Effort and Comparison Income: Survey and Experimental Evidence

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Abstract This paper combines ISSP survey data and experimental evidence from a gift-exchange game to determine the effect of status or relative income on work effort. We find a strong effect of others' incomes on individual effort decisions in both datasets. The individual's rank in the income distribution has a more powerful effect on effort than does others' average income, suggesting that comparisons are more ordinal than cardinal. We further show that, controlling for own income and income rank, the width of the relevant income distribution matters, with effort increasing in the distance from the bottom of the income distribution. Last, 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.

Résumé Cet article compare les données de l'enquête ISSP et celles issues d'un test expérimental d'un jeu d'échange de dons, afin d'identifier l'effet du statut et du revenu relatif sur l'effort au travail. Nous observons un effet important du revenu d'autrui sur les décisions d'effort individuelles dans les deux bases de données. Le rang de l'individu dans la distribution des revenus a un impact plus important sur l'effort que le revenu moyen des autres, ce qui suggère que les comparaisons sont davantage ordinales que cardinales. Nous montrons en outre que, contrôlant pour le revenu et le rang personnel, l'étendue de la distribution des revenus au sein du groupe de référence compte, l'effort individuel étant d'autant plus élevé que le revenu est éloigné du bas de la distribution. Enfin, l'effort est aussi affecté par les comparaisons inter-temporelles : ceux qui ont perçu des revenus plus importants ou ont bénéficié d'un rang plus élevé dans le passé exercent un effort plus faible à revenu courant donné.

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 has been devoted to the role of comparisons in explaining a number of behaviors, including financial market behavior (Campbell and Cochrane 1999), 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 relationship between relative income and labor market behavior. Quits have been shown to be negatively correlated with 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 employee effort within the job, although efficiency wage theories are built on the concept of income comparisons between co-workers and justify wage compression via equity concerns (Akerlof and Yellen 1990; Frank 1984).

In this paper we try to fill this gap. We analyze the influence of income comparisons on effort using both survey and experimental 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 is 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 suggest that under social comparisons, individual effort level will depend on both income and position in the income distribution. 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 survey and experimental 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 supplied at work: those who are relatively well-paid work harder.

Given own income, we 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 individuals compare up (to those who earn more), or down (to those who earn less), or both. Last, we consider 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 allows both of these criticisms to be side-stepped. Evidence is based on both experimental and survey data.

The experimental approach has the advantage of defining *a priori* the reference group, rather than having to infer it from survey data[1], and of avoiding any possible role for 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 joint 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 survey data come from the 1997 wave of the ISSP (International Social Survey Program). This survey includes data on both earnings and self-reported discretionary effort. We examine the correlation between effort and both reference group average earnings, and income rank within the reference group.

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

[1] More problematically, even if we are sure about 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.

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 other identical employees in different firms. 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]

We have four key findings. First, both the survey and experimental data show that individual effort depends not only on own incomes but also on relative incomes. 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. This suggests that comparisons are more ordinal than cardinal. Third, both survey and experimental data provide some evidence that individuals are more sensitive to downward than upward comparisons. The greater the distance of own income to the minimum income in the reference group, the higher the level of effort, at a given income level and income rank. 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.

[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.

The paper is organized as follows. Section 2 surveys the literature on social comparisons, utility and behavior. Section 3 presents the empirical strategy with respect to both the ISSP survey data and the laboratory experiment. 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 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 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

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

[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).

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

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

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 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

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

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 ϕ will be insignificant. On the other hand, if relative income is important in explaining behavior, then both β and ϕ 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

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

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. Equation (6) follows naturally from Adams (1963) equity theory, which states that, for person *i* and person *j*, individuals endeavor to ensure that the following equality holds:

$$\text{Outcome}_j / \text{Input}_j = \text{Outcome}_i / \text{Input}_i \quad (7)$$

The idea is that individual *i* has a comparison or reference person or group, *j*, and reduces his or her own input or effort until the ratio of their own income to effort is in line with that of the reference person.

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]

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

A recent literature in experimental economics examines the effects 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 only weakly employees' behavior, whereas firms reduce any income difference 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). 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 survey data and experimental data produced in the laboratory.

3.1. Survey Data on Work Effort

Our 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*".

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%

As a robustness check, we also use a second effort variable. Respondents are asked: "*Which of the following statements best describes your feelings about your job?*". The weighted percentages of ISSP respondents who chose each option are shown below:

I only work as hard as I have to	13.1%
I work hard, but not so that it interferes with the rest of my life	38.8%
I make a point of doing the best work I can, even if it does sometimes interfere with the rest of my life	48.1%

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" to the answers to the first effort question. Table 1 shows the number of observations and mean effort, ranked by country from the lowest to the highest effort.

Table 1. 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

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.[6]

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,[7] 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 y^* in equation (4) above.[8] 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.

There are a certain number of limitations to this survey-based approach. One salient point is that income is measured with a certain amount of error in surveys. More specific

[6] 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.

[7] 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).

[8] 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).

to this paper's concerns, the effort measure is self-reported rather than being observed directly. Last, we have posited a certain reference group (by country, gender, education, and age), but we cannot be sure that this is the most relevant one. For these reasons, we complement our survey analysis with experimental evidence on income and effort.

3.2. 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 have been designed.

The Benchmark Treatment consists of the standard gift-exchange game. 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 2.

Table 2. 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 this first-generation 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. 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).

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 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. In contrast, the firm is not informed about the other firms' income policies. 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.

Equilibrium of the game The equilibrium of this game is a minimum wage – minimum effort pair of decisions, [*w=20*, *e=0.1*], with selfish and rational players. 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. The 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)[9]. 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 that effort will 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.[10]

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 we organized. Two of these concerned the Benchmark Treatment and four the

[9] 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 could not be considered as intentional on the part of the employer.

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

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 which 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. 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. Each employee made 10 contract acceptance decisions and, if the contract was accepted, chose an effort level.[11] 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.

4. Results

Effort may be influenced by relative income or income rank, by the width of the income distribution, or by the income the individual received in the past if there are intertemporal comparisons.

[11] 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.

4.1 Effort and comparison income

We estimate discretionary effort equations on both datasets to determine whether income comparisons affect individual behavior in both survey and experimental data. 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 thousands of U.S. dollars per month in the ISSP data, and in experimental currency units in the experimental data. Estimation of effort in the ISSP survey data is via ordered Probit. Estimation of the influence of comparison income on effort in the experimental data is via random effects Tobit.[12]

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.[13]

It is also noticeable that social motivations lead employees to reciprocate higher incomes with greater effort as usually observed in the gift exchange game (Fehr, Gächter, and Kirchsteiger 1997; Fehr, Kirchsteiger, and Riedl 1998). We find a positive relationship between income and effort in both treatments, as shown by the mean income per effort level in Table 3. While the income-effort relationship looks steeper in the

[12] Tobit models are used in the experimental data to account for both the right and left-censoring of the effort variable.

[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 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.

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*
Benchmark										
Number obs.	98	22	16	12	11	9	4	5	2	1
Mean income	50.9	50.1	61.5	64.1	69.7	71.1	71.3	80.0	95.0	60.0
Information										
Number obs.	214	45	32	29	13	18	13	8	0	6
Mean income	44.4	59.2	65.4	64.0	69.6	75.6	80.8	79.4	0	93.3

The effort regression results are reported in Table 4 below. The first two panels correspond to the two different ISSP effort measures, and the third to the effort decisions in the Information Treatment of our experiment. Each panel consists of three regressions. The first regression controls for own income or income and hours of work. The second regression adds the normalized income rank: a higher value of this rank variable thus corresponds to a higher position in the reference group income distribution. The third regression replaces income rank by average reference group earnings.

In the ISSP data we also control for age, gender, education and marital status, and country dummies. In the experimental data we only control for gender since there is little variance in age, marital status and education. In the latter, we also include a "period 1" dummy variable, which takes the value 1 for the first period of the game and 0 otherwise, to account for a possible learning effect, and a "period 10" variable, which takes the value 1 for the last period of the experiment and 0 otherwise, to pick up end-game effects. Lastly, we control for a possible experience effect, with a dummy variable for having already participated in an economics experiment.

Table 4. Effort, Rank and Reference Group Earnings

Variables	ISSP Data						Experimental Data		
	Willing to work harder for firm to succeed			Works so hard that it interferes with life			Effort in the Information Treatment		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Own Income	0.052***	0.036**	0.054***	0.041***	0.008	0.039***	0.126***	0.100***	0.123***
	(0.011)	(0.014)	(0.010)	(0.012)	(0.015)	(0.011)	(0.008)	(0.012)	(0.008)
Income Rank		0.109**			0.215***			2.285***	
		(0.055)			(0.058)			(0.849)	
Comparison Income			-0.039			0.022			-0.038**
			(0.028)			(0.031)			(0.016)
Hours per week	0.010***	0.010***	0.010***	0.021***	0.020***	0.021***			
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)			
Male	0.056**	0.070***	0.080***	-0.251***	-0.224***	-0.265***	-0.674	-0.456	-0.245
	(0.026)	(0.027)	(0.029)	(0.028)	(0.029)	(0.032)	(0.117)	(0.393)	(0.375)
Age	0.001	0.002	0.002*	0.012***	0.0012***	0.011***			
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)			
Married	0.068**	0.070***	0.070***	0.038	0.042*	0.037			
	(0.027)	(0.027)	(0.024)	(0.025)	(0.025)	(0.026)			
Years of Education	0.009**	0.010**	0.012**	0.030***	0.032***	0.028***			
	(0.004)	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)			
Period 1							0.885**	1.022***	1.169***
							(0.383)	(0.384)	(0.402)
Period 10							-0.623	-0.583	-0.605
							(0.500)	(0.488)	(0.497)
Experience							-1.667***	-3.691***	-1.295**
							(0.595)	(0.587)	(0.515)
Country Dummies	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No
Constant							-4.994***	-3.145***	-5.406***
							(0.783)	(0.692)	(0.998)
Number of observations	9858	9858	9854	10068	10068	10064	378	378	378
Log-Likelihood	-13445.51	-13443.38	-13440.32	-9292.04	-9285.10	-9288.17	-435.70	-432.67	-433.83
Left-Censored obs.							214	214	214
Right-Censored obs.							6	6	6

Note: Robust standard errors in parentheses. *** significant at the .01 level; ** at the .05 level; * at the .1 level.

Table 4 shows that effort is almost always strongly correlated with own absolute income. This result also holds in the Benchmark Treatment. The same regression of effort in this base treatment, not reported here, has own absolute income positive and significant at the 1% level. Moreover, in the second regression in each panel, 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. In the experiment, a rise in rank of one position increases effort by 0.457 (=0.20*2.285), which is equivalent to a wage increase of 4.57 for given rank. Compared to average income per period (53.09), this represents a 8.6% rise. The rank/income elasticity is thus 0.43 (=8.6/20). With the ISSP data, a 20% rank increase is worth $606 per month, which is 32% of average income, giving 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 evidence from both survey and experimental data 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.[14] 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 the third column in each panel, average reference group income attracts a negative coefficient in two out of three regressions, but is not significant except in the experimental data. If we include both the normalized rank and the reference earnings in the third

[14] 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 by age.

regression (not reported here), 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.[15]

Other results in Table 4 show that effort increases with hours of work in the ISSP data. Also effort is higher for the married and the higher-educated. The estimates on the country dummies (not shown) largely reproduce the effort ranking in Table 1. Table 4 also shows that effort in experimental data is higher in the first round, and lower, but not significantly so, in the last round. There seems to be learning at the beginning of the game but no end-game effect. Last, those who have already participated in an experiment in the past exert lower levels of effort.[16]

The estimated coefficients on the male variable are of interest. These are positive and significant for the first ISSP effort variable (willingness to work hard). However, men are far less likely to agree with the statement that they work so hard that it interferes with their life. Rather than seeing this as a contradiction, we believe that this is a manifestation of the sharp differences in the time that men and women devote to domestic work. In the experimental effort regression, the male dummies are negative but not significant.

[15] 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.

[16] Kagel and Roth (1995) report many examples in which experience (of the game itself or of the same class of games) leads the subjects to play strategies closer to the equilibrium, although not systematically. This is particularly the case in auctions or public goods games. The impact of experience, considered here as learning how to play a particular class of game, has been particularly discussed in the case of voluntary contribution games. In our experiment, nobody had participated to a gift exchange game in the past but may have participated in a public goods game.

4.2 Upwards and Downwards Comparisons

Table 4 showed that individuals are motivated by both absolute and relative income. The specification in Table 4 treated all individuals as being equally important – they all contribute to mean income and to rank in the same way. However, it is also useful to ask if comparisons are symmetric upwards and downwards: Are some people more salient for comparisons than others?

This is easiest to see in the experimental data, where each individual is confronted with four other income offers that have been made to workers like herself before making her own effort decision. We estimate random effects Tobit models with own income and income rank, and look for up/down asymmetries by separately introducing the value of the minimum income in the distribution and the maximum income in the distribution.

We proceed in the same spirit in the survey data. Here, within each reference group, we calculate the income values corresponding to the 25th and 75th percentiles.[17] These are added separately to the effort regression which controls for income and income rank within the reference group. We also include controls for gender, age, education level, marital status, and country dummies.

If individuals are only sensitive to the first moment of the income distribution, then we expect these new variables to be insignificant. If, however, they respond to the spread of the distribution then we may expect some effect of these new variables on effort. In addition, if individuals are more sensitive to the bottom of the distribution than to the top (or vice versa), then the estimated coefficients on the two new variables will not be equal in absolute size. The results are in Table 5.

[17] These are only calculated for countries where income is measured continuously, rather than in bands (Hungary, Italy, Sweden, Poland and Switzerland).

Table 5. Effort: Upwards and Downwards Comparisons

Variables	Effort in ISSP Data		Effort in the Experimental Information Treatment	
	(1)	(2)	(1)	(2)
Own Income	0.045*	0.045*	0.108***	0.107***
	(0.024)	(0.024)	(0.013)	(0.014)
Income Rank	0.173*	0.178*	1.795**	1.965**
	(0.097)	(0.097)	(0.879)	(0.926)
25th Percentile / Minimum income	-0.107*		-0.026*	
	(0.058)		(0.015)	
75th Percentile /Maximum income		-0.075		-0.011
		(0.046)		(0.014)
Period 1			1.273***	1.032***
			(0.414)	(0.385)
Period 10			-0.607	-0.557
			(0.491)	(0.492)
Experience			-3.892***	-1.529***
			(0.612)	(0.593)
Gender	Yes	Yes	Yes	Yes
Age	Yes	Yes	No	No
Married	Yes	Yes	No	No
Years of education	Yes	Yes	No	No
Country dummies	Yes	Yes	No	No
Constant			-2.455***	-4.570***
			(0.781)	(1.098)
Observations	3250	3250	378	378
Log-Likelihood	-4286.55	-4286.96	-431.45	-431.92
Left-Censored obs.			214	214
Right-Censored obs			6	6

Note: Robust standard errors in parentheses. *** significant at the .01 level; ** at the .05 level; * at the .1 level.

With the introduction of income dispersion, own income and income rank remain positive and significant in both the survey and experimental effort equations. The dispersion measures themselves are only significant in one direction: downwards. In the ISSP data, individuals report working harder the greater is the distance between their own income and that of the 25th percentile. In the experiment, effort reciprocity is greater the larger the distance between the individual's own income and the minimum income revealed in the information treatment. It is worth emphasizing that these effects occur at given levels of both income and income rank.

These results emphasize the role of the income distribution, given the individual's position in it, in determining effort. If rank matters, then every point in the income

distribution potentially affects workers' effort. Table 5 suggests that there is an additional asymmetric distributional effect from incomes towards the bottom of the distribution. Comparisons seem more important downward than upward: individuals work harder when their income or rank is high, and when those beneath them are some distance away. This is consistent with loss-aversion in income rank.

4.3 Effort and Past Income

The results so far have discussed the relationship between comparison income and individual effort. Here we turn to comparisons to the income that the individual herself received in the past. The broad idea, as in the addiction literature, 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 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 with the experimental data only. Our *a priori* is that a higher past income will reduce current effort, with past income acting as a benchmark.

We pick up the effect of past income by including the running maximum income and the 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).[18] The regression results are shown in Table 6.

Table 6. Effort and Past Income in the Experimental Information Treatment

Variable	(1)	(2)	(3)
Income	0.116***	0.108***	0.105***
	(0.013)	(0.014)	(0.014)
Normalized Income Rank	1.865**	2.232**	2.585***
	(0.862)	(0.904)	(0.968)
Running Minimum Income	-0.002		0.004
	(0.013)		(0.017)
Running Maximum	-0.045***		-0.013
Income	(0.012)		(0.017)
Running Minimum Rank		1.217	-0.148
		(0.755)	(1.166)
Running Maximum Rank		-2.39**	-2.920**
		(1.25)	(1.504)
Male	1.182***	-0.717*	0.839**
	(0.452)	(0.371)	(0.407)
Period 10	-0.249	-0.384	-0.316
	(0.468)	(0.456)	(0.471)
Experience	-0.564	-1.433***	-0.541
	(0.519)	(0.498)	(0.683)
Constant	-3.138***	-3.652***	-2.777*
	(1.077)	(1.288)	(1.693)
Observations	338	338	338
Log-Likelihood	-364.14	-360.44	-363.37

Note: Standard errors in parentheses; *** statistically significant at the .01 level; ** at the .05 level: * at the .10 level

Table 6 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 (column 1), and the higher is best income rank achieved in the past (column 2). The running minima of income and rank do not seem to matter (although the positive coefficient on running minimum is almost significant at the ten per cent level). High income and high income rank seem to be particularly salient, however, and this evidence

[18] Data from period 1 are dropped as income (income rank) and running maximum/minimum income (income rank) necessarily coincide in this period.

suggests that they are used as benchmarks with which to evaluate the current offer's generosity, and thus the degree of reciprocity. The last regression in column 3 considers running maximum/minimum income and rank together and indicates that the past best position in the distribution matters more than the past best absolute income. Including the reference income in the regressions (not shown) does not change the level of significance of these key variables.

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 both ISSP survey and experimental data. We have four key findings.

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

Second, income rank (*i.e.* first, second, third 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.

Third, we suggest that the width of the income distribution matters independently of the individual's own position in it. Holding absolute income and income rank constant, individual effort is higher the greater the distance to the bottom of the distribution; distance to the top of the income distribution did not matter. To this extent, income comparisons are stronger downwards than upwards.

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 the experimental 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. Searching for the fair income also implies that individuals pay attention to both the top and the bottom of the income distribution, whereas for given income and income rank, individuals only care about the gap between their income and lower earnings.

A very general implication of our work is that combining survey analysis, based on subjective data, and experiments in a controlled environment serves as a validation exercise. While both approaches have been criticized for separate reasons, here they produce remarkably similar and consistent results.

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. They 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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