The Relative Income Hypothesis With and Without Self-reported Reference Wages*

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Abstract

Based on self-reported reference wages of Japanese union workers we provide new insights on the relative income hypothesis. The availability of a direct measure of peers' wages overcomes one of main limitations of previous empirical tests of the relative income hypothesis, i.e the subjectivity of the definition of reference group. Our data allows us to study the relationship between happiness and reference wages using the following measures of reference group wages: i) based on predicted individual wages; ii) based on cell averages from internal, as well as, iii) external data; iv) based on colleagues' average wage; and compare the results based on these proxies with the results based on the "true", self-reported, reference wage. We show that the proxies that have been used in the literature do not introduce a simple classical measurement error and that the bias can go in both directions. We propose a simple IV strategy when the elicited reference wage is not available that does not solve the bias but at least delivers an upper-bound of the true effect. We also deal with the possible endogeneity of self-reported reference wage due to an underlying pessimism that is correlated with life and job satisfaction.

Keywords: subjective well-being; relative utility; Mincer wage equation; comparison income

JEL classifications: D00; J28.

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1 Introduction

Ever since the publication of Smith, economist have noticed that individuals care about not only *absolute* levels of consumption and income but *relative* positions in their reference groups. Using macro data the seminal empirical work of Easterlin (1974) supported the relative utility hypothesis in a framework of happiness study.[1] Empirical investigation with micro data was initiated by Clark and Oswald (1996) and summarized by Clark et al. (2008). They argued that "the estimated coefficient on [reference income] variable in life satisfaction equations is negative, and equal in size to the positive coefficient on [own] income, suggesting that life satisfaction is totally relative in income".

One issue among previous studies on the relative utility hypothesis is how to construct appropriate measure of reference income. Clark et al. (2008) provided a summary of the literature, and argued that possible candidate methods include: i) based on predicted individual wages (Clark and Oswald (1996); Sloane and Williams (2000); Levy-Garboua and Montmarquette (2004); Senik (2004)); ii) based on cell averages from internal (Ferrer-i Carbonell (2005)), as well as, iii) external data (Cappelli and Sherer (1988); McBride (2001); Luttmer (2005); Clark et al. (2009b)); iv) based on colleagues' average wage (Brown et al. (2008); Clark et al. (2009a)); and finally the possibly "correct", self-reported, reference wage (Knight et al. (2009)).[2] This rich variety in proxy will be a corroborative evidence that choosing the right one is actually a difficult task for econometricians. Indeed, it has been authors' arbitrary choices and a convincing method to construct an appropriate measure has not been proposed. Also, with respect to studies with econometrically-predicted comparison income, underlying the approach

[1]For excellent expositions of the validity and applicability of happiness measures in economics research, see Hollander (2001) and Kahneman and Krueger (2006).

[2]Besides technical differences among different measures of comparison income, there would be a conceptual difference: some of comparison income capture the income of "people like me" while the others capture the income of those with whom the individual comes into close daily contact such as family, friends and work colleagues. (Clark et al. (2008)) Table 9 provides a brief summary of previous studies for readers' reference.

lies the premise that people will infer peers' wages in the exact same way econometricians do. This presumption has been criticized by several authors (Manski (1993) and Sloane and Williams (2000)) that this may not be the case, although they have failed to present any conclusive evidence to support their claims.

This paper utilizes survey data of Japanese union workers to address the above mentioned methodological issue shared among the previous studies on relative utility hypothesis. Specifically, with the great merit that we have data on self-reported *cardinal* measure of reference income, we compare our results based on this "correct" reference wage with the ones obtained from the other four types of "imposed" measures described above.[3] We also show that the proxies that have been used in the literature do not introduce a simple classical measurement error and that the bias can go in both directions. We propose a simple IV strategy when the elicited reference wage is not available that does not solve the bias but at least delivers an upper-bound of the true effect. We also deal with the possible endogeneity of self-reported reference wage due to an underlying pessimism that is correlated with life and job satisfaction.

The paper is organized as follows. We introduce our dataset in the next section. In section 3 we provide our empirical tests on the relative utility hypothesis. We also provide a theoretical argument and related empirical results about IV strategy when the elicited reference wage is not available. Section 4 provides concluding remarks.

[3]As far as we notice, Knight et al. (2009) is the only published work with subjectively reported reference income measures. Their measures, however, are *ordinal* ones and cannot be used for direct comparisons with the results from Mincerian reference wages, which is one of the main topics of the paper.

2 Data

2.1 Description

Our data set comes from the Comprehensive Survey of Labor Union Members, which was designed and applied by a group of psychologists at the International Economy and Work Research Institute. It comprises repeated cross-sections on about 130,000 Japanese union members working in Japanese firms listed on the Tokyo Stock Exchange (TSE) from 1990 to 2004. The survey requests that respondents provide self-assessments on their individual well-being at work and in life in general. In addition to this, other questions attempt to obtain information on workers' perceptions of their work environment.[4] The data set also allows us to control for individual demographic and socioeconomic characteristics, which include age, gender, educational attainment, tenure at the current firm, annual wage level, overtime hours worked, and workers' expectations of their peers' wages.

After cleaning the data and getting rid of some inconsistencies, we are left with approximately 90,000 observations. Table 1 shows some statistics that describe our data set. Workers in our sample are young with an average age of 35 years. Their average tenure is 14 years, which suggests relatively low mobility in the Japanese labor force. Moreover, union workers in Japan seem to be well-educated in general, as 46 percent of them have graduated from high school, 10 percent have some college experience, and 34 percent have completed their university-level degree. We also observe that 58 percent of workers are married and work an average of 23 hours of overtime per month. All of these individuals are regular full-time employees and union members. About one third of them hold blue-collar positions and close to one fifth of them perform some managerial role in the company. These numbers do not intend to provide an accurate depiction of the representative Japanese worker since the survey was administered exclusively to union

[4] The full list of question categories is available in the working paper version of this paper; see de la Garza et al. (2008), Appendix 1.

members employed by major public companies, as described above. This implies that the results below may not be generalizable to the entire Japanese labor force—for instance, our data set does not cover employees in high administrative positions as they are not allowed to take part in unions. Nonetheless, due to the large size of our sample and the breadth of coverage of 62 firms across a variety of industries from food to electronics to finance, we believe that this data set does reflect significant trends of the whole Japanese labor market.

One matter to keep in mind, however, is that our data undersamples women in the Japanese labor force. According to the World Bank's World Development Indicators data, women represent about 40 percent of the country's working population; in contrast, the share of female participation in our sample is only 23 percent. This issue is important, not particularly because of the different gender breakdown in our sample relative to that of the Japanese labor force, but because male and female workers differ significantly along other dimensions, as shown in Tables 2 and 3. For instance, while 39 percent of the male subsample obtained a college degree, only 16 percent of the female group achieved this goal. Interestingly, the share of married men is twice the proportion of married women (66 percent vs. 33 percent). Since, as discussed below, the literature has found significant correlations between various subjective well-being measures and individual characteristics such as gender, educational attainment, and marital status, pooling the male and female subsamples in our analysis may lead to results that would differ had we considered these two groups separately. We will thus keep this distinction in mind and will bring this issue up when necessary.

2.2 Subjective-Well Being

Each respondent is asked to provide information on his own level of life and job satisfaction from a list of 5 categories, where 1 corresponds to "least satisfied" and 5 denotes "most satisfied." The first thing to note is that there are significant differences between these two subjective well-being measures. First, the correlation between the two is a mere 27 percent. As Figure 1 suggests, the distribution of life happiness is more spread out and more skewed to the left than that of job satisfaction. Since the literature that investigates the relative utility hypothesis has utilized a wide variety of subjective well-being measures, the availability of these two variables makes possible to test whether our results are robust to the use of alternate metrics for satisfaction. For brevity, our empirical analysis will utilize life happiness as our dependent variable. However, we will provide some additional discussion when the use of job satisfaction as an alternative proxy for subjective well-being leads to different results.

2.3 Workers' Own Wages and Relative Wages

The survey also requests that workers mark down their own wage level from a list of 9 categories, where category 1 denotes annual wages of under 2 million yen and category 9 corresponds to an annual income level of over 10 million yen. In the analysis that follows, we measure individual wages as the mid-point in each of the 7 intermediate categories, and use *ad hoc* values close to the minimum and maximum wage levels for the two extremes. Thus, respondents who reported categories 1, 2, . . . , 9 as their wage level, were assigned annual wages of 1.5; 2.5; . . . ; 12 million yen, respectively. Alternative choices do not alter the main results. Additionally, we deflate this nominal measure using the Consumer Price Index to obtain real wages with 1990 as the base year.

Table 4 shows typical log wage regressions for men and women, separately. The first thing to notice is the very high R-squared one obtains using Japanese union workers.[5] For male workers controlling for age, tenure, education, hours worked, and marital status gives

[5]This feature depends only marginally on wages being reported in classes. When we smooth wages adding a disturbance term that is uniformly distributed between each cutoff point the R-squared drop by a maximum of 5 percentage points.

an R-squared of 66 percent. A similar regression using US data would probably generate an R-squared which is at least half that size. Across all specification the variation of log wages explained by the same observable characteristics for women is approximately 10 percentage points smaller than for men but still much larger than an equivalent regression using US data would give. Adding age and tenure dummies adds approximately another 5 percentage points to the R-squared, while firm dummies explain another 3 percentage points of the variation for men and 4 percentage points for women. Explaining a large part of the variation in wages minimizes the concern that wages might themselves depend on happiness, and thus be endogenous. The Japanese labor market is based on a very strict seniority system. The correlation between log-wages and age for the whole sample is 69 percent, while during the same years the Japanese surveys was conducted based on the Current Population Survey unionized workers of a the same group show a correlation between age and log-wages of just 21 percent. If job and life satisfaction do not vary discontinuously across age and tenure other than through tenure one can use age and tenure dummies as an instrument for wages. We pursue this strategy in Section 3.

2.4 Different measures of Reference Wage

One of the main advantages of our data is that they allow to replicate several measures of reference wages used in the literature and to compare them to the elicited one. For the elicited reference wage we use the information from the question phrasing as "What do you think the average wage of corporate employees who are your age and doing the same job, is?" The answer to this question is also chosen from a list of 9 categories, and we refer to it is as the "truly perceived" reference wage ($\bar{w}^*$) in contrast to predicted reference wage k ($\bar{w}^k$), where k denotes the method that is used to define the reference group. We use a continuous version of the variable like we did for the individual wage.

The reference wage from external data comes from the Basic Survey on Wage Structure

(BSWS)" released by the Ministry of Health, Labour and Welfare.[6] The cell is defined over gender (2), age category (9), education (4), and year (14). We use BSWS information on private companies with more than 1,000 employees.

Figure 2 shows density of (log) wages, self-reported reference wages and reference wages computed from external sources. From the figure we can see that two measures of reference income capture the pessimistic tendency among Japanese workers that their peers' are earning more than they do. Table 5 shows the descriptive statistics of the different measures of reference wage that have been computed. According to the table, the pessimistic tendency amongst Japanese workers can be estimated around 8 percent (15.484 – 15.398).[7] The log average of the external reference income is even higher than the log average of self-reported one by 2 percent. The reason for this is that in BSWS data information on "assistant general manager" (ka-cho in Japanese) is included while in our data set employees are non-supervisory or assistant managers.

3 Subjective Well-Being Regressions Using Self- Reported Reference Wages

Dividing workers' earnings into three categories, low, medium and high only 50 percent of workers in the lowest category reports being satisfied in life, while this number goes up to approximately 60 percent for workers with medium earnings and 70 percent for workers with high earnings. On the other hand one might assume that relative rather than absolute wages matter. Figure 3 does indeed show that workers with higher levels of life and job satisfaction tend to exhibit earnings that are larger than their perceived reference wage. Whether own wages matter more or less than reference wages seems to

[6]The data can be found of the following website www.mhlw.go.jp/english/database/db-l/.

[7]The average of our individual predicted wages is slightly higher than those of the other proxies of reference income generated from the data set. This happens because we loose about 8,000 observations due to missing industry information.

be an empirical question.

In order to examine the impact of absolute and relative wages on workers' subjective well-being, researchers typically estimate the following regression:

$$\mathrm{SWB}_i = \alpha_1 y_i + \beta_1 \bar{y}_i + x_i'\gamma + u_i, \tag{1}$$

where SWB_i represents a measure of worker i's subjective well-being, such as happiness or job satisfaction. This proxy for utility is assumed to depend on the worker's wage, y_i;[8] a reference wage that workers use as a benchmark to determine how well they do relative to their peers, $\bar{y}_i$; and a vector of individual characteristics, x_i, that includes age, tenure, gender, educational attainment, marital status, among other socio-demographic factors. As usual, the term u_i corresponds to an idiosyncratic error term.[9]

One crucial matter that the empirical literature investigating the relative utility hypothesis has faced relates to the difficulty in obtaining an appropriate reference wage measure. First, it is not obvious who belongs in the comparison group. Do workers compare themselves to other workers in the same company? In the same industry? To their relatives and friends? To their neighbors? To people of their same age and education level? To workers of the opposite sex? The possibilities are endless. A second and perhaps more subtle issue is that, ideally, the econometrician would like to have information on workers' perceptions about their peers' wages but such self-reported beliefs are typically

[8] In Section 2.3 we showed that most of the variability of Japanese union wages can be explained by the typical regressors used in Mincer type regressions. Later we will show how to deal with the possibility that wages are endogenous. Unless otherwise noted, all wages are in logs.

[9] This specification can be thought of as a reduced-form version of a standard utility function of the form:

$$U = U(c, \bar{c}, h),$$

where c is individual consumption, $\bar{c}$ is the level of consumption of a comparison group, and h is hours worked. The theoretical literature has investigated how much keeping-up-with-the-Joneses ultimately matters to consumers and what role social status plays in determining individual utility levels. These studies find that, in addition to their own levels of consumption, individuals care about their peers' consumption levels and their wealth rank relative to their comparison group, which validates the use of reduced-form models such as Eq. 1. For instance, see Cole et al. (1992), Corneo and Jeanne (1997), and Yamada (2008).

unobserved.

To solve this issue, researchers have followed mainly two approaches. The first one recurs to Mincer equations to predict the wage of a worker with certain individual characteristics, and then uses this predicted value as the reference wage in an equation such as 1. The second one computes wage averages within group cells, where the groups are well-defined according to given workers' characteristics. One obvious problem with these methodologies is that workers do not necessarily "compute" reference wages or even define comparison groups the way econometricians do. Moreover, these approaches imply that the comparison wage is some sort of average of other workers' wages that share similar socio-demographic characteristics, and thus ignore other factors, such as a worker's inherent pessimistic or optimistic attitude, which may influence the worker's perception of who her peers are and what they earn.[10]

In what follows, we investigate empirically the criticisms that the literature has raised against both the use of Mincer equations and the use of cell-averages to compute reference wages. One advantage that we have over previous studies is that our data set contains information on workers' perceptions about their peers' wages. We take these self-reported reference wages to be the "correct" benchmark that workers use to compare themselves against their peers, and compare our results to the findings that we would have obtained had we followed the alternative methodologies that Manski (1993), Lydon and Chevalier (2002), and others have deemed potentially flawed.

Before proceeding, a couple of remarks are in order. First, it is important to note that referring to this self-reported measure as the "correct" reference wage does not imply that our approach solves all of the problems that have afflicted the aforementioned alternative methodologies. For instance, by asking workers to report what they believe that other employees with their same characteristics *and in the same company* earn, the survey restricts workers to define their reference group to be precisely people within their same

[10]See Manski (1993) and Lydon and Chevalier (2002) for a more thorough critique of these approaches.

company. Still, on a more regular basis, workers may compare themselves to relatives, friends, or colleagues in other firms within the same industry or even in other industries. Moreover, the relevant reference group may not be stable over time. For instance, a recent graduate may compare herself to other recent graduates; but a person who has been out of school for 10 years may compare herself to former classmates and colleagues, to people in her same cohort and within her same tenure range, to supervisees and supervisors, and even to former selves. Nonetheless, we believe that empirical tests of the relative utility hypothesis that make use of reference wage data as provided by workers themselves should be regarded as a superior alternative to analyses that construct comparison income measures from Mincer equations or cell averages.

A second issue to bear in mind is that a worker's belief of her peers' wages may be endogenous as a relatively happier individual may have a more optimistic view of what her colleagues earn relative to herself. Conversely, a pessimistic attitude in general may lead a worker to think that she is underpaid with respect to her peers. With panel data, one could difference out time-invariant characteristics that are inherent to a worker, such as pessimism, and obtain unbiased estimates of the effect of absolute and relative wages on happiness. Unfortunately, our data set does not allow us to track individuals over time. However, our survey does ask two questions that are likely to capture workers' pessimistic attitudes along two different dimensions. The first question asks whether workers believe that their colleagues would help them if they were in need; the second question asks them if they are satisfied about the possibility of promotion within their company. Controlling for these two effects should capture at least some of the inherent pessimism or optimism of a given worker, which would in turn give us a more precise estimate of the reference wage effect on our subjective well-being measures.

3.1 Empirical Tests of the Relative Utility Hypothesis

This section discusses our estimation of Eq. 1 using happiness as our dependent variable and self-reported wages as our reference wage measure. We denote this possibly "correct" comparison measure as $\bar{y}_i^*$ to distinguish it from the imperfect proxy that the literature has constructed using the Mincer and the cell-average approaches, which we denoted $\bar{y}_i$. As discussed in Section 2, there are important differences between male and female workers in almost every dimension from wages to hours worked to educational attainment, which justifies our estimation of Eq. 1 for men and women separately. We will then show that pooling the male and female subsamples does not have any significant qualitative impact on our findings. Also, the errors u_i are allowed to be correlated within firms and, as will be shown in our robustness tests, different clusterings do not substantively affect our conclusions.

The empirical results in Table 6 corroborate the validity of the relative utility hypothesis. The absolute wage coefficient is consistently positive across specifications and is always statistically significant at the 1 percent level, which implies that workers' life satisfaction increases as their wage goes up. This is akin to Alesina et al. (2004)'s finding that "money buys happiness". Holding their wages constant, however, individuals tend to report lower levels of satisfaction when they perceive that their peers' wages are higher. These results hold for both the male and the female subsamples. It is important to note that the effect of absolute wages is stronger than that of relative wages; that is, if there were an across-the-board wage hike of, say, 10 percent, that was also reflected on worker i's perceptions of her peers' wages, the positive effect of worker i's wage increase on happiness would outweigh the negative impact that an increase on her peers' wages has on life satisfaction. This finding is confirmed by an F-test that rejects the null hypothesis that the sum of the coefficients on absolute and relative wages is equal to zero. Another important result is that the absolute value of the coefficients on both absolute wages and reference wages are between 35 to 50 percent higher for males than they are for females. Even though we only show one "representative" specification for the female subsample. in column 6, we observe that these gender differences are consistent when we separately run the same specification on the male and the female group.

The results also show that individual characteristics explain a significant amount of the variation in reported levels of life satisfaction. For instance, in the case of males, controlling for age, tenure, educational attainment, marital status, and dummies for whether the worker has a blue-collar position and whether it performs any managerial tasks in the company, increases the R-squared from 2.2 to 7.1 percent. As expected, given that these covariates have proven to be important determinants of wages, their inclusion has a significant impact on the absolute wage effect on happiness. In particular, accounting for these individual characteristics causes the impact of wages on life satisfaction to decrease from 0.69 to 0.56, a 19 percent decrease. Moreover, workers that tend to be more pessimistic about the helpfulness of their colleagues and about the prospects of promotion within their company tend to report lower levels of happiness. Thus, the inclusion of these two pessimism variables leads to a further decrease in the impact of wages on life satisfaction, from 0.56 to 0.47, an additional 16 percent drop. Such pessimistic attitudes also takes a toll on the reference wage coefficient, as the effect of this variable on happiness is reduced (in absolute value) from -0.39 to -0.30, a 23 percent drop relative to the no-control specification in column 1. One additional thing to note is that these two pessimism variables by themselves explain an additional 4 percent of the variation in life satisfaction, which corroborates the relevance of those characteristics that are inherent to the individual worker in determining his reported level of subjective well-being. Overall, we conclude that pessimism about colleagues' helpfulness at work and the possibilities of job promotions has large and significant negative effects on life satisfaction, and that these effects are of similar magnitude for both men and women.

One possibility that may have an impact upon the absolute and the relative wage coefficients is the average level of wages and their dispersion across the firm. On the one hand, higher company's average wages may be a proxy for job amenities. They may also have a positive effect on a worker's satisfaction level if we believe they provide a signal about her potential to rise within the firm's wage ladder (Clark et al. (2009a), Senik (2004), Hirschman and Rothschild (1973), Manski (2000)). Thus, if the worker believes she has greater possibilities of increasing her compensation in the future, the negative impact of higher peers' wages would decrease in absolute terms—which would then imply that omitting average wages in the happiness regression would overestimate the reference wage coefficient. On the other hand, greater wage dispersion within the firm may also have a significant impact both on satisfaction and on the effect of absolute and relative wages on happiness. For instance, if workers know that there is greater variance in compensation packages offered by their company, that may influence how they feel about their position in the wage spectrum relative to their colleagues. We explore these two issues by controlling for the *log* of average wages and the interquartile range of *log* wages within the firm. We observe that higher average wages in the firm has a positive effect on reported levels of life satisfaction for male workers, but not for females. The converse is true of the wage interquartile range—greater wage dispersion has a negative effect on happiness for women but this effect is insignificant for men. Additionally, the inclusion of these variables does not affect substantially the impact of absolute and reference wages on life satisfaction for either subsample; that is, the relative utility results still hold. This is also true for the pooled sample, although in that regression the coefficients on mean wages and the wage interquartile range are both statistically insignificant.

In sum, the findings reported in Table 6 seem to suggest that workers who earn higher wages are indeed happier. They also show that individuals who perceive that their colleagues earn more tend to report lower levels of satisfaction. A crucial point that has not been thoroughly discussed in the literature, however, is that, although both the absolute and the relative wage coefficients on happiness are strongly statistically significant on a consistent basis, the size of these effects is relatively small. Take for example the coefficients reported in column 1, which are the largest in absolute value among the different specifications. Even in this case, the coefficient on absolute wages of 0.69 implies that a one-standard-deviation increase in the wage of the average worker—equivalent to a 38 percent hike—would increase happiness by 0.27 points in a 5-point scale, or 8.2 percent increase relative to the mean. It is somewhat surprising that such a relatively large wage increase would have such a small impact on a worker's life satisfaction level, although this perhaps lends support to our earlier point that worker-specific characteristics constitute some of the most pertinent determinants of happiness. We do not believe that these relatively small coefficients should justify the dismissal of subjective well-being and reference wage studies. Nonetheless, we do believe that the literature should pay more attention to the magnitude of these effects.

3.2 Robustness Checks for Self-reported Reference Wages

For brevity we show our robustness check for the male sample only. Table 7 shows that our main results, shown in column 1, are relatively similar when we use job instead of life-satisfaction as a measure of wellbeing. Notice that the sample is slightly smaller sample since industry information is missing for approximately 6,000 men. The coefficients are smaller in absolute terms, but even for job-satisfaction own wages seem to matter more than reference wages. In column 3 we add year and industry dummies and the results do not change.

Next we address a possible endogeneity issue: workers might choose their reference group through residential location, and thus it might just be that people who are happy move to poorer neighborhoods, leading to the negative relationship that we observe, or that more wealthy neighborhoods generate positive externalities, like less crime, better schools, etc, which has a positive effect–and thus is positively correlated–with measures of wellbeing. Ideally we would like to control for neighborhoods fixed effects but such information is not available. Instead column 4 controls for company fixed effects. Our companies are quite large and might have more than one establishment,[11] and corresponding fixed effects might average across different location. Nevertheless, it is comforting that controlling for company fixed effects leaves our results practically unchanged.

In column 5 we perform a horse-race between self-reported reference wage and the reference wage from external data (see Section 2.4). The coefficient on the external reference wage has the right sign but is not significantly different from zero, while again the coefficient on the self-reported reference wages is practically unchanged. In column 6 we address the endogeneity of the wage using tenure dummies and age dummies as instruments. The identifying assumption is that thanks to the Japanese seniority system wages vary discontinuously over time due to either age effects or tenure effects, while age and tenure affect happiness independently only linearly.[12] The IV coefficient for both wages tend to be larger than the OLS ones.

Column 7 concludes our robustness checks. In that column we use an ordered probit instead of the OLS. The coefficients are not directly comparable with the OLS ones but the relative size of the coefficient on wages and the one on reference wages can be compared and the results are basically unchanged.[13] This result is in agreement with findings by Luttmer (2005) that the use of least-squares estimators in happiness studies does not impact negatively the general conclusions, even when ordered probit or logit models would be methodologically preferred given the hierarchical and non-linear nature

[11] We observe approximately 90,000 workers and 62 firms, which means that each company has on average 1,500 workers.

[12] Adding the squared value of age and tenure does not alter the IV results but the F-statistic of the instruments becomes very small.

[13] Nevertheless, the coefficients turn out to be similar in magnitude to the OLS case partly because the root mean squared error of the baseline regression is 1.04 and thus close to unity, to which the error term in the latent model of the ordered Probit is normalized.

of the dependent variable.

4 Testing the Relative Utility Hypothesis when Self-Reported Reference Wages are Unavailable

Most times data on perceived and self-reported reference wages, denoted with a star in this paper, are unobserved. What researchers usually use as reference wage is

$$\bar{y}_i = \alpha_0 + \beta_0 \bar{y}_i^* + \epsilon_i \quad , \tag{2}$$

a mismeasured version of the individuals' "truly" perceived reference wage. Notice that we are only imposing linearity, while, unlike in the classical measurement error problem, we allow the scale of the two measures of reference wages to be different. This represents a generalization of the classical measurement error model, where $\alpha_0 = 0$ and $\beta_0 = 1$. If, for example, people with low wages systematically perceive the reference wage to be larger then actual data would suggest $\overline{y}_i^* > \overline{y}_i$ for low levels of $\overline{y}$, and thus α_0 would be larger than 0 and β_0 smaller than 1. Like in the classical measurement error model we assume that the error term is uncorrelated with the true reference wage. Substituting Eq. 2 into Eq. 1 we get that

$$\mathrm{SWB}_i = \alpha_1 y_i + \beta_1(\beta_0 \bar{y}_i^* + \epsilon_i) + x_i'\gamma + \beta_1\alpha_0 + \mu_i + u_i, \tag{3}$$

In such a model a bias arises as a function of, both, the variance of ϵ_i and of the distance between β_0 and 1. In particular, assuming for simplicity that there are no other regressors, the probability limit of the OLS estimate of β_1 is

$$plim\widehat{\beta}_1^{ols} = \frac{COV(\beta_0\bar{y}_i^* + \epsilon_i, SWB_i)}{VAR(\beta_0\bar{y}_i^* + \epsilon_i)} = \frac{\beta_0 COV(\bar{y}_i^*, SWB_i)}{\beta_0^2 VAR(\bar{y}_i^*) + VAR(\epsilon_i)}$$
$$= \beta_1 \frac{\beta_0}{\beta_0^2 + VAR(\epsilon_i)/VAR(\bar{y}_i^*)} \tag{4}$$

Thus, unless $\beta_0 = \beta_0^2 + VAR(\epsilon_i)/VAR(\bar{y}_i^*)$, $\widehat{\beta}_1^{ols}$ is not consistent. The direction of the bias is unclear.

What if we use two independent proxies of $\bar{y}_i^*$? Using an IV strategy, for example, proxy a as an instrument for proxy b:

$$plim\widehat{\beta}_1^{iv} = \frac{COV(\beta_0^a\bar{y}_i^* + \epsilon_i^a, SWB_i)}{COV(\beta_0^a\bar{y}_i^* + \epsilon_i^a, \beta_0^b\bar{y}_i^* + \epsilon_i^b)} = \frac{COV(\bar{y}_i^*, SWB_i)}{\beta_0^b VAR(\bar{y}_i^*)} = \frac{\beta_1}{\beta_0^b} \tag{5}$$

Thus with IV the bias does not depend on the variance of the measurement error anymore, but a bias persists. Taking the ratio between $\widehat{\beta}_1^{iv}$ and $\widehat{\beta}_1^{ols}$ one can easily see that whenever OLS and IV give similar results the signal-to-noise ratio $VAR(\bar{y}_i^*)/VAR(\epsilon_i)$ has to be large. Unfortunately β_0 might still be different from one. Since workers with low wages tend to report a reference wage that is higher than their own wage, and *vice versa*, the regression of predicted reference wages on self-reported ones shows a positive constant term and a slope that is smaller than one. Given that $0 \leq \beta_0 \leq 1$ a first simple rule to minimize the bias for the IV case is to choose the specification which gives the lowest coefficient $\widehat{\beta}_1$.

Table 8 and 9 show our estimated $\widehat{\beta}_1^{ols}$ and $\widehat{\beta}_1^{iv}$ for a grand variety of proxies of reference wages. Columns 1 and 2 of Table 8 show the unbiased $\widehat{\beta}_1$, first with then without additional controls. The remaining columns use two different kind of proxies. Column 3 and 4 use the average log wage by education, age, and year extracted from Basic Survey on Wage Structure (BSWS). The coefficient is larger (in absolute terms) than the "true" when we do not control for other X's, while it smaller but still significant when we do control for other Xs. This external measure can easily be used as an instrument for proxies derived from the survey data. Columns 5 to 8 use a simple linear prediction of individual log wages as reference group. Following the literature we included as many regressors in the Mincerian equation as possible in order to meet the exclusion restriction.[14] Results are not very encouraging, at least not when other regressors enter the happiness equation, which is custom. The reason is that conditional on all other Xs the linear prediction and individual wages are highly collinear. If on top of that we add an instrumental variable that is conditionally not highly correlated with the mismeasured variable, and has a low $\widehat{\beta}_0$, we get such unreasonable results.[15]

Table 9 shows that survey-based cell averages provide more robust estimates, even when instrumented. Columns 1 to 4 use gender $\times$ age cells, while columns 5 to 8 add education, and columns 9 to 12 education and whether the workers has managerial tasks to the variables that define the cells. Unlike the linear prediction case, $\widehat{\beta}_1^{ols}$ looks reasonable even when we control for other Xs and instrumenting delivers estimates that are clearly a lower-bound for the true effect. If we had to pick one proxy, based on the IV results that control for other Xs we would clearly pick the second proxy, the one that computes cell averages based on educational levels, gender, and age groups. That is the specification with the highest $\widehat{\beta}_0$ (columns 7 and 8). Such a parsimonious definition of reference group, based on just age, gender, and education is in line with the definition used by the Leyden school. One way to pick the preferred OLS would simply be to pick the one that corresponds to the best IV (columns 5 and 6). It is interesting to note that while the IV is clearly downward biased, the measurement error variance pushes the OLS estimate in the right direction but by too much. At the end the true lies often in between the OLS and the IV estimates.

[14] We control for the following fixed effects, gender, age, year, industry, tenure, education, occupation, blue collar, marital status, job shock dummies, and for the log of average hours worked. See 10 for a list of papers that use wage regressions to predict reference wages.

[15] Other studies have found a positive correlation between life satisfaction and linear predictions of wages **?**.

5 Conclusions

In this paper, we investigate the relative utility hypothesis with varieties of proxy of reference income employed in previous studies. Having data on the self-reported *cardinal* measure of reference earnings we compare the unbiased estimate based on the elicited reference wage with estimates from proxies that have been widely used in other studies. While we do find strong evidence that reference wages matter we cannot support Easterlin's suggestion that life satisfaction depends negatively on reference wage as much as it depends positively on own wages.

The proxies for reference wage that have been used in the literature do not introduce a simple classical measurement error, with a bias that cannot be signed. Whenever the elicited reference is not available we propose a simple IV strategy that delivers an upper-bound of the true effect. According to our results using linear predictions of wages as reference wages perform very badly, even when instrumented, because the a multicollinearity problem generates very "weak" proxies. On the other hand, cell average wages based on educational levels, gender, and age groups from external data sets generate the most robust results.

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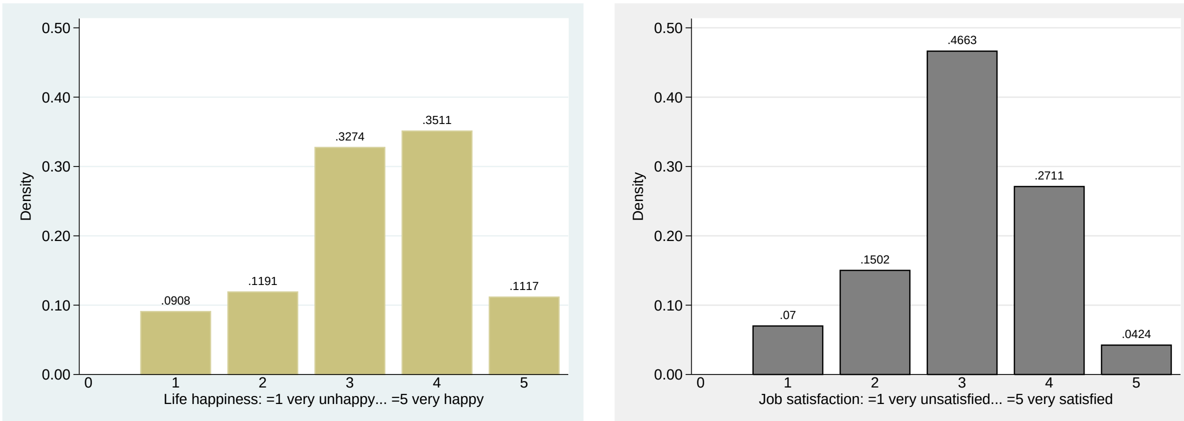

Figure 1: Happiness and Job Satisfaction

Table 1: Summary statistics for the Whole Sample

Variable	**Mean**	**Std. Dev.**
Female [0,1]	0.224	0.417
Age in years	34.926	9.965
Tenure in years	13.87	10.305
Middle school [0,1]	0.069	0.254
=1 if educ attainment is high school	0.464	0.499
Technical school [0,1]	0.024	0.152
Some college [0,1]	0.095	0.293
College [0,1]	0.27	0.444
Post-graduate [0,1]	0.067	0.251
Other degree[0,1]	0.011	0.105
Married [0,1]	0.584	0.493
owh	22.679	19.833
Blue-collar [0,1]	0.341	0.474
Managerial tasks [0,1]	0.171	0.377
N	91896	

Figure 2: Density of (log) wages, self-reported reference wages and reference wages computed from external sources

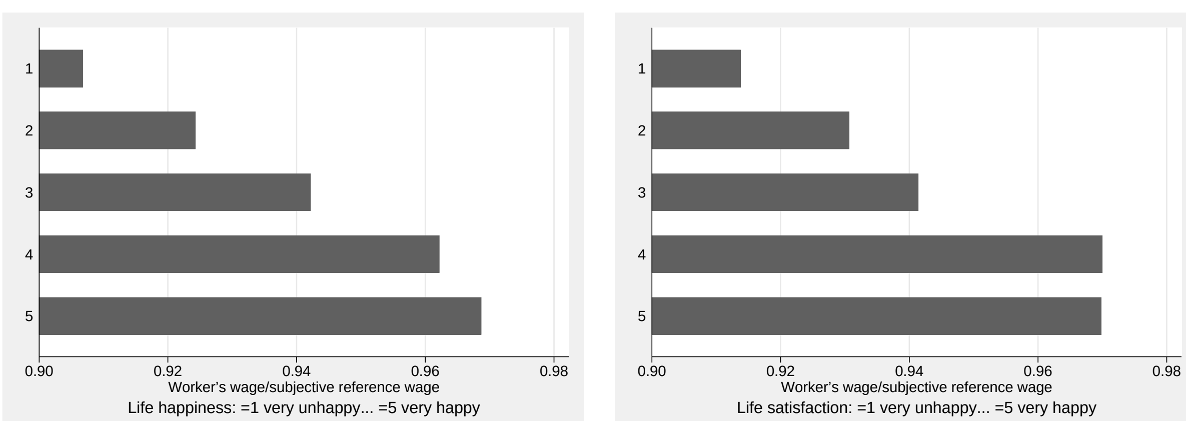

Figure 3: Relative Wage, Happiness and Job Satisfaction

Table 2: Summary statistics for the Female Sample

Variable	Mean	Std. Dev.
Age in years	30.284	8.839
Tenure in years	10.097	8.771
Middle school [0,1]	0.052	0.223
=1 if educ attainment is high school	0.459	0.498
Technical school [0,1]	0.03	0.171
Some college [0,1]	0.278	0.448
College [0,1]	0.15	0.357
Post-graduate [0,1]	0.014	0.116
Other degree[0,1]	0.017	0.128
Married [0,1]	0.33	0.47
owh	11.663	11.139
Blue-collar [0,1]	0.227	0.419
Managerial tasks [0,1]	0.02	0.141
N	20574	

Table 3: Summary statistics for the Male Sample

Variable	Mean	Std. Dev.
Age in years	36.266	9.869
Tenure in years	14.958	10.457
Middle school [0,1]	0.074	0.262
=1 if educ attainment is high school	0.465	0.499
Technical school [0,1]	0.022	0.146
Some college [0,1]	0.042	0.2
College [0,1]	0.304	0.46
Post-graduate [0,1]	0.083	0.276
Other degree[0,1]	0.01	0.098
Married [0,1]	0.657	0.475
owh	25.857	20.638
Blue-collar [0,1]	0.374	0.484
Managerial tasks [0,1]	0.215	0.411
N	71322	

Table 4: Mincer-type Wage Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Male Sample				Female Sample			
VARIABLES	log-wage	log-wage	log-wage	log-wage	log-wage	log-wage	log-wage	log-wage
Age in years	0.02***				0.01**			
	(0.00)				(0.00)			
Tenure in years	0.01***				0.02***			
	(0.00)				(0.00)			
Middle school [0,1]	-0.11***	-0.06**	-0.06***	-0.06***	-0.11**	-0.07	-0.06	-0.06
	(0.03)	(0.02)	(0.02)	(0.02)	(0.05)	(0.04)	(0.04)	(0.04)
Technical school [0,1]	-0.01	-0.03*	-0.00	-0.00	0.07***	0.05**	0.05***	0.05***
	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)
Some college [0,1]	0.06***	0.05***	0.05***	0.05***	0.05***	0.04***	0.06***	0.06***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)
College [0,1]	0.14***	0.11***	0.12***	0.12***	0.20***	0.19***	0.20***	0.20***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)
Post-graduate [0,1]	0.16***	0.14***	0.16***	0.16***	0.33***	0.32***	0.34***	0.34***
	(0.02)	(0.02)	(0.01)	(0.01)	(0.03)	(0.04)	(0.03)	(0.03)
Other degree[0,1]	-0.15***	-0.10***	-0.06***	-0.06***	-0.19***	-0.13***	-0.12***	-0.11***
	(0.03)	(0.02)	(0.01)	(0.01)	(0.05)	(0.03)	(0.04)	(0.04)
Hours worked (in logs)	0.05***	0.04***	0.04***	0.04***	0.04***	0.04***	0.04***	0.04***
	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)
Married [0,1]	0.16***	0.08***	0.09***	0.09***	0.03***	-0.01	-0.01*	-0.01**
	(0.01)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Pessimism: No help if needy [0,1]				-0.01***				-0.02***
				(0.00)				(0.00)
Career: No future promotion [0,1]				-0.01***				0.01**
				(0.00)				(0.00)
Age and Tenure dummies	no	yes	yes	yes	no	yes	yes	yes
Firm dummies	no	no	yes	yes	no	no	yes	yes
Observations	65052	65052	65052	65052	18181	18181	18181	18181
R-squared	0.661	0.725	0.753	0.754	0.565	0.611	0.650	0.650

Notes: All regressions additionally control for year fixed effects, industry fixed effects, a blue-collar dummy, a managerial tasks dummy, and occupation dummies. The IV regression has a first stage F-statist for the instruments equal to 260. The partial R-squared is 11 percent.

Table 5: Summary statistics for the Reference Wages

Variable	Mean	Std. Dev.	N
log Self-reported reference wage	15.484	0.412	94504
log wage	15.398	0.421	94504
Cell av. 1 (gender, age)	15.398	0.337	94504
Cell av. 2 (1 + education)	15.398	0.344	94504
Cell av. 3 (2 + managerial)	15.398	0.347	94504
Linear prediction	15.406	0.362	83233
Firm's average *log* wage	15.398	0.183	94504
log External reference wage	15.5	0.346	94494

Table 6: Relative utility hypothesis tests: OLS regressions of life satisfaction with self-reported reference wages

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep Var: Life Satisfaction			Males			Females	Pooled
log wage	0.69***	0.56***	0.47***	0.42***	0.42***	0.27***	0.40***
	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.02)
log self-reported reference wage	-0.39***	-0.38***	-0.30***	-0.29***	-0.29***	-0.16***	-0.25***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.04)	(0.03)
Hours worked (in logs)	-0.02	-0.06***	-0.05***	-0.05***	-0.05***	-0.07***	-0.05***
	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mean wages (in logs, by firm)				0.17***	0.14**	0.01	0.10
				(0.06)	(0.06)	(0.07)	(0.06)
Firm's *log* wage interquartile range					-0.07	-0.19***	-0.10
					(0.08)	(0.07)	(0.07)
Pessimism: No help if needy [0,1]			-0.37***	-0.37***	-0.37***	-0.41***	-0.38***
			(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Career: No future promotion [0,1]			-0.22***	-0.22***	-0.22***	-0.16***	-0.21***
			(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Female [0,1]							0.43***
							(0.02)
Age in years		-0.02***	-0.02**	-0.02*	-0.02*	-0.02	-0.02**
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Age squared		0.00	0.00	0.00	0.00	0.00	0.00*
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Tenure in years		-0.02***	-0.02***	-0.02***	-0.02***	-0.02***	-0.02***
		(0.01)	(0.00)	(0.00)	(0.00)	(0.01)	(0.00)
Tenure squared		0.00***	0.00***	0.00***	0.00***	0.00	0.00***
		(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Managerial tasks [0,1]		0.01	-0.03**	-0.03*	-0.03*	-0.05	-0.02
		(0.02)	(0.01)	(0.01)	(0.01)	(0.05)	(0.01)
Blue-collar [0,1]		-0.04**	-0.03**	-0.04**	-0.04**	-0.11***	-0.05***
		(0.02)	(0.01)	(0.01)	(0.01)	(0.03)	(0.01)
Middle school [0,1]		0.04**	0.04**	0.05**	0.05**	0.11*	0.06**
		(0.02)	(0.02)	(0.02)	(0.02)	(0.06)	(0.02)
Technical school [0,1]		0.08**	0.07**	0.07**	0.06**	0.04	0.06**
		(0.03)	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)
Some college [0,1]		0.02	0.02	0.02	0.02	0.16***	0.11***
		(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
College [0,1]		0.12***	0.09***	0.09***	0.09***	0.12***	0.09***
		(0.02)	(0.02)	(0.02)	(0.02)	(0.04)	(0.02)
Post-graduate [0,1]		0.16***	0.11***	0.10***	0.10***	0.10	0.09***
		(0.03)	(0.03)	(0.03)	(0.03)	(0.08)	(0.03)
Other degree[0,1]		0.01	0.02	0.02	0.02	-0.05	-0.02
		(0.04)	(0.04)	(0.04)	(0.04)	(0.03)	(0.04)
Married [0,1]		0.61***	0.61***	0.61***	0.61***	0.44***	0.57***
		(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Observations	71316	71316	71316	71316	71316	20570	91896
R-squared	0.022	0.071	0.109	0.110	0.110	0.089	0.116
log wage + *log* reference wage = 0?							
p-value	0.00	0.00	0.00	0.00	0.00	0.01	0.00

Table 7: Robustness checks for the relative income hypothesis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	OLS	OLS	OLS	OLS	OLS	IV	Ordered Probit
	Life-sat.	Job-sat.	Life-satisfaction				
log wage	0.42***	0.22***	0.45***	0.43***	0.42***	0.79***	0.41***
	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)	(0.22)	(0.03)
log self-reported reference wage	-0.29***	-0.15***	-0.29***	-0.29***	-0.29***	-0.35***	-0.29***
	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.11)	(0.03)
Hours worked (in logs)	-0.05***	-0.02**	-0.05***	-0.04***	-0.05***	-0.05***	-0.05***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
Pessimism: No help if needy [0,1]	-0.37***	-0.38***	-0.37***	-0.37***	-0.37***	-0.38***	-0.38***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Career: No future promotion [0,1]	-0.22***	-0.44***	-0.22***	-0.22***	-0.22***	-0.22***	-0.21***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Mean wages (in logs, by firm)	0.12**	0.15**	0.03		0.12**	-0.04	0.12**
	(0.06)	(0.07)	(0.07)		(0.06)	(0.08)	(0.06)
Interquartile range of log wages, by firm	-0.09	-0.14	-0.11		-0.09	-0.11	-0.11
	(0.09)	(0.09)	(0.07)		(0.09)	(0.11)	(0.09)
log external reference wage 1					-0.10		
					(0.07)		
Year and industry dummies	no	no	yes	yes	no	no	no
Firm dummies	no	no	no	yes	no	no	no
Observations	65052	65052	65052	65052	65046	65046	65052
R-squared	0.111	0.120	0.112	0.115	0.111	0.066	-

Notes: All regressions additionally control for year fixed effects, industry fixed effects, a blue-collar dummy, a managerial tasks dummy, and occupation dummies.

Table 8: Self-reported vs. estimated reference wage: OLS vs. IV (PART 1)

	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) OLS	(6) OLS	(7) IV	(8) IV
log wage	0.41***	0.42***	0.43***	0.35***	0.22***	0.32***	0.79***	-0.94**
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)	(0.38)
log Self-reported reference wage	-0.29***	-0.26***						
	(0.02)	(0.02)						
log external reference wage			-0.44***	-0.13***				
			(0.02)	(0.04)				
Linear prediction					-0.21***	0.06	-1.54***	6.43***
					(0.03)	(0.04)	(0.07)	(1.92)
log-hours worked	-0.09***	-0.05***	-0.08***	-0.06***	-0.07***	-0.06***	-0.01*	-0.18***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.04)
Other Xs	no	yes	no	yes	no	yes	no	yes
Observations	91886	91886	91886	91886	83223	83223	83223	83223
R-squared	0.057	0.115	0.013	0.072	0.006	0.073	.	.

Table 9: Self-reported vs. estimated reference wage: OLS vs. IV (PART 2)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12
	OLS	OLS	IV	IV	OLS	OLS	IV	IV	OLS	OLS	IV	IV
log wage	0.49***	0.35***	0.52***	0.37***	0.44***	0.35***	0.51***	0.37***	0.43***	0.35***	0.52***	0.37*
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02
Cell av. 1 (gender, age)	-0.55***	-0.11***	-0.61***	-0.83***								
	(0.02)	(0.04)	(0.02)	(0.23)								
Cell av. 2 (1 + education)					-0.46***	-0.16***	-0.56***	-0.60***				
					(0.02)	(0.04)	(0.02)	(0.17)				
Cell av. 3 (2 + managerial)									-0.45***	-0.14***	-0.57***	-0.71*
									(0.02)	(0.04)	(0.02)	(0.20
log-hours worked	-0.08***	-0.06***	-0.08***	-0.06***	-0.08***	-0.06***	-0.08***	-0.06***	-0.08***	-0.06***	-0.08***	-0.06*
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00
Other Xs	no	yes	no	yes	no	yes	no	yes	no	yes	no	yes
Observations	91886	91886	91886	91886	91886	91886	91886	91886	91886	91886	91886	9188
R-squared	0.016	0.072	0.016	0.068	0.013	0.072	0.012	0.070	0.012	0.072	0.012	0.07

Table 10: Literature survey

	Mincer wage approach								Cell average income				Cell av. income from external source					Subje ref. come
Authors	Clark and Oswald (1996)		Sloane and Williams (2000)		Lévy-Garboua and Montmarquette (2004)		Senik (2004)		Ferrer-i-Carbonell (2005)	Brown et al. (2008)	Clark et al. (2009a)	Cappelli and Sherer (1988)	Clark and Oswald (1996)	Mcbride (2001)	Blanchflower and Oswald (2004)	Luttmer (2005)	Clark et al. (2009b)	Kingt et (2009)
Method	Ordered probit	OLS	Ordered probit	OLS	Ordered probit	OLS	Ordered probit	Heckit	Ordered probit	Ordered probit	Ordered probit	OLS	Ordered probit	Ordered probit	Ordered logit	OLS	OLS	OLS/ Probit Order probit
Dependent variable	Job satisfaction	Expected income	Job satisfaction	Expected income	Job satisfaction	Expected income	Happiness	Expected income	General satisfaction	Four aspects of job sat.	Job satisfaction	Job satisfaction	Job satisfaction	Happiness	Happiness	Happiness	Economic Satisfaction	Happi
Income	Yes		Yes		Yes		Yes		Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Reference inc./orig.	<, – **		<,– **		+		<, + ***		– **	<, – **	<, + *	<, – ***	<, – **	<, – **	>, + **	<, – **	>, + **	<, – *
Reference inc./rep. with our data(i)	<, + ***		>, + ***		>, + ***		<, + ***		>, – *** (ii)	>, – *** (iii)	<, + *** (iv)	>, – *** (v)	<, + *** (vi)	<, – *** (vii)	>, – *** (viii)	N.A.(ix)	N.A.(ix)	>, – * (x)
Hours worked	Yes	No	Yes	Yes	No	Yes	No	No	No	Yes	Yes	No	Yes	No	No	Yes	No	Yes
Age	Yes	Yes (6)	Yes	No	No	No	No	Yes	Yes	Yes	Yes (3)	Yes	Yes	No	Yes	Yes	Yes (9)	Yes
Age squared	Yes		No	No	No	No	No	No	Yes	No		No	Yes	No	Yes	Yes		Yes
Tenure	No	Yes	Yes	Yes	No	Yes	No	Yes	No	No	No	Yes	No	No	No	No	No	No
Tenure sq.	No	Yes	Yes	Yes	No	Yes	No	No	No	No	No	No	No	No	No	No	No	No
Sex	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No	Yes
Education	Yes (3)	Yes (12)	Yes (5)	Yes (5)	Yes (4)	Yes (4)	No	No	Yes	Yes	Yes(5)	No	No	No	Yes	Yes	Yes(6)	Yes
Marital St./Children	No	Yes (5)	Yes	Yes (2)	Yes	Yes (2)	No	No	Yes	Yes	Yes	No	No	Yes	Yes (6)	No	Yes(6)	Yes (3
Regions	Yes (18)	Yes (18)	Yes (5)	No	Yes	Yes	Yes(7)	Yes(7)	Yes	Yes	Yes(13)	No	Yes (18)	No	Yes (44)	Yes (U.S State)	Yes(13)	Yes (2
Industry	Yes (10)	Yes (61)	No	Yes (10)	No	No	No	Yes(8)	No	Yes	Yes(6)	No	Yes (10)	No	No	No	No	No
Occupation	No	Yes (77)	Yes	Yes (8)	No	Yes (2)	No	Yes(10)	Yes	Yes	No	Yes (3)	No	No	No	No	No	No
Job class	Yes	Yes (2)	Yes (2)	Yes	No	No	No	No	No	No	Yes	No	No	No	No	No	No	No
Health status	Yes (3)	Yes (5)	No	No	Yes	Yes	No	No	No	No	Yes	No	Yes (3)	Yes (3)	No	No	Yes	Yes (2
Race	Yes (3)	No	Yes	Yes	No	No	No	No	No	No	No	No	Yes (3)	Yes	Yes (3)	Yes (5)	No	Yes
Religion	No	No	No	No	Yes	Yes	No	No	No	Yes	No	No	No	No	No	Yes (13)	No	No
Temporary contract	Yes	Yes	Yes	Yes	No	No	No	No	No	Yes	No	No	No	No	No	No	No	No
Incentive payment	No	Yes	No	Yes	No	No	No	No	No	No	No	No	No	No	No	No	No	No
Part time	No	Yes	Yes	Yes	No	Yes	No	No	No	No	No	Yes	No	No	No	No	No	No
Firm size	No	Yes (11)	Yes (3)	Yes (3)	No	No	No	No	No	Yes	Yes(3)	No	No	No	No	No	No	No
Union member	No	Yes	Yes	Yes	No	No	No	No	No	No	No	Yes	No	No	No	No	No	No
Trade union	No	Yes	Yes	Yes	No	No	No	No	No	Yes	No	No	No	No	No	No	No	No
Other control var.	Renter and Second job status	Organization type(7), accident (3), when work (9), sex mix at work (5), and pension member	Working condition (14), sex mix at work (4), manual, and training period	Training period, when work, and selectivity	Current and past wage gap, birth place, language, and satisfaction with leisure	Birth place, language, and satisfaction with leisure			Year fixed effects, familiy size (2), familiy condition, and social mean (4)	Average wage in the workplace and wage range	Subordinates and year fixed effects(7)	Working conditions (6), wage system, and layoff		Parental living standard (4)	Unemployed, retired, student, Keeping home, and others	Value of home, renter, population, job status (2), household size, geography (2), and fraction black in PUMA	See neighbors often, socioeconomic groups (3), No. years in Grid (5), and year (8)	Unem asset, communit variables (6), a attitudinal variables (6)

(i) Replication with our dataset; < when the absolute value of the coefficient on Ref. income is greater than that of own income, – otherwise; + the sign is positve, – the sign is negative; *** p<0.01, ** p<0.05, * p<0.1.
(ii) Categories over education, age, and region (50)
(iii) Comparison income as the field wage rank in the workplace enters in the happiness regression: negative sign confirms jelousy effects.
(iv) Proxy of comparison income as the average income in the workplace
(v) Categories over occupation and seniority; comparison made inside/outside of workplace
(vi) Categries over working hours (28) and sex; Data from 1991 New Earnings Survey. When education is contorlled, the sign turns negative significantly at 1% level.
(vii) The average income of everyone from 5 years younger than him to 5 years older than him; Data from 1994 current population survey
(viii) The average level of income in each of the different income quintiles within the person's State; Data from BEA state per capita income data
(ix) Comparison income with geometric information. Unfortunately, we do not have regional information in our dataset.
(x) Comparison income from survey; dummies of household income above/below village average (4)