisions, as there is too little data.

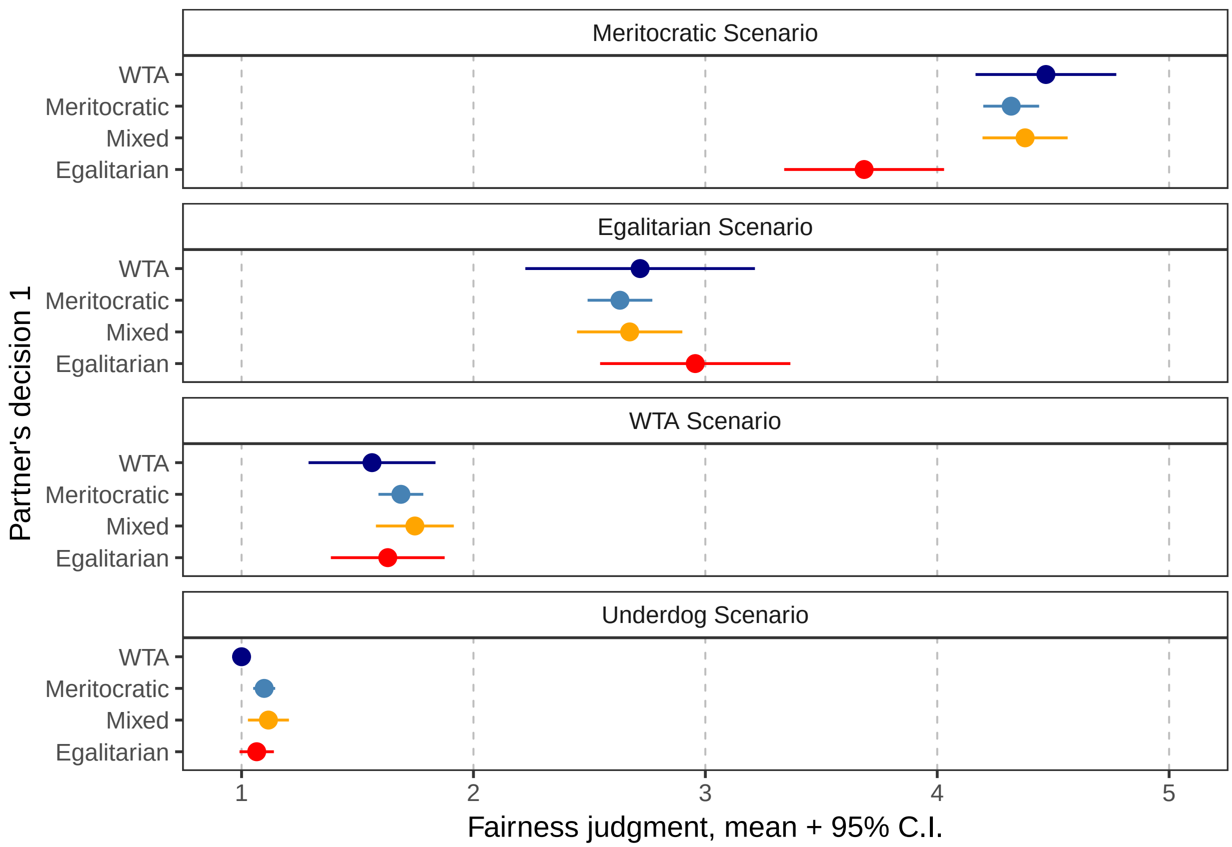

Figure 7: Questionnaire fairness judgments for scenarios, by partner's fairness category

to a significantly higher egalitarian attitude. Column 2 interacts the partner egalitarian dummy with the *Friends* treatment. The interaction is insignificant. Columns 3 and 4 add a control for the subject's own time 1 decision. As in the behavioural regressions, the coefficient on partner egalitarianism shrinks, but remains weakly significant, and the treatment interaction remains insignificant.

We also ran regressions interacting network measures with the partner's decision, similar to those in Table 4 columns 5-8. These (not shown) had broadly the same pattern of significance as in the behavioural regressions. The interaction of partner decision with number of shared friends was positive, but small and insignificant; the interaction with number of shared cliques was positive and significant.

So, the effect of partners' behaviour on judgments mirrors its effect on decisions. That strengthens our claim that participants are really recognizing and picking up a norm.

Table 6: Models of egalitarian attitude

	(1)	(2)	(3)	(4)
Intercept	-1.94^{***}	-1.87^{***}	-2.23^{***}	-2.12^{***}
	(0.26)	(0.27)	(0.23)	(0.23)
Partner egalitarian	0.95^{**}	0.76	0.51^{+}	0.29
	(0.34)	(0.47)	(0.30)	(0.49)
Friends treatment		-0.16		-0.25
		(0.18)		(0.16)
Partner egal. x Friends		0.35		0.42
		(0.68)		(0.61)
Own decision 1 egalitarian			2.76^{***}	2.78^{***}
			(0.33)	(0.33)
R^2	0.04	0.04	0.24	0.24
Adj. R^2	0.02	0.02	0.22	0.22
Num. obs.	459	459	454	454
RMSE	1.82	1.82	1.62	1.62

Standard errors clustered by pair. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{+}p < 0.1$. Not shown: gender/year dummies.

Long run correlations

The experiment put young people in an unfamiliar situation, where they had to decide what norm was appropriate. This might cast doubt on its external validity for more familiar decisions. We have argued that in fact, the interesting differences between people are about whether they apply a particular norm to a new situation. Still, we would like to know that choices in the experiment relate to other characteristics of our subjects. This will reassure us that we are measuring something meaningful about their world view.

In particular, we correlate individuals' egalitarianism with their score in Cognitive Abilities Tests (CAT), which are administered when they enter the school at age 11. These tests require no preparation and aim to test raw ability, rather than previous education. We know that people's experience can affect their ideas of fairness, for example via motivated reasoning (Konow 2000). We would expect scholastically weaker students to be more egalitarian in our experiment, because they will be less likely to think academic performance (like our maths test) deserves to be rewarded. Indeed, controlling for year-gender fixed effects, a one standard deviation increase in CAT score was associated with a 3.2 percentage point decrease in subjects' probability of being egalitarian in decision 1 (p = 0.024) and a change of -0.372 in their *egalitarian attitude* score (p < 0.001).

An alternative explanation would be that scholastically weaker students found it easier to make the egalitarian split. However, participants were provided with a calculation table, which gave the total money in the pot for a given total number of correct responses (e.g. 7 responses, £3.50); for a meritocratic allocation, subjects could just use this table to give each target 50p per correct response. Also, older subjects, who presumably would find such calculations easier, were no less egalitarian than younger subjects. Lastly, this hypothesis cannot explain the difference in fairness attitudes.

In adolescence, young people learn from parents, teachers and experience that academic success will shape their future productivity, and they form attitudes as to how this product should be fairly distributed. If this result holds on the macro level, we would expect low performing students to develop more egalitarian political attitudes, and for these attitudes to spread among friendship networks (the CAT score is highly correlated among friends).

Can our results explain stable differences in fairness norms within a population? On the one hand, the result that all peers, not just friends, are influential pushes against that. If adolescents were only influenced by their friends then different norms could be stabilized within different cliques. There are numerous examples of this in e.g. the sociology of deviance (Dishion et al. 1996). On the other hand, the lack of prestige bias in norm transmission makes it easier for multiple norms to exist in equilibrium (Henrich 2001). If popular peers were very influential, then a single norm could easily spread from them to "fixation" at 100% acceptance. We can get a rough sense of the implications of our results as follows. Suppose that each period, people observe one other person's decision, and their own next decision follows the conditional distribution shown in Figure 4. This gives us a transition matrix T. A row eigenvector of T, $a = aT$, gives a stable distribution of behavioural norms. Table 7 shows the result of this calculation, which is not far from the observed distribution in our experiment. Meritocrats have an absolute majority, but about 20% of the population remain egalitarian or mixed.

Table 7: Long run stable distribution of norms

Norm	Prevalence (%)
Underdog	4.70
Egalitarian	9.70
Mixed	11.80
Meritocratic	68.80
WTA	5.00

Conclusion

People's views of distributive fairness matter for economic and political outcomes, and different groups seem to hold different fairness norms. We showed that people can pick up norms from their peer group during youth. Our "lab in the field" environment gives us tight control over both the treatment and the response, allowing us to focus in detail on the moment of norm transmission. On the other hand, a limitation is that we do not examine long run effects on behaviour, nor other behaviours like punishing norm violators. So this work is complementary to field experiments on the transmission of social preferences in the long run (Kosse et al. 2016; Cappelen et al. 2016).

Existing work on social preferences tends to assume that people apply the same set of preferences in all contexts. But for differences between social groups and societies, the important question may be "in what contexts are different norms applied"? Our subjects understood both meritocratic and egalitarian norms. When their partner's behaviour conformed precisely to one of these, they could be persuaded to switch norms. This can be explained by a model in which subjects used their partner's decision as a guide to the appropriate norm in a specific, unfamiliar situation.

We looked at norm transmission via a single channel, the observation of peers' behaviour. Our experiment shows that young people may learn distributive norms from the choices of their peers, altering their behaviour and opinions as a result. Surprisingly, we found little evidence for biased transmission of norms: our participants were not more influenced by popular peers, and we saw no differences between the effect of friends and of non-friends. One possible exception is that friends within a tight clique with many shared friends were more influential. If true, this suggests that specific reference groups may play an important role in norm transmission. Future work could examine the importance of "preaching" and "punishment" channels, and could test if the transmission pattern is the same for different norms, such as altruism and reciprocity.

Appendix 1: matching algorithm

Within each year-gender group, we matched pairs of mutual friends using the Blossom algorithm which picks out pairs who are linked in a network. Since the algorithm cannot guarantee to match everyone, if this left any unmatched individuals, we then matched pairs of non-mutual friends, i.e. pairs where only one person had named the other as a friend. Lastly, any remaining individuals were matched with a random person (or with nobody for at most one person.

Appendix 2: proofs

The requirements to prove facts 1 and 2 are that $\alpha > 1 - 1/(2+q)$, that π is close to 1 and that s is close to 0.

Suppose that $\alpha > 1 - 1/(2+q)$. Then individuals who receive no signal will choose $m_i^1 = 1$ according to their prior. Individuals who observe $a_i = 1$ will also choose $m_i^1 = 1$. Individuals who observe $a_i = 0$ will have a posterior belief

$$\mu_i = \frac{(1-\pi)\alpha}{(1-\pi)\alpha + \pi(1-\alpha)}$$

For high enough π, this will ensure that $\mu_i < 1/(2+q)$, leading i to choose $m_i^1 = 0$. This proves fact 1.

Individuals who observe $m_{p(i)}^1 = 0$ can then infer that their partner's signal was $a_{p(i)} = 0$. If they have received no signal themselves, they will then update their prior just as their partner did and choose $m_i^2 = 0$ in the second decision. (Individuals who have observed $a_i = 1$ will not change their behaviour as the two signals cancel out. Individuals who have observed $a_i = 0$ will also not change their behaviour as they were already playing egalitarian.)

Subjects who observe $m_{p(i)}^1 = 1$ will update their prior slightly, but not by much if relatively few subjects receive a signal, since then most partners who are meritocratic simply received no signal themselves. Specifically, if their belief after their own signal (if any) is μ_i^1, their belief after observing their partner's signal is

$$\frac{(1-sp)\mu_i^1}{(1-sp)\mu_i^1 + (1-s(1-p))(1-\mu_i^1)}$$

where $1 - sp$ is the probability that the partner got no signal or the wrong signal when the true state is $m = 1$, and $1 - s(1-p)$ is the probability that the partner got no signal or the right signal when the true state is $m = 0$. For small enough s this approaches μ_i^1 and their belief does not change much. Thus, those who were already choosing $m_i^1 = 1$ will continue to do so; those who were choosing $m_i^1 = 0$ will also continue to do so if s is small enough. This proves fact 2.

Appendix 3: further analyses

Original analysis

Our original specification planned to regress raw meritocracy score at time 2 on partner's raw meritocracy score at time 1. Figure 8 plots this relationship, which is positive but insignificant. However, the distribution of meritocracy scores shown in Figure 1 is clearly non-normal, so standard errors will not be correct. Also, we suspected that the slope was driven by outliers, i.e. underdog or winner-takes-all decisions, having less effect on partners' decisions than egalitarian or meritocratic decisions. To confirm this, the curved line in Figure 8 is a loess smoother. Clearly the relationship between the two variables looks different for extreme

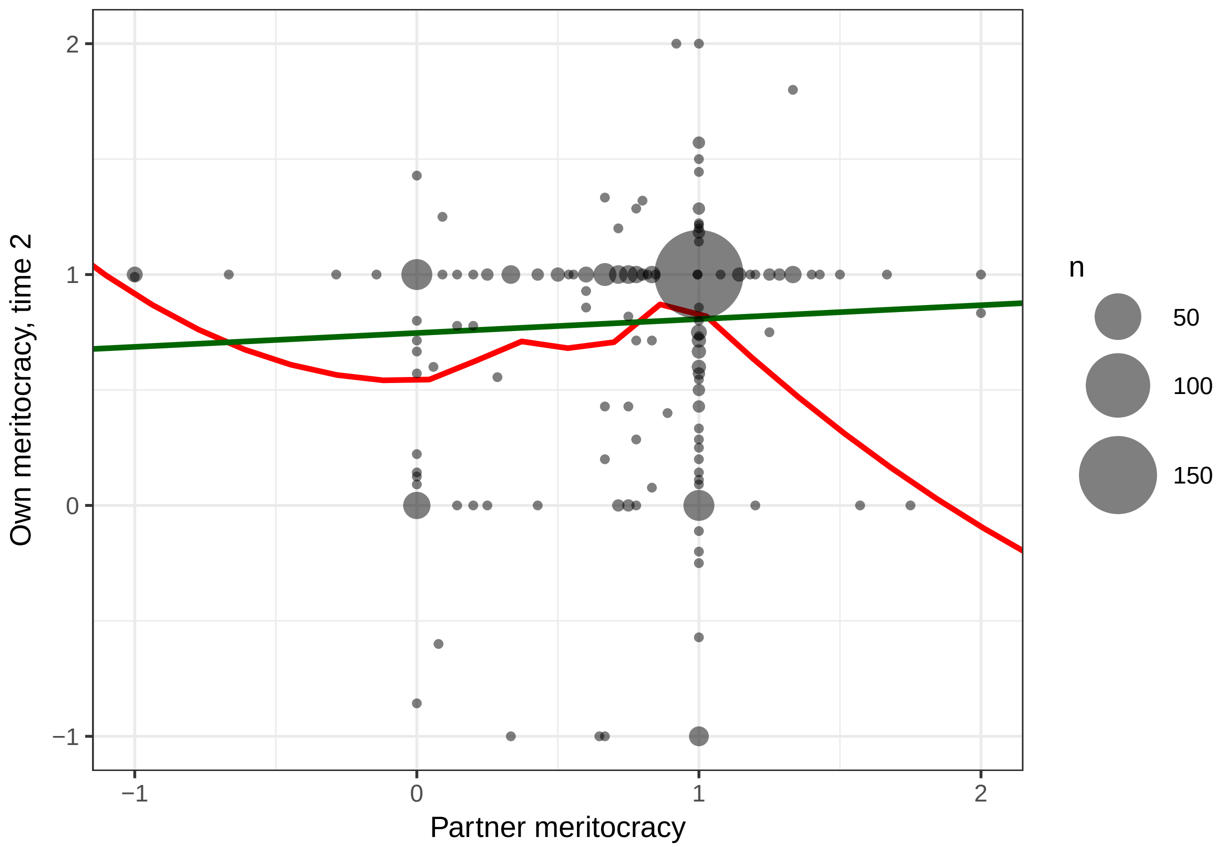

Figure 8: Bubble plot of meritocracy scores with smoothing and regression lines

values of meritocracy. This *ex post* observation motivated our choice to use fairness categories, rather than the raw meritocracy score, in our analysis.

Our deviation from our analysis plan raises concerns about robustness, multiple testing and the "garden of forking paths" (Gelman and Loken 2013). To address these, we run a series of regressions remaining as close to our original specification as possible, but removing outliers. Table 8 shows coefficients for partner's meritocracy score at time 1 in linear regressions of own meritocracy score at time 2, removing pairs where the partner's score was outside different lower and upper bounds. The last row excludes all "underdog" and "winner-takes-all" decisions. The effect of partner score increases and becomes significant when we remove only a few outliers. So, our original analysis becomes significant after making minimal changes. Nevertheless, we prefer our altered specification, as it more accurately reflects what subjects are doing.

Table 8: Coefficients on partner meritocracy score, regressions excluding outliers.

Lower bound	Upper bound	N	Coefficient	P value
-	-	459	0.059	0.230
-1.0	2.0	453	0.151	0.069
-0.5	1.5	442	0.271	0.007
0.0	1.0	418	0.288	0.008

S.E.s not clustered. Controls: year-gender dummies

Robustness checks

Table 9 runs a multinomial logit of subject's fairness category at time 2 on partner's fairness category at time 1. We exclude "underdog" decisions as there are too few for meaningful analyses. This is a more general specification than our main regressions, but the overall result is similar: egalitarian decisions by the partner make egalitarian choices by the subject more likely. They also have a marginally significant effect on mixed choices. Table 10 repeats this analysis, interacting partner fairness with the Friendship treatment. Table 11 repeats the multinomial logit including subject's own first decision as a control. The effects of Egalitarian partner decisions are robust.

Table 12 reruns the models in Table 4 using a logit specification. Results are similar to the probability linear models.

Table 13 reruns the models of *egalitarian attitude* in Table 6, using all the fairness categories as independent variables, rather than just egalitarianism (base category is meritocratic). Again, results are similar to the main specification.

Permutation tests

We rematch subjects in the Friend (Non-friend) treatment with a random friend (non-friend), who is of the same year, gender and treatment, and who was not the subject's actual partner. We then rerun our regressions. Repeating this 200 times gives us the distribution of coefficients under the null hypothesis that the actual partner with whom subjects were paired was irrelevant to their decision. We do this for Models 1 to 4 from Table 4, and for the meritocracy norm model (Model 1 from Table 6).

Figure 9 shows the results, plotting real coefficients against the central 95% intervals of coefficients from the permuted data. For models 2 and 4, we plot coefficients for both non-friends and friends (i.e. the coefficient on the main *Partner egalitarian* term, and this plus the coefficient on the interaction with *Friends*).

The main effect in Models 1 and 3 pass this test – they are outside the 95% interval under the null. So is the main effect for the meritocracy norm in Table 6. The effects for non-friends in Models 2 and 4 are also significant, while those for friends are not. Thus, we cannot reject that pairs of friends simply start with

Table 9: Fairness multinomial logit

	Egalitarian	Mixed	WTA
Intercept	-2.41^{***}	-1.83^{***}	-2.80^{***}
	(0.55)	(0.44)	(0.76)
Partner Egalitarian	1.86^{***}	0.96^{*}	-0.44
	(0.43)	(0.48)	(1.07)
Partner Mixed	0.38	0.51	0.23
	(0.44)	(0.37)	(0.52)
Partner WTA	0.02	-0.59	-0.61
	(0.68)	(0.79)	(1.07)
Friends treatment	0.15	0.70^{*}	0.05
	(0.34)	(0.32)	(0.45)
AIC	765.02	765.02	765.02
BIC	910.12	910.12	910.12
Log Likelihood	-346.51	-346.51	-346.51
Deviance	693.02	693.02	693.02
Num. obs.	416	416	416

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{+}p < 0.1$

similar attitudes. However, the insignificant interaction terms in Table 4, and the robustness of the effect for non-friends, make us suspect that the same social influence process is at work in both groups.

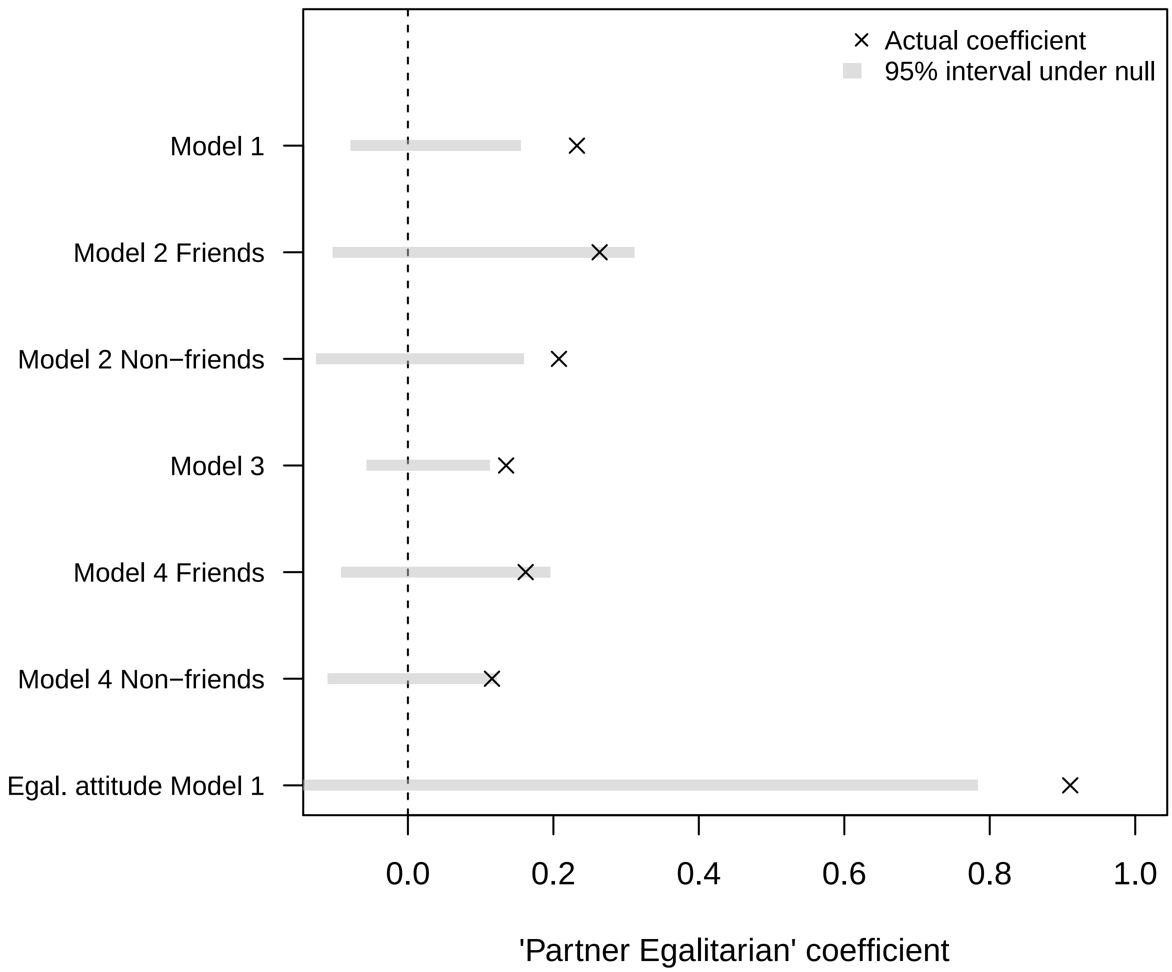

Figure 9: Permutation tests

Table 10: Fairness multinomial logit, including treatment interactions

	Egalitarian	Mixed	WTA
Intercept	-2.40^{***}	-2.03^{***}	-2.83^{***}
	(0.57)	(0.49)	(0.78)
Partner Egalitarian	1.90^{**}	0.81	-11.97
	(0.62)	(0.86)	(466.70)
Partner Mixed	-0.03	1.01^{+}	0.31
	(0.71)	(0.56)	(0.73)
Partner WTA	0.97	0.88	0.67
	(0.90)	(1.15)	(1.16)
Friends treatment	0.18	1.02^{*}	0.16
	(0.50)	(0.44)	(0.56)
Partner Egalitarian x Friends	-0.08	0.20	12.15
	(0.86)	(1.04)	(466.70)
Partner Mixed x Friends	0.69	-0.84	-0.18
	(0.91)	(0.73)	(1.03)
Partner WTA x Friends	-1.82	-2.24	-17.65^{***}
	(1.42)	(1.59)	(0.00)
AIC	773.70	773.70	773.70
BIC	955.08	955.08	955.08
Log Likelihood	-341.85	-341.85	-341.85
Deviance	683.70	683.70	683.70
Num. obs.	416	416	416

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{+}p < 0.1$

Table 11: Fairness multinomial logit, including own first decision

	Egalitarian	Mixed	WTA
Intercept	-4.18^{***}	-2.96^{***}	-3.30^{***}
	(0.85)	(0.56)	(0.82)
Partner Egalitarian	2.19^{***}	1.29^{*}	-0.31
	(0.62)	(0.56)	(1.12)
Partner Mixed	0.33	0.51	0.38
	(0.61)	(0.42)	(0.55)
Partner WTA	-0.05	-0.21	-0.68
	(1.02)	(0.86)	(1.13)
Friend treatment	-0.27	0.49	-0.02
	(0.47)	(0.35)	(0.47)
Decision 1 Egalitarian	5.29^{***}	2.64^{***}	-11.76
	(0.65)	(0.70)	(672.55)
Decision 1 Mixed	1.79^{**}	2.56^{***}	1.15^{*}
	(0.61)	(0.41)	(0.56)
Decision 1 WTA	0.86	2.76^{***}	2.74^{***}
	(1.18)	(0.59)	(0.62)
AIC	602.96	602.96	602.96
BIC	784.34	784.34	784.34
Log Likelihood	-256.48	-256.48	-256.48
Deviance	512.96	512.96	512.96
Num. obs.	416	416	416

$^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{+}p < 0.1$

Table 12: Logit models of egalitarian choice

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Intercept	-2.64^{***}	-2.66^{***}	-4.05^{***}	-3.97^{***}	-3.36^{***}	-3.87^{***}	-3.95^{***}	-4.01^{***}
	(0.54)	(0.56)	(0.72)	(0.78)	(0.88)	(0.71)	(0.89)	(0.73)
Partner egalitarian	1.62^{***}	1.82^{**}	1.62^{**}	1.88^{*}	2.43^{*}	1.22^{+}	1.31	1.62^{**}
	(0.45)	(0.64)	(0.54)	(0.83)	(1.07)	(0.64)	(1.05)	(0.59)
Friends		0.05		-0.18				
		(0.38)		(0.50)				
Partner egal. x Friends		-0.36		-0.54				
		(0.91)		(0.96)				
Own decision 1 egalitarian			4.05^{***}	4.07^{***}	4.04^{***}	3.94^{***}	4.03^{***}	4.03^{***}
			(0.46)	(0.46)	(0.47)	(0.46)	(0.47)	(0.45)
Self-other inclusion					-0.21			
					(0.14)			
Partner egal. x s-o inclusion					-0.26			
					(0.26)			
N shared friends						-0.36		
						(0.28)		
P. egal. x N shared friends						0.84^{+}		
						(0.43)		
Partner indegree							-0.01	
							(0.08)	
P. egal. x P. indegree							0.04	
							(0.14)	
Partner EV centrality								-1.39
								(2.62)
P. egal. x EV centrality								0.38
								(3.98)
AIC	292.09	295.85	189.01	192.22	185.32	190.04	192.83	192.70
BIC	329.25	341.27	230.19	241.64	234.47	239.46	242.16	242.12
Log Likelihood	-137.04	-136.92	-84.50	-84.11	-80.66	-83.02	-84.41	-84.35
Deviance	274.09	273.85	169.01	168.22	161.32	166.04	168.83	168.70
Num. obs.	459	459	454	454	444	454	451	454

S.E.s clustered by pair. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{+}p < 0.1$. Not shown: gender/year dummies.

Table 13: Models of egalitarian attitude

	(1)	(2)	(3)
Intercept	-1.84^{***}	-1.71^{***}	-2.09^{**}
	(0.28)	(0.29)	(0.79)
Partner Underdog	-0.69^{*}	-0.77	-0.44^{+}
	(0.30)	(0.49)	(0.26)
Partner Egalitarian	0.91^{*}	0.60	0.44
	(0.36)	(0.49)	(0.31)
Partner Mixed	-0.09	-0.50^{+}	-0.20
	(0.21)	(0.27)	(0.18)
Partner WTA	-0.13	-0.62	-0.06
	(0.38)	(0.53)	(0.39)
Friend treatment	-0.12	-0.42^{+}	-0.20
	(0.17)	(0.23)	(0.15)
Friend x Partner Underdog		0.17	
		(0.57)	
Friend x Partner Egalitarian		0.60	
		(0.71)	
Friend x Partner Mixed		0.84^{*}	
		(0.42)	
Friend x Partner WTA		0.89	
		(0.74)	
R^2	0.04	0.05	0.26
Adj. R^2	0.02	0.02	0.23
Num. obs.	459	459	454
RMSE	1.82	1.82	1.61

S.E.s clustered by pair. $^{***}p < 0.001$, $^{**}p < 0.01$, $^{*}p < 0.05$, $^{+}p < 0.1$. Not shown: gender/year dummies, time 1 fairness cat. (model 3).

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