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The Negative Externality of Peer Group Income: Evidence from Three Developed Economies

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Abstract

This paper examines the effect of peer group household income on happiness in three developed economies: the United States, Germany, and the United Kingdom, where we define peer groups by age, gender, and education. Using the most recent panel waves from the General Social Survey (GSS) and the European Social Survey (ESS), we find comparable results from all three countries, namely a negative coefficient of peer group household income that is statistically not different in absolute magnitude from the coefficient of the respondent's own household income. We find that this result is robust to an array of control variables, alternative estimators (including fixed effects), and income specification (linear vs. logarithmic). We interpret this as a possible explanation for the Easterlin Paradox because our estimates indicate that an equal increase in one's own household income and comparison household income (peer group income) leads to a zero-net gain in happiness.

Keywords: happiness economics, social comparison, peer group income, Easterlin Paradox

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1. Introduction

Underlying most of the elementary framework of microeconomics is the implicit assumption of noncompetitive consumption which implies that the consumption of one individual in no way affects the utility of anyone else. Naturally, prices, decided dually by supply and demand (the preferences of other individuals), affect the consumption of all individuals participating in the market, but here we are pursuing a different idea: people care about how much their peers are consuming and they are negatively affected by an increase in the consumption of their peers. This notion of competitive consumption stems primarily from the empirical findings of Easterlin (1974, 2016) who found an insignificant relationship between happiness and income on a macro-level but a significant one on a micro-level, such that an increase in all incomes is not associated with greater societal happiness, but at any given time people with higher incomes report higher levels of happiness. Comparing two isolated societies at different times, Frank (2005, p. 70-71) writes:

> "If the two societies were completely isolated from one another, there is no evidence to suggest that psychologists and neuroscientists would be able to discern any significant difference in their respective average levels of subjective well-being. Rather, we would expect each society to have developed its own local norm for (p.71) what constitutes adequate housing, and that people in each society would therefore be equally satisfied with their houses and other aspects of their lives."

And therefore, in this essay, we are proposing that peer groups constitute the norms and expectations of income for individuals. Fittingly, there exists evidence suggesting that preferences of individuals are interdependent (Alvarez-Cuadrado, Casado, & Labeaga, 2016; van de Stadt, Kapteyn & van de Geer, 1985), implying that comparisons between individuals and envy/aspirations may play a role in utility functions. Nonetheless, in a theoretic paper, Samuelson (2004) shows that Nature optimally incorporates relative consumption effects under imperfect information, regardless of the underlying sociological or psychological cause.

The issue of interconnected utility functions may seem irrelevant at first glance, but it has the potential to solve unanswered questions in the happiness economics literature, such as the Easterlin Paradox. Frank (2005) argues that this discrepancy can be explained by social comparisons as well as the ability of humans to adapt to new circumstances, which is evident in studies of the happiness of lottery winners that appear to show no long-term increase in happiness. Obviously in an empirical context such relative consumption effects need to be more

precisely defined in order to construct a meaningful measurement. As proposed by Clark (2012), there are several ways of measuring a relative consumption effect, such as the peer group consumption, consumption of friends, consumption in the past (which is basically an alternative formulation of hedonistically adaptative consumption) or aspirational consumption more broadly. The evidence suggests that these relative consumption effects do exist in terms of aspirational income (Frey & Stutzer, 2005) and reference group income (for example, see Ferrer-i-Carbonell (2005) for a study of panel data; see McBride (2010) for an experimental study). Relating to the Easterlin Paradox, this may explain why higher income individuals report greater levels of well-being (utility), but that no such relationship exists on a national level because the effect of income on well-being is relative to the income of their peer group (reference point of social comparison). If the relative income effect is large enough, the positive effect of an increase in individual income and the negative effect of peer group income may cancel each other. There is some evidence to suggest that past income does matter for utility in the short-run but not long-run and that people adapt slowly to an increase in peer group income (Li, Hsee & Wang, 2021). Therefore, we focus primarily on the effect of social comparison, although we include one control regression containing past income as a robustness check.

This essay uses data from the General Social Survey (GSS) running from 1991 to 2018 and data from the European Social Survey (ESS) running from 2010 to 2018. We will use these panel data for the United States, the United Kingdom and Germany to investigate our hypothesis of a negative externality of peer group income. This choice of countries is designed to make the results as comparable as possible because the three countries are somewhat culturally similar and have a comparable level of economic development. Furthermore, it can be argued that these countries have greater social mobility and stronger universal rights, such that income is not correlated with inherited social class in a country such as India (caste system) and better rights (in a country with corrupt juridical/political institutions). These countries are studied separably to avoid issues of cross-national comparisons such as regional differences, cultural interpretations of happiness, and translation issues of the question at hand (as two out of three countries use the same language), and prior level of economic development.

In chapter 2 we briefly review findings in the happiness economics literature that specifically relate to our study of peer group income, which leads into chapter 3 where we formulate our hypothesis. In chapter 4 we lay out our empirical approach that we will use to test our hypothesis. In chapter 5 we describe the data we use in our regressions and summarize general patterns in the data, e.g., distributions and time trends, that serve as justification for our

empirical approach as laid out in chapter 4. Finally, we present our results in chapter 6 and compare them to results in the literature, followed up by a sensitivity analysis in chapter 7. In chapter 8 we interpret the data using models of utility and then in chapter 9 we conclude our essay, referring back to the original problem formulation.

2. Literature Review

The field of happiness economics is generally recognized to have started with Easterlin (1974) who found a discrepancy between the relationship between subjective well-being (happiness) and income on a national versus individual level, indicating that raising the incomes of everyone appears not to lead to an increase in well-being for all, but at any given time people with higher incomes report higher levels of subjective well-being within a country. Similar findings have been replicated by many follow-up studies (Plagnol & Macchia, 2018; Easterlin, 2016). These findings have nonetheless been challenged in other studies. Stevenson and Wolfers (2008) argue that these findings are a statistical illusion and present evidence to the contrary, namely that economic growth does positively affect well-being. As explained by Graham, Chattopadhyay and Picon (2010), both sides of the debate may be right as these results depend on the time frame used, specification of the income variable and rate of economic growth. There are also issues concerning regional, cultural, and contextual (e.g., level of economic development) differences that are involved in multinational studies of happiness and income (Opfinger, 2016). Kaiser and Vendrik (2019) found a robust confirmation of the Easterlin Paradox for nine Northern and Western European nations over the long-term and found a non-robust rejection of the paradox for 11 Eastern European countries over the medium-term. As such, there seems to be a distinction between established capitalist Western countries and developing (transitional) countries. Caporale, Georgellis, Tsitsianis, and Yin (2009) found similar differences between Western and Eastern European countries concerning comparison income, with a negative significant coefficient for Western European countries.

To explain this phenomenon, several studies have looked at the effect of comparison income. In a within-country study of German households, Ferrer-i-Carbonell (2005) found that reference group income, based on education, age and region, was about as important for individual happiness as one's own income, albeit with a negative sign. As noted by Graham, Chattopadhyay and Picon (2010), such a single-country analysis may raise issues of broader applicability as cultural and contextual issues are at play. Nonetheless, in a multinational study, Grimes and Reinhardt (2019) found that within-country reference group income was of

approximately the same magnitude as one's own income, albeit with a negative sign, thus these effects roughly cancel each other. However, one difference in this study is that peer groups are defined based on employment status, implying that income comparisons are based on labor force status. In a study of married couples, Luttmer (2005) found that on a geographic level an increase in the incomes of one's neighbors led to a decrease in happiness, an effect that was greater for people more active in the community. A related study by Frey and Stutzer (2005) found that, using German panel data, aspirational income had a negative effect on happiness that was statistically significant and practically large, being approximately half the size of actual income in magnitude. As such, there may be a more general aspirational mechanism at play that partly uses peer group income as one of its arguments. Rojas (2019) similarly found that in Latin American countries, reference group income had a significant and negative effect on life satisfaction with a magnitude equal to approximately 57% of the respondent's own income. In this case, peer groups are based on age and gender but not education, which may be an issue as it is likely people compare themselves to people with the same level of education (e.g., unskilled versus skilled workers). Tsui (2014) found that peer group income, based on industry, age, class, education, and gender in Taiwan had a significant negative effect on happiness. One issue we take with this paper as well as Ferrer-i-Carbonell (2005) is the use of Oprobit over Ologit because, as we will show in the data section, the dependent variable is quite skewed and therefore allowing a greater variance in the error term seems appropriate. Furthermore, a general issue in the literature, based on the comments from Graham, Chattopadhyay and Picon (2010), is the lack of alternative income specifications as robustness checks.

The happiness economics literature uses panel data from surveys that may raise questions concerning exogenous causalities. Taking a more experimental approach, Kuhn, Kooreman, Soetevent, and Kapteyn (2011) found that higher-income households were happier overall, but lottery winnings did not significantly impact the subjective well-being of winners nor their nonwinning neighbors, although they significantly increased the probability of a new car purchase in the case when a neighbor won a car. However, in a related study, Oswald and Winkelmann (2019) found, on the contrary, that German lottery winnings were significantly more satisfied with life after winning and especially those who won large jackpots. The authors point out that other lottery studies often measured very small winnings. In another experimental study, McBride (2010) found that participants were negatively impacted by the payoff of their peers and participants tended to compare themselves to others like them.

In general, a problem in the literature is the use of old data, transnational pooling of data, too narrow or too broad peer group definitions, and questionable use of estimators/specifications. This may cause issues in several ways. Firstly, the increase in social media use in the last decade has led to an increase in social comparisons (see Briggs, Schoemann, Tucker, & Wirtz, 2021), which old data do not capture. Using more recent panel waves may result in an increase in the coefficient of comparison income. Secondly, as an illustration, using the expected income of full-time employed men aged 40-45 who work in construction in the same zip code as peer group income can end up noisily measuring the actual income of members or prospects of that peer group instead of their frame of income reference. Such issues are illustrated by Ifcher, Graham, and Zarghamee (2018), who found that peer zip code neighbors act as a positive externality whereas as regional neigbors act as a negative externality, implying the level of peer group abstraction is important. On the other hand, too broad peer group definitions, based only on productivity (Kühling & Welsch, 2015) or age and gender (Rojas, 2019), may not accurately reflect the true income point of reference of individuals. Therefore, in this essay, in comparison to related studies, we seek to use the most current data (2010-), a more appropriate estimator (Ologit instead of Oprobit), a more balanced and non-geographic peer group definition (age, education, and gender), and include an alternative, non-logarithmic income specification as a robustness check.

3. Problem Formulation

Studies in social psychology show that social comparisons are common and associated with negative emotions such as lower self-esteem (Eckles, Vogel, Rose, & Roberts, 2014), envy, guilt, and unmet cravings (Langer, White, Yariv, & Welch, 2006). Furthermore, negative upward comparisons generally tend to dominate (Olivos, Olivos-Jara, & Browne, 2021) regarding well-being and people seek out information about others to evaluate their own abilities and self-worth (Mussweiler 2020; Strickhouser & Zell, 2015). Mussweiler (2020, p. 46-47) concludes, “No matter whether deliberate or incidental, the effects social comparison have on the self are strong and far-reaching.” Combining such studies with findings in economics, such as the Easterlin Paradox, many models of utility functions incorporating such social comparison effects have been developed (see Clark, Frijters, & Shields, 2008; Fehr & Schmidt 1999; Andersson, 2009). The general idea is that people evaluate the utility of their income considering the income of their relevant others, which may include effects of status

signaling, envy, and pity (concern of inequality). For example, Fehr and Schmidt (1999) allow for a distaste for relatively lower consumption but also a concern for social inequality:

$$U_i = x_i - \alpha_i \max(x_j - x_i, 0) - \beta_i \max(x_i - x_j, 0)$$

However, stemming from the Easterlin Paradox, more recent models (Clark, Frijters, & Shields, 2008; Andersson, 2009) tend to focus on the distaste for relatively lower consumption, which is also more in line with findings in social psychology. Based on these theoretical frameworks and studies in social psychology, we postulate that there exists a robust negative externality of social comparison from peer group income, and we will test this hypothesis using the most recent panel data. The use of the most recent panel data is relevant as social media have recently subjugated people to constant idealized images of their peers, but this hypothesis draws inspiration primarily from social psychology and Frank (2005)'s notion of society forming norms for individuals, in which people interpret their circumstances relative to their peers. Illustrating this idea using a more general framework (Clark, 2012), we have $U(x_{it}, x_{it}^*)$, where x_{it}^* is the comparison income, and our main hypothesis is that $\frac{\partial U}{\partial x_{it}^*} < 0$ (Hypothesis 1) because of social income comparisons. Underlying causes of this negative sign can be explained by envy, signaling (see Kühling & Welsch, 2015) or as a reference frame of expectations (Frank, 2005). Relating our hypothesis to the literature, we can use Easterlin (1974)'s illustration:

$$U_{it} = \frac{C_{it}}{\sum_{j=1}^{k} a_j C_{jt}}$$

Where our hypothesis postulates that:

$$a_j = \begin{cases} \frac{1}{n_j} & \text{if person j is a peer group member} \\ 0 & \text{otherwise} \end{cases}$$

Specifically, we base the comparison income on the peer group income, measured in terms of the average, and define peer groups in terms of age group, educational degree and gender and allow for a yearly increase in the peer group incomes. Framed this way, this implies that the denominator is the expected peer group income (consumption) and that $\frac{\partial U}{\partial E(x_i|age,educ,sex,year)} < 0$. Pertaining to issues raised in the literature review, we prefer such a peer group definition to capture the effect of potential income, i.e., an evaluation of the respondent's income based on his or her point of reference, which we deem to be people with the same level of education (capturing job opportunities and human capital), age (capturing generational differences and tenures/work experience), and gender (capturing the effect of a

general gender wage gap). This definition differs, for example, from Grimes & Reinhardt (2019) who include labor force criteria but exclude education, Tsui (2014) who includes industry-level criteria and class dummies, Ferrer-i-Carbonell (2005)'s main definition which excludes gender, and Luttmer (2005) who uses low level geographic identifiers and only samples married people. As can be seen from these comparisons, there no commonly accepted peer group definition, but we prefer not to include too many defining characteristics such as low-level geographic location or industry dummies, which runs the risk of capturing local positive externalities such as public goods (better schools, less crime) or prospects (e.g., growing industry). On the other hand, we prefer to include education as it is a form of human capital that allows workers to distinguish themselves on the labor market, which also is more fixed than labor force status.

Ultimately, if we can capture a strong enough social comparison effect then the discrepancy between the significance of income on a micro and macro-level can possibly be explained by these social norms. If $\frac{\partial U}{\partial x_{it}} = -\frac{\partial U}{\partial x_{it}^*}$ (Hypothesis 1A), i.e., the marginal effects are of equal magnitude, then this implies that the positive effect of more income and the negative effect of comparison income may cancel each other and hence be a possible explanation for the Easterlin Paradox.

4. Empirical Approach

In our empirical context we need a way to measure utility. Panel data surveys often contain questions concerning life satisfaction and happiness, but for the sake of consistency we will only use questions regarding happiness. Conflating happiness with life satisfaction and vice versa is common in the literature but relating to concerns raised by Graham, Chattopadhyay and Picon (2010), we use only a question on happiness in this essay as we want the respondents to be asked the same question. Nonetheless, for our purposes we shall assume that happiness may be interpreted as utility, which is a standard assumption in the happiness economics literature.

Another relevant question is the reliability of these subjective measurements of happiness, but as demonstrated by Diener, Sandvik, and Seidlitz (1993a), subjective measurements of well-being are consistent over time and highly correlated with theoretically constructed measurements of happiness and life satisfaction; self-reported measures of well-being are adequate for examining homogenous cultures, but might cause trouble for cross-country analysis, as cultural factors come into play. This relates to our choice of countries in the

introduction. Furthermore, according to Veenhoven (1991), there is no systemic desirability distortion in the happiness variable, but there may be circumstantial distortions such as interviewee's mood, the weather, etc. Based on these papers, we will interpret our dependent variable, subjective happiness, as a meaningful and consistent measurement of well-being. Furthermore, we assume that happiness is synonymous with the concept of utility.

We will use two specifications of household income. The primary specification is the natural log of household income and the second specification is household income in a linear and quadratic term. These specifications are constructed to make utility concave with respect to income (i.e., marginal utility of income is decreasing). A similar approach to Ball and Chernova (2008) and Diener, Sandvik, & Seidlitz (1993b) is used to construct the household income variable, where it is assumed that the incomes that are recorded in brackets are uniformly distributed within each interval. Then the midpoint of each bracket is used as an approximation of the actual household income. Incomes recorded in the United States, Germany and the United Kingdom are adjusted for inflation using data from U.S. Bureau of Labor Statistics (2021), Federal Statistics Office of Germany (2021) and the Office for National Statistics (2021) respectively. US figures are converted into 2006 US dollars, whereas German and British figures are converted into 2015 euros and pounds respectively.

Assuming we have a representative sample of incomes, we can consistently (by the law of large numbers) estimate peer group income in a given year based on our demographic criteria by regressing income on the selected criteria variables and a time variable. The predicted values are by construction the conditional mean incomes of people with those characteristics in that year,

$$peerinc_{it} = \hat{\alpha}_0 + \hat{\alpha}_1 t + \hat{\alpha}_2 female_i + \sum_{n=1}^{5} \hat{\beta}_n agegroup_{it}^n + \sum_{j=1}^{k} \hat{\gamma}_j degree_{it} \quad \text{(Eq. 4.1).}$$

where a time variable is included to account for an increase in the average income over time. It is assumed that this growth is linear. The age groups are split into six groups: 15-24, 25-34, 35-44, 45-54, 55-64 and 65 and older. These groups are meant to roughly capture the different stages of life as it pertains to income, e.g., being a student, having a medium tenure and work experience, and being retired. The number of educational degree categories vary between datasets, but generally the categories capture the differences between the incomes of people with elementary, secondary, and higher education respectively. Our peer groups are restricted to each country, but not defined by geographic characteristics (e.g., region) within each country. This is partly to test an alternative peer group definition and partly because we are using recent survey waves (1991-2018, 2010-2018) and the recent increase in internet use and

mass media have made peer groups more large-scale. Finally, we arrive at the regression equation:

$$happiness_{it} = \alpha_0 + \alpha_1 \ln\text{inc}_{\text{it}} + \alpha_2 \ln peerinc_{it} + D'_{it}\pi + \sum_{n=1}^{k} c_n wave_n + u_{it} \qquad \text{(Eq. 4.2)}$$

where D_{it} is a set of demographic and socioeconomic controls, including marital status, religiosity, age in linear and quadratic form, labor force status (e.g., unemployed, employed full-time, etc), subjective health, and gender. These control variables and time dummies have been entered into the regression to control for external factors and unobservable events that might conflict with our results. For example, the financial crisis of 2008 is naturally tied to lower incomes and negative emotions, such as stress and disappointment. For the same reason, we include labor force status dummies. In a later sensitivity analysis, we will include the number of the household members, regional dummies, a minority ethnicity dummy, and hours usually worked in a week as additional controls. These are nonstandardized between datasets and are therefore not included in the primary regressions.

In particular, health is expected to be correlated with income in the United States because of a lack of access, but also the preferences of different income groups (see for example Matthew & Brodersen, 2018). The evidence for marriage influencing happiness is mixed (compare for example Frey & Stutzer, 2005, and Qari, 2014). Nonetheless, including a set of marriage dummies makes sense since we are using household income measurements and marital status (e.g., married vs. separated) allows us to control for different family arrangements. Household income might be related to marital status for women if the parental leave and similar responsibilities fall primarily on the wife. Furthermore, education is included as a regressor to control for unobservable factors such as social mobility and cognitive ability. It appears significant in many studies (see for example Frey & Stutzer, 2005; Reinhardt & Grimes, 2019). On the other hand, Helliwell and Putnam (2004) argue that education becomes insignificant given enough controls. Although these control variables are included in our regressions, the estimates themselves are not presented in the regression tables as we are not interpretating them directly, following the recommendation of Hunermund and Louw (2020).

As we are dealing with a discrete dependent variable measured on a limited scale, i.e., 1-3 or 0-10, it does not necessarily make sense to pretend that these variables are approximately continuous. Although OLS has some attractive properties such as ease of interpretation of coefficients, it causes the theoretical issue of constant marginal effects, depending upon the functional form used (e.g., income in linear form), which goes against utility theory that states that marginal utility is decreasing with income. These considerations give rise to an alternative

estimation technique such as an ordered response model, as the subjective well-being measurements can be assumed to be ordinal, nonetheless. Specifically, we will include Ologit estimates as the Ologit estimator deals with the issue of our limited dependent variable and, at the same time, is able to handle non-constant marginal effects. For ease of interpretation, however, we will primarily use OLS, while including Ologit to check for any potential issues stemming from the distribution of the dependent variable. For a general treatment of OLS see Verbeek (2017, Chapter 2). The choice of Ologit in particular comes from the uneven spread in the dependent variable as will be shown in the data section, which gives grounds for using Ologit over Oprobit as Ologit assumes a greater variance in the error term than Oprobit.

The following model has been constructed by combining aspects from Verbeek (2017, p. 230) and Greene (2007, p. 827) and adjusting it to our current context. We assume that there exists an underlying latent variable, $y_{it}^* = x_i'\beta + \epsilon_{it}$, that determines the observed outcome, such that:

$$
\begin{aligned}
&y_{it} = 0 \quad \text{if } y_{it}^* \leq 0 \\
&y_{it} = 1 \quad \text{if } 0 < y_{it}^* \leq \gamma_1 \\
&y_{it} = 2 \quad \text{if } \gamma_1 < y_{it}^* \leq \gamma_2 \\
&\vdots \\
&y_{it} = 10 \quad \text{if } y_{it}^* > \gamma_9
\end{aligned}
$$

This assumption allows us to calculate the probabilities as:

$$
\begin{aligned}
P(\{y_{it} = 0\}|x_{it}) &= P(\{y_{it}^* \leq 0\}|x_{it}) = \Phi(-x_{it}'\beta) \\
P(\{y_{it} = 1\}|x_{it}) &= P(\{0 < y_{it}^* \leq \gamma_1\}|x_i) = \Phi(\gamma_1 - x_{it}'\beta) - \Phi(-x_{it}'\beta) \\
&\vdots \\
P(\{y_{it} = 10\}|x_{it}) &= P(\{y_{it}^* > \gamma_9\}|x_i) = 1 - \Phi(\gamma_9 - x_{it}'\beta)
\end{aligned}
$$

In this case $\Phi(z) = \frac{1}{1+e^{-z}}$, which implies that the marginal effects are not constant as the derivative is a function of z.

5. Data and Descriptive Statistics

Data for the United States comes from the General Social Survey (GSS). Data for the United Kingdom and Germany comes from the European Social Survey (ESS). The GSS is a national representative survey of adults in the United States. The survey is conducted every other year (with the year 1993 being an exception to this regular gap), and our data runs 1991 to 2018. In all we use 15 rounds of the GSS. This timeframe has been chosen to be somewhat comparable to the European Social Survey data such that the time frames overlap. The ESS is a

multinational representative survey of adults in several European countries. Nonetheless, we shall only use data for Germany and the United Kingdom from this survey. The survey is conducted every other year from 2002 to 2020, but because of unavailability and inconsistent variables, data from 2002-2008 and 2020 have been omitted. As such, we have 5 waves of the ESS (2010-2018). As we are dealing with panel data, will be correct for autocorrelation by using the respondent's ID number as the cluster variable.[1]

In general, regardless of dataset used, observations have been dropped whenever the responses are missing or not interpretable, such as a response like "I don't know" or "not applicable." In a normal circumstance this may not be an issue with regards to the independent variables as the marginal effects may still be consistently estimated, but in our case we are also estimating peer group income. In case certain income groups systemically opt out, we may have an inconsistent estimate of peer group income as the sample may not be representative. The World Value Survey, for example, is commonly used in the literature, but as evidenced by Donnelly and Pop-Eleches (2018), the decile income rank variable in the WVS is significantly different from a uniform distribution in 234 out of 241 country data, which is one reason why alternative surveys are more favorable in our case, as a representative sample of incomes is needed for the peer group variable. The distribution of the income deciles/quintiles of each country is shown below. The distributions for the UK and Germany come directly from the item hinctnta, whereas the quintiles for the United States have been have manually constructed using the criteria:

$$Q_{it} = \begin{cases} 1 & \text{if } 0 \leq \mathrm{P_t(y_{it})} \leq 0.2 \\ 2 & \text{if } 0.2 < \mathrm{P_t(y_{it})} \leq 0.4 \\ 3 & \text{if } 0.4 < \mathrm{P_t(y_{it})} \leq 0.6 \\ 4 & \text{if } 0.6 < \mathrm{P_t(y_{it})} \leq 0.8 \\ 5 & \text{if } 0.8 < \mathrm{P_t(y_{it})} \leq 1 \end{cases}$$

where the income distribution, $P_t(*)$, is based on annual US Consensus Bureau (2020) data.

decile	1st	2nd	3rd	4th	5th	6th	7th	8th	9th	10th
DE	7.41%	8.88	9.71	10.23	10.50	9.94	11.05	11.41	9.85	11.03
UK	14.69%	12.84	11.09	8.46	8.46	8.70	9.21	9.05	7.77	9.55

Relative frequencies of reported income deciles for Germany and United Kingdom

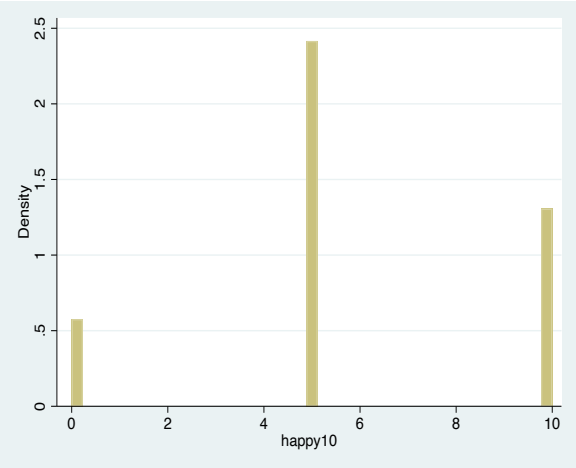
quintile	1st	2nd	3rd	4th	5th
US	21.28%	24.20	19.31	21.89	13.33

Relative frequencies of reported income quintiles for the United States

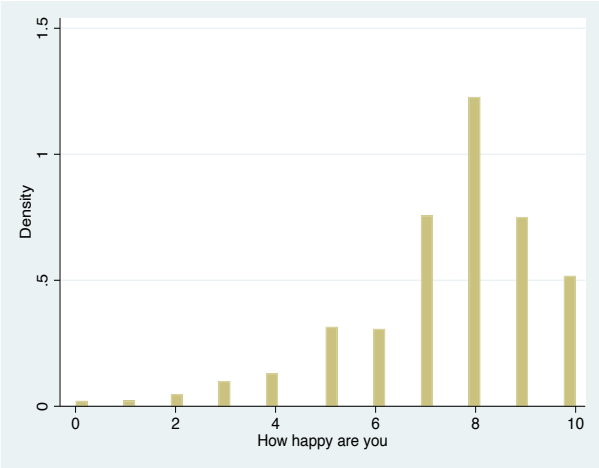
[1] This is done using the vce(*) command in Stata.

Overall, the income distributions are far from perfectly uniform. The 5th income quintile of the sample income distribution of the United States appears to be underrepresented, but the opposite seems to be the case for the German sample, where the top deciles are slightly overrepresented. It is difficult to tell whether this is due to simple sampling error, unrepresentative samples, error in the income estimation of respondents or something else entirely. A comparison of the results from all three countries allows us to overcome these discrepancies somewhat by looking at any significant deviations of coefficients between countries.

The dependent variable comes from the item 'happy' in each survey, which asks respondents "Taking all things together, how happy would you say you are" on a 0-10 scale in the ESS with zero meaning "extremely unhappy" and ten meaning "extremely happy." The GSS contains a similar happiness question on a 1-3 scale. To make the coefficients easier to compare across surveys, these values have been converted to a 0-10 scale in case of an OLS regression. Geometrically, this is equivalent to connecting a line between $(1,0)$ and $(3,10)$, which gives rise to the conversion formula $f(x) = 5x - 5$, such that 1 is assigned the value 0; 2 is converted into 5 and 3 is reassigned the value 10. At last, we will assume that these responses can be ordinally compared such that we may use OLS and Ologit to estimate the coefficients.



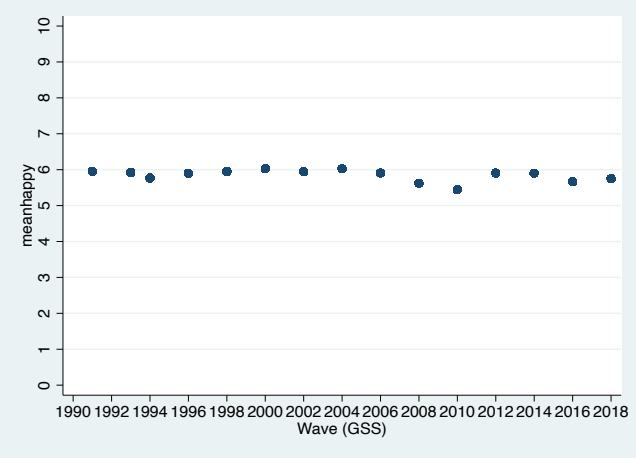
Distribution of happiness (GSS)

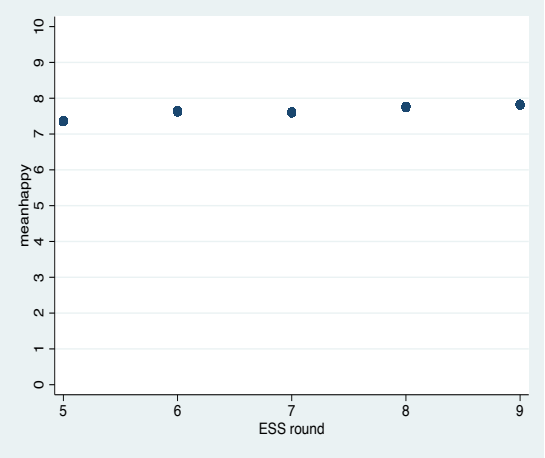


Distribution of happiness (ESS)

The distributions of the dependent variables exhibit left-skewedness with relatively fewer people reporting extremely low levels of happiness. This is not completely unexpected as social (e.g., cultural) expectations may cause respondents to overestimate their subjective happiness. Real issues may arise in case this overestimation is correlated with income, such that high income respondents report higher levels of happiness because this is what the culture (super-ego) expects of them. Ultimately, we will have to assume that this is not the case. This

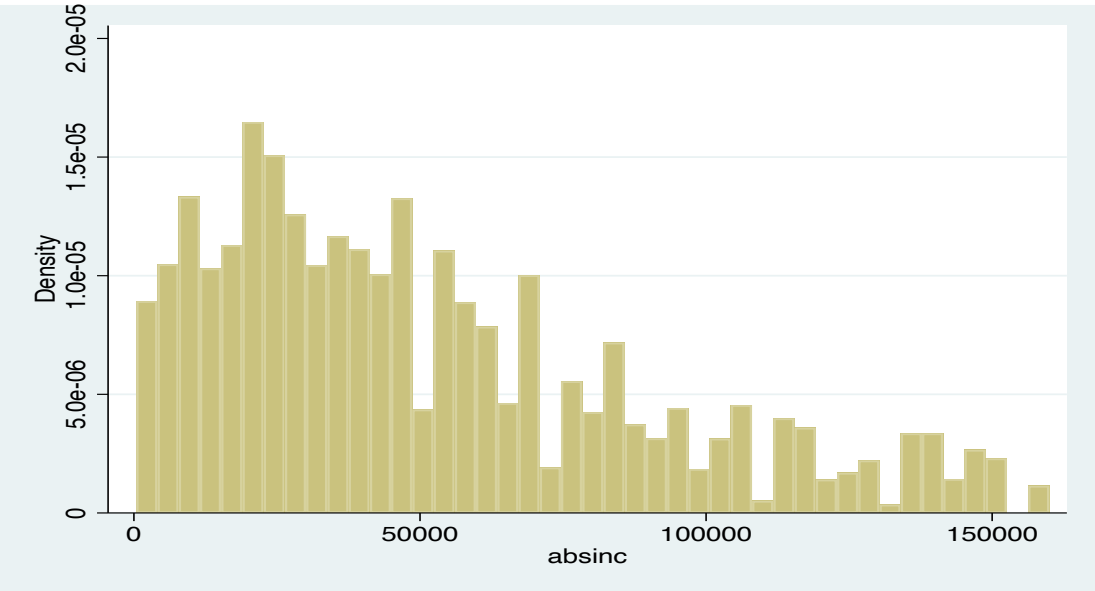
skewedness leads to our use of Ologit in contrast to Ferrer-i-Carbonell (2005) and Tsui (2014) who use Oprobit, which assumes a smaller variance.





Average Happiness 1990-2018 (GSS) *Average Happiness 2010-2018 (ESS)*

Although reported subjective happiness is remarkably rigid year to year, the years around 2008 seem to deviate from the trendline. In the ESS wave 5 is the year 2010, and so subjective well-being has increased somewhat since. This is only natural considering the drastic change in unemployment, income, household net worth, consumption, stress, etc., because of the economic circumstances resulting from the 2008 financial crisis. Therefore, we include wave dummies to control for such societal events as noted earlier.



Distribution of Real Incomes (GSS)

As evident by the graph above, the distribution of household income in the surveys exhibit a typical log-normal shape. The ESS distributions of the UK and Germany are very similar. Therefore, it is quite convenient to transform the real incomes using the natural logarithm. This furthermore also makes the interpretation of coefficients easier as we do not have to deal with

specific monetary amounts but rather %-changes in income. The coefficients in level terms are also expected to be small as the marginal effects of one unit of income on happiness are tiny. Overall, a natural log transformation is more in line with microeconomics, more practical and captures a more realistic relationship (utility is concave in income) between income and well-being, which is why we prefer it as our primary income specification.

Descriptive Statistics: United States

Variable	Obs	Mean	Std. Dev.	Min	Max
happy10	21699	5.849	3.199	0	10
lnrealinc	21699	10.485	1.012	5.991	11.983
lnrealpeerinc	21699	10.619	.645	7.168	11.813
age	21699	46.271	16.943	18	89
sex	21699	1.546	.498	1	2

Descriptive Statistics: Germany

Variable	Obs	Mean	Std. Dev.	Min	Max
happy	12276	7.615	1.786	0	10
lnrealinc	12290	10.27	.625	8.705	11.191
lnrealpeerinc	12254	10.411	.229	9.5	10.863
age	12283	49.841	17.909	15	102
sex	12290	1.481	.5	1	2

Descriptive Statistics: United Kingdom

Variable	Obs	Mean	Std. Dev.	Min	Max
happy	9084	7.535	1.884	0	10
lnrealinc	9097	9.992	.762	8.57	11.156
lnrealpeerinc	9066	10.193	.347	9.218	10.702
age	9062	51.504	17.817	15	95
sex	9097	1.548	.498	1	2

Some of the main variables of interest are summarized above. In all three datasets, women are assigned the value 2 and men 1. As the mean value of the item *sex* is 1.55 for the US and UK, then this means that women are overrepresented in these samples. On the other hand, men are slightly overrepresented in the German sample. It also must be noted that the GSS samples pre-tax household income, whereas the ESS samples post-tax net household income. As expected, the log of real incomes shows a greater range and standard deviation in the United States, compared to the UK and Germany. Mean happiness in the United States (rescaled) is also slightly lower than in Germany and the United Kingdom. Nonetheless, the mean is still slightly above the midpoint of the scale. Another important difference to notice is the age ranges: the ESS surveys 15 to 17-year-olds, whereas the GSS only surveys people aged 18 or older.

6. Results

Our starting point is the predicted peer group income values from Eq. (4.1). In the GSS and ESS the year 1991 and wave 5 respectively are normalized to $t = 1$. The results of peer group income are as expected: income grows over time, women earn less than men with the same

level of education and age, and educated people earn more. In general, the 65+ category earns the least. This is natural as social security/pension is their main source of income. The negative coefficient of the female dummy means that women in general report less household income than men, regardless of education and age. This gender income gap is present in all three countries. Overall, none of the predicted values are negative.

VARIABLES	(US) income	(DE) income	(UK) income
t	1,144***	2,494***	1,543***
	(27.32)	(105.9)	(121.9)
female	-7,083***	-1,679***	-3,158***
	(455.9)	(290.7)	(347.7)
age15(18)-24	271.0	9,569***	2,589***
	(965.7)	(615.4)	(786.2)
age25-34	1,892**	4,004***	5,021***
	(751.2)	(498.7)	(586.4)
age35-44	12,870***	8,840***	8,705***
	(745.5)	(486.8)	(551.3)
age45-54	15,745***	9,932***	9,656***
	(773.3)	(438.9)	(544.7)
age55-64	13,154***	5,929***	5,291***
	(826.2)	(449.8)	(538.9)
educ-lowersec		2,869**	
		(1,128)	
educ-uppsectier1		6,115***	
		(1,088)	
educ-uppsectier2		4,362***	
		(1,275)	
educ-advvoc		11,729***	
		(1,110)	
educ-lowtertiary		15,672***	
		(1,170)	
educ-hightertiary		20,164***	
		(1,134)	
educ-other		6,600*	
		(3,491)	
highschool	15,148***		
	(693.0)		
jrcollege	22,410***		
	(1,051)		
bachelor	39,076***		
	(828.5)		
graduate	52,388***		
	(971.8)		
educ-lowersec			6,216***
			(644.2)
educ-uppersec			6,999***

			(550.4)
educ-postsec			9,428***
			(1,010)
educ-tertiary			16,818***
			(506.3)
edu-other			7,314***
			(1,236)
Constant	14,941***	13,321***	13,455***
	(1,099)	(1,227)	(771.8)
Observations	21,699	12,254	9,066
R-squared	0.278	0.196	0.218

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Now that we have obtained the expected household incomes of each respondent, based on their demographic criteria, we use these values to estimate our main equation (4.2). Each set of regressions contains two specifications, one with peer group income and one without, such that we can compare the change of the log income coefficient once we include our measurement of peer group income. The first two regressions use Ologit and the second two regressions use OLS.

United States	(Ologit1)	(Ologit2)	(OLS1)	(OLS2)
VARIABLES	happy10	happy10	happy10	happy10
lnrealinc	0.160***	0.163***	0.230***	0.233***
	(0.0184)	(0.0185)	(0.0267)	(0.0267)
lnrealpeerinc		-0.205***		-0.286***
		(0.0626)		(0.0922)
age	-0.0269***	-0.0176***	-0.0383***	-0.0252***
	(0.00552)	(0.00626)	(0.00808)	(0.00915)
agesq	0.000325***	0.000231***	0.000463***	0.000332***
	(5.70e-05)	(6.44e-05)	(8.30e-05)	(9.38e-05)
female	0.0509*	0.0156	0.0776*	0.0277
	(0.0286)	(0.0302)	(0.0423)	(0.0446)
Constant			1.583***	3.963***
			(0.354)	(0.837)
Controls	Yes	Yes	Yes	Yes
Wave dummies	Yes	Yes	Yes	Yes
Observations	21,699	21,699	21,699	21,699
R-squared (*Pseudo)*	*0.0867*	*0.0869*	0.153	0.154

Robust standard errors in parentheses
Controls: Labor force status, education, marital status, religiosity, health.
*** p<0.01, ** p<0.05, * p<0.1

Starting with the United States, we can see that in all cases the natural log of the respondent's own household income has the expected sign and a high level of significance ($p < 0.01$). In accordance with economic theory, this means that an increase in actual household income has a positive effect on reported happiness, holding constant other variables such as health, marital status, and work status. Using the OLS coefficient and considering the level-log relationship, our estimate says that a 1% increase in household income leads approximately to a 0.00233 increase in happiness on a [0-10] scale. This small marginal effect is nonetheless in line with previous findings (see for example Ball & Chernova, 2008). Remarkably, this coefficient is not much affect by the inclusion of peer group income, neither in terms of magnitude nor significance. Controlling for peer group income does little to affect the standalone effect of the respondent's own income. As evidenced by inclusion of log of peer group income, the R^2 increases remarkably little by inclusion of the income variables, even though they are highly significant. Much like the small marginal effect, this indicates that there is more to happiness than income (see also Lindeløv, 2021).

In both specifications the natural log of peer group income has the expected sign and significance level in accordance with our hypothesis. Following a similar interpretation, a 1% increase in peer group income is associated with a -0.00286 decrease in happiness. Furthermore, peer group income in the OLS has a p-value of 0.002 and 0.001 in the Ologit specification. Therefore, under the criterium $p < 0.01$, we conclude that our hypothesis (1) for the United States seems to hold. Interestingly, the absolute size of the coefficient (in the OLS) is very close to the absolute size of respondent's actual household income. Therefore, we postulate that

$$\beta_{lnrealinc} + \beta_{lnrealpeerinc} = 0$$

and find that this test has a p-value of 0.5774 in the OLS specification. This means statistically that the coefficients have the same magnitude, albeit with opposite signs. Therefore, our estimates indicate that these effects cancel each other out, such that the respondent gets no net-gain in happiness from an equal increase in his own household income and his comparison income, which we define as the expected mean income conditional on age, education, gender, and time. If anything, his happiness decreases slightly. Practically the Ologit regression likewise shows a negligible absolute difference between the coefficients (although these are not directly comparable). Overall, the sign and significance level in the Ologit regressions correspond with the OLS results.

After controlling for peer group income, the female gender-dummy becomes insignificant and its coefficient size decreases. Nonetheless, the female dummy is only significant at $p < 0.10$ in the first regressions without controlling for peer group income. This can be contrasted with Helliwell & Putnam (2004) who found that women reported higher levels of life satisfaction in the United States, although this gender happiness gap was inconsistent globally. Age minimizes happiness at age 41.4 in the OLS specification without peer group income and age 38 with peer group income. However, it must be noted that the coefficients are statistically unchanged. It appears that our measure of peer group income has no effect on the U-shaped relationship between subjective well-being and age that is often found in the literature (see also Clark, 2019).

Germany	(Ologit1)	(Ologit2)	(OLS1)	(OLS2)
VARIABLES	happy	happy	happy	happy
lnrealinc	0.491***	0.502***	0.474***	0.482***
	(0.0344)	(0.0347)	(0.0314)	(0.0316)
lnrealpeerinc		-0.632***		-0.520***
		(0.225)		(0.201)
age	-0.0634***	-0.0560***	-0.0522***	-0.0465***
	(0.00760)	(0.00819)	(0.00667)	(0.00711)
agesq	0.000643***	0.000552***	0.000535***	0.000465***
	(7.74e-05)	(8.56e-05)	(6.70e-05)	(7.33e-05)
female	0.126***	0.0926**	0.0969***	0.0689**
	(0.0343)	(0.0361)	(0.0308)	(0.0325)
Constant			1.178***	6.257***
			(0.394)	(1.992)
Controls	Yes	Yes	Yes	Yes
Wave dummies	Yes	Yes	Yes	Yes
Observations	12,182	12,182	12,182	12,182
R-squared	*0.0557*	*0.0559*	0.200	0.200
(Pseudo)				

Robust standard errors in parentheses
Controls: Labor force status, education, marital status, religiosity, health.
*** p<0.01, ** p<0.05, * p<0.1

Moving on to Germany, we find comparable results. First, we see that a 1% increase in the respondent's actual household income leads approximately to a 0.00482 increase in happiness. This effect is significant at $p < 0.001$ in all four regressions. The coefficient increases somewhat after including peer group income, although not significantly. We can see that peer group income similarly to the US regressions has the expected sign and significance level. Specifically, peer group income has a p-value of 0.01 in the OLS specification and 0.005 in the

Ologit specification. Our estimate implies that a 1% increase in peer group household income leads to a -0.0052 decrease in happiness. The hypothesis

$$\beta_{lnrealinc} + \beta_{lnrealpeerinc} = 0$$

has a p-value of 0.8511 in the OLS regression. Therefore, statistically, this means an equal increase in the respondent's household income and his peer group's household income leads to a zero net-gain in reported happiness, much like with the case of the United States.

In a similar fashion, age minimizes happiness in Germany at age 48.8 in the OLS specification without peer group income and 50 in the specification with peer group income. This difference in coefficients is not statistically significant. The female gender-dummy retains its significance at $p < 0.05$ in the German regressions after controlling for peer group income, but the coefficients shrink in size. However, this difference in coefficients is not statistically significant.

United Kingdom	(Ologit1)	(Ologit2)	(OLS1)	(OLS2)
VARIABLES	happy	happy	happy	happy
lnrealinc	0.282***	0.286***	0.316***	0.319***
	(0.0340)	(0.0341)	(0.0329)	(0.0329)
lnrealpeerinc		-0.571***		-0.667***
		(0.217)		(0.202)
age	-0.0502***	-0.0387***	-0.0466***	-0.0333***
	(0.00770)	(0.00881)	(0.00722)	(0.00806)
agesq	0.000583***	0.000458***	0.000532***	0.000386***
	(7.61e-05)	(8.96e-05)	(7.05e-05)	(8.11e-05)
female	0.149***	0.0758	0.108***	0.0218
	(0.0396)	(0.0479)	(0.0383)	(0.0461)
Constant			3.059***	9.337***
			(0.390)	(1.940)
Controls	Yes	Yes	Yes	Yes
Wave dummies	Yes	Yes	Yes	Yes
Observations	8,972	8,972	8,972	8,972
R-squared (*Pseudo)*	*0.0423*	*0.0425*	0.159	0.160

Robust standard errors in parentheses
Controls: Labor force status, education, marital status, religiosity, health.
*** p<0.01, ** p<0.05, * p<0.1

Finally, our last sample from the United Kingdom shows similar results. The United Kingdom shows a similar level of significance of the peer group coefficient at $p < 0.01$ in the OLS and Ologit specification. Our point estimate of the respondent's own household income shows that a 1% increase in income approximately leads to a 0.00319 increase in happiness, whereas a 1% increase in peer group income leads to a -0.00667 decrease in happiness. The coefficient of the respondent's own household income is not affected by controlling for peer group income,

neither in terms of significance nor practical size. Although the coefficient in the British sample is somewhat greater relative to the respondent's own income, we must keep in the mind the large standard error. Following a similar analysis to the previous two countries, we find that

$$\beta_{lnrealinc} + \beta_{lnrealpeerinc} = 0$$

has a p-value of 0.0863 in the OLS specification. Keeping the large sample size in mind, because this difference is insignificant at $p<0.05$, we may infer that an equal increase in the respondent's own income and comparison income leads to a zero-net gain in happiness, just like in the samples from the United States and Germany.

In the United Kingdom, age minimizes happiness at age 43.8 in the OLS specification without peer group income and 43.1 with the peer group income control, but this difference is not significant. The female dummy loses its significant after inclusion of peer group income in both specifications. Although the standard error increases somewhat, it is mainly the coefficient itself that shrinks. On the other hand, peer group income seems to play absolutely no role in the relationship between happiness and age in all three countries. According to Schwandt (2016), this U-shaped relationship can be explained by unmet expectations of subjective well-being over one's lifecycle and these wrong expectations are not explained by socioeconomic variables. Our results indicate that this relationship is robust to inclusion of peer group income as well. Overall, the U-shaped relationship between age and happiness appears quite robust in our regressions, but a happiness gender gap (see also Helliwell & Putnam, 2004) is not quite apparent in our results.

All in all, we find coherent results for all three countries. Generally, incomes have small marginal effects on happiness and explain a tiny fraction of the variance in happiness as evidenced by the small increase in the R^2. Typically, the Ologit specifications show a pseudo R^2 of 4%-9%, whereas the OLS regressions typically show a R^2 of 15%-20%. Nonetheless, these marginal effects are highly significant. Using a different dataset (country and time-wise), estimator and peer group criteria, these results mirror, e.g., Grimes and Reinhardt (2019), Ferrer-i-Carbonell (2005), Caporale et al (2009), and Luttmer (2005). Regardless of specification, we find that the natural log of peer group income is highly significant with a negative coefficient. Although related papers in the literature rarely explicitly test for a significant absolute difference, our point estimates are most similar to Grimes and Reinhardt (2019) and Ferrer-i-Carbonell (2005) but differ a bit in magnitudes from Roja (2019) who used only age and gender as peer group criteria in a study of Latin American countries. Comparing our results to the literature, it appears that including or excluding one of the variables: gender,

education, industry identifiers, or labor force status in the peer group definition leads to comparable results of a significant, negative coefficient of peer group income. Similarly, our use of more recent data leads to the same conclusion concerning peer group income. In comparison to Ifcher, Graham, and Zarghamee (2018) who use different geographic identifiers in their peer group definition, it appears that using different levels of geographic identification can, on the other hand, lead to opposing results.

7. Sensitivity Analysis

As our analysis is based on a few implicit and explicit assumptions, we will conduct some a few control regressions to check for the robustness of our results.

VARIABLES	(**SA1**) happy10-OLS	(**SA2**) happy10-Ologit	(**SA3**) happy-OLS	(**SA4**) happy-OLS
lnrealinc	0.261***	0.160***	0.492***	
	(0.0291)	(0.0201)	(0.0352)	
lnrealpeerinc	-0.303***	-0.223**	-0.647***	
	(0.0992)	(0.0899)	(0.212)	
lnrealinc_lag		-0.000462		
		(0.0153)		
realinc				2.40e-05***
				(4.44e-06)
realincsq				-1.58e-10***
				(5.32e-11)
realpeerinc				-3.20e-05***
				(8.55e-06)
Controls	Yes	Yes	Yes	Yes
Age & gender[2]	Yes	Yes	Yes	Yes
Wave dummies	Yes	Yes	Yes	Yes
Extra controls	No	No	Yes	No
Constant	3.987***		6.800***	6.100***
	(0.903)		(2.100)	(0.253)
Observations	21,699	18,154	11,450	8,972
R-squared (*Pseudo)*	0.156	*0.091*	0.203	0.161
Number of id_	3,545			

Robust standard errors in parentheses
Controls: Labor force status, education, marital status, religiosity, health.
Extra controls: Total hours usually worked in a week, number of members of the household, regional state dummies (German states), dummy for belonging to minority ethnicity.
*** p<0.01, ** p<0.05, * p<0.1

[2] Gender is omitted in the SA1 (fixed effects) regression as it is time-invariant.

SA1: So far, we have not been able to control for unobservable effects such as personality. One might postulate that personality factors (optimism, work ethic, depressive tendencies) are tied to happiness and income, e.g.:

$$happiness_{it} = \alpha_0 + \alpha_1 \log(inc_{it}) + \alpha_2 \log(peerinc_{it}) + \cdots + \omega personality_i + error_{it}$$

our use of FE (as opposed to random effects) means that we are assuming $Cov(peerinc, fixed\ characteristics) \neq 0$. For a more general treatment of FE, see Wooldridge (2016, Chapter 14). We find that peer group income is still significant at $p < 0.01$ and that the sign and magnitude are left intact. Therefore, our findings are robust to any fixed differences between peer groups, e.g., personality types tied to different peer groups.

SA2: Secondly, we run a regression for the United States that includes the natural log of household income lagged to control for adaptation to income over time. We see that past income is insignificant at $p < 0.10$ with a negative coefficient, which indicates that more household income in the past does not decrease current reported happiness (similarly, Paul & Guilbert (2013) found that lagged income in Australia was insignificant). Lagged income is supposed to control for related psychological phenomena such as adaptation, i.e., people who are used to a higher income level are unhappier, but, more importantly for our purposes, the effect of peer group income is not altered by the inclusion of lagged log income.

SA3: Next, we run a regression for Germany with more control variables: hours usually worked in a work, number of members of the household, regional state dummies, and a dummy with value one if the respondent belongs to a minority ethnic group. These are not included in the main regressions because they are not standardized between datasets. Trivially, including more controls increases the R-squared to 20.3%, but these extra controls also increase the significance level of peer group income for Germany, while the difference in peer group income coefficients is not statistically significant. Peer group income is still negative and statistically of approximately the same absolute magnitude as the respondent's own income.

SA4: It is possible that our use of log of household income causes some statistical illusions. Without assuming a functional form for peer group income, we find that the quadratic term is not significant (not shown). However, using a level specification of income for the United Kingdom with a linear and a quadratic term for the respondent's and peer group's income in linear form shows that all three variables are significant at $p < 0.001$. The quadratic respondent's own income term is significant, whereas the peer group quadratic term is not. A test of cancellation of coefficients at the average respondent's own income cannot be rejected

at $p < 0.05$ in line with our primary income specification. This is also some indication that the marginal (dis)utility of peer group income is constant, albeit significant and negative.

8. Interpretation and Discussion

Following up on the framework introduced in the formulation, we can see that our constructed weights of comparison income (which we interpret as a proxy for consumption):

$$a_j = \begin{cases} \frac{1}{n_j} & \text{if person j is a peer group member} \\ 0 & \text{otherwise} \end{cases}$$

where peer groups are based on age, gender, and education, lead to a statistically significant coefficient of comparison income that is robust to an array of alternative specifications and country specifics (United States, Germany, and the United Kingdom), and has a sign that is in line with behavioral economic theory (see for example Fehr & Schmidt, 1999). The significance of the variables involved imply that $U(x_{it}, x_{it}^*)$ is a function of the respondent's income and his comparison income. Specifically, our estimates, because of the consistently negative coefficients, indicate that $\frac{\partial U}{\partial x_{it}^*} < 0$, i.e., utility is decreasing in comparison income. Interpretating happiness as utility, statistically and practically our estimates indicate that:

$$U(x_{it} + \Delta x_{it}, x_{it}^* + \Delta x_{it}) = U(x_{it}, x_{it}^*)$$

such that an individual in a developed economy is not made happier by an equal increase in all incomes of his peer group (including his own). This means that an increase in comparison income, which the individual has no control over, leads to a lower utility level. This also implies that an increase in the income of one of the peer group members negatively impacts all other members of that peer group. On an aggregate level, using the peer group mean as measurement, this negative externality of social comparison is so large as to cancel any positive effect from an equal change in the individual's own income. In a similar fashion to the model by Clark, Frijters, and Shields (2008), we find that this explains the Easterlin Paradox as consumption from income is competitive and individuals do not consume in isolation, but rather utility contains relative consumption effects. To answer a modified version of Easterlin's question, "Does raising the income of everyone equally leave everyone better off?", the answer for the United States, the United Kingdom and Germany seems to be "no" as social competition between individuals carries with it a sizable negative comparison externality. Nonetheless, as highlighted by Caporale et al (2009), this finding does not immediately apply to all economic contexts as the effects of absolute income vary depending on the level of economic

development. In a country with low economic development, higher peer group income may be associated with greater standards of living and expectations of economic prosperity, whereas large parts of consumption in a developed economy may be spent on conspicuous goods. This may explain why Roja (2019) in a Latin American study found a slightly smaller absolute magnitude of peer group income. Such an explanation is also in line with the differences of social comparisons in Western and Eastern European countries.

Relating to Frank (2005)'s statement about social norms, Schor (2015, p. 2) writes, "Luxuries turn into necessities with lower status, because everyone owns them, and the rich move on to the next new or more expensive thing. Absolute increases in spending yield social value only when they improve relative position. When increases in the standard of living are general, they are like a treadmill, merely keeping people from falling behind." Our results are evidence in favor of these notions of social comparisons regarding income that result in disutility for individuals in developed Western economies. This social comparison effect stands separately from adaptation (see **SA2)** and is generally so large as to cancel any gain in utility from an increase in individual's own income. In light of related literature, such an interpretation is sensible, as individuals adjust much slower to status than income (di Tella, Haisken-De New & MacCulloch, 2010) and people's tendencies to compare themselves to others like them (McBride, 2010). As individuals have no influence on the aggregate income level of their peer group, it may therefore be interpreted as being exogenous in the utility function. We see accordingly that the respondent's own income coefficient is not affected in any way by the inclusion of peer group income. Ultimately this implies that peer group income serves as a negative externality for individuals such that as people's peer groups get richer, individual members, ceteris paribus, become unhappier.

Such an interpretation of the evidence presented, where people derive disutility from the higher income of their peers because of social comparison, can serve as an explanation of the Easterlin Paradox in a general sense. In a developed economy where people have most, if not all, of their most basic material needs met, the largest effect of income/consumption is relative; people form their expectations of income based on people like them and disutility is generated by these expectations. This does not mean necessarily that income is irrelevant to well-being, but more so that utility is a dynamic concept that involve a conspicuous effect that we have found evidence for in three highly developed economies. Similarly, according to Frey and Stutzer (2005, p. 18), "aspirations are systematically affected by the average income in the community where people live. The richer one's fellow residents are, the higher is an individual's

aspiration level. This effect cannot be explained by a higher cost of living alone." However, such a conspicuous effect may not be present in less economically developed contexts.

Although we did not control for the number of children, the relationship between number of children and well-being appears to be weak at best in the literature (see Clark, 2019; Frey & Stutzer, 2005). On the other hand, although one of our control regressions controlled for the number of household members, using alternative income measurements, such as individual income or defining peer group household income by the number of adults or working adults in the household may be ideas for further research. However, such data was not available or inconsistent in the panels used in this essay. Similarly, although rarely dealt with in the literature, a concern in this essay is that we are not controlling for a potential distaste for inequality as illustrated in the Fehr and Schmidt (1999) model. Naturally, higher peer group income may capture some of this effect by measuring the correlation between the average income of certain peer groups and happiness (which we interpret as utility).

9. Conclusion

Regardless of certain oddities, such as an underrepresented top quintile in the US sample, different gender ratios in the samples, etc., we nonetheless find similar results for all three countries. Similarly, our findings are in accordance with the consensus in the literature, including papers using an experimental approach and longitudinal studies. Our conclusion is that utility is a function of peer group income because of a social comparison effect as theorized in microeconomic models. We have not, however, found the root cause of this social comparison negative externality, as it may be due to positional consumption (signaling), lower self-esteem, envy or simply because of a changing reference frame of income. Citing related literature, these different causes may nonetheless be contextual (see for example Kühling & Welsch, 2015). As predicted by related literature, we find that defining peer groups using demographic criteria leads to a significant negative coefficient with a subjective well-being variable as the dependent variable. In contrast to Ifcher, Graham, and Zarghamee (2018) who found vastly different signs of the coefficient depending on choice of geographic level, we find that our slightly different peer group definition leads to results that are similar to those in the literature. As one of our control regressions (SA3) suggests, including regional variables in the happiness regression itself does not alter this result. Like related papers in the literature, we find that peer group income has a negative effect on subjective well-being, despite using a different peer group definition and estimator, newer and alternative data, and studying more

countries. As predicted by microeconomic models, we find evidence that a general psychological negative externality stems from peer group income, i.e., $\frac{\partial U}{\partial x_{it}^*} < 0$ (Hypothesis 1). In our case, this effect is statistically so large as to cancel the positive effect of an increase in the respondent's own income, $\frac{\partial U}{\partial x_{it}} \approx -\frac{\partial U}{\partial x_{it}^*}$ (Hypothesis 1A), and these findings are insensitive to a linear income specification, extra controls (including members of the household), income adaptation, and a fixed effects assumption. As proved by the theoretic frameworks of Andersson (2009) and Clark, Frijters, and Shields (2008), our findings have larger implications. For example, it may serve as an explanation of the Easterlin Paradox and, secondly, it has implications for optimal tax policy, as people impose a negative externality on their peers by increasing their income, as well as the marginal propensity to consume.

This leads us to ideas for further research. Our control regression that uses income in linear form indicates that happiness may not be concave in peer group income as the quadratic term is not significant. Issues pertaining to loss aversion may be relevant here. Furthermore, the potential distaste for social inequality (as highlighted by the Fehr & Schmidt (1999) model) may be correlated with peer group income and happiness. Also, alternative income measurements may be appropriate. Better data with a control variable for social inequality and more information about the family arrangement can shed more light on these issues.

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Appendix A

Variable definitions (GSS)

Variable	Definition
lnrealinc	Natural log of midpoint estimate of pre-tax household income, adjusted for inflation.
lnrealpeerinc	Natural log of estimated pre-tax household income based on Eq. (4.1), adjusted for inflation.
happy10	Rescaled happiness: 10=Very happy, 5=Pretty happy, 0=Not too happy.
female	Gender dummy. 1=female, 0=male
age	Respondent's age
health	Subjective health. Excellent=4, good=3, fair=2, poor=1.
year	Time variable, 1991-2018 (two year gaps, except 1993).
degree	Respondent's degree. 0=Less than high school, 1=high school, 2=bachelor, 3=postgraduate
religious	Respondent's religiosity. 0=nonbeliever, 1=believer.
marital	Respondent's marital status. 1=married, 2=widowed, 3=divorced, 4=separated, 5=never married.
wrkstat	Labor force status. 1=working full-time, 2=working part time, 3=temporarily not working, 4=unemployed, 5=retired, 6=school, 7=keeping house, 8=other

Appendix B

Variable definitions (ESS)

Variable	Definition
lnrealinc	Natural log of midpoint estimate of net household income, adjusted for inflation.
lnrealpeerinc	Natural log of estimated net household income based on Eq. (4.1), adjusted for inflation.
realinc	Midpoint estimate of net household income, adjusted for inflation.
realpeerinc	Estimated net household income based on Eq. (4.1), adjusted for inflation.
happy	Subjective happiness: 10=Extremely happy, …, 0=Extremely unhappy.
female	Gender dummy. 1=female, 0=male
age	Respondent's age
health	Subjective health. Very good=5, good=4, fair=3, bad=2, very bad=1.
essround	Time variable, 2010-2018 (two year gaps).
eisced	Respondent's education (DE). 0=Not possible to harmonise into ES-ISCED, 1=ES-ISCED I less than lower secondary, 2= ES-ISCED II lower secondary, 3=ES-ISCED IIIb lower tier upper secondary, 4=ES-ISCED IIIa, upper tier upper secondary, 5=ES-ISCED IV advanced vocational sub-degree, 6=ES-ISCED V1 lower tertiary education BA level, 7=ES-ISCED v2 higher tertiary education >=MA level, 55=other.
rlgdgr	Respondent's religiosity. 0=not religious at all, …, 10=very religious.
maritalb	Respondent's marital status. 1=married, 2=civil union, 3=legally separated, 4=legally

	divorced/union dissolved, 5=widowed, 6=none of the above.
mnactic	Main activity last 7 days (labor force status). 1=paid work, 2=education, 3=unemployed looking for a job, 4=unemployed not looking, 5=permanently sick or disabled, 6=retired, 7=community or military service, 8=housework looking after children (others), 9=other
edulvla	Respondent's education (UK). 1=Less than lower secondary education, 2= lower secondary education completed, 3=upper secondary education completed, 4=post-secondary non-tertiary education, 5=tertiary education completed, 55=other
cregions	DE2=Bayern, DE4=Brandenburg, DE6=Hamburg, DE8=Mecklenburg-Vorpommern, DEA=Nordrhein-Westfalen, DEC=Saarland, DEE=Sachsen-Anhalt, DEG=Thüringen, DE1=Baden-Württemberg, DE3=Berlin, DE5=Bremen,DE7=Hessen, DE9=Niedersachsen, DEB=Rheinland-Pfalz, DED=Sachsen, DEF=Schleswig-Holstein
wkhtot	Total hours normally worked per week in main job overtime included. Range [0,120]
hhmmb	Number of people living regularly in as member of household. Range [1,12].
blgetmg	Belong to minority ethnicity group in country. 1=yes, 2=no.