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Relative Consumption Concerns or Non-Monotonic Preferences?

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Relative consumption concerns or non-monotonic preferences?

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Abstract

We conduct a classroom survey to investigate the willingness to sacrifice consumption in absolute terms in order to ascend above others in terms of consumption levels. In contrast to other studies using survey methodologies, participants are divided into a treatment and a control group. This allows us to distinguish whether choosing less in absolute terms is really induced by relative consumption concerns, or else by non-monotonic preferences. We find that relative consumption concerns provide a good explanation for choosing less in the case of some goods, while this is not the case for a number of other goods.

Keywords: status consumption, social status, behavioral economics, other-regarding preferences
JEL: C91, D63, D10

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1 Introduction

To date, there exist several studies which use survey data to analyze the magnitude of relative consumption effects for different goods. These include Solnick and Hemenway (1998, 2005), Johansson-Stenman et al. (2002), Alpizar et al. (2005), Carlsson et al. (2007), Solnick et al. (2007), Carlsson and Qin (2010), and Hillesheim and Mechtel (2010). The methodology of all these surveys is very similar: participants are asked to choose between two different states of the world, namely a positional and a non-positional scenario. In the non-positional scenario, participants consume less of a certain good than the average person. In the positional scenario, they consume an amount of this good which is higher than the average consumption level but smaller than the amount consumed in the non-positional scenario. Using this technique, it can be examined whether survey participants are willing to sacrifice consumption in absolute terms in order to improve their consumption rank in comparison to others.

All of these studies rest on the assumption that the only incentive for choosing the positional scenario is the improvement in individual rank. In contrast, we argue that choosing the positional scenario may also be motivated by other inducements. Preferences with regard to certain goods may not be monotone, implying that consuming more of a certain good may be not utility-enhancing. The underlying reasons can be manifold. An individual who dislikes cleaning the house may, for example, prefer a three-room apartment over a five-room apartment no matter how many rooms the average person's home has. Likewise, a pacifist may desire to minimize military expenditures and therefore prefers the positional scenario, but not actually cares whether her country's military expenses are higher or lower than the expenses of other countries. As the existence of such preferences was ruled out in previous surveys, the relative consumption effects associated with some goods may have been under- or overestimated.

In this paper, we aim to examine whether individuals choose the positional scenario due to relative consumption concerns or because of non-monotonic preferences. In order to separate the two, survey participants were divided into a control group and a treatment group. The members of both groups were asked to choose between two different states of the world. While participants in the treatment group were provided with a positional and a non-positional scenario analogous to previous studies, members of the control group did not receive any information about average consumption levels and were thus requested to simply

choose between a higher and a lower consumption level for a certain good. Using a between-subjects design allows us to calculate the treatment effect, i.e., the difference between the share of positional answers in the treatment group and the share of absolutely worse answers in the control group, as the measure for an item's degree of positionality.

Our results are intriguing: substantial and highly significant treatment effects can be found for parents' income and internship income, even though the share of positional answers for these two in the treatment group is not very high. For national defense spending, our results do not indicate a significant treatment effect, despite it having the highest share of positional answers in the treatment group. These two examples illustrate that determining the degree of positionality on the basis of the share of positional answers (as has been done in previous papers) can be misleading.

The paper proceeds as follows. In Section 2, we provide an overview of the related literature. Subsequently, we continue with a detailed description of the survey structure and methodology in Section 3.1, before results are presented and discussed in Section 3.2. In Section 4 we conclude with regard to the impact of our findings.

2 Related literature

In recent years, economists have once again turned their scholarly interest to the phenomenon of status-seeking behavior. Individuals compare themselves to others, for example with regard to the consumption levels of certain goods, in order to determine their individual rank in society. Thus, the relationship between individual consumption and the amount consumed by others enters the utility function. Relative consumption effects were already illustrated by Rae and Veblen in the 19th century and by Duesenberry (1949) in the middle of the 20th century.[1] Much later, Frank (1985) demonstrated analytically that if the status implications of different goods vary in scale, non-cooperative consumption decisions result in underconsumption of those goods which have only a weak positive impact on relative standing. An adequate tax scheme, with higher tax rates on the consumption of goods with a strong positive impact on relative standing, can avoid these inefficiencies. Gaining information about differing degrees of positionality for different goods in order to be able to implement such a tax scheme in the future is one important benefit of this field of research.

[1] Veblen's "Theory of the leisure class", first published in 1899, is available at Project Gutenberg; see Veblen (1997). Rae's original publication from 1834 was reprinted under a new title; see Rae (1905).

Relative income preferences have been investigated by scholars such as Zeckhauser (1991), who asked his (US) students whether they would prefer to have a per capita income of $\$22,000$ in Japan and $\$23,000$ in the United States, or $\$25,000$ in Japan and $\$24,000$ in the United States. Most of them preferred the first scenario. Tversky and Griffin (1991) consider relative income preferences by asking subjects whether they would rather work at magazine A or magazine B. Working for magazine A, they earn $\$35,000$, while their colleagues receive $\$38,000$. Magazine B pays them $\$33,000$, with others earning $\$30,000$. A significant share of participants chose magazine B, sacrificing income in absolute terms for the sake of higher relative standing within the firm. Johansson-Stenman et al. (2002) use comparable questions in order to test for positional concerns with respect to income. Their results show that participants care about relative income, but are also inequality averse: they prefer living in a society in which they are better off than the average person, but they also reveal a certain limit of acceptable income inequality.

Using survey questions of a very similar structure, Solnick and Hemenway (1998) test for positional concerns associated with different personal attributes and income. They provide evidence of strong positional concerns regarding not only an individual's personal income, but also her attractiveness and intelligence, illustrating that the concern for relative standing is not limited to income only. Taking a similar approach, Alpizar et al. (2005) consider a number of private goods in their survey: cars, housing, insurance, and vacation. They find that cars and housing are more positional than insurance and vacation, although the latter two nevertheless display a significant share of positional answers. The authors explain this finding with reference to the visibility of consumption of a good. In a second study, Solnick and Hemenway (2005) include additional private goods and expand the analysis to public goods as well. Their results show that relative consumption effects are, on average, more pronounced for public than for private goods. Moreover, Carlsson et al. (2007) use a random sample in Sweden in order to calculate the participants' degree of positionality, i.e., the share of marginal utility stemming from comparison with others. Their results indicate that the average degree of positionality is significantly larger than zero and increases with the visibility of a good. Building on previous studies, Solnick et al. (2007) focus on country differences between the US and China. In contrast to US participants, Chinese participants revealed strong positional concerns for vacation time and only weak positional concerns with regard to personal attractiveness and intelligence. With respect to income, the authors do not find a significant difference between the participants from the two countries. Furthermore,

Carlsson and Qin (2010) focus on relative income preferences of Chinese farmers and conclude that the rural population in China is similarly concerned with relative income. Finally, Hillesheim and Mechtel (2010) consider private and public goods in their survey and provide new explanations for a good's degree of positionality, namely whether a good is associated with non-psychological negative externalities and, in the case of a public good, whether it is characterized by positive spillover effects. Controlling for these two explanations, they find that there is no statistically significant difference regarding the degree of positionality between private and public goods.

3 Survey

3.1 Design and sample

The methodology used in the present paper is closely related to that used by Solnick and Hemenway (1998, 2005), Solnick et al. (2007), and Hillesheim and Mechtel (2010). Within these papers, participants were confronted with two possible states of the world, as can be illustrated by the following example:

> Below, there are two states of the world. You are asked to pick which of the two you would prefer to live in. If you do not have a preference, choose "I have no preference".
>
> (a) Your living space consists of 18 square meters; other people's living space consists of 15 square meters.
>
> (b) Your living space consists of 23 square meters; other people's living space consists of 28 square meters.
>
> (c) I have no preference.

Johansson-Stenman et al. (2002), Alpizar et al. (2005), Carlsson et al. (2007), and Carlsson and Qin (2010) use a similar question format, but participants are confronted with repeated choices between the scenarios with varying consumption levels in scenario (a).

In contrast to these previous studies, the participants of our survey were divided into a control group and a treatment group. Assignment to the two groups was random. Only the members of the treatment group were provided with questions in the format of the above

example. As illustrated by the following sample question, participants assigned to the control group did not obtain any information on "others" and therefore had to choose only between "more" and "less":

> Below, there are two states of the world. You are asked to pick which of the two you would prefer to live in. If you do not have a preference, choose "I have no preference".
>
> (a) Your living space consists of 18 square meters.
>
> (b) Your living space consists of 23 square meters.
>
> (c) I have no preference.

For both the treatment group and the control group, the second scenario (b) provided a higher level of individual consumption in absolute terms (which we will henceforth refer to as *absolutely better*), while the first scenario (a) resulted in a lower level of individual consumption in absolute terms (henceforth *absolutely worse*). In addition, for treatment group members, the second answer entailed a lower social position with respect to consumption of the good under consideration (i.e., was *non-positional*), while the first answer was associated with a higher social position (i.e., *positional*) and may therefore have been preferable for whom relative consumption was particularly important. Throughout the entire treatment group questionnaire, the positional answer was always the one which was less attractive from an absolute point of view.

Having introduced a control group allows us to isolate the treatment effect which is crucial for correctly deriving an item's degree of positionality. Previous studies using surveys with two states of the world argue that participants choose the positional answers because they care about relative consumption. However, non-monotonic preferences may lead participants to choose scenario (a) even if they don't care about the reference group. Thus, we argue that a control group is necessary in order to clearly separate relative consumption concerns from non-monotonic preferences.

To illustrate our point, let us assume that participant i's utility from the consumption of a certain good is given by

$$U_i = u_i(c_i, \widetilde{c}), \tag{1}$$

with c_i as individual i's consumption level of the respective good and $\widetilde{c}$ as the reference consumption level of this good. Relative consumption preferences imply that $\frac{\partial u_i}{\partial \widetilde{c}} < 0$. A

closer look at the two scenarios in the sample treatment group question clarifies that an increase in participant i's individual consumption level – namely going from scenario (a) to (b) – is associated with an even higher increase in reference consumption, $\frac{\mathrm{d}\widetilde{c}}{\mathrm{d}c_i} > 1$. For control group members, however, others' consumption is exogenous, $\frac{\mathrm{d}\widetilde{c}}{\mathrm{d}c_i} = 0$. Which of the two scenarios participant i will choose depends on the sign of the following expression:

$$\frac{\mathrm{d}U_i}{\mathrm{d}c_i} = \frac{\partial u_i}{\partial c_i} + \frac{\partial u_i}{\partial \widetilde{c}} \frac{\mathrm{d}\widetilde{c}}{\mathrm{d}c_i}. \tag{2}$$

A participant will choose the scenario with absolutely more consumption if this expression is positive, and the scenario with absolutely less consumption otherwise. If utility is monotonically increasing in individual consumption ($\frac{\partial u_i}{\partial c_i} \geq 0$), expression (2) can turn negative only if relative consumption preferences are sufficiently strong, namely, if the positive effect from a greater absolute level of consumption is overcompensated by $\widetilde{c}$ increasing above c_i. This is more likely to occur, the greater the value of $|\frac{\partial u_i}{\partial \widetilde{c}}|$ is.[2]

Since the second summand of expression (2) equals zero for control group members, they have no incentive to choose the scenario with absolutely less, if preferences regarding the good are monotone. But in the event of non-monotonic preferences, the first term of expression (2) may turn negative ($\frac{\partial u_i}{\partial c_i} < 0$). If this is the case, both control group members and treatment group members will choose the scenario with absolutely less consumption. Proposition 1 summarizes our reasoning:

Proposition 1 *(i) Relative consumption preferences can induce treatment group members to choose the scenario with absolutely less consumption of the good. This is more likely to occur, the greater the value of* $|\frac{\partial u_i}{\partial \widetilde{c}}|$. *(ii) Non-monotonic preferences can induce all participants to choose the scenario with absolutely less consumption of the good.*

Thus, if a treatment group member chooses the absolutely-worse scenario, we are not able to reconstruct whether she does so due to positional concerns or non-monotonic preferences. However, comparing the share of participants who chose absolutely less consumption of the good in the control group and in the treatment group respectively enables us to isolate the relative consumption effect. If the share of absolutely-worse answers in the treatment group

[2] If c_i is a bad rather than a good and preferences are monotone, $\frac{\partial u_i}{\partial c_i} \leq 0$ holds. Furthermore, positional concerns regarding this bad imply $\frac{\partial u_i}{\partial \widetilde{c}} > 0$. Thus, the non-positional scenario is chosen for $\frac{\mathrm{d}U_i}{\mathrm{d}c_i} < 0$, whereas the positional scenario is chosen for $\frac{\mathrm{d}U_i}{\mathrm{d}c_i} > 0$.

is significantly greater than the share of absolutely-worse answers in the control group, we interpret this difference as being due to relative consumption concerns. Our variable of central interest thus is the difference in scenario (a) choices, induced by the introduction of the reference group and referred to as *treatment effect* in the subsequent paragraphs. Accordingly, we argue that no conclusions can be drawn from the mere existence of a share of absolutely-worse answers greater than zero in the treatment group without knowing the share of absolutely-worse answers in the control group.

Our survey was conducted as a classroom survey in June 2010 with 200 participants, comprising 146 students in the treatment group and 54 students in the control group. All participants were second- and third-year students from the University of Tübingen with international economics, international business, or economics and business administration as their major subject. Table 1 illustrates the socio-economic characteristics (age, semester of study, gender, grade in *Abitur*) of the treatment and control groups and displays some very small differences between the two groups.[3] Two-sample t-tests show that the differences in the arithmetic means of participants' ages and the number of semesters they had studied are statistically significant. Nonetheless, given the small magnitude of the differences between the treatment group and the control group (for example, the average number of semesters studied for is 4.38 in the treatment group and 4.08 in the control group), we feel certain that they do not bias our results.[4]

	Treatment group	**Control group**	**p-value**
Age (arithm. mean)	22.07	21.44	0.0096***
Semester of study (arithm. mean)	4.38	4.08	0.0582*
Female (proportion)	0.64	0.65	0.8839
Grade in *Abitur*	1.62	1.55	0.2824

Table 1: Descriptive statistics: socio-economic variables. p-values of two-sample t-tests (Age, Semester of study, Grade in *Abitur*) and two-sample test of proportion (Female).

In total, the survey contained 16 questions featuring different goods and bads. On the one hand, we included several goods that had been investigated previously in order to enable

[3] *Abitur* is the German higher-level school leaving qualification which is necessary in order to enter university.

[4] From a theoretical point of view, a participant's age or the number of semesters a participant has studied for do not seem to have a definite effect on her tendency to systematically prefer one particular scenario. Additionally, the regression results in Section 3.2 indicate that neither age nor the number of semesters studied for have a statistically significant effect on participants' answers.

comparability with former studies. Those goods are, for example, personal income, air quality, living space, and government spending on national defense.[5] On the other hand, we included some other goods that may be particularly relevant for students, such as spending on education, the ratio of professors to students, or the level of college tuition fees.

In order to account for the tendency to choose the first answer simply because it is presented first, each questionnaire contained eight questions with the absolutely-better (non-positional) scenario as the first answer, and eight questions with the absolutely-worse (positional) scenario as first answer. Furthermore, two versions of each questionnaire were handed out, with the second version featuring all answers arranged in reverse order.

Participants' random assignment to the treatment or control group allowed choosing a between-subjects design. The descriptive statistics show that both groups are very similar with respect to participants' socio-economics characteristics. Nevertheless, there might also be reasons to prefer a within-subjects design. In particular, a within-subjects design allows a direct control for individual-specific effects. However, this type of design also has a number of shortcomings, especially regarding "order effects". If the treatment comes first, participants will know the background of the choice experiment in the control treatment at the second step, which might lead to a number of difficulties. For example, they might implicitly look at a reference group when going through the control treatment – simply because there was a reference group in the first treatment. In addition, there might occur problems of social desirability. A participant who chose the positional scenario in the treatment might strive against choosing the absolute scenario in the control setting in order to veil her true motives for the choice in the treatment. If participants go through the control treatment first, this will most likely also affect their choices in the treatment at the second step. Given that we cannot account for these effects, we feel confident that a between-subjects design with participants' random assignment to the control group and treatment group is appropriate here.

3.2 Survey results

Our first aim is to check whether there is a general treatment effect. Therefore, we will begin by taking a look at the share of absolutely-worse (positional) answers per participant.

[5]For those goods already investigated in former studies, the original questions were translated into German, then retranslated into English again by a second translator, and, finally, compared with the original questions as a test. This procedure ensures that the translation is as close to the original as possible.

Table 2 illustrates the results of three different OLS regressions with the share of absolutely-worse (positional) answers per participant as the dependent variable. We include the dummy variable *treatment group*, which takes the value of one whenever the individual was a member of the treatment group, and zero otherwise. Additionally, we follow Alpizar et al. (2005) and use personal characteristics (gender, age, the number of semesters studied for, and grade achieved at *Abitur*) as control variables.

	(1)	(2)	(3)
Treatment group	.099***	.0987***	.0939***
	(5.04)	(5.02)	(4.43)
Female		-.0308	-.0445**
		(-1.52)	(-2.09)
Age			.0098
			(1.29)
Semester of study			-.0084
			(-0.72)
Grade in *Abitur*			-.0007
			(-0.02)
Constant	.1574***	.1774***	.0171
	(10.22)	(8.21)	(0.13)
Number of observations	200	200	186
R^2	0.09	0.10	0.12

Notes: t-statistics in brackets; * significant at 10%; ** sign. at 5%; *** sign. at 1%

Table 2: OLS regression results. Dependent variable: share of positional (absolutely-worse) answers per participant. Robust standard errors.

Column (1) of Table 2 shows the results of a bivariate regression. The coefficient of the dummy variable for the treatment group is statistically significant at the 1% level and has a large numerical impact: the share of positional (absolutely-worse) answers is 9.9 percentage points higher for participants in the treatment group than for participants in the control group. Providing participants with the additional information of the existence of a reference group thus drives their decisions considerably. Adding further control variables in columns (2) and (3) hardly changes the numerical impact of the treatment group dummy variable at all. When considering the whole set of control variables, we find a gender effect that is statistically significant at the 5% level, indicating that women gave a smaller number of absolutely-worse (positional) answers. The coefficient tells us that the share of such answers for women is about 4.45 percentage points smaller than that for men. We do not find any significant effects with respect to the other personal characteristics control variables. Based

on these regression results, we can conclude that there is a statistically significant overall treatment effect.

	Percentage of responses			
	Treatment group		**Control group**	
	positional (abs. worse)	non-positional (abs. better)	abs. worse	abs. better
	(1)	(2)	(3)	(4)
Grade in *Abitur*	56	38	2	76
Parents' income	28	58	6	65
Income during internship	21	75	2	93
Education spending	30	63	11	83
Days studying for a test	46	33	30	59
Number of students per professor	33	53	17	69
Number of bicycle thefts	10	81	2	91
Air quality	8	90	0	94
National defense spending	72	17	67	11
Number of book purchases (univ. library)	13	73	9	69
Basic health research spending	14	80	11	80
National poverty rate	6	88	4	87
Outfit for job interviews	37	21	39	13
Level of college tuition fees	14	76	17	74
Living space	14	77	17	63
Hours of leisure spent in park	8	79	20	65
Average	**26**	**63**	**16**	**68**

Notes: Results of (1) & (2) and (3) & (4) do not total 100 because some respondents chose "I have no preference".

Table 3: Treatment group vs. control group: responses by type of good/bad.

As the next step, we take a detailed look at the different goods included in our survey. Table 3 illustrates the share of absolutely-better (non-positional) and absolutely-worse (positional) answers for each good, subdivided into the treatment and control groups. While, on average, the treatment group has a share of positional answers of 26 percent, the share of absolutely-worse answers in the control group is only 16 percent. This difference is statistically significant according to the regression results described above. Column (1) of Table 3 depicts considerable differences regarding the share of positional answers for different goods in the treatment group. While more than 50 percent of the answers are positional in the case of national defense spending and the grade attained at *Abitur*, the share is lower than 10 percent in the case of the national poverty rate, the number of days with unhealthy air quality, and the amount of leisure time spent in the park. These results for the treatment

group are in line with previous findings regarding the share of positional answers, which have already been discussed in detail by Solnick and Hemenway (1998, 2005), Solnick et al. (2007), and Hillesheim and Mechtel (2010). Taking a look at column (4) of Table 3 suggests that the assumption of strictly monotone preferences does not seem appropriate, as the share of absolutely-better answers is lower than 100 percent for every single good. Nevertheless, it is at least higher than 80 percent for five goods and bads, namely, government spending on education, national poverty rate, the number of times one's bicycle is stolen per year, income during an internship, and the number of days with unhealthy air quality. For two goods it even is below 20 percent: national defense spending and the outfit for a job interview.

	Treatment effect	
	absolute	relative
	(1) - (3)	(i)/(1)
	(i)	(ii)
Grade in *Abitur*	+54***	0.96
Parents' income	+23***	0.82
Income during internship	+19***	0.90
Education spending	+19***	0.63
Days studying for a test	+16**	0.35
Number of students per professor	+16**	0.48
Number of bicycle thefts	+8**	0.80
Air quality	+8**	1.00
National defense spending	+5	0.07
Number of book purchases (univ. library)	+4	0.31
Basic health research spending	+3	0.21
National poverty rate	+2	0.33
Outfit for job interviews	-2	-0.05
Level of college tuition fees	-2	-0.14
Living space	-3	-0.21
Hours of leisure spent in park	-12***	-1.50

Notes: absolute treatment effect in percentage points. * significant at 10%; ** sign. at 5%; *** sign. at 1% according to two-sample tests of proportion.

Table 4: Treatment effects: responses by type of good/bad.

In order to simplify the comparison between the treatment group and control group, column (i) in Table 4 shows the differences between Table 3's column (1) and column (3) for every single good and states whether these differences are statistically significant. Statistical significance is tested using a two-sample test of proportion for each good. Goods are sorted by the scale of the differences between column (1) and column (3). The most substantial and highly significant treatment effects can be found for the *Abitur* grade, parents' income,

internship income, and government spending on education. Less pronounced and less significant treatment effects are revealed for the number of days spent studying for a test, the number of students per professor, the number of times one's bicycle is stolen per year, and air quality. For the amount of leisure time spent in the park we even find a negative and significant treatment effect. For all the other goods included in the survey, we do not find a significant treatment effect.

For parents' income and income during an internship, the share of positional answers does not seem high at a first glance. But as the answers from the control group suggest that preferences for income are monotone, very pronounced and highly significant treatment effects occur: the introduction of a reference group has a significant impact on participants' choices. Furthermore, turning back to the two goods with the greatest share of positional answers in the treatment group – national defense spending and the *Abitur* grade – it becomes clear that only in the case of the *Abitur* grade can the high share of positional answers in the treatment group be interpreted as resulting from relative consumption preferences. For national defense, the high share of positional answers in the treatment group can be explained by non-monotonic preferences.

Finally, we compare the results derived using the methodology of the present paper with results obtained using the technique common in previous studies. Figure 1 depicts the 16 goods under consideration ranked, first, according to their share of positional answers in the treatment group on the vertical axis and, second, according to the treatment effect on the horizontal axis. Those goods marked with blue display a significant treatment effect. Allocations of the goods on the 45° line in Figure 1 in combination with troughout significant treatment effects would obviously render our separation of participants into a treatment group and a control group superfluous. However, we find that the two methods yield very different rankings. To be more precise, not a single good is located on the 45° line, which means that the two rankings do not coincide for one single good. For those goods above the 45° line, the conventional method yields an overestimation of their degree of positionality in comparison to the method presented in this paper. In contrast, for the goods below the 45° line, the conventional method implies an underestimation of the degree of positionality compared to the new method.

Figure 1 reveals that interpreting the share of positional answers in the treatment group as goods' "positionality" would lead to an overassessment of the positionality of, for example, national defense spending and the outfit for a job interview. In contrast, relative consumption

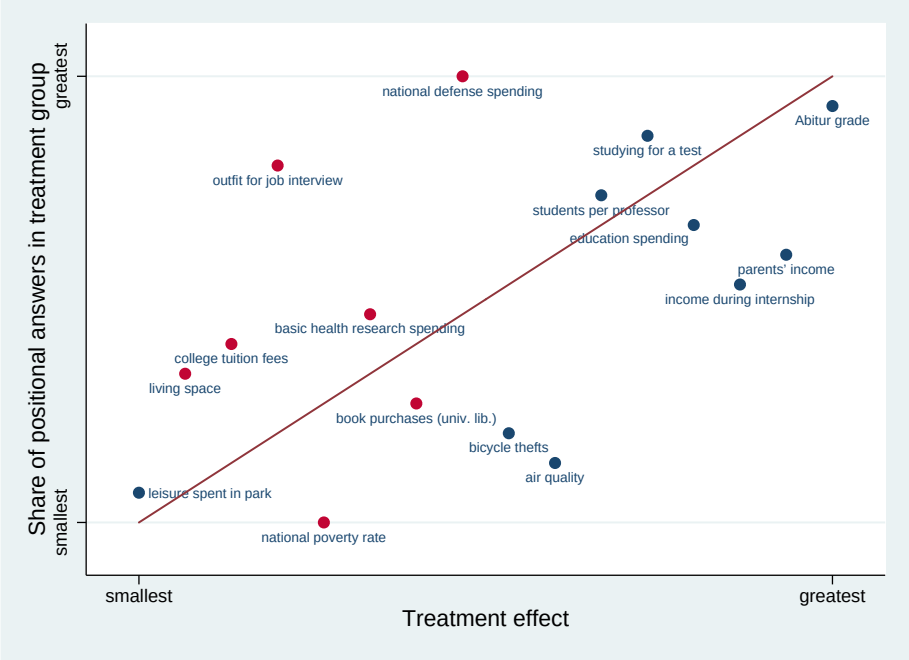

Figure 1: Goods ranked according to their treatment effect (horizontal axis) vs. goods ranked according to their share of positional answers in the treatment group (vertical axis).

concerns would be underestimated for, among others, income during an internship, parents' income, air quality, and the number of bicycle thefts. There is of course, by definition of these two measures, a positive correlation between both rankings, as a large treatment effect technically cannot occur for very small shares of positional answers in the treatment group. However, the large deviations between both rankings depicted in Figure 1 show that both measures lead to different rankings of the 16 goods with respect to their positionality. Given our reasoning above, we feel justified in claiming that differentiating between a treatment group and a control group is a necessary precondition for drawing conclusions regarding the markedness of relative consumption preferences.

A further test for the validity of the hitherto common method is to look at the "relative treatment effect". The relative treatment effect is calculated as the ratio of the treatment effect (column (i), Table 4) and the share of positional answers in the treatment group (column (1), Table 3), and illustrated in column (ii), Table 4. This measure can be interpreted as the proportion of positional answers in the treatment group that can effectively be explained by relative consumption concerns. Values of one would certainly obviate the need for our newly proposed methodology. However, in our survey, a value of one or slightly below exists

only for air quality and the *Abitur* grade, which means that participants chose the positional (absolutely-worse) scenarios not only due to relative consumption concerns, but also due to other reasons. These findings make a clear case for looking at the treatment effect rather than simply the share of positional answers, when approximating the degree of positionality in future studies.

4 Conclusion

The present paper builds on previous studies that investigate the magnitude of relative consumption effects for different goods. These studies all share a survey technique in which participants are asked to choose between non-positional and positional scenarios. In the non-positional scenario, individuals consume less than the average amount of a certain good. In the positional scenario, they consume an amount of this good which is greater than the average consumption level but smaller than the amount consumed in the non-positional scenario.

In contrast to all previous studies, we argue that choosing the positional scenario does not necessarily have to be induced by relative consumption concerns, but may be due to non-monotonic preferences, implying that one may prefer less over more with respect to certain goods. A participant who prefers less over more will choose the scenario with less, even if information about average consumption levels is not available. Thus, in order to separate relative consumption concerns from non-monotonic preferences, we divided the group of our survey participants into a control group and a treatment group. While participants in the treatment group were provided with a positional and a non-positional scenario to choose from, members of the control group did not receive any information about reference consumption levels and were faced only with two different possible levels of their own consumption of a certain good.

Our results indicate a highly significant treatment effect with a large numerical impact: the share of positional answers is 9.9 percentage points higher in the treatment group than in the control group. This result shows that providing participants with the additional information of the existence of a reference group leads to considerable changes in their choices. However, examining the goods included separately reveals strong differences between them. Substantial and highly significant treatment effects can be found, for example, for parents' income and internship income, although the share of positional answers for these two goods

in the treatment group is not very high. For national defense spending, our results do not indicate a significant treatment effect, although it has the highest share of positional answers in the treatment group. These two examples highlight the fact that determining the strength of relative consumption preferences simply on the basis of the share of positional answers in the treatment group, as commonly been done before, can be very misleading. Comparing the positionality ranking carried out in the present study with the ranking based solely on the share of positional answers illustrates that this conclusion does not hold only for these two goods, but also for the whole sample of 16 goods. We therefore conclude that having a control group in this type of relative consumption survey is necessary to control for non-monotonic preferences which might dramatically bias the results.

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Appendix

5 Survey questions

Below, there are two states of the world. You are asked to pick which of the two you would prefer to live in. If you do not have a preference, choose "I have no preference".

1. *a*) In your subject area there is one professor per 25 students; in this subject area at other universities there is one professor per 30 students.

b) In your subject area there is one professor per 20 students; in this subject area at other universities there is one professor per 15 students.

c) I have no preference. (Henceforth dropped in this list)

2. *a*) The payment you receive for doing an internship is €800 per month; the payment other interns receive is €1600 per month.

   b) The payment you receive for doing an internship is €400 per month; the payment other interns receive is €200 per month.

3. *a*) You have 18 square meters of living space; others have 15 square meters of living space.

   b) You have 23 square meters of living space; others have 28 square meters of living space.

4. *a*) Your government spends €100 billion on education; other countries spend €80 billion.

   b) Your government spends €120 billion on education; other countries spend €150 billion.

5. *a*) You have a €400 outfit to wear to job interviews; other people have €600 outfits.

   b) You have a €200 outfit to wear to job interviews; other people have €100 outfits.

6. *a*) Air quality in your community is unhealthy 6 days a month; in other communities 8 days a month.

   b) Air quality in your community is unhealthy 4 days a month; in other communities 2 days a month.

7. *a*) You spend 5 hours a week enjoying sunny weather in the park; others spend 3 hours a week enjoying sunny weather in the park.

   b) You spend 7 hours a week enjoying sunny weather in the park; others spend 9 hours a week enjoying sunny weather in the park.

8. On a scale of German grades from 1.0 (best) to 6.0 (worst):

   a) Your grade average for your school leaving examinations is 1.4; the grade average of others is 1.2.

b) Your grade average for your school leaving examinations is 1.7; the grade average of others is 2.0.

9. *a*) In your state, university tuition fees are €300 per semester; in other states, university tuition fees are €100 per semester.

   b) In your state, university tuition fees are €500 per semester; in other states, university tuition fees are €700 per semester.

10. Note that people are considered to be poor when they have less than half of the per-capita income and that per-capita income is the same in both worlds.

    a) In your country, 4% of the inhabitants are poor; in other countries, 2% of the inhabitants are poor.

    b) In your country, 6% of the inhabitants are poor; in other countries, 8% of the inhabitants are poor.

11. *a*) Your government spends €22 billion on national defense; other countries spend €15 billion.

    b) Your government spends €35 billion on national defense; other countries spend €50 billion.

12. *a*) The annual income of your parents is €280,000; the annual income of other students' parents is €560,000.

    b) The annual income of your parents is €140,000; the annual income of other students' parents is €70,000.

13. *a*) Your government spends €28 million on basic health research; other countries spend €20 million.

    b) Your government spends €36 million on basic health research; other countries spend €42 million.

14. *a*) You spend 20 days studying for an exam; other people spend 25 days.

    b) You spend 15 days studying for an exam; other people spend 10 days.

15. *a*) At your university, 2 books per student are purchased each semester; at other universities, 1 book per student is purchased each semester.

    b) At your university, 3 books per student are purchased each semester; at other universities, 4 books per student are purchased each semester.

16. *a*) Your bike gets stolen once this year; other people's bikes don't get stolen this year.

b) Your bike gets stolen twice this year; other people's bikes get stolen three times this year.