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Discrimination at Young Age: Experimental Evidence from Preschool Children

Abstract

Discrimination is an ubiquitous phenomenon in many societies, but little is known about its origins in childhood. In a framed field experiment, we let 142 three to six-year old preschool children allocate a fixed endowment between an in-group and an out-group receiver in two domains (gender and group affiliation). Discrimination is prevalent in our subjects, since they allocate more than half of their endowment to the in-group. The extent of discrimination does not differ between domains, suggesting that it is a universal, as opposed to a domain-specific, trait. Analyzing age dynamics, we find that discrimination develops with age.

JEL-Codes: C910, C930, D030.

Keywords: discrimination, children, experiment, fairness.

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1. Introduction

Discrimination against out-group members is a pervasive phenomenon in societies all around the world. Over the past decades, at least since the seminal contribution of Becker (1957), research in economics has accumulated an impressive evidence base for the prevalence of discrimination. This literature has shown that intergroup discrimination occurs in numerous settings, such as labor, housing, credit, or consumer markets, and that it is based on various attributes like race, gender, or religion (for surveys of the field, lab, and non-experimental literature, see Arrow (1998), Yinger (1998), Altonji and Blank (1999), Riach and Rich (2002), Anderson et al. (2006), List and Rasul (2011), Bertrand and Duflo (2017)). But while most of the scientific evidence on discriminatory behavior is based on adult samples, little is known about the development of discrimination early in life. In particular, economic studies on discrimination among very young children are extremely scarce. This is the research gap which we address in this paper.

In particular, we conducted an incentivized framed field experiment (Harrison and List, 2004) to study discrimination in 142 preschool children aged between three and six years. We measured taste-based discrimination using the simple other-other allocation task, which requires the decision maker to split a fixed endowment between two receivers from different groups, without keeping any part of the endowment for herself. Based on the notion that group identity is multidimensional (Akerlof and Kranton (2000)), we implemented two treatments to test whether the extent of discrimination is sensitive to the attributes which define group affiliation. In treatment *Gender Domain*, the in-group (out-group) receiver is a child with the decision maker's gender (the opposite gender). In treatment *Group Domain*, the in-group (out-group) receiver is a child from the decision-maker's own preschool group (another preschool). We randomly assigned children to one of the two treatments. After the decision in the assigned treatment was made, we also elicited the child's decision in the respective other treatment. This experimental design facilitates a clean between-subject comparison between treatment *Gender Domain* and *Group Domain*, and, at the same time, permits analyzing within-subject dynamics.

Our experiment yields three key findings. First, discrimination is prevalent among our subjects: In both tasks, the number of tokens allocated to the in-group receiver is significantly larger than the equal split of two tokens.[1] Second, the extent of discrimination is statistically indistinguishable between both treatments. This suggests that it is a rather universal trait among children, at least across the two group-defining attributes (gender and group affiliation) scrutinized in this paper. Our within-subject analysis reveals that almost half of our subjects (47%) allocate the exact same amount of tokens to the in-group in both treatments, whereas 22% (31%) allocate relatively more tokens to

[1] The initial endowment was four tokens.

the in-group in treatment *Gender Domain* (*Group Domain*). Third, discrimination develops with age: While we cannot reject the null hypothesis of no discrimination among our youngest cohort (aged three to four years), the tokens allocated to in-group members significantly exceeds the equal split among the middle cohort and the oldest cohort (aged four to five, and five to six years, respectively). This pattern is present in both treatments and it is also reflected in direct comparisons across age groups: The number of tokens allocated to the in-group is significantly higher in the two older cohorts than in the youngest cohort.

Our paper contributes to several strands of economic research. At the most general level, it adds to the growing literature studying economic behavior of children and adolescents (see Sutter et al. (2018) for a recent review). While children's distributional preferences have been under close scrutiny within this literature (e.g., Bettinger and Slonim (2006), Houser and Schunk (2009), Almås et al. (2010), Martinsson et al. (2011), Bauer et al. (2014), Angerer et al. (2015), Blake et al. (2015), Chen et al. (2016), Brocas et al. (2017), Sutter et al. (2018)), most papers thus far study school-aged children. We are only aware of few economic studies which cover children as young as the ones in our sample (e.g., Fehr et al. (2008), Gummerum et al. (2010), List and Samek (2013), Ben-Ner et al. (2017)). Our first contribution is therefore that we extend this small evidence base on distributional preferences of very young children.

The second contribution is that we gauge the development of intergroup discrimination at very young age. To date, the economics literature on discrimination among preschool children is extremely scarce.[2] The paper closest to ours is Fehr et al. (2008), who ran a series of binary dictator games with children aged three to eight years in Switzerland. They find that favoritism towards children from the decision maker's own (pre)school group develops with age. Similarly, List et al. (2017) study discrimination in dictator-game giving of three to five-year old children in the U.S. Surprisingly, they find that both White and Hispanic children send more resources to Black children than to White children. Black children send equal amounts. We extend this small literature by providing new evidence on the early origins of discrimination from preschool children in Austria.

Third, we contribute to the discrimination literature more generally by studying the stability of discriminatory behavior across domains within the same individuals. We thereby work towards an understanding of the extent to which discrimination is a general versus a domain-specific trait in humans. While previous work has investigated the generality of other economic preferences across domains (e.g., Einav et al. (2012), Ubfal

[2] Several previous papers in economics investigate the development of discrimination in school-aged children (e.g., Fehr et al. (2013), Angerer et al. (2016), Angerer et al. (2017)). While we focus on natural groups, a related strand of literature also investigates children's discriminatory behavior in the context of the minimal-group paradigm (see Dunham et al. (2011) and references therein). Our study is also related to developmental-psychology papers which study how children's sharing behavior towards hypothetical receivers is shaped by receivers' characteristics (e.g., Huppert et al (2018)).

(2016)), this study is, to the best of our knowledge, the first one to scrutinize within-subject stability of discriminatory behavior.

The rest of this paper is organized as follows. In section 2, we introduce our experimental design and procedure. In section 3, we present our results and section 4 concludes.

2. Experimental design and procedure

2.1 Design

Each subject participated in two other-other allocation tasks. In each task, the decision maker was anonymously matched with two other children (one from the decision maker's in-group, one from the out-group) and had to divide four tokens between the two. The fact that the decision maker must not keep any tokens for herself mutes potential strategic or selfish concerns, which makes the task well-suited for measuring pure taste-based discrimination (see Angerer et al. (2017)).[3]

We implemented two treatments which differ in the attributes which define in-group/out-group status: gender and preschool-group affiliation.[4] In treatment *Gender Domain*, the decision maker was informed that one receiver is a girl, and the other one is a boy (see Appendix B for experimental instructions and material). In treatment *Group Domain*, we informed the decision maker that one receiver was from her own preschool group, and the other was from another group in another preschool.[5]

We randomized children at the individual level either into treatment *Gender Domain* or *Group Domain*. The decision maker learned that there was a second task, the respective other treatment, only after the decision in the first task was made and recorded. This experimental design permits a clean assessment of treatment effects (comparison of allocations across treatments *Gender Domain* and *Group Domain* in the first task) and, at the same time, enables the analysis of individual dynamics across both treatments (within-subject comparison of both tasks).

[3] Using an even number of tokens as endowment allows us to test children's allocations against the equal-split benchmark (2 tokens), which seems to be a focal point for children from early on (e.g., Sloan et al. (2012)).

[4] Focusing on these two attributes seems natural given the state of the literature: Gender-based discrimination is well-documented in economics (e.g., Bertrand and Duflo (2017)), and various previous papers on discrimination in children use (pre)school groups to define in-group/out-group status (e.g., Fehr et al. (2008, 2013), Angerer et al. (2016)).

[5] Note that we explicitly informed the decision maker that she does not know the receivers' group affiliations (gender) in treatment *Gender Domain* (*Group Domain*). In treatment *Group Domain*, we exploited the fact that preschools define their groups using mascots (e.g., the mouse group, the hedgehog group, or the mushroom group) by using pictures of the decision maker's group mascot to convey group affiliation (see Appendix B for an example). Additionally, we independently randomized the order of explanation of the in-group and the out-group receiver within treatments.

2.2 Subject pool and procedure

The experiment was conducted during regular preschool hours in three preschools (ten preschool groups) in two Austrian municipalities in June 2018. We obtained the approval from the Internal Review Board of the University of Innsbruck, the Tyrolean State Board of Education, the heads of the involved preschools, as well as the consent from the parents of the involved children to run our experiment. All parents received a letter with general information about the study (which did not reveal its purpose) and a consent form. The head of one preschool asked us to implement an opt-out consent form for the sake of administrative ease. In the other preschools, we implemented an opt-in design.

An important challenge when conducting economic experiments with very young children is to ensure understanding and full attention. This is particularly critical in our setting because we implemented an anonymized experimental protocol which requires some level of abstraction. This design choice was motivated by the concern that lifting anonymity could bias children's behavior by introducing strategic considerations such as reciprocal motives (e.g., Fehr et al. (2008)). To foster comprehension, we employed a one-on-one protocol where each of our 14 experimenters explained the task to a single child at a time. The experimenters, who were mostly Education students, were trained to memorize the instructions of the task and explain it orally with some visual support.[6] In order to check for comprehension, each child had to repeat the instructions in her own words. Our analysis is based on those 142 children who were able to repeat them correctly. It is reassuring that only 8 additional children failed to do so (see Table 1).

As is standard for economic experiments with young children, we incentivized choices using tokens which could be exchanged for small presents in our experimental shop. The presents included sweets, hop balls, colored pencils, wax crayons, small plastic animals, books, rulers, and various stickers. The price of each present was one token, and they were handed out in sealed, non-transparent bags. To ensure the salience of the incentives, the experimenter introduced the child to the experimental shop before explaining the task. All decision makers received an unconditional participation fee of four tokens to be spent in the shop after the experiment.[7] They were also reminded that the receivers in the other-other allocation tasks could also buy presents with the tokens allocated to them. Both decisions were paid.

[6] Note that about a quarter of the children in our sample (23%) are not German mother tongue speakers (see column 1 of Appendix Table A1), with Turkish being the most common non-German mother tongue. To sidestep language-based comprehension problems, the respective children could decide between German or Turkish instructions (delivered by bilingual experimenters using a translated script).

[7] We used tokens of different colors to avoid confusion: The participation fee was paid in orange tokens (i.e., those which had to be spent by the decision maker), and white tokens were used for the other-other allocation tasks (i.e. those which had to be divided between the two receivers).

3. Results

We present our results in two steps. First, we provide evidence for the prevalence of discrimination in treatments *Gender Domain* and *Group Domain*, and investigate within-subject differences across treatments. Second, we analyze the relationship between discrimination and children's background characteristics.

3.1 Discrimination in treatments Gender Domain and Group Domain

Figure 1 depicts the average number of tokens allocated to the in-groups. The white bar in the left panel shows that the decision makers allocate an average amount of 2.43 tokens to receiver from their own gender respectively preschool group. This number is significantly higher than the equal split of two tokens (see p-values of a Wilcoxon signed rank test against the equal split at the bottom of the bar), which shows that discriminatory behavior is prevalent in our sample. Turning to treatment differences, the average number of tokens allocated to the in-group is 2.36 in treatment *Gender Domain* and 2.51 in treatment *Group Domain*. While both numbers are significantly larger than the equal split, the treatment difference of 0.15 tokens is statistically insignificant (p=0.142; Wilcoxon signed rank test). Thus, discrimination seems to be a rather general phenomenon among children because its magnitude does not depend on what attributes (gender or preschool group affiliation) are used for defining in-group/out-group status. While these findings are based on non-parametric analyses, OLS regressions where we cluster standard errors at the individual level yield the same results (see column 1 of Table 2).[8]

Our findings are corroborated in between-subject analyses which only use each child's choice in the randomly assigned first task.[9] In the right panel of Figure 1, we again find that the number of tokens sent to the in-group (2.35) is significantly larger than the equal split. This is true in treatments *Gender Domain* (see light grey bar, 2.29), in treatment *Group Domain* (see dark grey bar, 2.39), and when pooling both treatments (see white bar). Again, our OLS-regression analyses replicate these findings (see Appendix Table A2). It is reassuring that our results are robust when only focusing on first choices since they are, by design, unaffected by potential cross-treatment spillovers.

[8] Throughout the paper, we follow Angrist and Pischke (2009) and estimate our regression coefficients using OLS for the sake of easy interpretability. Note, however, that non-linear models yield qualitatively identical results (results available upon request). To take out order effects, all regressions include order dummies.

[9] 68 and 74 children were randomly assigned to treatment *Gender Domain* and *Group Domain* as their first decision task. To test whether the randomization successfully balanced our participants' observed characteristics across the two treatments, we investigate whether our covariates (age, gender, mother tongue, number of siblings, nursery attendance, number of days in afternoon care, and preschool group) can predict treatment status. Column 2 of Appendix Table A1 reports coefficients and p-values of regressions of the form $Treatment_i = \gamma_0 + \gamma_1 Covariate_i + \varepsilon_i$ for each of the covariates separately. It is reassuring that only one out of 18 regressions yields a coefficient γ_1 that is significant at the 10 percent level, which would be easily expected by pure chance. Likewise, regressing treatment status on all covariates simultaneously yields a p-value for joint significance of 0.851 (joint F-test). Thus, the balancing tests shows that random assignment worked as intended.

Next, we analyze within-subject treatment differences. Table 3 displays the joint distribution of choices made in treatments *Gender Domain* and *Group Domain*. The marginal distributions show that 48% of children in the former, and 46% of children in the latter treatment allocate two tokens to both receivers. Thus, egalitarian allocations seem to be a focal point for many children, which is consistent with insights from developmental psychology (e.g., Sloane et al. (2012)). Importantly, however, a non-negligible share of 38% allocate more tokens to the in-group, and only 13% allocate more tokens to the out-group in treatment *Gender Domain*. The numbers in treatment *Group Domain* are remarkably similar (44% versus 9%). The diagonal of the matrix shows that 47% choose the same allocation in both treatments, and the majority of 80% deviates by at most one token sent between both treatments. This high level of choice consistency across treatments suggests that discriminatory behavior is rather robust within individuals across domains. Furthermore, the area above (below) the diagonal of Table 3 shows that 22% (31%) allocate relatively more tokens to the in-group in treatment *Gender Domain* (*Group Domain*).

In Appendix Table A3, we investigate how these cross-treatment choice profiles relate to children's background characteristics. To this end, we regress indicators of whether a subject's in-group allocation is higher, equal, or lower in treatment *Gender Domain* versus *Group Domain* on our set of covariates. As it turns out, subjects' characteristics hardly predict choice profiles. The only exception is gender: Boys, as compared to girls, are more likely to choose the same allocation in both treatments (see column 2), and they are less likely to allocate more tokens to the in-group in treatment *Gender Domain* than in *Group Domain* (see column 3). This suggests that intergroup attitudes are particularly domain-independent among boys. We consider this gender difference interesting given the previous literature's finding that males tend to exhibit more intergroup discrimination than females (e.g., Fershtman and Gneezy (2001), Angerer et al. (2017), Lavy et al. (2018)). In the next section, we scrutinize the predictors of discrimination in greater depth.

3.2 The relationship between discrimination and children's background characteristics

Figure 2 presents the relationship between children's age and the number of tokens allocated to the in-group. While we cannot reject the null hypothesis of no discrimination in the youngest age cohort of three to four-year olds, discrimination is larger in magnitude, and statistically significant among the middle cohort (aged four to five years), and the oldest cohort (aged five to six years). This age pattern is very similar in treatments *Gender Domain* (left panel) and *Group Domain* (right panel). The finding that discrimination increases with age is also corroborated in our regression analysis in Table 2: The coefficient on the dummy variables for the middle cohort and the oldest cohort are positive and significant, revealing that older children allocate significantly more tokens to their in-groups than the youngest cohort. This finding is in line with Fehr et al. (2008)

and Angerer et al. (2016) who show that discrimination increases with age in binary dictator games and prisoner-dilemma games, respectively. Again, our results are robust to only considering children's first choices (see Appendix Table A2).

Children's background characteristics other than age turn out to be unrelated to discrimination. Motivated by increasing evidence that discrimination seems to be a predominately male phenomenon, we first investigate gender differences in discrimination. In contrast to Angerer et al. (2017), who find that six to eleven-year old boys discriminate more than girls in the other-other allocation task, the small and insignificant coefficients on the male-dummy in Table 2 reveal the absence of gender differences in our setting. This might suggest that the gender gap in discrimination develops only at early school age.[10] Finally, the regression in column 4, in which we add our full set of background characteristics, shows that, while the age effect remains robust, all other explanatory variables enter insignificantly. At the most basic level, these insignificant coefficients reiterate our above notion that discrimination is a rather general phenomenon since it is not driven by a particular subgroup of subjects.

4. Conclusion

Discrimination is a ubiquitous phenomenon which might entail large efficiency costs for society, e.g. through inefficient allocation of factors of production on the labor market, or through undermining public goods provision (Habyarimana et al. (2007)). But while the pervasiveness of adults' discriminatory behavior is well-documented in economics, evidence on its developmental origins in early childhood is scarce. This is unfortunate since understanding the early development of discrimination might prove very helpful for designing anti-discrimination policies that tackle the behavior before it becomes internalized (Hewstone et al. (2002)).

We address this research gap by studying the development of discrimination in a framed field experiment with 142 preschool children aged three to six years. We use the other-other allocation task which, due to its simplicity, is well-suited for studying discrimination in young children. We find that children consistently allocate more experimental tokens to their in-group than to their out-group. Discrimination is statistically indistinguishable between treatments *Gender Domain* and *Group Domain*, which differ in the attributes used to define in-group/out-group status (gender and preschool group affiliation, respectively). This suggests that discrimination is a rather general trait in children. Analyzing age dynamics, we find that discrimination gets more pronounced with age.

Based on our findings, we see at least two interesting avenues for future research. First, the present paper only investigates the development of early discrimination in two domains, gender and preschool group affiliation. It would be interesting to extend this evidence to other attributes, such as ethnicity, language group affiliation, or immigrant

[10] We consider scrutinizing the development of gender differences in discrimination with age an interesting avenue for future research.

status. Second, given that discrimination seems to develop early in life, we consider designing and testing interventions to mitigate discrimination early on a highly policy-relevant exercise.

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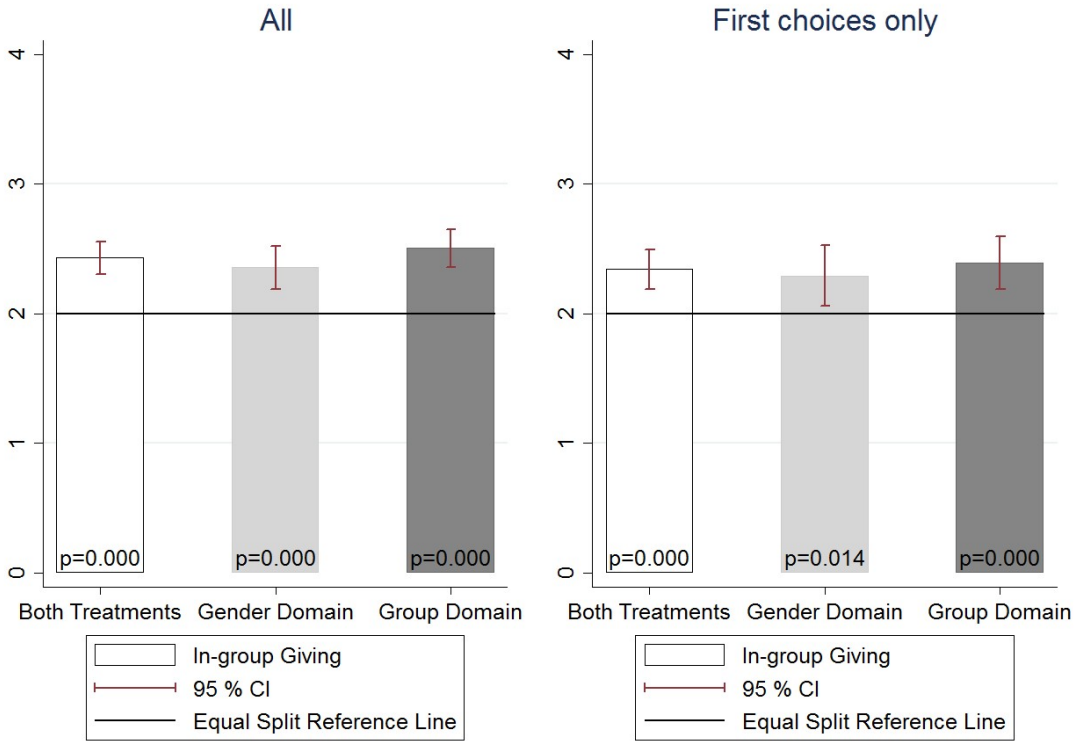
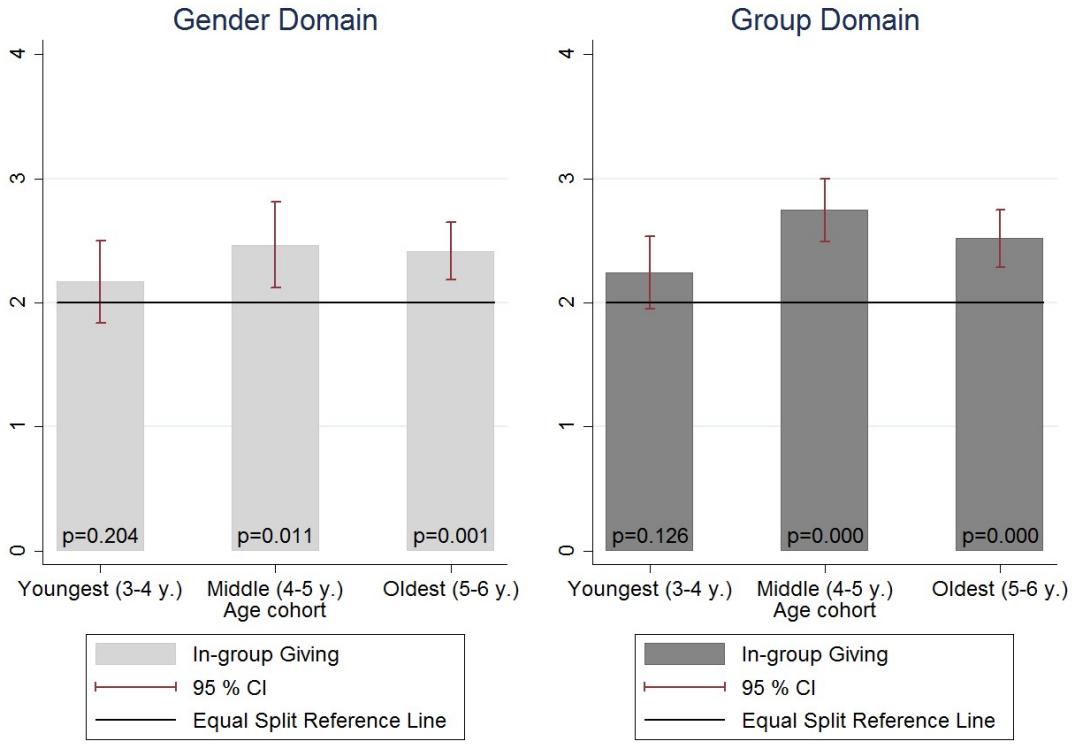
Tables and Figures

Figure 1: Average tokens allocated to in-group, by treatment



Notes: Left panel: All choices (N=284, 142, and 142 for both treatments together, treatment *Gender Domain*, and treatment *Group Domain*, respectively). Right panel: Analysis restricted to first choices only (N=142, 68, and 74 for both treatments together, treatment *Gender Domain*, and treatment *Group Domain*, respectively). The p-values depicted at the bottom of the bars stem from Wilcoxon signed rank tests of H_0: tokens sent to in-group = 2. Error bars, mean ± SEM.

Figure 2: Average tokens allocated to in-group, by treatment and age cohort.



Notes: Left panel: Treatment *Gender Domain*. Right panel: Treatment *Group Domain*. The p-values depicted at the bottom of the bars stem from Wilcoxon signed rank tests of H_0: tokens sent to in-group = 2. Error bars, mean ± SEM.

Table 1: Number of participants, by age, gender, and understanding.

Cohort	Female	Male	Total
Young (3 to 4 years)	23 (3)	18 (2)	41 (5)
Middle (4 to 5 years)	19 (1)	24 (0)	43 (1)
Old (5 to 6 years)	27 (1)	31 (1)	58 (2)
Total	69 (5)	73 (3)	142 (8)

Notes: The numbers in parentheses represent additional children who did not understand the task and were therefore excluded from the analysis.

Table 2: Determinants of tokens sent to in-group

	Dependent variable: Tokens sent to in-group			
	(1)	(2)	(3)	(4)
Group Domain	0.156		0.156	0.142
	(0.096)		*(0.097)*	*(0.099)*
Age cohort (baseline: 3-4 y.)				
Middle cohort (4-5 y.)		0.398**	0.398**	0.520***
		(0.171)	*(0.171)*	*(0.178)*
Oldest cohort (5-6 y.)		0.259*	0.259*	0.376**
		(0.150)	*(0.150)*	*(0.153)*
Male		-0.005	-0.005	-0.013
		(0.124)	*(0.125)*	*(0.128)*
Mother tongue German				-0.136
				(0.149)
Number of siblings				0.006
				(0.072)
Attended nursery				0.080
				(0.149)
Days per week in afternoon care				0.015
				(0.057)
Preschool-group dummies	No	No	No	Yes
Order dummies	Yes	Yes	Yes	Yes
Constant	2.447***	2.298***	2.223***	2.158***
	(0.100)	*(0.133)*	*(0.144)*	*(0.296)*
Observations	284	284	284	282
Clusters	142	142	142	141
R^2	0.015	0.035	0.042	0.085

Notes: OLS regressions. Robust standard errors (clustered at the individual level) in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, *** p < 0.10. Post-estimation Wald tests of H_0: $\beta_{constant} = 2$ and H_0: $\beta_{constant}+\beta_{group\ domain} = 2$ in column 1 are highly significant (p=0.000), thus confirming the prevalence of discrimination in treatment *Gender Domain* and *Group Domain*, respectively.

Table 3: Distribution of subjects' choice profiles across treatments

		Gender Domain					
	Tokens allocated to in-group	**0**	**1**	**2**	**3**	**4**	**Sum**
Group Domain	**0**	1 (1%)	0	0	0	0	1 (1%)
	1	0	0	7 (5%)	4 (3%)	1 (1%)	12 (8%)
	2	3 (2%)	7 (5%)	39 (27%)	10 (7%)	7 (5%)	66 (46%)
	3	1 (1%)	5 (4%)	17 (12%)	15 (11%)	2 (1%)	40 (28%)
	4	2 (1%)	0	5 (4%)	4 (3%)	12 (8%)	23 (16%)
	Sum	7 (5%)	12 (8%)	68 (48%)	33 (23%)	22 (15%)	142 (100%)

Notes: Numbers display the (relative) number of subjects with respective combination of choices in both tasks. Grey-shaded cells contain individuals who took the exact same choices in both treatments.

Appendix

Appendix A: Additional Tables

Table A1: Summary statistics and balancing test

Covariate	Mean [SD] (1)	Covariates (2)	
Age cohort			
Youngest cohort (3-4 y.)	0.289	0.013	*(0.093)*
Middle cohort (4-5 y.)	0.303	-0.020	*(0.092)*
Oldest cohort (5-6 y.)	0.408	0.007	*(0.086)*
Male	0.514	0.029	*(0.084)*
Mother tongue German	0.775	0.027	*(0.101)*
Number of siblings	1.183 [0.856]	0.044	*(0.045)*
Attended nursery	0.430	-0.064	*(0.085)*
Days per week in afternoon care	0.450 [1.200]	0.054*	*(0.032)*
Preschool group			
Group 1	0.106	-0.014	*(0.137)*
Group 2	0.134	0.055	*(0.124)*
Group 3	0.099	0.103	*(0.140)*
Group 4	0.141	0.025	*(0.121)*
Group 5	0.077	0.072	*(0.157)*
Group 6	0.127	0.088	*(0.126)*
Group 7	0.077	-0.125	*(0.153)*
Group 8	0.070	-0.085	*(0.162)*
Group 9	0.077	-0.026	*(0.157)*
Group 10	0.092	-0.188	*(0.136)*
Observations	142	142	

Notes: Column 1: Sample means; standard deviations in brackets (for non-dummy variables). Column 2: Each row reports the coefficients from regressions of the form $Treatment_i = \gamma_0 + \gamma_1 Covariate_i + \varepsilon_i$ (robust standard errors in parentheses). Regressing treatment status jointly on all covariates yields a p-value for joint significance of 0.851 (joint F-test). Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table A2: Determinants of tokens sent to in-group (first choices only)

	Dependent variable: Tokens sent to in-group			
	(1)	(2)	(3)	(4)
Group Domain	0.098		0.092	0.079
	(0.155)		*(0.153)*	*(0.153)*
Age cohort (baseline: 3-4 y.)				
Middle cohort (4-5 y.)		0.531**	0.529**	0.667***
		(0.218)	*(0.219)*	*(0.244)*
Oldest cohort (5-6 y.)		0.329*	0.329*	0.503**
		(0.190)	*(0.190)*	*(0.213)*
Male		0.012	0.015	0.046
		(0.151)	*(0.152)*	*(0.155)*
Mother tongue German				0.206
				(0.199)
Number of siblings				0.001
				(0.092)
Attended nursery				0.266
				(0.192)
Days per week in afternoon care				0.039
				(0.068)
Preschool-group dummies	No	No	No	Yes
Constant	2.294***	2.044***	1.996***	1.501***
	(0.117)	*(0.161)*	*(0.182)*	*(0.388)*
Observations	142	142	142	141
R^2	0.003	0.051	0.054	0.131

Notes: OLS regressions. Analysis restricted to first choices only. Robust standard errors in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, *** p < 0.10. Post-estimation Wald tests of H_0: $\beta_{constant}$ = 2 and H_0: $\beta_{constant}$+$\beta_{group\ domain}$ = 2 in column 1 are highly significant (p=0.013 and p=0.000), thus confirming the prevalence of discrimination in treatment *Gender Domain* and *Group Domain*, respectively.

Table A3: Characterizing subjects' choice profiles across treatments

	Amounts allocated to in-group in both treatments		
	Group > Gender (1)	Group = Gender (2)	Group < Gender (3)
Age cohort (baseline: 3-4 y.)			
Middle cohort (4-5 y.)	0.156	-0.117	-0.039
	(0.116)	*(0.115)*	*(0.110)*
Oldest cohort (5-6 y.)	0.055	-0.004	-0.051
	(0.115)	*(0.121)*	*(0.099)*
Male (=1)	-0.015	0.199**	-0.184**
	(0.083)	*(0.086)*	*(0.075)*
Mother tongue German	0.040	-0.037	-0.003
	(0.101)	*(0.105)*	*(0.082)*
Number of siblings	0.008	0.041	-0.049
	(0.054)	*(0.048)*	*(0.038)*
Attended nursery	-0.066	0.125	-0.058
	(0.091)	*(0.096)*	*(0.078)*
Number of days afternoon care	0.002	0.006	-0.009
	(0.036)	*(0.035)*	*(0.032)*
Preschool-group dummies	Yes	Yes	Yes
Order dummies (1=gender domain first)	Yes	Yes	Yes
Constant	0.177	0.468***	0.355***
	(0.179)	*(0.177)*	*(0.117)*
Observations	141	141	141
R^2	0.094	0.170	0.162

Notes: OLS regressions.. Dependent variables: Dummy variables (1=condition in column header is fulfilled, 0 else). Robust standard errors in parentheses. Significance levels: *** p < 0.01, ** p < 0.05, *** p < 0.10.

Appendix B: Experimental instructions and material

Experimental instructions

Instructions translated from German into English (the original German instructions are available on request). Instructions were explained individually to each child by one of the experimenters. In the following version, the first task was treatment *Group Domain*. Apart from randomizing the order of the treatment, we also independently randomized the order of explanation of the in-group and the out-group receiver within treatments. General instructions for the experimenter are italicized.

Treatment *Group Domain*

I have prepared a game for you. In this game, you receive these orange tokens (*show the orange tokens to the child and put them to the side*). How many tokens do I have here? You can put them in your wallet and purchase a gift for each token after the game. How many gifts can you buy for these tokens?
The game is about splitting tokens between two other children. The other children are selected by us (*show the yellow / red meeple to the child, these are the other children*). You do not know exactly who the other children are. That is a secret. They can be girls or boys. You only know that the yellow meeple is another child from your preschool group. What is your preschool group (*wait for the child to answer*)? So the yellow child is also from the (*name group, show group symbol card of the own preschool group*). For the red meeple, you only know that it is another child from another preschool group in another preschool. *(show group symbol card with the empty rectangle)* The other children also do not know who you are. Can you repeat what I just told you? (*Child must say: (i) that the yellow child is from the same preschool group, (ii) that the red child is from another preschool group in another preschool, (iii) that it does not know exactly who the other children are, and (iv) that the other children do not know who the child is.*)
The game works as follows:
You receive these white tokens at the beginning (*put white tokens in front of the child*). How many tokens do I have here? (*Answer: 4*). You have to decide how many of these 4 tokens you want to put into the envelope for the yellow meeple - that's the yellow envelope (*point to the box with the yellow envelope on the decision sheet, place the yellow meeple onto the envelope and put down the group symbol card of the own preschool group*) and how many tokens you want to put into the envelope for the red meeple – that's the red envelope (*point to the box with the red envelope on the decision sheet, place the red meeple onto the red envelope and put down the empty group symbol card of the other preschool group*). For every token that you put into the yellow envelope, the child from your preschool group will receive one gift. For every token that you put into the red envelope, the child from the other preschool gets one gift.

Could you please briefly repeat how the game works? (*Child must repeat (i) playing with a secret child from their own / other kindergarten group, (ii) that they do not know exactly who the other children are, (iii) that she gets 4 tokens to split (iv) that receivers are allowed to buy gifts with the tokens they receive, and that the child received 4 orange tokens for participating*).

Very good. Now you can decide how you want to distribute the tokens. Please put the tokens onto the respective boxes *(on the decision sheet)*. Take as much time as you like for your decision. I am going to turn around in order not to disturb you. Just tell me when you are done. (*The experimenter turns around so that the child clearly sees that the experimenter is not watching the decision. Once the child says it is done, the experimenter turns back around.*) Thank you for your decision (*write down the number of tokens that the child assigned to the yellow / red meeple*). Can you please tell me how many gifts the yellow and the red child can choose? Who were the yellow and the red children again? (*These questions help once again to check comprehension. If this question is answered incorrectly mark that the child did not understand the game*.)

Treatment *Gender Domain*

I have prepared a second game for you. This game is quite similar to the first game. Your task is again to distribute tokens *(show 4 white tokens)* between two children (*second decision sheet is placed next to the first*). The only difference is that the game is played with different children now. The other children are selected by us (*show the green / blue meeple to the child, these are the other children*). You do not know exactly who the other children are. That is a secret. You do not know in which preschool group they are. You only know that the green child is a (*gender of the decision-making child*) boy/girl and the blue child is a (*different gender*) boy/girl. The other children also do not know who you are. Did you recognize the difference to the first game (*point on the green respectively blue meeple and let the child explain the difference*)? Exactly, the other children are a boy and a girl.

What do you think, how does this game work? (*Answer: I will get 4 tokens to distribute between the two meeples. I have to decide how many of the 4 tokens I want to put into the green envelope, and how many tokens you want to put into the blue envelope. For every token put into the green envelope, the boy/girl (same gender as the decision-making child) will receive one gift. For every token that I put in the blue envelope, the boy/girl (different gender than the decision-making child) gets one gift*.)

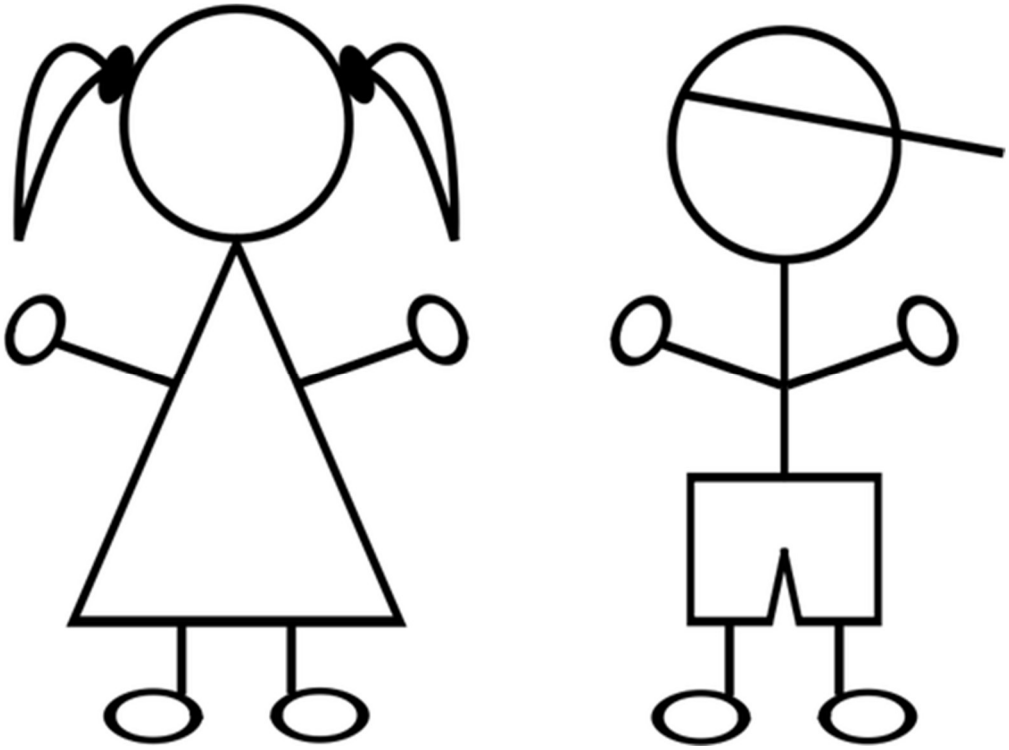
Very good. Now you can decide how you want to distribute the tokens. Please put the tokens onto the respective boxes. Take as much time as you like for your decision. I am going to turn around in order not to disturb you. Just tell me when you are done. (*The experimenter turns around so that the child clearly sees that the experimenter is not watching the decision. Once the child says it is done, the experimenter turns back around.*) Thank you for your decision (*write down the number of tokens that the child assigned to the green / blue meeple*). Can you please tell me how many gifts the green and the blue child can choose? Who were the green and the blue children again? (*These questions help once again to check comprehension. If this question is answered incorrectly mark that the child did not understand the game*.)

Please do not tell any other child about the game the game so that it is a surprise for them. With the four orange tokens you received, you can now choose gifts for yourself.

Group Symbol Cards (treatment *Group Domain*):

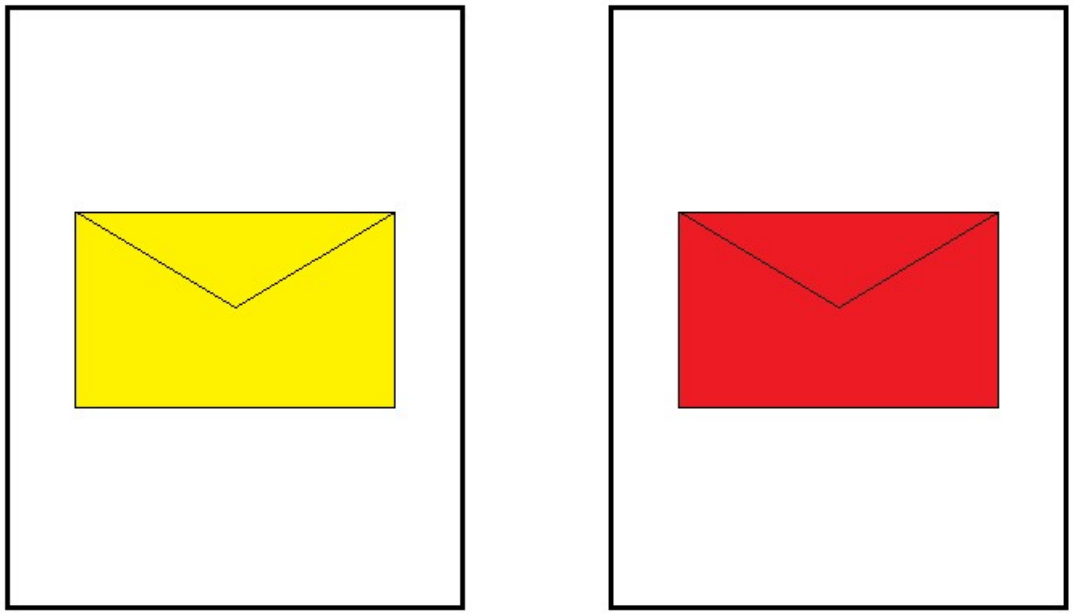
Example of group symbols for treatment *Group Domain*. The decision maker was a member of the pre-school's "Fliegenpilzgruppe" (mushroom group) and had to allocate her four tokens between another child from that group or another group in another preschool. The group symbol cards were used to symbolize the group affiliation of the receiver. The empty rectangle is a symbol for the unknown out-group.

Gender Symbol Cards (treatment *Gender Domain*):



Gender symbols for treatment *Gender Domain*. The decision maker had to allocate her four tokens between a child with the same gender or a child with the opposite gender. The gender symbol cards were used to symbolize the gender of the receiver.

Decision Sheet (for treatment *Group Domain*):



Decision sheet for treatment *Group Domain*. The decision maker had to allocate her four tokens between another child from the same preschool class (by putting tokens onto the yellow envelope) or another group in another preschool (by putting tokens onto a red envelope printed on the decision sheet). The decision sheet for treatment *Gender Domain* was identical, except for the colors of the envelopes (green and blue).

Photo of the experimental shop

Photo of the study set-up