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Assessing Cohort Aggregation to Minimise Bias in Pseudo-Panels

by

Rumman Khan

Abstract

Pseudo-panels allow estimation of panel models when only repeated cross-sections are available. This involves grouping individuals into cohorts and using the cohort means as if they are observations in a genuine panel. Their practical use is constrained by a lack of consensus on how the pseudo-panels should be formed, particularly to address potential sampling error bias. We show that grouping can also create substantial aggregation bias, calling into question how well pseudo-panels can mimic panel estimates. We create two metrics for assessing the grouping process, one for each potential source of bias. If both metrics are above certain recommended values, the biases from aggregation and sampling error are minimised, meaning results can be interpreted as if they were from genuine panels.

JEL Classification: C13 C23 C81 D10 O12

Keywords: Pseudo-panel; Estimation bias; Sampling error; Aggregation bias; Repeated Cross-Section; Household Surveys

Centre for Research in Economic Development and International Trade, University of Nottingham

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Outline

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1. Introduction

The advantages of using panel datasets, which include both time-series and cross-section dimensions, for empirical analysis are well known. However, in many settings such data may not be available due to the cost and difficulty of following the same set of individual agents over a sufficiently long period of time. Instead, what often is available is repeated cross-sections (henceforth RCS) where a different set of individuals are observed in each time period. Many household surveys, particularly those covering a long time span, are of this form. This is especially true in developing countries where, since the 1980s, many such surveys have been conducted under the World Bank's Living Standards Measurement Survey (LSMS) project. Consequently, pseudo-panels have increasing been used as they allow for panel-type estimation with RCS data.

By grouping individuals into cohorts based on common characteristics that are fixed over time, pseudo-panels can be created by treating the cohort means as if they are observations in an actual panel. This method was first developed by Deaton (1985) in order to estimate a linear fixed effects model. The literature has since expanded to incorporate more complex models that otherwise could only be estimated using panel data. Examples include dynamic models (Moffitt, 1993; Girma, 2000; Verbeek and Vella 2005), duration analysis (Güell and Hu, 2006), incorporating parameter heterogeneity (McKenzie, 2004; Antman and McKenzie 2007a), and allowing for cohort interactive effects (Juodis, 2017). Pseudo-panels were initially used for estimating life cycle models of consumption and labour supply (see Table 1b for examples), but have since been applied to a broad range of topics. These include agricultural production (Heshmati and Kumbhakar, 1997; Paul and Nehring 2007), estimating price elasticities (Gardes et. al., 2005; Meng et. al., 2014), demand for medical insurance (Propper, Rees and Green, 2001), and a range of issues within the field of development economics (see Table 1a for examples).

The main concern with estimating pseudo-panels is bias arising from sampling error due to the cohort sample means not being representative of the underlying cohort population. The literature addresses this by focusing on cell size (the number of individuals grouped together to form a cohort) and whether they are large enough that sampling error is minimised. However, there is no consensus and little guidance on how large they should be with suggestions ranging from 100 or less (Verbeek and Nijman, 1992; Imai et. al., 2014) to potentially several thousand (Devereux, 2007b). Furthermore, grouping individuals into cohorts may create aggregation bias, something the literature has mostly ignored, which may be exacerbated by the creation of larger cell sizes as that can only be done by reducing the number of cohorts that individuals are aggregated into. This makes the grouping process difficult as there can be a trade-off between which of the two sources of bias one addresses.

We show that sampling error cannot be addressed by focusing upon cell size alone as the variation created in the cohort data needs to be considered also. We combine these two factors into a single metric, called CAWAR, which can easily be calculated in practice. Using Monte Carlo simulations, we find critical values of CAWAR beyond which sampling error bias is minimised. Then, by applying pseudo-panels onto a panel dataset, aggregation bias is explored. We show the bias can be substantial, often negating any benefit to using pseudo-panels over OLS, which calls into question the validity of some existing applications. Aggregation bias is also linked to sampling error as both depend on the cohort level variation; the former can be assessed using a similar metric, called AWAR, which ignores cell size. We find critical values for the metric in an empirical application. To our knowledge we are the first to provide such measures that can be used to formally assess the grouping process, providing some much needed guidance to one of the main drawbacks of the practical application of pseudo-panels.

2. Sampling Error

2.1 Sampling Error in a Linear Fixed Effect Model

Consider the following static linear model with an additive unobserved individual specific effect θ_i

$$y_{it} = x_{it}'\beta + \theta_i + \varepsilon_{it} \qquad i = 1,2,\ldots,\mathrm{N} \quad t = 1,2,\ldots,\mathrm{T} \tag{1}$$

where y_{it} is the dependent variable of interest, x_{it} is a vector of explanatory variables, i indexes individuals, t indexes time periods, and ε_{it} is an idiosyncratic error term uncorrelated with y_{it}, x_{it}, and θ_i. Under such circumstances pooled OLS (POLS) is inefficient if $Cov(\boldsymbol{x}_{it},\theta_i)=0$ and a random effects model is appropriate. If instead $Cov(\boldsymbol{x}_{it},\theta_i)\neq 0$ then POLS is also biased and a fixed effects model, which eliminates θ_i using a within or first difference transformation, is appropriate. In many applications, the individual effects are likely to be correlated with explanatory variables, but fixed effects models can only be estimated if panel data exist where the same set of individuals are tracked over time.

Deaton (1985) suggests a methodology for consistently estimating parameters using repeated cross-sections, even in the presence of individual effects that are correlated with regressors and where a valid external instrument cannot be found. This is done by grouping individuals into cohorts based on common characteristics that are time-invariant and observed in all cross-sections, the classic example is year-of-birth of the individual or household head. Then by taking the means of each cohort in each time period a synthetic or 'pseudo' panel can be created by treating the cohort means as if they were observations in a genuine panel. Formally, this amounts to grouping the N individuals into c cohorts (where c=1,2,…,C), with each cohort having n_c members (also known as the cell size), and thus $N=C\times n_c$. The cohort version of the model in equation (1) is then:

$$\bar{y}_{ct}=\bar{\boldsymbol{x}}'_{ct}\bar{\beta}+\bar{\theta}_{ct}+\bar{\varepsilon}_{\mathrm{ct}}\qquad c=1,2,\ldots,\mathrm{C}\quad t=1,2,\ldots,\mathrm{T}\tag{2}$$

As there are additive individual fixed effects, there will be corresponding additive cohort fixed effects, shown by $\bar{\theta}_{ct}$. However, these cohort fixed effects, as they are the average fixed effects of all individuals in each cohort, may not be fixed over time because the set of individuals within each cohort changes over time. Furthermore, as $\bar{\theta}_{ct}$ is unobserved and will in general be correlated with $\bar{\boldsymbol{x}}'_{ct}$, neither cohort dummies nor a within or first difference transformation will account for the fixed effects. The only way to do so is if cell sizes are large enough that $\bar{\theta}_{ct}$ is a very good approximation of θ_c, the true cohort population fixed effect, which is fixed over time. In this case one can estimate (2) using OLS with cohort dummies, which is known as the efficient Wald estimator (henceforth EWALD) following Angrist (1991). Weighted least squares estimation using the square-root of the cell size as weights should be applied to address heteroscedasticity, which arises due to cell sizes varying across cohorts (Deaton, 1985; Dargay, 2007; Warunsiri and McNown, 2010).

If cell sizes are not large enough for $\overline{\theta}_{ct}$ to be considered a good approximations of θ_c, Deaton proposes an alternative errors-in-variables estimator (henceforth EVE). For this, an underlying unobserved cohort population version of equation (2) is proposed where the observed cohort sample means are considered as error-ridden estimates of the true population means. As the variances and covariances of these sample means can be easily calculated from the survey data, EVE can be used to incorporate a sampling error correction proposed by Fuller (1975, 1981). Deaton's EVE has since been shown to be biased when T is small as it over-corrects for sampling error, but bias-corrected EVEs have been proposed by Verbeek & Nijman (1993) and Devereux (2007a). Nevertheless the EVEs only correct for sampling error in simple linear models and even the introduction of a quadratic term would require more complex corrections (Wolter and Fuller, 1982; Kuha and Temple, 2003). Consequently, nearly all applications of pseudo-panel estimation have used the simpler and more flexible EWALD estimator.

2.2 Cell Size and Cohort Level Variation

As consistency of EWALD depends on $n_c \rightarrow \infty$ (Moffitt, 1993; Verbeek, 2008), the main focus regarding how to construct cohorts has been on whether cell sizes are sufficiently large. Verbeek & Nijman (1992) were the first to formally address this, showing that under certain assumptions the sampling error bias depends on two key factors; the true level of variance at the cohort population level (w_1), and the sampling error variance of the observed cohort means (w_2). Where:

$$w_1 = \lim_{C\to\infty} \frac{1}{CT} \sum_{c=1}^{C} \sum_{t=1}^{T} (x_{ct}^* - \overline{x}_c^*)^2 \qquad (3)$$

$$w_2 = \operatorname*{plim}_{C\to\infty} \frac{1}{CT} \sum_{c=1}^{C} \sum_{t=1}^{T} (\overline{x}_{ct} - x_{ct}^*)^2 = n_c^{-1} \sigma_v^2 \qquad (4)$$

x_{ct}^* are the unobserved cohort population means and $\overline{x}_c^* = \frac{1}{T} \sum_{t=1}^{T} x_{ct}^*$.

$\overline{x}_{ct}$ are the observed cohort sample means.

n_c is the cell size of cohort c.

σ_v^2 is the variance of individuals x_{it} observations in cohort c, capturing the homogeneity of individuals that are grouped together into a cohort.

As w_1 increases relative to w_2, the bias from sampling error decreases and so does the cell size required for consistent estimation. The authors show that if $w_1 / \sigma_v^2 = 0.5$ then cell sizes of 100-200 are sufficient for obtaining reasonably unbiased estimates. Whether this value of w_1 / σ_v^2 is appropriate in empirical applications is questionable; Devereux (2007b) has shown that even with cell sizes in the thousands, small sample biases may be difficult to eliminate. This discrepancy arises as Verbeek and Nijman do not account for the effect of a lack of time variation in the cohort level observations, which can further exacerbate the sampling error bias.

Although the issue of cell sizes and sampling error has dominated the pseudo-panel literature, both in theoretical and applied studies, the number of cohorts one aggregates into is also an important consideration. The matter is usually discussed as a bias-vs-efficiency trade-off; with fixed *N*, larger cell sizes (n_c) can only be obtained by reducing the number of cohorts, the latter results in reduced efficiency as there are fewer observations in the cohort panel. However, if we consider how cohorts are constructed in practice, thinking of cohort construction as a bias-vs-efficiency can be problematic. Tables 1a-c shows a sample of how cohorts have been constructed in various applications. In order to increase the number of cohorts, one has to either use additional construction variables or use finer categories of existing variables. This will change the underlying cohort population structure and therefore will also change w_1, σ_v^2, and time variation. It is therefore possible for bias to fall as c increases if there is an increase in w_1 / σ_v^2 or time variation that offsets the effects of a lower cell size.

Table 1a: Pseudo-Panel Estimation in Development Economics

Article	Cohorts	Cell size	Cohort construction
Bedi et al. (2004)	38	350	Districts in Kenya
Christiaensen & Subbarao (2005)	799	10	Communities in Kenya
Nicita (2009)	63		States in Mexico, location (urban or rural)
Antman & McKenzie (2007b)	15	100+	5 year age cohorts, education
Warunsiri & McNown (2010)	22 11	200+ 300+	1 year age cohorts 2 year age cohorts
Cuesta et al. (2011)	224	130	7 year age cohorts, gender, country
Sprietsma (2012)	108	130	States in Brazil, gender, ethnicity
Échevin (2013)	166	115.5	5 year age cohorts, education, region
Fulford (2014)	200+		5 year age cohorts, region, gender
Imai et al. (2014)	140	73.6	5 year age cohorts, region
Shimeles & Ncube (2015)	400+	500+	Age cohorts, gender, country
Arestoff & Djemai (2016)	175	230-580	1 year age cohorts, country
Himaz & Aturupane (2016)	21 11	318 608	1 year age cohorts 2 year age cohorts
Gómez Soler (2016)	6000+	70-80	Schools in Colombia

Table 1b: Pseudo-Panel Estimation of Consumption and Labour Supply

Article	Cohorts	Cell size	Cohort construction
Browning, Deaton & Irish (1985)	16	192	5 year age cohorts, type of worker
Banks, Blundell, & Preston (1994)	11	354	5 year age cohorts
Blundell, Browning, & Meghir (1994)	9	520	5 year age cohorts
Deaton & Paxson (1994)	56 14 11	300-400 200-400 150-200	1 year age cohorts for Taiwanese data 5 year cohorts for US data 5 year cohorts for British data
Alessie, Devereux, & Weber (1997)	5	250+	10 year age cohorts
Blundell, Duncan, & Meghir (1998)	8	142	10 year age cohorts, education
Fernandez-Villaverde & Krueger (2007)	10	350	5 year age cohorts
Attanasio et al. (2009)	15	500+	5 year age cohorts
Rupert & Zanella (2015)	6	180 2000+	5 year age cohorts 5 year age cohorts

Table 1c: Pseudo-Panel Estimation of General Models of Individual Behaviour

Article	Cohorts	Cell size	Cohort construction
Gassner (1998)	27	226	2 year age cohorts
Dargay & Vythoulkas (1999)	16	513	5 year age cohorts
Propper et al. (2001)	70	80	5 year age cohorts, region
Dargay (2002)	41	190	5 year age cohorts, location
Gardes et al. (2005)			10 year age cohorts, education
Campbell & Cocco (2007)	7 9 12	200+ 150+ 100+	5 year age cohorts 10 year age cohorts, region 5 year age cohorts, if homeowner or renter
Bernard et al. (2011)	25	131	Region, Size of house
Jiang & Dunn (2013)	15		5 year age cohorts
Meng et al. (2014)	72	140	5 year age cohorts, gender, socioeconomic status, region

Consider the example of moving from a cohort aggregation specification that uses just 5-year age bands to one that also includes the gender of the individual. If gender is independent and unrelated to any of the explanatory variables then one can expect w_1 and σ_v^2 to be unchanged. Essentially each existing cohort has been split in half at random, thus there is no change in the expected value of the cohort means (leaving w_1 unchanged). The random division also leaves the similarity of individuals who are grouped together into cohorts unchanged (so σ_v^2 is unaffected). However, the higher the correlation between gender and the explanatory variable, particularly if there is also an interaction between gender and the age profile, the greater the additional variation created in the underlying cohort population means (larger w_1) and the higher the degree of homogeneity amongst individuals pooled into cohorts (smaller σ_v^2). In addition, higher correlation is also likely to increase the degree of time variation in the cohort level data. Intuitively, if there is no correlation and grouping is random, then the expected value of the cohort observations would be identical in each time period. Any time variation at the cohort level would also be identical, in expectation, for all cohorts and will be cancelled out by the inclusion of time effects. Correlation between the cohort selection variable and the explanatory variable is therefore necessary for cohorts to have their own individual time variation, which is important to limit the small sample bias of EWALD (Devereux, 2007b). Thus it is possible to envisage settings where increasing c by including extra variables in the cohort specification may reduce sampling error bias.

As the cell size required to address sampling error can be anywhere between 100 or fewer to in the thousands, it is necessary to find the size required for different aggregation methods. While 100 is achievable in applied settings (Tables 1a-c show most studies meet this criteria), cell size in the thousands would be difficult to create apart from in a few large datasets and particularly not those constructed under the LSMS. Calculating the required cell size is difficult as it depends on the level of variation in the cohort data, which is hard to identify. This is because w_1 and σ_v^2 are based on unobserved cohort population means (x_{ct}^*), and it is unclear how time variation is to be captured to address the concerns raised by Devereux (2007b). We address this shortcoming by finding suitable proxies for the three types of variation, combining them into one metric, and calculating suitable cell sizes at different values of the metric.

2.3 Deriving and testing AWAR

To combine the three types of variation into a single measure, we incorporate time variation into the w_1 / σ_v^2 ratio used by Verbeek and Nijman (1992) to calculate their recommended cell size. To do this we add the additional assumption that the cohort level explanatory variables follow an AR[1] specification:

$$x_{ct}^{*} = \rho x_{c(t-1)}^{*} + e_{ct} \tag{5}$$

Where x_{ct}^{*} represents the true cohort population means and e_{ct} is IID $N(0, \sigma_e^2)$. The variance of the cohort population means (w_1) is:

$$w_1 = \frac{\sigma_e^2}{1 - \rho^2} \tag{6}$$

Verbeek and Nijman implicitly assume ρ=0 and therefore $w_1 = \sigma_e^2$, so the overall level of variation in the cohort population means (w_1) is equivalent to the genuine level of variation across cohort observations (σ_e^2). However once autocorrelation is introduced w_1 may increase without there being any additional genuine variation across cohorts. Without correcting for autocorrelation w_1 is likely to overestimate the variation across the cohort level observations. Therefore, the ratio of interest should be σ_e^2 / σ_v^2 rather than w_1 / σ_v^2, which can be calculated as:

$$\frac{\sigma_e^2}{\sigma_v^2} = \frac{w_1(1 - \rho^2)}{\sigma_v^2} \tag{7}$$

For practical purposes we find it useful to rescale the metric to use standard deviations rather than variances to give a wider range of values, as in practice the metric in (7) often lies between 0 and 0.3. One can consider w_1 as the variation *across* cohort observations, while σ_v^2 is the variation of individuals *within* cohorts. Hence, we call the measure Across-to-Within Autocorrelation Adjusted Ratio (AWAR), calculated as:

$$\text{AWAR} = \frac{\sigma_{\text{e}}}{\sigma_v} = \frac{w_1^{1/2}(1-\rho^2)^{1/2}}{\sigma_v} \tag{8}$$

We proxy $w_1^{1/2}$ using the standard deviation of the cohort sample means, weighted by the square-root of cell size if cell sizes vary across cohorts. For σ_v, we take the mean (weighted by the square root of cell size) of the standard deviations for individuals grouped in each cohort. Time variation, captured by ρ, is estimated using the autocorrelation coefficient obtained by regressing the cohort means on their first lag and a constant term, with the square-root of cell size used as weights. We conduct Monte Carlo simulations to show that sampling error bias changes with AWAR and then find the cell sizes required to minimise the bias for different AWAR values.

The Monte Carlo setup is based mainly on Collado (1998), who tests different pseudo-panel estimators for a binary response model. Although our purpose is different, the data generating process used leads to a simple calculation and implementation of AWAR. The setup also allows us to focus on sampling error bias in a linear model similar to Devereux (2007b). We first generate the cohort population means (x_{ct}^*) as an AR[1] process as shown in (5). The initial period values of the cohort population means are generated as an IID $N(0, w_1)$ process where w_1 is, as before, the variance of the cohort population means as shown in (6). Similarly, e_{ct} is generated as IID $N(0, \sigma_{\text{e}}^2)$ and we discard the first ten cross-sections to ensure the w_1 values are as shown in (6). Then the individual level explanatory variable is generated as:

$$x_{it} = x_{ct}^* + v_{it} \qquad v_{it} \sim \text{i.i.d. } N(0, \sigma_{\text{v}}^2) \tag{9}$$

By changing σ_{e}^2 and σ_{v}^2 we can change AWAR, which is the square-root of the ratio of these two variances. For different values of AWAR we can then also vary ρ. We generate the unobserved heterogeneity, which is correlated with the explanatory variable, following Verbeek and Nijman (1992) as:

$$\theta_{\text{c}} = \lambda \bar{x}_c^* + \xi_c \tag{10}$$

where $\lambda = 1$, $\bar{x}_c^* = \frac{1}{T}\sum_{t=1}^{T} x_{ct}^*$, $\xi_c \sim$ i.i.d. $N(0,1)$ and $\text{E}[\xi_c \mid x_{ct}^*] = 0$ for all $t = 1,\ldots,\text{T}$

Finally, we generate the dependent variable at the individual level as follows:

$$y_{it} = x^{*}_{ct}\beta + \theta_c + f_t + u_{it} \tag{11}$$

where β =1, f_t are time fixed effects and u_{it} ~ i.i.d. $N(0,1)$

We generate 50 cohorts in each period with 4 periods in total (c=50, T=4), and vary the number of individuals in each cohort (n_c) so in each time period we have $50\times n_c$ observations at the individual level. We conduct 10,000 repetitions for each set of simulations. The AWAR values are calculated from the true parameter values for σ^2_e, σ^2_v and ρ, and we also report the sample AWAR which uses the proxies from the cohort sample means as described above. In this way we can see if there are any significant differences between the true underlying AWAR values and the reported sample values. We vary the underlying AWAR values between 0.1 and 1 in steps of 0.1 for each n_c. We report the mean value of the coefficient estimates across the 10,000 replications and the root-mean-squared-errors (RMSE). As the true coefficient value is set equal to unity, the mean estimates can be used to assess the average degree of bias (can interpret this in percentage terms by subtracting one from the mean estimate). The RMSE can be interpreted as showing the average absolute bias in percentages.

Table 2 shows the results for varying n_c between 30 and 500 over different AWAR values, where the latter is altered by just changing σ^2_e while keeping σ^2_v=1 and ρ=0.5. As predicted, the sampling error bias falls as AWAR increases for each cell size and the reverse is also true, with the bias falling as cell size increases for each AWAR value. If we assume estimates have sufficiently low bias to be considered accurate when the bias is less than 10%, then the highlighted cells give an indication of the minimum AWAR value required for accurate estimates for different cell sizes. One sees that cell size of 30 can be accurate if AWAR is over 0.6 (0.64 for sample AWAR) but may not be accurate even with cell size of 500 if AWAR is close to 0.1, showing the large impact of AWAR on required cell size. Thus it is possible that creating more cohorts may lead to less biased estimates even if cell sizes fall tenfold or more as long as AWAR increases sufficiently. How much AWAR actually varies in empirical applications will be demonstrated later, where we show that AWAR can lie between 0.1 and 0.6 depending on the cohort aggregation method used.

Table 2: Simulation Results for Varying the Cell Size

AWAR	Cell size														
	30			50			100			200			500		
	Sample AWAR	Mean	RMSE	Sample AWAR	Mean	RMSE	Sample AWAR	Mean	RMSE	Sample AWAR	Mean	RMSE	Sample AWAR	Mean	RMSE
0.1	0.21	0.206	0.799	0.18	0.301	0.703	0.15	0.462	0.543	0.13	0.631	0.375	0.11	0.812	0.194
0.2	0.28	0.508	0.497	0.25	0.632	0.374	0.23	0.774	0.232	0.21	0.872	0.134	0.21	0.945	0.062
0.3	0.36	0.698	0.308	0.34	0.793	0.213	0.32	0.886	0.121	0.31	0.939	0.067	0.30	0.975	0.031
0.4	0.45	0.805	0.202	0.43	0.873	0.134	0.41	0.932	0.074	0.41	0.965	0.041	0.40	0.986	0.020
0.5	0.54	0.865	0.141	0.52	0.915	0.092	0.51	0.956	0.051	0.50	0.977	0.029	0.50	0.991	0.015
0.6	0.64	0.903	0.103	0.62	0.939	0.068	0.61	0.968	0.038	0.60	0.984	0.022	0.60	0.994	0.012
0.7	0.73	0.928	0.079	0.72	0.954	0.052	0.71	0.977	0.029	0.70	0.988	0.017	0.70	0.995	0.009
0.8	0.83	0.943	0.063	0.81	0.965	0.041	0.81	0.982	0.024	0.80	0.991	0.014	0.80	0.996	0.008
0.9	0.93	0.954	0.052	0.91	0.972	0.034	0.90	0.986	0.020	0.90	0.993	0.012	0.90	0.997	0.007
1	1.02	0.962	0.044	1.01	0.977	0.029	1.00	0.989	0.017	1.00	0.994	0.011	1.00	0.998	0.006

Note: The simulations set T=4, C=50, ρ=0.5, σ_v^2 =1 and σ_e^2 is altered to vary AWAR. All simulations are conducted with 10,000 replications. Sample AWAR are the average values across all simulations.

The results in Table 2 are presented to highlight the importance of cohort variation when considering cell size. They are not to be interpreted as cell size and AWAR combinations to use in practice. This is because the results are only robust to varying the number of cohorts and to varying AWAR by changing σ_{v}^2 rather than σ_{e}^2 (although the latter part shows that only the ratio of the two variances matters for addressing bias and not their individual magnitudes). These results are not robust to changes in *T*, with larger *T* reducing the required AWAR for each cell size. The results are also generally not robust across the values of ρ unless *T* is around 4 or 5. To avoid the cumbersome process of having to calculate all the bias minimising combinations of cell size and AWAR across all potential values of *T* and ρ that one may encounter empirically, we collapse AWAR and cell size into a single metric called CAWAR, cell size adjusted AWAR.

2.4 Deriving and testing CAWAR

Recall that in (7) for AWAR the denominator is the variance across individuals within a cohort (σ_{v}^2). This is not the same as the sampling error variance, w_2, identified as the other factor alongside w_1 that is important for determining the bias of pseudo-panel estimates. To capture sampling error fully, one must incorporate cell size alongside σ_v^2 as shown in (4). Combining (4) and (7) we can derive CAWAR as (12) below, which fully accounts for sampling error. The same proxies can be used as before as CAWAR is equivalent to the square of AWAR multiplied by cell size. We use the square of AWAR as it is not necessary to rescale using standard deviations.

From (4) $w_2 = \dfrac{\sigma_{\mathrm{v}}^2}{n_c}$, From (7) $\dfrac{\sigma_{\mathrm{e}}^2}{\sigma_v^2} = \dfrac{w_1(1-\rho^2)}{\sigma_v^2}$

$$\therefore \mathrm{CAWAR} = \frac{\sigma_{\mathrm{e}}^2}{w_2} = \frac{w_1(1-\rho^2)n_c}{\sigma_v^2} = (\mathrm{AWAR})^2 \times n_c \tag{12}$$

Tables 3a and 3b show that a CAWAR value of at least 12 produces accurate estimates (again defined as estimates with less than 10 percent sampling error bias, both on average and in absolute terms) irrespective of cell size and number of cohorts. The sample CAWAR values suggest that a slightly higher threshold of 13 may be required in actual applications using the proxies identified. These results are robust to different absolute magnitudes of σ_e^2 and σ_v^2, with only their ratio being of importance.

Table 3a: Varying Cell Size for CAWAR

Cell Size	CAWAR of 12		
	Sample CAWAR	Mean	RMSE
30	13.39	0.912	0.095
50	13.27	0.912	0.095
100	13.21	0.912	0.094
150	13.18	0.911	0.095
200	13.17	0.912	0.095
250	13.16	0.912	0.094
500	13.16	0.911	0.095
1000	13.15	0.912	0.095

Note: The simulations set T=4, C=50, ρ=0.5, σ_v^2=1 and σ_e^2 is altered to keep CAWAR=12 while n_c changes. All simulations are conducted with 10,000 replications.

Table 3b: Varying the Number of Cohorts for CAWAR

Number of Cohorts	CAWAR of 12		
	Sample CAWAR	Mean	RMSE
20	13.13	0.912	0.105
50	13.29	0.911	0.095
100	13.31	0.912	0.092
200	13.35	0.912	0.090
500	13.37	0.912	0.089

Note: The simulations set T=4, n_c=50, ρ=0.5, σ_v^2=1 and σ_e^2=0.24 CAWAR. All simulations are conducted with 10,000 replications.

Table 4 presents the recommended CAWAR and sample CAWAR values for different combinations of *T* and ρ for up to 10 time periods (most repeated cross-sections are unlikely to extend beyond this, although the relevant thresholds can easily be found by expanding the simulations). The recommended CAWAR value varies greatly across *T*; being as high as 20 when there are two time periods, to as low as 6 when *T*=10. For each *T*, there is also some variation in the recommended CAWAR across different ρ. Interestingly when *T* is larger (more than 5) a higher ρ reduces the required threshold, whereas it increases for lower *T*. When *T* is 4 or 5, the threshold is quite consistent over ρ. The thresholds are also consistent over T when the autocorrelation coefficient is lower than 0.4, especially when T≥4 the recommended threshold remains at 10 (11 using Sample CAWAR).

Table 4: CAWAR Thresholds for Different Timer Periods

ρ	2 Time Periods				3 Time Periods			
	CAWAR	**Sample CAWAR**	**Mean**	**RMSE**	**CAWAR**	**Sample CAWAR**	**Mean**	**RMSE**
0	14	14.87	0.933	0.085	12	12.99	0.923	0.086
0.2	14	14.83	0.923	0.096	12	13.06	0.915	0.094
0.4	18	18.79	0.928	0.090	12	13.13	0.907	0.103
0.6	20	20.75	0.926	0.092	14	15.28	0.913	0.096
0.9	20	22.14	0.914	0.105	14	15.70	0.906	0.104

ρ	4 Time Periods				5 Time Periods			
	CAWAR	**Sample CAWAR**	**Mean**	**RMSE**	**CAWAR**	**Sample CAWAR**	**Mean**	**RMSE**
0	10	11.05	0.910	0.097	10	11.08	0.909	0.096
0.2	10	11.08	0.901	0.105	10	11.11	0.904	0.101
0.4	10	11.18	0.898	0.109	10	11.21	0.902	0.103
0.6	12	13.39	0.911	0.096	10	11.40	0.902	0.102
0.9	12	13.76	0.908	0.098	10	11.83	0.906	0.099

ρ	8 Time Periods				10 Time Periods			
	CAWAR	**Sample CAWAR**	**Mean**	**RMSE**	**CAWAR**	**Sample CAWAR**	**Mean**	**RMSE**
0	10	11.08	0.909	0.094	10	11.08	0.909	0.093
0.2	10	11.11	0.907	0.095	10	11.13	0.908	0.094
0.4	10	11.22	0.910	0.093	10	11.24	0.912	0.090
0.6	8	9.40	0.897	0.106	8	9.40	0.903	0.099
0.9	8	9.85	0.916	0.087	6	7.85	0.904	0.098

Note: The simulations set n_c=50, C=50, σ_v^2 =1 and σ_e^2 is altered to vary CAWAR. All simulations are conducted with 10,000 replications.

3. Aggregation Bias

The main concern in estimation of pseudo-panels is bias arising from sampling error. CAWAR, by combining both the cell size and cohort level variation, is a useful practical measure for assessing and limiting the likelihood of such bias, something that is lacking in the literature. However, sampling error bias may not be the only source of bias arising from the cohort grouping process. There may be aggregation bias, which arises when moving from the individual to the cohort level, potentially due to the loss of variation in the cohort level data or the existence of non-linearities that are difficult to capture using cohort averages. Pseudo-panels were initially used to estimate life-cycle models of consumption and labour supply, where the unit of analysis was the age-cohort itself (Table 1b), hence aggregation bias was not a concern. Applications in other fields, particularly development, are interested mainly at household or individual level analysis. Aggregation bias now becomes a concern as cohort panels are interpreted as if they contain individual level data. There is awareness of this issue and pseudo-panel studies in development economics (Table 1a) generally have larger *c* and use more cohort selection variables than those used to estimate life-cycle models (Table 1b). It is difficult to know whether this sufficiently addresses aggregation bias as the literature has mostly neglected such concerns.

3.1 Separating Aggregation and Sampling Error

Aggregation bias is likely to be related to sampling error bias as w_1, σ_v^2, and time variation are also linked to aggregation bias. Larger w_1 and time variation indicates the cohort means capture more of the distribution of the underlying individual level data, while smaller σ_v^2 ensures groups are more representative of their underlying sub-populations as more homogenous individuals are grouped together. It therefore may be difficult to disentangle sampling error from aggregation bias. Nevertheless, it is necessary to do so as the presence of the latter calls into question whether pseudo-panels can be analysed as if they are genuine individual-level panel estimates, as many existing studies do.

Bias from aggregation and sampling error can be separated by estimating pseudo-panels using panel data. Sampling error bias arises from the fact that $\overline{\theta}_{ct}$ is not constant over time as the individuals in a cohort change over time when using RCS data. However, when panel data are used the individuals grouped into cohorts are fixed over time, thus $\overline{\theta}_{ct}$ is also fixed irrespective of cell size. As a result, we can adjust the number of cohorts which captures effects of aggregation without affecting sampling error. Using the panel fixed effects estimator, the 'true' coefficients can be estimated and compared to the pseudo-panel estimates, where the difference can be thought of as the bias from aggregation. This assumes that the panel estimates are the true values, which may not be true due to concerns regarding attrition and measurement error, which will be discussed later. If pseudo-panel estimates are all similar to each other and to the panel estimates, aggregation is not a concern and the focus of constructing cohorts should be mainly on addressing sampling error.

3.2 Data and Estimation

The dataset we use to investigate aggregation bias is the Uganda National Panel Surveys (UNPS), using the four waves - 2005/06, 2009/10, 2010/2011 and 2011/2012. The original 2005/06 data is taken from the Uganda National Household Survey which contained a nationally representative sample of about 7,400 households. The panel is constructed by re-interviewing 3,123 households from 322 of the original 783 enumeration areas located all over the country. As not all households were able to be re-interviewed there is some attrition over the waves. The dataset contains detailed information on household consumption, income, wealth, labour market activities as well as information on the characteristics of individuals in the household such as their age and education level. The surveys are consistent with LSMS, so are similar to survey data available for other developing countries (the variables and the methods used to construct cohorts can be replicated for other countries with similar data).

We estimate a model of household welfare, measured by the natural logarithm of monthly household consumption per adult equivalent member (labelled *lcons*), following Deaton and Zaidi (2002). This model is chosen because it is a useful baseline for many empirical studies using such household data and fixed effects are likely to be present due to unobserved time invariant factors like preferences, the intra-household bargaining process, and innate characteristics of household members. The estimation model is shown below and is a general model of household consumption, similar to Glewwe (1991) and Appleton (1996) for example:

$$lcons_{it} = \alpha + \beta_1 \boldsymbol{HHsize} + \beta_2 \boldsymbol{ltotasset} + \beta_3 \boldsymbol{secondary} + \beta_4 \boldsymbol{lnremit} + \beta_5 \boldsymbol{remittance} + \beta_6 age + \beta_7 age^2 + \beta_8 education + \beta_9 \text{location} + \beta_{10} gender + \beta_{11} region + \beta_{12} \sec tor + f_i + f_t + e_{\text{it}} \quad (13)$$

Explanatory variables capture household characteristics; the number of household members (***HHsize***), gender of household head, as well as their age and education level (split into none, primary or post-primary). Income and assets variables are captured by the main sector of employment of the household head (*sector*), a dummy for whether they also engage in some secondary occupation (***secondary***), a dummy for whether the household received any remittances in the last year (***remittance***) as well as the log of the amount received (***lnremit***), and the log of total assets owned by the household (***ltotasset***). Geographic characteristics are captured by the region the household is from and whether it is based in an urban or rural location (*location*). Individual effects (f_i) and time effects (f_t) are also included. The variables highlighted in bold are the ones of interest as they vary across time and thus are not factored out by the inclusion of fixed effects; OLS uses all the explanatory variables whereas fixed effects includes only the ones in bold. Another reason for focusing only on these variables is because the other variables will be used to construct cohorts and would have to be excluded from the pseudo-panel regressions. Hence the pseudo-panel version of the model is:

$$lcons_{ct} = \alpha + \beta_1 \boldsymbol{HHsize} + \beta_2 \boldsymbol{ltotasset} + \beta_3 \boldsymbol{secondary} + \beta_4 \boldsymbol{lnremit} + \beta_5 \boldsymbol{remittance} + f_c + f_t + e_{\text{ct}} \quad (14)$$

We construct cohorts using variables that are widely available and relevant to other researchers using similar LSMS data to make our findings as general as possible. Important conditions for construction variables are that they are time invariant, exogenous and observed for all households in the sample. We use five variables commonly used in the literature to construct cohorts: the age of the household head, their gender, their education level, the region the household is from and whether it is in a rural or urban location. We exclude the use of socioeconomic variables as these are likely to be endogenous and many households change categories over time, particularly when looking over a long time period. We exclude ethnicity because it is not highly relevant in Uganda (but may be for other countries). Various age bands have been used in the literature, the most common being 5 year bands, however others have also used 1 year, 2 year and 10 year bands. We construct cohorts based on 2 year, 5 year, 10 year and 17 year age bands, giving us a good range to assess aggregation. Furthermore, we only use households whose head is aged 18-67 at the time of each survey.

Details of the other four grouping variables are given in Table 5, showing the number of categories the variables are divided into and the proportion of households in each category. These other four construction variables can be combined with age cohorts in 16 different ways (4 individually, 6 in pairs, 4 combinations of three variables, one which contains all four variables and another one where none are used). Combining these 16 different ways with the four different age bands described above gives 64 potentially different grouping methods to choose from. To this we add another 15 methods where age is not used but just different combinations of the other four construction variables. This gives a total of 79 different ways cohorts may be constructed. However not all of the 79 are viable because some will produce too few observations at the cohort level to run pseudo-panel regressions for efficient estimation. We ignore all groupings that lead to fewer than 40 cohort level observations in total (i.e. at least 10 per wave), which leaves a total of 65 potential cohort construction methods to choose from. There is a wide spectrum of how aggregated the cohort groupings are, with the number of cohorts for each wave being as small as 10 to others being over 400. This will be crucial in identifying bias arising from aggregation.

Table 5: Cohort Construction Variables

Variable	Categories	Number of Observations	Percentage of Total
Region	Central	3,071	31.2
	Eastern	2,261	23.0
	Northern	2,421	24.6
	Western	2,096	21.3
Gender	Male	7,102	72.1
	Female	2,747	27.9
Location	Rural	7,459	75.7
	Urban	2,390	24.3
Education	None	1,482	15.0
	Primary	5,277	53.6
	Post-primary	3,090	31.4

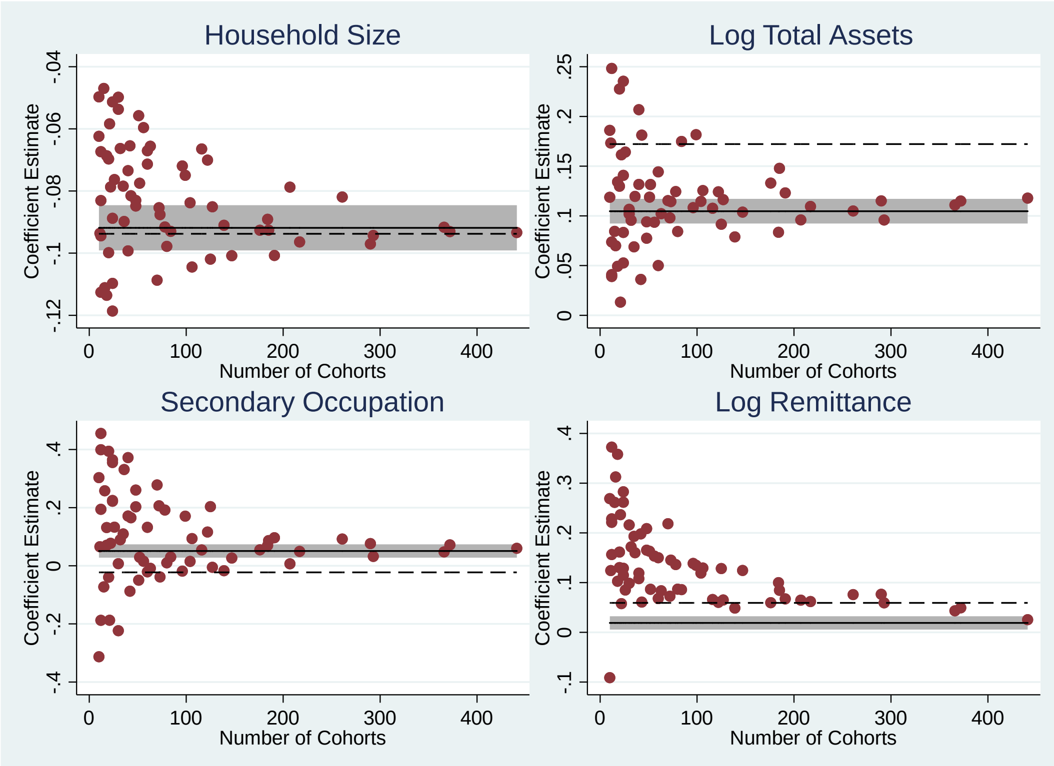
Due to attrition and household members splitting off to form news ones, which the UNPS also tracks, only around a half of all the households appear in all four waves (1,711 out of 3,404). Although using just these households would completely mitigate the sampling error problem and isolate aggregation bias, it may lead to bias from attrition both in the panel and pseudo-panel estimates. To limit this, we estimate using the full sample of households (where around three quarters of households appear in at least two waves), meaning sampling error is not fully addressed. We include the results using the fully balanced panel in Appendix A and they are largely identical. We estimate equation (14) with all 65 cohort specifications using EWALD with weights based on the square-root of cell size to address heteroscedasticity. We also drop all cohorts that contain less than two households to ensure we retain the grouped element and pseudo-panel results are not dominated by cohorts which essentially identify a single household thus making it more akin to a genuine panel.

3.3 Results

Results from the 65 pseudo-panel regressions are summarised in Figure 1, which graph the coefficient estimates for the four main explanatory variables against the number of cohorts created. We only show the estimates for *lnremit* and not *remittance* as their coefficients follow almost identical patterns. We also include the pooled OLS estimate (the dashed line), the 'true' panel fixed effects estimate (the solid line) and its 95% confidence interval (the shaded area).

Immediately evident is the large impact aggregation into cohorts has on pseudo-panel estimates, which range from being very close to the genuine panel estimates to being far worse than OLS. This latter issue, of being worse than OLS, highlights the point that the reason many pseudo-panel estimates are performing poorly is not because the cohort effects are failing to account for the individual effects but instead are coming from biases due to the aggregation process itself. This is best typified by the household size estimates where the OLS and fixed effects (henceforth FE) coefficients are almost identical, meaning accounting for individual heterogeneity has little effect. Nevertheless, most of the pseudo-panel estimates perform rather poorly, being quite far away from the "true" FE estimate and its 95% confidence interval. A similar problem occurs for the log remittances estimates, where most pseudo-panel estimates perform worse than OLS, containing a larger upward bias than attributable to the absence of fixed effects. Figure A1 in Appendix A shows the equivalent results for the fully balanced panel, which are qualitatively similar.

Figure 1: Pseudo-panel estimates of the 65 cohort construction methods



Note: The scatter plots are the pseudo-panel estimates of equation (14) obtained by the 65 different cohort construction methods. The solid line shows the "true" fixed effects estimates of equation (13) for just the main variables in bold and excluding the time invariant additional controls. The grey area is its 95% confidence interval. The dashed line shows the pooled OLS estimates of equation (13), including all the additional controls but not accounting for the individual effects. Both the OLS and fixed effects estimates are based on the household level panel data with the data trimmed at the 1st and 99th percentiles.

For log total assets, accounting for unobserved heterogeneity is necessary as the FE estimate is significantly smaller in magnitude than the OLS estimate. The pseudo-panel estimates generally outperform the OLS estimates, as the inclusion of cohort effects picks up the unobserved heterogeneity, and many of the estimates are very close to the "true" FE values. However, a significant proportion offer little improvement over OLS or are worse, showing that a reduction in bias from the inclusion of cohort effects can be offset by increased bias coming from aggregation. One can see a similar pattern for secondary occupation, for which including fixed effects is essential: OLS indicates a negative and significant coefficient whereas FE results in a positive and significant coefficient. The pseudo-panel estimates pick up the effect of fixed effects, with the vast majority of coefficient estimates being positive, although many are still highly biased (the magnitudes are often five to ten times larger than the "true" FE coefficient). It also demonstrates just how much impact cohort construction has on the coefficient estimates, which can vary from being smaller than -0.3 to larger than +0.4 even though the OLS and FE estimates are -0.02 and +0.05 respectively.

Estimates of all four variables improve as the number of cohort increases, consistent with bias from aggregation. In addition, for all variables except log remittance, aggregation does not affect the bias in a specific way: the pseudo-panel estimates are just as likely to be biased upwards as downwards. When the number of cohorts are low estimates appear random, often varying with a large distribution around a mean that is close to the 'true' fixed effects coefficient. When the number of cohorts is larger, at around 150-200 or more, pseudo-panel estimates generally converge to the 'true' coefficients. Consequently, pseudo-panel estimates can suffer from substantial aggregation bias, which in some cases can be so large that pooled OLS is a better alternative. However, cohorts can be constructed in a way to limit this and achieve estimates similar to panel fixed effects.

Table 6 contains a few of the pseudo-panel results, as well as the OLS and FE estimates, to demonstrate this. The third and fourth columns give examples of poor aggregation which results in unstable and inaccurate coefficient estimates, whereas the final two columns give examples of the opposite. The former pair are based on common cohort aggregation methods that can be found in the literature; one uses just 2-year age cohorts (similar to Warunsiri and McNown, 2010) and the other uses 5-year age bands combined with a geographic variable (location in our case). Both have cell sizes of around 100 or more, which is commonly thought as being sufficient to ensure accurate estimates. This calls into question studies that use similar aggregation methods and interpret results at the household or individual level, where the low c cause both bias and efficiency issues. Whether this can be addressed by ensuring c is more than 150-200, like our results imply, will be addressed later. The last two columns show that aggregation bias can be addressed using commonly available construction variables, producing pseudo panel estimates that are similar in terms of size and significance to panel results. Nevertheless, these estimates, which have more than 300 cohorts in each time period, have standard errors that are far larger than the FE and OLS, estimates indicating efficiency concerns remain even if bias is addressed, causing potential concerns regarding inference. In Appendix B we demonstrate some alternative ways of estimating pseudo-panels which produce the same coefficient estimates but have different standard errors and other qualities that may prove useful.

Table 6: Panel and Pseudo-Panel estimates of Household Welfare Model

	Pooled OLS	Fixed Effects	Pseudo-Panel 1	Pseudo-Panel 2	Pseudo-Panel 3	Pseudo-Panel 4
HHsize	-0.094***	-0.092***	-0.076***	-0.058***	-0.093***	-0.093***
	(0.003)	(0.004)	(0.015)	(0.018)	(0.007)	(0.007)
ltot_asset	0.172***	0.105***	0.164***	0.013	0.118***	0.115***
	(0.005)	(0.006)	(0.037)	(0.048)	(0.012)	(0.012)
secondary	-0.022*	0.051***	0.133	-0.187	0.060*	0.072**
	(0.012)	(0.012)	(0.162)	(0.123)	(0.031)	(0.034)
lnremit	0.059***	0.019***	0.085	0.237***	0.025*	0.049***
	(0.007)	(0.007)	(0.060)	(0.067)	(0.015)	(0.016)
remittance	-0.652***	-0.205**	-0.953	-3.392***	-0.298*	-0.543***
	(0.080)	(0.080)	(0.719)	(0.863)	(0.181)	(0.186)
Observations	8,645	8,645	9,849	9,848	8,790	9,426
Cohorts			26	21	441	372
Cell Size			98	123	5	6
Cohort Specification			2 year	5 year, Location	2 year, Region, Location, Education, Gender	2 year, Region, Education, Gender
<u>AWAR</u>						
HHsize			0.28	0.28	0.55	0.52
ltot_asset			0.27	0.29	0.64	0.60
secondary			0.17	0.18	0.59	0.53
lnremit			0.17	0.12	0.74	0.66

Notes: The OLS and fixed effects estimates are based on the household level panel data with the data trimmed at the 1st and 99th percentiles. The pseudo-panel estimates each use a different cohort specification, otherwise they are estimated identically. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

One potential concern with the above interpretation is that it assumes the fixed effects estimate is unbiased and is therefore the true coefficient with which one can assess the pseudo-panel estimates. This may not be the case if there are other sources of bias which leads to more aggregated pseudo-panel estimates being less biased than the panel estimates. The first source of bias is attrition, which may be more detrimental to panel estimates than pseudo-panel estimates even if the latter is also based on the same panel data. This is because aggregation into cohorts may reduce the impact of attrition as it is likely to have a less severe impact on the cohort means. Another source of bias is from measurement error in the household level data, which may be 'averaged out' by using cohort means (Antman & McKenzie, 2007b). Constructing more aggregated cohorts would reduce the bias arising from both these potential sources. However, as the results show that estimates from more aggregated pseudo-panels vary greatly and diverge from the fixed effects estimates in a non-systematic manner, we can be confident that such additional biases do not play a big role and leave the main conclusions regarding aggregation bias unchanged.

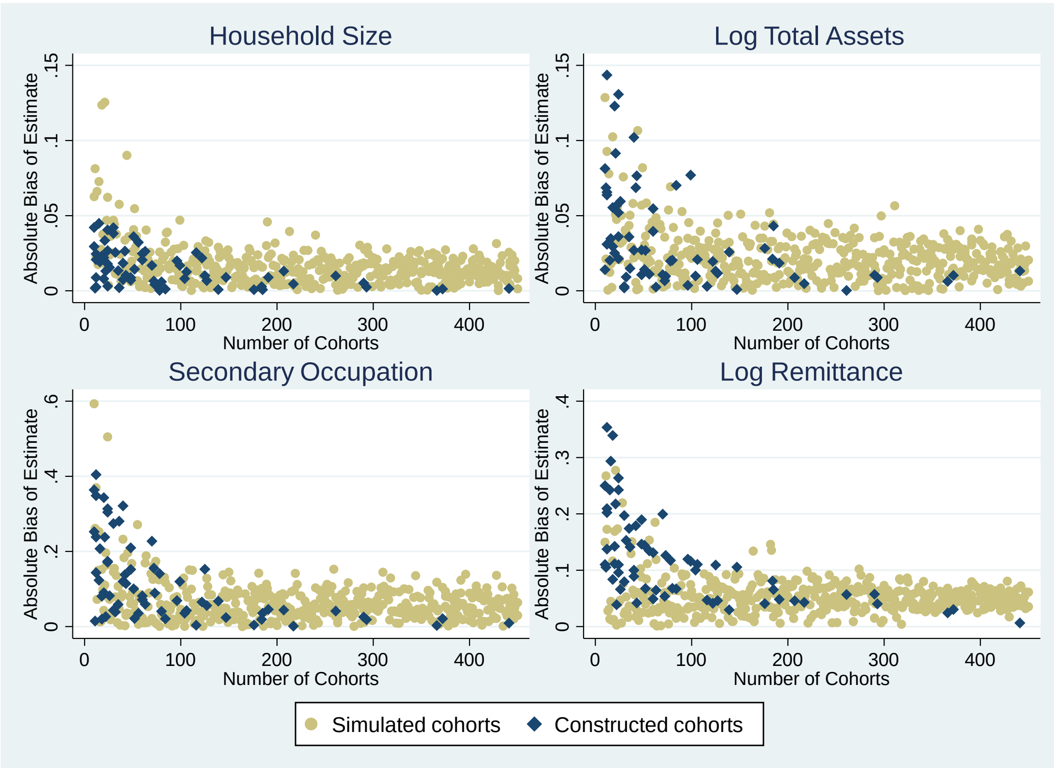
The previous section showed there was no specific cell size which sufficiently addressed sampling error as it depends on the level of variation in the cohort data. For similar reasons, it is unlikely that aggregation bias can be addressed by creating a specific number of cohorts and instead the bias depends on how representative the cohort level data are of the underlying households. The latter, to a certain degree, will be related to the number of cohorts and hence it may appear that aggregation bias can be reduced simply by increasing c. To demonstrate this, we compare the 65 pseudo-panel estimates based on cohort aggregation using the five construction variables mention to those estimated from cohorts constructed with random assignment, which we call simulated cohorts. The cohorts are created by randomly assigning households into a cohort for all time periods and varying the number of cohorts households are assigned to from 10-450. Thus, we have an additional 440 estimates using the simulated cohorts[1]. We graph the absolute bias (calculated as the difference between the pseudo-panel and fixed effects estimates) of the simulated and constructed cohorts against the number of cohorts created in Figure 2. When c is low, particularly when it is below 40, the constructed cohorts perform just as poorly as when cohort assignment is random. The bias of the simulated cohorts does fall somewhat as c reaches 100, with fewer estimates containing extreme levels of bias. However, beyond this point estimates do not improve, retaining the same level of absolute bias on average. In contrast, the estimates from constructed cohorts improve as c increases until the bias becomes negligible.

The driver of the improvements for the constructed cohorts is not the increase in c but changes in the cohort level variation as c changes due to the addition of extra construction variables in the cohort specification or the use of finer sub-categories of

[1] In fact we have 453 additional cohorts in the sample due to the difficultly of creating a precise number of cohorts when c is large and households are not observed in all periods.

age. Previously we discussed the link between aggregation bias and sampling error, particularly the link with w_1, σ_v^2, and time variation. As these three types or variation are combined to form the AWAR metric, AWAR can potentially be used to assess the likelihood of aggregation bias. CAWAR is unlikely to be suitable as it allows larger cell size to offset low variation, which is justifiable for sampling error but not aggregation.

Figure 2: Bias of constructed and simulated cohorts



Notes: The y-axis calculates the absolute different between the pseudo-panel estimate and the FE estimate. Constructed cohorts refers to the 65 cohorts constructed where assignment is based on construction variables from the dataset, while simulated cohorts refers to cohorts created by random assignment.

3.4 AWAR and Aggregation Bias

We calculate the AWAR values for all 65 cohort specifications using the proxies previously identified. Figure 3 plots the AWAR against the absolute bias for each explanatory variable. A clear negative relationship exists between AWAR and bias for all four regressors and there appears to be a critical point, around AWAR of 0.5, beyond which all estimates have low bias. This is not to say that below this threshold all estimates are inaccurate, as the figure shows estimates similar to FE can be obtained even at low AWAR. Instead the threshold ensures confidence that any estimate produced will not suffer from substantial bias caused by the aggregation process. In Table 6 we report the AWAR statistics for the four pseudo-panel regressions, showing the link between AWAR and how well the panel estimates are replicated. Like the estimates in the final two columns of Table 6, we find regressions where all explanatory variables have AWAR of at least 0.5 generally produce estimates that improve on pooled OLS and are similar to FE. In our application, only 4 of the 65 specifications meet this condition, highlighting that this may be quite a strict condition to meet. However, all the cohort specifications that failed to meet this condition generally have one or more poorly estimated regressor, such that the OLS estimate is less biased.

Figure 3: AWAR and Estimation Bias

Notes: The y-axis is calculates the absolute different between the pseudo-panel estimate and the panel fixed effects estimate.

Evidently, AWAR is a useful measure for assessing the potential for aggregation bias, which we have shown can be rather large when estimating pseudo-panels. Whether the critical value (AWAR of around 0.5) found in our empirical example can be used as a general recommendation for applications in other studies is unclear. One reason to believe they may be is because the threshold is stable across the four regressors which are vastly different in their nature; one is continuous, one truncated and the other two are either categorical or binary. They all have different distributions but still require similar AWAR values. The four regressors also capture different properties and characteristics of households with little reason to suspect they are highly correlated with each other or share similar correlations with the cohort construction variables. Consequently, it is likely that similar thresholds would apply for other variables from different datasets.

A greater concern is whether the thresholds change when moving from panel data to repeated cross-sections. The within variation (which captures σ_v^2) is unlikely to change between the two types of data as the heterogeneity of households grouped into cohort will not be affected. With panel data, as cohort membership is constant, the cohort means are likely to be more highly correlated across time than with data that has different individuals in each cross-section. Thus, panel data will have less time variation (higher ρ) and potentially lower across variation (lower w_1) as the latter picks up some time variation as well as cross-sectional variation. Consequently, repeated cross-sections would generally have higher AWAR values (as w_1 is higher and ρ is lower) than panel data. This could affect the thresholds we find if panel data naturally has lower AWAR than RCS irrespective of aggregation. We consider this in Appendix C, where we re-estimate the above model using the same dataset, once again creating the 65 cohort specifications but using just the first two waves. In Figure C2 we randomly drop households so that each only appears once, hence getting rid of the panel dimension, while in Figure C1 we retain the panel setting. The estimates in Figure C2 contain both aggregation and sampling error bias, while for those in Figure C1 the sampling error is severely reduced due to the use of panel data. We find AWAR is generally lower for panel data, but AWAR of 0.5 still seems a reasonable threshold for RCS even though it is hard to assess aggregation bias due to the additional presence of sampling error.

4. Implementing AWAR and CAWAR

The two metrics developed help address different but connected sources of bias one encounters when estimating pseudo-panels; sampling error and aggregation. The bias from the latter can be so substantial that it negates any benefit of using pseudo-panels over simple OLS. Hence, it is important to ensure the cohorts created have AWAR close to 0.5 as well as meeting the required CAWAR value needed to limit sampling error. Whether these thresholds are met in practice remains a concern, particularly as so few cohort aggregation methods met the AWAR threshold in our empirical example. We attempt to draw some inferences regarding the likelihood of other datasets meeting our suggested criteria using results from our empirical application and the Monte Carlo simulations for AWAR.

The results in the final two columns of Table 6 show that our dataset requires a large number of cohorts in order to meet the AWAR threshold, such that average cell sizes fall to as low as 5 or 6. Two other cohort specifications also meet the AWAR threshold (not reported) but again they have cell sizes of 6 and 8 respectively. The AWAR simulations in Table 2 suggest cell size needs to be at least 50, possibly 30, to avoid sampling error bias if the data was repeated cross-sections[2]. The comparison of Figures C1 and C2 shows that the same cohort aggregation process produced higher AWAR for RCS than panel data. With actual RCS some of the more aggregate cohort specifications (which have higher cell size) will meet the AWAR threshold. Even then, they are unlikely to have cell sizes higher than 10-15. Therefore, the dataset would need to be 3-5 times larger to obtain cell sizes needed to address sampling error while also meeting the AWAR threshold for aggregation. Given our dataset has around 2,500 households in each wave, this implies RCS data may require at least 7,000 observations in each wave to address both sources of bias. Many RCS datasets are large enough to meet this condition, particularly those used in development economics. For example, most studies in Table 1a have datasets with the required number of observation in each wave. These calculations assume the cohort specifications and their respective AWAR values taken from our empirical application is representative of other datasets. This is

2 The results in Table 2 are applicable to our empirical example as the simulations have the same T and the results are robust for different values of ρ and c.

not necessarily true; the required number of observations will be lower if there is greater correlation between the cohort construction variables and the explanatory variables, while the reverse is also true. Nevertheless, it may still be a useful benchmark for pseudo-panels with similar data.

5. Conclusion

Addressing sampling error is a crucial part of estimating pseudo-panel models but there is little guidance and consensus regarding how this should be done. We create a measure (called CAWAR), which combines the cell sizes created along with three important sources of variation in the cohort data, to assess the likelihood of sampling error. Using Monte Carlo simulations, we find critical values for the measure beyond which sampling error bias is minimised. We also show that when pseudo-panels are used to estimate individual level models they can suffer from substantial aggregation bias. As aggregation and sampling error biases are related, a similar measure (called AWAR) can be used to assess the former. Using panel data, we estimate pseudo-panels to isolate aggregation bias and find recommended values for AWAR where this bias is minimised.

Ensuring CAWAR and AWAR meet the recommended values should be the starting point of validating pseudo-panel estimation, particularly for individual level models. This can be quite a strict requirement that some datasets are unable to fulfil, implying they may be unsuited to pseudo-panel estimation. It is therefore important to confirm the veracity of these recommended values, particularly as CAWAR has only been tested using simulations and AWAR using a panel dataset. The testing we have conducted is suited to isolating the two different sources of bias, thus what is required for future work is testing them in combination. One way to do this is using a large panel data set where the "true" coefficient can be estimated using panel fixed effects. Then a random subset of the population can be drawn for each time period to create a repeated cross-section with which pseudo-panels can be estimated. The dataset would need to be large enough that the subsets in each time period have enough observations to produce AWAR and CAWAR that meets the recommended values.

Another possibility would be to use a more complex Monte Carlo setup where the individual level data is first generated and then grouped into cohorts using construction variables that have varying degrees of correlation with the explanatory variables, in order to capture a more realistic aggregation process. This allows the simulations to address both sampling error and aggregation, in contrast to our simulations that focus exclusively on the former. One could also test if the recommended thresholds change for different models as application of pseudo-panels has moved beyond the simple linear fixed effects model considered here. Some pertinent examples are nonlinear models (particularly ones with a binary response variable), dynamic models, and ones with parameter heterogeneity.

One final avenue for future work is combining different datasets (whether they are RCS or panel) by matching on cohorts. If aggregation has been fully addressed, cohorts can be thought of as representative households and hence one may be able to combine data from different surveys. For example, it may be possible to combine many of the Demographic and Health Surveys with household income/expenditure surveys as long as they share the same variables required for cohort construction. There are some important concerns that arise from this, particularly regarding the sampling methods used for the different surveys and the effect of having a different set of individuals not just in each time period but also for different variables within a time period. However, if such a merger is possible it would widen the scope of research and allow the estimation of models not possible before.

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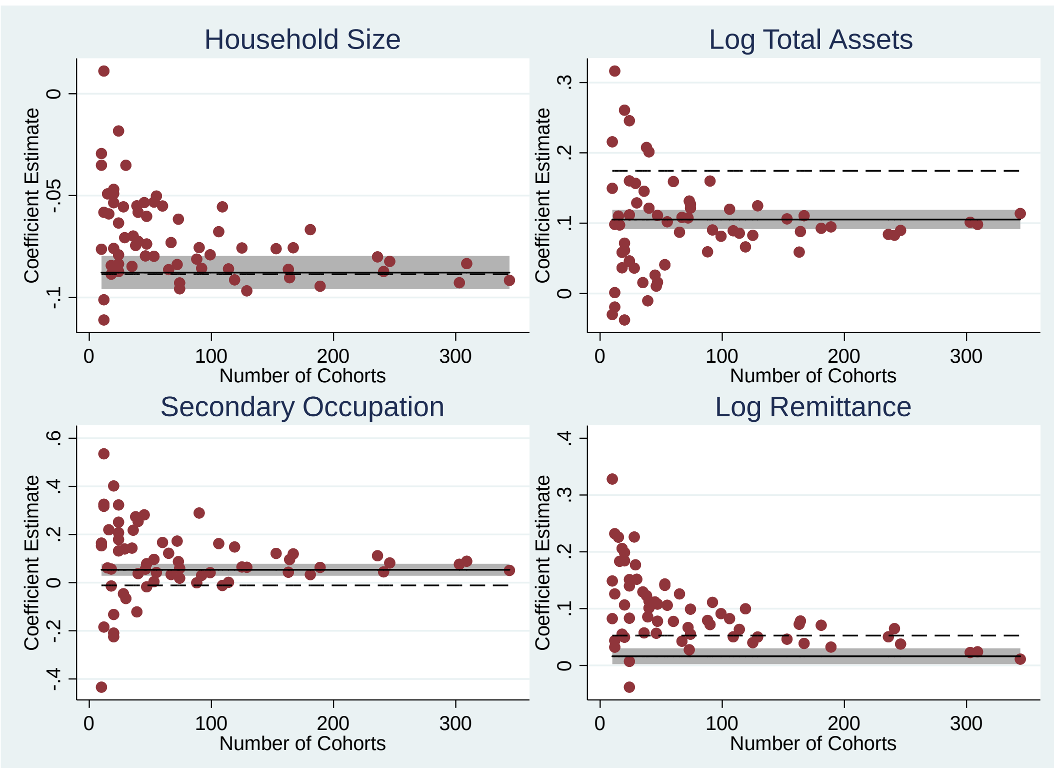
Appendix A: Replication of Pseudo-Panel results using balanced panels

We reproduce all pseudo-panel results using just households observed in all four waves, thus ensuring sampling error is eliminated as cohort membership is fixed. However, this comes at the expense of potentially incurring attrition bias with just over half of all households being in the balanced panel. Figure A1 and Table A1 shows that using balanced panels does not affect the results in the main analysis. From Figure A1 (the corollary to Figure 1), one sees that aggregation bias is still evident, with the pattern of bias across the number of cohorts being largely the same. The balanced panel does produce less biased estimates, with more appearing in the 95% confidence interval of the 'true' coefficients, but the effect is small. Table A1 also produces similar results to its counterpart (Table 6), particularly in terms of bias. However the examples which meet the AWAR threshold have poor inference with some variables losing significance. This is due to the loss of observations in the balanced panel, although the poor inference can be addressed by using alternative estimation techniques discussed in Appendix B. Figure A2 shows the relationship between AWAR and aggregation bias is also unchanged.

Figure A1: Pseudo-Panel Estimates using Balanced Panels



Notes: Same as for Figure 1 but using balanced panels

Table A1: Replication of Table 6 using Balanced Panels

	Pooled OLS	Fixed Effects	Pseudo-Panel 1	Pseudo-Panel 2	Pseudo-Panel 3	Pseudo-Panel 4
HHsize	-0.089***	-0.088***	-0.079***	-0.054**	-0.092***	-0.083***
	(0.003)	(0.004)	(0.016)	(0.021)	(0.008)	(0.008)
ltot_asset	0.174***	0.105***	0.112**	-0.038***	0.114***	0.098***
	(0.006)	(0.007)	(0.043)	(0.013)	(0.012)	(0.012)
secondary	-0.011	0.054***	0.251*	-0.225*	0.051	0.089***
	(0.013)	(0.013)	(0.146)	(0.119)	(0.031)	(0.034)
lnremit	0.053***	0.016**	-0.038	0.199***	0.011	0.024
	(0.008)	(0.007)	(0.075)	(0.060)	(0.016)	(0.016)
remittance	-0.572***	-0.170**	0.243	-2.745***	-0.138	-0.277
	(0.089)	(0.083)	(0.930)	(0.762)	(0.191)	(0.188)
Observations	6,580	6,580	6,842	6,843	5,819	6,344
Cohorts			24	20	344	309
Cell Size			73	88	4	5
Cohort Constructors			2 year	5 year, Location	2 year, Region, Location, Education, Gender	2 year, Region, Education, Gender
AWAR						
HHsize			0.27	0.27	0.52	0.49
ltot_asset			0.26	0.36	0.64	0.59
secondary			0.15	0.15	0.62	0.55
lnremit			0.13	0.08	0.77	0.66

Notes: Same as Table 6

Figure A2: AWAR and Estimation Bias with Balanced Panels

Notes: Same as Figure 3 but using balanced panels

Appendix B: Alternative Pseudo-Panel Estimation Techniques

The focus so far has been on the bias of pseudo-panel estimates and ensuring they are as close as possible to the "true" fixed effects values. This is not the only concern as Table 6 shows that pseudo-panel estimates are also far less efficient than fixed effects, with standard errors generally being at least two or three times larger. This has important consequences for inference as the pseudo-panel estimates are less significant in a number of cases. To a certain degree this is inevitable due to the reduction in the number of observations when moving from the household to the cohort level. In Table B1 we consider some alternative ways of estimating pseudo-panels which produce the same coefficient estimates but different standard errors. The baseline method (which we have been using thus far) is shown in Column 3, where the data is aggregated to the cohort level and cohort dummies are included to account for fixed effects, what we term Cohort LSDV. Cohorts are constructed using 2 year age bands and all four of the other construction variables as this specification yielded the closest estimates to the "true" FE estimates (which are included in Column 1 to aid comparison).

The first method considered is estimating using an instrumental variables (IV) approach where the model is estimated at the household level but including cohort dummies and using the cohort means as instruments for the household level variables. As this approach keeps the data at the household level, one may expect the standard errors to be lower than for Cohort LSDV as there are more observations. This is not the case with standard errors for the IV being almost identical. It also demonstrates that cohort aggregation is a similar process to instrumenting and grouping can be as stringent a process as selecting a valid instrument. For the second method we keep the data at the cohort level but use fixed effects estimation (labelled Cohort FE) rather than cohort dummies as this increases the degrees of freedom and. The results in Column 4 indicate that, if anything, the standard errors are slightly larger. The coefficients are also slightly different as fixed effects estimates requires cell sizes (which are used as regression weights) to be constant over time whereas LSDV allows them to vary.

Table B1: Alternative techniques for estimating Pseudo-panels

Aggregation level	Household		Cohort		Household with cohort observations	
	FE	IV	Cohort LSDV	Cohort FE	LSDV Cohort Y	LSDV House-hold Y
HHsize	-0.092***	-0.093***	-0.093***	-0.094***	-0.093***	-0.093***
	(0.004)	(0.007)	(0.007)	(0.007)	(0.003)	(0.008)
ltot_asset	0.105***	0.118***	0.118***	0.118***	0.118***	0.118***
	(0.006)	(0.012)	(0.012)	(0.013)	(0.006)	(0.014)
secondary	0.051***	0.060*	0.060*	0.063**	0.060***	0.060
	(0.012)	(0.032)	(0.031)	(0.032)	(0.016)	(0.037)
lnremit	0.019***	0.025	0.025*	0.026*	0.025***	0.025
	(0.007)	(0.016)	(0.015)	(0.016)	(0.008)	(0.019)
remittance	-0.205**	-0.298	-0.298*	-0.308*	-0.298***	-0.298
	(0.080)	(0.189)	(0.181)	(0.187)	(0.092)	(0.226)
Observations	8,645	8,790	1,727	1,727	8,790	8,790
R-squared	0.48	0.67	0.91	0.71	0.92	0.56

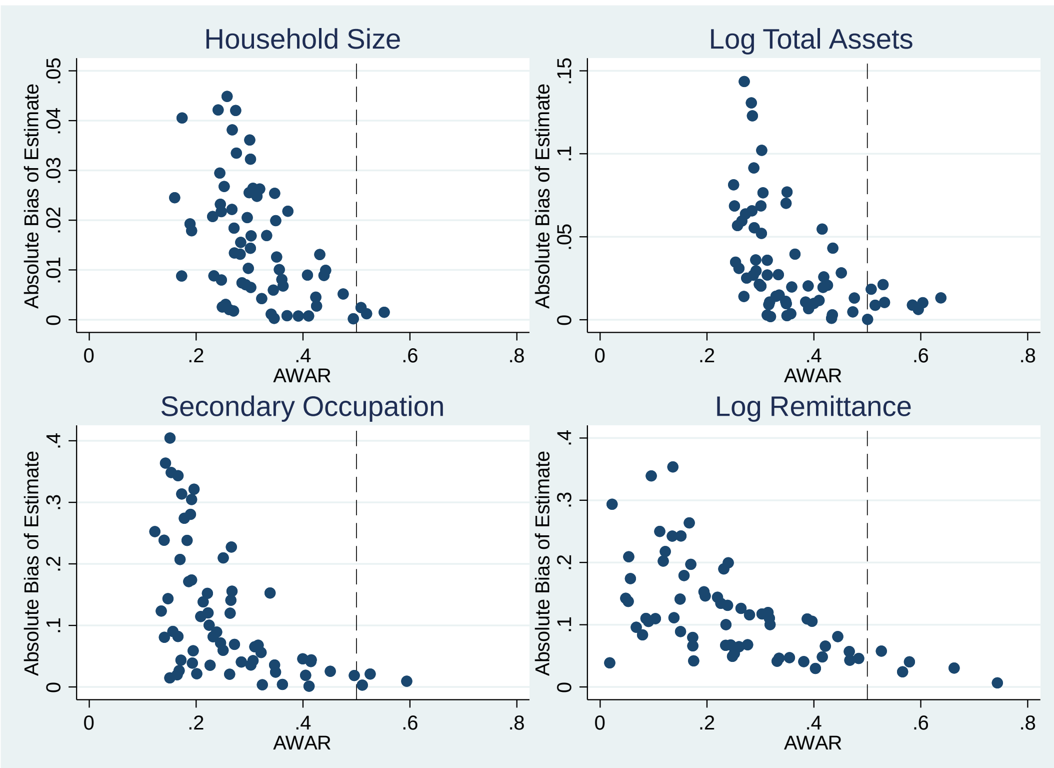
Note: The first two columns estimate using household level data to either estimate fixed effects regression using the panel nature of the data or an IV regression using cohort means as instruments. Columns 3 and 4 use cohort means to create a cohort level panel and account for cohort fixed effects either using cohort dummies (Column 3) or fixed effects (Column 4). Cohort LSDV has been used in the main analysis to estimate pseudo-panels. Columns 5 and 6 maintains the data at the household level but the values of the explanatory variables are the cohort means. Column 5 uses the cohort means for the dependent variable whereas Column 6 instead uses the household level values. Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1

The final two methods are similar and involve estimating at the household level but using the cohort mean observations rather than the household level observations. This amounts to the cohort mean observations being repeated in the dataset the same number of times as their cell sizes. Cohort dummies are included to account for fixed effects and the regression is weighted by the negative of the square-root of the cell for consistency with the weighting used in the cohort level regressions. The methods differ in that 'LSDV Cohort Y' uses cohort mean values for the dependent variable as well as the independent variables, whereas 'LSDV Household Y' does so only for the independent variables and keeps the dependent variable values at the household level. Both approaches have a certain advantage over using Cohort LSDV. Cohort Y produces standard errors that are very similar to the FE errors, resulting in identical inference with estimates having the same significance levels. Although estimating in such a manner is questionable as it essentially involves artificially expanding the number of observations by replication, it is the only method that produces errors similar to the FE values. Thus it is best to continue using Cohort LSDV as the baseline, however one could include Cohort Y alongside it in order to get inference closer to those at the household level. The Household Y approach is not as useful for getting improved standard errors, but has the advantage of producing an R-squared that is more consistent with those from household level models. The R-squared is slightly larger than for the FE model but far below those from other pseudo-panel models where the value often exceeds 0.9. Thus if one is interested in a goodness-of-fit measure for pseudo-panel models then the R-squared from a Household Y model may be the most suitable option.

Appendix C: Comparing AWAR on Panel and RCS data

In order to compare AWAR values across panel and RCS data we convert our panel data into RCS by randomly dropping households so that the same household does not appear in more than one time period. As we have four waves this would mean dropping three quarters of our observations, leaving too few for creating suitable cohorts. Thus we only use the first two waves, meaning only half the observations in each wave are dropped. We re-estimate using all 65 cohort specifications for the first two waves of the panel and RCS versions of our data. We then calculate the AWAR value and the absolute bias, which is calculated using the panel fixed effects coefficients as the 'true' values, with results presented in Figures C1 and C2. While the panel data results only contain aggregation bias, the RCS will suffer from both aggregation and sampling error, hence the latter has larger levels of absolute bias. RCS data also produces larger AWAR values, with many more specification meeting the 0.5 threshold. Nevertheless, even with the additional sampling error bias, a threshold of 0.5 still looks suitable for RCS data.

Figure C1: AWAR and Estimation Bias of first two waves of Panel Data



Notes: Same as Figure 3 but just using first two waves of data

Figure C2: AWAR and Estimation Bias of first two waves without the panel dimension

Notes: Same as Figure 3 but just first two waves of data and randomly dropping households to create a repeated cross-section