Online Appendix for:

Maximum Effort in the Minimum-Effort Game

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March 28, 2010

Abstract

This online appendix contains supplemental material to the paper "Maximum Effort in the Minimum-Effort Game". We first report details on our procedures. We provide information on how we recruited our subjects and how the experiment was conducted, and we describe in detail how we determined the share of Danes in our groups of participants. Second, we provide further analyses of subjects' behavior in our minimum-effort experiments. These include robustness checks of our regressions, details on the development of behavior across periods, analyses of how individual subjects change their efforts in reaction to whether their choice was the minimum effort or not and how this differs with individual characteristics, and tests that consider average minimum efforts rather than last-period minimum efforts. Finally, this appendix contains the experimental instructions.

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1 Details of the Experimental Procedures

We first report details of how we recruited our subjects and how the experiment was conducted. Next, we describe in detail how we determined the share of Danes in our groups of participants.

1.1 Recruitment and conduct of the experiment

Since our main result is an (unexpected) subject-pool effect, it is incumbent upon us to explain our procedures in great detail. Specifically, we will show that our procedures do not differ from standard procedures adopted in previous research.

Recruitment and conduct of the experiment were as follows. The experiments were run in the Laboratory for Experimental Economics at the University of Copenhagen (LEE) in November and December 2006. Subjects were recruited from a database using the online recruitment system ORSEE (Greiner, 2004). The database has more than 2000 entries. Participants in the database come from all over the campus of the University of Copenhagen.

Our specific subject pool comprises 170 subjects, 62.3% of which are Danish (according to our ex-post classification explained below), and 41.8% are female. Among our subjects, 28.8% studied economics, 11.2% law and the remaining 60% where students from a broad set of fields, including biology, mathematics, political science, sociology, theology, music, history, medicine and others.

In terms of procedures, participants gathered in front of the laboratory before the experiment. Next, following the procedures at LEE, participants were checked against the

list of names registered. They then entered the laboratory. Written instruction were distributed. Then the computerized experiment began. After all decisions had been entered into the terminals, subjects answered a post-experimental questionnaire on the screen and were then paid individually in an adjacent room.

No form of communication was allowed during the experiment. Subjects were told that they must not ask questions aloud and, if any questions came up, they were answered only privately. At no stage between entering the laboratory and having been paid at the end were subjects able to communicate in any way about details of the experiment. Subjects were seated at standard laboratory cubicles separated by walls so that they could neither see each other nor others' screens.

Since the recruitment database is large, it is unlikely that friends (let alone a group of friends) were invited to the same session. At least, we have no reason to believe that it was more likely than in other experiments that friends would show up together.

In any session, at least two groups were run simultaneously. More precisely, we had two groups with nine subjects, three or four groups with six subjects, or four or six groups with four subjects in each session. Because of this procedure, subjects could not identify with whom they were put together in one group. The smaller the group size, the less likely such identification is. Thus, while subjects were able too see each other before they entered the lab (and thus were able to identify the possible presence of friends), the existence of multiple groups precludes that other group members could be identified. Similarly, they could guess the share of Danish participants in the session, without knowing precisely the share of Danes in their group.

Of course, by coincidence, a group of subjects studying the same major could arrive in one session and such a group could theoretically contain if not friends then at least loose

acquaintances. However, it is not the case that group homogeneity (in terms of the field of study) and coordination success are positively correlated.[1]

Finally, the language of the experiment was English. That is, we used a language that is a foreign language to almost all participants, including the Danes. Subjects in the database from which our participants were recruited who are not Danish are typically in programmes that are taught in English. Therefore, they should all have a sufficient proficiency in English. Danish students in turn have in general good English skills. Thus we have no reason to believe that Danes had substantially better or worse comprehension of the instructions than non-Danes.

1.2 Determination of the share of Danes

Our participants were recruited among University of Copenhagen students without controlling for nationality. In order to derive the share of Danes for each group, we employed native Danes as research assistants who classified whether or not someone was Danish according to the subject's name. This is possible as Danish names are quite typical and easy to recognize.

To be precise, we had two native Danes (student assistants at the Laboratory for Experimental Economics at the University of Copenhagen who did not participate in the

[1]There is one group with 12 out of 18 participants studying economics. In this session, all three groups with six participants converged to the Pareto-dominant equilibrium. But then we have the other extreme with 16 participants studying 15 different subjects, thus not very likely to contain large groups of friends, where also all four groups of four converge to the Pareto-dominant equilibrium. As another counter-example, in a session with a relatively high share of law students (six out of 16), who might have known each other, three out of four groups converge to the worst equilibrium. Thus neither our procedures nor our results suggest any indication that our results are to a significant degree driven by subjects coming to the lab as clusters of acquaintances. In any case, even if, say, a third of a session consisted of a group of friends, they could not know how many of their friends ended up in their own group as noted above.

experiment) classify the participants as Danish / non-Danish. They agreed on 160 out of 170 cases (94.1%). We let a third Danish assistant judge the remaining cases. Most of the cases classified as non-Danish are clear-cut. For example, 13 subjects (7.6%) had unambiguous slavic names, and several other subjects had Asian, Spanish, or French names.

Despite this high degree of agreement among the classifiers, our method may not be error free. However, as we will argue now, even if such errors exist, this is not really troublesome for our results and, moreover, a perfect method to obtain data on nationality does not seem to be available anyway.

Firstly and most importantly, while our procedure cannot completely rule out errors in the classification, any such errors can only *weaken* any effects we may find in the data analysis. Both possible types of errors, assessing a Dane as a non-Dane and vice versa, can only blur the analysis of the impact of the share of Danes per group. There is no reason whatsoever suggesting that possible classification errors could systematically bias the results as the classification was made by the student assistants based only on the names without link to the experimental data.

More specifically, our procedure might, for example, err in classifying some second- or third-generation Danes of, say, Turkish origin as non-Danes. On the one hand, if these behave no different than those with Danish grandparents, this error should again weaken any effect we find. On the other hand, if they do behave differently, this would suggest that the effect we find applies only to those who have been Danish for generations. To discriminate between these cases, we would need more detailed data (for example, on nationality of parents and grandparents). We might also err in classifying some Swedish, Norwegian or Northern German exchange students as Danish. The effect we find, however, is far too big to be driven by this small hypothetical subset.

Are there better feasible methods for obtaining nationalities then? One could simply ask for nationality in a post-experimental questionnaire. Note, however, that economists tend to distrust data that are not incentivized. Thus self-reported nationality would not have been error free either.

One procedure that would ensure the correct recording of nationality is asking participants to bring their passports. Leaving aside cases of students with more than one nationality, this procedure would indeed be error free. However, it would also be rather unusual in the context of an economics experiment and therefore could cause biases in behavior. For example, the passport procedure might trigger thoughts like "oh, today, I am here to represent my country" with the participants (a thought, we note aside, that is ultimately not exactly wrong). Thus, this is neither a first-best solution.

In any event, we failed to ask for nationality because, originally, we intended to test an entirely different hypothesis with the Copenhagen subject pool, and therefore we did not ask for nationality in the questionnaire. The sessions we ran were supposed to serve as the baseline treatments where, judging from previous studies, coordination failure should be frequent. Only when the results turned out to be contrary to that expectation did we decide to test for a country effect. At that point, we could not ask subjects for their nationality any more.

2 Further Analyses of Behavior in the Minimum-Effort Experiments

In this section, we report robustness checks for our regressions (Section 2.1) and details on the development of behavior across periods (Section 2.2). We also provide analyses of

how individual subjects change their efforts in reaction to whether their choice was the minimum effort or not and how this differs with individual characteristics (Section 2.3), and present tests that consider average minimum efforts rather than last-period minimum efforts (Section 2.4).

2.1 Robustness checks

	OLS			ordered probit		
	Coefficient	t	p	Coefficient	z	p
GroupSize	-0.631	-2.41	0.024	-0.555	-2.66	0.008
DaneShare	4.784	2.91	0.008	3.595	2.67	0.008
MaleShare	-0.243	-0.13	0.901	-0.470	-0.41	0.680
EconShare	1.746	1.01	0.322	0.873	0.71	0.480
Info	-0.107	-0.12	0.904	0.192	0.35	0.725
Constant	5.269	2.74	0.011			
# Observations	31			31		
	R^2 (adj.): 0.442 (0.331)			Pseudo R^2: 0.264		

Table 1: Coefficients for OLS and ordered probit regression of $e_{\min}$ with group size as cardinal variable. Each group counts as one observation only.

In the main paper, we made several statements about the robustness of our results for alternative econometric specifications. Here, we report the details.

First, regarding the use of dummies for group size rather than a cardinal group size variable, we have done the following analysis. If we include the cardinal variable GroupSize instead of the two dummies for sizes $n = 6$ and $n = 9$ (see Table 1), the results are qualitatively the same, that is, GroupSize is negative and significant and DaneShare is positive and significant and no other variable is significant. If we exclude the groups of size nine, (see Table 2) GroupSize is still negative but insignificant, DaneShare is significant

	OLS			ordered probit		
	Coefficient	t	p	Coefficient	z	p
GroupSize	-0.124	-0.29	0.771	-0.392	-1.32	0.186
DaneShare	5.691	3.42	0.003	4.183	2.54	0.011
MaleShare	-0.625	-0.34	0.735	-0.814	-0.69	0.491
EconShare	0.735	0.40	0.693	0.509	0.32	0.750
Info	0.159	0.20	0.846	0.331	0.58	0.565
Constant	2.740	1.08	0.293			
# Observations	27			27		
	R^2 (adj.): 0.472 (0.346)			Pseudo R^2: 0.266		

Table 2: Coefficients for OLS and ordered probit regression of $e_{\min}$ with group size as cardinal variable and excluding groups of size 9. Each group counts as one observation only.

and no other variable is significant. As for the regressions with the group size dummies, OLS and ordered probit yield qualitatively the same results.

Second, while our regressions have shown that giving information on the individual efforts has no significant effect in the aggregate, the raw data suggests that detailed information has a negative effect in groups of size four and a positive effect in groups of size six. We thus consider here the effects of information separately for group sizes four and six. Table 3 reports OLS regressions where we replace the Info dummy with separate dummies Info4 and Info6, which are the interaction effects of having detailed information provided and the respective group size dummies. (We exclude groups of size nine here because for this group size information was not varied. Including these groups does not affect significance of any variable). Indeed, the coefficients for Info4 and Info6 are both significant, with the former being negative and the latter being positive. Info4, however, is not significant anymore at the 5% level when we restrict the regression to groups of size four only.

The positive effect of information in groups of size six is not surprising and is in line

	$n = 4, 6$			$n = 4$			$n = 6$		
	Coeff.	t	p	Coeff.	t	p	Coeff.	t	p
$n = 6$	−2.404	−2.73	0.013						
DaneShare	6.632	4.97	< 0.001	7.074	5.96	< 0.001	9.834	2.35	0.047
MaleShare	0.155	0.11	0.916	−1.222	−0.86	0.410	2.902	0.86	0.414
EconShare	−2.994	−1.70	0.104	−0.578	−0.34	0.743	−8.077	−1.73	0.122
Info4	−2.960	−2.81	0.011	−1.887	−2.06	0.070			
Info6	2.526	2.81	0.011				3.293	2.53	0.035
Constant	3.854	3.32	0.003	3.313	3.31	0.009	−0.221	−0.08	0.936
# Observations	27			14			13		
R^2 (adj.)	0.688 (0.594)			0.881 (0.828)			0.515 (0.272)		

Table 3: Coefficients for OLS regressions of $e_{\min}$ with interaction effects of information dummy and group size dummies, Info4: information is given and group has size 4, Info6: information is given and group has size 6. Each group counts as one observation only.

with the literature. Why the effect of information differs for the two group sizes and in particular the apparent negative effect of information for groups of size four is difficult to explain. This might just be an artefact of the group composition with respect to the share of Danes. All groups of size four without detailed information had at least two Danes and reached a final-period minimum effort of 7. In contrast, in the groups with information, four groups had three or four Danes and all converged to a final-period minimum effort of 7, whereas the remaining four groups had at most one Dane and reached a final-period minimum effort of 1 or 2. The regression with the share of Danes as a cardinal variable appears to underestimate its effect because already groups with a share of 1/2 reach the maximum effort. Thus the failure to coordinate efficiently is then partly attributed to the information.[2]

[2]We note that one cannot conclude in reverse that the information effect is driving the result for the share of Danes. In the groups with information, the share of Danes varies from 0 to 1 and exactly the

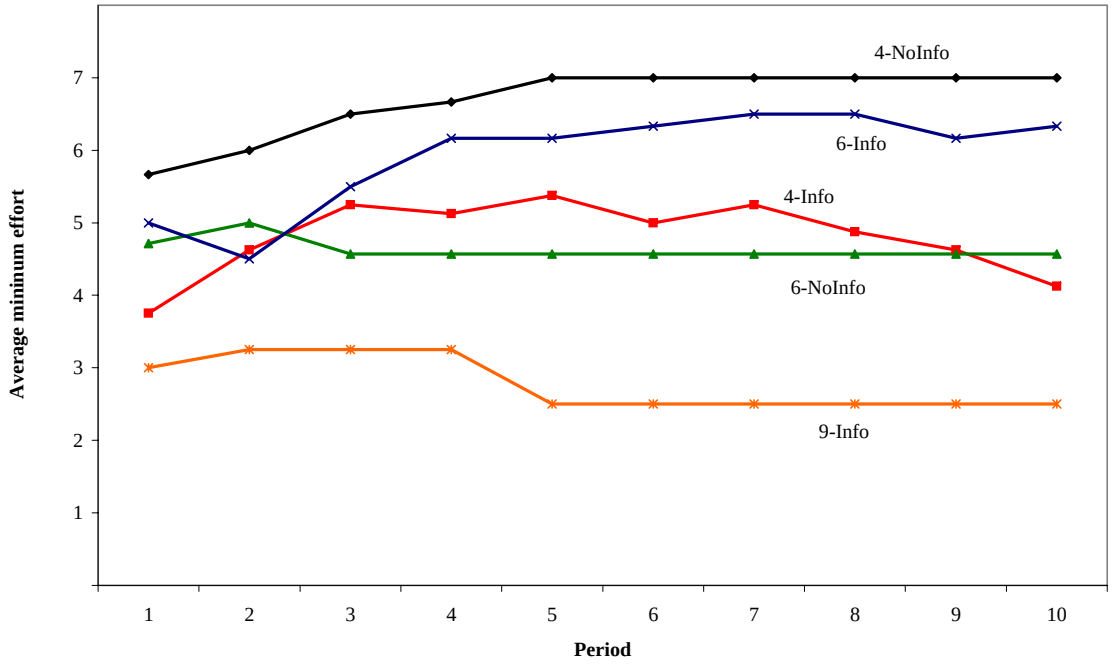
More importantly for the main point of our paper, considering the information effect separately by group size and even restricting the analysis to groups of only one size still shows a significant effect of the share of Danes (with the coefficient even being larger than for the regression with the information effect aggregated).

2.2 Development of minimum efforts across periods

Figure 1 shows the time trend of the average minimum effort by treatment. It is obvious that our results differ from those of previous experiments with similar groups sizes not only in terms of the final-period minimum efforts, but also in the time trends. Whereas Camerer (2003, p. 383) notes that "In groups of six or more, the first-period minima are never above 4, and usually deteriorate to 1 by the fifth period", minimum efforts actually increase in two of our treatments (including one with group size six) and a marked deterioration does not occur in the other treatments either.

groups with a high share of Danes coordinate efficiently.

Figure 1. Development of average minimum effort (across groups) over time



On a disaggregate level (see Figures 2 to 6), in contrast to previous results as summarized by Camerer, among our 13 groups of size six, only two groups deteriorate to 1. As opposed to that, six groups start with a minimum effort above 4 and in seven groups the minimum effort actually increases over time.

Considering all our treatments, we note that in the majority (16 out of 26) of the groups that are not constant at a minimum effort of 7, the minimum effort increases from the first period to the last. This trend in itself is, however, not completely inconsistent with Camerer's summary of the time trend, because where the group starts determines to a large degree where it ends up and there is something of a watershed at a minimum effort of 4 in Period one. With one exception (Group seven in Treatment 4-Info, which starts at 5, then moves up to 7 and then oddly collapses) all groups that start with a minimum effort larger than 4 increase the minimum effort over time unless they already start at 7. Specifically,

five groups have a constant minimum effort of 7, nine start at 5 or 6, but quickly stabilize at 7, and one group starts at 5 and is constant at 6 from Period two on. The nine groups starting at a minimum effort of 4 show a heterogenous picture: five increase their minimum effort (three converge to 7, one slowly reaches 7, but ends at 6 and one quickly converges to 6), two are constant at 4, and two converge to 1 (with large groups being less successful than small groups).

Consistent with Camerer's summary, none of the six groups that start with a minimum effort below 4 ever reach a minimum effort above 4. Two are constant at 1, two converge to 1, and in the remaining two the minimum effort fluctuates between 1 and 4. The observation that around a starting point of 4 a small difference in one or the other direction typically leads to large differences by the last period makes sense of the observation that while the Danish subjects choose only a slightly higher effort in the first period, the effect of the share of Danes is large in the last period (another part of the explanation is that Danes adjust their efforts differently between the first two periods, see below).

Figure 2. Development of Minimum Effort over Time - 4-Info

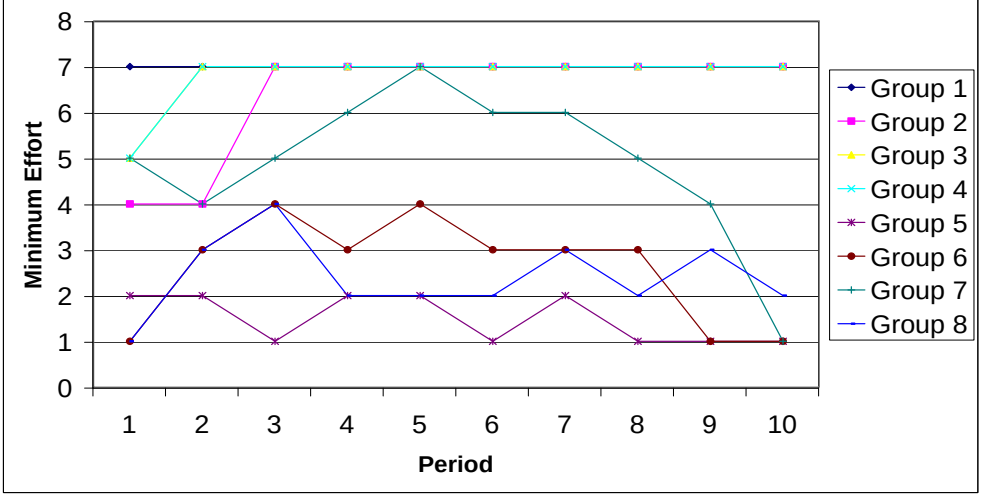
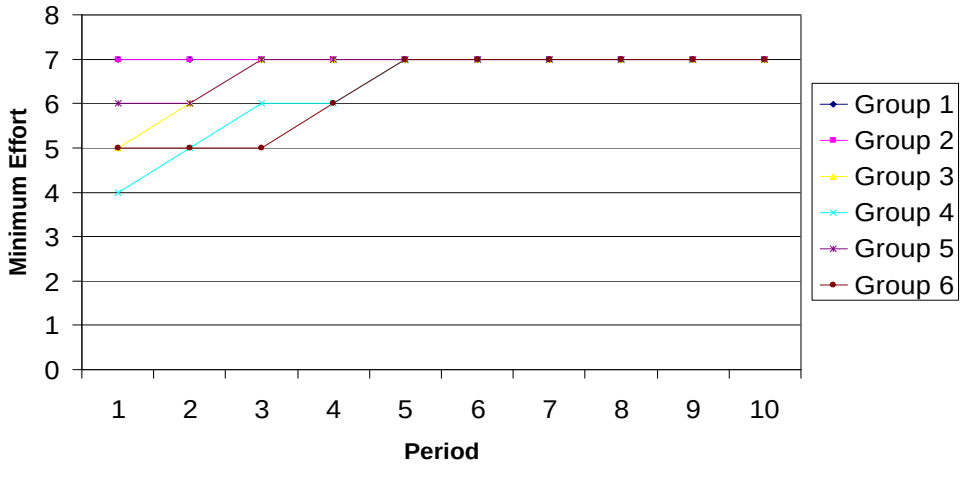


Figure 3. Development of Minimum Effort over Time - 4-NoInfo



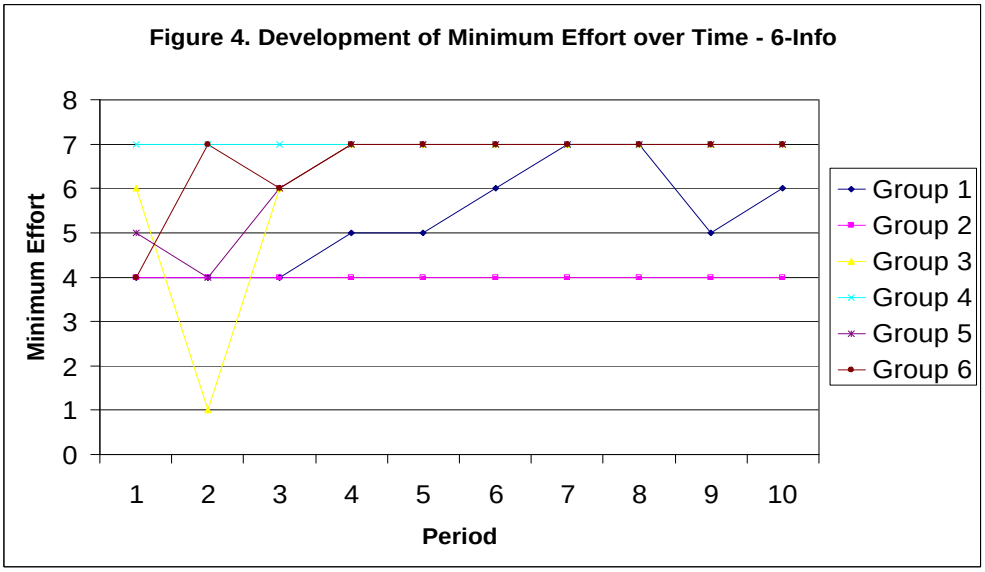


Figure 4. Development of Minimum Effort over Time - 6-Info
Minimum Effort
0
1
2
3
4
5
6
7
8
1
2
3
4
5
6
7
8
9
10
Period
Group 1
Group 2
Group 3
Group 4
Group 5
Group 6

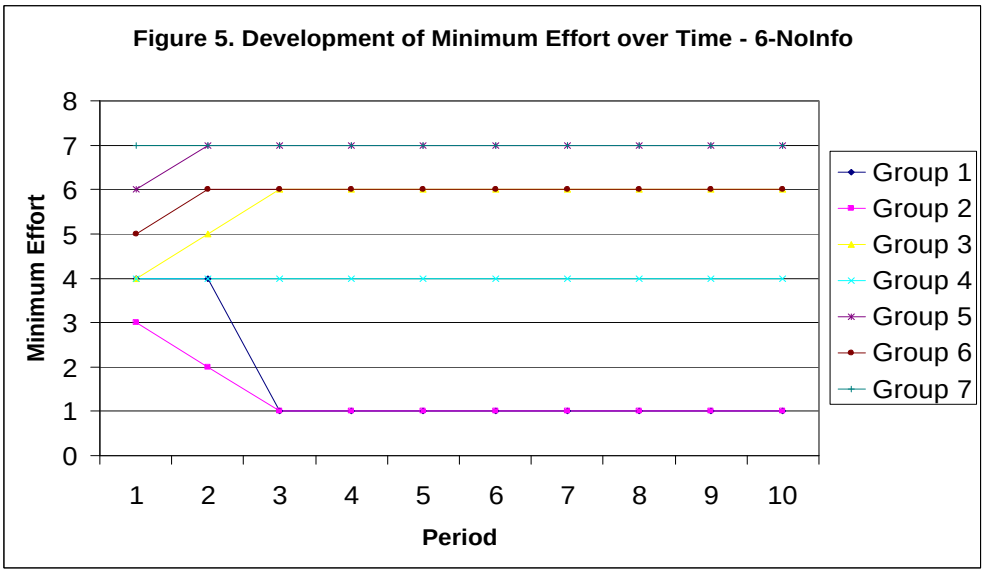
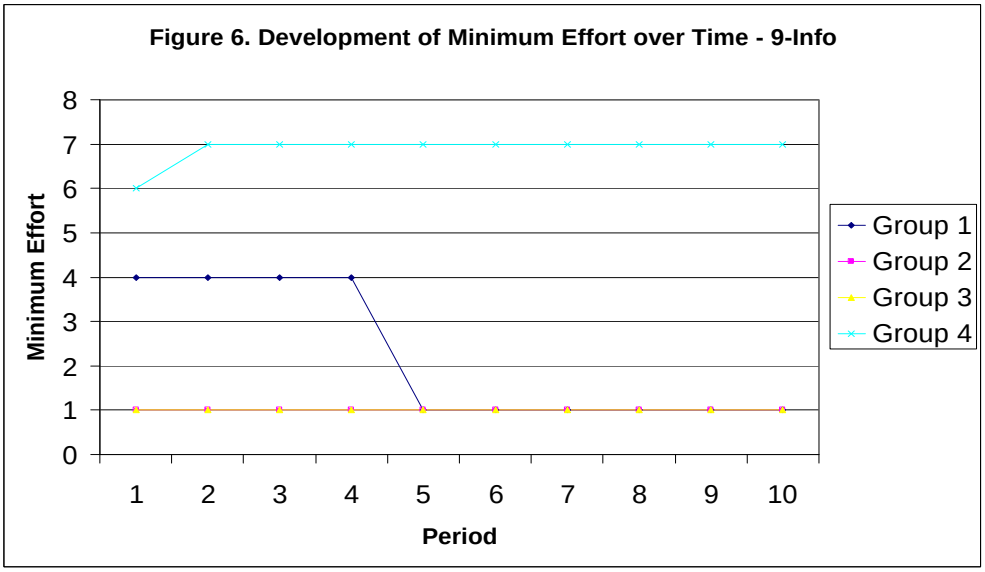


Figure 5. Development of Minimum Effort over Time - 6-NoInfo
Minimum Effort
0
1
2
3
4
5
6
7
8
1
2
3
4
5
6
7
8
9
10
Period
Group 1
Group 2
Group 3
Group 4
Group 5
Group 6
Group 7



2.3 Changes in individual effort

In this section, we consider how subjects change their effort from one period to the next. Specifically, we check whether they respond to the fact that their effort choice was the group minimum or not. As Figures 2 to 6 above show, in many groups there is little change in the minimum effort beyond the second or third period and the trend is often determined by the second period. Therefore, in a first step, we just look only at how subjects change their effort from the first to the second period.

First, we run a linear regression of the change in effort, conditioning on subjects' first period effort, the difference between their effort and the minimum effort in the first period, and dummies for being Danish, male, and an economics students (see Table 4, column (1)). The results suggest that all else equal, the effort change by Danes is significantly larger than that of non-Danes and that of economists is marginally significantly smaller than that

of students of other fields. If, instead of including the difference between own and minimum effort, we include a dummy whether the subject's own effort was the minimum effort (see Table 4, column (2)), we still obtain a highly significant positive effect of being Danish, but the economics dummy is far from significant then.

	OLS (1)			OLS (2)		
	Coefficient	t	p	Coefficient	t	p
DiffToMinEffort	-0.201	-4.13	< 0.001			
WasMinEffort				0.358	1.88	0.062
OwnPeriodOneEffort	-0.301	-4.74	< 0.001	-0.358	-5.45	< 0.001
Dane	0.603	3.25	0.001	0.598	3.11	0.002
Male	0.146	0.89	0.376	0.201	1.18	0.238
Econ	-0.384	-1.94	0.054	-0.282	-1.38	0.170
Constant	1.848	5.29	< 0.001	1.662	4.03	< 0.001
# Observations	170			170		
R^2 (adj.)	0.301 (0.280)			0.245 (0.222)		

Table 4: OLS regressions for changes in individual effort from Period one to two, conditioning on difference between own and minimum first-period effort (DiffToMinEffort, column 1) or on dummy whether own first-period effort was minimum effort (WasMinEffort, column 2), on own first-period effort, and on dummies for being Danish, male or an economics student.

Second, we run probit regressions for whether a subject would raise his or her effort conditioning on the subject's own first-period effort, a dummy whether the subject's first-period effort was the minimum effort, and dummies for being Danish, male, and an economics student (see center column in Table 5). Being Danish has a positive, but insignificant effect, but the effect of studying economics is negative and significant at 10%. Thus when controlling for whether the subject's own previous effort was the minimum effort, Danes are (insignificantly) more likely to raise their effort, whereas economists are less likely to do so.

Since Danish subjects on average start with a higher effort, the more interesting question

is whether they are less likely to lower their effort if it was not the minimum effort. Thus, we also run probit regressions for whether a subject would decrease his or her effort conditioning on the same variables as above. We find that Danes are significantly less likely to lower their effort, but we find no gender effect and no effect for economics. Hence controlling for their previous effort and whether it was the minimum effort or not, Danes are only insignificantly more likely to raise their efforts, but arguably more importantly (given that they tend to start with higher efforts), they are significantly less likely to lower their effort.[3]

	Effort Increase			Effort Decrease		
	Coefficient	z	p	Coefficient	z	p
WasMinEffort	0.547	1.65	0.099	−0.630	−1.88	0.060
OwnPeriodOneEffort	−0.686	−5.52	< 0.001	0.064	0.58	0.562
Dane	0.445	1.27	0.205	−0.784	−2.63	0.009
Male	0.275	0.79	0.431	−0.349	−1.36	0.175
Econ	−0.795	−1.65	0.099	0.291	0.88	0.378
Constant	2.395	3.50	< 0.001	−0.755	−1.10	0.272
# Observations	170			170		
Pseudo R^2	0.495			0.097		

Table 5: Probit regression for whether subjects increase or decrease their effort from Period one to two on dummy whether own first-period effort was minimum effort (WasMinEffort), on own first-period effort, and on dummies for being Danish, male or an economics student.

We also ran the same regressions as above not only for the change from Period one to Period two, but for all changes between periods (controlling for the fact that we have repeat observations from the same subject by adding subject-level random effects). Over

[3]If instead of dummies for the individual characteristics we use interaction effects with dummies for having chosen the minimum effort or not, we obtain qualitatively the same results, that is economists are less likely to increase their effort conditional on having chosen the minimum effort, whereas Danes are less likely to decrease their effort conditional on not having chosen the minimum effort.

the whole course of the experiment, controlling for the subject's effort and whether it was the minimum effort, Danes are actually significantly less likely to increase their effort. The reason is probably that many of the Danes have reached the maximum possible effort level by Period two. Danes are, however, still significantly less likely to decrease their effort (though the effect is only marginally significant if we restrict the analysis to Period three and beyond). In the linear regression for changes in effort, there is no significant effect of being Danish over the whole experiment.

2.4 Tests of average minimum effort across all periods

In the paper, we focus on the minimum effort in the last period. Here, we consider in addition the average of the minimum effort in each group across all ten periods. The results are slightly less pronounced than for the last period alone, but the main results are the same.

Specifically, in a linear regression for the average minimum effort conditioning on dummies for group sizes six and nine and the share of Danes, males and economics students in the group, we find a significant negative effect for group size nine, a significant positive effect of the share of Danes, but no other significant effect, see Table 6 (we do not report an ordered probit regression here, because the average minimum effort is not restricted to just a small number of different values). The coefficient for the share of Danes is somewhat smaller than for the last period only. Given that, as we have seen above, groups tend more towards the extreme equilibria over time, this is to be expected.

	Coefficient	t	p
$n = 6$	-0.417	-0.58	0.565
$n = 9$	-3.596	-3.25	0.003
DaneShare	4.212	3.13	0.005
MaleShare	1.073	0.71	0.483
EconShare	0.420	0.28	0.785
Info	0.187	0.27	0.788
Constant	2.306	2.02	0.055
# Observations	31		
R^2 (adj.)	0.492 (0.365)		

Table 6: Coefficients for regression of the average minimum effort across the ten periods. Each group counts as one observation only.

3 Experimental Instructions

These are the experimental instructions for treatment "4 Info". The instructions for the other treatments differed only in the number of group members or in dropping the paragraph about the feedback about individual effort choices.

Instructions

Welcome to our experiment. During this experiment, you will be able to earn a considerable amount of money. Earnings will be calculated in "Experimental Monetary Unit" (EMU). We will pay you an initial capital of 300 EMU to start with. Your final earnings will depend on your own decision and the decisions of other participants. You will be paid in cash at the end of the experiment. 10 EMU correspond to 1 DKK.

Please read the instructions carefully. If you have a question, please raise your hand and an instructor will come over and answer your question in private. Please do not communicate from now on with the other participants.

The rules

The experiment consists of 10 periods. At the beginning you will be divided into four groups of four participants. The groups will stay the same for all 10 periods of the experiment. This means that you will always interact with the same three participants.

The rules of the experiment are as follows. In each of the 10 periods, each participant has to choose a number from 1 to 7 (1, 2, 3, 4, 5, 6, 7). In each period the smallest number chosen in each group will be identified.

Your payoff is determined according to the following payoff table:

	Smallest Number in your Group						
Your Number	7	6	5	4	3	2	1
7	130	110	90	70	50	30	10
6	-	120	100	80	60	40	20
5	-	-	110	90	70	50	30
4	-	-	-	100	80	60	40
3	-	-	-	-	90	70	50
2	-	-	-	-	-	80	60
1	-	-	-	-	-	-	70

Table 7: Payoffs in EMU.

Let us explain the table: Your earnings in EMU in each period depend on your own choice (indicated by the first column of the table "your number" 7, 6, ..., 1) and the smallest number chosen in your group, including your number (indicated by the first row of the table 7, 6, 5, ..., 1). Since choices can be a number from 1 to 7, the smallest number can range from 1 to 7. The payoffs are derived in the same way for all participants, that is, the table is the same for all of them.

Your payoff in EMU is determined by the cell in the row of your decision and the column of the smallest number in your group. Examples are given below. (In the table there are cells with "-". This indicates that a combination of your choices and the smallest chosen number in your group is not possible. For example, if your decision is 4, the smallest number in your group cannot be higher, so it cannot be 7, 6, or 5.)

Examples:

- you chose 3 and the smallest number in your group was 3: your income is 90 EMU (row "3" and column "3")
- you chose 5 and the smallest number in your group was 3, your income is 70 EMU (row "5" and column "3")
- you chose 5 and the smallest number in your group was 4, your income is 90 EMU (row "5" and column "4")
- you chose 4 and the smallest number in your group was 1, your income is 40 EMU (row "4" and column "1")
- you chose 3, then the smallest chosen number in your group cannot be 4 (there is a "-" in row "3" and column "4.")
- you chose 7 and the smallest number in your group is 4, your income in EMU is (WRITE DOWN YOUR PAYOFF)

At the end of the period, you will be informed about the smallest number chosen in your group and your own income. On the next computer screen, we will also inform you about

the numbers all four group members have chosen (including your own). On this screen, each participant in your group is given a number for identification. This number will, however, be changed from period to period, so you will not be able to track the behavior of one particular other participant across periods.

Payments:

Once again, at the end of the experiment your earnings will be paid to you in cash at a rate of 10 EMU = 1 DKK. As mentioned, we will pay you an initial capital of 300 EMU to start with.

References

Camerer, C. (2003). Behavioral Game Theory. Experiments in Strategic Interaction. Princeton: Princeton University Press.

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