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# ABSTRACT

# Effort and Comparison Income: Experimental and Survey Evidence*

This paper considers the effect of status or relative income on work effort combining experimental evidence from a gift-exchange game with 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 others' average income, suggesting that comparisons are more ordinal than cardinal. We then show that effort is also affected by comparisons over time: those who received higher income offers or had higher income rank in the past exert lower levels of effort for a given current income and rank.

JEL Classification: M54, J33, A13, C92, D63

Keywords: effort, comparison income, rank, peak-end, experiments

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## 1. Introduction

One of the perhaps rare subjects that has inspired research across a variety of social science disciplines is that of status or social comparisons. A growing literature in economics is devoted to the role of comparisons in explaining a number of phenomena, including financial market behavior (Campbell and Cochrane 1999), unemployment hysteresis (Clark 2003), criminal activity (Glaeser, Sacerdote and Scheinkman 1996), or subjective well-being (Clark and Oswald 1996; Brown, Gardner, Oswald and Qian 2005; Ferrer-i-Carbonell 2005; Luttmer 2005).

One part of this literature has focused on the role of relative income in labor market outcomes. Quits have been shown to be negatively correlated with a reference wage given by the average wage in the firm for similar workers (Galizzi and Lang 1998); women's labor force participation is influenced by relative income (specifically, whether the woman's husband earns less than the woman's sister's husband: Neumark and Postlewaite 1998); and rank in the local income distribution is a good predictor of migration (Stark and Taylor 1991). 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, although efficiency wage theories are built on the concept of income comparisons (Akerlof and Yellen 1990) and relative concerns are considered to explain 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. It is commonplace to assume that wages have both incentive and selection effects (Prendergast 1999; Lazear 2000) and that reciprocity motivates employees to respond to high wages by greater effort (Akerlof and Yellen 1990; Fehr,

Kirchsteiger and Riedl 1993). Empirically, however, higher wages are not always associated with higher effort (Gneezy and Rustichini 2000). This can be 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; Baumeister 1984), 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 level depends on both own income and position in the relevant income distribution, due to social comparisons. In the context of worker effort, this reference income produces classic omitted variable bias.

We introduce an explicit measure of reference group income into both experimental and survey data to isolate the effect of income comparisons on worker effort. There is a strong correlation between work effort and own income in all of our specifications. We also identify robust effects of income comparisons on effort: those who are paid relatively well work harder.

Given own income, we can test which of relative income (i.e. the ratio of own income to comparison income) and income rank in the reference group is most important in determining effort: in other words, 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 changes over time in the individuals' own income or rank matter in determining their effort at work.

Many find the idea of social comparisons seductive, but conclusive empirical proof of their existence has been elusive. This is partly because 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 (Manski 1993). Our empirical strategy for analyzing

social comparisons in effort decisions, based on both experimental and survey data, allows both of these criticisms to be side-stepped.

The experimental approach has the double advantage of defining *a priori* the reference group, rather than having to infer it from survey data[1], and limiting contextual effects. In addition, it relies on actual and costly decisions instead of subjective reported behavior. Survey data, on the other hand, has the clear advantage of larger sample sizes, and avoids the criticism that laboratory experiments are to an extent unrealistic, either because participants are unrepresentative, or because behavior in an experiment is not typical of that in real life. The combined use of both survey and experimental data is still very recent (Fehr, Fischbacher, von Rosenbladt, Schupp and Wagner , 2003; Brown, Gardner, Oswald, and Qian 2005; Carpenter and Seki , 2005; Cummings, Matinez-Vazquez, McKee and Torgler, 2005) and can be seen as a joint test of robustness. If there are consistent patterns in both types of data, we can have greater confidence in the external validity of laboratory experiments.

Our laboratory experiment extends the gift-exchange game between an employer and an employee, as introduced by Fehr, Kirchsteiger and Riedl (1993), by allowing income comparisons between employees from various firms. The reference group for employees in this experiment consists of other employees participating in the same experimental session and placed under similar experimental conditions. All employees have the same cost of effort function. In contrast with recent attempts to show how co-workers' wages within firms affect effort (Güth, Königstein, Kovacs and Zala-Mezo 2001; Charness and Kuhn 2004), we set up firms with only one employee, and a reference group consisting of

[1] This ability to define precisely the reference group, and their income, is a considerable advantage. In survey data, even if we are sure who is in the reference group, we have to suppose that individuals know what others earn. Alan Krueger (personal communication) has carried out work asking people in two companies to identify another employee at the company that they were close to and then to estimate that person's wage. The correlation between this estimated wage and what the other employee actually earned (using the company's payroll records) was low.

other identical employees in different firms. This type of comparison has not been previously considered in the experimental literature. Focusing on a within-firm reference group instead would require us to consider multi-employee firms, and skill differentials to produce differentiated wage offers to employees. The between-firm comparison design allows us to consider *a priori* similar employees (who should thus 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 of the treatments (the Information 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. In the Benchmark Treatment no such information is given. Both treatments are conducted according to a perfect stranger matching protocol. This game is well-suited to study how income comparisons affect the extent of reciprocity between employers and employees.[2]

To test the robustness of our experimental results, we 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. We use the ISSP data to examine the correlation between effort and both reference group average earnings, and income rank within the reference group.

We have three key findings. First, both the survey and experimental data show that individual effort depends not only on own income but also on relative income. This stands in sharp contrast to standard economic analysis of the income-effort relationship. Second, rank in the income distribution matters more than the level of relative income, so

[2] In the rest of the paper, when we evoke income comparisons in the experimental data, 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 that depends on the level of effort chosen by the employee.

that comparisons are more ordinal than cardinal. Last, income is compared over time. For a given current income and income rank, effort is lower the higher the maximum income the individual received in previous periods; an analogous result is found for past income rank.

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.

## 2. Literature

The existing literature on social interactions or 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:

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

which (most often) gives rise to a decision rule for *i*'s utility-maximizing choice of behavior *a* 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[3], others' behavior can affect my own utility, but not my

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

behavior. There are equally problems with the utility approach, via equation (1): in particular, we do not necessarily know how to measure individual utility, $U_i$.

Interactions in behavior have been widely modeled econometrically despite the identification problems emphasized by Manski (1993).[4] 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 as well. One interpretation is that this 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 appeals to proxy measures of utility, such as life satisfaction, job satisfaction, and happiness. 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, testing specifications such as

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

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

[4] Recent contributions in this vein have analyzed saving (Duflo and Saez 2002), tax evasion (Fortin, Lacroix and Villeval 2004), labor supply (Woittiez and Kapteyn 1998; Aronsson, Blomquist and Sacklén 1999), and students' success at school (Arcidiacono and Nicholson 2005; Sacerdote 2001).

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 behavior as a function of both absolute and relative income

$$a_i=a(y_i, y_j, ...) \tag{4}$$

which can be operationalized empirically as

$$a_i = A_0 + \beta y_i + \phi y_j + \gamma' X_i + \varepsilon_i \tag{5}$$

This is the approach that we take in this paper. If only own income matters in explaining *i*'s behavior, then the estimated value of $\phi$ will be insignificant. On the other hand, if relative income is important in explaining behavior, then both $\beta$ and $\phi$ will be significant. If action *a* is normal then we expect $\beta > 0$ and $\phi < 0$.

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

$$e_i=e(y_i, y^*, ...) \tag{6}$$

where we expect $e_1 > 0$ but $e_2 < 0$. Here $y^*$ is considered to be 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 depending on reference income, and not only on his or her own wage.

Much of the efficiency wage literature is also based on the 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). However, empirical evidence that workers' effort does in fact depend on relative income remains slight.[5]

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 (2004), 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 retaliation from the lower-paid employee in terms of effort. 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 observed by Güth, Königstein, Kovacs and Zala-Mezo (2001) in a principal-agent game in which information about the contracts offered to each employee is manipulated. They show that principals tend to reduce the income differential between employees 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 productivity differences are considered as 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).

[5] 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).

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.

## 3. 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 will help to test the external validity of the experimental evidence.

### *3.1. 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 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 (2004). Workers do not differ in ability and they do not have to form beliefs about the relationship between incomes and other employees' productivity. A Benchmark Treatment and an Information Treatment were designed.

The Benchmark Treatment consists of the standard gift-exchange game. The use of this standard game guarantees 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 $w \in [20,120]$ 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, $e \in [0.1,1]$. The higher the level of effort, the higher the firm's profits but the greater the effort cost *c(e)* borne by the employee. This effort cost is convex, as shown in Table 1.

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 |

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

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

where $v$ is an exogenous redemption value; in our experiment, $v$=120. This expression guarantees that the firm cannot make any 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 knowledge. 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 so that no subject is with another subject more than once, and this is made

[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.

common knowledge in 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 offered by four other firms to their employee 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 a level of 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 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 might be possible that, in both treatments, income and effort be above the theoretically predicted levels. Indeed the literature has shown that an employee typically reciprocates a high (low) income offer by choosing a high (low) effort level that increases

(decreases) her firm's payoff (Fehr, Gächter and Kirchsteiger 1997; Fehr, Kirchsteiger and Riedl 1998).[7] In addition, it is also possible that information about the income distribution may affect the effort level 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 of 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 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 leaves us with a total of 200 wage offers in the Benchmark Treatment and 400 wage offers in the Information Treatment. Each employee made 10

[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.

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

contract acceptance decisions and, if the contract was accepted, chose an effort level.[9] 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. Each subject earned on average €14 from the experiment.

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

### *3.2. 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 (http://www.issp.org). The key variables in our empirical analysis are effort, earnings and hours of work. Individual yearly labor market earnings are converted to U.S. dollars using Purchasing Power Parities from the OECD. Hours of work are measured at the weekly level.

Our dependent variable is effort at work. In the ISSP, this 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*".[10] This question is remarkably close to the

[9] In the Benchmark Treatment, 20 contracts were rejected (respectively 22 contracts in the Information Treatment). We thus obtained 180 effort decisions in the Benchmark Treatment and 378 in the Information Treatment. The possibility of rejection may introduce selection bias in the analysis of the effort decisions. The small number of rejections does not however enable a two-stage estimation procedure to correct for potential selection bias. To avoid this problem, we could have eliminated in the protocol the stage at which subjects decide to accept or reject a contract, but doing so would not respect the standard game. The small proportion of rejections, coming from various different subjects, should not however bias the analysis.

[10] This is similar to variables used in Management to capture organizational commitment. See Jaworski and Kohli (1993).

context of the gift exchange game, in which any level of effort above the minimum increases the firms' profit but decreases the employee's payoff.

The weighted distribution of answers 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 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". Table 2 shows the number of observations and mean effort, ranked by country from the lowest to the highest 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.[11]

[11] 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

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 |

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 in fairly broad demographic groups,[12] here country, gender, education and age. There are three education groups (10 or fewer years of education, 11 to 13 years education, and over 13 years education), and three age groups (16 to 29, 30 to 44, and 45 to 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

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.

$y^*$ in equation (6) above.[13] Comparison income for individual $i$ in cell $j$ is calculated excluding $i$'s own income, which obviates the need to cluster the regressions at the reference group level.

## 4. 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.

### *4.1 Effort and comparisons to others*

We estimate discretionary effort equations on both datasets to determine whether income comparisons affect individual behavior in both experimental and survey data. In the experiment, the average income offered is 53.51 (Standard Deviation 19.7) in the Benchmark Treatment and 53.09 (S.D. 20.0) in the Information Treatment. Both are clearly above the equilibrium wage of 20 (one-tailed t-test, $p<.0001$) but there is no significant difference in average income between the two treatments. Firms do anticipate reciprocity from their employees, but they do not expect comparisons between them.

Two different specifications of comparison income are used. The first is in normalized rank form, defined as: rank in cell or group / number of observations in cell or group, with a correction for ties. 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 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. Estimation of the influence of comparison income on effort in the experimental data is via random effects Tobit. The use

[12] See for example van de Stadt, Kapteyn, and van de Geer (1985). A summary of the whole Leyden research programme in poverty and well-being is provided by van Praag and Frijters (1999).

of a Tobit model is justified by the number of left-censored observations in the sample. Table 3 shows that minimum effort was chosen 98 times out of 180 accepted contracts (54.4%) in the Benchmark Treatment, and 214 times out of 378 accepted contracts (56.6%) in the Information Treatment.[14] Estimation of effort in the ISSP survey data is via ordered Probit.

We find a positive relationship between income and effort in both treatments, as shown by the mean income per effort level in Table 3. This is typically observed in the gift exchange game (Fehr, Gächter and Kirchsteiger 1997; Fehr, Kirchsteiger and Riedl 1998), and is consistent with social motivations leading to reciprocity. While the income-effort relationship looks steeper in the Information Treatment, the joint presence of income and comparison income makes such bivariate conclusions untrustworthy.

Table 3. Average income and effort levels in accepted contracts

| *Effort level* | *.1* | *.2* | *.3* | *.4* | *.5* | *.6* | *.7* | *.8* | *.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 |

The effort regression results are reported in Table 4 below. The first panel corresponds to our experimental data and the second panel to the ISSP data. The first panel consists of six regressions. Regressions (1) and (3) control for own income in the Benchmark and in

[13] 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).

[14] 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 if it is attributable to the dissemination of income information. If the latter, some fraction of minimum effort decisions are motivated by social comparisons rather than selfishness.

the Information Treatments, respectively. In regressions (2) and (4), we add normalized income rank in the Benchmark and in the Information Treatments, respectively: a higher value of this rank variable thus corresponds to a higher position in the reference group income distribution. Since the subjects are not informed about their income rank in the Benchmark Treatment, this "placebo" variable should not be significant except if income and rank are strongly collinear. The next two regressions concern the Information Treatment only. Regression (5) replaces income rank by average reference group earnings (excluding own income), and the sixth regression includes both income rank and comparison income. Regressions (7) to (10) follow the same logic with the ISSP data: we include first own income, then add normalized income rank or comparison income, and last compare the joint influence of own income, rank and average reference group income.

In the ISSP data we also control for hours of work, age, gender, education and marital status, and country dummies. In the experimental data we only control for gender and post-baccalaureat years of education since there is little variance in age or marital status. In addition, controlling for a set of 9 period dummies (taking the first period as the reference) conditions for a learning effect. Last, we add a control for sessions to capture possible unobserved characteristics.

Table 4. Effort, Rank and Reference Group Earnings

| Variables | Experimental Data | | | | | | Survey Data | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Benchmark Treatment | | Information Treatment | | | | Willing to work harder for firm to succeed | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Own Income | 0.121*** | 0.102*** | 0.126*** | 0.096*** | 0.122*** | 0.099*** | 0.052*** | 0.036*** | 0.054*** | 0.039*** |
| | (0.012) | (0.019) | (0.009) | (0.013) | (0.009) | (0.014) | (0.010) | (0.013) | (0.010) | (0.014) |
| Income Rank | | 1.451 | | 2.469*** | | 2.089** | | 0.109** | | 0.096* |
| | | (1.093) | | (0.876) | | (1.006) | | (0.053) | | (0.057) |
| Comparison | | | | | -0.034** | -0.014 | | | -0.039 | -0.020 |
| Income | | | | | (0.017) | (0.019) | | | (0.028) | (0.031) |
| Hours per | | | | | | | 0.010*** | 0.010*** | 0.010*** | 0.010*** |
| Week | | | | | | | (0.001) | (0.001) | (0.001) | (0.001) |
| Male | -1.436** | -1.338** | -0.250 | -0.628 | 0.443 | -0.623 | 0.056** | 0.070*** | 0.080*** | 0.080*** |
| | (0.615) | (0.575) | (0.539) | (0.465) | (0.794) | (0.464) | (0.024) | (0.024) | (0.029) | (0.029) |
| 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*** |
| | | | | | | | (0.024) | (0.024) | (0.024) | (0.024) |
| Years of | 1.396*** | 1.436*** | -0.210 | -0.112 | -0.117 | -0.102 | 0.009** | 0.010*** | 0.012*** | 0.011*** |
| Education | (0.404) | (0.402) | (0.262) | (0.197) | (0.288) | (0.196) | (0.004) | (0.004) | (0.004) | (0.004) |
| Country dummies | | | | | | | | | *Yes* | |
| Period dummies | | | *Yes* | | | | | | | |
| Session dummies | | | *Yes* | | | | | | | |
| Constant | -13.126*** | -13.56*** | -5.262*** | -5.586*** | -3.698** | -4.82*** | | | | |
| | (2.205) | (2.199) | (1.286) | (1.099) | (1.449) | (1.474) | | | | |
| Number of obs. | 180 | 180 | 378 | 378 | 378 | 378 | 9854 | 9854 | 9854 | 9854 |
| Left-cens. obs. | 99 | 99 | 214 | 214 | 214 | 214 | | | | |
| Right-cens. obs. | 1 | 1 | 16 | 16 | 16 | 16 | | | | |
| Log-Likelihood | -216.0 | -215.1 | -426.5 | -422.8 | -424.6 | -422.5 | -13441.2 | -13443.4 | -13440.3 | -13438.9 |
| Wald $\chi 2$ | 134.4 | 138.8 | 250.6 | 269.9 | 256.4 | 269.0 | | | | |
| P > $\chi 2$ | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | | | | |

Note: (Robust) standard errors in parentheses in (ISSP) experimental data analysis . *** significant at the .01 level; ** at the .05 level; * at the .1 level.

Table 4 shows that effort is always strongly correlated with own absolute income at the 1% level. This result also holds in the Benchmark Treatment of the experiment. Moreover, in regressions (4) and (8), normalized rank attracts a positive and significant coefficient. For the same number of dollars/experimental units earned, individuals are willing to work harder the higher their position in the reference group income distribution. Not surprisingly, normalized income rank is not significant in the Benchmark Treatment (regression 2). It should be noted that this effect of normalized rank is robust to alternative specifications (not reported here); for example when we include own income squared in the regressions with the experimental data, rank is still significant at the 5% level. In the experiment, a rise in rank of one position (out of five) increases effort by 0.494 (=0.20*2.469), which is equivalent to a wage increase of 5.15 for given rank. Compared to average income per period (53.09), this represents a 9.7% income rise. The rank/income elasticity is thus 0.485 (=9.7/20). With the ISSP data, a 20% rank increase is worth $606 per month, which is 32% 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.

[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.

In regressions (5) and (9), average reference group income attracts a negative coefficient, but is significant only in the experimental data. If we include both normalized rank and reference earnings in the same regression (columns (6) and (10)), this marginally significant effect disappears, whereas the coefficient associated with rank remains significant. Our second key result is therefore that ordinal comparisons, as measured by normalized rank in the income distribution, are a more powerful predictor of employee behavior than cardinal comparisons, i.e. the differences in earnings expressed in currency units.[16]

Other results in Table 4 show that in the Information Treatment of the experimental data, gender and education have no significant impact on effort, whereas in the Benchmark Treatment they do. We have checked that the inclusion of these demographic variables in regressions 3 to 6 does not change the significance of the income variables. In the ISSP data, controlling for rank or average income, effort is higher for the males, 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. The estimates on the country dummies in the ISSP regressions (not shown) largely reproduce the effort ranking in Table 1.

### *4.2 Effort and comparisons over time*

The results so far have discussed the relationship between others' income and own effort. Here we 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

[16] This result concurs with that in Brown, Gardner, Oswald and Qian (2005), 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.

been tested with measures of satisfaction in panel data (see Clark 1999; Weinzierl 2005), but not with measures of behavior such as effort. In parallel, a separate literature on time-inseparability in behaviors such as consumption and labor supply has developed.

One difficulty in these literatures has been to ensure that *ceteris paribus* holds over the long time-periods between waves. 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. Our *a priori* is that higher past income will reduce current effort, with past income acting as a benchmark.

We pick up the effect of past income by including running maximum income and running minimum income as additional explanatory variables. That is, does effort at time *t* depend 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).[17] The period dummies in this regression pick up the fact that the running minimum (the running maximum) mechanically decreases (increases) over time, and avoid any spurious correlation between running minimum and maximum and the dependent variable. The usual demographic and session dummies are also included. The regression results are shown in Table 5.

[17] Data from period 1 are dropped as income (income rank) and running maximum/minimum income (income rank) necessarily coincide in this period. Therefore, period dummies include periods 3 to 10.

Table 5. Effort and Past Income in the Experimental Information Treatment

| Variable | (1) | (2) | (3) |
|---|---|---|---|
| Income | 0.106*** | 0.098*** | 0.107*** |
| | (0.013) | (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 | *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 |

Note: Standard errors in parentheses; *** statistically significant at the .01 level; ** at the .05 level: * at the .10 level. The demographic and session variables are the same as in Table 4.

Table 5 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 evidence 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 almost significant (12%).

## 5. 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, 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 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 choice may derive from inequality-aversion (see for example Fehr and Schmidt 1999; Bolton and Ockenfels 2000): 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: their effort does not depend on within-period comparisons as such, but by the search for the norm. However, if this were the case, they 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. In addition, they should also care about both own best and worst wages in the past, which is not the case.

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.

Over 20 years ago, Bob Frank (1985) suggested that firms can trade off status and wages. This paper has shown that these two are indeed substitutes in terms of inciting worker effort. Worker effort is lower in 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 levels of effort. 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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