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A re-evaluation of the dynamic effects of (reference) income on life satisfaction with GSOEP and UKHLS data

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Abstract

Do people adapt to changes in income? This paper shows that there is no evidence of adaptation to income in GSOEP (1984-2015) and UKHLS (1996-2015) data. Following the empirical approach of Vendrik (2013), I arrive at this surprising answer by estimating (dynamic) life satisfaction equations, in which I simultaneously enter contemporaneous and lagged terms for a respondent's own household income and their estimated reference income. Additionally, I instrument for own income and include lags of a large set of controls. Furthermore, I find that people also do not adapt to changes in reference income. Instead, reference income effects may be subject to reinforcement over time. To explain my findings, a comprehensive account of the puzzling and often divergent results of Ferrer-i Carbonell and Van Praag (2008), Binder and Coad (2010), Di Tella et al. (2010), and Pfaff (2013) is given. What was found to be adaptation to raw household income in these studies turns out to have been driven by reinforcement of an initially small negative effect of household size that grows large over time. Implications of this result for the estimation of equivalence scales with subjective data are discussed.

Keywords: *subjective well-being, life satisfaction, adaptation, income comparisons, equivalence scales, GSOEP, BHPS, Understanding Society, UKHL*S
JEL codes: *D6, I31*

1 Introduction

When people earn more, they tend to become more satisfied with their lives. However, with time, people may grow accustomed to higher incomes. As a consequence, people's life satisfaction tends to decline. This phenomenon is commonly called adaptation. In the present study, I show that *no* significant adaptation to income can be observed in two of the data-sets (GSOEP and UKHLS[1]) that are most commonly used in the study of happiness.

With this paper I follow a large literature that evaluates whether people adapt to various life events more generally (for studies on adaptation in non-pecuniary domains, see e.g. Angeles, 2010; Clark et al., 2008; Hanglberger and Merz, 2011). There are some previous studies that investigate whether we adapt to income in life satisfaction (e.g. Di Tella et al., 2010; Di Tella and MacCulloch, 2010; Vendrik, 2013; Ferrer-i Carbonell and Van Praag, 2008; Wolbring et al., 2013). However, those studies do not form a consensus. Despite this lack of consensus, a number of authors, particularly those working in the capabilities approach, simply assume that adaptation to income in life satisfaction has been empirically established (Sen, 1985; Nussbaum, 2000; Qizilbash, 2006; Schokkaert, 2007; Jayawickreme and Pawelski, 2012; Graham and Nikolova, 2015) - a presumed fact which can then be used to undermine the normative significance that may be put upon measures of life satisfaction. Furthermore, adaptation has previously been put forward as a possible explanation for the well-known and much debated Easterlin Paradox (Clark et al., 2008; Clark, 2015; Easterlin, 2016). Thus, there is some importance to assessing whether adaptation to income in life satisfaction does in fact occur.

Before doing so, it is useful to clarify my terminology. By adaptation to income I mean that the absolute size of the long-run[2] effect of income on life satisfaction is smaller than the absolute size of the contemporaneous effect of income on life satisfaction. One may further distinguish between partial and full adaptation. Partial adaptation refers to a situation in which the absolute size of the long-run effect is indeed smaller, but where the long-run effect is nevertheless significant and of the same sign. In contrast, full adaptation occurs in situations where the contemporaneous effect is significant, but the long-run effect is zero, or insignificant. Following Vendrik (2013), I call the opposite of adaptation "reinforcement". Reinforcement occurs when long- and short-run effects have the same sign and when the absolute size of the long-run effect of income on life satisfaction is larger than the absolute size of the short-run effect[3]. An illustration of reinforcement, as well as full and partial adaptation is given in figure 1.

In almost all studies (except Vendrik, 2013), adaptation to income is exclusively taken to be adaptation to *own* income. However, this need not be so. Given that we know that the incomes of peers (henceforth, "reference incomes") tend to have negative contemporaneous effects on life satisfaction (e.g. McBride, 2001; Ferrer-i Carbonell, 2005; Vendrik and Woltjer, 2007), it may well be that such reference incomes could also be subject to either adaptation or reinforcement. If so, it then becomes useful to evaluate what the dynamic effect of joint income, i.e. the combined effect of own and reference income is. Doing so is particularly important, since only observing a nil or negative long-run effect of joint income would be compatible with the postulate of the Easterlin Paradox that the long-run time-series correlation of

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[1]Which is a combination of the older British Household Panel Study (BHPS) and its successor Understanding Society.

[2]I take the long-run to be a situation in which income and life satisfaction are unchanging and in equilibrium with respect to each other.

[3]There may be cases where the sign of the short-run effect is different from the long-run effect and where both effects are significant. I do not give a separate name to such cases.
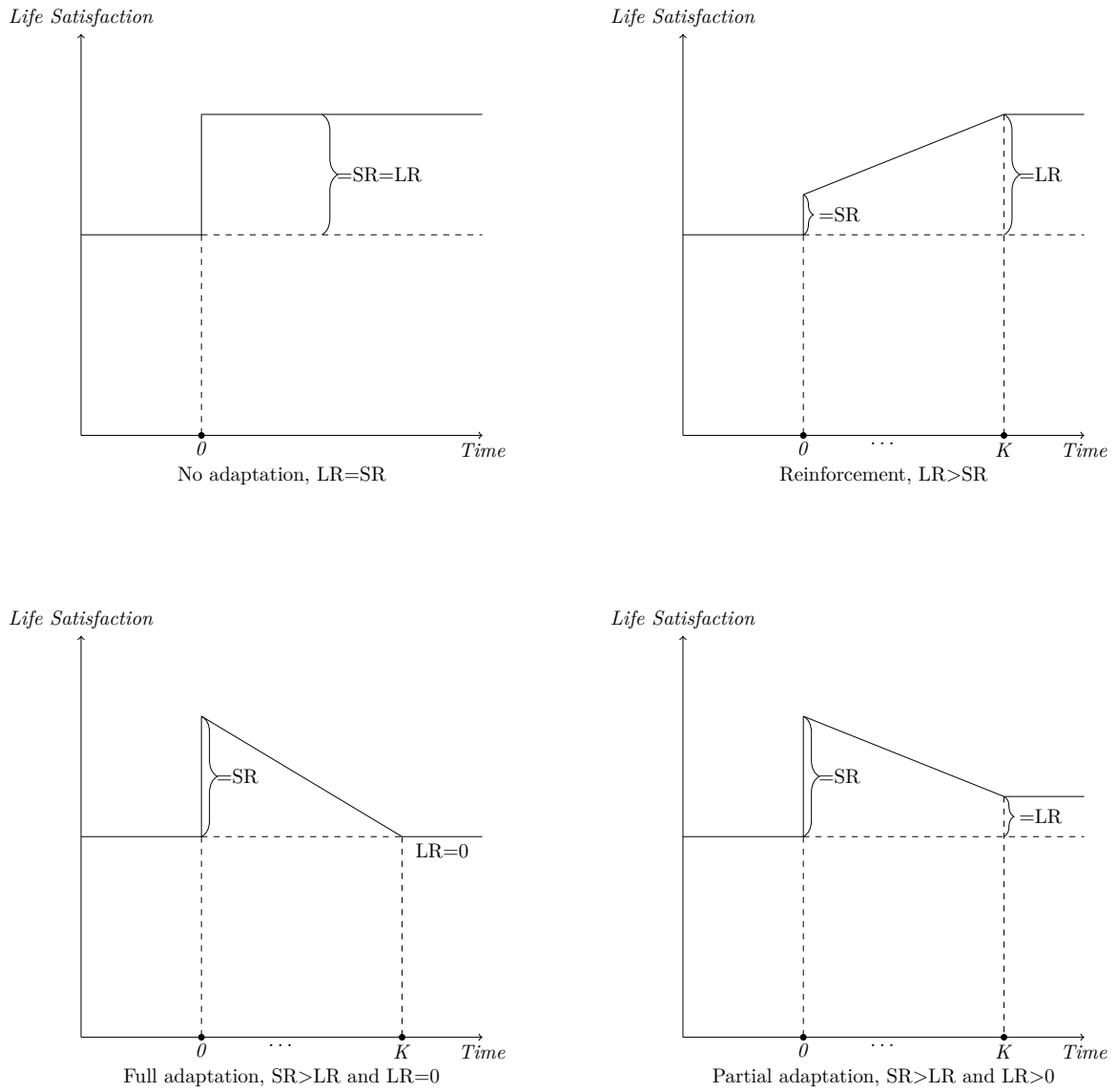
Figure 1: Adaptation types. In all cases a positive and persistent income shock occurs at time zero. I assume that higher incomes have a positive effect on life satisfaction and that the adaptation process ends after K periods.

GDP per capita and life satisfaction fails to be significantly positive.

The present paper builds on the empirical approach of Vendrik (2013), which appears to be the most robust study on adaptation to income to date. I give a much needed attempt at corroboration of that paper's main ideas with updated GSOEP data (with ten additional waves). This attempt at corroboration seems well justified, since it appears that the results of Vendrik (2013) suffer from a crucial omitted variable bias, namely his omission of partner employment status. With the addition of this variable, all evidence of adaptation that Vendrik (2013) obtains is eradicated.

Beyond that, as the first in the literature on adaptation to income, I explicitly compare estimates obtained from the two main surveys used in the literature on subjective well-being, i.e. the GSOEP and UKHLS. Given that it often seems in the literature as though results obtained from surveys that are representative of the population of one particular country are generalizable to *all* industrialized countries, it is especially important to investigate whether this implicit assumption does in fact hold. This is especially true today, when it has become apparent that many results in psychology and economics are not replicable (Earp and Trafimow, 2015; Chang and Li, 2018), while there is still a lack of attempts at replication (Hamermesh, 2007). Moreover, I give a comprehensive account of why one may observe significant adaptation with raw household income, while one finds much less or no adaptation when using equivalized household income.

My key substantive findings are summarized as follows: Firstly, no adaptation to income can be observed in any of the preferred specifications. This holds true independently of the country considered, whether reference or own income is analyzed, and whether an instrument for own income was used or not. Secondly, all previous evidence of adaptation was driven by omitted omitted variable bias; most crucially via the omission of lags of controls for household composition and partner employment status. Thirdly, reference income effects are large, negative, and not subjected to adaptation. As a consequence, my long-run estimates of joint-income, i.e. the combined effects of own and reference income are always insignificant and close to zero. These results imply that as everybody grows richer, nobody grows happier. This unfortunate finding is in line with the Easterlin paradox.

The next section provides a review of the literature. Section 3 outlines my empirical approach. Section 4 presents my data. In section 5, I present my results. In section 6 I make some concluding remarks. Additional robustness checks are available in the Appendix.

2   Literature Review

The earliest studies on adaptation to income were arguably those of the "Leiden School" by Van Praag and his collaborators (e.g. Van Praag, 1971). In the main, this series of studies found that responses to an "income evaluation"[4] question were an increasing function of the respondent's income. In other words, richer individuals would evaluate higher incomes as worse, thus giving indication of what was then termed a "preference drift" and which was interpreted as adaptation to income[5]. In the somewhat more modern literature, Clark (1999) is among the first to exploit panel data to assess adaptation to income. Using the first two waves of the BHPS, he estimates an ordered probit regression on job satisfaction in which

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[4]Which asks: *While keeping prices constant, what after-tax total monthly income would you consider for your family to be: "very bad", "bad" "sufficient", "insufficient", "good", "very good".*

[5]However an explanation in terms of reference incomes explains this result equally well and was pursued in e.g. Kapteyn et al. (1978)

Life Satisfaction

SR^{own}

LR^{own}

0 ... K Time

Long- and short-run effects of own income; partial adaptation

$+$

Life Satisfaction

SR^{ref}

LR^{ref}

0 ... K Time

Long- and short-run effects of reference income; reinforcement

$=$

Life Satisfaction

SR^{abs}

LR^{abs}

0 ... K Time
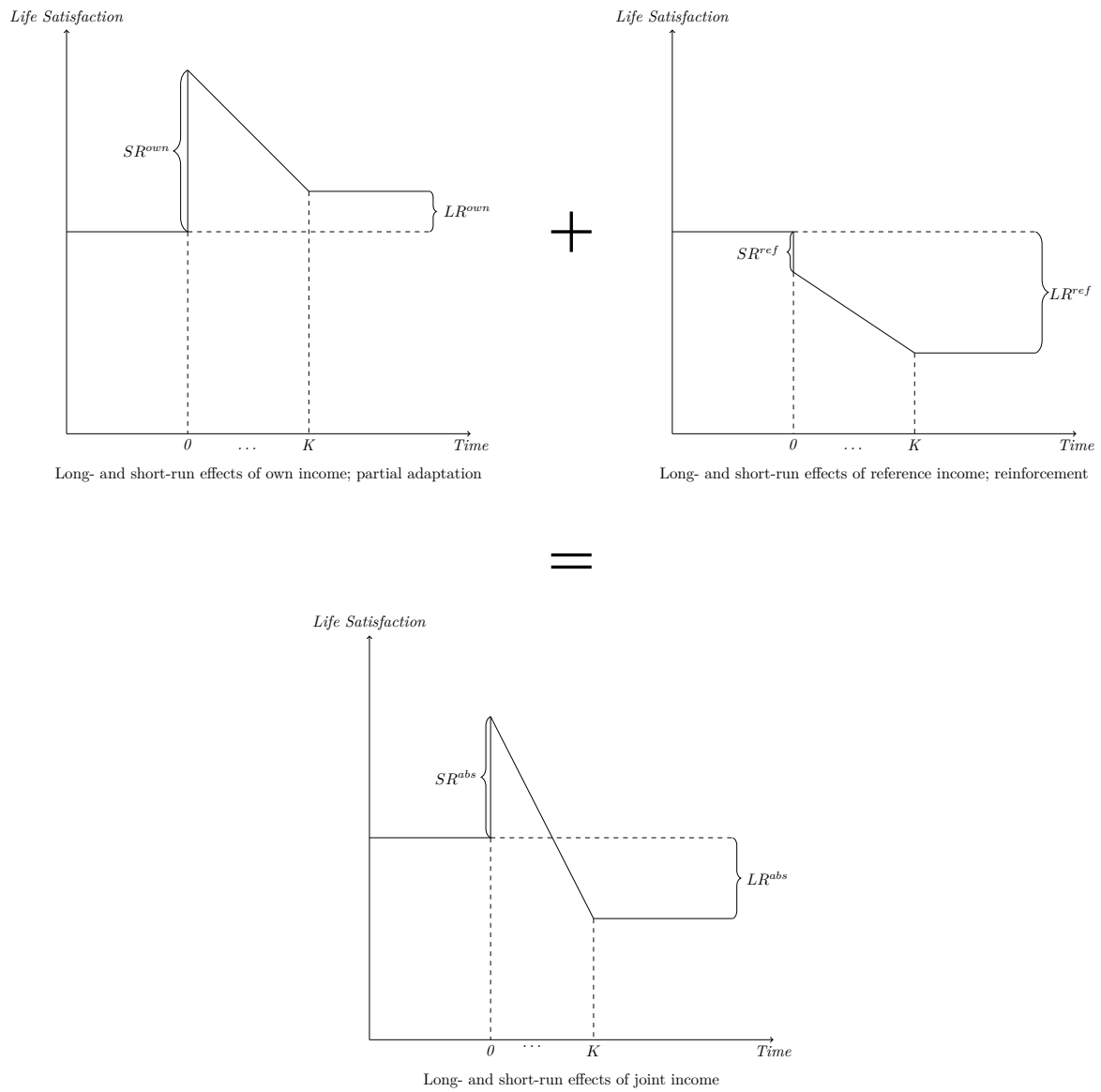
Long- and short-run effects of joint income

Figure 2: Illustration of how different adaptation/reinforcement patterns in own and reference income may yield a distinct dynamic pattern in joint income. I assume partial adaptation in own income and reinforcement in reference income. A positive short-run and a negative long-run joint income effect is the result.

both current and one-year lagged gross monthly income are entered. Both variables enter the regression significantly. Moreover, while the contemporaneous effect is positive, the lagged term is negative and of roughly equal absolute size. This implies full adaptation. Another study is that of Burchardt (2005) who uses all of the first ten waves of the BHPS, and regresses income satisfaction on current net household income and current change in net HH income. In the main she finds not evidence of adaptation, but of reinforcement instead.

The aforementioned studies are not considering adaptation in life satisfaction, but rather adaptation in particular domain satisfactions (income, job). In contrast, Stutzer (2004) appears to be one of the first studies to pose the question of adaptation to income in life satisfaction in particular. Using Swiss Household Panel (SHP) data he regresses life satisfaction on the ln of household income, as well as responses to either a question on sufficient or minimum acceptable income. These latter responses, interpreted as measures of income aspirations, have a significant negative effect that is only slightly smaller in absolute size than the coefficient for income. In a further step, he shows that income aspirations are an increasing function of own income. In combination, these two sets of results may be viewed as evidence of adaptation[6].

The most well known and widely cited[7] study on adaptation to income in life satisfaction is that of Di Tella et al. (2010). Because of that, I will take their approach as my baseline for the analysis to follow in section 5. Using data from GSOEP (1984-2000; restricted to West Germany), they regress life satisfaction on the contemporaneous level of (the ln of) real post-tax household income, as well as up to 4-year lags of this income measure. To test for adaptation, they perform tests on the significance of the sum of these lags as a well as the sum of lags and the contemporaneous coefficient. In their main analysis, they obtain a large positive contemporaneous effect (= 0.23), find clear evidence for adaptation ($\sum$ lags = 0.15), and cannot reject the hypothesis of full adaptation (see table 1, columns 2 and 5)[8]. When distinguishing between party-political lines, both groups benefit from higher incomes, but only leftists adapt. They further find no evidence of adaptation for the self-employed, but full adaptation for the employed. When distinguishing between men and women, this result only holds for females, finding no evidence of adaptation for men. In a follow-up, Di Tella and MacCulloch (2010) replicate these findings with up to 7 rather than 4 year lags and by further dividing the sample into homeowners and those that are renting. They only find evidence of adaptation for home owners, thus providing evidence that the degree to which individuals adapt to income levels may be moderated by their level of wealth.

Ferrer-i Carbonell and Van Praag (2008) follow a somewhat different strategy. Also using GSOEP data (2000-2004), they use first differences to account for individual fixed effects. They also use post-tax household income as their measure of income and include up to 4-year lags. They run four specifications, one without any controls, one with levels of a standard set of contemporaneous demographic controls, one using the first differences of all controls, and finally one with their lagged first differences. When using

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[6]Easterlin (2005) follows a similar strategy of trying to infer adaptation via observing aspirations. Easterlin uses nationally representative American data from 1978 and 1994 that asked respondents what sets of goods (and life circumstances) they deem desirable for a good life, and which of these goods they already possess. He shows that for each of the cohorts he constructs, both the number of desired and owned goods increase across the period by roughly the same amount. In so far as what people deem part of the "good life" (p.520), in fact contributes to life satisfaction, these results may - as Easterlin states - indicate complete adaptation. However, an alternative interpretation in terms of relative consumption against an external contemporaneous benchmark would also explain these results.

[7]Google Scholar citation count: 483 (31.10.2018)

[8]This is in contrast to their simultaneous results on social status for which they also find adaption, but can reject the hypothesis of full adaption (see their table 1, columns 4 and 5).

no controls they find adaptation of about 60%. However, when using contemporaneous terms of their controls, they obtain evidence of full adaptation. In contrast, when using all lags of the first-differences, observed adaptation is reduced to as little as 40%. Ferrer-i Carbonell and Van Praag do not offer an explanation for these strange results.

Pfaff (2013) makes use of both GSOEP (1992-2010, separating East- and West- Germany) and BHPS (1996-2008) data. His estimation strategy is in the main identical to that of Di Tella et al. (2010). He obtains effect sizes that are up to 5 times larger for Germany than for the UK, but does not comment on this puzzling result. Moreover, he experiments with different measures of income, namely raw real labor income, real net household income and equivalized real net household income. It turns out that he only finds evidence of (complete) adaptation when either using labor income or raw real net household income; but not when using equivalized household income. Like Ferrer-i Carbonell and Van Praag in the case of their peculiar results, Pfaff does not give an explanation for this strange result, but simply notes that equivalized incomes are to be preferred for what he views as theoretical reasons (see p.7). Binder and Coad (2010), who use a somewhat different estimation strategy, face the same puzzlement as Pfaff (2013) : when using raw household income they obtain evidence of adaptation, but when using equivalized household income they fail to do so.

In addition to the above, a small number of studies have found much weaker adaptation (Di Tella and MacCulloch, 2010) or no adaptation at all (Crettaz and Suter, 2013; Clark et al., 2016) when looking at those at the bottom of the income distribution in particular. Clark et al. (2016) use GSOEP data (1985-2012) and regresses life satisfaction on a series of dummies indicating the length of poverty spells (whilst controlling for individual fixed effects and a set of standard demographic controls). An individual is deemed to be in poverty in any given year if her annual equivalized household income falls below 60% of the country's median equivalent household income. They find that the effect of being in poverty does not decline with time, thus implying the absence of adaptation in life satisfaction to poverty. Crettaz and Suter (2013) using Swiss data, perform a similar analysis in that they regress life satisfaction on a continuous variable, indicating the number of years spent in poverty, while controlling for contemporaneous household income and demographic controls. Unlike Clark et al. (2016) they only exploit cross-sectional variation in life satisfaction, and do not use individual fixed effects. They find no effect of the length of the poverty spell on life satisfaction beyond that of current household income, thus also indicating complete absence of adaptation[9].

Finally, Frijters et al. (2008), using Australian panel data, finds partial adaptation in individuals who experienced a major improvement or worsening of their financial situation in the preceding 12 months. In contrast, Paul and Guilbert (2013), who make use of the same data, find no evidence of adaptation when using household income with up to four year lags. However, given that they - contrary to almost all other literature - also find no evidence of a significant contemporaneous effect, they interpret this finding to be most likely be driven by attenuation bias from measurement error in the income variable.

There is a large literature on reference incomes. However, almost all of them simply focus on the contemporaneous effects of reference incomes. Although there is a small literature that measures reference incomes directly via the subjective assessments of respondents (e.g. Senik, 2009; Mayraz et al., 2009; Goerke and Pannenberg, 2015; Dumludag et al., 2016; Clark et al., 2017), most studies compute reference

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[9]In this setup, a positive coefficient would have been indicative of adaptation. Their definition of poverty is the same as that of Clark et al. (2016)

incomes on the basis of cell means in income of a presumed reference group. Such groups may either be defined in spatial terms (e.g. Luttmer, 2005; Kingdon and Knight, 2007; Ifcher et al., 2016), or in terms of similar demographic characteristics, particularly age and education (e.g. McBride, 2001; Ferrer-i Carbonell, 2005; Vendrik and Woltjer, 2007; Vendrik, 2013). However, and with the exception of particularly narrow spatially defined reference groups (Ifcher et al., 2016), this choice seems not to matter, yielding negative effects for contemporaneous reference incomes that tend to roughly equal the contemporaneous effects of own income[10]. There are only very few studies on adaptation/reinforcement to reference incomes in life satisfaction. Exceptions are the studies of D'Ambrosio and Frick (2012), and Vendrik (2013). Unfortunately, D'Ambrosio and Frick (2012) only use a lag of one year for own income. Moreover, their specification for the (dynamic) effects of reference incomes is estimated with separate terms for the income gap of the respondent with respect to those that are richer or poorer than the respondent (the static part), and the income gap to those that became richer or poorer than the respondent (the dynamic part). This kind of formulation makes it very difficult to understand the dynamics and relative strengths of the (joint) long-run, short-run and adaptation effects of own and reference income.

Vendrik (2013) may be the most robust investigation on both adaptation to own as well as reference income. He also uses GSOEP data (1984-2007; restricted to West Germany). In the main, he combines the approaches of Ferrer-i Carbonell and Van Praag (2008) and Di Tella et al. (2010) and regresses life satisfaction on the current ln of equivalized real net household income and reference income as well as lags of both variables[11]. He also includes lags of all his control variables. Moreover, he includes a term for one-year lagged life satisfaction, yielding an error correction model that implicitly models effects of unincluded lags of income in the further past (also see section 3.2). In the main analyses he instruments income using the approach of Luechinger (2009) and Luttmer (2005). When simply using observed income, Vendrik is unable to obtain any evidence of adaptation to own income. However, in his (seemingly) more robust regressions where he instruments for own income he does obtain evidence of adaptation. With respect to reference income he finds an insignificant and negative contemporaneous effect of reference income that is reinforced over time to yield a significantly negative long-run effect of reference income that is larger than that of own income. Jointly considered, this hence yields a significant positive short-run effect of joint income, to which more than full adaptation occurs, such that he finds a negative but insignificant long-run joint income effect.

A number of points may be taken from this review of the literature. First and with respect to adaptation to reference and joint income, the only (to my knowledge) viable study is the one of Vendrik (2013). Second, whether we observe adaptation to own income seems to depend on whether we use equivalized or raw household income, and whether we instrument for own income. Only in the case of raw household income or instrumented income, do we observe adaptation (e.g. Pfaff (2013) and Binder and Coad (2010)). Third, we observe less adaptation to own income when including lagged terms of

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[10]With respect to particularly volatile economies, this negative effect may not hold and may instead be dominated by a positive "signaling" effect. See Senik (2004, 2008). Given that West Germany and UK are no such volatile countries in the estimation period, this should not be a major concern for me.

[11]He also initially includes a lead of income and controls in his regression. Also given that Vendrik (2013) finds no anticipation effect when instrumenting for own income, I omit this further extension in all estimations of the present paper. The null finding for anticipation to own income when using the income instrument (see below), suggests reverse causality for his estimates of the anticipation term when using observed income (as noted on p.138). If this is indeed the case, and since income can be assumed to be serially correlated, this inclusion of the leads of observed own income is an instance of "conditioning on a collider" (Elwert and Winship, 2014), which should lead to biased estimates of all other terms.

controls (Ferrer-i Carbonell and Van Praag, 2008; Vendrik, 2013). Fourth, individuals on very low incomes (Crettaz and Suter, 2013; Clark et al., 2016) or less wealthy individuals (Di Tella and MacCulloch, 2010) adapt less than richer ones[12]. This is arguably in line with the literature on basic needs that postulates a core minimum that individuals have to achieve (e.g. Doyal and Gough, 1991). Fifth, there seem to be almost no studies doing cross-country comparative work. The working paper of Pfaff (2013) is the only exception, but he does not comment on the comparative aspect. The present study attempts to address each of these five points.

3 Empirical strategy

3.1 The basic model

I start with a distributed lag model very similar to that of Di Tella et al. (2010), with the main difference being my inclusion of reference incomes:

$$LS_{i,t} = \sum_{k=0}^{K} \beta_{0-k} \ln(y_{i,t-k}) + \sum_{k=0}^{K} \gamma_{0-k} \ln(y_{i,t-k}^{ref}) + \phi X_{i,t} + \mu_i + \tau_t + \epsilon_{i,t} \tag{1}$$

Here $LS_{i,t}$ is the (reported) life satisfaction of individual i at time t, $y_{i,t}$ is a measure of own household (HH) income, $y_{i,t}^{ref}$ is a measure of reference income, μ_i is an individual fixed effect, τ_t is a year fixed effect and $e_{i,t}$ is assumed to be a normally distributed error term. $X_{i,t}$ is a vector of time-varying controls thought to be correlated with both income and life satisfaction. These include: dummies for numbers of children and adults in the household, employment status of respondent, employment status of respondent's partner, dummy for child birth, age, age-squared, marital status, housing tenure, job hours, dummy for illness, region dummies, and a region-specific linear time trend. These controls are similar to those chosen by Burchardt (2005), Di Tella et al. (2010) and Vendrik (2013). A precise description of each of these variables is given in the next section and table A1 of the Appendix. I define reference incomes by the mean income of the set of individuals up to 5 years older or younger than the respondent and that have the same educational attainment as the respondent (as measured by a collapsed version of the CASMIN classification, see table A1).

In this set-up, the contemporaneous, or short-run (SR), effect of own income on life satisfaction is given by β_0. In general the long-run (LR) effect is given by $\sum_{k=0}^{\infty} \beta_{0-k}$, but I assume $\beta_{0-k} = 0 \,\forall k > K$ in the special case of equation (1). Thus in equation (1), the LR effect is given by $\sum_{k=0}^{K} \beta_{0-k}$. Finally, $\sum_{k=1}^{\infty} \beta_{0-k}$ gives the total effect of adaptation[13], i.e. the degree to which the effect of income dissipates with time. We may also generally say that adaptation is given by the difference between the long-run and the contemporaneous effect (i.e. $ADAP = LR - SR$). All of this is illustrated in figure 3[14]. Of

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[12]In a related vein, Peng (2017) finds that the effects of reference incomes are moderated by individual's level of income.

[13] Or $\sum_{k=1}^{K} \beta_{0-k}$ in the case of (1))

[14]The study of Wolbring et al. (2013), where they also attempt to study adaptation to income in life satisfaction, pursues an altogether different approach. They exclusively use lagged first differences (up to three) and no term for the level of income, i.e $LS_{i,t} = \gamma_o \Delta \ln(y_t) + \gamma_1 \Delta \ln(y_{t-1}) + \gamma_2 \Delta \ln(y_{t-2}) + \phi X_{i,t} + \mu_i + \epsilon_{i,t}$. They state that if the effect of the first differences declines as one moves to higher order lags (i.e. if $\gamma_0 > \gamma_1 > \gamma_2$), there is evidence of adaptation. This is indeed what they find. However, such an interpretation is not warranted. All this would imply is that more recent *changes* in income have a stronger effect on life satisfaction. In fact, their model presumes that full adaptation will occur. This can be see by rewriting the model as $LS_{i,t} = \gamma_o \ln(y_{i,t}) + (\gamma_1 - \gamma_0) \ln(y_{i,t}) + (\gamma_2 - \gamma_1) \ln(y_{i,t-2}) + (-\gamma_2) \ln(y_{i,t-3}) + \dots$. Applying the formula above gives $LR = 0$ (all terms cancel), $SR_0 = \gamma_o$, and $ADAP = -\gamma_o$.
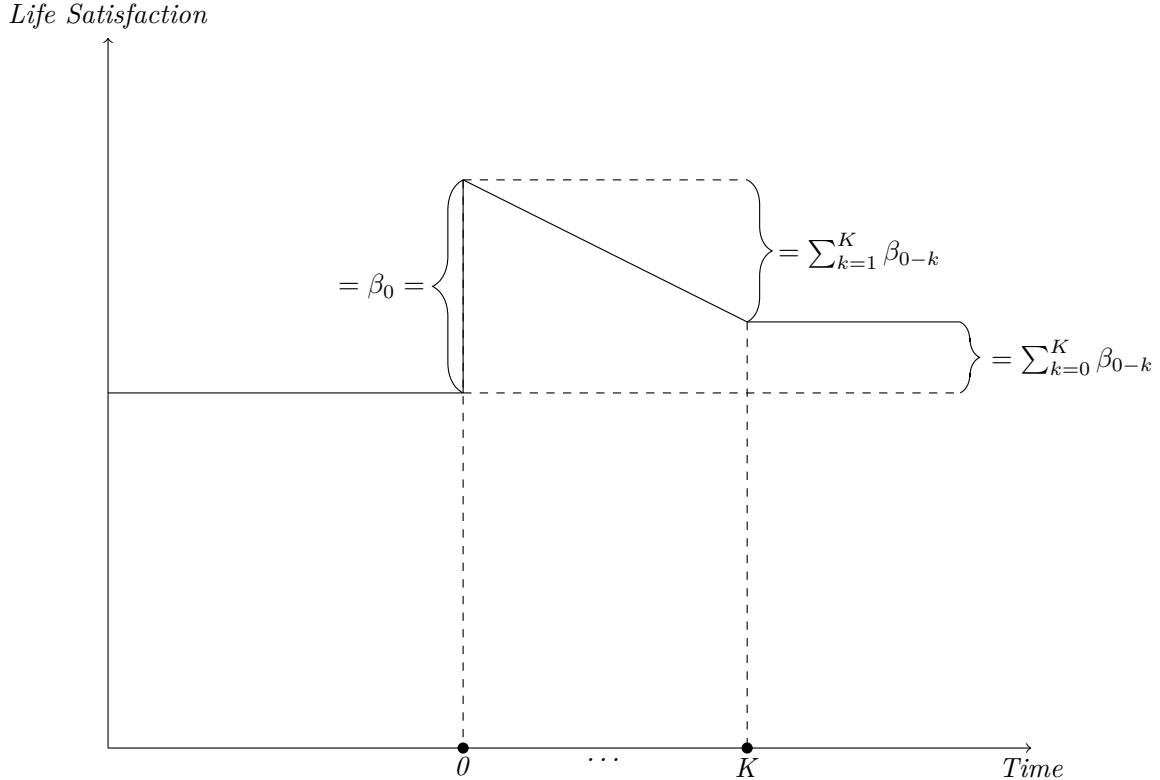
Figure 3: Illustration of the process modeled in (1) and (2). A positive and persistent income shock in own income is experienced at time zero. I assume partial adaptation.

course, the same reasoning applies to the estimates of the SR, LR, and adaptation effects of reference incomes. When looking for the respective joint effects, I may just take the sums of the effects of own and reference income.

The review in section 2 has made clear that controlling for (at least some) lagged terms of controls can drastically change results. The reason for this may be as follows: controlling for X_{it} stems from the belief that X_{it} is correlated with y_{it}^{own} and LS_{it}. If this is so, we must also also believe that $X_{i,t-k}$ is correlated with $y_{i,t-k}^{own}$. Given the literature on adaptation to various life events (Angeles, 2010; Clark et al., 2008; Hanglberger and Merz, 2011), we may have further reason to believe that at least some variables in X_{it-k} will also be correlated with LS_{it}[15]. If this is so, my estimates of the long-run, short-run and adaptation effects would have to be biased. In particular, any observed adaptation to income may be driven by unobserved adaptation to changes in any of the covariates in X. Hence I should further extend the model by adding lags of all control variables that are not perfectly collinear with its contemporaneous values[16]. This is similar to the approach of Ferrer-i Carbonell and Van Praag (2008) and Vendrik (2013). The model thus becomes:

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[15]It may further be that elements in an appropriately constructed vector X_{it}^{ref}, e.g. levels of unemployment in the reference group, would behave similarly. I omit this complication from the present investigation.

[16]This turns out to make a large difference to my results. The aforementioned studies investigating adaptation to non-monetary life events may therefore also be affected by this problem.

$$LS_{i,t} = \sum_{k=0}^{K} \beta_{0-k} \ln(y_{i,t-k}) + \sum_{k=0}^{K} \gamma_{0-k} \ln(y_{i,t-k}^{ref}) + \sum_{k=0}^{K} \phi_{0-k} X_{i,t-k} + \mu_i + \tau_t + \epsilon_{i,t} \qquad (2)$$

When attempting to compare effects obtained from UKHLS and GSOEP, I restrict the observation window to 1996-2015 for both data-sets to account for the limited coverage of the UKHLS, use the same income concept (at baseline: real net household income)[17], and use comparable controls (see Appendix table A1). I treat life satisfaction as cardinal rather than ordinal. Ferrer-i Carbonell and Frijters (2004), Van Praag and Ferrer-i Carbonell (2008) or Riedl and Geishecker (2014) showed that treating income as cardinal rather than ordinal makes very little difference to ratios of coefficients and levels of significance.

3.2 Two extensions

Though more robust than the empirical strategy pursued in most other studies (in particular by virtue of the uncommon use of terms for the lags of controls), there are four further objections to this approach that I try to alleviate. First, income is measured with error. Such error may be driven by individuals not being able to accurately report on their income, the need to impute income in some cases, or inaccuracies in the tax-simulation routines used to arrive at net incomes. Such measurement error may lead to attenuation bias towards zero of the estimated own income effects. Second, there may unobserved costs to income generation, such as stress of higher commuting times, which may present a downward bias to the estimated own income effects. Third, there may be effects of covariates lagged further than K years, omission of which will yield biased estimates of all included terms. Fourth, life satisfaction may be an autoregressive process that operates over and above observables. In other words it may be that life satisfaction at t is partially caused by life satisfaction at $t-1$, where the causation is not mediated by any further variables. I now discuss my remedies for these issues.

3.2.1 Instrumenting

To alleviate the first and second concern, I instrument income following the approach first pursued by Luttmer (2005) and later adopted by Luechinger (2009) and Vendrik (2013). This approach goes as follows: For each wave, I predict real individual labor earnings using the respondent's age, age-squared, gender and a large set of occupation and industry dummies[18]. Thus for each wave, I run a regression of the form $y_i^{labour} = \alpha + \beta_1 MALE + \beta_2, AGE + \beta_3 AGE^2 + \delta_{ind} INDUSTRY_i + \phi_{occ} OCCUPATION_i + \epsilon_i.$, where δ_{ind} and ϕ_{occ} are industry- and occupation-specific parameters. In the German case, I use 4-digit ISCO88 codes for occupation, and 2-digit NACE codes for industry. In the UK case, I use 3-digit SOC codes for occupation, and 2-digit SIC codes for industry. In the next step, I sum the predicted earnings of each individual in the household. I then equivalize this measure for household size and use it to instrument for observed equivalized household income.

There are two interrelated threats to the exclusion restriction of this instrument. The first concern is that there may be other time-varying effects on life satisfaction of industry or occupation beyond those

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[17]I choose not to convert Pounds into Euros. Under the assumption that the separate series for inflation used to obtain a household income measure in constant prices in each country are comparable, my use of lns for all income terms in all estimations, as well as my use of year fixed effects, renders such a conversion unnecessary.

[18]For robustness, I also ran regressions with alternative specifications in which I used a separate dummy for each industry and occupation combination in each wave, and/or where I removed age and/or sex from the regressions. Each of these alternative gave similar results and are available upon request.

of time-varying pecuniary returns. On the one hand, it may be that individuals in the same occupation and industry may form part of the individual's reference group. Although I control for reference incomes defined over education and age, there may thus nevertheless be an uncontrolled-for negative effect that would downwardly bias the IV estimate. On the other hand, it may be that general work conditions in some industries have gotten better, while pay has increased too. This would lead to an upward bias in the IV estimate.

The second concern is that respondents may choose to select into particular industries or occupations in anticipation of changes in income or other benefits that may impact life satisfaction. Pischke and Schwandt (2012) note that industry choice is indeed correlated with fixed characteristics of individuals. Given my use of the ln of income and my use of individual fixed-effects, selection into industries or occupations that is driven by the overall mean of income (or the mean of other benefits) across the observation window is not a concern. However, when people actively switch industries or occupations in anticipation of *changes* in industry- or occupation-specific pecuniary or non-pecuniary returns, and when the propensity to switch is correlated with unobservables, my estimates may still be biased.

I partially guard against these concerns by later adding contemporaneous and lagged values of all industry and occupation dummies to the model. The inclusion of these dummies then controls for all effects of industry and occupation that do not run via individual labor incomes. In that case, bias may only occur when individuals actively switch industries and occupations in anticipation of hedonic returns (an upward bias), or via negative reference income effects that are captured by my generated income variable (a downward bias).

3.2.2 Addition of lagged life satisfaction

To robustify my main estimates against the third and fourth concern, I, add a term for lagged life satisfaction to the model. This yields an autoregressive distributed lag model of the following form :

$$LS_{i,t} = \sum_{k=0}^{K} \beta_{0-k} \ln(y_{i,t-k}) + \sum_{k=0}^{K} \gamma_{0-k} \ln(y_{i,t-k}^{ref}) + \sum_{k=0}^{K} \phi_{0-k} X_{i,t-k} + \rho LS_{i,t-1} + \mu_i + \tau_t + \epsilon_{i,t} \quad (3)$$

Assuming that ρ is positive and smaller than 1, this alleviates the same worries Vendrik (2013) seeks to solve with his error-correction model, but is a simpler and more intuitive representation[19]. By repeated substitution, equation (3) can be rewritten as follows (for brevity I omit terms for reference income, controls and errors):

$$LS_{i,t} = \beta_0 \ln(y_{i,t}) + (\beta_{-1} + \rho \beta_o) \ln(y_{i,t-1}) + (\beta_{-2} + \rho \beta_{-1} + \rho^2 \beta_0) \ln(y_{i,t-2}) + \ldots \quad (4)$$

$$+ (\beta_{-4} + \rho \beta_{-3} + \cdots + \rho^K \beta_o) \ln(y_{i,t-K}) + \sum_{m=1}^{\infty} \rho^m (\sum_{l=0}^{K} \rho^l \beta_{-K-l}) \ln(y_{i,t-K-m}) + \ldots$$

Defining β_{0-k}^*, γ_{0-k}^*, and ϕ_{0-k}^* appropriately this can in turn be rewritten as:

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[19]Indeed, the model of equation 3 could be rewritten to yield an error-correction model.

$$LS_{i,t} = \beta_0 \ln(y_{i,t}) + \sum_{k=1}^{\infty} \beta_{0-k}^* \ln(y_{i,t-k}) + \gamma_o \ln(y_{i,t}^{ref}) + \sum_{k=0}^{\infty} \gamma_{0-k}^* \ln(y_{i,t-k}^{ref}) + \tag{5}$$

$$\phi_{0-k} X_{i,t} + \sum_{k=0}^{\infty} \phi_{0-k}^* X_{i,t-k} + \mu_i^* + \tau_t^* + \epsilon_{i,t}^*$$

I thus implicitly model the effects of indefinitely long lags of own and reference income, as well as all controls. There are two equivalent sets of assumptions: Life satisfaction, beyond the part that is explicitly modeled, is assumed to be an autoregressive process of order 1, with $0 \le \rho < 1$. Equivalently, beyond those that are explicitly modeled, I assume the effects of income and all controls at $t - K - m$ to exponentially decay at rate ρ^m for $m > 0$. This homogeneous rate of decay is also assumed for the effects of the unobserved time-varying controls at $t - K - m$ for $m > 0$. The latter is a somewhat stringent assumption.

To obtain the SR_k, LR, and adaptation effects, the same formulas as those presented in section 3.1 can be applied to equation (5). The sum $\beta_0 + \sum_{k=1}^{\infty} \beta_{0-k}^*$, i.e. the long-run effect of own income, can be expressed as $\frac{\sum_{k=0}^{K} \beta_{0-k}}{1-\rho}$ (the same goes for reference incomes)[20]. Unfortunately, in a fixed effects setting, the OLS estimate of ρ will be downwardly biased for finite T. I therefore apply the bootstrap bias correction of De Vos et al. (2015) to guard against this concern.

Unfortunately, Vendrik (2013) finds a negative small-T bias in the estimate of ρ, which he claims to go beyond the Nickell Bias. This is why he fixates ρ to an estimate he obtains from a balanced panel with $T = 21$. I face the same problem: When using my full German sample I obtain an estimate of $\rho = 0.13$. However, when only selecting respondents who are observed in every wave (which thus yields an effective T of 29), my estimate of ρ increases to 0.26, which is very close to Vendrik's estimate of 0.27 (see his discussion on page 146). In turn, when selecting the same subset of respondents, but randomly selecting an observation window of seven periods (which is closest to mean T when using the full set of respondents) for each of these respondents, I get an estimate of $\rho = 14$. This is thus very close to the estimate obtained from the full set of respondents. I therefore conclude that the smaller estimate of ρ from the full sample indeed arises from a further, unaccounted-for small-T bias[21], and that the larger estimate for ρ I obtain when $T = 28$ is less biased and not driven by selection effects that could have arisen from either the time periods considered or the set of respondents included. Throughout I therefore fixate ρ to the estimate which I obtain with largest possible T.

3.3 Determining the number of lags

A tricky question is what the appropriate number of lags K is. On the one hand, one would wish to include as many lags as possible, in order to make the assumptions made for equations (2) and (3) as plausible as possible. On the other hand, adding more lags is problematic for two reasons: First the addition of each lag causes the loss of a large number of panel observations. Second, levels of income at t and $t-1$ are highly collinear, thus also leading to a loss in efficiency of the estimates. Vendrik determines the number of lags by first starting with some very large number of lags. He then conducts F- and t-tests

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[20]To see this, set LS and $\ln(y)$ to their unchanging long-run values in equation (3) and rearrange.

[21]This bias also does not appear due to the my use of the particular bias correction of De Vos et al. (2015). When instead using the Arellano-Bond estimator or the bias correction of Bruno (2005) I get a very similar results.
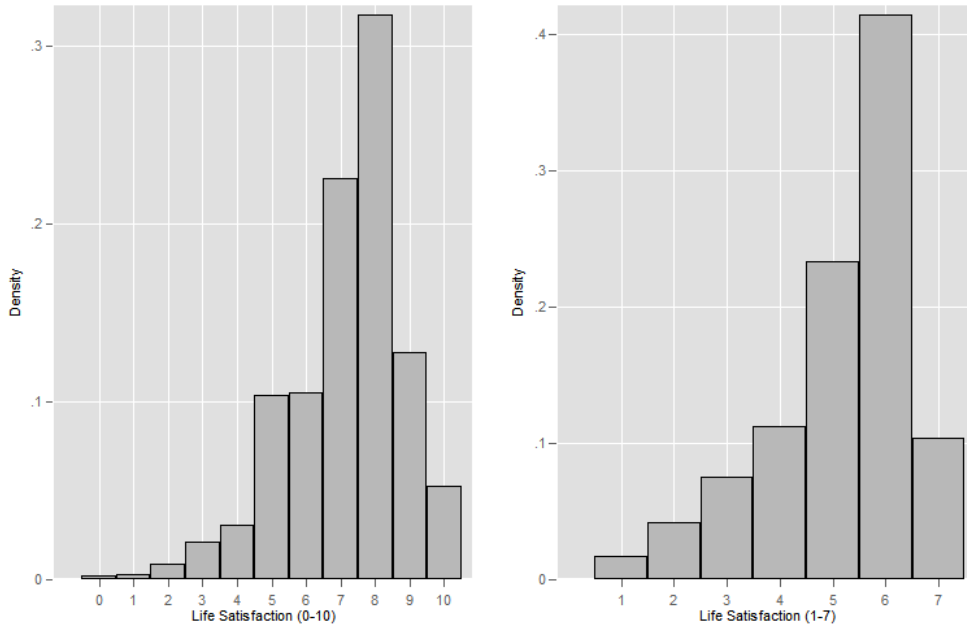
Figure 4: Distribution of life satisfaction in Germany (left) and the UK (right).

of the (joint) significance the highest lag of own and reference income. If all tests fail to reject the null hypothesis of (joint) equality to zero, he removes all highest order lags and repeats the process until he obtains at least one rejection. At baseline, this leads him to use a maximum lag length of 3, while when including lagged life satisfaction and instrumenting for income, he finds an appropriate lag length of 1.

When I apply this approach (using either equation (2) or (3) and instrumenting income) I obtain a lag length of 1 for both the UK and Germany. Given that I have no need to use such a short lag length, I decided to be more conservative and chose a lag-length of $K = 3$ for my baseline estimates. This choice is primarily motivated by the observation of this being the maximum lag length at which I continue to obtain a sufficiently large Kleibergen-Paap F-statistic (>10) to get reliable IV estimates for almost all subgroups that I consider. This choice of $K = 3$ is much closer to the choice of Di Tella et al. (2010), Pfaff (2013) and Ferrer-i Carbonell and Van Praag (2008) and many others in the literature who use a lag length of four. However, as it turns out, these considerations turn out not to be very important, since my IV results on the full samples are largely robust to lag lengths between 1 and 5 for both the UK and Germany. At higher order lags my instruments become weak in the UK and the reference income effect is estimated rather imprecisely. See tables A4 and A5 in the Appendix for the relevant regressions.

4   Data

At baseline, I make use of all available GSOEP waves (1-32), spanning the time 1984-2015. For comparative purposes, I also use all available waves of the UKHLS, which combines waves 6-18[22] of the BHPS

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[22]I exclude waves 1-5, as life satisfaction is not observed for these waves. Life satisfaction is also not observed in wave 11. Hence, this wave is excluded, too.

and waves 1-7 of Understanding Society. This translates to the years 1996-2015. Previous work has indicated that incomes at the very top and the very bottom of the income distribution suffer from large measurement error (Berthoud and Bryan, 2011; Figari, 2012). Moreover, self-employed individuals are known to either under- or over-report their incomes (Hurst et al., 2013). For these reasons, I exclude the top and bottom 1% within each wave of household income, as well as all self-employed individuals from the analysis. Since very few individuals younger than 20 years or older than 70 years are observed to have labor earnings (which is needed to instrument household income), I also exclude all individuals that fall outside of this range.

Table 1: Descriptive Statistics

	N	Mean	Median	SD	Min	Max
Germany(1984-2015)						
Life satisfaction	152457	7.175	8.000	1.636	0.000	10.000
HH income (2005 €)	152457	39559	36709	16269	5797	125339
Eq. HH income (2005 €)	152457	21340	19581	8714	2731	115780
Ln of HH income (2005 €)	152457	10.503	10.511	0.414	8.665	11.739
Ln of eq.HH income (2005 €)	152457	9.895	9.882	0.380	7.913	11.659
Ln of ref.eq.HH income (2005 €)	152457	9.848	9.815	0.191	9.217	10.403
Germany (1996-2015)						
Life satisfaction	111427	7.161	7.000	1.616	0.000	10.000
HH income (2005 €)	111427	40611	37629	17019	5921	125339
Eq. HH income (2005 €)	111427	22209	20319	9159	3152	115780
Ln of HH income (2005 €)	111427	10.525	10.536	0.425	8.686	11.739
Ln of eq.HH income (2005 €)	111427	9.933	9.919	0.386	8.056	11.659
Ln of ref.eq.HH income (2005 €)	111427	9.879	9.854	0.199	9.217	10.403
UK (1996-2015)						
Life satisfaction	97365	5.175	5.000	1.301	1.000	7.000
HH income (2005 €)	97365	29229	27060	14306	1349	113796
Eq. HH income (2005 €)	97365	16247	14954	7540	910	98944
Ln of HH income (2005 €)	97365	10.158	10.206	0.522	7.207	11.642
Ln of eq.HH income (2005 €)	97365	9.593	9.613	0.463	6.814	11.502
Ln of ref.eq.HH income (2005 €)	97365	9.634	9.639	0.222	8.656	10.065

In the UKHLS, Life Satisfaction is measured on a 1-7 scale, with respondents first being presented the statement *"Here are some questions about how you feel about your life. Please tick the number which you feel best describes how dissatisfied or satisfied you are with the following aspects of your current situation. 1 = Not satisfied at all 7 = Completely satisfied".* This is followed by a set of questions relating to various life domains. After these, respondents are presented with a separate question reading: *"Using the same scale, how dissatisfied or satisfied are you with your life overall?".* Within the GSOEP, life satisfaction is measured on a 0-10 scale, with respondents answering the question *"In conclusion we would like to ask you about your satisfaction with your life in general, please answer according to the following scale: 0 means completely dissatisfied and 10 means completely satisfied: How satisfied are you with your life, all things considered?".* As can be discerned from figure 4, the distribution of responses to these questions is also very similar across countries. Regarding income, in both cases I first use annual post-government (net of taxes and benefits) household income. The relevant income variable is provided by Levy and Jenkins (2012) for the BHPS, while it is directly provided in Understanding Society and the German SOEP. I deflate both income variables using the consumer price index (base=2005) and use the modified OECD scale to equivalize household incomes. This scale assigns a weight of 1 to the first member of the

household, a weight of 0.5 to every subsequent adult, and a weight of 0.3 for every child in the household. Definitions for all control variables can be found in Appendix table A1.

Table 1 provides summary statistics for my main variables of interest - household income and life satisfaction. The overall standard deviation of life satisfaction is roughly 1.62 in the German sample, whereas for the British sample it is roughly 1.30. Thus, the ratio of standard deviations $(1.62/1.30 \approx 1.25)$ is reasonably similar to the ratios of the response scales $(11/7 \approx 1.57)$. This gives further assurance for my comparative purposes[23].

5 Results

5.1 Baseline results for Germany

In this section, I present my results for my baseline estimations of variants of equations (1) and (2) using GSOEP data for the period 1984-2015. In section 5.2, I give results for equations (2) and (3) using instrumented income. Thereafter, in section 5.3, I then estimate similar models for both the UK and Germany on a more restricted time period. In section 5.4, I give results for different sub-groups. When I say that an effect or coefficient is *significant*, I mean that I obtain a p-value below 0.05. When I say *marginally significant*, I mean $0.05 \leq p < 0.1$. When I say *marginally insignificant*, I mean $0.2 > p \geq 0.1$. When I say *insignificant*, I mean $p \geq 0.2$.

Table 2 gives results for variants of equations (1) and (2). The estimates of column 1 are closest to those of Di Tella et al. (2010) in that real net household income with contemporaneous controls only are used (i.e. equation (1) is estimated). Like Di Tella et al. (2010), I find significant adaptation to own income that is roughly equal in magnitude to that which they find (-0.120 vs. -0.150; see their table 1 column 2). The contemporaneous effect that I obtain is similar to theirs (0.305 *vs.* 0.230). However, especially given my much larger samples (I have 15 extra years available), I obtain a significant long-run effect, implying partial rather than full adaptation to own income. Concerning reference incomes, I find an insignificant and negative short-run effect[24]. However, the long-run effect is statistically significant, negative and sizable (=-0.262).

In column 2, I again use raw household income to estimate equation (2), which now includes lags of all controls. This extension slightly diminishes the short-run effect of own income. More importantly, however, the long-run effect of own income is now slightly larger than the short-run effect. Thus, all evidence of adaptation observed in column 1 vanishes in column 2. Difference of the same trend were also seen by Ferrer-i Carbonell and Van Praag (2008) and Vendrik (2013)[25]. As explained in section 3.1, this suggests that significant adaptation observed in column 1 is the result of omitted variable bias, i.e. adaptation to one of the control variables which are correlated with income. With respect to the effects

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[23]We need not expect these ratios to equal: It may well be that there is less variance in life satisfaction in Germany than in the UK.

[24]In all regressions I use equivalized reference incomes. When using reference incomes that are not equivalized I find a positive short-run effect and a zero long-run effect. This is in line with the results of Pfaff (2013) in his table B.2, columns 1 and 2. A possible explanation for this is that the effects of household composition of those in the reference group follow a similar dynamic as the effects of the respondent's own household composition (see main text below): It may be that reference incomes computed on the basis of raw household income pick up (possibly age-specific) variation in the household compositions of the reference group. If so, a positive contemporaneous reference income effect might really just reflect that many in the reference group e.g. marry, which might - if the respondent anticipates to marry soon, too - in turn provide a positive signal to the respondent.

[25]Vendrik (2013) also suggests that there may be adaptation in household composition, a point I develop below.

Table 2: Baseline results for equation (2), West Germany (1984-2015)

	(1)	(2)	(3)	(4)
	HH income, contemp. controls	HH income, lagged controls	Eq.HH income, contemp. controls	Eq.HH income, lagged controls
SR effect of own income	0.305***	0.257***	0.274***	0.254***
	(0.023)	(0.023)	(0.023)	(0.023)
LR effect of own income	0.186***	0.277***	0.271***	0.280***
	(0.032)	(0.037)	(0.035)	(0.037)
Adaptation to own income	-0.120***	0.020	-0.003	0.026
	(0.025)	(0.032)	(0.030)	(0.032)
SR effect of reference income	-0.136	-0.132	-0.166	-0.134
	(0.132)	(0.139)	(0.131)	(0.139)
LR effect of reference income	-0.262**	-0.268*	-0.316**	-0.273**
	(0.126)	(0.137)	(0.126)	(0.137)
Adaptation to reference income	-0.126	-0.136	-0.150	-0.139
	(0.127)	(0.161)	(0.127)	(0.161)
SR effect of joint income	0.169	0.125	0.108	0.120
	(0.132)	(0.140)	(0.132)	(0.140)
LR effect of joint income	-0.076	0.009	-0.044	0.007
	(0.125)	(0.139)	(0.126)	(0.138)
Adaptation to joint income	-0.246*	-0.116	-0.152	-0.113
	(0.128)	(0.163)	(0.128)	(0.163)
Serial Correlation	0.038	0.035	0.039	0.035
Average T	7.830	7.830	7.830	7.830
Number of Persons	19472	19472	19472	19472
Number of Observations	152457	152457	152457	152457

Estimated with the 'reghdfe' command of Correia (2018). Individual and year fixed-effects & (lagged) controls included.

Clustered standard errors in parentheses. + p<0.2, * p<0.10, ** p<0.050, *** p<0.010

of reference incomes, the addition of lagged controls has virtually no effect.

In columns 3 and 4, I perform the same estimations as in column 1 and 2, but use equivalized household income instead. For both columns, my results are very similar to the results of column 2. My estimates of the short-run and long-run effects of own income are roughly equal and hence give no evidence of either adaptation or reinforcement, independent of whether the lags of controls are included or not. My estimates for reference income are again not qualitatively changed in these regressions.

Recall that the joint income effects are simply the sums of the own and reference income effects. These coefficients may be interpreted as the marginal effects of equally sized and permanent income changes of both the respondent and all members of the respondent's reference group. These estimates therefore give an answer to the question of whether everybody gets happier when everybody gets richer. The answer is no: I generally find positive but insignificant short-run effect, and insignificant (and more negative) long-run effects. All adaptation to joint income is insignificant.

That significant adaptation to own income is observed when using non-equivalized household income and no adaptation is observed when using equivalized household income (and including contemporaneous controls in both cases), is precisely the result that puzzled Pfaff (2013). It may seem odd that the results for non-equivalized household income are very strongly dependent on the inclusion of the lags of controls, while those for equivalized household income are not. However, equivalizing (the lags of) household income implicitly entails controlling for terms for the lags of the variable that determines the equivalization scale, i.e. the numbers of adults and children in the household. This can be seen as follows: Let $y_{i,t}^{eq}$ be equivalized HH income and $y_{i,t}^{defl}$ be non-equalized HH income. The equivalization scale is given by $EQ_{i,t} \equiv (1 + (nadults_{i,t} - 1)) * 0.5 + nchildren_{i,t} * 0.3$. Hence: $y_{i,t}^{eq} \equiv \frac{y_{i,t}^{defl}}{EQ_{i,t}}$. We can then write the model in, e.g., (2), using equivalized income, as:

$$LS_{i,t} = \sum_{k=0}^{3} \beta_{0-k} [\ln(\frac{y_{i,t-k}^{defl}}{EQ_{i,t-k}})] + ... \tag{6}$$

.

We can rewrite this to give:

$$LS_{i,t} = \sum_{k=0}^{3} \beta_{0-k}^{inc} \ln(y_{i,t-k}^{defl}) + \sum_{k=0}^{3} \beta_{0-k}^{comp} \ln(EQ_{i,t-k}) + ... \tag{7}$$

with (by assumption) $\beta_{0-k}^{inc} = -\beta_{0-k}^{comp}$. Hence, each income related coefficient in the models using equivalized income implicitly includes controls for the ln of the numbers adults and children. The fact that results in columns 3 and 4 vary only very little therefore means that the omitted variables which drove the divergence in results in columns 1 and 2 were to a large extent some combination of the number of children and the number of adults. We hence seem to adapt not to changes in income, but to changes in household composition. These results give a comprehensive explanation for why Pfaff (2013) and Binder and Coad (2010) find adaptation to income when using non-equivalized income, but fail to do so when using equivalized income: That which is observed as adaptation to own income in the non-equivalized case, is really reinforcement of the negative household composition effect that is controlled for in the equivalized case. This is further analyzed in section 5.5. There I investigate whether the effects of additional adults or of additional children drive these results, and whether the OECD equivalence scale over- or undercompensates in the short- and long-run.

Given that I use the most comprehensive set of controls in columns (2) and (4), I take these to be my preferred specifications. Notice that the results in these columns are, as one would expect, nearly identical. I therefore conclude that I obtain no valid evidence of adaptation for own income, insignificant reinforcement for reference income, and insignificant adaptation to joint income. The oft-cited evidence of adaptation to own non-equivalized income observed by Di Tella et al. (2010) appears entirely driven by reinforcement of the negative effect of household composition. I further find evidence of significant long-run effects for own (positive) and reference (negative) income, which in conjunction yield an insignificant long-run effect of joint income that is very close to zero. Taken together, these findings support the relative-income explanation for the Easterlin Paradox.

5.2 Extended results for Germany

Table 3: Extended results for equation (2) and (3), West Germany (1984-2015)

	(1)	(2)	(3)	(4)
	Instr.Eq. HH income, lagged controls	Instr.Eq. HH income, lagged controls, lagged LS	Instr.Eq. HH income, lagged controls, indust. & occup.	Instr.Eq. HH income, lagged controls, lagged LS, indust. & occup.
Lagged life satisfaction		0.263		0.263
SR effect of own income	0.687***	0.715***	0.409**	0.421**
	(0.181)	(0.188)	(0.169)	(0.175)
LR effect of own income	0.523***	0.514***	0.465***	0.464***
	(0.117)	(0.130)	(0.118)	(0.133)
Adaptation to own income	-0.163	-0.201	0.055	0.043
	(0.171)	(0.180)	(0.159)	(0.168)
SR effect of reference income	-0.203+	-0.267*	-0.182+	-0.236*
	(0.142)	(0.140)	(0.142)	(0.139)
LR effect of reference income	-0.321**	-0.352**	-0.317**	-0.351**
	(0.143)	(0.152)	(0.143)	(0.152)
Adaptation to reference income	-0.118	-0.085	-0.135	-0.115
	(0.166)	(0.174)	(0.166)	(0.174)
SR effect of joint income	0.484**	0.448**	0.227	0.185
	(0.207)	(0.211)	(0.201)	(0.204)
LR effect of joint income	0.202+	0.162	0.148	0.112
	(0.156)	(0.168)	(0.160)	(0.173)
Adaptation to joint income	-0.282+	-0.286+	-0.080	-0.072
	(0.212)	(0.221)	(0.207)	(0.217)
Kleibergen-Paap F statistic	54.945	56.511	67.360	68.450
Serial Correlation	0.029	-0.219	0.028	-0.217
Average T	7.830	7.831	7.830	7.831
Number of Persons	19472	19381	19472	19381
Number of Observations	152457	151765	152457	151765

Estimated with Correia's 'reghdfe' command.

Coefficient on lagged life satisfaction obtained with De Vos' & Ruyssen's 'xtbcfe' command.

Individual and year fixed-effects & (lagged) controls included.

Clustered standard errors in parentheses. + p<0.2, * p<0.10, ** p<0.050, *** p<0.010

As explained in section 3.2, while more robust and comprehensive than many of the results in the previous literature, the results of section 5.1 may nevertheless suffer from a negative bias in the own income effect via measurement error and unobserved costs to income generation. The results may further suffer from a downward bias in the estimate of adaptation via the omission of higher order lags and biases of unknown direction via the omission of further time varying controls. In table 3, I therefore present estimates in which I try to correct for these potential problems using the approach given in section 3.2.

In column 1 of table 3, I take the same specification as that of table 2, column 4, but instrument for all own income terms. This yields long- and short-run effects for own income that are roughly twice the size of the observed own income effects. This difference is roughly in line with the results of Powdthavee (2010), Luechinger (2009) or Knight and Gunatilaka (2010)[26]. Although the long-run effect is now somewhat smaller than the short-run effect, my estimate of adaptation continues to be insignificant (p=0.34). This result contradicts the finding of Vendrik (2013) who finds significant adaptation to own income when instrumenting. In the Appendix (table A2) I investigate the sensitivity of my estimates to the exclusion of each control. That exercise reveals that when I fail to include partner's employment, I do obtain marginally significant estimates of adaptation. It turns out that there is a large positive contemporaneous effect of partner employment that is subject to full adaptation after three periods. Especially for Women, partner employment is strongly correlated with household income. As Vendrik (2013) does not include partner's employment status (nor does any other study that I am aware of), it seems that the estimates of Vendrik (2013) also suffer from omitted variable bias[27].

Looking at reference incomes, my effect estimates for both the short-run and the long-run are now slightly larger than in table 2 but qualitatively unaffected. When adding the effects of own income and reference income, I now obtain a large and significant short-run effect of joint income. This short-run effect of joint income is then subject to (marginally insignificant) adaptation, yielding a marginally insignificant (but positive), long-run effect of joint income.

In column 2, I present my first estimate of equation (3). The inclusion of lagged life satisfaction to the model makes little substantive difference to any of the results[28], albeit yielding a slightly larger estimate for the short-run effect of own income and the effect of reference income (which is now marginally significant). The fact that there is very little change in results when including lagged life satisfaction or not suggests that a lag length of $K = 3$ is sufficient if the effects of higher order lags indeed decay exponentially. In table A4 of the Appendix I provide a further test for this. There, I take the specification of table 3, column 1 but let the number of lags K vary between 1 and 5. This reveals that the effects of own income are indeed robust to changes in the lag length, with my estimate of adaptation always being

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[26]Knight and Gunatilaka (2010) and Powdthavee (2010) use different instruments to the one employed here. Vendrik (2013) and Luechinger (2009) use the same kind of instrument. The fact that different instruments produce similar changes in results when comparing OLS and IV estimates, suggests that it is not a violation of the exclusion restriction that is driving these changes.

[27]In the Appendix (table A6) I also verify a result from section 5.1: When instrumenting for *raw*, rather than *equivalized* household income it is possible to obtain significant evidence of partial adaptation to own income. When, and only when, one fails to include lags for household composition, significant adaptation can be observed there. However, the discussion in section 3.1 shows that doing so would be mistaken.

[28]Note, however, how my estimate of first order serial correlation in the residuals turns much more negative with the inclusion of lagged life satisfaction. Under the null of a dynamically complete model in a fixed effects settings, I should expect the serial correlation to equal $\frac{-1}{T-1}$ (see Wooldridge (2010), p.275). When including lagged life satisfaction in column 2 of table 2 I obtain a deviation from that of 0.07 ($= \frac{-1}{7.83-1} - (-)0.22$). When failing to include life satisfaction I obtain a deviation from this expected serial correlation of -0.18 ($= \frac{-1}{7.83-1} - 0.03$). The inclusion of lagged life satisfaction therefore makes the model more dynamically complete.

insignificant. However, it seems as though the long-run reference income effect is not quite as stable, yielding somewhat larger estimates with greater lag lengths. Though this was already suggested by the slightly larger estimated reference income effect found for the dynamic model in column 2 of table 3, these results suggest that the reference income effect may be subject to very slow reinforcement that may take longer than three years. Hence, my estimate of the long-run reference income effect is possibly slightly biased towards zero at baseline for Germany. This also implies that my estimated joint income effect may be somewhat upwardly biased.

Columns 3 and 4 of table 3 address a concern raised in section 3.2, namely that the instrument may pick up further non-pecuniary positive effects of industry and occupation choice. I test for this by including contemporaneous and lagged values of all industry and occupation dummies to the regression. In doing, so my instrument can now only pick up the effects of varying pecuniary returns to industry-occupation combinations via their prediction of labor incomes, while all non-pecuniary effects will be captured by the industry and occupation dummies. The inclusion of these dummies depresses the estimates of the short- and long-run effects of own income. Importantly, the estimate of the short-run effect is depressed more than the estimate of the long-run effect, thus now yielding insignificant *reinforcement* rather than insignificant *adaptation.* Thus, even the insignificant finding of adaptation in columns 1 and 2 vanishes and reverses sign in these more robust estimations. My results for reference incomes are unaffected by the inclusion of these dummies.

5.3   Main results for comparison of Germany and the UK

I now turn to comparative results for Germany and the UK. I am not aware of any previous paper in the literature on adaptation in life satisfaction that has been strongly comparative. The working paper by Pfaff (2013) uses the BHPS and GSOEP simultaneously, but is not very comparative. This general lack of cross-country comparison is problematic: Much of the previous happiness literature seems to implicitly view results that are obtained with data from only one country (i.e. Germany with GSOEP) as indicative of facts beyond this particular country (i.e. rich industrialized democracies). Pursuing a comparative approach is an informal test of this presumed external validity. Put differently, in response to a lack of replication and/or replicability in economics and psychology (Earp and Trafimow, 2015; Chang and Li, 2018), using UKHLS data alongside GSOEP data can be viewed as a replication of what is thought to be known from GSOEP data. To make results comparable I use the same set of controls for both countries and restrict the estimation period for the German sample to that available for the British sample. Table 4 presents my results.

In columns 1 and 2, I replicate the estimate of table 3 column 1 using UKHLS data and the restricted GSOEP sample. There is no statistically significant evidence of adaptation for either country. Indeed, I observe insignificant reinforcement for the British sample. The coefficient estimate for the UK is somewhat smaller than for Germany. However, given the smaller number of response categories and the smaller standard deviation for life satisfaction in the British data, the substantive long-run effect of own household income appears somewhat larger in the UK than in Germany[29]. Turning to reference incomes, I find that the long-run effect of reference income for the restricted German sample is similar to the

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[29]Standardizing effect sizes via the number of response categories yields that the UK effect equals 144% ($\frac{11/7 \times 0.44}{0.48} = 1.46$) of the German effect. Standardizing via the respective standard deviations yields that UK's effect equals 114% of the German effect ($\frac{1/1.30 \times 0.44}{1/1.61 \times 0.48} = 1.14$).

Table 4: Comparative results for variants of equations (2) and (3)

	(1) UK (1996-2015): Instr.eq. HH income, lagged controls	(2) W.Germany (1996-2015): Instr.eq. HH income, lagged controls	(3) UK (1996-2015): Instr.eq. HH income, lagged controls, indust. & occup.	(4) W.Germany (1996-2015): Instr.eq. HH income, lagged controls, indust. & occup.	(5) UK (1996-2015): Instr.eq. HH income, lagged controls, lagged LS, indust. & occup.	(6) W.Germany (1996-2015): Instr.eq. HH income, lagged controls, lagged LS, indust. & occup.
Lagged life satisfaction					0.183	0.263
SR effect of own income	0.289***	0.502**	0.171+	0.155	0.145	0.241
	(0.111)	(0.212)	(0.124)	(0.198)	(0.133)	(0.209)
LR effect of own income	0.440***	0.475***	0.379***	0.381***	0.404**	0.437***
	(0.127)	(0.137)	(0.130)	(0.141)	(0.179)	(0.164)
Adaptation to own income	0.151	-0.027	0.208+	0.226	0.259+	0.197
	(0.121)	(0.201)	(0.127)	(0.188)	(0.164)	(0.200)
SR effect of reference income	-0.244*	-0.284*	-0.214+	-0.232+	-0.225+	-0.285*
	(0.141)	(0.155)	(0.143)	(0.154)	(0.162)	(0.154)
LR effect of reference income	-0.390**	-0.340**	-0.381**	-0.303**	-0.394*	-0.376**
	(0.165)	(0.151)	(0.168)	(0.150)	(0.220)	(0.163)
Adaptation to reference income	-0.146	-0.056	-0.167	-0.071	-0.168	-0.091
	(0.167)	(0.181)	(0.170)	(0.181)	(0.216)	(0.191)
SR effect of joint income	0.044	0.218	-0.043	-0.077	-0.081	-0.044
	(0.149)	(0.236)	(0.155)	(0.228)	(0.172)	(0.237)
LR effect of joint income	0.050	0.135	-0.003	0.078	0.010	0.061
	(0.185)	(0.162)	(0.190)	(0.166)	(0.250)	(0.184)
Adaptation to joint income	0.006	-0.083	0.041	0.155	0.091	0.105
	(0.177)	(0.236)	(0.181)	(0.228)	(0.234)	(0.241)
Kleibergen-Paap F statistic	79.468	40.690	60.323	50.241	56.007	51.232
Serial Correlation	-0.176	-0.001	-0.186	-0.003	-0.343	-0.238
Average T	4.763	6.962	4.763	6.962	4.431	6.968
Number of Persons	20443	20665	20443	20665	19312	20560
Number of Observations	97365	143878	97365	143878	85562	143252

Estimated with Correia's 'reghdfe' command. Individual and year fixed-effects & (lagged) controls included.

Coefficient on lagged life satisfaction obtained with De Vos' & Ruyssen's 'xthcfe' command.

Clustered standard errors in parentheses. + p<0.2, * p<0.10, ** p<0.050, *** p<0.010

the full German sample, yielding a significantly negative effect. Even before standardizing, the long-run reference income effect for the UK is again similar but somewhat larger in absolute size than the German one (-0.39 *vs.* -0.34). As was the case in table 3, I observe insignificant reinforcement of the reference income effect in both cases.

In columns 3 and 4, I add dummies for each industry and occupation to provide the same check of the instrument as given in the previous section and discussed in section 3.2.1. For both countries, this again results in a much diminished short-run effect, but a fairly stable long-run effect for own income. As was the case in the previous section, I now find insignificant reinforcement for Germany rather than insignificant adaptation to own income. My estimates for reference income are again only marginally affected. Finally, columns 5 and 6 give results when further adding lagged life satisfaction, where the coefficient on lagged life satisfaction is fixated in the way described in section 3.2.2. My results are qualitatively robust to this addition.

Throughout all of these analyses, both short- and long-run effects of joint income (i.e. the combined effects of own and reference income) are insignificant in both countries. Thus, it seems that neither in the UK nor in Germany would making everybody richer, make everybody happier. In the Appendix I provide further robustness checks. In table A5, I investigate whether the British estimates are sensitive to lag length. Just as was the case for Germany, my results are largely robust to the choice of the number of lags. In table A7, I reestimated these models with instrumented raw (rather than equivalized) household income. I again find significant adaptation to own income whenever (and only when) I fail to include for household composition. This also agrees with my German results. Finally, table A8 replicates the analysis of table 2 for the UK; thus using observed rather than instrumented own income. This yields very small estimates, which agrees with the results of Pfaff (2013). Given that the BHPS does not directly provide net income data and had to be generated with external information by Levy and Jenkins (2012), it is likely that these estimates using observed income suffer from severe attenuation bias due to measurement error. This indicates that it is paramount to use valid instruments in the case of this British data.

5.4 Results for sub-groups

I now turn to investigating whether there are differences across population groups, and whether these differences are homogeneous across the two countries I am investigating. Tables 5 (Germany) and 6 (UK) give results for when I stratify my sample between men and women (columns 1 and 2), those on the political left and right (columns 3 and 4), and those that are either above or below median household income at time of the survey (columns 5 and 6)[30]. The first two subgroups were also used in Di Tella et al. (2010), and thus allow for a further attempt at corroboration of these studies' main findings.

Di Tella et al. (2010) find that only women adapt to own income, and that they fully adapt. As could be expected, I cannot confirm this finding. Instead I find insignificant reinforcement of the own income effect for both men and women, and a much larger positive long-run effect for women than for men[31], the latter of which is even marginally insignificant. Concerning reference income, I also find such stronger

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[30]Apart from restricting the sample, these specifications are the same as those given in table 3 column 4 (Germany), and table 4 column 3 (UK).

[31]It turns out that the stronger adaptation for women that Di Tella et al. (2010) find is driven by much stronger adaptation to partner employment and household composition for women that for men. These additional regressions are available from the author.

Table 5: Results for variants of equation (2) for various subgroups, West Germany (1984-2015)

	(1) Instr.Eq. HH income, lagged controls, indust. & occup., males	(2) Instr.Eq. HH income, lagged controls, indust. & occup., females	(3) Instr.Eq. HH income, lagged controls, indust. & occup., right-wing	(4) Instr.Eq. HH income, lagged controls, indust. & occup., left-wing	(5) Instr.Eq. HH income, lagged controls, indust. & occup., below median	(6) Instr.Eq. HH income, lagged controls, indust. & occup., above median
SR effect of own income	0.070	0.309	0.018	0.160	0.636**	-0.120
	(0.216)	(0.313)	(0.361)	(0.383)	(0.293)	(0.453)
LR effect of own income	0.228+	0.581***	0.641**	0.349+	0.760***	0.056
	(0.145)	(0.213)	(0.271)	(0.237)	(0.273)	(0.255)
Adaptation to own income	0.158	0.272	0.623*	0.190	0.124	0.176
	(0.207)	(0.287)	(0.355)	(0.371)	(0.229)	(0.304)
SR effect of reference income	-0.006	-0.335+	0.183	-0.419+	-0.395+	-0.063
	(0.202)	(0.205)	(0.316)	(0.286)	(0.246)	(0.188)
LR effect of reference income	-0.233	-0.500**	-0.368	-0.513*	-0.778***	-0.205
	(0.192)	(0.223)	(0.311)	(0.267)	(0.247)	(0.182)
Adaptation to reference income	-0.227	-0.165	-0.551+	-0.094	-0.383+	-0.142
	(0.234)	(0.244)	(0.372)	(0.333)	(0.296)	(0.211)
SR effect of joint income	0.065	-0.025	0.200	-0.259	0.241	-0.183
	(0.266)	(0.342)	(0.456)	(0.411)	(0.375)	(0.439)
LR effect of joint income	-0.005	0.081	0.273	-0.163	-0.018	-0.149
	(0.205)	(0.277)	(0.386)	(0.301)	(0.341)	(0.281)
Adaptation to joint income	-0.070	0.107	0.073	0.096	-0.259	0.034
	(0.274)	(0.344)	(0.461)	(0.424)	(0.353)	(0.332)
Kleibergen-Paap F statistic	42.346	20.599	12.989	9.816	40.085	30.045
Serial Correlation	0.030	0.021	-0.052	-0.031	-0.044	-0.008
Average T	7.997	7.670	5.782	6.121	5.846	6.687
Number of Persons	9512	9960	4426	6433	10851	12752
Number of Observations	76068	76389	25589	39374	63439	85277

Estimated with Correia's 'reghdfe' command. Individual and year fixed-effects & (lagged) controls included.
Clustered standard errors in parentheses. + p<0.2, * p<0.10, ** p<0.050, *** p<0.010

Table 6: Results for variants of equation (2) for various subgroups, UK (1996-2015)

	(1) Instr.Eq. HH income, lagged controls, indust. & occup., males	(2) Instr.Eq. HH income, lagged controls, indust. & occup., females	(3) Instr.Eq. HH income, lagged controls, indust. & occup., right-wing	(4) Instr.Eq. HH income, lagged controls, indust. & occup., left-wing	(5) Instr.Eq. HH income, lagged controls, indust. & occup., below median	(6) Instr.Eq. HH income, lagged controls, indust. & occup., above median
SR effect of own income	0.140	0.276+	0.352+	-0.009	0.718***	-0.026
	(0.178)	(0.189)	(0.217)	(0.257)	(0.242)	(0.375)
LR effect of own income	0.459**	0.428**	0.317+	0.418*	0.913***	0.125
	(0.186)	(0.183)	(0.230)	(0.247)	(0.300)	(0.326)
Adaptation to own income	0.319*	0.151	-0.035	0.428*	0.195	0.151
	(0.188)	(0.190)	(0.222)	(0.245)	(0.242)	(0.189)
SR effect of reference income	-0.322+	-0.244	-0.395+	-0.032	-0.110	-0.390**
	(0.200)	(0.211)	(0.260)	(0.253)	(0.237)	(0.189)
LR effect of reference income	-0.518**	-0.300+	0.156	-0.507*	-0.501*	-0.254
	(0.261)	(0.231)	(0.290)	(0.297)	(0.286)	(0.227)
Adaptation to reference income	-0.196	-0.057	0.552*	-0.475*	-0.391+	0.136
	(0.254)	(0.242)	(0.309)	(0.288)	(0.286)	(0.231)
SR effect of joint income	-0.182	0.033	-0.043	-0.041	0.608**	-0.416
	(0.228)	(0.222)	(0.261)	(0.285)	(0.298)	(0.368)
LR effect of joint income	-0.059	0.127	0.474+	-0.089	0.412	-0.130
	(0.281)	(0.257)	(0.340)	(0.314)	(0.357)	(0.374)
Adaptation to joint income	0.123	0.095	0.516+	-0.048	-0.196	0.286
	(0.261)	(0.260)	(0.324)	(0.309)	(0.318)	(0.253)
Kleibergen-Paap F statistic	28.596	30.100	21.359	12.943	32.355	31.814
Serial Correlation	-0.193	-0.190	-0.249	-0.239	-0.244	-0.229
Average T	4.767	4.758	4.020	4.121	3.969	4.242
Number of Persons	8984	11460	7337	8748	10696	11700
Number of Observations	42831	54531	29492	36049	42452	49635

Estimated with Correia's 'reghdfe' command. Individual and year fixed-effects & (lagged) controls included.

Clustered standard errors in parentheses. + $p<0.2$, * $p<0.10$, ** $p<0.050$, *** $p<0.010$

effects for German women than for German men (the latter effect is again insignificant). Thus, German women benefit more from greater household income, but are also harmed more from higher incomes of others. I do not observe these differences in British sample; there I find roughly the same long-run effects for men and women when looking at own income, and an effect of reference incomes that is even larger for men (and marginally insignificant for women). I can only speculate to explain this cross-country difference. It might be that there are further uncontrolled-for hedonic costs to income generation that are picked up by my instrument. If so, and given that (in my sample) women in Germany are less likely to earn more than their partners (18%) than women in the UK (23%), women in Germany may on average be better able to benefit from higher shared household incomes without suffering the hedonic costs of income generation. Another possibility is that the assumption of equal income sharing within a household (Jenkins, 1991) is not met. If women in Germany are able to capture a relatively greater control over household income with each additional Euro than British women and German men, I should observe a greater long-run effect for women German women over German men and British women.

With regard to political orientation[32], my results go largely counter to Di Tella et al. (2010) and are not fully consistent across countries. Di Tella et al. (2010) find adaptation to own income for those on the left, and larger long-run effects for those on the right. I can confirm the latter finding: In Germany, for those on the right of the political spectrum, I find a much larger long-run effect of own income, while for those on the left the effect is even marginally insignificant. However, unlike Di Tella et al. (2010), I find insignificant reinforcement rather than adaptation in both groups. Moreover, in the UK, own income effects are not much different across these groups, with the effect even being slightly larger for those on the left. Nevertheless, differences in long-run reference income effects are consistent across both countries. In each case, the long-run reference income effect is negative and much larger (in absolute size) for those on the left than for those on the right (where they are insignificant in both countries).

A striking pattern emerges when distinguishing between those above and those below median income. In both countries, short- and long-run effects of own income are insignificant and very close to zero when considering those above the median, while they are very sizable and significant for those below the median[33]. This results is in line with the claim that higher incomes do not yield *any* additional utility after a certain threshold, i.e. that we satiate with respect to income. All previous evidence of this claim that I am aware of has been either purely cross-sectional and concerned with the short-run only (Jebb et al., 2018) or concerned with GDPpc rather than individual income (Proto and Rustichini, 2013). My result also runs counter to the evidence of Kahneman and Deaton (2010)[34] and Stevenson and Wolfers (2013) (the latter only study GDPpc). In line with previous evidence from Peng (2017), I find a similar pattern when considering reference incomes: the long-run effects are much larger in absolute size for

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[32]In both the German and the British case, I define my sub-sample via variables asking respondents to declare their party preference. In Germany, if their preference is one of Social Democrats (SPD), Greens (Grüne), Lefts (Die Linke), or Pirates (Piraten), or any combination thereof, they are are treated as being on the left. If their preference is one of Christian Democrats (CDU, CSU), Liberals (FDP), Nationalists (NPD), or Alternative for Germany (AFD), they are treated as being on the right. In the UK, if their preference is one of Labour, Scottish National Party, Plaid Cymru, Greens, or Sinn Fein, they are treated as on the left. If their preference is one of Conservatives, Liberal Democrats, Ulster Unionist, Alliance Party, UK Independence Party, or British National Party they are treated as being on the right.

[33]Here I also find no adaptation for either group. No adaptation for those below the median agrees with the aforementioned studies of Clark et al. (2016) and Crettaz and Suter (2013) that study those below the poverty line. However, given that I do not find adaptation for any group, this is not surprising. Instead it is likely that these studies do not find adaptation simply because they use equivalized household incomes, not because there is anything special in this respect about those below the poverty line.

[34]Though they find no satiation with respect to evaluative measures of subjective well being, they do find satiation with respect to affective measures of subjective well being.

those below the median than for those above the median. This again holds for both countries.

As a proxy for wealth, and following Di Tella and MacCulloch (2010), I also ran estimations in which I distinguish between homeowners, and individuals who rent. These can be found in the Appendix. In the German case, as both the long-run effects of own and reference income were much larger for those who rent than for homeowners, these estimations corroborated my findings concerning those above and those below the median of the income distribution. Interestingly, my estimate of adaptation is sizable (=-0.49) and marginally insignificant (p=0.15) for homeowners. Throughout all of my analyses, this was the only time that I found potential credible evidence of adaptation. However, in the British case these estimations gave largely implausible results, showing insignificant and negative short- and long-run effects of own income for those who rent.

5.5 Investigating adaptation/reinforcement to household composition

In sections 5.1 and 5.3, it became clear that results that found evidence of adaptation to own income, e.g those in Pfaff (2013), Di Tella et al. (2010), Di Tella and MacCulloch (2010), or Binder and Coad (2010) were probably driven by adaptation/reinforcement to (aspects of) household composition. I will now take up this issue in a little more depth.

Recall that in section 5.1, I say that the model of equation (2)[35] can be rewritten as:

$$
\begin{aligned}
LS_{i,t} = &\sum_{k=0}^{K} \beta_{0-k}^{inc} \ln(y_{i,t-k}^{defl}) + \sum_{k=0}^{K} \beta_{0-k}^{comp} \ln(EQ_{i,t-k}) \\
&+ \sum_{k=0}^{K} \gamma_{0-k} \ln(y_{i,t-k}^{ref}) + \sum_{k=0}^{K} \phi_{0-k} X_{i,t-k} + \mu_i + \tau_t + \epsilon_{i,t}
\end{aligned}
\tag{8}
$$

With $EQ_{i,t} \equiv (1 + (nadults_{i,t} - 1)) * \gamma_1 + nchildren_{i,t} * \gamma_2$, where $\gamma_1 = 0.5$ and $\gamma_2 = 0.3$ are set by the OECD as weights attached to the number of additional adults and children. This formulation shows most clearly that when using (lagged) equivalized income, one implicitly includes a (lagged) control for household size.

Notice that in this set-up it is assumed that for all k, $\beta_{0-k}^{comp} = -\beta_{0-k}^{inc}$. If one thinks that household income should be equivalized with respect to life satisfaction, this is a substantive assumption. In so far as many in the literature view life satisfaction as a proxy for utility (e.g. Layard et al. (2008), Stutzer (2004), Bayer and Juessen (2015)), and view equivalence scales as a means by which to assess - loosely speaking - how much more money one would need in an n-person household to attain the same utility as living alone (Rojas, 2007; Ferrer-i Carbonell and Van Praag, 2008; Deaton and Muellbauer, 1986), this appears plausible.

Only when it is in fact true that, $\beta_0^{comp} = -\beta_0^{inc}$, is the contemporaneous life satisfaction effect of own equivalized income comparable across household compositions. Similarly, only when it is true that $\sum_{k=0}^{\infty} \beta_0^{inc} = \sum_{k=0}^{\infty} -\beta_0^{comp}$, is the long-run life satisfaction effect of own equivalized income comparable across household compositions. When it is for example the case that $\beta_0^{inc} - \beta_0^{comp} > 0$, then the negative effect of an increase in household size is smaller than is assumed by the equivalization scale. In that case,

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[35]The same would hold for equation (5)

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Table 7: Results for variants of equation (2): Undercompensation or overcompensation?

	(1)	(2)	(3)	(4)
	W.Germany (1984-2015): Instr.HH income, lagged controls, indust. & occup.	UK (1996-2015): Instr.HH income, lagged controls, indust. & occup.	W.Germany (1984-2015): Instr.HH income, lagged controls, indust. & occup., lagged ln(#adults) & ln(#children)	UK (1996-2015): Instr.HH income, lagged controls, indust. & occup., lagged ln(#adults) & ln(#children)
SR effect of own income	0.289*	0.133	0.332**	0.166+
	(0.163)	(0.114)	(0.164)	(0.119)
LR effect of own income	0.381***	0.331***	0.426***	0.382***
	(0.105)	(0.104)	(0.117)	(0.126)
Adaptation to own income	0.093	0.198*	0.094	0.217*
	(0.154)	(0.116)	(0.155)	(0.122)
SR effect of HH size	-0.120	-0.177+		
	(0.164)	(0.126)		
LR effect of HH size	-0.468***	-0.567***		
	(0.100)	(0.104)		
Adaptation to HH size	-0.348**	-0.390***		
	(0.152)	(0.122)		
SR Under-/over-compensation	0.169***	-0.045		
	(0.039)	(0.040)		
SR Under-/over-compensation	-0.086**	-0.236***		
	(0.043)	(0.050)		
SR effect of number of adults			-0.127	-0.149*
			(0.107)	(0.085)
LR effect of number of adults			-0.350***	-0.406***
			(0.079)	(0.089)
Adaptation to number of adults			-0.223**	-0.257***
			(0.101)	(0.086)
SR effect of number of children			0.045	0.010
			(0.042)	(0.038)
LR effect of number of children			-0.057**	-0.105***
			(0.027)	(0.028)
Adaptation to number of children			-0.102**	-0.116***
			(0.041)	(0.037)
Kleibergen-Paap F statistic	66.163	65.183	68.033	63.165
Serial Correlation	0.030	-0.185	0.030	-0.186
Average T	7.830	4.763	7.830	4.763
Number of Persons	19472	20443	19472	20443
Number of Observations	152457	97365	152457	97365

Estimated with Correia's 'reghdfe' command. Individual and year fixed-effects & (lagged) controls included.

Clustered standard errors in parentheses. $+$ p<0.2, * p<0.10, ** p<0.050, *** p<0.010

and if one would wish to equivalize on life satisfaction effects, one ought to decrease the weight on either the number of adults (γ_1) or the number of children (γ_2).

I pursue the question of under- vs over-compensation in columns 1 and 2 of table 7. There, I estimate equation (8), and thus let the weights on $\ln(y^{defl})$ and $\ln(EQ)$ be estimated freely (and remove my control for child birth). For both countries, the negative effect of household composition is much larger in the long-run than in the short-run, implying significant reinforcement. Furthermore, in Germany the OECD equivalence scales overcompensates (i.e. $\beta_0^{inc} - \beta_0^{comp} > 0$) in the short-run, but undercompensates (i.e. $\sum_{k=0}^{3} \beta_{0-k}^{inc} - \sum_{k=0}^{3} \beta_{0-k}^{comp} < 0$) in the long-run. In the UK, the OECD scale neither under- nor overcompensates in the short-run, but severely undercompensates in the long-run. Such a result is noteworthy for the following reason: Several prior studies have derived equivalence scales using subjective satisfaction data. Examples of this are Schwarze (2003), Borah et al. (2016), Biewen and Juhasz (2017) or Van Praag and Ferrer-i Carbonell (2008) in the German case, Bollinger et al. (2012) and Van Praag and Ferrer-i Carbonell (2008) in the British case, or Rojas (2007) in the Mexican case. In all of these studies, they find that the OECD equivalence scale tends to overcompensate. However, each of these studies only estimated short-run equivalence scales, failing to take into account reinforcement in the negative effect of household composition. My results imply that an equivalence scale that takes into account adaptation and reinforcement to own income and household composition, would yield much smaller economies of scale in the long-run. Importantly these would be even smaller than implied by the OECD.

It is a further question whether it is the number of adults or the number of children that drives these results. It would have been nice to estimate β_{0-k}^{comp}, γ_1 and γ_2 freely (as do Biewen and Juhasz (2017)), but this would not be estimable linearly and thus be not easily accomplished in my panel-IV context. I therefore estimated the following, simpler equation in columns 3 and 4:

$$LS_{i,t} = \sum_{k=0}^{3} \beta_{0-k}^{inc} \ln(y_{i,t-k}^{defl}) + \sum_{k=0}^{3} \beta_{0-k}^{adu} \ln(nadults_{i,t-k}) + \sum_{k=0}^{3} \beta_{0-k}^{kid} \ln(1 + nkids_{i,t-k}) \tag{9}$$
$$+ \sum_{k=0}^{3} \gamma_{0-k} \ln(y_{i,t-k}^{ref}) + \sum_{k=0}^{3} \phi_{0-k} X_{i,t-k} + \mu_i + \tau_t + \epsilon_{i,t}$$

Independent of the country considered, the negative effect of additional adults in the household always far exceeds that of additional children, even beyond the difference implied by the OECD scale. I even find positive (but insignificant) short-run effects of additional children, which turn negative and significant in the long-run. These results for children are in line with previous results that show full adaptation to child-birth (see Angeles (2010); Clark et al. (2008); Clark and Georgellis (2013)). Finally, the specification of equation (9) allows me to derive short- and long-run equivalence scales (cf. Rojas, 2007). Taking a single adult living alone as the reference case, I can find the amount of additional income necessary to obtain the same life satisfaction as when living with additional n^{adu} adults and n^{kid} children. This is given by:

$$R_{SR}(Y_0 \rightarrow Y_{n^{adu},n^{kid}}) = \exp\left[\ln\left(\frac{1}{n^{adu}}\right) * \frac{\beta_0^{adu}}{\beta_0^{inc}} + \ln\left(\frac{1}{1 + n^{kid}}\right) * \frac{\beta_0^{adu}}{\beta_0^{inc}}\right] \tag{10}$$

To see this, solve for y^{defl} in equation (9). Then take the ratio of this expression when $nadults = n^{adu}$ to when $nadults = 1$, and when $nkids = n^{kids}$ to when $nkids = 0$ and simplify. For the long-run replace
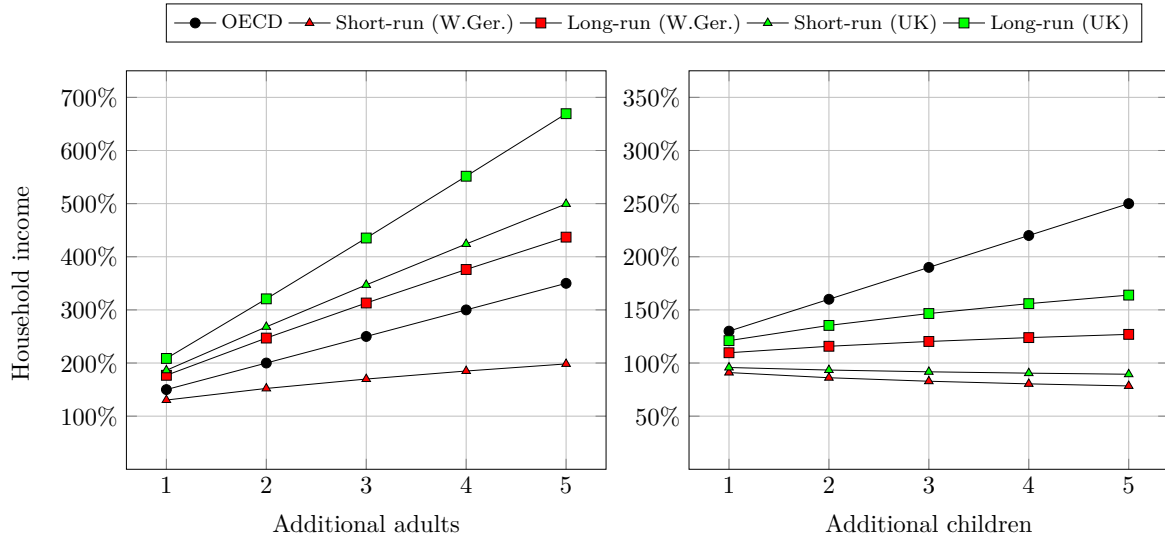
Figure 5: Illustration of equivalence scales computed from equations (10) and (11) on the basis of parameter estimates from table 7, columns 3 (Germany) and 4 (UK). In both panels a single adult living alone is taken to be the reference case. The panel on the left shows the amount of household income (in % of the reference case) necessary to compensate (i.e. to obtain the same level of life satisfaction as in the reference case) for each additional adult in the household. The right panel show the amount of household income necessary to maintain constant life satisfaction with each additional child in the household.

the short-run coefficients β_0^{inc}, β_0^{adu}, and β_0^{kid} by their long-run equivalents $\sum_{k=0}^{3} \beta_{0-k}^{inc}$, $\sum_{k=0}^{3} \beta_{0-k}^{adu}$, and $\sum_{k=0}^{3} \beta_{0-k}^{kid}$ to obtain:

$$R_{LR}(Y_0 \to Y_{n^{adu}, n^{kid}}) = \exp\left[\ln\left(\frac{1}{n^{adu}}\right) * \frac{\sum_{k=0}^{3} \beta_{0-k}^{adu}}{\sum_{k=0}^{3} \beta_{0-k}^{inc}} + \ln\left(\frac{1}{1+n^{kid}}\right) * \frac{\sum_{k=0}^{3} \beta_{0-k}^{kid}}{\sum_{k=0}^{3} \beta_{0-k}^{inc}}\right] \qquad (11)$$

I plot results for that exercise in figure 5. Full tables are given in the Appendix. From figure 5 it becomes evident that the OECD equivalence scales completely overcompensates for additional children, both in the short- and in the long-run. However, the OECD scale only overcompensates for additional adults in the short-run for Germany. In the long-run, the OECD scale severely undercompensates for the effect of additional adults. Indeed, in the British case there appear to be no economies of scale in the long-run at all[36]. In so far as the German welfare state is much more comprehensive with respect to child-care than the British (thus lessening the burden of additional children), and more willing to support partnered households, these observed country differences may be driven by the institutional structure of these countries.

6   Conclusion

Substantively I make three main points. First, I find no evidence of adaptation to own income. All cases in which my estimate of adaptation is significant and negative occur when omitting variables relating to household composition. This finding is in direct disagreement with most of the previous literature.

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[36]Note that I control for marital status. Therefore much of the positive effect of being in a multi-adult household will be captured by this variable.

Second, I do not find significant adaptation or reinforcement for reference income. Third, both own income and reference income are very sizable and of roughly equal absolute magnitude in almost every specification. This implies that in every specification the long-run effect of joint income is insignificant. Thus, if everybody gets richer, nobody becomes happier. Although this is a sad result, it validates the much discussed Easterlin Paradox.

Methodologically three points are worth noting: First, instrumenting for own income is paramount. I find much larger income effects when instrumenting, especially with respect to the UK. The pronounced rise in effect sizes when instrumenting is in line with the IV results of Vendrik (2013), Powdthavee (2010), Luechinger (2009) and Knight and Gunatilaka (2010). Second, the computationally and conceptually difficult addition of lagged life satisfaction, i.e. the use of a dynamic rather than a static model is unnecessary. All my results were qualitatively unchanged with this further complication. Third, and though used throughout the paper as the most robust option, the addition of lags of controls is only crucial with respect to the number of adults in the household and when using raw household income. This is because the negative effect of the number of adults in the household is subject to strong reinforcement, while it is also very strongly correlated with household income. In the present context, and when using equivalized household income, one may omit the addition of lags of controls and obtain very similar results. However this need not necessarily be so in other contexts, which means that many of the aforementioned studies on adaptation to other life domains may also suffer from this kind of omitted variable bias.

Beyond these points, my estimations for sub-samples show that there are differences in results across the UK and Germany. Thus, the dynamic effects of own and reference income are not invariant to context or should be viewed as universal psychological constants: they seem socially embedded. It would be useful to unpack these heterogeneities further than was possible here. Furthermore, an important limitation of this study is the use of yearly data. If adaptation to income occurs much faster than within the first year, such adaptation could not be detected (Ferrer-i Carbonell and Van Praag, 2008). To tackle this problem with temporally more fine-grained data (perhaps the Australian data Frijters et al. (2008) use) seems like a useful avenue for future research. However, given that I do find sizable short-run effects of income, it seems that adaptation would have to follow from a very large, and possibly implausibly large, initial effect of income. Moreover, my significant long-run effects imply that any adaptation must be partial at best. Nevertheless, and to quote Ferrer-i Carbonell and Van Praag (2008), *the discussion is not finished yet.*

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Appendix

Table A1: Description of Controls (continued on next page)

Concept	GSOEP		UKHLS	
Number of children	Dummies for:		Dummies for:	
	0	(54.7%)	0	(59.6%)
	1	(20.7%)	1	(16.9%)
	2	(17.9%)	2	(17.6%)
	3	(5.20%)	3	(4.87%)
	4	(1.09%)	4	(0.90%)
	5	(0.27%)	5	(0.12%)
	6 or more	(0.12%)	6 or more	(0.03%)
Number of adults	Dummies for:		Dummies for:	
	1	(9.92%)	1	(12.8%)
	2	(59.9%)	2	(62.0%)
	3	(18.9%)	3	(16.4%)
	4	(8.90%)	4	(7.14%)
	5	(1.87%)	5	(1.27%)
	6 or more	(0.53%)	6 or more	(0.34%)
Employment status	Dummies for:		Dummies for:	

Concept	GSOEP		UKHLS	
	Non-working	(10.5%)	Lt.sick or disabled	(3.35%)
	Retired	(1.14%)	Retired	(9.70%)
	Unemployed	(2.25%)	Unemployed	(1.97%)
	Student	(1.00%)	Student	(0.62%)
	Maternity leave	(1.61%)	Family Care	(6.65%)
	Working	(81.6%)	Working	(77.5%)
Partner employment status	Dummies for:		Dummies for:	
	Working	(60.9%)	Working	(58.9%)
	No partner / NA	(22.1%)	No partner / NA	(24.0%)
	Unemployed	(1.99%)	Unemployed	(1.39%)
	Non-working	(15.1%)	Non-working	(15.4%)
Age	Mean=43.39	SD=11.03	Mean=45.35	SD=12.03
	Min=23	Max=70	Min=22	Max=70
Age squared	Mean=2004	SD=973.7	Mean=2202	SD=1119
	Min=529	Max=4900	Min=484	Max=4900
Marital status	Dummies for:		Dummies for:	
	Married/Couple	(73.5%)	Married	(66.4%)
	Single	(16.7%)	Single	(11.5%)
	Widowed	(1.49%)	Widowed	(2.07%)
	Divorced	(6.21%)	Divorced	(8.13%)
	Separated	(1.79%)	Living as couple	(11.9%)
	Other	(0.32%)		
Housing	Dummies for:		Dummies for:	
	Own	(51.2%)	Own	(79.8%)
	Rented	(48.8%)	Rented privately	(7.86%)
			Social housing	(12.3%)
Working hours	ln(yearly working hours)		ln(weekly working hours)	
	Mean=6.31	SD=2.72	Mean=2.71	SD=1.49
	Min=0	Max=8.80	Min=0	Max=4.61
Ill health	Dummy over 0-10 subj. health variable =1 if subj.health < 5	(13.3%)	Dummy over 1-7 subj. health variable =1 if subj.health < 4	(22.4%)

Table A1: (continued)

Concept	GSOEP		UKHLS	
	=0 if subj.health > 4	(86.6%)	=1 if subj.health > 3	(77.6%)
Birth of children	Dummies for		Dummies for	
	Birth	(4.03%)	Birth	(3.47%)
	No birth	(96.0%)	No birth	(96.5%)
Education	Dummies for:		Dummies for:	
	Inadequate/elementary	(15.2%)	No Qualification	(11.2%)
	Mittlere Reife	(33.5%)	GCSE	(23.0%)
	Abitur & Ausbildung	(33.3%)	A-levels	(20.9%)
	Tertiary education	(16.6%)	Tertiary degree	(23.3%)
	Other/missing	(1.44%)	Other higher	(12.1%)
Region	Bundeslander dummies	(11)	Govt. region dummies	(11)
Wave	Waves dummies	(28)	Waves dummies	(16)
Regional time trend	Interactions of region and waves		Interactions of region and waves	

Table A2: Sensitivity to removal of controls, Germany (1984-2015)

	Own income			Reference income		
Variable omitted	Short-run	Long-run	Adaptation	Short-run	Long-run	Adaptation
Number of children	0.327*	0.476***	0.149	-0.222+	-0.391***	-0.169
Number of adults	0.375**	0.412***	0.036	-0.178	-0.319**	-0.141
Employment status	0.533***	0.528***	-0.004	-0.218+	-0.390***	-0.171
Partner employment status	0.787***	0.525***	-0.262*	-0.224+	-0.313**	-0.089
Age	0.409**	0.465***	0.055	-0.182+	-0.317**	-0.135
Age-squared	0.375**	0.433***	0.058	-0.271*	-0.272*	-0.001
Marital status	0.394**	0.470***	0.075	-0.180	-0.316**	-0.137
Housing tenure	0.431***	0.488***	0.057	-0.186+	-0.316**	-0.130
Working hours	0.409**	0.464***	0.056	-0.182+	-0.317**	-0.135
Ill health	0.447***	0.424***	-0.022	-0.194+	-0.377**	-0.182
Child birth	0.357**	0.460***	0.102	-0.171	-0.317**	-0.146
Education	0.404**	0.459***	0.054	-0.080	-0.259**	-0.179+
Wave	0.387**	0.430***	0.043	-0.362***	-0.624***	-0.261**
Region	0.407**	0.465***	0.058	-0.178	-0.309**	-0.132
Regional time trend	0.409**	0.464***	0.054	-0.165	-0.281**	-0.116

Based on specification of table 3, column 3. Each row shows estimates after removing the corresponding control

Table A3: Sensitivity to removal of controls, UK (1996-2015)

	Own income			Reference income		
Variable omitted	Short-run	Long-run	Adaptation	Short-run	Long-run	Adaptation
Number of children	0.179+	0.436***	0.257**	-0.243*	-0.402**	-0.159
Number of adults	0.176+	0.393***	0.218*	-0.212+	-0.373**	-0.161
Employment status	0.174+	0.383***	0.209+	-0.258*	-0.472***	-0.214
Partner employment status	0.237*	0.375***	0.138	-0.243*	-0.388**	-0.145
Age	0.173+	0.384***	0.211*	-0.208+	-0.370**	-0.162
Age-squared	0.152	0.342***	0.190+	-0.234*	-0.453***	-0.219+
Marital status	0.119	0.356***	0.237*	-0.178	-0.387**	-0.209
Housing tenure	0.174+	0.377***	0.203+	-0.215+	-0.388**	-0.173
Working hours	0.171+	0.377***	0.206+	-0.220+	-0.391**	-0.171
Ill health	0.165	0.343**	0.178+	-0.267*	-0.452**	-0.186
Child birth	0.165+	0.380***	0.215*	-0.206+	-0.383**	-0.177
Education	0.168+	0.374***	0.207+	-0.113	-0.152	-0.039
Wave	0.178+	0.320**	0.142	-0.034	-0.915***	-0.882***
Region	0.171+	0.373***	0.202+	-0.212+	-0.377**	-0.165
Regional time trend	0.170+	0.377***	0.207+	-0.220+	-0.378**	-0.158

Based on specification of table 4, column 3. Each row shows estimates after removing the corresponding control

Table A4: Results for equation (2) with varying lag length, West Germany (1984-2015)

	(1) Instr.Eq. HH income, lagged controls, K=1	(2) Instr.Eq. HH income, lagged controls, K=2	(3) Instr.Eq. HH income, lagged controls, K=3	(4) Instr.Eq. HH income, lagged controls, K=4	(5) Instr.Eq. HH income, lagged controls, K=5	(6) Instr.Eq. HH income, lagged controls, K=6
SR effect of own income	0.403***	0.394**	0.409**	0.389**	0.492**	0.375*
	(0.141)	(0.156)	(0.169)	(0.188)	(0.203)	(0.225)
LR effect of own income	0.564***	0.522***	0.465***	0.467***	0.362**	0.511***
	(0.086)	(0.101)	(0.118)	(0.140)	(0.165)	(0.191)
Adaptation to own income	0.162	0.129	0.055	0.078	-0.130	0.136
	(0.130)	(0.140)	(0.159)	(0.180)	(0.200)	(0.227)
SR effect of reference income	-0.199*	-0.175+	-0.182+	-0.189	-0.088	-0.158
	(0.117)	(0.129)	(0.142)	(0.151)	(0.162)	(0.174)
LR effect of reference income	-0.294***	-0.315**	-0.317**	-0.449***	-0.488***	-0.637***
	(0.110)	(0.126)	(0.143)	(0.163)	(0.185)	(0.209)
Adaptation to reference income	-0.095	-0.140	-0.135	-0.259+	-0.400*	-0.479**
	(0.116)	(0.143)	(0.166)	(0.185)	(0.207)	(0.230)
SR effect of joint income	0.204	0.219	0.227	0.200	0.405*	0.217
	(0.161)	(0.181)	(0.201)	(0.220)	(0.238)	(0.260)
LR effect of joint income	0.270**	0.208+	0.148	0.018	-0.126	-0.126
	(0.119)	(0.139)	(0.160)	(0.184)	(0.213)	(0.241)
Adaptation to joint income	0.066	-0.011	-0.080	-0.181	-0.530**	-0.343
	(0.148)	(0.178)	(0.207)	(0.232)	(0.258)	(0.286)
Kleibergen-Paap F statistic	429.922	134.312	67.360	38.026	23.825	17.201
Serial Correlation	0.038	0.032	0.028	0.024	0.017	0.014
Average T	7.819	7.767	7.830	8.053	8.495	8.275
Number of Persons	28307	23587	19472	15829	12624	11172
Number of Observations	221322	183212	152457	127475	107246	92452

Estimated with Correia's 'reghdfe' command.

Individual and year fixed-effects & (lagged) controls included.

Clustered standard errors in parentheses. + p<0.2, * p<0.10, ** p<0.050, *** p<0.010

Table A5: Results for equation (2) with varying lag length, UK (1996-2015)

	(1) Instr.Eq. HH income, lagged controls, K=1	(2) Instr.Eq. HH income, lagged controls, K=2	(3) Instr.Eq. HH income, lagged controls, K=3	(4) Instr.Eq. HH income, lagged controls, K=4	(5) Instr.Eq. HH income, lagged controls, K=5	(6) Instr.Eq. HH income, lagged controls, K=6
SR effect of own income	0.191**	0.219**	0.171+	0.184	0.293+	0.320
	(0.088)	(0.105)	(0.124)	(0.159)	(0.216)	(0.338)
LR effect of own income	0.282***	0.293***	0.379***	0.399**	0.472**	0.386+
	(0.083)	(0.107)	(0.130)	(0.161)	(0.219)	(0.300)
Adaptation to own income	0.090	0.073	0.208+	0.216+	0.179	0.066
	(0.086)	(0.108)	(0.127)	(0.161)	(0.209)	(0.337)
SR effect of reference income	-0.310***	-0.306**	-0.214+	-0.164	-0.296+	-0.231
	(0.116)	(0.130)	(0.143)	(0.162)	(0.183)	(0.242)
LR effect of reference income	-0.364***	-0.382***	-0.381**	-0.267+	-0.549**	-0.232
	(0.120)	(0.146)	(0.168)	(0.196)	(0.236)	(0.324)
Adaptation to reference income	-0.054	-0.075	-0.167	-0.103	-0.253	-0.002
	(0.112)	(0.144)	(0.170)	(0.201)	(0.240)	(0.325)
SR effect of joint income	-0.118	-0.087	-0.043	0.020	-0.003	0.090
	(0.121)	(0.139)	(0.155)	(0.175)	(0.214)	(0.282)
LR effect of joint income	-0.082	-0.089	-0.003	0.132	-0.077	0.154
	(0.125)	(0.158)	(0.190)	(0.231)	(0.307)	(0.425)
Adaptation to joint income	0.036	-0.002	0.041	0.113	-0.074	0.064
	(0.112)	(0.149)	(0.181)	(0.215)	(0.269)	(0.391)
Kleibergen-Paap F statistic	347.843	122.179	60.323	29.530	13.913	3.416
Serial Correlation	-0.142	-0.165	-0.186	-0.213	-0.206	-0.114
Average T	5.236	4.962	4.763	4.465	4.234	5.732
Number of Persons	31370	25327	20443	16574	13018	5878
Number of Observations	164239	125662	97365	74011	55122	33692

Estimated with Correia's 'reghdfe' command.

Individual and year fixed-effects & (lagged) controls included.

Clustered standard errors in parentheses. + p<0.2, * p<0.10, ** p<0.050, *** p<0.010

Table A6: Extended results on household composition, W.Germany (1984-2015)

	(1) Instr.HH income contemp. controls, indust. & occup.	(2) Instr.HH income, contemp. controls, indust. & occup., lagged number of adults & children	(3) Instr.HH income, contemp. controls, indust. & occup., lagged HH composition	(4) Instr.HH income, lagged controls, indust. & occup., contemp. HH composition	(5) Instr.HH income, lagged controls, indust. & occup., contemp. number of adults & children
SR effect of own income	0.480***	0.423**	0.354**	0.474***	0.402***
	(0.123)	(0.169)	(0.161)	(0.124)	(0.121)
LR effect of own income	0.267***	0.385***	0.459***	0.270***	0.341***
	(0.093)	(0.105)	(0.116)	(0.092)	(0.097)
Adaptation to own income	-0.213***	-0.039	0.105	-0.204***	-0.061
	(0.069)	(0.148)	(0.152)	(0.072)	(0.074)
SR effect of reference income	-0.222*	-0.202*	-0.227*	-0.223*	-0.243*
	(0.122)	(0.122)	(0.121)	(0.128)	(0.128)
LR effect of reference income	-0.327***	-0.343***	-0.378***	-0.310**	-0.368***
	(0.118)	(0.119)	(0.119)	(0.129)	(0.129)
Adaptation to reference income	-0.105	-0.141	-0.151	-0.087	-0.124
	(0.119)	(0.121)	(0.121)	(0.150)	(0.150)
SR effect of joint income	0.258+	0.222	0.127	0.251+	0.159
	(0.167)	(0.198)	(0.191)	(0.167)	(0.165)
LR effect of joint income	-0.060	0.042	0.081	-0.040	-0.027
	(0.128)	(0.133)	(0.137)	(0.139)	(0.139)
Adaptation to joint income	-0.318**	-0.180	-0.046	-0.291*	-0.186
	(0.137)	(0.177)	(0.177)	(0.160)	(0.159)
Kleibergen-Paap F statistic	281.903	72.319	70.100	274.292	244.350
Serial Correlation	0.034	0.034	0.033	0.033	0.032
Average T	7.838	7.838	7.838	7.838	7.838
Number of Persons	23803	23803	23803	23803	23803
Number of Observations	186564	186564	186564	186564	186564

Estimated with Correia's 'reghdfe' command. Individual and year fixed-effects & (lagged) controls included.
Lags for number of adults and children include dummies for each variable and number.
Lags for household composition additionally include marital status, child birth and partner employment status.
Clustered standard errors in parentheses. + $p<0.2$, * $p<0.10$, ** $p<0.050$, *** $p<0.010$

Table A7: Extended results on household composition, UK (1996-2015)

	(1) Instr.HH income, contemp. controls, indust. & occup.	(2) Instr.HH income, contemp. controls, indust. & occup., lagged number of adults & children	(3) Instr.HH income, contemp. controls, indust. & occup., lagged HH composition	(4) Instr.HH income, lagged controls, indust. & occup., contemp. HH composition	(5) Instr.HH income, lagged controls, indust. & occup., contemp. number of adults & children
SR effect of own income	0.319***	0.238*	0.184+	0.314***	0.243**
	(0.104)	(0.126)	(0.125)	(0.104)	(0.106)
LR effect of own income	0.190*	0.311**	0.361***	0.195*	0.272***
	(0.099)	(0.125)	(0.127)	(0.100)	(0.101)
Adaptation to own income	-0.129**	0.074	0.176+	-0.119**	0.029
	(0.059)	(0.123)	(0.124)	(0.061)	(0.069)
SR effect of reference income	-0.289**	-0.269*	-0.265*	-0.268*	-0.261*
	(0.136)	(0.137)	(0.137)	(0.142)	(0.141)
LR effect of reference income	-0.303**	-0.390**	-0.391**	-0.300*	-0.319*
	(0.154)	(0.156)	(0.155)	(0.167)	(0.166)
Adaptation to reference income	-0.014	-0.121	-0.126	-0.033	-0.057
	(0.126)	(0.133)	(0.131)	(0.163)	(0.162)
SR effect of joint income	0.030	-0.031	-0.081	0.047	-0.018
	(0.147)	(0.156)	(0.155)	(0.149)	(0.149)
LR effect of joint income	-0.113	-0.079	-0.031	-0.105	-0.047
	(0.156)	(0.167)	(0.170)	(0.174)	(0.174)
Adaptation to joint income	-0.143	-0.047	0.050	-0.152	-0.028
	(0.131)	(0.148)	(0.150)	(0.166)	(0.168)
Kleibergen-Paap F statistic	160.627	60.054	62.249	161.706	154.513
Serial Correlation	-0.184	-0.184	-0.185	-0.185	-0.186
Average T	4.763	4.763	4.763	4.763	4.763
Number of Persons	20443	20443	20443	20443	20443
Number of Observations	97365	97365	97365	97365	97365

Estimated with Correia's 'reghdfe' command. Individual and year fixed-effects & (lagged) controls included.

Lags for number of adults and children include dummies for each variable and number.

Lags for household composition additionally include marital status, child birth and partner employment status.

Clustered standard errors in parentheses. + p<0.2, * p<0.10, ** p<0.050, *** p<0.010

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Table A8: Results for equation (2) using observed income, UK (1996-2015)

	(1) HH income, contemp. controls	(2) HH income, lagged controls	(3) Eq.HH income, contemp. controls	(4) Eq.HH income, lagged controls
SR effect of own income	0.083***	0.069***	0.075***	0.068***
	(0.017)	(0.017)	(0.017)	(0.017)
LR effect of own income	0.032+	0.108***	0.098***	0.111***
	(0.025)	(0.030)	(0.027)	(0.030)
Adaptation to own income	-0.051**	0.038+	0.023	0.042*
	(0.021)	(0.026)	(0.023)	(0.026)
SR effect of reference income	-0.201+	-0.181+	-0.207+	-0.181+
	(0.129)	(0.133)	(0.129)	(0.133)
LR effect of reference income	-0.240*	-0.312*	-0.279*	-0.313*
	(0.145)	(0.161)	(0.145)	(0.161)
Adaptation to reference income	-0.039	-0.130	-0.072	-0.132
	(0.121)	(0.159)	(0.122)	(0.159)
SR effect of joint income	-0.118	-0.112	-0.132	-0.113
	(0.129)	(0.133)	(0.129)	(0.133)
LR effect of joint income	-0.207+	-0.204	-0.180	-0.202
	(0.144)	(0.161)	(0.144)	(0.161)
Adaptation to joint income	-0.090	-0.092	-0.049	-0.089
	(0.122)	(0.160)	(0.122)	(0.160)
Serial Correlation	-0.174	-0.177	-0.174	-0.177
Average T	4.763	4.763	4.763	4.763
Number of Persons	20443	20443	20443	20443
Number of Observations	97365	97365	97365	97365

Estimated with Correia's 'reghdfe' command. Individual and year fixed-effects & (lagged) controls included.

Clustered standard errors in parentheses. + $p<0.2$, * $p<0.10$, ** $p<0.050$, *** $p<0.010$

Table A9: Results for home owners versus non-owners in W.Germany and UK

	(1) W.Germany (1996-2015): Instr.eq. HH income, lagged controls, indust. & occup., renting	(2) W.Germany (1996-2015): Instr.eq. HH income, lagged controls, indust. & occup., home owners	(3) UK (1996-2015): Instr.eq. HH income, lagged controls, indust. & occup., renting	(4) UK (1996-2015): Instr.eq. HH income, lagged controls, indust. & occup., home owners
SR effect of own income	0.326+	0.619*	-0.276	0.245+
	(0.224)	(0.356)	(0.334)	(0.150)
LR effect of own income	0.578***	0.134	-0.156	0.435***
	(0.184)	(0.186)	(0.469)	(0.147)
Adaptation to own income	0.252	-0.486+	0.120	0.190
	(0.198)	(0.339)	(0.365)	(0.148)
SR effect of reference income	-0.182	-0.167	-0.276	-0.280*
	(0.214)	(0.201)	(0.422)	(0.157)
LR effect of reference income	-0.567**	-0.221	-0.738+	-0.258+
	(0.225)	(0.202)	(0.541)	(0.186)
Adaptation to reference income	-0.385+	-0.053	-0.461	0.021
	(0.261)	(0.230)	(0.491)	(0.190)
SR effect of joint income	0.144	0.452+	-0.552	-0.034
	(0.294)	(0.343)	(0.437)	(0.172)
LR effect of joint income	0.011	-0.087	-0.893*	0.177
	(0.254)	(0.233)	(0.542)	(0.216)
Adaptation to joint income	-0.133	-0.539+	-0.341	0.211
	(0.303)	(0.351)	(0.516)	(0.203)
Kleibergen-Paap F statistic	40.652	21.564	20.597	43.314
Serial Correlation	-0.008	-0.007	-0.283	-0.187
Average T	6.733	7.156	3.981	4.775
Number of Persons	10924	10829	4776	16195
Number of Observations	73552	77494	19012	77339

Estimated with Correia's 'reghdfe' command. Individual and year fixed-effects & (lagged) controls included.

Clustered standard errors in parentheses. $+$ p$<$0.2, * p$<$0.10, ** p$<$0.050, *** p$<$0.010

Table A10: Short- and long-run equivalence scales for Germany

		Number of children						
		0	1	2	3	4	5	6
Number of Adults	1	1.000/ 1.000	0.910/ 1.097	0.862/ 1.158	0.829/ 1.203	0.804/ 1.239	0.784/ 1.270	0.768/ 1.296
	2	1.303/ 1.769	1.186/ 1.940	1.123/ 2.048	1.080/ 2.128	1.048/ 2.192	1.022/ 2.246	1.001/ 2.293
	3	1.522/ 2.470	1.385/ 2.709	1.311/ 2.859	1.261/ 2.971	1.223/ 3.061	1.193/ 3.136	1.169/ 3.201
	4	1.698/ 3.130	1.546/ 3.433	1.463/ 3.623	1.407/ 3.765	1.365/ 3.878	1.332/ 3.974	1.304/ 4.056
	5	1.850/ 3.761	1.684/ 4.124	1.594/ 4.353	1.533/ 4.524	1.487/ 4.660	1.451/ 4.775	1.421/ 4.874
	6	1.983/ 4.369	1.805/ 4.792	1.709/ 5.058	1.643/ 5.256	1.594/ 5.415	1.555/ 5.548	1.523/ 5.663

Calculated with parameter estimates from the specification of table 7, column 3.

First element of each cell refers to the short-run, second element refers to the long-run

Table A11: Short- and long-run equivalence scales for the UK

		Number of children						
		0	1	2	3	4	5	6
Number of Adults	1	1.000/ 1.000	0.958/ 1.211	0.934/ 1.354	0.917/ 1.466	0.905/ 1.559	0.894/ 1.639	0.886/ 1.710
	2	1.863/ 2.086	1.784/ 2.526	1.740/ 2.825	1.709/ 3.058	1.685/ 3.252	1.666/ 3.420	1.650/ 3.568
	3	2.681/ 3.208	2.568/ 3.884	2.504/ 4.343	2.459/ 4.702	2.425/ 5.000	2.398/ 5.258	2.375/ 5.487
	4	3.471/ 4.353	3.324/ 5.270	3.241/ 5.893	3.184/ 6.380	3.140/ 6.785	3.104/ 7.135	3.075/ 7.445
	5	4.241/ 5.516	4.061/ 6.678	3.960/ 7.468	3.890/ 8.084	3.836/ 8.598	3.793/ 9.041	3.757/ 9.434
	6	4.995/ 6.693	4.784/ 8.103	4.664/ 9.062	4.582/ 9.810	4.518/ 10.433	4.467/ 10.971	4.425/ 11.447

Calculated with parameter estimates from the specification of table 7, column 4.

First element of each cell refers to the short-run, second element refers to the long-run