In Frijters et al. (2014), we investigate the extent to which childhood characteristics are predictive of adult life satisfaction using data from two British cohort studies; the 1958 National Child Development Study (NCDS) and the 1970 British Cohort Study (BCS). Our main methodological approach involved linearly regressing average adult life satisfaction on a large covariate set that included variables measuring childhood traits and circumstances. Our findings from these analyses are that variables observed up to age 16 predict around 7% of the variation in adult life satisfaction, and that these variables capture approximately 15% of the fixed component of adult life satisfaction. Non-cognitive traits, captured by parent and teacher-assessed behavioural problems, are an important proportion of this 15%. Layard et al. (2014) in the same issue of the Economic Journal, find similar results in terms of the predictability of adult life satisfaction, and the important role of childhood non-cognitive traits, from their detailed analysis of the BCS.

Our study also contains numerous robustness and sensitivity analyses that support our main findings, which take into account the comments of four anonymous referees. These include: estimating several models that explore the potential effects of sample attrition; estimating ordered probit models of adult life satisfaction (rather than linear models); estimating linear regression models of life satisfaction at age 50 on childhood circumstance, contemporaneous socio-economic variables and long lags of adult life satisfaction; and estimating dynamic panel-data models of adult life satisfaction. It is the latter two robustness analyses that Piper and Pugh (2015), henceforth abbreviated to PP, have focused on in their methodological comment.

Before discussing PP’s main claims, we first note that PP have chosen not to investigate our analysis using data from the NCDS or BCS, and instead use data from the British Household Panel Survey (BHPS) and the German Socio-Economic Panel (SOEP), which have a very different sample and survey design than the cohort studies. They claim that “neither author has access to either the NCDS or to the BCS”, which we find difficult to understand. Data from the NCDS and BCS are readily available (just a few clicks of the mouse) from the UK Data Archive, and it is also the UK Data Archive that manages access to the BHPS data they use. However, PP state that it is “convenient” for them to use data from the BHPS and SOEP; we agree that it would be ‘inconvenient’ for them if they had to undertake the detailed data work with the NCDS and BCS that was required for our study. We feel that if researchers want to intimate that results from a published article are flawed, then they should do the authors the courtesy of using the same data when it is readily available.

PP do not explicitly state which of our main findings they disagree with. The first claim by PP is that regressing age 50 life satisfaction on past life satisfaction (from ages 46, 42 and 33) is “problematic because the OLS point estimates for the lags of the life satisfaction variables are biased upwards” (pp.2). Importantly, PP seem to have missed that the aim of our analysis was not to estimate the causal effects of past life satisfaction. Rather, the primary aim was to estimate the proportion of variation in life satisfaction that can be predicted. We therefore interpret the coefficients on prior life satisfaction as a mix between the correlation with fixed unobservables and lagged-dependent variable effects, just as PP themselves do. Even if we took the upper bound possibility that the coefficients were entirely driven by correlations between fixed traits, we still get the same result with a decomposition of the variation over time; i.e. that fixed traits maximally explain 40% of the total variation in adult life satisfaction. This figure is broadly consistent with a number of studies that have used alternative methods, including analysis of the BHPS and SOEP. For our stated aim, the use of OLS is entirely appropriate. Accordingly, we note in our conclusion that “we are unable to make any strong causal statements”. Hence PP are attributing a claim of causality to us that we did not make and that we expressly rejected. As such, their comments are miss-directed.
The second claim by PP is that the presentation of results from our dynamic panel data models of adult life satisfaction is inadequate due to the lack of robustness tests. They then present a series of possible robustness tests using BHPS and SOEP data.

Our paper includes a discussion of the model estimated and its main underlying assumptions: “Equation (8) is estimated using the Blundell and Bond (1998) system estimator, which assumes no autocorrelation in the random error term and that the fixed-effect is uncorrelated with the first observed first-difference”. We did not however include any analysis of whether the dynamic panel data model estimates are sensitive to different instrumentation. PP demonstrate using BHPS and SOEP data that estimates from dynamic panel data models can be sensitive to the type and number of instruments employed, which is a very well-known result (though they themselves do not report the model fit, which is the main object of our analysis). Indeed, they advocate using the same methods we have (system GMM), so we are unclear what they find problematic about our analysis.

The proposition that there are insufficient robustness tests of our dynamic panel data model results is debateable: our study omitted such tests because the dynamic panel data model was itself a secondary robustness analysis for our main results. Naturally, it is difficult given journal space constraints (the paper is at the maximum word length) for empirical studies to explore the sensitivity and robustness of each estimation result, and it is even more difficult to explore the sensitivity and robustness of every sensitivity and robustness result. As you would expect, we ran several different model specifications, finding similar results in terms of fit to the ones we reported. However, we agree that applying this type of model to cohort data is novel and should be a focus of future work, which is why it was only included as a robustness test in our paper. We also believe that the Features Section of the Economic Journal is an appropriate place to highlight such ideas.

In summary, the critique of PP is misplaced and poorly thought through: it is based on confusing our claims of correlation and predictability with claims of causality; on using different data than ours; and on insisting on robustness tests of robustness tests in a context where this did not affect our primary results. Tellingly, they do not actually reference a statement in our conclusion that they disagree with, nor do they pursue any question of clear economic interest, so we are unsure why our study has attracted their criticism.

Paul Frijters (University of Queensland)
David W. Johnston (Monash University)
Michael A. Shields (Monash University)

REFERENCES

