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What's in a Name?

- An ethnical discrimination experiment

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Abstract: This paper presents results from an ethnical discrimination experiment that was conducted in one of Sweden's most "problematic" cities with respect to the integration process of refugees. The subjects confronted three different bargaining games; one trust game, one social exclusion and coalition formation game; and one battle of the sexes game. No general discrimination effect was detected. However, one specific effect was that Non-Swedes were less likely to be chosen as bargaining partners in the coalition formation game. Another specific effect was found in the males' trust and ultimatum responses; Non-Swedes had higher returns on being generous compared to Swedes.

Keywords: integration, ethnical discrimination, bargaining, experiments.

JEL: C78, C91, J71.

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1 Introduction

Politicians and social scientists have repeatedly recognized that one of Sweden's most critical future problems is the integration of the large number of refugee immigrants that came to the country during the last decades. This problem is especially challenging for Sweden since recently the country received in relation to its population more refugees than most other EU countries. In addition to this Sweden suffered from a severe economic crisis in the first half of the nineties. These circumstances have been a menace to the traditional Swedish sympathetic view and mentality towards immigrants.

The integration process in Sweden has been everything but successful the last two decades. Refugee immigrants from the 80s and 90s have had difficulties in entering the labor market (see e.g., Ekberg (1990, 1999) and Scott (1999)). The jobs that are offered are usually low paying jobs that require little or no education. In the larger cities immigrants tend to live in concentrated areas where crime rates, the fraction receiving public assistance etc. are much higher than in neighboring areas. In some schools in these areas the fraction of pupils having Non-Swedish background is above 50 percent.

In order to address the integration problem from a policy perspective it is essential to have a solid understanding of the factors involved and to pin-point obstacles towards successful integration. Clearly, economic integration problems can be measured in many ways and many explanations have been suggested in the literature. It is out of scope of this paper to review this research at any depth. However, to view the present study in a larger context let us briefly mention some studies that are relevant to the Swedish case. One explanation that has been studied by Borjas (1985) and Lalonde and Topel (1991) refers to the quality and the characteristics of the immigrant cohort. Referring to the Swedish case Roth (1999) notes that one possible explanation of the integration problem may have to do with the

new character of the immigrant groups. Since the 80s the immigrant cohorts consist less of economic immigrants and more of refugee immigrants. The cohorts are also more ethnically and culturally distant from the Swedes than earlier cohorts. This means that these cohorts may have to acquire more human capital (in terms of e.g., the proficiency of the Swedish language) in order to catch up with the natives. Another class of explanations relates to changes in the economy that may affect the immigrant's possibilities to participate in the economic life (see e.g., Scott, 1999). For instance, one conceivable explanation to recent high unemployment rates among certain refugee immigrant groups is that the severe recession in Sweden in the beginning of the nineties coincided with the arrival of large refugee immigrant groups (from e.g., Bosnia). These groups have had a hard time getting access to the labor market. It is also conceivable that individuals with a foreign background are more vulnerable to severe downturns than other groups. A third possible explanation is that there may simply be discrimination against refugee immigrants. This essay focuses on the last factor.

This paper does not pose the question *if* ethnic discrimination takes place in Sweden. Frequently reported cases where employers and house owners have discriminated against persons based on ethnicity are sufficient evidence for its occurrence, but not of its dispersion. Rather, this study tries to find out to what extent ethnic discrimination can be explained by general mentality factors in bargaining situations. Is it possible to isolate economic ethnical discrimination effects triggered by dislike, sense of superiority, or distrust against ethnic minorities? If this is the case integration policies can be more effectively targeted at mentality factors. If not, the integration policy and the debate in media that often focus on discrimination issues can set itself free from a paralyzing issue and go on and deal with more important integration problems with renewed strength. Hence, we argue that the results from studies of this type can (in combination with other studies) have integration policy implications.

We have chosen to study five general mentality aspects that are considered to be important in bargaining situations. The first aspect concerns the degree of *trust*. It is fairly uncontroversial to argue that trust is an important aspect in many bargaining situations and in economic transactions in general (see e.g., Knack and Keefer, 1997, and Glaeser et al., 2000). Trust involves expectations towards the other party, which may help the bargaining process in many situations. The second aspect concerns *reciprocity* and it refers to the degree to which a bargaining party is willing to repay another bargainer's action. The third aspect concerns the *tendency to exclude* another party. In order to become a bargaining party it is obviously important not to be excluded from the outset. Hence, we study the tendency to exclude another party from forming coalitions. The fourth, aspect concerns the *degree of tolerance* to low offers. If a low offer from subjects with some characteristics are perceived as more provocative than offers from others, this is likely to have consequences in the bargaining process. The fifth aspect recognizes that bargaining involves both strong motives to coordinate and conflicts of interest. If for some reason (e.g., conventions or stereotypes) one bargaining party is considered to be *dominant* in relation to the other bargaining party, then this is likely to affect the bargaining outcome. Three different one shot games have been applied to capture these effects. To trigger potential ethnically conditioned behavior each player was told his or her co-players' names, which could be either typical Swedish names or Non-Swedish sounding names.

In the following we shall start out by some brief comments on the experimental method to study discrimination behavior in section 2. We then present the general design in section 3. In section 4, 5, and 6 we describe the one-shot games and the experimental results for each of the bargaining mentality aspects mentioned above.

2 Discrimination and the Experimental Method

The object of this study is twofold. First, as has been noted above, the aim is to study ethnic discrimination in Sweden. Secondly, the object is to develop the methods to study ethnic discrimination in general. The method to study behavior when the subjects receive a controlled signal of their co-player's sex or ethnicity is relatively new in studies of bargaining behavior.¹ As such, the present paper can be regarded as a complement to Fershtman and Gneezy's (2001) study (which henceforth will be referred to as FG) that concern one trust game, one ultimatum game and one dictator game. FG detect ethnic discrimination of an ethnic minority (i.e., so called Eastern Jews) in the trust game. However, no significant discrimination is obtained in the dictator game and in the ultimatum game the ethnic minority receives higher average ultimatum offers. Our study provides Swedish data for the trust game and provides also observations for two games that have previously not been studied under varying ethnicity signals, namely one coalition formation game with discriminatory exclusion possibilities and one Battle of the Sexes game. It is likely that the cause behind ethnic discrimination is multidimensional, we therefore think it is worthwhile to increase the numbers of aspects studied so forth.

The study of economic experimental discrimination behavior is an unexplored field. Inspired by e.g., Heckman's (1998) analysis Holm (2000b) discusses the experimental approach in conjunction with other approaches to study economic discrimination effects like e.g., regression analyses of official statistical data and audit studies. It is argued that the experimental approach has an advantage to the other approaches when it comes to controlling variables and therefore also in isolating general discrimination effects, but it has also a

¹ Clearly, face to face bargaining experiments have been conducted for decades. However, these situations have been considered uncontrolled (see e.g., Roth, 1995). It seems that Holm (2000a) and Solnick (1998) in the case of gender signals and Fershtman and Gneezy (2001) in the case of combined ethnicity and gender signals are among the first studies that have applied this method.

disadvantage in that the observations in experiments are done in an artificial decision situation. Hence, the experimental approach should be seen not as a substitute but as a useful complement to the other approaches.

It should also be noted that the economic experimental method have two advantages compared to sociological studies of attitudes towards ethnic minorities. First, in sociological studies subjects are usually asked to evaluate various statements about the minority, which means that the subjects are aware of the purpose of study and might act unnaturally or even politically correct. Secondly, in such studies there is no evident “cost” for the subjects to answer the questions in a way that is inconsistent to their true beliefs. Contrary to this, subjects in economic experiments have monetary incentives to act according to their beliefs.

At this stage, it seems that the experimental observations are too few to be seriously used to evaluate theories. However, it should be clear that experimental discrimination studies could have theoretical implications. In experiments (as opposed to in typical audit studies) the subject is placed in a rather general bargaining situation. This means that differences in bargaining behavior towards different ethnic groups are hard to explain by situation specific statistical discrimination theories as put forth by Arrow (1973). Furthermore, if discrimination is based on preferences for certain groups or characteristics as argued by Becker (1957), these preferences are in principle possible to observe in experimental behavior.

3 Design

In this section we shall describe the overall design of the experiment. We will start analyzing the principles that guided the sample selection and then will describe how the experiment was practically designed.

3.1 Sample Selection

The selection of subjects was not primarily guided by the goal of getting a representative sample for the Swedish population. Such an approach would demand a larger subject pool and a research budget of a different magnitude. Below we will present the main criteria that motivated the sample selection.

- 1. The subjects should be selected from regions where refugee immigration and integration problems have been significant.*

The refugee immigrants are not evenly distributed over Sweden. For natives that never or infrequently have interacted with immigrants the integration process can hardly start, which means that the behavior of such a group cannot explain integration problems in the long run.

The degree to which the integration process has been successful also differs largely between regions. This can be demonstrated by looking at the employment rates for a specific ethnic group in different regions. Ekberg (2000) found that while the employment rate in the most successful municipality was 86% (for male refugees from Bosnia) the corresponding employment rate for the same group was 14% in the municipality of Malmoe. When focusing on integration problems it is research economical (as a first step) to study them where they are present. Hence, we have chosen to make the experiment with subjects

from the city of Malmö in the southern part of Sweden. Immigration has been significant in this city and it is well known that the integration problems have been severe here.

2. The subjects should be able to understand the instructions and the strategic situations in the experiment.

The experimental situation may seem abstract and difficult to understand for some people. To reduce the probability that the subjects did not understand the instructions we chose individuals who are used to text descriptions containing abstract parts. This criterion was accomplished by choosing subjects that had completed two years on a theoretical program in senior high school.

3. The subjects should be accessible in sufficient numbers. A necessary condition for doing experimental research is that it is possible to get voluntary subjects in sufficient numbers. This is especially important in discrimination experiments since it is not advisable to use repeated interactions that are treated independently. Ethnic discrimination is a delicate matter to investigate and to observe natural behavior repetitions involving signals about ethnicity should be used with care; sooner or later the repetitions will reveal the more specific purpose of the experiment and this is likely to lead to politically correct behavior. To get a sufficiently large group of subjects 11 classes of last year students from three different senior high schools were made available to the experiment. In Sweden most of these students are 18 years old and this group can be regarded as a representative pool for those students who continue with studies at the university level.²

Two things should be said about this group. First of all, the subjects are young. This can be a problem since one can expect that an 18 years old student is less experienced

and less likely to have developed independent opinions on various issues compared to older groups. Furthermore, earlier Swedish sociological studies of attitudes have found that younger persons have more generous and less hostile attitudes towards immigrants compared to older persons (see e.g. Heyman et al., 1975). On the other hand young persons are more interesting to study since they are likely to be more receptive than older persons which means that the attitudes of this group may reflect many different groups' views that compete about their attention (i.e., parents, teachers, media etc). From this perspective the attitudes of the younger generation can be regarded as a sensitive concentrate of the surrounding society. Second, by choosing students following the theoretical programs we get a bias in the sense that those participating in the experiment have (compared to the average 18 year old Swede) higher grades, higher motivation in School, parents with higher incomes etc.³ From a future policy perspective the behavior of this group may be more interesting to study than a more representative one. The reason is that the selection of subjects is biased towards the group of citizens that have a higher probability of getting an influential position in the future society.⁴

3.2 The Practical Design

The experiment was based on verbal and written instructions and questionnaires. The sessions took place in the classrooms at the participating schools in October and November 1999. A session started either at the beginning or at the end of a class and included five minutes of verbal general information about the experiment and a period of 15 - 20 minutes where

² This means that with regard to future occupations and education, this group can be considered broader or more representative for the population than subject pools selected from specific university programs or business schools, which are not uncommon samples in experimental research.

³ While these biases should be noted they should not be exaggerated. The sample consisted both of classes with very good records and classes with a mediocre performance.

⁴ Although, it is clear that uneducated people also can hurt others by discriminatory behavior, the consequences are likely to be worse on average if a doctor, a managing director or a journalist exhibit discriminatory behavior.

subjects filled in questionnaires. In order to keep the subjects' behavior natural and to avoid politically correct biases the subjects were never informed that the specific purpose of the experiment was to study how signals of ethnicity affected their behavior. The subjects were informed that the study concerned experimental bargaining behavior.

Each subject received an envelope that contained five separate questionnaires. Each questionnaire described a strategic situation where the players were informed that he played against one co-player (in four situations) or two co-players (in one situation). The subjects were also informed that they probably did not know their co-players and that subjects from other classes and schools participated. Each one of the five strategic situations referred to one of the five mentality aspects discussed earlier.

On each questionnaire the name of the co-player was given. Actual matching between players and co-players could not be practically arranged.⁵ Instead a set of names was generated to fictitious co-players according to the following principles.⁶

Typical Swedish first and last names were given to the "Swedish co-players". The Non-Swedish names were selected from a pool of Non-Swedish students from a different school in Malmö and can be regarded as a random sample of the ethnical composition of immigrants in the city. In order to control for that not other aspects than ethnicity was reflected in the names both the Swedish and the Non-Swedish names contained 50% female names and the length of the names were about the same in terms of letters.

⁵ There were several reasons for this. One reason was time restrictions: we only got access to the subjects from their teachers for a limited time. Another important reason was that since immigrants are a minority actual matching would result in relatively few observations where a player had an immigrant co-player. Furthermore, the relatively low average turnout in some of classes, the necessity to signal a name, and the sequential structure of some of the games, would mean that actual matching would produce a substantial group of subjects that never got a response on their first period actions (e.g., their ultimatum offers). An additional consideration was that ethnical discrimination is illegal in Sweden. By using fictitious co-players the risk of inducing illegal behavior in the experiment was avoided.

⁶ This element of deception was conducted under considerations of the ethical guidelines of The Swedish Council for Research in the Humanities and Social Sciences. However, since many experimental economists use stricter guidelines than these we describe and discuss the deception technology used in this experiment in a note that is available from the author.

By participation in the experiment the subjects earned “points” that were counted as Swedish crowns (SEK). A subset consisting of about 5 percent of the subjects was randomly selected to get their points converted to real Swedish crowns.⁷ The expected after tax hourly experimental earning was about SEK 80 (corresponding to \$8.5) for each subject that participated, which is significantly higher than the average hourly after tax wage for this age group Sweden.⁸

4.Trust

It is commonly accepted that trust is very important in many bargaining situations. In negotiations characterized by trust verbal promises are taken seriously and handshakes may be a substitute to a signed contract when closing a deal. Clearly, bargaining in such an environment reduces the cost for control and monitoring. To focus on the specific trust aspect in bargaining we have chosen a game that have been experimentally studied in different variants by a number of different researchers.⁹ However, none of these except FG exposed the subjects to signals about the co-players ethnicity. To improve comparability between FG’s study on Israeli students and the present study, we have chosen a design that follows this as much as possible. This does not mean that the studies are identical. However, both studies apply the same game (developed by Berg, Dickhaut and McCabe, 1995); both use a questionnaire method; and both subject pools got roughly the same information.

In the trust games studied, a player A receives SEK 200.¹⁰ Player A can then choose to send a sum, denoted by x , between 0 and 200 to a co-player B. The sum received by

⁷ The information given to the subjects about their payoffs and the questionnaires are available from the author.

⁸ The group selected to get their points converted earned about \$50 on their 20 minutes of participation.

⁹ See e.g., Fehr, Kirchsteiger and Riedl (1993), Guth, Ockenfels, and Wendel (1994), Berg, Dickhaut and McCabe (1995), Fehr, Gächter, and Kirchsteiger (1997), and Fershtman and Gneezy (2000).

¹⁰ Note, that we refer to SEK, but to be precise the players received points that with a certain probability could be converted to real SEK after the experiment. Thus, by SEK we henceforth mean points.

B will be tripled. B can then choose to send back a sum, denoted by y , between 0 and $3x$. The players will then receive their payments, which means that A receives $200 - x + y$ and B receives $3x - y$.

Given that B returns a larger sum than initially sent ($x < y$) A can gain by sending B a sum. However, A and B do not know each other and cannot communicate. The only information about B that A get is his name which may be Swedish or Non-Swedish.

It should now be clear why the game is called a trust game; if A is confident that B reciprocates by sending back at least as much as initially received, then A will be more likely to send money to B than if he does not trust B. The subgame perfect Nash equilibrium for A is not to send any money and for B not to return any money. However, as we shall see below and which has been established before by for instance Berg, Dickhaut and McCabe (1995) this is not what subjects typically do in these games. A substantial proportion of the A and B players choose strictly positive amounts.

Results

We are interested in whether the degree of trust (revealed by the sums, i.e., x , sent by the subjects playing the A role) is contingent upon ethnicity. FG found a strong discrimination effect in that Israeli men (of both Eastern and Western origin) sent on average a much lower sum to B players with names indicating Eastern origin. If the B player's name indicated Western origin 76% of the initial sum was sent. However, B players with eastern names only received 40% of the initial sum. It should be noted that the discrimination effects were only detectable in male behavior and it was directed against males.

FG demonstrated that that the trust game is potentially sensitive for co-player ethnicity. The evidence from the Swedish study does however not produce any strong discrimination effects. When looking at the pooled data the A players sent on average (SEK)

96.7 when the name of the co-player was Swedish and the group receiving a Non-Swedish name sent 94.6. The differences are larger when we look at the average amount sent by the subgroups when these are categorized in terms of the subjects' gender and the co-player's ethnicity, which are presented in Table 1.

<i>Gender of subjects:</i>	males	females
<i>Treatment</i>		
Swedish names	105.2 (59)	88.4 (60)
Non-Swedish	98.3 (49)	91.6 (57)

Table 1. Average contributions in various subgroups. The numbers in parentheses indicate the number of subjects in each subgroup.

Like in FG it is primarily the males that exhibit relatively less trust towards the minority group.¹¹ Ethnicity matters less in the females' trust behavior. In fact females are on average more trusting towards the Non-Swedish group. FG also found that discrimination was primarily directed towards other males. This was also observed in this experiment; males sent on average 110.2 to Swedish males but only 98.6 to Non-Swedish males.¹² Hence, the relative magnitude of discrimination in various subgroups is consistent with the findings of FG in that the strongest discrimination effect is found in the male group and is directed towards other males. Although, this discrimination effect should be noted it is not of the same magnitude as the one found in the group of Israeli students and it is not statistically significant.¹³ To

¹¹ In an absolute sense males are more trusting; males sent on average 102.1 to their co-players whereas females only sent 90.2. This difference is interesting per se and may be related to other experimental studies. However, it is not significant. The null hypothesis (that the males' and females' trust levels come from the same distribution) cannot be rejected ($p = 0.14$ with a Wilcoxon/Mann-Whitney test).

¹² It should be noted that the gender signal of the co-player for those receiving a Non-Swedish co-player should be regarded as noisy since one can expect that the subjects might have problems in determining the gender of their co-players.

¹³ Using the standard non-parametric test for situations like this (i.e. the Wilcoxon/Mann-Whitney test) the significance level is $p = 0.70$ for the male group.

conclude, the results do not reveal any strong discrimination effect in trust behavior among the subjects.

4.1 Reciprocity

The subjects also had to make decisions as responders, i.e., they were taking the B role in the trust game. The fact that many of the subjects do return a substantial proportion of the amount sent to them and that the sum that is returned depends largely on the amount sent to them is usually considered to be a sign of reciprocity. Reciprocity has also been proposed by Fehr et al (1997) to be a conceivable contract enforcement device. Hence, if the degree of reciprocity in bargaining towards Non-Swedes differs from the degree exhibited towards Swedes this may produce discriminatory outcome.

To investigate this we let the subjects get the information that their fictitious co-players had sent a given amount to them. We let the majority of the subjects get the somewhat provocative information that their co-players had sent only one fourth of the initial sum (which was SEK 50). The rest of the subjects were given the information that their co-players had sent them one half of the initial sum (i.e., SEK 100).

Results

The average amount returned, y , was 44.6 for the subjects that received the provocative treatment and 96.5 for those receiving the lenient treatment. The various subgroups' behaviors are given in Table 2.

Gender of subjects: males females

Treatment:

Provocative (SEK 50)

<i>Swedish names</i>	46.0 (39)	46.7 (38)
<i>Non-Swedish names</i>	35.9 (22)	46.3 (32)

Lenient (SEK 100)

<i>Swedish names</i>	93.4 (19)	96.4 (23)
<i>Non-Swedish</i>	108.8 (28)	82.6 (24)

Table 2. Average amount returned (y) in the various subgroups. The numbers in parentheses indicate the number of subjects in each subgroup.¹⁴

There is an indication of a moderate discrimination effect in the male group that received the provocative treatment. On average these males returned about 20 percent more to the Swedes than to Non-Swedes. One might say that Non-Swedes were punished harder. On the other hand those males that got the lenient treatment rewarded the Non-Swedes more generously than the Swedes. Hence, one can interpret this as an indication that Non-Swedes have a steeper reward curve (for their degree of trust). However, these differences are not statistically significant and therefore only indications. There is not a corresponding tendency in the female group. Those receiving the provocative treatment did return about the same amounts to Swedes and Non-Swedes. Contrary to the males, females that received the lenient treatment rewarded Non-Swedes less.

¹⁴ The total number of subjects in this situation was 225. One subject did not fill in the questionnaire correctly.

5 Social Exclusion and Coalition Formation

In order to achieve economic equality it is important that various groups have the same possibility to participate in economic transactions or economic coalitions. If some groups are excluded from the gains of economic transactions because of their ethnicity, this will clearly be a disadvantage to these groups. A strategic situation that captures this aspect, but for other purposes, has been developed by Okada and Riedl (1999). In this game player A can choose between sharing a sum of money, x , between himself and *two* other players, B and C, or he can choose to share a somewhat smaller sum, y , between himself and *one* other player, B or C. In order for anyone to get any money at all, those invited to share the money with A must accept A's offer. Thus, after a coalition formation stage (where A chooses a subset of players to deal with) the two last stages in the game represent a one or two person ultimatum game. Okada and Riedl (1999) have conducted this experiment in both Austria and Japan and shown that the fraction of the subjects that choose to share with only one player is relatively large and depending on the relation between x and y .¹⁵

The exclusion game was modified in two important respects. First, since the focus of our study was ethnic discrimination, the co-players (i.e., B and C) were baptized with Swedish and Non-Swedish names. In the case player A chose to share with just one player he also had to choose which one to share with (and, thus, which one to exclude). Second, we also made a change in the rules of the game. In order to make it simpler for the player to discriminate even in the case where he shared with two persons, we only required

¹⁵ Okada and Riedl (1999) found that when $y \approx 0.93x$, 83 percent of the subjects choose to share with one person. The same figure was 51% for $y \approx 0.70x$.

that one of the two players accepted A's offer.¹⁶ In this case it is less risky for player A to choose to share the larger sum with two players.

Results

Two questionnaires related to this game, one where the subjects acted in the player A role and one where they acted as respondents to an ultimatum offer (i.e., as player B or C). In the perspective of ethnic discrimination, an interesting stage in the game is the coalition formation stage where A chooses co-player(s). Thus, we shall start to present the result for the subjects' behavior in this stage. In the experiment we let $x = 300$ (SEK) and $y = 280$ (SEK). We got data from 218 subjects. Forty percent of the subjects chose to share the sum with only one player.¹⁷ The average offer in this case was about 130.¹⁸ In the group that chose one co-player 56 had to exclude a co-player with a Swedish or a Non-Swedish name. It turned out that 57 percent excluded a Non-Swedish name and the rest (i.e. 43 percent) excluded the Swedish name. If we apply the binomial distribution we can reject the hypothesis that the probability for excluding a Swedish name is equal to the probability of excluding a Non-Swedish name at $p = 0.06$, which is close to significant. Hence, in this case the direction of exclusion behavior is consistent with the hypothesis of ethnic discrimination and close to statistically significant.

It is also important to check the level of the offers. From a game theoretic perspective one could hypothesize that a player believing that he can exploit a weaker party (e.g., a ethnic minority group) would choose to bargain with that weaker party and make a low offer in the following ultimatum game. Hence, this would be discriminatory behavior, but

¹⁶ This means that if the players mainly are motivated by their experimental payoff and considerations of reciprocity, then one would expect that many players would choose to share the larger sum (x) with two persons, but in such a way that only one of the players got a positive sum.

¹⁷ We excluded 8 subjects that did not fill in the questionnaire correctly.

¹⁸ The subjects offered on average 46% of the initial sum (i.e., 280). The corresponding average offer (for the same relation between x and y) was 43% in the study of Okada and Riedl (1999).

the weaker party would not be excluded. However, there are no notable differences between the average offer to a Swedish name and the average offer to Non-Swedish name.¹⁹ This result is in line with FG who did not detect any significant ethnically triggered differences in ultimatum offers.

Let us also comment on the behavior of the group that chooses to share the initial sum with two co-players. This was the choice of the majority. Although there was the possibility of spontaneous discrimination by sending a large sum to only one of the players, the most common offer was to send 100 to each of the co-players.²⁰ Only 9 subjects (or 4 percent of the subjects) chose to send different amounts to their co-players. Even if the majority in this subgroup gave the larger amount to co-players with Swedish names, the group is too small to legitimate any inferences.

5.1 The Degree of Tolerance

We also let the subjects make decisions as ultimatum responders, i.e., each subject faced a given ultimatum proposal. It is a well-known experimental fact that ultimatum responders turn down offers in a way that is difficult to reconcile with traditional game theory. Hence, there must be other factors (e.g., social, cultural or psychological) involved in this behavior that is not directly related to selfish monetary incentives. We wanted to check whether co-player ethnicity played a role in this behavior. Consequently, each subject was given one out of two

¹⁹ Both males and females offered slightly more to Non-Swedes. Females offered on average 127.1 to a Swedish co-player and 128.9 to a Non-Swede. Males offered 129.9 to Swedish co-players and 130.3 to Non-Swedish co-players.

²⁰ It should be mentioned that compared to the results of Okada and Riedl (1999) the fraction choosing the two co-players coalition is much higher in this study. These differences are likely to depend on the modifications done in the experiment and may have interesting implications for the interpretations of reciprocal behavior in this type of games. However, this question is a separate one that needs to be addressed with new experiments.

offers from a fictitious co-player; the provocative offer was SEK 50 of an initial sum of SEK 280 and the standard offer was SEK 100.

Results

The average acceptance rate (i.e., the fraction that accepted the offer) of those subjects that got the most provocative treatment was 0.603 when they had a Swedish co-player and 0.555 when they had a Non-Swedish co-player. This indicates a somewhat tougher (or less forgiving) attitude against Non-Swedes. However, when we look at the standard treatment the situation is reversed and the effect is stronger; the acceptance rates towards Swedes and Non-Swedes were 0.736 and 0.891.²¹ Hence, the pattern is similar to the one that we observed in male reciprocity behavior. Non-Swedes are not treated consistently unfavorably; they are more likely to be rejected (punished) when proposing a very low offer and they are more likely to be accepted (i.e. rewarded) punished when proposing a moderate offer. Although, this tendency is most clearly marked in the male group, it can be observed in both the male and female groups in Table 3.

<i>Gender of subjects:</i>	<i>males</i>	<i>females</i>
Treatment:		
<i>Standard (SEK 100)</i>		
<i>Swedish names</i>	0.586 (21)	0.852 (27)
<i>Non-Swedish</i>	0.865 (37)	0,917 (36)
<i>Provocative (SEK 50)</i>		
<i>Swedish names</i>	0.588 (34)	0.621 (29)
<i>Non-Swedish names</i>	0.500 (8)	0,600 (10)

Table 3. Average acceptance rates in the various subgroups. The numbers in parentheses indicate the number of subjects in each subgroup.²²

²¹ In this case the difference between the groups receiving the Swedish co-player name and the group that got a Non-Swedish name is close to statistically significant at the 5 percent level. A CHI2 test will reject that ethnicity does not matter at $p = 0.056$.

²² The total number of subjects in this situation was 202. In this questionnaire the subjects were also required to fill in their minimum acceptable offer. About 10 percent filled in this question in a way that was inconsistent

6 Coordination with Conflicts of Interest

In many bargaining situations, the parties may strictly prefer a coordinated outcome to an uncoordinated one. At the same time, there may be many conceivable coordinated outcomes (i.e., equilibria) and the parties' preference orderings over these may differ. In such situations there is no unique game theoretical prediction. However, as pointed out by Schelling (1960), it is conceivable that the parties use contextual information in bargaining situations. For instance if some bargaining party is considered to be dominant or the natural leader, his preferences may be decisive for the bargaining outcome. A game that captures this situation is the "Battle of the Sexes" game.²³

Each subject faced a Battle of the Sexes game in which subjects were to split SEK 300 with a co-player into unequal shares. In order to get some money the subject and his fictitious co-player had to choose shares without communicating so that the sum of their shares equaled 300. If the sum was either more or less both players received zero. The subjects could choose between two ways of splitting the money; the "hawkish" strategy that gives 200 to the subject (and 100 to the co-player), and the "dovish" strategy that gives 100 to the subject (and 200 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.

with their answer to the offer. We excluded these subjects. Note also that due to a sampling error; the subgroups receiving the provocative offer was more likely to have a Non-Swedish name than a Swedish name and the reverse was the case for the subgroup receiving the lenient treatment.

²³ In a series of experiments, Holm (2000a) demonstrate that when subjects get information about their co-players' sex in the "Battle of the Sexes game" the subjects (both male and females) behave more hawkish against female co-players than against male co-players.

Results

In all, 226 subjects participated in this part of the experiment. Half the subjects received a Non-Swedish co-player name and the other half got a Swedish co-player name. The proportion of the group that chooses the larger amount (i.e., 200) for themselves can describe the average degree of hawkishness in a subgroup. In the group that had the Non-Swedish co-player names, 53 percent choose the larger amount for themselves. The corresponding fraction was 54 percent when they had a Swedish co-player name. Hence, ethnicity of the co-player did not seem to affect average behavior in the subject pool. This conclusion is also valid for the male and female subgroups, which is demonstrated in Table 4 below.

	<i>Gender of subjects:</i>	<i>males</i>	<i>females</i>
Treatment:			
	<i>Swedish names</i>	55.6 (54)	54.2 (59)
	<i>Non- Swedish names</i>	53.7 (54)	52.5 (59)

Table 4. The percentage choosing the hawkish strategy (i.e., SEK 200) in the various subgroups. The numbers in parentheses indicate the number in each subgroup of subjects.

7. Concluding Remarks

The economic importance of successful future integration of refugee immigrants in Sweden (and in many other EU countries) can hardly be exaggerated. Despite the fact that there is a widespread suspicion in the media and among social scientists that one important obstacle to integration is ethnic discrimination in economic situations, research projects that systematically try to isolate discrimination effects and to disentangle components in discrimination behaviors are hard to find. This paper can be seen as an attempt to partly fill this gap by addressing the question with an experimental method.

The experiment was designed to study ethnical discrimination effects in bargaining situations. We conjectured discrimination in bargaining could have many sources and should therefore be studied in different strategic situations. Each subject encountered three different games; one trust game; one social exclusion and coalition formation game, and one battle of the sexes game. The results do not give any support for the existence of any general or strong specific discrimination effect. The strongest specific effects associated with the ethnicity variable were observed in the male reciprocity behavior and in the ultimatum response behavior. In these situations it seemed that Non-Swedes were harder punished for non-generous or non-trusting behavior compared to the Swedes. At the same time the rewards for generous and trusting behavior were higher for Non-Swedes than for the Swedes. One might say that Non-Swedes had a higher return on trusting and generous behavior. Another relatively strong specific effect was that subjects were more likely to exclude a Non-Swedish co-player compared to a Swedish co-player in the coalition formation game.

Given Fershtman and Gneezy's (2001) results that indicated strong trust specific discriminatory behavior among Israeli university students, the weak effects detected here is somewhat surprising. After all, the subjects were chosen from a region that is considered one of Sweden's most "problematic" ones with respect to integration problems. Although, more research is needed to explain whether the differences between e.g., Swedes and Israeli subjects are due to culture or something else in the experimental environment, it is difficult not to regard this result as something promising for the future prospects of the integration process in Sweden. It may be that the mentality aspect of ethnic discrimination is an exaggerated problem in Sweden, at least when it comes to the younger generation. Hence, for the group that participated in this experiment we dare to answer the question posed in the title of the paper by: "not much".

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