Essays on Development and Behavior Economics: An Impact Evaluation of the "Bolsa Família" - Conditional Cash Transfer on Education and the Effect of Leadership Identity on Group Cooperation and Elite Capture

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Introduction

The dissertation at hand is divided into three chapters. Chapter 1 presents an impact evaluation of a widely spread conditional cash transfer in Brazil called "Bolsa Família". We analyze the impacts of the program on two educational outcomes: enrollment and attendance. In our analysis we consider the heterogeneous effects of the program. Chapter 2 and 3 study the influence of leaders' identity on the group dynamics. Chapter 2 focuses on the effect of leaders' identity on cooperation while Chapter 3 studies the dynamics of embezzlement or what we call elite capture.

Chapter 1 conducts an impact evaluation of Brazil's Conditional Cash Transfer Program "Bolsa Família". The program provides grants to households below the poverty line conditioned to school attendance and medical care. Reaching more than 13 million families, this is one of the biggest programs of its kind in the world. Yet research on its impact is limited. To deal with the non-experimental nature of the program we use Propensity Score Matching (PSM). The use of an ample Brazilian household survey - Pesquisa Nacional de Amostra de Domicílio (PNAD) - makes it possible to match beneficiaries and non beneficiaries on a large set of observable characteristics. In this way we are able to present convincing causal inference. Moreover, thanks to the richness of the data set, we can study the impacts of the Program by age, gender and regions. Our results point to a significant increase in Enrollment rates and Attendance among recipients. Furthermore, when different groups and regions are compared, we find that recipients in less developed regions benefited more from program participation than groups in less developed regions, indicating that the program was able to close the gap in education between more and less developed areas.

Chapter 2 studies the effect of leader's identity on cooperation. Due to the increasing demands of more competitive markets traditional producer cooperative leaders often need to be replaced by managers with specialized skills. While managers can bring technical expertise that leads to

better economic outcomes, the loss in representativeness of members' interest might risk the sustainability of the organization.

Using a laboratory experiment, we test the impact of leader's skills and identity on groups' cooperation. Our design uses a three stage procedure in groups formed by 4 subjects. In the first stage we generate identity in the lab allowing participants to interact for 10 minutes while solving a joint task and competing with other groups. The second stage is instrumental and is designed to measure participant's ability in a real effort task. The last stage consists on a modified public good game in which participants have to decide which proportion of their endowment they allocate to the group account and the individual account. Investments in the individual account are multiplied by 1 while investments in the group account are multiplied by a factor $M<1$ that is an increasing function of the leader's outcome of a real-effort game. The more the Leader works, the higher the multiplier, M. In the experiment we use a 2X2 design that combines two levels of skills of the leaders and shared or non-shared identity of the leader and the group members. Our hypothesis is that leaders who share the same identity as the group members are more motivated and work harder in the real effort task than out-side leaders. We expect that group members will anticipate it and contribute less when led by an out-side leader than by an inside leader. Leaders with higher skills are expected to perform better in the real effort task and hence attract more cooperation. However it is not clear whether the gains of higher skills are negatively affected by the lack of identity of the out-side leader.

Our findings show that the lack of identity decreases leaders' productivity, while members successfully anticipate it and cooperate significantly less. When leaders' skills and social identity are directly compared, we find that the gains in cooperation levels due to leaders' skills, is just enough to make up for the loss in identity. We do find, however, that out-group skilled leaders were able to sustain cooperation over time, while groups led by random in-group presented a continuous cooperation decrease over time.

Chapter 3 analyses a negative aspect of social identity. Small communities in developing countries (such as tribes, smallholders' association and cooperatives for example), who share language, ethnicity and culture, are often inclined to suffer from an abusive behavior from their local leaders. This abusive behavior often occurs during long periods of time becoming even close to be an accepted political norm. In Chapter 3 we question if the high social identity condition in the community can be the cause of abusive behavior being hence the trigger to a perpetuation mechanism of power abuse in the community. We investigate two channels of how such a perpetuation mechanism can occur: trust and willingness to monitor. We hypothesize that groups who share the same identity with the leader trust him more, and would hence be less willing to pay for a monitoring mechanism. As trust decreases scrutiny, leaders would feel free to increase embezzlement.

To do that, we present a laboratory experiment where groups formed by three subjects playing a three-stages game. In stage one subjects solved a group task similar to the group task presented in Chapter 2. In stage two, one subject was randomly assigned as group leader and other two subjects as group members. To allow members' payoff to be dependent on leaders' performance, we implemented the effort task followed by a trust game. The effort task was to sum 5 two digit numbers during 30 seconds. A minimal performance of 1 correct sum by the leader generated a multiplier A which would take a value between 0.8 and 1.9. The non-achievement of the minimal performance would set A=0.8. Members received 20 points as endowment, which they could decide to keep, where it would be multiplied by 1, or to pass it to the leader, where it would be multiplied by A. Leaders received a fixed amount of 20 points plus the amount of points leaders decided to take from each group member. In stage three subjects played a lottery game. We use a 2X2 design combining in- and out-group treatments with monitoring and without monitoring possibility.

Our findings show that members led by in-group leaders were less willing to monitor while this lack of scrutiny significantly increased amount captured. We did not however find that trust by itself is significantly higher in homogenous groups, which questions the identity as a cause of perpetuation mechanism.

Chapter 1

Conditional Cash Transfers in Brazil: Treatment Evaluation of the "Bolsa Família" Program on Education

Conditional Cash Transfers in Brazil: Treatment Evaluation of the "Bolsa Família" Program on Education

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Abstract

Brazil's "Bolsa Família" conditional cash transfer program (BFP) serves as a substantial poverty alleviation program in Brazil. It is the biggest program of its kind in the world, reaching more than 13,000,000 families. The BFP awards grants to eligible poor families, allowing increased consumption in the short-term, while building human capital in the long-term through requirements for school attendance and health care. In this paper, we evaluate the effect of this program on educational outcomes. In particular we study heterogeneous treatment effects over age, region, gender and setting (rural/urban). Using Propensity Score Matching (PSM), we find that the probability of school enrollment rises by around 4 percentage points for recipient children. We find that this enrollment effect is higher for children living in less developed regions. In North/Northeast and in rural areas enrollment rates increase up to 5 and 7% respectively for beneficiaries of the program. Our results also point to a positive impact on school attendance with recipients missing 0.3 school days less in the last two months than non-recipient.

Keywords: conditional cash transfers, propensity score matching, education

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1.1 Introduction

Since 2001 Brazil has undertaken a series of policies to decrease inequality and improve human development indicators. One of these policies is the "Bolsa Família Program" (BFP, previous to 2004 known as "Bolsa Escola"), a Conditional Cash Transfer (CCT) program which has benefited about 13,000,000 families to date. The objectives of the program are two-fold. First, in the short-run, by directly transferring cash to recipient families, the program attempts to increase income, boost consumption and alleviate poverty. Second, in the long-run, through conditionalities attached to education and health, it attempts to increase human capital and consequently break the inter-generational cycle of poverty. Despite being the biggest program of its kind in the world, few studies have analyzed its impact.

The objective of this paper is to evaluate the impact of BFP on two educational outcomes: enrollment and attendance. Our study uses country-wide micro data which, by providing a rich set of variables, allows us to construct a high-quality counterfactual group using Propensity Score Matching (PSM). We are also able to provide an overall impact assessment analyzing heterogeneous treatment effects across age, gender, locality and region (rural, urban).

Cash transfer programs have been exhaustively analyzed in Central and Latin America. For instance, it has been shown that PROGRESA, a social program in Mexico rolled out in a randomized fashion, has had positive effects on education through increasing enrollment and test scores while decreasing dropout and repetition rates (Behrman et al., 2005 and 2000; Schultz, 2004). It has also been shown to serve as a safety net for education among the poor (de Janvry, 2006b). Other CCTs programs in Latin America had positive effects in education. In Nicaragua school enrollment increased by 13 percent (Maluccio and Flores, 2005) in addition to language and personal behavioral skills improving (Macours et al., 2008). In Honduras, school enrollment rose by 3 percent (Glewwe and Olinto, 2004), in Ecuador by 10 percent for recipients in the first income quintile (Schady and Araujo, 2008; Oosterbeek et al., 2008) but had no effect on test

scores. Ponce and Bedi (2008) and Levy and Ohls (2010) find that, for recipient children, there is an increase of 0.5 days per month in attendance.

Fewer empirical studies consider the Brazilian CCT. Barros et al. (2006) estimated a program response of up to 20 – 25 percent reduction in inequality, and a 16 percent decrease in extreme poverty between 2001 and 2005. By using a micro-econometric simulation Bourguignon et al. (2003) estimate that 60% of poor children aged between 10 and 15 enroll in school due to program participation. Using PSM, Cardoso and Souza (2004) analyze the "Bolsa Escola" program utilizing the 2000 census data. They find that children beneficiaries of the program were 3 to 4 percentage points more likely to go to school than a comparable group of non-beneficiaries. De Janvry et al. (2006a) analyze the impact of "Bolsa Escola", finding an overall reduction of 8 percentage points in the dropout rate of recipients in northeastern States. Nevertheless, the decreased dropout followed by an increase of 0.8% in the failure rate. This effect is explained due the targeting characteristics of the program, which is able to maintain drop-out threatened children in school but who also present lower motivation and skills to finish the school year. Similarly, using school level data, Glewwe and Kassouf (2008) find a reduction of 8 percentage points in the dropout rate in primary schools. Oliveira et al. (2007) covers a country-wide effect at the individual level and confirms findings from previous studies on the positive effects of BFP on school attendance, as well as a lower proportion of children dropping out. However, they find a negative effect of BFP on successful completion of the school year.

One drawback of these studies is that the analysis on heterogeneous effects is limited. We believe that knowing how programs affects different groups is essential for policy makers in particular for targeting strategies. For instance, is the grant by itself enough to incentivize childrens' enrollment in areas where access to school is restricted (e.g. rural areas)? Is the grant able to bring boys at working age back to school? Furthermore, we are interested in the regional disparities in Brazil. The country presents high heterogeneity across regions with regard to

poverty and development indicators. This disparity is in line with the development indicators. While the Human Development Index (HDI) of the whole country was 0.803 in 2006, the HDIs of the North and Northeast were 0.733 and 0.772, respectively. The South, Southeast and Midwest presented significantly higher development indicators, at 0.837, 0.835 and 0.824, respectively (BCB, 2009). Based on this heterogeneity, is "Bolsa Família" able to overcome regional inequalities?

To the best of our knowledge, this is the first study that focuses on the heterogeneous impacts of the BFP across locations, regions and age groups. We confirm previous findings regarding the positive impact on educational output. The probability of enrollment rises by around 4 percentage points for recipient children aged between 6 and 17 in 2006. On school attendance, beneficiary children miss around 0.3 less days of school (during the preceding two months) when compared to non-beneficiaries. Additionally, we find that the BFP's impact is higher in poorer locations (North and Northeast) and rural areas. Despite the positive impacts of the program, we find that the impact of the program on the enrollment and attendance outcomes slightly fell by 1 percent between the years of 2004 and 2006.

1.1.1 The Bolsa Família Program

The BFP gives monthly grants to each child of poor families, as well as a basic grant to families living in extreme poverty (currently with per capita income (PCI) less than $70 BRL ($ ~31 USD)). There are three kinds of benefits which are distributed based on the level of poverty, number of children and the age of the children. The "basic benefit" assigns $70 BRL a month to families in extreme poverty. The "variable benefit" grants poor families $32 BRL (~$14 USD) for each child aged between 0 and 15 and the "teenager benefit" grants children between 16 and 17 years old who attend school $38 BRL (~$ 17 USD).

Once in the program, the family receives a grant for all its children. The participation is conditional on at least 85% school attendance for children between the ages of 6 to 15. For

children of ages 16 and 17, the attendance obligation for recipients falls to 75%. Regarding medical care, parents are obligated to allow children to get required vaccines until they turn 6, as well as prenatal medical assistance and breast feeding mothers.

The cash transfer is granted for the household, and not at the individual level. Therefore, once in a beneficiary household, all children are bounded to the school attendance conditionality. However, a teenager who has already turned 16 has the right to abdicate his grant and therefore does not need to meet the conditionality if he decides to work instead of study.

Despite the existence of the proxy means test which targets eligible families with per capita income below R$ 140, some evidence point that it was not tightly administered during the analyzed period (Handa and Davis, 2006). Changes in the state of poverty were not checked which made exclusion of ineligibles rare; income is self-reported and poorly checked while income from informal work cannot be traced. In 2004, three different programs called "Bolsa Escola", "Bolsa Alimentação" and "Auxílio Gás" were integrated and called "Bolsa Família", while recipients did not passed through eligibility checks. Moreover, its institutional framework is decentralized at municipality system, permitting regional disparities in targeting and monitoring.

1.2 Impact Evaluation: Concepts and method

When analyzing policy measures with non-experimental studies, we would ideally look at how a non-treated individual would perform if the same individual would have received treatment. However, there is no possibility of observing this effect for the same person, i.e., once with treatment and once without. This problem can be solved by searching a comparable counterfactual group, whose only difference is the participation in the program.

In cases such as the "Bolsa Família", where the proxy means test were not tightly administered and take-up is less than 100%, the probability increases of having eligible's which should be

treated, but aren't, while non-eligible's which shouldn't be treated, are in fact receiving the benefit. In these cases where contamination between treated and non-treated is eminent, Propensity Score Matching is a particularly appropriate methodology, due to a higher likelihood of finding comparable households with same income and households characteristics, but differ about treatment status.[2]

PSM creates the counterfactual group based on a probability of receiving treatment, this probability being determined by a set of characteristics, the so called propensity score (Rosenbaum and Rubin, 1983). The estimated coefficients are then used to predict the individual probability (propensity) of being beneficiary of the program. In the second stage treated individuals will be matched to non-treated individuals that present the same or similar propensity score. Hence PSM is able to replicate an experimental data set by finding a non-treated observation that presents the same propensity score and is hence comparable to a treated one. Once a non-treated individual fulfills the same characteristics as the treated one, treatment effects are measured by differences in means in the outcome between both groups.

The validity of the method is conditional on two assumptions. The Conditional Independence Assumption (CIA) or balancing property certifies that the vector of explained variables X fully determines the participation rule regarding treated and non-treated outcome, hence being able to overcome treatment selection bias. Thus, after controlling for Xs, we can say that the outcome of individuals without treatment is independent of their participation status. This condition guarantees that treated and non-treated individuals are comparable to each other, assuring that the CIA holds. The second condition is called Overlap (or common support) condition and certifies that for each characteristic X, there are individuals from the treated and non-treated group, indicating an overlapping trend between both hence allowing their direct comparison.

[2] We do, however, also consider using Regression Discontinuity Design (RDD) but did not find any discontinuity around the income threshold of the means test, turning RDD unsuitable.

Once the propensity score is calculated, matching is performed based on fitting weights assigned to the neighborhood of the probability of being treated. After the weights are applied on the control group, the matching estimator is calculated on the difference between treated and non-treated. Conditional on the assumptions above, the matching estimator is then:

$\hat{\alpha}^{M} = \sum_{i\epsilon T}\{y_i - \sum_{j\epsilon C} \widetilde{w}_{ij} y_i\} w_i$, where T and C stand for the treatment and control group, i for the treated individuals in the treatment group T, j for non-treated individuals in the control group C, $\widetilde{w}_{ij}$ for the weights on j for group i, while w_i stands for the reweighting that is needed to build the distribution for the beneficiary group.

1.2.1 Dataset

In the analysis we use data of the annual national household survey (Pesquisa Nacional de Amostra de Domicilios – PNAD). In 2004 the PNAD covered 399,354 individuals and 139,157 households while in 2006 the PNAD included 410,241 individuals and 145,547 households. Both covered all 27 Brazilian states. For our analysis, we focus on school children aged between 6 and 17.

1.3 Application and Findings

We implemented a probit model in order to calculate the probability of being a "Bolsa Família" recipient. Since our intention is not to estimate selection into treatment but instead use it as a simplifying tool for the multidimensionality problem we follow the recommendation of Rubin and Thomas (1996) and included all variables which, despite having a questionable level of significance and respective importance, are able to provide a better fit. The model was calculated with the psmatch2 command following Leuven and Sianesi (2003) and included 71 variables on

assets, child characteristics, household head characteristics, mothers' characteristics and household characteristics.[3]

Table 1.1: Balancing Results – Key Variables

		Mean				T-test	
Variable	Sample	Treated	Control	% bias	% reduct bias	t-value	p-value
Asset Index	Unmatched	-1.13	0.11	-92.1		-128.86	0.00
	Matched	-1.13	-1.22	6.4	93.1	7.24	0.00
Black	Unmatched	0.07	0.06	4.1		5.88	0.00
	Matched	0.07	0.07	-0.9	78.2	-1.06	0.29
Years of education of household head	Unmatched	4.63	7.87	-82.4		-111.53	0.00
	Matched	4.62	4.58	1.1	98.7	1.58	0.12
Number of household members	Unmatched	5.76	4.63	58.3		85.46	0.00
	Matched	5.76	5.74	0.9	98.5	0.95	0.34
Number of children	Unmatched	3.26	2.25	68.5		101.18	0.00
	Matched	3.26	3.28	-1.3	98.1	-1.36	0.17
Year of the oldest child	Unmatched	13.42	12.84	17.9		24.61	0.00
	Matched	13.42	13.39	0.9	94.7	1.19	0.23

Source: PNAD 2006 – pstest output

A sample on balancing test with some key variables is presented in Table 1.1 (see whole balancing test in Appendix 1.1, One to One matching). The test shows a t-test for the equality of the means of treated and non-treated, before and after matching, present the standardized bias, and the percentage of bias reduction. It is possible to observe that for most of the variables, the difference becomes not significantly different after matching. Furthermore, the percentage of bias reduction is high, indicating that for the broader majority, the balancing condition holds.[4]

Table 1.2 shows the result on the common support condition. Column 1 present the number of observations out of common support and Column 2 present observations within common

[3] Variables used: asset index (polychoric PCA (Olsson, 1979), included variables by request) race (four dummies for being indigenous, black, asian or mixed race/mulatto), age, birth registered, head age and head age squared, the interaction of head age and eligibility, head is literate, head is female, education years of the household head (linear and squared), and type of work executed by the head (In the agriculture, processing, construction, commerce, food, transport, public service, social service, and domestic industries, as well as other industry, services, work and undefined groups. mother alive, mother lives in the household, 26 state dummies, 8 rural/urban dummies, 2 area dummies (slums and indigenous villages), number of children, number of members in household, age of the oldest child. Even though we recognize its importance, we weren't able to introduce municipality dummies in the PSM, since the IBGE (Brasilian institute of geography and statistics) strictly prohibit the access to the id code at this level.

[4]Some variables are not balanced however. Instead, they even present an increase in estimation bias. For a robustness check, we run the estimations without variables that can over- or underestimate our results, such as the ones presenting a high percentage of bias increase (indian, female head, some area and state dummies, for example) and even the asset index, since after matching, the control group is significantly poorer than treated. We find no substantial differences in our estimates, however. The effect of the treatment varies ± 0.5% in response to these robustness checks, while the exclusion of the asset index slightly decrease treatment effect on 1%. We therefore decide to include all variables considered important, mainly because they represent a low percentage of the bias and are considered to be important for the fit.

support area by treatment for the year 2006. For the total sample analysis, we will work with 85,854 observations, with 55,903 untreated and 29,951 treated children aged between 6 and 17. The common support condition holds for all other sub samples. Regarding the concern expressed by Bryson et al. (2002) overparameterizing barely constrained the common support area. For results of common support by sub-samples, see Appendix 1.1.

Table 1.2: Common Support Results – Complete Dataset

	off Support	on Support	Total
Complete Sample (6 - 17 years old)			
Untreated	1,657	55,903	57,560
Treated	27	29,951	29,978
Total	1,684	85,854	87,538
Teenager Sample (10-15 years old)			
Untreated	1,704	26,815	28,519
Treated	14	16,024	16,038
Total	1,718	42,839	44,557
Children (6-9 years old)			
Untreated	979	17,885	18,864
Treated	29	9,960	9,989
Total	1,008	27,845	28,853

Source: PNAD 2006, own calculations (psmatch2 output)

We use four matching algorithms: One-to-one matching (OO), nearest neighbor (NN), caliper and Kernel.[5] One-to-one matching consigns a weight of 1 if the score of the treated individual is the most nearby standing neighbor to the non-treated individual, and zero otherwise. Radius matching was proposed by Dehejia and Wahba (2002), and is a modification of caliper matching where the counterfactual is in one particular range of the propensity score. The Radius matching consists of using the set of individuals in the control group within the caliper. Kernel matching calculates weights according to the distance in the propensity scores between treated and non-treated individuals, where the weight assigned to the treated individual tends to be closer to one as the Kernel function draws nearer to the matched individual, while it falls as the propensity score of the matched observation becomes more distant. The implementation of all the three methods provides us with robust treatment effects. For one-to-one matching, the counterfactual

[5] Results for Kernel analyzes were only reported for the sub-sample analysis. Local linear analyzes were also used but are not reported, since estimators rarely present big differences between matching methods overall.

group was constructed with replacement; for nearest neighbor matching, we use five nearest neighbors; we used a caliper of 0.01 for the Radius matching, and for Kernel matching and local linear matching, the bandwidth is 0.06.

1.4 Findings

1.4.1 Impacts between 2004 and 2006

We evaluate the educational impact of the program on two different variables: enrollment and attendance. We compare the effect of the program at two different points of time, 2004 and 2006.

Table 1.1 shows the enrollment rates by year and treatment, as well as the average treatment effect on the treated (ATT) on enrollment for the complete sample of children aged between 6 and 17. In 2004 and 2006, the percentage of children enrolled was 92.96% and 93.12%, respectively. The BFP had a positive impact on human capital investments; beneficiary children were 4.7% to 4.5% more likely to be enrolled in 2006. In 2004, the impact of the program on enrollment was even larger and children beneficiaries of the program were 5.6% to 5% more likely to be enrolled in the school. While the overall enrollment rates slightly improved between 2004 and 2006, the treatment effect of the program fell by half to one percent point depending on the matching indicator which might indicate that the effect of the cash transfer faded over time.

Table 1.3: Effects on Enrollment: 2004 and 2006

BFP effect on enrollment													
Matching method	2004 92.96 %					2006 93.12 %					2004/2006 -0.16%		
	treated	controls	diff	s.e.	t-stat	treated	controls	diff	s.e.	t-stat	diff		t-stat
Unmatched	94.33%	93.85%	0.48%	0	2.33	93.15%	93.46%	-0.31%	0	-1.74	1.18%		
OO	94.33%	88.73%	5.60%	0	14.28	93.15%	88.47%	4.68%	0	13.04	0.92%	**	1.73
NN	94.33%	89.26%	5.07%	0	17.54	93.15%	88.65%	4.51%	0	16.4	0.57%	*	1.42
Radius	94.33%	89.28%	5.06%	0	20.39	93.15%	88.50%	4.65%	0	19.4	0.41%		1.18

Source: PNAD (IBGE, 2004 and 2006), own calculation

Table 1.4 shows the results for school attendance two months previous to the survey, in 2004 and 2006. It is possible to observe that BFP has a positive and significant impact on attendance;

children missed 0.35 fewer days over the last two months or about 2.1 days less accumulated. When both years are compared, we find that school attendance slightly increased over time and children missed 0.41 days less in 2004 compared with 2006, while the effect of BFP on attendance stayed constant over the two years analyzed.

Table 1.4: Effects on Attendance (Schooldays missed): 2004 and 2006

BFP effect on attendance												
Matching	2004 2.28					2006 1.87					2004/2006 0.41	
	treated	Controls	diff	s.e.	t-stat	treated	Controls	diff	s.e.	t-stat	diff	t-stat
Unmatched	2.51	2.26	0.25	0.05	4.73	1.86	1.70	0.16	0.03	5.79		
OO	2.51	2.85	-0.34	0.10	-3.58	1.86	2.15	-0.28	0.06	-5.05	-0.06	-0.51
NN	2.51	2.87	-0.36	0.07	-4.83	1.86	2.21	-0.35	0.04	-8.03	-0.01	-0.51
Radius	2.51	2.86	-0.36	0.07	-5.44	1.86	2.19	-0.32	0.04	-8.27	-0.04	-0.46

Source: PNAD (IBGE, 2004 and 2006), own calculation

1.5 Heterogeneous Treatment Effects

While finding an overall effect of "Bolsa Família", we are also interested in impact differences between age groups, regions, genders and areas (rural/urban). To estimate the impact of the program on different groups we first constructed the counterfactual within the respective sub-sample and performed the matching on the observations of the sub-sample. The next section presents the impacts of the program for different groups in 2006 (see Appendix 1.2 for overview of the heterogeneous effects in 2004).

Table 1.5 presents the average treatment effects of the treated ATT for different groups. We find that there are no significant differences in enrollment rates or attendance between boys and girls beneficiaries of the program when compared with the control group (t-stat: 0.72 and 1.14 respectively for the OO, see Appendix 1.3)[6].

[6] T-test = $\frac{\hat{\beta}_1 - \hat{\beta}_2}{\sqrt{se_1{}^2 + se_2{}^2}}$

Table 1.5: Impact of the Program by Group [7]

	Enrollment				Attendence			
Group	OO	NN	Radious	Kernel	OO	NN	Radious	Kernel
Gender								
Males	4.80%***	4.38%***	4.37%***	4.36%***	-0.37***	-0.38***	-0.34***	-0.33***
Females	4.28%***	4.11%***	4.22%***	4.22%***	-0.24***	-0.24***	-0.29***	-0.27***
Diff	0.52%	0.27%	0.15%	0.14%	-0.13	-0.13*	-0.06	-0.06
Age Group								
6-9 years	4.02%***	3.83%***	3.68%***		-0.54***	-0.45***	-0.38***	
10 - 17 years	4.26%***	4.15%***	4.03%***		-0.27***	-0.27***	-0.28***	
Diff	0.24%	0.32%	0.35%		-0.27***	-0.18**	-0.10	
Location								
North	3.70%***	3.77%***	4.19%***	4.17%***	-0.26	-0.22	-0.21	-0.24
North-East	4.32%***	5.03%***	5.27%***	5.23%***	-0.21	-0.21	-0.21	-0.21
South-East	1.98%***	2.39%***	2.40%***	2.39%***	-0.32	-0.29	-0.29	-0.28
South	1.20%	2.09%***	2.06%***	2.02%***	-0.14	-0.21	-0.14	-0.11
Midwest	1.05%	2.58%***	2.75%***	2.61%***	0.08	0.13	0.17	0.16
Diff to South-East								
North	1.73%*	1.38%*	1.79%**	1.78%**	0.06	0.07	0.08	0.05
North-East	1.98%***	2.39%***	2.40%***	2.39%***	0.1***	0.08***	0.07***	0.07***
South	0.78%	0.30%	0.34%	0.37%	-0.06	-0.07	-0.08	-0.05
Midwest	0.93%	-0.19%	-0.35%	-0.22%	-0.10	0.06	0.05	0.03
Region								
Urban	3.28%***	3.55%***	3.50%***	3.41%***	-0.09***	-0.09***	-0.10***	-0.09***
Rural	5.91%***	5.65%***	5.51%***	5.49%***	-0.73***	-0.60***	-0.60***	-0.61***
Diff	2.63%***	2.10%***	2.01%***	2.08%***	-0.63***	-0.51***	-0.50***	-0.52***

Source PNAD (IBGE 2006) – own calculations
Ttest results: *significant at 10%, ** significant at 5%, *** significant at 1%

We compare the effect of the program for two age groups. The first group is composed of younger children who are between 6 and 9 years old while the second group is children between 10 and 17 years. We find that both older and younger children have a 4% higher probability of being enrolled when treated than the comparable group. However we do not find significant differences between both groups.

Program participation also has a positive effect on attendance. Depending on the matching estimator used, we find that younger children miss between 0.38 and 0.54 days less when treated than the comparable group. For teenagers, the effect is also positive; they missed around 0.27 less days of school than the counterfactual group. Furthermore, we find that younger children miss 0.2 days of school less than older children. This difference is significant at 1 and 5 percentage level (t-stat: 2.33 and 1.96 respectively, see Appendix 1.4).

[7] For detailed overview of the results see Appendix 1.3, Appendix 1.4 and Appendix 1.5.

We turn now to the analysis on the regional disparities of Brazil. While the South-East (SE) region is the most populated, urbanized and industrialized region (strongly driven by its two biggest cities Sao Paulo and Rio de Janeiro), the Northeast (NE) is characterized by a semi-arid climate and an economy driven mainly by tourism and antiquated agriculture. While the Midwest (MW) is also characterized by its predominantly agricultural activities, modern technology and low population density distinguish it from the Northeast. In general, the North (N) is mainly covered by Amazonian forests, while the ecology of the South (S) allows diversified activities from agriculture to industry, and is overall more developed than the other regions, in particular to the North and Northeast. Under this country feature we will now analyze the heterogeous impacts of the program took place over the regions.

Overall, we find a significant positive effect of cash transfers on enrollment in all regions. In the North, recipients had a 4% higher probability of being enrolled than non-beneficiaries. Additionally, this number was 5% in the Northeast, and around 2% in the South-East, South and Midwest. We did not find positive effects on attendance.

When we compare the effect of the program with respect to the South East region, one of the most developed regions of the country, we find that the effect of the program in the North is around 1.7 percentage points higher than when compared to the SE; moreover, this comparison achieves up to 2.4 percentage points higher enrollment rates for the NE when compared to the SE. This difference is statistically significant for the comparisons SE-N and SE-NE (t-test between 1.57 and 2.4 for the SE-N comparison and 2.53 and 4.57 for the SE-NE comparison). Yet we do not find any differences when the SE region is compared to more developed region, such as the South and the Midwest. Notwithstanding were the results on attendance, where the difference among regions were not present. Nonetheless, the results presented here provide some evidence that cash transfers have been able to mitigate the gap between regions, while it is higher in less developed regions than in more developed ones. This convergence effect might be

explained by lower initial enrollment in the North and Northeast region, which stayed at 91,45% and 92,76% respectively, when compared to the Southeast region (94,45%).

We also found that the effect of BFP varies across areas - while it increases enrollment in rural areas by around 5.5 %, while it is estimated to be about 3.5 % in urban areas for treated children. The two percentage points difference between rural and urban areas is significant for all matching estimators. We also find significant differences on attendance. The effect of BFP on comparable recipients in rural areas is significantly higher than in urban areas. Rural recipients miss, on average, 0.5 less days than comparable recipients in urban areas.

Just as before we find evidence supporting the positive effect of the program to overcome differences between regions and promote convergence in school enrollment.

1.6 Conclusion

In this paper we used Propensity Score Matching to evaluate the impact of the biggest cash transfer program in the world, "Bolsa Familia Program". We find that despite the lack of monitoring and enforceability of the conditions of this program during the analyzed period, it has a positive effect on human capital investments. Enrollment rates increases by four percentage points for children between the ages of 6 and 17, and estimates on attendance show that treated children miss 0.30 days of school less than untreated ones. Despite the positive effect of the program, we find that overtime the effect on enrollment slightly decreased by around 1% between 2004 and 2006. We do not find significant differences on the effects of the program across gender or age groups. One of the main achievements of “Bolsa Família” is its power to close the development gap between regions. Particularly the less developed in the North and North-East and the rural areas benefited more from the grants than the South and South-East and urban areas.

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Chapter 2

Leadership Working Behavior and Group Cooperation: An Experiment of Group Identity

Leadership Working Behavior and Group Cooperation: An Experiment of Group Identity

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Abstract

In order to deal with crises, organizations often bring expert leaders from outside. However, the lack of identification of the outside expert and the members of the organization can result in decreased performance of the organization. Using an experiment to investigate the role of identity and skills of the outside leader on the performance of the organization, we find that outside leaders are less committed than inside leaders which can motivate a decrease in cooperation. While leader's skills mitigate this effect, members of the organization fail to recognize it. Hence the gains in productivity that the skilled out-side leader bring are not enough to compensate the effect of their lack of identity with the organization.

Keywords: Social Identity, Leadership, Public Good game, Lab Experiment

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2.1 Introduction

Leaders affect the performance of organizations in various ways. For example, they help to overcome problems of asymmetric information by signaling the advantages of cooperation (Hermalin 1998, 2013). In organizations that maintain hierarchical institutions, leaders can discipline followers by imposing sanctions or providing rewards. In situations where there are multiple equilibria, leaders can help to coordinate actions (Van Huyck, et al. 1992; Weber et al. 2001). Leaders can also motivate group members or help shape their institutional cultures (Schein 2004). Most of all, leaders are productive assets and can affect the performance of their organizations through their dedication, effort and skills (Rosen 1982; Smith et al. 1984; Connelly et al. 2000). At the same time, leaders are highly mobile and are often replaced. For instance, political leaders are re-elected every four to six years; CEOs move to different posts, retire or are dismissed; and sport teams hire new coaches or change team captains. The replacement of leaders can rejuvenate an organization by bringing new ideas. Alternatively, replacement can help to discipline leaders to decreasing power abuse (Datta and Rajagopalan 1998; Ocasio 1994). Nonetheless, leader replacement could be damaging for an organization. The lack of identification of the leader and members of an organization could lead to a decrease in performance. The leader might feel as though they are a stranger to the organization, being less committed to work for it, while members of the organization might be less willing to cooperate with the outside leader. These two effects could result in decreased organizational performance.

Identity, or the process of self-categorization in which individuals subscribe emotional value to the group to which they feel identified with (Tajfel, 1974 and Turner, 1982), has been found to promote in-group favoritism and out-group discrimination (e.g. Akerlof and Kranton 2000; Bernhard et al., 2006; Goette et al., 2006; McLeish and Oxoby 2007; Li et al.,2011; Eckel and Grossman 2005; Tremewan 2010). Favoritism for in-group leaders has also been reported. For instance, Platow et al. (1997), Platow and van Knippenberg (2001) and Haslam and Platow

(2001) show that in-group leaders receive more support and are perceived to be fairer than out-group leaders. Yet the effect of the leader's identity on the performance of the organization has seldom been considered. Few studies use observational data to study the effect of an outside leader on firm performance (e.g. Huson et al. 2004; Lauterbach et al. 1999; Shen and Canella 2002; Zhang and Rajagopalan 2004). The main problem of those studies is that it is difficult to disentangle the effects of identity as the replacement of leaders is not random. Moreover, with this type of data, it is not possible to understand the channels by which identity affects performance. Laboratory experiments can overcome those limitations and provide valuable insights.

This paper uses a highly simplified controlled experiment to explore the effect of identity and skills of the leader on the organization's performance. We consider a scenario common to most organizations in which the dedication and efforts of the leader affect the success of the organization. For instance, the success of politicians in passing laws (or attract funding) depends on how hard they lobby the reforms among parliamentarians (donors). Similarly, the success of companies and producer groups depends on the ability of the leader to open new markets. Hence, in our experiment, we consider a modified public good game in which the marginal return from contributing to the public good depends on the productivity of the leaders in a real effort task. In our experiment, we vary exogenously the identity of the leader allowing for the leader to be either part of the organization (in-group) or an outsider (out-group). Furthermore, to account for the fact that in-group leaders might be less qualified that out-group leaders, we vary the skill level of the leader. Hence, we compare the performance of the organization with randomly selected leaders versus leaders selected according to highest skill level. In other words, we ask: Is it better to have a leader who identifies with the group he/she represents but who does not have the best qualification for the job, or a leader who is qualified for the job but is a stranger to the organization? Our hypothesis is that outside leaders who lacks identification with the members of the organization, have lower inner motivation to work. In turn, group

members anticipate this and decrease support towards the outside leader. These two forces lead to a decrease in the performance of the organization compared with an organization led by an inside leader.

Experimental studies have examined how leaders affect group performance. For instance, it has been shown that the decision of the first player (leader) affects the decisions of the followers, inducing higher cooperation levels (Potters et al. 2005 and 2007; Clark and Sefton 2001; Moxnes and van der Heijden 2003; Meidinger and Villeval 2002). Furthermore, Gächter et al. (2010) and De Cremer and Knippenberg (2005) show that more cooperative leaders or leaders that incur larger sacrifices can lead the group to achieve more cooperative outcomes. Additional studies have shown that leaders can increase group cooperation by imposing sanctions or offering rewards to group members (van der Heijden, Potters, and Sefton 2009; Güth et al. 2007; Gürerk et al., 2009; Rivas and Sutter 2009; Levati et al. 2007; Glöckner et al. 2011). In the framework of a coordination game as the weak link game, Brandts and Cooper (2007) and Brandts, et al. (2007) show that coordination traps can be avoided when someone acts as a leader and sets an example that pulls laggards after them. In a different framework, Kuang, et al. (2007) show that the effectiveness of a leader to solve coordination problems depends on the leader's motives. We make contributions to this area of research by focusing on the impact on the performance of the organization of a new type of leader: the productive leader.

To the best of our knowledge, only two papers that study the effect of identity on group performance are De Cremer Van Vugt and (1999) and De Cremer and Van Vugt (2002). However while they consider the question on how salience of group identity affects cooperation in a public good game and the effectiveness of leaders, these papers do not consider how the identity of a leader affects cooperation. Unlike previous papers that use fictional leaders, in our experiment, the leader plays an active role so that we can track two forms of discrimination:

discrimination from group members against the out-group leader and discrimination from the out-group leader against group members.

Experimental methods have previously been used to study organizational change. For instance, Weber and Camerer (2003) and Weber et. al. (2001) investigate a merger failure. While they focus on differences in communication style (which are erroneously attributed to failure by the leader), we focus on a more general aspect of culture as is identity or the feeling of being part of the organization.

2.2 Experimental Design

The experimental design is structured in three stages[9]. In the first stage, we induce identity. At the beginning of the experiment, participants were randomly divided into groups of four. The groups could be one of two colors: green or blue. While participants know their color, they do not know who is in their group as there is more than one group with the same color. Participants were presented a picture and on the side a list of hidden objects within the picture. Their task was to find the hidden objects and to type the number of row and column where the object was found. While solving this task, participants were able to chat with other member of their group using a chat box. In order to induce participants to cooperate, we explained that answers will be valid only if all four members of the group typed the correct answer. Moreover, to make identity more salient, the task was played as a tournament where groups of different colors competed against each other. In order to avoid income effects, participants did not receive monetary incentives in this task. Instead, winning groups received a congratulation message at the end of the session once the payout was announced. Participants were given 10 minutes to solve the task. By having a task in which participants solve a joint task, communicate and compete with others, we

[9] Instructions can be provided upon request

expect to generate a strong form of identity (See Eckel and Grossman, 2005 and Chen and Li, 2009).

The second stage is instrumental and is used to classify participants according to the performance in a real effort task. However, participants do not know this until the next stage once their task is over. Using Gill and Prowse (2012) real effort tasks, participants had 60 seconds to position up to 48 slides. Slides were positioned at zero and could be moved as far as 100. The task was to position the slide exactly at 50. While solving the task, participants knew the exact location of each slide, how many slides they positioned correctly and how much time remained. To avoid potential income effects, we did not use economic incentives in this task.

In the third stage, participants played a modified public good game. In the modified public good game, participants were assigned one of two roles: leader or group members. Leadership roles were assigned either randomly or according to the performance in the second stage real effort task. The roles remained constant over the experiment. Leaders were presented the Gill and Prowse real effort task again and had 60 seconds to position slides. Moreover, we explicitly provided them an outside option as all participants received a second picture with hidden objects. For each round, leaders received a fix payment of 25 points independently of the number of slides correctly positioned or the number of objects found in the picture (which is not recorded)[10].

Group members participated in a repeated modified public good game with random ending points between 10 and 15 periods. For each period, participants received 20 points of endowment, and their task was to distribute the endowment between a private and group account. Points invested in the private account returned 1 point, while points invested in the group account returned $a<1$ to all group members. The value of the multiplier, a, was determined according to the number of slides correctly positioned by the leader. Table 2.1

[10] Experimental points were transformed to Euros at an exchange rate of 100 points = 2.5 Euros.

shows the distribution of the correctly positioned slides by the leader and the corresponding value of the multiplier. While participants knew the return of the slides positioned by the leader, during the game, they did not know the exact number of slides correctly positioned.[11] Participants were paid according to the points earned over all periods. Table 2.1 shows the distribution of the correctly positioned sliders by the leader and the correspondent value of *a*. Only the leader knew the exact number of correctly positioned sliders. The modified public good game was played for t periods, where t was randomly chosen between 10 and 15. (see instructions in Appendix 2.4)

Table 2.1: Multiplier

Number of sliders correctly positioned by the leader	Multiplier to each group member
Less than 6	0.3
Between 6 and 8	0.4
Between 9 and 16	0.5
Between 17 and 20	0.6
More than 20	0.8

Between each round, we elicited expectations from participants on the performance of the leader and from the leader on the expected contribution of group members. Answers to expectations were incentivized.

Our experiment uses a 2x2 design that combines two different identities of the leader and two different selection mechanisms of the leader (see Table 2.2). In treatments one and three, leaders and group members share the same identity (belonging to the same group during the group identity induction stage), while in treatments two and four, leaders have a different identity than the group members (they did not belong to the same group in the group identity induction stage). In the first two treatments, leaders are randomly selected while in treatments three and four, the best participant in the second stage real effort task from each group is selected as a leader.[12]

[11] Participants could have determined the value of the multiplier given that they received information on group contributions.
[12] To avoid strategic bias, participants did not know it.

Table 2.2: Treatments

		Identity	
		In-group	Out-group
Selection of the leader	Random	Treat 1	Treat 2
	Skilled	Treat 3	Treat 4

The individual payoff function (Π) for the group member, i, and leader, L, and period, t, is given by:

$$\Pi_{ti} = (20 - c_{it}) + a(f_{Lt}) \Sigma c_{it} \text{ ,}$$

$$\Pi_{tL} = 25$$

Where, c_i is the amount invested in the group account, f_L is the number of slides correctly positioned by the leader and Σc_i is the total amount invested in group account by all group members. If the leader positions 6 slides or more, we have a social dilemma in which individually it is better to invest in the private account ($a<1$) but socially better to invest in the group account ($an>1$), where $n=3$. If less than 6 slides are correctly positioned, the individual and socially optimal solution is to invest zero in the group account.

Given that solving the real effort task is costly for participants–they need to concentrate and work under time pressure–and that there is no reward associated to performance, leaders would have no incentive to position slides correctly. Hence, $an<1$ and the optimal private and social decision is to contribute zero to the public good in each round. Optimal contribution decisions do not change over treatments.

2.3 Experimental Procedures

We implemented a lab experiment with 348 students from different disciplines within the period of November 2010 to October 2011. About half of the participants were male. Recruitment was conducted by email through the Online Recruitment System for Economic Experiments - ORSEE (Greiner, 2004) Participants received a show up fee of 2 EUR plus earnings from all

rounds in the experiment. The average earning was 17.86 EUR. In total, we conducted 29 sessions. Table 2.3 shows an overview of the treatments, sessions, and number of participants.

Table 2.3: Number of Observations by Treatment

Treatment	Sessions	Subjects	Groups	Observations (period<11)	%
Random in-group	8	100	25	1,000	28.74
Random out-group	8	92	23	920	26.44
Skilled in-group	7	80	20	800	22.98
Skilled out-group	6	76	19	760	21.84
Total	28	348	87	3.480	100.00

To ensure that the tasks were fully understood, we provided examples. Also, in the case of the second stage real effort task, we allowed participants a practice round. Pay-out of the public good game was explained by using examples. Additionally, we implemented control questions before participants solved their task.

2.4 Results

2.4.1 Descriptive Statistics

We start the analysis by comparing group performance in the identity task to see if group induction was homogeneous over treatments. Figure 2.1 shows Kernel densities of the number of objects found in the identity task. The distribution of the number of found objects was very similar throughout treatments with the exception of the treatment out-group leader. On average, participants in this treatment found 3.42 objects, while in other treatments, they found between 4.91 and 5.16 objects. We find significant differences in the distribution of objects found in treatment skilled out-group compared with other treatments (Wilcoxon ranksum test, p-value <0.001). In the coming analysis, we control for the mean number of objects found.

Our second analysis attempts to establish whether leaders are comparable across treatments. Figure 2.2 presents the Kernel distributions of slides correctly possitioned in the second task (before participants were assigned the role of leaders) by participants selected as leaders. On

average, participants selected as leaders in the out-group treament managed to position more slides correctly than in-group leaders. In treatments where leaders where selected randomly, out-group leaders outperformed in-group leaders possitioning 4.17 slides more during the second task (Wilcoxon rank sum test p-value: 0.0057), while in treatments where leaders were selected according to skills this value was 2.51 (Wilcoxon rank sum test p-value: 0.0099). This result suggest that comparisons by treatment need to be controled for differences in initial ability of the leaders.

Figure 2.1: Kernel Density on Number of Objects Found in the Identity Task

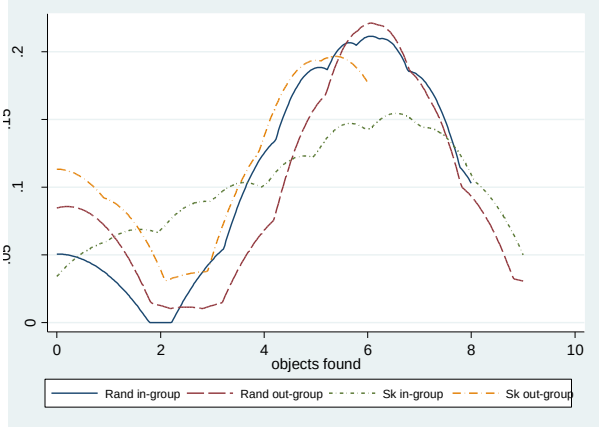
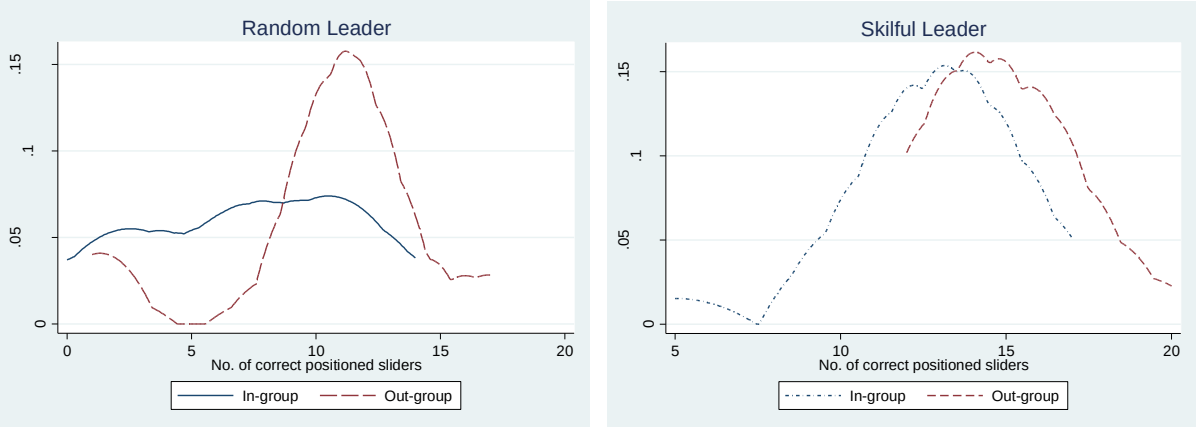


Figure 2.2: Number of Slides Correctly Positioned by Leader and by Treatment in Second Stage

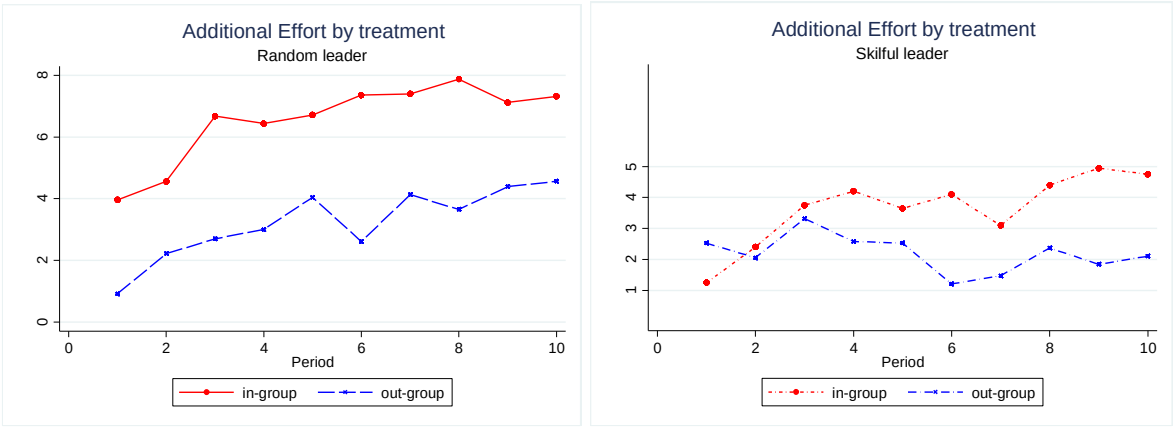


Panel A Panel B

In the third task, participants selected as leaders worked on a real effort task while participants selected as group members had to decide on contributions to the public good game. Descriptive analysis allows us to observe the effects of identity on leader productivity and member

cooperation. Given that we observed differences in productivity during the second stage (number of slides correctly possitioned by the leader) by treatment, we construct a measure that takes this difference into account. Hence, we look at the additional productivity of the leader or the number of slides correctly positioned by the leader in each period (once they knew they were selected as leaders), minus the number of slides correctly possitioned in the second stage (before they knew they would be selected as leaders). Figure 2.3 presents the additional productivity of the leaders in each of the treatments.[13] Panel A compares additional productivity for randomly selected in-group and out-group leaders, while panel B presents the results for leaders that are selected according to skills. We find that the change in productivity is significantly higher for in-group leaders than out-group leaders independent of whether the leader is selected randomly or by skills (Wilcoxon rank sum test p-value <0.001).

Figure 2.3: Effect of Identity on Real-Effort Task



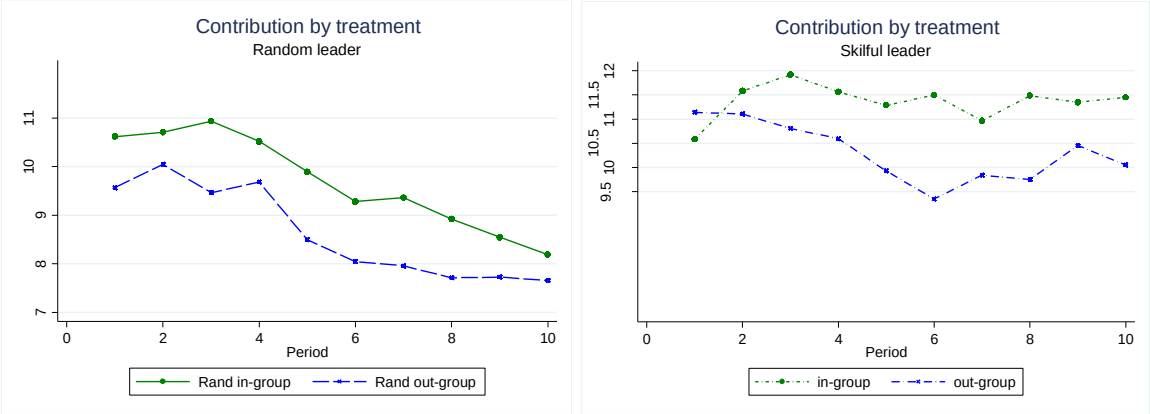
Panel A Panel B

Figure 2.4 presents the level of cooperation by treatment. We find that over all periods, members with randomly selected leaders contributed significantly more when the leader shared identity with the group members than when led by out-group leaders (Wilcoxon ranksum test, p=0.004).

[13] Approaching the effect of identity on additional number of slides correctly positioned might bias the results in the case where skilled out-group leaders approach the maximum number of slides that is possible to position within 60 seconds.

On average, participants invested 1.063 points more when led by a random in-group leader. This finding also holds for treatments where the leader is selected according to skills. In this case, we find that in in-group treatment members cooperated on average 1.065 points more (Wilcoxon ranksum test p-value:0.01) than in the treatment with out-group leaders. Interestingly, we find that the cooperation pattern changes for groups with skilled leaders compared with groups with random leaders. While the cooperation pattern among groups with random leaders presents the traditional downward slope, groups with skilled leaders sustain higher cooperation levels over time.

Figure 2.4: Effect of Leaders Identity on Contribution



Panel A Panel B

2.4.2 Regression Analysis

While the preliminary descriptive analysis hints at significant effects due to identity on leader productivity and group member contributions, in this section, we use panel data analysis to check the robustness of the results. Table 2.4 presents the results of the regression analysis. The first four columns present a Random Effect Panel Regression on the number of slides correctly positioned (Productivity). Standard errors are clustered by id. Columns 5 to 8 present regression results on a Tobit Random Effects Model on number of points invested in the group account (Contribution). We use observed information (OIM) standard errors. We present separate models for random and skilled leaders. In the regressions, we control for leader productivity in the

second stage (before leadership assignment), group performance in the identity task (identity control) and session specific effects *(*session dummies). In addition, columns 2, 4, 6 and 8 add a set of controls and interaction terms with the identity variable (out-group).

Table 2.4: Regression Results

	Number of slides correctly positioned: Random Effects panel regression					Number of points contributed: Tobit random effects panel regression - marginal effects reported (dy/dx)			
	Random leader		Skilful leader			Random leader		Skilful leader	
	Coeff.		Coeff.			dy/dx		dy/dx	
	(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)
out-group	-9.054**	-7.430*	-0.456	0.792	out-group	-7.810***	-5.468***	-6.100**	-4.962**
	(3,975)	(4,336)	(0.999)	(1,333)		(2,409)	(1,911)	(2,597)	(2,243)
Contribution (t-1)		0.0414		0.00806	Contribution (t-1)		0.114***		0.141***
		(0.0275)		(0.0148)			(0.0190)		(0.0207)
Period		0.303***		0.214*	Period		-0.299***		-0.0738
		(0.0931)		(0.116)			(0.0432)		(0.0488)
Outgroup* Contribution(t-1)		-0.0561		0.0212	Multiplier (t-1)		6.345***		5.067***
		(0.0398)		(0.0438)			(1,851)		(1,778)
Out-group* Period		-0.0521		-0.294					
		(0.121)		(0.273)	**Marginal Effects by InGroup and OutGroup Leader**				
					Contribution (t-1)				
					InGroup		0.161***		0.148***
							(0.0289)		(0.0326)
					OutGroup		0.067**		0.141***
							(0.0278)		(0.0314)
					Multiplier (t-1)				
					InGroup		8.002***		7.156***
							(2.5098)		(2.5896)
					OutGroup		4.729*		2917
							(2.8104)		(2.5158)
					Period				
					InGroup		-0.332***		-0.109
							(0.6497)		(0.0678)
					OutGroup		-0.266***		-0.037
							(0.0618)		(0.0737)
Leaders' Ability	YES	YES	YES	YES		YES	YES	YES	YES
Identity control	YES	YES	YES	YES		YES	YES	YES	YES
Session dummies	YES	YES	YES	YES		YES	YES	YES	YES
constant	6,285	3,142	-0.162	-2,256					
	-4,956	-5,386	-4,874	-5,187					
N	480	432	390	351		1440	1296	1170	1053

Standard errors in parentheses

* p<0.1, ** p<0.05, *** p<0.01

We find that leaders discriminate in favor of their own group. Outside leaders perform relatively worse than inside leaders. However, this effect is only significant when leaders are randomly selected. For skillful leaders, there are no significant effects of identity on performance in the real effort task. This could indicate that skilled participants have an internal motivation to work

in the task (i.e. they enjoyed it more than others, or wanted to increase performance) so their behavior was not affected by the treatment. Although we expected that leaders productivity would depend on group member cooperation—as they would feel that their effort may payoff only when participants contribute to the public good — we find no significant effects to member contributions. Not surprisingly, we found that positive learning effects and leader performance increased over time.

Regression results of the Tobit Random Effects Model support the findings of a negative effect of out-group leader on cooperation. Cooperation is significantly lower when participants are led by an out-group leader compared with an in-group leader independently of whether the leader is selected randomly or based on skills. As expected, contributions increase with contributions of other group members. Contribution also increases with the lag value of the multiplier, *a*. Member contributions increase between 0.51 and 0.63 points if the multiplier increases by 0.1 point. However, we find a difference in the contribution patterns between random and skilled leaders. While we observe a decreasing level of contributions over periods for random leaders, for skillful leaders, contributions do not decrease significantly over time. This indicates that skilled leaders are able to sustain higher cooperation levels.

We consider the independent marginal effects for treatments with in-group and out-group leaders and present the results in Table 2.4. Interestingly, we find that groups with in-group leaders reciprocate cooperation of other group members to a greater extent than groups with an out-group leader. While in random treatments contributions increase by 0.16 if other contribution increases by 1 point in in-group treatments, it increases only 0.07 in treatments with an out-group leader. The lower sensitivity to group member contributions in groups with out-group leader could be related with the "treat of identity". In this case, group members might attach a value to keep a positive image in front of the external leader so members cooperate despite observing a decrease in contributions by other group members. We also find that group members are more

sensitive to the return of the public good (multiplier) when led by an in-group leader compared to an out-group leader. This effect, however, is not significant.

Given that the multiplier is not constant over treatments, differences in contributions could be attributed to differences in the return to the public good. Controlling for the leader's initial level of ability, and the lagged multiplier should be enough to control for these differences. Yet, as an additional robustness check, we compare groups with the same multiplier. We run the same regression analysis considering groups which have the same multiplier (0.5) in 8 or more periods and in 9 or more periods. We find that the results are robust although as expected the significance is lower.

Turning back to the question: what is best for cooperation: a skilled out-group leader or a random in-group leader? Are gains in productivity that skilled out-group leaders bring able to outweigh the loss in cooperation due to their lack of identification with the group? Figure 2.4 presents average cooperation levels for the four treatments included in our design. The green-line in Panel A refers to contribution levels for a randomly selected in-group leader, while the blue line in Panel B refers to contributions for a skillful out-group leader. Using the Wilcoxon rank sum test, we find that the contribution level is not significantly different for random in-group leaders or skilled out-group leaders (Wilcoxon ranksum test p value 0.1094). To test the robustness of this result, we estimate the models in Table 2.4 taking into account the interaction effect of identity and skills. Table 2.5 presents the results of the linear combination of coefficients and marginal effects. As expected, we find that the lack of identity decreases leader productivity while leader skills increase it. The net effect of identity and skills imply a slightly negative effect on productivity, yet this effect is not significant once we control for contribution of group members and period. Consistent with our previous results, we find that even though cooperation decreases for out-side leaders, the positive effect of skilled leaders is just enough to compensate for this effect. Hence, we conclude that having a skillful out-group leader is similar

to having a random in-group leader except that skillful leaders are able to sustain higher cooperation levels over time. This effect might be associated with confidence in the leader as this effect remains even when we control for the value of the multiplier, and contribution of others in the group.

Table 2.5: Identity vs. Skills – Pooled data

	Effort: RE panel regession		Contribution: Tobit random effects panel regression - marginal effects reported		
	(1)	(2)		(3)	(4)
Linear Combination			*Marginal effects*		
Effect of Identity	-10.70**	-11.12**	Effect of Identity	-7.406***	-5.231*
(β_1)	(4.586)	(4.908)	(β_1)	(2.556)	(2.690)
Effect of Skills for Out-group	3.673	4.483	Effect of Skills for Out-group	5.090	3.746
$\beta_2+\beta_3$	(2.889)	(3.130)	$\beta_2+\beta_3$	(2.646)	(2.717)
Net Effect Identity and Skills	-7.027***	-6.527	Net Effect Identity and Skills	-2.312	-1.484
$\beta_1+\beta_2+\beta_3$	(4.586)	(4.026)	$\beta_1+\beta_2+\beta_3$	(2.558)	(2.690)
N	870	783		2610	2349

Standard errors in parentheses
* p<0.1 ** p<0.05 *** p<0.01"

2.5 Conclusion

Using an experimental approach, we find evidence that supports the intuition that out-group leaders can have a negative impact on organizations. Our results indicate that when out-group leaders are not highly skilled, they are less willing to work for the group than in-group leaders. Group members seem to anticipate this and cooperate less with an out-group than with an in-group leader. However, the negative effect of identity is compensated by leader skills. Skilled leaders do their best for the organization independently of their identity. Nonetheless, group members fail to recognize this and cooperate less when they have a skilled out-group leader than when they have a skilled in-group leader. Despite being less productive, random in-group leaders bring about the same levels of cooperation as skilled out-group leaders. In other words, the gains of higher skills from the out-group leader are just enough to compensate the lack of identity. These results indicate that if organizations are to select a leader, it is best to select skilled leaders from inside. In many cases, organizations might lack human capacity among their members. Hence, training members within the organization to assume leadership roles seems to

payoff. This is, however, a long term task. Therefore, having an skilled outside leader could be an alternative.

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Chapter 3

Elite capture and Group Identity: Is Monitoring Effective Against Power Abuse?

Elite capture and group identity: Is monitoring effective against power abuse?

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Abstract

Elite Capture characterizes leaders' power abuse through the privation and/or embezzlement of common resources. In development literature, Elite Capture is fairly often reported while partly even accepted in many small communities which can potentially cause a perpetuation mechanism. This paper analyzes how group identity can perpetuate abuse of power by leaders. Particularly, we test the hypothesis that scrutiny is lower among in-group leaders compared to an out-group leader which leads to an increase in power abuse. Our results confirm that members in homogenous identity groups are less willing to pay for monitoring, and that this lack of scrutiny increases leaders' embezzlement.

Keywords: Social Identity, Leadership, Trust game, Embezzlement, Lab Experiment

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3.1 Introduction

Leaders play important roles in their organizations. Yet, by being empowered to lead, leaders might abuse their power and self-reward themselves. Embezzlement or elite capture is fairly common and accepted phenomena in local communities in developing countries. Communities seem to accept it as a form of reward for the local leader. The objective of this paper is to investigate the causes of power abuse. Particularly, we consider whether social identity, defined as a sense of identification to a group to which individuals consign emotional attachment, causes and perpetuates power abuse. We consider whether in-group leaders benefit from higher trust and lower monitoring, which enables them to abuse their power more than out-group leaders.

Elite capture describes a situation where local elites deny access to common resources to part of the group members or capture these resources for themselves (Dutta, 2009; Bardhan and Mookherjee, 2005). It becomes an important phenomenon in the context of development as inefficiencies in the local development projects are often related to it (Platteau and Abraham, 2002). For instance, Powis (2007) report evidence of such elite capture in rural India, Darmawan (2012) in Indonesia, and Wong (2010) in Bangladesh and Ghana. Borras et al. (2007) find evidence that a marked-led agrarian reform in the Philippines benefited only the elite. Thorp et al. (2005) describe the exclusion of poorer members among different types of group communities such as credit groups, women's groups, scavenger groups and producer associations. A particularly interesting example of elite capture is given in a west-African country (not cited by the authors) by Platteau and Gaspart (2003); the authors describe a system of capture strongly affixed in the political norm characterized by resource appropriation, financial malpractices such as falsifying of accounts, invoice over-reporting and under-performance of contractors due to low-quality materials. Even after a process of strengthening the associations' institution, the local political norm of capture became so solid, that even

democratic elections would not change the system where elite capture was predominant and known in the community.

A set of papers relates the causes of power abuse to lack of suitable monitoring. Using a randomized field experiment, Reinikka and Svensson (2004) find that informing schools and parents in local Ugandan communities about a large school grant program through newspaper campaigns increased monitoring and reduced capture. Olken (2005) find a decrease of capture in response to top-down monitoring, while Björkman and Svensson (2007) find an improvement in the quality and quantity of health service provisions in response to bottom-up monitoring.

Further papers relate the cause of capture to community characteristics. Bardhan and Mookherjee (2005) for example argue that the typical poor governance structure of the projects where power and money are distributed at the local level facilitates collusion and, hence, makes these more susceptible to elite capture. Relating capture to community composition, Mansuri and Rao (2003) argue towards community heterogeneity as a cause of capture; with higher income inequality projects perform worse. Similarly, Iversen et al. (2006) discovered that a highly ethnically diverse forestry community in Terai, Nepal was highly susceptible to elite capture. In Burkina Faso, Bernard et al. (2008) find an association between ethnic heterogeneity and beneficiary exclusion of poor villagers. Further evidence was provided by Alesina et al. (1999), who find lower shares spent on the public good in ethnically more fragmented regions in the US.

Experimental evidence of elite capture is provided by van der Heijden et al. (2009), who test a hierarchical against a non-hierarchical firm structure, and find a positive effect of leaders once they are positioned to distribute groups' endowments. The authors compare two different institutional settings which are vulnerable to opportunistic behavior in a Public Good Game (PGG): (1) equally shared revenues without a leader and (2) reallocation of revenues determined by the leader. They find that leaders do not appropriate revenues. Instead, they are efficient in disciplining group members by withholding money for shirkers and rewarding cooperators.

A different twist of this result provide de Cremer and van Dijk (2005). The authors find that the entitlement feeling increased leaders' retained amount. Interestingly, Galinsky et al. (2003) find a paradox on leaders' taking and contributing behavior. Leaders are more prone to take from a common pool, while controversially present higher contribution to the public good. The authors relate these findings to leaders' entitlement, arguing that the power related to this position creates a tendency towards taking an action, regardless of its neutral, pro-social or antisocial outcome.

In a paper more directed to the elite capture concept, D'Exelle and Riedl (2008) study two dimensions of power abuse: elite capture, measured by resource distribution, and social exclusion, measured by voting against group members. The results show that leaders tend to search for the approval of the majority leading to a collusion mechanism; this collusion mechanism would enhance exclusion of the poor in case they voted against the current leader. In this setting, punishment mechanism would not work as a channel to overcome power abuse, but it would turn against the poor, as instead of a reduction in leaders' share, it induces a revenge mechanism and ends in exclusion. Alternatively, they find that poor leaders take a more equity inclined distribution strategy.

While there are some papers analyzing leaders' allocation preferences, only a few relate it to group identity. Among those few, Smith (2012) finds that in-group members privilege other in-group members against out-group members, even when bearing efficiency losses. Yet, groups with leaders were found less likely to harm outsiders (Ellman and Pezanis-Christou, 2007).

This paper contributes to the literature on elite capture by providing experimental evidence on one of the mechanisms that might perpetuate power abuse. Particularly, we consider shared social identity to be a cause of lack of scrutiny over the in-group leader. The lack of scrutiny is reflected in lower willingness of group members to monitor, which results in higher levels of power abuse. Our results point towards a higher willingness to pay for monitoring when the leader is an outsider. Yet, in the absence of monitoring, contributions decrease and proportional

outtake is higher. Moreover, we find that scrutiny is more effective when the leader is an outsider.

This paper is organized as follows: section 3.2 discusses the experimental design and hypothesis. Section 3.4 describes the data and presents the results. The last section concludes with some discussion.

3.2 Experimental Design

We divided the experiment in four stages: group identity induction, leadership selection, a trust game and a lottery game. The first stage induced identities using a procedure similar to the group identity task described in Section 2.2. In the first stage participants solved a common task while communicating and competing with other group members. Participants were randomly divided in groups of three and each group was assigned a blue or a green color. We asked participants to find the highest number of differences on two similar pictures. During the task, participants had the chance to communicate with the other group members through a chat-box. The solution of the task was typed in individually but was considered valid only if all the members typed in the same solution. The task was designed as a tournament, so that green groups were competing against blue groups. To avoid income effects, participants did not receive monetary incentives to solve this task. Instead, at the end of the experiment winning groups received a congratulation message.

The second stage was used to introduce a hierarchical structure or an elite in the group. Elites can take many forms related to social divisions such as class, casts, wealth, religious affiliations, lineage, etc. where an essential characteristic of the elites is privileged access to power. Accordingly, we defined our elite as a leader with additional – exogenous – access to power. We assigned him/her randomly and fixed over all periods within each group.

The third stage consisted in a real effort task followed by a modified trust game. To avoid house money effects[15] participants solved a real effort task in which they earn their endowment. The real effort task was to sum 5 two digit numbers. Participants had 30 seconds to solve as many summations as possible. To avoid income heterogeneity, all participants received 20 points as endowment regardless of their performance in the task. Once group members have earned their endowment, they participated in a trust game with two trustors and one trustee. The trustee was the selected leader, while the trustors were the other two participants in the group. Trustors had to decide how many points they wanted to pass to the trustee leader and how many points they wanted to keep to themselves. At this stage, leaders' where asked about amount of passed points they expect the members to pass to them, while members where asked about the amount of points they expect the leader to take. Corrected answers were rewarded by 4 points, answers differing by 1 point were rewarded by 3 points and 2 were rewarded by 1 point. The amount kept to themselves was multiplied by one while the amount passed was multiplied by the factor A. Hence, points passed by participant i, p_i, generated a value of Ap_i. The task for the trustee leaders was to decide how much of the value generated he wanted to return to the trustors. This decision was made independently for each of the group members. The value of the factor A depended on the performance of the trustee leader in the real effort task. If the trustee leader solved less than one sum correctly, A took the value 0.8. Otherwise the multiplier was randomly assigned and took a value between 0.8 and 1.9. By making the factor A dependent on leaders' performance in the real effort task, we intended to generate the feeling of entitlement. Yet, the randomness of the multiplier does not allow members to infer either leaders' effort, or the amount of captured points.

[15] The house money effect (Thaler and Johnson 1990) characterizes a risk seeking behavior when precedent from a winning situation (called by gamblers house money), in contrast to earned money.

In order to insure comparability between groups and sessions, the factor A was predetermined and fixed over periods once that the leader solved at least one sum correctly. Table 3.1 presents the factor used over periods.

Table 3.1: Value of the Factor A Used over Periods

	Period						
	One	Two	Three	Four	Five	Six	Seven
A	1.9	1.5	1.5	1.5	1.1	1.9	1.5

Note: this value was used conditional on the leader being able to solve at least one sum correctly

We use a 2x2 between subject design. Participants were matched either with an in-group or an out-group leader. In-group leaders corresponded to groups with homogeneous identities where the leader and group members played in the same group in the identity induction task. Consequently, in out-group treatments, blue leaders were assigned to play with green groups and vice-versa. Moreover we implemented the monitoring mechanism dependent on the treatment. In treatments with monitoring, we implemented costly monitoring. Group members were able to pay two points to observe the amount of points that the leader took from the respective member. Participants did not receive payments from the investments of other group participants. After the members' took the monitoring decision in each period, leaders were informed if they were being observed by at least one member, but this did not have monetary consequences for the leader. By informing whether monitoring was taking place or not, but not on how many members were monitoring, we could decrease the dimension of observability from three (not observed, observed by one member, observed by two members) to two (observed or not observed). The modified trust game was played for seven periods. Table 3.2 presents the treatments.

Table 3.2: Treatments

	No monitoring	Monitoring
In-group	Treat 1	Treat 3
Out-group	Treat 2	Treat 4

Individual payoff functions for group members and leaders are:

$$\text{Group Members' payoff: } \Pi_{Mi} = (e - p_i) + Ap_i - T_i - M_i$$

Leaders payoff: $\Pi_L = e + T_1 + T_2$,

where *e* stands for endowment, p_i denotes the amount of points that group member *i* passed to the leader, *A* the random multiplier between 0.8 and 1.9, T_i the amount of points that the leader embezzle from group member *i*, and M_i group member *i*'s monitoring costs.

On the last stage of the experiment, participants played a lottery game that follows Holt and Laury (2002) design. The results are primarily used to control for risk aversion. To ensure that risk aversion was not affected by the treatments and as such fulfills the control purposes, we tested for statistical significance among treatments finding no evidence supporting any affect of treatment induction on risk aversion.

3.3 Hypothesis and Predictions

From Group Identity Theory (Akerlof and Kranton, 2000) we adopt the premise that identity enhances expectations regarding individuals' behavior within the same group, driving individuals actions according to their social identity. We predict that the adapted behavior which is align to groups expectation would also positively affect trust levels within the group. Follows the predictions:

Hypothesis 1: group identity enhances trust in the leader, increasing passed points and decreasing willingness to pay towards monitoring the leader.

Hypothesis 2: the lack of monitoring from members and the consequent unobservability and privacy increases capture from leaders who share the same identity with members.

3.4 Data and Results

We conducted lab experiments with 234 students from different areas of study at Georg-August University of Göttingen (Germany) between June and October 2012. Subjects' recruitment was done through the Online Recruitment System for Economic Experiment ORSEE (Greiner, 2004).

Average earning per student and experiment was 16.91 EUR. Table 3.3 shows the number of observations by treatment.

Table 3.3: Observations by Treatment

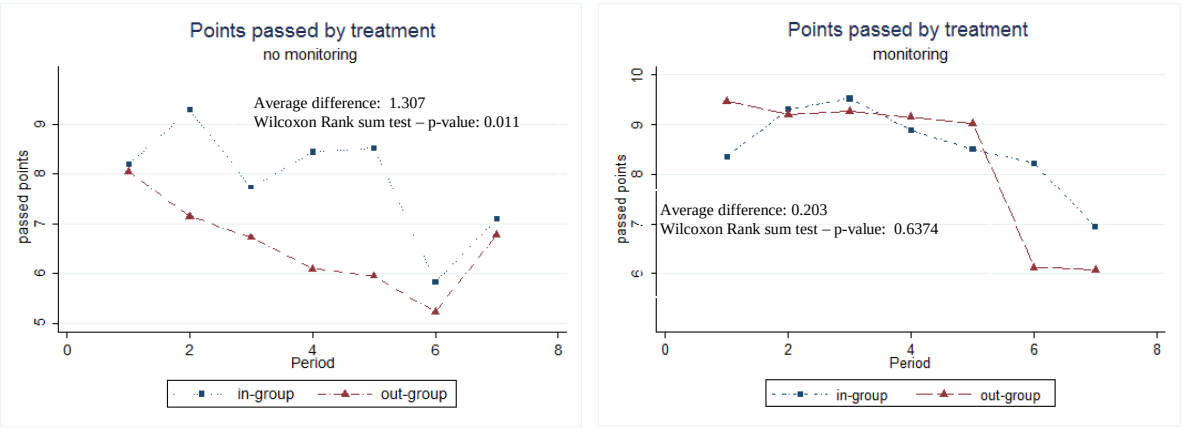
Treatment	Sessions	Subjects	Groups	Observations	%
In-group no monitoring	3	60	20	420	25.64
Out-group no monitoring	3	60	20	420	25.64
In-group with monitoring	3	54	18	378	23.08
Out-group with monitoring	3	60	20	420	25.64
Total	12	234	78	1638	100

3.4.1 Descriptive Statistics

a) Trust

Figure 3.1 shows the mean of points passed from members to the leader by leader type without monitoring possibility (panel A) and with (panel B) the monitoring mechanism. 25% of members in in-group treatment paid to observe leaders action (Treatment 3), while 27.5% of members with out-group leaders decided to monitor (Treatment 4).

Figure 3.1: Amount of Points Passed by In and Out-Group Leaders with and without Monitoring



Panel A Panel B

Panel A indicates more trust in leaders who share group members' identity. We observe that members led by in-group leaders passed on average 1.30 more points to the leader than those led by out-group leaders (Wilcoxon Rank sum test with p-value 0.011). However, this effect vanishes when members are able to monitor leaders' embezzlement behavior (panel B). Using

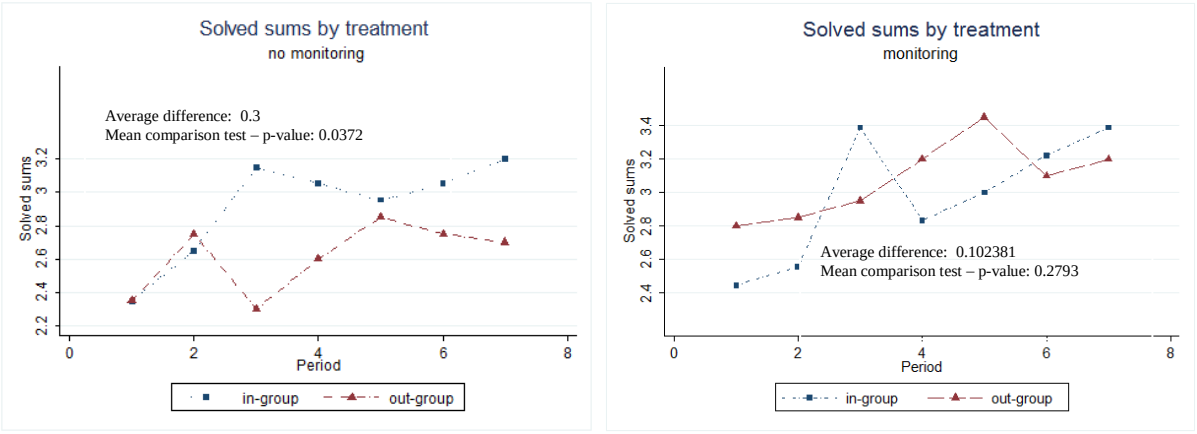
the Wilcoxon Rank sum test, we cannot reject the hypothesis of equal distribution in the number of points passed at any reasonable level of significance (p-value 0.64). Interestingly, we find that members led by in-group leaders do not significantly increase the amount of points passed when given the possibility to monitor the leader (comparison between blue lines in panels A and B; Wilcoxon Rank sum test with p-value 0.25). In contrast, members led by out-group leaders strongly react to this option. The number of points passed increased by 1.76 points under monitoring as compared with no monitoring for participants with an out-group leader (comparison between red lines in panels A and B; Wilcoxon Rank sum test with p-value 0.00). This indicates that monitoring is important to overcome the mistrust in the out-group leader, while it does not enhance trust in in-group treatment. We suspect that a shared identity between group members and leaders is associated with a level of trust high enough to make monitoring redundant.

b) Effort

Figure 3.2 presents the results of the real effort task - mean of solved sums in stage three of the experiment - by treatments. Panel A shows that, without the monitoring mechanism, in-group leaders were able to solve 0.3 more sums then out-group leaders on average (Mean-comparison test with p-value 0.0372).This difference in effort is not present under monitoring possibility (see panel B; Mean-comparison test with p-value 0.2793). Similarly, means of correctly calculated sums for in-group leaders with and without monitoring do not differ significantly (comparison between blue lines, panel A and B; Mean-comparison test with p-value 0.3719). However, we find a higher performance in the real effort task when out-group leaders' work under the monitoring mechanism (comparison between red lines, panel A and B; Mean-comparison test with p-value 0.0014). These strengthen the belief that monitoring is an effective tool to incentivize out-group leaders to work harder, while it has no effect on in-group leaders. Still, by comparing the effort levels of in-group leaders without monitoring (panel A; blue line) with out-

group leaders with monitoring (panel B; red line), we find that despite their positive reaction to monitoring, out-group leaders are not able to outperform in-group leaders in terms of effort (Mean-comparison test with p-value 0.1675).

Figure 3.2: Effort by Leaders' Identity with and without Monitoring

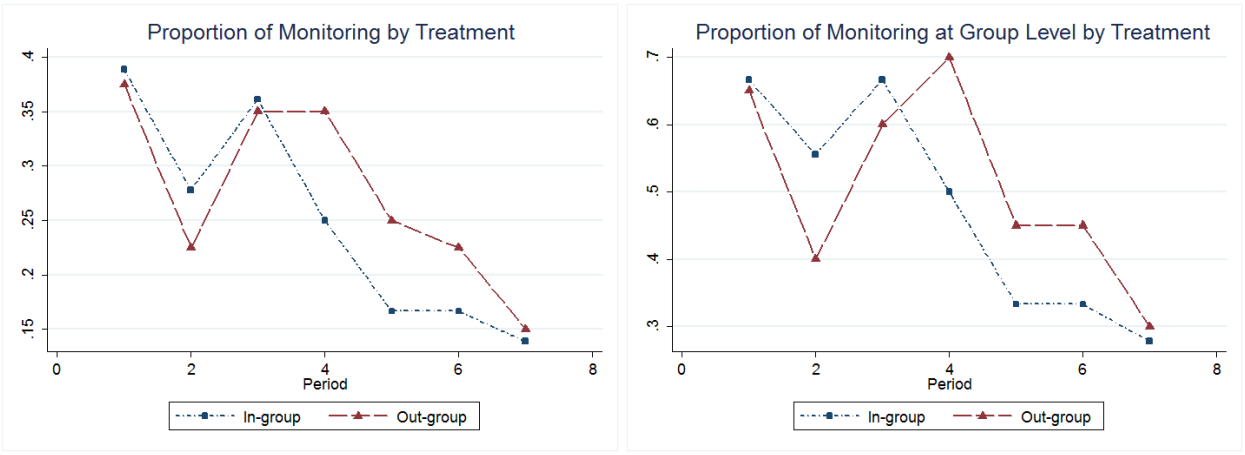


Panel A Panel B

c) Monitoring decision

Figure 3.3 shows the proportion of participants who monitored the leader by leaders' identity and per period. When comparing groups led by in-group leaders and groups led by out-group leaders, we see that the percentage of members who decided to monitor per period is similar from periods one to three (Two-group test of proportions with p-value 0.6613). There is, however, a shift in tendency after period three. Starting from period four, the proportion of members who paid to monitor was 6.3% higher when led by out-group leaders (Two-group test of proportions with p-value 0.0898). This tendency is similar at group level (Panel B measured by proportion of groups where at least one group member decided to monitor the leader). Between periods one and three the differences is not statistically different (Two-group test of proportions with p-value 0.887), but they turn 11.4% higher for out-group leaders after period three (Two-group test of proportions with p-value 0.0223).

Figure 3.3: Proportion of Participants who Paid to Monitor the Leader by Leaders' Identity

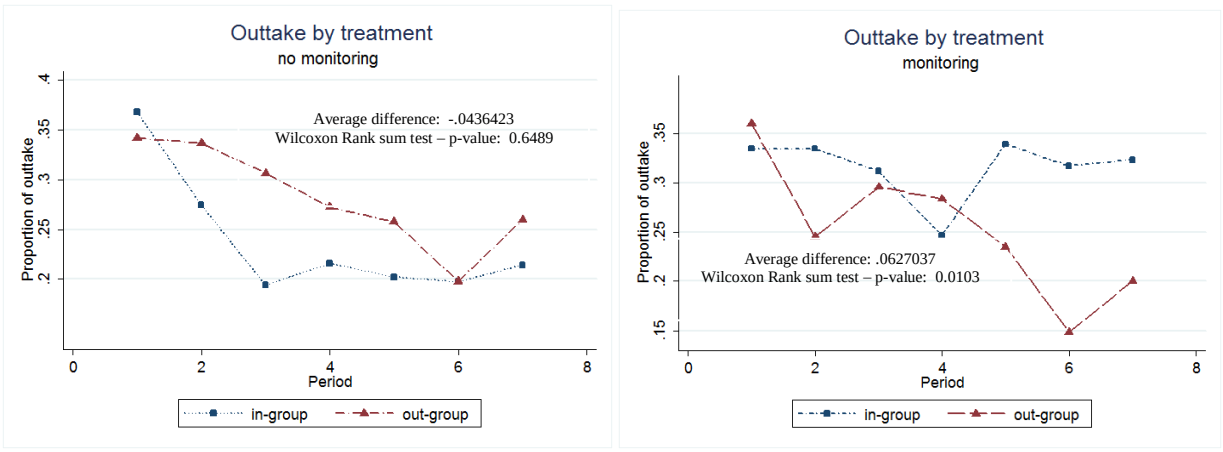


Panel A Panel B

d) Elite Capture

In order to identify how the monitoring mechanism can affect elite capture, we explicit three possible cases that can occur: (1) monitoring is not possible (and does not occur) (2) monitoring is possible, but by members' choice does not occur and (3) monitoring is possible and by members' choice occurs. The leader is informed if at least one group member is monitoring him.

Figure 3.4: Proportional Outtake by Leaders' Identity with and without Monitoring



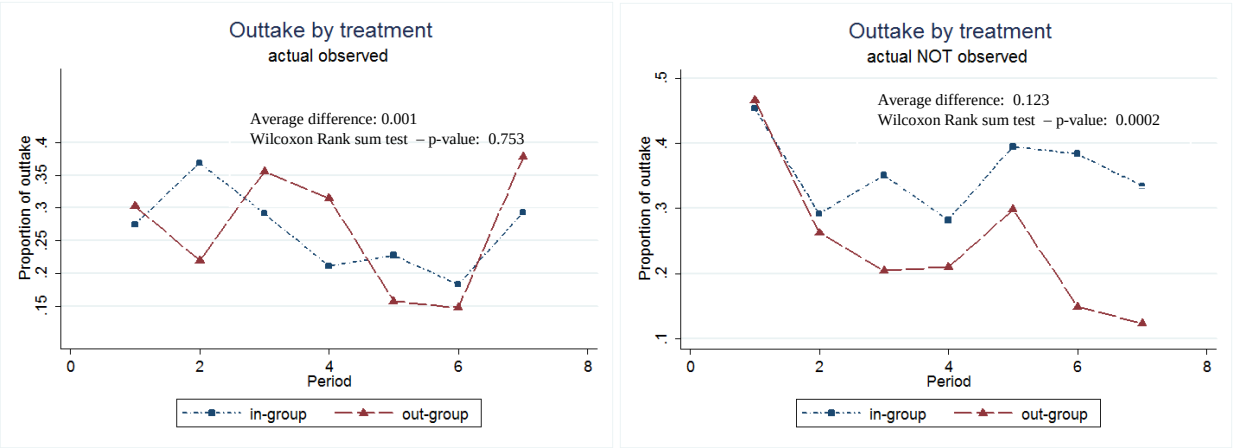
Panel A Panel B

Figure 3.4 shows the proportion of points taken out of the value generated (points each member passed multiplied by *A*) in treatments where monitoring is not possible and where monitoring is possible (independently if it occurs or not). Although out-group leaders tend to take a higher

proportion of the value generated than in-group leaders without a monitoring mechanism, this difference is not significant (Figure 3.3 Panel A; Wilcoxon Rank sum test with p-value 0.65). With the possibility to monitor, this pattern changes and in-group leaders keep on average six percentage points more to themselves than out-group leaders (Figure 3.3 Panel B; Wilcoxon Rank sum test with p-value 0.0103).

Figure 3.5 presents the average fraction of points taken in each period by leaders' identity when the leader was actually being observed and actually not being observed. Panel A presents the comparison between in-group and out-group leaders when the leader is as a matter of fact observed by at least one group member. It is possible to see that once leaders are actually observed, their capture behavior does not differ between in-group and out-group leaders. Panel B shows the mean of proportional outtake for each period by treatment when the leader knew that no member actually observed the embezzled amount (despite having the opportunity). Here, a change in behavior between different types of leaders becomes evident; in-group leaders take on average 12% more points than out-group leaders when they are certain that nobody observes them (Panel B; Wilcoxon Rank sum test with p-value 0.0002).

Figure 3.5: Proportional Outtake by Identity and Actual Monitoring



Panel A Panel B

While in-group leaders take a higher proportion of the value generated when group members decide not to monitor compared to groups that monitor (comparison between blue lines in panels

A and B; Wilcoxon Rank sum test with p-value 0.035), this behavior does not occur among out-group leaders (comparison between red lines in panels A and B; Wilcoxon Rank sum test with p-value 0.508).

This result provides first evidence that sharing the same identity can in fact be detrimental to groups as it instigates power abuse of the leader; when not observed, in-group leaders keep a significantly higher amount of points, while out-group leaders do not increase embezzlement when not observed.

3.4.2 Regression Analysis

This section provides causal inferences about the relationship between identity, trust, monitoring and capture. Table 3.4 presents a tobit random effect panel regression on trust measured by passed points (columns one and two), a probit random effect panel regression on probability to monitor (column three), and a fractional logit (Papke and Wooldridge, 1996) for elite capture measured by the proportional capture (columns four and five) with robust standard errors. Trust is measured by the amount of points passed to the leader from 0 to 20. Monitoring is measured by a dummy variable taking the value one if a member opted to monitor the leader. Elite capture is the share of points taken by the leader with regard to the amount of points passed after being multiplied by A, varying from 0.00 to 1.00. Columns one and four present results without the monitoring possibility (Treatment 1 and 2), while columns two, three and five present results with the monitoring possibility (Treatment 3 and 4).

Column one and two show the amount of points passed by the members to the leader, while not having the possibility to monitor (column one) and having the possibility to monitor (column two). The regression considers the following variables: *out-group* for groups led by out-group leaders and *Effort,* a dummy taking the value one if the leader reached the minimal effort. To capture identity specific effects, we include two interaction terms with the variable *out-group*, *out*no monitoring* for deciding against observing the leader, and *out*lagged return*, for the

amount of points returned to the member in previous period. Lastly we include a *risk aversion* indicator following Holt and Laury (2002) taking the values 0 to 10 increasing the more risk-averse, *session dummies* and a continuous *period* variable. Variables on monitoring were not present in treatment 1 and 2 (column two).

Table 3.4: Regression Analysis on Trust, Monitoring and Elite Capture – Marginal Effects Reported

		RE Tobit – Trust		RE Probit Monitor	Fractional logit – Elite Capture	
		Treat 1 and 2 no monitoring	Treat 3 and 4 monitoring	Treat 3 and 4 monitoring	Treat 1 and 2 no monitoring	Treat 3 and 4 monitoring
		(1)	(2)	(3)	(4)	(5)
out-group		-0.510	-0.575	0.873**	0.180***	-0.115***
		(-0.65)	(-0.78)	(2.02)	(4.98)	(-2.85)
Effort					-0.0129*	-0.0126
					(-1.68)	(-1.24)
marginal effects	***dy/dx***					
out*lagged passed points	*Out-group=0*			0.070***		
				(2.31)		
	Out-group=1			0.0402*		
				(1.8)		
out*E(capture)	*Out-group=0*			0.005		
				(0.08)		
	Out-group=1			0.080*		
				(1.76)		
out*no monitoring	*Out-group=0*		-0.351			0.140***
			(-0.45)			(2.73)
	Out-group=1		-1.403***			-0.014
			(-2.05)			(-0.38)
out* lagged return	*Out-group=0*	0.556***	0.544***			
		(10.17)	(8.1)			
	Out-group=1	0.621***	0.615***			
		(11.80)	(12.31)			
out*E(passed points)-passed points	*Out-group=0*				-0.009***	0.000
					(-4.62)	(0.11)
	Out-group=1				-0.015***	-0.007***
					(-4.95)	(-2.8)
out*total votes accumulated	*Out-group=0*					0.033
						(1.67)
	Out-group=1					0.001
						(0.04)
risk aversion		-0.182**	-0.111	-0.0234	-0.00280	-0.0151***
		(-2.17)	(-1.28)	(-0.48)	(-0.65)	(-3.18)
Period		-0.0169	-0.290***	-0.111**	-0.0162***	-0.0222**
		(-0.18)	(-2.82)	(-2.32)	(-2.79)	(-2.39)
Session dummies		YES	YES	YES	YES	YES
N		480	438	438	560	511

t and z statistics in parentheses
* p<0.1 ** p<0.05 *** p<0.01"

In contrast to previous descriptive findings in Figure 3.1,the regression results on trust show that having an outsider as a leader does not interfere with the amount of passed points to the leader as *out-group* turns insignificant (columns one and two). As expected, the amount of points returned by the leader in previous period is positively correlated with the amount of points passed for members with in-/ and out-group leaders. Additionally, we find that the lack of actual monitoring significantly decreases passed points to out-group leaders (column two). When members did not observe out-group leaders take up, members decreased passed points by 1.403, while we did not find any evidence of a significant response to monitoring decision from members led by in-group leaders. This result points towards a higher effectiveness of monitoring on trust among groups with outsiders as leader.

Additionally, we find a decrease in passed points by 0.182 in response to an increase in risk aversion, which disappears once the monitoring mechanism is put into place. This result points to the fact that monitoring successfully crowds out risk aversion.

Column three presents results on determinants of the decision to monitor the leader. We find that members led by out-group leaders are 87.3% more prone to opt towards observing leaders' take-up. As expected, when passed points were higher, members used monitoring as a tool to mitigate possible future losses. Our results show that the higher the amount of passed points in the previous period, the higher the probability to monitor leaders' actions. In addition, higher expected capture (*E(capture)*) among members led by out-group leaders implies higher willingness to pay for monitoring; if the expected capture increases by one point, probability to monitor the out-side leader increases by 8%. This effect turns insignificant for members in in-group leader treatments. This is first evidence towards higher suspicion of group members when led by out-group leaders. These are, consequently, more willing to pay for monitoring.

Columns four and five present the results of a fractional logit model on leaders' proportional embezzlement, or elite capture. Complementarily to the described variables above, we

considered the following determinants of elite capture: *no monitoring*, when leaders were not observed by at least one group member, leaders' effort measured by solved sums (*Effort*), and *E(passed points) – passed points*, which is the difference between points leaders expected members to pass and the actual amount of points passed. Furthermore, we control for a possible revenge mechanism from the leader as a response to members' monitoring decisions. We include a numeric variable indicating the amount of times that the leader was observed by at least one member in the given period (*total votes accumulated).* In addition, we include two interaction terms *out* {E(passed points) – passed point}s* and *out*total votes accumulated.* The remaining variables are the same.

When monitoring was not possible, out-group leaders captured about 18% more than in-group leaders (column four). Additionally, we find that higher effort is associated with less take-up by leaders, which indicates that committed leaders are less susceptible to power abuse.

Column five shows the same regressions with the possibility to observe the leader. We find that embezzlement behavior of in-/ and out-group leaders changes once group members have the possibility to monitor; while out-group leaders captured 18% in treatments without monitoring possibility, total effect on embezzlement from outside leaders becomes 11.5% lower under monitoring possibility. This finding indicates that out-group leaders respond to the monitoring mechanism while for homogenous identity groups it might damage leaders' good behavior. Besides, we find that effort becomes insignificant given the option to monitor which indicates that monitoring possibility potentially reduces the effort by committed leaders.

Marginal effects of the interaction terms confirm the descriptive analysis related to Figure 3.5. In-group leaders embezzle on average 14% more conditional on being actually observed than otherwise, which indicates that the lack of observability increases their capture. Together with the higher capture of in-group leaders once members are given the possibility to observe, this finding indicates that the possibility to observe might harm the group's social identity. The

monitoring mechanism might trigger feelings of mistrust and disappointment by the in-group leaders, which could potentially explain higher embezzlement given a possibility to monitor. However, we do not find any evidence of revenge behavior by leaders, as *total votes accumulated* turn out insignificant.

Overall, our results indicate that members led by out-group leaders are 87.3% more willing to pay for monitoring (column three); while actual observability increases their trust by 1.4 points (column two). We find that out-group leaders embezzle more (column four), but once they are potentially monitored, they capture 11.5% less then in-group leaders (column five). Furthermore, when knowing that they are not observed, in-group leaders abuse their power by capturing 14% more than out-group leaders (column five).

3.5 Conclusion

Our paper discusses the role of identity in a perpetuation mechanism of power abuse by group leaders. In the reported results we find evidence that group members are less willing to pay towards monitoring a leader whose identity they share; furthermore, not only is monitoring less present among groups with homogeneous identities, it also does not have an impact on in-group members' trust. When analyzing embezzlement behavior without the possibility to monitor we find that out-group leaders have a higher appropriation rate; however, this pattern changes with the offer to monitor. We then observe an increase in embezzlement by in-group leaders. Furthermore, the absence of actual monitoring boosts capture among in-group leaders while out-group leaders react to monitoring possibility with lower embezzlement. These findings lead us to the conclusion that monitoring is an effective tool for groups with heterogeneous identities, while it damages social capital among homogeneous identity groups, turning leaders exceptionally susceptible to power abuse.

3.6 References

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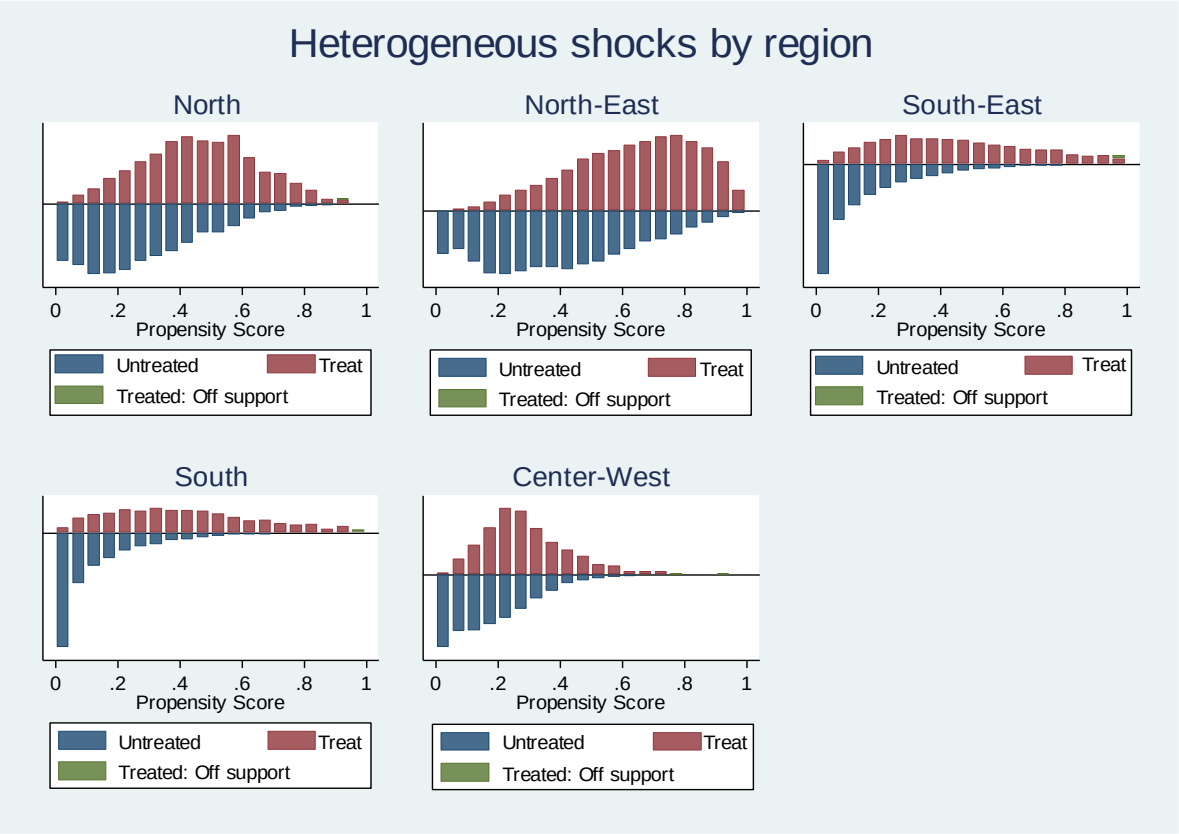
Appendix

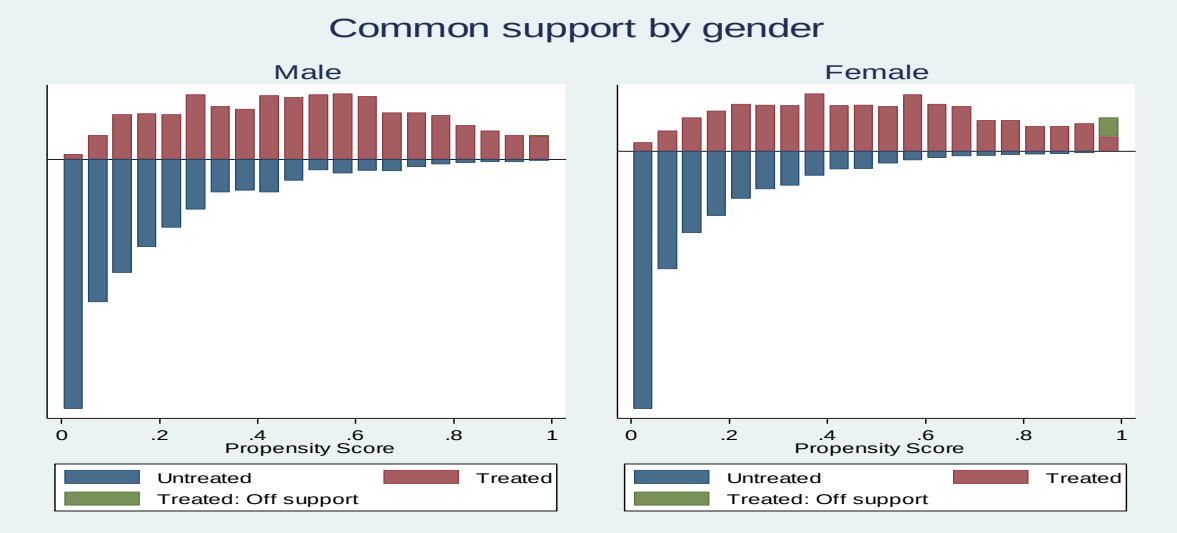
Appendix 1.1 - Region of Common Support

Panel A



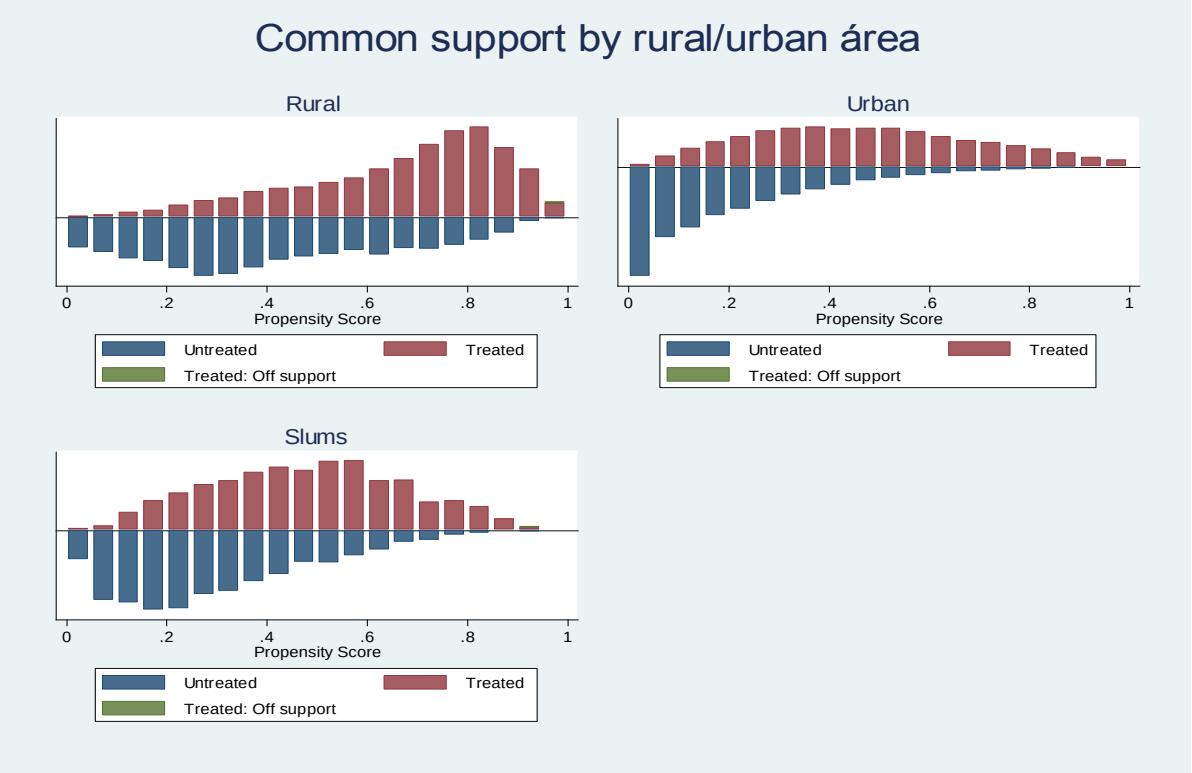
Source: PNAD 2006, own calculations

Panel B



Source: PNAD 2006, own calculations

Panel C



Source: PNAD 2006, own calculations

Appendix 1.1 - Balancing test – Complete Sample

		Mean				t-test	
Variable	Sample	Treated	Control	%bias	% reduct|bias|	t	p>|t|
asset_index	Unmatched	-1.1329	0.11148	-92.1		-128.86	0.000
	Matched	-1.1338	-1.2199	6.4	93.1	7.24	0.000
indian	Unmatched	0.00296	0.00287	0.2		0.23	0.821
	Matched	0.0029	0.00384	-1.7	-980.6	-1.97	0.048
black	Unmatched	0.07106	0.06082	4.1		5.88	0.000
	Matched	0.07116	0.0734	-0.9	78.2	-1.06	0.291
asian	Unmatched	0.0015	0.00373	-4.4		-5.79	0.000
	Matched	0.0015	0.00234	-1.6	62.7	-2.33	0.020
parda	Unmatched	0.64173	0.4613	36.9		51.62	0.000
	Matched	0.64169	0.64746	-1.2	96.8	-1.48	0.140
age	Unmatched	11.327	11.598	-8.0		-11.16	0.000
	Matched	11.325	11.257	2.0	74.7	2.48	0.013
birth_registered	Unmatched	0.9989	0.99797	2.4		3.18	0.001
	Matched	0.99893	0.99743	3.8	-61.0	4.31	0.000
UF1	Unmatched	0.0151	0.01999	-3.7		-5.14	0.000
	Matched	0.01515	0.01745	-1.8	53.0	-2.23	0.026
UF2	Unmatched	0.01925	0.01265	5.3		7.68	0.000
	Matched	0.01912	0.02562	-5.2	1.6	-5.39	0.000
UF3	Unmatched	0.02644	0.02458	1.2		1.66	0.096
	Matched	0.02626	0.03306	-4.3	-267.6	-4.91	0.000
UF4	Unmatched	0.00895	0.00542	4.2		6.12	0.000
	Matched	0.00897	0.00801	1.1	72.6	1.29	0.197
UF5	Unmatched	0.0675	0.06371	1.5		2.17	0.030
	Matched	0.06739	0.08004	-5.1	-233.0	-5.93	0.000
UF6	Unmatched	0.00316	0.01598	-13.2		-16.88	0.000
	Matched	0.00317	0.0036	-0.4	96.6	-0.91	0.361
UF7	Unmatched	0.01962	0.01471	3.8		5.45	0.000
	Matched	0.01968	0.02108	-1.1	71.5	-1.21	0.225
UF8	Unmatched	0.03977	0.01387	16.1		24.57	0.000
	Matched	0.03973	0.04284	-1.9	88.0	-1.91	0.056
UF9	Unmatched	0.02996	0.00963	14.6		22.48	0.000
	Matched	0.02996	0.02919	0.6	96.2	0.55	0.579
UF10	Unmatched	0.10761	0.04563	23.5		35.2	0.000
	Matched	0.10746	0.10042	2.7	88.6	2.82	0.005
UF11	Unmatched	0.02454	0.01277	8.7		12.92	0.000
	Matched	0.02462	0.02332	1.0	89.0	1.04	0.298
UF12	Unmatched	0.03708	0.01184	16.4		25.20	0.000
	Matched	0.03713	0.02889	5.4	67.4	5.65	0.000
UF13	Unmatched	0.09098	0.05235	15.0		22.04	0.000
	Matched	0.09105	0.09038	0.3	98.3	0.28	0.776
UF14	Unmatched	0.03182	0.01246	13.2		19.99	0.000
	Matched	0.03189	0.02903	2.0	85.2	2.04	0.041
UF15	Unmatched	0.01869	0.01306	4.5		6.53	0.000
	Matched	0.01858	0.01732	1.0	77.5	1.17	0.242
UF16	Unmatched	0.13621	0.07446	20.2		29.72	0.000
	Matched	0.13655	0.12734	3.0	85.1	3.33	0.001
UF17	Unmatched	0.09517	0.08659	3.0		4.23	0.000
	Matched	0.09528	0.08994	1.9	37.8	2.25	0.024
UF18	Unmatched	0.01816	0.01846	-0.2		-0.32	0.750
	Matched	0.01812	0.01695	0.9	-284.4	1.09	0.276
UF19	Unmatched	0.02215	0.06961	-22.8		-29.86	0.000
	Matched	0.02222	0.02072	0.7	96.8	1.27	0.205
UF21	Unmatched	0.02903	0.05609	-13.4		-18.06	0.000
	Matched	0.02882	0.02809	0.4	97.3	0.54	0.589
UF22	Unmatched	0.00868	0.03212	-16.6		-21.56	0.000
	Matched	0.00871	0.00724	1.0	93.7	2.02	0.043
UF23	Unmatched	0.0414	0.07642	-14.9		-20.12	0.000

	Matched	0.0413	0.03913	0.9	93.8	1.35	0.177
UF24	Unmatched	0.01081	0.02336	-9.7		-12.93	0.000
	Matched	0.01084	0.01448	-2.8	71.0	-3.98	0.000
UF25	Unmatched	0.01696	0.02627	-6.4		-8.73	0.000
	Matched	0.01701	0.01932	-1.6	75.3	-2.11	0.035
UF26	Unmatched	0.0257	0.04657	-11.2		-15.11	0.000
	Matched	0.02569	0.02923	-1.9	83.1	-2.65	0.008
UF27	Unmatched	0.01031	0.03578	-17.0		-22.15	0.000
	Matched	0.01028	0.01171	-1.0	94.4	-1.68	0.092
rural1	Unmatched	0.00486	0.00554	-1.0		-1.33	0.185
	Matched	0.00487	0.00597	-1.5	-60.8	-1.84	0.066
rural2	Unmatched	0.04037	0.01387	16.4		25.03	0.000
	Matched	0.0401	0.0375	1.6	90.2	1.65	0.099
rural3	Unmatched	0.00037	0.00081	-1.8		-2.43	0.015
	Matched	0.00037	0.00047	-0.4	77.4	-0.6	0.548
rural4	Unmatched	0.00113	0.00041	2.6		3.94	0.000
	Matched	0.00113	0.0008	1.2	53.5	1.31	0.189
rural5	Unmatched	0.25981	0.10782	40.0		59.58	0.000
	Matched	0.26046	0.25102	2.5	93.8	2.65	0.008
urban2	Unmatched	0.01064	0.00994	0.7		0.98	0.328
	Matched	0.01054	0.00897	1.6	-125.0	1.95	0.051
urban3	Unmatched	0.00492	0.00458	0.5		0.71	0.477
	Matched	0.00487	0.00417	1.0	-103.0	1.28	0.201
favela	Unmatched	0.05191	0.04736	2.1		2.97	0.003
	Matched	0.05191	0.05648	-2.1	-0.6	-2.47	0.013
aldeia	Unmatched	0.00096	0.00019	3.2		5.13	0.000
	Matched	0.00097	0.0005	1.9	39.7	2.11	0.035
head_age	Unmatched	42.965	43.423	-4.0		-5.55	0.000
	Matched	42.987	42.741	2.1	46.4	2.63	0.008
head_age2	Unmatched	1974	2023.7	-4.4		-6.15	0.000
	Matched	1975.3	1959.7	1.4	68.7	1.72	0.086
IT_headage_Z	Unmatched	0.44849	0.44805	0.0		0.01	0.989
	Matched	0.44725	0.58511	-3.2	-31324.7	-3.66	0.000
head_lite	Unmatched	0.70195	0.894	-49.3		-73.92	0.000
	Matched	0.70121	0.71118	-2.6	94.8	-2.68	0.007
head_educ	Unmatched	4.6334	7.8692	-82.4		-111.53	0.000
	Matched	4.6229	4.5803	1.1	98.7	1.58	0.115
head_educ2	Unmatched	32.979	81.28	-80.8		-105.47	0.000
	Matched	32.842	31.42	2.4	97.1	4.31	0.000
head_work_~	Unmatched	0.32918	0.11677	52.8		79.00	0.000
	Matched	0.32972	0.3029	6.7	87.4	7.06	0.000
head~d_other	Unmatched	0.00685	0.01178	-5.1		-6.95	0.000
	Matched	0.00687	0.00674	0.1	97.3	0.20	0.842
head_work_~f	Unmatched	0.09018	0.12564	-11.4		-15.76	0.000
	Matched	0.09014	0.09231	-0.7	93.9	-0.92	0.357
head_work_~n	Unmatched	0.10841	0.08704	7.2		10.30	0.000
	Matched	0.10819	0.11453	-2.1	70.3	-2.47	0.014
head_wor~rce	Unmatched	0.09886	0.15635	-17.3		-23.63	0.000
	Matched	0.09859	0.10512	-2.0	88.6	-2.65	0.008
head_work~od	Unmatched	0.02753	0.03271	-3.0		-4.20	0.000
	Matched	0.02749	0.02696	0.3	89.7	0.40	0.688
head_work_~t	Unmatched	0.03059	0.06266	-15.3		-20.43	0.000
	Matched	0.03056	0.02859	0.9	93.9	1.42	0.155
head_wor~lic	Unmatched	0.02451	0.06374	-19.2		-25.35	0.000
	Matched	0.02459	0.02179	1.4	92.9	2.28	0.023
h_service	Unmatched	0.02208	0.05457	-17.0		-22.49	0.000
	Matched	0.02165	0.02139	0.1	99.2	0.23	0.822
head_wor~tic	Unmatched	0.07356	0.05211	8.8		12.78	0.000
	Matched	0.0733	0.08721	-5.7	35.1	-6.27	0.000
he~r_service	Unmatched	0.02437	0.03224	-4.7		-6.54	0.000
	Matched	0.02422	0.02419	0.0	99.6	0.03	0.979
head~k_other	Unmatched	0.02853	0.06624	-17.8		-23.69	0.000
	Matched	0.02826	0.02659	0.8	95.6	1.25	0.211

head_work~ed	Unmatched	0.00545	0.00249	4.7		7.05	0.000
	Matched	0.00547	0.00581	-0.5	88.7	-0.55	0.585
mother_alive	Unmatched	0.985	0.97997	3.8		5.28	0.000
	Matched	0.98499	0.98345	1.2	69.5	1.51	0.132
mother_liveHH	Unmatched	0.9028	0.85261	15.4		21.04	0.000
	Matched	0.90305	0.89364	2.9	81.3	3.81	0.000
head_female	Unmatched	0.27557	0.27324	0.5		0.74	0.461
	Matched	0.27367	0.28635	-2.8	-443.3	-3.46	0.001
nr_HHhabit	Unmatched	5.7571	4.6323	58.3		85.46	0.000
	Matched	5.7578	5.7407	0.9	98.5	0.95	0.340
children	Unmatched	3.2556	2.2518	68.5		101.18	0.000
	Matched	3.2561	3.2753	-1.3	98.1	-1.36	0.174
oldest_chi	Unmatched	13.418	12.842	17.9		24.61	0.000
	Matched	13.418	13.387	0.9	94.7	1.19	0.234

Source: PNAD 2006, own calculation (pstest output)

Appendix 1.2 - Heterogeneous Treatment Effects 2004 (One-to-One Matching)

	Enrollment					Attendance				
by age	treated	controls	Diff	s.e.	t-stat	treated	controls	diff	s.e.	t-stat
6-9 years	95.41%	92.34%	3.07%	0.006	5.03	2.19	2.78	-0.59	0.152	-3.88
10-17 years	93.88%	89.17%	4.71%	0.005	9.95	2.64	2.80	-0.17	0.119	-4.76
	diff			t-stat		diff			t-stat	
younger vs. older	-1.64%			3.36	***	-0.42			-2.20	**
by gender										
male	93.15%	87.00%	6.15%	0.01	10.67	2.68	2.88	-0.20	0.14	-1.38
female	95.56%	91.53%	4.03%	0.00	8.24	2.33	2.80	-0.47	0.13	-3.58
	diff			t-stat		diff			t-stat	
Male-Female	2.12%			2.81	***	0.27			1.40	
by area										
rural	93.47%	86.37%	7.10%	0.01	6.97	2.33	2.83	-0.51	0.23	-2.21
urban	94.58%	89.99%	4.59%	0.00	10.48	2.56	2.72	-0.16	0.11	-1.50
	diff			t-stat		diff			t-stat	
rural-urban	2.51%			2.26	**	-0.34			-1.36	
by region										
N	93.85%	87.89%	5.96%	0.010	5.96	1.87	2.00	-0.12	0.148	-0.84
NE	95.02%	89.95%	5.07%	0.006	8.48	3.00	3.42	-0.43	0.191	-2.24
SE	94.43%	90.03%	4.40%	0.008	5.47	2.07	2.61	-0.53	0.211	-2.53
S	92.15%	90.35%	1.80%	0.011	1.58	2.46	2.45	0.00	0.193	0.02
Midwest	94.23%	90.92%	3.31%	0.012	2.79	1.87	2.13	-0.26	0.194	-1.36
	diff			t-stat		diff			t-stat	
N-S	4.16%			2.74		-0.30			- 0.52	
NE- S	3.27%			2.53		-0.43			- 1.59	
SE- S	2.60%			1.86		-0.54			- 1.88	**
Midwest - S	1.50%			0.91		-0.27			- 0.98	

Source: PNAD 2006, own calculation

Appendix 1.3 - Heterogeneous Treatment Effects by Gender

Heterogeneous shocks by gender										
Matching	Enrollment					Attendance				
	treated	controls	diff	s.e.	t-stat	treated	controls	diff	s.e.	t-stat
Male										
	92.83%					1.82				
OO	92.31%	87.51%	4.80%	0.01	9.18	1.94	2.30	-0.37	0.08	-4.64
NN	92.31%	87.93%	4.38%	0.00	10.91	1.94	2.31	-0.38	0.06	-6.00
Radius	92.31%	87.94%	4.37%	0.00	12.57	1.94	2.28	-0.34	0.06	-6.18
Kernel	92.31%	87.95%	4.36%	0.00	12.72	1.94	2.27	-0.33	0.06	-6.02
Female										
	93.42%					1.70				
OO	94.03%	89.75%	4.28%	0.00	8.63	1.79	2.03	-0.24	0.08	-3.07
NN	94.03%	89.92%	4.11%	0.00	10.81	1.79	2.03	-0.24	0.06	-3.96
Radius	94.03%	89.81%	4.22%	0.00	12.78	1.79	2.07	-0.29	0.05	-5.26
Kernel	94.03%	89.81%	4.22%	0.00	12.89	1.79	2.06	-0.27	0.05	-5.00
ttest for statistically signifcantly differences: Male-Female										
	diff		t-stat			diff		t-stat		
OO	0.52%		0.72			-0.13		1.14		
NN	0.27%		0.48			-0.13		1.51	*	
Radius	0.15%		0.30			-0.06		0.76		
Kernel	0.14%		0.30			-0.06		0.82		

Source: PNAD 2006 – own calculations

Appendix 1.4 - Heterogeneous Treatment Effects by Age

Heterogeneous shocks by age group										
Matching	Enrollment					Attendance				
	treated	controls	diff	s.e.	t-stat	treated	controls	diff	s.e.	t-stat
6-9 years										
OO	96.00%	91.99%	4.02%	0.005	7.62	1.85	2.39	-0.54	0.094	-5.79
NN	96.00%	92.17%	3.83%	0.004	9.99	1.85	2.30	-0.45	0.071	-6.30
Radius	96.00%	92.32%	3.68%	0.003	11.25	1.85	2.23	-0.38	0.064	-5.89
10-17 years										
OO	91.73%	87.47%	4.26%	0.005	9.09	1.87	2.14	-0.27	0.070	-3.82
NN	91.73%	87.58%	4.15%	0.004	11.30	1.87	2.14	-0.27	0.055	-4.94
Radius	91.73%	87.70%	4.03%	0.003	12.46	1.87	2.15	-0.28	0.050	-5.61
ttest for statistically significantyl differneces: Primary-Secondary (6-10 vs 10-17)										
	diff		t-stat			diff		t-stat		
OO	0.00		-0.35			-0.27		-2.33		***
NN	0.00		-0.59			-0.18		-1.96		**
Radius	0.00		-0.76			-0.10		-1.20		

Source: PNAD 2006, own calculation

Appendix 1.5- Heterogeneous Treatment Effect by Region - 2006

Heterogeneous shocks by region										
Matching	Enrollment					Attendance				
	treated	controls	diff	s.e.	t-stat	treated	controls	diff	s.e.	t-stat
North										
	91.45					1.65				
no Matching	92.19%	91.40%	0.81%	0	1.64	1.87	1.54	0.33	0.08	4.33
OO	92.19%	88.49%	3.70%	0.008	4.49	1.87	2.13	-0.26	0.128	-2.02
NN	92.19%	88.43%	3.77%	0.006	5.9	1.87	2.09	-0.22	0.102	-2.12
Radius	92.19%	88.00%	4.19%	0.006	7.27	1.87	2.08	-0.21	0.092	-2.26
Kernel	92.19%	88.02%	4.17%	0.006	7.3	1.87	2.11	-0.24	0.092	-2.56
NE										
	92.76					1.88				
no Matching	93.72%	92.42%	1.30%	0	4.4	1.88	1.88	0	0.05	-0.02
OO	93.72%	89.40%	4.32%	0.006	7.52	1.88	2.09	-0.21	0.096	-2.23
NN	93.72%	88.69%	5.03%	0.004	11.18	1.88	2.23	-0.21	0.078	-4.5
Radius	93.72%	88.45%	5.27%	0.004	12.91	1.88	2.22	-0.21	0.072	-4.68
Kernel	93.72%	88.49%	5.23%	0.004	13.07	1.88	2.19	-0.21	0.071	-4.4
SE										
	94.45					1.84				
no Matching	93.10%	95.06%	-2.06%	0	-5.85	1.87	1.82	0.05	0.06	0.92
OO	93.10%	91.12%	1.98%	0.007	2.73	1.86	2.18	-0.32	0.108	-2.94
NN	93.10%	90.71%	2.39%	0.006	4.27	1.86	2.16	-0.29	0.082	-3.53
Radius	93.10%	90.70%	2.40%	0.005	4.99	1.86	2.15	-0.29	0.074	-3.84
Kernel	93.10%	90.71%	2.39%	0.005	5.03	1.86	2.15	-0.28	0.073	-3.83
South										
	92.89					1.65				
no Matching	91.03%	93.65%	-2.79%	0.01	-4.79	1.85	1.6	0.25	0.08	3.13
OO	91.03%	89.83%	1.20%	0.008	1.07	1.85	1.98	-0.14	0.155	-0.88
NN	91.03%	88.94%	2.09%	0.009	2.3	1.85	2.05	-0.21	0.123	-1.68
Radius	91.03%	88.97%	2.06%	0.008	2.57	1.85	1.97	-0.14	0.112	-1.2
Kernel	91.03%	89.00%	2.02%	0.008	2.6	1.85	1.95	-0.11	0.109	-0.97
Midwest										
	93.77					1.48				
No Matching	94.31%	93.98%	0.27%	0.01	0.45	1.73	1.41	0.33	0.09	3.79
OO	94.31%	93.26%	1.05%	0.009	1.16	1.75	1.67	0.08	0.144	0.56
NN	94.31%	91.73%	2.58%	0.007	3.52	1.75	1.62	0.13	0.114	1.16
Radius	94.31%	91.56%	2.75%	0.007	4.21	1.75	1.58	0.17	0.107	1.59
Kernel	94.31%	91.70%	2.61%	0.006	4.08	1.75	1.59	0.16	0.106	1.54
ttest for statistically significantly differences from SE										
SE-N										
OO	1.73%			1.57	*	0.06			-0.35	
NN	1.38%			1.62	*	0.07			-0.56	
Radius	1.79%			2.39	**	0.08			-0.64	
Kernel	1.78%			2.4	**	0.05			-0.39	
SE - NE										
OO	1.98%			2.53	***	0.1			-3.68	***
NN	2.39%			3.68	***	0.08			-4.46	***
Radius	2.40%			4.55	***	0.07			-4.82	***
Kernel	2.39%			4.57	***	0.07			-4.85	***
SE-S										
OO	0.78%			0.59		-0.18			-0.95	
NN	0.30%			0.28		-0.08			-0.56	
Radius	0.34%			0.36		-0.15			-1.11	
Kernel	0.37%			0.4		-0.18			-1.33	
SE-Midwest										
OO	0.93%			0.8		-0.24			-0.35	
NN	-0.19%			-0.21		-0.16			-1.12	
Radius	-0.35%			-0.43		-0.11			-0.87	
Kernel	-0.22%			-0.28		-0.12			-0.91	

Source: PNAD 2006, own calculation

Appendix 2.1- Regression by Treatment – Chapter 2

	Number of slides correctly positioned: Random Effects panel regression				Number of points contributed: Tobit random effects panel regression - marginal effects reported (dy/dx)				
	Random leader		Skilful leader			Random leader		Skilful leader	
	Coeff.		Coeff.			dy/dx		dy/dx	
out-group	-9.054**	-7.430*	-0.456	0.792		-7.810***	-5.468***	-6.100**	-4.962**
	-3.975	-4.336	-0.999	-1.333		-2.409	-1.911	-2.597	-2.243
Contribution (t-1)		0.0414		0.00806					
		-0.0275		-0.0148					
Period		0.303***		0.214*					
		-0.0931		-0.116					
Out-group*Contribution (t-1)		-0.0561		0.0212	in		0.161***		0.148***
		-0.0398		-0.0438			-0.0289		-0.0326
					out		0.067**		0.141***
							-0.0278		-0.0314
Outgroup*Multiplier					in		8.002***		7.156***
							-2.5098		-2.5896
					out		4.729*		2917
Out-group*Period		-0.0521		-0.294			-2.8104		-2.5158
		-0.121		-0.273	in		-0.332***		-0.109
							-0.6497		-0.0678
					out		-0.266***		-0.037
							-0.0618		-0.0737
Multiplier (t-1)							6.345***		5.067***
							-1.851		-1.778
Leaders' Ability	0.818***	0.841***	1.114***	1.166***		-0.0395	-0.102	-1.035***	-0.908***
	-0.266	-0.274	-0.321	-0.325		-0.114	-0.107	-0.232	-0.239
Identity control	0.118	0.126	-0.406	-0.454		0.0748	0.000961	-0.0567	-0.0255
	-0.3	-0.312	-0.306	-0.321		-0.198	-0.184	-0.201	-0.201
Session dummies									
1	-2.395	-2.773				-0.216	0.0178		
	-2.805	-3.211				-2.148	-1.969		
2	4.473	4.905				7.096***	6.139***		
	-3.025	-3.391				-2.442	-2.284		
3			4.351***	4.693***				-5.091**	-4.620**
			-1.661	-1.786				-2.296	-2.323
4			2.255	2.491				-6.684***	-5.704**
			-1.94	-2.041				-2.423	-2.454
5	-8.705*	-9.122*				-0.0713	0.983		
	-5.115	-5.327				-2.26	-2.094		
6	-7.015**	-7.309**				-1.523	0.289		
	-2.932	-3.448				-2.239	-2.084		
7	3.126	3.338				4.865**	4.116*		
	-2.942	-3.245				-2.46	-2.286		
8	-4.316	-4.404				-1.73	-0.563		
	-3.025	-3.341				-1.988	-1.836		
9			7.824***	8.236***				-12.85***	-10.43***
			-2.73	-2.827				-3.001	-3.073
10	6.217*	6.677*				6.450**	5.257**		
	-3.25	-3.608				-2.531	-2.38		
11			5.166**	5.891**				-9.180***	-7.834***
			-2.296	-2.404				-2.803	-2.83

12	9.420**	9.727**			3.393	2.964		
	-3.785	-3.924			-2.506	-2.348		
13			6.283***	6.744***			-4.300*	-3.864*
			-2.178	-2.391			-2.32	-2.341
14			4.253	3.828			4.765**	4.000*
			-2.855	-2.978			-2.347	-2.371
15	-11.35**	-10.95**			-6.403***	-3.643*		
	-4.76	-5.057			-2.147	-2.046		
16			-0.476	-0.768			0.934	1.008
			-1.463	-1.588			-2.34	-2.337
17			3.500***	3.966***			-3.341	-2.455
			-0.49	-0.551			-2.557	-2.569
18			-1.371	-2.027			5.421**	4.582*
			-2.317	-2.669			-2.33	-2.344
19			6.770**	6.571**			3.222	2.312
			-2.927	-2.995			-2.346	-2.368
20	-7.488*	-6.807			-5.243**	-2.819		
	-4.417	-4.764			-2.219	-2.072		
23	-0.821	-0.687			5.121*	4.921*		
	-3.376	-3.619			-2.844	-2.621		
26	5.259	5.267			4.042	3.419		
	-3.504	-3.737			-2.819	-2.608		
27	1.646	2.1			-0.306	-0.266		
	-4.65	-5.061			-2.429	-2.228		
28	-0.00319	0.405			2.214	1.843		
	-2.845	-3.022			-3.472	-3.189		
_cons	6.285	3.142	-0.162	-2.256				
	-4.956	-5.386	-4.874	-5.187				
N	480	432	390	351	1440	1296	1170	1053

Appendix 2.2 - Pooled Regression – Chapter 2

	Effort: RE panel regession			Contribution: Tobit random effects panel regression - marginal effects reported	
	-1	-2		-3	-4
out-group	-10.70**	-11.01**		-5.488***	-4.246**
	-4.586	-4.891		-1.51	-1.653
skilled	-3.226	-2.582		3.354*	2.863
	-3.691	-4.112	*dy/dx*	-1.753	-1.752
			skilled		
out-group*skilled	6.899	7.065	*Out-group=0*	1.669	2.189
	-4.691	-5.019		-2.448	-2.48
			Out-group=1	5.090*	3.746
				-2.646	-2.717
Contribution (t-1)		0.0221			0.133***
		-0.0166			-0.014
Multiplier (t-1)					5.486***
					-1.249
Period		0.285***			-0.182***
		-0.0645			-0.0318
skilled*Period		-0.217	*Skilled=0*		-0.292***
		-0.162			-0.045
			Skilled=1		-0.071
					-0.048
Leaders' Ability	0.839***	0.859***		-0.207**	-0.239**
Identity Control	-0.219	-0.226		-0.102	-0.099
S1	-4.187	-5.007		0.324	0.398
	-3.628	-3.922		-2.532	-2.425
S2	4.205	3.942		7.388***	5.614**
	-3.115	-3.198		-2.424	-2.336
S3	0.954	0.992		-2.367	-2.552
	-1.541	-1.762		-2.141	-2.058
S4	-0.725	-0.659		-4.438*	-4.210*
	-1.52	-1.631		-2.302	-2.219
S5	-10.19*	-11.05*		1.168	1.71
	-5.741	-6.039		-2.599	-2.498
S6	-8.318**	-9.139**		-0.533	0.708
	-3.745	-4.076		-2.577	-2.483
S7	3.068	2.774		5.326**	3.871*
	-2.983	-3.083		-2.432	-2.338
S8	-6.123*	-6.795*		-1.536	-0.657
	-3.703	-3.947		-2.403	-2.307
S9	3.155*	3.187*		-5.351**	-4.761*
	-1.789	-1.862		-2.608	-2.507
S10	5.410*	5.075		6.392***	4.460*
	-3.275	-3.336		-2.476	-2.388

S11	0.121	0.369	-3.32	-3.357
	-1.376	-1.588	-2.541	-2.439
S12	9.384**	9.368**	4.158*	3.101
	-3.656	-3.711	-2.466	-2.371
S13	2.749	2.835	-1.162	-1.425
	-2.369	-2.681	-2.142	-2.061
S14	5.643**	5.411**	2.889	2.015
	-2.658	-2.739	-2.439	-2.346
S15	-13.45**	-13.90**	-6.555***	-4.356*
	-5.225	-5.456	-2.54	-2.461
S16	0.673	0.497	0.701	0.646
	-1.134	-1.291	-2.458	-2.354
S17	3.500***	3.879***	-3.351	-2.682
	-0.228	-0.203	-2.768	-2.654
S18	-0.315	-0.799	2.909	2.413
	-2.056	-2.323	-2.423	-2.324
S19	7.216**	7.127**	2.792	1.624
	-3.366	-3.476	-2.531	-2.431
S20	-9.587*	-9.706*	-4.133	-2.22
	-5.223	-5.58	-2.603	-2.507
S23	-0.551	-0.906	5.761**	4.763*
	-3.388	-3.525	-2.8	-2.687
S24	-3.825**	-4.274**	1.663	1.19
	-1.557	-1.845	-2.577	-2.477
S25	-1.759	-2.208	0.282	0.208
	-4.598	-5.072	-2.397	-2.294
S26	5.151	4.774	3.986	2.95
	-3.545	-3.722	-2.787	-2.675
S29	0.288	0.549	2.67	1.968
	-2.878	-2.972	-3.421	-3.279
_cons	8.851*	7.135		
	-4.603	-4.979		
N	870	783	2610	2349

Standard errors in parentheses

="* p<0.1 ** p<0.05 *** p<0.01"

Appendix 2.3 - Regression by Effort-Support Area (Robustness Checks)

	Contribution (interaction terms included but not reported)					
	Random leader			Skilful leader		
	(1)	(2)	(3)	(4)	(5)	(6)
	no support	strict support	support	no support	strict support	support
out-group	-5.468***	-3.780	-4.291*	-4.962**	-11.24***	-4.649***
	(-2.86)	(-1.22)	(-1.70)	(-2.21)	(-24.15)	(-2.69)
Contribution (t-1)	0.114***	0.0274	0.0691**	0.141***	-0.00578	0.0863**
	(6.01)	(0.74)	(2.33)	(6.83)	(-0.13)	(2.38)
Multiplier (t-1)	6.345***	5.944	10.87***	5.067***	10.90*	11.00**
	(3.43)	(1.21)	(2.93)	(2.85)	(1.83)	(2.12)
Period	-0.299***	-0.395***	-0.305***	-0.0738	-0.149*	-0.0290
	(-6.91)	(-5.01)	(-4.48)	(-1.51)	(-1.82)	(-0.39)
Leaders' Ability	YES	YES	YES	YES	YES	YES
Identity control	YES	YES	YES	YES	YES	YES
Session dummies	YES	YES	YES	YES	YES	YES
N	1296	432	594	1053	243	324

t statistics in parentheses
* p<0.1 ** p<0.05 *** p<0.01

Appendix 2.4 - Instructions to Experiment 1 - Chapter 2

Instructions

Welcome! Thanks for participating in our experiment. Please turn off mobile phones and any other electronic devices. These must remain turned off for the duration of this session. During the experiment, please do not talk to any of the other participants. All interaction with others will be via the computer. Feel free to ask questions at any time during the experiment. Raise your hand and a monitor will come to you.

In this experiment you will have the possibility to earn some money. The amount you earn will depend on your decision and the decisions of the other participants in your group. You will be paid in private and in cash at the end of the experiment. During the experiment we will refer to experimental points. Each experimental point is the equivalent of 0.04 Euro. Hence, 100 experimental points are equivalent to four Euros.

100 points =4 Euro

All decisions are completely anonymous and your identity will not be revealed to anyone in the experiment or outside the experiment.

Procedures

The experiment consists of three tasks. Each task has its own instructions that should be read in steps. We will indicate which page you should read when. Wait for our indication.

At the beginning of the experiment, participants are divided in 4 groups of 4 participants. Half of the groups are blue and the other half is green. The group color remains constant through the experiment.

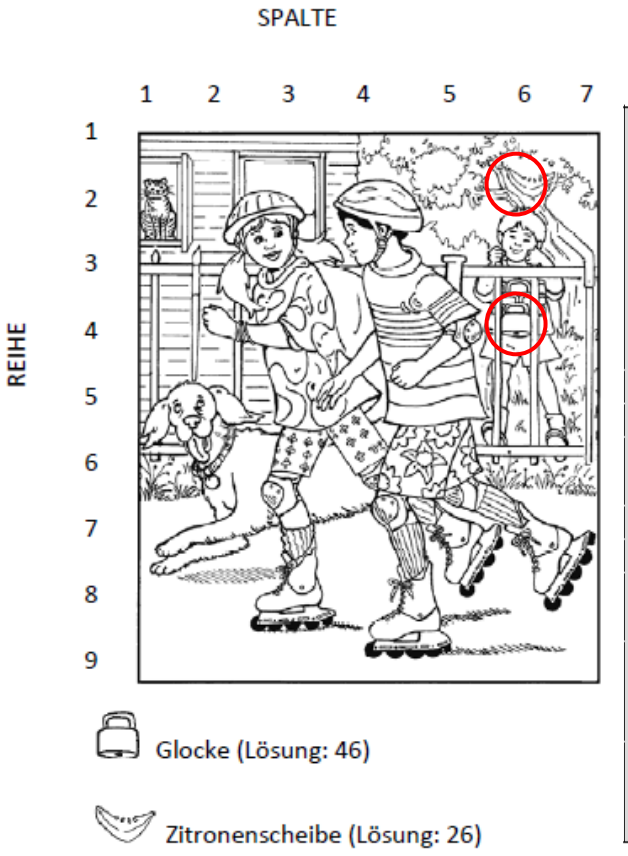
First task

The first task is a group task. **The group task is only completely solved when ALL participants of your group typed in the solution.** During the task your group participants will be able to chat with the member of your group and find together a solution. The chat box is positioned on the left side of the computer screen. You have 10 minutes to solve the task during which you can communicate with your group members. **Please be aware that you need to type all the solutions your group found and press "ok" before the 10 minutes runs out.**

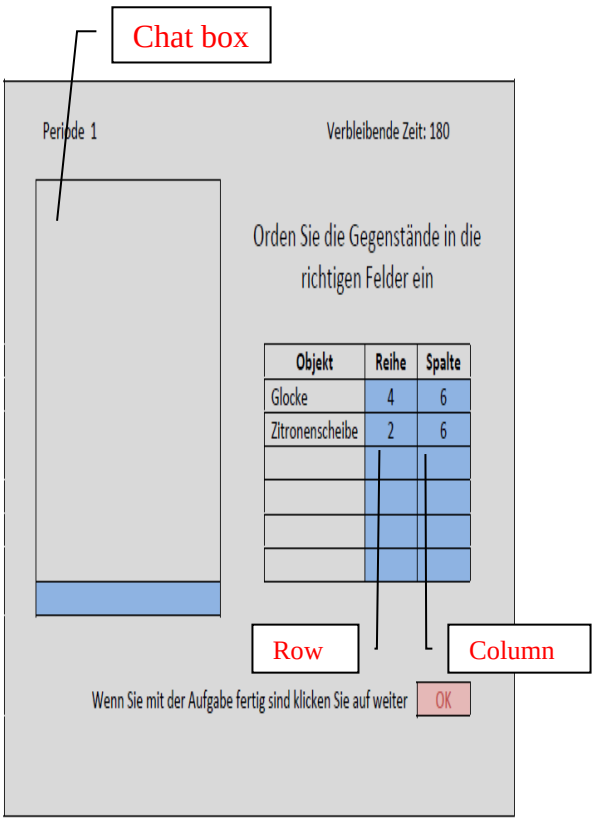
All participants are presented a figure and a list of objects. The task is to find the hidden objects in the figure. The top and left hand side of the figure contains the identification numbers of the rows (Reihe) and columns (Spalte), respectively. You need to enter this information in the boxes that appear on the right hand side of the screen.

Below a sample of the task.

Figure



Screen



The group that finds more objects during 10 minutes will be the winning group. At the end of the experiment you will know if you belonged to the winning group as a congratulations message will appear. Please click on “ok” if you are ready.

Second task

You are presented with a screen with 48 sliders. Each slider is initially positioned at 0 and can be moved as far as 100. Your task is to position the slider at exactly 50. Each slider has a number to its right showing its current position. You can use the mouse in any way you like to move each slider. You can readjust the position of each slider as many times as you wish. The number of points you have accumulated at any moment is displayed on the top of the screen. This task will be performed as a test trial and subsequently will be performed for 1 round. You will have 60 seconds to solve the task. Below an example:

a. First position b. positioned at 50

The Test trial will start now. Please click on “ok” once you are ready.

Third task

A participant is randomly selected as leader. The leader is presented with a new slider screen for 1 minute. His task is to position the slider in 50 using the mouse. The more sliders the leader positioned correctly, the higher the payment for each group member. Though, the payment of the leader is fixed. **The leader receives 25 points each round.** Only the leader knows the exact number of sliders he positions correctly**.** The leader will also have the option to solve another task based on task I (See Appendix II). Hence, this is completely independent and has no effect on the results of the experiment.

Group members who have not been selected as leaders receive 20 experimental points, which we will refer to as endowment. The task is to decide how to distribute the endowment between two accounts: a **private account** and a **group account**. For each point that you keep in your *private account* you receive **one** point back. For each point invested in the ***group account*** **you and other group members** get a payment. The payment that is received depends on the amount of sliders that the leader positions correctly as follows:

.

Table 1

Number of sliders correctly positioned by the leader	**Multiplier to each group member**
Less than 6	0.3
Between 6 and 8	0.4
Between 9 and 16	0.5
Between 17 and 20	0.6
More than 20	0.8

This stage lasts between 10 and 15 rounds, the actual ending point is randomly determined. You will be paid the total number of points that you accumulated during this part of the experiment.

Expectations

While the leader is positioning the sliders you will be asked how many sliders you expect the leader to position correctly. If you guess correctly, you will receive 4 points. For one unit difference from the actual value you will receive 1 point less. For two units difference you will receive 3 points less. And for 3 units difference you will receive no payment.

The leader will also be asked how many points he expects other participants to invest in the group account. If the leader guesses correctly, he/she will receive 4 points. For one unit difference from the actual value he/she will receive 1 point less. For two units difference he/she will receive 3 points less. And for 3 units difference you will receive no payment.

Payoff Function

Private account + Group account X Multiplier = total payment

Example

Suppose you invest 12 points in your private account and that in total 20 points were invested in the group account. If the leader positions 18 sliders correctly, then your payment is:

Private account	+	Group account	X	Multiplier	=	total payment
= 12	+	20	X	0.6		
= 12	+	12			=	24

Suppose you invest 4 Points in the private account and that in total 50 points were invested in the group account. If the leader positions 7 Sliders correctly, then your payment is:

Private account	+	Group account	X	Multiplier	=	total payment
= 4	+	50	X	0.4		
= 4	+	20			=	24

In the next screen you will need to solve understanding questions. Please click on “ok” once you are ready.

Questions

Assume that the investment in the **group account is 30 points**,

1. How many points will each group member receive from the group account if the leader positions 7 sliders correctly?	
2. Further assume that you invest 10 points in the private account. How many points would you receive from the private account?	
3. What is your total payment?	
4. What would be the leaders´ payment?	

Appendix 3.1- Instructions to Experiment 2 - Chapter 3

Instructions

Welcome! Thanks for participating in our experiment. Please turn off mobile phones and any other electronic devices. These must remain turned off for the duration of this session. During the experiment, please do not talk to any of the other participants. All interaction with others will be via the computer. Feel free to ask questions at any time during the experiment. Raise your hand and a monitor will come to you.

In this experiment you will have the possibility to earn some money. The amount you earn will depend on your decision and the decisions of the other participants in your group. You will be paid in private and in cash at the end of the experiment. During the experiment we will refer to experimental points. Each experimental point is equivalent to 0.025 Euro. Hence, 100 experimental points are equivalent to 2.5 Euros.

100 points=2.5 Euros

Besides you will receive 2 Euros corresponding to show up fee.

All decisions are completely anonymous and your identity will not be revealed to anyone in or outside the experiment.

Procedures

The experiment consists of four task. Each task has its own instructions that should be read in steps. We will indicate which page you should read and when. Please wait for our indication and read the following instructions very carefully.

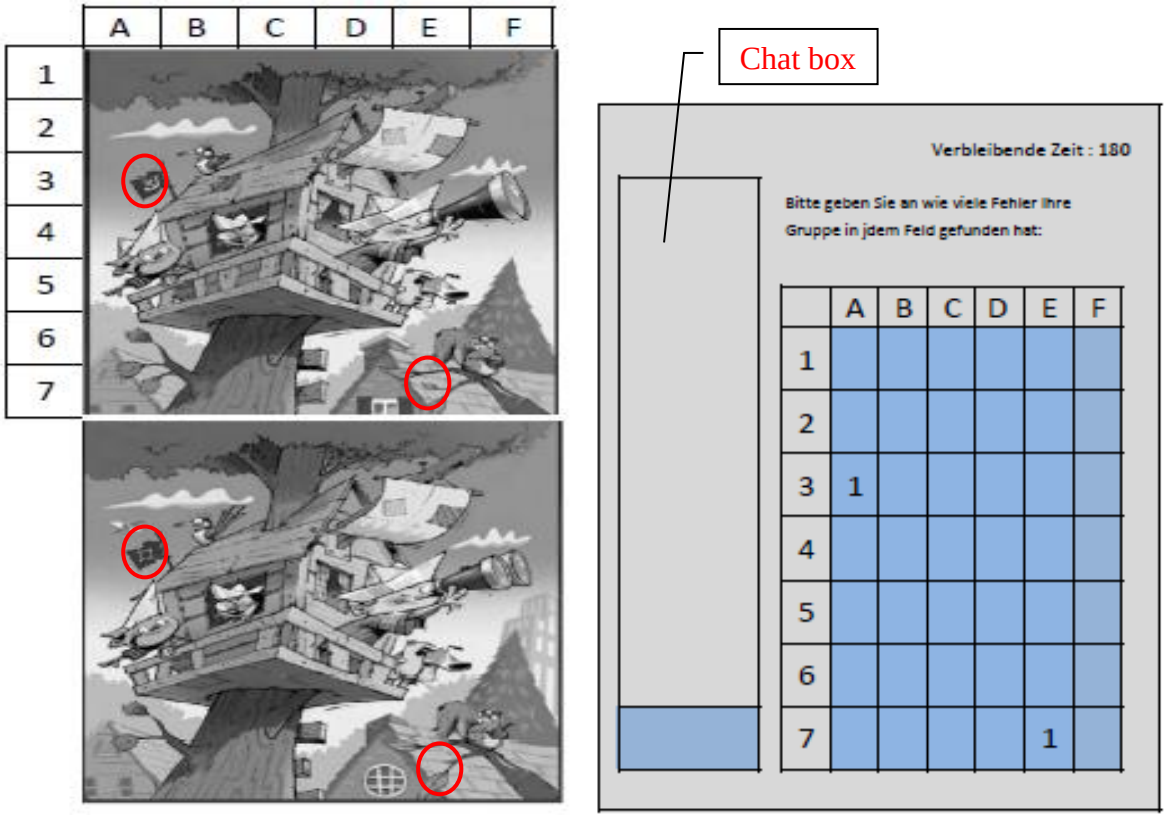
First stage

At the beginning of this task, participants are divided in groups of 3. Each group receives either a blue or a green color. The group color remains constant through the experiment.

You will receive two pictures. Your task is to find the differences between them. The group that finds more differences will be the winning group. Groups compete against groups of different color, hence participants in Green groups will be competing against participants in Blue groups. During the task, you and other participants in the group will be able to chat with each other to exchange information using a chat box that is placed on the left side of the computer screen. Messages from other members will be displaced above and you will have the chance to write on the cell below. On the top of the screen you will see how many seconds are left before the task finishes. The group that finds **together** more differences between the two figures during 10 minutes will be the winning group. At the end of the experiment you will know if you belonged to the winning group as a congratulations message will appear. **Please note that the solution would only be considered correct if all participants in the group have typed it. From each solution that was typed in wrongly, 1 point will be discounted. Please be also aware that you need to press "ok" before the 10 minutes runs out.**

The figure below presents an **example** of the figures that you will observe. The figure on the left hand side refers to the image that you will receive in print form.

Printed figure Computer screen



The rows of the printed figure are marked with numbers while the columns are marked with letters. The combination of the Letter and number indicate the location of the object. The task of your group is to find the differences between the figures and type in the number of differences that you found in each cell. For example, in the figure below you see that the flag is different in both pictures. The flag is located in column A and row 3, so, the correct answer will be 1.

On the right hand side of the computer screen you will receive a table with the same coordinates as the printed figure (see a sample on the left hand side of the figure above). Your task is to enter the number of differences that you found for a particular cell in the picture. Following the example, you should enter the number 1 in the cell corresponding to the coordinates A3.

Please click on “ok” if you are ready to start this task.

Second Stage

One participant in the group will be randomly selected as leader while the other two participants will conform the group. A message in the screen will announce your role in the group.

Similarly as in the previous exercise, you will be presented with five two digit numbers. Your task is to sum them up. You are not allowed to use any calculator that helps you solving the task. After entering the value and pressing ok 5 more numbers are displayed. You have 30 seconds to solve the task.

After solving this task, participants will receive 20 points. The task is to decide how much they want to pass to the leader. The value that they pass to the leader will multiply by **A.** The value of **A** depends on two factors: the performance of the leader in the summing task and luck. If the leader solved less than 1 calculation correctly ***A*** *would be 0.8.* However, if the leader solves more than 1 sum correctly **A** will be a random number between 0.8 and 1.9. The probability that **A** occurs is normally distributed, which means that its higher for values around the mean (1.35) and lower for values near to the tales (0.8 and 1.9).The actual value will be determined by luck. After the points passed to the leader multiply, the leader will have to decide how to distribute them. Hence the leader can decide how much he wants to pass to each player and how much he wants to keep for himself/herself. The leader will receive 20 points plus the value he/she keeps for his/her own.

This stage of the game will repeat for 7 rounds. Your payment will be the sum of points that you accumulate during this stage.

Example

1. Suppose that participant 1 pass 8 points to the leader. If the leader solves more than 1 sum correctly, and **A** is 1.5, the value generated with the points passed by participant 1 is 12. If the leader decides to take 3 points from participant 1 the payment by participant 1 would be:

Total payment = Points kept + Points Passed * A - Leaders take up

= 12 + (8 X 1.5) – 3
= 12 + 12 – 3 = 21

If participant 2 passes 12 points to the leader the value generated with the points passed by him would be 18 points. If the leader takes 2 points from participant 2, the payment to participant 2 would be:

Total payment = Points kept + Points Passed * A - Leaders take up
= 8 + (12 X 1.5) – 2
= 8 + 18 – 2 = 24

The leader receives 20 points for solving this task, plus the points he takes from participant 1 and 2. Hence, the payment to the leader would be:

Total payment = 20 + Points from 1 + Points from 2
= 20 + 3 + 2 = 25

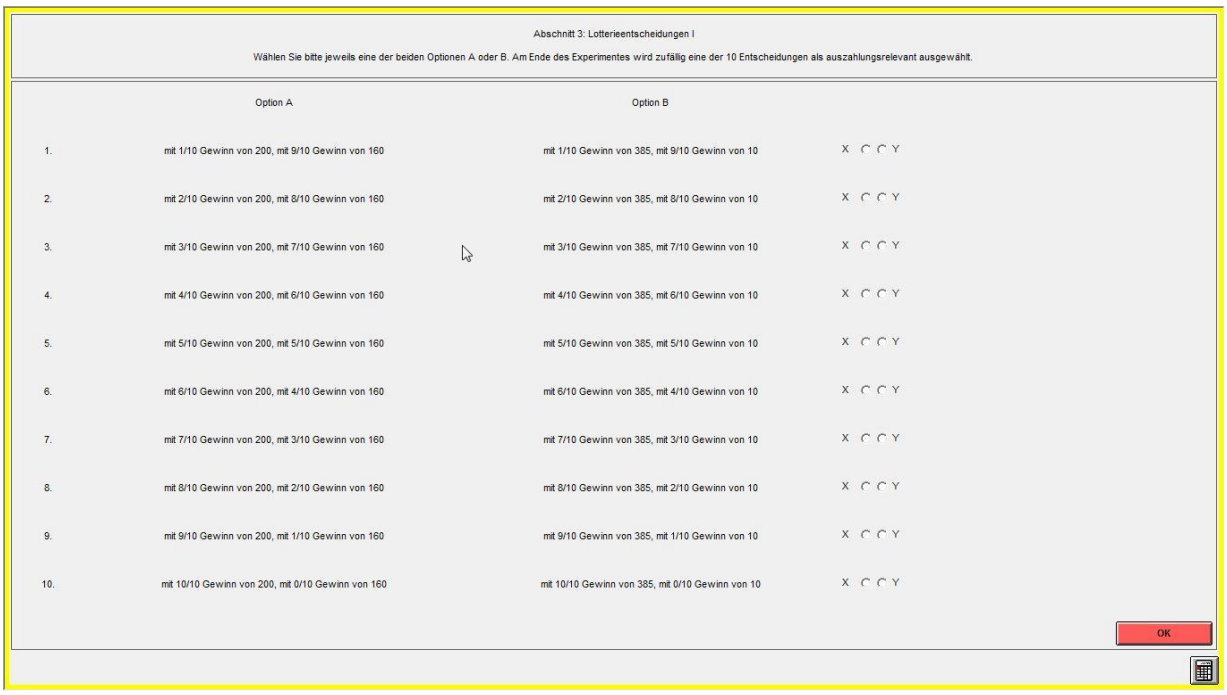
Questions

Assume you are a group member that took following investment decision: Pass 10 points to the leader and kept 10 points for you. Please answer following questions:

1. How many points would you receive from the value you kept?	
2. How many points would you receive if you pass 10 points and A is 1.5?	
3. How much would you receive from the value you passed if the leader takes 3 points from you?	
4. What is your total payment?	
5. What would be the leaders´ payment if he takes 5 points from participant 2?	

Third Stage

In this part of the experiment you are presented with 10 decisions. Each decision presents two options: "Option A" and "Option B." Your task is to indicate in the final column your preferred option. While you will make ten choices and record these in the final column, one of them will be used in the end to determine your earnings. After you have made all of your choices, we will randomly select one of the decisions for payment. Each decision has an equal chance of being used in the end. Even though you will make ten decisions, only one of these will end up affecting your earnings, but you will not know in advance which decision will be used.



Now, please look at Decision 1 at the top. Option A pays 200 points one out of 10 times (1/10), and it pays 160 points nine out of 10 times (9/10). Option B yields 385 points one out of 10 times (1/10), and it pays 10 points nine out of 10 times (9/10). To determine your payment, a random number between 1 to 10 will be selected. In this example, if number 1 is selected you will received either 200 points or 385 points depending on your decision. If a number between 2 and 10 is selected you will receive 160 points or 10 points depending on your decision.

XXX

The other Decisions are similar, except that as you move down the table, the chances of the higher payoff for each option increase. In fact, for Decision 10 in the bottom row, the die will not be needed since each option pays the highest payoff for sure, so your choice here is between 200 pennies or 385 pennies.

To summarize, you will make ten choices: for each decision row you will have to choose between Option A and Option B. You may choose A for some decision rows and B for other

rows, and you may change your decisions and make them in any order. The computer will select a two random numbers. One will determine the decision to pay and the other earnings for the Option you chose for that Decision.