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Working Paper

Stereotypes, Sex Discrimination and Paranoia - an experimental study

Working Paper, No. 2000:1

Provided in Cooperation with:
Department of Economics, School of Economics and Management, Lund University

Suggested Citation: Holm, Håkan J. (2000) : Stereotypes, Sex Discrimination and Paranoia - an experimental study, Working Paper, No. 2000:1, Lund University, School of Economics and Management, Department of Economics, Lund

This Version is available at:
https://hdl.handle.net/10419/259831

Stereotypes, Sex Discrimination and Paranoia

- an experimental study

Abstract: We analyze results from two categories of experiments where the subjects received controlled signals about the sex of their co-players. In a series of Battle of the Sexes experiments the subjects played more hawkish against women than against men. The impact of the sex signal was most pronounced among female subjects. In the second category of experiments we develop a measure of discrimination effects. We then survey discrimination effects across 32 subject groups from Israel, Sweden, UK and USA. The results indicate discrimination against females in experimental bargaining. This discrimination behavior is significant among females but not among males.

Keywords: Sex discrimination, gender differences, bargaining, experiments, coordination. (JEL-code: J7, C7, C9)

Stereotypes, Sex Discrimination and Paranoia

- an experimental study

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Biographical Note.

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1. Introduction

The aim of this paper is to study how controlled signals about a bargaining party's sex affect experimental behavior. It has been established in various field studies that discrimination occurs in certain economic transactions.[1] However, until recently there have been virtually no studies on discrimination effects in experimental economics.[2] This is somewhat surprising since the possibility of controlling variables in experiments ought to be particularly valuable in studies of discrimination behavior, where situation specific variables can be important.

In order to study discrimination behavior, subjects have to be able to base their discriminatory action on some controlled signal that reveal information about their bargaining parties. In this paper we will study how information about the opponent's sex affects the subjects' behavior in two classes of bargaining situations. The first class refer to Battle of the Sexes (henceforth, BS) situations where each bargaining party cannot act favorably or unfavorably against their bargaining opponents without knowing or guessing their opponents' choices. In Holm (1998) it was shown that signals about the bargaining opponents gender affected the subjects choices and improved the coordination behavior in some BS experiments. The most striking effect was that both males and females played more "hawkish" when they knew that their opponent was female compared to when it was a male. However, although the subjects discriminated between different gender signals in a technical sense, it can be argued that (due to the coordination aspect) this is not discrimination in the meaning that females are necessarily treated unfavorably. The second class of bargaining situations has the property that one party can unambiguously act favorably or unfavorably against their opponent without knowing the opponent's action. This is, for instance, the case

[1] See the audit studies of Neumark (1996) and Ayres and Siegelmann (1995).

[2] Recent exceptions to this are Fershtman and Gneezy (1998), Holm (1997, 1998) and Solnick (1998). The first paper establishes the presence of experimental ethnic discrimination in a trust game in Israel. The second and third papers analyze and demonstrate significant gender based coordination attempts in a series of "BS" experiments conducted both in Sweden and in USA. The last paper studies the effects of the co-players' gender in a Ultimatum game.

for both the proposer and the responder in ultimatum games. In this class of situations we demonstrate that it is less problematic to define and measure experimental discrimination effects.

In this paper we shall focus on gender differences. Do males and females respond similarly to controlled signals about their co-players' sex in bargaining situations? We will try to answer this question by studying all reported bargaining experiments that belong to either of the two classes of bargaining situations mentioned above and that apply adequate treatments of the subjects. Adequate treatment means in particular that the subjects' are given a controlled signal that reveals the co-player's sex and that the experiment involves monetary incentives.

In the first class of bargaining situations we analyze the results of Holm's (1998, 2000) studies of BS games that were run in USA and in Sweden. The experimental data from this series of experiment suggest that women on average are more sensitive to the gender of their co-player than men. If we accept that the magnitude of sex discrimination in a population can be measured as the subjects' average sensitivity to the gender signal, then this means that the women are more inclined to sex discrimination than the men. However, we detect cultural differences. Whereas the Swedish female and male sensitivity to the gender signal is about the same, the American females were much more sensitive to the gender signal compared to the males. Among the American subjects, almost all gender signal sensitivity can be attributed to the female group's tendency to behave more hawkish against females than against males.

To investigate this issue further a measure is developed to study discrimination effects for the second class of bargaining situations. The discrimination effect measure is positive when a subject group treats female co-players unfavorable (compared to males) and it is negative when females are treated favorable. From the experimental literature we found 16

male and 16 female subject groups that received treatments that were adequate and satisfied the conditions to be included in the present study.[3] The discrimination effect was positive in 25 out of 32 observations, which indicate discrimination against females in these experiments. Furthermore, discrimination against females was more common among the female subject groups than among the male subject groups; 15 out of 16 female subject groups had a positive discrimination effect whereas the same relation among the male groups was 10 out of 16.

In this paper we will also try to discuss economic theoretical explanations for the main part of the observed behavior and relate our results to the meta-analytical studies on gender differences in psychology (see e.g., Eagly, 1995). According to the psychological research, peoples' stereotypes about gender are in general supported by empirical facts. This means that there may be an economic rationale for people to hold them and use them. For instance, in the BS we claim that there is a natural correspondence between gender stereotypes and the strategy choices that the parties utilize in trying to coordinate. In order to explain that females are more sensitive to gender signals than males we argue that economic institutions (e.g., discrimination policies, anti-discrimination laws) are designed to help and protect the discriminated party which means that the expected value of being observant to gender will be higher for females.

The paper is organized as follows. In section 2 we discuss different methods of studying discrimination. Applying economic experiments to issues regarding discrimination is an unexplored field. In order to motivate this endeavor we try to assess the pros and cons of the experimental method in relation to other methods used. We then describe a series of BS experiments in section 3. In section 4 a measure to evaluate discrimination effects are presented and the discrimination effects for a class of experimental bargaining situations are

[3] These subject groups were from Israel, Sweden, UK and USA and the discrimination effects were obtained from the following studies: Fershtman and Gneezy (1998), Solnick (1998), Solnick and Schweitzer (1999), Scharleman et al.(1999), and Holm, (2000).

studied. In section 5 we try to give an economic explanation to our observations. Finally, in section 6 we discuss some implications of our results.

2 Discrimination Effects and the Experimental Method

As pointed out before, economic experimental discrimination behavior is an unexplored field. This motivates a section discussing the experimental method compared to the more commonly used methods.

Usually we mean that discrimination takes place when somebody because of sex or ethnicity is treated differently and often with negative consequences for the discriminated party. For a number of reasons it is difficult to isolate discrimination effects in field studies.[4] Besides, the more subtle problems to be mentioned below, one obvious reason is that people are reluctant to admit discriminatory actions and some may be unconscious about their own discrimination behavior if directly asked.

Evidence on discrimination is either derived indirectly by studying gender differences (such as wage gaps) from existing statistical data by regression analyses or more directly from observations in audit studies. There are several well-known problems with both methods and I shall mention some of the most important ones. In studies of the former type there is a potential omitted variable bias which means that if discrimination is defined as the residual, then if some important explanatory variable is left out the discrimination effect may be larger or smaller than it really is. Thus, the gender differences may be generated by other factors than discrimination.[5] Secondly, it may also be the case that the regression includes

[4] For a discussion see e.g., Gunderson (1989), Goldin (1990) and Heckman (1998).
[5] For studies see e.g., O'Neill and Polachek (1993), Polachek (1981) and Andrisani (1984).

variables that are treated as controls, which are affected by discrimination.[6] For instance, if females have expectations about future labor market discrimination, this may affect their human capital acquisition and hence the gender wage gap. Treating human capital as a control would then hide part of the discrimination effect.

A prerequisite for direct discrimination to take place, the discriminatory actions have to be based on a signal about category (like sex or ethnicity).[7] The audit method relies on forming audit pairs that ideally are similar in all respects deemed as important (e.g., education, attractiveness, experience) except that they differ by category. Heckman (1998) has pointed out a complex of problems with this method. For instance, although the audit pairs are matched to control for the most obvious factors, there are a number of unobserved variables that may systematically affect those making choices (e.g., employers) in a way that appears to be discriminatory when it is not (or the results may not indicate discrimination when discrimination actually takes place). There are also conflicting views about how much information the auditors should be given. For instance, if the auditors are informed that the study concerns discrimination this may affect how the auditors behave and what information they observe which may bias the study.[8]

We argue that the experimental method can enrich discrimination research not because it lacks flaws, but because it mitigates some problems associated with field studies and thereby makes a useful complement.

There are a number of reasons why the possibility of better controlling variables in experiments allow for studies in which direct discrimination effects can more narrowly be

[6] See for instance Goldberg (1996).
[7] Once the signal is known there are various theories to explain discrimination. This literature ranges from theories about preferences for certain groups (Becker, 1957), differences in labor supply elasticities (Madden, 1973), signaling theories (Milgrom and Oster, 1987), employer prejudices (Bergmann, 1971), differences in working life (Goldin, 1986) to theories about male and females interaction within a household (Lundberg and Pollak, 1996; and Francois, 1998).
[8] However, Yinger (1998) notes that under some circumstances when for instance the auditors are exposed to discriminatory behavior, having told the auditors the purpose of the study may help them to preserve the accuracy of their observations.

studied and isolated.[9] First, although experiments also involve unobservable variables, experimental research has generated a catalogue of variables affecting experimental behavior and their expected effects are in many cases well documented. This means that the interpretation of the results is likely to be less uncertain and if uncertainty remains new and more targeted experiments can usually be conducted. Second, the study of discrimination in experiments can focus on more general aspects of discrimination behavior that are blurred by the situation specific aspects of audit studies. If we take Heckman's (1998) remarks seriously, then in order to conduct and interpret an audit study like the one by Ayres and Siegelman (1995) that is based on experienced car dealers' behavior, it is not sufficient to know the audit methodology and economic theory. It is also necessary to have substantial knowledge about the Chicago car dealers' market. Third, like audit studies but unlike statistical regression analyses the experimental method allows for more close studies of the mechanisms involved in discrimination since the data reveals individual decisions. This means that more complex forms of discrimination can be detected and analyzed, which we hope our experimental results below will demonstrate. Finally, it has been convincingly argued by e.g., Kagel and Roth (1995) that the process of designing and observing experiments often stimulates the generation and modification of theory. The experimental study of discrimination behavior should be no exception.

To balance our presentation let us mention some problems associated with the experimental method. First, experiments concern more or less artificial situations, which means that the observed experimental behavior may deviate from natural behavior. Secondly, often there are a set of practical limitations to experiments (e.g., in terms of monetary

[9] The experimental method also has a research ethical advantage to audit studies in that those subjects involved in the study are volunteers. Although, the subjects may not know the "whole truth" about the experiment before they participate, they will at least know they participate in experimental situation and they can be informed about the "whole truth" quickly afterwards.

resources and access to relevant subject groups) that may generate a somewhat fragmented body of knowledge.

3 The Battle of the Sexes

In this section we shall first present and analyze the observations from two similar BS (henceforth BS) experiments conducted in Sweden and in USA. After that we shall present the results from two additional BS studies in Sweden make some conclusions regarding the observations for this type of games.

3.1 An experiment conducted in Sweden and in USA

In the Swedish experiment 145 undergraduate students were recruited from the introductory course in economics at the School of Economics and Management at Lund University. The American subject group consisted of 164 undergraduates from Northwestern University from the same category of students as in the Swedish study (i.e., undergraduates following the introductory course in Economics). The general design of the experiments, the information to the subjects, the questionnaire and the experimental sessions were the same in almost every detail and is presented in the Appendix.[10]

Each subject faced the problem of sharing $100 with an anonymous male or female student co-player.[11] Hence, the only pieces of information the subjects got about their co-players were their sex and that they were students. In order to get some money the subject and his co-player had to choose without communicating so that the sum of their shares

[10] Additional details about the experiment are available in Holm (1998) and can be obtained from the author.

equaled $100. If the sum was more or less both players received zero. The subjects could choose between two ways of sharing: the "hawkish" strategy that gives $60 to the subject (and $40 to the co-player), and the "dovish" strategy that gives $40 to the subject (and $60 to the co-player). Clearly, the hawkish strategy is the optimal one if the subject believes that the probability that the co-player plays the dovish strategy is sufficiently high and the dovish strategy is optimal otherwise. To avoid unnatural behavior and demand effects the experiments were designed as not to reveal that the experiment concerned discrimination effects.

We deliberately choose the BS game since we expected it to be sensitive to gender signals. By combining coordination motives with conflict of interest the BS game motivates the subject to search for possible clues to coordinate on. This also means that one should be careful when generalizing from observations in the BS game.

3.1.1 Basic Results

The subjects' choices are displayed in Table 1. There are four subgroups: female subjects playing with female co-players - FF; females playing with males - FM; males playing with males - MM; and males playing with females - MF.

[11] The experiment also contained three other questions that are presented in Appendix 1. Note also, that in the Swedish study the subjects shared SEK 500.

	Swedish subjects	American subjects
FF	66.7	47.5
FM	35.3	20.6
MM	51.9	50.0
MF	68.3	52.3

Table 1: The proportion (in percent) of the subgroups that choose the hawkish strategy. (Source: Holm, 1998).

As we can see in Table 1 groups with female co-player's (i.e., groups ending with an F) have a significantly higher play of the hawkish strategy. Holm (1998) demonstrates i) that a gender label effect exists and ii) that the effect can be analyzed as a focal point in the Swedish population.[12] In the Swedish population males and females effectively coordinate their behavior through the gender signal in a relatively symmetric way so that a high average male hawkishness against females is matched by a high female dovishness against males. The American experimental behavior also exhibit sensitivity to the gender signal, but of a different character. Contrary, to the two-sided effect in the Swedish subject group the US discrimination pattern is mainly one sided. This has consequences for the expected payoff in the game. Let p_{ij}, be the proportion of gender $i \in \{M, F\}$ that chooses the hawkish strategy when they know that their opponent belongs to gender $j \in \{M, F\}$. For instance, in the Swedish group we can read in Table 1 that $p_{FF} = 0.667$ and that $p_{MF} = 0.683$. Based on the frequencies in Table 1 the average expected payoff for a subject of gender *i* when meeting a subject of gender *j* is calculated by $\pi_{ij} = p_{ij}(1 - p_{ji})60 + (1 - p_{ij})p_{ji}40$ and given in Table 2.[13]

[12] These results are in line with Schelling's (1960) general reasoning about the importance of contextual "non-economic" salient information and Roth's and Murnighan's (1982) observation that "non-relevant" information affects experimental bargaining behavior.

[13] To make the Americans' and Swedes' expected payoffs comparable we have calculated the Swedish subjects' expected payoff "as if" they shared $100 instead of SEK500.

	Swedish subjects	American subjects
FF	22.3	24.9
FM	24.4	22.5
MM	25.0	25.0
MF	31.0	28.8

Table 2: The expected average payoff for the various subgroups.

The symmetrical “discrimination” behavior in the Swedish groups with mixed sexes (i.e., FM and MF) enhances coordination, which results in higher payoffs compared to the corresponding American groups. The relative low payoff in the American mixed groups can either be regarded as a coordination failure or as the cost of paranoid thinking. As a coordination failure the American MF group can be “blamed” for not understanding that the gender signal can be exploited given the behavior of the American FM group. The relative low payoff for the American FM group can also be described as a cost of paranoia in that the FM group plays "as if" the gender signal mattered to the males, which it did not.

3.1.2 The Impact of the Gender Signal

One indication of the impact of the gender signal is simply the absolute increase in frequency of a behavior conditioned on the signal: For instance, 68.3 percent of the Swedish males play hawkish against females, but only 51.9 percent of them play hawkish against a male co-

player. The absolute change in units of percentages is given by $|68.3 - 51.9| = 16.4$. Table 3 contains the figures of the absolute strength of the gender signal in the male and female groups.

	Swedish experiment	American experiment
Female subjects:	31.4	26.9
Males subjects:	16.4	2.3

Table 3: Absolute differences (in units of percentages) in the play against females and males.

3.1.3 Gender Differences

The question of gender effects in experiments concerning economic decision-making is open from a general point of view. The effects depend on what behavior the experiment considers and the details of the experimental design.[14] Fershtman and Gneezy (1998) report ethnic discrimination among Israeli male students, but not among female students. Our results also indicate gender differences, but of a different kind. The sensitivity to gender signals among females is clearly higher for both the Swedish and the American subject groups compared to the corresponding male gender sensitivity (see Table 3). If we test for homogeneity in the whole group of American and Swedish female subjects we can reject the null hypothesis (of homogeneity) at $p = 0.0007$. If we make the same test for the males we cannot reject the null hypothesis (since $p = 0.22$). Thus, whereas the co-player's gender is highly significant in the female group it is not significant in the male group. This relative difference is especially large

[14] For experimental studies on gender differences see: Bolton and Katok (1993) and Eckel and Grossman (1998) for dictator game play; Mason et al (1991) for duopolistic play; Brown-Kruse and Hummels (1993), Eckel and Grossman (1996) and Nowell and Tinkler (1993) for public good contributions; Fershtman and Gneezy (1998) and Croson and Buchan (1999) for trust games; Powel and Ansic (1998) and Schubert et al. (1999) for risk

among the American subjects; the American male subjects do not exhibit any notable average gender sensitivity at the same time as there is a substantial and significant gender sensitivity in the female group.[15]

3.1.4 Cultural Differences and Consistency

Cultural differences in economic experimental behavior have been reported by Roth et al. (1991) in ultimatum game behavior. In Table 1 we note that in all subject groups the average frequencies of hawkish play are higher for the Swedish subjects compared to the American subjects. If we test for homogeneity concerning the strategy choices for the two subject groups homogeneity can be rejected at $p = 0.018$.

It should also be mentioned that Roth et al (1991) detected that although cultural differences existed, the ultimatum game behavior within a cultural group was "consistent" in that cultural groups with high average offers also had low average rejection thresholds. This is observation is not directly supported by our data. One way of looking at the consistency within the different cultures is to look at the expected coordination rate within each culture, which will be given by : $\eta_{ij,ji} = p_{ij}(1-p_{ji}) + (1-p_{ij})p_{ji}$. Without any signals to coordinate on the maximal coordination rate will be 0.5, which occurs when $p_{ii} = 0.5$.[16] The coordination rate is given by $\eta_{MF,FM} \approx 0.51$ for the American mixed subject group and it is $\eta_{MF,FM} \approx 0.55$ for the Swedish group. However, if we hypothetically let the American female group meet the

attitudes. Furthermore, a number of studies in sociology, psychology and political science have demonstrated gender effects in non-economic behavior. See Eckel and Grossman (1998) for a brief review.

[15] Homogeneity between the American female groups that received different gender labels can be rejected at a statistical significant level ($p = 0.017$).

[16] Hence, values above 0.5 indicate that the subjects succeed in using signal as a coordination device.

Swedish male group the coordination rate would increase to 0.61. These observations do not support the hypothesis about cultural consistent experimental behavior.

3.2 Two Additional BS experiments

We shall now report the observations from two additional Swedish subject groups that received the BS treatment. The first subject group participated in an experiment (that is described in Holm (1998)) that was designed to test for gender effects similar to the ones described earlier. The second subject group was a control group that participated in an ethnical discrimination experiment (reported in Holm, 2000). In these experiments the payoff structure was changed and the gender signal consisted of a male or female name of the co-player.[17] In the first experiment 161 subjects were recruited from the same category of students as in the experiments discussed before. The subjects in the second experiment were fewer and younger; 112 subjects were recruited from three different secondary high schools in Malmö, Sweden. The result from these experiments is given in Table 4 and 5.

	1st group	2nd group
FF	55.3	53.3
FM	35.7	55.2
MM	55.3	48.0
MF	77.1	62.1

Table 4: The proportion (in percent) of the groups that choose the hawkish strategy. (Source: Holm, 1998, 2000).

	1st group	2nd group
Female subjects:	19.6	1.9
Males subjects:	21.8	14.1

Table 5: Absolute differences (in units of percentages) in the play against females and males.

On average the subjects exhibited again an overall increased hawkishness against female co-players. In the first group there is a symmetric and strong impact of the gender signal in both the male and female groups. In the second group the effect is smaller in both groups. In the second group of females, the impact of the gender signal is weak and the effect is reversed, which means that the subjects played slightly more hawkish against males.

3.3. Concluding Remarks

In all we have four observations on experimental behavior in BS games. In all four experiments the subjects played on average more hawkish against female co-players. In three of these the impact of the gender signal was statistically significant. Hence, our data motivates us to conclude that the controlled gender signal has impact on experimental behavior. However, care should be taken when generalizing; some observations indicate that cultural factors and age factors in the subject pool may matter.

We also detected substantial gender differences in gender sensitivity in the American subject group; American female subjects played significantly more hawkish against females than against males at the same time as the gender signal did not notably affect the

[17] The proportion in earnings between a hawk and a dove in a pure equilibrium was altered from 2:3 (i.e., 40:60)

males behavior. If we look at the average gender sensitivity in all four experiments then the average gender sensitivity was 20.0 in the female groups and 13.6 in the male groups.[18] However, if we exclude the American experiment there is virtually no gender difference in the remaining samples.[19] While this indicate that cultural factors may be important with regard to gender differences in gender sensitivity, it is relatively safe to say that the experimental results indicate that the female subjects' tendency to play more hawkish against females than against males is hardly smaller than the corresponding male tendency.

4. Discrimination in Bargaining

Due to the coordination aspect in the BS game playing more hawkish against a certain group does not necessarily mean treating the group unfavorably. Thus, it can be argued that playing more hawkish against females than against males is not discrimination but a use of stereotypes to coordinate. In one technical sense the subjects discriminates between gender signals, but it is not discrimination in the sense that they necessarily treats one group unfavorably. To make more general conclusions regarding experimental evidence of sex discrimination in bargaining we will now consider a class of strategic situations where it is less problematic to determine what it means to treat a group unfavorable.

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to 1:2.

[18] In these averages each experiment are given the same weight. If we base the weight on the sizes of the subject groups female gender sensitivity would increase somewhat while the male gender sensitivity would be about the same.

[19] In the three Swedish subject groups the average male gender sensitivity is 17.4 and the average female gender sensitivity is 17.6.

4.1 A Measure of Discrimination

Let player *i* and co-player *j* either sequentially or simultaneously choose actions from their action sets and denote the players actions and action sets by $a_i \in A_i$ and $a_j \in A_j$. Consider a set of bargaining situations that satisfy the following condition: for all $a_i \in A_i$, the utility function $u_i(a_i, a_j)$ is *weakly monotonic* in a_j. In these bargaining situations player *i* can be said to be *unambiguously affected* by *j* s choice. [20] To see this, note that a decrease in the value of a_j can only make *i* worse off or indifferent and an increase in a_j can only make *i* better off or indifferent. We will use this general property to define what it means to act *unfavorably* against a group in strategic situations. Suppose that a group of individuals playing the *j* role play against two different groups denoted by α and β that both play the *i* role.

Definition: *Unfavorable treatment*: Consider the class of bargaining situations where *i* is unambiguously affected by *j*. Denote by a_j^{α} and a_j^{β} the average action choice of the *j* group when playing against the α and β groups respectively. We then say that a group β is treated unfavorable compared to the α group if $a_j^{\alpha} > a_j^{\beta}$.

Examples:

i) *Binary choice*: Suppose the group of *j* players are respondents in an ultimatum game and that the α and β groups make the ultimatum proposals. In this case each *j* player makes a binary choice $A_j = \{0,1\}$ between accepting the offer ($a_j = 1$) and rejecting it ($a_j = 0$). Suppose that for a given value of the offer (i.e., a_i) 50 percent of the *j* players accept the offer

when playing against group α players but only 20 percent accept the same offer when it comes from members of the β group. Clearly, group β is unfavorably treated and we have that $a_j^{\alpha} = 0.5 > a_j^{\beta} = 0.2$.

ii) *Choosing a number in an interval*: Suppose the group of *j* players make the ultimatum proposals and that the α and β groups are the respondents. If the *j* players on average offer 45 when playing against the α players but only 35 when playing against the β group, then group β is treated unfavorably since $a_j^{\alpha} = 0.45 > a_j^{\beta} = 0.35$.

We will now introduce a simple measure that captures both the direction and the magnitude of unfavorable treatment.

Definition: *The discrimination effect:* Denote the maximum number that the *j* players can choose by $\bar{a}_j$. We define the discrimination effect in group *j* against group β as:

$$\Delta_j^{\beta} = \frac{a_j^{\alpha} - a_j^{\beta}}{\bar{a}_j} \times 100 \qquad (1)$$

Firstly, note that (1) can be applied both to binary cases (where $A_j = \{0,1\}$) and to cases where subjects choose a number in an interval (where $A_j = [0, \bar{a}_j]$). Secondly, from the definition it should be clear that Δ_j^{β} indicates the percentage change in average behavior due to the signal about group category (i.e., α or β).[21] Thirdly, since, a_j is non-negative it follows that

[20] In fact, we shall study situations that satisfy a more strict criterion of unambigouity; for all $a_i \in A_i$, the utility function $u_i(a_i, a_j)$ is *non-decreasing* in a_j and *strictly increasing* in a_j for some $a_i \in A_i$.

[21] However, depending on whether a_j is a binary or a number in an interval the interpretation of Δ_j^{β} differs somewhat. In example i) above $\Delta_j^{\beta} = 30$ should be interpreted as the units of percentage decrease in the average

$-100 \leq \Delta_j^\beta \leq 100$. A positive discrimination effect (i.e., $\Delta_j^\beta > 0$) means that the β group is unfavorable treated by group *j* and a negative indicates that the β group is favorably treated. Finally, without any systematic discrimination the mathematical expectation of Δ_j^β should be zero.

4.2 The discrimination effect in a series of experiments

Recently, a number of experiments have been conducted from which it is possible to extract data on the discrimination effect in strategic situations where a certain group is unambiguously affected by another group as described above. Whereas Solnick (1998) focus only on the gender issue, the other studies focus on gender in connection to some other controlled experimental variable like physical attractiveness (Solnick and Schweitzer, 1999), the effect of smiling (Scharleman et al., 1999), and ethnicity (Fershtman and Gneezy, 1998; and Holm, 2000). All these experimental studies concern two player bargaining situations and possess the following properties that are important when studying the sex discrimination effect:

i) The subjects are made aware of the sex of their co-players, but are not explicitly informed that the sex of their co-players is a critical experimental variable. These aspects are important to allow the experimental behavior be contingent on gender and to obtain natural behavior.

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j population's choice of the favorable action when meeting a β player compared to the choice towards an α player. In example ii) the discrimination effect (i.e., $\Delta_j^\beta = 10$) should be interpreted as the units of percentage decrease in the number (e.g., the dollar amounts) chosen by the average *j* population when meeting a β player compared to the number chosen when meeting an α player.

ii) The sex of the co-player is given as a controlled signal. The signals used include written names (Solnick (1998), Fershtman and Gneezy (1998), Holm (2000)), photographs of co-players (i.e., Solnick and Schweitzer (1999) and Scharleman et al (1999)). Consequently, we do not include studies of face to face interaction. The reason for doing this is not that face to face situations are uninteresting, but that it is much more difficult to isolate the pure gender effect in such situations (where a number of physical, psychological and social attributes are revealed to the bargaining parties).[22] However, it should be stressed that the more controlled situations we consider here can yield different results from face to face experiments.[23]

iii) The subjects have real monetary incentives in the experiments. In experimental economics real monetary incentives have since long ago been recognized as a necessary ingredient for making the experimental results convincing (see e.g., Mosteller and Nogee, 1951 and Kagel and Roth, 1995).[24]

Let us now study the discrimination effect for various strategic situations. In each situation the behaviors of both men and women against male or female co-players are studied. We let female subjects take the role as the β group and let the males be the α group. The discrimination effect against females will then be $\Delta_j^F = \left(\left(a_j^M - a_j^F\right)/\overline{a}_j\right)\times 100$, where $j \in \{M, F\}$ and M denotes males and F denotes females. A positive sign of the discrimination effect indicates unfavorable treatment of females and a negative sign the opposite.

[22] If these factors cannot be controlled for, it can be argued that face to face situations should be considered as uncontrolled. For a discussion of face to face experiments, see e.g., chapter 4 in Kagel and Roth (1995).
[23] For instance, Eckel and Grossman (1998) report male "chivalrous" behavior towards females in a face to face ultimatum response situation.
[24] However, it should be mentioned that non-economic studies of experimental bargaining behavior do not always include monetary payoffs (see e.g. King and Hinson (1994)).

Ultimatum games

In the most common form of the ultimatum game a proposer initially receives a sum of money of the experimenter. [25] The proposer may then send a share of this sum to a responder who may accept or reject the offer. If the offer is accepted, the proposer and the responder receive money according to the proposal. If it is rejected then neither the proposer nor the responder receives any money. It is not difficult to imagine bargaining processes in reality, where parties end up in ultimatum like situations. For instance, an employer may make a wage offer to a job applicant and the latter can accept or reject it. As indicated in the examples above, it is possible to study the discrimination effects in both proposer and responder behavior.

Ultimatum proposals: Observations from Ultimatum experiments are available in Solnick (1998), Solnick and Schweitzer (1999), and Holm (2000). In Solnick and Schweitzer (1999) two observations are obtained for each gender signal depending on whether the photograph represents a physical attractive or unattractive subject. The discrimination effects for the subject groups are presented in Table 6 and the calculations to obtain them are given in Appendix 2. As we can see from the results there is a positive discrimination effect against women in all eight groups. However, the discrimination effects are relatively small.

[25] For an introduction to ultimatum games, see Guth, Schmittberger and Schwarz (1982) and Kagel and Roth (1995).

	Females Δ_F^F	Males Δ_M^F
Solnick (1998)	8.2 (29)	3.0 (36)
Solnick and Schweitzer (1999)		
unattractive	1.0 (43)	2.8 (35)
attractive	2.7 (43)	3.1 (35)
Holm (2000)	11.9* (17)	1.1* (16)

Table 6. Discrimination effects in ultimatum proposals. Parentheses indicate the number of subjects in each group. The starred (*) figures are included to get the presentation as complete as possible, but have weaknesses for the variables that are studied.[26]

Ultimatum responses: Again we have observations from three different experiments. Two of these (Solnick (1998) and Solnick and Schweitzer (1999)) apply the strategy method in the ultimatum game which means that the data is based on the responders' minimum acceptable offers. In order to get the variables consistent with our general framework a_j^i is defined as the average maximum amount that the respondent (of group *j*) allows the proposer (of group *i*) to keep for himself. Holm (2000) applies the game method where each responder receives one offer out of two amounts (a low or a high offer)[27]. This means that each subject makes a binary choice (accept or reject) and that a_j^i is the average acceptance rate in group *j* against group *i*. Again, we see the same pattern as before in all but one male group where the discrimination effect is positive. Furthermore, note that the average magnitude of the discrimination effect is higher in the responder behavior than in the proposer behavior above.

[26] The figures from Holm (2000) refer to the behavior of a small group of subjects that chose to play a two person ultimatum game when they also had the option of playing a three person ultimatum game. Hence, besides that the groups are small, there might be a selection effect in this data.

	Females Δ_F^F	Males Δ_M^F
Solnick (1998)	13.3 (27)	9.4 (38)
Solnick and Schweitzer(1999)		
unattractive	0.1 (10)	1.4 (20)
attractive	1.5 (10)	4.0 (20)
Holm (2000)		
low offer	34.8 (30)	20.6 (35)
high offer	21.0 (27)	-18.5 (18)

Table 7. Discrimination effects in ultimatum responses. Parentheses indicate the number of subjects in each group.

Trust and reciprocity games

In this class of games a sum is initially given to player A who can choose whether to send an amount to another player B.[28] The amount sent to B is multiplied by a factor and B can then decide whether to return some money to A. It can be held that A's decision captures what we normally would call "trust" and that B's behavior captures reciprocity. [29]

It is commonly accepted that trust is important in many bargaining situations. One obvious reason is that it reduces the need for costly tools for control and monitoring. Reciprocity has also been recognized by Fehr, Gachter and Kirschsteiger (1997) as a potentially important contract enforcement device in bargaining.

Trust: Data from three different experiments are available for studies of discrimination effects in trust situations. Fershtman and Gneezy (1998) and Holm (2000) use a design that is similar

[27] To be more exact, each respondent was either confronted with a given proposal of SEK 50 or SEK 100, when the proposer had been given SEK 280.
[28] For a more elaborate presentation of this type of game, see Berg, Dickhaut and McGabe (1995).

to the one used by Berg, Dickhaut and McGabe (1995). The average amount sent to the B players (belonging to group *i*) is given by a_j^i. Both Fershtman and Gneezy (1998), and Holm (2000) focus on ethnic discrimination. The combined ethnicity and gender signal is given by Western and Eastern Jewish names in Fershtman and Gneezy (1998), which means that we get two observations of the discrimination effect in this study. Also Holm (2000) studies the effects of ethnicity. The gender signal is given by Swedish and Non-Swedish names.[30] However, since we here focus on the gender aspect we will only consider the group that received Swedish names.[31]

Scharlemann et al. (1999) use a variant of the trust game where the subjects make an initial binary choice of whether or not to trust a co-player. In this case a_j^i is the average trusting rate in the *j* group against the *i* group. Each subject can see a photograph of their co-player that is either smiling or has a neutral facial expression. This means that for each gender we get data on discrimination effects both for the group that got a smiling male or female co-player and the group that played against a co-player with a neutral facial expression.

In all we obtain discrimination effects for 10 different groups. In three of these the discrimination effect is negative. All negative discrimination effects are observed in male groups, which means that there is a consistent positive discrimination effect in all female groups. Hence, whereas females tend to treat other females unfavorable in trusting situations, the results are inconclusive for the male groups.

[29] It should be noted that each decision also may involve aspects of altruism and that trust can be linked to the more general concept of social capital (see Glaeser et al. (1999)).
[30] The Non-Swedish names where obtained among a group of refugee immigrants (e.g., names from Bosnia, Iran, Turkey, Somalia.)
[31] The reason is that one can expect that the subjects have difficulties to distinguish between female and male non-Swedish names.

	Females (Δ_F^F)	Males (Δ_M^F)
Fershtman and Gneezy (1998)		
Western names	6.2 (106)	31.8 (134)
Eastern names	2.8 (111)	-24.0 (132)
Holm (2000)	1.8 (60)	5.3 (59)
Sharlemann (1999)		
Smiling face	19.6 (*)	-26.1 (*)
Neutral face	29.7 (*)	-10.2 (*)

Table 8. Discrimination effects in trust decisions. Parentheses indicate the number of subjects in each group. Stars (*) indicate that the number of subjects in each subgroup is not reported.[32]

Reciprocity: There is only available results from one experiment where it is possible to study how the responders in a trust game are influenced by information about the co-player's sex.[33] In Holm (2000) the groups of subjects got two different treatments; one group was informed that their co-player had sent them a relatively large sum and the other group was told that their co-players had sent them a relatively small sum. The results in Table 9 demonstrate negative discrimination effects in all but one female group. Hence, these observations deviate from the previous pattern of mainly positive discrimination effects. This may indicate that different mechanisms are dominant in this particular bargaining situation, but we cannot exclude that these negative effects is due to random variations in the data. The number of observations in this situation is too small to warrant any conclusions regarding discrimination effects in reciprocity behavior.

[32] The total number of subjects in Sharlemann et al (1999) is 120.

	Females (Δ_F^F)	Males (Δ_M^F)
Holm (2000)		
small sum	- 6.5 (38)	-9.9 (39)
large sum	11.4 (23)	-0.4 (19)

Table 9. Discrimination effects in female and male groups. Parentheses indicate the number of subjects in each group.

4.3 Concluding Remarks

In the presentation above we use reported observations from 16 female and 16 male subject groups. To study discrimination behavior in all groups a general measure of discrimination effects is developed. Each group has been subject to different treatments that allow for calculations of sex discrimination effects in bargaining situations, where one party is unambiguously affected by the other party's action. If gender signals did not matter to the subjects it is reasonable to expect that the probability of a positive discrimination effect is equal to the probability of a negative one.[34] There is a significant indication on discrimination against females; the discrimination effect was positive in 25 out of 32 subject groups. If we regard these observations as independent and apply the binomial distribution we can strongly reject that there is no discrimination ($p = 0.00078$). Furthermore, there are notable gender differences in discrimination behavior. In the female groups 15 discrimination effects are positive and one is negative, which means we can strongly reject that there is no

[33] Fershtman and Gneezy (1998) only report responder behavior for a male subgroup and Scharleman et al. (1999) did not use actual co-players.

[34] To make this assertion we must add some assumption about the shapes of the distributions of the discrimination effects. Since, the measure of discrimination effect is developed in this paper and hence used for the first time there is too little data to make qualified guesses about its distribution. To avoid technicalities we make the (sufficient) assumption that the distributions are symmetrical.

discrimination effect in the female groups ($p = 0.00024$). Noting that there are 10 negative and 6 positive discrimination effects among the male groups there is not a corresponding consistent and significant unfavorable treatment against women among the male groups ($p = 0.12$). Thus, in these experiments we can conclude that females consistently act more unfavorably against other females than against males and that this is not necessarily the case for males. This is also revealed by the average discrimination effect; it was 10 in the female groups and -0.4 in the male groups.

The discrimination effects in absolute terms ($\left|\Delta_j^F\right|$) are in general higher when the action is a binary choice compared to when the choice variable is a number within an interval. The average absolute discrimination effect in the case of binary choices for all groups is 17.4 whereas the average discrimination effect when the choices are numbers in an interval is 6.8. One conceivable reason for this is that, unlike in binary choices, certain numbers in a given interval may appear to the subjects as the "natural" choice (like, for instance, proposing 50 percent of the initial sum in an ultimatum game or in a trust game). The presence of such behavioral regularities is likely to reduce the variation in the choices and possibly overshadow the gender signal to a certain degree.

5. Explanations

One obvious way to interpret the fact that gender signals significantly affect experimental behavior in BS games is that the co-players gender are (more or less) consciously used as a coordination device according to the theories outlined by Schelling (1960).[35] However, this does not explain why females and not males get the smaller portion and it certainly does not

[35] See Holm (1998).

explain why females have a higher sensitivity for gender signals than males. Furthermore, it does not explain why females are more inclined to discriminate against females than males.

We will not be able to develop a consistent theory that fully explains all aspects of this behavior. However, we will point at a number of conceivable explanations to some of our observations. In doing this we combine rational choice theory with results from recent findings in social psychology.

It is not obvious how to explain these experimental phenomena with standard economic discrimination theory. Of course, it is possible to twist Becker's (1957) preference argument and claim that the experimental behavior simply reveals that both men and women have preferences for giving males the larger part and that females have stronger gender preferences than males. However, this is just to rephrase the results in a different terminology and such an "explanation" does neither make justice to the experimental results nor to Becker's discrimination theory.

A conceivable explanation to the fact that subjects on average are more likely to distribute the larger amount to the males in the experiment is that they consciously or unconsciously make use of stereotypes. Using stereotypes means according to Eagly (1995) that females are considered more socially sensitive, friendly, concerned with others' welfare, whereas males are considered dominant, independent and aggressive. Now, if the subjects associate general gender stereotypes to the probability that the co-player chooses the hawkish strategy in a BS game or that he does not accept an ultimatum proposal, the stereotypes obviously point in the direction that females are less likely to play such a hawkish strategy.[36] The expected payoff from playing hawkish against a co-player playing according to the

[36] Technically this hypothesis is an example of statistical discrimination (see Arrow, 1972 and Phelps, 1972). However, in this case the unobservable characteristics (that are equivalent to e.g., "productivity" in labor market discrimination models) are general mentality attributes in bargaining.

female stereotype is higher than it is against the male stereotype, which explains the average tendency to let males have the larger share.[37] [38]

Why are modern young bright students using stereotypes? In the BS game it clearly helps the students to coordinate and improve their payoff and this is so independent of if the stereotypes accurately describe real behavioral gender differences or not. Thus, this reference to stereotypes is satisfactory from a game theoretical point of view. However, our results could be considered more economically relevant if the presence of stereotypes can be given a rational explanation. In that case the use of stereotypes reflects something more than an effect that crops up in experiments. Part of such an explanation can be found in recent meta-analytical studies in psychology. In this type of studies, established quantitative measures are used to summarize results from various research areas. Several such studies have concluded that peoples' gender stereotypes in general are supported by empirical observations.[39] Thus, for instance, people tend to believe that males are more aggressive than females and this is, in fact true according to several studies.[40] We then get an explanation for the use of stereotypes if we combine these findings with standard economic theory that simply says that people make use of information that improve their payoffs. According to this explanation, not only are our subjects using a stereotype to improve their outcomes in the experiment. The stereotypes are likely to have a value for them also in making better predictions in the world around them.

The reasoning above both explains the direction of and the presence of discrimination behavior. But, why are women more sensitive to gender signals than men? One relatively straightforward economic explanation is that women have incentives to be more

[37] It should be noted stereotypes about risk attitudes lead in the same direction; if e.g., males are more willing to take risks they are considered to be more likely to play the hawkish strategy in a BS game.
[38] Note, that whereas it is relatively easy to associate gender stereotypes to strategies in the BS game and the Ultimatum game, it is more problematic in the case of trust games. However, it is possible that since males are considered less socially sensitive a player may believe that he has to send more money to a male co-player than to a female co-player to trigger reciprocal actions.
[39] See e.g., Eagly (1995) for references.

observant to their rights in potential sex discrimination situations. The reason for this is that affirmative action policies and laws in general are designed to support the discriminated party, which is more likely to be a woman than a man. Thus, the expected value of being informed and sensitive to gender is higher for women. We know of no study that has investigated this question empirically. However, in the spirit of our hypothesis Browne (1997) observes that American female business students on average have stronger beliefs than males that pure discrimination and male opposition to women (in management) explain lower participation of women in upper management.[41]

Let us finally show that it is possible to construct other explanations to the higher female gender sensitivity. A more far-fetched hypothesis inspired by evolutionary theory and/or learning theory is that the behavior reflects a more general reminiscence of behavioral situations, where it has been more important for females to take into account the gender of the other party than for a male. In bargaining situations there is always a potential conflict lurking, that eventually may lead to physical violence. Now, due to average relative physical weakness and to higher male aggressiveness a female's expected gain from a conflict with a man would on average be smaller than the male's expected gains. As a consequence, the optimal female behavioral strategy may be to never challenge men, but only women. However, for a substantial part of the males it is possible that the optimal strategy may be to always go for the largest part independent of the other party's sex. Now, if some subjects bring with them reminiscences of strategies like these into the experimental situation, the sex of the opponent will be important to these females but not to the corresponding males. This will affect the population's average gender signal sensitivity.

[40] See e.g., Bettencourt and Miller (1996).
[41] Clearly, believing in the presence of sex discrimination is not the same as sensitivity to gender signals. However, the results are consistent with our results and the hypothesis above in that someone with stronger prior beliefs in discrimination also has a stronger reason to be observant to gender signals.

6. Conclusion and Implications

The purpose of this paper is to study and analyze experimental sex discrimination. It is argued that economic experiments can be one important complementary way to understand and isolate discrimination behavior in economic transactions. The results from two similar BS experiments conducted in Sweden and USA reveal a cultural difference between the Swedish and the American group in that the average play of the hawkish strategy is higher among the Swedish subjects than among the Americans. Furthermore, this effect can mainly be attributed to differences in the female groups.

In general, subjects played more hawkish against female co-players than against male co-players. The impact of gender signals (about the subjects' co-players) on the observed behavior was higher among females than males. However, whereas the gender sensitivity in the Swedish subject groups were relatively balanced between the sexes, almost all gender sensitivity in the US subject group can be attributed to the females. In fact, whereas there is practically no evidence that American males play more hawkish against females than against males, it is significant that females do it.

To learn more about experimental discrimination behavior in general and to investigate if the effects in the BS experiment were specific for just that type of game, we also present results for a class of bargaining situations where it is possible to define what it means to treat one group unfavorably. We provide a measure of the direction and the strength of discrimination behavior. Thirty-two subject groups were identified as having received adequate experimental treatments that allow for calculations of sex discrimination effects in bargaining situations. Viewed as isolated experiments, the discrimination effects in each subject group may often seem relatively small and unimportant. However, when the effect is

studied consistently across all experiments a systematic pattern emerges. There is a significant indication on discrimination against females; in 25 out of 32 subject groups females were on average treated unfavorably compared to males. Gender differences in discrimination behavior reminding of the observations in the BS experiments were also detected. Over 90 percent of the female groups played on average more favorably towards males than towards females. The corresponding figure for the male groups was less than 70 percent.

By combining Schelling's (1960) theories about focal points, with recent findings in psychology stating that people in general are accurate in their gender stereotypes it is possible to give a rational explanation to some of our experimental observations. To explain that females are more sensitive to gender signals we offer two economic explanations based on the conjecture that females have a higher expected value of being sensitive to gender signals.

We have stressed that experimental results should be interpreted with care. The BS experiment is designed to be sensitive to gender signals and the field as such is relatively new. This makes extrapolations of the results even more hazardous. For instance, in relatively unexplored fields, one cannot exclude that there are hidden variables in the experiments that are related to gender and gender signals in a way that is not yet understood. However, the fact that the co-player's gender matter in experimental behavior may reflect important behavioral aspects of real discrimination in economic transactions and it would be a sin of omission not to mention them. Furthermore, even if it is quite possible that these experimental findings say little about real behavior they inspire the formulation of new questions and hypotheses that may be important. The experimental data presented in this paper clearly challenges the cliché that the causes to economic sex discrimination are to be found in male chauvinism. Rather the data demonstrates that differences in earnings depend primarily on an unwarranted female carefulness or paranoia when encountering male co-players. For instance, the substantial

earnings gap that was reflected in American males having 28 percent higher average experimental earnings than American females in a BS experiment can almost entirely be attributed the female tendency to play “dovish” against male co-players.

These findings have potential policy implications. They stress that economic discrimination may involve a mentality factor and that policies aiming at abolishing discrimination also must target the female syndrome of unjustified defensiveness towards men especially in bargaining situations. In the light of these experiments it may even be that some policies promote rather than work against gender based earnings gaps and sex discrimination.[42]

If we allow for bounded rationality the public exposure of debating and implementing anti-discrimination policies may even escalate the "mentality effects". If e.g., media exaggerates certain "sex discrimination cases" this may reinforce stereotypes and discrimination expectations so that the stereotypes loose proportions. This in turn may increase females' awareness and priors of being discriminated, which may reduce their subjective expected payoff and thus their willingness of challenging men for higher positions or to take higher education in professions traditionally dominated by males. As a consequence, the gender gap may not decrease, even if strict anti-discrimination policies are implemented. The process described above works as a paralyzing self-fulfilling prophecy for women and confirms their discrimination beliefs. Clearly, this process can go on without any male discrimination behavior.

[42] To give a concrete example, in Sweden various affirmative action policies and voluntary programs have been implemented to promote women to higher positions in the scientific community and as executives in industry and trade. Clearly, if we extrapolate the experimental results it is questionable if it is advantageous for a female subordinate to have a manager of the same sex in bargaining situations, since on average the female manager might be "weaker" against men than against women. Needless, to say this reasoning does not extend to problems such as the dynamic effects of affirmative action policies that has been analyzed by e.g., Coate and Loury (1993) and experimentally by Corns and Schotter (1996).

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APPENDIX 1

This appendix contains the information given to the American subjects and the text on the questionnaire. The information and the questionnaire in the Swedish experiment were similar in almost every detail and are available in Holm (1998).

1

Information about the Experiment

You have been paired with a co-player. If you and your co-player are able to coordinate your choices, then you will earn points in the experiment. The payoff (in terms of points) depends partly on your choices and partly on your co-player's choices. However, your co-player's choices will be unknown to you and he/she will not know your choices.

Your answers will give you points that will be counted in dollars. You can earn up to $161 (and no less than $ 0) depending on how well you succeed. A number of winners will be randomly selected among those who answer the questions. The winners will get the value that he/she has earned in the experiment. The probability of being selected as a winner is 3%, which means that on average about one participant out of thirty will be selected. Hence, your task is to collect as many points as possible by choosing strategically and by guessing your co-player's choices.

Instructions.

1. Please, fill in your own name and postal address at the top of the Questionnaire (next page)! (Your answers' will be anonymous and will only be used for the purpose of research. Your name and address are needed to make it possible to identify and pay the winners.)
2. Please check that your co-player is classified into a category!
3. Please fill in the questionnaire and when you have finished, please hand in the questionnaire to the experimenter (as silently as possible)!

Good luck!

2

Questionnaire (Part I)

Your Name:_____ Co-player category:
Postal Address:_____ Male Student
_____
_____

Instruction: You will confront four strategic situations, where your payoff depends partly on your own choices and partly on your co-player's choices. Your task is to earn as many points (in dollars) as possible.

1. You are going to choose between Right and Left.
Points: If you choose the same direction as your co-player, then you will earn $30 each. (That is if you both choose Right or if you both choose Left). If you and your co-player choose different directions, nobody will earn anything.

Circle *one* alternative!

Alternatives: Left Right

2. You are going to choose between Right and Left.
Points: Again, you have to choose the same direction as your co-player in order to gain anything. You will earn $30 each if both of you choose Right. If both choose Left, then you will earn $31 each.

Circle *one* alternative!

Alternatives: Left Right

3

3. You are going to choose a distribution.
Points: You and your co-player have the opportunity of sharing $100. In order to get the money you and your co-player have to agree on how to share the money. If both choose the same distribution, you will get your share of the chosen distribution and your co-player will get his/her share. If your choices lead to disagreement about how to share the money, both receive zero dollars. (See the examples below).

Circle *one* alternative below!

Alternatives:

You get $60 and your co-player gets $40.
You get $50 and your co-player gets $50.
You get $40 and your co-player gets $60.

Explanation and Examples: Notice that when you have chosen one alternative there is only one alternative that your co-player can choose if you agree about how to share the money.
Example:
i) If you have chosen the uppermost alternative, agreement requires that your co-player has chosen the lowermost alternative. (In this case, you will get $60 and your co-player will get $40.)
ii) If you have chosen the middle alternative, agreement requires that your co-player also has chosen the middle alternative. (In this case, both will get $50.)
iii) If you have chosen the lowermost alternative, agreement requires that your co-player has chosen the uppermost alternative. (In this case, you will get $40 and your co-player will get $60.)

4. You are going to choose a distribution.
Points: You and your co-player have the possibility of sharing $100. The problem is the same as in question 3, but here you and your co-player have fewer ways to share the money.

Circle *one* alternative below!

Alternatives:

You get $60 and your co-player gets $40.
You get $40 and your co-player gets $60.

Appendix 2

This appendix contains the data used to calculate the discrimination effects in section 4. Information of the experimental design etc. can be obtained in respective paper. The papers are presented in the order they are referred to in the tables.

Solnick (1998): We start with the study conducted by Solnick **(**1998**)**. In this study the maximum offer was \$10 (i.e., $\overline{a_j} = 10$) and the average offer from females to other females (a_F^F) was 4.31; average female offers to males (a_F^M) was 5.13; average male offers to females (a_M^F) was 4.43, and average male offers to males (a_M^M) was 4.73. Using the measure of the discrimination effect we get:

$$\Delta_F^F = \frac{5.13 - 4.31}{10} \times 100 = 8.2 \text{ and } \Delta_M^F = \frac{4.73 - 4.43}{10} \times 100 = 3.0$$

Solnick (1998) reports the average minimum acceptable offers for each group. If we take the difference between the maximum offer (i.e. 10) and the minimum acceptable offer we get the maximum amount that the responder allows the proposer to allocate to himself. For the different groups we get $a_F^F = 5.85$; $a_F^M = 7.18$; $a_M^F = 6.61$ and $a_M^M = 7.55$. Using the same method to calculate the discrimination effects as above we get $\Delta_F^F = 13.3$ and $\Delta_M^F = 9.4$.

Solnick and Schweitzer (1999): In this paper the relation between gender and physical attractiveness on a number of photographed subjects is studied. A photograph of a female or a male subject is either classified by a control group as attractive or unattractive. The

most attractive and the most unattractive photographs are then selected and presented to a different subject pool that both play in the proposer and responder role. This means that two observations are obtained for each gender signal depending on whether the photograph represents an attractive or unattractive subject.

The maximum amount to propose was $10 and the observations regarding proposer decision against unattractive female and male subjects are given by the following figures: $a_F^F = 4.60$; $a_F^M = 4.70$; $a_M^F = 4.44$ and $a_M^M = 4.72$ which gives $\Delta_F^F = 1.00$ and $\Delta_M^F = 2.8$. The corresponding figures for attractive female and male subjects were: $a_F^F = 4.60$; $a_F^M = 5.07$; $a_M^F = 4.48$ and $a_M^M = 4.79$ which gives $\Delta_F^F = 2.7$ and $\Delta_M^F = 3.1$.

Depending on the attractiveness we also get two different observations for the responder decisions. The maximum amount that the responder allows an unattractive proposer to allocate to himself is given by $a_F^F = 6.47$; $a_F^M = 6.48$; $a_M^F = 6.72$ and $a_M^M = 6.86$. The discrimination effects will then be $\Delta_F^F = 0.1$ and $\Delta_M^F = 1.4$. The corresponding figures for an attractive proposer are $a_F^F = 6.08$; $a_F^M = 6.23$; $a_M^F = 6.43$ and $a_M^M = 6.83$. The discrimination effects will be $\Delta_F^F = 1.5$ and $\Delta_M^F = 4.0$.

Holm (2000): In Holm (2000) each subject is confronted with a number of one shot bargaining situations. For each situation the subjects were told that they were matched with a co-player with a certain name.[43] The observation of proposer behavior in ultimatum bargaining is based on those subjects that choose to play the ordinary (two-player) ultimatum game, when they had the opportunity to play a three player ultimatum game.

[43] Note that the main objective of this study was to investigate ethnical discrimination effects against refugee immigrants in Sweden. Since these groups are minorities, sufficiently many observations from real matchings were not possible to obtain (and because of some other more practical reasons) the fictitious co-player names were used. This element of deception was conducted under considerations of the ethical guidelines of The Swedish Council for Research in the Humanities and Social Sciences.
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Hence, the observations are based on the behavior in a subgame, which means that there might be a selection effect present.[44] Furthermore, all observation we report here is based on the subjects that received a Swedish co-player name.[45] In this study the maximum amount the proposer could offer was SEK 280 (i.e., $\overline{a_j} = 280$) and the average offers was $a_F^F = 125$; $a_F^M = 158$; $a_M^F = 137$ and $a_M^M = 140$. The discrimination effects will then be $\Delta_F^F = 11.9$ and $\Delta_M^F = 1.1$.

The observation of responder behavior is less problematic. One group received the information that their co-player had offered them SEK 50 and the other group received the information that they had been offered SEK 100. The average acceptance rates in the first group were: $a_F^F = 0.438$; $a_F^M = 0.785$; $a_M^F = 0.5$, and $a_M^M = 0.706$ which result in $\Delta_F^F = 34.8$ and $\Delta_M^F = 20.6$. In the group that received the larger sum the acceptance rates were: $a_F^F = 0.727$, $a_F^M = 0.938$; $a_M^F = 0.8$, and $a_M^M = 0.615$. The corresponding discrimination effects were $\Delta_F^F = 21.0$ and $\Delta_M^F = -18.5$.

In Holm (2000) the subjects also participated in a trust game as described above. Each A player was asked to allocate SEK 200 between himself and a B player. The amount sent to B was tripled and B had then an opportunity to return money to A. The average amounts sent by subjects that played the A role were: $a_F^F = 86.7$, $a_F^M = 90.2$; $a_M^F = 99.6$, $a_M^M = 110.2$. We then get $\Delta_F^F = 1.8$ and $\Delta_M^F = 5.3$. Like in the study of responder behavior in the ultimatum situation the B players received information that the co-player either had sent them SEK 50 or SEK 100. Those receiving the smaller amount returned on average the following amounts: $a_F^F = 51.6$, $a_F^M = 41.8$; $a_M^F = 53.2$, and $a_M^M = 38.4$. Keeping in

[44] The subjects' played a game that Riedl and Okada (1999) call a social exclusion and coalition formation game.

[45] The reason for this was to make sure that the gender signal was clear to all subjects; for some of the non-Swedish co-player names one can expect that that the subjects had problems in distinguishing a female from a male co-player name.

mind that $\overline{a_j} = 150$ we get $\Delta_F^F = -6.5$ and $\Delta_M^F = -9.9$. In the case the subjects received SEK 100, the groups returned the following average amounts: $a_F^F = 77.8$, $a_F^M = 112.1$; $a_M^F = 95.0$, and $a_M^M = 93.7$. In this case we have that $\overline{a_j} = 300$ and consequently that $\Delta_F^F = 11.4$ and $\Delta_M^F = -0.4$.

Fershtman and Gneezy (1998): In the trust game of Fershtman and Gneezy (1998) each A player was asked to allocate NIS 20 between himself and a B player. The amount sent to B was tripled and B had then the opportunity to return some money to A. The average amounts sent to B players with Western names were: $a_F^F = 11.3$, $a_F^M = 12.53$; $a_M^F = 10.79$, $a_M^M = 17.16$. We then get $\Delta_F^F = 6.2$ and $\Delta_M^F = 31.8$. The corresponding figures for the Eastern B players were $a_F^F = 10.38$, $a_F^M = 10.94$; $a_M^F = 10.43$, $a_M^M = 5.62$, $\Delta_F^F = 2.8$ and $\Delta_M^F = -24.0$.

Scharlemann et al (1999): In this study the subjects played a binary variant of the trust game where consequently $\overline{a_j} = 1$. All subjects played the A role but were informed that they played against co-players whose photographic images were shown to the subjects before the play.[46] The co-player was either smiling or had a neutral facial expression. The average trust rates against neutral co-players were $a_F^F = 0.286$, $a_F^M = 0.583$; $a_M^F = 0.667$, and $a_M^M = 0.565$. The resulting discrimination effects were $\Delta_F^F = 29.7$ and $\Delta_M^F = -10.2$. The corresponding figures for the groups playing against a smiling co-player were $a_F^F = 0.429$, $a_F^M = 0.625$; $a_M^F = 1.00$, $a_M^M = 0.739$, $\Delta_F^F = 19.6$ and $\Delta_M^F = -26.1$.

[46] In reality the subjects played against a pre-programmed computer.