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How much does money really matter? Estimating the causal effects of income on happiness

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Abstract There is a long tradition of psychologists finding small income effects on life satisfaction (or happiness). Yet the issue of income endogeneity in life satisfaction equations has rarely been addressed. The present paper is an attempt to estimate the causal effect of income on happiness. Instrumenting for income and allowing for unobserved heterogeneity result in an estimated income effect that is almost twice as large as the estimate in the basic specification. The results call for a reexamination on previous findings that suggest money buys little happiness, and a reevaluation on how the calculation of compensatory packages to various shocks in the individual's life events should be designed.

Keywords Income · Life satisfaction · Instrumental variables · Longitudinal · Happiness · Cost-benefit analysis

JEL Classification C33 · I0

1 Introduction

Perhaps one of the most famous questions in social science is how much does money really matter to our happiness? While cross-sectional micro data estimations for

"Once people have high incomes (by current world standards), additional increases in wealth have a very small influence on subjective well-being." *Ed Diener, psychologist*.
"Happiness with life appears to be increasing in the USA. The rise is so small, however, that it seems extra income is not contributing dramatically to the quality of people's lives." *Andrew Oswald, economist*.
"Those who say money can't buy happiness don't know where to shop." *Anonymous*.

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advanced industrialized countries often suggest a positive and statistically significant correlation between life satisfaction and income, it is not one would generally call a strong one; DeNeve and Cooper (1999) quote a mean correlation coefficient between income and subjective well-being of 0.17 (over 85 independent samples).[1] This leads to many surveys in this field to conclude that the relationship between money and happiness is slight or non-existent (Myers 1992; Diener and Biswas-Diener 2002; Layard 2005; Nettle 2005).

There are various sources of bias associated with micro data estimation of the effect of income in life satisfaction equations. First, neglecting unobserved heterogeneity which may be correlated with both income and life satisfaction will result in what psychologists called a "personality bias" on the obtained estimates (see Kahneman et al. 1999, for a review). For example, there is a large body of evidence in psychology to suggest that people who are extravert and resilient are more likely to be happy with life, as well as more productive in the labor market in various ways (e.g. Judge et al. 1997; Kivimaki et al. 1997; Salgado 1997). This is reflected by the evidence that happy people earn more than others in general (see, e.g. Lyubomirsky et al. 2005), resulting in an upward bias in the estimates. Second, there is increasing evidence that it is the relative income rather than absolute income that drives satisfaction (e.g. Frank 1985; Clark and Oswald 1996; McBride 2001; Ferrer-i-Carbonell 2005; Luttmer 2005). In the likely case that own income is correlated with other people's incomes, the negative correlation between others' incomes and life satisfaction implies that both panel and cross-sectional estimates of the income effect without appropriate controls on others' incomes would be biased downward. Third, in an adaptation or aspiration model we would expect the role of income to be underestimated if the rate of adaptation or aspiration to income cannot be appropriately controlled in the estimation (Easterlin 2001; Stutzer 2004; Clark and Oswald 1996; Di Tella et al. 2007; Rayo and Becker 2007). Fourth, incomes are likely to be highly positively correlated with various other factors such as working hours, time spent away from family and loved ones, and time spent commuting to and from work, all of which are potentially strongly negatively correlated with one's global evaluation of life satisfaction and are often recorded with a lot of missing values in standard surveys. Thus, without sufficient controls on these choice variables it would imply that estimates of the effect of income on life satisfaction would be biased downward. A fifth source of potential bias is attenuation bias (i.e. measurement error that is independent of the level of income) which can bias the estimated income effect towards zero. Because of these reasons, it is arguable that the income–happiness correlation found in recent econometric work, while persuasive of a statistical significant link, is not a causal one. In addition to this, because there are both positive and negative biases involved, the direction of the overall bias is unclear on a priori grounds.

Understandably there are very few existing empirical works that deal with the income endogeneity issue. The main reason for this is because it is difficult to think of a natural experiment or a randomized experiment of a large scale where money is

[1] For further discussions on this topic, see, for example, Brickman et al. (1978), DeNeve and Cooper (1999), Diener and Oishi (2000), Frey and Stutzer (2002).

randomly allocated to treatment and control groups. To the best of my knowledge, the only two well-known large-scale studies that have a natural experiment flavor are given by Frijters et al. (2004) and Gardner and Oswald (2007). Using a longitudinal data on life satisfaction for the East Germans, Frijters et al. (2004) found a sizeable increase in life satisfaction from an exogenous increase in real income per capita as a result from the reunification. Gardner and Oswald (2007) applied the idea of medium-sized lottery winners as an exogenous source of income on longitudinal data of mental health for Britain and found a significant gap in mental well-being between those with no wins and the other with small wins even after 2 years of winning the lottery.[2]

Another approach would be to find appropriate instrumental variables (IV) that correlate with incomes but are not correlated with life satisfaction beyond their effects on the endogenous regressor. However, it is very difficult to find such instruments in secondary survey datasets. One might hypothesize the use of lagged income to instrument for the current income in a longitudinal data setting. Nonetheless, the use of lagged income is open to objections because of the adaptation argument that the current level of life satisfaction is not independently determined by income in the previous time period (Di Tella et al. 2007). To the best of my knowledge, there are currently two existing studies that attempted to instrument for income in happiness regressions. The first one is the study by Knight et al. (2008) on subjective well-being in rural China. Using cross-section data with a 5-point scale happiness measure, they used survey respondent's father's years of education and the value of productive assets to instrument for household income and found that the size of the income coefficient estimated by OLS increased from 0.17 to around 0.58 with income instrumented. In a panel study of happiness, Luttmer (2005) used predicted household earnings, where the prediction is based on industry×occupation of the survey respondent and the spouse and national earnings information by industry×occupation and time period, to instrument for income in the happiness equation. He found that the estimated IV income coefficient to be about three times as large as the one obtained in the baseline happiness estimation. Nonetheless, the extent of unobserved heterogeneity bias on the estimated income effect relative to standard simultaneity bias is unknown from the two studies as Knight et al.'s paper is cross-sectional in nature and the IV estimates are not reported in Luttmer's study when individual fixed effects have been included in the regressions.

To deal with the endogeneity issues, this paper adopts the IV approach with controls on individual fixed effects, thereby addressing both unobserved heterogeneity and omitted time-varying variables bias. I draw upon a set of instrumental variables which are not used before in the estimation of life satisfaction equations. These are the exogenous over-time variations in the proportion of household members with payslip information to provide instruments for income. The results of our empirical investigation suggest that both sources of endogeneity are statistically important in our sample. While the inclusion of individual effects significantly reduces the size of the estimated

[2] Another paper that uses IV approach to estimate the income effect is by Lydon and Chevalier (2002). The instrumental variable used in their paper is the partner's earning, which is used to instrument for the respondent's income. However, the dependent variable used in the study is self-rated job satisfaction rather than life satisfaction.

income coefficient in life satisfaction equation, correcting for omitted time-varying variables leads to *larger* income coefficients than those obtained when both sources of endogeneity bias are ignored. This leads to drastic changes in the calculated shadow prices for many of the other determinants of life satisfaction, which is one of the core policy implications of happiness research in which compensation to shocks from non-marketable influences on life satisfaction such as disability and bereavements can be worked out by looking at how much income would have to rise to keep the person at the same level of subjective well-being (see, for example, Powdthavee 2005; Van Praag and Baarsma 2005; Frey et al. 2007; Oswald and Powdthavee 2008a,b).

Section 2 outlines the data used in this paper. Estimation results are reported in Sect. 3. The estimates suggest of a significant overall negative bias on income in ordinary least squares, random effects, and fixed effects estimation of life satisfaction equations. Section 4 concludes the paper.

2 Data

The dataset used in this analysis is the British Household Panel Survey (BHPS). This is a nationally representative sample of persons aged 16 and over in 1991, who have been re-interviewed every year after. The study interviewed separately all adult members of the household with respect to their income, employment status, marital status, health, and attitudes. There is both entry into and exit from the panel, leading to unbalanced data with an increased number of individual interviews over time. This is due to the inclusion of children who turn 16 in the original household sample, refresher samples, and of the new members of household formed by original panel members.

As well as questions on socio-economic status, individuals were also asked from Wave 6 onwards to indicate how satisfied they are with their life from 1 (*very dissatisfied with life*) to 7 (*very satisfied with life*). The life satisfaction (LS) question is located in a self-completed section of the BHPS survey, which is strategically placed at the end of the questionnaire after individuals had been asked about their household and individual characteristics to avoid any framing effects of a particular event dominating responses to the LS question.

Income data, which is real equivalent household income in our case, are derived from individual responses in the Household Finance section where each survey respondent is asked to state their sources and the amount of incomes received in a year. During the interview, the interviewer will also ask to see the respondent's payslip wherever applicable. The income variable used in this analysis is the derived household income deflated by consumer price index, as well as divided by the square root of the number of people living in the household to make a real equivalent household income variable. I use the log form of income rather than its level as this has been shown in the literature to fit the happiness models better (for a review, see Layard et al. 2008).

I analyze all individuals of working age (16–65) with at least one other member living in the same household for the years 1996–2004 (Waves 6–14).[3] The unbalanced

[3] LS question was only introduced in wave 6 onwards, but was left out in Wave 11.

panel with nonmissing information on LS (Sample 1) includes 67,868 observations or 17,683 individuals.[4] Descriptive statistics are reported in Table 4 in Appendix.

The instrument used in this analysis is the proportion of household members with payslip information. In every wave, the BHPS asks their interviewers to try and see the actual payslip of the survey respondent. The payslip is usually issued by the respondent's employer, and typically contains information on gross income and all taxes and any other deductions such as retirement plan contributions, insurances, garnishments, or charitable contribution taken out of the gross amount to derive at the final net amount of pay. Where the payslip is shown to the interviewer, the information about income is likely to be more accurate.

In the BHPS, 32% did not show the interviewer their payslip, 14% showed their latest payslip, i.e. a payslip that was issued to them in the most recent month, 1% showed their earlier payslip, i.e. a payslip that was issued to them earlier in the year, and 54% either did not receive a payslip from their employer in that year or payslip was inapplicable to them.[5] Of those in full-time employment, 59% did not show their payslip, 26% showed their latest payslip, 2% showed their earlier payslip, and 12% did not receive a payslip. I use the proportions of household member with early and latest payslip seen by the interviewer and the proportion of those who have payslip but did not show it to the interviewer to instrument for log of real household income per capita.

The idea is that there is a direct correlation between the proportion of household members showing and not showing their payslip to the interviewer and that of household income as household income is bound to have been measured more accurately where the proportion of household member who showed payslip is high. However, there is no reason to expect LS to be affected by whether or not the interviewer sees the payslip. This is only the case if employment of other household members, which is highly correlated to the proportion of household members with payslip (correlation of 0.85), does not have a direct impact on LS. Nonetheless, given that unemployment and disability of other household members have been shown in the literature to have a direct impact on the respondent's self-reported well-being even if household income is not affected (see, e.g. Clark 2003; Powdthavee 2009), it is unlikely that the instrument will pass the exclusion restriction of no direct impact on LS *if* employment of other household members is not conditioned in the estimation. Therefore, in order to satisfy the exclusion restriction conditions, the control variables used in both income (first-stage) and LS (second-stage) regressions will also include, alongside standard control variables, proportions of other unemployed members, other disabled members, other retired members, and other members who are inactive in the labor market such as students and those looking after home.

3 Estimation results

Table 1 reports the estimates for the ordinary least squares (OLS), standard random effects (RE), standard fixed effects (FE), fixed effects with instrumented income

[4] The missing rate for LS is 7% across all waves.

[5] This also includes the self-employed.

Table 1 OLS, RE, FE, FE-IV, and RE-IV estimates

Dependent variable: life satisfaction	OLS	RE	FE	FE-IV		RE-IV	
				Reduced-form income	IV life satisfaction	Reduced-form income	IV life satisfaction
Ln(real household income/square root of household size)	0.105 (0.010)**	0.058 (0.007)**	0.019 (0.009)*		0.206 (0.101)*		0.190 (0.073)**
Proportion of all household members with early payslip seen				0.229 (0.025)**		0.282 (0.024)**	
Proportion of all household members with latest payslip seen				0.248 (0.014)**		0.299 (0.012)**	
Proportion of all household members not showing the payslip				0.214 (0.012)**		0.245 (0.010)**	
Control variables							
Proportion of (other) unemployed household members	−0.265 (0.039)**	−0.211 (0.025)**	−0.141 (0.027)**	−0.120 (0.015)**	−0.102 (0.035)**	−0.202 (0.013)**	−0.169 (0.034)**
Proportion of (other) disabled household members	−0.353 (0.044)**	−0.201 (0.027)**	−0.055 (0.033)+	−0.150 (0.018)**	−0.012 (0.041)	−0.207 (0.014)**	−0.160 (0.035)**
Proportion of (other) retired household members	0.063 (0.032)*	0.000 (0.023)	−0.004 (0.029)	−0.176 (0.015)**	0.045 (0.039)	−0.183 (0.013)**	0.038 (0.031)
Proportion of (other) non-labor categories	−0.020 (0.021)	−0.031 (0.016)*	−0.036 (0.018)*	−0.179 (0.010)**	0.010 (0.030)	−0.272 (0.008)**	0.017 (0.031)
Number of dependent children (aged < 16)	−0.015 (0.010)	0.001 (0.008)	0.020 (0.009)*	−0.107 (0.005)**	0.041 (0.015)**	−0.120 (0.004)**	0.017 (0.012)
Male	−0.067 (0.016)**	−0.064 (0.015)**				0.041 (0.008)**	−0.070 (0.016)**
Age	−0.067 (0.005)**	−0.062 (0.004)**				0.028 (0.002)**	−0.065 (0.004)**

Table 1 continued

Dependent variable: life satisfaction	OLS	RE	FE	FE-IV		RE-IV	
				Reduced-form income	IV life satisfaction	Reduced-form income	IV life satisfaction
Age-sq/100	0.083 (0.006)**	0.078 (0.004)**	0.036 (0.007)**	−0.028 (0.003)**	0.041 (0.007)**	−0.024 (0.002)**	0.081 (0.005)**
Living as a couple	−0.091 (0.022)**	−0.065 (0.017)**	−0.001 (0.023)	0.000 (0.012)	−0.001 (0.023)	0.001 (0.009)	−0.065 (0.017)**
Widowed	−0.498 (0.100)**	−0.497 (0.068)**	−0.294 (0.105)**	−0.028 (0.054)	−0.291 (0.106)**	−0.040 (0.036)	−0.494 (0.068)**
Divorced	−0.587 (0.057)**	−0.486 (0.039)**	−0.138 (0.059)*	−0.121 (0.030)**	−0.117 (0.061)+	−0.188 (0.020)**	−0.464 (0.041)**
Separated	−0.659 (0.087)**	−0.551 (0.058)**	−0.376 (0.071)**	−0.077 (0.037)*	−0.363 (0.072)**	−0.119 (0.030)**	−0.537 (0.058)**
Never married	−0.309 (0.029)**	−0.243 (0.024)**	−0.119 (0.037)**	0.078 (0.019)**	−0.133 (0.037)**	0.067 (0.012)**	−0.251 (0.024)**
Self-employed	0.055 (0.026)*	0.025 (0.020)	−0.001 (0.025)	−0.044 (0.013)**	0.021 (0.028)	−0.052 (0.011)**	0.046 (0.023)*
Unemployed	−0.425 (0.036)**	−0.378 (0.022)**	−0.289 (0.025)**	−0.041 (0.013)**	−0.268 (0.028)**	−0.108 (0.012)**	−0.350 (0.027)**
Retired	−0.023 (0.036)	−0.047 (0.025)+	−0.005 (0.029)	−0.093 (0.010)**	0.039 (0.037)	−0.130 (0.008)**	−0.015 (0.031)
Disabled	−0.905 (0.051)**	−0.733 (0.026)**	−0.453 (0.033)**	−0.154 (0.015)**	−0.421 (0.038)**	−0.148 (0.013)**	−0.702 (0.032)**
Other non-labor categories	−0.023 (0.021)	−0.038 (0.015)*	−0.032 (0.018)+	−0.101 (0.017)**	−0.003 (0.024)	−0.149 (0.014)**	−0.009 (0.021)
Education: other qualification	0.125 (0.094)	0.121 (0.079)	0.268 (0.181)	0.221 (0.093)*	0.228 (0.183)	0.049 (0.042)	0.115 (0.079)
Education: apprenticeship	−0.042 (0.073)	0.024 (0.060)	0.205 (0.151)	−0.026 (0.077)	0.214 (0.151)	0.049 (0.032)	0.017 (0.061)
Education: CSE	−0.001 (0.045)	−0.003 (0.038)	0.039 (0.089)	0.017 (0.045)	0.037 (0.089)	0.093 (0.020)**	−0.015 (0.039)
Education: commercial qualification	−0.037 (0.059)	0.033 (0.051)	0.024 (0.119)	0.140 (0.061)*	−0.002 (0.121)	0.180 (0.027)**	0.008 (0.053)
Education: GCE O level	−0.055 (0.028)+	−0.031 (0.023)	0.027 (0.057)	0.114 (0.029)**	0.005 (0.058)	0.200 (0.012)**	−0.059 (0.027)*
Education: GCE A level	−0.088 (0.030)**	0.003 (0.024)	0.114 (0.057)*	0.031 (0.029)	0.109 (0.057)+	0.187 (0.013)**	−0.024 (0.028)
Education: nursing qualification	−0.095 (0.080)	−0.023 (0.061)	−0.030 (0.119)	0.080 (0.061)	−0.041 (0.120)	0.327 (0.032)**	−0.067 (0.066)
Education: other higher	−0.078 (0.028)**	−0.012 (0.022)	0.064 (0.052)	0.043 (0.027)	0.056 (0.053)	0.222 (0.012)**	−0.043 (0.028)
Education: teaching qualification	−0.077 (0.058)	−0.001 (0.051)	0.031 (0.191)	0.107 (0.098)	0.013 (0.192)	0.438 (0.027)**	−0.059 (0.060)

Table 1 continued

Dependent variable: life satisfaction	OLS	RE	FE	FE-IV		RE-IV	
				Reduced-form income	IV life satisfaction	Reduced-form income	IV life satisfaction
Education: first degree	−0.116 (0.032)**	−0.062 (0.028)*	−0.021 (0.068)	0.123 (0.035)**	−0.041 (0.069)	0.409 (0.015)**	−0.118 (0.041)**
Education: higher degree	−0.132 (0.047)**	−0.041 (0.045)	0.052 (0.105)	0.241 (0.054)**	0.010 (0.108)	0.530 (0.024)**	−0.113 (0.060)+
No known health problems	0.338 (0.013)**	0.230 (0.010)**	0.132 (0.012)**	0.013 (0.006)*	0.130 (0.012)**	0.024 (0.005)**	0.227 (0.010)**
Household size	0.009 (0.009)	−0.007 (0.007)	−0.027 (0.009)**	−0.238 (0.004)**	0.018 (0.025)	−0.238 (0.003)**	0.024 (0.018)
Own home outright	0.097 (0.021)**	0.081 (0.015)**	0.044 (0.020)*	−0.024 (0.010)*	0.049 (0.020)*	0.017 (0.008)*	0.080 (0.015)**
Observations	67,868	67,868	67,868	67,868	67,868	67,868	67,868
R^2	0.0932						
Number of persons	17,683	17,683	17,683	17,683	17,683	17,683	17,683
Within R^2		0.0137	0.0155	0.2277	0.0064	0.2225	0.0116

+ < 10%; * < 5%; ** < 1%

OLS Ordinary least squares, *RE* random effects, *FE* fixed effects, *FE-IV* fixed effects instrumental variables, *RE-IV* random effects instrumental variables

Standard errors are in parentheses. Life satisfaction is on a 7-point scale (1 = very dissatisfied, …, 7 = very satisfied). Wave dummies and regional dummies are included in all regressions. The reference groups for the control variables are female, married, in full-time employment, no formal education, and rent home or home mortgage variable (FE-IV), and random effects with instrumented income variable (RE-IV) models. The dependent variable is life satisfaction, LS, measured cardinally (on the 1–7 scale). Note that all regressions include standard controls, including dummy variables for whether the survey respondent has any known health problems, gender, age-squared, marital status, employment status, education, household size, homeownership status, number of children aged 16 and under, and wave and regional dummies.[6] Including these control variables also allow us to make comparable trade-offs between changes in some of the individual's life events such as becoming unemployed or disabled and how much income would be required to rise in order to offset the negative impacts on LS brought about by such shocks. In addition to this, proportions of employment statuses of other household members are also controlled for in the regressions in order to ensure that the instrument passes the exclusion restriction of no direct impact on the outcome variable.

Assuming cardinality in LS, OLS on the pooled sample yields an estimated coefficient on income of 0.105, with a robust standard error of 0.010.[7] However, the size of the estimated income coefficient is reduced by nearly half to 0.058—though continuing to be statistically well-determined at the 1% level—once we allow unobserved time-invariant effects to vary randomly across individuals. The difference in the RE and FE estimates on income is even more striking. Conditioning on the individual fixed effects causes the absolute value of the estimated income coefficient to reduce from that obtained in the RE model by approximately 67%; the estimated FE coefficient on income is now significant at the 5% level at 0.019, with a standard error of 0.009. This is consistent with prior studies that found positive personality traits bias in happiness-income relationship (e.g. Frank 1985; Ferrer-i-Carbonell and Frijters 2004; Powdthavee 2008). It is also consistent with the idea that measurement error in income can lead to greater attenuation bias in FE estimates (since much of the signal in income is differenced out, the signal-to-noise ratio is lower) which will also lead to a small β for the FE estimator than the RE estimator.

The FE income coefficient is consistent only if income is orthogonal to the overall error component. The FE-IV approach can be applied in the case where there is a violation of such condition. To address the problem of omitted time-varying variables bias, as well as unobserved heterogeneity, the next two columns of Table 1 report the reduced-form log of real income per capita equation and the FE-IV estimates. There is a marked increase in the income coefficient estimated by FE-IV compared to that estimated by the standard FE estimator; the estimated FE-IV coefficient on income is 0.206, with a statistically well-defined standard error of 0.101. Comparing between the two FE estimators, the FE-IV coefficient on income is around ten times the size of the estimated FE income coefficient and almost twice the size of the estimate obtained in the OLS regression. Note that the identification is achieved only through the inclusion of (i) the proportion of household members with early payslip seen, (ii) the proportion

[6] Gender and age are dropped from fixed effects regressions. Age dummies can be included instead of age-squared, although this does not lead to a change in the paper's conclusion.

[7] Although the main income variable used in this paper is the log of real household income per capita, the income coefficient remains virtually the same when other log forms of income such as log of real household income, nominal household income, and real equivalent household income are used.

of household members with latest payslip seen, and (iii) the proportion of household members who have a payslip but did not show it to the interviewer, which are all statistically significant in the reduced-form income fixed effects equation,[8] as well as conditioning for employment of other members in the household. These results strongly suggest that, although neglecting heterogeneity biases upward the income coefficient in a significant way, the direction of the overall bias is *negative* once we correct for the omitted time-varying factors that correlate positively with income but are negatively correlated with LS. This conclusion is supported by the inclusion of the overall error component obtained from the income equation to provide a Hausman t test for the endogeneity of income in LS equation (see Smith and Blundell 1986; Harmon and Walker 1995). The estimated FE coefficient on the income residual is negative, sizeable and statistically significant at conventional levels at -0.194, with a standard error of 0.100. The results are also consistent with previous findings that found a downward bias on income (Luttmer 2005; Knight et al. 2008).

Although estimators that ignore unobserved heterogeneity are inconsistent, it is instructive to contrast the estimates obtained from the FE-IV approach with those estimated by RE-IV. The latter are presented in the last two columns of Table 1. The coefficient on income estimated by RE-IV model is slightly smaller in absolute value than that obtained by FE-IV. However, the size of other estimated coefficients on the person's socio-economic variables appear to differ significantly between the two models; the Hausman's test yields a statistically well-determined $\chi^2(55)$ statistic of 885.69, which suggests that we can reject the null hypothesis that the differences in coefficients between FE-IV and RE-IV are not systematic. Hence, it can be concluded that the statistical consequences of neglecting unobserved heterogeneity in the estimation of income and other personal factors in LS regression equations are serious whether single or simultaneous equations estimators are used.

Other general results of Table 1 are consistent with previous work on LS. For example, men report, on average, lower LS than women, and that LS has a U-shaped relationship with age. Poor health is associated negatively with LS. The married have the highest level of LS and the separated has the lowest. Unemployment and disability are very detrimental to well-being. For a review on the structure of happiness equations, see Oswald (1997).

One question of interest is whether or not the instruments are consistent in producing sufficient exogenous variations in income. Table 2 summarizes statistical tests for the instruments' robustness when estimated by the FE-IV estimator.[9] We ask three questions about the quality of the instruments. First, can we reject the null hypotheses of weak instruments? This is tested by examining whether or not (i) the inclusion of the instruments leads to any improvement in the R^2 of the reduced-form equation, and (ii) the F statistic on excluded instruments in the reduced-form income equation are statistically significant (Bound et al. 1995; Stock and Yogo 2005). The partial R^2

[8] It is worth nothing that, while people without payslips earn significantly less than people with payslips, those living in households with a more accurate record of income still earn statistically significantly higher than those from households where a large proportion of household members have a payslip but did not show it to the interviewer.

[9] The statistics are produced when (4) is run by xtivreg2 command in STATA 10.0.

Table 2 Consistency checks for FE-IV

1. Partial R^2 of excluded instruments:	0.0082
2. Weak identification test (Cragg-Donald Wald F statistic, p value = [.]):	138.73 (0.000)
3. Underidentification tests	
Ho: matrix of reduced form coefficients has rank = K1 − 1 (underidentified)	
Ha: matrix has rank = K1 (identified)	
Anderson canon. correlation. N*CCEV LM statistic: Chi-sq(3), p value = [.]	413.24 (0.000)
Cragg-Donald N*CDEV Wald statistic: Chi-sq(3), p value = [.]	416.68 (0.000)
4. Weak-instrument-robust inference	
Tests of joint significance of endogenous regressors B1 in main equation	
Ho: B1 = 0 and overidentifying restrictions are valid	
Anderson-Rubin Wald test: $F(3,50127)$, p value = [.]	1.43 (0.231)
Stock-Wright LM S statistic: Chi-sq(3), p value = [.]	4.30 (0.231)
5. Sargan statistic (overidentification test of all instruments):	
Chi-sq(2), p value = [.]	0.379 (0.827)

Standard errors are in parentheses, unless stated otherwise

obtained from regressing income against the instruments, once common exogenous variables have been partialled out, has a value of 0.0082, and a test on the excluded instruments in the reduced-form equation yields an F statistic of 138.72. Both of these results compare favorably to the statistics reported in Bound et al. (1995) and Stock and Yogo (2005), which suggest that we can comfortably reject the null hypothesis of weak instruments. Second, can we be reasonably satisfied that the instruments have adequately identified the reduced-form equation? Both Anderson and Cragg-Donald Wald statistics suggest that we can comfortably reject the null hypothesis of under-identification, which means that the reduced-form income equation is identified in which the excluded instruments are correlated with the endogenous regressor. Lastly, can we satisfy the overidentifying restrictions by accepting the null hypothesis that the coefficients of the endogenous regressors in the structural equation are jointly equal to zero? Here, the null hypothesis is that the instruments are correlated with LS beyond their effects on income. Anderson-Rubin Wald and Stock-Wright statistics yield F statistic and $\chi^2(3)$ statistic of 1.43 and 4.30, which are statistically insignificantly different from zero at all conventional levels. The conclusion of zero correlations between LS and the remaining instruments *conditioning* on the assumption that a subset of instrument is valid is also supported by the Sargan $\chi^2(2)$ statistic, which is statistically insignificant with a p value of 0.827. Thus, we can be reasonably satisfied that the instruments are consistent in producing robust instruments for income in LS regressions.

Table 3 gives comparable results for our alternative model, whereby the ordering of the LS responses is resumed. However, there is, unfortunately, no accepted procedure for the panel estimation of ordinal data with fixed effects, and no such command is available in standard statistical software. Following Winkelmann and Winkelmann (1998), I therefore convert the 7-point LS score into a (1,0) dummy for having the LS scores of 6 and 7 (which roughly cuts the sample in half) and re-estimate Table 1's specifications using standard panel (or conditional) logit technique.

Table 3 Conditional logit estimates

Dependent variable: life satisfaction	CL with uninstrumented income	CL with instrumented income
Ln(real household income/square root of household size)	0.053 (0.027)+	0.512 (0.309)+
Observations	37251	37405
R^2 P	0.011	0.011

$+ < 10\%$

The life satisfaction variable used in this analysis takes a value of 1 if LS $\geq$ 6, and 0 otherwise. Robust standard errors are in parentheses. Wave dummies and regional dummies are included in all regressions. Other controls are as in Table 1

CL Conditional logit

Consistent with the results obtained in Table 1, the estimated conditional logit coefficient on instrumented income continues to be larger than that obtained in the panel logit estimator with uninstrumented income variable. When comparing the two CL estimates, the coefficient on instrumented income is almost ten times the size of the coefficient on uninstrumented income. Thus, these results seem to support previous work that it makes qualitatively no differences whether one assumes cardinality or ordinality in LS data (Ferrer-i-Carbonell and Frijters 2004).

One way to illustrate the differences in the size of the income coefficient between different estimators is to calculate the shadow price of another variable in the LS equation, preferably an exogenous one. Viewing the event of being struck by disability as a potentially exogenous event, I examine how much money would be required to compensate for the drop in LS from becoming disabled in the dataset. To compensate for disability in the FE model, income would have to be $e^{0.453/0.019} = e^{24}$ higher, on average, than the current household income. This is extremely large. It implies that, for an average real household income per capita of US \$17,000 in the sample, the compensation package would have to be higher than US \$ 4×10^{14}. On the contrary, income would have to be $e^{0.421/0.206} = e^{2.04}$ higher, on average, than the current household earnings to compensate for becoming disabled in the FE-IV model. Thus, for an average real household income per capita of US \$17,000, the implied compensation package would be approximately US \$114,000, which seems like a much more reasonable estimate for a compensatory package that could be awarded to the person in court.[10]

4 Conclusions

This paper is an attempt to estimate the causal effect of income on happiness. I use a set of new instrumental variables which are not considered before in the literature—namely, the proportions of those who showed the interviewer their payslip and those who have payslip but did not show—to instrument for income in life satisfaction equations.

[10] As for the OLS, RE and RE-IV models, to compensate for disability real household income per capita would have to increase by US \$94 millions, US \$80 millions, and US \$666,000, respectively.

Using the British Household Panel Survey dataset, I exploit both single and simultaneous equations panel data estimators to address both sources of endogeneity: unobserved heterogeneity and omitted time-varying factors bias. The results strongly suggest that, although neglecting unobserved heterogeneity causes income to be underestimated in LS equations the direction of the overall bias on income once both sources of endogeneity are accounted for is *negative*. This is consistent with the hypothesis that there is a significant attenuation bias associated with income and that income typically correlates positively with other things that we sometimes cannot sufficiently control for in our model such as work hours and relative incomes, most of which are known in the literature to be negatively correlated with LS.

There is at least one important public policy implication of this paper's findings, which concerns the way income is normally used in cost-benefit analysis—i.e. in the calculation of compensatory damages through the application of happiness equations (see, for example, Van Praag and Baarsma 2005; Frey et al. 2007; Dolan and White 2007; Oswald and Powdthavee 2008a,b; Sunstein 2007). Although the estimated FE-IV coefficient on income of 0.2 may not be considered a large effect in absolute value (in the sense that income has to rise by a large amount to raise LS by 1-point on a 7-point scale), its relative importance on happiness when compared to other determinants of LS is significantly improved from models where either sources of endogeneity are neglected. Without correcting for unobserved heterogeneity or the omitted time-varying variables problem or both, the compensation packages—which would be the amount awarded to the person by governments or by judges in courts—will be drastically overestimated (as illustrated in the case of compensating for disability in this paper). Future research in this area should therefore aim to control for both sources of endogeneity when trying to work out the true value of income against other socio-economic influences on happiness.

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Appendix A

See Table 4.

Table 4 Descriptive statistics for the British Household Panel Survey, 1996–2004

	M	SD
Life satisfaction	5.224	1.233
Ln(real household income/square root of household size)	8.475	0.819
Proportion of all household members with early payslip seen	0.015	0.087
Proportion of all household members with latest payslip seen	0.164	0.285
Proportion of all household members not showing payslip	0.421	0.368
Proportion of (other) unemployed household members	0.037	0.169
Proportion of (other) disabled household members	0.043	0.186

Table 4 continued

	M	SD
Proportion of (other) retired household members	0.078	0.256
Proportion of (other) non-labor categories	0.421	0.368
Number of dependent children (aged $<$ 16)	0.777	1.044
Male	0.482	.499
Age	39.277	13.331
Age-sq/100	17.204	10.813
Living as a couple	0.150	0.357
Widowed	0.006	0.080
Divorced	0.020	0.142
Separated	0.005	0.076
Never married	0.178	0.383
Self-employed	0.080	0.271
Unemployed	0.038	0.193
Retired	0.057	0.233
Disabled	0.043	0.202
Other non-labor categories	0.148	0.355
Education: other qualification	0.006	0.083
Education: apprenticeship	0.013	0.114
Education: CSE	0.040	0.197
Education: commercial qualification	0.021	0.144
Education: GCE O level	0.200	0.400
Education: GCE A level	0.147	0.354
Education: nursing qualification	0.011	0.107
Education: other higher	0.230	0.420
Education: teaching qualification	0.023	0.149
Education: first degree	0.109	0.311
Education: higher degree	0.026	0.161
No known health problems	0.460	0.498
Household size	3.314	1.242
Own home outright	0.187	0.390
Total N = 67,868		

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