Happiness and the Persistence of Income Shocks

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We reassess the empirical effects of income and employment on self-reported well-being. Our analysis makes use of a two-step estimation procedure that allows us to apply instrumental variable regressions with ordinal observable data. As suggested by the theory of incomplete markets, we differentiate between the effects of persistent and transitory income shocks. In line with this theory, we find that persistent shocks have a significant impact on happiness while transitory shocks do not. This also has consequences for inference about the happiness effect of employment. We find that employment per se is associated with a non-significant decline in happiness.

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Do individual economic conditions contribute to a person’s well-being or happiness? Frey and Stutzer (2002) and Clark et al. (2008) survey the large body of literature devoted to this question. For our purpose, we may summarize two main findings of this literature. First, in the cross-section, there is a small but significantly positive correlation between household income and self-reported well-being (happiness). Second, employment correlates positively with happiness beyond its effect on income. These findings, on the surface, challenge standard (macro-)economics, where, e.g., the costs of a recession are assumed to originate from the decline in consumption but such costs are partly offset by an increase in leisure as employment declines.

Our paper qualifies both findings summarized above, provides an interpretation of the findings in terms of incomplete insurance markets, and thereby contributes to the literature on happiness. We show that the contribution of income shocks to a person’s happiness depends crucially on the persistence of these shocks. Persistent shocks translate substantially into happiness, whereas transitory shocks do not. Taking this differential impact into account changes the inference about the contribution of employment to happiness. The evidence for a significantly positive effect of employment disappears.

At the same time, our paper contributes to the consumption-(self-)insurance literature. Not only do concepts developed in this literature lend themselves naturally to the analysis of happiness data, but also key results found in the consumption literature carry over. This is reassuring for the findings of the consumption literature; previous studies had to regularly rely on imputed consumption data since most household panels offer only limited coverage of consumption (mostly limited to food expenditures); see, e.g., Blundell et al. (2008). Happiness data, by contrast, are observed in the same panel as income. A second key advantage is that, unlike consumption, happiness is measured at the individual and not at the household level, a fact that we exploit when estimating the effect of individual employment and which opens up new possibilities for empirical research investigating consumption smoothing beyond the household level.

The starting point of our analysis is to relate survey answers on life-satisfaction to the intertemporal planning problem of an agent, where the agent’s lifetime utility corresponds to the discounted stream of expected future felicity. The reaction of both,

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1 See in particular Clark and Oswald (1994), Clark et al. (2001), and Clark (2003).
2 See, e.g., Deaton (1992), Blundell and Preston (1998), and Blundell et al. (2008).
(period) felicity and (lifetime) utility, will crucially depend on the persistence of income changes, whenever markets are incomplete. This allows us to link the size of the (within-person) happiness-income relation to how well agents can insure against shocks. Consider as an extreme case a model of complete markets. In this case, households face complete consumption insurance with respect to idiosyncratic income shocks irrespective of their persistence, so that the coefficient of a cross-sectional regression of happiness on income should be zero once the aggregate state is controlled for. The other extreme model of the world is a world where households do not have access to any storage and cannot trade any claims with each other (“autarky”). There, consumption equals income, and felicity responds equally to transitory and persistent income shocks. Lifetime utility, by contrast, does not, as persistent income changes also predict future felicity increases.

The modern (macro)economic literature has shown that when moving away from either of the two extreme assumptions – (insurance) markets are neither complete nor completely absent – the persistence of income shocks becomes important for the extent of consumption smoothing. As a consequence, the pass-through of income shocks on felicity and lifetime utility depends on the nature of income changes. The workhorse of heterogeneous-agent macroeconomics, the standard incomplete markets model (see Bewley (1980), Huggett (1993), and Aiyagari (1994)), assumes that households can only self-insure idiosyncratic labor market risk using state-uncontingent assets. Kaplan and Violante (2010), applying a method developed by Blundell et al. (2008), show that both in the standard incomplete markets model and in consumption data for the US, households are better able to smooth transitory than persistent shocks to their incomes.

We show that the differential impact of income shocks of different persistence has important consequences for happiness regressions, i.e. for regressions that relate life-satisfaction to income changes.

First, the coefficient in an ordinary regression of happiness on income is a weighted average of the coefficients of transitory and persistent income shocks. Second and con-

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3 Of course, when one takes labor risk into account, the marginal utility of consumption may depend on the amount of leisure and hence complete markets no longer necessarily imply equalization of consumption. In addition, equalization of well-being only holds conditional on leisure and only if the marginal utility of consumption is independent of leisure.

4 Parts of the happiness literature (e.g., when calculating income compensation for, say, airport noise; see van Praag and Baarsma (2005)) seem to start out from a felicity interpretation when interpreting the empirical results, calculating compensating income differentials.

5 The only paper we are aware of that argues for an insurance interpretation of the low income coefficient in a happiness regression is Dehejia et al. (2007).
sequently, this can introduce an (omitted variable) bias in the estimated coefficients of other variables as persistent and transitory income are latent variables. The estimated effect of employment on happiness is a candidate example for such biased estimate.

Consider a person who moves from employment to non-employment and that unemployment benefits expire after one year. This person experiences a negative persistent income shock (due to the drop in labor income) until she manages to move back into employment. During eligibility, unemployment benefits compensate for the immediate drop in income, yet only for 12 months, resulting in a positive transitory income shock.\footnote{Beyond this effect from benefits of limited duration, skill losses in unemployment (see, e.g., Arulampalam (2001)) and skill gains in employment also introduce systematic differences between the immediate and the long-term change in income through employment. Again, the immediate loss in income due to non-employment is smaller than the long-term loss.}

This change in the composition of income will lead to an additional drop in happiness (beyond the effect of declining income), if the permanent income change decreases happiness more than the transitory one increases it (as suggested by the theory of incomplete markets). This excess drop in happiness will then be attributed by an OLS regression to the change in employment, as this regression cannot differentiate between transitory and persistent income.

In fact, applying Blundell et al.’s (2008) framework to happiness data from the German Socio-Economic Panel (SOEP) we show: First, persistent income shocks translate significantly more strongly to happiness than do average income shocks (more than twice as much). Second, transitory shocks do not significantly contribute to happiness, i.e., are perfectly insured. Third, this leads to a strong upward bias in the coefficient estimate on employment in a happiness regression. When we control for the bias by differentiating between transitory and persistent shocks, the point estimate for the effect of employment on happiness turns from significantly positive to insignificantly negative.

The remainder of this paper is organized as follows: Section I develops the econometric model and methodology, Section II introduces the data set, Section III presents results, and Section IV relates our partial insurance results to some existing findings about the differentially strong effects of income and employment on happiness in different economic environments. Section V concludes, an appendix follows.

\section{Model and Methodology}

The economic analysis of self-reported well-being (here: life-satisfaction) data usually starts off with applying an ordered probit model to the data and then discussing the
effects of various (control) variables, most importantly income and employment status.\footnote{The literature has discussed a number of potential econometric problems in this setup, in particular those stemming from fixed effects in happiness and income, and has proposed solutions thereto; see, e.g., Frijters et al. (2004b).} We deviate from this tradition only in one, but as it turns out, important point that we borrow from the consumption/incomplete-markets literature. We consider a dynamic setup in which an agent faces an income process that is subject to persistent and transitory shocks. The agent self-insures by borrowing and lending in a risk-free asset.

We first elaborate on the challenges that such setup creates, both in estimating and interpreting the link of self-reported life-satisfaction to income. Then, we present empirical strategies to identify the effects of persistent and transitory shocks using an instrumental variable approach and discuss the conditions under which the proposed instruments will be valid. Finally, we extend the setup to include the effect of employment.

A. Persistence of Income, Intertemporal Planning, and Life-Satisfaction

To study the effects of income shocks of different persistence on life-satisfaction, consider an agent $i$ whose log income $y_{it}$ at time $t$ is composed of a fixed individual component $\mu_y$, a deterministic component, $g(z_{it})$, depending on observables, $z_{it}$, a transitory stochastic component $\psi_{it}$ and a persistent stochastic component $x_{it}$ such that

$$y_{it} = y^*_i + g(z_{it}),$$
$$y^*_i = x_{it} + \psi_{it} + \mu^y_i,$$
$$x_{it} = x_{it-1} + \epsilon_{it},$$

where $\psi_{it}$ and $\epsilon_{it}$ are i.i.d. shocks and $x_{it} = 0$ at the time of labor market entry.

The agent can use her income $y_{it}$ for consumption and savings in a non-contingent claim $a$ that pays dividend $r$. Assume that the agent has a flow utility, or “felicity”, function $u(c_{it})$ in consumption that is three-times continuously differentiable with $u' \geq 0$, $u'' \leq 0$ and $u''' \geq 0$. The agent seeks to maximize her lifetime utility over her remaining
S periods of life given her time-preference rate $\rho$:

$$U_{it} = \max E \sum_{s=0}^{S} \frac{1}{(1 + \rho)^s} u(c_{it+s}),$$

thus smoothing consumption by adjusting her asset holdings $a_{it+s+1}$ such that

$$c_{it+s} = y_{it+s} - a_{it+s+1} + (1 + r)a_{it+s}, \quad a_{it+s+1} > b,$$

where $b$ is an exogenous borrowing limit.

The recursive form of the planning problem is

$$U(x, \psi, a, S) = \max_{a'} \left\{ u \left[ y(x, \psi, S) - a' + (1 + r)a \right] + \frac{1}{1 + \rho} EU(x', \psi', a', S - 1) \right\},$$

$$U(x, \psi, a, 0) = u \left[ y(x, \psi, 0) + (1 + r)a \right].$$

First observe that the planning problem has typically two distinct stochastic state variables related to income, $(x, \psi)$. The persistent component, $x$, can be expected to have a larger impact on utility and felicity than the transitory one, $\psi$. Persistent changes in income are harder to smooth through self-insurance and, in addition, they are larger in present value terms as they last longer.

Second, and this complicates the mapping of survey answers on life-satisfaction to the choice problem, there are at least two potential theoretical objects that may be linked to an answer the agent gives to a survey question about her current well-being: her felicity, $u$, and her utility $U$.\footnote{If answers to such question relate at all immediately to the choice problem, see Kimball and Willis (2006).} Her felicity, $u$, and her utility $U$.\footnote{A third possibility could be that life-satisfaction relates to some backward-looking measure of past felicity.}

A priori it is not entirely clear whether the survey answer relates more closely to felicity or utility and linking the survey answer to just one concept is not entirely satisfactory. On the one hand, a respondent who expects high consumption growth in the future will likely respond more positively compared to a respondent who expects consumption to remain constant. At the same time, current events such as having had a bad/long work...
day, the weather etc. are known to feature too much into the individual’s response to the life-satisfaction survey question (see Schwarz and Strack, 1999) to be in line with a pure utility interpretation of the survey answer.\textsuperscript{10}

Importantly, however, the question which of the objects (felicity or utility) is more closely related to the survey answer is only relevant for the interpretation of estimation results. The econometric treatment, and with it the fundamental challenge to identification, are unaffected by this. As we will show below, the identification strategy purely relies on the time-series properties of the state variables, which are the same under either interpretation. Hence, the identifying assumptions hold for one interpretation whenever they hold for the other. For our empirical approach, we can therefore introduce a latent life-satisfaction function, \( v \), which corresponds to the survey answer on life-satisfaction. This function is a convex combination of felicity and utility, \( v := \lambda U + (1 - \lambda) u \).

For simplicity, we assume that this latent life-satisfaction function is additively separable in observable characteristics, \( z_{it} \), a person fixed effect, \( \mu_v^i \), and an i.i.d. term \( \xi_{it} \) such that the answers to the survey question on life-satisfaction are generated by an ordered Probit model for:

\begin{align}
(5) & \quad v_{it} = v^*_i + f(z_{it}). \\
(6) & \quad v^*_i = v^{**}(x_{it}, \psi_{it}, a_{it}) + \mu_v^i + \xi_{it},
\end{align}

where \( \xi \) captures i.i.d. influences on the survey answer (like time of day, weekday, weather, or measurement error).

We are interested in estimating the marginal effects of the income state variables on \( v^{**} \). The effects of observables \( f(z_{it}) \) are removed in a first-stage reduced-form regression. This regression also equips us with estimates for the latent \( v^*_i \) as residuals, see Section I.E. From these estimates, fixed effects can be removed by first differences. We then

\textsuperscript{10}In particular, the fact that most other survey questions in the SOEP data refer to the previous year makes it likely that respondents overweight the more recent felicity, as would be suggested by the findings summarized in Schwarz and Strack (1999).
apply a first-order approximation to the latent life-satisfaction function $\Delta v^*(x_{it}, \psi_{it}, a_{it})$:

$$\Delta v_{it}^* = \frac{\partial v^{**}}{\partial x} \Delta x_{it} + \frac{\partial v^{**}}{\partial \psi} \Delta \psi_{it} + \frac{\partial v^{**}}{\partial a} \Delta a_{it} + \Delta \xi_{it}$$

(7) $$\Delta v_{it}^* = \alpha_x \Delta x_{it} + \alpha_\psi \Delta \psi_{it} + r_{it} := \alpha_a \Delta a_{it} + \Delta \xi_{it}.$$  

The joint residual $r_{it}$ is composed of the original error term $\Delta \xi_{it}$ and the effect of assets, $\alpha_a = \frac{\partial v^{**}}{\partial a}$, which cannot be estimated in the absence of asset data. Beyond the narrow interpretation of $a$ as assets as in the planning problem (4), we can read $a$ as a catch-all for history dependence, e.g. through adaptation, backward-lookingness, or habits.\(^{11}\)

### B. Moment Conditions

Equation (7) cannot be directly estimated for two reasons. First, the latent level of life-satisfaction $v_{it}^*$ is not directly observable, and second, we do not separately observe the persistent and the transitory income components, but only observe income $y_{it}$ or residual income $y_{it}^*$ after controlling for observables. Assume for the moment that $v_{it}^*$ was observed – we come back to this in Section I.E.

To derive estimation equations for the effects of permanent and transitory income shocks on life-satisfaction, take first-differences of the residual income process (1), $\Delta y_{it}^* = \Delta x_{it} + \Delta \psi_{it}$, and replace the respective component in (7) by either $\Delta x_{it} = \Delta y_{it}^* - \Delta \psi_{it}$ or $\Delta \psi_{it} = \Delta y_{it}^* - \Delta x_{it}$, such that

(8) $$\Delta v_{it}^* = \alpha_x [\Delta y_{it}^* - \Delta \psi_{it}] + \alpha_\psi \Delta \psi_{it} + r_{it} = \alpha_x \Delta y_{it}^* + \left(\alpha_\psi - \alpha_x\right) \Delta \psi_{it} + r_{it},$$  

combined residual I

(9) $$\Delta v_{it}^* = \alpha_x \Delta x_{it} + \alpha_\psi [\Delta y_{it}^* - \Delta x_{it}] + r_{it} = \alpha_\psi \Delta y_{it}^* + \left(\alpha_x - \alpha_\psi\right) \Delta x_{it} + r_{it}.$$  

combined residual II

Both equations, (8) and (9), describe the same regression. The identification of the income sensitivities $\alpha_x$ and $\alpha_\psi$ depends on finding instruments that are orthogonal to

\(^{11}\)For the special where $\lambda = 0$ and $u$ is logarithmic in consumption this framework is obviously identical to Blundell et al. (2008) or Kaplan and Violante (2010), who estimate the response of log-consumption to persistent and transitory income shocks. These papers also show that a log-linear approximation of the consumption policy function is relatively precise.
either one or the other of the combined residuals. Blundell et al. (2008) and Kaplan and Violante (2010) propose such instruments as \( y_{it+1}^* - y_{it-2}^* \) and \( \Delta y_{it+1}^* \), respectively.\(^{12}\) For identification, these instruments exploit the assumed time-series properties of \( \Delta \psi \) and \( \Delta x \) and additionally the following two assumptions:

\[
\begin{align*}
(\text{No Foresight}) & \quad E(\epsilon_{it+1} r_{it}) = E(\psi_{it+1} r_{it}) = E(\psi_{it} r_{it}) = 0, \\
(\text{Short Memory}) & \quad E(\epsilon_{it-1} r_{it}) = E(\psi_{it-2} r_{it}) = 0.
\end{align*}
\]

“No Foresight” implies that current shocks to life-satisfaction \( r_{it} \) do not predict future income growth. Under this assumption, we can identify \( \alpha_\psi \) from the moment condition

\[
\left( \Delta v_{it}^* - \alpha_\psi \Delta y_{it}^* \right) \Delta y_{it+1}^* = 0.
\]

thus using \( \Delta y_{it+1}^* \) as an instrument for \( \Delta y_{it}^* \) in (9) (see Appendix A for a derivation).

With the additional “Short Memory” assumption, i.e. past income shocks do not predict life-satisfaction growth, we can estimate \( \alpha_x \) from (8) from the moment condition

\[
\left( \Delta v_{it}^* - \alpha_x \Delta y_{it}^* \right) \left( y_{it+1}^* - y_{it-2}^* \right) = 0.
\]

Kaplan and Violante (2010) discuss in detail the identifying assumptions introduced above for consumption choice in a model of incomplete markets. As in their setup, the “No Foresight” condition holds whenever the individual has no better information on income growth (in \( t + 1 \)) than the econometrician.

The “Short Memory” assumption requires that \( \Delta a_{it} \) (narrowly understood: change in assets) at time \( t \) does neither respond to persistent shocks at time \( t - 1 \) nor to transitory shocks at time \( t - 2 \), as the change in assets is part of the combined residual in (7). This holds strictly in models where the permanent-income hypothesis holds.\(^{13}\) There, assets follow a random walk driven by transitory shocks and consumption moves one-to-one

\(^{12}\)These papers look at a setup analogous to ours where (imputed) consumption is the left-hand-side variable (instead of life-satisfaction) and income follows the same process as in (1).
\(^{13}\)Sufficient conditions are \( S = \infty, b = -\infty, u(c) = c^2 \) and \( (1 + r) \beta = 1 \), or \( u(c) = -\sigma^{-1} \exp(-\sigma c) \) and income shocks being in levels not in logs, see Wang (2003).
with persistent income.

On the other hand, the moment restrictions will not hold strictly when the asset process is mean reverting, e.g., when generated by a model with borrowing constraints and CRRA felicity function. Changes in assets $\Delta a_{it}$ co-vary negatively both with transitory shocks in $t - 2$ and with persistent shocks in $t - 1$. When an agent receives a positive transitory income shock in $t - 2$, she is going to save part of it in $t - 2$ and subsequently consume a fraction of these extra savings every period. Hence, a positive transitory shock in $t - 2$ predicts declining assets in period $t$. A persistent shock in period $t - 1$ changes consumption roughly equal to the time preference rate of the agent times the increase in human capital, $\frac{\partial}{\partial x} E \left[ \sum_{s=0}^{S} (1 + r)^{-s} y^*(x_s, \psi_s) \right]$. If $\rho > r$ this is larger than just what the dividend payment on the human-capital increase would be, which in turn is approximately the current income increase $\Delta y^*(x, \psi)$. In other words, the agent decreases assets $a_{it}$, preponing consumption in light of a positive persistent income shock.

In both cases, the “Short Memory” condition will not hold exactly. Since $\psi_{it-2}$ and $\epsilon_{t-1}$ show up with an opposite sign in the moment restriction (see (11)), the direction and size of the bias depends on the relative importance of both shocks, the strength of the savings reaction to either shock, and the speed of mean reversion in assets.

Importantly, when the “Short Memory” condition holds, it holds independent of the exact relation of life-satisfaction to felicity or utility, i.e. independent of $\lambda$. When this condition does not hold, it is a quantitative question how strong the bias in the IV estimates can expected to be. Kaplan and Violante (2010) provide a detailed analysis and find at least for consumption demand that the bias is acceptably small. We extended their simulation analysis to check the quality of the moment conditions in our setting involving felicity or utility growth instead of consumption, i.e. for a numerical version of (4). In Appendix C we show the bias to be negligible in our setting, too.

C. Improving the Estimation Equations: Lagged Life-Satisfaction

Even though we find the “Short Memory” assumption to hold approximately in the numerical simulations presented in Appendix C, this assumption might still be problematic in actual data if assets or life-satisfaction data in general show mean-reverting behavior.
Such mean reversion could for example result from adaptation or consumption habits. Both produce stronger predictable changes in life-satisfaction than asset accumulation/decumulation in a standard incomplete markets model. We therefore provide a strategy to robustify our empirical method against excess mean reversion in life-satisfaction.

To fix ideas, decompose changes in life-satisfaction into surprise changes and predicted ones:

$$\Delta v^*_{it} = v^*_{it} - E_{t-1}v^*_{it} + E_{t-1}(\Delta v^*_{it}).$$

$E_{t-1}(\Delta v^*_{it})$ are predictable changes in life-satisfaction, while $v^*_{it} - E_{t-1}v^*_{it}$ are surprise changes in life-satisfaction. The potentially problematic “Short Memory” assumption means that the only predicted changes come from reverting past transitory income shocks $\psi_{it-1}$ and past error terms $\xi_{it-1}$, both of which are serially uncorrelated but enter (7) over-differenced. Conversely, this assumption is violated if other (state) variables predict changes in life-satisfaction.

This suggests we can robustify the estimation against such violations by proxying for further predictable changes in life-satisfaction, e.g. from adaptation. For this purpose, we augment our estimation equation by $\tau v^*_{it-1}$ as predictor of life-satisfaction growth, so that:

$$\Delta v^*_{it} = \alpha_x \Delta x_{it} + \alpha_\psi \Delta \psi_{it} + \tau v^*_{it-1} + \Delta \xi_{it}. \quad (12)$$

We estimate the parameters $\alpha_x, \alpha_\psi$ and $\tau$ from the following sets of moment conditions:

$$E \left[ (\Delta v^*_{it} - \tau v^*_{it-1} - \alpha_x \Delta y^*_{it}) (y^*_{it+1} - y^*_{it-2}) \right] = 0, \quad (13a)$$
$$E \left[ (\Delta v^*_{it} - \tau v^*_{it-1} - \alpha_x \Delta y^*_{it}) v^*_{it-2} \right] = 0,$$
$$E \left[ (\Delta v^*_{it} - \tau v^*_{it-1} - \alpha_\psi \Delta y^*_{it}) \Delta y^*_{it+1} \right] = 0, \quad (13b)$$
$$E \left[ (\Delta v^*_{it} - \tau v^*_{it-1} - \alpha_\psi \Delta y^*_{it}) v^*_{it-2} \right] = 0.$$

Understanding life-satisfaction data as measuring elation (i.e. news about felicity) or accumulated past felicity are further potentially challenging concepts. The former does not square well with the high persistence of life-satisfaction we find in the data, the latter squares little with the evidence on the reasoning of survey respondents as presented in Ross et al. (1986).
i.e. we assume that “No Foresight” holds and that the “Short Memory” condition holds conditional on past life-satisfaction. This makes the latter assumption substantially weaker. We need to instrument $v_{it-1}^*$ by $v_{it-2}^*$ due to the over-differenced terms $(\Delta\psi_{it}, \Delta\xi_{it})$. In Appendix C, we show that our procedure is successful in removing potential estimation biases arising from excess mean reversion in life-satisfaction, irrespective of life-satisfaction corresponding to utility or to felicity.

D. Employment

Many empirical studies on life-satisfaction include other variables than income in the regressions with a structural interpretation in mind. Most prominent is estimating the effect of employment on life-satisfaction. Augmenting equations (8) and (9) by an effect $\gamma$ of a change in employment, $\Delta n_{it}$, we see that a least-squares estimate of $\gamma$ will be biased under the instrumentation for transitory and persistent shocks, respectively, if

\begin{align}
\text{cov}(\Delta n_{it}, (\alpha_{\psi} - \alpha_x)\Delta\psi_{it} + r_{it}) &\neq 0, \\
\text{cov}(\Delta n_{it}, (\alpha_x - \alpha_{\psi})\epsilon_{it} + r_{it}) &\neq 0,
\end{align}

because, when transitory and persistent shocks are latent, this necessarily introduces an omitted variable into the regressions.

Recall that $\epsilon_{it}$ and $\Delta\psi_{it}$ measure the persistent and transitory changes in income, respectively. If employment drops, then income permanently drops until employment returns to its initial level. At the same time, if there are unemployment benefits or short-time work benefits that expire after a period, the initial drop in income is smaller than the long-term drop in income (keeping hours worked at the now lower level), hence $\Delta\psi > 0$ when $\Delta n < 0$. Therefore, $\text{cov}(\Delta n_{it}, \epsilon_{it}) > 0$ and $\text{cov}(\Delta n_{it}, \Delta\psi_{it}) < 0$.\(^\text{15}\)

Importantly, these likely correlations produce an upward bias of the least-squares estimate of $\gamma$ in any specification that does not instrument employment change if persistent income shocks translate more strongly into life-satisfaction than do transitory ones,\(^\text{15}\)Asset accumulation might introduce an additional, but likely small, correlation between $r_{it}$ and employment changes, which becomes exactly zero in the case of GHH preferences.
\( \alpha_x > \alpha_\psi \). This holds as long as markets are not complete, i.e. as long as the persistence of income shocks is important to the agent.

To find an instrument for employment change, we can assume that employment exhibits serial autocorrelation, such that

\[
 n_{it} = \rho_h n_{it-1} + \omega_{it}; \quad \rho_h < 1,
\]

so that we can identify \( \gamma \) from the moment conditions:\(^{16}\)

\[
 E \left[ (\Delta v^*_it - \gamma \Delta n_{it} - \alpha_x \Delta y^*_it) n_{it-2} \right] = 0 \\
 or \; E \left[ (\Delta v^*_it - \gamma \Delta n_{it} - \alpha_\psi \Delta y^*_it) n_{it-2} \right] = 0,
\]

using the other moment conditions (10) and (11) to identify \( \alpha_\psi \) and \( \alpha_x \). In addition to the “No Foresight” and “Short Memory” assumptions, we need to assume

(Limited Tenure Effects) \( E(n_{it-j} \epsilon_{it}) = E(n_{it-j} \Delta \psi_{it}) = 0 \) if \( j \geq 2 \).

One can understand these conditions as particular versions of the “No Foresight” condition. Employment in \( t-2 \) neither changes the persistent income shock in period \( t \) nor the transitory shocks in periods \( t-1 \) and \( t \). If the “Limited Tenure Effects” condition is violated, this means that past employment predicts current income growth; hence, the agent has some foresight about income growth. For example, if there is sluggish learning on the job or skill losses in unemployment, the condition \( E(n_{it-j} \epsilon_{it}) = 0 \) might be violated. Similarly, if the size of unemployment benefits in \( t \) depends on employment histories beyond \( t-1 \), the condition \( E(n_{it-j} \Delta \psi_{it}) = 0 \) can be violated.\(^{17}\)

Since there is evidence for skill losses in unemployment and skill gains in employment,\(^{18}\) we take it to be more likely that the condition \( E(n_{it-j} \epsilon_{it}) = 0 \) is violated, which means that it is potentially problematic to identify the effects of transitory income shocks along

\(^{16}\)Technically, we also need to assume that innovations to hours \( \omega_{it} \) are not perfectly correlated with income shocks, such that there is independent variation in hours.

\(^{17}\)The duration of unemployment benefits in Germany depends on the length of the previous employment spell and increases to a maximum of 12 months (for those under 50 years old) after 24 month of employment. Hence, there might be some correlation, which, however, should vanish if using \( n_{it-3} \) as an instrument instead of \( n_{it-2} \); yet this implies losing additional observations. In the Appendix, we provide results for this specification.

\(^{18}\)See Jacobson et al. (1993), Neal (1995), and Couch and Placzek (2010).
with employment. We therefore focus on identification of the employment effect from an IV-regression that identifies the pass-through of permanent shocks, \( \alpha_x \), using the moment conditions:\(^{19}\)

\[
\begin{align*}
E[(\Delta v_{it}^* - \alpha_x \Delta y_{it}^* - \gamma \Delta n_{it}) n_{it-2}] &= 0, \\
E[(\Delta v_{it}^* - \alpha_x \Delta y_{it}^* - \gamma \Delta n_{it})(y_{it+1}^* - y_{it-2}^*)] &= 0.
\end{align*}
\]

**E. Constructing Latent Felicity from Observed Happiness**

So far, we have established an instrumental variable regression to estimate the effects of persistent and transitory income shocks and employment on latent utility, assuming this latent utility is observable. While latent utility is not observable, we do observe self-reported life-satisfaction in the data we use. This variable is reported on a scale of 0 to 10. We assume that this happiness variable is generated from an ordered probit model, where happiness \( h_{it} \) is determined by

\[
(18) \quad h_{it} = j \text{ if } v_{it}^{**} \in (\tilde{c}_j, \tilde{c}_{j+1}]
\]

The latent \( v_{it}^{**} \) is determined as in (5) and all error terms \( \mu_i, \mu_i^u, \epsilon_{it}, \psi_{it}, \xi_{it} \) are assumed to be normally distributed. Moreover, \( v_{it}^{**} \) is scaled such that \( v_{it}^* \) has unit variance.

Under these assumptions we can estimate the cutoff values \( \tilde{c}_j \) and the statistical (not necessary causal) effect of controls \( f(z_{it}) \) by a standard ordered probit estimator. Note that we should not give causal interpretation to these estimates, since they will also include correlations of controls with fixed effects and income shocks.\(^{20}\) The cutoff values are scaled appropriately to be compatible with \( v_{it}^* \) having a unit variance. Since we obtain an estimate \( \tilde{f}(z_{it}) \) for each household-year, we can infer an interval \( V_{it} = (\tilde{c}_{h_{it}}, \tilde{c}_{h_{it}+1} - \tilde{f}(z_{it})) \) in which \( v_{it}^* \) must have been fallen. Together with the normality

\(^{19}\)Results for \( \gamma \) using the alternative moment condition that identifies \( \alpha_{\psi} \) are qualitatively similar.

\(^{20}\)Frijters et al. (2004b) suggest an estimator to obtain consistent estimates in the presence of fixed effects for the ordered probit setup. We do not employ their estimator in our first-stage regression since we are not interested in obtaining structural estimates in this first stage. While their estimator is more efficient in the presence of fixed effects than our estimation procedure, the advantage of the latter is that it is easily extended to the IV regressions we need to do.
assumption for $v_{it}^*$ this means we can calculate the conditional expected value $\overline{v}_{it}^*$ for residual latent life satisfaction

\begin{equation}
\overline{v}_{it}^* = \frac{\int_{v \in V_{it}} v \phi(v)}{\Phi(V_{it})},
\end{equation}

where $\phi$ is the density and $\Phi(V)$ the probability of $V$ for a standard normal distribution.

Replacing $v_{it}^*$ with $\overline{v}_{it}^*$ in the estimation equations derived in Section I.A renders the previously derived estimators feasible. It introduces measurement error, but only to the dependent variable, which does not bias estimations. The huge advantage of this procedure is that we can apply standard linear regression techniques once $\overline{v}_{it}^*$ is estimated, and hence, we can, e.g., use first differences to control for fixed effects. It can be understood as a generalization of van Praag’s (2004) probit-OLS procedure; see also van Praag and Ferrer-i Carbonell (2006). In contrast to the pure probit-OLS procedure, we do not need to assume normality for $f(z_{it})$ in the first-stage ordered probit regression.

II. Data

We use data on subjective well-being from the German annual socio-economic panel (SOEP). The SOEP is a representative longitudinal study of households and individuals and covers information on household composition, employment, incomes, health and satisfaction indicators. Our analysis uses data from 1984-2010. In the baseline specification, we restrict the sample to household heads and spouses between 25-55 years of age, consider West German households only, and drop observations from the migrant and high-income samples. In an alternative specification, we split the sample by gender. To control for outliers, we drop those households that fall in the top-bottom 0.25 percentiles of residual incomes from a first stage regression (see below) in each year. We then re-estimate the first-stage income regression for the cleaned sample. Table 1 shows summary statistics of the variables used in the final estimation sample. We provide further information on the data in Appendix B.

Individual life-satisfaction is measured on an integer scale from 0 to 10.\textsuperscript{21} To measure income, we use post-government income in real terms, which represents the combined

\textsuperscript{21}The survey question is: “How satisfied are you with your life, all things considered?” 0 means completely dissatisfied, 10 means completely satisfied.
Table 1—Descriptive Statistics

<table>
<thead>
<tr>
<th>Variable</th>
<th>Mean</th>
<th>Std. Dev.</th>
<th>Min</th>
<th>Max</th>
</tr>
</thead>
<tbody>
<tr>
<td>Life-Satisfaction (0-10)</td>
<td>7.14</td>
<td>1.74</td>
<td>0</td>
<td>10</td>
</tr>
<tr>
<td>Income (in logs)</td>
<td>10.44</td>
<td>0.50</td>
<td>7.10</td>
<td>12.64</td>
</tr>
<tr>
<td>Age (in years)</td>
<td>40.84</td>
<td>8.29</td>
<td>25</td>
<td>55</td>
</tr>
<tr>
<td>Household size</td>
<td>3.03</td>
<td>1.25</td>
<td>1</td>
<td>6</td>
</tr>
<tr>
<td>No. of children</td>
<td>0.88</td>
<td>1.01</td>
<td>0</td>
<td>4</td>
</tr>
<tr>
<td>Schooling (semesters)</td>
<td>24.10</td>
<td>5.19</td>
<td>14</td>
<td>36</td>
</tr>
<tr>
<td>Satisfaction with health (0-10)</td>
<td>6.94</td>
<td>2.12</td>
<td>0</td>
<td>10</td>
</tr>
</tbody>
</table>

Fraction of Respondents who are ...

<p>| | |</p>
<table>
<thead>
<tr>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>Living with a spouse</td>
<td>84%</td>
</tr>
<tr>
<td>Female</td>
<td>51%</td>
</tr>
<tr>
<td>Employed</td>
<td>80%</td>
</tr>
<tr>
<td>Disabled</td>
<td>6%</td>
</tr>
</tbody>
</table>

income after taxes and government transfers in the previous year of all individuals in the household. In our baseline specification, we estimate the life-satisfaction effect of employment by coding a dummy variable $e_{it}$ that defines a person as being employed if supplying more than 520 hours of market work per year, being equivalent to more than a quarter of full-time employment. Alternatively, we use a broader definition, where persons having positive wages and working at least 52 hours are classified as employed, or estimate the effect log hours-worked for those persons who supply positive hours.

We define employment on the basis of hours worked, categorizing respondents into two labor-market states employed/non-employed instead of using a multi-state labor force status. We do so to avoid the difficulties involved in interpreting the various labor market transitions a multi-state labor force status brings. For example, in our data 50% of out- and in-flows from/to employment involve “non-participation” when using a multi-state measure. So it is hard to argue that all of these persons are indeed not participating in the labor market at least in some form. Importantly, the binary employment measure is closer to a standard formulation of preferences–orderings over bundles of consumption and leisure (we also consider a specification with hours worked). Of course, one needs
to take into account in the interpretation of our results that some of the non-working persons may be voluntarily unemployed, while others are not, such that our results may mask some underlying heterogeneity. Yet, when a person can be classified as an obvious non-participant, she should be dropped from the analysis. Therefore, we drop individuals who are on maternity leave, in school, or in the military.

As a robustness check, we also provide results using data from the British Household Panel Survey (BHPS). Since the BHPS includes only household gross income variables, but not household net income, we extend the data by the estimates for net annual household incomes provided by Jenkins (2011). Appendix B provides further details.

III. Estimation Results

The first step of our analysis is to regress household incomes on a large set of control variables \(z_{it}\), i.e., to estimate (1). The controls include year-dummies, dummies for each year of schooling, dummies for age, for marital status, for living with a spouse, for the number of children, for the various levels of self-reported health status, number of hospital days (in 6 groups), for disability and interaction terms of schooling coded in 5 levels with a second order age polynomial. We include information on both spouses in cases where a household is composed of more than one adult. We use the same set of variables for our first step ordered probit regression of happiness, i.e., to estimate (5). This gives us estimates of \(v_{it}^*\) and \(y_{it}^*\) as defined in Section I. Also for employment \(e_{it}\), we condition out the effect of observables \(z_{it}\).

A. Happiness, Income, and Employment

We then use these data to estimate the effect of income and employment on happiness, i.e., we regress \(u_{it}^*\) on \(y_{it}^*\) and \(e_{it}\). Table 2 summarizes the main results of this exercise. While the simple OLS regression (Column i) suggests some significant positive effect of income on happiness (a 40% increase in income has roughly the same effect as living with a spouse), this coefficient drops significantly when using first differences to control for
Table 2—Life-Satisfaction, Income, and Employment

<table>
<thead>
<tr>
<th>Estimation Method</th>
<th>i</th>
<th>ii</th>
<th>iii</th>
<th>iv</th>
<th>v</th>
<th>vi</th>
<th>vii</th>
<th>viii</th>
</tr>
</thead>
<tbody>
<tr>
<td>Moment</td>
<td>OLS</td>
<td>FD</td>
<td>FD</td>
<td>IV</td>
<td>IV</td>
<td>IV</td>
<td>IV</td>
<td>IV</td>
</tr>
<tr>
<td>Restriction(s)</td>
<td>&amp; wealth &amp; wealth</td>
<td>y_it ((\alpha))</td>
<td>0.32</td>
<td>0.24</td>
<td>0.23</td>
<td>0.07</td>
<td>0.07</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.02)</td>
<td>(0.02)</td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.05)</td>
<td>(0.05)</td>
<td></td>
</tr>
<tr>
<td>Income Shocks</td>
<td>- transitory, (\psi_{it} (\alpha_{\psi}))</td>
<td>0.07</td>
<td>0.45</td>
<td>0.49</td>
<td>0.50</td>
<td>0.47</td>
<td>0.47</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.07)</td>
<td>(0.09)</td>
<td>(0.11)</td>
<td>(0.07)</td>
<td>(0.07)</td>
<td>(0.07)</td>
<td>(0.07)</td>
<td></td>
</tr>
<tr>
<td></td>
<td>- persistent, (x_{it} (\alpha_{x}))</td>
<td>0.45</td>
<td>0.49</td>
<td>0.50</td>
<td>0.47</td>
<td>0.47</td>
<td>0.47</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.07)</td>
<td>(0.09)</td>
<td>(0.11)</td>
<td>(0.07)</td>
<td>(0.07)</td>
<td>(0.07)</td>
<td>(0.07)</td>
<td></td>
</tr>
<tr>
<td>Employment, (e_{it} (\gamma))</td>
<td>0.06</td>
<td>-0.11</td>
<td>-0.14</td>
<td>-0.15</td>
<td>-0.15</td>
<td>-0.15</td>
<td>-0.15</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.15)</td>
<td>(0.17)</td>
<td>(0.09)</td>
<td>(0.09)</td>
<td>(0.09)</td>
<td>(0.09)</td>
<td></td>
</tr>
<tr>
<td>Persist. income (x) wealth</td>
<td>-0.15</td>
<td>-0.15</td>
<td>-0.15</td>
<td>-0.15</td>
<td>-0.15</td>
<td>-0.15</td>
<td>-0.15</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(0.09)</td>
<td>(0.09)</td>
<td>(0.09)</td>
<td>(0.09)</td>
<td>(0.09)</td>
<td>(0.09)</td>
<td>(0.09)</td>
<td></td>
</tr>
</tbody>
</table>

Note: Standard errors are in parenthesis. OLS refers to an OLS estimation of \(u_{it}^{*}\) on \(y_{it}^{*}\), FD to the same regression using first-differences to control for fixed household effects; in Column iii we include an employment dummy \(e_{it}\) as an additional regressor. The IV-regressions in Columns iv - viii refer to the method of moments estimators for transitory and persistent shocks discussed in Section I, with the moment restrictions given in the equations referred to in the third row of the table. The IV-regressions all control for fixed effects by first-differencing. Column vii augments the regression from vi by an interaction of persistent income shocks and wealth. Column viii refers to a regression where we first condition out lagged happiness from happiness growth. Both the income and employment variables have been regressed on the same set of controls we included in the first-stage ordered probit regression for happiness. Residuals from these regressions are used as regressors.

This finding is in line with what other researchers have found (see Ferrer-i Carbonell and Frijters (2004)): there are households whose members are both permanently more happy and permanently earn more. Since these differences are fixed, we cannot identify what causes what and causation may go either way. A household may permanently earn more because its members are permanently happy or may be permanently more happy because its members permanently earn more.

The instrumental variable regressions in Table 2 show that, in fact, persistent shocks influence happiness more strongly than transitory ones (Columns iv and v). When instrumenting in order to identify the effect from permanent income shocks (Column v), the income coefficient is 0.45 and hence twice as large as for the average income shock (Column 22Table 10 in Appendix D shows that the coefficient estimates from the OLS estimator are not significantly different from a one-step ordered probit estimation including not only the controls but also income.

22
ii), while transitory income shocks have no (significant) impact on happiness (Column iv). Note that our IV-regressions still control for fixed effects by first-differencing.

Since we find a strong difference in the effects of transitory and persistent income shocks on happiness, our theoretical considerations from Section I suggest an upward biased estimate of the effect of employment on happiness. In fact, this is the case, as a comparison of Columns iii and vi in Table 2 reveals, which present the estimates from regressions augmented by employment. In Column vi employment is instrumented as discussed in Section I.D.

A naive interpretation of the OLS estimate in first differences (Column iii) suggests that a household suffers from losing employment just as much as from a $\frac{\hat{\gamma}}{\hat{\alpha}} \approx 30\%$ extra decline in income beyond the one caused by employment loss. In other words, a non-employed person would be indifferent between working and earning 70% of the unemployment benefits or not working and earning full unemployment benefits. Yet, as our IV procedure (in Column vi) shows, this finding is just an artifact of not controlling for the correlation of non-employment spells with permanent and transitory income shocks. Once we do so, the effect of employment on happiness becomes negative but insignificant. The point estimate suggests that a household needs to be compensated permanently by an income exceeding unemployment benefits by $-\frac{\hat{\gamma}}{\hat{\alpha}_x} \approx 20\%$ (Column vi) in order to be indifferent between working and not working. Of course, one needs to be careful in interpreting this number since there is much estimation uncertainty with an insignificant estimate for $\gamma$.

This caveat arises from the need to instrument employment change by past employment; see (17). For our dummy variable approach to employment, this means that identification comes from comparing the growth in happiness of persons who are employed with those who are not employed in period $t - 2$, controlling for income and characteristics. The idea behind this is that those who work in $t - 2$ lose their job with some probability, while those who do not work pick up work with some probability. Since we control for all other characteristics and income, the difference in the growth of happiness of the two groups must be due to changes in employment. Of course, this is a fairly indirect identification, which is reflected in the wide confidence bounds.

Column vii of Table 2 shows the results for an estimation where we interact the
persistent income shock and wealth. In the 2002 and 2007 surveys, households were asked about their wealth, and we use this information to impute wealth in all other years by making use of the individual’s characteristics $z_{it}$ and income two years earlier. While we cannot interpret the direct effect of wealth, the imputation summarizes household characteristics according to their wealth prediction, and the IV-regression then asks whether the pass-through of persistent shocks systematically varies with these characteristics. We find that the higher the imputed wealth, the lower the pass-through of shocks $\alpha_x$. For the income-consumption pass-through, this is one of the key predictions of the permanent income hypothesis with finite life; see Blundell et al. (2008).

Column viii in Table 2 repeats the estimation from Column v but first eliminates the growth in happiness that is predictable from past happiness as described in Section I.C. The point estimate for the pass-through of persistent income shocks increases slightly.

B. Robustness

Next, we perform several robustness checks for our findings. First, we relax the random-walk assumption for income and assume a lower-bound estimate of $\rho = 0.9$ in line with what Bayer and Juessen (2012) report as an estimate from SOEP data and construct pseudo-differences of $\overline{v}_{it}$, $y_{it}^*$, and $e_{it}$. Results are shown in Table 3. We instrument with the instruments for income suggested in Kaplan and Violante (2010), i.e., with $y_{it+1}^* - \rho y_{it}$ in Column i and by $y_{it+1} - \rho^3 y_{it-2}$ in Columns ii and iii (and employment change with $e_{t-2}$). Using pseudo-differences yields basically the same picture as under the unit-root assumption for income.

As a second set of robustness checks, we use an average annual employment of 1 hour per week as an indicator of being employed or replace the employment measure with the number of log hours worked (Columns iv and v of Table 3). Our results remain qualitatively unchanged; the negative effect of hours worked is even marginally significant. Our results change somewhat if we replace employment with time spent in unemployment. We find that a person becomes less happy the more time she spends in unemployment (but again the result is insignificant; see Column vi), but for those

\footnote{The direct effects are included in the regression but not reported in the table. Also Headey and Wooden (2004) look at the happiness effects of income and wealth for a sample of Australian households and find that wealth affects happiness more strongly than income does. They do not look at wealth-income interactions.}
Table 3—Robustness Checks I

<table>
<thead>
<tr>
<th>Moment Restriction(s)</th>
<th>Pseudo-Differences for $\rho = 0.9$</th>
<th>lower un- BHPS</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>i (10)</td>
<td>ii (11)</td>
</tr>
</tbody>
</table>

Income shocks
- all, $y_{it}^*$ ($\alpha$)
- transitory, $\psi_{it}$ ($\alpha_{\psi}$)
- persistent, $x_{it}$ ($\alpha_x$)

Employment, $e_{it}$ ($\gamma$)

Fraction of Yr. in Unempl.

Note: See notes to Table 2. In i we instrument $y_{it}^* - \rho y_{it-1}$ by $y_{it+1}^* - \rho y_{it}$ and in ii and iii, we instrument $y_{it}^* - \rho y_{it-1}$ by $y_{it+1}^* - \rho^3 y_{it-2}^*$. In Column iv, we define a person to be employed if working more than 52 hours in the reporting year; in Column v we replace the employment indicator with log hours worked. This restricts the sample to persons who have worked at least one hour. In Columns vi and vii we replace employment with time spent in unemployment in the current year. In Column vii we restrict the sample to persons who have worked at least 520 hours in $t-2$. Columns viii and ix report results using BHPS data.

Table 4—Robustness Check II: Sample Splits

<table>
<thead>
<tr>
<th></th>
<th>Men</th>
<th></th>
<th>Women</th>
<th></th>
<th>Public Employees</th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>FD</td>
<td>IV-t</td>
<td>IV-p</td>
<td>FD</td>
<td>IV-t</td>
<td>IV-p</td>
</tr>
<tr>
<td>Income</td>
<td>0.25</td>
<td>0.09</td>
<td>0.40</td>
<td>0.22</td>
<td>0.05</td>
<td>0.59</td>
</tr>
<tr>
<td>(all / t. / p.)</td>
<td>(0.02)</td>
<td>(0.07)</td>
<td>(0.13)</td>
<td>(0.02)</td>
<td>(0.06)</td>
<td>(0.13)</td>
</tr>
<tr>
<td>Employment</td>
<td>0.19</td>
<td>-0.11</td>
<td>0.02</td>
<td>-0.16</td>
<td></td>
<td></td>
</tr>
<tr>
<td>(0.03)</td>
<td>(0.34)</td>
<td>(0.02)</td>
<td>(0.17)</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Note: See notes to Table 2. IV-t refers to the transitory shock instrumentation (10), and IV-p to the persistent shock instrumentations (11) and (17). In the last three columns we restrict the sample to public employees.
who are employed in $t-2$, more time in unemployment actually increases happiness; see Column vii. This suggests that unemployment is particularly depressing for persons who are only weakly attached to the labor market. In another (unreported) specification, we augment the regression from Column vi with an indicator of long-term unemployment and observe that this indicator picks up all of the negative effect of unemployment.

Additionally, we repeat our estimations using household data from the UK. In Columns viii and ix, we report the results for the non-instrumented first difference estimator and the IV estimator that identifies the effects of persistent shocks and employment, respectively. While the first-differenced OLS results suggest that income does not affect happiness, our IV results confirm once more that this is driven by transitory income shocks. In fact, our IV regressions show that the pass-through of persistent income shocks in the UK is actually very similar to what we estimated for Germany. Also for the effect of employment on happiness we find the same picture as before. While the first-differenced OLS suggests a significant increase in happiness from employment, the point estimate under IV estimation is negative.

In a third set of robustness checks, we split the sample into men and women, and look at public employees only. The results are reported in Table 4. Qualitatively, the results for the sample split according to gender do not differ from our estimates when pooling men and women. Also the results for public employees are reconfirming our identification idea. For civil servants, transitory income shocks can be considered fairly unimportant given the nature of the compensation schemes in the German civil service. Hence, we expect the IV and FD estimates to be similar and this is what we find.

C. Interpretation

So far, our empirical analysis has documented that persistent and transitory income shocks have a differential effect on life-satisfaction. In Section I.A, we have discussed that life-satisfaction as such might relate (at least) to two different objects in the dynamic planning problem of the household: (period) felicity and (lifetime) utility. To interpret the empirical findings, we now come back to the question which of the two objects is more likely to be related to the survey answer that is our dependent variable.
The interpretation of the life-satisfaction answer in terms of utility or felicity is important in particular for what standard preference assumptions predict for the sign of the employment effect. The standard assumption on felicity functions is that agents prefer leisure over labor. Hence, if life-satisfaction mostly corresponds to felicity, standard preferences predict the (insignificantly) negative coefficient we estimate. For a lifetime utility interpretation, there is no clear prediction about the sign of the employment effect, as the relation between utility and employment depends on labor market frictions. There are two effects on utility: A direct one from changing employment itself and an indirect one from the implied change in the (potential) wage, keeping labor income constant. The indirect one is always negative. More work at constant income implies lower wages and the agent is better off at higher wages.

For the direct one it depends on the labor market. In a frictionless labor market where agents can freely decide to work or not, optimality implies that the marginal effect of labor, hence the direct effect, is zero; so the prediction of a negative coefficient on labor prevails. For a frictional labor market, optimality implies a positive direct effect as the worker can, when working, always choose not to work but not vice versa. Hence, in a frictional labor market the expected sign of the employment effect is undetermined. 24

To shed light on the question whether life-satisfaction is closer to (period) felicity or (lifetime) utility, we re-estimate our model with an interaction term in age. If the answer to the survey question relates strongly to lifetime utility, one would expect to find that persistent income shocks get less important for older persons. Accordingly, under the lifetime utility interpretation, we would expect that $\alpha_x$ decreases in age. However, Table 5 shows that this is not what we find. We find this pass-through to be increasing instead.

As a second approach to the question, we use a calibrated incomplete markets model to try to discriminate between both concepts, using the model’s quantitative predictions. We run the IV regressions discussed in Section I.B on simulated felicity and utility data from this model and compare the relative size of the pass-through of persistent and transitory shocks for both concepts, respectively. Note that only the relative size of the

24If the Limited Tenure Effects assumption does not hold and e.g. period $t - 1$ employment predicts period $t + 1$ wage growth, there is a further effect on utility that typically should be positive even in a frictionless labor market.
Table 5—Felicity vs. Lifetime Utility: Sample Splits and Simulation Results

<table>
<thead>
<tr>
<th></th>
<th>i</th>
<th>ii Simulation: Felicity</th>
<th>iii Simulation: Utility</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>SOEP</td>
<td>∆y × age</td>
<td>∆y × age</td>
</tr>
<tr>
<td>transitory</td>
<td>0.07 (0.05)</td>
<td>0.20</td>
<td>0.01</td>
</tr>
<tr>
<td></td>
<td>0.07 (0.05)</td>
<td>-0.003 (0.006)</td>
<td>0.19 -0.01 0.01 -0.003</td>
</tr>
<tr>
<td>persistent</td>
<td>0.45 (0.07)</td>
<td>1.69</td>
<td>1.73</td>
</tr>
<tr>
<td></td>
<td>0.45 (0.07)</td>
<td>0.02 (0.01)</td>
<td>1.69 0.01 1.75 -0.04</td>
</tr>
</tbody>
</table>

Note: See also notes to Table 2. We add an interaction term in age and estimate its effect modifying moment conditions (11) and (10) accordingly. The non-interacted income effect corresponds to the income effect at the sample average age. Columns (ii) and (iii) repeat the exercise for simulated utility/felicity data from an incomplete markets life-cycle model calibrated to the German economy and SOEP income data, see Appendix C for details. Simulated utility/felicity data is normalized to have unit variance.

Coefficients can be interpreted due to the cardinal nature of (Von-Neumann-Morgenstern) intertemporal utility functions which finds its counterpart in the normalization of the variance of $v$ in our estimation procedure.

In the empirical data, the estimated ratio $\alpha_x/\alpha_\psi$ is about 7. In the simulated data, this ratio is 8.5 for felicity and about 150 for lifetime utility. Thus, in the data, we find too little sensitivity with respect to persistent shocks for both interpretations, but far too little for the utility interpretation - of course this argument assumes the incomplete markets model to be valid. Put differently, if life-satisfaction was predominantly capturing lifetime utility, persistent shocks needed to be far better insured than in our calibrated standard incomplete markets model. Yet, it is unclear which real-world factors should give rise to such high degree of insurance and why they would not show up that strongly in consumption data, for which the incomplete markets model has been shown to be a reasonable description by and large.

Finally, also the survey evidence presented in Ross et al. (1986) is more supportive for the felicity than for the utility interpretation. Ross et al. (1986) investigate the
psychological processes in responding to surveys. They find that survey respondents primarily reason about current events (affect), they secondarily reason about the future, while other factors such as past experience seem to play a minor role.

IV. Discussion

How do our findings relate to the previous economic literature on happiness? The literature has documented sizable differences in the income sensitivity of life-satisfaction between social groups and between countries. Households with more access to informal insurance markets (e.g. through religious groups, see Dehejia et al. (2007)) show both a weaker consumption-income and a weaker happiness-income relationship. Less developed countries show a higher (within-person) income sensitivity of happiness (see, e.g., Graham and Pettinato (2002)), which is also true for transition economies (see Frijters et al. (2004a, 2006) and Caporale et al. (2009)). Since these countries have less developed financial markets, our study provides a potential explanation. One can also read the study of Lelkes (2006) in this vein, which provides evidence for the happiness-income relation in Hungary in 1992 and 1998 and shows that during transition the income-happiness relation declined over time. This might reflect both the development of financial markets and the slow convergence of the within-country asset distribution to its new steady state, meaning that self-insurance abilities in transition economies initially are below their long-run level; see, e.g., Fuchs-Schündeln (2008).

With respect to our results concerning the happiness effects of being employed, some comments may be in order. One should not read the results as “unemployment does no harm”. First, the strong difference between the IV and FD estimates points to important long-run income effects of unemployment; see, e.g., Arulampalam (2001) and, in the happiness context, Knabe and Rätzel (2011). Second, our regressions control for the happiness effects of health, and given that there is a literature that discusses the effects of unemployment on health, there may be indirect effects of unemployment on happiness through health, which we keep constant. Third, our two-state description of the labor market in employment/non-employment does not allow us to discriminate well between non-participation and unemployment and the degree of detachment from the labor market, and all may have different effects on happiness.  

See also Knabe et al. (2010), who find that the unemployed spend more time on activities they consider
V. Conclusion

This paper has reassessed the link between household income, employment and happiness in light of an incomplete markets setup, where households can only self-insure against income shocks. This limited ability to insure predicts a positive relationship between income and happiness. More important, it predicts that shocks with different persistence have a different impact on happiness. This is exactly what we find in the happiness data we analyze. While persistent income shocks have an impact on happiness, transitory income shocks do not and are hence perfectly insured. In addition to this point, we show that disregarding the differential impact of income shocks with different persistence also biases inference on the impact of other factors on happiness, in particular employment. We show that once one controls for the differential effects of persistent and transitory income shocks, employment per se no longer contributes significantly to a person’s well-being.

REFERENCES


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Appendices

A. Moment Conditions

A.1. Transitory Income Shocks

The instrument used to identify the effect of transitory income shocks, $\alpha_\psi$, needs to be uncorrelated with the combined residual in the estimation equation for transitory shocks (9) but needs to be correlated with the change in income, $\Delta y_{it}^*$. The next year’s growth rate in income $\Delta y_{it+1}^*$ is a valid instrument, because

$$\Delta y_{it+1}^* = y_{it+1}^* - y_{it}^* = \psi_{it+1} - \psi_{it} + \epsilon_{it+1}$$

(20)

is not correlated with the combined residual in (9),

$$(\alpha_x - \alpha_\psi) \Delta x_{it} + r_{it} = (\alpha_x - \alpha_\psi) \epsilon_{it} + r_{it},$$

as $\epsilon_{it}, \psi_{it+1}, \psi_{it}$ and $\epsilon_{it+1}$ are all mutually uncorrelated by the assumptions on the income process and $r_{it}$ is uncorrelated to $\psi_{it+1}, \psi_{it}$ and $\epsilon_{it+1}$ under the “No Foresight” condition. However, the instrument correlates (negatively) with the current income growth rate through this year’s transitory income shock, $\psi_{it}$, as

$$\Delta y_{it}^* = y_{it}^* - y_{it-1}^* = \psi_{it} - \psi_{it-1} + \epsilon_{it}$$

(21)

so that the instrument is informative. Intuitively, the next year’s growth rate in income captures the transitory movements in income as they are reversed in the future.

A.2. Persistent Income Shocks

To identify the effect of persistent income shocks, $\alpha_x$, we can use the instrument

$$y_{it+1}^* - y_{it-2}^* = \psi_{it+1} - \psi_{it-2} + \epsilon_{it-1} + \epsilon_{it} + \epsilon_{it+1},$$

(22)
as this instrument is uncorrelated to the combined residual in the estimation equation for persistent shocks (8),

$$(\alpha_{\psi} - \alpha_{x}) \Delta \psi_{it} + r_{it} = (\alpha_{\psi} - \alpha_{x}) (\psi_{it} - \psi_{it-1}) + r_{it},$$

as $\psi_{it-2}, \ldots, \psi_{it+1}$ and $\epsilon_{it-1}, \ldots, \epsilon_{it+1}$ are all mutually uncorrelated by the assumptions on the income process and $r_{it}$ is uncorrelated to $\psi_{it+1}, \psi_{it}$ and $\epsilon_{it+1}$ under the “No Foresight” condition and is also uncorrelated to $\psi_{it-2}$ and $\epsilon_{it-1}$ under the “Short Memory” assumption. However, the instrument correlates (positively) with the current income growth rate, $\Delta y_{it}^* = \psi_{it} - \psi_{it-1} + \epsilon_{it}$, through this year’s persistent income shock, $\epsilon_{it}$, so that the instrument is informative. Intuitively, the three-year growth rate in income captures the persistent movements in income and correlates with the persistent but not the transitory shock in year $t$.

**B. Data**

**B.1. SOEP**

Table 6 summarizes the variables used in the baseline estimations and their keys in the SOEP data. Table 7 provides information on the number of observations as well as on the number of observations we lose due to sample selection.

<table>
<thead>
<tr>
<th>Variable</th>
<th>Key</th>
<th>Variable</th>
<th>Key</th>
</tr>
</thead>
<tbody>
<tr>
<td>Overall life satisfaction</td>
<td>p11101</td>
<td>Indicator - wife/spouse in HH</td>
<td>h11112</td>
</tr>
<tr>
<td>HH post-government income</td>
<td>i11102</td>
<td>Subjective satisfaction with health</td>
<td>m11125</td>
</tr>
<tr>
<td>Employment status of individual</td>
<td>e11102</td>
<td>Disability status</td>
<td>m11124</td>
</tr>
<tr>
<td>Annual work hours of individual</td>
<td>e11101</td>
<td>Age of individual</td>
<td>d11101</td>
</tr>
<tr>
<td>Relation to HH head</td>
<td>d11105</td>
<td>Marital status</td>
<td>d11104</td>
</tr>
<tr>
<td>Number of persons in HH</td>
<td>d11106</td>
<td>Number of years of education</td>
<td>d11109</td>
</tr>
<tr>
<td>Number of children in HH</td>
<td>d1110726</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

*Note:* The variables are from the 100%-sample version of the Cross-National Equivalent File of the SOEP ($$PEQUIV$$-files).
Table 7—Sample Selection

<table>
<thead>
<tr>
<th>Description</th>
<th>Observations</th>
</tr>
</thead>
<tbody>
<tr>
<td>Initial number of observations</td>
<td>224,127</td>
</tr>
<tr>
<td>After constraining to ages 25-55</td>
<td>134,494</td>
</tr>
<tr>
<td>After accounting for missings in education</td>
<td>132,554</td>
</tr>
<tr>
<td>After accounting for missings in income</td>
<td>132,524</td>
</tr>
<tr>
<td>After accounting for missings in happiness</td>
<td>132,173</td>
</tr>
<tr>
<td>After accounting for missings in health satisfaction</td>
<td>131,989</td>
</tr>
<tr>
<td>After taking out persons in maternal leave, education, military service</td>
<td>127,799</td>
</tr>
<tr>
<td>After removing outliers:</td>
<td></td>
</tr>
<tr>
<td>Final number of observations for first-stage regressions</td>
<td>127,185</td>
</tr>
<tr>
<td>Final number of observations for which all instruments can be constructed</td>
<td>77,112</td>
</tr>
</tbody>
</table>

B.2. BHPS

The BHPS data we use provide annual information for the years 1991-2008. However, the question on life satisfaction is available only for the years 1996-2008 and is missing in 2001. We keep only households living in England.\textsuperscript{27} In the BHPS, the life satisfaction question takes the form: "How dissatisfied or satisfied are you with your life overall?" and is coded on a scale from 1 (not satisfied at all) to 7 (completely satisfied).

\textsuperscript{27}The BHPS started with mainly households living in England. In later sample waves, households from Wales, Scotland, and Northern Ireland were added to the BHPS, which implies that these economically diverse parts of the UK do not have a constant sampling weight.
C. Estimating Life-Satisfaction Regressions in a Simulated Incomplete Markets Model

In this section, we present estimation results for life-satisfaction regression estimated on simulated data generated from a life-cycle incomplete markets model (in partial equilibrium). We provide results both for equating life-satisfaction with life-time utility, with felicity, and for a specification where we allow for adaptation via mean reversion in life-satisfaction.

C.1. Model Setup

We consider a household who enters the labor market at age 25 with zero assets. Labor income follows the process described in (1) until retirement at age 60; log income before entering the labor market is normalized to zero. The household then lives for another 25 years and dies with certainty at age 85. During retirement the household receives a pension equal to a fraction $\phi$ of its last persistent labor income, $x_{i,60} + f(60)$. We disregard income differences in education but set $f(z)$ equal to a 2nd order polynomial in age, capturing the observed life-cycle profile of labor incomes.

The household has a CRRA felicity function in consumption, $u(c) = \frac{c^{1-\theta}}{1-\theta}$, with risk aversion $\theta$, and maximizes expected lifetime utility as in (4) by saving in an asset that pays a risk-free return $r$. We assume the household cannot borrow, $b = 0$.

C.2. Calibration

We calibrate the economy to the SOEP data. We equate labor income with post-government household (non-asset) income. We assume a risk aversion of $\theta = 2$. The interest rate $r$ equals 2% roughly in line with real returns (net of growth) in Germany. The replacement rate is calibrated to match the relative average (non-asset) income of households net of taxes and transfers with a 55-59 year-old household head and households whose head is between 70 and 74 years old. This yields a replacement rate of roughly 62.9% and includes state pensions and also incomes from compulsory company

28We make this assumption in order to simplify calculations. In reality, both company and public pensions are mostly based on average income over the working life.
pension plans, but not private pension insurance income (counted as asset income). We calibrate the discount factor to match the median asset to median income ratio in the SOEP of 2.7 (for households with a household head between age 25 -55) . Table 8 summarizes the parameter values.

When we check the potential bias that arises from adaptation, we match the estimated degree of mean reversion in life-satisfaction in our sample, which is $1 - \tau = 1 - .14$.

<table>
<thead>
<tr>
<th>Parameter</th>
<th>Value</th>
<th>Parameter</th>
<th>Value</th>
</tr>
</thead>
<tbody>
<tr>
<td>Discount Factor, $\beta$</td>
<td>.937</td>
<td>Real Return, $r$</td>
<td>2%</td>
</tr>
<tr>
<td>Risk Aversion, $\theta$</td>
<td>2</td>
<td>Replacement Rate, $\phi$</td>
<td>62.9%</td>
</tr>
<tr>
<td>Variance of:</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Transitory Shocks, $\sigma^2_\psi$</td>
<td>.0203</td>
<td>Persistent Shocks, $\sigma^2_\epsilon$</td>
<td>.0153</td>
</tr>
<tr>
<td>Deterministic Log-Income Profile by Age (2nd order Polynomial)</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>$\omega_0$</td>
<td>0.037</td>
<td>$\omega_1$</td>
<td>0.026</td>
</tr>
<tr>
<td>$\omega_2$</td>
<td>-0.001</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

C.3. Simulation Results

We solve this model economy and simulate the behavior of 8000 agents over their life cycle (roughly the average number of observations per age in the SOEP), tracking their income, their utility and their felicity as well as the marginal utilities/felicities with respect to the two income states variables. We then estimate our happiness-income regressions on this simulated utility and felicity data. The simulation is repeated 100 times. Table 9 presents the average estimates from this exercise and compares them to the average marginal utilities/felicities. As in our data, the sample is restricted to the 25 to 55 year old. We also run as an experiment a setup where we add an autoregressive

29Since compulsory company pensions make up a large share of pension benefits in Germany, we do not restrict the replacement rate to the statutory rates from the public pension system.
component to the simulated felicity/utility data such that

\[ \nu_{it}^{\text{observed}} = (1 + \tau) \nu_{it-1}^{\text{observed}} + \Delta \nu_{it}^{\text{simulated}} \]

. We apply both, the estimator without controlling for autocorrelation, as described in Section I.B (eq. 10,11), as well as the one that controls for autocorrelation, as described in Section I.C (eq. 13a,b). When the data is generated assuming no adaptation, the bias from the violation of the moment conditions through asset accumulation is small. In this case, the estimator misses the true relative effect by only 5%. With adaptation, the bias becomes more sizable and can be as large as 19% (for utility), but our procedure from Section I.C can effectively reduce this bias.

D. Similarity of second step OLS and single step ordered probit estimators

We check the robustness of our two-step estimation procedure by comparing two-step OLS estimates on income and a standard single-step ordered probit regression. As Table 10 shows, running a two-step estimation procedure instead of a single-step one does not significantly change results.

E. Further Regression Results

Table 11 provides estimation results from regressions excluding self-reported health status in the first stage, from a version that uses \( e_{t-3} \) as an instrument for employment, and from using the transitory shock identification to estimate the employment coefficient.
Table 9—Estimation Results from Simulated Data

<table>
<thead>
<tr>
<th>Felicity</th>
<th>Average Elasticity to:</th>
<th>Transitory</th>
<th>Persistent</th>
<th>(True)</th>
<th>0.19</th>
<th>1.59</th>
<th>8.40</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Estimates w/o Adaptation</td>
<td>Baseline (10) &amp; (11)</td>
<td>Robustified (13b) &amp; (13a)</td>
<td>Baseline (10) &amp; (11)</td>
<td>Robustified (13b) &amp; (13a)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>OLS</td>
<td>0.55</td>
<td>1.11</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>IV Transitory</td>
<td>0.20</td>
<td>0.20</td>
<td>0.38</td>
<td>0.38</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>IV Persistent</td>
<td>1.69</td>
<td>1.66</td>
<td>2.83</td>
<td>3.22</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Persistent Transitory</td>
<td>8.63</td>
<td>8.50</td>
<td>7.38</td>
<td>8.36</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Utility</th>
<th>Average Elasticity to:</th>
<th>Transitory</th>
<th>Persistent</th>
<th>(True)</th>
<th>0.01</th>
<th>1.64</th>
<th>156</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Estimates w/o Adaptation</td>
<td>Baseline (10) &amp; (11)</td>
<td>Robustified (13b) &amp; (13a)</td>
<td>Baseline (10) &amp; (11)</td>
<td>Robustified (13b) &amp; (13a)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>OLS</td>
<td>0.36</td>
<td>0.57</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>IV Transitory</td>
<td>0.01</td>
<td>0.01</td>
<td>0.02</td>
<td>0.02</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>IV Persistent</td>
<td>1.75</td>
<td>1.75</td>
<td>2.44</td>
<td>2.71</td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Persistent Transitory</td>
<td>153</td>
<td>153</td>
<td>136</td>
<td>150</td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
Table 10—Similarity of Two-step and One-step Estimation

<table>
<thead>
<tr>
<th></th>
<th>OLS</th>
<th>O-Probit</th>
</tr>
</thead>
<tbody>
<tr>
<td>Income</td>
<td>0.32</td>
<td>0.32</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.01)</td>
</tr>
</tbody>
</table>

Note: Standard errors are in parentheses. OLS refers to the two-step estimation described in the main text, where we first estimate a model for happiness using ordered probit and a model for income using an OLS estimator using the same set of control variables in both regressions. We then generate residuals that we regress on each other linearly. O-probit refers to a single-step ordered-probit estimation that includes income along with the control variables. The table reports only the coefficient estimate on income.

Table 11—Further Robustness Checks

<table>
<thead>
<tr>
<th>w/o health satisfaction in first stage</th>
<th>Using $e_{t-3}$ as Instrument</th>
<th>Transitory shock analogue to (17)</th>
</tr>
</thead>
<tbody>
<tr>
<td>FD</td>
<td>IV-t</td>
<td>IV-p</td>
</tr>
<tr>
<td>Income</td>
<td>0.14</td>
<td>-0.08</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.04)</td>
</tr>
<tr>
<td>Employment</td>
<td>0.03</td>
<td>-0.13</td>
</tr>
<tr>
<td></td>
<td>(0.01)</td>
<td>(0.13)</td>
</tr>
</tbody>
</table>

Note: See Notes to Table 4. The first three columns exclude self-reported health satisfaction from the first stage regressions. The fourth column uses $e_{t-3}$ instead of $e_{t-2}$. The final column uses the transitory shock analogue to moment condition (17) together with (10).