• 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: The time aggregation problem is not related to the frequency of shocks, but rather the distribution of shocks throughout the year. I agree that permanent shocks may occur at a low frequency and 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 (bottom of page 5, top of page 6 \*\*\*\*\*\*\*\*\*\*\*\*\*\*\*\*\*\*\*\*\*\*\*\*\* ) that I hope clarifies this:*

*“Allowing for jumps accommodates low-frequency events, such changing job or getting a promotion, that may only occur 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 polluted by 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.