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Landeghem, B.V. (2019) Stable traits but unstable measures? Identifying panel effects in self-reflective survey questions. Journal of Economic Psychology, 72 (June 2019). pp. 83-95. ISSN 0167-4870

https://doi.org/10.1016/j.joep.2019.02.006

Stable Traits but Unstable Measures? Identifying Panel Effects in Self-Reflective Survey Questions

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January 2019

Abstract

Economists and psychologists often measure aspects such as utility, preferences, and personality traits through self-assessment modules in longitudinal household surveys. This paper investigates to what extent such measures are subject to a panel effect or panel conditioning, that is, whether people answer the questions differently the more experience they have answering such questions. First, the paper makes a more general contribution to the literature on panel effects and makes explicit identification issues that arise in different types of empirical strategies. Next, the empirical analysis exploits a design feature of the UK Household Longitudinal Survey that introduces random variation in survey experience within a calendar year. The analysis first confirms the existence of such a panel effect in general life satisfaction, a pattern previously established in other data with a slightly different identification strategy. The data also provide evidence of panel effects in domain satisfactions, although these are less straightforward to interpret. This finding is important if researchers consider repeated measurements of such traits in household surveys to investigate their stability over time for a society or for an individual: the paper illustrates how conclusions on time trends in the subjective data for this case study are influenced if panel effects are ignored.

Keywords: Panel Effects; Subjective Data; Self-Reflective Questions; Identification.

JEL Codes: C18; D60.

1 Introduction

Let's imagine two companies A and B. Company A has a *temperature check* in place and regularly asks its employees in an online survey about their satisfaction. The CEO of company B likes the idea and decides to implement a similar policy. The results of company Bs first *temperature check* reveal that employee satisfaction is significantly higher in company B than in company A. Do these results imply that company B has a more friendly working environment, a different mix of employees perhaps, ... or can the difference in outcomes be explained by the difference in employees' experience with answering surveys? The latter is the topic that will be studied in this paper. Do people report different well-being scores, ceteris paribus, the longer they have been participating into a panel? Or, in other words, are answers to well-being questions subject to panel conditioning or a panel effect?

The hypothetical example above illustrates the importance of the topic, as we can obviously think of many similar scenarios. But the topic is also relevant when we want to look at time trends within panels. There is a growing concern that indeed subjective well-being data are affected by survey experience, but current evidence is mixed. An important empirical challenge is however that with panel data, it is hard to disentangle panel effects from other factors, more in particular time effects. This paper aims to contribute to the empirical evidence on panel effects in various measures of subjective well-being. The analysis exploits the set-up of a recent panel which arguably induces random variation in survey experience in each calendar year, and a robustness check compares the trend in panel data with the trend in repeated cross-sections. In addition, the paper makes a more general contribution to the literature on panel conditioning by discussing the identification issues specific to identifying panel effects in regular panel data, an exposition which might help us to interpret (contradicting) results and to avoid confusion in future work.

Recently, the topic of panel conditioning has received systematic attention in the literature. Das et al. (2011) offer a cross-disciplinary overview of the literature on panel conditioning and subsequently present a case study in which they compare the answers of two samples interviewed at the same time, a refreshment sample with first-time respondents and a more experienced sample with second-time respondents. Under a range of different assumptions with respect to attrition biases in the more experienced sample, the authors' overall conclusion is that panel conditioning tends to be present in knowledge questions but not in attitudinal questions. Recently, Fisher (2015) has argued that even income data, which are key in many microeconometric panel data analyses, are more accurate in the second round than in the first round. Moreover, there is recent convincing evidence that survey participation can have a substantial impact on people's actual behaviour, such as hygiene care (Zwane et al., 2011). Crossley et al. (2017) find that asking people detailed questions about their expenditures and needs in retirement subsequently significantly reduces their savings rate.

An early contribution in the context of well-being is the work by Sharpe and Gilbert (1998), who find in a lab experiment that testing individuals for depression twice within a one-week interval leads to a decrease in self-reported negative emotions and does not seem to have an effect on self-reported positive emotions. However, the very specific, small sample of college undergraduates and the short time span that elapsed between the two sessions make it hard to extrapolate these results to nationwide panel data with yearly intervals between interviews. The steep decline in average well-being scores in the first rounds of a panel widely used for happiness research, the German Socioeconomic Panel (SOEP), has been noticed and discussed by several researchers, such as D'Ambrosio and Frick (2007, 2012), Di Tella and MacCulloch (2006), Frijters and Beatton (2012), Kassenboehmer and Haisken-Denew (2012) and Landua (1992). Relying solely on consecutive rounds in a panel makes it, however, hard to disentangle panel effects from genuine time shocks. Van Landeghem (2014) uses the Swiss Household Panel and the German SOEP to compare average levels of life satisfaction for newcomers (refreshment samples) with more experienced respondents. Those who have been in the panel for several years report a significantly lower level of life satisfaction than newcomers and the difference becomes even more pronounced after applying corrections for panel attrition. Two further studies use consecutive waves in panel data to study panel effects. Wooden and Li (2013) use balanced and unbalanced Australian panel data and cannot confirm the negative panel effect on mean life satisfaction scores as reported in Van Landeghem (2012, 2014) but they find very large time effects, those in the male sample being opposite to those in the female sample. Chadi (2013) notes that in his fixed effects regressions the estimates for panel effects heavily depend on the parametrization of control variables.

This paper will proceed as follows. Section 2 discusses identification problems that one encounters when trying to estimate panel effects by extending general panel data well-being regressions with *time-in-panel* variables. Section 3 elaborates on the main identification strategy applied in this paper. Section 4.1 presents the empirical results on panel effects in life satisfaction and domain satisfactions, while Section 4.2 illustrates the consequences of estimates for time effects if we were to ignore the existence of panel effects. Section 5 concludes the paper.

2 Measuring Time-in-Panel Effects in Observational Panel Data: Identification Issues

The statistical analysis of this paper aims to measure whether having more experience in answering surveys with well-being questions has an impact itself on reported well-being scores. As is common in empirical exercises, we endeavour to come as close to a ceteris paribus investigation as possible. Does reported well-being depend on the number of times one has participated into the panel, all else equal? This section aims to illustrate that one needs a source of random variation in panel experience within a given calendar year to measure panel effects, and that such a source is generally not available in regular panel datasets.

When using a large household panel to investigate panel effects, it seems especially key to find a way to control for time effects. Indeed, panels tend to interview a sample of the population around the same time of the year, and then repeatedly interview these same people with regular (e.g. yearly) intervals. Obviously, calendar time on the one hand, and the number of panel participations on the other hand, are then linearly related. Of course, often panels do not interview exactly the same people each year. The below exposition aims to illustrate different assumptions under which panel effects can be identified in classical panel data regressions, and the biases that can be introduced if these assumptions are violated. As we discuss and explain below, either we need to rely on not all members being present in each round (selective presence) or we need to make normalizing assumptions regarding the time effects.

2.1 Identification Relying on Selective Presence

To obtain a better view on the different scenarios that can occur in a panel dataset and which are relevant for our analysis, let us imagine a world in which a panel survey is conducted over three consecutive periods of one year. In period 1, all respondents are, by definition, first-time respondents. Some will enter the panel later, which implies that there are also first-time respondents in periods 2 and 3. Some first-time respondents will continue to participate and others will drop out. This means that there are second-time respondents in both periods 2 and 3. In period 2, second-time respondents will have participated in period 1, while second-time respondents in period 3 will have either responded in period 2 for the first time or in period 1 but not in period 2. Finally, in period 3, there will also be third-time respondents, those who responded to the survey in all periods. In sum, with respect to the key independent variables for the analysis of panel effects, we can identify seven groups in which we can classify all individuals. An extract of a dataset for the key independent variables for these seven prototype individuals is given in Table 1. The first column contains a person identifier and the other five columns are a vector of independent variables. The second column shows the overall constant α; the third and fourth columns each show a dummy for participating for the second and third times, respectively, denoted p_2 and p_3; and the fifth and sixth columns show a dummy for periods 2 and 3, respectively, denoted t_2 and t_3.[1] If we run a pooled OLS (or ordered logit) regression, for example, the five independent variables would be linearly unrelated in a panel with these seven categories of individuals. Obviously, one cannot just restrict the analysis to a balanced panel (group 7). One can impose some restrictions, but it will be necessary to also have individuals from another group, for example those who only participated in periods 2 and 3 (group 4), or in periods 1 and 3 (group 5).

The identification will crucially depend on the fact that not all individuals take part in each round. Being absent in one or more rounds might well be correlated with reported well-being scores as well (see e.g. Heffetz and Rabin, 2013 or Gardner and Oswald, 2004). The latter correlation is then picked up by the panel participation dummies and time dummies if it cannot be controlled for appropriately, as we will illustrate in Section 2.3.

Obviously these issues occur in many applications investigating self-reported well-being, or in panel data applications in general. However, the nature of the problem is conceptually different than in other applications. In the example outlined above, identification will crucially depend on the panel being unbalanced, while this is not necessary and even undesirable in other applications. While improving the response

[1] The dummies for the first survey participation p_1 and the first period t_1 are omitted, since these categories form the baseline.

rate might benefit estimates in other applications, it might not benefit the analysis of panel effects.

One can still think of ways to address attrition bias. More in particular, well-being regressions often control for fixed effects, and this can solve the problem of nonrandom absence if the effect is stable over time within an individual. However, it is then easy to see that the identification will hinge on temporary attritors, in our example individuals who are present in rounds 1 and 3 but not in round 2 (group 5). Temporary attrition is not necessarily just correlated with a fixed trait but might be caused by life events to which an individuals well-being has not adapted yet after re-entering the panel.

2.2 Identification Relying on Normalizing Time Effects

Instead of exploiting the unbalancedness of a panel, one can obtain estimates for the panel effects by putting restrictions on the estimates for the time effects.

Also while not all panels might do efforts to track individuals who have skipped one round, some techniques such as first-differencing would lead to the loss of instances of temporary attrition if they are present. To illustrate how identification might work under a scenario where identification does not come from temporary attrition or late entrance, let us first-difference the prototype dataset in Table 1, of which the results are shown in Table 2. We lose in this setting individuals 1, 2 and 3, as there is only one observation for those individuals. We also lose individual 5 as there is a time gap between the first and second observation. The constant is omitted because it cancels out during the transformation and including a constant term in a first-difference framework would be equivalent to a linear time trend.

The columns (or the variables) in Table 2 are now however characterized by a linear dependency which can be written as follows:

$$-\ddot{p_2} - 2 \times \ddot{p_3} + \ddot{t_2} + 2 \times \ddot{t_3} = 0 \tag{1}$$

Where $\ddot{}$ is the first-difference operator.

One could still estimate coefficients for $\ddot{p_2}$ and $\ddot{p_3}$ if the researcher (or the statistical software package used) drops one of the time variables. For example, one could opt for omitting $\ddot{t_2}$ from the analysis, which basically means that we make the assumption that the time effect on well-being in periods 1 and 2 are equal. If this assumption is not correct, we introduce a baseline slope rather than a baseline value.

2.3 Illustration

This subsection aims to illustrate the severity of biases when the identifying assumptions prove not to be correct. We will offer an example under both the scenario of identification relying on selective presence and identification relying on normalizing time effects. The illustrations are based on simulated data. The simulations only differ in the way the dependent variables are defined, and the distribution of observations across time period*panel participation cells are summarized in Table 3.

The first two simulations aim to illustrate the nature and severity of the bias when identification relies on non-presence which happens to be nonrandom. In the first simulation, selective presence is random and effects could be identified. In the second one, selective presence is non-random. More specifically, these two simulations are as follows:

$$y_{it} = -0.1 \times p_2 - 0.3 \times p_3 + 0 \times t_2 + 0.5 \times t_3 + \epsilon_{it} \tag{2}$$

And:

$$y_{it} = -0.1 \times p_2 - 0.3 \times p_3 + 0 \times t_2 + 0.5 \times t_3 + \mu_{it} + \epsilon_{it} \tag{3}$$

Variables are defined as in Section 2.1 and ϵ_{it} is a normally distributed error term of mean zero with standard deviation one. μ_{it} is the term which is added in the second version of the simulated dependent variable to introduce nonrandomness. It has the value of 0.5 for observations of individuals who did not take part in round 1, and it takes the value of 0.5 in round 3 for those individuals who only take part in round 1 and 3 but not in round 2.

The first two columns of Table 4 show estimates for these two respective simulations using pooled OLS regressions of the form:

$$y_{it} = \alpha + \beta_2 p_2 + \beta_3 p_3 + \gamma_2 t_2 + \gamma_3 t_3 + u_{it} \tag{4}$$

Where u_{it} is a residual term. Specification 1 in Table 4 shows estimates for the first simulated variable and the true parameters (as defined in the simulation) are nicely within the 95% confidence intervals of the estimates. Specification 2 reflects the alternative scenario of selective presence being nonrandom: even though the bias relates to only 6.2% of observations in rounds 2 and 3, the impact on the estimated coefficients is very significant as these observations are crucial for the identification of the effects as discussed above. Moreover, the paths of panel effects and time effects do not simply shift such that the estimates are correct up to a constant, but the paths seem to rotate. A persistent small deviation from nonrandomness can hence lead to huge biases when examining long horizons.

Next, building on the first-difference example given in Section 2.2, we simulate a third dependent variable such that:

$$\ddot{y_{it}} = -0.1 \times \ddot{p_2} - 0.3 \times \ddot{p_3} + 0 \times \ddot{t_2} + 0.5 \times \ddot{t_3} + \ddot{\epsilon_{it}} \tag{5}$$

Where $\ddot{.}$ is the first-difference operator, and $\ddot{\epsilon_{it}}$ is a normally distributed error term of mean zero with standard deviation one. As discussed under Section 2.2, we cannot include $\ddot{p_2}$, $\ddot{p_3}$, $\ddot{t_2}$ and $\ddot{t_3}$ as independent variables simultaneously due to multicollinearity, but we could obtain estimates for the panel effects by omitting one of the first-differenced time dummies. The two regressions that are presented in Table 4 in respectively columns 3 and 4 are as follows.

$$\ddot{y_{it}} = \beta_2 \ddot{p_2} + \beta_3 \ddot{p_3} + \gamma_3 \ddot{t_3} + u_{it} \tag{6}$$

and

$$\ddot{y_{it}} = \beta_2 \ddot{p_2} + \beta_3 \ddot{p_3} + \gamma_2 \ddot{t_2} + u_{it} \tag{7}$$

As in our simulation, the coefficient on $\ddot{t_2}$ is set to zero, the former would make the correct identifying assumption, which is that the second time effect is equal to the benchmark time effect and panel effect. The latter specification in Equation (7) makes an incorrect assumption. Leaving out $\ddot{t_3}$ implies setting the coefficient γ_3 equal to zero but, in the simulation, $\gamma_3 = 0.5$. This specification hence causes us to define not only a baseline level but also a baseline slope.

The results in the third column of Table 4 show estimates that are very close to the simulated values, well within the 95% confidence intervals. The fourth column, based on an equation that makes an incorrect assumption, shows that the estimates for second- and third-time participation are heavily biased. Again, it is clear that we did not simply add a constant to the estimates. Instead, the curvature of how panel effects and time effects build up across the number of participations has rotated, reflecting that the incorrect identifying assumption implies a baseline slope with a nonzero gradient. The nature of the bias is hence very similar as the one shown in column 2.

When analysing real data, we typically do not know which normalizing assumption would be correct and it is hard to convincingly argue that selective presence is random (even after controlling for many observed factors).

The example above illustrates that we need to look for particular settings in which data offer us exogenous variations in the number of survey participations within a calendar year.

3 Empirical Strategy

3.1 Case Study and Empirical Setup

As Section 2 argues, measuring time-in-panel effects in subjective data with observational panel studies is not obvious. Finding a setting in which there is an exogenous variation in the number of panel participations within the same calendar year is crucial. This application will use the UK Household Longitudinal Study (UKHLS).

The UKHLS is the successor of the British Household Panel Survey. It is maintained by the Institute for Social and Economic Research based at the University of Essex. The paper uses the first five rounds, with the first round starting in 2009. This paper will consider the general population sample, which covers England, Scotland, and Wales.[2]

Unlike the Swiss Household Panel and the German SOEP, the UKHLS does not contain panel refreshments yet. However, it has a similar feature, which has also been used by Fisher (2015) to investigate income reporting across panel participations. Many well-known European household panels try to concentrate the interviews within a certain period of the year. In the UKHLS, however, to spread the workload at the start of the project, the main sample was randomly split up into 24 monthly subsamples. The first monthly subsample was contacted for the first time in January 2009, while the 24th

[2]See Knies (2015) for an extensive overview of the dataset.

monthly subsample was contacted in December 2010. Each subsample was subject to the same procedures in terms of being recontacted after unsuccessful attempts and the UKHLS stopped trying to contact households if there was no successful interview after three months. An important characteristic of the dataset regarding this study is that, even though a round has 24 monthly samples, the UKHLS endeavoured to reinterview individuals with an interval of 12 months. This means that, by construction, there is exogenous variation within a calendar year in the number of panel participations. If we define monthly samples 1 to 12 as belonging to the Early Sample and monthly samples 13 to 24 as belonging to the Later Sample, then we can observe that, in 2010, the Later Sample was interviewed for the first time and the Early Sample for the second time. In 2011, the Later Sample was interviewed for a second time and the Early Sample for a third time, and so on. Table 5 provides a schematic overview.

We can test for panel effects using the following general regression specification:

$$\begin{aligned} y_{it} = {} & \alpha + \gamma_2 t_{2011} + \gamma_3 t_{2012} + \gamma_4 t_{2013} \\ & + \text{Early}_i * (\beta_2 t_{2010} + \beta_3 t_{2011} + \beta_4 t_{2012} + \beta_5 t_{2013}) + \sum_{k=2}^{12} \delta_k m_k + u_{it} \end{aligned} \tag{8}$$

Where y_{it} is the dependent variable for individual i in period t. In the empirical application, we use several dependent variables derived from reflective survey questions, which are discussed in the Appendix. The term α is an overall constant; t_{2010} to t_{2013} are time dummies for the years 2010 to 2013, respectively; Early_i a sample indicator that takes the value one if individual i is in the Early sample and zero otherwise; and m_2 to m_{12} are year-of-month dummies for February to December, respectively. The coefficients of the interaction terms between the time dummies, on the one hand, and the sample indicator Early_i, on the other hand, measure panel effects, since these terms exploit the within-year variations in panel participation. More specifically, β_2 measures the effect of participating for the second time versus the first time, β_3 measures the effect of participating for the third time versus the second time, β_4 measures the effect of participating for the fourth time versus the third time, and, finally, β_5 measures the effect of participating for the fifth time versus the fourth time. A joint significance test of the β-coefficients gives us a clue on whether the panel effect is dynamic over time. If β_3 to β_5 are all zero, and β_2 is significantly different from 0, it would mean that a panel effect only occurs between the second versus the first time. If all β-coefficients are equal, it would mean that the panel effect builds up over time and that the difference in average well-being between the Early Sample and Later Sample remains the same over time due to the accumulation of panel effects. If measures recover from a panel effect after several rounds, the β-coefficients switch sign.

While the β coefficients measure panel effects, it is very important to stress that the γ coefficients in the empirical application are *not* measuring pure time effects but, rather, a mixture of panel effects and time effects. Indeed, while the γ coefficients capture an actual change in the dependent variable over time, they also capture that both the Early Sample and Later Sample have participated one more time in period t than in period $t-1$.

3.2 Estimation Sample

For regressions of the form as in Equation (8) to produce unbiased results, it is important that the variation of panel participations between the two samples within a calendar year can be considered exogenous. There are two concerns in this respect. First, we need to ensure that both samples have been drawn in a similar way from the population. Second, people can drop out of a panel after N rounds for a variety of reasons. We know that future panel attrition is a good predictor of the current values of subjective variables (e.g. Gardner and Oswald, 2004) and, if, indeed, panel attrition happens to be nonrandom, a comparison of individuals who participated $N+1$ and N times within the same calendar year could give a biased estimate of panel effects.

The analysis will therefore only consider the balanced panels of individuals who have participated in the survey in all five rounds.[3] However, individuals in the Early Sample have to survive until 2013, while individuals in the Later Sample have to survive until 2014 in order to have participated five times and subsequently be included in the balanced sample. Since older individuals face an increasing risk of death or other causes of dropout over time in relation to younger individuals, we restrict the analysis to those who were not older than 58 in 2010.

[3]The analysis will also drop individuals for whom there have been one or more proxy interviews. The small variation across rounds and dependent variables in the number of observations is due to item nonresponse.

Throughout the remainder of the paper, we refer to the age-restricted balanced panel (comprised of the Early Sample and the Later Sample) as the Estimation Sample. In total, the Estimation Sample consists of approximately 5800 individuals from the Early Sample and 6200 from the Later Sample.

It is obviously hard to verify the statement (made in the data manual) that these monthly samples are randomly drawn. Indeed, nearly all variables can be affected by a panel effect. Either people can have changed their behaviour, can have more experience in answering the questions, or display survey fatigue. Moreover, for some key factual questions, a Dependent Interviewing tool was used in round 2 onwards: after answering a question, respondents were able to see the previous round's answers, which could alert them of mistakes.

However, when we look at age and gender, two variables that have been corrected with administrative data, we have strong indications that the Early Sample and Later Sample within the Estimation Sample are comparable. The equality of gender ratios across the two groups cannot be rejected, with a Chi-squared test of 0.07 (p-value = 0.78). Similarly, the equality of ages cannot be rejected at conventional significance levels either (Mann–Whitney test = 0.77, p-value = 0.44).

4 Empirical Results

4.1 Panel Effects in Subjective Data

The baseline empirical results based on Equation 8 are shown in Table 6. Let us first consider the results for overall life satisfaction in the first column. As discussed in the introduction, the downward time trend in life satisfaction for some panels (particularly the German SOEP) has been observed by many and there is evidence that this downward trend is due to panel effects rather than a true change in well-being within society. It is interesting to see that, for a different country and using a related though slightly different identification approach, a panel effect in data measuring overall life satisfaction seems to be confirmed. All β coefficients are negative in this model and three of the four are statistically significant. In absolute terms, we measure effects of 3.4%, 4.4%, and 3.1% of a standard deviation for participating for the second versus the first time, the fourth versus the third time, and the fifth versus the fourth time, respectively.

The null hypothesis that $\beta_2 = \beta_3 = \beta_4 = \beta_5 = 0$ can be rejected with an F-test of 3.48 (p-value = 0.007). One might however be concerned that the effects measured are due to small differences in samples rather than due to a panel effect, and a joint significance test would under such a scenario be very misleading. We hence need to investigate the results a bit further. First, we can note that in the raw data, life satisfaction in 2009 for the Early Sample is slightly though not significantly higher than life satisfaction for the Later Sample in 2010. The scores are 5.210 and 5.196 respectively, on a 7-point scale (see Table 11 in the Appendix). If we want to assume that there is no time effect between 2009 and 2010, the sampling bias, if any, would be in the opposite direction of the panel effect. Second, since we obviously prefer not to rely on this latter assumption, we collected repeated cross-sectional UK life satisfaction data from 8 Eurobarometer studies conducted over the years 2009 and 2010 (10,537 observations in total). We merged these data with the data for 2009 and 2010 from the Early Sample of the UKHLS. After standardizing life satisfaction for both the Eurobarometer and UKHLS samples separately (with mean zero and standard deviation one), we regressed standardized life satisfaction on a constant, a 2010 year dummy, a UKHLS dummy and an interaction of the latter two. The coefficient on the 2010 dummy is positive but small and insignificant, which suggests that the above-mentioned assumption of a negligible time effect could be reasonable. The coefficient on the interaction term, however, amounts to -0.062, and is statistically significant with a P-value of 0.022, which indicates a discrepancy in trends between the panel data of the UKHLS and the repeated cross-sectional data of the Eurobarometer. The negative interaction term is in line with the existence of a negative panel effect.

The other models suggest that there could also be panel conditioning for domain satisfaction, since, for all models, the joint significance test for the β coefficients indicate that we can reject the null at conventional significance levels. However, it needs be said that the results are often a bit less straightforward than in the case of general life satisfaction. Satisfaction with income seems to be most resistant to panel effects, with only a significant and negative coefficient for a β_3 of -0.11.

Next, one could wonder whether the estimates would improve if further factors are controlled for beyond the month-of-year dummies. Obvious candidates are gender and a quadratic in age (in 2010), since these variables are checked against administrative data and hence cannot be subject to panel conditioning themselves. The first-round values of other controls are included as well (i.e. the 2009 values for the Early Sample and the

2010 values for the Later Sample), which one could believe do not change much over time: marital status dummies, education dummies, and the number of children in the household. The results are shown in Table 7 and they show us that our conclusions do not change substantially: the p-values of the joint significance tests of the β coefficients even slightly improve.

Finally, while the respondents have taken part in all five rounds, some have skipped some of the questions, creating item nonresponses. Around 4% of the individuals who answered the subjective questions in the last round skipped these in the first round; hence, survey experience seems to reduce item nonresponse. To check whether the panel effect is driven by people who skipped the questions in early rounds but then answered them in later rounds, the regressions from Table 6 are repeated on those individuals who answered the subjective question in all five rounds. The results, shown in Table 8, show that the effect even becomes slightly more pronounced. As for the overall life satisfaction question, the P-value of the F-test for the joint significance of the β coefficients is lower for this smaller sample (0.0011 vs. 0.0075).

4.2 Consequences of Ignoring Panel Effects in Empirical Applications: An Illustration for Time Effects

While the nature of the UKHLS allows to measure panel effects through regressions, as in Equation 8, we can equally endeavour to use the setup to measure pure time effects that are not contaminated by panel effects and to compare them with time effects that are *not* corrected for the possible existence of panel effects.

To measure pure time effects, we could think of running a regression for each panel round $j = 1 \dots 5$ separately, which takes the form

$$y_{ij} = \alpha + \gamma_j t_{2009+j} + u_{ij} \tag{9}$$

where t_{2009+j} is a time dummy for 2010 in the first regression on data for round 1 (if $j = 1$), for 2011 in the second regression on data for round 2 (if $j = 2$), and so forth.

To lay the groundwork, let's consider the case of $j = 1$. In this case, we analyse data from the first round of the panel. In wave 1, the Early sample is interviewed for the first time in 2009, while the Later sample is interviewed for the first time in 2010. As the number of panel participations is the same for everyone, the coefficient $\gamma_{2009+j} = \gamma_{2010}$ hence measures a pure time effect, the difference in well-being between calendar years 2010 and 2009.

Table 9 summarizes the results of this exercise. The year-on-year comparisons in Table 9 show us very limited changes. Only general life satisfaction and income satisfaction seem to be responsive to time effects, which can be expected, since life satisfaction has been found to be affected by business cycles (De Neve et al., 2018). The very limited changes across the short period of our dependent variables is very reassuring, since one would not expect these measures to change much within a population over a short and relatively quiet time span. The opposite could indicate fundamental differences between the Early Sample and Later Sample.[4]

Next, after verifying pure time effects (taking into account the potential existence of panel effects), we could explore the consequences of measuring time effects ignoring those panel effects. Table 10 aims to serve this purpose and its layout is the same as that of Table 9. It reports the results from OLS regressions on the standardized subjective measures. Each OLS regression is run on a different subsample of the data. These subsets contain observations pooled across two consecutive periods k and $k-1$ and across the Early Sample and Later Sample. Since k takes values between 2010 to 2014, this means that there are five subsamples: a first containing data pooled across all data from 2010 and 2009, a second containing data pooled across 2011 and 2010, and so on.

The regression equations are of the form

$$y_{ik} = \alpha + \beta_k t_k + u_{it} \tag{10}$$

Results in Table 10 paint quite a different picture than in Table 9. For all satisfaction measures, we can find significant year-to-year changes, often significant with a p-value well below the 1% significance benchmark.

[4] Since we work with a balanced panel, our sample ages over time. However, since the time span is very short and since subjective variables generally change smoothly over the life cycle, it seems reasonable to assume that the contamination of the results by age effects is negligible.

5 Conclusion

This paper studied the phenomenon of panel conditioning or a panel effect, the fact that self-reported well-being scores might depend, ceteris paribus, on how often one has participated into a panel. The paper first makes explicit identification issues that could arise when trying to estimate panel effects and which have been causing confusion in this growing literature. The exposition shows that in regular panel data regressions which also control for potential time effects, identification of the panel effects either relies on members not being present in each round or on restrictions put on the estimates of time effects. Simulations illustrate the severity of the bias for estimates of panel and time effects if non-presence is not random or if the restrictions put on the time effects are not entirely correct.

Second, the paper makes an empirical contribution by exploiting a design feature of the UKHLS that arguably introduces random variation of panel experience within each calendar year. The analysis confirms the existence of a negative panel effect in overall life satisfaction, and it demonstrates potential panel effects in domain satisfaction.

Finally, the paper also demonstrates how ignoring potential panel effects can lead to wrong conclusions: while pure time effects (after taking into account panel effects) show no or very little significant year-to-year changes in the different subjective measures, an analysis not taking into account these panel effects suggests that the subjective variables under study are very susceptible to year-to-year time shocks.

Many panel datasets contain satisfaction data in each round. Questions that explicitly ask people to state their preferences or to assess aspects of their personality are often only measured once for each individual, but the growing interest in these measures is leading to growing demand in repeated measurement as well. The results of this paper could add to the debate on the method and frequency of collection of these data. For example, if an empirical framework does not necessarily require that one control for individual fixed effects but, rather, for group fixed effects, one could gather the required data through repeated cross sections and transform these into pseudo-panel data (Deaton, 1985). This paper could thus make a case for a repetition of the Gallup preference module described by Falk et al. (2016) rather than for spending resources on repeated measurements within a panel study.

While this paper is concerned with (ways of) identifying panel effects, it does not aim to make strong statements about the mechanisms behind them and, at this stage, explanations can only be speculative. Indeed, self-reflective questions on well-being are not easy to answer: in later rounds, fewer people skip these questions than in early rounds, but the analysis in this paper suggests that a diminishing item nonresponse does not seem to be driving the effect. Studer and Winkelmann (2011) have found that people who took more time to answer a well-being question give, on average, a lower rating. This is in line with the findings of a negative panel effect, where experienced respondents might factor in more aspects. Chadi (2013) finds that encountering a new interviewer can lead to an increased reported life satisfaction score; hence, a trust relationship could make experienced respondents more confident in disclosing a lower score. Qualitative research that, for example, confronts respondents with changes in responses over time or quantitative research based on experiments which, for example, randomly change the survey mode could help us better understand the causes of this phenomenon.

Finally, it is clear that much work still needs to be done on panel effects: the existence of a panel effect should be tested for on a large variety of personality measures and stated preferences. We should also obtain better insights into the circumstances under which they do or do not occur. However, opportunities to investigate panel effects are rather scarce given the identification challenges. Hence, when designing new panel studies or innovation panels, one might want to keep research on panel effects in mind and try to ensure that there is *random* variation of panel participations within a calendar year.

Acknowledgments

I have benefited from many discussions and conference presentations to develop my research on panel conditioning. For this project, I would especially like to thank the Institute for Social and Economic Research to have given me the opportunity to present my work. In particular, I am grateful to Tom Crossley for the discussion on the identification issues, and to Paul Fisher and Annette Jaeckle, who have pointed me to the UKHLS to extend the empirical analysis. I also thank Sarah Brown, Steven McIntosh, Arne Risa Hole, Karl Taylor, the editor and three anonymous referees for their valuable comments. This project also benefited from financial support from the European Commission (Grant H2020-MSCA-IF-2014 660955).

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A Subjective Data: Overview and Description

The UKHLS contains a range of variables useful for the purpose of this study. They are all gathered throughout a self-completion questionnaire, which means that the interference of the interviewer should have a minimal impact on the results.

This appendix presents a few subjective well-being variables (general and domain satisfactions) available in the UKHLS.

All rounds of the UKHLS contain the following questions concerning general life and domain satisfaction.

> Please tick the number which you feel best describes how dissatisfied or satisfied you are with the following aspects of your current situation. 1 = Completely Dissatisfied, 7 = Completely Satisfied
>
> - Your health;
> - The income of your household;
> - The amount of leisure time you have;
> - Your life overall.

By construction, the four variables have a minimum value of one and a maximum value of seven. To obtain an idea on how raw scores vary over time, Table 11 shows the means and standard deviations of the satisfaction variables for each calendar year, for the Early Sample and Later Sample separately. Interestingly, all the columns suggest a downward trend, a phenomenon that has been noticed in other panel datasets. Such descriptive statistics obviously offer us only a mixture of time effects and panel effects.

Table 1: Extract Prototype Panel for the Key Independent Variables when Studying Panel Effects

Person ID	α	p_2	p_3	t_2	t_3
1	1	0	0	0	0
2	1	0	0	1	0
3	1	0	0	0	1
4	1	0	0	0	0
4	1	1	0	1	0
5	1	0	0	0	0
5	1	1	0	0	1
6	1	0	0	1	0
6	1	1	0	0	1
7	1	0	0	0	0
7	1	1	0	1	0
7	1	0	1	0	1

In this table, α is an overall constant; p_2 and p_3 are dummies for second- and third-time survey participation, respectively; and t_2 and t_3 are dummies for periods 2 and 3, respectively.

Table 2: First-Differenced Version of the Dataset in Table 1

Person ID	$\ddot{p}_2$	$\ddot{p}_3$	$\ddot{t}_2$	$\ddot{t}_3$
4	1	0	1	0
6	1	0	-1	1
7	1	0	1	0
7	-1	1	-1	1

Table 3: Distribution of Observations across Periods and Panel Participation Cells in the Simulated Data

Period	No. of Times in Panel	Observations
1	1	12931
2	1	319
2	2	11359
3	1	322
3	2	777
3	3	10017

Table 4: Estimates of Panel Effects on Simulated Data for Different Identifying Assumptions

	Estimates				
VARIABLES	Spec1	Spec2	Spec3	Spec4	True Value
β_2	-0.0712	-0.3596***	-0.0898***	0.1752***	-0.1
	(0.044)	(0.044)	(0.009)	(0.032)	
β_3	-0.2674***	-0.9713***	-0.3089***	0.2210***	-0.3
	(0.044)	(0.044)	(0.066)	(0.014)	
γ_2	-0.0021	0.2920***		-0.2650***	0
	(0.044)	(0.044)		(0.032)	
γ_3	0.4950***	1.1989***	0.5299***		0.5
	(0.044)	(0.044)	(0.063)		
	-0.0168*	-0.0168*			
	(0.009)	(0.009)			
Observations	35,725	35,725	21,638	21,638	
R-Squared	0.048	0.048			

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

Table 5: Maximum Number of Participations in the Panel by Survey Year and by Subsample

Survey Year	*Early Sample*	*Later Sample*
2009	1	NA
2010	2	1
2011	3	2
2012	4	3
2013	5	4
2014	NA	5

Table 6: Effect of Panel Participations on Subjective Data: Results from OLS Regressions on Standardized Data

VARIABLES	Overall	Health	Inc	Leisure
β_2	-0.0345*	-0.0373**	0.0180	-0.0220
	(0.019)	(0.019)	(0.019)	(0.019)
β_3	-0.0242	-0.2226***	-0.1156***	-0.0473***
	(0.018)	(0.018)	(0.019)	(0.018)
β_4	-0.0437**	0.0467**	0.0139	0.0343*
	(0.018)	(0.018)	(0.018)	(0.018)
β_5	-0.0309*	-0.0161	0.0214	-0.0056
	(0.018)	(0.018)	(0.018)	(0.018)
p-Value F-Test Joint Significance β Coefficients				
	0.0075	0.0000	0.0000	0.0185
γ_2	-0.0682***	-0.0274	-0.0104	-0.0188
	(0.018)	(0.018)	(0.019)	(0.018)
γ_3	-0.1001***	-0.2599***	-0.1239***	-0.0931***
	(0.018)	(0.018)	(0.018)	(0.018)
γ_4	-0.1632***	-0.2336***	-0.1161***	-0.0905***
	(0.018)	(0.018)	(0.018)	(0.018)
Month = 2	-0.0301	-0.0398*	-0.0090	0.0039
	(0.022)	(0.022)	(0.022)	(0.022)
Month = 3	-0.0180	-0.0198	0.0021	-0.0222
	(0.022)	(0.022)	(0.022)	(0.022)
Month = 4	0.0114	-0.0171	0.0046	0.0097
	(0.023)	(0.023)	(0.023)	(0.023)
Month = 5	-0.0001	-0.0258	-0.0169	-0.0145
	(0.023)	(0.023)	(0.023)	(0.023)
Month = 6	-0.0172	-0.0170	-0.0042	-0.0291
	(0.022)	(0.022)	(0.022)	(0.022)
Month = 7	0.0102	-0.0152	-0.0248	0.0367*
	(0.022)	(0.022)	(0.022)	(0.022)
Month = 8	0.0502**	-0.0191	0.0081	0.0571**
	(0.023)	(0.023)	(0.023)	(0.023)
Month = 9	0.0112	-0.0271	0.0043	-0.0379*
	(0.022)	(0.022)	(0.022)	(0.022)
Month = 10	-0.0040	-0.0145	0.0063	-0.0090
	(0.023)	(0.023)	(0.023)	(0.023)
Month = 11	-0.0314	-0.0285	-0.0453**	-0.0618***
	(0.022)	(0.022)	(0.022)	(0.022)
Month = 12	-0.0197	-0.0057	-0.0175	-0.0415*
	(0.022)	(0.022)	(0.022)	(0.022)
Observations	47,282	47,302	47,273	47,284
R-Squared	0.004	0.011	0.003	0.002

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

Table 7: Effect of Panel Participations on Subjective Data: Repeating the Analysis of Table 6 with Additional Controls from Wave 1

VARIABLES	Overall	Health	Inc	Leisure
β_2	-0.0343*	-0.0359*	0.0152	-0.0203
	(0.018)	(0.018)	(0.018)	(0.018)
β_3	-0.0249	-0.2240***	-0.1178***	-0.0436**
	(0.018)	(0.018)	(0.018)	(0.018)
β_4	-0.0435**	0.0478***	0.0103	0.0375**
	(0.018)	(0.018)	(0.018)	(0.018)
β_5	-0.0326*	-0.0172	0.0145	-0.0040
	(0.018)	(0.018)	(0.018)	(0.018)
p-Value F-Test Joint Significance β Coefficients				
	0.0051	0.0000	0.0000	0.0215
γ_2	-0.0675***	-0.0262	-0.0110	-0.0197
	(0.018)	(0.018)	(0.018)	(0.018)
γ_3	-0.0992***	-0.2600***	-0.1221***	-0.0927***
	(0.018)	(0.018)	(0.018)	(0.018)
γ_4	-0.1611***	-0.2324***	-0.1127***	-0.0899***
	(0.018)	(0.018)	(0.018)	(0.018)
Age in 2010	-0.0470***	-0.0231***	-0.0431***	-0.0618***
	(0.003)	(0.003)	(0.003)	(0.003)
Age in 2010 squared	0.0005***	0.0002***	0.0005***	0.0008***
	(0.000)	(0.000)	(0.000)	(0.000)
Female	0.0275***	-0.0385***	-0.0096	0.0067
	(0.009)	(0.009)	(0.009)	(0.009)
Month-of-year dummies	YES	YES	YES	YES
Marital Status in Wave 1				
Married	0.2738***	0.1120***	0.2192***	0.0142
	(0.012)	(0.012)	(0.012)	(0.012)
Civil partnership	0.3873***	0.0134	0.4627***	0.1293*
	(0.071)	(0.072)	(0.071)	(0.072)
Separated but legally married	-0.0727**	-0.0528*	-0.2213***	-0.1402***
	(0.029)	(0.029)	(0.029)	(0.029)
Divorced	-0.0273	-0.0759***	-0.1118***	-0.1164***
	(0.018)	(0.018)	(0.018)	(0.018)
Widowed	0.0183	0.0475	-0.0607	-0.0286
	(0.043)	(0.043)	(0.043)	(0.043)
Separated from civil partner	0.2973	0.1450	0.0429	-0.0903
	(0.368)	(0.374)	(0.370)	(0.370)
Other higher degree	-0.1114***	-0.1394***	-0.2115***	-0.0440***
	(0.015)	(0.015)	(0.015)	(0.015)
A-level	-0.1591***	-0.1447***	-0.2808***	-0.0509***
	(0.013)	(0.013)	(0.013)	(0.013)
GCSE	-0.1874***	-0.1871***	-0.3442***	-0.0359***
	(0.013)	(0.013)	(0.013)	(0.013)
Other qualification	-0.3086***	-0.3346***	-0.4519***	-0.0754***
	(0.018)	(0.019)	(0.019)	(0.019)
No qualification = 9	-0.4021***	-0.3807***	-0.5375***	-0.0930***
	(0.020)	(0.020)	(0.020)	(0.020)
Number of kids in wave 1	-0.0147***	-0.0088*	-0.0541***	-0.0562***
	(0.005)	(0.005)	(0.005)	(0.005)
Observations	47,255	47,275	47,246	47,257
R-squared	0.043	0.037	0.056	0.022

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

Table 8: Effect of Panel Participations on Subjective Data: Repeating the Analysis of Table 6 on Individuals with No Item Nonresponse for the Dependent Variable in One or More Waves

VARIABLES	Overall	Health	Inc	Leisure
β_2	-0.0359*	-0.0413**	0.0079	-0.0216
	(0.020)	(0.020)	(0.020)	(0.020)
β_3	-0.0328	-0.2363***	-0.1232***	-0.0502**
	(0.020)	(0.020)	(0.020)	(0.020)
β_4	-0.0608***	0.0358*	0.0094	0.0374*
	(0.020)	(0.020)	(0.020)	(0.020)
β_5	-0.0355*	-0.0261	0.0174	0.0040
	(0.020)	(0.020)	(0.020)	(0.020)
p-Value F-Test Joint Significance β-coefficients				
	0.0011	0.0000	0.0000	0.0284
γ_2	-0.0606***	-0.0183	-0.0165	-0.0126
	(0.020)	(0.020)	(0.020)	(0.020)
γ_3	-0.0810***	-0.2586***	-0.1228***	-0.0934***
	(0.020)	(0.020)	(0.020)	(0.020)
γ_4	-0.1566***	-0.2282***	-0.1187***	-0.0952***
	(0.020)	(0.020)	(0.020)	(0.020)
Month = 2	-0.0029	-0.0183	0.0280	0.0255
	(0.024)	(0.024)	(0.024)	(0.024)
Month = 3	-0.0199	-0.0222	0.0105	-0.0148
	(0.024)	(0.025)	(0.025)	(0.024)
Month = 4	0.0167	-0.0084	0.0222	0.0216
	(0.025)	(0.025)	(0.025)	(0.025)
Month = 5	0.0357	-0.0065	0.0177	0.0091
	(0.025)	(0.025)	(0.025)	(0.025)
Month = 6	0.0078	-0.0118	0.0321	-0.0186
	(0.024)	(0.024)	(0.025)	(0.024)
Month = 7	0.0257	0.0132	-0.0035	0.0656***
	(0.024)	(0.024)	(0.024)	(0.024)
Month = 8	0.0911***	0.0042	0.0381	0.0804***
	(0.025)	(0.025)	(0.025)	(0.025)
Month = 9	0.0438*	-0.0139	0.0318	-0.0123
	(0.024)	(0.025)	(0.025)	(0.024)
Month = 10	-0.0014	-0.0258	0.0192	0.0034
	(0.025)	(0.025)	(0.025)	(0.025)
Month = 11	-0.0074	-0.0170	-0.0278	-0.0422*
	(0.024)	(0.024)	(0.025)	(0.024)
Month = 12	0.0151	0.0136	0.0268	-0.0125
	(0.024)	(0.025)	(0.025)	(0.024)
Observations	38,348	38,388	38,328	38,372
R-Squared	0.005	0.012	0.004	0.003

Standard errors in parentheses

*** p<0.01, ** p<0.05, * p<0.1

Table 9: Time Effects in Subjective Data: Results from OLS Regressions on Standardized Data

	Overall	Health	Inc.	Leisure
2010 vs. 2009	-0.009	-0.008	-0.051	-0.009
	(0.017)	(0.018)	(0.018)***	(0.018)
Observations	11.491	11,483	11,477	11,488
2011 vs. 2010	-0.034	0.010	-0.029	0.003
	(0.018)*	(0.018)	(0.018)	(0.018)
Observations	11.485	11,500	11,484	11,487
2012 vs. 2011	-0.008	-0.010	0.002	-0.028
	(0.018)	(0.019)	(0.018)	(0.018)
Observations	11,982	11,981	11,978	11,982
2013 vs. 2012	-0.019	-0.021	-0.006	-0.033
	(0.018)	(0.019)	(0.019)	(0.018)*
Observations	12,085	12,084	12,084	12,087
2014 vs. 2013	0.061	0.002	0.051	0.010
	(0.018)***	(0.019)	(0.018)***	(0.018)
Observations	12,058	12,058	12,056	12,058

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table 10: Estimates of Time Effects Ignoring the Existence of Panel Effects: Results from OLS Regressions on Standardized Subjective Data

	Overall	Health	Inc.	Leisure
2010 vs. 2009	-0.026	-0.026	-0.042	-0.019
	(0.015)*	(0.015)*	(0.016)***	(0.016)
Observations	17,046	17,041	17,033	17,042
2011 vs. 2010	-0.063	-0.119	-0.076	-0.032
	(0.013)***	(0.013)***	(0.013)***	(0.013)**
Observations	23,164	23,185	23,159	23,165
2012 vs. 2011	-0.042	-0.100	-0.050	-0.034
	(0.013)***	(0.013)***	(0.013)***	(0.013)***
Observations	23,771	23,779	23,764	23,775
2013 vs. 2012	-0.057	-0.004	0.012	-0.017
	(0.013)***	(0.013)	(0.013)	(0.013)
Observations	24,118	24,117	24,114	24,119
2014 vs. 2013	0.045	-0.006	0.062	0.008
	(0.016)***	(0.016)	(0.016)***	(0.016)
Observations	18,284	18,286	18,284	18,283

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

Table 11: Mean and Standard Deviation of Subjective Variables per Calendar Year for the Early and Later Samples

VARIABLES	Overall	Health	Inc	Leisure
		Early Sample		
2009	5.2098	5.0036	4.5109	4.4935
	(1.3892)	(1.6085)	(1.6690)	(1.5705)
2010	5.1449	4.9253	4.4546	4.4440
	(1.4458)	(1.6263)	(1.6921)	(1.5922)
2011	5.0586	4.5552	4.2068	4.3720
	(1.4814)	(1.8318)	(1.7325)	(1.6024)
2012	4.9802	4.6192	4.2330	4.3841
	(1.4920)	(1.7694)	(1.7507)	(1.6154)
2013	4.9059	4.5553	4.2605	4.3226
	(1.5189)	(1.7822)	(1.7124)	(1.6139)
		Later Sample		
2010	5.1962	4.9901	4.4235	4.4791
	(1.4177)	(1.6196)	(1.7227)	(1.5987)
2011	5.0941	4.9424	4.4054	4.4485
	(1.4752)	(1.6410)	(1.7066)	(1.6057)
2012	5.0463	4.5373	4.2097	4.3271
	(1.5062)	(1.8364)	(1.7494)	(1.6346)
2013	4.9517	4.5832	4.2231	4.3311
	(1.5292)	(1.8075)	(1.7785)	(1.6359)
2014	4.9965	4.5588	4.3477	4.3397
	(1.4979)	(1.7744)	(1.7246)	(1.6187)