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Does an Educated Mind Take the Broader View?

A field experiment on in-group favouritism among microcredit clients

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Abstract

A number of studies document an in-group bias in social dilemma situations. While group structure and dynamics are important in shaping in-group favouritism, less attention has been paid to individual characteristics affecting favouritism. Using data from dictator games conducted among 523 microcredit clients in Angola, this paper analyzes the effect of education on in-group favouritism. When addressing the endogeneity of education, we find that education increases in-group bias. This goes against the conventional view that education broadens the perspectives of an individual. In addition, our results suggest that in-group favouritism is related to gender, family background and access to particular forms of networks.

Keywords: in-group favouritism, parochialism, field experiment, social preferences, microcredit

JEL classification: C72, C93, D03, O12

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1 Introduction

Several recent studies in experimental economics find evidence for an in-group bias in social dilemma situations. In-group favouritism or parochialism has been found in variants of the dictator game, where dictators give more to members of their own group than to outsiders (Fehr et al. 2011; Bernhard et al. 2006; Güth et al. 2009; Chen and Li 2009), and in prisoner's dilemma and similar games, where cooperation is more frequent between group members than with outsiders (Goette et al. 2006; Charness et al. 2007; Ruffle and Sosis 2006). Some work has been done to examine what types of groups generate an in-group bias and under what conditions, and to identify the motives behind in-group favouritism. Less is known about how in-group favouritism develops, or its relation to individual characteristics. In a study of children aged 8 to 17, Fehr et al. (2011) find that parochialism increases with age, becoming significant in the teenage years. Beyond the dimension of age, however, we know little about how individual characteristics affect people's parochial preferences or their susceptibility to norms of in-group favouritism.

This paper analyzes the effect of education on in-group favouritism. Data was collected among 523 microcredit clients in Luanda, the capital of Angola. A standard dictator game with an in-group and an out-group version based on credit group affiliation was conducted to elicit information on in-group bias. The results show that subjects on average allocate a positive amount both to members of their own credit group, and to outsiders, which is consistent with other—regarding preferences such as altruism or egalitarian norms. Moreover, subjects allocate significantly more to recipients who are members of their own credit group than to outsiders, which confirms previous findings on in-group favouritism. In estimating the effect of education on in-group favouritism, we instrument for education to address the possibility that education is endogenous. The results show a strong positive effect of education on in-group favouritism. In other words, rather than make individuals more broad-minded, education seems to promote parochialism and a narrow group focus.

Our results contribute to and complement the literature on in-group favouritism in several ways. The design of Fehr et al. (2011) does not permit a distinction between effects due to mental development occurring naturally, and effects of education. While other studies corroborate the finding that distributional preferences evolve with age (Almås et al. 2010), in principle it is possible that the results of Fehr et al. partly reflect a positive effect of education on in-group favouritism. Though based on data from a population of adults, our results indeed suggest that this may be the case.

Like Bernhard et al. (2006), our study is from a context where formal institutions for legal enforcement or redistribution are weak or dysfunctional, and social norms can be expected to play a large role in regulating social interaction. However, where Bernhard et al. analyze favouritism towards one's own ethnic group, we find evidence for favouritism based on more short-lived social group constructions. In this sense, our study is also similar to that of Goette et al. (2006) and Ruffle and Sosis (2006) in looking at effects of real social groups. Such groups have a history of interaction which may be essential to their effect, compared to minimal groups constructed for the purpose of an experiment, whose effect may depend on their salience (Goette et al. 2006; Charness et al. 2007).

A positive effect of education on in-group favouritism also has some wider implications. A key idea in microfinance is that in the absence of collateral, joint group liability for loans creates incentives for repayment through social pressure from other groups members. Our results suggest that more educated people are more willing to prioritize in-group considerations over distributional demands from outside. In other words, increasing the education of microcredit clients may not only increase their success in business (de Mel et al. 2008; Berge et al. 2010), but may also contribute to the viability and sustainability of microcredit arrangements through enhanced identification with the credit group.

On the other hand, the positive effect of education on in-group favouritism can also be seen in a less favourable light. Education is often seen as important in broadening the perspectives of individuals, in making them focus on the greater good rather than the special interests of a more limited social group. In modernization theory, for instance, the increase in education that comes with increases in income is assumed to lead to a greater chance of democracy, since '[e]ducation presumably broadens men's outlooks' (Lipset 1959: 79). One possible interpretation of our results is, however, that education promotes particularism rather than universalism, and a more educated population therefore does not necessarily press for a more impartial institutional order. Viewed in this way, our results are broadly consistent with recent findings of Friedman et al. (2011) that education may strengthen stated attitudes of ethnic identification while having no effect on democratic attitudes.

The paper is structured as follows. Section 2 presents the contextual background of our study, the design of the experiment, and the identification strategy for estimating the effect of education. Section 3 presents the data and descriptive statistics. Section 4 presents our main results, followed by a discussion of robustness. Section 5 concludes with a discussion of the results and directions for further research.

2 Background and methodology

2.1 Background

For our experiment, we used subjects from the client pool of the Angolan microcredit institution Kixicredito. Kixicredito is the largest non-commercial microcredit institution in Angola. Established in 1999, it has a total of 8600 active clients in 12 branches across the country (African Development Bank 2010). Kixicredito clients are organized in solidarity groups consisting of 10-30 clients, with joint liability for loans. Membership in solidarity groups is the result of self-selection, and after an initial orientation phase, groups meet bi-weekly. Both self-selection and socialization through frequent meetings are potential sources of in-group favouritism. Our experiment included members from 51 randomly selected groups in two Kixicredito branches in central Luanda, Sâo Paulo and Hoje Ya Henda, a total of 539 clients.

2.2 Experimental design and procedures

The experiment was conducted as part of a larger survey during a six week period from February to March 2010.[1] The experiment took the form of an anonymous, one-shot standard dictator game. Each of the 539 subjects, 37 per cent of whom were male, participated in two versions of the game placing them in two different types of distributive situations. In the first version, subjects were told that at the end of the survey they would receive 500 kwanza, and that they could choose to keep this money or give some or all of it to an (anonymous) member of their own credit group (see Appendix 1 for a translated version of the precise instructions). In the second version, subjects were told that they would receive an additional 500 kwanza, and that they could choose to keep this money or give some or all of it to an (anonymous) recipient who was not a member of their own credit group. All subjects played the role of dictator twice, once in each version of the game. The subjects also participated as recipients twice, receiving one transfer from a dictator in their own credit group and one transfer from a dictator from another credit group.

In-group favouritism predicts a higher allocation in the first version of the game than in the second. Since each subject played the game in the two versions sequentially, this introduces a possible order effect, so some caution is advised in interpreting the absolute levels of in-group favouritism. As our main focus is on what explains differences in in-group favouritism between individuals, however, this is of limited concern. At the time of the experiment, the sum of 500 kwanza equalled about US$5.40, a substantial amount compared to daily wages or profits among Luandan microcredit clients (median daily profits in our sample are about US$17).

The experiment was conducted in the field, at the bi-weekly meetings of the credit groups. Since the groups do not meet at a central location, but at different locations throughout the city, this posed some challenges in terms of the physical set-up. Bringing the clients to some central location for the experiment would have been difficult and prohibitively costly, due to the extreme traffic congestion in Luanda. At each location, care was taken to preserve anonymity by taking the subjects out of hearing range from each other. Experiments were conducted manually with pen and paper in Portuguese by local enumerators, overseen by a supervisor. For logistical reasons, the design was single blind. At the end of the full survey, which lasted about 30 minutes, subjects were paid discreetly in cash according to the total amount kept in the two distributive situations. Funds allocated by each subject to in-group and out-group recipients were placed in differently marked envelopes. The in-group envelopes from the current group, and the out-group envelopes from the group last visited were shuffled by the supervisor, and one envelope of each type handed to each subject. There was no participation fee.

[1] The fact that the experiment took place over a period of six weeks, raises the possibility that communication across credit groups could affect the choices of groups surveyed later in the process. However, regressions of in-group favouritism on week dummies reveal no significant differences over time.

2.3 Estimation strategy

In addition to data on in-group favouritism from the experiment, the survey generated data on a number of background variables for the subjects. These include variables reflecting age, gender, education, family background, and more. In this paper, we attempt to identify the causal effect of education on in-group favouritism. A key problem in estimating this effect, is that education may be endogenous. If education is correlated with some unobserved variable affecting in-group favouritism, ordinary least squares (OLS) estimates will not be consistent. For instance, unobserved time preferences may have an effect on education as less patient individuals are less likely to forgo current consumption for increases in future consumption due to schooling. Time preferences may also affect the extent to which subjects favour their own group, but the direction of the effect is less obvious, as impatience may entail greater investment in relationships to people close to you, but it may also entail generally less investment in others whether close to you or not. OLS estimates of the effect of education on in-group favouritism may therefore be either too high or too low.

Friedman et al. (2011) use a randomized scholarship competition to address the endogeneity of education, and find that education strengthens stated attitudes of ethnic identification. Our experimental data permits analysis of the effect of education on in-group bias in terms of actual distributive choices rather than stated preferences. We address the endogeneity of education through instrument variable estimation, i.e. by using a variable correlated with education but not with in-group favouritism. The causal effect of education on in-group favouritism is identified by using a family background variable as an instrument for education, a dummy variable indicating whether the father of the client was a farmer.[2]

The instrument variable estimation procedure used is captured by equations (1) and (2). The education of individual i in credit group g is regressed on the instrument Z_{ig}. Predicted education values from this estimation are then used in the in-group favouritism equation. Both equations contain a vector of individual-level covariates X_{ig} (details are discussed below and in the next section). As the level of in-group favouritism likely depends on credit group composition and dynamics, we include dummies for credit group affiliation, represented by the group specific intercepts δ_{1g} and δ_{2g}. We cluster standard errors at the credit group level (of which there are 51 in our sample).

$$education_{ig} = \beta_1 Z_{ig} + \gamma_1 X_{ig} + \delta_{1g} + v_{ig} \quad (1)$$

$$ingroupfavouritism_{ig} = \beta_2 education(predicted)_{ig} + \gamma_2 X_{ig} + \delta_{2g} + \varepsilon_{ig} \quad (2)$$

[2] While there are no previous studies of in-group favouritism instrumenting for education, similar instruments have been employed in studies of effects of education on business profitability (Fafchamps and Minten 2002; Kolstad and Wiig 2011).

Having a father who was a farmer likely increases opportunity costs of going to school, with an expected negative relation to education. The instrument is highly correlated with education, passing the standard test of instrument strength. To be valid as an instrument, the father's status as a farmer also has to be unrelated to any unobserved element of the in-group favouritism equation. As the credit group dummies capture all relevant variation between groups, questions about instrument validity have to focus on the correlation with unobserved elements at the individual level. One concern could be that growing up in an agricultural environment exposes individuals to specific forms of distributive norms related to in-group favouritism, either as a result of industry-specific bargaining processes, or due to a correlation of agricultural activity with other cultural or background characteristics such as ethnicity. We address these concerns in a series of robustness tests, adding dummies for province of origin of the client and occupational status of the mother as covariates to capture norms due to a rural background, and adding a range of family and cultural background variables (including ethnicity, language and religion) as covariates to capture any other forms of normative influence.

One potential source of bias remains which our data do not permit us to address. Since people choose whether or not to become clients of the Kixicredito system, the actual clients need not be representative of the population of potential microcredit clients. If becoming a client is affected by some unobserved variable correlated with unobserved elements of the in-group favouritism equation, our estimates may be biased. This is a problem that is difficult to address, as the last census in Angola was conducted in 1970 and other population data is absent. Drawing a representative sample of microcredit clients and non-clients from a larger population is therefore not feasible, which precludes the use of a Heckman selection model to address potential selection bias.

3 Data

A total of 539 Kixicredito clients participated in the experiment. Missing values for some covariates and the instrument reduces the sample to 523 observations in the main estimations. The variables used for the main estimations are summarized in Table 1. Our dependent variable is the difference between the amount given to a fellow credit group member and to an outsider in the dictator game. An alternative dependent variable would be the ratio of the in-group and the out-group amount, we show in the robustness section that this does not qualitatively change results. Education is measured as years of education, constructed from responses to a survey question of highest class attended. Initial estimations included both age and gender as covariates, but as only the latter proved significant we dropped age from our main specification. Other covariates include two family background variables, the number of languages spoken by the father of the client, and a dummy variable reflecting access to newspapers in the home of the client during childhood. In addition, a dummy for whether a client knows the manager of a local NGO is included as a measure of client social networks.

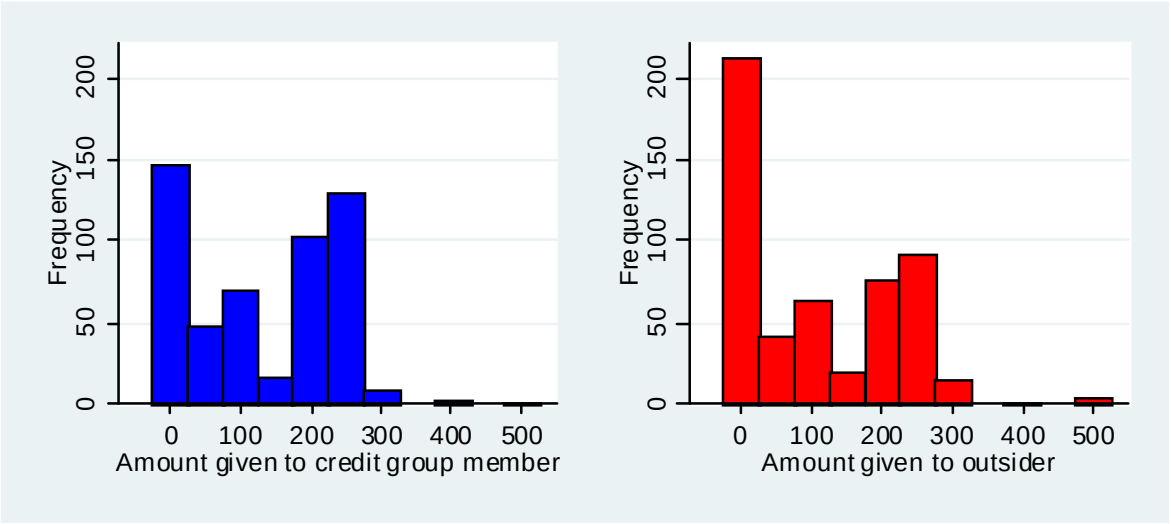
Table 1. Table of main variables

Variable	Explanation
Dependent variable	
In-group favouritism	Amount given to credit group member in dictator game minus amount given to non-group member
Independent variables	
Education	Years of education
Male	Dummy = 1 if gender of respondent is male
Father languages	Number of languages spoken by father
Newspaper	Dummy = 1 if newspapers were accessible at home during childhood
Network NGO	Dummy = 1 if respondent knows the manager of an NGO
Instrument	
Father peasant	Dummy = 1 if father's main occupation was peasant

Source: Based on authors' survey data (see text).

Figure 1 provides a graphical representation of the choices made in the two versions of the dictator game. The blue histogram in the left hand panel shows the amounts given to fellow credit group members on the horizontal axis, and the number of respondents choosing each amount on the vertical axis. Consistent with patterns from other dictator game experiments, the modal choice is to give nothing (chosen by 28 per cent of subjects), while the second most common choice is to give half the endowment (i.e. 250 kwanza, chosen by 25 per cent of subjects). The red histogram in the right hand panel shows choices in the second version of the game, where the recipient is someone outside the credit group of the dictator. Again, the modal choice is to give nothing. However, giving nothing is chosen by a larger number of dictators (41 per cent) than in the version where the recipient is a fellow credit group member. Though patterns of choices are fairly similar across the two versions of the game, this also means that most positive amounts are given less frequently when the recipient is an outsider. For instance, only 17 per cent of subjects transfer half the endowment in the second version of the game.

Figure 1. Histograms of amounts given in dictator game



Source: Based on authors' survey data (see text).

This is also reflected in the summary statistics for the main sample, presented in Table 2. The average client gives 23.5 kwanza more to an in-group recipient than to an outsider. This reflects that the average amount given to a fellow group member is 131 kwanza (about 26 per cent of the endowment), while the average amount given to an outsider is 107.5 kwanza (21.5 per cent of the endowment). The amount given to a fellow group member is significantly higher than the amount given to an outsider ($p<0.001$), consistent with in-group favouritism. The median client gives the same amount (100 kwanza) to a fellow group member and an outsider, however. As indicated by Figure 1 and Table 2, there is considerable variation in amounts given and in their difference.

Table 2. Summary statistics, main sample

Variable	Obs	Median	Mean	Std. dev.	Min	Max
In-group favouritism	523	0	23.52	120.63	-500.00	500.00
Education	523	7	6.88	3.87	0.00	17.00
Male	523	0	0.37	0.48	0.00	1.00
Father languages	523	2	2.34	0.87	1.00	6.00
Newspaper	523	0	0.40	0.49	0.00	1.00
Network NGO	523	0	0.15	0.36	0.00	1.00
Father peasant	523	0	0.32	0.47	0.00	1.00

Note: In-group favouritism is amount (in Angolan kwanza) given to credit group member in dictator game minus amount given to non-group member. Education is in years, father languages the number of languages spoken by respondent's father. Male, newspaper, network NGO, and father peasant are dummy variables.

Source: Based on authors' survey data (see text).

The average participant in the experiment has almost seven years of education, which is about the same as the median. 37 per cent of the sample is male, which corresponds well to the a general proportion of male clients in the Kixicredito institution of about one-third. Average outstanding loansize in the sample is about US$1000 (not shown in table), which also corresponds to the average loansize in the institution. On the dimensions where a comparison can be made, the sample therefore seems fairly representative of the population of clients from which it was drawn. The average participant's father spoke about 2 languages, 40 per cent had access to newspapers at home during childhood, 15 per cent know the manager of a local NGO, and 32 per cent had a father who was a farmer.

4 Results

4.1 Main results

Results from the two stages of our main IV estimation are presented in the first two columns of Table 3 (IV regression 1), and column three presents results from the corresponding OLS regression for purposes of comparison. Results from the first stage of the IV regression shows that the dummy variable for whether the father of a client was a farmer has a highly significant and (as expected) negative relation to education. A test of whether the instrument should be in the education equation yields an F statistic of

31.78, well above the conventionally required level of 10 (cf. Staiger and Stock 1997). Results for the other covariates in the first stage are as one would expect. Male clients have more years of education, as do clients from a family background where the father spoke more languages and where newspapers were available. There is also a positive association between the social network of a client and education.

Table 3. Main results

	IV-regression 1		**OLS regression**
	First stage	Second stage	
Dependent variable	*Education*	*In-group favouritism*	*In-group favouritism*
Education		19.066**	-0.768
		(7.75)	(2.10)
Male	2.557***	-54.903**	-3.371
	(0.30)	(23.43)	(16.15)
Father languages	0.546***	-19.577*	-5.057
	(0.17)	(10.24)	(7.45)
Newspaper	1.081***	-36.003**	-10.672
	(0.29)	(15.67)	(10.94)
Network NGO	0.928**	-53.081**	-33.537*
	(0.38)	(20.61)	(19.76)
Father peasant	-1.729***		
	(0.31)		
Constant	6.623***	73.018***	111.586***
	(1.56)	(17.46)	(27.41)
Group dummies	Yes	Yes	Yes
N	523	523	523

Cluster robust standard errors in parentheses. *** indicates significance at the 1% level, ** at 5%, * at 10%.

Source: Based on authors' survey data (see text).

The main result at the top of column two is that education has a positive effect on in-group favouritism. The effect is both statistically and economically significant, indicating that an added year of education raises the difference between the amounts given to group members and outsiders by 19 kwanza. Does this mean that more educated clients increase in-group allocations, or that they reduce allocations to outsiders? Additional regressions using the amounts given in the two versions of the game as dependent variables suggest that it may be a bit of both (results reported in Appendix 2). Point estimates indicate that an added year of education increases the amount given to a group member by about 10 kwanza, while reducing the amount given to an outsider by about 9 kwanza. However, neither of these estimates are significant, making it hard to draw firm conclusions. In any case, the result that education increases in-group favouritism runs counter to the intuition that education broadens the minds of individuals, instead they appear to become more parochial. The result from the IV regression taking the endogeneity of education into account is also significantly different from that of the OLS regression in the third column, which indicates a negative but insignificant effect of education on in-group favouritism. The downward bias of the OLS estimate suggests that any unobserved variable in the in-group favouritism equation has a correlation to education of the opposite sign to its correlation with in-group favouritism. In other words, if time preferences are the unobserved variable, and impatience reduces education, impatience increases in-group favouritism.

As for the covariates, male clients exhibit substantially less in-group favouritism than female clients. There can be several explanations for this, one is that men and women have different normative inclinations to favour their immediate social group, another could be that women in our context are more vulnerable and hence see it as in their interest to forge closer ties with their fellow group members. Having a father who spoke many languages and access to newspaper in the home during childhood appear to be negatively associated with in-group favouritism, in other words it seems that these are factors related to more of a cosmopolitan outlook. Finally, clients whose social network includes the manager of a local NGO also appear to be less parochial by giving more to outsiders while not giving less to group members. However, the latter results do not establish causation, it is for instance possible that individuals less inclined to favour their own group members are also more likely to establish contact with the manager of a local NGO. Finally, the group dummies are generally significant suggesting that group composition and dynamics are important for the development of in-group favouritism.

Other covariates which proved insignificant and are hence not included in the main specification reported in Table 3 include age, marital status, the number of years a subject has been a client of Kixicredito, the degree of interaction with and geographical distance to other credit group members, membership in various organizations (political, professional and more), and other types of social networks. Since our experiment was conducted with adult subjects, the fact that we do not find an effect of age on in-group favouritism does not run counter to the findings of Fehr et al. (2011) using children as subjects. It may simply mean that favouritism develops at an early age, but does not change significantly later in life. However, our results point to the need to disentangle effects of age and of education in studies of in-group favouritism in children. The absence of an effect of the number of years subjects have been clients of Kixicredito suggests that any socialization is less a matter of the length of inclusion in a credit group, than the characteristics and dynamics of the group as captured by the group dummies.

4.2 Robustness

The main result on the effect of education is robust to the addition of a number of covariates. The first three columns of Table 4 show results when adding dummies for ethnic group (IV regression 2), language (IV regression 3), and religion (IV regression 4). Only the second stage of the IV regressions are shown, as results from the first stage are not very different from those presented earlier. As indicated by the results, adding these covariates does not change results, the point estimate changes little and the effect of education is significant and positive. The main result is thus not sensitive to the inclusion of variables reflecting cultural differences such as ethnicity, language or religion.

To address the possibility that growing up in a rural environment is associated with the development of particular norms or preferences, the fourth column (IV regression 5) adds a dummy variable for whether the mother of a client had farming as her main occupation, as well as a set of dummies for the province of origin of clients. The farming status of the mother is not significant in the in-group favouritism equation, and while the province dummies are jointly significant ($p<0.001$), the estimate for the education effect is increased and remains significant. In the final column of Table 4 (IV regression 6) we also add a number of other family background variables to capture

potential socialization at the family level beyond the variables included in the main specification. Neither the level of education of the father, mother or elder sibling of the client has a significant association with in-group favouritism, nor do we find a significant relation for the number of languages spoken by the mother. The main result on the effect of education remains unchanged.

Table 4. Robustness tests

	IV-regression 2	**IV-regression 3**	**IV-regression 4**	**IV-regression 5**	**IV-regression 6**
	Second stage	Second stage	Second stage	Second stage	Second stage
Dependent variable	*In-group favouritism*	*In-group favouritism*	*In-group favouritism*	*In-group favouritism*	*In-group favouritism*
Education	17.954**	19.768**	20.374***	25.939*	25.582*
	(7.76)	(8.13)	(7.57)	(13.65)	(13.20)
Male	-53.773**	-57.957**	-57.827**	-74.585**	-72.615**
	(22.92)	(24.41)	(22.76)	(36.74)	(35.49)
Father languages	-17.286*	-19.424*	-20.471*	-18.160	-15.682
	(10.06)	(10.30)	(10.31)	(12.15)	(12.14)
Newspaper	-33.575**	-35.102**	-37.057**	-40.494**	-39.913**
	(14.93)	(15.88)	(16.54)	(19.56)	(19.41)
Network NGO	-52.821**	-60.039***	-55.095**	-61.799**	-58.910**
	(20.37)	(20.87)	(21.07)	(23.39)	(22.47)
Mother peasant				2.514	4.325
				(20.09)	(19.19)
Father education					0.349
					(2.11)
Mother education					-0.919
					(2.53)
Mother languages					0.691
					(5.94)
Sibling education					-1.025
					(1.41)
Constant	168.422***	-75.436**	-90.848	235.542***	14.775
	(39.71)	(34.56)	(58.42)	(60.38)	(37.18)
Group dummies	Yes	Yes	Yes	Yes	Yes
Ethnic dummies	Yes	No	No	No	No
Language dummies	No	Yes	No	No	No
Religion dummies	No	No	Yes	No	No
Province dummies	No	No	No	Yes	Yes
N	522	523	522	518	510

Cluster robust standard errors in parentheses. *** indicates significance at the 1% level, ** at 5%, * at 10%.

Source: Based on authors' survey data (see text).

As our dependent variable, we have used the difference between amounts given to a fellow group member and an outsider. An alternative approach is to measure in-group favouritism as the ratio of the amounts given. In Table 5 we report results for this alternative dependent variable, which we call relative in-group favouritism. Column one reports the second stage of the IV regression, and column two the corresponding OLS estimates. The results show that changing the way the dependent variable is specified does not change results much. Education has a positive and significant effect on relative in-group favouritism in the IV regression (but not the OLS regression). Male clients favour their fellow group members less than female clients, and the other variables have the same signs as in previous regressions, the only difference is the lack of significance of the number of languages spoken by the father. Our results are thus largely robust to the re-specification of the dependent variable.

Table 5. Regressions using relative in-group favouritism as dependent variable

	IV-regression	**OLS regression**
	Second stage	
Dependent variable	*Relative in-group favouritism*	*Relative in-group favouritism*
Education	13.347**	-0.425
	(6.57)	(1.51)
Male	-29.710*	6.072
	(17.62)	(10.13)
Father languages	-13.243	-3.161
	(8.05)	(5.36)
Newspaper	-22.324*	-4.734
	(12.09)	(7.21)
Network NGO	-32.092**	-18.521*
	(12.88)	(10.03)
Constant	50.066***	101.453***
	(11.22)	(18.50)
Group dummies	Yes	Yes
N	523	523

Cluster robust standard errors in parentheses. *** indicates significance at the 1% level, ** at 5%, * at 10%.

Source: Based on authors' survey data (see text).

5 Discussion and conclusion

This paper presents evidence on in-group favouritism among microcredit clients in Luanda, Angola. Our results indicate that while there is an in-group bias among microcredit clients, there is also a great deal of variation in the extent to which clients favour members of their own group. Controlling for group differences, we find that parochial preferences or susceptibility to norms of in-group favouritism are related to a number of individual characteristics. In particular, using an instrument variable approach, we identify a positive causal effect of education on in-group favouritism. This result is striking in view of conventional arguments that education broadens the perspectives of individuals, making them less parochial and more universalistic. We find the opposite to be the case.

One possible explanation for our somewhat paradoxical result may be that there is heterogeneity in effects of education on in-group favouritism. Rather than average treatment effects, our results may then reflect local average treatment effects for the groups whose education are affected by our instrument for education. Further results from first stage estimations suggest inter alia that the effect of our instrument on education is stronger for women than for men, and weaker for clients from an ethnic minority background. If effects of education are heterogeneous, our estimate may thus reflect positive effects of schooling on in-group favouritism for female and non-minority participants. One possible conjecture which should be followed up in further studies is that these groups may be more likely to be made susceptible to in-group norms through schooling compared to male or minority participants.

There are of course also other ways in which to interpret our main result. One possibility could be that more educated subjects better understand that it may be in their

interest to favour fellow group members, since there is joint liability for loans and/or since giving low amounts may breed negative group sentiments making future interaction more difficult. Since the two versions of the game were played sequentially, another possibility is that more educated subjects learn how to play the game more quickly, thus reducing the amount given in the second version more sharply. While this may explain the apparently lower amounts given by educated subjects in the out-group version of the game, it does not by itself explain the possibly higher amounts given in the in-group version.

The single-blind, sequential design also present the possibility that our results reflect experimenter demand effects, where educated subjects are more adept at picking up and adapting their behaviour to cues about the objective of the experiment. While care was taken to avoid giving cues as to the objective, it has been argued that the dictator game is inherently susceptible to these kinds of effects (Zizzo 2010). Educated subjects may in principle infer from the difference between the two versions of the game that the experiment is about in-group favouritism, and so may choose to reduce their given amount more from the first version to the second. It is, however, not obvious that this is the most likely inference subject could make, it is equally plausible that the objective of the experiment is perceived as being about impartiality. Since the experiment was conducted by enumerators not previously known to the subjects, and their credit officer was not present during interviews, there is little reason to believe that it would be easy for subjects to discern the objective of the experiment. Moreover, as the survey of which the experiment was a part provides a form of non-desceptive obfuscation by focusing on completely different issues, experimenter demand effects may not be too much of an issue in our case. The results also hold when controlling for computational skills, which can be seen as a proxy for the ability of subjects to make logical inferences of this kind.

Our results contribute to the experimental economics literature on in-group favouritism, which has only to a limited extent looked at effects of individual characteristics such as education. Moreover, the results provide a start to addressing the important policy question of whether and how other-regarding preferences are ‘susceptible to policy interventions in education – a question that is still open to thorough investigation’ (Fehr et al. 2011: 3). Our findings suggest that education has a less straightforward and possibly less benign effect than conventionally believed, as the length of exposure to an education system may reinforce biases in other-regarding preferences. This may of course have as much to do with the nature and quality of an education system as with the degree of exposure, a question our data does not permit us to address, but which deserved further scrutiny in future studies.

An important and related question is whether our results are particular to an Angolan context, or generalize to other countries. The uncovered effect of education on in-group favouritism may reflect the way in which the Angolan education system works rather than schooling more generally. A number of studies in sociology have analyzed how an education system may serve to reproduce rather than reduce social inequality (e.g. Bourdieu and Passeron 1977). The high degree of economic inequality and social stratification in Angola may have resulted in an education system which highlights or triggers in-group inclinations in those exposed to it. However, the sociology of education in Angola does not appear to have been the subject of scientific inquiry. Understanding the mechanisms through which education affects other-regarding preferences and in-group biases is important from a policy perspective. As our data does not easily permit analysis of these issues, this is a matter for further research.

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Appendix 1. Experimental material

Procedures:

The experiment was conducted as part of a larger survey during a six week period from February to March 2010. The first three sections of the survey posed questions on client background (gender, marital status, age, and so on), the scale and profitability of the subjects' enterprises, and on education. The experiment was conducted at the end of the section on education, and an English translation of the oral instructions is presented below. After the experiment, the survey included a final section on social capital. Throughout the survey, anonymity was attained by bringing subjects out of hearing range from each other. The full survey took on average 30 minutes. At the end of the survey, a supervisor paid subjects discreetly in cash. The amount given to a fellow group member was placed in an unmarked envelope, which was then sealed. The amount given to an out-group recipient was placed in an envelope marked with an X, which was also sealed. When all interviews in a group had been completed, each set of envelopes was shuffled. Each subject was then handed one unmarked envelope from the current group and one marked envelope from the previous group interviewed.

Experimental instructions:

C. 10 It's very important that you say to the respondent the following: "As not everyone participating in this survey will be asked the following questions, please do not discuss them with anyone afterwards."

Then ask:

At the end of this survey, we will give you a sum of 500 kwanza. You may choose to keep this money, or give some or all of it to another member of your solidarity group. The person the money is given to will be randomly picked by us. You will not know who this other member is, and he/she will not know who you are. How much of the 500 kwanza do you want to give to the other member of your group?

Answer: _____ kwanza

We will also give you an additional 500 kwanza. You may choose to keep this money, or give some or all of it to a person outside your solidarity group. The person the money is given to will be randomly picked by us. You will not know who this person is, and he/she will not know who you are. How much of the 500 kwanza do you want to give to the person outside your group?

Answer: _____ kwanza

Appendix 2: Additional results

Table A.1: Regressions using amount given in each version of game as dependent variables

	IV-regression A1	**IV-regression A2**
	Second stage	Second stage
Dependent variable	*Amount given to group member*	*Amount given to outsider*
Education	9.926	-9.139
	(6.51)	(6.95)
Male	-20.667	34.236
	(19.33)	(21.83)
Father languages	2.956	22.533**
	(6.71)	(9.00)
Newspaper	-26.251**	9.752
	(12.32)	(13.83)
Network NGO	-2.405	50.677***
	(16.28)	(15.22)
Constant	144.299***	71.281***
	(10.95)	(14.94)
Group dummies	Yes	Yes
N	523	523

Instrument for education is a dummy variable for whether father's main occupation was peasant. Cluster robust standard errors in parentheses. *** indicates significance at the 1% level, ** at 5%, * at 10%.

Source: Based on authors' survey data (see text).