Sex Discrimination or Paranoia?

- gender differences in experimental discrimination behavior

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Abstract: We present and analyze results from two similar economic sex discrimination experiments conducted in USA and Sweden involving more than 300 subjects. The experimental data supports the conclusion that females are more inclined to discriminate by sex than males. In the US subject group almost the whole discrimination effect can be attributed to females playing relatively unfavorable towards subjects of their own sex. We also detect significant cultural differences between the Swedish and American population.

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

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1. Introduction

The aim of this paper is to analyze experimental discrimination effects. 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. Recent exceptions to this are Fershtmann and Gneezy (1998) and Holm (1999).[2] The former paper establishes the presence of experimental ethnic discrimination in a trust game in Israel and the latter analyzes and demonstrates significant sex discrimination effects in a series of "Battle of the Sexes" experiments conducted both in Sweden and in USA. Whereas Holm (1999) focuses on detecting experimental discrimination interpreted as focal points, this paper will analyze the relative strength of male and female discrimination and cultural differences using the same experiments.

The experimental data 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. The relative dominance of female to male discrimination is strongest among the American subjects, where almost the whole discrimination effect can be attributed to the female group's tendency to treat other females unfavorably. Furthermore, we also detect a cultural difference between American and Swedish subjects in that the Swedes on average more often than the Americans play the "hawkish" or aggressive strategy.

In this paper we will also try to give an economic theoretical explanation for the main part of the observed behavior and to demonstrate that our results are consistent with recent 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

[1] See the audit studies of Neumark (1996) and Ayres and Siegelmann (1995).
[2] The critical idea to use co-player labels in these types of experiments was first developed in Holm (1997).

supported by empirical facts. This means that there may be an economic rationale for people to hold them and use them. In the battle of the sexes 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 the experimental design and the results in section 3. Different measures to evaluate the discrimination effect are presented and the results are given in terms of the various measures. In section 4 we try to give an economic explanation to our observations. Finally, in section 5 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. However, the meaning of the concept has expanded and sometimes it simply refers to

treating certain groups badly. We will use the word in a more precise and technical sense and say that discrimination occurs if a group of subjects on average acts differently towards a counterpart if information about the counterpart's category (e.g., in terms of sex or race) differs.

For a number of reasons it is difficult to isolate discrimination effects in field studies.[3] 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.[4] Secondly, it may also be the case that the regression includes variables that are treated as controls, which are affected by discrimination.[5] 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).[6] The audit method relies on

[3] For a discussion see e.g., Gunderson (1989), Goldin (1990) and Heckman (1998).
[4] For studies see e.g., O'Neill and Polachek (1993), Polachek (1981) and Andrisani (1984).
[5] See for instance Goldberg (1996).
[6] 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

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.[7]

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 studied and isolated.[8] 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 are 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

working life (Goldin, 1986) to theories about male and females interaction within a household (Lundberg and Pollak, 1996; and Francois, 1998).

[7] 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.

[8] 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.

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

3 A Discrimination Experiment

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.[9]

Each subject faced the problem of sharing $100 with an anonymous male or female student co-player.[10] 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 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 Battle of the Sexes game, (henceforth BSG) since we expected it to be sensitive to discrimination. By combining coordination motives with conflict of interest the BSG 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 BSG.

[9] Additional details about the experiment are available in Holm (1999) and can be obtained from the author.
[10] The experiment also contained three other questions that are presented in the Appendix. Note also, that in the Swedish study the subjects shared SEK 500.

3.1 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.

	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, 1999).

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 (1999) demonstrates i) that a gender label effect exists (i.e., sex discrimination takes place) and ii) that the effect can be analyzed as a focal point in the Swedish population.[11] 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 discrimination, but of a different character. Contrary, to the two-sided discrimination 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.[12]

	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

[11] 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.

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.

3.1.1 The Magnitude of Discrimination

One indication of the strength of discrimination is simply the absolute increase in frequency of a behavior conditioned on the discriminatory 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 discrimination 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.

The experimental data reveals that the absolute discrimination effects in the Swedish subject groups are higher than for the corresponding American groups. To relate the discrimination effect to the variability between the subgroups we have constructed an index with the mean error between the subgroups for each nationality as a base. Let m^N denote the

[12] 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.

mean error for nationality $N \in \{S, U\}$ (where *S* and *U* denote Swedish and American subjects, respectively), then index is given by:[13]

$$\Delta_i^N = \frac{|p_{iF} - p_{iM}|}{m^N} \times 100 . \qquad (1)$$

Thus, if $\Delta_i^N = 100$ the discrimination effect is as large as the mean error. The figures for this index are presented in Table 4:

	Swedish subjects	American subjects
Female subjects	263	244
Male subjects	139	20

Table 4: Male and female discrimination indexes relating to the mean error.

If we only concentrate on the magnitude of the discrimination we can see in Table 3 that the discrimination effect ranges from being more than 2.5 times the mean error to only one fifth of it. It can also be noted that the index in Table 4 gives the same ranks to each subgroup as the as the absolute discrimination effects in Table 3.

[13] We use the following weighted mean error: Let

$\bar{p} = \frac{1}{4}(p_{MM} + p_{MF} + p_{FM} + p_{FF})$,

be the average hawkish play for a given nationality. The mean error is then given by:

$\frac{1}{4}(|\bar{p} - p_{MM}| + |\bar{p} - p_{MF}| + |\bar{p} - p_{FM}| + |\bar{p} - p_{FF}|)$.

3.1.2 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 gender effects in sex discrimination that are evident in Table 3 and 4 say that the female discrimination is clearly higher for both the Swedish and the American subject groups compared to the corresponding male discrimination. 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 among the American subjects; the American male subjects do not exhibit any notable average discrimination behavior at the same time as there is a substantial and significant discrimination behavior in the female group.[15]

[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 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 ($p = 0.017$).

3.2 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 Swedish male group the coordination rate would increase to 0.61. Although, these observations do not directly contradict the hypothesis about cultural consistent experimental behavior, they do not support it.

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

4. Theory

One obvious way to interpret the fact that gender signals significantly affect experimental behavior from a game theoretical perspective is that the co-players gender are (more or less) consciously used as a coordination device according to the theories outlined by Schelling (1960).[17] However, this does not explain why females and not males get the smaller portion and it certainly does not explain why females have a higher sensitivity for gender signals than males.

We will point at a number of conceivable explanations to this behavior. 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 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 some set 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, the stereotypes obviously point in the direction that females are less

[17] See Holm (1999).

likely to play the hawkish strategy.[18]. The expected payoff from playing hawkish against a co-player playing according to the female stereotype is higher than it is against the male stereotype, which explains the average tendency to let males have the larger share.

Why are modern young bright students using stereotypes? In this 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.[19] Thus, for instance, people tend to believe that males are more aggressive than females and this is, in fact true according to several studies.[20] 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

[18] 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 bargaining attributes that are summarized in the term "hawkishness".
[19] 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.[21]

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 situations of unequal sharing 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.

[20] See e.g., Bettencourt and Miller (1996).
[21] 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.

5. Conclusion and Implications

The purpose of this paper is to 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 BSG 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.

The experimental data supports the conclusion that females are more inclined to discriminate by sex than males. We provide a number of different descriptive measures of the strength of discrimination and independent of measure the female groups have the highest discrimination tendency. However, whereas the discrimination behavior in the Swedish population is relatively balanced between the sexes[22], almost all discrimination that takes place in the US subject group can be attributed to the females. In fact, whereas there is practically no evidence that American males discriminate between the sexes, it is highly significant that females do it.

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 the coordination behavior in the experiment. 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.

[22] The balanced and symmetrical discrimination behavior among the Swedish subjects is also supported by a somewhat differently designed discrimination experiment reported in Holm (1999).

We have stressed that experimental results should be interpreted with care. Furthermore, this particular experiment is designed to be sensitive to discrimination effects, which make extrapolations of the results even more hazardous.[23] 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 from the US study clearly challenges the cliché that the causes to economic sex discrimination are to be found in male chauvinism. Rather the data on the Swedish and US subjects demonstrates that differences in earnings depend primarily on an unwarranted female carefulness or paranoia when encountering male co-players. Thus, the substantial earnings gap that was reflected in American males having 28 percent higher average experimental earnings than American females in the mixed subject pool, can almost entirely be attributed the female tendency to discriminate co-players of their own sex.

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.[24]

[23] For instance, the strategy set does not include an equal split alternative, which may be available in many real-life situations. On the other hand, in some important situations equal sharing is not optional. This is the case in situations when there is only one in a team that can be promoted to a certain managerial position.

[24] 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" towards men than towards 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).

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 confirm their discrimination beliefs. Clearly, this process can go on without any male discrimination behavior.

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APPENDIX

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 (1999).

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!

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. 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.