Dear reviewer,

Many thanks for your comments. I have revised the letter accordingly, and written responses to each of your points below (your bullet points in plain text, my response in bold italic).

Your sincerely,

Edmund Crawley

• You write: “There is little formal evidence on the distribution of shocks throughout the year. While this assumption is unlikely to be strictly true, it is more reasonable than the implicit assumption of BPP that shocks all occur 1st January each year.” I agree – but then, if I want to think really seriously about frequency of shocks, I would say that I don’t expect people to face permanent shocks very frequently. In fact, I think that economically significant permanent shocks happen at a frequency *lower* than a year! (Do people get a promotion every month or quarter? I wish I did.)

***Response: I agree that permanent shocks may occur at a low frequency. However, the time aggregation problem is not related to the frequency of shocks, but rather the distribution of shocks throughout the year. That is why I see it as important that the shock process, although in continuous time, is allowed to have jumps. I have added a sentence (first paragraph of page 6) that I hope clarifies this:***

***“Allowing for jumps accommodates low-frequency events, such changing job or getting a promotion, that may occur only once every few years, but when they do occur they can be at any point in the year.”***

• I wonder if some of the time aggregation bias may be attenuated if you control for seasonal effects in the first stage (before residualizing). In the PSID there is information on the date of the interview, and could be used as control (.

***Response: I have done this in the online appendix B4. I controlled for the interview date as a dummy variable divided into nine bins, reflecting the fact that earlier PSID waves report the interview date in this way. The results are almost identical to those in table 1 of the main paper. I have included this as an appendix rather than in the main paper as I want it to be clear that the quantitative change in estimates is purely the result of changing the model to accommodate time aggregation, and not due to other changes in the data.***

• I am surprised you comment on the estimates of ψ but say almost nothing on the estimates of φ (partial insurance wrt permanent shocks) – which is now much smaller than in BPP (implying way too much insurance - I am skeptical of this result). I know people are fixated with trying to reconcile the low ψ with the higher Parker et al-style estimates, but I’m not even sure they are the same object. Anyway, the point is that welfare-wise, φ is waaay more important than ψ ; and your lower estimate of it has much stronger welfare implications than a higher ψ. It makes me think there is misspecification somewhere, because, again, I’m skeptical such a low estimate make sense. You should explain where this low estimate is coming from (especially because, if I understand correctly, (8) tells us that the BPP IV moment should still identify φ unbiasedly). Is it the extra noisier moments playing too much role? What if you use just the IV expressions and exact identification? Alternatively, suppose you use (8) to pin down φ and then (9) and the income moments to pin down the other parameters – what do you get? That my help understand what is going on.

***Response: I agree that for welfare* φ** **is far more important than ψ. I have added the low φ as a puzzle in the abstract, as well as a new section on page 9 titled “A New Puzzle: Too Much Permanent Insurance?”**

**My investigations indicate that the low φ is a robust feature of the CEX/PSID data, and it is the high estimate obtained in the original BPP paper that is a hard to explain outlier.**

1. **Using equation (8) to directly identify φ recovers a value of 0.35, in line with the time-aggregated number.**
2. **I have repeated the BPP estimation using the original (non-time-aggregated) moments, but restricting the permanent variance to be non-time-varying. When I do this, I again recover 0.35 for φ (ψ remains close to zero). The estimate seems to be particularly sensitive to allowing the first and last years of permanent variance be different from the middle years. When I allow the first and last years’ variance to be free, but restrict the middle years to be non-time-varying, the estimate of φ gets close to the 0.65 obtained in the original BPP paper.**
3. **This sensitivity to the time-varying nature of permanent variance is not seen in the time-aggregated model. With time aggregation, the estimate for φ is close to 0.35 regardless of whether I allow the permanent variance to be time varying or not. Furthermore, in the time aggregated model the estimate of φ is not sensitive to the assumptions on the persistence of transitory shocks, where in the original BPP paper φ changes from 0.47 to 0.65 when persistence is introduced (see table 1).**
4. **The low φ is consistent will more basic features of the data, which would be hard to explain if φ were high:**
   1. **A simple cross-sectional regression of log nondurable consumption on log income in the CEX data recovers a coefficient of 0.17. The differences in cross sectional income are composed predominantly of permanent shocks (and idiosyncratic fixed effects), so if φ were high we would expect this coefficient to be much higher.**
   2. **A regression of the 10 year change in consumption on the 10 year change in log after tax income using the BPP imputed data recovers a coefficient of 0.23. Again, over 10 years the change in income will be predominantly permanent in nature, so this low coefficient is inconsistent with a high φ.**

**I conclude that the high φ estimated in the original BPP paper is a result of some interaction between time-aggregation and time-varying risk, but I have not been able identify the exact nature of this.**

**Note that the high ψ is incompatible with the assumption that consumption moves as a random walk, at least if the household budget constraint is to hold. This is a possible source of misspecification in the model. In appendix B5 I extend the model to allow for exponential decay in the consumption response to transitory shocks, and find it has a half life of approximately one year. However, this extension has not helped in understanding the high φ found in the original BPP paper.**

**While I believe the low φ is a robust feature of the CEX/PSID data, my leading hypothesis is that this a result of measurement error in consumption being correlated with income in the CEX data. I cite my own work using similar techniques on Danish data, and Gelman et al. who use gasoline prices to proxy for permanent income shocks, as examples where φ is estimated to be much higher.**

**In the chapter “Is the Consumer Expenditure Survey Representative by Income?" of the book “Improving the Measurement of Consumer Expenditures” the authors suggest such measurement error correlation is likely. They state of the CEX data, “The ratio of spending to income at low-income levels seems implausibly high, and the ratio of spending to income at the top seems implausibly low."**