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*Research Article*

**Another ‘futile quest’?**

**A simulation study of Yang and Land’s  
Hierarchical Age-Period-Cohort model**

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## Table of Contents

1	Introduction	334
2	The Age-Period-Cohort identification problem	335
3	Yang and Land's HAPC model	339
4	Simulation design	343
5	Simulation results	347
5.1	Does the HAPC model work when period and cohort effects are normally distributed in the DGP?	350
5.2	Does the model work when there is a trend in the cohort effects in the DGP?	350
5.3	Does grouping of cohorts in the fitted model help to achieve the correct answer?	350
5.4	What happens if there are groupings in the DGP as well as the fitted model?	351
5.5	What does including a linear cohort (mean-age) term in the fixed part of the model do?	351
6	Discussion	352
7	Acknowledgements	354
	References	355
	Appendix	360

## **Another ‘futile quest’? A simulation study of Yang and Land’s Hierarchical Age-Period-Cohort model**

**Andrew Bell<sup>1</sup>**

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### **Abstract**

#### **BACKGROUND**

Whilst some argue that a solution to the age-period-cohort (APC) ‘identification problem’ is impossible, numerous methodological solutions have been proposed, including Yang and Land’s Hierarchical-APC (HAPC) model: a multilevel model considering periods and cohorts as cross-classified contexts in which individuals exist.

#### **OBJECTIVE**

To assess the assumptions made by the HAPC model, and the situations in which it does and does not work.

#### **METHODS**

Simulation study. Simulation scenarios assess the effect of (a) cohort trends in the Data Generating Process (DGP) (compared to only random variation), and (b) grouping cohorts (in both DGP and fitted model).

#### **RESULTS**

The model only works if either (a) we can assume that there are no linear (or non-linear) trends in periods or cohorts, (b) we control any cohort trend in the model’s fixed part and assume there is no period trend, or (c) we group cohorts in such a way that they exactly match the groupings in the (unknown) DGP. Otherwise, the model can arbitrarily reapportion APC effects, radically impacting interpretation.

#### **CONCLUSIONS**

Since the purpose of APC analysis is often to ascertain the presence of period and/or cohort trends, and since we rarely have solid (if any) theory regarding cohort groupings, there are few circumstances in which this model achieves what Yang and Land claim it can. The results bring into question findings of several published studies using the

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HAPC model. However, the structure of the model remains a conceptual advance that is useful when we can assume the DGP has no period trends.

## **1. Introduction**

Social scientists have long been interested in how social processes change over time. Since at least the 1960s (Ryder 1965) this interest has developed into an understanding that change over time can occur in three conceptually distinct ways. First, individuals can age, such that change occurs over their individual life courses. Second, change can occur by cohorts, such that a new birth cohort will be intrinsically different to a previous one regardless of their age. And third, change can occur by periods, such that characteristics of a given occasion affect individuals living through that occasion, again regardless of when they were born and how old they are in that year. Which of these age, period, and cohort (APC) effects are important for a given research question is of profound importance to researchers in many disciplines who are interested in how things change over time.

A problem arises when attempting to model all three of these effects simultaneously, because the three terms are exactly mathematically dependent. This 'identification problem' has been known about for decades, and potential solutions to the problem have been proposed, sparking debate; particularly in sociology (Firebaugh 1989; Mason et al. 1973; Sasaki and Suzuki 1987; Yang, Fu and Land 2004) and medical sciences (Osmond and Gardner 1989; Robertson and Boyle 1986, 1998a, 1998b; Tu, Smith, and Gilthorpe 2011). Over the past 30 years there have been a plethora of solutions proposed, but none have been shown to be foolproof. In fact, many in social science (Glenn 1976, 2005; Goldstein 1979) see the separation of the three terms as not just difficult, but as a "futile quest" (Glenn 1976):

"The continued search for a statistical technique that can be mechanically applied always to correctly estimate the effects is one of the most bizarre instances in the history of science of repeated attempts to do the logically impossible." Glenn (2005, p. 6)

This paper considers one of these solutions, proposed recently by Yang and Land (2006) with some additional methodological caveats discussed in subsequent papers (Frenk, Yang and Land 2013; Yang 2006; Yang and Land 2008). The methodology has

already been employed in a number of empirical applications, studying social trends in happiness (Yang 2008a), voter turnout (Dassonneville 2013), obesity (Reither, Hauser and Yang 2009), religious service attendance (Schwadel 2010), and cannabis use (Piontek et al. 2012), to name a few. With the relatively speedy uptake of this method, understanding how and when it works is of profound importance.

We thus use a simulation study to assess the model: the assumptions that it makes, the bias that occurs when those assumptions are broken, the situations in which it could be of use and the situations in which it is not. In doing so we bring into question both the value of the model as a ‘solution’ to the identification problem, and the conclusions of a number of empirical studies that have used it. This is not to say that the conceptual distinction between age, period, and cohort is valueless, nor that the model could not be of use in a number of situations in finding substantive conclusions. However we hope our study will warn against the mechanical application of this, or any, age-period-cohort model, without any critical forethought about what the model can and cannot achieve. In short, there is no technical solution to the identification problem without the imposition of strong (and correct) a priori assumptions.

The paper proceeds as follows. The next section reviews the conceptual arguments that have accompanied the development of various attempts to solve the APC identification problem. The paper then assesses the model suggested by Yang and Land, considering its conceptual structure, its technical specification, and some problems that we see with Yang and Land’s justification of it. Section 4 outlines the simulation study that we use to assess the model, and section 5 presents the results from this, before the implications of these results are discussed in section 6.

## 2. The Age-Period-Cohort identification problem

When studying longitudinal social processes, researchers are in essence looking at change. However the form of that change can be multifaceted. The distinction between ageing, change between periods, and change over cohort groups has been considered for decades. Ryder (1965) was one of the first to make the distinction, by considering cohorts as a source of social change rather than thinking of change as occurring over time through successive years. The division is well summarized by this fictional dialogue by Suzuki (2012 p. 452):

- A: I can’t seem to shake off this tired feeling. Guess I’m just getting old. [Age effect]  
B: Do you think it’s stress? Business is down this year, and you’ve let your fatigue build up. [Period effect]  
A: Maybe. What about you?

B: Actually, I'm exhausted too! My body feels really heavy.

A: You're kidding. You're still young. I could work all day long when I was your age.

B: Oh, really?

A: Yeah, young people these days are quick to whine. We were not like that. [Cohort effect]

Having made this distinction, it is worth noting that there are other ways in which change can be categorized. Firebaugh (2008) distinguishes between social change (studied with repeated cross-sectional data) and individual change (studied with panel data). This can similarly be conceived as within- and between-individual change (Bell and Jones 2013c): an individual can age, or individuals can differ because they were born into a different cohort<sup>3</sup>. Thus individuals are nested within a context (their cohort group) and cohort change is considered across those contexts. Another conceptual distinction is between change within an individual and change by cohort replacement (Firebaugh 1989, 2008). Here, cohort replacement effects are the result of old cohorts dying or leaving the sample and young cohorts joining the sample (a mixture of age and cohort effects), and within-cohort change over time (which can be thought of as a mixture of age and period effects). Thus, whilst the age-period-cohort division remains conceptually attractive and is linked in various ways to the divisions proposed here, it is not necessary to model all three in order to study social and individual change in a manner that is both robust and meaningful.

Furthermore, there is a problem in attempting to distinguish between the effects of these three sources of change in a statistical model. The three are perfectly correlated, such that:

$$Age = Period - Cohort \quad (1)$$

In such an equation we are always able to know the value of one of the three terms, if we know the value of the other two. As such, each of the following three (and an infinite number more) data generating processes (DGPs) will produce exactly the same data. Take the first, in which the dependent variable  $Y$  is created by linear age, period, and cohort effects, each with a value of 1:

$$Y = (1 * Age) + (1 * Period) + (1 * Cohort) \quad (2a)$$

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<sup>3</sup> In a repeated cross-sectional study the mean age of a cohort of individuals will be equivalent to the cohort variable itself (Freitas and Jones 2012). Usually in such studies period effects are assumed to be non-existent; this is done on the basis of theory regarding the processes that are thought to be operating.

Substituting *Period* with *Age + Cohort* gives us

$$Y = 2 * Age + 2 * Cohort \quad (2b)$$

And then substituting *Age + Cohort* with *Period* gives us

$$Y = 2 * Period \quad (2c)$$

As such, a DGP with equal age, period, and cohort linear effects (2a) would create the same data as a DGP with (larger) age and cohort effects and no period effects (2b), and the same data as a DGP with just a single (larger) period effect (2c), because of the dependence expressed in equation 1<sup>4</sup>. This would be the case regardless of whether there are additional non-linear effects in the DGP: you could, for example, add an age-squared term to each of equations 2a-c without affecting the identification problem between the other terms.

Whilst different DGPs can be clearly conceived for linear effects, it is perhaps less obvious that a similar problem occurs with non-linear effects. For example, consider the following DGPs, each of which, again, will produce identical data.

$$Y = 1 * Cohort^2 \quad (3a)$$

Substituting *Cohort* with *Period – Age* and expanding gives us

$$Y = Period^2 - 2 * Period * Age + Age^2 \quad (3b)$$

Then, substituting the second *Period* term for *Age + Cohort* and simplifying gives us

$$Y = Period^2 - 2 * Cohort * Age - Age^2 \quad (3c)$$

Whilst some of these scenarios may be more plausible than others, the choice between these DGPs can only be made on the basis of theory, not the data. The same problem applies to other non-linear effects. Log effects are less easy to split apart because  $\log(a + b)$  does not have an additive decomposition; so, for example, the term  $\log(Period)$ , whilst mathematically identical to  $\log(Age + Cohort)$ , cannot be split mathematically into separate age and cohort trends in a DGP. However, following Box

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<sup>4</sup> This problem can be viewed from a more technical standpoint as a rank deficiency in the design matrix  $\mathbf{X}$  of a regression  $\mathbf{Y} = \mathbf{X}\mathbf{b} + \mathbf{e}$ . That is, the matrix  $\mathbf{X}^T\mathbf{X}$  cannot be inverted when  $\mathbf{X}$  includes all of APC, meaning that parameters  $\mathbf{b}$  associated with APC cannot be estimated by OLS  $[(\mathbf{X}^T\mathbf{X})^{-1}\mathbf{X}^T\mathbf{Y}]$  without applying constraints (e.g. see Luo and Hodges 2013).

and Draper (1987:424), who state that “all models are wrong, but some are useful”, we argue that a real-life DGP is not the same as a mathematical approximation to it. As such, the presence in a DGP of a mathematical approximation that is ‘un-confoundable’ does not suggest that the supposedly real-life, non-mathematical process that it represents is accurately portrayed.

It follows that, given a dataset, it is impossible to know which of the infinite possible linear or non-linear effect combinations such as those listed above actually produced the data. Without the help of time machines or age accelerators (Suzuki 2012) it is impossible to assess the effect of one of the terms independently of the others, because keeping any two of the APC terms constant automatically holds the third constant as well.

It is worth reiterating that this dependency lies not with a model that is fitted to the data, nor with the data itself, but with the underlying processes that created the data. That is, APC are confounded in the population, not just in the sample; the confounding is mathematical or logical, and as such cannot simply be solved by manipulation of the data or the model (Goldstein 1979). Failing to realize this has led to a number of solutions being proposed which allow the model to be fitted but which produce arbitrary results. For example, Robertson and Boyle (1986) ‘solved’ the problem by aggregating the data in such a way to produce ‘non-overlapping cohorts’ which are not perfectly collinear. Similarly, Mason et al. (1973) propose a method which constrains two age groups, period groups, or cohort groups to be aggregated together. However both of these methods, whilst able to produce a result, will not produce the correct result unless that constraint or aggregation exactly matches constraints in the (unknown) DGP. Glenn (1976) shows that in the case of the Mason et al model, even non-linear effects will be biased where constraints are imposed arbitrarily (see also Glenn 2005:14). And Osmond and Gardner (1989) show that different aggregations will produce wildly different results: unless those aggregations are present in the (unknown) DGP they will produce incorrect results more often than not. Very solid theory about aggregations in the DGP is required for such methods to be of any use, and unfortunately that theory is rarely forthcoming in applied research.

It is clear from the above that aggregation does not solve the identification problem; it merely hides it beneath coarser data. The fact that the model is able to estimate a solution simply means that the constraint or aggregation used is forcing the model to arbitrarily make a choice, on the basis of the researcher’s arbitrary aggregation, not the data itself. Whatever parameterization of the model is used to make the model fit, the problem of multiple possible underlying DGPs is not solved because it is the model that is being changed, not the DGP.

Despite these clear difficulties in attempting to model age, period, and cohort simultaneously, the desire to be able to explore all three terms remains, and as such

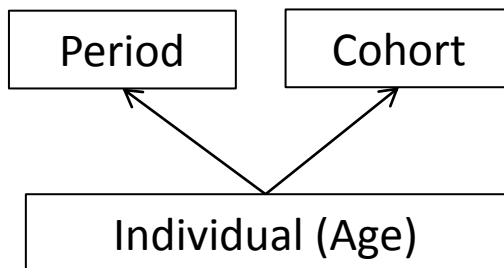


there have been numerous attempts to ‘solve’ the problem, including Yang’s Intrinsic Estimator<sup>5</sup> (Yang 2008b; Yang, Fu and Land 2004; Yang et al. 2008), Tu’s Partial Least Squares Regression (Tu, Smith, and Gilthorpe 2011), and Schmid and Held’s Bayesian APC Modelling and prediction (BAMP) software (Schmid and Held 2007). Whilst these authors are often clear that their methods are not a panacea, they are often far from explicit about the situations in which the models do and do not work. This paper looks at a single proposal, the Hierarchical Age Period Cohort (HAPC) model (Yang and Land 2006). However, we hope that the results found in this paper will act as a cautionary tale to anyone considering using a model that claims to disentangle APC effects automatically.

### 3. Yang and Land’s HAPC model

The ‘solution’ to the APC identification model proposed by Yang and Land uses a multilevel model (also called random-effects, hierarchical linear, or mixed model) with a cross-classified structure. This structure treats individuals as nested within both periods and cohorts, and this must be conceived of as cross-classified because there is no exact nesting of periods into cohorts, or vice versa. As the data are (repeated) cross-sectional, an individual is only observed at one age and one period. Periods and cohorts are thus treated as random effect contexts at a higher level, in which individuals reside. Age is specified as an individual-level variable and is included in the fixed part of the model as a (potentially non-linear) function. The model structure can thus be expressed in a classification diagram as shown in Figure 1.

**Figure 1: Cross-classified structure of Yang and Land’s HAPC model for repeated cross-sectional data**



<sup>5</sup> See Luo (2013a) for a simulation study providing a critique of the Intrinsic Estimator.

Conceptually, this presents a new insight to APC processes that is quite enticing. Treating periods and cohorts as contexts, and age as an individual characteristic, is intuitive to some degree because we move from one period into another as time passes, and we belong to cohort groups that have common characteristics, whereas ageing is a process that occurs within an individual. Multilevel models in general assume that the higher-level residuals are independently and identically distributed (IID). Thus, period effects are considered as, for example, ‘the effect of being in 1990’, independent of the periods around it, rather than ‘the effect of moving from 1989 to 1990’, a linear effect that is constant across all periods and so equal to the effect of moving from 1990 to 1991; similarly, cohort effects are considered in terms of ‘the effect of being born in 1960’ rather than ‘the (linear) effect of societal change between children born in 1959 and 1960.’ Whilst periods and cohorts are unlikely to be truly independent even if there is no trend (near cohorts are likely to be more alike than cohorts that are far apart in time), techniques can be used to allow autocorrelation to be taken into account in the model (Stegmueller 2014). Crucially, conceiving of separate periods and cohorts as quasi-independent contexts that have (random) effects individually, rather than as part of an overall continuous linear fixed effect, changes how the collinearity of APC works. In the cross-classified structure, because we are modelling a different facet of period and cohort effects to that of the age effects (random variation compared to a linear trend), we are able to model both a period and a cohort effect for individuals whilst still controlling for their age; the effects are not collinear in the same way as they are for linear trends.

Given this conceptual structure, the model is specified<sup>6</sup> algebraically as follows:

$$\begin{aligned}
 y_{i(j_1j_2)} &= \beta_{0j_1j_2} + \beta_1 Age_{i(j_1j_2)} + \beta_2 Age_{i(j_1j_2)}^2 + e_{i(j_1j_2)} \\
 \beta_{0j_1j_2} &= \beta_0 + u_{1j_1} + u_{2j_2} \\
 e_{i(j_1j_2)} &\sim N(0, \sigma_e^2), u_{1j_1} \sim N(0, \sigma_{u1}^2), u_{2j_2} \sim N(0, \sigma_{u2}^2)
 \end{aligned} \tag{4}$$

The dependent variable,  $y_{i(j_1j_2)}$  is measured for individuals  $i$  in period  $j_1$  and cohort  $j_2$ . The ‘micro’ model has linear and quadratic age terms, with coefficients  $\beta_1$  and  $\beta_2$  respectively, a constant that varies across both periods and cohorts, and a level 1 residual error term. The macro model defines the intercept in the micro model by a non-varying constant  $\beta_0$ , and a residual term for each period and cohort. The period, cohort,

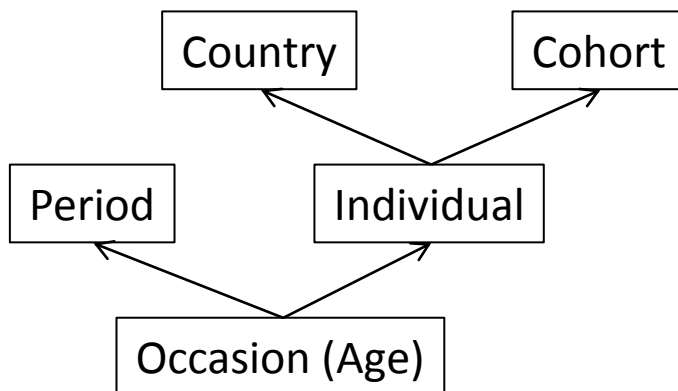
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<sup>6</sup> Brackets in the subscript indicate that levels are at the same level of the hierarchy. This notation is in the manner of Goldstein, Browne, and Rasbash (2002:3304).

and level-1 residuals are all assumed to follow a Normal<sup>7</sup> distribution, each with variances that are estimated.<sup>8</sup>

Multilevel models also allow for additional levels to be included, where data is available. As previously stated, with repeated cross-sectional data a given individual is only observed at one age and in one period. However, if instead of repeated cross-sectional data we have panel data, an additional individual level could be included, thus allowing multiple observations (and so multiple ages and periods) per individual (Suzuki 2012). Further, if the data has some kind of geographical indicator (such as neighbourhoods, or countries), then these could also be considered as an additional level in this conceptual model. As such, Yang and Land's original three-level model can (conceptually at least) be extended to account for other contexts in which measurements exist, leading to a more complex structure such as that shown in Figure 2.

**Figure 2: Potential extension to the structure of Yang and Land's HAPC model**



Note: Where panel (rather than repeated cross-sectional) data is available, there are multiple occasions (and thus multiple periods and ages) per individual. Individuals are still nested within birth cohorts. Those individuals can also be nested within geographical units (here, countries) where the data is present.

<sup>7</sup> Non-Normal distributions are also possible. For example where the response variable is categorical the level-1 residual would not be assumed Normal (see Jones and Subramanian 2013).

<sup>8</sup> It may be that it is unnecessary to include both period and cohort residuals in the model. As such, Yang and Land recommend comparing the cross-classified model to nested models of observations in periods and observations in cohorts, and comparing them on the basis of likelihood ratio tests (Frenk, Yang, and Land 2013).

In addition to extra levels in the model's random part, covariates can be added to the fixed part of the model, and these can vary at the individual level (in the micro model) or at the period or cohort levels (in the macro model). Random coefficients could be placed on any of the fixed part coefficients, so that, for example, the effect of age can be allowed to vary by cohort group. Whilst it is certainly true that you can have too complex a model, these possibilities make it seductive where there is data that allow it and research questions that require such complexity.

However, such extensions are of little use if the model does not function as we wish it to. Conceiving of cohorts and periods as discrete temporal entities rather than as continuous temporal variables does not make them so in the DGP of real life. Any age, cohort, or period trends (linear, or otherwise) in the DGP will still have the potential to be confounded by this process because multiple combinations of them will produce identical data, and no model is able to distinguish between identical datasets.

Yang and Land concur (Yang and Land 2006) that the cross-classified structure does not solve the identification problem, as any age effect could in fact be a combination of period and cohort effects, and so on. They offer two solutions to this problem within the above framework: they suggest (1) specifying age as a quadratic polynomial, and (2) grouping cohorts into (for example) five-year intervals. They argue that only the former is necessary to allow identification and thus 'resolve' the problem outlined above:

"The underidentification problem of the classical APC accounting model has been resolved by the specification of the quadratic function for the age effects."  
Yang and Land (2006:84)

"An HAPC framework does not incur the identification problem because the three effects are not assumed to be linear and additive at the same level of analysis"  
Yang and Land (2013:191)

"This contextual approach ...helps to deal with (actually completely avoids) the identification problem"  
Yang and Land (2013:71)

However, following the argument of section 2, we regard with some scepticism the ability of either of these techniques to solve the identification problem. Including a quadratic term in the model does not put it in the DGP, nor does it remove any linear term from the DGP. In any case, a quadratic term in the DGP can itself be confounded. Similarly, grouping the data does not put those groupings in the DGP and so will not

solve the identification problem either. Where there are linear or non-linear age, period, and cohort effects there is the potential for these effects to be confounded.

Yang and Land argue that the model can be estimated using maximum likelihood, but that, where there are few cohorts or few periods, the model should be estimated using Monte Carlo Markov Chain (MCMC) methods (Yang 2006). In a later paper (Yang and Land 2008) they also argue that there is a choice to be made between fixed and random effects for period and cohort terms. The advantages of random effects as specified above is that it uses fewer parameters, it is likely to be more efficient, and it is more easily extendable than the fixed effects version. The downside is that the model estimates will be biased if covariates are correlated with the higher-level residuals. This is likely to be problematic here because, if there is a cohort trend, with repeated cross-sectional data the age variable will always be correlated with the cohort residuals, because older cohorts will have a higher age than newer cohorts over the time period being studied (this point is not addressed in Yang and Land's paper<sup>9</sup>). A solution to this could be to include the group (cohort<sup>10</sup>) mean of the age coefficient as a variable in the fixed part of the model (Bell and Jones 2013c; Mundlak 1978); however in this case, the cohort mean of age is identical to the cohort variable itself. As with any model that has both age and cohort linear terms in the fixed part of the model, it must assume that there is no linear period trend.

#### 4. Simulation design

The preceding discussion raises a number of issues regarding the situations in which Yang and Land's conceptually attractive method works, and the situations in which it does not. We are particularly interested in how the model treats linear period or cohort trends, compared to randomly distributed effects that are assumed by the HAPC model. Such linear trends are likely to be common in many datasets, since they express any progress (or regress) that occurs over time; even if the trends do not continue outside of the data range they can still be expressed as linear trends for the available data range. For the model to work in the presence of linear effects it must be able to convert

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<sup>9</sup> Indeed, the paper uses an example where the FE and RE solutions produce quite clearly different results (the FE result is nine times the size of the RE result!). However, by running a Hausman Test (Hausman 1978) on all coefficients simultaneously rather than individually they ignore the differences in the age coefficient and claim erroneously that there is no problem in this regard (Yang and Land 2008:317-318, and Table 4 on p.319).

<sup>10</sup> With repeated cross-sectional data the period group mean will in general not vary a great deal, because of the way the data is sampled.

accurately these linear<sup>11</sup> cohort or period trends into a separate random effect for each cohort or period. Further, we are interested in whether the grouping of cohorts aids in this conversion, as is claimed by many in the literature (e.g. see Page et al. 2013). This includes not just assuming the presence of certain groups in the fitted model, but also understanding how the model copes with actually occurring groupings in the DGP. This might occur if people born in a time-period of longer than a year share particular characteristics (for example, those born in the 1960s, or 'baby-boomers'). Finally, if the model is able to do the above successfully, we want to know the extent of bias caused by correlation between the age variable and the cohort residuals, and the ability of the methods suggested by Mundlak (1978) and Bell and Jones (2013c) to solve these problems within the HAPC framework.

These issues can be reduced to the following five questions:

- i. Does the HAPC model work when periods and cohort effects are Normally distributed in the DGP?
- ii. Does the model work when there is a linear trend in the period or cohort effects in the DGP?
- iii. Does grouping of cohorts in the fitted model help to achieve an answer in line with the true DGP?
- iv. What happens if there are groupings in the DGP as well as the fitted model (both matching those in the fitted model, and not)?
- v. Does including a linear cohort (mean age) term in the fixed part of the model remove bias from the random part, and solve any biases caused by correlation between the age variable and the cohort residuals?

Thus, these five questions inform the design of the simulation scenarios outlined in Table 1. The first scenario tests the model's capability where there are no period or cohort trends, thus answering question (i) above. The second scenario adds a cohort trend (a linear cohort effect of 0.1), allowing us to find an answer to question (ii). Scenario 3 will find an answer to question (iii) by grouping cohorts in the fitted model. Scenarios 4 and 5 will provide an answer to question (iv), assessing the effect of grouping in the DGP and the fitted model, both when those groupings match (4) and when they do not (5). Finally, scenarios 6 and 7 aim to answer question (v), including a cohort term in the fixed part of the fitted model, and assessing the performance of the model in the presence of a linear cohort trend (6), and then a period trend (7). In all

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<sup>11</sup> The same applies to non-linear period and cohort trends in the DGP, which would also need to be converted into discrete effects, and not reassigned into other APC combinations as in equation 3. However, for the sake of simplicity, in these simulations we focus on linear trends.

cases there are significant age, period, and cohort effects in the DGP, but the nature of those effects (linear, random variation, etc.) varies between the scenarios.

**Table 1: DGPs and fitted models for ten scenarios for simulation<sup>12</sup>**

Scenario	DGP equation	Fitted model	Discrepancy between DGP and fitted model
1	$y = 1 + (0.1 * Age) - (0.005 * Age^2) + u_c^{(1)} + u_p + e_{L1}$	$y_{i(j_1j_2)} = \beta_{0j_1j_2} + \beta_1 Age e_{i(j_1j_2)} + \beta_2 Age e_{i(j_1j_2)}^2 + e_{i(j_1j_2)}$ $\beta_{0j_1j_2} = \beta_0 + u_{j_1}^{(1)} + u_{j_2}$	None
2	$y = 1 + (0.1 * Age) - (0.005 * Age^2) + (0.1 * Cohort^{(1)}) + u_c^{(1)} + u_p + e_{L1}$	As scenario 1	Cohorts in DGP not IID
3	As scenario 2	$y_{i(j_1j_2)} = \beta_{0j_1j_2} + \beta_1 Age e_{i(j_1j_2)} + \beta_2 Age e_{i(j_1j_2)}^2 + e_{i(j_1j_2)}$ $\beta_{0j_1j_2} = \beta_0 + u_{j_1}^{(5)} + u_{j_2}$	Cohorts in DGP not IID; different cohort groupings
4	$y = 1 + (0.1 * Age) - (0.005 * Age^2) + (0.1 * Cohort^{(5)}) + u_c^{(5)} + u_p + e_{L1}$	As scenario 3	Cohorts in DGP not IID
5	$y = 1 + (0.1 * Age) - (0.005 * Age^2) + (0.1 * Cohort^{(7)}) + u_c^{(7)} + u_p + e_{L1}$	As scenario 3	Cohorts in DGP not IID; different cohort groupings
6	As scenario 2	$y_{i(j_1j_2)} = \beta_{0j_1j_2} + \beta_1 Age e_{i(j_1j_2)} + \beta_2 Age e_{i(j_1j_2)}^2 + e_{i(j_1j_2)}$ $\beta_{0j_1j_2} = \beta_0 + \beta_3 Cohort_{j_1}^{(1)} + u_{j_1}^{(1)} + u_{j_2}$	None
7	$y = 1 + (0.1 * Age) - (0.005 * Age^2) + (0.1 * Period) + u_c^{(1)} + u_p + e_{L1}$	As scenario 6	Periods in DGP not IID
	$u_c \sim N(0,1)$ for cohorts, $u_p \sim N(0,1)$ for periods, and $e_{L1} \sim N(0,4)$	$e_{i(j_1j_2)} \sim N(0, \sigma_e^2)$ , $u_{j_1} \sim N(0, \sigma_{u_1}^2)$ , $u_{j_2} \sim N(0, \sigma_{u_2}^2)$	

<sup>12</sup> If the method works, the estimate of the cohort variance ( $\sigma_{u_1}^2$ , the variance of the residuals  $u_{j_1}$ ) in the fitted model should combine both the variance of the random cohort term ( $u_c$ ) and the linear cohort trend ( $0.1 * Cohort$ ) when they are both present in the DGP. The bracketed superscripts on the cohort terms in both the DGP and the fitted model refer to the cohort groupings. So a grouping of five on the cohort terms in the DGP means that cohorts were grouped into five-year intervals and these groups used as the units making up the cohort normal distribution ( $u_c$ ) and/or linear effect ( $Cohort$ ) found in the DGP. In the fitted model a grouping of five on the cohort residual term means that the fitted model uses five-year groups to define the cohort's random effects units ( $u_{j_1}$ ). A value of 1 in either column means there is no grouping. All variables in both the DGP and the fitted model were grand-mean-centred.

For each scenario 1000 datasets were randomly generated and the scenario model was fitted to each of these datasets. Each dataset consisted of 20,000 individuals with a random uniform distribution of ages (between 20 and 60) and periods (between 1990 and 2010<sup>13</sup>). Cohorts were calculated on the basis of these values and the dependency expressed in equation 1. The dependent variables were generated with trends and residuals<sup>14</sup> as specified by the DGPs in Table 1. The data were generated in Stata, and the models were estimated using MCMC<sup>15</sup> Gibbs sampling in MLwiN v2.25 (Rasbash et al. 2013) with the `runmlwin` (Leckie and Charlton 2013) command in Stata. When using MCMC methods a number of issues need to be considered. First, we must choose what starting values should be used. Here, as we are simulating our data, we know what the true values are. As such, we use these values as starting values. Whilst we would not have this information in reality, doing the simulations this way allows us to assess biases in the model unencumbered by any issues of convergence and bad starting values. If the model fails to work when we are telling it the answer, we can be pretty sure that something is seriously wrong!

Despite this, it is still important to test for convergence of each model that we run. In order to do this we create a version of the Potential Scale Reduction Factor (PSRF) (Brooks and Gelman 1998; Gelman and Rubin 1992). The original version of this compares the variance of five different chains (from five different starting values) compared to the variance of the pooling of all five of these chains. As we are not interested in starting values here (and know that the starting values we are using could not be better), instead we use a single chain divided in five, and compare the 95% coverage intervals of each of the five chains to that of the whole chain. In addition to this we use the Effective Sample Size (ESS) for each parameter (which is automatically calculated by MLwiN) to assess whether the chain has been run for long enough for sufficient 'independent' draws to characterize the distribution of the parameter. Third, we visually inspected a small sample of the trace plots of the parameters (it is obviously not feasible to visually inspect the trace plots for all parameters from all 7000 models run)<sup>16</sup>. Finally, we use hierarchical centring (Browne 2009: 401) at the cohort level to

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<sup>13</sup> We additionally tested the model using a wider range of years (between 1970 and 2010 rather than 1990 and 2010). The results were substantively similar.

<sup>14</sup> Where cohorts were grouped, residuals were calculated [with the variance (of 1) as stated in Table 1] and then averaged over those groupings, before being included in the DGP. For grouped cohort trends, cohort groups were assigned the value of their earliest year, before being included (centred) in the DGP [with the coefficient (of 0.1) as stated in Table 1].

<sup>15</sup> We use MCMC rather than maximum likelihood methods because of the small number of periods that are in our sample (and which is typical of the data we use). Stegmueller (2013) finds that having only a small number of higher level units will significantly bias variance estimates using ML, whilst affecting MCMC estimations only slightly. See also Browne and Draper (2006).

<sup>16</sup> An example of these trace plots (from the first model run from scenario 3) can be found in this article's appendix. As can be seen, the parameters are behaving well in their estimates, with no long-running trends or multimodality in the Markov chains.



reduce dependency in the chains. We have found that a chain of 100,000 iterations, with a 5,000-iteration burn-in, is in general more than sufficient to achieve good values for both the PSRF and the ESS for the vast majority of the simulations.

## 5. Simulation results

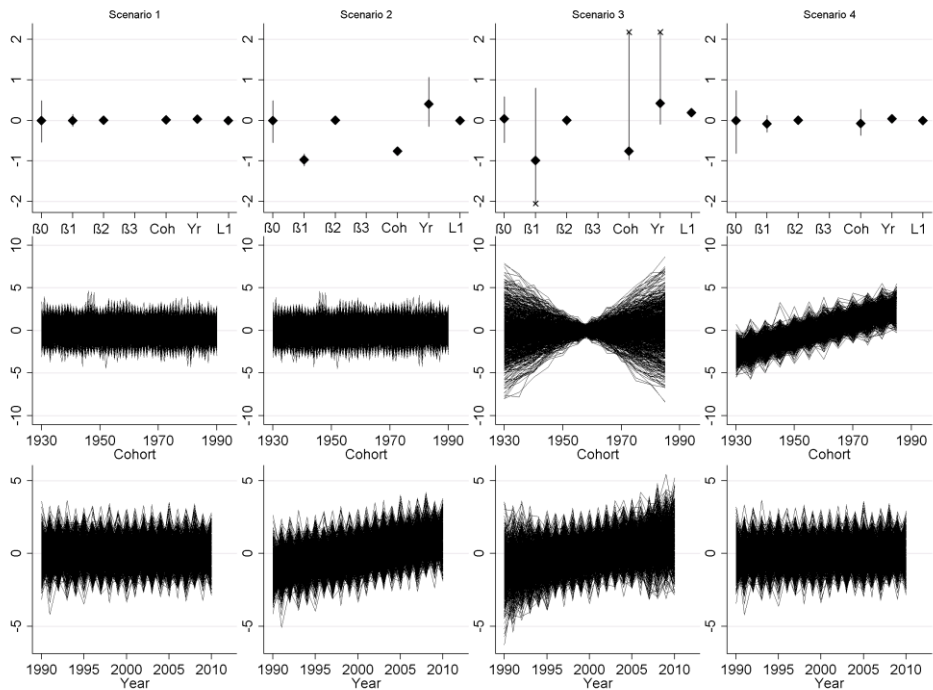
The full simulation results (and the Stata code used to create them) can be found online, and graphs representing the results of each of the seven simulation scenarios can be found in Figure 3. The first row of graphs shows the relative bias<sup>17</sup> of the estimates (the medians<sup>18</sup> of the monitoring chain) for each of the parameters in the model. If the model estimates match the DGP the median bias of each parameter should be zero. The second row shows the cohort shrunken residuals, where each line represents one of the 1000 simulation runs for each scenario. We expect these to appear as random white noise, except where there is a cohort linear trend in the DGP (i.e., scenarios 2-5); in which case we would hope to see that trend in the residuals (given the size of the linear trends in comparison to the size of the random variances in the DGPs). Similarly, the final row shows the period level residuals, and again, with the exception of scenario 7 where there is a period trend in the DGP, we would hope that these appear as random white noise.

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<sup>17</sup> Relative bias is calculated as the difference between the estimated and actual values, divided by the actual value.

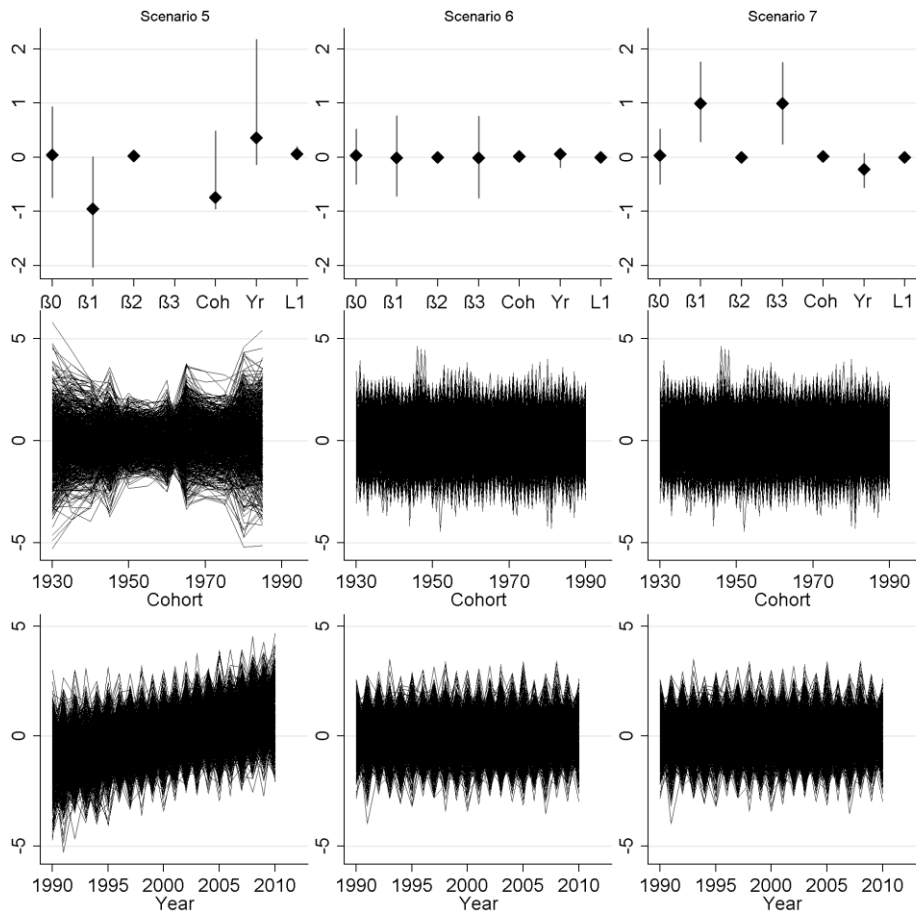
<sup>18</sup> The median was found by Browne and Draper (2006) to be the least biased point summary (rather than mean, or mode) of the MCMC parameter chains. However the results were not substantively different when the mean or mode was used.

**Figure 3:** Results for each of the simulation scenarios explicated in Table 1.  
**Row 1:** Relative bias (median, and 95% coverage intervals<sup>19</sup>) of each of the parameters in the model. If the parameter estimates are unbiased all the medians should lie on the zero line.  
**Row 2:** Cohort-level shrunken residuals as estimated for each of the 1000 simulations.  
**Row 3:** Period-level shrunken residuals as estimated for each of the 1000 simulations



<sup>19</sup> Some of the parameter estimates for scenario 3 had relative biases with coverage intervals extending beyond the range of the graph; these have been cropped out, as indicated by 'x'.

**Figure 3: (Continued)**



Here, the results are structured by the five questions outlined above.

### **5.1 Does the HAPC model work when period and cohort effects are normally distributed in the DGP?**

Scenario 1 illustrates a situation in which there is no cohort or period trend, but there is random cohort and period variation. The model does not group cohorts, nor is there any grouping of the data in the DGP. As can be seen in Figure 3, the model estimates are largely unbiased<sup>20</sup>, and the model has not put any erroneous trends in the cohort or period residuals (they appear as white noise). As such, in situations where there is no period or cohort trend in the data the HAPC model seems to work, and this applies even when cohorts are ungrouped.

### **5.2 Does the model work when there is a trend in the cohort effects in the DGP?**

Scenario 2 is the same as scenario 1, but with an additional linear cohort trend in the DGP. As can be seen, the model suffers from severe bias. The age and cohort trends (linear effects of 0.1) have effectively been combined by the model to instead estimate a linear period trend of 0.1, as shown in the period residuals, and eliminating the age and cohort trends (which do not appear in the model estimates at all). This is unsurprising because the data would be identical if it had been created by a simple linear period trend rather than the combination of age and cohort that actually created the data. To reiterate, we know that the MCMC starting values we have used are as good as they can be, so these cannot be blamed for the poor performance of the model.

### **5.3 Does grouping of cohorts in the fitted model help to achieve the correct answer?**

Scenario 3 is the same as scenario 2, except that cohorts are grouped into five-year groups prior to estimation. It is often argued (e.g. by Page et al. 2013) that this can solve the identification problem, but, as the graphs in column 3 of Figure 3 show, this is not the case. The model suffers from exactly the same average bias as in scenario 2. What is different is that the model is much less reliable in how it assigns effects to age period and cohort. Whilst scenario 2 always (erroneously) assigned the entire trend to period instead of cohort and age, the results from scenario 3 produced a range of combinations of APC linear effects. Each of these combinations produces the same data so the combination that a given model chooses cannot be based on the specific DGP

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<sup>20</sup> There is some positive bias in the period variance term. This is caused by the small number of periods in the data.

that created the data. For example, some of the 1000 simulations produce estimates equivalent to linear effects of -0.2 for age, -0.2 for cohorts, and +0.3 for periods; effects which would produce the same data as the effects of +0.1 for age and +0.1 for cohorts in the DGP. There is also some positive bias in the level 1 variance, but this is unsurprising – there is some variance between single-cohort years but within cohort groups, which the model can only account for at level 1.

#### **5.4 What happens if there are groupings in the DGP as well as the fitted model?**

Scenario 4 fits the same model as scenario 3, but the data was generated with cohorts grouped in the same five-year intervals used to define cohorts by the model being fitted. As can be seen, the model estimates match the DGP relatively well. The model correctly assigns a trend to the cohort residuals and not to the period residuals. There is some bias (in this case, about 9%) in the estimation of the age coefficient; this is caused by correlation between the age variable and the cohort level residuals. But in comparison to the bias present in the other scenarios this is relatively minor.

However, it is not enough to simply know that the cohorts are grouped in some way; it is necessary to know exactly how those groups are formed. Scenario 5 investigates mismatched groupings between the DGP and the fitted model, and it has the same problems of bias found for scenarios 2 and 3. In some cases we may be able to make educated guesses as to how cohorts are grouped – baby boomers may share characteristics, for example – but it would be very rare to be able to delineate accurate cohort groupings because we rarely have theory that is so exact in the social sciences. Whilst we may agree that the baby boomers share characteristics, it would be difficult to reliably say exactly when the baby boomers started and finished.

#### **5.5 What does including a linear cohort (mean-age) term in the fixed part of the model do?**

We have seen in scenario 4 that, when we have solved the identification problem (albeit in a way that is rarely practical with real-life data), there remains some bias because the age variable is correlated with the cohort residuals. In previous work we (Bell and Jones 2013c) have argued that this bias can be solved by including the group mean of the biased variable, decomposing the variable's effect into a 'within' and a 'contextual' effect. However the cohort mean of this age variable is exactly collinear with the cohort variable itself. As such, including it in the model as a linear fixed effect is equivalent to

including the cohort variable, and in doing so assuming that there is no linear period effect in the DGP.

When such a model is fitted (scenario 6) we see that the problem of bias in the age variable is solved. The age and cohort trends are correctly estimated in the fixed part of the model, whilst the random period and cohort variation is correctly estimated in the random part without the trend. The problem with this model is that it assumes there is no period trend. When this assumption is violated (as in scenario 7), the period trend (of 0.1) is redistributed into age and cohort effects, which are then overestimated (on average the age effect is estimated as 0.2 rather than 0.1, whilst the period effect is estimated as 0.1 rather than 0). No model-fitting criterion will be able to help choose the correct model (age and period vs. age and cohort linear trends) because both models will fit the data equally well<sup>21</sup>. As such this model is not able to correctly assign APC trends. Indeed, it is clear to us that no model can.

## 6. Discussion

We hope that this paper will function as a warning to those hoping to disentangle APC effects. The results make clear that no technical solution can break the logical or mathematical relationship of age, period, and cohort without strong a priori assumptions<sup>22</sup> being imposed and being correct. Whilst we have only addressed one method here, other methods have also been proposed in recent years (e.g. Tu, Smith and Gilthorpe 2011), and we would encourage anyone considering using them to run simulations of the sort used here first, to ensure that the assumptions being made are appropriate for the research project at hand. Research on one such method, the Intrinsic Estimator (Yang et al. 2008), has already revealed similar issues (Luo 2013a, 2013b) and shown the impossibility of what the model purports to do (Fienberg 2013; Held and Riebler 2013).

Unsurprisingly, given the results of our simulation, we have found papers with results that we believe may be misinterpretations of the underlying process. A recent example using the HAPC method (Dassonneville 2013) looked at voter turnout volatility over time in the Netherlands. Much like the results from scenarios 2, 3, and 5 of our simulation study, they find a strong, approximately linear trend in their period-

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<sup>21</sup> Indeed, we ran these two models, using the first set of simulated data from scenario 7. The result was two models with substantively different parameter estimates but identical DICs and deviances – that is, model diagnostics are unable to tell these fundamentally different models apart.

<sup>22</sup> From a Bayesian perspective, such assumptions can be thought of as informative priors that can be imposed in MCMC estimation. We have shown in previous work (Bell and Jones 2013b) that these priors must be very strong and correct to pull the estimations to the truth. Unfortunately, such knowledge is not usually forthcoming.

level residuals (equivalent to a linear effect of around +0.01), which they argue goes against the prevailing view that societal change occurs by cohort replacement.

“The result that cohort effects are much less important in explaining volatility than period effects are, raises a number of questions with regard to previous findings. It is remarkable that research on time effects explaining the decrease in turnout in Western Europe does find generations to be crucial.”

Dassonneville (2013:9-10)

We suspect that, much like our simulation results, the period trend that they find is erroneous. It could easily have been produced by a combination of a cohort and an age effect. As such, rather than the found age effect of -0.02 and period effect of approximately +0.01, the true DGP could have included a linear age effect of -0.01 and a cohort effect of +0.01; the data resulting from these two possible DGPs would be identical and indistinguishable.

This paper on voting is not alone in finding trends in periods and cohorts that are potentially problematic. Reither, Hauser and Yang (2009) and Yang and Land (2013:215-222) find that there is a very significant period trend in obesity; that “the pattern of predicted probabilities for U.S. adults shows a monotonic increase over time, with no sign of abatement in recent periods of observation” (Reither, Hauser and Yang 2009:1443). As we have shown elsewhere (Bell and Jones 2013a), this could have been the result of cohort effects and not periods – again the results are indistinguishable in a practical application. Conversely, Schwadel (2010:13) argues that his model shows evidence for “a large across-cohort decline in [religious] service attendance when control variables are included in the model”. Whilst Piontek et al. (2012) do not find a significant overall period or cohort variation in cannabis use, their results are suggestive of a period trend in some of their models, and they express surprise at the lack of a cohort trend. All of these results have to be treated with some circumspection, with the identified APC effects being possibly formed by a combination of other APC trends in the DGP.

Having said this, we do think that the conceptual structure that underlies the HAPC model can be valuable. It is necessary to understand the difference between a linear (or, indeed, a non-linear) trend and random variation. If there are linear (or non-linear) trends in the DGP no model will be able to tell them apart, even if they are treated as random variation as they are in the HAPC model. Researchers must use theory to decide which of the possible APC combinations makes the most sense. However, the HAPC model is able to assess random variation in periods and cohorts, so long as any trends are absorbed in the fixed part of the model. These may well be of substantive

interest, telling us, for example, that baby boomers have a higher level of literacy (a cohort effect), or that voter turnout in America was particularly high in the 1960 election between Kennedy and Nixon (a period effect): both of these results can be interpretations of the examples used by Frenk, Yang and Land (2013)<sup>23</sup>. In both of these cases it seems that the age terms in the fixed part of the model accounted for all APC trends<sup>24</sup>. However, where this is not the case a cohort term should be included in the fixed part of the model, as in scenario 6 in this paper. This of course makes the assumption that there is no linear (or non-linear) period trend, and this assumption, whilst often reasonable (for example, see McCulloch 2013), must be made explicitly.

Finally, it is worth pointing out that there are other challenges associated with using the HAPC model. We have not addressed issues of MCMC starting values and how sensitive the model is to these. The model also assumes that periods and cohorts are independent of each other; this assumption is likely to be broken as cohorts and periods that are near will usually be more related than those that are far apart. However, methods to overcome this have been suggested elsewhere (Stegmueller 2014), so we do not address them here.

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<sup>23</sup> The former example is also used in Yang and Land's other papers on HAPC models (Yang 2006; Yang and Land 2006, 2008). In addition, Yang's (2008a) paper on social inequalities in happiness also found minimal trends in the period and cohort residuals, meaning the results of that paper are probably justifiable.

<sup>24</sup> The authors do not recognise this, however; they do not distinguish between linear trends and random variation. In the case of the literacy example they even argue that their results suggest "that there has been an intercohort decline in vocabulary knowledge" (Yang and Land 2006:93).



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## Appendix

Trace plots for the deviance and each of the parameters for the first model under scenario 3. As can be seen, the model is behaving well – there is no evidence of trending or multimodality and the traces appear like white noise. All other model estimates tested behaved in a similar way.

