

Public Policy and Individual Labor Market Discrimination: An Artefactual Field Experiment in China

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Abstract

Labor market discrimination is frequently observed, but it is unclear what determines the basis of discrimination and in particular which role policy plays. We study discrimination based on the *hukou* system, segregating Chinese citizens in groups of migrants and locals in urban China. We hired household aids as participants in our artefactual field experiment on their natural labor market. We use a gift exchange game to study labor market discrimination using groups of either locals or migrants as employers and workers in our treatments. We find that official discrimination based on *hukou* status also implies individual-level discrimination. To identify whether discrimination is statistical or *taste*-based we introduce a new experimental game, the wage promising game, a variation on the gift exchange game with a cheap talk wage promise by the employer. Our data suggests that discrimination is *taste*-based: Status is exogenous for our participants, migrants and locals behave similarly and discrimination increases when potential reasons for statistical discrimination are removed.

Keywords labor market discrimination · artefactual field experiment · hukou
JEL classification J7 · C93 · P36

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1 Introduction

The importance of labor market discrimination for economic outcomes has been and continues to be an intensely debated topic in the academic literature, particularly because it can distort the labor market by decoupling productivity from wages paid. Discrimination, that is, a change in behavior towards groups of people with certain characteristics, exists in basically all societies. Some attributes that are a basis for discrimination are relatively obvious or at least have a natural foundation, such as height for a basketball player, others are arbitrary. When reasons for discrimination are arbitrary, it is rarely obvious what motivates discrimination on a behavioral level. This is particularly critical for understanding *taste*-based elements of discrimination (as introduced by Becker, 1971), in contrast to statistical discrimination.

While it is easy to think of a *taste* as intrinsic and exogenous evolving on a purely individual level, public policies and official institutions are also candidates for establishing discrimination based on tastes; for example, they could not only help to identify suitable cut-offs for statistical comparisons like a job certification or an academic degree, but could also give rise to artificial *tastes*. We study this impact of policies on individual-level discrimination in the labor market using a real, policy-induced attribute, the Chinese *hukou* status. The *hukou* system institutionally segregates the labor force into two types, locals and migrants. Being classified as a migrant deprives individuals of institutional “benefits”, i.e. (mainly) access to public housing, employment and education. We study how the presence of this system influences individually motivated *taste*-based discrimination in an environment where different status is assigned to seemingly identical individuals.

We use an experimental framework that allows us to identify discrimination based on *tastes* and based on statistical beliefs.¹ To study the discrimination of migrants, we hired household aids on their normal labor market in Nanjing for an artefactual field experiment (see Harrison and List, 2004, for a classification). In our experiment we capture discrimination by controlling *hukou* status, which is unchangeable and exogenous for our participants, by matching either groups of all locals and all migrants to the roles of employ-

¹Despite being independent by construction in reality *taste*-based and statistical discrimination can interplay through psychological dynamics; for example, receiving low wage offers that are perceived as unfair can cause disadvantaged workers to shirk, leading to a dual labor market where high and low reciprocity employer-worker equilibria are present (as could be reflected in the model by Akerlof and Yellen, 1990).

ers and workers in a gift exchange game (GEG). We also introduce a new game, the wage promising game (WPG) that - in conjunction with the GEG - allows us to disentangle statistical from *taste*-based discrimination: In the WPG employers first make a non-binding wage offers; the worker observes this offer and chooses her effort level; finally, the employer observes the effort choice and chooses the final wage she wants to pay. Thus, in the WPG lower effort cannot explain lower wages paid to migrants as wages are not based on beliefs.

In our experimental results we find that discrimination based on *hukou* status is observable when wage offers are binding. Discrimination further increases when non-binding promises can be made as migrants get promised similar wages, but get paid significantly less in their final wages for the same effort provided. In contrast to the finding that the status of the counterpart motivates discrimination, own status does not lead to differing decisions (e.g. based on an underdog identity, see Akerlof and Kranton, 2000), indicating that it is rather *taste*-based than statistical discrimination we observe. Hence, official segregation based on *hukou* status is not a reflection of statistical characteristics, but a motivator of *taste*-based discrimination. This observation is true although *hukou* status for our experimental subjects, who as we show are otherwise homogenous, is an exogenously imposed characteristic and the socio-economic group we are studying is effectively unable to change their status.

Our approach is closest to the study by Fershtman and Gneezy (2001) who show how stereotypes between students with different family history (Ashkenazic and Eastern Jews) can motivate *taste*-based discrimination using trust and dictator games. While Fershtman and Gneezy (2001) are studying arbitrary discrimination based on family history, we are particularly interested in the role of public policy, or official institutions that motivate discrimination.² In our analysis the state has attributed *hukou* status to our subjects exogenously almost as in a laboratory setting, like studied by Ball et al. (2001) who artificially induce status to laboratory participants.³ They find that lower status participants become more submissive in interaction. We differ from their approach when making use of a real public policy for our *treatment* when studying our subjects on their normal labor market for whom

²Additionally, we observe repeated interaction and we use the choice method instead of the strategy method. We find that learning does not reduce discrimination.

³Ball et al. (2001) give participants a performance task and then assign status; however, status is independent of performance in the task.

hukou status is exogenous and unchangeable, making them a particularly well-suited subject pool.⁴ By this we are able to study how a real institution leads to changes in motivations for discrimination and to disentangle statistical from *taste*-based discrimination by introducing the WPG.

Furthermore, we add to the understanding of the *hukou* system and its social consequences as described by Wang and Zuo (1999) and investigated more quantitatively in the subsequent literature, mainly using survey data (e.g. Démurger et al., 2009). We address this highly policy-relevant topic by employing an experiment that eliminates factors difficult to measure in surveys.

Our paper continues with a brief description of the *hukou* system. Section 3 describes our experimental design and our participants. We then present our experimental results and discuss their implications in the final section.

2 Segregation based on the *hukou* system

China's *hukou* or household registration system requires people to live and work only where they have official permission to do so. It was officially introduced in 1958 to regulate migration, particularly between rural and urban regions. Every citizen is to be registered as either agriculture (rural) or non-agricultural (urban) and *hukou* status is passed on from parents to their children. This classification leads to a separation within the city between locals (urban *hukou* individuals registered in the region of their work place) and migrants (individuals from an agricultural area, or another urban area). For almost two decades, holders of rural *hukou* were barred from working in urban areas. Since the beginning of economic reforms in the late 1970s, small numbers of rural migrants were allowed to enter the cities. As China's export and service industries began to grow rapidly in the 1980s and 1990s, increasing numbers of rural workers have been permitted to migrate to the cities to meet the demand for cheap labor. However, while migrants are allowed to work in the cities, their rural *hukou* status does not allow them access to many benefits that automatically accrue to urban *hukou* holders.

⁴Using non-student subjects in experiments is not completely uncommon in the literature (e.g. Gächter and Herrmann, 2010), but studying individuals in their naturally occurring environment and in field experiments is still relatively new (but see List, 2006; Maréchal and Thöni, 2007).

Originally *hukou* status was derived from birth place and was not negotiable (with very few exceptions). However, since the mid-1990s, this rigidity has been relaxed and some local governments can now grant urban *hukou* to holders of rural *hukou* who own stipulated levels of real estate or assets in the cities. Sometimes *hukou* status is also adapted as rural districts or land are integrated into an expanding city surface; in these cases people can be compensated for the loss of land through a change in their *hukou* status. Despite such relaxation, the dualistic structure has generally remained intact, as in practice very few rural migrant workers are wealthy enough to take advantage of this policy change and the effects of geographical incorporation are globally small. While the *hukou* system has continued to be dynamic, segregation based on this invariable status is still relevant for most (low income and low education rural) migrants, especially in economically attractive metropolitan areas.

Due to its social and economic consequences - particularly being cut off from public education, housing and employment, changing the *hukou* system has been in the political discussion as well as a topic of the economic literature. Wang and Zuo (1999) outline the rather precarious conditions that urban migrants live in, describing the situation of migrants such that “[m]ost rural migrants arrive in cities to take up marginal jobs that are characterized by long hours, poor working conditions, low and unstable pay, and no benefits - jobs which are unattractive to urban residents” (p.277). Wang and Zuo (1999) point out that migrants often work longer hours and work for lower pay - indicating that status and discrimination lead to changes in migrants’ behavior, which could blur statistics of income discrepancies between the social groups and make them appear more benign.

The literature has evaluated various aspects of the *hukou* system and has tried to get a clearer picture of its consequences. Liu (2005) estimates the value of an urban *hukou* to rural individuals to be between 2,741 Yuan to 45,654 Yuan,⁵ based on accessibility of quality education, better-remunerated jobs and a better quality of life. The major channel for differences is the attainability and price of education, which illustrates that the *hukou* system leads to a persistence of social inequalities, a result also reported in a simulation approach by Whalley and Zhang (2007).

Focusing on labor market outcomes, Lu and Song (2006) observe that

⁵At a fixed exchange rate of 0.1207 at the time this research was published this reflected values of USD 330 and 5,510 (between 22% and 55% of per capita GDP).

working conditions are to the detriment of migrants, mainly through fringe benefits that are unavailable to migrants. Démurger et al. (2009) show that unstable working conditions are prevalent among migrants and that significant differences in hourly wages exist. Limited access to certain jobs due to *hukou* status restrictions, hence discrimination, is a main contributor to these differences. Zhang (2010) makes the same point that institutional discrimination based on *hukou* status reduces the number of jobs available to migrants, increases their job search costs and the cost of losing jobs. Based on these search cost arguments, migrants stay in jobs longer than their counterparts with local *hukou* status and similar characteristics such as age and education. Hence, migrants remain in jobs with less desirable working conditions and wages, implying that status affects behavior. However, it is less clear in how far status also leads to changes in motivations (or preferences) for discrimination on an individual level.

3 Experimental design and participants

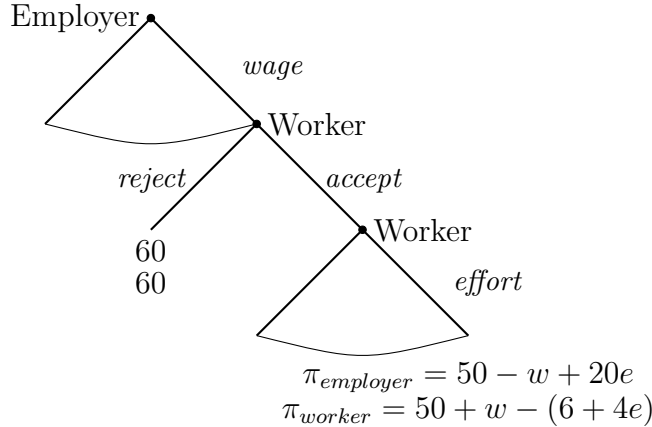
3.1 Experimental game and implementation

We used an experimental gift exchange game (GEG, Fehr et al., 1993) to look at potential differences in behavior of locals, i.e. those with local *hukou* status, and migrants, i.e. those with *hukou* status from outside of the city. The GEG is often understood as a stylized version of and interpreted similar to how Akerlof (1982) describes the relationship between employers and workers as an exchange of gifts.⁶ Interpreted in a labor market context, the game allows a first mover, the employer, to make a *wage offer* - a suggested transfer - to a second player, the worker. The worker can accept or reject the offer. In case the worker rejects, the game ends and both players get paid an outside option. In case the offer is accepted, the worker chooses a level of *effort* - a return transfer - which is costly to her but benefits the employer such that social surplus is increased. Figure 1 describes the sequential structure of the game.

For our experimental instructions, we used a labor market framing, describing the game to participants as the interaction between employers and

⁶ It can also be interpreted as a game to measure trust. General trusting attitudes between two individuals are often also studied using the more generic trust (or investment) game introduced by Berg et al. (1995).

Figure 1: Game tree of the GEG



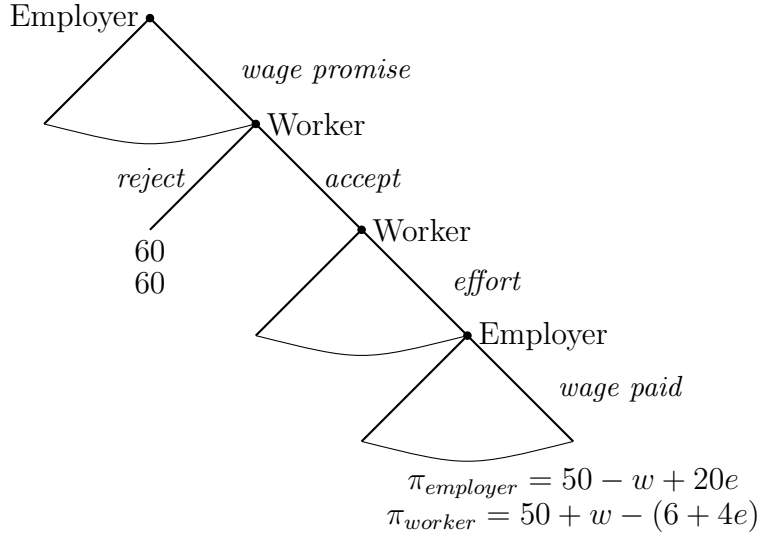
workers. Additionally, we used very simple and labor market specific language.

We also used a second game, the wage promising game (WPG). In this game the employer first makes a non-binding wage offer to the worker. Based in this offer the worker decides about accepting the offer. In case the worker rejects, the game ends and both players get paid an outside option. In the case the offer is accepted the worker chooses the level of effort. The employer observes the effort and then sets the (final) wage.⁷ We had two reasons for using this second design: First, it captures labor markets where workers are less protected, in particular where it is hard to ensure that wages are paid. Second, it allows us to differentiate between statistical and *taste*-based discrimination when comparing the results to those from the GEG as in the WPG employers know the effort of the worker before deciding about the wage; i.e., wages are not based on the workers expected effort (or trustworthiness, which we were informed about through the results in the GEG). The game structure of the wage promising game is illustrated in Figure 2.

We tried to keep the payoff functions for employers and workers simple and used values for the level of wages that could be understood as real wages paid in the labor market. I.e., wage offers and final wages had to be between 5 and 100 experimental dollars and in steps of 5. Returned efforts had to

⁷ This design, while (to our knowledge) being novel in the literature, relates to other experiments in which the first tangible decision has to be taken by the worker under uncertainty about their final remuneration (e.g., see Abeler et al., 2010; Rosaz, 2010).

Figure 2: Game tree of the WPG



be between 1 and 10 in steps of 1 in case the worker had accepted the wage offer. Our decision variables and payoff functions are similar to others in the literature (Gächter and Fehr, 2002). In both games the payoff for the employer as well as for the worker in case the worker rejected were $\pi_{employer} = \pi_{worker} = 60$. In case the worker accepted, the payoffs were (dependent on wages w and effort e):

$$\pi_{employer} = 50 - w + 20e \quad (1)$$

and

$$\pi_{worker} = 50 + w - (6 + 4e) \quad (2)$$

Both games have a unique subgame perfect Nash equilibrium (in pure strategies) in which both players receive the outside option: Wage payments will always be minimal as more than minimum effort, which is - out of equilibrium - the best response of workers, cannot be enforced (GEG) or because they are the optimal choice when paid *ex post* (WPG). Thus workers reject all offers.⁸ However, while this is the equilibrium in both games, we previous

⁸In the GEG wage offers above 10 are risky: If the worker accepts the offer and chooses the minimum effort (which is the least costly option for the worker), the employer would be worse off than by getting the outside option. For workers in turn it is only (potentially)

experimental evidence lets us expect positive reciprocity in both games. Besides preference to reciprocate, this behavior may also stem from concerns for efficiency -the mutual profit of employers and workers is increased by higher effort of the worker. Gächter and Fehr (2002) provide an overview of a large part of studies that make use of the GEG using various specifications. For the WPG we also expect that wage promises will be perceived as relevant, at least if employers avoid lying in the signals they send (as in Gneezy, 2005). Furthermore, guilt aversion (Charness and Dufwenberg, 2006) might play a role in decisions (but see Ellingsen et al., 2010). More discussion of the strategic nature of the variables is also included in the appendix.

The two games were played for 8 consecutive rounds each. The order of the games was changed between sessions. In each of the 8 rounds pairs of one worker and one employer were randomly rematched. From all 16 rounds, 4 rounds were randomly determined for final payoff. This was done to avoid wealth effects in the course of the experiment and to make periods independent in the sense that reputation-building should not play a role. Participants were assigned their role as either a worker or an employer at the beginning of the experiment and remained in this role for all rounds. We organized sessions controlling for *hukou* status of participants. We matched groups such that we had groups of 8 participants (= 1 session) which purely consisted of locals, groups that purely consisted of migrants, groups in which the employer role was always taken by a local and the role of the worker was always taken by a migrant as well as the reverse. Table 1 shows the number of participants in each constellation used in the experiment.⁹

In case the two *hukou* groups interacted, the status of the counterpart was explicitly communicated to participants on their computer screens. I.e., the instructions screen included a phrase saying that in the current session the role of the employer was always assigned to a person with local *hukou* and the role of the worker was always assigned to a person with migrant

profitable to accept wage offers of 20 or more - even if they choose minimum effort. In the WPG wage promises are just cheap talk, as they are not binding for final wage decisions. Any positive effort is hence risky for the worker compared to the safe option of rejecting.

⁹The small number of participants in the constellation migrant employer - local worker resulted from the elimination of 16 participants due to a mistake in seating them. As we did not have more participants of the required type available at the time, we did not rerun these sessions. We also did not play the game reversal in this constellation, as we had already collected plenty of information on the game order effect. Finally, we reduced the number of observations in this particular matching (and not in others), as it appeared to be the one with the least policy relevance for us.

Table 1: Participants by constellation

| | | Employer | |
|---------------|----------------|-----------------|----------------|
| | | <i>Local</i> | <i>Migrant</i> |
| Worker | <i>Local</i> | 80 (48,32) | 24 (24,0) |
| | <i>Migrant</i> | 96 (56,40) | 80 (32,48) |

The table shows the number of participants for each constellation. The numbers in brackets indicate for how many of these participants the GEG or WPG were played first, respectively.

hukou, or the corresponding reverse. While this information was included on the screen, it was not read out to participants together with the other descriptions, which the participants were asked to read along. The reason for this was to avoid demand effects greater than what would be observable in a real-life situation.¹⁰ The instructions to participants can be found in the appendix.

The experiment was done in a computer laboratory using experimental software z-tree (Fischbacher, 2007). We chose a computerized laboratory setting as it allowed us to collect data on interactions between participants in the two games relatively easily and to observe whether patterns of interaction and discrimination changed over periods. Using computers also facilitated employing the choice method, which seems desirable to us in a setting investigating a potentially emotional decision of discrimination.

The complete experiment lasted around 1.5 to 2 hours. Seating and instructing participants made up the largest part of this time.¹¹ Our conversion rate from experimental to local currency was 5 experimental dollars

¹⁰*Hukou* status is often not directly observable, but can easily be looked for on a person's identity card. Similarly, we provided information about status, but did not directly point to it, although participants knew that we previously checked their status and provided them with participant numbers based on their name. Hence, our treatment variable of whether participants were matched with a migrant or a local counterpart was only introduced by one sentence on the computer screen.

¹¹According to observations by the experimenters, these non-student participants did not need much more time for making decisions than students we let the experiment do in the weeks before in sessions organized to familiarize the local team of experimenters with conducting economic experiments. This local team of 6 students conducted the experimental sessions. Furthermore, at least one of the authors was additionally present at each of the sessions.

= 1 Yuan. Consequently, participants received between a minimum of 40 and up to 120, averaging around 80 Yuan - or 40 Yuan per hour - for their participation in the experiment. This compares to usual hourly pay between 15 to 20 Yuan.¹²

3.2 Experimental participants

We recruited household aids, so-called *ayis*, as our experimental participants from the city of Nanjing, a group that was particularly suitable for our study. In Nanjing this occupation is pursued by both locals and migrants, and the groups are comparable across *hukou* status; for example, cultural differences (e.g. in cooking styles or dialects) are relatively small for our sample. This, together with the fact that we are studying a low income group enables us to treat *hukou* status as exogenous.

We hired our participants through their normal labor market channels and based on usual hiring conditions for this type of workers. Some participants were also recruited through a local trading school.¹³ More detail on *ayis*, their labor market in Nanjing and our recruitment procedures is included in the appendix.

We ran experiments on 5 days with a total of 37 groups and 8 participants in each group. Sessions took place over the course of 7 weeks in November and December of 2010. Due to our requirements and the necessity to use groups of 8 participants, 26 participants (9%) were sent home after paying them a show-up fee. Due to a mistake in matching participants, 2 groups had to be dropped from the sample, leaving us with 280 participants of which 99% were females. We collected 2240 individual decisions for each of the two experimental games.

We corroborated our assumption of comparability across *hukou* status, which made household aids suitable as experimental subjects, using a computer-based questionnaire at the end of our experiment. Although we were unable to ensure that every participant completed the questionnaire, most participants answered most of the questions. Main summary statistics from the questionnaire are included in Table 2.

The data confirms our conjecture about comparability. Most variables are similar across the groups and differences are as expected. Migrants have

¹²Higher than regular market level payments were necessary, as we ran experiments on Sundays, where opportunity costs were higher.

¹³These individuals are identified with the variable “certificate” in later analysis.

Table 2: Summary statistics

| Variable | local <i>hukou</i> | | | migrant <i>hukou</i> | | |
|-------------------------------------|--------------------|--------------|----------------|----------------------|----------------|-----|
| | Mean | SD | N | Mean | SD | N |
| Age | 44 | 7.4 | 136 | 41 | 6.7 | 136 |
| Income | 1911 | 703 | 108 | 1982 | 578 | 123 |
| Weekly work hours | 35 | 23 | 108 | 37 | 26 | 124 |
| Rent per month | 276 | 301 | 107 | 398 | 314 | 114 |
| Employers | 3.7 | 2 | 108 | 3.8 | 2 | 124 |
| Fraction male | 0 | | 140 | 0.02 | | 140 |
| Fraction married | 0.98 | | 136 | 0.99 | | 136 |
| Number of children | 1.19 | 0.4 | 109 | 1.51 | 0.6 | 118 |
| Number of siblings | 3.6 | 1.5 | 111 | 4.02 | 1.8 | 123 |
| Party members | 0.04 | | 108 | 0.05 | | 124 |
| Job certificate | 45 | | 108 | 52 | | 124 |
| Education | | Freq. | Percent | Freq. | Percent | |
| <i>senior high school and above</i> | 45 | 33 | | 17 | 13 | |
| <i>junior high school</i> | 80 | 59 | | 77 | 57 | |
| <i>primary school</i> | 10 | 7 | | 42 | 31 | |
| <i>uneducated</i> | 1 | 1 | | 0 | | |
| <i>Total</i> | 136 | | | 136 | | |
| Changing employers | | | | | | |
| <i>frequently</i> | 7 | 6 | | 9 | 7 | |
| <i>occasionally</i> | 22 | 20 | | 25 | 20 | |
| <i>rarely</i> | 79 | 73 | | 90 | 72 | |
| <i>Total</i> | 108 | | | 124 | | |

more children, and have more brothers and sisters, reflecting general demographic factors within the Chinese population. We are studying a low-income and low-education group with a small fraction of party members. From the selection of *ayis* into our experiment, we might still draw on higher quality individuals in terms of education, as we required reading and writing abilities. However, this should not amplify the discriminatory effect we are studying.

The only critical difference between the groups is in terms of educational levels. This difference is best explained by generally lower levels of educational attainment in (migrant's original) rural areas. Table 3 provides support for this conjecture. We therefore regard the two groups as comparable in the sense that the level of education does not hint to deep-rooted (e.g.

ability-based) differences between the two groups. Nevertheless, levels of education in our subject pool could change the opportunity costs of our participants outside of the laboratory. If anything, our experimental approach mediates this effect and makes decisions of the two groups more comparable. Furthermore, we used a control variable on educational level when testing the robustness of our results in later analysis and found no strong effect (both statistically and economically). We conclude that the two groups are remarkably similar, making them suitable to be studied in an experimental study that draws on the notion that *hukou* status can be treated as an exogenous factor.

Table 3: General levels of education in urban and rural areas

| | Uneducated | Primary school | Junior high school | Senior high school or more |
|--------------|------------|----------------|--------------------|----------------------------|
| <i>Rural</i> | 10% | 38% | 42% | 9% |
| <i>Urban</i> | 3% | 17% | 35% | 44% |

Source: National Bureau of Statistics of China (2010)

4 Experimental results

4.1 Hypotheses

In the analysis of our experimental data we tried to investigate if discrimination against the migrant group is observable. We did so while disentangling statistical discrimination, hence treating the migrant group differently mainly because they react differently in the experiment, from non-statistical discrimination, which we understand mainly as *taste*-based discrimination or, as migrant status is originating from an (arbitrary) official institution, the exploitation of the lower social status of migrants. We also expected discrimination to be observable independent of whether the employer of the worker role is taken by the migrant.

We hypothesized that institutional discrimination of migrants motivates individual-level, *taste*-based discrimination. Our conjecture is that the mechanism for this is that official discrimination helps to establish a social perception that migrants are an inferior social group who can be treated worse and exploited more. As a result, in our experimental games we expected that

migrants get paid lower wages although they do not fundamentally behave differently from locals. If anything, they return more to their counterparts, as they are aware of *taste*-based discrimination against them and are used to over-compensate for this. We furthermore expected the games to help us identify what role statistical discrimination plays in decisions: Discrimination by employers in the gift exchange game could be partly driven by beliefs that migrants will return more, while in the wage promising game the wage decision is made *ex post*, meaning that statistical reasons cannot explain differences in discrimination.

As we assumed discrimination to be primarily *taste*-based, we expected discrimination of migrants to be lower for wage promises in the wage promising game and higher in the final wage decision compared to wage offers in the gift exchange game. Finally, if discrimination is mainly *taste*-based, we expect that giving less to migrants will be independent of the role taken in the experiment and that migrants also receive less effort when they are in the employer role. Furthermore, if statistical discrimination does not play a significant role, own status of employers and workers does not lead to significant differences in decisions depending on whether the decision maker is a migrant or a local.

4.2 Experimental data

The analysis of our data shows that discrimination of migrant counterparts is observable and that it is different between the games; furthermore, we also find that the *hukou* status of individuals has, if anything, only a weak effect on own decisions. Together these results indicate that official discrimination based on *hukou* status leads to individual-level discrimination which is *taste*-based rather than statistically motivated.

In order to come to these conclusions we analyzed our experimental data taking account of the structure of the data (distribution of the decision variables), the strategic nature of decision variables and the way we designed our treatments. For example, we expected conditional and reciprocal relationships (higher wage offers and wage promises are responded to by higher effort and higher effort by higher final wages); simple overview of the decision variables clearly confirm this conjecture.

As we used multiple periods of decisions in our games, we also checked for time effects (earlier and later periods) as well as for individual decision histories of our participants. Furthermore, we controlled for game order

effects (when the wage promising game was played first, wage offers, effort levels and final wages were approximately 10% higher). The main aspects are described below; more detailed information is included in the appendix.

Structure of the decisions variables

In the gift exchange game we find a high fraction of wage offers in the high and maximum category ($w \geq 70$). These wage offers are reciprocated by workers, leading to a large fraction of high efforts. Furthermore, there is a second peak of efforts in the rejection and minimum-effort region which to a large degree reflects low wage offers that cannot be responded with higher effort levels without incurring a loss to the worker (compared to the outside option of rejecting the offer). Some non-reciprocal (selfish payoff-maximizing) decisions are also observable.

In the wage promising game decision variables and reciprocal patterns are very similar to patterns in the gift exchange game. This indicates that the *cheap talk* character of the wage offer does not play a big role for either employers nor workers. Employers seem to be unwilling to break their promises and reciprocate high efforts with high final wages. However, as final wages are conditional on wage offers, the acceptance of the offer and on effort (both of which are themselves conditional) they require careful interpretation.

Table 4 provides an overview of the decision variables in the two games. They are separated by treatments and game order, as we found significant order effects in our analysis. Reading from these overview figures, a tendency of discrimination against migrants can be observed, although differences do not seem to be significant. However, it has to be taken into account that not all decisions are unconditional and we therefore proceeded to further analysis using more controls.

Having identified the general structure, we analyzed the experimental data with respect to our hypotheses in more detail. Reflecting the nature of our decision variables and structure of the data, we analyzed employer and worker decisions separately. In order to take account of the conditional nature of the variables and to get a better picture about possible patterns and dynamics of discrimination, we used (OLS) regression analysis. We chose our functional specifications reflecting the structure of the data as well as after considering various alternatives.¹⁴ Both for employer and worker decisions

¹⁴This included, for example structural models with a breakpoint at $w=20$, below which

Table 4: Summary statistics looking on the importance of the game order and the status of experimental counterparts

| | | GEG first | | | | WPG first | | | |
|-----------------|------------|--------------------------|-------------|----------------------------|-------------|--------------------------|-------------|----------------------------|-------------|
| | | <i>Local counterpart</i> | | <i>Migrant counterpart</i> | | <i>Local counterpart</i> | | <i>Migrant counterpart</i> | |
| Variable | | Mean | <i>s.d.</i> | Mean | <i>s.d.</i> | Mean | <i>s.d.</i> | Mean | <i>s.d.</i> |
| GEG | wage offer | 68 | <i>31</i> | 72 | <i>26</i> | 79 | <i>27</i> | 74 | <i>26</i> |
| | effort | 5.3 | <i>4.1</i> | 4.6 | <i>4.1</i> | 5.9 | <i>3.9</i> | 5.0 | <i>3.8</i> |
| WPG | wage offer | 70 | <i>31</i> | 72 | <i>25</i> | 83 | <i>25</i> | 76 | <i>24</i> |
| | final wage | 71 | <i>33</i> | 63 | <i>33</i> | 76 | <i>31</i> | 72 | <i>29</i> |
| | effort | 5.9 | <i>4.0</i> | 4.8 | <i>3.8</i> | 6.6 | <i>3.6</i> | 5.4 | <i>3.8</i> |

we considered specifications that included our main control variables and further information from our post-experimental questionnaire. These included information on income, work hours, rental payments, employer turnover, education level, age, marital status, gender and membership in the communist party.¹⁵ However, in our final specification we only included questionnaire variables that had been significant in prior specifications and did not reduce the sample too much. Including or excluding demographic controls did not significantly influence the effect of the treatment variables.

For both employers and workers we use binary treatment variables of playing with migrants and of being a migrant oneself (indicated by the variables Migrant employer and Migrant worker). Furthermore, we included binary variables for the order of the games (GEG first) and for the days on which the experiment was conducted, as we found that these had been significant in essentially all of our specifications. We report results from two different specifications for both employers and workers: One including our two treat-

workers should reject offers.

¹⁵As the summary statistics hint to a wide distribution for some of the questionnaire variables, we used (logarithmically) smoothed variables for income, hours worked, rental payments and the number of employers to mitigate the effect of outliers.

ment variables and a few controls,¹⁶ and one adding further controls that were to reflect individual-specific effects (i.e. lagged and first period decisions as an indicator of individual type) and learning about likely responses of experimental counterparts (their responses in the previous period). We conjectured expectations about reciprocal behavior of experimental counterparts to most likely be unconditionally revealed in the first period of a game. For this reason we included this first period decision in our regressions as an individual baseline for later decisions, although this meant that we had to exclude the first period from the regression analysis. Reflecting the potential importance of the first period, we also report the results of t-tests on our treatment variables using only the first period of a game.

4.3 Employer and worker behavior

Employer behavior

Table 5 shows that employers discriminate against migrants in both games. In the gift exchange game migrants are offered lower wages of approximately one to two units or, measured at the average wage offer, between 6% and 13% less than their local counterparts. In the wage promising game, discrimination in (*cheap talk*) wage offers becomes smaller than in the gift exchange game and loses statistical significance, indicating that offers to locals and migrants are similar. In contrast, discrimination is significantly higher in the final wage decision: Migrants receive more than two units of final wages less given the same level of effort, corresponding to about 18% less at the mean final wage. Hence, discrimination of migrants is clearly observable once the decision is tangible. Furthermore, the difference between the games increases when individual effects are taken into account.

In contrast to this result an individual's own *hukou* status did not lead to statistically significant differences in employer decisions. If anything, migrants made higher offers and paid higher final wages, allocating less of the mutual profit to the employer. However, as the statistical significance of own status was never above the 5% level in any other specification used, we avoid reading too much into this result.

¹⁶We used date and game order controls as they had a significant influence in our specifications before and we further added demographic controls when these had a significant influence in prior specifications.

Table 5: Regression analysis of employer decisions

| | Wage offer (GEG) | | Wage offer (WPG) | | Final wage (WPG) | |
|---------------------------|------------------|---------|------------------|---------|------------------|-----------|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | β | β | β | β | β | β |
| | (S.E.) | (S.E.) | (S.E.) | (S.E.) | (S.E.) | (S.E.) |
| Migrant worker | -9.57** | -3.65** | -6.42* | -2.42 | -11.82*** | -13.77*** |
| | (4.08) | (1.58) | (3.63) | (1.77) | (3.36) | (3.18) |
| Migrant employer | 7.38* | 1.25 | 0.77 | -2.30 | 4.83 | 9.70** |
| | (4.03) | (1.45) | (3.87) | (1.67) | (4.11) | (4.04) |
| Wage offer in t=1 | | 0.19*** | | 0.10** | | |
| | | (0.04) | | (0.04) | | |
| Wage offer _{lag} | | 0.59*** | | 0.61*** | | |
| | | (0.05) | | (0.06) | | |
| Effort _{lag} | | 0.15 | | 0.20 | | |
| | | (0.19) | | (0.22) | | |
| Effort | | | | | 3.04*** | 3.89*** |
| | | | | | (0.55) | (0.64) |
| Wage offer | | | | | 0.56*** | 0.51*** |
| | | | | | (0.06) | (0.08) |
| Final Wage in t=1 | | | | | | 0.23*** |
| | | | | | | (0.08) |
| Education | | | | | 0.80 | -2.16 |
| | | | | | (2.55) | (2.19) |
| Male | | | | | -19.30 | -27.55* |
| | | | | | (19.80) | (14.23) |
| Party member | | | | | 2.08 | -5.35** |
| | | | | | (4.22) | (2.67) |
| Day effects | Yes | Yes | Yes | Yes | Yes | Yes |
| Game order effects | Yes | Yes | Yes | Yes | Yes | Yes |
| <i>N</i> | 1120 | 980 | 1120 | 980 | 781 | 551 |
| <i>R</i> ² | 0.10 | 0.57 | 0.07 | 0.51 | 0.43 | 0.52 |

*** indicates statistical significance at the 1% level, ** at the 5% level and * at the 10% level

Putting more emphasis on unconditional decisions in the first period of

the game, we also performed t-tests of wage offers and final wages between treatments. While offers are higher when made to locals, we found no significant differences except for final wages, which were marginally significantly lower when made to migrants. This, together with the results reported in table 5 indicates that discrimination increases in the course of the experiment.

Furthermore, the results in table 5 indicate that *taste*-based discrimination seems to play a major role in the decision to discriminate: Discrimination in the wage promising game in terms of wage offers decreases when the wage offer is not binding for final payoffs any more, but increases in terms of payoff-relevant wages. This means that discrimination becomes stronger once it can be done more easily, showing that *tastes* seem to be a major motivator for paying migrants less.

The effects of our control variables are as expected. The wage offer in the wage promising game positively influences final wages, although from a strategic point of view being just cheap talk. Efforts provided to employers in the previous period have no significant impact on the wage offer, meaning that learning effects are small in the course of the game. While effort levels provided in the previous period play no or only a minor role in the gift exchange game, efforts provided in the wage promising game have a positive impact on decisions over final wages. Notably, the coefficient on this variable is just somewhat below the cost of an extra unit of effort (which would be 4), meaning that employers compensate workers (on average) close to their marginal cost for further units of effort.

Other control variables that had a potentially significant impact on decisions were the level of education, party membership and gender. However, education was only marginally significant in some specifications and its economic significance was low. Interpreting the role of party membership seems difficult as the effect is only based on 6 party members out of 140 individuals. A similar conclusion is true for the gender variable as there were only three male participants; for this reason we do not investigate these issues further here despite their high statistical and economic significance. Dropping these three variables from the estimations reported in Table 5 in order to increase the sample does not lead to qualitative changes our results.

Worker behavior

In a second step we looked at worker decisions. As can be seen in Table 6, playing with a migrant employer led to lower levels of effort provided. This

relationship was true for both the gift exchange game as well as for the wage promising game.¹⁷

Table 6: Regression analysis of worker decisions

| | Effort (GEG) | | Effort (WPG) | |
|---------------------------|--------------------|-------------------|--------------------|--------------------|
| | (7) | (8) | (9) | (10) |
| | β | β | β | β |
| | (S.E.) | (S.E.) | (S.E.) | (S.E.) |
| Migrant employer | -1.15** (0.57) | -0.83** (0.33) | -1.29*** (0.47) | -1.17*** (0.42) |
| Migrant worker | -0.42 (0.61) | 0.23 (0.29) | -0.65 (0.51) | -0.09 (0.44) |
| Wage offer | 0.06*** (0.01) | 0.05*** (0.01) | 0.06*** (0.01) | 0.05*** (0.01) |
| Effort in t=1 | | 0.26*** (0.04) | | 0.35*** (0.05) |
| Effort _{tah} | | 0.34*** (0.05) | | |
| Final Wage _{tag} | | | | 0.01 (0.00) |
| Age | 0.10*** (0.03) | 0.07*** (0.02) | 0.05* (0.03) | 0.03 (0.02) |
| Party member | -1.98*** (0.70) | -0.53 (0.45) | | |
| Day effects | Yes | Yes | Yes | Yes |
| Game order effects | Yes | Yes | Yes | Yes |
| <i>N</i> | 928 | 812 | 1088 | 807 |
| <i>R</i> ² | 0.28 | 0.52 | 0.30 | 0.44 |

*** indicates statistical significance at the 1% level, ** at the 5% level and * at the 10% level

The economic significance of the effect is noticeable and would correspond

¹⁷As for employer decisions, we also looked at first period decisions separately. Efforts provided to migrants were lower, but only insignificantly so (GEG) or marginally significant (WPG). This again indicates that discrimination of migrants increases towards later rounds.

to lower efforts of approximately 15% in the gift exchange game and approximately 19% in the wage promising game, measured on the average effort provided. Hence, again tastes are the major driver of discrimination also from a worker perspective and differences in trust, which would play a bigger role in the wage promising game, play a comparatively small role. In contrast to these results depending on the status of the experimental counterpart, the worker's own *hukou* status did again not change the effort provided to the employer in a statistically significant way. This insignificance, understood together with the result on employer decisions shows that statistical discrimination cannot explain why employers made lower offers to migrants, as own *hukou* status did not change the effort decision.

We included the wage offer to participants as our main control variable. The effect of the wage offer is significant and this effect is almost identical across games. Additionally, the response to wage offers is close to an equilibrium: To receive one more unit of effort, an employer would have to offer a wage increase of 20, which corresponds to the extra profit the employer would earn from this extra unit. Hence, employer offers are, on average, optimal given worker responses.

Using the same reasoning as for employer decisions, we also included age and party membership as further control variables, as these had proven significant for some of the specifications. However, due to the relatively low economic significance of age and the small number of party members, we have not further investigated them here.

5 Discussion

Further interpretations

In our experimental results we find that discrimination of migrant counterparts is observable both from employers as well as from workers, but that own *hukou* status does not lead to major changes in decisions. The implications of these results and their society-wide impact through the *hukou* system are less obvious. And what is the impact of the system on individual attitudes when different groups are dealing with each other in daily life? While the *hukou* system officially segregates Chinese citizens, official segregation does not automatically have to lead to discrimination on an individual level. Nevertheless, official segregation and discrimination might be interconnected; for

example, segregation could sustain an (already existing) propensity to discriminate and exploit a weaker social group if it legitimizes discrimination or provides groups of people with sufficient power to discriminate.¹⁸

As could be seen in our analysis, discrimination of the migrant group was observable from employers as well as from workers. This indicates that discrimination is independent of roles taken in labor market and based on factors such as lower preferences to cooperate with migrants, the conjecture that migrants deserve to be worse off, or the assumption that they are more submissive and content with smaller amounts. However, since migrants do not behave differently than locals, discrimination just leads to lower levels of reciprocity and less overall surplus available. Furthermore, the worse treatment of migrants does also not seem to rely on statistical beliefs, or lower trust in migrants, as discrimination again increased in the wage promising game when moving from binding and up-front wages to ex post wage payments after observing effort. Furthermore, our findings also suggest that participants seem to have adequate beliefs about likely (or maybe socially appropriate) reactions of others, as average decisions seem to follow equilibrium response patterns, which again suggests that *tastes* for discrimination, rather than statistical beliefs or different trust in one of the groups drives behavior.

In our experiment we investigated two different games that we chose to reflect different labor market contracts, one in which non-binding wage offers are made, and one in which wage offers are binding. Allowing for non-binding wage promises and allowing the employer to determine the final wage at the end of the round leads to higher effort levels at slightly lower wages. Cooperation between employers and workers remains high and even increases with the introduction of wage promises. The reason for this is that employers are able to use high wage offers as (true) positive signals. We find that locals and migrants do not react differently to the two institutional frameworks. However, discrimination of migrants significantly increases when non-binding promises become possible, as the weaker bargaining position of workers is exploited more when she is a migrant.

Another possible interpretation for what we observe is that we just observe different treatments of in-group versus out-group members. For exam-

¹⁸The necessity to reach a certain income level through a limited number of income channels available to the discriminated group could generate such a relationship when power is unequally distributed dependent on *hukou* status.

ple, Ruffle and Sosis (2006) report results from kibbutz members that are more cooperative amongst themselves, but not different from normal citizens once they interact with these other citizens. However, the *hukou* system segregates individual only on the basis of geographical characteristics and not on the wish to join a disadvantaged group, and for our experiments status was an essentially exogenous factor. Furthermore, there is no reason to discriminate, as locals and migrants are not fundamentally different. Nevertheless, the *hukou* system could still be an in-group coordination device helping individuals to determine the borders of their group. In any case, this still does not make the discrimination of migrants based on the *hukou* system desirable.

Limitations

While we have derived interesting results, our approach also has clear limitations. We identify the occupational group of household aids and study their behavior in relation to participant's *hukou* status. While this group did lent itself particularly well to being studied in our experiment, as being quite homogeneous across *hukou* groups and being unable to change their status, it evidently remains an open question in how far we can generalize from our results to the general population. For example, for this low socio-economic group discrimination based on *hukou* status is likely to be more central than for the average citizen. I.e., how participants that are more experienced in the topic is a context-dependent empirical question Hannan et al. (for example MBA students with work experience were more willing to cooperate than undergraduates in a study by 2002). Furthermore, *tastes* for discrimination can depend on context, as even experiments on the easily observable attribute gender shown mixed results depending on the experimental framework (Eckel and Grossman, 2008). We regard our artifactual field experiment as an insightful approach studying this policy-relevant question of the impact of the *hukou* system in this light, but we also tried to be more conservative with our conclusions and restrict our interpretations to the labor market.

While we framed experimental decisions such that the descriptions were more understandable for participants, a situation in which choices were communicated electronically between participants must have been unfamiliar to participants who are employed in physical jobs and often had little experience using computers. However, participants got through the experiment confidently and relatively quickly and the share of *strange* decisions is not

much higher than is often observable when studying student populations.¹⁹

Finally, our experiment only allows us to make statements about possible discrimination once employers and workers with different *hukou* status actually interact. However, discrimination might already take place earlier, for example at the hiring level. For example, Slonim and Guillen (2010) report experimental results indicating that discrimination mainly occurs when individuals chose their partner of interaction. In the labor market of household aids, such discrimination probably takes place as well; i.e., agents we asked about this stated that about one third of clients request to hire only locals.²⁰ We force individuals of different status to interact in our treatments, making it more difficult to predict in how far the behavior we observe can be representative of actual discrimination. Furthermore, migrants usually do not take an employer role in reality. However, we regard being able to control for this counterfactual as one of the advantages of running experiments, allowing us to observe attitudes that can be latent in reality.

Concluding remarks

We find that migrants are discriminated, although the experiment eliminates many factors that might be the basis for discrimination in reality and that cannot be captured in survey data, such as unobservable or perceived work quality. Our results indicate that discrimination is not based on different behavior of migrants, i.e. their *hukou* status does not seem to have grown roots into migrants identity. Hence, there is no reason for statistical discrimination. Furthermore, discrimination also does not seem to be founded on (potentially mistaken) beliefs about the trustworthiness of migrants - although lower trust in migrants might also play a small role, at least judging by the increase of discrimination from the worker side when moving to the wage promising game. However, we do not observe much updating of beliefs about reactions of migrants and locals: Weak statistical significance in discrimination in the first period of the game becomes significant when using all

¹⁹Apparently, the definition of *strangeness* in decisions can be arbitrary; however, most experimenters will know that some participants follow decision patterns which are hard to make sense of. In our game a high or maximum effort in response to a low or minimal wage offer would be a candidate for *strange* decisions.

²⁰Reasons for this are usually the hope of hiring more *civilized* workers and individuals that have similar preferences with respect to cooking styles; i.e. these are qualitative factors that we eliminated in our laboratory approach.

periods and past decisions of experimental counterparts do not have a significant influence on the decision variables. It hence seems that *taste*-based discrimination provides the best explanation for observed behavior.

Our two different labor market frameworks show that migrants are discriminated against and exploited more when employers can make non-binding wage promises. This increases the undesirability of the *hukou* system, as (generally) the introduction of non-binding wage promises increased efficiency in the sense of more mutual surplus being created for employers and workers - even when wages become binding in later periods, as employers and workers embark on a more cooperative path of positive reciprocity.²¹

Finally, we can try to use our experimental results to evaluate the *hukou* system. The system had long been used to limit migration to urban areas by making it less attractive for rural citizens to move and *hukou* status was used as the official vehicle to do this. However, our experimental results show that the system also seems to have created motivations of individuals to discriminate against the migrant group, probably by establishing a social perception that it is acceptable to treat migrants less kind. However, disadvantaging migrants can create contempt and frustration among migrants, who do not seem to be fundamentally different from locals; this frustration of migrants is unlikely to have been desired by the *hukou* system.

But will just abolishing the official *hukou* system lead to disappearance of discrimination? In a study using the Indian caste system, Hoff and Pandey (2005) found that eliminating available information on caste status (of children in an experiment) led to the elimination of behavioral differences. Similarly, Afridi et al. (2010) applied the same experiment to Chinese school children using the *hukou* system instead of information on casts and come to a similar conclusion. This gives rise to the assumption that official labeling of individuals through the *hukou* system is actually a driver (and not just a mirror) of discrimination. Our results support this view and give rise to the hope that abolishing the system will also mediate individual-level discrimination.

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²¹The game order effect had a strong positive effect on wage offers in both games, leading to higher wage offers throughout all periods when the wage promising game is played first.

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A Experimental Instructions

The following is a translated version of the experimental instructions for the case in which the gift exchange game was played first and in which locals were in the role of the employer and migrants in the role of the worker. The treatment is included in italics (here, but not on the original screen). Screens 1-3 always occurred in the order provided, while the game-specific screens were reversed between sessions.

A.1 Screen 1

Please enter your participant number.

A.2 Screen 2

Instructions part 1:

This is an experiment of experimental economics. Please read the note below before everything starts.

The note is very important, because your understanding of it and the choice you make will affect the final result of the experiment.

All the information in this experiment will remain confidential.

During the experiment, you cannot talk to any other participants.

All the decisions and choices you make are made anonymously and no one will know about the choice maker's identity, be it other participants or the monitors of this experiment.

Whenever you have any question, please raise your hand and do not communicate with other participants.

A.3 Screen 3

Instructions part 2:

In the current experiment, you will play a role as either employer or worker and the role you play will stick to you throughout the whole experiment.

In today's game, the Nanjing locals will keep playing the role of employer, and non Nanjing local will keep playing the role of worker.

There are 16 rounds in this experiment and in each round you will be randomly regrouped with another participant.

More specifically, in each round, every worker will meet a new employer and vice versa.

The amount of money shown on the computer screen is called experimental dollars, your income and payment will be calculated by those experimental dollars.

Your actual final reward will be affected by: 1. your decision, 2. the exchange from experimental dollars into RMB, 3. your total income in the 4 round of experiment randomly chosen by the computer.

The exchange rate between experimental dollar and RMB is:

100 dollars = 5RMB

A.4 Screen: Gift exchange game

The structure of the game

As is mentioned before, the experiment consists of 16 rounds of game. And there are 3 stages in every round.

Stage 1:

The employer will raise a salary proposal to the worker. The salary should be between 5 to 100 dollars.

Stage 2:

The worker will decide whether to accept the proposal.

If the proposal is rejected by the worker, then the round ends. And both employer and worker get the same amount of income which is 60 experimental dollars.

If the proposal is accepted, then the worker gets the salary in the proposal and pays 6 dollars as a fixed cost of the work.

Stage 3:

In this stage, the worker will choose the level of effort they made in the work from level 1 to level 10. The higher the level, the more efforts the worker makes.

The level of efforts will affect the income of the employer.

With 1 level increase in effort, the worker will bring 20 dollars income to the employer while the worker himself needs to sacrifice 4 dollars for the effort he makes.

Generally, if the worker accepts the proposal of the employer, the income of both sides in this round should be:

For the employer:

$50 - \text{salary} + 20 * \text{the level of effort the worker chose to make in the work}$

For the worker:

$50 + \text{salary} - 4 * \text{the level of effort the worker chose to make in the work} - 6$

A.5 Screen: Description of the wage promising game

In the last 8 rounds of game, the game will be a little bit different from previous games. Each round of game will be divided into 4 stages.

Stage 1:

The employer will raise a salary proposal to the worker. The salary should be between 5 to 100 dollars.

Stage 2:

The worker will decide whether to accept the proposal.

If the proposal is rejected by the worker, then the round ends. And both employer and worker get the same amount of income which is 60 experimental dollars.

If the proposal is accepted, then the worker gets the salary in the proposal and pays 6 dollars as a fixed cost of the work.

Stage 3:

In this stage, the worker will choose the level of effort they made in the work from level 1 to level 10. The higher the level, the more efforts the worker makes.

The level of efforts will affect the income of the employer.

With 1 level increase in effort, the worker will bring 20 dollars income to the employer while the worker himself needs to sacrifice 4 dollars for the effort he makes.

Stage 4:

The employer will be informed about the level of effort that the worker chooses.

The employer can change the amount of salary in the proposal according to worker's level of effort.

In other word, the employer does not have to pay the salary in the proposal of stage one. He can readjust the amount of salary. The salary should be between 5 to 100 dollars

A.6 Screen: Practice questions

In the experiment, please use the calculator on the right of the screen. Here is a little practice which will help you understand the game and the calculator

Question 1

If the employer proposed a 50-dollar salary, and the worker chooses level 2 of effort, then the income of both sides are:

Question 2

If the worker rejects the proposal:

Question 3

If the employer proposed a 100-dollar salary, and the worker chooses level 10 of effort, then the income of both sides are:

B Recrutement of participants

The subjects we recruited were housekeepers, or household aids in Nanjing, the provincial capital of Jiangsu, China, which had a population of around 7.7 million (in 2009).²² Housekeeping services comprise activities like cleaning, cooking or caring for elderly, children and pets. The housekeepers were, besides their *hukou* status deemed to be comparable, particularly with respect to the distribution of education, age and gender, making them suitable for an experimental study. As most migrant housekeepers in our study came from rural places within a distance of 5-6 hours drive from Nanjing and only few came from the inner or western (hence further distant) regions, cultural differences between our groups were relatively small. Focusing on this low-skill and low-income group also allowed us to mitigate the problem that wealth and education (through higher income) are the major ways to change status and obtain local *hukou*, resulting in a potentially causal relationship between *hukou* status and income or education. For the group of housekeeping subjects, status changes driven by income or education should be a negligible factor, which makes *hukou* status an exogenous label for our participants.

²²The housekeeping sector in Nanjing has a both local and migrant labor force. Historically, the housekeeping labor force was dominated by rural workers without local *hukou*. Since the late 1980s, more and more local workers joined the housekeeping service industry as state-owned enterprises laid off low-skilled or abundant workers during institutional reform or privatization. Among these unemployed workers, the low-skilled or aged women had difficulties in getting hired again in privatized enterprise or other business companies. As a result, many of these women stayed at home or worked as housekeepers.

We controlled for *hukou* status throughout the recruitment process of our participants. However, status was not itself part of our advertisement. We hired our participants on the regular labor market for housekeepers. Most housekeepers are self-employed and (or) represented through working agencies. An estimated number of over 1000 such agencies cover six urban districts as well as the suburban area, each typically representing around 100 *ayis*. *Ayis* in turn often seek employment through more than one channel, being represented by more than one agent as well as searching for job opportunities privately in their local community.

Making use of this infrastructure, we recruited participants using several channels. We collected contact information online, used local newspapers and contact information on blackboards on which housekeepers advertised their services.²³ Using these sources, we contacted agencies via phone and made an appointment with the agents if they agreed to. Some of the agencies doubted about the credibility and security of the experiment and refused to offer their help, probably afraid of leaking information on their *ayis*. To convince them we tried to meet agents in person and had an interview with them.

During the recruitment process, we also became aware of over 100 *ayis* taking training courses at a local trade college and made use of these participants as well. Those *ayis* had to take a paper exam and a practical exam on housekeeping service before receiving their qualification certificate. This certificate is not a requirement for housekeepers, but can be of additional merit. We were able to gather information about the *hukou* status of those *ayis* and their educational background, which allowed us to assess the required level of qualification. In order to use these helpers, we organized permission of the college to arrange experiments in the time between two exams. Once the helpers finished the experiment, they could go ahead taking their practical exam on housekeeping. Furthermore, before the experiment, we conducted a short interview with candidates to further eliminate unqualified *ayis*. Participants were also required to take a computer training test before entering into the test round of the experiment, which facilitated the process of the experiment.

To avoid agencies with overlapping pools of housekeepers, we located

²³The websites we used for our online recruitment were <http://nanjing.liebiao.com/jiazheng/> and <http://www.zhongguoyuesao.com/>. Helpful newspapers were Yangtze Evening Post and Modern Express; furthermore, some participants were recruited using university Blackboards.

agencies that were far away from each other. We particularly made use of two agencies from the *Qing Huai* and *Xuan Wu* districts that are at a 20 minutes driving distance. The two agents committed to the recruitment of migrant and local *ayis* for a commission fee of 10 Yuan for each qualified participant with literacy or 5-6 years schooling. This is a comparable fee to what other market participants pay for commissions. We asked agents to inform the candidate helpers of our requirement (i.e. literacy and information on their *hukou* status; moreover, we excluded helpers below 18 years of age) as well as about the payment opportunities. We guaranteed each participant a minimum payment for joining the experiment of 40 Yuan. Most of the *ayis* were motivated to join the experiment by a possible payoff up to over 100 Yuan based on their performance.

C Experimental data

The following section provides further information about the strategic meaning of the decision variables and the distribution and structure of our experimental data.

C.1 Strategic meaning of the variables

In both the gift exchange game and the wage promising game the first decision variable in each period is the wage offer by employers. However, the strategic importance of this offer is different between the two games. In the gift exchange game, the offer is binding and tangible. Knowing this, together with the fact that the incentive structure of the workers is such that no, or only minimal efforts should be returned when workers maximize their own payoffs, employers should offer wages of less than 25 (these would always be rejected) if they assume selfish workers. Higher offers only make sense if employers believe that workers do not only maximize their own payoff, but will share the mutual profit from higher wages in such a way that higher wages are also beneficial for the employer. This belief about a mutually beneficial response by the worker can be interpreted as *trust* in the worker's *trustworthiness*.

A large amount of the experimental literature indicates that participants *trust* each other to some degree and would predict that employers chose wages in the middle range, judging their experimental counterparts as having

a medium level of *trustworthiness*; this would imply wage offers mainly in the medium range. As we chose the parameters for our payoff functions in line with the literature, we would expect that we will also observe many decision in the medium range. Our results generally confirm these predictions and even show a higher willingness to trust workers. I.e., we find a surprisingly small fraction of wage offers in the low and medium category (wage offers ≤ 70) and a high fraction in the high and maximum category in the gift exchange game.

In the wage promising game the wage offer is not tangible as it does not determine final payments. In a sense it is just *cheap talk* and consequently any distribution of wage offers is equally reasonable, if workers interpret wage offers in the wage promising game as meaningless. The picture changes, however, if employers avoid lying about the final wages they are willing to pay (as results by Gneezy, 2005, had indicated). I.e., deviations from the wage promise would be (e.g. psychologically) costly and avoided by employers (also see Charness and Dufwenberg, 2006, about guilt aversion). As a result wage offers could serve as true signals to workers. The fact that wage offers in the wage promising game follow a very similar distribution as in the gift exchange game points out to such a relationship.

Worker efforts are the second decision variable in both games. In the gift exchange game the level of effort chosen is the back-transfer from the worker to the employer and can be interpreted as the worker's *trustworthiness*. The incentive structure for effort levels is also clear: If wage offers are below 25, they should always be rejected, as the outside option will be more valuable than accepting and providing the lowest possible effort. All higher offers should be accepted and the payoff-maximizing option is to return the minimum effort of one. However, if workers reciprocate more generous wage offers, they should respond to higher wage offers with higher effort levels. This motivates the assumption that any non-minimum efforts in the gift exchange game are conditional choices and we observe this conditional relationship in our experiment.

In the wage promising game the strategic meaning of efforts is different. As wage offers are not tangible, workers take the role of the trusting party when choosing the level of effort and should only (accept and) choose high effort levels believing that these will be reciprocated by employers. However, effort levels are again not necessarily unconditional, if the wage offer by the employer in the first stage is a signal containing true information. Indeed, if the (psychologically) binding effect of wage promises is strong, we might even

expect effort response levels that are similar to the ones in the gift exchange game.

We find that these (conditional) relationships are true and that effort chosen is very similar across games. That is, on average wage offers and efforts are positively related. This positive reaction pattern is qualitatively true for both games and also quantitatively surprisingly similar across games. Hence, somewhat reflecting the distribution of wage offers in the first stage, efforts are distributed such that there are many choices in the maximum range.²⁴ While there are only relatively few effort choices in the middle range, a second peak of efforts is in the rejection and minimum effort range. However, as efforts are conditional choices, rejections and low efforts in many cases simply reflect responses to low wage offers by employers.

In the wage promising game the final wage paid is the last decision variable. For a profit-maximizing employer paying minimum wages, irrespective of the efforts returned, is the always optimal strategy. However, this is not what is observed; i.e. high levels of reciprocity can be observed, as employers pay high (and often maximum) wages, although on average employers return slightly lower wages than promised in the first stage of the game and although the fraction of minimum wages increases compared to the gift exchange game. However, as final wage decisions are conditional on wage promises and returned effort, raw figures should be interpreted with care.

C.2 Wage offers

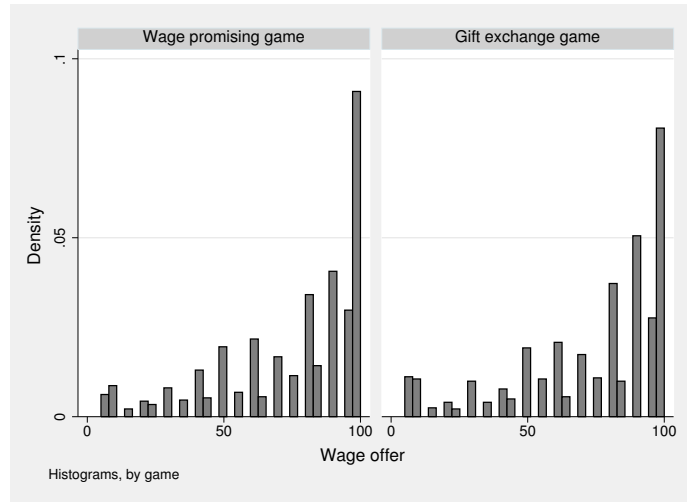
Figure 3 shows the distribution of wage offers in the two experimental games, illustrating the large amount of high wage offers and the similarity of distributions across games.

C.3 Efforts

Efforts chosen by workers are conditional variables; in the gift exchange game wage offers lower than 25 should be rejected, leading to an effort of zero. For higher wages the payoff-maximizing strategy is to provide minimum effort of one. In the wage promising game all offers should be rejected if the workers

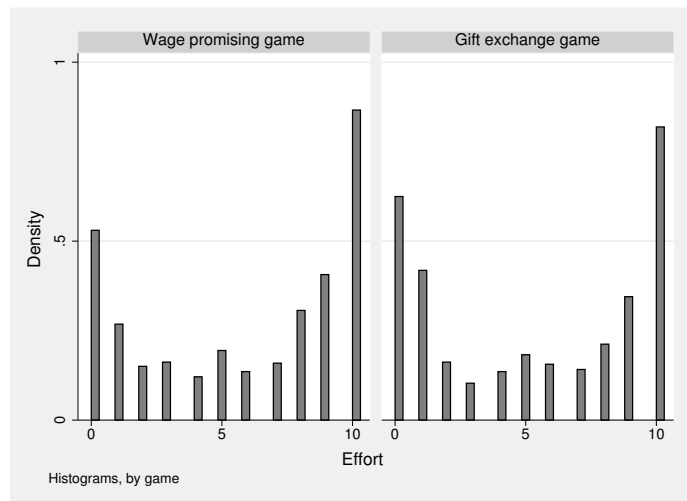
²⁴I.e., there is a noticeable fraction of rejections, which are recorded as an effort level of 0, a large number of minimum efforts and a high number of maximum efforts. This last aspect is particularly worth noticing as maximum efforts lead to an allocation of more than $\frac{1}{2}$ of the mutual profit to the employer, which is not commonly observed in experiments.

Figure 3: Histograms of Wage offers



do not believe that these offers have any meaning. However, our results show that relatively few offers are rejected, that there are few minimum efforts and that there is a large number of maximum efforts. Figure 4 illustrates this and also shows that the distribution of efforts is very similar across games.

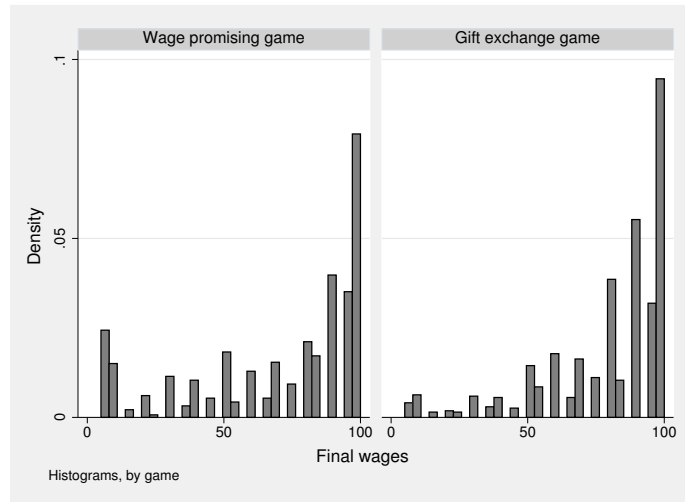
Figure 4: Histograms of returned efforts



C.4 Final wages

Final wages can only be changed in the wage promising game. Figure 5 provides a picture of the final wages paid by employers. It can be observed that high levels of positive reciprocity are observable and that high wages are paid in high fractions. However, the fraction of minimum wages increased between the games and the fraction of high and maximum wages decreased. This indicates that employers generally paid lower wages than they had promised. As the decision over final wages might be conditional on effort levels provided by the workers as well as on own wage offers in the first stage, these overview have to be interpreted with care.

Figure 5: Histogram of final wages

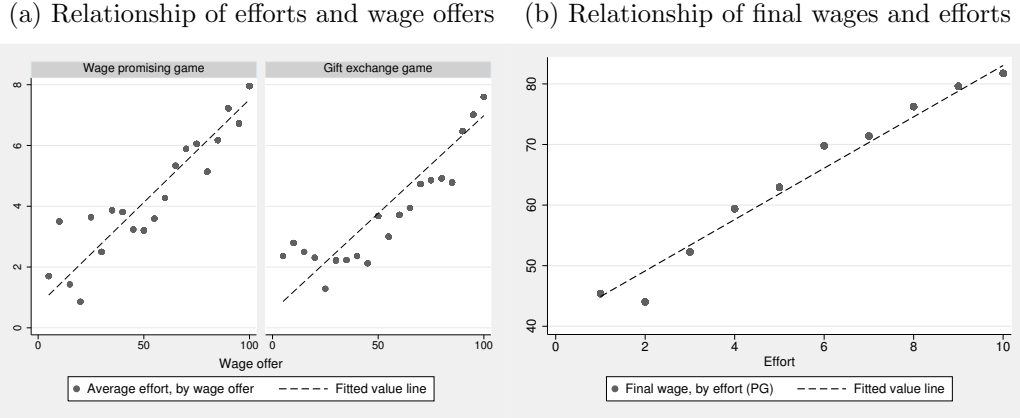


C.5 Interrelations

Throughout our analysis we assume that decision variables (wage offers, efforts and final wages) influence each other. This conjecture is sustained when looking at average decisions taken by our experimental participants, as can be seen in Figure 6. As can be seen in Figure 6a, on average higher wage offers are answered by higher effort levels in both games. While the relationship is not strictly increasing, a general relationship appears evident. Furthermore, the relationship between wage offers and returned efforts appears to be very similar for both games, indicating that workers interpret wage offers in the

wage promising game as quite meaningful - almost as if they were binding. The relationship between effort levels provided in the wage promising game and average final wages paid is shown in Figure 6b.

Figure 6: Simplified reaction patterns



C.6 Time effects

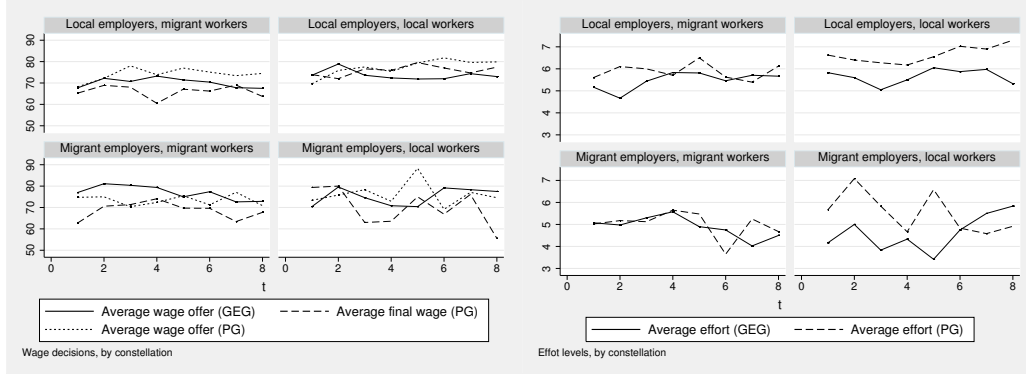
The experiment was designed such that all periods are independent and that reputation effects through repeated interaction would not play a role. This was done using random matching between experimental participants. Since participants were informed about this matching, there should not have been any reputation building effects. While we have no means of controlling if our participants understood that they would always be matched anew each period, we did not find any evidence for reputation-building effects in the data. Figure 7a and b show the developments of average employer and worker decisions over time and separated by our four treatment groups. We do not observe such time trends for any of the variables on an aggregate level, but we allowed for individual-level updating in our analysis.

Wage offers, effort levels and final wages are relatively stable over the periods of the game and only small deviations from the average decision in the previous period are observable. No trend is obvious. When having observed strong deviations in a period, average decisions revert to the overall mean within the next two periods. However, considering the fact that decisions are partly reactions to decisions by other individuals and an experimental

Figure 7: Development of average decisions during the games

(a) Employer decisions

(b) Worker decisions



history, it makes sense to control for effects of previous periods in the data when using statistical tools.

C.7 Game order effects

We reversed the order of the games to be able to control for ordering effects. Table 7 provides with an overview of average decision variables depending on the order of the games. Average levels of all variables were higher when the wage promising game was played first. Roughly speaking, in this case, wage offers, effort levels as well as final wages were all about 10% higher. Wilcoxon rank sum tests of differences depending on the order of the games report a significant difference at a 99% confidence level for all variables. Consequently, potential game ordering effects should be kept in mind for further analysis. However, the effect was not equally important for employers and workers, as the order of the games did not play very significant role in worker decisions. This means that higher levels of cooperation that were observable when the wage promising game was played first (as can be read out of Table 4) are driven by the higher willingness of employers to cooperate and make higher wage offers; this enabled employers and workers to embark on a more reciprocal, cooperative path. Back-translated to the context of a firm this would mean that the possibility to signal a positive, high-wage attitude to employees might induce higher worker efforts, which the employer is consequently willing to reward. This positive effect even persists when

moving back to a regime with binding wage offers.

Table 7: Summary statistics looking on the importance of the game order and experimental counterpart

| | | <i>Gift exchange game first</i> | | | <i>Wage promising game first</i> | | |
|---------------------|-----------------|---------------------------------|------------|----------|----------------------------------|------------|----------|
| | Variable | Mean | <i>SD</i> | N | Mean | <i>SD</i> | N |
| Gift exchange game | wage offer | 70.96 | <i>28</i> | 576 | 76.26 | <i>27</i> | 544 |
| | effort | 5.04 | <i>4.0</i> | 576 | 5.54 | <i>3.9</i> | 544 |
| Wage promising game | wage offer | 71.01 | <i>27</i> | 576 | 79.22 | <i>25</i> | 544 |
| | final wage | 65.86 | <i>33</i> | 467 | 74.21 | <i>30</i> | 473 |
| | effort | 5.55 | <i>4.0</i> | 576 | 6.11 | <i>3.7</i> | 544 |

C.8 Session Effects

In our analysis we made use of (dummy variable) controls for experimental date effects. Variables that controlled for experimental dates had a large effect on the levels of wage offers and final wages, i.e. particularly for the payoff-relevant decisions for employers. While we have no apparent explanation for the direction and size of these differences, their existence might be explained by the fact that our experiments were not all conducted at the same location; for example, on one day sessions were conducted at a trading school. Furthermore, the number of participants that came in on a given day varied from 8 to 88. Sessions on day 4 and 5, which show the largest deviation from baseline day 1, involved lower numbers of participants. Dropping them from the estimation did not invalidate the results reported in Table 5.

As controlling for the day of the experiment showed significant effects, we also tried controls for each session (i.e. each group of 8) in order to determine if we would need to be even more careful in our analysis. However, as we used individual-clustered standard errors we did not have sufficient degrees of freedom any more. Therefore, we dropped all insignificant demographic

control variables, which increased the sample as some of the participants had not answered all questions after finishing the experiment. This allowed us to make judgments about the usefulness of including controls for every session; it did not substantially improve our estimation. Therefore, we reverted to a model using only day controls.

D Further specifications

Different specifications for the wage offer in the gift exchange game

| | β (S.E.) | β (S.E.) | β (S.E.) | β (S.E.) |
|---------------------------|------------------|--------------------|-------------------|--------------------|
| Wage offer _{t=1} | 0.190*** (0.047) | 0.269*** (0.049) | 0.239*** (0.048) | 0.191*** (0.045) |
| Wage offer _{t-1} | 0.556*** (0.067) | 0.493*** (0.064) | 0.553*** (0.050) | 0.587*** (0.053) |
| Effort _{t-1} | 0.486** (0.211) | 0.347* (0.206) | -0.051 (0.187) | 0.146 (0.192) |
| GEG first | -0.809 (1.859) | -6.112** (2.613) | 0.181 (4.165) | -5.422*** (1.835) |
| To migrant | -3.350* (1.775) | -8.249*** (1.866) | 0.256 (9.059) | -3.655** (1.584) |
| Migrant | 0.077 (1.804) | 2.498 (2.023) | -1.465 (5.847) | 1.245 (1.450) |
| Income | -1.712 (2.268) | -1.365 (2.291) | | |
| Work hours | 0.403 (0.964) | 0.490 (1.087) | | |
| Rent | -0.525 (1.008) | -0.043 (1.114) | | |
| Employer number | -1.477 (1.195) | -0.978 (1.440) | | |
| Education level | -1.356 (1.399) | -0.277 (1.563) | | |
| Age | -0.089 (0.165) | -0.014 (0.158) | | |
| Marital status | 1.899 (2.534) | 2.211 (3.040) | | |
| Male | -0.961 (2.247) | -1.498 (2.775) | | |
| Party member | 0.446 (2.135) | -3.226 (2.773) | | |
| Date | | | | |
| Day 2 | | -4.318 (3.117) | -2.971 (5.131) | -4.201** (1.952) |
| Day 3 | | -12.157*** (4.005) | -3.195 (9.168) | -9.791*** (2.733) |
| Day 4 | | -32.323*** (5.750) | -16.396 (10.731) | -23.053*** (7.281) |
| Day 5 | | -13.541*** (3.270) | -1.589 (6.552) | -10.448*** (2.858) |
| Session | | | | |
| Session 2 | | | 1.829 (1.960) | |
| Session 3 | | | -2.631 (2.825) | |
| Session 4 | | | 0.844 (6.364) | |
| Session 5 | | | -3.758 (7.545) | |
| Session 6 | | | 1.052 (3.303) | |
| Session 7 | | | 5.794 (9.517) | |
| Session 8 | | | 5.740** (2.297) | |
| Session 9 | | | 4.091 (12.128) | |
| Session 10 | | | 7.874 (6.321) | |
| Session 11 | | | 14.333 (10.321) | |
| Session 12 | | | 7.187 (6.490) | |
| Session 13 | | | 2.368 (2.623) | |
| Session 14 | | | 0.904 (6.635) | |
| Session 15 | | | 3.826 (11.918) | |
| Session 16 | | | -1.624 (10.068) | |
| Session 17 | | | 3.754 (4.387) | |
| Session 18 | | | 7.605 (7.663) | |
| Session 20 | | | -6.813 (17.145) | |
| Session 21 | | | 2.999 (8.497) | |
| Session 22 | | | -3.452 (8.151) | |
| Session 23 | | | 2.396 (14.128) | |
| Session 24 | | | -0.153 (5.888) | |
| Session 25 | | | 4.971 (14.230) | |
| Session 28 | | | 1.966 (10.819) | |
| Session 29 | | | -14.998 (10.014) | |
| Session 32 | | | -14.741** (6.755) | |
| Session 36 | | | 5.951 (3.602) | |
| N | 637 | 637 | 980 | 980 |
| R ² | 0.493 | 0.515 | 0.582 | 0.565 |

Different specifications for the wage offer in the wage promising game

| | β (S.E.) | β (S.E.) | β (S.E.) | β (S.E.) |
|---------------------------|------------------|--------------------|--------------------|------------------|
| Wage offer _{t=1} | 0.119** (0.052) | 0.170*** (0.054) | 0.131*** (0.046) | 0.098** (0.044) |
| Wage offer _{t-1} | 0.570*** (0.069) | 0.524*** (0.070) | 0.537*** (0.058) | 0.614*** (0.059) |
| Effort _{t-1} | 0.329 (0.329) | 0.208 (0.302) | 0.050 (0.232) | 0.201 (0.223) |
| GEG first | -4.838** (2.121) | -7.041** (2.758) | -5.931 (6.338) | -4.583** (2.155) |
| To migrant | -3.314 (2.059) | -8.448*** (2.398) | 14.180 (10.409) | -2.415 (1.771) |
| Migrant | -2.955 (2.219) | -0.290 (2.504) | 1.566 (7.182) | -2.300 (1.665) |
| Income | -2.214 (2.412) | -2.657 (2.626) | | |
| Work hours | 0.666 (1.059) | 1.431 (1.267) | | |
| Rent | -1.144 (1.397) | -0.509 (1.532) | | |
| Employer number | -0.056 (1.672) | 0.630 (1.924) | | |
| Education level | -0.975 (1.680) | 0.279 (1.940) | | |
| Age | -0.287 (0.179) | -0.191 (0.149) | | |
| Marital status | 0.042 (2.481) | -0.610 (2.672) | | |
| Male | -0.665 (2.887) | 1.481 (3.573) | | |
| Party member | 0.564 (3.798) | -2.137 (4.312) | | |
| Date | | | | |
| Day 2 | | -0.620 (3.294) | -2.639 (10.690) | -0.238 (2.144) |
| Day 3 | | -3.904 (4.081) | -6.396 (11.873) | -1.912 (3.130) |
| Day 4 | | -34.428*** (6.489) | 1.858 (11.659) | -11.880* (6.513) |
| Day 5 | | -11.602*** (4.012) | 9.698 (6.971) | -3.787 (3.153) |
| Session | | | | |
| Session 2 | | | 6.402 (5.210) | |
| Session 3 | | | -7.412 (6.452) | |
| Session 4 | | | -12.387 (9.771) | |
| Session 5 | | | -5.793 (9.582) | |
| Session 6 | | | 3.401 (5.712) | |
| Session 7 | | | 20.418* (12.286) | |
| Session 8 | | | 6.105 (5.268) | |
| Session 9 | | | 24.015 (16.402) | |
| Session 10 | | | 1.339 (11.696) | |
| Session 11 | | | 23.861* (13.381) | |
| Session 12 | | | -6.093 (11.491) | |
| Session 13 | | | -2.555 (7.868) | |
| Session 14 | | | -9.411 (11.891) | |
| Session 15 | | | 20.131 (16.491) | |
| Session 16 | | | 1.327 (12.522) | |
| Session 17 | | | 0.030 (8.841) | |
| Session 18 | | | 0.321 (11.810) | |
| Session 20 | | | 19.116 (22.106) | |
| Session 21 | | | 1.880 (9.351) | |
| Session 22 | | | 1.371 (7.909) | |
| Session 23 | | | 20.701 (16.472) | |
| Session 24 | | | 4.768 (3.557) | |
| Session 25 | | | 21.913 (16.728) | |
| Session 28 | | | -16.198 (12.285) | |
| Session 29 | | | -10.286 (12.109) | |
| Session 32 | | | -19.194*** (6.904) | |
| Session 36 | | | -1.265 (4.240) | |
| N | 637 | 637 | 980 | 980 |
| R ² | 0.474 | 0.499 | 0.537 | 0.507 |

Different specifications for the final wage decision in the wage promising game

| | β (S.E.) | β (S.E.) | β (S.E.) | β (S.E.) |
|---------------------------|--------------------|--------------------|--------------------|--------------------|
| Final wage _{t=1} | 0.259*** (0.089) | 0.253*** (0.085) | 0.261*** (0.093) | 0.235*** (0.076) |
| Wage offer | 0.513*** (0.122) | 0.482*** (0.099) | 0.484*** (0.082) | 0.514*** (0.083) |
| Effort | 3.830*** (0.845) | 3.848*** (0.824) | 3.951*** (0.658) | 3.891*** (0.640) |
| GEG first | -0.627 (4.407) | -10.572* (5.435) | -14.841* (8.555) | -9.984** (4.304) |
| To migrant | -7.200 (4.337) | -13.065*** (3.761) | -2.823 (12.109) | -13.766*** (3.183) |
| Migrant | 1.070 (4.531) | 8.468* (4.391) | 19.515** (9.726) | 9.699** (4.038) |
| Income | 8.278 (7.676) | 10.042 (6.861) | | |
| Work hours | 0.348 (2.453) | 0.781 (2.471) | | |
| Rent | 5.766* (3.114) | 6.095** (2.678) | | |
| Employer number | -3.564 (2.856) | -0.952 (3.313) | | |
| Education level | -6.037* (3.216) | -5.412* (3.191) | -1.159 (3.444) | -2.165 (2.194) |
| Age | 0.082 (0.428) | -0.043 (0.389) | | |
| Marital status | -1.785 (8.444) | 3.322 (5.532) | | |
| Male | -32.115** (15.433) | -28.768* (15.186) | | -27.553* (14.230) |
| Party member | -5.957 (6.094) | -10.686** (4.763) | -5.295* (2.804) | -5.355** (2.674) |
| Date | | | | |
| Day 2 | | -18.421*** (5.708) | -30.270** (14.776) | -15.377*** (4.464) |
| Day 3 | | -21.575*** (7.294) | -42.015** (19.513) | -19.691*** (6.263) |
| Day 4 | | -5.100 (12.638) | -10.683 (17.812) | -13.247 (16.470) |
| Day 5 | | -32.607*** (5.004) | -31.946*** (6.737) | -26.318*** (4.496) |
| Session | | | | |
| Session 4 | | | -16.335 (10.550) | |
| Session 5 | | | -17.455 (10.877) | |
| Session 5 | | | -3.352 (3.933) | |
| Session 7 | | | 5.535 (12.049) | |
| Session 8 | | | -6.029 (6.398) | |
| Session 9 | | | 28.240 (22.859) | |
| Session 10 | | | 6.410 (20.779) | |
| Session 11 | | | 12.001 (16.759) | |
| Session 13 | | | 10.590 (12.739) | |
| Session 14 | | | -1.374 (13.092) | |
| Session 15 | | | 30.653 (22.597) | |
| Session 16 | | | -12.954 (17.118) | |
| Session 17 | | | 10.650 (12.124) | |
| Session 18 | | | 6.058 (15.188) | |
| Session 22 | | | 12.996 (16.656) | |
| Session 24 | | | 9.584 (8.399) | |
| Session 25 | | | 25.570 (22.377) | |
| Session 28 | | | -3.939 (22.126) | |
| Session 29 | | | 19.059 (12.273) | |
| Session 36 | | | -28.685* (15.390) | |
| N | 429 | 429 | 551 | 551 |
| R ² | 0.491 | 0.547 | 0.555 | 0.518 |

Different specifications for the effort decision in the gift exchange game

| | β (S.E.) | β (S.E.) | β (S.E.) | β (S.E.) |
|-----------------------|------------------|------------------|-------------------|------------------|
| Effort _{t=1} | 0.240*** (0.053) | 0.242*** (0.053) | 0.280*** (0.049) | 0.255*** (0.044) |
| Wage offer | 0.049*** (0.008) | 0.047*** (0.008) | 0.057*** (0.007) | 0.051*** (0.007) |
| Effort _{t-1} | 0.387*** (0.061) | 0.372*** (0.059) | 0.295*** (0.052) | 0.337*** (0.050) |
| GEG first | 0.173 (0.308) | -0.145 (0.380) | 1.332*** (0.345) | -0.142 (0.346) |
| To migrant | -0.769** (0.358) | -0.760** (0.372) | -1.416* (0.825) | -0.833** (0.325) |
| Migrant | 0.080 (0.365) | -0.244 (0.380) | -1.802* (0.923) | 0.227 (0.286) |
| Income | -0.282 (0.243) | -0.430 (0.289) | | |
| Work hours | 0.316* (0.175) | 0.337* (0.174) | | |
| Rent | 0.175 (0.176) | 0.175 (0.183) | | |
| Employer number | 0.194 (0.280) | 0.194 (0.282) | | |
| Education level | -0.058 (0.291) | -0.193 (0.323) | | |
| Age | 0.061* (0.031) | 0.057* (0.032) | 0.068*** (0.021) | 0.065*** (0.021) |
| Marital status | 1.135 (0.689) | 1.027 (0.675) | | |
| Party member | -0.703 (0.521) | -0.909* (0.733) | -0.672 (0.488) | -0.530 (0.454) |
| Date | | | | |
| Day 2 | | -0.721 (0.582) | 2.498** (1.061) | -0.118 (0.443) |
| Day 3 | | -0.639 (0.713) | 2.828* (1.510) | -0.544 (0.615) |
| Day 4 | | -1.810** (0.871) | -0.789 (1.427) | -1.269* (0.746) |
| Day 5 | | -0.866 (0.681) | -0.388 (0.868) | -0.213 (0.595) |
| Session | | | | |
| Session 4 | | | 1.359 (1.937) | |
| Session 5 | | | 1.334 (1.379) | |
| Session 6 | | | 1.369* (0.769) | |
| Session 7 | | | -1.007 (1.330) | |
| Session 8 | | | 1.900* (1.139) | |
| Session 9 | | | -4.648*** (1.289) | |
| Session 10 | | | -0.410 (1.215) | |
| Session 11 | | | -2.046 (1.256) | |
| Session 12 | | | 1.372 (1.351) | |
| Session 13 | | | -1.304 (0.927) | |
| Session 14 | | | -1.139 (1.158) | |
| Session 15 | | | -4.100*** (1.429) | |
| Session 16 | | | -1.636 (1.318) | |
| Session 17 | | | 1.319 (1.026) | |
| Session 18 | | | 0.170 (1.252) | |
| Session 22 | | | -0.662 (1.296) | |
| Session 24 | | | -0.792 (1.493) | |
| Session 25 | | | -3.300 (2.141) | |
| Session 28 | | | 3.000*** (1.045) | |
| Session 29 | | | 1.481 (1.461) | |
| Session 36 | | | 1.708*** (0.390) | |
| N | 602 | 602 | 812 | 812 |
| R ² | 0.545 | 0.548 | 0.558 | 0.522 |

Different specifications for the effort decision in the wage promising game

| | β (S.E.) | β (S.E.) | β (S.E.) | β (S.E.) |
|---------------------------|---------------------|---------------------|---------------------|---------------------|
| Effort _{t=1} | 0.316*** (0.064) | 0.277*** (0.065) | 0.360*** (0.060) | 0.349*** (0.053) |
| Wage offer | 0.050*** (0.007) | 0.049*** (0.008) | 0.053*** (0.007) | 0.050*** (0.007) |
| Final wage _{t-1} | 0.013** (0.006) | 0.011** (0.005) | 0.007* (0.004) | 0.007 (0.004) |
| GEG first | 0.658 (0.455) | 0.348 (0.671) | 1.973** (0.845) | 0.186 (0.487) |
| To migrant | -0.299 (0.537) | -0.405 (0.534) | -2.968** (1.351) | -1.167*** (0.423) |
| Migrant | 0.096 (0.601) | -0.222 (0.849) | -0.567 (1.590) | -0.094 (0.441) |
| Income | 0.399 (0.441) | -0.342 (0.487) | | |
| Work hours | 0.364 (0.278) | 0.326 (0.269) | | |
| Rent | 0.341 (0.266) | 0.418 (0.265) | | |
| Employer number | -0.009 (0.432) | 0.167 (0.463) | | |
| Education level | 0.731 (0.455) | 0.654 (0.479) | | |
| Age | 0.076 (0.046) | 0.088* (0.049) | 0.047** (0.023) | 0.031 (0.024) |
| Marital status | 1.457 (1.333) | 0.719 (1.441) | | |
| Party member | -0.161 (0.612) | -0.307 (0.654) | | |
| Date | | | | |
| Day 2 | | -0.023 (0.912) | 1.864 (1.457) | -0.141 (0.581) |
| Day 3 | | -0.322 (1.002) | 3.944* (2.121) | 0.185 (0.651) |
| Day 4 | | -4.266** (1.718) | -0.915 (1.908) | -3.712*** (0.947) |
| Day 5 | | -0.492 (1.298) | 2.901*** (1.054) | 0.096 (0.680) |
| Session | | | | |
| Session 2 | | | 0.572 (1.412) | |
| Session 4 | | | 3.568* (2.111) | |
| Session 5 | | | 3.115 (2.049) | |
| Session 6 | | | 2.510** (1.065) | |
| Session 7 | | | 1.844 (1.808) | |
| Session 8 | | | 1.684 (1.165) | |
| Session 9 | | | -1.456 (2.415) | |
| Session 10 | | | 0.735 (1.964) | |
| Session 11 | | | 0.739 (1.772) | |
| Session 12 | | | 3.420** (1.627) | |
| Session 13 | | | 0.534 (1.144) | |
| Session 14 | | | 1.598 (1.754) | |
| Session 15 | | | -1.836 (2.467) | |
| Session 16 | | | 1.689 (1.902) | |
| Session 17 | | | 3.155*** (1.173) | |
| Session 18 | | | 3.536** (1.638) | |
| Session 20 | | | -2.555 (3.418) | |
| Session 21 | | | 0.610 (1.730) | |
| Session 22 | | | -1.371 (1.712) | |
| Session 23 | | | -2.418 (2.986) | |
| Session 24 | | | 2.562** (1.024) | |
| Session 25 | | | -0.297 (2.986) | |
| Session 28 | | | 1.523 (1.863) | |
| Session 29 | | | -1.149 (1.773) | |
| Session 32 | | | -2.151 (1.662) | |
| Session 36 | | | 2.945 (1.982) | |
| N | 510 | 510 | 807 | 807 |
| R ² | 0.403 | 0.424 | 0.499 | 0.437 |