DISCRIMINATION BY GENDER AND SOCIAL DISTANCE

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Abstract: We examine experimentally how a person's generosity depends on the sex of that person, on the sex of the person who is the target of the generous act, and on the degree of anonymity between the interacting parties. In our data fewer men than women give non-zero amounts; men receive less than women; and less is given when subjects receive money publicly on stage than when payments are private. The results shed light on gender-related selfishness and discrimination, and suggest that it may be problematic to organize experimental findings in terms of social distance.

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1. Introduction

How does the generosity of a person depend on that person's sex, and on the sex of the person who is the target of the generous act? How do the answers to this question depend on the degree of anonymity between the parties? We provide answers based on an experiment.

Our approach relies on *the dictator game*, a popular tool in experimental research introduced by Forsythe, Horowitz, Savin & Sefton (1994), and possibly the simplest vehicle for investigating generosity: One person (*the dictator*) divides a sum of money between her-/himself and another person (*the recipient*), and payments are made accordingly. In our design, the dictator is informed about the sex of the recipient via the wording of the instructions, and we observe the sex of the dictator. We furthermore manipulate the degree of anonymity between the subjects, by varying the circumstances under which payments are made. In the treatment with *private payment* (*PP*), the instructions explained that the subjects could pick up their payments without directly identifying themselves to other participants in the experiment. In the alternative treatment with *on stage payment* (*OS*), the instructions explained that dictators would receive their payments in a lecture hall with a few hundred co-students present (clapping and cheering, as it turned out and could reasonably have been expected).

Hence, in total we have 2×2×2=8 categories of observations that can be classified in terms of three treatment variables: the sex of the dictator, the sex of the recipient, and the payment condition. The motivation for the first two treatment variables is obvious in a study of discrimination, while the last one is included to shed some light on the possible importance of the social setting for understanding discriminatory behavior. We shall investigate the importance of each of these three

treatment variables systematically, and compare our findings with the following three important sets of results that have previously been documented.

Discrimination

Fershtman & Gneezy (2001) and Holm (2000) pioneered the approach of examining discrimination in the lab by informing subjects of certain co-player characteristics. Our design is most closely related to Fershtman & Gneezy's. They study discrimination by ethnicity and gender in several games, within a subject pool of Israeli Jewish students. They report evidence on male discrimination by ethnicity. The one game in which discrimination does *not* occur is the dictator game (a finding which prompts the conclusion that discrimination is determined by ethnic stereotypes, rather than by "taste" as in Becker (1957)), and a recent study by Ben-Ner, Kong & Putterman (2001) actually finds that females discriminate (against other females) in this game. We shall examine whether also in our setting (with Swedish students, and without differential information about ethnicity) discrimination does or does not occur in a dictator game. If we do find evidence of discrimination, we wish to know whether it is a predominantly male or female phenomenon.

Are women less selfish than men?

A great many researchers in different fields have grappled with this issue, but there is no general consensus. Eckel & Grossman (1998) cite some of this evidence and note that a variety of confounding factors contaminate comparisons between the different studies. They conduct a dictator game experiment, related to the study by Bolton & Katok (1995) but intended to eliminate all factors other than gender-related differences in selfishness that might influence results. Their design is "double-blind";

the subjects were anonymous both towards one another and to the experimenter. Eckel & Grossman state (pp. 732-3):

> Our results indicate that women are less selfish than men when confounding factors are eliminated…. Having established this baseline difference in men and women, it is appropriate now for research to address the issue of how other parameters of the experimental setting influence the behaviour of men and women.

This is where we pick up, adding the aspects of discrimination and anonymity to Eckel & Grossman's gender-and-generosity perspective. We ask if Eckel & Grossman's results are robust with respect to these changes. We also make a comparison with the results of Andreoni & Vesterlund (2001) and Ben-Ner *et al* (2001), both of whom study gender-differences in dictator games.

Social distance

This term is defined by the *Encyclopedia of Psychology* (2000) as "the perceived distance between individuals or groups". The concept has a long history in social science research. Bogardus (1928) developed a scale to measure it based on statements such as "I would marry this person" and "I would have this person excluded from the country", which suggests that he had a rather multi-faceted notion in mind. When economists have picked up on the concept, they have focused on one particular aspect: anonymity. Two influential studies are Hoffman, McCabe, Shachat & Smith (1994) and Hoffman, McCabe, & Smith (1996), who investigate how the social distance, represented as the degree of anonymity between the subjects and the experimenter, affects the amount of money donated in dictator games. They find that selfishness increases with social distance, a finding which has inspired or spawned

several other studies.[1] We will connect to this issue, although all our treatments involve considerably less anonymity than any of Hoffman *et al*'s. For example, even the *least* anonymous treatments of Hoffman *et al* ensured that subjects could not learn each other's decisions, something which our *most* anonymous treatment (PP) does not ensure (due to our use of "code numbers"; cf. Section 2 below). In light of earlier result it would nevertheless be natural to expect dictators to donate more money in the OS condition than in the PP condition, since the former condition involves less anonymity than the latter condition. We shall investigate whether this is so, and thus whether the predicted relation between social distance and selfishness extends to a context where the social distance is relatively small.

The paper is structured as follows: The details of the experimental procedure are spelled out in Section 2. In section 3 we report the experimental results, focusing on the impact of each of our three treatment variables (dictator's sex, recipient's sex, PP vs OS condition). In Section 4 we discuss the three issues of discrimination, selfishness and social distance. In that section we also examine how the constitution of our subject pool (students in economics and business) may have influenced the results. Section 5 offers concluding remarks.

2. The procedure

The experiment was carried out in the spring of 2000 at Stockholm University. The subject pool consisted of 388 students, which is an unusually large number for experiments of this kind. All subjects were enrolled in the introductory

[1] See Bohnet & Frey (1999a and 1999b), Bolton, Katok & Zwick (1998), Charness & Gneezy (2000), Frolich, Oppenheimer & Kurki (2001), Frolich, Oppenheimer & Moore (2000), and Johannesson & Persson (2000).

microeconomics course, which involved ten auditorium lectures (to which all students were invited) and five seminar sections with 20-30 students in each (where attendance was mandatory). The experiment was carried out in connection with these seminar sections, and run by teaching assistants.

We asked the teaching assistants to distribute a one-page instruction sheet (see the Appendix) to each student present. Every student in a seminar group got instructions with the same wording, and hence were part of the same treatment. A minimum of three seminar sections were allocated to each treatment. Among these were sections with female as well as male teaching assistants, and with business as well as economics students. Teaching assistants were asked to give no information about the experiment other than that participation was voluntary.

The instruction sheet (see the appendix) informed the student that participation meant stating a "code number" (see below), and answering a question concerning how to divide 1000 Swedish kronor (≈$110 at the time of the experiment) between the student her/himself and another student in the course. That other student was referred to in one of two different ways:

- *as a randomly selected female student in the course,* or
- *as a randomly selected male student in the course.*

In this way dictators were informed about the sex of the recipient. Note that unlike Holm (2000), Fershtman & Gneezy (2001), and Charness & Gneezy (2000), we did not use *names* as signals. Names may convey other information than sex, such as ethnicity (Fershtman & Gneezy), or may avoid disclosing sex if only a family name is given (Charness & Gneezy). In our design nothing other than sex is signaled.

In order to participate, a student had to specify a "code number", consisting of the student's initials plus the last four digits of her/his social security number. Swedish social security numbers have ten digits, specifying year-month-day of birth plus four digits where the penultimate one is *even* for a woman and *odd* for a man. Our design makes crucial use of this last feature. It allows us to separate the data according to the dictator's sex.[2] The code numbers do not automatically reveal a subject's identity, although, with some effort, one might be able to figure out someone's identity, given her/his initials plus last four digits. This feature should reduce the anonymity between the participants, in all our treatments.

The instructions furthermore stated that one of the participants in the experiment would be selected at random, and payments would be made to that student/dictator and the recipient with whom she/he was matched, the amounts each received being determined by the dictator's decision.[3] The selection was to be made at an auditorium lecture (one of those to which all students were invited) 10 days later.

The instructions went on to describe what *dictators* (if selected) would have to do in order to get paid. One of the following two possibilities applied:

- *to pick up their payments in private, by showing up at our offices some time during the next few weeks*, or
- *to identify themselves during an auditorium lecture, and to come on stage to get paid (the amount being announced, a few hundred people watching)*.

[2] For example, the Swedish social security number of one of the authors of this paper is 640421-2034. Exercise: Try to figure out which one!

[3] We paid 1000 kronor to one pair of students for each wording of the instructions and we had between 73 and 94 participants for each wording. We did not mention this in the instructions since we did not indicate that different treatments were included in the experiment.

These alternatives define the PP (private payment) and OS (on stage) controls. Payments to *recipients* were always made anonymously, *i.e.* according to the first of these conditions.

Our design involves role-reversal, in the sense that a participant might get paid either according to his/her own decision, or as a selected recipient. However, our random payment scheme procedure was run without replacement, so subjects could only be paid for one of these positions.

Note that our design generates the eight types of observations discussed in the introduction, depending on the sex of the dictator, the sex of the recipient, and the payment condition (PP versus OS). Since the sex of the dictator is observed via the code numbers, only the sex of the recipient and the social distance condition needed to be indicated via the wording of the instructions. Hence, we had altogether four different wordings of the instructions.

3. Results

3.1 Participation rates

32 out of 388 students chose not to participate (i.e., they chose not to specify code numbers and decide how to split 1000 kronor). The remaining 356 students chose to take active part in the experiment, by specifying code numbers and deciding on how to split 1000 kronor. In three out of these 356 cases the code number did not correspond to the four-digit standard and in one case it was illegible. These four particular answers are not included at all in our data analysis, so our analysis will count 352 participating students.

Note that the non-participating students make up "observations" too; the decision not to participate is in itself an interesting one. The participation rate varied

according to treatment. TABLE 1 displays, for each of our eight cells, the number of students who actively chose to participate in the experiment, and the number of students who chose not to participate.

	PRIVATE PAYMENT (PP)		ON STAGE PAYMENT (OS)	
	TO FEMALE	TO MALE	TO FEMALE	TO MALE
FROM FEMALE	54 / 1	26 / 2	43 / 9	47 / 1
FROM MALE	38 / 3	47 / 3	51 / 11	46 / 2

TABLE 1: *Number of participants/non-participants for each type of observation*

Is the participation rate different under different treatments? TABLE 1 gives the answer. Visual inspection indicates that the participation rates split into two categories; the rate is considerably lower in the two cells with on stage payment and a female recipient. Note that it takes *both* the OS payment condition and a female recipient for participation rates to go down. The difference is confirmed by a chi-square test ($P<0.05$), which rejects the hypothesis that the participation rates are the same when either of the two cells with female recipient and on stage payment is compared with any of the other cells.[4]

3.2 Donated amounts

FIGURE 1 conveys a first impression of the overall structure of the data, indicating the frequencies with which different amounts {0-50, 51-100, ..., 951-1000}

[4] The non-participants in the OS payment and female recipient treatments came from two different seminar sections, with two different teaching assistants (one of them male, the other female). These non-participants were also about equally divided between male and female in each section. There is thus nothing obvious other than the treatment that could have caused the non-participation.

were donated. Looking at the whole sample of participating subjects, the average amount donated was 275 kronor, which is reasonably consistent with previous findings.[5] The amounts donated are concentrated around 0 kronor and 500 kronor.

[Fig. 1 about here]

We now focus on three questions:

(i) *Do men and women receive different amounts?*

(ii) *Do men and women donate different amounts?*

(iii) *Does the payment condition matter?*

In order to provide answers, we study how the amount of money donated varies with the sex of the dictator, the sex of the recipient, and whether or not the subjects had to go on stage in order to get paid.

TABLE 2 below displays means (and medians) of amounts donated for the eight cells. Visual inspection suggests some systematic patterns in the data. More money seems to be donated to women, particularly when the dictator is a man. Furthermore, and to our great surprise, dictators seem overall to donate *less* money in the OS treatment than in the PP treatment.

[5] Compare the previously cited papers, or the results surveyed by Roth (1995). In our data, equal splits are somewhat more frequent than in most other studies (Section 4.2 gives exact numbers).

	PRIVATE PAYMENT		ON STAGE PAYMENT	
	TO FEMALE	TO MALE	TO FEMALE	TO MALE
FROM FEMALE	334 (425)	323 (400)	264 (250)	226 (200)
FROM MALE	352 (499)	276 (223)	269 (139)	180 (5.5)

TABLE 2: *Means* (*medians*) *donated*

We shall analyze whether our different treatment variables induce differences in behavior if we consider all the data, splitting the whole data set into two parts according to which treatment variable is in focus. FIGURE 2 shows the difference in means for each of the three treatment variables, taken one at a time.

[Fig. 2 about here]

We now turn to formal statistical tests to determine whether or not the observed differences are significant. Since the distribution of donations is non-normal, we use non-parametric methods (Wilcoxon-Mann-Whitney).[6] We first ask if men and women receive different amounts. In that case we would expect to reject the following hypothesis:

H_1: *Female and male recipients receive the same donations.*

A two-sample Wilcoxon-Mann-Whitney rank-sum test of the null hypothesis that the two samples are from the same distribution rejects H_1 ($P = 0.01$) in a two-

[6] In the Wilcoxon-Mann-Whitney test the data is ranked and the test assumes a continuous distribution, where the probability of a tie is zero. When ties occur, as in our data, average ranks are assigned to tied

tailed test. The difference in means and medians indicates that female recipients receive larger donations than male ones.

Next, we ask if men and women differ in their behavior. In that case we would expect to reject the following hypothesis:

H_2: *Female and male dictators make the same donations.*

Although (as seen in FIGURE 2) on average women give more than men, this difference is not significant. H_2 is not rejected at conventional levels of significance ($P = 0.16$).

Finally, we ask if social distance matters, in the sense that behavior differs between the PP and OS treatments. In that case we would expect to reject the following hypothesis:

H_3: *The same donations are made in the PP and OS treatments.*

H_3 is rejected ($P = 0.001$). We conclude that larger donations are made in the PP case.[7]

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data and the test statistic is calculated accordingly. The effect of correcting for ties is small (see Siegel & Castellan (1988 pp. 134-136)).

[7] The conclusions drawn in this section are supported in that an alternative and often used approach to analyzing data, the (parametric) analysis of variance (ANOVA), gives the same results as the nonparametric tests we have used. Using ANOVA, the effects of "Female recipient" and "OS payment" are significant (P=0.03 and P=0.002 respectively); the effect of "Female dictator" is not (P=0.57).

3.3 The proportions who give nothing

It turns out that our data can be usefully analyzed from a different perspective, by focusing on the proportions of persons who give nothing to the recipient. To see this, it is instructive to first note that if we restrict attention to the 269 out of 352 data points involving non-zero donations, then the differences in behavior (as recorded in Section 3.2) become less conspicuous. TABLE 3 and FIGURE 3, which are derived just like TABLE 2 and FIGURE 2 except that zero donations are excluded, are suggestive of this. The treatment effects on mean donations are smaller when zero donations are excluded.

	PRIVATE PAYMENT		**ON STAGE PAYMENT**	
	TO FEMALE	**TO MALE**	**TO FEMALE**	**TO MALE**
FROM FEMALE	347 (455)	400 (500)	334 (400)	322 (400)
FROM MALE	418 (500)	382 (500)	415 (500)	276 (275)

TABLE 3: *Means* (*medians*) *of positive* (>0) *donations*

More precisely, when we perform analogous tests based on the new data set created by dropping the zero donations from the original set, none of the hypotheses H_1, H_2, and H_3 can be rejected.[8] Thus for the censored data set, we cannot reject the hypotheses that there are no differences between treatment groups.

[8] The significance levels for the associated Mann-Whitney-Wilcoxon tests are P=0.21 for H_1, P=0.43 for H_2, and P=0.17 for H_3. The donations in the dataset are roughly symmetric around the equal split (cf. FIGURE 1*)*. The hypothesis that these donations are normally distributed cannot be rejected, so we have a choice between non-parametric methods and the parametric analysis of variance (ANOVA). The choice does not matter; ANOVA tests confirm the non-significance result (P=0.23, P=0.22, and P=0.14 for the three variables "Female donator" "Female recipient", and "On stage payment").

[Fig. 3 about here]

Given this finding, it seems natural to categorize the data according to whether or not the dictator gave nothing to the recipient. Using this categorization we create a new variable, "the percentage who give nothing", which is now our dependent variable. TABLE 4 shows these percentages for the eight treatment groups, and FIGURE 4 presents these percentages according to treatment variable.

	PRIVATE PAYMENT		**ON STAGE PAYMENT**	
	TO FEMALE	**TO MALE**	**TO FEMALE**	**TO MALE**
FROM FEMALE	4%	19%	21%	30%
FROM MALE	16%	28%	35%	35%

TABLE 4: *Proportion of subjects who give nothing*

The tendencies observed in TABLE 4 and FIGURE 4 are reminiscent of the tendencies regarding the amounts donated for the full dataset (cf. TABLE 2, FIGURE 2). In order to test this statistically, we investigate three hypotheses which are motivated just like H_1, H_2, and H_3, except that we focus on the proportion who give nothing rather than the amount donated.

[Fig. 4 about here]

The first question is whether the frequency of zero donations differs according to the recipient's sex. In that case we would expect to reject the following hypothesis:

H_1*: *Female and male recipients receive nothing equally often.*

A chi-square test for two independent samples rejects H_1* (P=0.03) in a two-tailed test. This confirms the robustness of the pattern displayed in TABLE 4, namely, that more men than women receive donations of zero.

The second question is whether men and women differ in their behavior. If they do we should expect to reject the following hypothesis:

H_2*: *Female and male dictators give nothing equally often.*

H_2* is rejected (P=0.01). We conclude that more men than women give nothing. The finding can be compared with our earlier finding (in *Section 3.2*) of no significant gender difference in amounts donated.

The third question we consider is whether social distance matters, in the sense that behavior differs between the PP and OS treatments. In that case we expect to be able to reject the following hypothesis:

H_3*: *Nothing is given equally often in the PP and OS treatments.*

H_3* is rejected (P=0.001). This corroborates the previous conclusion that people are less inclined to give when they know that their behaviour will become public knowledge.[9]

[9] Using logistic regression we found that also taken simultaneously each of the three treatment variables have a significant effect on the decision to donate (P=0.01, P=0.05 and P=0.001 for the three variables "Female donator" "Female recipient" and "On-stage payment"). The direction of the effect is positive for "Female donator" and Female recipient" and negative for "On-stage payment". The results are thus confirmed.

4. Discussion

In this section we discuss our results from the viewpoints of discrimination (4.1), selfishness (4.2), and social distance (4.3), connecting to key findings in the studies by Fershtman & Gneezy, Eckel & Grossman, and Hoffman *et al*, and some related work. We also discuss some aspects concerning the constitution of our subject pool that have relevance for the interpretation of our results (4.4).

4.1 Is there discrimination?

In our study we find that there is discrimination by gender in that women receive higher donations than men do. In the literature on discrimination in labor markets (see e.g. Altonji & Blank, 1999) gender-based discrimination is always perceived as leading to women receiving *less* (i.e. a lower wage) than men. In relation to this empirical conclusion, the phenomenon that we observe might be termed "reverse" discrimination.

Our result agrees with Fershtman & Gneezy in that we found discrimination, but differs from theirs in that we found it in a dictator game. They observe discrimination both in a trust game and in an ultimatum game, but not in their dictator game. Fershtman & Gneezy argue that if discrimination would be found in the dictator game it would be caused by a taste for discrimination, since it is not due to any (correct or incorrect) beliefs about the recipient's attitudes. This argument suggests that the subjects in our study may have a taste for discrimination.

The experimental literature on gender differences suggests some possible explanations. First, the dictator game can be seen as a "charity game". If charitable donations reflect the perceived needs of recipients, and if it is felt that women earn

less income and own less wealth than men, then women's needs may appear greater than men's needs, and women would receive more. This explanation appears consistent with some of the results of Holm & Engseld (2001), who examine a dictator game where the dictator knew the gender and income of the recipient. They find that low-income women receive considerably higher donations than high-income men. However, this explanation appears somewhat contrived in our context; the students in the microeconomics class at Stockholm University probably all have about the same incomes, irrespective of sex.

An alternative explanation for the result relies on a combination of two different effects found by Eckel & Grossman (2001) in an ultimatum game. They label these effects *chivalry*—men are more generous to women than to men, and *solidarity*—women are more generous to women. These two effects combined could produce our result where *both* men and women donate more to women than to men.

In Fershtman & Gneezy's study discrimination, when it occurs, is a predominantly male phenomenon. In our experiment there is a tendency for each sex to discriminate, although we must pool the data for both female and male dictators to get an overall significant effect (P=0.11 for female dictators; P=0.09 for male dictators; P=0.01 for the pooled data).

Ben-Ner *et al* (2001) also investigate the effect of the dictator knowing the gender of the recipient, using almost the same high-anonymity procedure as Eckel & Grossman. They find that gender only has significance when women donate to women, and there the effect is negative. The directional hypothesis that women donate less to women finds little support in our data, where women on average donate more to women than to men (see also TABLE 2).

4.2 Are women less selfish than men?

Eckel & Grossman find that women's donations are significantly higher than men's donations. In our study, although average donations from women are higher than those from men, the difference is not statistically significant. On the other hand, when looking at the proportions of men and women who give nothing we do find that significantly more men do so.

Our design differentiates between two different gender-based generosity effects. First, there is the effect of the gender of the dictator. Second, there is the effect of the gender of the recipient. Each of these effects could give rise to a difference between how much men receive from men and how much women receive from women. Eckel & Grossman focus on the generosity of different donators (i.e. the first effect) and do not disclose any information about the sex of the recipient. To ensure subject anonymity with respect to the experimenter, while yet allowing for observation of the dictators' sex, they have separate sessions for female and male dictators.

In view of our finding that subjects are more generous to females, the question arises whether Eckel & Grossman's subjects could have made some (tacit) assumption about the sex of their recipients based on the sex of themselves and the other subjects in their session. Could it be that the women subjects thought that their recipients were female and the men subjects that their recipients were male? In that case, the recipient gender effect that we have found (i.e. that subjects are more generous to women) would have strengthened Eckel & Grossman's result (that women are more generous). This could explain why the female generosity effect is somewhat weaker in our design.

Andreoni & Vesterlund (2001) study gender and generosity in a modified dictator game, where the size of the pie is dependent on how much the dictator donates. In their experiment, the relative price of one monetary unit donated varies (between one third and three), and so does the initial size of the pie. We can compare our results with their two cases (differing in initial pie size) with a relative price of one. There they find that women on average give more than men, but the difference is significant in only one of the two cases, a result which is consistent with our somewhat weak female generosity effect.

Another interesting effect found by Andreoni & Vesterlund is that women are more likely than men to be "equalitarians", i.e. to choose a division that gives equal payoffs, while men are more likely to either give nothing or to choose the division that leads to the maximum size of the pie. We too find that men are more likely than women to give nothing. The effect of the size of the pie cannot be tested in our data, but we can check for a gender difference in the proportion of subjects who donate exactly 500 kronor. Overall in our data, 33 percent act as equalitarians in this sense. The proportion of equalitarians among the men is lower than that among the women, 30 percent compared with 37 percent, but the difference is not significant (P=0.14 in a two-tailed test).

Our results are more difficult to reconcile with recent evidence reported by Ben-Ner *et al*, whose female dictators gave *smaller* amounts to female recipients. Perhaps this difference is due to the different degrees of anonymity in the designs (low in our case, high in theirs)?

4.3 How does social distance matter?

Hoffman *et al* (1994, 1996) conjecture that social distance may influence the subjects' motivation. They report results from dictator game experiments in which the social distance is varied by varying the conditions of anonymity/privacy under which the subjects decide on the size of donations. The main finding is that selfishness increases with anonymity. Two other studies report results which point in the same direction; Bohnet & Frey (1999a) vary the extent to which recipient and dictator are identified to one another, while Charness & Gneezy (2000) investigate dictator (and ultimatum) games where subjects are told their counterpart's family names. Both find that more knowledge increases dictator generosity..

Both the PP and the OS treatments in our study involve far less anonymity than any of Hoffman *et al*'s. The OS treatment at least seems to involve less anonymity than any treatment of Bohnet & Frey and Charness & Gneezy. Yet, in the light of these authors' findings, it would be natural to expect dictators to donate more money in the OS case than in the PP case. A recent study by Rege & Telle (2001) may seem to fuel this expectation further, although they study a different game. In a public goods experiment, they find that subjects increase their contributions if their identity and contribution are revealed. Against this background, we were surprised to find that our results go in the opposite direction.

This suggests to us that the notion of social distance is multi-faceted (as early researchers like Bogardus (1928) seemed to assume). For researchers looking for the "right" model of subject preferences it would be rather convenient to simply include in the utility an additively separable and linear argument measuring social distance. Our result suggests that this route is blocked, or at least that anonymity (among

subjects or between subject and experimenter) can not be accepted as an unambiguous proxy for social distance.

Other recent results support the hypothesis that it may be problematic to conceive of anonymity as a useful way to manipulate social distance. Frolich, Oppenheimer & Moore (2000) report that as anonymity is increased in dictator games, subjects in the dictator position may start disbelieving the very *existence* of a recipient. This is a problem, since however the concept of social distance is defined there must surely be at least one other person involved. Frolich, Oppenheimer & Kurki (2001) report evidence suggesting other ways in which changes in design breed doubts in the minds of subjects about the details of the experimental context. One interpretation of all this is that as anonymity is manipulated, other considerations that may affect social distance tend to change alongside. A similar mechanism may well be at work in our design; going on stage in the OS treatment may involve a whole new array of considerations, for example whether a dictator suffers from stage fright or is an exhibitionist, on top of the reduction of anonymity.

We suspect that similar concerns may also influence the results of other experimental studies on the importance of social distance. For example, in the recent dictator game study by Johannesson & Persson (2000) the authors replicate Hoffman *et al*'s treatment with the highest social distance treatment, with the subjects being students at the Stockholm School of Economics. This treatment is compared with a control in which the recipients are randomly selected persons from the Swedish population, to whom donations are sent by mail. Johannesson & Persson suggest that since the interacting subjects are no longer students at the same university, this "removes any possible remaining reciprocity" in the design. Thus, they attempt to maximize the social distance. (As it turns out, Johannesson & Persson do not find a

significant difference in donations between their two treatments.) We find it reasonable to suspect, however, that Johannesson & Persson's imaginative design introduces a completely new aspect in the minds of the dictators. They may feel intrigued by the thought of the recipient opening a letter to find a lot, or a little, money from the Stockholm School of Economics (no letter was sent if *no* money was donated). Such a recipient is likely to be quite surprised and will possibly react quite differently from a student who has chosen to participate under the benchmark design, and this anticipation may influence the dictators' decisions.

4.4 The subject pool

How general are our results on discrimination, gender and social distance? The natural place to start such a discussion is to consider whether our results could be dependent on our choice of subjects, which was a class of introductory economics students at Stockholm University. We discuss two aspects.

The first aspect relates to the literature on the influence of studying economics on cooperativeness. The evidence is mixed. In both questionnaire data and in a prisoner's dilemma game Frank, Gilovich & Regan (1993, 1996) find that studying economics has detrimental effects on cooperativeness. Yezer, Goldfarb & Poppen (1996) expose this hypothesis to a "lost letter" experiment and find that economics students are considerably *more* likely than others to return ten dollars anonymously. This result is contradicted by Frank & Schultze (2000) who find that economics students are more corrupt than others.

A recent study by Frey & Meier (2000) offers a more refined perspective, to which our results can be linked. Our subject group of introductory economics students actually consisted of two sub-groups, business students on the one hand and students

who take economics as part of a non-business degree on the other.[10] Frey & Meier finds significant differences in behaviour between two similar groups, classified as "political economists" and "business students". They investigate historical data on actual donations of all students at the University of Zürich during three academic years. One of their results is that business students donate considerably less than political economists.

We have checked whether our results conform with Frey & Meier's finding. TABLE 5 shows the data from our experiment on how the numbers of subjects in each treatment group are divided between business students and non-business students. (These numbers add upp to the participating number in each group in TABLE 1.)

	PRIVATE PAYMENT (PP)		**ON STAGE PAYMENT (OS)**	
	TO FEMALE	**TO MALE**	**TO FEMALE**	**TO MALE**
FROM FEMALE	16 / 38	18 / 8	16 / 27	22 / 25
FROM MALE	17 / 21	28 / 19	17 / 34	17 / 29

TABLE 5: *Number of participating students* (*business/non-business*) *for each cell*

If the business/non-business variable has a significant effect on the amount donated or on the proportion that donates, we should be able to reject one or both of the following hypotheses:

H_4: *The two student categories make the same donations.*

[10] These student groups were separated into different seminar groups which makes it possible to distinguish between them in the data.

H_4*: *The two student categories give nothing equally often.*

Neither H_4 nor H_4* is rejected (P=0.46 for H_4 and P=0.68 for H_4*). However, two sub-group differences emerge: First, if we consider only donations to males, business students give significantly less than non-business students.[11] Second, looking at non-business students alone, donations under the OS treatment are significantly lower than in the PP treatment, while within the business students sub-group that difference is smaller (comparing means) and not significant.[12] These differences suggest that the magnitude of some of our results on discrimination, gender and social distance may vary between business and non-business student groups. Note however that the direction of our results remains the same.

A second aspect, which may matter with on-stage payment, has to do with the preferences of observers as perceived by decision makers. Most papers show that the presence of an observer has a positive effect on contributions. In our case, the reverse is true. One may suspect that the effect of the observer depends on what the decision maker thinks of the observer's preferences. In our case the subjects knew that the observers would be economics students in an economics lecture. In economics class, one is supposed to be selfish, right? If the students care about the opinion of their instructor and the other students, this might have made them more conscious that being selfish is okay. One may conjecture that behavior on-stage would look different in a class taught more about group behavior (*e.g.* a social psychology class) than about selfish behavior. We leave an examination of this conjecture for future research.

[11] The mean donation to men from non-business students is 296 kronor and from business students 186 kronor, with P=0.0006 in a Whitney-Mann-Wilcoxon test.

[12] The mean donation from non-business students in the OS treatment is 214 kronor and in the PP treatment 351 kronor, with P=0.0004. The mean donation from *business students* is 248 kronor in the OS treatment and 291 kronor in the PP treatment, with P=0.17.

5. Concluding remark

Most of economic theory presumes that decision makers are rational and selfish. Evidence from many sources—experiments, interviews, field studies, introspection—indicates that this picture is in serious need of revision. Economists are increasingly calling for more research aimed at revealing what sociological or psychological features are important determinants of economic decision making, and for the incorporation of these concerns into economic theory.[13] We provide some empirical input to this process. Our ex ante aim was to shed light on gender differences in selfishness and discriminatory behavior; in retrospect, the results also raise some doubts concerning the merits of organizing experimental findings in terms of social distance.

Our results on discrimination may seem somewhat surprising, as they involve more favorable treatment of women than of men. Many economic studies have suggested the opposite pattern. It is probably a good idea to be cautious in drawing far-reaching conclusions from this result. The dictator game setting we have studied is rather special and not necessarily relevant to understanding discrimination in the marketplace. Still, a word of caution may be conferred also to those who believe that women are always discriminated against. At the very least, we have provided an example where this is not the case.

We have also discussed how our results shed some light on gender related differences in selfishness, and the (complicated) effects related to manipulation of the degree of anonymity between subjects. It is interesting to relate all these findings to the recent work on "social preferences", which attempts to find out how to best

[13] See, for example, Rabin (1998) and Fehr & Falk (2001).

represent the preferences of economic agents when they are not completely selfish (see Fehr & Schmidt 2001 and Sobel 2001 for surveys). It is noteworthy that the models in this literature fall short of capturing the effects we have found. Consider, for example, the models of Bolton & Ockenfels (2000) and Fehr & Schmidt (1999), according to which decision makers are averse to unequal payoff distributions.[14] While their model allows inequity aversion to vary in strength from one decision maker to another, it makes reference neither to the sex of various players nor to the degree of anonymity between them.

Consider, furthermore, the following: We have found that when explaining the variation in how much money is donated by dictators, an important feature that is affected by all three treatment variables is how frequently nothing is given (cf. Section 3.2). The size of the positive donations, on the other hand, is not affected by the treatment variables. One interpretation of this pattern is that a dictator's decision is made in two stages: the first decision is whether or not to donate, the second decision determines the exact amount, and the influence of the treatment variables on this process goes mainly via the first stage. If this pattern is valid for our dictator game, a similar two-step mental procedure is perhaps also valid for other games. However, the models of social preferences that exist do not seem to take into account cognitive elements of this nature. Thus, there seems to be scope for interesting future research, in which we propose that experimental and theoretical work should go hand in hand.

[14] See Engelmann & Strobel (2001) for a recent experimental investigation of these and some other models.

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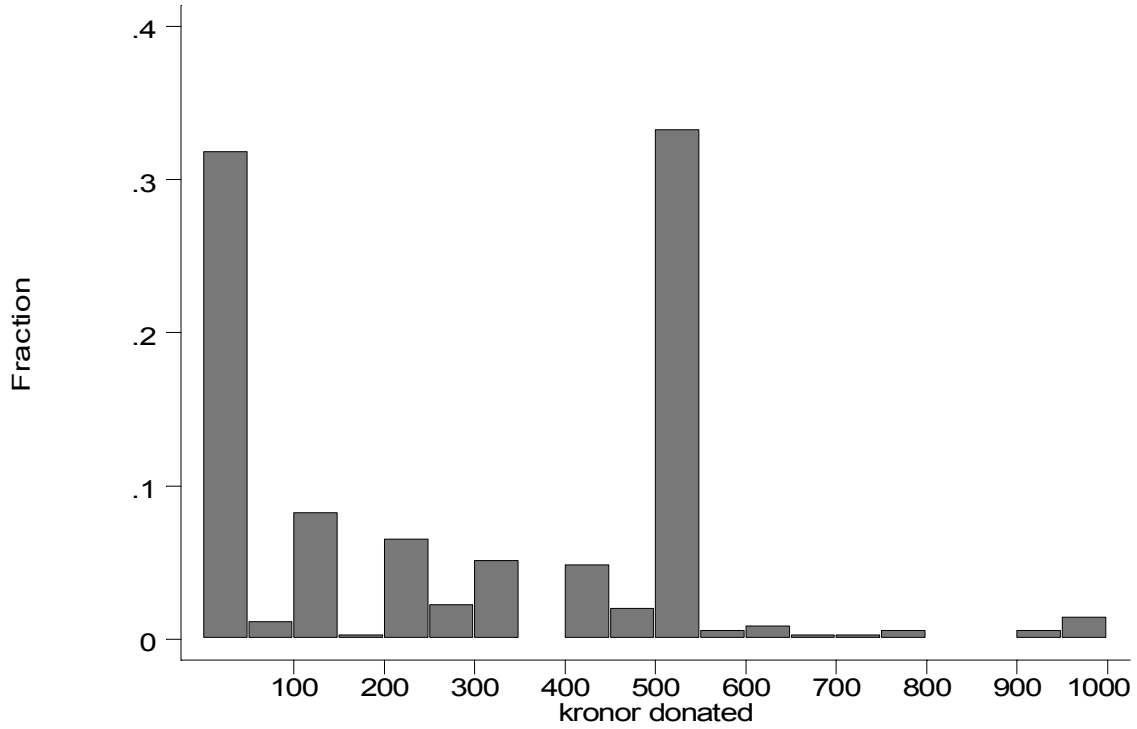

FIGURE 1: *Amounts donated for the whole sample* (*N* = 352)

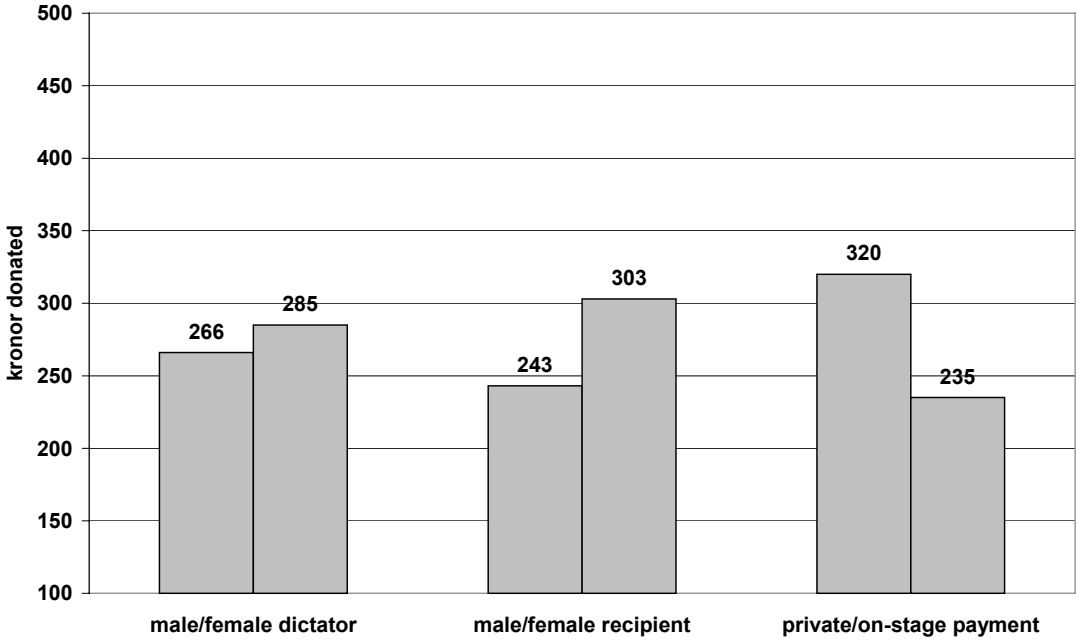

FIGURE 2: *Treatment effects on mean donations* (*overall mean* = 275 *kronor*; *N* = 352)

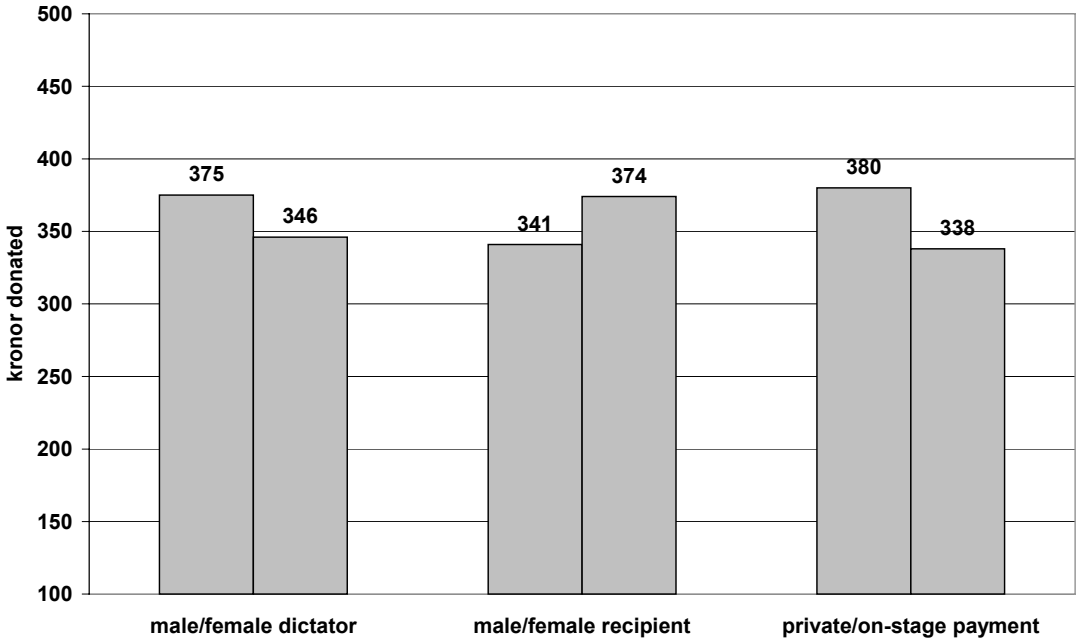

FIGURE 3: *Treatment effects on means of strictly positive donations* (*overall mean* = 360 *kronor*; *N* = 269)

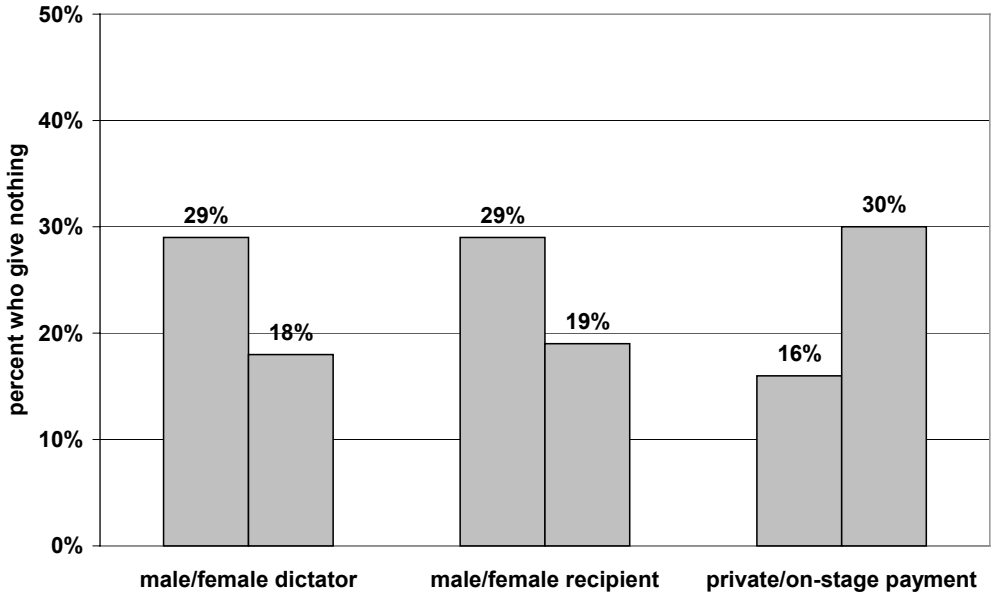

FIGURE 4: *Treatment effects on proportion of subjects who give nothing* (*overall proportion* = 24%, *N* = 352)

Appendix: Instructions (translated from Swedish)

{The text below was given to subjects in treatments with on stage payment and female recipient. Substitute "man", "his" & "he" for "woman", "her" & "she" throughout to get the wording for treatments with a male recipient. The underlined text was not given in the treatments with private payment. The instruction in these treatments read "They can then both recieve their" rather than "She can then recieve her ".}

AN EXPERIMENT

Introduction

You are invited to take part in an experiment. You will determine a division of 1000 kronor between yourself and a randomly chosen woman who is also in the introductory economics course this term. Participation in the experiment is voluntary. You participate by filling out and handing in this form here and now.

All participants in the experiment have a chance of winning money. Using a lottery and a code number (see below), we will select one participant in the experiment during a lecture in the Aula Magna on March 1, 2000 at 11 a.m. This person's code number and decision will be announced. <u>If the person is present and steps up on stage he/she will receive payment (minus tax) according to his/her decision. If the person does not come on stage the chance to win money disappears, and we will select a new person, etc.</u> When a person has been selected in this manner, we will use a lottery mechanism to select the randomly selected woman that is affected by the decision. We will announce her code number. <u>She can then recieve her</u> money (minus tax) by contacting us before 12 noon on March 30, 2000.

The task

Your task is thus to determine a division of 1000 kronor between yourself and a randomly selected woman who is also a student in the introductory economics course this term. The division must add upp to 1000 kronor for your answer to be valid. It is important that you choose your decision without discussing it with anyone else in the room. Make your decision by filling in the amounts below:

DIVISION: _____ kronor to myself,

_____ kronor to the randomly selected woman

Your anwer will soon be collected. Your code number and answer will only be announced if you are selected through the lottery as has been described above. Otherwise, you will remain completely anonymous in relation to all other participants in the experiment.

In order to pay you money we will need to know that the person receiving the money really is the one selected in the lottery. Since the code number below is connected with social security number and initials we can check your identification. Now fill in your code number:

Your code number: __ __ __ __ (last 4 digits of the soc.sec.no.) __ __ (your initials)

When you have filled in all the information above, fold this form (once) and put it before you on the desk.