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Effort and Comparison Income
Experimental and Survey Evidence

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Abstract
This paper considers the effect of status or relative income on work effort, combining experimental evidence from a gift-exchange game with the analysis of multi-country ISSP survey data. We find a consistent negative effect of others' incomes on individual effort in both datasets. The individual's rank in the income distribution is a stronger determinant of effort than is others' average income, suggesting that comparisons are more ordinal than cardinal. In the experiment, effort is also affected by comparisons over time: those who received higher income offers or enjoyed higher income rank in the past exert lower levels of effort for a given current income and rank.

Keywords: Effort, comparison income, rank, peak-end, experiments
JEL Classification: M54, J33, A13, C92, D63

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A growing literature in economics is devoted to the role of social comparisons in explaining various phenomena, including financial market behavior, criminal activity, and subjective well-being. One part of this literature has focused on income comparisons and labor-market outcomes: quits are negatively correlated with a reference wage given by the average wage in the firm for similar workers; women's labor force participation is influenced by income comparisons; and rank in the local income distribution is a good predictor of migration. These behaviors mostly concern job choice. However, little is known about the impact of relative income on how hard employees actually work within the job, even though efficiency wage theories are built on the concept of income comparisons and relative concerns are appealed to as an explanation of wage compression (Frank 1984).

In this paper we try to fill this gap. We analyze the influence of income comparisons on effort using both experimental and survey data. We suggest that such income comparisons may explain why some of the empirical evidence on the wage-effort relationship is mixed: while it is commonplace to assume that wages have incentive effects, or that higher wages make up for higher effort in a compensating differential set-up, higher wages are not always associated with greater effort in empirical work. This has variously been explained by a crowding-out effect of monetary rewards on intrinsic motivation (Frey and Oberholzer-Gee 1997), supra-optimal motivation generating choking under pressure (Ariely, Gneezy, Loewenstein and Mazar, 2005), or an earnings target which bounds effort at some threshold (Camerer, Babcock, Loewenstein and Thaler 1997). Here we test an alternative hypothesis: that individual effort depends on both own income and the individual's position in the relevant income distribution. Due to social comparisons those who are paid relatively well work harder. We test whether others' income matters, and ask, given own income, which of relative income and income rank in

the reference group is most important in determining effort: are social comparisons cardinal or ordinal? We further ask whether income comparisons are not only horizontal (i.e. to other individuals at the same point in time) but also intertemporal, so that the time profile of individual income or rank helps to explain current effort at work.

Conclusive empirical proof of the existence of social comparisons is elusive, as it is difficult to know to whom individuals compare, and because individuals' behavior may be correlated within a group, not because they compare to each other, but because they are exposed to common unobserved environmental factors, or share similar characteristics. The experimental approach adopted here has the double advantage of defining *a priori* the reference group and limiting any contextual effects. In addition, it relies on actual and costly decisions instead of subjective reported behavior. Survey data, on the other hand, has the advantage of larger sample sizes, and avoids the criticism that laboratory experiments are to an extent unrealistic. The combined use of both survey and experimental data is still very recent (Fehr, Fischbacher, von Rosenbladt, Schupp and Wagner, 2003; Carpenter and Seki, 2005; Brown, Gardner, Oswald, and Qian, 2008) and can be seen as a joint test of robustness. If there are consistent patterns in both types of data, we can then have greater confidence in the external validity of laboratory experiments.

Our laboratory experiment extends the standard gift-exchange game between an employer and an employee by introducing income comparisons between employees from different firms. The reference group for employees consists of employees in other firms participating in the same experimental session. In this between-firm comparison design, employees are *a priori* similar and are thus expected to receive the same equilibrium wage. In the first stage of this game, the employer offers a wage contract. In the second stage, employees who accept the contract decide on their effort level. In one treatment we

can identify income comparisons, as we inform employees, before they choose their effort level, about the wages offered by a sub-set of other employers in the labor market. Testing the robustness of our experimental results required a dataset with information on discretionary effort that closely resembled the experimental design. The survey data come from the 1997 wave of the ISSP (International Social Survey Program), which includes information on both earnings and self-reported discretionary effort. The questions asked in this survey are extraordinary close to the context of the gift-exchange game, in that the employee's reported effort is explicitly oriented towards improving the firm's outcome.

The paper is organized as follows. Section 2 briefly surveys the literature on social comparisons, utility and behavior. Section 3 presents the empirical strategy with respect to both the survey and the experimental data. Section 4 reports the results from both data sources, and Section 5 discusses these results and concludes.

1. Literature

The existing literature on social comparisons can be broadly divided up into two strands: that on behavior and that on utility. This division can be illustrated by a direct utility function:

$$U_i=U(a_i, a_j, ...) \text{ for } j \neq i \quad (1)$$

in which individual i's utility, U_i, depends on both her own actions and those of relevant others, a_i and a_j. This utility function most often gives rise to a decision rule for i's utility-maximizing behavior a^*_i as:

$$a^*_i= f(a_j, ...) \quad (2)$$

The behavior and utility approaches to social interactions attempt to find empirical counterparts to (2) and (1) respectively.

There are a number of drawbacks to the behavioral approach. First, data on behavior is not always particularly accurate. Second, behavior often reflects the intersection of supply and demand, whereas we are interested here in individuals' preferences. Lastly, under separability conditions[1], others' behavior can affect my own utility, but not my behavior. There are equally problems with the utility approach, via equation (1): in particular, we do not necessarily know how to best measure individual utility, U_i.

Interactions in behavior have been widely modeled econometrically, despite the identification problems emphasized by Manski (1993).[2] Many of these studies have concluded that social interactions do indeed influence behavior, in the sense that 'if you do more of something, then I am likely to do more of it as well'. One interpretation is that this correlation reflects a concern for status or relative standing. Another is the possibility that individuals might be learning from each other about how pleasant or dangerous goods or activities are (so that their behaviors are correlated), rather than caring about their status. Rival explanations emphasize the perhaps key role of common omitted variables such as contextual effects, although much care is typically exercised in the empirical literature to defuse this interpretation.

An alternative approach to identifying interactions appeals to proxy measures of utility, such as life satisfaction, job satisfaction, and happiness (Ferrer-i-Carbonell, 2005; Clark, Frijters, and Shields, 2008). Perhaps because of a scarcity of surveys which measure both proxy utility and behavior adequately, most attention has been concentrated on the role of income comparisons in the utility function. Empirical estimation has thus mostly been based on the indirect utility function, V_i, testing specifications such as

[1] Formally, $d^2U_i/da_i da_j = 0$.

[2] Recent contributions in this vein have analyzed saving (Duflo and Saez 2002), tax evasion (Fortin, Lacroix and Villeval 2007), labor supply (Aronsson, Blomquist and Sacklén 1999), and students' success at school (Arcidiacono and Nicholson 2005; Sacerdote 2001).

$$V_i=V(y_i, y_j, ...) \text{ for } j \neq i \quad (3)$$

in which utility depends on own and relevant others' incomes, y_i and y_j respectively, rather than its direct counterpart (1) above.

Both the behavior and utility approaches require that the reference group be identified: to whom does the individual compare? There are a number of potential candidates, including the individual's peer group (those who share the same characteristics), others in the same household, spouse/partner, friends, neighbors, work colleagues, and the individual herself in the past.

An approach to modeling social comparisons which combines both of the above would be to consider *i*'s behavior, a_i, as a function of both her own income and that of her reference group, y_i and y_j:

$$a_i=a(y_i, y_j, ...) \quad (4)$$

This can be operationalized empirically as

$$a_i = A_0 + \beta y_i + \phi y_j + \gamma' X_i + \varepsilon_i \quad (5)$$

The β coefficient in equation (5) shows the extent to which individual behavior, a_i, depends on own income. If only own income matters in explaining *i*'s behavior, then the estimate of ϕ will be insignificant. On the other hand, if income comparisons matter for behavior then both β and ϕ will be significant. If action *a* is normal then we expect $\beta > 0$ and $\phi < 0$. This is the empirical approach that we take in this paper.

The behavior we consider here is effort expanded at work: we ask whether workers' effort, e_i, depends on how much others earn, modeling

$$e_i=e(y_i, y^*, ...) \quad (6)$$

where we expect $e_1 > 0$ but $e_2 < 0$. Here y^* is some transformation of the income vector of other people who are in individual i's reference group. The idea is that individual i has a comparison or reference person or group, j, and reduces his or her own input or effort as reference income rises, given his or her own wage.

Much of the efficiency wage literature is based on this idea of the comparison of one's own wage to those of co-workers (Akerlof and Yellen 1990) or of workers in other firms (Summers 1988; Johansen and Strøm 2001).[3] However, to the extent of our knowledge, empirical evidence that workers' current effort does in fact depend on relative income or on own past income remains slight.[4]

A recent literature in experimental economics has looked for evidence of social comparisons among co-workers. In the first stage of the game proposed by Charness and Kuhn (2007), a principal can propose different wages to her two employees. These employees may have either homogenous or heterogeneous ability levels, but the direction and magnitude of these differences are unknown to employees. In the second stage of the game, employees choose their level of effort. According to the treatment, wages are either public or private. Income comparisons are shown to affect employees' behavior only weakly, whereas firms reduce income differentials between co-workers for fear of low-effort retaliation from the lower-paid employee. In other words, firms anticipate a negative effect from income comparisons on effort that is not actually observed in workers' behavior. This wage compression effect was also found by Güth, Königstein, Kovacs and Zala-Mezo (2001) in a game in which information about the contracts offered

[3] Wage comparisons and effort have also been evoked in the business-cycle literature (Collard and de la Croix, 2000).

[4] One fascinating exception is Mas (2006), who exploits a natural experiment to consider the relationship between relative wages and effort. New Jersey police unions bargain over wages with their municipal employer, with disputes being settled by an outside arbitrator. Controlling for the actual pay level awarded, Mas finds that 12% more crimes per capita are solved (cleared) when unions win their case compared to when they lose. This is interpreted as evidence that police effort depends on pay relative to some reference point. By way of contrast,

to each employee was manipulated. They show that principals tend to reduce the income differential between agents when contract information is made public.

In these experiments, productivity differences are introduced between co-workers in order to motivate firms to vary their wage offers. The weak reaction by employees to subsequent income comparisons may show that workers consider productivity differences to be a fair source of income differentials. In our experiment, on the contrary, all employees have the same productivity, each firm only employs one worker, and income differences result from firms' various choices (and not from any skill differences between workers). Gächter and Thoeni (2005) provide another experimental test using the strategy method: subjects are asked to report their effort decision in reaction to various hypothetical income distributions. They identify a large subset of individuals who reduce their effort when faced with income inequality. In our experiment, incomes are actually chosen by real firm-subjects, and we infer the influence of income comparisons from individuals' observed effort decisions.

2. Empirical strategy

Our empirical strategy is based on the joint use of experimental data produced in the laboratory and survey data. The survey data analysis helps to check the external validity of the experimental evidence. When we evoke income comparisons here, we define income as the wage offered by the employer to the employee, i.e. we do not take into account the cost of effort which will depend on the level of effort chosen by the employee.

2.1. Experimental Design

The game. We identify the impact of income comparisons on effort using a version of the standard gift-exchange game (Fehr, Kirchsteiger and Riedl 1993). Each session involves

most of the experimental work on the impact of others' income tests for inequality aversion and focuses on distribution decisions through choices over tax rates, transfers or the distribution of income (see Cowell 2004).

twenty subjects who are divided into two groups, ten in the role of firms and ten in the role of employees. Roles are attributed at random and are kept constant throughout the session. All employees have the same characteristics, in contrast to Güth *et al.* (2001) and Charness and Kuhn (2007). Workers do not differ in ability and thus do not have to form beliefs about the relationship between other employees' productivity and their incomes. A Benchmark Treatment and an Information Treatment were implemented.

The Benchmark Treatment consists of the standard gift-exchange game. The use of this standard game ensures that our results can be directly compared to those from previous experiments before we introduce a new element in the Information Treatment. In each of the ten periods of the game, each firm is matched randomly with an employee. Each period consists of two stages.

In the first stage, the firm offers a contract consisting of a wage $w \in \left[20,120\right]$ to its employee. In the second stage, the employee decides whether to accept or reject the contract. If the contract is rejected, both the firm and the employee receive nothing. Upon acceptance, the employee has to choose his level of effort, $e \in \left[0.1,1\right]$.[5] The greater is the employee's effort, the higher are the firm's profits but also the greater is the effort cost *c(e)* borne by the employee. This effort cost is convex, as shown in Table 1.

In the standard gift-exchange game, the employer's payoff is:

$$\pi^P = \left(v - w\right)e$$

[5] It is important to separate these two stages in order to respect the standard gift-exchange game and to check the comparability of our results in the Benchmark treatment to those in previous work. As argued in the literature on gift-exchange games, this design allows us to disentangle the effort decision from the job entry decision and to analyze whether income comparisons exert the same influence, if any, on both decisions. Combining the decision to accept an offer and the choice of effort in one single stage would simplify the design but this requires that the decision to accept a contract offer and effort choice be determined in the same way, or that rejecting a contract is equivalent to complete shirking on the job. As the results will show, we reject this hypothesis. In any case, there is no guarantee that an agent would choose to exert zero effort were she not allowed to reject a contract offer.

where v is an exogenous redemption value; in our experiment, v=120. This expression guarantees that the firm does not make a loss even if the employee chooses the minimum level of effort. The employee's payoff is:

$$\pi^A = w - c(e) - 20$$

with a fixed labor market participation cost of 20 (corresponding to travel costs, say).[6]

These payoff functions are common information. At the end of the period, the firm is informed about the level of effort chosen by the employee, and both the firm and the employee are informed about their respective payoffs. In each new period, the pairs of firms and employees are randomly reshuffled. We implement a perfect stranger matching protocol in the sense that no agent is matched with any principal more than once, and this is common information from the instructions. This allows us to rule out any reputation-building behavior (Gächter and Falk 2002).

The Information Treatment has the same structure. The difference lies in the fact that at the end of the first stage, after the firm's income offer is revealed, the employee is told about the income offers received from their firms by four other employees in the same period. Employees can thus compare their own income to the income offered to other *a priori* similar employees on the labor market (but not co-workers) before rejecting or accepting the contract, and thus before choosing effort. We choose to display only partial information about other income offers (in each period, four other randomly-chosen income offers, instead of the whole distribution) to produce a greater variety of income distributions within the reference group. In addition, this procedure allows the relative income effect to be identified separately from any period effect. In contrast to the employee, the firm is not informed about the other firms' income policies. This reduces

[6] This fixed cost, which has to be covered by firm's wage offer, exists only to avoid having a minimum wage of zero in the set of firm's actions; it has no implications for the theoretical predictions of the game.

the likelihood that firms behave in a different way in the two treatments. This is justified by the fact that we are mainly interested in the comparison between employees.

Equilibrium of the game. The equilibrium of this game with selfish and rational players is a minimum wage – minimum effort pair of decisions, [*w=20*, *e=0.1*]. The minimum wage contract should be accepted since the employee has no better alternative. Equally, the employee should accept the contract and choose the same (minimum) effort level in both treatments since the incomes offered by other firms do not enter into the standard individual utility function. Firms should thus offer the same (minimum) income in both treatments.

However, it is possible that, in both treatments, income and effort be above their theoretically-predicted levels. The existing literature has shown that employees typically reciprocate high (low) income offers by choosing high (low) effort levels that increase (decrease) the firm's payoff (Fehr, Gächter and Kirchsteiger, 1997).[7] In addition, information about the income distribution may affect effort in the Information Treatment if individuals are sufficiently sensitive to income comparisons. If subjects make horizontal comparisons (i.e. among employees), we may expect effort to be positively correlated with both relative income and income rank. On the contrary, as firms are never informed about the income distribution, there is no reason why their behavior should differ across treatments.[8]

Procedures. The experiment was conducted in the experimental laboratory at GATE, Lyon, France, using the Regate software (Zeiliger 2000). A total of 120 undergraduate students, from three local Engineering and Business schools, participated in one of the six

[7] One might argue that individuals may also reciprocate higher income rank and higher relative income with higher effort in the Information Treatment. However, in our experiment, firms were never informed about the income distribution. As a consequence, ranking can not be considered as intentional on the part of the employer. In any case, employees would presumably only reciprocate relative income if it were valuable to them, which is exactly what we want to demonstrate.

sessions organized. Two of these concerned the Benchmark Treatment and four the Information Treatment. No-one participated in more than one session. Upon arrival, the subjects drew a label from an envelope, indicating the name of their computer. The instructions (see the Appendix) were distributed and read aloud. The subjects then filled out a questionnaire that allowed us to check their understanding of the rules of the game. Questions were answered in private.

The subjects subsequently discovered their role (firm or employee)**.** The program paired firms and employees randomly and anonymously. As the game was repeated 10 times under a perfect stranger matching protocol, each firm made an income offer to each of the employees. This yields a total of 200 wage offers in the Benchmark Treatment and 400 wage offers in the Information Treatment. Each employee made 10 contract-acceptance decisions and, if the contract was accepted, chose an effort level. The next section concentrates on the analysis of these effort decisions.

Each session lasted one hour on average, including the payment that was carried out in a separate room. The experiment was conducted in experimental currency units with 100 points equal to 4 Euros. Each subject earned on average €14 from the experiment, including a show-up fee of €4. This indicates that in each period, the subjects earned on average €1.

Compared with survey data, this experimental approach presents many advantages: income is perfectly measured, effort is observed directly instead of being self-reported, and the reference group is controlled. However, the artificiality of the laboratory may cast doubts on the external validity of the experimental results. For these reasons, we complement our experimental analysis with survey evidence on income and effort.

[8] A firm cannot know whether a lack of reciprocity is due to comparisons or to the employee's selfishness.

2.2. Survey Data on Work Effort

The survey data, multi-country and cross-section, come from the 1997 Work Orientations module of the International Social Survey Programme, the ISSP (the data and documentation are freely available from http://www.issp.org). The key variables in our empirical analysis are effort, earnings and hours of work. Income is measured as individual yearly labor market earnings, converted to U.S. Dollars using Purchasing Power Parities from the OECD. Hours of work are measured at the weekly level.

The variable we wish to explain is effort at work. This is almost never observed directly in survey data. To compare our experimental evidence to (larger-scale) survey data, we require a survey in which employees report their willingness to exert effort in order to improve their employer's outcome, as in the experimental set-up where the employee provides extra effort at his own cost in order to increase her employer's earnings. The question we appeal to in the ISSP data is crafted to measure discretionary effort, and is thus arguably well-suited to our analysis. All those in employment are asked to indicate the extent to which they agree with a number of statements. One of these is: "*I am willing to work harder than I have to in order to help the firm or organization I work for to succeed*".[9] This question is remarkably close to the context of the gift exchange game, in which any level of effort above the minimum is voluntary and increases the firms' profit, but decreases the employee's payoff. It may seem reasonable to assume that that "helping the firm to succeed" in the survey data is analogous to improving its payoff, and conversely that increasing the firm's earnings in the experiment is comparable to helping it to succeed.

[9] This is similar to variables used in Management to capture organizational commitment: see Jaworski and Kohli (1993). It could also, of course, reflect the employee's effort to prevent the firm going bankrupt; however, even in this context, employees provide costly effort in order to improve the firm's outcome.

The weighted distribution of the five possible responses to this question in the 1997 ISSP is shown below in percentages.

Strongly agree	16.7%
Agree	42.4%
Neither agree nor disagree	24.2%
Disagree	12.0%
Strongly disagree	4.5%

Keeping only full-time or part-time employees aged 16-65 yields a sample of 12 000 observations over 17 countries (considering the two Germanies separately). Missing values on earnings, hours of work and effort finally produce a regression sample of around 10 000 observations.

We are interested in describing differences in the response to the effort question between individuals. A first pass is to look at the cross-country pattern in discretionary effort. To do so, we allocate a value of 5 to "strongly agree" through to a value of 1 for "strongly disagree" to the question described above. Table 2 shows the number of observations and mean effort, ranked by country from the lowest to the highest average effort.

There is something of a country pattern in the degree of social reciprocity at work. Mediterranean countries are broadly towards the bottom of this ranking, while workers in Anglo-Saxon countries are on average more willing to work hard to help their firm or organization. Portugal is an exception to this general rule, appearing towards the top of the ranking.[10]

[10] There is a strong correlation between unemployment and the mean of this effort variable. The average OECD standardised unemployment rate in 1997 of the lowest seven countries in the ranking was 12.3%, as against 5.9% for the ten highest-ranked. Both the Pearson and Spearman correlations between mean effort and the unemployment rate are significant at better than the 2% level. One interpretation is that social reciprocity allows firms and employees to attain Pareto-superior employment outcomes.

The main thrust of our paper is, conditional on country, to see how workers' discretionary effort is related to individual demographic and job characteristics. We are especially interested in the role of income. We control for individual income and hours of work, but also for reference group income. This latter is defined in a similar way to that in the Leyden school: by calculating average values of income over fairly broad demographic groups,[11] here country, gender, education and age. There are three education groups (up to 10 years of education, 11-13 years of education, and over 13 years of education), and three age groups (16-29, 30-44, and 45-65). There are thus 17 (country) * 2 (sex) * 3 (education) * 3 (age) = 306 reference groups. These average income measures are called comparison income in the regression tables, and correspond to y^* in equation (6) above.[12] Comparison income for individual i in cell j is calculated excluding i's own income.

3. Results

Effort may be influenced by own income, by relative income or income rank, or by the income the individual received in the past if there are intertemporal comparisons. We estimate discretionary effort equations on both experimental and survey data to determine whether income comparisons affect individual effort behavior.

3.1 Effort and comparisons to others

The average wage offered by firms in the experiment is 53.51 (Standard Deviation 19.7) in the Benchmark Treatment and 53.09 (S.D. 20.0) in the Information Treatment (where wage offers are constrained to lie between 20 and 120). Both average figures are clearly above the equilibrium wage of 20 (one-tailed t-test, $p<.0001$) but are not significantly

[11] See for example van de Stadt, Kapteyn, and van de Geer (1985).
[12] This cell-average approach does not suffer from the identification problems which occur when y^* is predicted in a regression framework, as the cell-average income is not a linear function of the X variables (the variables which define the cells – here country, age, gender and education).

different from each other. Firms do anticipate reciprocity from their employees, but they do not change their wage offers when income comparisons appear.

Two different specifications of comparison income are used. The first is normalized rank, defined as: rank in cell or group / number of observations in cell or group, with a correction for ties. This is a measure of how the individual's income is ranked relative to the other members of the reference group in the Information Treatment and in the ISSP survey data (and not relative to all individuals). In the experimental data, the rank determines the position of the subject relative to the four other group members for whom wage information was revealed. This measure is bounded between just over zero for the bottom-ranked income in the cell to one for the top-ranked income. The second comparison measure is average reference group income, excluding the individual's own income. Average and individual earnings levels are expressed in experimental currency units in the experimental data, and in thousands of U.S. Dollars per month in the ISSP data.

We estimate the influence of income comparisons on effort in the experimental data via random effects Tobits. The use of Tobit models is justified by the number of left-censored observations in the sample. Table 3 displays the distribution of effort levels and mean income per effort level. It shows that minimum effort (i.e. 0.1) was chosen 98 times out of 180 in accepted contracts (54.4%) in the Benchmark Treatment, and 214 times out of 378 in accepted contracts (56.6%) in the Information Treatment.[13] Not taking this data censoring into account would likely bias the coefficients.

[13] If we consider individuals instead of decisions, we observe that only a minority of subjects behave selfishly. Defining as selfish individuals those subjects who choose the minimum effort in at least 8 periods out of 10, we have 35% of selfish people in the Benchmark and 27.5% in the Information Treatment. We cannot however determine whether this difference is inherent to the very nature of the subjects involved in the two treatments or

Table 3 indicates a positive relationship between income and effort in both experimental treatments. This is typically observed in the gift-exchange game (Fehr, Gächter and Kirchsteiger 1997), and is consistent with social motivations leading to reciprocity. While the income-effort relationship looks somewhat steeper in the Information Treatment, the joint presence of income and comparison income makes such bivariate conclusions untrustworthy.

The main effort regression results using the experimental data are shown in Table 4, and those based on the ISSP survey data in Table 5. Table 4 consists of two panels. The left panel displays the results of six regressions in which the dependent variable is the effort choice of subjects who accepted a contract offer. The right panel, which we will discuss below, presents the results of alternative specifications that check the robustness of the results. Most regressions are estimated as Tobits, which account for both left- and right-censoring (the first of which is endemic in our data). In addition, since each subject is observed a number of times (up to 10 times if the subject accepts all the contract offers), we appeal to panel data methods, and estimate all of the regressions in the left-hand panel with random effects. In the Benchmark (Information) Treatment, 20 (22) contracts were rejected. Our left-hand panel sample thus consists of 180 effort decisions in the Benchmark Treatment and 378 in the Information Treatment.

Regressions (1) and (3) consider the role of own income in the Benchmark and Information Treatments, respectively. Regressions (2) and (4) add normalized income rank as an explanatory variable: higher values of this rank variable correspond to higher positions in the reference group income distribution. Since subjects are not informed about their income rank in the Benchmark Treatment, this "placebo" variable should be insignificant there, except if income and rank are strongly collinear. The last two

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if it is attributable to the dissemination of income information. If the latter, some fraction of minimum effort

regressions in the left-hand panel refer to the Information Treatment only. Regression (5) replaces income rank by average reference group earnings (excluding own income), and regression (6) includes both income rank and average group earnings. All of the experimental effort regressions control for both gender and number of post-baccalaureat years of education.

Table 5 reports the results of four analogous estimations on the ISSP survey data, in which the dependent variable is the degree of willingness to work harder to help the company or organization to succeed. Each individual is observed only once and we have 9854 observations. Ordered Probit regressions are estimated as the survey effort question allows five ordered responses. We follow the same logic as for the experimental data: regression (1) includes own income only; regression (2) adds normalized income rank; regression (3) replaces rank by comparison income; and regression (4) estimates the joint influence of own income, rank and average reference group income. We also control for hours of work, age, gender, education and marital status, and include country dummies. The standard errors in this Table are clustered at the reference group level.[14]

The results in Tables 4 and 5 show that effort is strongly correlated with own absolute income at the 1% level in both treatments of the experimental data and in the survey data. Regressions (4) in Table 4 and (2) in Table 5 reveal the influence of others' income: normalized income rank attracts a positive and significant coefficient conditional on own income. For the same number of Dollars/experimental units earned, individuals are willing to work harder the higher is their position in the reference group income

decisions are motivated by social comparisons rather than selfishness.

distribution. Unsurprisingly, normalized income rank is insignificant in the Benchmark Treatment (regression 2 in Table 4), where individuals are unaware of their rank. In the experiment (column 4), a rise in rank of one position (out of five) increases effort by 0.57 (=0.20*2.87), which is equivalent to a wage increase of 6.52 for given rank. Compared to average income per period (53.09), this latter represents a wage rise of 12.3%. The rank/income elasticity is thus 0.614 (=12.29/20). In the survey data, a 20% rank increase has the same effect on effort as an extra $623 per month, which is 33% of average income, yielding a rank/income elasticity of 1.6. This higher elasticity may reflect the wider distribution of income in the survey data, the fact that rank matters more "in real life", or that rank is more important when reputation-building is possible.

The experimental evidence thus points to income position within the reference group as being an important determinant of how much discretionary effort workers provide, over and above the actual income they receive, which latter has been the focus of the literature to date. This is confirmed by the survey data analysis.[15] This, to our knowledge, is one of only a small number of empirical findings pointing to relative income and status as a determinant of employees' behavior.

In regressions (5) in Table 4 and (3) in Table 5, average income in the reference group attracts a negative coefficient, which is significant only for the experimental data. If we include both normalized rank and reference earnings in the same regression (column (6) in Table 4 and column (4) in Table 5), this marginally significant effect disappears, whereas the coefficients associated with rank remain positive and significant. Our second key result is therefore that ordinal comparisons, as measured by normalized rank in the income

[14] Income is entered in levels. Entering all of the cardinal income variables as logs produces similar results, but is not preferred by the data (the log likelihood is lower).

[15] The ISSP results are largely unchanged when we drop the 20% of observations which are found in reference groups with 30 observations or less, or if we use a less aggregated reference group by dropping education, or age.

distribution, are a more powerful predictor of employee behavior than are cardinal comparisons, i.e. from others' earnings expressed in currency units.[16]

Other results in Table 4 show that in the experimental data, gender and education have a marginally significant negative effect on effort in the Benchmark Treatment but no significant impact in the Information Treatment. In the ISSP data, controlling for rank or average income, effort is higher for men, the married and the higher-educated. The difference between the experimental and the ISSP data may reflect the far smaller variance in the demographic variables in the student subject-pool than in the ISSP data. Last, the estimates on the country dummies in the ISSP regressions (not shown) largely reproduce the effort ranking in Table 1.

3.2 Robustness checks

To check the robustness of our experimental results, we have considered a number of alternative specifications, some of which are reported in the right-hand panel of Table 4. For the sake of simplicity, we only report the estimations including both own wage and normalized rank.

First, columns (7) and (8) reproduce columns (4) and (6), but allowing for a less restrictive form of correlation between error terms at the individual level than random effects. The estimation method here is a Tobit with clustered standard errors at the individual level. Similar estimations with clustered standard errors have been carried out for each of the previous models. The main message from these regressions is that the results in the left-hand panel of Table 4 are unaffected by this clustering. Clustering

[16] This result concurs with that in Brown, Gardner, Oswald and Qian (2008), where income rank is shown to outperform average reference group income in three satisfaction equations (influence over the job, achievement, and supervisor's respect). For the fourth dependent variable, satisfaction with pay, both rank and reference group income attract significant coefficients.

increases the standard errors, but both own income and rank remain significant in columns (7) and (8).

The main results reported above were based only on those subjects who accepted a contract (and consequently reported an effort level). Alternatively we can include those who rejected the contract, imagining that they would have provided zero effort. In this case, no observations are excluded. We thus estimate in column (9) a random effects Tobit model in which the left-censoring is set at effort level 0 rather than 0.1; column (10) shows the equivalent estimates from random effects Generalized Least Squares estimation. Both regressions use all 400 observations, as opposed to 378 previously. We find that, controlling for absolute income, rank continues to exert a significant effect on effort.

These regressions are based on the strong assumption that rank affects the decision to reject an offer and the choice of minimum (but positive) effort to the same degree. To test this hypothesis, we next estimate a random effects Probit for the decision to accept an offer, with the same explanatory variables as previously: the results are shown in column (11). The probability of accepting an offer depends on the absolute wage proposed, but is not affected by income rank. A potential explanation is that contract acceptance is a blunt decision, while there is more latitude in effort choice. It is therefore important to respect the sequential structure in the gift-exchange game separating offer acceptance from the choice of effort. This also explains why treating offer rejection as the choice of zero effort reduces the significance of rank (from the 1% to the 5% level).

Bearing this in mind, we proceed to an alternative two-step estimation procedure that respects the sequential nature of the game in order to correct for any selection bias from the exclusion of the observations corresponding to the rejected contracts. We first consider the random effects Probit estimated in column (11) as a selection equation,

producing the inverse of the Mill's ratio (IMR). We then explain effort, conditional on contract acceptance, corrected for selection bias via the introduction of the IMR as an explanatory variable. This second equation is estimated as a random effects Generalized Least Squares with clustered standard errors in column (12), and as random effects Tobit (which we prefer, given the importance of left-censoring) in column (13). Both specifications show that rank continues to affect effort (at the 5% significance level). The results from GLS estimation suggest that a rise in income rank of one place (for example 4^{th} to 3^{rd}), which corresponds to a rise in the rank variable of 0.2, will increase effort by two to three ticks on the ten-point (0.1-1) scale, as 0.2*1.235=0.25.

The robustness checks therefore all deliver the same conclusion: regardless of the form of the correlation between the error terms at the individual level (random effects or clustered), and regardless of the way in which contract rejection is treated, individual effort is sensitive to income rank.[17]

3.3 Effort and comparison income across groups

Our main results in Tables 4 and 5 concern average effects over all individuals in the sample. However, we may suspect that certain groups react to relative income in different ways. In particular, based on recent experimental evidence on the impact of gender on competition or social preferences, we consider whether the impact of rank on effort is different for men and women in both the experimental and the survey data.

The experimental results in Table 4 include interactions between "Male" and both own income and income rank. The estimated coefficients on these interactions are always

[17] We have also estimated models using the Chamberlain procedure (results available upon request). More specifically, we add $\overline{X}_i$ (the average individual rank of the individual in all previous periods) to the random effects Tobit model. Our results remain unchanged. Equally, Generalized Least Squares with fixed effects yields similar conclusions.

insignificant, showing that men and women react to income similarly in determining their effort choice. An alternative way of looking at sex differences in the experimental is shown in the first panel of Table 6, where separate regressions are run for men and for women. We lose some significance here, due to the smaller sample size, but again the size of the estimated coefficients is similar by sex.

The ISSP survey sample is more heterogeneous, allowing a number of different scenarios to be tested. As well as gender, we can here consider a potential role for the environment in which wages and effort are decided. We consider potential moderating effects of union membership, sector (public vs. private), and managerial status. The regressions in Table 5 are run separately for each of these different groups. In the case of union, sector and manager, the reference group is redefined according to the respective variables (so that managers consider their rank amongst other managers); the reference group is already defined by sex.

The estimated coefficients on the income terms from specifications (2) and (3) in Table 5 are shown in the bottom part of Table 6, for the eight sub-groups under consideration. First, as in the experimental data, there are no sharp differences between men and women. Income rank attracts a positive coefficient for both groups, although significant only at just outside the ten per cent level, due to smaller sample sizes, while the estimated coefficient on average income in the reference group is negative but very insignificant.

The third panel considers union status, and here differences do arise. Effort for non-union workers is related to own income only, with no role for income comparisons. Effort for union workers is very sensitive to income rank, perhaps indicating the key role of wage fairness in union negotiations. The fourth panel reveals little difference in the qualitative effort results between the public and private sectors. Last, the effort of workers

in non-supervisory positions is only affected by their own income. However, workers with managerial responsibilities are sensitive to income comparisons, particularly in terms of income rank.

While Table 6 does show that effort for some groups of workers is more sensitive to income comparisons than for others, it is worth noting that in the ISSP data, "comparison income", the average Dollar amount earned by others in the reference group, is insignificant in all of the eight groups considered. Comparisons continue to be ordinal rather than cardinal.

3.4 Effort and comparisons over time

The results so far have discussed the relationship between others' income and own effort. We now turn to comparisons to the income that the individual herself received in the past. The broad idea is that past exposure to higher incomes may reduce the utility associated with current incomes and thus decrease the current level of effort. This hypothesis has been tested with measures of satisfaction in panel data (see Clark 1999; Weinzierl 2005) but, as far as we know, not with measures of behavior such as effort. In parallel, a separate literature has developed on time-inseparability in behaviors such as consumption and labor supply.

One difficulty in these literatures has been to ensure that *ceteris paribus* holds over the long time-periods between waves of survey data. Experimental data are ideally-suited to testing models of habituation since we impose the same environment over time, especially in the perfect-stranger framework where there is no role for reputation building. We therefore investigate the role of previous income in determining current levels of effort, by estimating random effects Tobit models on the experimental data only. The dependent

variable is the choice of effort conditional on contract acceptance. Our *a priori* is that higher past income will reduce current effort, as past income acts as a benchmark.

We pick up the effect of past income by including running maximum income and running minimum income as additional explanatory variables. We thus ask whether effort at time *t* depends on the highest (lowest) income the individual had been offered up to and including time *t*. We carry out an analogous analysis with respect to rank to determine whether effort is influenced more by past income or by past income rank. This running maximum/minimum specification is inspired by the peak-end transformation, which has been used to model how a flow of pain is converted into a final global evaluation (Redelmeier and Kahneman 1996).[18] The period dummies in this regression pick up the fact that the running minimum (maximum) mechanically weakly decreases (increases) over time, and avoid any spurious correlation between running minimum and maximum and the dependent variable. The usual demographic variables are also included. The results appear in Table 7.

Table 7 shows that the past matters: for a given income and a given income rank, effort is significantly lower the higher is the most generous income offer received in the past (regression (1)), and the higher is the best income rank achieved in the past (regression (2)). In contrast, running minimum income and running minimum rank do not influence the current level of effort. This suggests that high past income and income rank are used as benchmarks with which to evaluate the current offer's generosity, and thus the degree of reciprocity. Regression (3) compares the influence of the two past income measures. The best past rank in the income distribution (significant at the 2% level) matters more than best past absolute income, which is itself borderline significant (12%). The

insignificance of the interaction between gender and rank shows that, as above, men and women react to rank in the same way.

4. Discussion and conclusion

Evidence for the role of status or comparisons in determining behavior remains elusive. In this paper we have looked for effects of income comparisons on discretionary work effort in experimental data. We then compare the experimental findings to results from large-scale survey data. We have three key findings.

First, effort at work depends both on the individual's own income, and on what others earn, both in the experimental and survey data. Our results thus contribute to the still small literature showing that comparisons affect behavior via actual costly decisions and not only self-reported well-being. We further believe this to be one of the first papers to combine experimental and survey data to do so.

Second, income rank (*i.e.* first, second, … in the relevant distribution) is a better predictor of effort decisions than is average reference group income. As such, comparisons are ordinal rather than cardinal.

Last, in the experimental data, the income profile over time matters in itself. Those who received higher income or higher income rank in the past supply less effort today, at a given current income and income rank. This result is potentially important for understanding for example the frequent failure of mergers. While the literature has concentrated on the role of income, mergers may involve substantial changes in rank as well; we have shown the latter to be a strong determinant of motivation.

There are a number of explanations of the rank-sensitivity of effort. We have presented our results in terms of income comparisons and concern for status. Alternatively, effort

[18] Data from period 1 are dropped as income (income rank) and running maximum/minimum income

choice may derive from inequality-aversion (see for example Fehr and Schmidt 1999): those who receive a high income increase their effort so as to reduce the difference between their own earnings (i.e. income minus effort cost) and those of lower (and particularly the lowest) income workers. While it is difficult to distinguish cleanly between theories, we note that inequality-aversion would predict a stronger effort role for others' incomes than for income rank, whereas in both experimental and survey data we find the opposite. Also inequality-aversion does not explain the role of past income and income rank in explaining current effort, whereas income comparisons do.

Another alternative explanation of our results is that workers learn what the "fair income" is in the group: in this case, their effort does not depend on within-period comparisons as such, but on the search for the norm and learning. Subjects learn progressively how their current firm's behavior compares to that of other firms; this would also explain why past wages negatively affect current effort, everything else being equal. As such, our regressions might capture a comparison effect based on learning. Although the subjects likely do learn the average wage over time, we do not believe that this learning entirely replaces the rank effect, for a number of reasons. First, if we were observing learning in the experiment, employees should reject more offers over time as they learn what the fair income is, and should reject more contracts in the Information Treatment than in the Benchmark Treatment. Neither of these predictions holds. Second, if only learning is present, income rank should be insignificant, or should at least be less important than the reference income within the group. However, reference income in the experiment is less significant than is rank, and when we include both variables in the regression at the same time, only rank remains significant. In the survey data, reference income is never significant. Last, in the experiment, the employees should also care about

(income rank) necessarily coincide in this period. The period dummies therefore refer to periods 3 to 10.

both own best and worst wages in the past, which is not the case. As such, we believe that an interpretation in terms of rank and status-seeking is the most consistent with all of our experimental and survey findings.

One general implication of our work is that combining experiments in a controlled environment and survey analysis, based on subjective data, serves as a validation exercise. While both approaches have been criticized for separate reasons, here they produce remarkably similar and consistent results about the importance of income rank on effort decisions. Another validation procedure would consist of asking the experimental subjects to perform a real effort task instead of picking numbers from a table. This would constitute a natural extension of this paper.

Over 20 years ago, Bob Frank (1985) suggested that firms can trade off status and wages. In the context of between-firm comparisons, this paper has shown that these two are indeed substitutes in terms of inciting worker effort. Worker effort is lower in the face of both absolutely and relatively low incomes, where this relativity concerns both others in the same period and oneself in previous periods. This may explain why firms favor income secrecy, and also why the same income at a point in time might produce different effort levels. The results also demonstrate the concrete advantage accruing to firms paying rising income profiles. More generally, income comparisons, both to others and to oneself in the past, seem to be a pervasive element of economic life.

Appendix: Experimental Instructions in the Information treatment

General information

You are going to participate in an experiment on the labor market for the MiRE- Ministry of Social Affairs. If you read these instructions carefully, you can earn a considerable amount of money. The amount of your earnings depends not only on your decisions, but also on the decisions of the other participants you will interact with. During this session, your earnings will be calculated in points, with

100 points = 4 Euros

At the end of the session, all the profits you have made in each period will be added up and converted into Euros. In addition, you will receive a show-up fee of 4 Euros. Your earnings will be paid to you in cash in a separate room in order to preserve confidentiality.

At the beginning of the session, each of the 20 participants will be assigned one of two roles: 10 participants will be "employees" and 10 participants will be "firms". Your computer screen will inform you about your role. You will keep the same role throughout the session. You will never be informed of the identity of the participants you will interact with.

The labor market consists of 10 periods.

Decision-making in each period

Each period consists of two stages.

- In the first stage, each firm is paired randomly and anonymously with an employee. Each firm makes a income offer to his employee. The employee is informed of the income offer made by his firm and he is also informed of the income offers made by 4 other firms randomly chosen in the room.

  The employee can accept to work for the income offered by his firm or not to accept his firm's offer. If the employee accepts the offer, he proceeds to the second stage.

- In the second stage, the employees who have accepted an offer must decide on their quantity of work.

The details of the procedure are explained below.

Please note that in each new period, the firm-employee pairs are reshuffled. You are sure not to interact more than once with the same firm or with the same employee if you are a firm.

Information about the labor market in each period

1. At the beginning of the period, the firm makes an offer to the employee. This income is between 20 and 120 points. Information about this income offer will be communicated to 4 other employees.
2. The employee is informed about both the income offer made by his firm and the income offers made by 4 other firms to their employees. These firms are chosen randomly.
3. The employee can accept the offer from his firm and work. He can reject the offer and, in this case, he does not work: both he and the firm earn nothing for the current period. Only the firm is informed about the acceptance or the rejection of his offer by his employee.
4. If the employee has accepted the income offer, he receives his income and must decide on his quantity of work. The firm is informed about this quantity of work but neither other firms nor other employees are informed about it. The employee must bear a transportation cost of 20 points.

How are payoffs in each period determined?

The employee's payoff

1. If the employee has rejected his firm's offer, his payoff is zero for the period.
2. If the employee has accepted his firm's offer, the employee receives his income. He must subtract from this income both a transportation cost of 20 points and the cost associated with the quantity of work he has chosen.
3. The employee determines his quantity of work in choosing a number in between .1 and 1, as indicated in the Table below. The smallest quantity of work is .1 and the largest is 1. The higher the number chosen, the greater the quantity of work, and the higher the firm's payoff.
4. The greater the quantity of work chosen, the higher is the associated cost to the employee. The Table below shows how costs vary with the quantity of work.
5. In the case that the income offer is accepted, the employee's payoff in points is determined as follows:

Employee's payoff in points in each period =

Income – cost of the quantity of work – transportation cost

Transportation cost = 20 points
Relationship between the quantity of work and the associated cost

Quantity of work	.1	.2	.3	.4	.5	.6	.7	.8	.9	1
Associated cost	0	1	2	4	6	8	10	12	15	18

The employer's payoff

1. At the beginning of each period, the firm receives 120 coupons from the experimenter that can be used to pay the income of the current period. If the firm offers a income of 120 points to his employee and if this offer is accepted, then the firm has no coupons left. If the firm offers a income of 20 points to his employee and if this employee accepts this offer, then he has 100 coupons left. More generally, the firm keeps:

   120 coupons – the income paid to the employee

2. How are the remaining coupons converted into points? The number of coupons kept by the firm is multiplied by the quantity of work chosen by the employee. The result indicates the firm's payoff in points for the current period. Then,

   Firm's payoff in points in each period =

   (number of coupons – income) * quantity of work

3. If the employee does not accept his offer, the firm loses its coupons and its payoff is zero for the current period.

At the end of the period, the firm and his employee are informed about their respective payoffs.

At the end of each period, the next starts automatically. The firms and the employees are re-matched randomly to form new pairs.

Throughout the entire session, you are not allowed to talk if not invited to do so. Any violation of this rule will result in being excluded from the session and not receiving payment. If you have any questions regarding these instructions, please raise your hand. Your questions will be answered in private.

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Table 1. The cost of effort in the experiment

Effort *e*	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1
Cost *c(e)*	0	1	2	4	6	8	10	12	15	18

Table 2. Mean Discretionary Effort by Country: ISSP 1997.

Country	*Employees interviewed*		*Mean Effort*
	No.	*%*	
USA	775	6.47	3.93
Canada	546	4.55	3.75
Portugal	843	7.03	3.71
Switzerland	1 727	14.41	3.65
Denmark	600	5.01	3.64
Great Britain	545	4.55	3.63
Japan	607	5.06	3.62
Hungary	626	5.22	3.60
Czech Republic	526	4.39	3.60
Norway	1 366	11.40	3.59
East Germany	261	2.18	3.59
West Germany	648	5.41	3.52
Sweden	793	6.62	3.42
Spain	387	3.23	3.35
Poland	564	4.71	3.26
Italy	475	3.96	2.96
France	698	5.82	2.85
Total	11 987	100.00	3.55

Note: The question is "*I am willing to work harder than I have to in order to help the firm or organization I work for to succeed*", with responses coded from 5 for "*strongly agree*" through to 1 for "*strongly disagree*".

Table 3. Average income and effort levels in accepted contracts

Effort level	*0.1*	*0.2*	*0.3*	*0.4*	*0.5*	*0.6*	*0.7*	*0.8*	*0.9*	*1*	*Total*
Benchmark											
Number obs.	98	22	16	12	11	9	4	5	2	1	180
(%)	(*54*)	(*12*)	(*9*)	(*7*)	(*6*)	(*5*)	(*2*)	(*3*)	(*1*)	(*1*)	(*100*)
Mean income	50.9	50.1	61.5	64.1	69.7	71.1	71.3	80.0	95.0	60.0	53.5
Information											
Number obs.	214	45	32	29	13	18	13	8	0	6	378
(%)	*(57)*	*(12)*	*(8)*	*(8)*	*(3)*	*(5)*	*(3)*	*(2)*	*(0)*	*(2)*	(100)
Mean income	44.4	59.2	65.4	64.0	69.6	75.6	80.8	79.4	0	93.3	53.1

Table 4. Effort, rank and comparison income in the experimental data

	Regression models						*Robustness tests*						
Treatments	*Benchmark Treatment*		*Information Treatment*				*Information Treatment*						
Dependent Variables	*Effort in accepted contracts*						*Effort in accepted contracts*		*Effort for all offers*		*Acceptance*	*Effort in accepted contracts*	
Models	RE Tobit[a]	RE Tobit	RE Tobit	RE Tobit	RE Tobit	RE Tobit	Tobit with clustered SE		RE Tobit	RE GLS[b]	RE Probit	RE GLS with clustered S.E.	RE Tobit
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Own Income	0.106***[c]	0.085***	0.121***	0.088***	0.117***	0.091***	0.093***	0.112***	0.049***	0.046***	0.144***	0.45***	0.088***
	(0.017)[d]	(0.026)	(0.011)	(0.015)	(0.011)	(0.016)	(0.018)	(0.020)	(0.008)	(0.008)	(0.044)	(0.010)	(0.015)
Income Rank		1.349		2.871***		2.401**	2.792**	2.235*	1.193**	1.029**	0.997	1.235*	2.886***
		(1.396)		(1.038)		(1.143)	(1.260)	(1.359)	(0.537)	(0.519)	(1.260)	(0.717)	(1.047)
Comparison					-0.034**	-0.019		0.026					
Income					(0.017)	(0.019)		(0.029)					
Male	-3.231*	-3.875*	-1.248	-1.161	-1.389	-1.243	-0.774	-0.729	0.034	0.001		-0.205	-1.158
	(1.963)	(2.014)	(1.377)	(1.362)	(1.374)	(1.365)	(1.631)	(1.710)	(0.540)	(0.537)		(0.666)	(1.360)
Male*Income	0.0313	0.024	0.015	0.039	0.016	0.042	0.048	0.075	0.019	0.019		0.019	0.039
	(0.024)	(0.035)	(0.017)	(0.029)	(0.016)	(0.029)	(0.046)	(0.048)	(0.014)	(0.013)		(0.020)	(0.029)
Male*Rank		1.469		-2.209		-2.382	-3.019	-5.016	-1.118	-1.105		-0.952	-2.209
		(2.326)		(1.977)		(1.986)	(2.842)	(2.854)	(0.943)	(0.914)		(1.006)	(1.975)
Years of	1.284	1.277	0.044	0.045	0.071	0.061	-0.173	-0.106	-0.168	-0.162		-0.153	0.046
Education	(0.850)	(0.851)	(0.403)	(0.401)	(0.400)	(0.400)	(0.358)	(0.328)	(0.170)	(0.181)		(0.199)	(0.401)
Inverse Mill's Ratio												0.128**	0.177
												(0.571)	(1.578)
Period dummies				*Yes*							*Yes*		
Session dummies				*Yes*				*Yes*			*No*		*Yes*
Constant	11.128***	11.099***	5.918***	-6.066***	-4.062*	-5.029**	-5.465***	-7.539**	-0.440	-0.217	-4.224***	-0.489	-6.095***
	(3.687)	(3.684)	(1.934)	(1.929)	(2.106)	(2.193)	(2.345)	(2.807)	(0.813)	(0.854)	(1.350)	(1.019)	(1.945)
Number of obs.	180	180	378	378	378	378	378	378	400	400	400	378	378
Left-cens. obs.	99	99	214	214	214	214	214	214	22			214	214
Right-cens. obs.	1	1	6	6	6	6	6	6	6			6	6
R^2										0.507		0.49	
Log-Likelihood	-215.692	-214.151	-427.339	-423.448	-425.264	-422.978	-467.098	-482.757	-689.231		-48.002		-423.442
Wald $\chi 2$	108.31	110.44	226.48	236.08	230.63	236.58			362.66	342.15	14.46	173.71	236.73

Notes: [a] RE Tobit=Random Effects Tobit; [b] RE GLS=Random Effects Generalized Least Squares; [c] *** Significant at the 0.01 level; ** at the 0.05 level; * at the 0.1 level; [d] Standard errors in parentheses

Table 5. Effort, rank and comparison income in the survey data: Ordered Probits

	Willingness to work harder for the firm to succeed			
	(1)	*(2)*	*(3)*	*(4)*
Own Income	0.052*** [a]	0.035***	0.054***	0.039***
	(0.011) [b]	(0.014)	(0.011)	(0.014)
Income Rank		0.109**		0.096*
		(0.055)		(0.056)
Comparison			-0.039	-0.020
Income			(0.034)	(0.035)
Hours per	0.010***	0.010***	0.010***	0.010***
Week	(0.001)	(0.001)	(0.001)	(0.001)
Male	0.056**	0.070***	0.080**	0.080**
	(0.026)	(0.027)	(0.032)	(0.032)
Age	0.001	0.002	0.002	0.002
	(0.001)	(0.001)	(0.001)	(0.001)
Married	0.068**	0.070***	0.070***	0.071***
	(0.027)	(0.027)	(0.027)	(0.027)
Years of	0.009**	0.010***	0.012***	0.011***
Education	(0.004)	(0.004)	(0.005)	(0.005)
Country dummies			*Yes*	
Number of obs.	9854	9854	9854	9854
Log-Likelihood	-13441.2	-13439.1	-13440.3	-13438.9

Notes: [a] *** significant at the .01 level; ** at the .05 level; * at the .1 level; [b] Robust standard errors in parentheses.

Table 6. Effort, rank and comparison income in subgroups

	Effort in accepted contracts (experimental data)			
	Men		*Women*	
Own Income	0.106***	0.134***	0.093***	0.119***
	(0.031)	(0.016)	(0.016)	(0.019)
Income Rank	2.726		2.564**	
	(2.294)		(1.215)	
Comparison Income		-0.068*		-0.041*
		(0.041)		(0.021)
	Willingness to work harder for the firm to succeed (ISSP)			
	Men		*Women*	
Own Income	0.049**	0.069***	0.013	0.033**
	(0.020)	(0.016)	(0.018)	(0.014)
Income Rank	0.107		0.138	
	(0.071)		(0.088)	
Comparison Income		-0.054		-0.046
		(0.041)		(0.048)
	Non-Union		*Union*	
Own Income	0.031**	0.041***	0.036	0.089***
	(0.016)	(0.013)	(0.033)	(0.026)
Income Rank	0.052		0.247***	
	(0.063)		(0.092)	
Comparison Income		-0.023		-0.020
		(0.037)		(0.054)
	Public		*Private*	
Own Income	0.010	0.045*	0.066***	0.092***
	(0.025)	(0.023)	(0.024)	(0.018)
Income Rank	0.174*		0.124*	
	(0.094)		(0.074)	
Comparison Income		-0.022		-0.044
		(0.055)		(0.038)
	Not Manager		*Manager*	
Own Income	0.029	0.035*	0.027	0.061***
	(0.024)	(0.019)	(0.021)	(0.017)
Income Rank	-0.003		0.215**	
	(0.066)		(0.094)	
Comparison Income		-0.075		-0.031
		(0.047)		(0.041)

Notes: [a] *** significant at the .01 level; ** at the .05 level; * at the .1 level; [b] standard errors in parentheses. Other control variables as in Tables 4 and 5 for the experimental and survey results respectively. The experimental results come from random effect Tobits, the survey results come from Ordered probits with robust standard errors.

Table 7. Effort and past income in the experimental Information Treatment

Dependent variable	*Effort level in accepted contracts*		
Models	*RE Tobit (1)* [a]	*RE Tobit (2)*	*RE Tobit (3)*
Income	0.106*** [b]	0.098***	0.107***
	(0.013) [c]	(0.012)	(0.013)
Normalized Income Rank	2.368***	3.034***	2.844***
	(0.864)	(0.868)	(0.896)
Running Minimum Income	- 0.009		
	(0.015)		
Running Maximum Income	- 0.022*		- 0.038
	(0.013)		(0.024)
Running Minimum Rank		0.639	
		(0.904)	
Running Maximum Rank		- 4.259***	- 3.396**
		(1.417)	(1.453)
Demographic variables [d]	*Yes*	*Yes*	*Yes*
Period dummies	*Yes*	*Yes*	*Yes*
Session dummies	*Yes*	*Yes*	*Yes*
Constant	- 6.421***	- 5.296***	- 5.144***
	(1.171)	(1.307)	(1.308)
Observations	338	338	338
Left-Censored obs.	197	197	197
Right-Censored obs	5	5	5
Log-Likelihood	-351.655	-349.642	-349.446
Wald $\chi 2$	332.93	352.34	349.72
$p > \chi 2$	0.000	0.000	0/000

Notes: [a] RE Tobit=Random Error Tobit; [b] *** significant at the .01 level; ** at the .05 level; * at the .10 level; [c] Standard errors in parentheses; [d] The demographic and session variables are the same as in Table 4.

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