Essays in Empirical Public Economics

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Chapter 1

General Introduction

This dissertation consists of three self-containted chapters which contribute to different strands of the literature in empiricial public economics and experimental economics. In Chapter 2 (co-authored with Jana Friedrichsen), we investigate the relationship between macroeconomic conditions and political support for democracy in Western European countries. In Chapter 3 (co-authored with Paolo Masella and Stephan Meier), we analyze in a simplified Gift Exchange Game experiment whether and how group membership influences the effectiveness of control mechanisms and under which conditions control can have a detrimental effect on agents' prosocial behavior. Finally, in Chapter 4 (co-authored with Evguenia Winschel), we test within an experimental setting whether asymmetric information influences prosocial behavior in a variant of the Dictator Game.

Additional material for each chapter which is not included in the main text, such as data descriptions, descriptive statistics, experimental instructions, and additional results, can be found in the appendix of this dissertation. The bibliography at the end of this thesis contains references from all three chapters.

1.1 Chapter 2: Political Support in Hard Times: Do People Care about National Welfare?

Over the last five years, the economies of Southern Europe have experienced a deep downturn. With rising unemployment rates, negative GDP growth rates, and government debt crises, governments and other democratic institutions have been under pressure to cope with the economic problems while sustaining the support among the population. At the same time, in these countries governments have been voted out of office. Moreover, peoples' attitudes towards the way the democratic system works have worsened substantially in these countries as suggested by survey data from the Eurobarometer.

In this chapter, we provide evidence that the macroeconomic conditions during the last years can, to a large extent, explain the observed deterioration of satisfaction with democracy.

Our analysis uses individual-level survey data on satisfaction with democracy and country-level data on growth, inflation, and unemployment from 1976 to 2010 for sixteen Western European countries. In contrast to national-level analyses, our approach of combining individual level and aggregate data has several advantages. First, both macroeconomic variables and personal controls are simultaneously influential and reveal important factors at the individual level. In particular, individual unemployment, education, age, and perceived life satisfaction are significant correlates. Furthermore, our results provide suggestive evidence against a pure self-interest explanation of political support: When analyzing the effect of macroeconomic variables on subgroups of the population (e.g., unemployed versus employed individuals), growth and unemployment rates exhibit homogeneous effects on satisfaction with democracy even though their real implications should differ across subgroups of the population.

1.2 Chapter 3: Control and Group Identity

A central element of agency theory are incentives and control mechanisms. They represent the most important tools for principals to make selfish agents act according to the principals' ends. However, there is substantial empirical evidence that incentives and control can have a detrimental effect, that they "crowd out" agents' effort (Frey and Jegen, 2001). So far, it is not well understood under which conditions this effect can occur.

In this chapter, we analyze whether group identity can potentially influence the efficacy of control mechanisms. Our main question is whether it affects the extent of crowding out.

We test the impact of group identity on the effectiveness of a control mechanism in a laboratory experiment in which all participants belong to one of two different artificially created groups. Principals have to decide whether or not they want to control their agent by setting a minimum transfer level. Agents then decide on tranfers. The results show that hidden costs of control are as strong in between-group matchings as in within-group matchings. But while the overall effectiveness of control does not depend on the social distance between principal and agent through joint group identity, the mechanisms for how control is perceived are group-specific.

In within-group interactions control has a negative effect because agents expect the principal not to control. When principals control nevertheless, agents reduce their transfers significantly as a reaction. If the agent and the principal do not share the same group identity, however, the mechanism is different: Keeping agents' beliefs about the principal's behavior constant, agents perceive control as more hostile in a between-group matching. In the end, in both between- and in within-group matchings the implementation of a control mechanism has costs but for different reasons.

1.3 Chapter 4: Efficiency Concern under Asymmetric Information

Over the last decades, economic experiments have produced evidence that people regularly deviate from a purely selfish maximization of their own payoffs (Camerer, 2003). In most experiments, a substantial share of subjects is willing to accept a cost in order to increase the payoff of other subjects. In light of that, alternatives to the classical model have been proposed. One prominent example are social preference theories (e.g., Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000; Charness and Rabin, 2002) which specify preferences as not only including own payoffs but also payoffs of others.

These theories are relevant for many policy issues. When facing the decision whether to implement a certain policy proposal, an individual's support for the proposal may be influenced by its effect on other individuals. The insights of social preference theories can help to understand when proposals are supported and can enhance the design of reforms that succeed. The empirical evidence for social preferences, however, has been established within a relatively narrow experimental setting. In particular, in most of the experiments subjects have complete information about their payoffs and other subjects' payoffs.

In this chapter, we relax the assumption of complete information and test whether asymmetric information affects prosocial behavior in a laboratory experiment when subjects exhibit social preferences. In our experiment, a dictator decides whether to make a costly transfer to a recipient; conditional on the transfer the recipient then obtains a benefit which is larger than the dictator's costs. As main treatment we vary the information the dictator has available. Under symmetric information he is perfectly informed about the benefits whereas under asymmetric information he only knows the distribution from which the benefit is drawn while the recipient is fully informed.

Furthermore, to test whether the reward leads to crowding out in our setting, we introduce a monetary reward for prosocial behavior as a second treatment variable. Under asymmetric information the incentive may interact with the uncertainty. Subjects may "perceive" the reward level as being related to the unknown benefit level and interpret the reward as a signal (Bolle and Otto, 2010) and may behave less prosocially.

Our results indicate that under symmetric information a substantial share of individuals behaves prosocially. Moreover, individual behavior is consistent with social preference theories. Most subjects who transfer behave according to efficiency concern (Charness and Rabin, 2002), but we also observe subjects whose behavior is in line with inequality aversion (Fehr and Schmidt, 1999), respectively maximin preferences (Charness and Rabin, 2002).

We find that the predictions of social preference theories in the asymmetric information case describe actual behavior quite accurately. In particular, as predicted by these theories, subjects do not react negatively to asymmetric information and do not behave less prosocially. Under asymmetric information, when dictators do not know the exact value of the benefit, more subjects make a transfer than for any benefit level under symmetric information. Hence, in our experiment the information asymmetry improves efficiency. On average, recipients get a higher transfer when the dictator decides without knowing the exact benefit level. Lastly, the monetary reward has a mostly positive though not statistically significant effect and does not result in crowding out in our setting.

Chapter 2

Political Support in Hard Times: Do People Care about National Welfare?

2.1 Introduction

During the Great Recession since 2007 European countries such as Spain, Greece, Portugal, and Ireland have experienced a phase of economic hardship unprecedented in the last decades. In Spain, for instance, the unemployment rate increased by 11.4 percentage points between 2006 and 2010. Following the economic downturn were the political repercussions: Mass demonstrations took place in many cities as people wanted to express their dissatisfaction with the economic situation and how it was dealt with[1]. Until late 2011 the five EU member countries which were hit hardest economically, Greece, Ireland, Italy, Portugal, and Spain, had overturned their governments. Political actors as well as observers noted that democratic institutions themselves could suffer under adverse economic conditions. For instance, in summer 2010, the president of the European Commission, José Manuel Barroso, expressed his fear that "democracy might disappear" (Groves, 2010) in the most heavily affected Southern European countries; macroeconomic conditions could worsen to an extent that would be impossible to deal with for governments and would therefore make them susceptible to popular uprisings (Groves, 2010). Survey data from the Eurobarometer, using 'satisfaction with democracy' (SWD) as a measure of attitudes towards democracy, shows indeed

[1] Examples of the broad media coverage of the protests are Donadio and Sayare (2011) and Tremlett and Hooper (2011).

that in the phase of economic downturn peoples' attitudes have worsened substantially. In Spain, for instance, satisfaction with democracy decreased by about 20 percentage points between 2006 and 2010.

In this chapter, we show that the economic harshness during the last years can, to a large extent, explain the observed deterioration of satisfaction with democracy. Moreover, growing dissatisfaction reflects a pattern already present before 2007, a positive relationship between economic performance and satisfaction with democracy. Combining individual-level survey data on SWD with country-level data on growth, inflation, and unemployment from 1976 to 2010 for sixteen Western European countries, we find that national economic performance does affect individuals' attitudes towards democracy and the effects are non-negligible in size. Using estimation results from data collected before 2007, a drop in the order of 19 to 24 percentage points in satisfaction with democracy was to be expected for countries which experienced substantially lower growth and higher unemployment rates than during normal times. These estimates compare well with the decreases of around 20 percentage points measured for Ireland, Greece, and Spain. We also correctly predict Portugal to be an outlier; based on economic data we estimate a decrease of 5.9 percentage points in satisfaction scores while the observed decrease was 4 percentage points.

While we find both growth and unemployment rates to be significant, the latter are quantitatively much more important for SWD. When the growth rate decreases by one standard deviation, SWD is on average 3 percentage points lower; a standard deviation increase in unemployment, however, comes about with a decrease of 7 percentage points. This finding illustrates why "jobless growth" as a policy outcome is problematic and why politicians might want to focus on employment policies even though growth is also important to ensure citizens' support.

The contribution to the literature is fourfold. First, the time frame chosen allows us to show the regularity behind political protest in times of economic crisis: People get less satisfied with a democratic system during economic slowdown. The effects on political support were stronger in recent years because the economic turmoil was more severe than ever before. Secondly, we show that macroeconomic variables and personal controls are simultaneously influential and we assess their relative importance. Resorting to individual level data uncovers important drivers of satisfaction with democracy, which remain undetected in national-level analyses. In particular, individual unemployment, education, age, and perceived life satisfaction are significant correlates. Thirdly, we present evidence against a pure self-interest explanation of political support: Growth and unemployment rates exhibit homogeneous effects on SWD even though their real implications differ across subgroups of the population.[2] Finally, since we include several macroeconomic indicators at the same time, we can also assess the robustness of previous work relying on subsets of these indicators.

In Section 2.2 we relate our research to the existing literature. In Section 2.3 we summarize our hypotheses (2.3.1), describe the dataset (2.3.2), and introduce our empirical model (2.3.3). We present our results in Section 2.4 and discuss implications with respect to a self-interest explanation of political support and a policy trade-off between inflation and unemployment (Phillips curve) in Section 2.5. We present robustness checks in Section 2.6 and conclude in Section 2.7.

2.2 Related Literature

Satisfaction with democracy is part of the broader concept of political support. Support can be simply affectional (acceptance or identification with an entity) or it can derive from satisfaction with its outputs (Easton, 1957). Norris (1999b) distinguishes the following five dimensions: political community, regime principles, regime performance, regime institutions, political actors. Research on political support (see, e.g., Norris (1999a) for an introduction) often focuses on government popularity as a dependent variable and thus refers to the most specific dimension: 'political actors'. However, during severe economic crises more than the competence of current governments is called into question. SWD is then a more suitable indicator of political support since it gives us an evaluation of the 'system's performance'.[3] SWD has the additional advantage of being less influenced by personal sympathy for politicians or ideological attachment to a specific party.

There is some evidence that voters evaluate macroeconomic outcomes retrospec-

[2] Inflation in contrast exhibits heterogeneous effects but is insignificant for parts of the population.

[3] See also Dalton (1999) who argues that SWD gives an instrumental evaluation of the performance of democracy.

tively and vote accordingly in subsequent elections, but also prospective voting has been proposed as an explanation and received some empirical support. Since this literature is very broad, we refer the interested reader to the surveys on vote and popularity functions by Nannestad and Paldam (1994) and "Voting and the Macroeconomy" by Hibbs (2006). Revolutionary action or political extremism are likely to indicate the absence of political support and constitute another facet of the related literature. Brückner and Grüner (2010) find a negative relationship between growth and right-wing extremist voting at the aggregate level for 16 Western European countries. Moving to the micro-level, Lubbers et al. (2002) show how support of extreme right-wing parties increases with unemployment for the same set of countries. MacCulloch and Pezzini (2007) employ survey data from 64 countries and provide evidence that the preference for revolution increases when the economy performs poorly.[4]

Previous work employing the same indicator as we do, SWD, often uses data aggregated at the national level or covers relatively short time periods. Results thereby rely to a large extent on cross-country variation and individual characteristics are ignored.[5] Furthermore, there is hardly any systematic evidence on the role of macroeconomic factors. Using national-level data, Wagner et al. (2009) find significant effects of institutional quality on the satisfaction with democracy and Clarke et al. (1993) document effects of inflation and unemployment. We are aware of only two studies in SWD employing individual-level data: Halla et al. (2011) investigate the role of environmental policy for individuals' satisfaction with democracy, while Wells and Krieckhaus (2006) study the effect of corruption on democratic satisfaction. The latter study uses only few points in time and cannot properly take into account changes in national economic conditions over time.[6] To the best of our knowledge only Halla et al. (2011) use a long time dimension combined with individual level data, but their

[4]Both Brückner and Grüner (2010) and Lubbers et al. (2002) use data from the Eurobarometer as we do. While the latter only rely on few data points in time, the former use the Mannheim trend file covering 1970 to 2002. In contrast, MacCulloch and Pezzini (2007) employ three waves of the World Value Survey for their analysis.

[5]Satisfaction with democracy is an individual attitude and depends on individual characteristics, which are therefore crucial in the analysis of determinants of democratic support. While aggregate-level analyses can, in principle, incorporate individual characteristics as averages, this is not usually done, but the individual dimension is left out completely. In contrast, we explicitly take into account the individual level information which the Eurobarometer provides.

[6]All of these studies rely on data from the Eurobarometer for Western European countries. Wells and Krieckhaus (2006) also consider Central and Eastern European countries.

data ends in 2001 and thus excludes the recent years. Wagner et al. (2009) and Halla et al. (2011) do include several macro-economic indicators simultaneously but do not discuss the economic relevance and relation between those.[7]

In this chapter, we build on these studies and extend them in several dimensions. We compile a dataset covering 16 Western European countries for the period from 1976 to 2010. We thereby extend the sample used by Halla et al. (2011) by another decade. We also use variation in country-specific economic conditions over time in addition to cross-country variation. The use of individual-level data with a long time dimension allows controlling for important factors at the individual level such as sex, age, and labor force status; we abstract from cultural differences in political attitudes by using country-fixed effects. We show how important individual characteristics are in determining democratic satisfaction and relate our results to findings from aggregate level studies. Furthermore, we discuss the role of various macroeconomic factors and show how previous findings depend on the selection of only a subset of them.

2.3 Hypotheses, Data, and Model Specification

2.3.1 Hypotheses

Earlier research posited a link from macroeconomic performance to political support based on the presumption that "voters hold the government responsible for economic events" (Lewis-Beck and Paldam, 2000, Responsibility Hypothesis) without detailing the channels of influence. A plausible mechanism, which we believe also applies to satisfaction with democracy, is the following: Economic conditions determine future well-being. Growth increases expected income, inflation reduces the real value of wealth and income, and higher unemployment implies higher risk of job or income loss. Therefore, individuals value, e.g., high growth as an indicator of increasing national welfare and high inflation and high unemployment as signs of decreasing welfare. Going beyond the theory of pure self-interest, individuals may also care about the well-being of others. Macroeconomic performance illustrates the democratic system's capacity to provide collective well-being. This constitutes another reason for economic performance

[7]The former study by Wagner et al. (2009) uses the average of the ordinal SWD score as dependent variable. Due to the ordinality of SWD it is problematic to interpret their results quantitatively.

to increase individuals' satisfaction with democracy.

Based on the preceding argument we expect that, ceteris paribus, an individual's democratic satisfaction is

- increasing in national growth,
- decreasing in inflation and unemployment.

Furthermore, we expect that individual income and employment status have similar effects. We hypothesize that an individual's democratic satisfaction is

- increasing in individual income,
- lower in case of personal unemployment.

Moreover, we expect a strong positive correlation between general life satisfaction and satisfaction with democracy. We believe individuals do not perfectly discriminate between their personal lives and their political surroundings when asked for their subjective evaluations. For instance, individuals who are generally optimistic and very happy, should on average also be more positive towards democracy. Thus, we expect an individual's democratic satisfaction to be

- increasing in general life satisfaction.

More generally, we expect that democratic satisfaction has similar determinants as has life satisfaction. We therefore adopt hypotheses from the happiness literature.[8] We hypothesize that an individual's democratic satisfaction is higher if he or she is married, better educated, and out of the labor force and that men are less satisfied.

2.3.2 Data

Our analysis combines survey data with national macroeconomic data in 16 countries for up to 33 years. Individual level data was obtained from the Eurobarometer and macroeconomic data from the OECD (2011). Descriptive statistics for all included national and individual variables are displayed in Tables A.2 and A.3 in Appendix A.1.

[8]For the macroeconomic variables the happiness literature supports the hypotheses stated above. See for instance Frey and Stutzer (2002a).

The tables show variation in the dependent variable 'SWD' as well as in the explanatory variables 'growth', inflation', and 'unemployment' within countries over time. Figure A.1 in Appendix A.1 illustrates that SWD varies over time. Furthermore, it reveals that there are substantial differences in levels of SWD across countries possibly due to cultural idiosyncrasies. Exact variable definitions can be found in Table A.1 in Appendix A.1.

Individual Level Variables: the Eurobarometer

The Eurobarometer data set is a repeated cross section of individuals in the European Union (EU). It covers five of the six founding EU members in 1970 (France, Belgium, Netherlands, Germany, Italy) since 1970, Luxembourg is included since 1973, and other countries were added when they joined the European Union, respectively when official negotiations for accession began. In every wave, about 1000 respondents per country complete the questionnaires. We use the Mannheim Eurobarometer Trend File 1970-2002 (European Commission, Brussels, 2008) and append nine additional waves to extend the dataset until 2010 (European Commission, Brussels, 2002, 2003, 2004a,b, 2006, 2007, 2009, 2010).

As indicator of support for democracy we used 'satisfaction with democracy' or SWD. This indicator refers to the following question: 'On the whole, are you very satisfied, fairly satisfied, not very satisfied or not at all satisfied with the way democracy works in <country>?'.[9] This question asks, in line with our research interest, how people evaluate their democracy's current performance and not whether they are in favor of the democratic idea per se.

The variable SWD was collected for the first time in 1973 and then every year from 1976 to 2010 except for the years 1996 and 2008. Our sample comprises France, Belgium, The Netherlands, Germany (since 1991 including East Germany), Italy, Luxembourg, Denmark, Ireland, the United Kingdom, Greece (included since 1981), Spain and Portugal (both included since 1985), Norway (included 1990-1995), Finland (included since 1993), Sweden and Austria (both included since 1995).

From the Eurobarometer we also obtain standard demographic controls as well as

[9] <country> is replaced by the name of the country in which the respondent was interviewed.

information on general life satisfaction. In contrast to the other controls, the latter is not an objective measure but an attitudinal statement: People were asked how satisfied they are with their lives.[10]

National Level Variables

Macroeconomic data was obtained from the OECD. Total GDP (constant prices), GDP per capita, inflation rates, and unemployment rates were downloaded from the OECD database OECD.StatExtracts, which is available online. We transform GDP per head to GDP per head in 1000 US$ (constant prices, constant PPPs), for ease of interpretation of coefficients. Since the distribution of inflation is very skewed, we would like to use a log-transformation as, e.g., Wagner et al. (2009) do, but a log-transformation is only feasible for positive observations. Around 2009 and 2010, however, Belgium, Ireland, and Portugal experienced negative inflation rates. In order not to lose these observations, we adopt a hybrid function of inflation as proposed by Khan and Senhadji (2001):

$$f(\text{inflation}_{it}) = (\text{inflation}_{it} - 1)\mathbf{1}_{\text{inflation}_{it} \leq 1} + \log(\text{inflation}_{it})\mathbf{1}_{\text{inflation}_{it} > 1} \tag{2.1}$$

The function $f(\text{inflation}_{it})$ is linear in inflation_{it} for values of inflation rates below or equal to one and logarithmic for inflation rates greater than one. The breakpoint one is chosen such that the transformation is continuous.

For robustness checks we also employed "The Comparative Political Data Set 1960-2007" by Armingeon et al. (2009). It contains political and institutional variables on a (mostly) annual basis for 23 democratic countries for the period of 1960 to 2007. From this dataset we extracted information on national budget deficits, national government debt, and the share of social transfers.

[10] Analogously to satisfaction with democracy there are four answer categories: 1=not at all satisfied, 2=not very satisfied, 3=fairly satisfied, 4=very satisfied. We constructed dummies, where 'not at all satisfied' represents category 1, 'satislife2' category 2 etc. The omitted category is 3, people indicating to be fairly satisfied with their life.

2.3.3 Model Setup and Specification

Our model employs data at the individual level instead of country averages. This allows us to include individual level characteristics. We estimate a linear probability model using the following equation:

$$\mathrm{SWD}_{itc} \quad = \quad \beta_0 + \mathrm{macro}_{tc}\beta_1 + \mathrm{individual}_{itc}\beta_2 + \mathrm{fe}_t + \mathrm{fe}_c + u_{itc} \qquad (2.2)$$

where observations are indexed by i for individuals, by c for the country in which the individual participated in the survey, and by t for the year of the survey. The dependent variable 'SWD' as well as individual controls vary at the individual level nested in years and countries, indexed by itc. Macro controls only vary at the year-country level, indexed by tc. All estimations include country fixed effects fe_c as well as survey year fixed effects fe_t, and we correct standard errors for clustering at the country level.

We estimate different specifications of equation (2.2). All have individual satisfaction with democracy as dependent variable on the left hand side but, on the right hand side, we varied which variables we included in the vectors 'macro' and 'individual'. This will be discussed in the context of the results in Section 2.4.

SWD is a dummy derived from the question how satisfied an individual is with the way democracy works in his or her country. It collapses answers 'very satisfied' and 'fairly satisfied' into 'satisfied' (SWD=1) and answers 'not very satisfied' and 'not at all satisfied' into 'not satisfied' (SWD=0). We use this binary recode since it is less susceptible to noise. In our opinion this outweighs the loss in information on the strength of individuals' democratic support.

Models with binary dependent variables are often estimated as nonlinear models such as logit or probit, which explicitly take the domain restriction into account. Instead we present results from a linear probability model, i.e., from OLS estimation of equation 2.2 as is suggested by Angrist and Pischke (2009). We also estimated a logit model and expectedly find very similar results; in case of differences we find that our model choice goes against finding significant effects. Results from the logit model are available in Appendix A.2 (Tables A.5 and A.6).[11]

[11] We also estimated an ordered logit model using the original 4-point scale of SWD, which confirms

2.4 Results

An advantage of our approach over estimations based on aggregates is that we analyze the role of both individual and national variables. Individual unemployment, education, income, and age are likely to be relevant for SWD and are not captured in aggregates. Neglecting individual variables therefore means neglecting potentially important driving factors of SWD and their interaction with aggregate factors. We also discuss whether, in addition to their personal economic situation, people also take the national performance into account when evaluating the political system.[12] We first address the impact of the macroeconomic variables (Section 2.4.1) and then the effects of individual level variables (Section 2.4.2). We also elaborate on the role of personal life satisfaction. Thereafter, we present estimations at the aggregate level.

2.4.1 Macroeconomic Variables

We included different macroeconomic indicators successively in addition to individual characteristics to shed light on the relative importance of each of them. Since a large literature on the relationship between democracy and economy focusses on GDP (e.g Acemoglu and Robinson, 2006; Przeworski, ed, 2000), we use GDP per head as starting point. Our main interest, however, lies in growth, inflation, and unemployment which vary substantially over time and have been proved influential in previous studies on SWD (Wagner et al., 2009) and right-wing extremism (Knigge, 1998; Brückner and Grüner, 2010). Furthermore, these variables are more responsive to economic policy in the short to medium run and are more likely to be targeted by policy makers. The following results are summarized in Table 2.1.

We find that economic growth is always statistically significant and so is national unemployment. The sign of the coefficients is as expected positive in case of growth and negative for the unemployment rate. Per capita income and inflation do not gain significance. Without other macroeconomic controls except for per capita GDP one

the results from the binary case which we discuss in Section 2.4. Marginal effects are strongest for the outcome 'fairly satisfied'. See Appendix A.2, Table A.7.

[12] The significance of macroeconomic variables does not necessarily imply a collectivist motive. Macroeceonomic variables may be solely important because they affect beliefs and expectations about individual well-being. See discussion in Section 2.5.2.

Table 2.1: Impact of Macroeconomic and Individual Level Variables on SWD (Individual Data)

dependent: SWD	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
macroeconomic variables								
GDP per head	0.0066	0.0054	0.0056	0.0010	0.0011	0.0010	0.0045	0.0045
	(0.004)	(0.004)	(0.004)	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)
growth		0.0131***	0.0131***	0.0105***	0.0116***	0.0106***	0.0060*	0.0060*
		(0.004)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
f(inflation)			-0.0054	-0.0188	-0.0200	-0.0218	-0.0324*	-0.0326*
			(0.016)	(0.012)	(0.012)	(0.013)	(0.019)	(0.019)
UE rate				-0.0172***	-0.0187***	-0.0194***	-0.0170***	-0.0171***
				(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
individual variables								
unemployed	-0.0512***	-0.0509***	-0.0511***	-0.0470***	-0.1122***	-0.1086***	-0.0399***	-0.0352***
	(0.006)	(0.006)	(0.006)	(0.005)	(0.009)	(0.008)	(0.007)	(0.007)
out of LF	-0.0018	-0.0019	-0.0020	-0.0015	-0.0053	-0.0042	-0.0013	0.0019
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.004)
married	-0.0000	0.0000	0.0000	0.0007	0.0266***	0.0257***	-0.0002	-0.0039
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.006)
male	0.0069*	0.0068*	0.0068*	0.0067*	0.0031	0.0033	0.0061	0.0061
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
age	-0.0023***	-0.0023***	-0.0023***	-0.0025***	-0.0052***	-0.0049***	-0.0025***	-0.0026***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
age^2	0.0000***	0.0000***	0.0000***	0.0000***	0.0001***	0.0001***	0.0000***	0.0000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
intermediate education	0.0092	0.0095	0.0095	0.0089	0.0219**	0.0222***	0.0073	0.0042
	(0.007)	(0.007)	(0.007)	(0.007)	(0.008)	(0.007)	(0.009)	(0.008)
higher education	0.0273*	0.0278*	0.0278*	0.0266*	0.0511***	0.0512***	0.0161	0.0098
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)	(0.016)	(0.015)
still studying	0.0288*	0.0296*	0.0295*	0.0282*	0.0597***	0.0602***	0.0156	0.0093
	(0.014)	(0.014)	(0.014)	(0.014)	(0.016)	(0.016)	(0.017)	(0.015)
not at all satisfied	-0.3419***	-0.3402***	-0.3401***	-0.3371***			-0.3536***	-0.3511***
	(0.025)	(0.024)	(0.024)	(0.023)			(0.028)	(0.029)
not very satisfied	-0.2484***	-0.2479***	-0.2479***	-0.2456***			-0.2645***	-0.2626***
	(0.017)	(0.017)	(0.017)	(0.016)			(0.020)	(0.020)
very satisfied	0.0753***	0.0750***	0.0751***	0.0745***			0.0805***	0.0791***
	(0.005)	(0.005)	(0.005)	(0.005)			(0.006)	(0.005)
poor								-0.0097
								(0.006)
rich								0.0142**
								(0.005)
survey FE	yes	yes	yes	yes	yes	yes	yes	yes
nation FE	yes	yes	yes	yes	yes	yes	yes	yes
N	606504	606504	606504	602545	602545	660546	353132	353132
R^2	0.1373	0.1388	0.1388	0.1433	0.0961	0.0968	0.1501	0.1504

* $p<0.10$, ** $p<0.05$, *** $p<0.01$
Dependent variable is a dummy.
Standard errors are corrected for clustering at nation level.
(4) is the reference for robustness checks. In (5) we restrict attention to the subsample were life satisfaction is available but do not include it. In (6) we exclude life satisfaction from the estimation. (7) is estimated on the reduced sample where income is available, (8) controls for income groups.
The chosen order of inclusion of macroeconomic variables is irrelevant for our results (see Table A.4 in Appendix A.2).

percentage point higher growth comes on average with a 1.3 percentage points higher probability of satisfaction (Column 2). When all three macroeconomic variables are included, growth obtains a smaller coefficient than before but remains significant at the 1% level (Column 4).[13] An increase by one percentage point in the unemployment rate comes on average with a decrease of 1.7 percentage points in satisfaction with democracy.

When we interpret the coefficients with respect to variation in the explanatory variable, we find that unemployment is much more important than growth is. A one standard deviation increase above the mean in growth rates implies an increase in SWD of about 3 percentage points. An unemployment rate of one standard deviation above the mean comes with a decrease of more than 7 percentage points in SWD, more than twice as much.

When we compare our results to Halla et al. (2011), we observe important differences.[14] While they also report a significant and positive effect of growth, they find a significant effect of inflation and GDP, two variables which are insignificant in our study. From Table 2.1 it is clear that the difference cannot be due to their omitting unemployment rates. Even if we omit unemployment rates, inflation does not gain significance. Since we use a binary recode of SWD while Halla et al. (2011) use the four-point scale, we suspected the differences result from this modeling choice. In Section 2.6.2 we therefore also discuss an ordered logit estimation. The results indicate that differences to Halla et al. (2011) do not stem from using a binary model. In 2009, many European countries have experienced deflationary episodes. We strongly suspect that this is driving the differences in results. While we do not find a significantly negative effect of inflation on SWD if we include the recent years, we do find a significant effect for the period before 2009 (see Table A.8, Column 2 in Appendix A.2).

[13]The reduction in coefficient size is intuitive as unemployment and inflation are both negatively correlated with growth in our dataset such that the coefficient on growth is upward biased if we omit those. Still, the significance indicates that growth has an influence on attitudes in addition to what was captured by inflation and unemployment. One explanation is that growth proxies for expectations of income, inflation, and employment in the future. We discuss this hypothesis in more detail in Section 2.6.1.

[14]Since we are not interested in environmental policy measures, we compare our results to the findings without environmental policy. It seems noteworthy, though, that most variables at the macro level become insignificant once Halla et al. (2011) include environmental policy measures.

2.4.2 Individual Characteristics

At the individual level we included dummies for being unemployed and not being part of the labor force, as well as education, sex, age, and marital status.[15] We expected these variables to exhibit a similar relationship with SWD as they have with general life satisfaction. We also controlled for personal life satisfaction. The inclusion of individual characteristics shows that they in fact matter and are important to be taken into account when we want to assess the implications of macroeconomic factors on satisfaction with democracy.

In line with the hypotheses, individual unemployment, education, and age are significant and have the expected signs. People being unemployed showed a 4.7 percentage points lower probability of being satisfied with democracy (Table 2.1, Column 4). It is evident that individuals' views on the democratic system were affected by the national labor market as well as the individual situation at the same time. National unemployment rates are an important factor beyond individual unemployment and vice versa. Education was included in dummy categories. The results indicate that those with higher education (*higher education*, finished school at the age of 20 or later) and those still studying (*still studying*) evaluate democracy more favorably than those with only basic or no full-time education at all (omitted category). The influence of age is u-shaped. Older people were less satisfied with democracy but the relationship reverses at some point in life. In contrast to the expected negative sign, the male dummy obtained a significant, positive coefficient. Those who were out of the labor force did not evaluate democracy significantly differently than those who were employed. Marital status did not reach significance either.

As expected, life satisfaction is strongly positively correlated with SWD. Being not at all satisfied with one's life translated into a probability of not being satisfied with democracy that is 33 percentage points higher than for a person who was fairly satisfied with her life. Those who stated to be 'not very satisfied' with their life in general were still less likely to be satisfied with democracy (-25 percentage points) and those who were very satisfied with their life had a 7.5 percentage point higher probability to also

[15] Income is not available for recent years and when it is available introduces a strong selection effect. We therefore do not include it in our benchmark model but discuss reasons and consequences of this decision under robustness (Section 2.6).

express satisfaction with the way democracy works. This indicates a close link between the perceived personal situation and the view on the democratic system.

Life satisfaction as well as SWD are subjective measures and we are aware of concerns regarding the use of subjective variables as dependent and explanatory at the same time (Bertrand and Mullainathan, 2001). However, many studies indicate that macroeconomic variables also affect individual life satisfaction and happiness (see e.g. Di Tella et al. (2001, 2003); Deaton (2008); Dreher and Öhler (2011)) and ignoring this will likely introduce a bias into the results, in particular since life satisfaction is also known to be correlated with many of our individual level controls (see for instance Frey and Stutzer (2002b)).

In our case, the inclusion of life satisfaction hardly affects the coefficients of macro variables. In case of changes, omitting life satisfaction overstates the importance of the macroeconomy. Specifically, comparing Columns 4 and 6 in Table 2.1, it is clear that quantitative findings from the specification with life satisfaction are more conservative than they are without it. The coefficients of unemployment and age become larger when life satisfaction is omitted and the coefficient of married becomes significantly positive. Furthermore, the effects of education appear stronger. The results imply that the effects of unemployment, age, marital status, and education are overestimated when life satisfaction is not included. The coefficients of macroeconomic variables change very little; growth and unemployment slightly increase when life satisfaction is not included. Also note that the changes in coefficients are not due to a selection effect. In Column 5 we show results from the estimation model without life satisfaction on the sample where the variable is available. There is hardly any difference between Column 6 (full sample) and Column 5 (restricted sample).

With respect to individual characteristics our results are very similar to Halla et al. (2011), qualitatively. The signs of all coefficients are the same with one exception: In contrast to Halla et al. (2011), we do not find a significantly positive effect of being married on SWD. In Section 2.6.2 we show that this difference is most likely due to the omission of life satisfaction in their study. Life satisfaction should be included in analyses of SWD since it is likely to provide a lower bound on the role of other factors.

Table 2.2: Impact of Macroeconomic Variables on Percentage SWD (Country Panel)

dependent: SWD	(1)	(2)	(3)	(4)
GDP per head	0.0044***	0.0034**	0.0031**	0.0017
	(0.001)	(0.001)	(0.001)	(0.001)
growth		0.0118***	0.0121***	0.0076***
		(0.003)	(0.003)	(0.002)
f(inflation)			0.0013	-0.0030*
			(0.002)	(0.002)
UE rate				-0.0190***
				(0.002)
survey FE	yes	yes	yes	yes
country FE	yes	yes	yes	yes
N	483	483	482	476
R^2	0.7421	0.7537	0.7540	0.8101

* p<0.10, ** p<0.05, *** p<0.01
Dependent variable is the average of the SWD dummy in a given country.
Standard errors are corrected for clustering at nation level.

2.4.3 Aggregate Level Regressions

Analyses at the country level cannot inform about how individual satisfaction scores are formed but have to collapse either the ordered data to an average or a binary recode to a percentage measure of support. Changes in these national averages can come by various channels and are less likely to be informative than an analysis with data at the individual level. In this section, we report results from estimations where we ignored the individual dimension of our data set and checked whether there is a relationship between satisfaction with democracy and macroeconomic conditions at the aggregate country level. These results can then also be compared with previous studies on SWD that used country averages over time as observations. We used the year-wise country averages of the SWD dummy as dependent variable, which represents the percentage of people who are satisfied with democracy in a given year in a country.[16]

Comparing aggregate estimations (Table 2.2) with our individual-level approach (Table 2.1), it becomes evident that coefficients have the same sign and a similar size. In the aggregate estimations growth obtains a slightly smaller coefficient: With the full set of macroeconomic controls the aggregate specification gives a coefficient of 0.0076 instead of 0.0105 with individual level data. The coefficient on unemployment is slightly larger (in absolute terms) in the aggregate (-0.0190) than at the individual

[16]When we use average satisfaction scores instead of the average over SWD-dummies as dependent variable, results are qualitatively the same. Results are presented in Table A.9 in Appendix A.2.

level (-0.0172). In contrast to our results using individual level data, at the aggregate level inflation is (weakly) significant.

Our results at the aggregate level are broadly consistent with studies by other authors. Growth is significantly positive, unemployment and inflation are significantly negative (compare for instance Wagner et al., 2009; Clarke et al., 1993). Looking at Columns 2 to 3, a one percentage point increase in growth is associated with an increase in the share of the population stating that they are satisfied with democracy of about 1.2 percentage points. In the full specification (Column 4), however, the coefficient of growth decreases. Most likely the upward differential in the coefficient of growth (Columns 3 to 4) comes from higher growth capturing also the impact of reduced unemployment on democratic satisfaction.[17] An increase in national unemployment of 1 percentage point decreased satisfaction with democracy by almost 2 percentage points on average.

2.5 Discussion

2.5.1 Economic Relevance: Satisfaction Scores during the Great Recession

Our results suggest that, on average, satisfaction with democracy should have decreased by non-negligible numbers in the context of the Great Recession. We have estimated our model on pre-2007 data and compute predicted changes in satisfaction with democracy due to worsening economic conditions. Using data until 2006, growth and unemployment are significant with coefficients of 0.0089 and -0.0167, respectively. Individual unemployment is significant with -0.0444.[18] Based on these coefficients we expect that individuals experiencing developments of growth and unemployment rates as were observed in Ireland, Spain, and Greece from 2006 to 2010 exhibited a decrease

[17] If unemployment rates change mainly because of changes in economic growth, then it is even informative to look at regressions with growth only.

[18] We restricted our sample to the years 1973-2006 and estimated our baseline specification (see Table 2.1 Column 4 for full sample results). Demographic controls obtain coefficients very similar to the full estimation discussed before. Results for this estimation are provided in Table A.8 in Appendix A.2.

in SWD by about 21 (Ireland), 24 (Spain)and 14 (Greece) percentage points.[19] In fact, for these countries, we observe a substantial decrease of average SWD by about 20 percentage points as compared to the situation before the Great Recession. In Ireland, satisfaction with democracy fell from 0.78 in 2006 to 0.58 in 2010 according to Eurobarometer data; in Spain in the same period from 0.74 to 0.53, in Greece from 0.54 to 0.30.[20]

The above calculation is a rough estimate but matches surprisingly well with actual developments. There are important caveats in that, in our prediction, we only consider macroeconomic variables and the coefficients are based on annual data. The economic downturn, however, stretches over more than one period and if macroeconomic conditions are poor over longer horizons, the picture may change. It is possible that people adapt to worsening economic conditions such that their satisfaction is on average affected less than if there is only a short downturn. It is, however, also imaginable that individuals become increasingly dissatisfied if the macroeconomy fails to recover for several years. Our approach cannot speak to these hypotheses.

Some tentative implication for economic policies can be drawn from our results. Economic policies that result in good economic performance can increase people's political support directly via national economic performance and indirectly when the effects materialize at the individual level. Importantly, however, our results also reveal the limitations of these policies. Crucial for political support is personal life satisfaction which cannot be easily addressed by economic policy and might not be an appropriate political target either.

2.5.2 Channels of Influence: Micro or Macro? Selfish Citizens or Collectivists' Concerns?

In principle, micro-level data allows us to assess how important are correlates of SWD at the micro level relative to those at the national level. Unemployment manifests

[19] Changes in real growth rates between 2006 and 2010 were -5.7 percentage points for Ireland and -4.2 for Spain. Greece experienced a decrease in its growth rate of 9 percentage points in 2010 (Eurostat, 2011a). Unemployment rates increased by 9.2 percentage points in Ireland, by 11.6 in Spain, and by 3.7 in Greece between 2006 and 2010 (Eurostat, 2011b).

[20] Our prediction for Portugal is much lower with an expected decrease in SWD by 5.9 percentage points. This compares well with the actually experienced drop in SWD by 4 percentage points from 0.31 to 0.27.

itself directly at the individual level. A change in the national unemployment rate leads to a change in employment status for some citizens. At the individual level, being unemployed is associated with a 4.7 percentage point decrease in satisfaction with democracy. To have the same effect the national unemployment rate would have to increase by 2.7 percentage points. At the aggregate level the picture is different though. To assess the effect of an increase in unemployment at the national level, we aggregate the individual effects of being unemployed on SWD for those who become unemployed and compare it with the direct effect of the change in the unemployment rate on SWD. We find that the effect running through individual unemployment is an order of magnitude smaller than the effect of the unemployment rate: Suppose unemployment increases by 1 percentage point. Then, the direct effect is -0.0172. The indirect effect from individuals becoming unemployed is $0.01 \cdot (-0.047) = -0.00047$ and the total effect is the sum of the two, i.e. -0.01767.[21] However, this comparison of individual versus national level determinants takes into account only one period. Taking a longer-term perspective the effect of individual unemployment is larger: Since unemployed individuals are less satisfied with democracy than their employed peers, a change in national unemployment implies a persistent level effect in SWD. Even when unemployment rates do not worsen in subsequent periods, as long as unemployment is not cut back again, those who became unemployed remain less satisfied and imply on average lower SWD in every period after.

This comparison does not tell why national level variables seem to be relevant for individuals' satisfaction with democracy. National unemployment rates can be influential due to pure self-interest since it is, for instance, indicative of the risk of getting unemployed, of wage developments, or upcoming job opportunities. A similar argument holds for growth rates: Their being significant does not imply that individuals care about the performance of their country as a greater good. It can simply mean that they value growth as an indicator of higher transfers, better public services, or lower taxes; factors which all materialize at the individual level and highlight the self-interest dimension of national performance. We show that the effects we find are unlikely to be driven by narrow self-interest alone by looking at subgroups of the population. We analyze separately the population with only basic or no education at all and elderly

[21] We take the estimated coefficients from Table 2.1, Column 4.

people (Table 2.3), as well as the unemployed and those who are not part of the labor force (Table 2.4).

The first interesting finding is that the effects of growth and unemployment rates are very stable across the subgroups we analyze. A second interesting finding is the heterogeneity in the effect of inflation on SWD which we discuss in the next subsection. National unemployment rates are significant for all subgroups and are not significantly different in size. This means that unemployment rates are not more important for the less-skilled even though they have arguably a higher risk of becoming unemployed. Unemployment rates are relevant no matter whether an individual is still in work or already unemployed, and they are significantly correlated with SWD for those in as well as those out of the labor force. Most interestingly, unemployment rates are highly significant also for those aged 60 or above, who to a large degree will not be directly affected since they do not actively participate in the labor market anymore and will never reenter. A potential explanation is that these individuals often have children who are in working age and therefore they care more. Unfortunately, we cannot test this hypothesis. The Eurobarometer does not contain information on parenthood but only lists the number of children aged under 15.[22]

Our findings that unemployment plays an important role independent of being directly affected are similar to the results by Falk et al. (2011) in the context of right-wing extremist crimes. They analyze the effect of regional unemployment rates on right-wing extremist crimes in German states. While the unemployment rate has a positive and statistically significant effect on right-wing extremist crimes, the youth unemployment rate does not have a statistically significant influence. So, given that most right-wing extremist crimes are comitted by youg men, as in our analysis other factors than personal experience of unemployment seem to explain the overall effect of the unemployment rate.

We cannot conclude that individuals care about the performance of their country per se, e.g., for collective welfare reasons. Still, the effects are unlikely to be driven by pure self-interest. Low-skilled versus high-skilled, unemployed versus employed, and

[22]We included this information on children and found no effect. Elderly people living together with children under 15 years do not react to unemployment rates any differently than elderly people not living together with children. Neither do individuals with children according to this definition react any differently than those without children.

Table 2.3: Analysis of Subgroups: Education and Elderly People

dependent: SWD	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
GDP per head	0.0011	0.0063	0.0011	0.0012	0.0008	0.0010	0.0026	0.0010	0.0010	0.0010
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)	(0.004)
growth	0.0108***	0.0109***	0.0108***	0.0102***	0.0110***	0.0114***	0.0119***	0.0105***	0.0105***	0.0105***
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)
f(inflation)	-0.0205	-0.0239	-0.0209	-0.0206	-0.0296**	-0.0189	-0.0136	-0.0186	-0.0186	-0.0228*
	(0.013)	(0.019)	(0.013)	(0.013)	(0.012)	(0.012)	(0.013)	(0.012)	(0.012)	(0.012)
UE rate	-0.0173***	-0.0188***	-0.0179***	-0.0173***	-0.0172***	-0.0170***	-0.0188***	-0.0176***	-0.0171***	-0.0171***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
low educ.*UE rate			0.0015							
			(0.003)							
low educ.*growth				0.0017						
				(0.003)						
low educ.*f(inflation)					0.0262**					
					(0.010)					
old*UE rate								0.0026		
								(0.002)		
old*growth									-0.0000	
									(0.001)	
old*f(inflation)										0.0207***
										(0.006)
unemployed	-0.0470***	-0.0389***	-0.0469***	-0.0470***	-0.0465***			-0.0448***	-0.0453***	-0.0456***
	(0.005)	(0.009)	(0.005)	(0.005)	(0.005)			(0.005)	(0.005)	(0.005)
low education	-0.0184*		-0.0316	-0.0229*	-0.0513***					
	(0.009)		(0.026)	(0.013)	(0.014)					
old						0.0111		-0.0151	0.0069	-0.0170
						(0.008)		(0.020)	(0.008)	(0.010)
age, age^2	yes	yes	yes	yes	yes	no	no	no	no	no
education dummies	no	no	no	no	no	yes	yes	yes	yes	yes
ind. controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
survey FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
nation FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
N	547515	203033	547515	547515	547515	613061	134230	602545	602545	602545
R^2	0.143	0.1529	0.143	0.143	0.1435	0.1418	0.129	0.1432	0.1431	0.1434

* $p<0.10$, ** $p<0.05$, *** $p<0.01$
Dependent variable is a dummy.
Standard errors are corrected for clustering at nation level.
The dummy *low education* takes a value of 1 for basic education and 0 for higher education or still studying.
The dummy *old* takes value 1 for those aged 61 and above, 0 otherwise.
In (1) we adjust the model from Column 4 in Table 2.1, and control for low education instead of several education dummies. (1) to (5) are estimated on a subsample excluding those who are still studying. (2) is estimated on the subsample with low education. (6) is estimated for all observations where age is not missing, (7) is estimated on the subsample being older than 60 years old, (8) to (10) use the full sample.

Table 2.4: Analysis of Subgroups: Employment Status and Labor Force Participation

dependent: SWD	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
macroeconomic variables										
GDP per head	0.0010	0.0014	0.0010	0.0010	0.0010	0.0010	0.0023	0.0010	0.0010	0.0010
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
growth	0.0105***	0.0099***	0.0105***	0.0105***	0.0105***	0.0105***	0.0100***	0.0105***	0.0100***	0.0106***
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
f(inflation)	-0.0188	-0.0110	-0.0188	-0.0188	-0.0194	-0.0263**	-0.0163	-0.0188	-0.0187	-0.0244*
	(0.012)	(0.011)	(0.012)	(0.012)	(0.012)	(0.012)	(0.011)	(0.012)	(0.012)	(0.012)
UE rate	-0.0172***	-0.0174***	-0.0172***	-0.0172***	-0.0172***	-0.0171***	-0.0174***	-0.0176***	-0.0172***	-0.0171***
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
unempl.*UE rate			0.0004							
			(0.002)							
unempl.*growth				0.0006						
				(0.001)						
unempl.*f(inflation)					0.0088	0.0155**				
					(0.006)	(0.006)				
out of LF*UE rate								0.0010		
								(0.001)		
out of LF*growth									0.0011	
									(0.001)	
out of LF*f(inflation) f						0.0144***				0.0127***
individual variables										
unemployed	-0.0470***		-0.0505***	-0.0486***	-0.0564***	-0.0643***		-0.0465***	-0.0471***	-0.0476***
	(0.005)		(0.016)	(0.008)	(0.008)	(0.009)		(0.006)	(0.005)	(0.005)
out of LF	-0.0015	0.0000	-0.0015	-0.0015	-0.0014	-0.0188***		-0.0102	-0.0043	-0.0168***
	(0.004)	(0.000)	(0.004)	(0.004)	(0.004)	(0.005)		(0.013)	(0.004)	(0.005)
ind. controls	yes	yes	yes	yes	yes	yes	yes	yes	yes	
survey FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	
nation FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	
N	602545	35253	602545	602545	602545	602545	265592	602545	602545	602545
R^2	0.1433	0.1485	0.1433	0.1433	0.1433	0.1435	0.1356	0.1433	0.1433	0.1435

* p<0.10, ** p<0.05, *** p<0.01
Dependent variable is a dummy.
Standard errors are corrected for clustering at nation level.
(1) is the reference estimation from Table 2.1, Column 4. (2) is estimated on the subsample of the unemployed. (7) is estimated on the subsample not being part of the labor force. (3) to (6) and (8) to (10) use the full sample.

elderly versus younger people are very differently exposed to labor market conditions such that we expect heterogeneous effects according to the self-interest model. Not finding such differences implies other factors are at work.

One explanation for our findings are collectivist welfare concerns. Individuals may have a 'true preference' for democracy because it is believed to be the system that is best able to provide collective welfare. Growth and low unemployment are success indicators of this system's performance and can make individuals be satisfied with democracy even when it does not directly maximize their expected personal income since their true preference implies a concern for collective welfare (see Sen (1977) for a similar argument). Another explanation is that individuals take general equilibrium effects and their consequences at the individual level into account. For instance, they anticipate cuts in transfers or increases in taxes when the economic situation is worsening.

2.5.3 Heterogeneous Effects and the Trade-Off between Unemployment and Inflation

An enduring economic policy debate concerns a possible trade-off between inflation and unemployment which societies may face. Assuming such a relationship, we would like to know which is the trade-off between inflation and unemployment in terms of satisfaction with democracy. In this section, we use our estimation results to analyze the relative costs of inflation and unemployment in terms of changes in SWD. In the full sample, however, inflation rates never gained significance preventing this type of analysis.[23] We therefore analyze subgroups separately and find that in contrast to growth and unemployment, inflation exhibits heterogeneous effects. In terms of satisfaction scores, a trade-off between inflation and unemployment exists, but it is a different trade-off for different parts of the population.

High inflation rates exhibit a significantly negative effect on the higher skilled individuals, those younger than 60 years, and those in the labor force. In the analysis using the full sample, this was blurred by inflation not affecting low skilled individuals,

[23] If we look at the subsample where information on income is available, inflation is significant. From Table 2.1, Column 7 we read that the loss in satisfaction with democracy from a 1 percentage point increase in unemployment equals the effect from an increase in f(inflation) by 0.537 percentage point (the sum of the direct and indirect effect of unemployment divided by the effect of inflation: $(-0.0170 - 0.01 \cdot 0.0399)/(-0.0324) = 0.537$).

the elderly and those out of labor force. Inflation does not gain significance in the full sample (Columns 1 and 6 in Table 2.3) and neither does it in the subsample of low skilled (Column 2 in Table 2.3), in the subsample of the elderly (Column 7 in Table 2.3), in the subsample of unemployed (Column 2 in Table 2.4), or in the subsample of those out of the labor force (Column 7 in Table 2.4). When we include an interaction term between the subgroup and inflation, we however obtain a negatively significant effect of inflation and a positively significant interaction term (Table 2.3, Column 5 with respect to education, Column 10 with respect to age and Table 2.4 Column 10 with respect to not being part of the labor force).

Inflation does not seem to affect employed and unemployed individuals differently when we look at Column 5 in Table 2.4. However, the control group here is all individuals who are not unemployed, including those who are not part of the labor force. When we include an additional interaction term to allow for a differential effect of inflation on those out of the labor force, we find that the interaction with inflation is highly significant for both subgroups, the unemployed and those out of the labor force (Table 2.4, Column 6). Inflation is found to be negatively associated with satisfaction with democracy only for those who actively participate in the labor market and have a job.

We now re-examine the inflation-unemployment trade-off accounting for the heterogeneity in effects. On average, younger Europeans experience the same loss in satisfaction with democracy for a 1% point increase in unemployment rates and an increase by 0.77 in f(inflation)[24].[25] The elderly are much more concerned with unemployment. Under the assumption of a linear effect, inflation being insignificant would imply that the elderly preferred an arbitrarily large increase in inflation to prevent unemployment from rising. If the effect we find for the elderly was significant, the same decrease in satisfaction would be computed for an increase by 8.36 in f(inflation) as compared to a 1% point increase in unemployment.[26] A similar picture

[24]Since we use a transformation of inflation we cannot compute the trade-off in terms of percentage points. For low inflation rates f(inflation) is linear (up to 1) or almost linear. A 1% point increase in unemployment rates is associated with the same loss in SWD as a 0.77% point increase in inflation when inflation is low.

[25]From Table 2.3, Column 10 we obtain $0.77 = (-0.0171 + 0.01 \cdot (-0.0456))/(-0.0228)$.

[26]$8.36 = (-0.0171+0.01\cdot(-0.0456))/(-0.0228+0.0207)$. With an average share of people aged sixty and above of 21.89% the aggregate effect would then be $0.2189 \cdot 8.36 + (1 - 0.2189) \cdot .77 = 2.43$. Due to the logarithmic transformation of inflation this relates to an extremely high tolerance (about the

obtains when we split the population into those with low and high education. For individuals with higher education, an increase by 0.6 in f(inflation) is associated with the same satisfaction cost as a 1% point increase in unemployment.[27] For those with low education, inflation is insignificant.[28]

The derived numbers can be interpreted as marginal rates of substitution between f(inflation) and unemployment. Our results indicate a very low importance of inflation in the aggregate when we look at satisfaction with democracy. This is in contrast to Di Tella et al. (2003) who analyze life satisfaction scores and find that the marginal rate of substitution between inflation and unemployment is 1.66. Aggregate numbers hide however, that there is an important heterogeneity across subgroups of the population (not addressed in Di Tella et al. (2003)). Not everybody agrees on unemployment being more costly than inflation. For instance, the higher educated and the younger seem to accept relatively higher unemployment rates and desire lower inflation, as compared to the less educated and elderly, respectively.

2.6 Robustness

In this section, we address important issues to demonstrate the robustness of our findings. First, we investigate the importance of lagged macro variables for our results and possible reverse causality issues. In this context we also discuss the possible role of (growth) expectations. Secondly, we present results from alternative specifications such as logit and ordered logit as well as a model where we control for income. Finally, we argue why not controlling for institutional quality is without loss of generality.

2.6.1 Lagged Growth, Growth Expectations, and Endogeneity

Growth rates from previous periods may be influential in addition to contemporaneous rates because real effects need time to materialize. Thus, we tested whether lagged

tenfold) for inflation as compared to unemployment rates. This is consistent with the insignificance of inflation in the full sample.

[27]From Table 2.3, Column 5 we obtain $0.60 = (-0.0172 + 0.01 \cdot (-0.0465))/(-0.0296)$.

[28]If the effect was significant at the size we find, an increase by 5.2 in inflation would be associated with the same satisfaction cost as a 1% point increase in unemployment since from Table 2.3, Column 5 $5.20 = (-0.0172 + 0.01 \cdot (-0.0465))/(-0.0296 + 0.0262)$. With an average share of people with low education of 38.27% the aggregate effect would be $0.3827 \cdot 5.20 + (1 - 0.3827) \cdot 0.60 = 2.36$.

growth rates have an impact on SWD. Column 1 in Table 2.5 is our benchmark model which we have discussed before (Section 2.4 and Table 2.1, Column 4). Column 2 shows that lagged growth does not have a significant influence on SWD and including it in the regression hardly affects the coefficients of the other macroeconomic variables. Growth and unemployment rates remain significant, inflation is still insignificant. The result is intuitive as the development of unemployment rates as well as inflation is at least partly determined by economic development and thus lagging behind. If we omit lagged growth and it has a positive influence on employment today and a positive influence on satisfaction, then the coefficient on unemployment is downward biased because unemployment has a negative effect on satisfaction. The argument for inflation is analogous. Coefficients on individual controls are almost unaffected. As lagged growth rates did not gain significance, we did not include them in any other regression.

Table 2.5: Lagged Growth and Endogeneity

dependent: SWD	(1)	(2)	(3)	(4)
macroeconomic variables				
GDP per head	0.0010	0.0008	0.0018	0.0004
	(0.003)	(0.004)	(0.003)	(0.001)
growth_t	0.0105***	0.0095***	0.0057**	0.0040*
	(0.003)	(0.002)	(0.002)	(0.002)
growth_{t-1}		0.0028		
		(0.002)		
growth_{t+1}			0.0102***	0.0062***
			(0.002)	(0.002)
$f(\text{inflation})_t$	-0.0188	-0.0195	-0.0216*	-0.0139
	(0.012)	(0.012)	(0.011)	(0.011)
UE rate	-0.0172***	-0.0168***	-0.0173***	-0.0061***
	(0.003)	(0.003)	(0.003)	(0.002)
$\overline{SWD}_{t-1}$				0.5549***
				(0.063)
ind. controls	yes	yes	yes	yes
survey FE	yes	yes	yes	yes
country FE	yes	yes	yes	yes
N	602545	602545	592075	546239

* p<0.10, ** p<0.05, *** p<0.01
Dependent variable is a dummy.
Standard errors are corrected for clustering at nation level.
(1) is the reference estimation from table 2.1, Column 4.

An important objection to the results presented in Section 2.4 and Table 2.1 is that not growth has an influence on SWD but instead higher satisfaction levels lead to better economic performance. We undertake a robustness check regarding this endogeneity issue and conclude that our results are not a pure artefact of endogenous growth rates.

First, we included future growth rates (Column 3 of Table 2.5). Future growth obtains a coefficient even larger in size than the coefficient of contemporaneous growth.

This might be due to reverse causality, i.e., satisfaction with democracy driving growth rates, but could also be caused by serial correlation of growth rates. In both cases, however, this is not the entire story since contemporaneous growth and unemployment remain significant, in line with our hypothesis that growth has an effect on SWD. The effect which remains when we include future growth can be considered a lower bound on the effect of growth on SWD. A third explanation for why future growth is significant is that it proxies for growth expectations. Growth expectations in turn are likely to have a positive effect on satisfaction scores. These expectations may be influenced by growth forecasts and media reports. In this case, the coefficient on future growth should not be ignored and the full effect of growth on SWD, summing over contemporaneous and future growth it is 1.82 percentage points, is even larger than the previously estimated 1.21 percentage points. Since our data does not allow to control for expectations, we cannot distinguish these hypotheses.

Second, we also included the average lagged satisfaction with democracy at the country level average of SWD (Column 4). By doing so, we control for the link potentially running from SWD to growth in the next period. Furthermore, the coefficient on future growth rates controls for correlation between SWD today and growth tomorrow. Thus, the coefficient of growth in Column 4 reflects only contemporaneous correlation between SWD and growth. This is more likely to be an effect from growth on SWD than an effect from contemporaneous SWD on contemporaneous growth. Since satisfaction with democracy on average does not change very fast, this absorbs a large part of the variation and might make inference less reliable. The effect of growth is still about 40% as large as in the main analysis and marginally significant.

2.6.2 Alternative Specifications

In the following, we show that our results are robust to several alternative specifications. We begin with discussing the relevance of personal income and accounting for time trends. Furthermore, we check for the relevance of recoding our dependent variable and of using a linear model. Satisfaction with democracy is originally available at a scale with four categories which we chose to recode as a binary measure of democratic satisfaction. In all regressions so far we employed a linear probability model. In this section, we compare our results to (i) a logit model and (ii) an ordered logit model which makes use of the four categories of SWD.

Income

Income was not asked in every year and not at all in the years after 2004 such that a substantial number of income observations is missing. Since we are particularly interested in including the recent recessionary years and to avoid a selection effect, we do not control for income in our main analysis. Our robustness check indicates that income does not affect our results beyond a selection effect driven by the availability of the income measure. We are therefore confident that our result would be robust to the inclusion of income if it was fully available. Income is only recorded in country-specific classes. To obtain a comparable measure across countries, we computed country-specific income deciles and categorized individuals in three groups 'rich', 'middle income', and 'poor' according to their decile and chose 'middle income' as the omitted category.[29]

We illustrate in Table 2.1, Column 7 that the effects of inflation and growth are indeed different in the subsample for which income information is available. As compared to the benchmark in Column 4, the coefficient of growth decreases substantially to .006 and inflation becomes marginally significant. The coefficient on individual unemployment becomes smaller, and neither sex nor education are significant anymore. Column 8 in the same table shows that beyond this selection effect the inclusion of income seems to have little effect on the results. We do find a small effect of income though. Rich people have a slightly increased probability to be satisfied with democracy compared to middle income earners. There is no significant effect for individuals with low income.

Time Trends

Sometimes it is argued that analyses as the one we undertake should include a time trend to avoid spurious regression results due to underlying trends in the variables.

[29]Details on the variable definition can be found in Table A.1 in Appendix A.1.

To address this issue, we estimated a model including country specific time trends. We find that our specification without time trends leads to more conservative results with respect to growth. However, regarding unemployment rates, the inclusion of a country-specific time trend leads to marginally lower coefficients (the coefficient goes down from .0172 to .0155). The absolute size of the effect of unemployment rates on SWD should therefore be taken with caution. For details see Table A.10 in Appendix A.2.

Logit

Above, we only describe results from ordinary least squares regressions (linear probability model) although our dependent variable is binary. In our opinion the advantages in terms of interpretation and simplicity of the linear model outweigh potential gains from the nonlinear model (for a discussion see also Angrist and Pischke (2009)). However, we also estimated a logit model which explicitly restricts the outcome variables to lie between zero and one. Results are qualitatively the same and quantitatively close to those from the OLS. All marginal effects lie above the coefficients estimated by OLS and therefore our model choice gives rather conservative results. For details see Table A.5 and Table A.6 in Appendix A.2.

Ordered Logit

While we believe that the analysis is more rigorous when a binary recode is used, we also analyzed determinants of SWD using the original, ordered outcome. This exercise provides little additional insight. All variables which obtained significance in the binary model are significant in the ordered logit and go in the same direction but, in addition, inflation becomes significant. Higher inflation rates were associated with lower probabilities of being very or fairly satisfied and higher probabilities of being not very or not at all satisfied with the way democracy works. When we sum the marginal effects for the two lower categories, we obtain a value by and large comparable in size to the sum of the marginal effects for the two upper categories but with opposite signs.[30] This is consistent with the view that the results in the binary recode are

[30]The lower categories are answers ‘not at all satisfied’ and ‘not very satisfied’, the upper categories are the answers ‘fairly satisfied’ and ‘very satisfied’ with the way democracy works.

driven by individuals switching from being not satisfied to being satisfied with the way democracy works. It indicates that the binary recode does not come with a substantial loss of information. For details see Table A.7 in Appendix A.2.

We have shown before that inflation is insignificant in the binary model with and without unemployment rates (see Section 2.4.1, Table 2.1). When we estimate an ordered logit model with and without national unemployment rates, we find that inflation is significant only when we include unemployment as well. This contrasts with Halla et al. (2011) who omit unemployment rates but find inflation to be significant. Since they include further macro variables, this might be driven by those.[31] We conclude that the decision which macro variables to include plays an important role for the results. We argue in the following subsection 2.6.3 that our results do not seem to be affected by the omission of policy variables similar to those included in Halla et al. (2011). We therefore consider our results more robust.

2.6.3 Institutional Quality and Policy Measures

Our analysis assumes that democratic institutions in Western Europe did not change over the relevant time horizon and we do not include a control for institutional quality. We argue that this is not a restriction for several reasons. First, the binary Democracy-Dictatorhsip measure as discussed in Cheibub et al. (2009) is constant at 1 for all country-year pairs in our sample, indicating stable democracies. Consequently, our results would remain the same if we controlled for institutional quality in this sense. Secondly, our results are robust to the use of alternative indicators of institutional quality, the Polity IV index (Marshall et al., 2011) and the Freedom House Index (Freedom House, 2011). Both have often been used but are also criticized (see for instance Cheibub et al. (2009)). Controlling for either of the two indicators does not affect our findings and the indicators remain insignificant as shown in Table A.11 in Appendix A.2. The Freedom House Index is only available until 2008. Omitting the years 2009 and 2010 from the analysis does affect the results, in particular inflation becomes significant. The effect comes only from the sample restriction, though, and is

[31] While they control for GDP and population and find both significant with opposite signs, we find that GDP per head never gains significance. This is consistent with each other and we therefore do not discuss it further.

not related to institutional quality. We do not include elective fractionalization, which is also sometimes included in analyses like ours, since it never gained significance in a very related analysis of SWD by Halla et al. (2011). Thirdly, in an analysis of political preferences in Central and Eastern European countries, Grosjean and Senik (2011) find no significant effect of market liberalisation on support for democracy. This supports our view that even though there have been major changes for example in the organization of the European common market these changes are not of major concern.

Another possible objection to our analysis is that it is not the macroeconomic outcomes that influence citizens' satisfaction but instead policies implemented by governments. We therefore test for the effects of debt and deficit levels and also include two measures which proxy for social spending, (1) the population aged 65 and above as a percentage of total population and (2) social security transfers as a percentage of GDP. All four variables are taken from Armingeon et al. (2009). Unfortunately, we have information on deficits and debt levels only until 2007, and information on social transfers only until 2000 such that we cannot compare the results with policy variables directly to those from the main analysis.[32] To be able to assess the relevance of policy measures, we estimate the main model on the subsample for which all policy variables are available and then included the policy measures. The restriction to the subsample changes results substantially, in particular inflation becomes significant due to the omission of recent years with relatively low inflation rates. However, the inclusion of policy measures does not lead to additional changes. In contrast to Halla et al. (2011), none of the policy variables gains significance. Results are provided in Table A.12 in the Appendix.

2.7 Conclusion

The European debt crisis has had a severe impact on European democracies. In the five most heavily affected EU member countries, Greece, Ireland, Italy, Portugal, and Spain governments have been voted out office. More than that, demands by the various protestors go beyond the deselection of governments. People's perception of

[32] Moreover, these variables are missing for Luxembourg, Denmark, Portugal, Spain, Norway, and Austria in some years earlier than 2001. These drawbacks are the reasons why we only present this as a robustness exercise and exclude it from the main analysis.

the democratic system has changed in the course of the crisis, not only in Greece but also in many other European countries.

This chapter shows that the changing attitudes towards democracy were to be expected as a consequence of extremely poor national economic performance. Lower growth rates and higher unemployment rates were both associated with fewer respondents stating they were satisfied with the way democracy works. For drops in growth rates and rise in unemployment rates as experienced for example by Spain or Ireland, our simple annual estimate of a drop in satisfaction with democracy by 19 to 24 percentage points is close to actual changes in satisfaction with democracy which were around -20 percentage points. Moreover, our analysis shows that the unemployment rate is substantially more important than the inflation rate in shaping attitudes towards the democratic system and also more important than the growth rate. From that perspective any policy intended to improve peoples' satisfaction with the democratic system should prioritize job creation.

While not contradicting previous work, our analysis uncovered important new aspects. First, growth and unemployment rates were found simultaneously significant while, in contrast to previous research, inflation is insignificant. This difference is driven by including the years 2009 and 2010.[33] Moreover, while inflation is not significant for the period we consider for the entire population, it has a significantly negative effect on individuals who are higher skilled, younger than 60, or have a job. Secondly, our results show that individual variables, in particular individual unemployment, education and age, are an important driver of satisfaction with democracy. Moreover, perceived life satisfaction has a strong effect and its inclusion increased explanatory power substantially (with respect to R^2). This last result is a challenge for policy-makers and future research because it is not obvious whether economic policy should indeed target individuals' life satisfaction and if it should how it can do so.

Finally, while individual controls are important, they do not make macroeconomic variables irrelevant. National aggregates like unemployment and growth have a significant effect beyond what materializes at the individual level. It is beyond

[33] These years were markedly different: Some countries went through a phase of very low inflation rates and some countries even experienced a period of deflation. When we restricted our sample to the period before 2008, the significance of inflation was restored.

the scope of our analysis to provide a clear-cut answer why national indicators are significant. However, our results suggest that a collectivist perspective plays a role. If peoples' evaluation of democracy was driven by pure self-interest, we would expect a differential effect of growth and unemployment across subgroups of the population (for instance, skilled versus unskilled). The lack thereof suggests that collectivist concerns for national economic performance play a role.

Chapter 3

Control and Group Identity

3.1 Introduction

An important premise in agency theory is the power of incentives: Without proper incentives and control, agents would not put in enough effort and would shirk substantially. As a consequence, principals need to use incentives and control systems in order to make agents perform. While incentives do increase effort in certain settings (e.g., Lazear, 2000), they can also have detrimental effects, i.e., individuals put in *less* effort in the presence of monetary incentives than without (for reviews of the literature, see Frey and Jegen, 2001; Fehr and Falk, 2002; Gneezy et al., 2011; Bowles and Polanía-Reyes, 2012). Control mechanisms can also be problematic as results by Falk and Kosfeld (2006) indicate. The implementation of a control mechanism that forces agents to put in at least some effort can have substantial "hidden costs" leading to lower effort than if agents are not controlled by the principal. While there is growing evidence that incentives and control can be detrimental in principal-agent situations, the *conditions* when they turn detrimental are less understood.

In this chapter, we investigate whether "hidden costs of control" are particularly relevant when the principal and the agent have a close relationship (as argued in Frey, 1993; Dickinson and Villeval, 2008). Detrimental effects of control mechanisms might be restricted to interactions among friends, family members, or in-group members as they are more likely to be perceived by the agent as distrust and a violation of a psychological contract (e.g. Fehr and Falk, 2002; Sliwka, 2007; Ellingsen and Johannesson, 2008; von Siemens, 2011).

However, in many situations incentives and control are used *not* within-groups but in between-group interactions. For example, many principal-agent relationships are between firms, i.e., buyers and suppliers. Even within firms, the principal often belongs to and identifies with a different and very salient team or group: The management (white collar) controls the workers (blue collars) or provides incentives to the salesforce. It is well known that in such between-group interactions, trust is lower to begin with (e.g. Fershtman and Gneezy, 2001; Chen and Li, 2009; Falk and Zehnder, 2007) and control might be less detrimental as agents do not expect their out-group principals to be trusting. If the effect is less detrimental in between-group interactions, then it might be less relevant in a situation in which agents do not identify with the same group as the principals.

We test the impact of group identity on the effectiveness of a control mechanism in an experiment à la Falk and Kosfeld (2006) in which all participants belong to one of two different groups (the group manipulation is implemented similar to Chen and Li, 2009). Principals have to decide whether or not they want to control their agent by setting a minimum transfer level. Agents then decide on transfers. Our experiment is able to replicate both that a) control can be detrimental and reduce transfers and that b) group membership leads to substantial in-group bias, i.e., agents provide higher transfers for in-group principals. More importantly, our setting allows analyzing whether the hidden cost of control are group-specific, i.e., whether control is perceived differently in within-group and between-group matchings.

The results show that hidden costs of control are as strong in between-group matchings as in within-group matchings. But while the overall effectiveness of control seems not to depend on the social distance between principals and agents through joint group identity, the mechanisms for how control is perceived are group-specific. In within-group interactions hidden costs of control occur because agents expect the principal not to control. When principals control nevertheless, agents reduce their transfers significantly as a reaction. If the agent and the principal do not share the same group, however, the mechanism is different: Keeping agents' beliefs about the principal's behavior constant, agents perceive control as more hostile in a between-group matching. This "hostility effect" is consistent with previous findings (Chen and Li 2009 and Götte et al. 2012a), who show that when minimal groups are artificially generated, punishment for misbehavior is stronger in between-group than in within-group interactions.[1]

Hence, our results show that control can have a detrimental effect not only in principal-agents relationship with close social ties. Control is perceived very differently in between- vs. within-group matchings indicating that group membership in principal-agent relationships is important. In the end, in both between- and in within-group matchings the implementation of a control mechanism has "hidden costs" but for different reasons.

This chapter proceeds as follows: In Section 3.2 we briefly discuss the related literature. In Section 3.3 we present the experimental design. Section 3.4 discusses our behavioral hypotheses based on a simple model that illustrates the different effects group identity can have on control. Section 3.5 presents the results and Section 3.6 concludes.

3.2 Related Literature

Our experiment is related to two strands of the literature, the literature on group identity and the literature on crowding out of incentives.

Akerlof and Kranton (2005) is one of the first papers in economics that stresses the importance of group identity for organizational design – introducing a long research tradition in social psychology to economics.[2] Since then, there is a growing empirical literature in economics on the effects of group identity on individual behavior.

There exist two approaches. One is to exploit existing natural groups and the other one is to induce group identity artificially. A typical example of the former is Bernhard et al. (2006) who implement a Dictator Game with third-party punishment in Papua New Guinea.[3] Subjects from two distinct indigenous tribes show significant in-group bias as third parties punish more harshly when the victim belongs to the punisher's

[1]When real groups are considered, however, as in Götte et al. (2006) and Bernhard et al. (2006), individuals punish violators of social norms harsher in between-group interactions only if the victim of the violation belongs to their own group.

[2]In the management literature, identification with a firm is also been argued to be an important aspect of firms' performance through improving coordination and cooperation (e.g., Kogut and Zander, 1996).

[3]See Chen and Li (2009) for an overview of earlier experiments relying on priming of existing groups.

group. The obvious problem with natural groups is that unobserved variables may affect both group identity and behavior.

Artificially induced groups can be a solution to this problem. For instance, Götte et al. (2006) conduct a field experiment using randomly assigned platoons in an officer training program of the Swiss Army.[4] Similar to Bernhard et al. (2006) they also find an in-group bias with respect to punishments. Additionally, they find more cooperation in a prisoner's dilemma between in-group members.

Moreover, there are a number of laboratory experiments which introduce group identity artificially. In the lab, a feeling of membership in a group can be artificially induced using several procedures such as team tasks and tournaments (Eckel and Grossman, 2005), artwork preferences (Tajfel and Turner, 1979; Chen and Li, 2009), communication (Chen and Li, 2009), or payoff commonalities for members of the group (Charness et al. (2007)). Eckel and Grossman (2005) find higher cooperation rates in a public good game within groups when identity is sufficiently strong. Similarily, Charness et al. (2007) show an effect of group identity in the Battle of the Sexes as well as in the Prisoner's Dilemma. However, the size of the effect depends on the salience of groups. In an elaborate setting, Chen and Li (2009) implement variants of the Dicator and the Ultimatum Game and compare different group identity treatments. The authors find a significant effect of group identity on social preferences and reciprocity.

The second strand of literature to which our analysis is related is the growing literature in economics on psychological effects of incentives. Besides the evidence by Falk and Kosfeld (2006) that control can have hidden costs, a number of laboratory experiments find similar effects for incentives. For instance, Fehr and Rockenbach (2003) provide evidence that a fine reduces prosocial behavior in a Gift Exchange Game (other examples include: Fehr and List, 2004; Fehr et al., 2007; Gächter et al., 2010). Moreover, there is also evidence for a negative effect of incentives from field experiments (e.g., Gneezy and Rustichini, 2000; Frey and Oberholzer-Gee, 1997).[5] Lastly, hidden benefits of delegation, i.e., the flipside of control have also been observed (Charness et al., 2011). Even though there is plenty of evidence that crowding out can

[4]See also Götte et al. (2012a) and Götte et al. (2012b) for similar experiments.

[5]For a review of the literature on crowding out see (Gneezy et al., 2011; Bowles and Polanía-Reyes, 2012)

occur, relatively little is known about the conditions under which incentives or control mechanisms are more likely to have detrimental effects.

To the best of our knowledge there is no paper that investigates the interaction between group identity and crowding out. There are, however, a few papers which analyze how social distance influences the efficacy of incentives and control schemes. Bandiera et al. (2009, 2010) show how social connections *among agents* have impact on the effectiveness of different incentive schemes. Dickinson and Villeval (2008) generate interpersonal relationships between principals and agents by removing subject anonymity, introducing subjects to one-another and allowing them to engage in five-minutes of face-to-face interactions. They find that monitoring is no more efficient when anonymity is lifted and social exchange between principal and agent is allowed.

Riener and Wiederhold (2012) investigate behavior in a modified design based on Falk and Kosfeld (2006). They reduce social distance between a principal and an agent using a "team building" task. As a task subjects play a Weakest-Link game.[6] Given their prior common experience, subjects then interact in two altered versions of the design by Falk and Kosfeld (2006). The authors compare the behavior of this group to a control group which had no prior common interaction. As in Falk and Kosfeld (2006) they observe hidden costs of control. Moreover, the effect of control is particularly negative for subjects who completed the team task and expect no or only a low level of control. In contrast to their design, our group treatment is not limited to two persons and more importantly is not based on the same two subjects interacting repeatedly in the same constellation. Moreover, in our setting all subjects experience the same type of interaction whereas in their setting the two team members interact before the simplified Gift Exchange Game, but the control group only interacts in the simplified Gift Exchange Game.

[6]Additionally, the two subjects can use a chat function where they discuss the Weakest-Link game before they make a decision with the subject they are paired with. After the Weakest-Link Game they rate the fairness of the subject they are paired with. Subjects are then informed about the evaluation of their fairness.

3.3 Experimental Design

To analyze how group identity affects the hidden cost of control, participants are, first, assigned to different groups and, second, interact in a stylized principal-agent relationship in which the principal can incentivize the agent by imposing a control mechanism. The choice of the principal-agent interaction and the group treatment were guided by the desire to create a setup in which control can be detrimental and in which group identity is quickly established while still being artificially created.

3.3.1 Group Manipulation

We manipulate group identity in the lab using three steps similar to Chen and Li (2009):[7] First, each individual was presented a pair of five pictures by two artists (Klee and Kandinsky). For each pair, subjects had to choose one painting. Subjects were then ranked according to their choices and assigned to either the "Kandinsky" or the "Klee" group. To guarantee equally large groups, there was a random draw that assigned subjects such that groups were balanced in the case of an initially unbalanced assignment.[8] A subject knew that her own and the choices of the other subjects would be used to assign two equally large groups. Subjects were not informed about the details. After each subject had made a decision, participants learned to which group they had been assigned.

Secondly, each subject participated in a quiz.[9] Within their own group, subjects could communicate via a chat program to discuss the questions. While subjects could discuss with group members which answers were correct, each subject had to answer individually and in private, i.e., decisions were not observed by other subjects.

Thirdly, to further strengthen group identity we introduced an incentivized group competition using the outcome in the quiz as scores: The group with the highest

[7] We thank Yan Chen and Sherry Li for providing us with the original z-tree code and the original instructions of Chen and Li (2009). Moreover, we thank James Tremewan for providing us with the z-tree code for the chat treatment that was used in Tremewan (2010).

[8] Subjects from the initially larger group were reassigned to the smaller group by using the ranking of choices. Among those subjects who had the same ranking and were closest in their choices to the smaller group, a random variable was drawn which then determined which subject was reassigned.

[9] The quiz comprised five questions covering two more painting pairs and three political-economic issues regarding Germany (the rate of inflation, the budget of the Department of Defense, and the number of secretaries in the federal government).

average of correct answers received a prize of 8 EUR per group member. Results by Eckel and Grossman (2005), Charness et al. (2007), and Götte et al. (2012b) indicate that competition among groups induced by financial rewards have a strong effect on group identity. The prize was announced before the quiz started. The outcome of the group competition, however, was only revealed at the end of the experiment when subjects had already made all their decisions.

The groups are almost 'minimal groups' according to the standards in psychology (e.g. Tajfel and Turner, 1979) and are not real groups as in, e.g., Fershtman and Gneezy (2001); Bernhard et al. (2006). The advantage of using this approach is that the groups are neither endogenously formed nor do they differ in any observable characteristics like ethnicity which would make inference about the effect of group identity *per se* very difficult.[10] Another approach would have been to use randomly assigned real groups (e.g., Götte et al., 2006). Our approach allows us to see whether a minimal identity already has an effect.[11]

3.3.2 Principal-Agent Interaction

After we manipulated group identity, subjects played a principal-agent game based on the design by Falk and Kosfeld (2006).[12] The agent decides on whether to make a transfer. The transfer is tripled by the experimenter and represents the payoff of the principal. Before the agent decides, the principal can restrict the agent's choice set to an exogenously given lower bound (the minimum transfer the agent has to make) or leave the agent's set unrestricted.

An agent's payoff is $y_A = 24 - e$. He can choose e from $e \in \{0, ..., 24\}$ points if he is not controlled by the principal and from $e \in \{1, ..., 24\}$ points if he is controlled by the principal. We explicitly choose parameters that are most likely to generate hidden

[10]Methodologically, we vary the social distance between principal and agents without lifting the anonymity as other studies had to do. Previous approaches make it difficult to isolate anonymity effects from effects of the closeness of the principal-agent relationship (e.g., Dufwenberg and Muren, 2006; Bohnet and Frey, 1999; Dickinson and Villeval, 2008).

[11]In half of the sessions we also added a Dictator Game stage to see whether it can further increase group identity. The Dictator Game stage took place after the group manipulation and before the simplified Gift Exchange Game stage described in this section. As it does not change the results qualitatively, we analyze all the sessions together in the main text and relegate the results separated for the condition with and without a Dictator Game to Appendix B.4 (see Tables B.4 and B.5).

[12]We thank Matteo Ploner and Anthony Ziegelmeyer (Ziegelmeyer et al., 2012) for providing us with the original instructions and the original z-tree code of Falk and Kosfeld (2006).

costs of control as we are interested in studying when these costs are most prevalent.

The principal's payoff is $y_P = e \cdot 3$. Hence, the principal can secure himself a payoff of at least 3 points if he controls the agent.

Subjects were randomly assigned to be either an agent or a principal for the whole experiment. Subjects played two rounds of this principal-agent interaction. In the first round, one half of principals and agents was randomly matched with a counterpart from the same group, the other half was randomly matched with a counterpart from the other group. In the second round, each subject was matched in the opposite constellation. When we check whether there are any order effects by controlling for the round, the results do not change.[13]

All interactions were anonymous. Subjects only knew the group membership of the subject they interacted with, i.e., whether the counterpart belonged to the same or to the other group. Subjects also knew that they would be matched with a different participant in the second round.[14]

Subjects learned the outcome of the interactions only after the second round was completed. Subjects were then paid one randomly drawn round.

As in Falk and Kosfeld (2006) we used the strategy method to elicit a response by agents for the two possible cases, being controlled and not being controlled in each round. While it is possible that the strategy method influences results, two points have to be noted: First, the empiricial evidence that the strategy method leads to a different behavior than the direct-response method is mixed. In a recent survey, Brandts and Charness (2011) report on 29 studies investigating the differences between the strategy and the direct-response method. Out of these twenty-nine studies, four find differences, nine find mixed evidence, and sixteen find no differences. Secondly and more importantly, Falk and Kosfeld (2006) conduct a control treatment (SR10) where they do not find any significant differences between the two methods. As we are using the same design, we are confident that our results are robust to the direct-response method, too.[15]

[13]Results are not reported here. They are available upon request.

[14]Subjects did not know in advance that they would be matched with an in-group member in one round and an out-group member in the other round.

[15]Falk and Kosfeld (2006) is also among the surveyed articles in Brandts and Charness (2011) which do not find differences.

In addition, we elicited beliefs from agents as well as from principals.[16] After they had made a decision in each round, we asked them what they believed the principal (agent) would do.[17]

3.3.3 Procedural Details

The experiment was conducted in the experimental lab at Mannheim University in September 2011. The experiment was computerized using z-tree (Fischbacher, 2007). Subjects were students from the University of Mannheim from different fields. They were recruited using the online system ORSEE (Greiner, 2004).

In total 128 subjects participated in 16 sessions. A session lasted between 35 and 45 minutes. One point in the experiment represents 0.5 EUR and average earnings were 16.80 EUR (about $23).

3.4 Behavioral Predictions

We are interested in what influences the effect of control and how it depends on group identity of the principal and the agent. We set $\pi(m, \tau)$ for the effect of control, that is, the transfer when an agent is not controlled (which we denote with t) minus the transfer when an agent is controlled. $\pi(m, \tau)$ depends on two factors: m, the matching between an agent and a principal, and τ, the beliefs of an agent about the action a principal will decide to undertake. The matching is $m = w$ if the agent and the principal belong to the same group (within-group matching), and $m = b$ otherwise (between-group matching); τ can take on only two values, $\tau = n$ if the agent expects to be trusted by the principal and therefore not to be controlled by him, and $\tau = c$ if he expects to be controlled.

Group identity of the principal and the agent can influence the effectiveness of

[16] As in Falk and Kosfeld (2006) belief elicitation was not incentivized. Subjects were informed that their responses had no influence on their payoffs.

[17] The exact question for the agent was: "What do you think, will participant A constrain you to transfer at least 1 point?". For the principal it was: "What do you think, how many points will participant A transfer if you allow him to decide freely?" and "What do you think, how many points will participant A transfer if you constrain him to transfer at least 1 point?".

Note, that the belief is binary and it does not allow to measure different intensities, i.e., how likely subjects think a control decision is.

incentives in several ways. Below, we outline three channels through which group identity can impact how agents react to control. While the first channel is concerned with the level of prosocial behavior in the absence of control, the other two deal with how control is perceived by an agent.

1. "Prosocial effect": In general, hidden costs of control can only be observed if agents make positive transfers without control (prosocial behavior, $t > 0$). As such, control is effective if agents do not transfer without control, i.e., if their transfer is zero when not being controlled ($t = 0$).

   It is typically observed that shared group identity substantially increases prosocial behavior within-group (for a review of research on in-group bias, see Hewstone et al. (2002)). As a result, control is going to be more effective in a between-group matching because of two effects. First, more agents will be pushed from a zero transfer to the minimum transfer of 1 in a between-group matching. Secondly, in a between-group matching, there are fewer agents behaving prosocially and thus fewer agents whose transfer can be crowded out by the incentive. If $s_m(t = 0)$ is the fraction of agents that chooses a transfer t of zero in a matching m and $s_m(t > 1)$ the fraction of agents that chooses a transfer t *larger* than 1 in a matching m, we then expect both $s_w(t = 0) < s_b(t = 0)$ and $s_w(t > 1) > s_b(t > 1)$.

While control mechanically has a positive effect for those individuals who are selfish, for individuals behaving prosocially and whose transfers thereby can be crowded out, we discuss two channels through which control can have a negative effect.

2. "Trust effect": Control as an incentive scheme can have 'hidden costs' as it signals distrust of the principal towards the agent (e.g. Fehr and List, 2004; Falk and Kosfeld, 2006). Agents punish principals more harshly if they do not expect to be distrusted by the principal. In the above framework, we therefore expect $\pi(m, n) - \pi(m, c) \equiv \gamma > 0$, for all m and conditional on a transfer larger than 1. Therefore, independent of the matching m, the crowding out will be stronger when an agent does not expect to be controlled ($\tau = n$).

   Denote with b_m the share of agents who believe to be controlled in a matching m (conditional on transfers larger than 1). We expect that group membership will affect this share of agents, that is, in a between-group matching more agents will expect to be controlled ($b_b > b_w$). As a result, the share of individuals that will be disappointed by a principal's decision to control will be higher in a within-group matching.

3. "Hostility effect": A violation of expected trust might be just one reason why control can have hidden costs. Given an agent's expectation about principals' actions, an agent might perceive control as more or less 'offensive' as he or she will interpret what control signals about a principal's view on the agent or attribute the action by the principal to his or her attitudes or traits. Group identity can change this perception of actions from in- or out-group members substantially. The famous 'group attribution error' (Allison and Messick, 1985) as one way of group specific 'social inference' (Tajfel, 1982; Hastie, 1983) shows that the same actions are perceived differently if taken by an in- or out-group member. Holding expectations constant, control by an out-group principal is expected to be perceived more negatively than control by an in-group member. That is, we expect $\pi(b,\tau) - \pi(w,\tau) \equiv \delta > 0$, for all τ and transfers larger than 1. So, for given expectations about being controlled or not, control will increase crowding out more strongly in a between-group matching than in a within-group matching.

As a last step we derive the formula for the difference between the average effect of control in a within-group matching and the average effect of control in a between-group matching. To simplify notation, we set $\pi(w,c) \equiv \kappa$. Then, putting the three effects from above together, we get as the difference-in-difference, $\Pi_w - \Pi_b$:[18]

$$\begin{aligned}\Pi_w - \Pi_b = &\underbrace{(s_b(t=0) - s_w(t=0)) + [(s_w(t>1) - s_b(t>1)]\,\kappa}_{\text{Prosocial Effect}} \\ &+ \underbrace{[s_w(t>1)(1-b_w) - s_b(t>1)(1-b_b)]\,\gamma}_{\text{Trust Effect}} \\ &- \underbrace{s_b(t>1)\delta}_{\text{Hostility Effect}}\end{aligned} \tag{3.1}$$

[18] For the details of the derivation see Appendix B.2

The three different effects of group membership on the effect of incentives are countervailing. While the ‘prosocial effect’ and the ‘trust effect’ predict more hidden costs of control in within-group interactions, the ‘hostility effect’ predicts more hidden costs in between-group matchings. The net effect in within-group compared to between-group interactions is therefore unclear. We will show in the next section the net effect first and then disentangle the different channels by using information collected during the experimental stage on both the transfers provided by the agents and whether they expect to be controlled by the principal.

3.5 Results

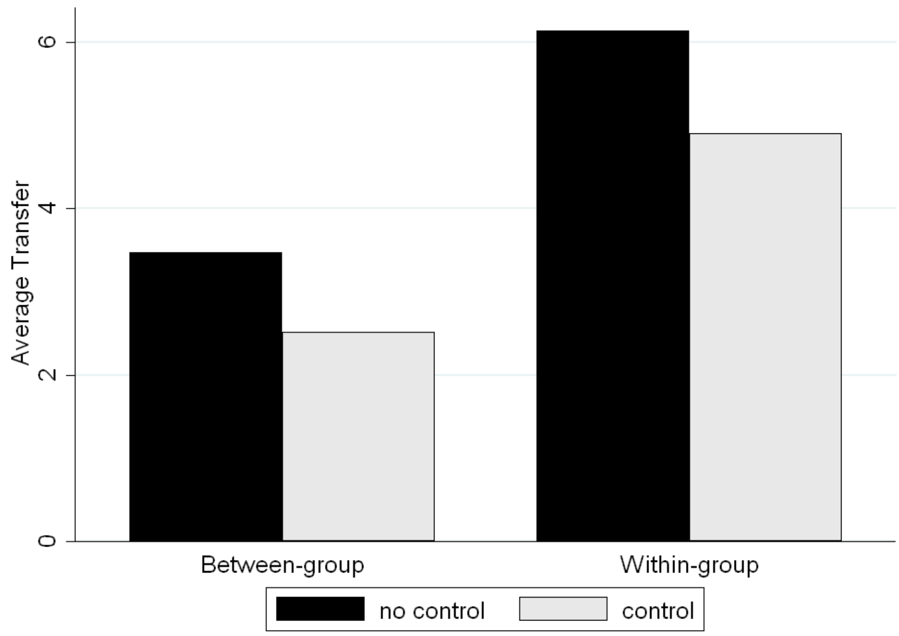
We present the results in two steps: First and most importantly, we discuss the behavior of agents. This part presents the net effect of control on agents in within- and between-group interactions and then shows whether some of the channels of crowding out are matching-specific. Secondly, we discuss principals’ decision to control.

3.5.1 Agents’ Behavior

Our first result replicates the findings in Falk and Kosfeld (2006):

> **Result 1:** *We observe “hidden costs of control”: Control reduces average transfers by agents.*

Figure 3.5.1 depicts agents’ average transfers depending on matching (between-group or within-group) and choice of the principal (to control or not to control). Across the two treatments transfers are 4.8 points when agents are not being controlled and decline to 3.7 points when agents are being controlled. The average transfer in a between-group (within-group) matching when not being controlled is 3.4 (6.1) points. When being controlled, the transfer decreases to 2.5 points, or 73% of the transfer under no control, in a between-group matching (4.9 points, respectively 80%, in a within-group matching). In Table 3.1 we report results from an OLS regression of agents’ transfer choices in control and group matching. Column 1 shows that control has a significantly negative effect on agents’ transfer choices. When comparing our results to Falk and Kosfeld (2006), two points are evident. First, the average transfer



Notes: The figure depicts average individual transfers by agents as a function of control and matching.

Figure 3.1: Agents' Average Transfer

by agents who are not controlled is 5.02 points and therefore in the range between our within-group and between-group results.[19] Secondly, however, the average crowding out is stronger than in our case (average transfer when being controlled is 2.44 points, respectively 48%).[20]

> **Result 2:** *Group identity affects agents' transfers. Independent of control decisions, agents transfer more in a within-group matching than in a between-group matching.*

In line with previous research we observe a clear effect of group identity. Agents transfer more points when they are matched with a participant of their own group independently of whether they are controlled or not. In a between-group matching agents transfer 55% of the average transfer in a within-group matching. Table 3.1 shows that the effect is statistically highly significant. Our results are similar to Chen and Li (2009, p.442). They find that distributional preferences are affected by group

[19] Taken from Table 1 (p. 1619) treatment C5. We transformed the numbers into our 24-point-scale to make them comparable. Also note that in Falk and Kosfeld (2006) every transfer by the agent is doubled and not tripled as in our design.

[20] Note that other experiments also report a smaller crowding out effect of control (Ziegelmeyer et al., 2012; Kessler and Leider, 2012).

Table 3.1: Agents' Transfers Conditional on Control and Group Matching

Dependent Variable: Transfers	(1)	(2)
Control (=1)	-1.094***	-0.953***
	(0.242)	(0.291)
Within-group matching (=1)	2.531***	2.672***
	(0.663)	(0.699)
Control x Within-group		-0.281
		(0.314)
Constant	3.539***	3.469***
	(0.444)	(0.474)
N	256	256

Notes: Standard errors are in parentheses. All columns report results of OLS regressions. Dependent variable is the individual transfer. Standard errors are clustered at the individual level.
Level of significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

identity. In particular, charity towards in-group members increases by 47% compared to out-group members.

Result 3: *We observe no group identity effect on the average effect of control.*

While group identity affects the transfers, it has no impact on how agents react when being controlled. The average difference between the 'effect of control' between within- and between-group matching (i.e., the diff-in-diff) is small (0.28) and not statistically significant as shown in Table 3.1, Column 2.

While the net effect does not differ, in the following, we decompose the diff-in-diff as described in section 3.4 into three effects: prosocial, trust, and hostility. We first provide evidence for the existence of these three effects and show that they depend on group matching. Then, we identify the parameters described in section 3.4 and disentangle the effects within the diff-in-diff.

Result 4: *Group identity affects the distribution of transfers under no control. More agents transfer zero and fewer agents transfer more than 1 in a between-group matching.*

Panel A (Panel B) in Figure 3.2 shows the distribution of transfers when the agent is not controlled (is controlled) by the principal. Panel A illustrates that group identity affects this distribution. The difference between the two distributions is statistically significant (Mann-Whitney test, p<0.01). For the overall effect of control two points are important: First, the share of agents who transfer zero is larger in a between-group matching ($s_b(t = 0) = 0.28$) than in a within-group matching ($s_w(t = 0) = 0.12$) and this difference is statistically significant (Fisher's exact test, p<0.05). Therefore, for a larger share of agents control is binding in between-group interactions. Secondly, the share of agents who expend more than 1 under no control - whose transfers thereby can be crowded out - is smaller in a between-group matching ($s_b(t > 1) = 0.66$) than in a within-group matching ($s_w(t > 1) = 0.86$). This difference is also statistically significant (Fisher's exact test, p<0.05).

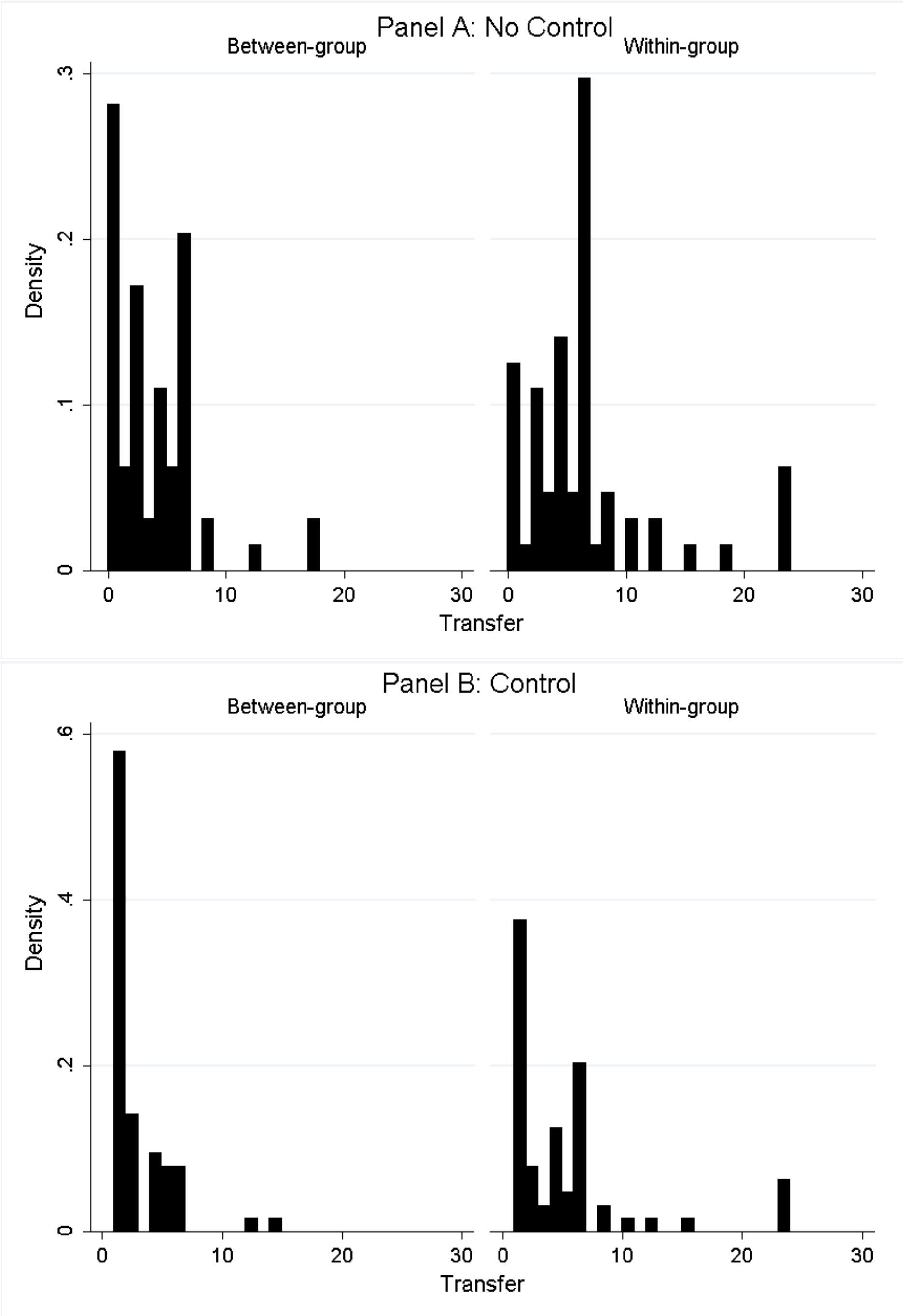
In the following, we discuss the two other channels outlined in Section 3.4, trust and hostility. Since they are only relevant to individuals who transfer more than one point when not being controlled, in the empirical analysis further on we only focus on these subjects. In Appendix B.4, we provide results including all individuals and not only subjects who exhibit a transfer greater than zero.

> **Result 5:** *Agents who expected to be trusted by principals, that is, agents who do not expect to be controlled, reduce transfers more strongly.*

The crowding out is mostly driven by agents who initially expected to be trusted by the principal, that is, by agents who did not expect to be controlled by them. They show the strongest negative reaction to control: On average, the crowding out, i.e., the reduction of transfers when being controlled, is around 1 point for agents who expect the principal to control. It more than doubles to 2.2 points when they expect not to be controlled. The difference of distributions is statistically significant (Mann-Whitney test, p<0.001). In our model, this "trust effect" corresponds to γ.

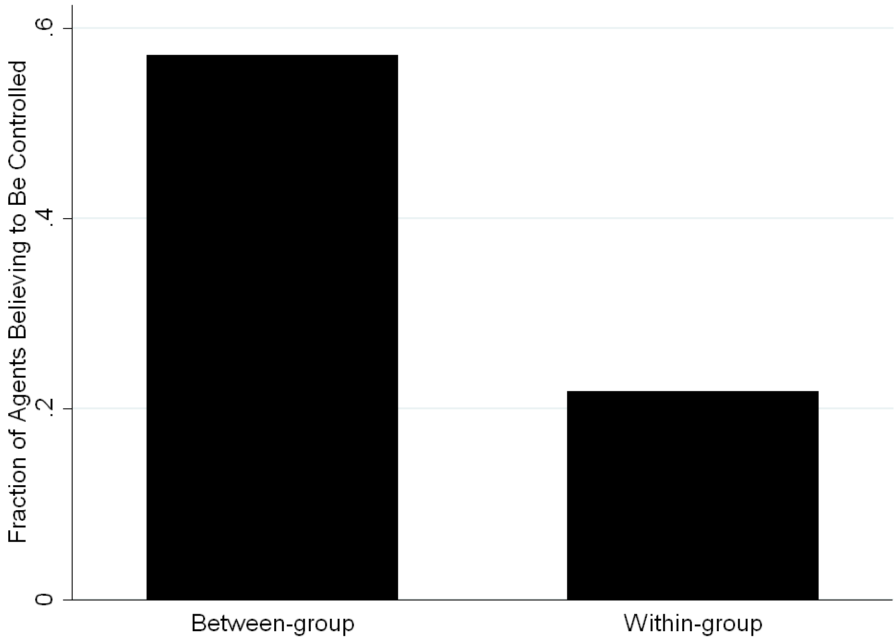
> **Result 6:** *Group identity affects agents' beliefs about the principal's behavior. A larger fraction of agents expects the principal to control in a between-group matching.*

Figure 3.3 displays agents' beliefs about the principal's decision to control by matching when we restrict the sample to agents that transfer more than 1 when not being controlled, that is, we include only agents whose transfers can potentially be crowded out. The share of agents who expects to be controlled in a between-group matching, $b_b = 0.57$, is significantly larger than in a within-group matching, $b_w = 0.21$



Notes: Panel A shows the distribution of agents' transfers with no control for between- and within-group interactions. Panel B shows the respective distribution when the principal decides to control.

Figure 3.2: Distribution of Agents' Transfers



Notes: The figure depicts average individual beliefs about being controlled as a function of matching. We restrict the sample to agents that transfer more than 1 when not being controlled.

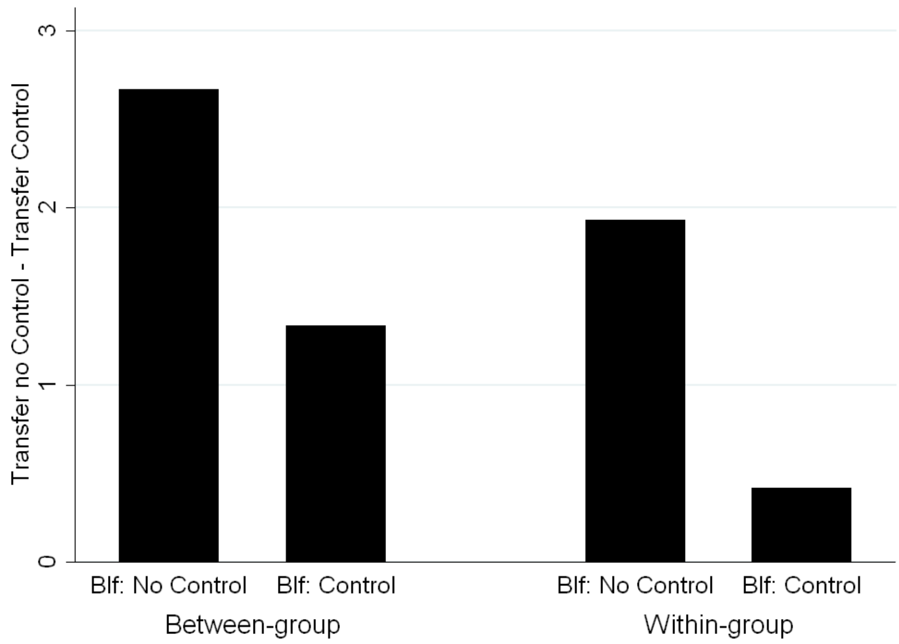
Figure 3.3: Average Beliefs about Principal's Decision to Control

(Fisher's exact test, p<0.001).

Result 5 shows that agents' transfers are crowded out more strongly if they expect principals not to control them. As group identity influences the beliefs of the agents (Result 6), combining these two findings leads crowding out to be stronger in situations in which principals and agents identify with the same group. This result is supported by the evidence Riener and Wiederhold (2012) provide. In their setting, where two persons interact, those who have shared a common interaction before and experienced the other as relatively fair, react more negatively towards control, in particular when they expect that the principal uses no or only low levels of control. In our setting, however, there is a countervailing effect:

Result 7: *Agents exhibit a "hostility effect". Keeping beliefs constant, agents reduce transfers more strongly in between-group than in within-group matchings.*

Once we control for their beliefs we find that agents react more negatively to being controlled in a between-group matching. Figure 3.4 shows the average crowding out,



Notes: The figure depicts average crowding out, i.e., transfers under no control - transfers under control, as a function of agents's beliefs and matching. We restrict the sample to agents that transfer more than 1 when not being controlled.

Figure 3.4: Average Crowding Out Depending on Agents' Beliefs and Matching

i.e., the transfer when not being controlled minus the transfer when being controlled, for different matchings and different beliefs of agents (being controlled or not being controlled). The figure again focuses on agents who transfer more than 1 when not being controlled. When expecting that the principal controls, agents' transfer is crowded out by 1.3 points on average in a between-group matching, while it is only crowded out by 0.43 points in a within-group matching (30% of crowding out in a between-group matching). Similarly, when agents believe that principals will not control, crowding out amounts to 2.6 points in a between-group matching but 1.9 points in a within-group matching (73% of crowding out in the between-group matching).

Table 3.2 shows the hostility effect in an OLS regression in which the dependent variable is the extent of crowding out. To capture the "hostility effect" we add as independent variables, first, a dummy for whether the principal-agent interaction is within-group and, second, we add agents' beliefs about principal's behavior. The specification of main interest is Column 1 on which the estimation of our simple model is based. Additionally, we include transfer when not being controlled (Column 2) and transfer when not being controlled squared (Column 3) as controls to demonstrate the robustness of our results. Lastly, in Columns 4 to 6 we use an alternative measure of crowding out as a dependent variable, the difference between transfers when not being controlled and when being controlled divided by the transfer when not being controlled, to show that results are also robust to alternative measurements of crowding out. The results indicate that for all specifications the hostility effect (which is captured in our model by δ) is significant, i.e., control has more "hidden costs" in between-group interactions if beliefs about principals' behavior are kept constant.

Table 3.2: Agents' Crowding Out

Dependent Variable:	Diff (1)	Diff (2)	Diff (3)	Fraction (4)	Fraction (5)	Fraction (6)
Within-group matching (=1)	-0.806* (0.460)	-1.002* (0.505)	-1.008** (0.489)	-0.166*** (0.0530)	-0.145** (0.0581)	-0.145** (0.0585)
Agent's Belief (Control=1)	-1.419*** (0.500)	-1.414*** (0.485)	-1.228*** (0.445)	-0.186** (0.0723)	-0.186** (0.0718)	-0.186** (0.0717)
Transfer (Control=0)	no	yes	yes	no	yes	yes
[Transfer (Control=0)]^2	no	no	yes	no	no	yes
N	97	97	97	97	97	97

Notes: Standard errors are in parentheses. All columns report results of OLS regressions. We restrict the sample to agents that transfer more than 1 when not being controlled. Columns (1) - (3): dependent variable is the individual difference between transfers under no control and under control. Columns (4) - (6): dependent variable is the individual difference between transfers under no control and under control divided by transfers under no control. Standard errors are clustered at the individual level.
Level of significance: * p<0.10, ** p<0.05, *** p<0.01

Using the three effects of group identity on the efficacy of incentives, (1) prosocial effect, (2) trust effect, (3) and hostility effect, we can now decompose the diff-in-diff and illustrate the relative importance of each of the three effects.[21]

Using these parameters, we get for the decomposition of the diff-in-diff:

[21]See Appendix B.3 for the details of the calculations.

$$\begin{aligned}\Pi_w - \Pi_b = & \underbrace{(s_b(t=0) - s_w(t=0)) + [s_w(t>1) - s_b(t>1)]\,\kappa}_{\text{Prosocial Effect: 0.26}} \\ & + \underbrace{[s_w(t>1)(1-b_w) - s_b(t>1)(1-b_b)]\,\gamma}_{\text{Trust Effect: 0.59}} \\ & - \quad \underbrace{s_b(t>1)\delta}_{\text{Hostility Effect: -0.52}} \\ & = 0.33\end{aligned} \tag{3.2}$$

These calculations show that the effects are countervailing: While the "prosocial" and the "trust" effect make control more detrimental in a within-group setting, the "hostility" that agents exhibit in a between-group matching has an influence in the opposite direction. In total, the difference between the average effect of control in a within-group matching and the average effect of control in a between-group matching is close to zero.[22]

3.5.2 Principals' Behavior

We now turn to principals' decision to control:

Result 8: *The majority of principals does not control.*

On average, 28% of principals control the agent. So, similar to Falk and Kosfeld (2006) we find that a substantial share of principals controls the agents even though the majority of principals does not control.[23]

Result 9: *Group identity affects principals' decisions: More principals control in a between-group matching.*

Principals' decision to control is influenced by group identity. The share of principals who control is 37% in a between-group and 18% in a within-group interaction. The difference is statistically significant (Fisher's exact test, p<0.03).

From an income maximization perspective principals do not behave optimally as not

[22] Also note that the estimated diff-in-diff of 0.33 seems to match the actual diff-in-diff of 0.28 quite well despite the set of simplifying assumptions used in Section 3.4.

[23] In treatment C5 (table 3, p. 1621) they observe that a fraction of 0.26 of principals controls the agent.

controlling is the best choice (on average). Note, however, that half of these subjects who control (wrongly) believe that agents will transfer 1 point or less.[24] Their behavior therefore is consistent with income maximization given their (incorrect) beliefs about agents' behavior. Also note that group identity affects the distribution of principals who expect low transfers. Around 18% (5%) of principals expect a transfer not larger than 1 in a between-group (within-group) matching.

3.6 Conclusion

While we know that incentive systems and control mechanisms designed for selfish individuals may not always be effective but can backfire, little is known about when crowding out is particularly severe. In this experiment we test whether the social relationship between the principal and agent, i.e., whether they share the same group identity, affects the impact of control on the performance provided by the agents.

The results confirm that agents react negatively to control. While the overall effect of control is the same when agent and principal belong to the same group and when they belong to different groups, the mechanisms which trigger the hidden costs of control are distinct and do depend on group identity. In general, agents react more negatively when they expect principals not to control. If agent and principal share the same group identity, agents are more likely to expect not to be controlled leading to crowding out. If agent and principal belong to different groups, the hidden costs of control are due to a different mechanism. Holding agents' beliefs constant, they interpret controlling from an out-group principal as more 'hostile' and as a consequence reduce their transfers to the principals. In the end, control has 'hidden costs' in both in-group and out-group matchings – but for different reasons.

The results have important implications. First, many principal-agent interactions are between individuals from different ethnic, racial or religious groups, or between different firms or, within the same firm, between workers belonging to different departments or with different responsibilities and wages. Our findings suggest that such group structures may interact with control schemes and potentially also with

[24]The other half of subjects who control expect on average 4.9 points. Note that their expectation is substantially smaller than the average expectation for transfers larger than 1 point of subjects who do not control (7.1 points).

incentive systems.

Secondly, we isolate two different mechanisms through which control devices have detrimental effects on individual performance. We believe this may help further our understanding of the general conditions under which control mechanisms or incentives have the potential to be detrimental and of the factors that are likely to determine the size of the hidden costs of control. Moreover, the group-specific mechanisms for how control is perceived indicate that it is important to take group structures into account. As suggested by Akerlof and Kranton (2005), group identity does affect principal-agent relationships and the way incentives work. Whether the principal and the agent share the same identity, i.e., are in-group members, changes in important ways how incentive and control mechanisms are perceived.

Future work should extend this line of research to investigate the impact of group identity on the efficacy of incentives, such as bonuses or penalities, but also on other aspects of principal-agent relations, for example hold-up problems.

Chapter 4

Efficiency Concern under Asymmetric Information

4.1 Introduction

In simple experiments such as the Ultimatum Game, the Trust Game, or the Dictator Game subjects regularly deviate from a selfish maximization of payoffs and behave prosocially (Camerer, 2003). To explain these deviations, it has been proposed to extend individuals' preferences by a social component while keeping the assumption of utility maximization. Prominent examples of this approach are inequality aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000), efficiency concern, and maximin preferences (Charness and Rabin, 2002).

If subjects' behavior is guided by social preferences, these theories should help to understand how individuals react to policy proposals as most policies not only involve the individual's own payoff but affect others' payoffs as well. Moreover, for policy makers a concern for efficiency is a particularly interesting motive because it could allow to implement welfare increasing allocations even though standard theory based on purely selfish agents will predict these policies to fail. For instance, an economic reform may be implemented despite the fact that a majority incurs a loss: When the gains for those who profit are sufficiently large and some individuals who are negatively affected are concerned with efficiency, these individuals may tip the scales such that the reform is implemented nevertheless.

Most policy questions such as reform proposals are notoriously plagued by information asymmetries. Individuals will be better informed about how a reform affects themselves than how it affects others. The laboratory experiments providing evidence for social preferences are conducted in an environment where agents have full information about the costs and benefits of all agents. The question is whether individuals whose behavior is in line with social preference theories if they are perfectly informed about the effect of the reform, still behave prosocially when they are *not* perfectly informed about the gains others have.

In this chapter, we investigate whether asymmetric information affects prosocial behavior of subjects whose behavior is in line with an efficiency concern under symmetric information. Furthermore, as any policy that affects the efficiency of an allocation will also have distributional consequences (e.g., decrease inequality), we also investigate how subjects, whose behavior is in line with inequality aversion under symmetric information, behave under asymmetric information.

In the case of asymmetric information it is possible that subjects behave less prosocially than they do under symmetric information for two reasons. First, subjects with a concern for efficiency face the risk that their transfer produces only little value; maybe too little to make them behave prosocially if they knew the value exactly. Asymmetric information may thereby influence behavior: Instead of choosing the risky[1] (and prosocial) option they may choose the safe (and selfish) one. Secondly, cognitive dissonance theory (e.g., Konow, 2000), which has been proposed as an alternative to the utility maximization paradigm, may provide another explanation why behavior may change under asymmetric information. According to this theory, individuals experience cognitive dissonance due to the internal conflict they are exposed to. On the one hand, they want to keep their endowment, but on the other hand they feel obliged to behave prosocially. Under asymmetric information where subjects do not know the exact benefits, this dissonance may be partly resolved as they may perceive it as more justified not to transfer and therefore behave less prosocially.

We conduct a laboratory experiment with the following design: Two subjects, A and B, are matched; both subjects have an initial endowment; A makes a binary decision about a money transfer to B. Accepting the transfer causes known costs for A and benefits b for B, with the benefits being larger than the costs. As treatment we vary

[1]Risky with respect to the other's payoff.

the information A has about the benefit level b. While A is informed about the exact level of b in the control setting, he does not know the exact level in the treatment.

We focus on this simple asymmetry for three reasons. First, it is natural to assume that even if people do not know the exact effect of a policy on themselves, they still can evaluate it much better than an effect on others. Hence, for simplicity we assume that A knows the costs precisely. Secondly, with asymmetric information on both sides the identification of effects becomes more complicated and to consider only one side on its own is a necessary first step. Moreover, identification is also the reason why we focus on a simple non-strategic interaction. It is clear that once B can influence the outcome, $A's$ behavior under asymmetric information may change; and obviously it becomes even more complicated once we allow for group decision-making (i.e., voting).

Thirdly, we think that the above setting has applications. Consider, for instance, donations. While people will have a more or less clear idea of the costs of a donation, they will not be perfectly certain about the benefits their transfers generate, whereas the recipient will have a clear idea. Depending on the information available, the willingness to donate will vary. Similarly, if several options are available, a preference for a particular one may reflect that it produces a more certain effect. For instance, Jacobsson et al. (2007) find evidence that subjects donate more to smoking diabetes patients when they can do so in the form of nicotine patches instead of monetary transfers. While the authors interpret this finding as evidence for a paternalistic nature of giving, a complementary explanation is that subjects want to maximize social surplus. The nicotine patches are socially valuable (as supporting others to quit smoking and thereby reducing a negative externality) and at the same time the more certain option than giving money where it is unclear whether it is not spent in a socially less valuable way (e.g., on tobacco or sweets). The results by Eckel and Grossman (1996) point in a similar direction. They find that dictators are more willing to donate if they know that the Red Cross receives the money and not an anonymous person in the laboratory. Again, one can interpret the Red Cross alternative as less risky with respect to its social value.

Besides the information asymmetry, we introduce a monetary reward for prosocial behavior as a further treatment variable. There is substantial experimental evidence that incentives can "crowd out" prosocial behavior (see Bowles and Polanía-Reyes

(2012); Gneezy et al. (2011) for surveys of the literature). We want to test whether the incentive has the same effect under symmetric and asymmetric information. Under asymmetric information the incentive could interact with the uncertainty. Subjects could "perceive" the reward level as being related to the unknown benefit level and interpret the reward as a signal (Bolle and Otto, 2010).

In previous laboratory experiments finding crowding out, this effect cannot occur by design because there is no uncertainty. With evidence from the field this is different (e.g., Frey and Oberholzer-Gee, 1997; Gneezy and Rustichini, 2000): Subjects do not know exactly the benefits/costs they impose on others and hence could interpret the reward as a signal of them.

Our main results are that under symmetric information (i) a substantial share of individuals (36%) behaves prosocially, (ii) 18% of subjects make a choice that is consistent with a concern for efficiency, 7% of subjects make a choice that is consistent with inequality aversion, and 11% transfer independently of the value of b, their transfers are in line with an efficiency concern or maximin preferences.

Moreover, subjects on average behave more prosocially when the exact benefit level is unknown compared to a situation where the level is known. More subjects transfer under asymmetric information than for any level of b under symmetric information.

Furthermore, we do not find substantial evidence for crowding out. The monetary reward has a positive though not statistically significant effect.

At the individual level, behavior under asymmetric information is consistent with predictions based on prominent examples of social preference theories (Fehr and Schmidt, 1999; Charness and Rabin, 2002), and we do not find evidence that information asymmetries have a negative effect.

As a consequence and given the parameters chosen in our experiment, the information asymmetry actually *improves* the efficiency of the allocation. Under symmetric information, where A knows the exact degree of efficiency, subjects, whose choices are consistent with inequality aversion, only transfer when the efficiency gain is not too large, whereas subjects, whose choices are consistent with an efficiency concern, only transfer when the degree of efficiency is sufficiently large. Under asymmetric information, however, both types tend to transfer.

To the best of our knowledge we provide the first experimental design in which an information asymmetry regarding the degree of efficiency is introduced in a basic non-strategic setting. In contrast to existing experiments, only the information which the dictator A has available varies while B is fully informed. $A's$ decision does neither make his payoff nor $B's$ payoff more risky. Moreover, this is the first experiment that investigates whether a financial incentive can have a negative effect under both asymmetric and symmetric information in order to distinguish different channels of crowding out. However, in our experiment we do not find substantial evidence for crowding out at all. Furthermore, our findings also confirm previous results (e.g., Engelmann and Strobel, 2004): A concern for efficiency plays a crucial role in explaining prosocial behavior. Lastly, our experiment provides evidence that simple information asymmetries do not reduce prosocial behavior.

The chapter is organized as follows: In the next section, we discuss the related literature. In Section 4.3, we present the experimental design followed by a discussion of behavioral hypotheses in Section 4.4. Section 4.5 presents the results and Section 4.6 concludes.

4.2 Related Literature

The experimental evidence regarding prosocial behavior in simple two-person experiments such as the Trust Game, the Ultimatum Game, and the Dictator Game is extensive. The central and robust finding is that subjects regularly deviate from purely selfish behavior though to varying degree.[2] One motive to explain deviations from the selfish benchmark that received particular attention has been inequality aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000): Subjects are not only guided by their own payoff but experience disutility when payoffs differ between participants. More recently, the explanatory power of these theories has been questioned.[3]

The experiments by Charness and Rabin (2002) and Engelmann and Strobel (2004) discriminate between alternative theories of social preferences (inequality aversion, maximin preferences, and efficiency concern) and evaluate their explanatory power

[2]For reviews of the literature see Camerer (2003) and specifically for the Dictator Game see Engel (2011).

[3]See for example Engelmann and Strobel (2002) for a discussion.

for subjects' behavior in simple distribution games. In Charness and Rabin (2002), two to three players are involved; in some of the games one subject decides alone (e.g., variants of the Dictator Game) and in some games two subjects together influence the allocation (e.g., variants of the Ultimatum Game). In Engelmann and Strobel (2004), one subject has to choose among three alternatives where each alternative allocates transfers to him and to two other subjects. Both articles report a substantial share of subjects who are willing to accept private costs when they can thereby increase the total sum of payoffs. Moreover, maximin preferences play an important role for explaining subjects' behavior whereas - in contrast to previous experiments - inequality aversion has less explanatory power.[4]

A few papers (e.g., Mitzkewitz and Nagel, 1993; Rapoport and Sundali, 1996; Huck, 1999; Güth et al., 1996) introduce asymmetric information regarding the outcome in simple two-person games. In contrast to our experiment, however, the asymmetric information is on the other side: The proposer of a bargaining outcome is aware of the exact amount of money to be distributed while the recipient is not. Only in Klempt and Pull (2009), the information asymmetry is on the side of the proposer. They find that dictators demand a higher share of the pie compared to a situation where they know the size of the pie. As in their setting uninformed dictators run a risk that a high proposal leads to a zero transfer when the actual pie is small, it is not clear what drives the effect: information or risk attitudes.[5] In contrast to their approach, we are not concerned with a fixed pie that can be distributed but with a decision that increases the pie. In our setting, asymmetric information concerns the increase of efficiency a dictator can create by behaving prosocially.

The paper which is closest to our design with regard to the information asymmetry is the first treatment in Dana and Weber (2007). In their setting, a dictator has to make a choice between two allocations where he initially only observes his own payoff but not the payoff for the recipient. Subjects can choose to be informed about the other's payoff before they make a decision while the recipient is neither informed about the dictator's choice to reveal nor about his transfer decision. The recipient only observes his own

[4]Other experiments also find evidence that individuals' choices are in line with an efficiency concern (e.g., Kritikos and Bolle, 2001).

[5]In their setting, the pie can take on two values, 8 and 20. When a dictator is not informed about the value and, for instance, proposes 9 for himself but the actual pie is 8, then he receives nothing.

payoff. Hence, he cannot distinguish whether the dictator behaved selfishly or chose to stay uninformed and decided without knowing what would be the prosocial choice. They find that a significant share of dictators prefers to be uninformed and decides more often to behave selfishly compared to a benchmark where dictators are informed about the other's payoff right from the beginning. Hence, they provide evidence that subjects want to appear as prosocial instead of "truly" being altruistic.[6]

In our experiment, the information asymmetry differs. Player B is perfectly informed about the level of benefit and about $A's$ decision. A in contrast, does not know the benefit level and he has no possibility of learning the value. Moreover, in Dana and Weber (2007) the equal and the efficient choice are the same (versus a more selfish and inequality increasing outcome). In contrast, in our case the benefit can take on different values. While the choice is always efficiency increasing, its distributional consequences vary over different values of b.

A recent strand of literature (Brennan et al., 2008; Güth et al., 2008; Bradler, 2009) investigates how risk regarding other subjects' payoffs affects prosocial behavior. In these experiments, a dictator has to make choices among lotteries instead of outcomes. Hereby, attitudes towards private and collective risks can be evaluated. The paper which is closest to ours is Brennan et al. (2008).[7] In their experiment, each dictator is required to evaluate four different allocations. Each allocation assigns a payoff to the dictator and to the recipient either in a probabilistic or in a deterministic way. The experiment shows that dictators' behavior is significantly different when they face risks regarding their own payoff compared to a situation with no risk. Yet, the authors do not find evidence that the risk recipients are exposed to affects dictators' decisions.[8]

[6]There are several papers that investigate whether subjects are more selfish when the actions of dictators are not fully observable (e.g., Andreoni and Bernheim (2009) and the "multiple dictator" treatment as well as the "plausible deniability" treatment in Dana and Weber (2007)). They find a significantly less generous behavior of dictators relative to the standard game thus also supporting the view that subjects are not truly prosocial but want to appear as if. In contrast to these papers, we are not concerned with uncertainty about the decision. In our setting, subjects know exactly whether the other subject behaved prosocially or not; asymmetric information only concerns the possible allocation outcomes.

[7]Güth et al. (2008) conducted a similar experiment where they added another dimension: time preferences. Regarding risk, with which our analysis is concerned, they obtain similar results.

[8]Rohde and Rohde (2011) similarly find only weak evidence that the risk recipients' are exposed to influences dictators. Results by Bradler (2009) indicate that subjects are willing to risk parts of their own payoff when they can thereby increase the payoff of the recipient from zero (respectively, from a very small amount).

In our setting, under asymmetric information A has to make a decision when he does not know the exact efficiency parameter. The main difference between our analysis and the latter strand of literature is that dictators affect the risks of others while in our setting they do not. In these papers two different kind of risk concerns regarding the others' payoff are intertwined. First, as those papers posit, it may be that dictators put themselves in the shoes of others and do not want to expose recipients to risk. Secondly, however, if subjects exhibit social preferences based on outcomes, then they face a risk themselves: When choosing a lottery, they face an uncertain outcome regarding the others' payoff directly affecting their own utility. In the above designs, both effects are mixed and not distinguishable. Moreover, A is exposed to direct risks that affect his own payoff as well, which can influence his attitude towards the risk of B. In our approach, these effects are disentangled. Subject A is not exposed to risks regarding his own payoff. We also do not consider a situation in which A affects the risk of B: In our setting, the gain B receives when A transfers is determined before A makes a decision.

Lastly, our experiment is related to the literature on crowding out.[9] For instance, Gneezy and Rustichini (2000) find that a financial punishment for late-comers actually increases late-coming by parents in an Israeli kindergarden. Similarly, Frey and Oberholzer-Gee (1997) conducted a study in Switzerland where subjects were offered a compensation for the willingness to accept a nuclear waste facility in their community. The willingness to accept the facility decreased from 50% to 25% when a financial compensation was offered. Besides, numerous laboratory experiments exist which find a similar effect, typically in simple 2-persons-interactions such as the Gift Exchange Game (e.g., Fehr and Rockenbach, 2003) or even simpler interactions regarding control mechanisms (e.g., Falk and Kosfeld, 2006).

4.3 Experimental Design

We implement the following experimental design: There are two agents, A and B. A has an endowment, e_A, of 100 points (100 points are equivalent to 10 EUR), B has an endowment, e_B, of 50 points. Only A makes a (binary) decision. He can decide

[9]For surveys of the literature see Bowles and Polanía-Reyes (2012) and Gneezy et al. (2011).

Part 1	b is *not* known to A
Part 2	b is *not* known to A. A receives a reward if he transfers.
Part 3	b is known to A. A receives a reward if he transfers.
Part 4	b is known to A.

Table 4.1: Overview over Parameters in the 4 Parts of the Experiment

whether he wants to transfer 20 points. If A transfers, B receives a benefit b, with $b \in \{25, 30, 40, 50, 60, 70\}$. The decision reflects a situation where an efficiency gain is possible. We chose the initial endowments such that a transfer will result in an efficiency gain *and* a decrease in inequality for low values of b ($b \leq 30$). For values of $b > 30$ inequality increases in b. Hence, depending on the exact value of b a decision to transfer can be motivated by a concern for efficiency or by a concern for equality.[10]

The experiment consisted of 4 parts. Table 4.1 provides an overview over the parameters that change in each part.

4.3.1 Treatments

The main treatment variable in our experiment is whether A knows the exact benefit b when he makes a decision or whether his knowledge about b is limited to the distribution from which b is drawn.

In part 1, participants in the role of A have to make a decision under asymmetric information with respect to b. The exact value of b is determined in the following way: Participants are presented an urn from which a value for b is drawn *before* subjects make a decision. The urn contains the following balls each representing one value of b: $25, 30, 40, 50, 60, 70$. Agent A is not informed about the ball which is drawn while it is disclosed to agent B. Moreover, it is public information that B knows the exact benefit. Subjects make only one decision whether to transfer or not.

In part 4, participants in the role of A make decisions under symmetric information. That is, A knows the exact value of b. We use the strategy method for a complete response by subject A for each value of b ($25, 30, 40, 50, 60, 70$). This is crucial because it allows us to describe the values of b for which subjects are willing to transfer under symmetric information and whether their behavior is consistent with their decision

[10] If endowments were such that A had an endowment smaller than or equal to B, any transfer would always increase inequality. Then, we could only focus on the efficiency motive.

under asymmetric information. Subjects had to make one decision (transfer yes/no) for each level of b. After they had made their decisions, one ball from the urn was drawn which determined which decision was payoff-relevant for part 4.

Part 2 (3) is identical to part 1 (4), but as an additional treatment we introduce a reward for A if he transfers the 20 points. The goal is to investigate whether a reward has a differential effect under asymmetric and symmetric information. In part 2, the reward r could take on two values, $r_L = 5$ and $r_H = 10$. The subjects only knew that it holds that $r \in [5, 10]$ and that the exact value of r was determined after b had been drawn, but they did not know how the reward was chosen. In fact, the reward was determined by a lottery after b had been drawn and in all cases $r = 5$ was drawn.[11] After the reward was determined, all subjects were informed about the exact value of r. In part 3, the reward was fixed with $r = 5$. Before making a decision, subjects knew the exact value of r.

At the end of the experiment, one of the four parts was randomly drawn. This part determined the final payoff of participants.

4.3.2 Procedural Details

Subjects were randomly assigned to either role A or role B at the beginning of the experiment.[12] They kept this role over the course of the experiment. Subjects knew that the experiment comprised four parts, but they did not know the content of each part in advance. Subjects received separate instructions at the beginning of each part with the information that instructions for subsequent parts would follow.

As we are interested in whether crowding out could occur under asymmetric information due to a signaling effect of the reward, we let subjects choose under asymmetric information first. Symmetric information followed afterwards because we wanted to avoid that the randomly determined benefit level would influence subjects' beliefs under asymmetric information. Since we similarly cannot exclude the possibility that behavior under asymmetric information affects behavior under symmetric information, we also did a robustness check where we changed the sequence

[11] The probability is 1/10 for the high reward and 9/10 for the low reward.

[12] Subjects in the role of B had no decision to make in the experiment, but we elicited their beliefs about what they thought subjects A would do.

of parts. Subjects then first decided under symmetric information (see Section 4.5.4). So, in addition to analyzing behavior from four subsequent parts one can also compare the first decisions from our baseline sessions and the robustness sessions without the problem that they may have been affected by previous actions or parameters.

We ran five sessions with 90 subjects in the sequence of parts described above and three more robustness sessions with 60 subjects. The experiments were conducted at the experimental laboratory at the University of Mannheim in March to May 2012 (baseline sessions) and November 2012 (robustness sessions). The experiment was computerized using z-tree (Fischbacher, 2007). Subjects were students from the University of Mannheim from different fields. They were recruited using the online system ORSEE (Greiner, 2004). Each session took between 35 and 40 minutes and comprised 16-20 subjects. The average earnings were 7.80 EUR.

4.4 Behavioral Predictions

Under asymmetric information subjects make one choice: Whether they want to transfer or not. To understand whether the information asymmetry has an effect on behavior, it is crucial to put it into context with behavior when subjects know the benefit level. For instance, suppose subject i does not transfer under asymmetric information. Without further information it is not clear whether the subject behaves generally selfishly (independently of the asymmetric information) or whether asymmetric information has an effect on his behavior. Suppose, as an extreme example, that the same subject, who did not transfer under asymmetric information, transfers for all benefit levels when he knows b exactly. Then, we can argue that asymmetric information had an effect on his behavior. The information what subjects do under symmetric information is necessary to evaluate the effect of the information asymmetry. Hence, we begin with a discussion how subjects in the role of A behave under symmetric information (part 4). To understand what behavior we can expect, we follow the literature and base our predictions on prominent social preference theories.

Next, we extrapolate these theories to the asymmetric information case and discuss what behavior these theories predict when subjects do not know the exact benefit level b (part 1) conditioning on their behavior under symmetric information. Moreover, we will argue why we may observe a deviation from these predictions when subjects react to the risk they are exposed to and we will also contrast the predictions with an alternative explanation of why behavior may change under asymmetric information, a cognitive dissonance approach. Lastly, we will discuss how the introduction of the reward (parts 2 and 3) can affect behavior.

4.4.1 Symmetric Information

We focus on three possible motives for prosocial behavior that are prominent in the literature: efficiency concern, maximin preferences, and inequality aversion. We will first discuss the simple utility specification of Charness and Rabin (2002) modeling a concern for efficiency and maximin preferences. Secondly, we will discuss inequality aversion as modeled by Fehr and Schmidt (1999). In both cases we will briefly describe the utility functions and state which transfer patterns are predicted. All derivations can be found in Appendix C.2.

Efficiency Concern and Maximin Preferences

Following Charness and Rabin (2002), the utility of an individual i in the role of A is:

$$U_A^i(\pi_A, \pi_B) = (1-\lambda^i)\pi_A + \lambda^i \left[\delta^i \cdot \min\left[\pi_A, \pi_B\right] + \left(1-\delta^i\right)(\pi_A + \pi_B)\right]$$

where π_A and π_B are the monetary payoffs of A and B, respectively. Parameter $\lambda^i = 0$ corresponds to purely selfish preferences. For $0 < \lambda^i < 1$, $\delta^i = 0$ means that prosocial behavior is only driven by an efficiency concern, i.e., a desire to maximize total payoffs, and $\delta^i = 1$ means that prosocial behavior is only driven by maximin preferences, i.e., a desire to maximize both players' minimal payoff.

Applied to our setting, subject i has to compare two levels of utility. The utility $U_A^i(100, 50)$ if i does not transfer:

$$U_A^i(100, 50) = (1-\lambda^i)100 + \lambda^i \left[\delta^i 50 + \left(1-\delta^i\right)(100+50)\right] \qquad (4.1)$$

and for a given value of b the utility $U_A^i(80, 50+b)$ if i transfers.

$$U_A^i(80, 50 + b) = (1 - \lambda^i)80 + \lambda^i \left[\delta^i \cdot \min\left[80, 50 + b\right] + \left(1 - \delta^i\right)(80 + 50 + b)\right] \quad (4.2)$$

Given the utility function, when do subjects transfer? For the sake of exposition, we focus on two discrete cases: Subjects either have a pure efficiency concern ($\delta^i = 0$) or have pure maximin preferences ($\delta^i = 1$).

Efficiency Concern When subjects have a concern for efficiency, they will trade-off their costs with the benefits and the consequent efficiency gain. Given their individual parameter λ^i and the value of b, they will transfer if

$$U_A^i(80, 50 + b) > U_A^i(100, 50) \Leftrightarrow \lambda^i > \frac{20}{b}. \quad (4.3)$$

Hence, the more an individual i cares for efficiency (the higher λ^i) the lower the minimal value of b for which i would transfer. Thus, subjects with an efficiency concern should exhibit the following transfer pattern: Either they do not transfer at all or they transfer for a particular value of b^* and all values $b > b^*$.

Maximin Preferences Subjects with maximin preferences will transfer the 20 points if it holds that:

$$U_A^i(80, 50 + b) > U_A^i(100, 50) \Leftrightarrow (1 - \lambda^i)80 + \lambda^i \min\left[80, 50 + b\right] > 100 - \lambda^i 50 \quad (4.4)$$

The transfer pattern of subjects with maximin preferences is similar to the transfer patterns of efficiency concern: Either they do not transfer at all, or they transfer for a particular value of b^* and all values $b > b^*$. In contrast to efficiency concern, there are only two thresholds: either $b = 30$ or $b = 25$.[13]

[13] In the case where prosocial behavior is not solely driven by an efficiency concern, respectively maximin preferences, ($\delta^i \in (0, 1)$) the transfer pattern is analogous: Either subjects do not transfer at all, or they transfer for a particular value of b^* and all values $b > b^*$.

Inequality Aversion

In contrast to maximin preferences, the specification of Fehr and Schmidt (1999) can lead to behavior that is consistent with transfers for low values of b but no transfers once the benefit level is above 50. The utility function by Fehr and Schmidt (1999) for the two-player case is as follows,

$$U_A^i(\pi_A, \pi_B) = \pi_A - \alpha^i \max[0, \pi_B - \pi_A] - \beta^i \max[0, \pi_A - \pi_B]$$

with the assumption that $\beta^i \leq \alpha^i$ and $0 \leq \beta^i < 1$. Based on this utility function, the following transfer patterns are possible: Individual i either never transfers or he transfers for $b = 30$ alone, or for $b = 30$ and $b = 25$, or for $b = 30$, $b = 25$, and $b = 40$, or for $b = 30$, $b = 25$, $b = 40$, and $b = 50$. However, he will never transfer for values of $b = 60$ or $b = 70$.[14]

The social preference theories which we discuss share that - depending on the unobserved utility parameters $\lambda^i, \delta^i, \alpha^i, \beta^i$ - subjects behave either selfishly or prosocially. The theories, however, differ with regard to the transfer patterns they predict over the range of possible values of b if subjects are sufficiently prosocial. In particular, inequality aversion and efficiency concern predict differing patterns. As indicated in the theoretical discussion above and supported by the empiricial evidence (cf. Engelmann and Strobel (2007)), we do not expect that behavior will only follow one type of preferences. Instead, we expect heterogeneity with respect to selfishness as well as with respect to the type of social preferences.

To summarize our discussion, social preference theories predict the following

[14]See Appendix C.2 for the details.

patterns:

Hypothesis 1:

a) *Efficiency concern: Subjects* either *will transfer for a certain level of b^* and for all values of b above this threshold* or *will not transfer at all.*

b) *Maximin preferences: Subjects* either *will transfer for a certain level of b^* and for all values of b above this threshold* or *will not transfer at all. The threshold is either $b = 25$ or $b = 30$.*

c) *Inequality aversion: Subjects* either *will transfer for $b = 30$, or for $b = 30$ and $b = 25$, or for $b = 30$, $b = 25$, and $b = 40$, or for $b = 30$, $b = 25$, $b = 40$, and $b = 50$,* or *will not transfer at all.*

4.4.2 Asymmetric Information

Under asymmetric information A does not know the value of b. Given the lottery which determines the value of b, the expected value is roughly 45. We will discuss first how subjects should behave according to the social preference theories presented above. As before, for details of the derivations see Appendix C.2. Then, we will outline explanations why they could deviate from these predictions.

Social Preferences

Charness and Rabin (2002) and Fehr and Schmidt (1999) Predictions What behavior can we expect for different types of social preferences under asymmetric information? For an individual i who behaves according to inequality aversion one can show that the expected utility from transferring under asymmetric information is smaller than the utility from transferring for $b = 40$ under symmetric information but larger than the utility from transferring for $b = 50$ under symmetric information. Hence, if i transfers for $b = 50$, he should transfer under asymmetric information. If, however, i does not transfer for $b = 40$, he should not transfer under asymmetric information.

For an individual whose behavior is in line with an efficiency concern the situation is analogous. One can show that the expected utility from transferring under asymmetric information is smaller than the utility from transferring for $b = 50$ under symmetric information but larger than the utility from transferring for $b = 40$ under symmetric information. Hence, if i transfers for $b = 40$, he should transfer under asymmetric information. If, however, i does not transfer for $b = 50$, he should not transfer under asymmetric information.

Thus, we can formulate the following hypothesis for efficiency concern and inequality aversion:

Hypothesis 2: *CR/FS Asymmetric Information*

a) *Efficiency concern: Subjects will not transfer under asymmetric information if they do not transfer for $b = 50$ under symmetric information.*

b) *Efficiency concern: Subjects will transfer under asymmetric information if they transfer for $b = 40$ under symmetric information.*

c) *Inequality aversion: Subjects will not transfer under asymmetric information if they do not transfer for $b = 40$ under symmetric information.*

d) *Inequality aversion: Subjects will transfer under asymmetric information if they transfer for $b = 50$ under symmetric information.*

For maximin preferences, the hypotheses one can derive are weaker as one can only observe three patterns under symmetric information: Subjects either do not transfer at all, or they transfer for all values of b, or they transfer for all values of b larger than 25. In the case that subjects do not transfer at all, they should obviously not transfer under asymmetric information. Similarly, if subjects transfer for all values of b, obviously they should transfer under asymmetric information. If subjects only transfer for $b > 25$, there exists a narrow parameter range of λ^i such that they should not transfer under asymmetric information whereas otherwise they should.[15] In our experiment, however, the last case does not play a role because we do not observe

[15] More precisely, when $\frac{24}{60} < \lambda^i < \frac{24}{59}$, then the utility from not transferring $(100 - 50\lambda^i)$ is smaller than the utility from a transfer with 30, 40, 50, 60, or 70 but larger than the expected utility $(80 - \frac{5}{6}\lambda^i)$.

subjects who do not transfer for $b = 25$ but for all values above 25.

Hypothesis 3: *CR Asymmetric Information II*

a) *Maximin preferences: Subjects will not transfer under asymmetric information if they do not transfer for any value of b under symmetric information.*

b) *Maximin preferences: Subjects will transfer under asymmetric information if they transfer for all values of b under symmetric information.*

Deviations from CR/FS Asymmetric Information Prediction The utility functions we consider are not linear, but given our parameters they imply that under asymmetric information subjects behave as under symmetric information when the benefit is equal to the expected value. In contrast to the decision under symmetric information however, under asymmetric information subjects face a risk: If subjects transfer, a value of b can realize for which they would not transfer if they knew it for certain. Alternatively, they can behave selfishly and do not transfer. Then, they risk that they choose not to transfer for a value of b under which they would transfer if they had the information available. As it is the case with decisions in other settings where solely their own payoff is at risk, in our design subjects may be affected by the risk regarding the other's payoff and deviate from the CR/FS predictions.[16] In this case, we will observe a more selfish behavior at the individual level when they are negatively affected by risk: Subjects who transfer for both $b = 40$ and $b = 50$ may not transfer under asymmetric information.[17]

Cognitive Dissonance Approach

So far we assumed that behavior follows the standard approach: maximization of utility functions with the additional ingredient of social preferences. There is, however, an alternative approach based on cognitive dissonance theory (Konow, 2000) which has been used to explain behavior in the dictator game and which could become relevant

[16]Note that neither Fehr and Schmidt (1999) nor Charness and Rabin (2002) discuss the possibility of risk regarding the others' payoffs and it is not obvious how to implement it within their framework.

[17]Alternatively, we can also observe a more prosocial behavior when subjects react positively to the risk: Subjects who do neither transfer for $b = 40$ nor for $b = 50$ under symmetric information, will then transfer under asymmetric information.

in particular under asymmetric information. According to this theory, subjects want to achieve a high payoff for themselves and to behave fair at the same time. They experience dissonance when decisions have to be made where these two objectives are in conflict. Moreover, "[t]he agent is motivated to reduce dissonance and may, generally speaking, do so by altering behavior, e.g., when the dictator takes less, and/or by changing beliefs, e.g., when the dictator believes it is fair to take more than the fair amount." (Konow, 2000, p. 1076). The experiment by Dana and Weber (2007) provides evidence for such a mechanism as a substantial share of dictators decides not to get informed about the consequences of their decision and also behaves less prosocially.[18]

Under symmetric information subjects are exposed to dissonance of being nice and keeping the money for themselves. Under asymmetric information the dissonance may be partly resolved by the uncertainty about b. Subjects have some "moral wiggle room" to justify selfish behavior, because values of b may realize under which they feel less compelled to transfer. Hence, they may reduce transfers compared to the symmetric information situation and behave less prosocially than the CR/FS predictions. Summarizing the alternatives to the CR/FS predictions:

> **Hypothesis 4:** *Subjects deviate from the CR/FS predictions and behave less prosocially: Subjects who transfer under both $b = 40$ and $b = 50$ may not transfer under asymmetric information.*

4.4.3 Effect of Reward

In part 2, we introduce a reward for prosocial behavior. The reward r can take on two values, $r_L = 5$ and $r_H = 10$. The introduction of the reward can affect behavior in two ways. First, the reward alters the price of transferring money: The prosocial action costs less, it may thereby shift thresholds at which subjects are willing to transfer downwards in the case of an efficiency concern and in the case of maximin preferences. In the case of inequality aversion it may increase the set of values under which an

[18] More supportive evidence can be found in Haisley and Weber (2010). In their experiments dictators form self-serving beliefs about the likelihood that a positive payoff of the recipient realizes under ambiguity. Subjects also behave more selfishly compared to a situation where the likelihood is known.

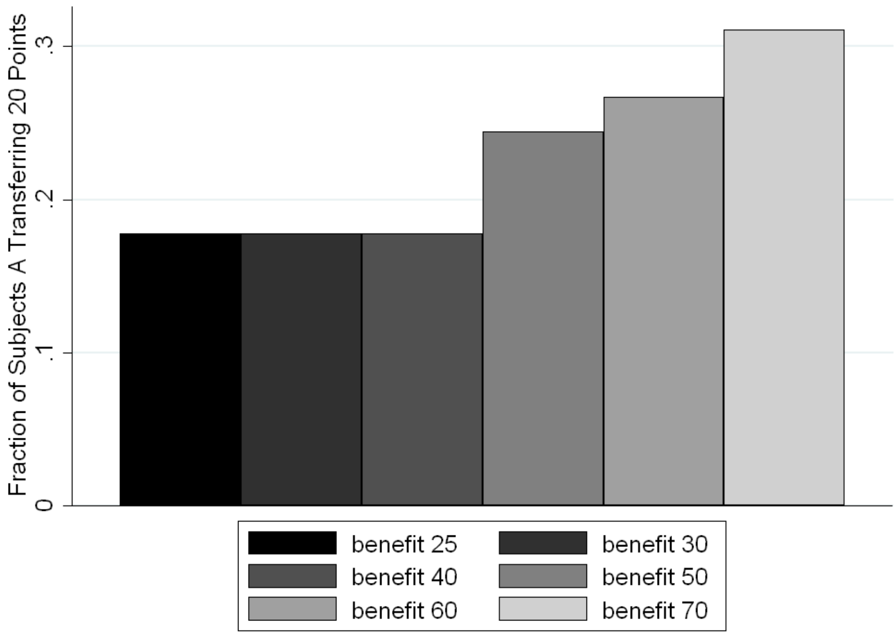
individual is willing to transfer. Thus, we can formulate the following hypothesis:

Hypothesis 5: *The reward decreases the costs of behaving prosocially under asymmetric and symmetric information and will have two effects: The number of subjects who transfer weakly increases as some subjects who did not transfer before may be willing to transfer with reward. Moreover, under symmetric information for subjects whose behavior is in line with social preferences and who transferred without the reward, the set of values of b for which they transfer weakly increases.*

Instead of merely changing the cost parameter - which would have the same effect on payoffs as the reward - we introduce this change as a reward to test whether an incentive can lead to crowding out in our setting. Under asymmetric information the incentive could interact with the uncertainty. Subjects may "perceive" the reward level as being related to the unknown benefit level and interpret the reward as a signal (Bolle and Otto, 2010) (a low reward level as a signal for low values of b). In previous laboratory experiments that find crowding out, this effect is ruled out because by design there is no uncertainty. Looking at the evidence from the field (e.g., Frey and Oberholzer-Gee, 1997; Gneezy and Rustichini, 2000), however, where subjects have private information about the benefits/costs they impose on others, a signal effect can play a role. We also introduced a fixed reward in part 3, where information is symmetric, as a control. On the one hand, we want to check whether there is a price effect under symmetric information and on the other hand we want to rule out that crowding out - so we observed it under asymmetric information - is driven by factors which occur under symmetric information as well, for instance framing effects (see Bowles and Polanía-Reyes, 2012). Hence, our alternative hypothesis is:

Hypothesis 6: *The reward crowds out subjects' willingness to transfer, i. e., the number of subjects who transfer weakly decreases and the set of values under which a subject is willing to transfer will weakly shrink under symmetric information. If crowding out is driven by a signaling effect, it should be limited to transfers under asymmetric information.*

Figure 4.1: Percentage of Transfers - Symmetric Information



Notes: The figure depicts the share of individuals who transfer for each level of benefit in part 4. The exact level of b is known. $N = 45$.

4.5 Results

We begin our discussion with behavior under symmetric information, then we discuss behavior under asymmetric information and the effect of the reward. Finally, we will compare results from our baseline sessions to the robustness sessions.

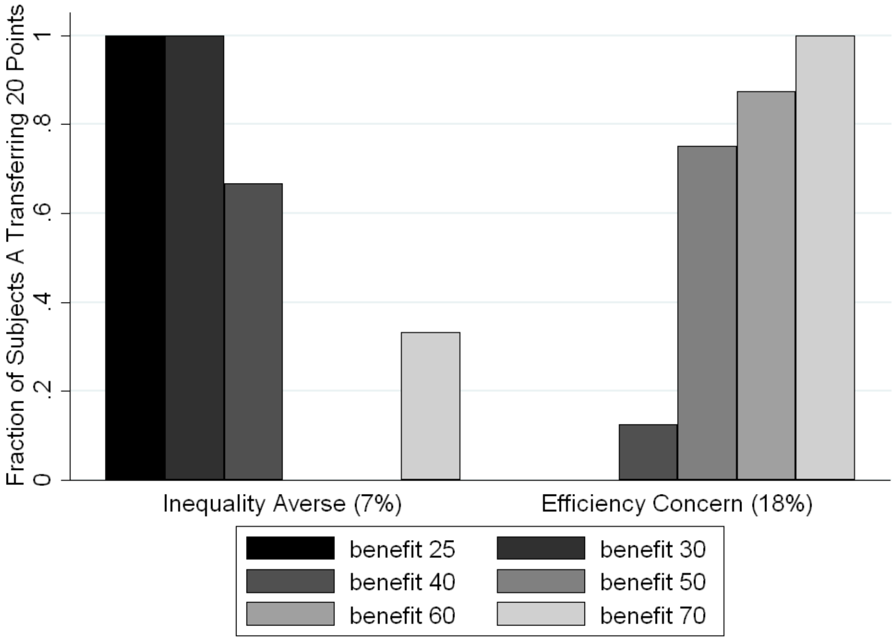
4.5.1 Symmetric Information

In part 4, where participants A had to make a decision for different known levels of benefits, between 17% and 31% of subjects transfer 20 points. As depicted in Figure 4.1 the share of individuals behaving prosocially depends on the benefit level. The first bar represents the share of individuals transferring when $b = 25$, the second bar when $b = 30$, and so on. The rate of transfers is highest when the benefit level is at its maximum and lowest for benefit levels below 50 points.

We observe transfer patterns as predicted by social preferences (Hypothesis 1): We can assign each dictator to one of four patterns.[19] Subjects who do not transfer

[19]The assignment is based on behavior in part 4. As we will discuss in section 4.5.3 below, we

Figure 4.2: Transfer Patterns for Different Values of b



Notes: The sample is restricted to those subjects whose behavior follows the prediction by either efficiency concern or inequality aversion. The exact level of b is known. The percentage indicates the frequency in the overall population. $N = 11$. *One subject only transferred at b smaller than 40 but also for the value of* $b = 70$ *in part 4. We assigned this subject to pattern "inequality aversion".*

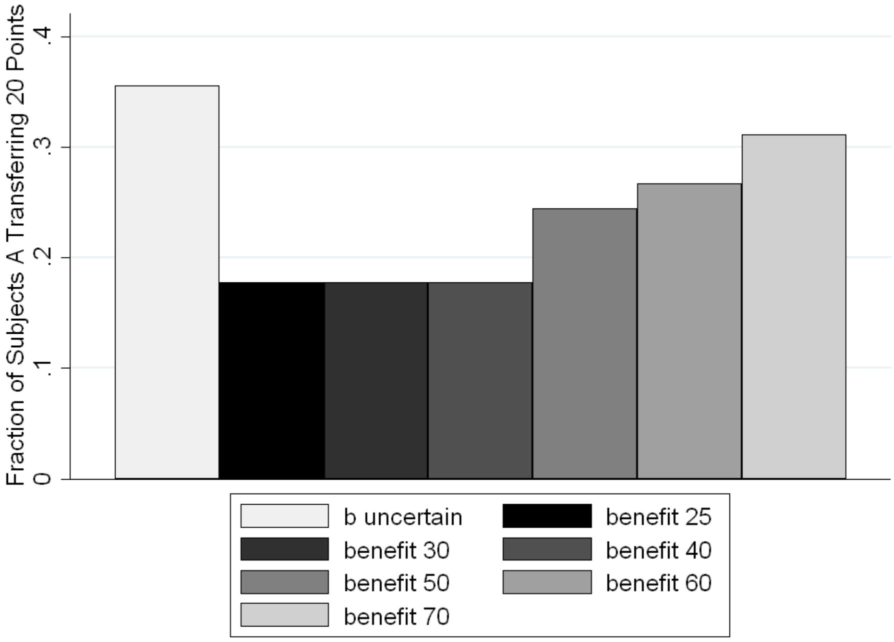
independently of the benefit level (64% of all subjects), subjects who transfer for all values of b and whose behavior is thereby in accordance with a concern for efficiency as well as maximin preferences (11%). Moreover, we observe subjects whose behavior is in line with an efficiency concern (18%) and subjects whose behavior is in line with inequality aversion (7%).[20]

In Figure 4.2 we depict the behavior of these last two subgroups for all values of

thereby have an upper bound on selfish behavior. Some subjects behave prosocially when they are given a reward in part 3.

[20]We have 90 pairs and thereby 45 dictators. 29 are selfish, 8 behave according to efficiency concern, 5 to either efficiency concern or maximin preferences, and 3 acccording to inequality aversion.

Figure 4.3: Percentage of Transfers - Symmetric vs Asymmetric Information



Notes: The figure depicts the share of individuals in the role of A who transfer. The first bar refers to transfers in part 1 under asymmetric information. Bars 2-7 depict transfers for each level of benefit in part 4. $N = 45$.

b.[21]

Result 1: *Without reward 18% of subjects show a behavior which is in line with an efficiency concern; 7% behave in a way that is predicted by inequality aversion; 11 % behave in a way that is both in line with an efficiency concern and maximin preferences; and 64% behave selfishly.*

4.5.2 Asymmetric Information

Turning to behavior under asymmetric information, it becomes evident that the share of subjects transferring is larger than under each value of b under symmetric information. The first bar in Figure 4.3 depicts the fraction of individuals transferring in part 1 under asymmetric information. Comparing it to behavior under symmetric information (part 4, represented by bars 2 to 7), in particular when the benefit level is low, subjects are more likely to transfer money.

[21]By definition the patterns for the other groups are degenerate. Either subjects transfer for all values of b or for none.

Table 4.2: Dictators' Transfer Decision

Dependent Variable:	Individual Transfer (=1)
Transfer when benefit=25 (=1)	-0.178**
	(0.0665)
Transfer when benefit=30 (=1)	-0.178**
	(0.0665)
Transfer when benefit=40 (=1)	-0.178**
	(0.0665)
Transfer when benefit=50 (=1)	-0.111*
	(0.0659)
Transfer when benefit=60 (=1)	-0.0889
	(0.0627)
Transfer when benefit=70 (=1)	-0.0444
	(0.0638)
Constant	0.356***
	(0.0729)
N	315

Notes: Standard errors are in parentheses. Table reports results from an OLS regression. Dependent variable is the individual transfer decision. Explanatory variables are the benefit levels under symmetric information. The omitted category is the asymmetric information case. Standard errors are clustered at the subject level. *Level of significance:* * p<0.10, ** p<0.05, *** p<0.01

When the benefit level is between 25 and 40 points, only 17% of subjects are willing to transfer compared to 36% under asymmetric information. Table 4.2 depicts results of an OLS regression with the individual transfer decision as dependent variable. As regressors we include the benefit levels under symmetric information, the omitted category is the decision under asymmetric information. The regression confirms what the graph already pointed out: Fewer prosocial decisions are made under symmetric information compared to the case of asymmetric information. The differences for benefit levels below 60 are statistically significant.[22]

[22] We also estimated a random effects model. The results are very similar. Furthermore, we did a Wilcoxon signed-rank test. For benefit levels 25, 30, and 40 the null-hypothesis can be rejected at the 5% level, for $b = 50$ at the 10% level.

Result 2: *Under asymmetric information more subjects transfer than under each single value of b under symmetric information. For $b < 60$ the effect is statistically significant.*

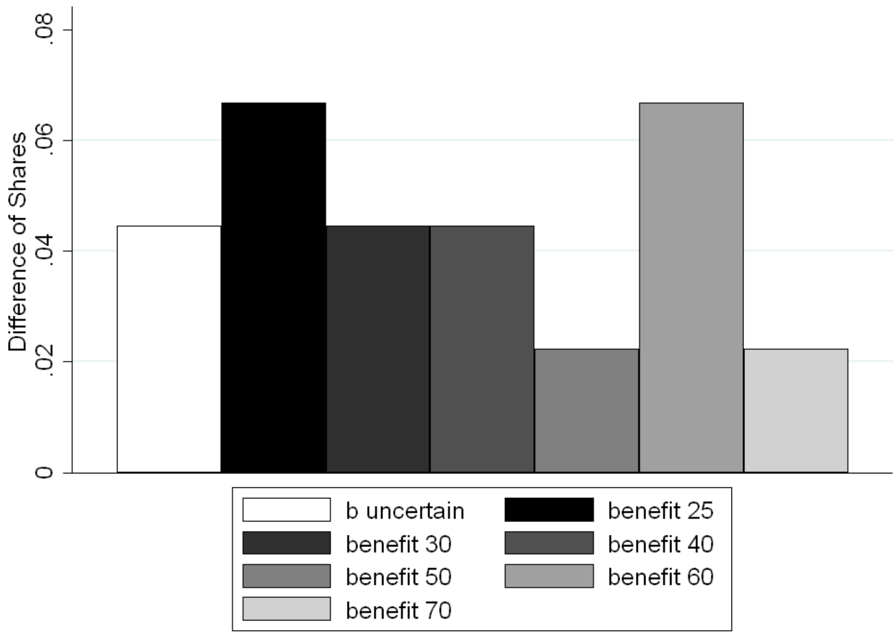
From Figure 4.3 it is clear, that in the aggregate subjects do not behave less prosocially under asymmetric information. On the contrary, on average they are more willing to transfer when they do not know the exact value of b.

The reason why we observe this is a combination of two effects: First, as is clear from the previous section, when b is known, different subjects transfer for different values of b. For low values of b subjects whose behavior is consistent with inequality aversion transfer. They do not transfer, though, when the benefit reaches a certain threshold. In contrast, subjects whose behavior is in line with an efficiency concern transfer from a certain threshold on but not when the value is low. Secondly, individual transfers are not reduced by asymmetric information. We observe six subjects who, given their choices under symmetric information, should transfer under asymmetric information according to social preferences. All of them transfer. Hence, no subject who should behave prosocially according to social preferences chooses not do so. Moreover, we observe seven subjects for whom, given their behavior under symmetric information, both a transfer and no transfer under asymmetric information would be in line with social preferences. In fact, four out of these seven transfer. Lastly, we find 32 subjects who transfer *neither* for $b = 40$ *nor* for $b = 50$. Five of them transfer nevertheless. So, if at all, we find some evidence for increased prosocial behavior and therefore evidence against Hypothesis 4.

Result 3: *For most subjects behavior is consistent with social preference theories and their choices under symmetric information. There is no evidence for a negative effect of asymmetric information.*

Combining that subjects are not negatively affected by asymmetric information and that we observe different types which separate when information is symmetric but both transfer when information is asymmetric, results in higher transfers under asymmetric information in the aggregate. As a consequence, earnings for subjects in the role of B are substantially higher when A decides under asymmetric information. Each value of b is equally likely and the unconditional decision to transfer (0.36) is

Figure 4.4: The Effect of Reward on Transfers



Notes: Bar 1 depicts the difference between the shares of individuals who transfer in part 2 and the share of individuals who transfer in part 1. Bars 2-7 depict the differences between the shares of individuals who transfer in part 3 and the share of individuals who transfer in part 4 for each level of benefit. $N = 45$.

the same for all values of b. Under symmetric information, each value of b is also equally likely as before. Yet, as subjects make a transfer decision for each value of b, the conditional probability may vary over b and is smaller than the probability under asymmetric information. Hence, players B have a higher chance to obtain a transfer: 36% under asymmetric information versus 22% under symmetric information.[23] And their expected transfer is 45% higher: 11 points under symmetric information versus 16 points under asymmetric information.[24]

4.5.3 Reward

In the second and third part of our experiment, we introduce a reward for participant A if he chooses the prosocial action. In all cases, the reward was 5 points.

Figure 4.4 depicts the difference between shares of subjects who transfer with and

[23] Probability to obtain a transfer is equal to $\frac{1}{6}(17 + 17 + 17 + 24 + 26 + 31)\% = 22\% < 36\%$

[24] $E[b] = \frac{1}{6}(0.17 \cdot 25 + 0.17 \cdot 30 + 0.17 \cdot 40 + 0.24 \cdot 50 + 0.26 \cdot 60 + 0.31 \cdot 70) = 11 < 0.36 \cdot 45 = 16$

Table 4.3: Dictators' Transfer Decision - Reward

Dependent Variable:	Individual Transfer (=1)
Transfer when benefit=25 (=1)	-0.167***
	(0.0505)
Transfer when benefit=30 (=1)	-0.178***
	(0.0484)
Transfer when benefit=40 (=1)	-0.178***
	(0.0427)
Transfer when benefit=50 (=1)	-0.122**
	(0.0534)
Transfer when benefit=60 (=1)	-0.0778
	(0.0504)
Transfer when benefit=70 (=1)	-0.0556
	(0.0535)
Reward (=1)	0.0444
	(0.0350)
Constant	0.356***
	(0.0615)
N	630

Notes: Standard errors are in parentheses. Table reports results from an OLS regression. Dependent variable is the individual transfer decision. Explanatory variables are the benefit levels under symmetric information. The omitted category is the asymmetric information case. Standard errors are clustered at the subject level. *Level of significance:* * p<0.10, ** p<0.05, *** p<0.01

without reward. The first bar depicts the comparison of transfers under asymmetric information (part 2 versus part 1) and bars 2-7 represent the comparison of transfers under symmetric information (part 4 versus part 3) for each level of benefit.

The reward has a positive effect but it is small: More subjects choose to transfer money in part 2 compared to part 1. Mirroring the positive effect, when the reward is withdrawn, we observe a negative effect: The willingness to transfer decreases. For each value of b we observe fewer transfers in part 4 than in part 3. On the one hand, the effect is driven by individuals who transfer in part 3 but stop transferring in part 4. On the other hand, for subjects, who still transfer in part 4, the set of values for which they transfer decreases. In Table 4.3 we depict results of an OLS regression similar to

Part 1 [4]	b is known to A.
Part 2 [1]	b is *not* known to A
Part 3 [3]	b is known to A. A receives a reward if he transfers.
Part 4 [2]	b is *not* known to A. A receives a reward if he transfers.

Table 4.4: Overview over Parameters in the Robustness Sessions

the baseline in Table 4.2. We include the reward as a dummy variable. It confirms that the reward has a positive effect but also shows that the effect is not statistically significant.[25]

Hence, we do not find significant evidence that a reward has a negative effect on prosocial behavior in our experiment (Hypothesis 6), the incentive works as standard price theory would predict (Hypothesis 5) and we also do not observe a differential effect of the price under symmetric and asymmetric information (when comparing part 2 with 3 versus 1 with 4).

Result 4: *The reward increases the number of subjects who transfer and the set of values of b for which subjects are willing to transfer under both symmetric and asymmetric information.*

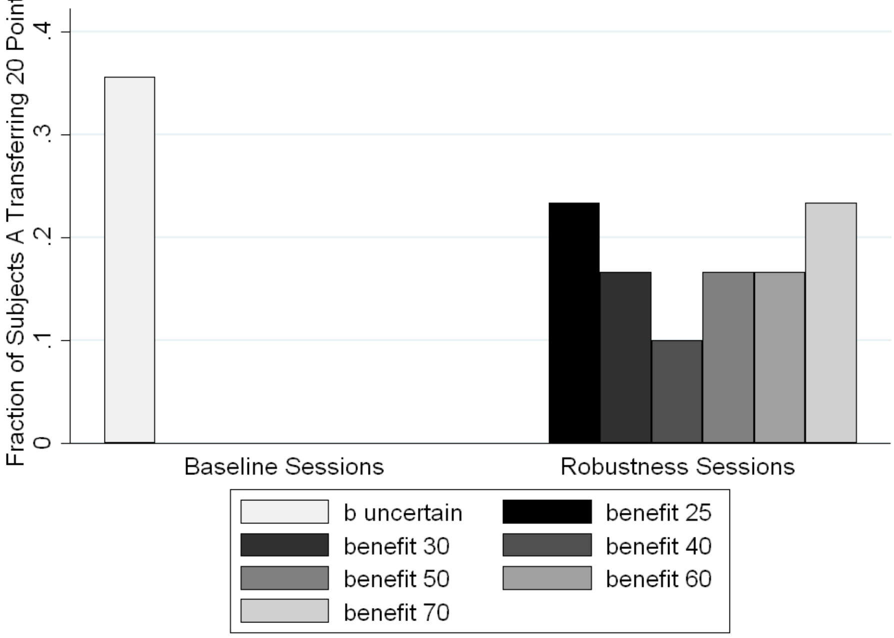
4.5.4 Robustness Check - Sequence of Parts

As in our experiment subjects participate in the different treatments in a sequence, it may be possible that subjects' behavior is influenced by the order of treatments. For instance, there may be a decay of prosocial behavior over time and the difference which we observe between part 1 and 4 merely an artefact of the design. To check the robustness of our results, we ran three more sessions with 60 participants where we changed the order of the parts. Table 4.4 provides an overview of the new order (the old order is depicted in "[]"). Most importantly, we let subjects decide first under symmetric information. So, we can compare this decision to the baseline sessions where subjects decided under asymmetric information in part 1 to see whether it still holds that in the aggregate subjects behave less prosocially under symmetric information.

In Figure 4.5 we depict only part 1 for both the baseline as well as the robustness

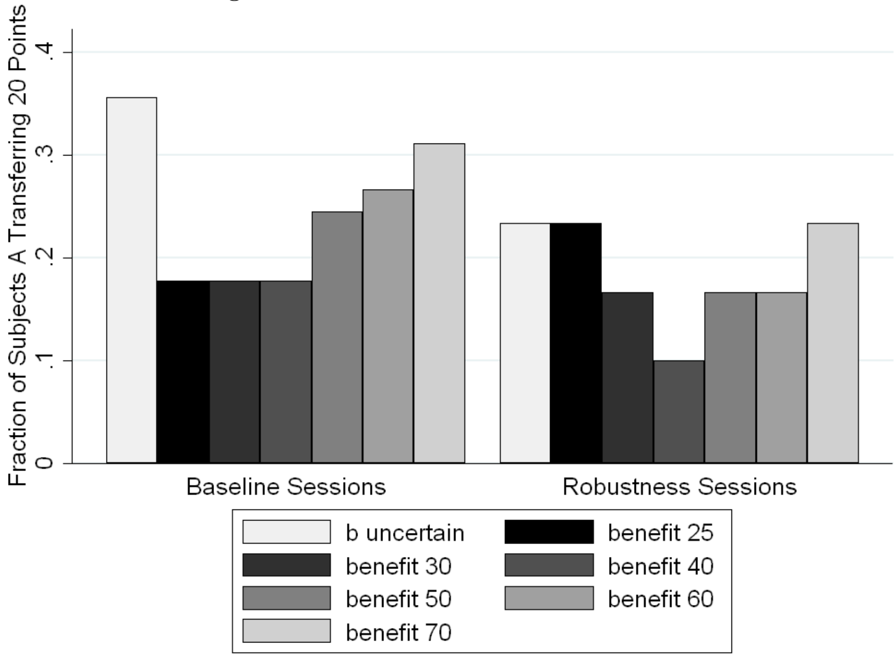
[25] As before we also estimated a random effects model (cf. Footnote 4.5.2). We also did a Wilcoxon signed-rank test. There was no significant difference. Also note that, besides finding no evidence for crowding out on average, we do not observe substantial and statistically significant crowding out at the individual level. Overall, only few subjects react to the reward at all.

Figure 4.5: Percentage of Transfers under Symmetric and Asymmetric Information - 1st Part only



Notes: The figure depicts the share of individuals in the role of A who transfer for the baseline sessions and the robustness sessions in part 1. The first bar refers to transfers under asymmetric information in the baseline sessions. Bars 2-7 depict transfers for each level of benefit under symmetric information in the robustness sessions.

Figure 4.6: Percentage of Transfers - Baseline versus Robustness Sessions



Notes: The figure depicts the share of individuals in the role of A who transfer for the baseline sessions and the robustness sessions. The first bar refers to transfers under asymmetric information. Bars 2-7 depict transfers for each level of benefit under symmetric information.

sessions. A direct comparison between transfer levels reveals that under asymmetric information subjects transfer more than for each level of benefit b under symmetric information. In fact, the difference is even more pronounced than in the comparison of part 1 and 4 within the baseline sessions. For $b = 30$, $b = 50$, and $b = 60$ the difference is statistically significant (Fisher's exact test, $p < 0.07$) and for $b = 40$ as well (Fisher's exact test, $p < 0.02$).

In Figure 4.6 we depict the decision to transfer under asymmetric information (bar 1) and the decisions to transfer under symmetric information for different values of b (bars 2 - 7). In the robustness sessions, subjects transfer under asymmetric information at least as much as for each level of b under symmetric information; for $b = 40$ the difference is statistically significant at the 5% level (Wilcoxon signed-rank test). So, as before the expected income for subjects in the role of B is higher under asymmetric information. However, in general there are lower transfers and smaller differences in the robustness sessions than in the baseline sessions.

What is the reason for this difference? If we look at what happens under symmetric information, it becomes apparent that there is a further difference. Relative to the baseline sessions more subjects transfer for low levels of b. What is different is the composition of types: In contrast to the baseline sessions, we observe more inequality averse subjects and no subject who always transfers (independently of the exact value of b). At the individual level, the behavior under asymmetric information is very similar to what we observe in the baseline sessions. We observe two subjects who, given their choices under symmetric information, should transfer under asymmetric information. Both of them transfer. Hence, no subject who should behave prosocially according to social preferences chooses not do so. Moreover, we observe five subjects who conditional on their transfer decision under symmetric information may either transfer or not transfer. Four out of these five subjects transfer. Lastly, we find 23 subjects who transfer *neither* for $b = 40$ *nor* for $b = 50$. Two of them transfer nevertheless under asymmetric information.

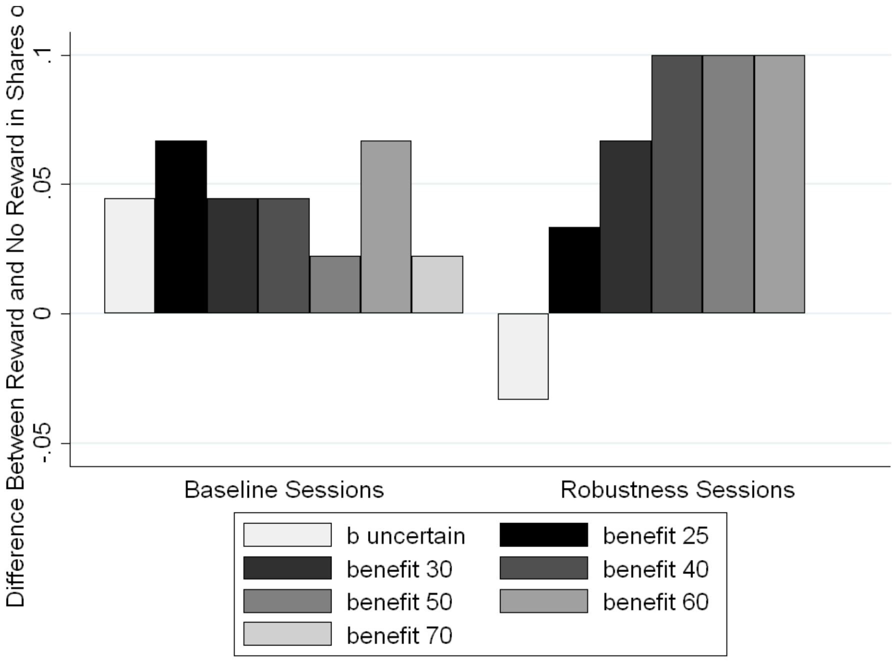
So, regarding individual behavior, the reaction to asymmetric information is very close to the baseline sessions. Yet, as we observe a different composition of types and thereby a different behavior under symmetric information, it is not surprising that there is also a difference under asymmetric information.

In Figure 4.7 the effect of the reward is depicted. There are slight differences between the robustness and the baseline sessions. Under symmetric information the effect of the reward is positive in both experiments even though the effect is stronger in the robustness sessions. Under asymmetric information, the effect actually has a negative effect in the robustness sessions. However, the overall magnitude of the reward's effect is small. Moreover, the difference is far from being statistically significant (Wilcoxon signed-rank test).

4.6 Conclusion

Economic experiments indicate that people regularly deviate from purely selfish behavior; they care not only about their own payoff but also about the payoff of others. When individuals face the decision to support a policy proposal, they find themselves in a similar situation: The proposal will have consequences for themselves but at the

Figure 4.7: The Effect of Reward on Transfers - Baseline Sessions versus Robustness Sessions



Notes: Bar 1 depicts the difference between the shares of individuals who transfer with reward and the share of individuals who transfer without reward under asymmetric information. Bars 2-7 depict the difference between the shares of individuals who transfer with reward and the share of individuals who transfer without reward for each level of benefit under symmetric information.

same time for others. Hence, when we observe in the laboratory that subjects behave prosocially and transfer patterns follow predictions by social preference theories, these theories can help to understand individual support for certain policies. Moreover, they can allow to implement policies that would fail under purely selfish behavior.

In this chapter, we investigate one aspect which plays a role in decisions in the field and has not yet been addressed in the context of laboratory experiments that try to measure social preferences: asymmetric information. In the field, decisions have to be made under a different information setting than in the laboratory. The gains and costs a person will realize when a policy is implemented will often be private information. In contrast, in most laboratory experiments subjects have plenty of information about the exact outcomes available. With this experiment we take a first step to investigate whether information asymmetries influence prosocial behavior of individuals. We compare transfers when a dictator has perfect information about the benefit he generates for the recipient with a situation where he only knows the distribution of benefits that may realize. The recipient, however, is fully informed about the benefit he receives.

We find that (i) in this setting 36% of subjects behave prosocially and transfer, (ii) 18% of subjects make choices that are consistent with a concern for efficiency, 7% make choices which are consistent with inequality aversion, and 11% transfer independently of the value of b, whose choices are therefore in line with an efficiency concern or maximin preferences, and (iii) under asymmetric information transfers do not decline. Even though subjects face the risk of transferring at a benefit level which they would not choose if they knew the level for sure, the level of transfers is stable and in fact even slightly higher than the highest level under symmetric information. Lastly, the introduction of a reward weakly increases transfers. Only under asymmetric information in the robustness sessions there is a negative effect of the reward. However, as it is the case when the reward has a positive influence, the overall effect of the reward is small and not statistically significant.

Our results suggest that individual behavior is not negatively affected by asymmetric information. In the aggregate, subjects, whose behavior is either in line with inequality aversion or with an efficiency concern, both tend to transfer under asymmetric information. Consequently, more subjects transfer under asymmetric information than for any single value of b under symmetric information.

What are possible next steps and what are potential items on an agenda for future research? First, we think it would be interesting to experiment with different information asymmetries than the one we provide. In our setting, asymmetric information leads to convergent choices and thereby increased prosocial behavior for subjects with different types of social preferences. However, one can also think of situations where this will not happen, for instance with individuals who exhibit different thresholds than the ones observed in our experiment, with allocations that have a larger range of possible benefits, or with different lotteries. Then, under asymmetric information overall support may not be larger than for each value of b. In general, it would be interesting to see under which conditions asymmetric information has a positive, neutral, or even negative effect on efficiency.

Secondly, in our setting subjects knew the lottery that determined the benefit level. In many decisions in the field this is not very plausible; subjects will rather face ambiguity. Therefore, it could also be interesting to investigate how subjects behave when the outcomes or its likelihood are ambiguous.

Another direction would be to explore how asymmetric information affects prosocial behavior in settings closer to actual reform situations. In our experiment, for instance, a next step would be to include strategic interaction, where not only A makes a decision but also B and eventually groups of subjects that will vote on the transfer. First, it could be interesting to analyze the role of different majority thresholds on individual behavior and whether subjects behave more or less prosocially compared to the dictator case. Secondly, one could also test whether the efficiency concern is affected by the game itself and whether other distributional (or selfish) concerns become more important. Thirdly, in contrast to the present setting where there is no interaction, with strategic interaction intentions of behavior may become more important.

Social preference theories can help to understand when people support certain reform proposals and when they do not. In experimental but also in theoretical work the focus has been on providing evidence and finding explanations for the observed deviations from selfish behavior. If we want to take these valuable insights further, there are important questions which need to be addressed.

One fundamental problem is how social preferences should be taken into consideration when designing public policy. How do we evaluate allocations when individuals care not only about their own but also about others' payoffs? As Bergstrom (2006)[26] points out, even under complete information and in very simple examples regarding the implementation of a public project the aggregation of social preferences is not trivial. And this issue becomes even more problematic once we allow for alternative motives of prosocial behavior, such as paternalistic preferences or image concerns, or, as we emphasized throughout this chapter, when we allow for private information.

[26] See also the references therein.

Appendix A

Appendix to Chapter 2

A.1 Variable Definition and Descriptive Statistics

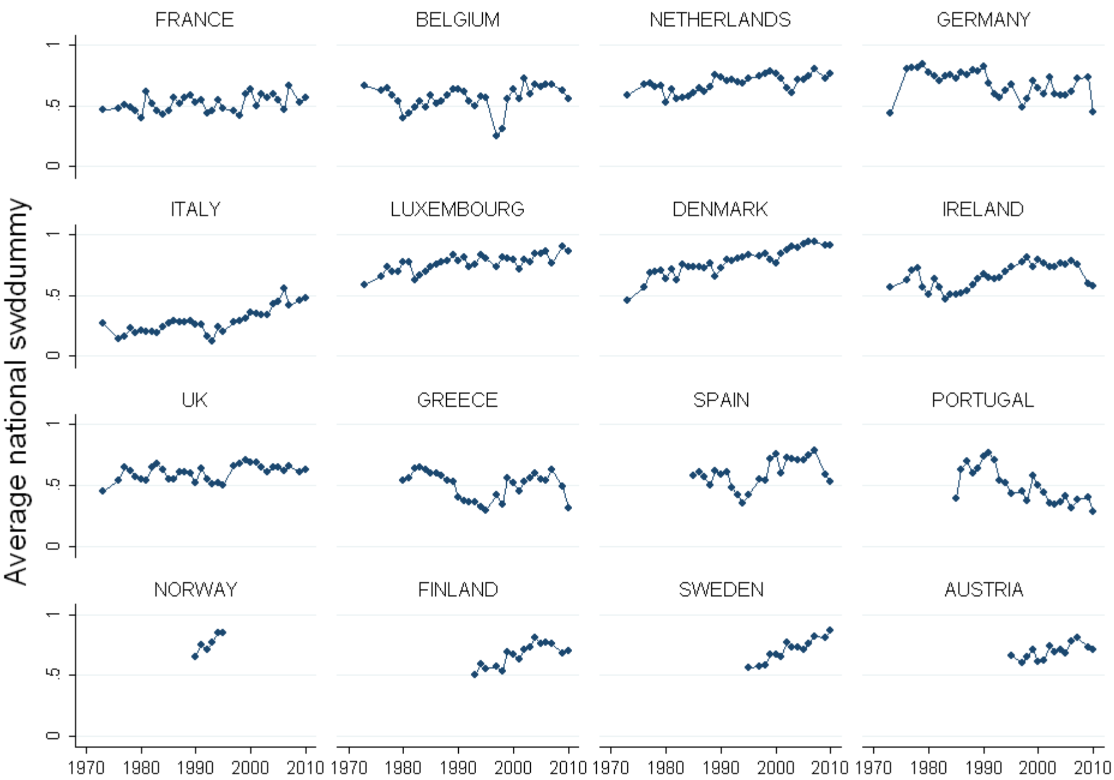


Figure A.1: Percentage of Individuals Satisfied with Democracy over Time (Weighted)

Table A.1: Definitions of Variables Used

variable name	series name / explanation	source
macroeconomic variables		
GDP per head	1. Gross domestic product: GDP per head, US$, constant prices, constant PPPs, OECD base year; rescaled by factor 1/1000	OECD (2011)
UE rate	Rate of unemployment as % of civilian labour force	OECD (2011)
growth	GDP, growth rate	OECD (2011)
f(inflation)	$f(\text{inflation}_{it}) = (\text{inflation}_{it} - 1)\mathbf{1}(\text{inflation}_{it} \leq 1) + \log \text{inflation}_{it}\mathbf{1}(\text{inflation}_{it} > 1)$. The function $f(\text{inflation}_{it})$, as proposed in Khan and Senhadji (2001), is linear in inflation_{it} for values of inflation rates below or equal to one and logarithmic for inflation rates greater than one. The breakpoint one is chosen such that the transformation is continuous.	OECD (2011)
debt	Gross government debt (financial liabilities) as a percentage of GDP	Armingeon et al. (2009)
deficit	Annual deficit (government primary balance) as a percentage of GDP	Armingeon et al. (2009)
elderly	Population 65 and over as a percentage of population	Armingeon et al. (2009)
sstran	Social security transfers as a percentage of GDP	Armingeon et al. (2009)
polity4	Index of institutional quality which is originally coded on a scale from 0 to 10 (highest quality). Since in our sample the index only varies from 8 to 10 we recode as follows: polity4==2 if polity takes the highest value of 10, polity4==1 if polity takes the value 9, polity4==0 if polity takes the lowest value of 8 in our sample.	Marshall et al. (2011)
individual variables		
SWD	Answer to the question "On the whole, are you very satisfied, fairly satisfied, not very satisfied, or not at all satisfied with the way democracy works in <country>? Would you say you are ...?", 1=not at all satisfied, 2=not very satisfied, 3=fairly satisfied, 4=very satisfied	Eurobarometer
SWD dummy	SWD dummy=1 if (SWD=3 or SWD=4); SWD dummy=0 if (SWD=2 or SWD=1)	own calculation
unempl	dummy for those being unemployed at the time of the survey	Eurobarometer
out of LF	dummy for those not in the labour force, subsuming housewives, students, military, and retired	Eurobarometer
married	dummy for being 'married' or 'living as married'	Eurobarometer
male	dummy for males	Eurobarometer
age	age of the respondent in years	Eurobarometer
education	age when full-time education was finished. We use this variable to construct 5 dummies as described below.	Eurobarometer
basic education	age when full-time education was finished: 'up to 15 years' or 'no full-time education'	Eurobarometer
intermediate education	age when full-time education was finished: 16 to 19 years	Eurobarometer
higher education	age when full-time education was finished: 20 years or older	Eurobarometer
still studying	age when full-time education was finished: still studying	Eurobarometer
income	Income is coded in categories which vary over time and from country to country. We use this variable to defer the relative positions of individuals in the income distribution.	Eurobarometer
poor	dummy for individuals whose income is in the lowest three income deciles	Eurobarometer
middle income	dummy for individuals whose income is in the four middle income deciles	Eurobarometer
rich	dummy for individuals whose income is in the three highest income deciles	Eurobarometer
life satisfaction	Answer to the question "On the whole, are you very satisfied, fairly satisfied, not very satisfied, or not at all satisfied with the life you lead? Would you say you are ...?" 1=not at all satisfied, 2=not very satisfied, 3=fairly satisfied, 4=very satisfied. We use this variable to construct 4 dummies corresponding to the 4 answer categories.	Eurobarometer

Table A.2: Summary Statistics for the Macro Variables

MACRO	F	B	NL	D	I	L	DK	IRL	UK	GR	E	P	N	FIN	S	A	*all*
GDP/head ($)	21.83	23.55	24.48	22.27	21.77	40.89	24.47	20.07	21.95	17.54	19.31	15.50	28.61	25.77	29.00	29.22	23.70
	(3.41)	(4.34)	(4.95)	(4.01)	(3.67)	(14.33)	(4.36)	(9.14)	(4.83)	(3.20)	(3.38)	(2.68)	(1.68)	(3.77)	(3.04)	(2.20)	(8.33)
growth (%)	2.18	2.12	2.33	2.07	1.98	4.25	2.00	4.63	2.23	1.80	2.94	2.67	3.43	2.82	2.95	2.04	2.61
	(1.47)	(1.69)	(1.85)	(1.97)	(2.07)	(3.09)	(2.16)	(3.81)	(2.07)	(2.64)	(2.13)	(2.58)	(1.09)	(3.40)	(2.63)	(2.00)	(2.51)
f(inflation)	4.41	3.40	2.87	2.50	7.00	3.48	4.48	5.87	5.26	11.37	4.19	6.04	2.67	1.37	1.13	1.68	4.60
	(4.07)	(2.47)	(2.07)	(1.64)	(5.92)	(2.62)	(3.51)	(5.90)	(4.80)	(8.05)	(2.20)	(4.92)	(0.96)	(0.85)	(0.99)	(0.68)	(4.74)
UE rate (%)	8.34	9.73	6.49	7.13	9.57	1.77	7.04	10.49	7.46	8.63	16.61	6.40	5.56	10.53	7.24	4.30	8.06
	(1.79)	(2.32)	(2.92)	(2.19)	(1.87)	(0.86)	(2.05)	(4.61)	(2.41)	(2.09)	(4.93)	(1.89)	(0.42)	(3.12)	(1.55)	(0.50)	(4.01)
debt (%)	49.53	106.89	72.74	47.19	105.02	8.22	62.92	70.59	46.30	81.25	59.15	67.08	34.77	53.79	65.63	n.a.	66.01
	(17.26)	(23.90)	(14.90)	(14.35)	(16.14)	(1.94)	(13.65)	(27.08)	(5.94)	(32.46)	(10.22)	(3.86)	(5.72)	(7.18)	(11.90)	n.a.	(29.89)
deficit (%)	-0.73	1.37	0.31	-0.23	-0.82	1.00	1.98	-0.52	-0.30	-1.20	-0.07	0.18	-2.74	1.46	1.01	n.a.	0.10
	(1.10)	(4.16)	(2.04)	(1.85)	(3.56)	(2.16)	(3.71)	(4.11)	(2.47)	(3.13)	(2.37)	(1.65)	(1.90)	(4.75)	(2.69)	n.a.	(3.10)
elderly (%)	14.65	15.36	12.76	16.28	15.49	13.70	14.95	11.06	15.49	15.22	15.15	15.02	16.16	15.08	17.39	n.a.	14.69
	(1.26)	(1.29)	(1.03)	(1.65)	(2.48)	(0.36)	(0.51)	(0.27)	(0.53)	(1.92)	(1.75)	(1.83)	(0.15)	(0.82)	(0.19)	n.a.	(1.96)
sstran (%)	17.33	17.04	23.42	17.08	15.69	18.89	17.09	13.11	13.33	14.34	15.25	11.68	16.41	20.40	19.47	n.a.	16.53
	(1.04)	(1.08)	(5.46)	(1.13)	(1.16)	(3.71)	(2.02)	(2.53)	(1.55)	(1.67)	(1.97)	(1.13)	(0.51)	(2.94)	(1.12)	n.a.	(3.78)
#observations	33	33	33	33	33	33	33	33	33	29	24	24	6	16	14	14	424

Standard deviations in brackets below estimates.
Calculations use only the years used for the regressions, i.e. 1976-1994, 1997-2010.
Countries are abbreviated according to international vehicle registration codes. Since 1991 East-Germany is included. Before data refers only to West-Germany.
GDP per head in US$1000, constant prices, constant PPPs, reference year 2000.
Missing observations: UE rate in 2010; debt, deficit, elderly for 2009, 2010, and Austria; sstran for 2001-2010 and Austria.
Sources: OECD, for details see table A.1.

Table A.3: Summary Statistics for the Individual Variables

	F	B	NL	D	I	L	DK	IRL	UK	GR	E	P	N	FIN	S	A	*all*
SWD	0.52	0.56	0.68	0.69	0.27	0.77	0.77	0.65	0.60	0.50	0.59	0.53	0.76	0.66	0.70	0.68	0.59
	(0.50)	(0.50)	(0.47)	(0.46)	(0.45)	(0.42)	(0.42)	(0.48)	(0.49)	(0.50)	(0.49)	(0.50)	(0.42)	(0.47)	(0.46)	(0.47)	(0.49)
unempl	0.06	0.07	0.04	0.05	0.05	0.01	0.05	0.07	0.06	0.04	0.07	0.05	0.05	0.06	0.05	0.04	0.06
	(0.24)	(0.26)	(0.20)	(0.21)	(0.22)	(0.12)	(0.23)	(0.26)	(0.23)	(0.20)	(0.26)	(0.22)	(0.22)	(0.24)	(0.22)	(0.19)	(0.23)
out of LF	0.42	0.44	0.50	0.43	0.48	0.48	0.38	0.46	0.42	0.49	0.50	0.45	0.39	0.43	0.39	0.39	0.44
	(0.49)	(0.50)	(0.50)	(0.49)	(0.50)	(0.50)	(0.48)	(0.50)	(0.49)	(0.50)	(0.50)	(0.50)	(0.49)	(0.49)	(0.49)	(0.49)	(0.50)
married	0.65	0.65	0.68	0.60	0.59	0.65	0.66	0.58	0.64	0.66	0.58	0.63	0.60	0.59	0.63	0.60	0.63
	(0.48)	(0.48)	(0.47)	(0.49)	(0.49)	(0.48)	(0.47)	(0.49)	(0.48)	(0.47)	(0.49)	(0.48)	(0.49)	(0.49)	(0.48)	(0.49)	(0.48)
male	0.49	0.50	0.48	0.49	0.48	0.51	0.50	0.50	0.48	0.49	0.48	0.47	0.53	0.46	0.51	0.46	0.49
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
age	42.91	44.17	43.10	44.77	42.90	43.75	44.96	41.92	44.89	43.70	43.29	44.72	41.53	46.17	47.97	44.64	44.02
	(17.82)	(17.96)	(17.04)	(17.69)	(17.47)	(17.34)	(18.01)	(17.77)	(18.51)	(17.85)	(18.79)	(18.92)	(17.42)	(18.40)	(18.07)	(17.06)	(17.94)
education																	
basic	0.26	0.25	0.23	0.37	0.48	0.26	0.27	0.29	0.40	0.45	0.47	0.63	0.14	0.18	0.16	0.28	0.34
	(0.44)	(0.43)	(0.42)	(0.48)	(0.50)	(0.44)	(0.44)	(0.45)	(0.49)	(0.50)	(0.50)	(0.48)	(0.35)	(0.38)	(0.37)	(0.45)	(0.47)
interm.	0.41	0.41	0.39	0.41	0.25	0.40	0.23	0.51	0.43	0.28	0.24	0.18	0.29	0.28	0.29	0.49	0.36
	(0.49)	(0.49)	(0.49)	(0.49)	(0.43)	(0.49)	(0.42)	(0.50)	(0.49)	(0.45)	(0.43)	(0.38)	(0.45)	(0.45)	(0.46)	(0.50)	(0.48)
higher	0.24	0.25	0.28	0.15	0.16	0.25	0.40	0.10	0.12	0.18	0.16	0.10	0.43	0.42	0.43	0.16	0.21
	(0.43)	(0.43)	(0.45)	(0.36)	(0.36)	(0.43)	(0.49)	(0.31)	(0.33)	(0.38)	(0.37)	(0.30)	(0.49)	(0.49)	(0.49)	(0.36)	(0.41)
still stud.	0.09	0.08	0.10	0.07	0.12	0.09	0.09	0.09	0.05	0.10	0.12	0.09	0.14	0.13	0.12	0.08	0.09
	(0.29)	(0.28)	(0.30)	(0.26)	(0.32)	(0.29)	(0.29)	(0.28)	(0.22)	(0.30)	(0.32)	(0.29)	(0.35)	(0.33)	(0.32)	(0.27)	(0.29)
no full-time	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01	0.00	0.00	0.00	0.00	0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)	(0.05)	(0.01)	(0.01)	(0.02)	(0.08)	(0.06)	0.00	0.00	(0.02)	(0.04)	(0.03)
life satfct.																	
...satisfied																	
not at all	0.06	0.03	0.01	0.02	0.07	0.01	0.01	0.04	0.03	0.12	0.04	0.08	0.01	0.01	0.01	0.02	0.04
	(0.24)	(0.18)	(0.10)	(0.15)	(0.25)	(0.11)	(0.08)	(0.19)	(0.18)	(0.32)	(0.19)	(0.28)	(0.11)	(0.11)	(0.08)	(0.13)	(0.20)
not very	0.17	0.11	0.05	0.13	0.21	0.05	0.03	0.09	0.10	0.27	0.18	0.26	0.05	0.07	0.04	0.11	0.13
	(0.38)	(0.31)	(0.22)	(0.33)	(0.41)	(0.23)	(0.17)	(0.29)	(0.29)	(0.44)	(0.38)	(0.44)	(0.22)	(0.25)	(0.20)	(0.32)	(0.34)
fairly	0.62	0.58	0.49	0.64	0.60	0.52	0.36	0.52	0.55	0.48	0.58	0.61	0.49	0.62	0.53	0.60	0.55
	(0.48)	(0.49)	(0.50)	(0.48)	(0.49)	(0.50)	(0.48)	(0.50)	(0.50)	(0.50)	(0.49)	(0.49)	(0.50)	(0.48)	(0.50)	(0.49)	(0.50)
very	0.14	0.28	0.45	0.21	0.12	0.41	0.60	0.35	0.32	0.14	0.21	0.05	0.45	0.30	0.42	0.27	0.28
	(0.35)	(0.45)	(0.50)	(0.41)	(0.33)	(0.49)	(0.49)	(0.48)	(0.47)	(0.35)	(0.41)	(0.23)	(0.50)	(0.46)	(0.49)	(0.44)	(0.45)
income																	
rich	0.23	0.26	0.25	0.25	0.25	0.22	0.24	0.23	0.24	0.25	0.22	0.26	0.26	0.26	0.23	0.24	0.24
	(0.42)	(0.44)	(0.43)	(0.44)	(0.43)	(0.41)	(0.43)	(0.42)	(0.43)	(0.43)	(0.41)	(0.44)	(0.44)	(0.44)	(0.42)	(0.43)	(0.43)
middle	0.42	0.38	0.40	0.40	0.36	0.42	0.41	0.41	0.42	0.38	0.43	0.39	0.40	0.38	0.35	0.38	0.40
	(0.49)	(0.48)	(0.49)	(0.49)	(0.48)	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)	(0.50)	(0.49)	(0.49)	(0.49)	(0.48)	(0.49)	(0.49)
poor	0.35	0.36	0.35	0.34	0.39	0.36	0.35	0.36	0.34	0.37	0.35	0.35	0.35	0.35	0.42	0.38	0.36
	(0.48)	(0.48)	(0.48)	(0.47)	(0.49)	(0.48)	(0.48)	(0.48)	(0.47)	(0.48)	(0.48)	(0.48)	(0.48)	(0.48)	(0.49)	(0.49)	(0.48)
#obs	52474	52065	54349	53298	56052	22012	53992	51233	53606	46516	37230	36471	7826	20676	17822	18590	673914

Calculations use only observations from the years used in the regressions and observations where all variables used in the regressions are not missing. #observations varies slightly for some variables.
Standard deviations in brackets below estimates.
Countries are abbreviated according to international vehicle registration codes. Since 1991 East-Germany is included. Before data refers only to West-Germany.
Income is only available from 1973 to 1994 and from 1997 to 2003.
Source: Eurobarometer.

A.2 Additional Results

Table A.4: Order of Inclusion of Macro Variables Does Not Matter

dependent: SWD	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
macroeconomic variables									
GDP per head				0.0054	0.0068	0.0013	0.0056	0.0006	
				(0.004)	(0.004)	(0.003)	(0.004)	(0.003)	
growth	0.0141***			0.0131***			0.0131***	0.0106***	0.0106***
	(0.004)			(0.004)			(0.004)	(0.002)	(0.003)
f(inflation)		0.0001			-0.0049		-0.0054		-0.0184
		(0.018)			(0.018)		(0.016)		(0.012)
UE rate			-0.0175***			-0.0171***		-0.0162***	-0.0175***
			(0.003)			(0.003)		(0.003)	(0.003)
ind. controls	yes	yes	yes	yes	yes	yes	yes	yes	yes
survey FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
nation FE	yes	yes	yes	yes	yes	yes	yes	yes	yes
N	607486	607486	602545	606504	606504	602545	606504	602545	602545
R^2	0.1382	0.1364	0.142	0.1388	0.1373	0.1421	0.1388	0.143	0.1433

* p<0.10, ** p<0.05, *** p<0.01

Dependent variable is a dummy.

Standard errors are corrected for clustering at nation level.

Table A.5: Impact of Macroeconomic and Individual Level Variables on SWD (Individual Data) - Logit

dependent: SWD	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
macroeconomic variables								
GDP per head	0.0092*	0.0076	0.0078	0.0014	0.0014	0.0016	0.0057	0.0057
	(0.005)	(0.005)	(0.005)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)
growth		0.0145***	0.0145***	0.0121***	0.0116***	0.0126***	0.0069*	0.0069*
		(0.005)	(0.005)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
f(inflation)			-0.0056	-0.0214	-0.0235	-0.0214	-0.0384	-0.0385
			(0.019)	(0.015)	(0.015)	(0.014)	(0.024)	(0.024)
UE rate				-0.0199***	-0.0215***	-0.0208***	-0.0197***	-0.0198***
				(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
individual variables								
unemployed	-0.0589***	-0.0587***	-0.0589***	-0.0551***	-0.1178***	-0.1216***	-0.0476***	-0.0420***
	(0.007)	(0.007)	(0.007)	(0.006)	(0.010)	(0.010)	(0.009)	(0.008)
out of LF	-0.0020	-0.0022	-0.0023	-0.0018	-0.0046	-0.0057	-0.0017	0.0020
	(0.004)	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)	(0.005)	(0.005)
married	0.0002	0.0002	0.0002	0.0011	0.0286***	0.0295***	-0.0003	-0.0047
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.006)	(0.007)
male	0.0081*	0.0080*	0.0080*	0.0080*	0.0038	0.0036	0.0074	0.0074
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)
age	-0.0027***	-0.0027***	-0.0027***	-0.0028***	-0.0054***	-0.0057***	-0.0029***	-0.0030***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
age^2	0.0000***	0.0000***	0.0000***	0.0000***	0.0001***	0.0001***	0.0000***	0.0000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
intermediate education	0.0104	0.0107	0.0107	0.0100	0.0233***	0.0235***	-0.0188	-0.0113
	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.008)	(0.020)	(0.018)
higher education	0.0312*	0.0317*	0.0317*	0.0306*	0.0557***	0.0556***	-0.0104	-0.0067
	(0.016)	(0.016)	(0.016)	(0.016)	(0.015)	(0.015)	(0.014)	(0.014)
still studying	0.0331**	0.0339**	0.0338**	0.0327**	0.0649***	0.0652***	-0.0005	-0.0004
	(0.016)	(0.016)	(0.016)	(0.016)	(0.017)	(0.017)	(0.007)	(0.007)
not at all satisfied	-0.3680***	-0.3669***	-0.3669***	-0.3652***			-0.3823***	-0.3802***
	(0.013)	(0.013)	(0.014)	(0.012)			(0.013)	(0.014)
not very satisfied	-0.2650***	-0.2649***	-0.2649***	-0.2637***			-0.2868***	-0.2849***
	(0.012)	(0.012)	(0.012)	(0.012)			(0.014)	(0.014)
very satisfied	0.0855***	0.0854***	0.0854***	0.0853***			0.0911***	0.0896***
	(0.006)	(0.006)	(0.006)	(0.006)			(0.007)	(0.007)
poor								-0.0116*
								(0.007)
rich								0.0166***
								(0.006)
survey FE	yes	yes	yes	yes	yes	yes	yes	yes
nation FE	yes	yes	yes	yes	yes	yes	yes	yes
N	606504	606504	606504	602545	660546	602545	353132	353132
Pseudo R^2	0.1057	0.1069	0.1069	0.1106	0.0737	0.0732	0.1159	0.1161

* $p<0.10$, ** $p<0.05$, *** $p<0.01$
Dependent variable is a dummy.
Marginal effects. When independent variable is a dummy, discrete change of dummy variable from 0 to 1.
Standard errors are corrected for clustering at nation level.
(4) is the reference for robustness checks. In (5) we restrict attention to the subsample were life satisfaction is available but do not include it. In (6) we exclude life satisfaction from the estimation. (7) is estimated on the reduced sample where income is available, (8) controls for income groups.

Table A.6: Lagged Growth and Endogeneity - Logit

dependent: SWD	(1)	(2)	(3)	(4)	(5)	(6)
macroeconomic variables						
GDP per head	0.0014	0.0011	0.0026	0.0023	0.0009	0.0010
	(0.004)	(0.004)	(0.004)	(0.004)	(0.002)	(0.002)
growth_t	0.0121***	0.0110***	0.0064**	0.0053*	0.0046*	0.0048**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
growth_{t-1}		0.0030		0.0029		-0.0006
		(0.003)		(0.003)		(0.003)
growth_{t+1}			0.0118***	0.0116***	0.0068***	0.0068***
			(0.003)	(0.003)	(0.003)	(0.002)
f(inflation)	-0.0214	-0.0221	-0.0249*	-0.0255*	-0.0163	-0.0162
	(0.015)	(0.014)	(0.014)	(0.013)	(0.013)	(0.013)
UE rate	-0.0199***	-0.0195***	-0.0201***	-0.0197***	-0.0068***	-0.0068***
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)
$\overline{\text{SWD}}_{c,t-1}$					0.6426***	0.6436***
					(0.069)	(0.069)
ind. controls	yes	yes	yes	yes	yes	yes
survey FE	yes	yes	yes	yes	yes	yes
country FE	yes	yes	yes	yes	yes	yes
N	602545	602545	592075	592075	546239	546239
Pseudo R^2	0.1106	0.1106	0.1111	0.1111	0.1158	0.1158

* p<0.10, ** p<0.05, *** p<0.01
Dependent variable is a dummy.
Marginal effects. When independent variable is a dummy, discrete change of dummy variable from 0 to 1.
Standard errors are corrected for clustering at nation level.
(1) is the reference estimation from table A.5, column (4).

Table A.7: Results - Ordered Logit

dependent:	(1)				(2)				(3)			
SWD Scores	SWD1	SWD2	SWD3	SWD4	SWD1	SWD2	SWD3	SWD4	SWD1	SWD2	SWD3	SWD4
macroeconomic variables												
GDP per head	-0.0005	-0.0009	0.0009	0.0004	-0.0005	-0.0008	0.0009	0.0004	-0.0025	-0.0039	0.0043	0.0021
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)	(0.002)
growth	-0.0040***	-0.0075***	0.0080***	0.0035***	-0.0044***	-0.0069***	0.0076***	0.0037***	-0.0055***	-0.0087***	0.0095***	0.0047***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)	(0.001)
f(inflation)	0.0074**	0.0141**	-0.0149**	-0.0066**	0.0093**	0.0147**	-0.0162**	-0.0078**	0.0033	0.0052	-0.0058	-0.0028
	(0.004)	(0.007)	(0.007)	(0.003)	(0.004)	(0.007)	(0.007)	(0.004)	(0.006)	(0.010)	(0.011)	(0.005)
UE rate	0.0062***	0.0118***	-0.0124***	-0.0055***	0.0075***	0.0120***	-0.0132***	-0.0063***				
	(0.001)	(0.002)	(0.002)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)				
individual characteristics												
unemployed	0.0166***	0.0291***	-0.0328***	-0.0129***	0.0509***	0.0638***	-0.0839***	-0.0308***	0.0536***	0.0660***	-0.0874***	-0.0322***
	(0.003)	(0.004)	(0.005)	(0.002)	(0.005)	(0.006)	(0.008)	(0.003)	(0.005)	(0.006)	(0.009)	(0.003)
out of LF	0.0001	0.0002	-0.0002	-0.0001	0.0013	0.0020	-0.0023	-0.0011	0.0016	0.0025	-0.0028	-0.0014
	(0.001)	(0.003)	(0.003)	(0.001)	(0.002)	(0.003)	(0.004)	(0.002)	(0.002)	(0.003)	(0.004)	(0.002)
married	0.0012	0.0024	-0.0025	-0.0011	-0.0106***	-0.0166***	0.0185***	0.0087***	-0.0103***	-0.0161***	0.0179***	0.0085***
	(0.001)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.003)	(0.001)	(0.002)	(0.002)	(0.003)	(0.001)
male	-0.0049**	-0.0094**	0.0099**	0.0044**	-0.0032	-0.0050	0.0055	0.0027	-0.0031	-0.0049	0.0054	0.0026
	(0.002)	(0.004)	(0.004)	(0.002)	(0.002)	(0.003)	(0.004)	(0.002)	(0.002)	(0.003)	(0.004)	(0.002)
age	0.0007***	0.0013***	-0.0013***	-0.0006***	0.0019***	0.0030***	-0.0033***	-0.0016***	0.0019***	0.0029***	-0.0032***	-0.0016***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
age^2	-0.0000***	-0.0000***	0.0000***	0.0000***	-0.0000***	-0.0000***	0.0000***	0.0000***	-0.0000***	-0.0000***	0.0000***	0.0000***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
education												
intermediate	-0.0022	-0.0043	0.0045	0.0020	-0.0082***	-0.0132***	0.0144***	0.0071***	-0.0084***	-0.0135***	0.0146***	0.0073***
	(0.003)	(0.005)	(0.005)	(0.002)	(0.003)	(0.005)	(0.005)	(0.003)	(0.003)	(0.005)	(0.005)	(0.003)
higher	-0.0089*	-0.0175*	0.0180*	0.0084*	-0.0202***	-0.0340***	0.0353***	0.0189***	-0.0207***	-0.0348***	0.0360***	0.0195***
	(0.005)	(0.010)	(0.011)	(0.005)	(0.005)	(0.009)	(0.009)	(0.005)	(0.005)	(0.009)	(0.009)	(0.005)
still studying	-0.0092*	-0.0182*	0.0186*	0.0088*	-0.0226***	-0.0396***	0.0396***	0.0227***	-0.0232***	-0.0405***	0.0403***	0.0235***
	(0.005)	(0.010)	(0.010)	(0.005)	(0.006)	(0.010)	(0.010)	(0.006)	(0.006)	(0.010)	(0.009)	(0.006)
life satisfaction												
... satisfied												
not at all ...	0.2929***	0.1268***	-0.3480***	-0.0716***								
	(0.017)	(0.010)	(0.014)	(0.005)								
not very ...	0.0976***	0.1231***	-0.1698***	-0.0508***								
	(0.007)	(0.007)	(0.012)	(0.003)								
very ...	-0.0439***	-0.0920***	0.0875***	0.0485***								
	(0.003)	(0.007)	(0.006)	(0.005)								
survey FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
nation FE	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes	yes
N	602545	602545	602545	602545	660546	660546	660546	660546	664511	664511	664511	664511
Pseudo R^2	0.0789				0.0482				0.0456			

* $p<0.10$, ** $p<0.05$, *** $p<0.01$
Table reports marginal effects for scores.
The function f(inflation) is linear in inflation$_{it}$ for values of inflation rates below or equal to one and logarithmic for inflation rates greater than one as proposed by Khan and Senhadji (2001). See table A.1 for further details.
Sample covers period 1973-2010 excluding 1974, 1975, 1996, 2008 as SWD is not available in these years. All 16 Countries are included, but the periods covered for each country vary depending on the time of their accession to the European Union. See tables A.2 and A.3.
All estimations include dummies for survey years and nations.
(2) is estimated on reduced sample where life satisfaction is available. (3) uses an enlarged sample where observations from waves without life satisfaction are included.

Table A.8: Sample Restrictions: (1) 1973-2006, (2) 1973-2008

dependent: SWD	(1)	(2)
macroeconomic variables		
GDP per head	0.0029	0.0023
	(0.003)	(0.003)
growth	0.0089**	0.0091**
	(0.004)	(0.004)
f(inflation)	-0.0322*	-0.0313*
	(0.017)	(0.017)
UE rate	-0.0167***	-0.0169***
	(0.002)	(0.002)
ind. controls	yes	yes
survey FE	yes	yes
nation FE	yes	yes
N	561582	576656
R^2	0.1438	0.01442

* p<0.10, ** p<0.05, *** p<0.01
Dependent variable is a dummy.
Standard errors are corrected for clustering at nation level.

Table A.9: Impact of Macroeconomic Variables on Average SWD Scores (Country Panel)

dependent: SWD	(1)	(2)	(3)	(4)
GDP per head	0.0060**	0.0042*	0.0043*	0.0021
	(0.002)	(0.002)	(0.003)	(0.002)
growth		0.0214***	0.0215***	0.0136***
		(0.005)	(0.005)	(0.004)
f(inflation)			-0.0001	-0.0076***
			(0.003)	(0.003)
UE rate				-0.0331***
				(0.003)
survey FE	yes	yes	yes	yes
country FE	yes	yes	yes	yes
N	483	483	482	476
R^2	0.7596	0.7716	0.7716	0.8220

* p<0.10, ** p<0.05, *** p<0.01
Dependent variable is the average of the SWD scores in a given country.

Table A.10: Results Including Time Trends

dependent: SWD	(1)	(2)	(3)	(4)
macroeconomic variables				
GDP per head	0.0010	-0.0053	0.0010	0.0011
	(0.003)	(0.008)	(0.003)	(0.002)
growth	0.0105***	0.0108***	0.0105***	0.0063***
	(0.003)	(0.002)	(0.003)	(0.002)
f(inflation)	-0.0188	-0.0232*	-0.0188	-0.0173
	(0.012)	(0.011)	(0.012)	(0.010)
UE rate	-0.0172***	-0.0155***	-0.0172***	-0.0145***
	(0.003)	(0.002)	(0.003)	(0.003)
time - time trend - survey year FE				
time			0.0030	
			(0.002)	
country specific time trend	no	yes	no	no
ind. controls	yes	yes	yes	yes
survey FE	yes	yes	yes	yes
nation FE	yes	yes	yes	yes
N	602545	602545	602545	602545
R^2	0.1433	0.1486	0.1433	0.1397

* p<0.10, ** p<0.05, *** p<0.01
Dependent variable is a dummy.
Standard errors are corrected for clustering at nation level.
(1) is the reference estimation from table 2.1, column (4).

Table A.11: Influence of Institutional Quality: Polity IV Index and Freedomhouse Data

dependent: SWD	(1)	(2)	(3)	(2)	(4)	(5)	(6)
gdphead	0.0010	0.0001	0.0006	0.0023	0.0023	0.0022	0.0023
	(0.003)	(0.004)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
growth	0.0105***	0.0121***	0.0109***	0.0091**	0.0091**	0.0092**	0.0091**
	(0.003)	(0.002)	(0.003)	(0.004)	(0.004)	(0.003)	(0.003)
loginf	-0.0188	-0.0156	-0.0182	-0.0313*	-0.0313*	-0.0321*	-0.0313*
	(0.012)	(0.012)	(0.012)	(0.017)	(0.017)	(0.018)	(0.017)
uerate	-0.0172***	-0.0164***	-0.0164***	-0.0169***	-0.0169***	-0.0167***	-0.0169***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
institutional quality							
polity4			-0.0284				
			(0.035)				
freedomstatus					0.0000		
					(0.000)		
polrights						0.0245	
						(0.058)	
civillib							-0.0021
							(0.016)
ind. controls	yes	yes	yes	yes	yes	yes	
survey FE	yes	yes	yes	yes	yes	yes	
nation FE	yes	yes	yes	yes	yes	yes	
N	602545	546751	602545	576656	576656	576656	576656
R^2	0.1433	0.1433	0.1435	0.1443	0.1443	0.1443	0.1443

* p<0.10, ** p<0.05, *** p<0.01
Dependent variable is a dummy.
Standard errors are corrected for clustering at nation level.
(1) is the reference estimation from Table 2.1, column 4. (2) is estimated on the reduced sample for which the polity IV index is equal to its highest value 10. (3) is estimated on the subsample where the freedomhouse data is available, i.e., years 2009 and 2010 are dropped. (4), (5), and (6) control for the indicators 'freedom status' (1=free, .5=partly free, 0=not free), 'political rights', and 'civil liberties' respectively. 'Political rights' and 'civil liberties' are measured on a one-to-seven scale, with one representing the highest degree of Freedom and seven the lowest.

Table A.12: Impact of Policy Variables

dependent: SWD	(1)	(2)	(3)	(4)	(5)	(6)	(7)
macroeconomic variables							
GDP per head	0.0010	-0.0013	-0.0038	-0.0013	-0.0002	0.0045	0.0038
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.009)	(0.009)
growth	0.0105***	0.0070*	0.0079***	0.0070**	0.0068*	0.0062	0.0054
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
f(inflation)	-0.0188	-0.0460***	-0.0468***	-0.0462***	-0.0456***	-0.0508**	-0.0536**
	(0.012)	(0.015)	(0.015)	(0.015)	(0.015)	(0.020)	(0.019)
UE rate	-0.0172***	-0.0184***	-0.0159***	-0.0186***	-0.0189***	-0.0184***	-0.0162***
	(0.003)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)	(0.005)
policy variables							
debt			-0.0007				
			(0.001)				
deficit				-0.0008			
				(0.003)			
elderly					0.0060		
					(0.012)		
sstran							-0.0043
							(0.003)
ind. controls	yes	yes	yes	yes	yes	yes	yes
survey FE	yes	yes	yes	yes	yes	yes	yes
nation FE	yes	yes	yes	yes	yes	yes	yes
N	602545	522403	522403	522403	522403	395936	395936
R^2	0.1433	0.1496	0.1498	0.1497	0.1497	0.1553	0.1554

* $p<0.10$, ** $p<0.05$, *** $p<0.01$
Dependent variable is a dummy.
Standard errors are corrected for clustering at nation level.
(1) is the reference estimation from table 2.1, column (4). (2) is estimated on reduced sample where debt, deficit, and elderly is available. (6) is estimated on reduced sample where debt, deficit, elderly, and sstran is available.
The variables debt, deficit and elderly are available until 2008, sstran only until 2000.

Appendix B

Appendix to Chapter 3

B.1 Descriptive Statistics

Table B.1: Agents' Behavior by Matching

Within-Group Matching

	mean	sd	median	min	max	N
Transfer if Control=0	6.140625	5.811503	6	0	24	64
Transfer if Control=1	4.90625	5.764475	4	1	24	64
Agent's Belief (Control=1)	.265625	.4451569	0	0	1	64

Between-Group Matching

	mean	sd	median	min	max	
Transfer if Control=0	3.46875	3.771105	2	0	18	64
Transfer if Control=1	2.515625	2.63669	1	1	15	64
Agent's Belief (Control=1)	.671875	.4732424	1	0	1	64

Table B.2: Principals' Behavior by Matching

Within-Group Matching

	mean	sd	median	min	max	N
Decision (Control=1)	.1875	.3933979	0	0	1	64

Between-Group Matching

	mean	sd	median	min	max	N
Decision (Control=1)	.375	.48795	0	0	1	64

B.2 Derivation of Equation 3.1

The crowding out effect for matching m is:

$$\Pi_m = -s_m(t=0) + s_m(t>1)\left[b_m \pi(m,c) + (1-b_m)\pi(m,n)\right] \tag{B.1}$$

Using $\pi(w,c) = \kappa$ and $\pi(w,n) = \kappa + \gamma$, we get in a within group matching:

$$\begin{aligned}\Pi_w &= -s_w(t=0) + s_w(t>1)\left[b_w \pi(w,c) + (1-b_w)\pi(w,n)\right] \\ &= -s_w(t=0) + s_w(t>1)\left[b_w \kappa + (1-b_w)(\kappa+\gamma)\right] \\ &= -s_w(t=0) + s_w(t>1)\left[\kappa + (1-b_w)\gamma\right] \end{aligned} \tag{B.2}$$

Using $\pi(b,c) = \kappa + \delta$ and $\pi(b,n) = \kappa + \gamma + \delta$, we can derive the crowding out effect in a between group matching:

$$\begin{aligned}\Pi_b &= -s_b(t=0) + s_b(t>1)\left[b_b \pi(b,c) + (1-b_b)\pi(b,n)\right] \\ &= -s_b(t=0) + s_b(t>1)\left[b_b(\kappa+\delta) + (1-b_b)(\kappa+\delta+\gamma)\right] \\ &= -s_b(t=0) + s_b(t>1)\left[\kappa+\delta + (1-b_b)\gamma\right] \end{aligned} \tag{B.3}$$

Lastly, substracting B.3 from B.2 yields

$$\Pi_w - \Pi_b = \underbrace{(s_b(t=0) - s_w(t=0)) + [s_w(t>1) - s_b(t>1)]\,\kappa}_{\text{Prosocial Effect}} \quad \text{(B.4)}$$
$$+ \underbrace{[s_w(t>1)(1-b_w) - s_b(t>1)(1-b_b)]\,\gamma}_{\text{Trust Effect}}$$
$$- \underbrace{s_b(t>1)\delta}_{\text{Hostility Effect}}$$

B.3 Calibration of the Model

We now turn to the decomposition of the diff-in-diff as described in section 3.4. In Table B.3, we regressed the individual crowding out on agent's beliefs and matching for agents who transfer more than 1 under no control. This allows us to identify κ, γ, and δ. The regression (see Table B.3) delivers the following parameter values:

- $\pi(w, c) = \kappa$=2.71-1.41-0.8=0.5
- $\pi(w, n) = \kappa + \gamma$ =2.71-0.8=1.91
- $\pi(b, c) = \kappa + \delta$ =2.71-1.41=1.3
- $\pi(b, n) = \kappa + \delta + \gamma$ =2.71

Hence, $\gamma = 1.41$ and $\delta = 0.8$. From above we already have $s_b(t=0) = 0.28$, $s_b(t>1) = 0.66$, $s_w(t=0) = 0.12$, $s_w(t>1) = 0.86$, $b_b = 0.57$ $b_w = 0.18$. Using these parameters, we get for the decomposition of the diff-in-diff:

$\Pi_w - \Pi_b = (s_b(t=0) - s_w(t=0)) + [s_w(t>1) - s_b(t>1)]\,\kappa +$
$[s_w(t>1)(1-b_w) - s_b(t>1)(1-b_b)]\,\gamma - s_b(t>1)\delta$

$= 0.16 + (0.86 - 0.66) \cdot 0.5 + (0.86 \cdot 0.82 - 0.66 \cdot 0.43) \cdot 1.4 - 0.66 \cdot 0.8$

=0.26 (Prosocial Effect) $+$ 0.59 (Trust Effect) - 0.52 (Hostility Effect)

$= 0.33$

Table B.3: Crowding Out of Agents' Transfers

Dependent Variable: Diff	(1)
Within-group matching (=1)	-0.806*
	(0.460)
Agent's Belief (Control=1)	-1.419***
	(0.500)
Constant	2.716***
	(0.558)
N	97

Notes: Standard errors are in parentheses. Table reports results of OLS regression. Dependent variable is the individual difference between transfers under control and under no control. We restrict the sample to agents that transfer more than 1 when not being controlled. Standard errors are clustered at the individual level.
Level of significance: * p<0.10, ** p<0.05, *** p<0.01

B.4 Robustness

Dictator Game In this section, we provide regression results of our baseline estimations (see Table 3.1 and Table 3.2) separately for No-Dictator and Dictator Treatments.

Table B.4: Agents' Transfer- No-Dictator and Dictator Treatment

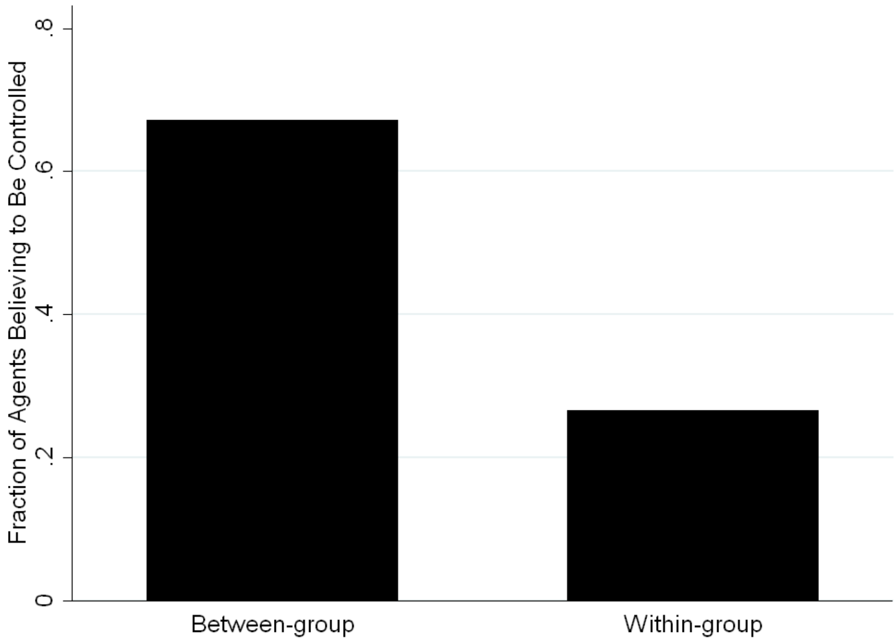
Dependent Variable: Transfer	(1)	(2)	(3)	(4)	(5)	(6)
Control (=1)	-1.094***	-0.953***	-0.813***	-0.844**	-1.375***	-1.063**
	(0.242)	(0.291)	(0.270)	(0.315)	(0.402)	(0.498)
Within-group matching (=1)	2.531***	2.672***	1.469*	1.438*	3.594***	3.906***
	(0.663)	(0.699)	(0.747)	(0.775)	(1.081)	(1.144)
Control x Within-group		-0.281		0.0625		-0.625
		(0.314)		(0.276)		(0.567)
Constant	3.539***	3.469***	3.422***	3.438***	3.656***	3.500***
	(0.444)	(0.474)	(0.618)	(0.634)	(0.651)	(0.723)
N	256	256	128	128	128	128

Notes: Standard errors are in parentheses. All columns report results of OLS regressions. Dependent variable is the individual transfer. Column 1 and 2 are the baseline estimations including the full sample. Column 3 and 4 are the estimations restricted to the No-Dictator sample and Column 5 and Column 6 are the estimations restricted to the Dictator sample. Standard errors are clustered at the individual level.
Level of significance: * $p<0.10$, ** $p<0.05$, *** $p<0.01$

Table B.5: Agents' Crowding Out - No-Dictator and Dictator Treatment

Dependent Variable: Diff	(1)	(2)	(3)
Within-group matching (=1)	-0.806*	-0.807*	-0.893
	(0.460)	(0.409)	(0.842)
Agent's Belief (Control=1)	-1.419***	-0.907*	-2.040**
	(0.500)	(0.478)	(0.840)
Constant	2.716***	2.086***	3.474***
	(0.558)	(0.455)	(1.007)
Transfer (Control=0)	no	no	no
[Transfer (Control=0)]^2	no	no	no
N	97	48	49

Notes: Standard errors are in parentheses. All columns report results of OLS regressions. Dependent variable is the individual difference between transfers under no control and under control. We restrict the sample to agents that transfer more than 1 point when not being controlled. Column 1 is the baseline estimation including the full sample. Column 2 is the estimation restricted to the No-Dictator sample and Column 3 is the estimation restricted to the Dictator sample. Standard errors are clustered at the individual level.
Level of significance: * p<0.10, ** p<0.05, *** p<0.01



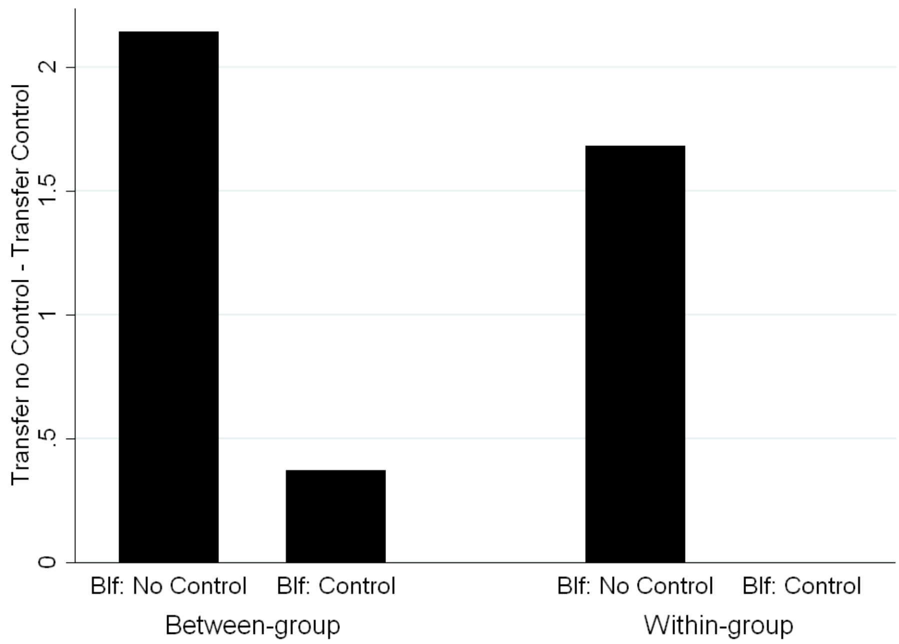
Notes: The figure depicts average individual beliefs about being controlled as a function of matching.

Figure B.1: Average Beliefs about Principal's Decision to Control - Full Sample

Full Sample In this section, we reproduce our estimation of Table 3.2 including the full sample. That is, also subjects who exhibit a transfer of zero or one when not being controlled are now included in the estimation. Clearly, from our discussion of the behavioral model in Section 3.4 this inclusion is not adequate in the light of our proposed explanation of crowding out. By definition, we can only observe crowding out when subjects transfer more than 1 point.[1] Moreover, trust as well as hostility can only play a role for subjects for whom we observe crowding out. Nevertheless, we present these results here to show the robustness of our estimations and graphs presented in the main text.

Comparing Table B.6 to Table 3.2, it becomes evident that results are very similar. The only difference appears in specification (1): When the full sample is included, the coefficient of the matching dummy (*Within-group matching (=1)*) changes to -0.4 and is not significant anymore. Once we control for the transfer, the size as well as significance

[1]For subjects who transfer zero when not being controlled crowding out can actually be negative as those subjects are pushed to a transfer of at least one. That is also why, when we control for agents who believe to be controlled in a within-group matching, we observe aggregate crowding out of zero (Figure B.2).



Notes: The figure depicts average crowding out, i.e., transfers under no control - transfers under control, as a function of agents's beliefs and matching.

Figure B.2: Average Crowding Out Depending on Agents' Beliefs and Matching - Full Sample of this coefficient is restored. The difference is to be expected as in a between-group matching a disproportional share of subjects transfers zero or one thereby exhibiting no or even negative crowding out. This obviously upwards biases the estimator of the matching dummy.

Table B.6: Agents' Crowding Out - All Subjects

Dependent Variable:	Diff	Diff	Diff	Fraction	Fraction	Fraction
	(1)	(2)	(3)	(4)	(5)	(6)
Within-group matching (=1)	-0.421	-0.773*	-0.801**	-0.153***	-0.145**	-0.148**
	(0.396)	(0.395)	(0.393)	(0.0530)	(0.0596)	(0.0594)
Agent's Belief (Control=1)	-1.729***	-1.467***	-0.965***	-0.214**	-0.215***	-0.195**
	(0.454)	(0.397)	(0.352)	(0.0804)	(0.0804)	(0.0796)
Transfer (Control=0)	no	yes	yes	no	yes	yes
[Transfer (Control=0)]^2	no	no	yes	no	no	yes
N	128	128	128	102	102	102

Notes: Standard errors are in parentheses. All columns report results of OLS regressions. Dependent variable is the individual difference between transfers under control and under no control. Standard errors are clustered at the individual level.
Level of significance: * p<0.10, ** p<0.05, *** p<0.01

B.5 Instructions

This is the English translation of the instructions (original in German). Instructions of part I were displayed directly on screen; instructions of part II were handed out. Beginning at the 11th screen, the participant had a banner displaying the group she belonged to at the top of every screen, which is not displayed here. The instructions of part I are based on the instructions of Chen and Li (2009). The instructions of part II are based on the instructions of Falk and Kosfeld (2006).

Part I

[1st screen]

You are now participating in an economic experiment. The income you can earn in the course of this experiment depends on your decisions and the other participants‘ decisions. The experiment consists of two parts and <no of participants> persons take part.

During the experiment, your income will be denoted in points. Your final income consists of the income you earn in the two parts of the experiment. At the end of the experiment, you will be paid out in cash. Please note that:

2 points = 1 € .

All decisions will be made anonymously. At the end of the experiment, each participant will be paid out in private.

If you have any questions, please ask for assistance. Your question will be answered at your workplace. Please do not talk to any of the other participants during the experiment.

[2nd screen]

In Part 1 everyone will be shown 5 pairs of paintings by two artists. The paintings were created by two distinct painters. Each pair of paintings consists of one painting being made by each artist. For each pair, please choose the painting you prefer. According to the paintings you and the other participants choose, you (and the other participants) will be classified into two equally large groups.

The participants you are grouped with will be the same for the rest of the experiment.

After Part 1 has finished, you will be given further instructions.

[3rd screen]

Now, please choose which painting you prefer by clicking on one of the buttons.

After everyone has submitted answers, you will be privately informed of which group you are in.

[4th to 9th screen]

Pair #: Please select which painting you prefer

<Picture 1> <Picture 2>

I prefer this painting I prefer this painting

[10th screen]

You are assigned to the < "Kandinsky" - "Klee" > group.

The number of people in your own group is <Number of Group Members> .

[11th screen]

You will now receive two more paintings (painting #6 and #7) and 3 skill questions. Please select the artist who you think created the paintings and answer the skill questions as best as you can, respectively. The correct answers of a group will be summed up and divided by the number of group members. The group which achieves the highest average will receive a prize of 16 points per group member. That is, each member of this group will receive an additional payment of 16 points at the end of the experiment. The other group obtains no payment.

In the case of a tie, the computer will randomly determine one group which receives the prize.

You will be informed of which group received the prize at the end of the experiment.

Meanwhile, you can use a group chat program to get help from or offer help to other members in your own group. Except for the following restrictions, you can type whatever you want in the lower box of the chat program. Messages will be shared among all the members from your own group. You will not be able to see the messages exchanged among the other group. People in the other group will not see the messages from your own group either.

Restrictions on messages
1. Please do not identify yourself or send any information that could be used to identify you (e.g. age, subject, sex, etc.).
2. Please refrain from using obscene or offensive language.

[12th screen]

<Picture 6> This painting is by <“Klee“> is by <“Kandinsky“>
<Picture 7> This painting is by <“Klee“> is by <“Kandinsky“>

Type your comments below then press Enter

<Chat Box>

[13th screen]

Question 1: What was the inflation rate in Germany in 2010 (in %)?
Question 2: What was the budget of the German Department of Defense in 2007 (in Million EUR)?
Question 3: How many members does the current federal cabinet of Germany (as of June 2011) have?

Answer 1: The inflation in Germany in 2010 was a) 1.1 % b) 1.8 % c) 2.1 %
Answer 2: The federal budget was a) 24 519 b) 27 578 c) 31 560
Answer 3: The current cabinet comprises (June 2011) the following number of members
a) 13 b) 16 c) 17

Type your comments below then press Enter

<Chat Box>

[14th screen]

You have finished part I of the experiment. As soon as all participants have completed part I, the experiment continues.

Part II

This part of the experiment comprises two rounds. You can earn points in both rounds. As before it holds that:

2 points = 1 Euro

Your final income in part II will be determined by one of these rounds. The round determining your income will be randomly drawn by the computer at the end of the experiment. It is the same round for each participant.

At the beginning of part II, the computer will randomly draw each participant either as participant A or as participant B. **This assignment will be kept the same during part II. That is, a participant is either participant A or participant B during the two rounds.**

In both rounds, two participants A and B are randomly associated.

In the second round, each participant A will be associated with another participant B than in the first round.

Hence, the association is such that the same participants A and B will never be associated twice.

No participant knows with whom he is associated meaning that all decisions are made anonymously. Yet, each participant knows the group membership of the participant he is associated with.

At the beginning of each round, participant A receives an amount of 24 points. Participant B receives no points.

Participant A's decision:

Participant A can decide how many points he wants to transfer to participant B. The experimenter triples each point which A transfers to B. Thus, each point which A transfers to B reduces A's income by one point and increases B's income by three points.

The formula for calculating income is as follows:

Participant A's income: 24 - transfer

Participant B's income: 0 + 3*transfer

The following examples will clarify the income formulas:

Example 1: A transfers 0 points to B. The incomes are then 24 for A and 0 for B.
Example 2: A transfers 4 points to B. The incomes are then 20 for A and 12 for B.
Example 3: A transfers 16 points to B. The incomes are then 8 for A and 48 for B.

Participant B's decision:

Before A decides how many points he wishes to transfer to B, B can determine a minimum transfer. In particular, B can constrain his associated participant A to transfer him at least 1 point. However, he can also decide not to limit participant A and thus leave his transfer decision completely free.

Therefore, there are two cases:

Case 1: Participant B constrains participant A to transfer at least 1 point to him. In this case, participant A can transfer any (integer) amount **between 1 and 24** to B.

Case 2: Participant B allows participant A to decide on his transfer freely and does not constrain him to transfer at least 1 point to him. In this case, participant A can transfer any (integer) amount **between 0 and 24** to B.

Therefore, the experiment consists of two stages:

Stage 1:

In stage 1, B decides if he will constrain A to transfer at least 1 point to him, or if he will allow A to decide freely.

Stage 2:

In stage 2, A decides which amount he will transfer to B. This amount lies

- between 1 and 24, if B *constrains* A to transfer at least 1 point;

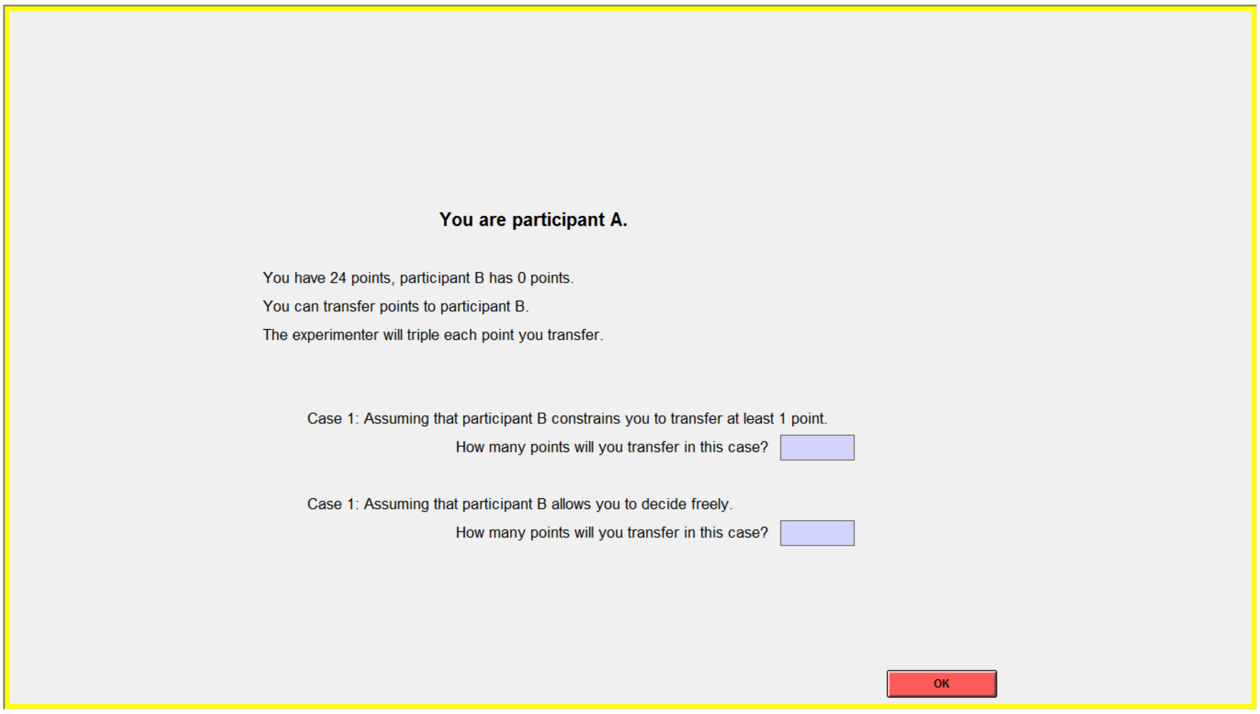
or

- between 0 and 24, if B *does not constrain* A to transfer at least 1 point.

The round is completed as soon as A has decided how many points he will transfer to B.

A and B will make their entries with the help of a computer monitor.

Please note: Participant A must decide which amount he will transfer to B **before he knows whether or not B will constrain him to transfer at least 1 point**. This means that A has to make two decisions. A can make his entries with the help of this monitor:



Therefore, A indicates how many points he transfers to B when B constrains him to transfer at least 1 point (case 1) and how many points he will transfer when he is free to decide (case 2).

Which of the decisions is relevant for A depends on what B decides. If he constrains A to transfer at least 1 point, the decision A gives under case 1 applies. If he leaves A free in his decision, the point amount which A indicates under case 2 applies.

A final income monitor will inform you of the randomly drawn round and the resulting incomes.

At the end of the experiment, your point income of the randomly drawn round will be converted to EUR. This amount will be paid out to you in cash. If you belong to the group which won the prize in the first part of the experiment, you will be paid out this prize in addition.

Do you have any questions?

Please solve the following control questions. They have no consequence on your income and only serve to check if all participants in the experiment have understood the rules.

Question 1: Assume that participant B allows A to decide freely. A transfers 4 points. What are the incomes?

Income for A:

Income for B:

Question 2: Assume that participant B constrains A to transfer at least 1 point. A transfers 1 point. What are the incomes?

Income for A:

Income for B:

Question 3: Assume that participant B allows A to decide freely. A transfers 0 points. What are the incomes?

Income for A:

Income for B:

Please raise your hand when you have solved the control questions.

Appendix C

Appendix to Chapter 4

C.1 Descriptive Statistics

Table C.1: Dictators' Transfer Decisions - Summary Statistics Baseline Sessions

	Mean	Standard Deviation	Median
1st Part			
Transfer (=1)	.3555556	.4840903	0
2nd Part (with reward)			
Transfer (=1)	.4	.4954337	0
3rd Part (with reward)			
Benefit=25: Transfer (=1)	.2444444	.4346135	0
Benefit=30: Transfer (=1)	.2222222	.4204375	0
Benefit=40: Transfer (=1)	.2222222	.4204375	0
Benefit=50: Transfer (=1)	.2666667	.4472136	0
Benefit=60: Transfer (=1)	.3333333	.4767313	0
Benefit=70: Transfer (=1)	.3333333	.4767313	0
4th Part			
Benefit=25: Transfer (=1)	.1777778	.3866458	0
Benefit=30: Transfer (=1)	.1777778	.3866458	0
Benefit=40: Transfer (=1)	.1777778	.3866458	0
Benefit=50: Transfer (=1)	.2444444	.4346135	0
Benefit=60: Transfer (=1)	.2666667	.4472136	0
Benefit=70: Transfer (=1)	.3111111	.4681794	0
$N = 45$			

Table C.2: Dictators' Transfer Decisions - Summary Statistics Robustness Sessions

	Mean	Standard Deviation	Median
2nd Part			
Transfer (=1)	.2333333	.4301831	0
4th Part (with reward)			
Transfer (=1)	.2	.4068381	0
1st Part			
Benefit=25: Transfer (=1)	.2666667	.4497764	0
Benefit=30: Transfer (=1)	.2333333	.4301831	0
Benefit=40: Transfer (=1)	.2	.4068381	0
Benefit=50: Transfer (=1)	.2666667	.4497764	0
Benefit=60: Transfer (=1)	.2666667	.4497764	0
Benefit=70: Transfer (=1)	.2333333	.4301831	0
3rd Part (with reward)			
Benefit=25: Transfer (=1)	.2333333	.4301831	0
Benefit=30: Transfer (=1)	.1666667	.379049	0
Benefit=40: Transfer (=1)	.1	.3051286	0
Benefit=50: Transfer (=1)	.1666667	.379049	0
Benefit=60: Transfer (=1)	.1666667	.379049	0
Benefit=70: Transfer (=1)	.2333333	.4301831	0
$N = 30$			

C.2 Derivations Section 4.4

In this section, we show under which parameter constellations individuals are willing to transfer with efficiency concern, maximin preferences, and inequality aversion. Moreover, we derive the conditions that allow us to formulate Hypotheses 2 and 3.

C.2.1 Symmetric Information - Efficiency Concern and Maximin Preferences

Efficiency Concern Given his individual parameter λ^i and the value of b, player i will transfer if

$$U_A^i(80, 50 + b) > U_A^i(100, 50) \Leftrightarrow \lambda^i > \frac{20}{b}. \tag{C.1}$$

Given λ^i, i will transfer at least for $b = 70$ if $\lambda^i > \frac{2}{7}$. If $\lambda^i > \frac{4}{5}$ holds, then individual i will transfer for all values of b in our experiment. For $\frac{2}{7} < \lambda^i < \frac{4}{5}$ let b^* denote the lowest value of b for which i is willing to transfer. Individual i will transfer for b^* and all values of b with $b > b^*$. If, however, $\lambda^i < \frac{2}{7}$ holds, then i does not weight the efficiency gain highly enough for any b in the experiment that the possible values of b suffice to compensate individual i for his own payoff loss. In that case i will never transfer. Hence, subjects with an efficiency concern should exhibit the following transfer pattern: Either they do not transfer at all, or they transfer for a particular value of b^* and all values $b > b^*$.

Maximin Preferences Subjects with maximin preferences will transfer the 20 points if it holds that:

$$\begin{aligned} U_A^i(80, 50 + b) &> U_A^i(100, 50) \\ (1 - \lambda^i)80 + \lambda^i \min[80, 50 + b] &> 100 - \lambda^i 50 \\ \lambda^i(\min[80, 50 + b] - 30) &> 20 \\ \lambda^i &> \frac{20}{(\min[80, 50 + b] - 30)} \end{aligned}$$

If $\lambda^i > \frac{2}{5}$, i will transfer to achieve a more equal allocation for values of $b \geq 30$. If $\lambda^i > \frac{20}{45}$, i will also transfer for $b = 25$. If, however, $\lambda^i < \frac{2}{5}$, i will not transfer as selfishness motives dominate.

C.2.2 Symmetric Information - Inequality Aversion

In this section, we derive what transfer patterns we should observe for inequality aversion. To begin with, note that it holds that
$U_A(80,80) > U_A(80,75) > U_A(80,90) > U_A(80,100)$ for all parameters as one can directly see from the following equations (and using that $\alpha^i \geq \beta^i$):

$$\begin{aligned}
U_A(80,80) &= 80 \\
U_A(80,75) &= 80 - 5\beta^i \\
U_A(80,90) &= 80 - 10\alpha^i \\
U_A(80,100) &= 80 - 20\alpha^i
\end{aligned}$$

Hence, whenever an individual is willing to transfer for $b = 50$, he will be willing to transfer for values of $b < 50$ as well. Next, we show that there actually exist parameters such that subjects may transfer for $b = 50$.

$$
\begin{aligned}
U_A(80, 100) > U_A(100, 50) &\Leftrightarrow \\
80 - 20\alpha^i > 100 - 50\beta^i &\Leftrightarrow \\
50\beta^i - 20\alpha^i > 20 &\Leftrightarrow \\
50\beta^i - 20 > 20\alpha^i &\Leftrightarrow \\
\frac{5}{2}\beta^i - 1 > \alpha^i &\Rightarrow \\
\frac{5}{2}\beta^i - 1 > \alpha^i \geq \beta^i &\Rightarrow \\
\frac{5}{2}\beta^i - 1 > \beta^i &\Leftrightarrow \\
\frac{3}{2}\beta^i > 1 &\Leftrightarrow \\
\beta^i > \frac{2}{3}
\end{aligned}
$$

For values of $b > 50$, however, no parameters exist such that individuals will transfer. Suppose $U_A(80, 110) > U_A(100, 50)$ held, then

$$
\begin{aligned}
U_A(80, 110) > U_A(100, 50) &\Leftrightarrow \\
80 - 30\alpha^i > 100 - 50\beta^i &\Leftrightarrow \\
50\beta^i - 30\alpha^i > 20 &\Leftrightarrow \\
50\beta^i - 20 > 30\alpha^i &\Leftrightarrow \\
\frac{5}{3}\beta^i - \frac{2}{3} > \alpha^i &\Rightarrow \\
\frac{5}{3}\beta^i - \frac{2}{3} > \alpha^i \geq \beta^i &\Rightarrow \\
\frac{2}{3}\beta^i > \frac{2}{3} &\Leftrightarrow \\
\beta^i > 1
\end{aligned}
$$

Yet, by assumption $\beta^i < 1$. Thus, for $b = 60$ no admissible parameters exists, such that a transfer makes an individual better off.

C.2.3 Asymmetric Information

Inequality Aversion We will proceed as follows. We will show it always holds that the utility from transferring under asymmetric information $E(U_A(\pi_A, \pi_B)|transfer = YES)$ is smaller than the utility from transferring for $b = 40$ but larger than for $b = 50$ under symmetric information, that is, it holds that $U_A(80, 90) > E(U_A(\pi_A, \pi_B)|transfer = YES) > U_A(80, 100)$. Hence, if it holds that player i transfers for $b = 50$, i.e., $U_A(80, 100) > U_A(100, 50)$, then it also holds that $E(U_A(\pi_A, \pi_B)|transfer = YES) > U_A(100, 50)$. Thus, a transfer for $b = 50$ then implies that a player should transfer under asymmetric information. On the other hand, if player i does not transfer for $b = 40$, i.e., $U_A(80, 90) < U_A(100, 50)$, it implies that $E(U_A(\pi_A, \pi_B)|transfer = YES) < U_A(100, 50)$ and therefore a player will also not transfer under asymmetric information.
For $\alpha^i, \beta^i > 0$ consider:

$$\begin{aligned}
U_A(80, 90) - E(U_A(\pi_A, \pi_B)|transfer = YES) &= \\
80 - 10\alpha^i - \left(80 - \frac{1}{6}\left[5\beta^i + 100\alpha^i\right]\right) &= \\
-10\alpha^i + \frac{5}{6}\beta^i + \frac{100}{6}\alpha^i &= \\
\frac{40}{6}\alpha^i + \frac{5}{6}\beta^i > 0 &
\end{aligned}$$

And lastly, as $\beta^i \leq \alpha^i$:

$$\begin{aligned}
E(U_A(\pi_A, \pi_B)|transfer = YES) - U_A(80, 100) &= \\
80 - \frac{1}{6}\left[5\beta^i + 100\alpha^i\right] - \left[80 - 20\alpha^i\right] &= \\
-\frac{5}{6}\beta^i + \frac{20}{6}\alpha^i > 0 &
\end{aligned}$$

Efficiency Concern For efficiency concern, it holds that an individual i who transfers for $b = 40$ should transfer under asymmetric information. However, an individual who does not transfer for $b = 50$, should not transfer under asymmetric information. The argument is analogous to the inequality aversion case. First, note that it holds that $U(80,100) > E(U(\pi_A,\pi_B)|transfer = YES) > U(80,90)$ (for $\lambda^i > 0$).

$$\begin{aligned}
E(U(\pi_A,\pi_B)|transfer = YES) - U(80,90) &= \\
(1-\lambda^i)80 + \frac{1}{6}\lambda^i\left[155+160+170+180+190+200\right] - \left[(1-\lambda^i)80 + \lambda^i(170)\right] &= \\
80 - 80\lambda^i + \frac{1055}{6}\lambda^i - \left[80 + 90\lambda^i\right] &= \\
80 + \frac{575}{6}\lambda^i - \left[80 + 90\lambda^i\right] &= \\
\frac{35}{6}\lambda^i > 0
\end{aligned}$$

$$\begin{aligned}
E(U(\pi_A,\pi_B)|transfer = YES) - U(80,100) &= \\
80 + \frac{575}{6}\lambda^i - \left[80 + 100\lambda^i\right] &= \\
-\frac{25}{6}\lambda^i < 0
\end{aligned}$$

So, if it holds that i transfers for $b = 40$, i.e., $U(80,90) > U(100,50)$ which is fulfilled for $\lambda^i > \frac{1}{2}$, then i will also transfer under asymmetric information. On the other hand, if it holds that i does not transfer for $b = 50$, i.e., $U(80,100) < U(100,50)$ which holds for $\lambda^i < \frac{2}{5}$, i will not transfer under asymmetric information.

C.3 Instructions

The original instructions were in German. In the following, we provide an English version.

General Instructions for Participants

You are now participating in an economic experiment. Please, read the following instruction carefully. It explains everything what you need to know for the participation in the experiment. If you have any question, please, just raise your hand. Your question will be answered at your workplace. Apart from that, any sort of communication during the experiment is forbidden. If you violate this rule, you will be excluded from the experiment and will not receive any payment.

The experiment consists of four parts. You obtain a separate instruction for each part.

In all four parts you can earn points. It holds that:

10 Points = 1 Euro

Your final payment will be determined by the payment earned in one out of the four parts comprising the experiment. At the end of the experiment, the experimenter draws from an urn. The draw will determine one out of the four parts for all participants. You will receive the payment which you earned for this part in cash.

After each part you will be informed how many points you earned for this part. You obtain no information concerning the earnings of other participants.

The Experiment

The computer randomly assigns either the role of Participant A or Participant B to each participant. At the beginning of the experiment, your computer will inform you whether you are Participant A or Participant B.

This assignment does not change during the experiment. Each participant will stay either Participant A or Participant B during all four parts of the experiment.

In all four parts two participants, A and B, are randomly assigned to each other.

In each part of the experiment, another Participant B is assigned to a participant A. As a result, the same two participants will never be assigned to each other more than once.

No participant knows whom he is assigned to. That means all decisions are anonymous.

Do you have any questions? If yes, please raise your hand. The experimenter will answer your question at your workplace.

Please, read the instruction for part 1 of the experiment on the next pages.

You obtain the instructions for parts 2 to 4 at the beginning of the respective part.

Part 1

Participant **A** obtains an amount of 100 points. Participant **B** obtains an amount of 50 points.

The Decision of Participant A

A has to decide if he is ready to spend 20 points in order to increase the payment of **B** by **b** points. That means instead of the amount of 50 points **B** obtains the payoff of **50 + b**. **A** can spend either 20 points or nothing.

If **A** decides not to spend 20 points, both participants obtain their original payoffs: **A** receives 100 and **B** 50 points.

The exact value of **b** is determined before the start of the actual experiment: the experimenter draws one ball from an urn.

The urn contains the following balls, each represents one value of **b**:

1 ball with b=25, 1 ball with b=30, 1 ball with b=40,

1 ball with b=50, 1 ball with b=60, 1 ball with b=70

Participant **B** will be informed on his screen which ball has been drawn. Participant **A** however, will not be informed about the exact value of **b**.

The Decision of Participant B

In contrast to Participant **A**, Participant **B** is informed about the exact value of **b** at the beginning of part 1.

Participant **B** makes no decisions.

Please, answer the following control questions. These question do not influence your payments and only serve to check if all participants understood the rules of the experiment correctly.

Question 1. Assume that Participant A decides to spend 20 points. What will be the payoffs if the ball with b = 30 is drawn?

Payoff for A:

Payoff for B:

Question 2. Assume that Participant A decides *not* to spend 20 points. What will be the payoffs if the ball with b = 50 is drawn?

Payoff for A:

Payoff for B:

After you have answered the questions, please, raise your hand. The experimenter will check your answers at your workplace. When all the participants are ready, we start with the actual experiment.

Part 2

Participant **A** obtains an amount of 100 points. Participant **B** obtains an amount of 50 points.

The Decision of Participant A

A has to decide if he is ready to spend 20 points in order to increase the payment of **B** by **b** points. That means instead of the amount of 50 points **B** obtains the payoff of 50 + b. **A** can spend either 20 points or nothing.

If **A** decides not to spend 20 points, both participants obtain their original payoffs: A receives 100 and **B** 50 points.

The exact value of **b** is determined before the start of the actual experiment: the experimenter draws one ball from an urn.

The urn contains the following balls, each represents one value of **b**:

1 ball with b=25, 1 ball with b=30, 1 ball with b=40,

1 ball with b=50, 1 ball with b=60, 1 ball with b=70

Participant **B** will be informed on his screen which ball has been drawn. Participant **A** however, will not be informed about the exact value of **b**.

If **A** decides to spend 20 points, he obtains a reward - **r** points. It holds that **5 points ≤ r ≤ 10 points.** The exact value of **r** will be determined after b is determined. Participant **A** will be informed about the value of **r** on his display.

The Decision of Participant B

In contrast to Participant **A**, Participant **B** is informed about the exact value of **b** at the beginning of part 1. Participant **B** will be informed about the value of **r** on his display.

Participant **B** makes no decisions.

Part 3

Participant **A** obtains an amount of 100 points. Participant **B** obtains an amount of 50 points.

The Decision of Participant A

A has to decide if he is ready to spend 20 points in order to increase the payment of **B** by **b** points. That means instead of the amount of 50 points **B** obtains the payoff of 50 + **b**. **A** can spend either 20 points or nothing.

If **A** decides not to spend 20 points, both participants obtain their original payoffs: **A** receives 100 and **B** 50 points.

b can obtain different values. **A** has to make a decision for each of the following cases:

1. **b = 25**
2. **b = 30**
3. **b = 40**
4. **b = 50**
5. **b = 60**
6. **b = 70**

In the end of part 3, the experimentator draws one ball from the urn, which will determine the decision problem, relevant for the payoff in this part of the experiment.

If **A** decides to spend 20 points, he obtains a reward - **r** points. It holds that **5 points ≤ r ≤ 10 points.** Participant **A** will be informed about the value of **r** on his display.

The Decision of Participant B

Participant **B** makes no decisions. Participant **B** will be informed about the value of **r** on his display.

.

Please, answer the following control questions. These question do not influence your payments and only serve to check if all participants understood the rules of the experiment correctly.

Question 1. Assume that Participant A decides to spend 20 points. What will be the payoffs in the case (1)?

Payoff for A:
Payoff for B:

Question 2. Assume that Participant A decides *not* to spend 20 points. What will be the payoffs in the case (4)?

Payoff for A:
Payoff for B:

After you have answered the questions, please, raise your hand. The experimenter will check your answers at your workplace.

Part 4

Participant **A** obtains an amount of 100 points. Participant **B** obtains an amount of 50 points.

The Decision of Participant A

A has to decide if he is ready to spend 20 points in order to increase the payment of **B** by **b** points. That means instead of the amount of 50 points **B** obtains the payoff of **50 + b**. **A** can spend either 20 points or nothing.

If **A** decides not to spend 20 points, both participants obtain their original payoffs: **A** receives 100 and **B** 50 points.

b can obtain different values. **A** has to make a decision for each of the following cases:

1. **b = 25**
2. **b = 30**
3. **b = 40**
4. **b = 50**
5. **b = 60**
6. **b = 70**

In the end of part 3, the experimentator draws one ball from the urn, which will determine the decision problem, relevant for the payoff in this part of the experiment.

The Decision of Participant B

Participant **B** makes no decisions.

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Eidesstattliche Erklärung

Hiermit erkläre ich, die vorliegende Dissertation selbständig angefertigt und mich keiner anderen als der in ihr angegebenen Hilfsmittel bedient zu haben. Insbesondere sind sämtliche Zitate aus anderen Quellen als solche gekennzeichnet und mit Quellenangaben versehen.

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