Psychological methods (2021)

**DOI:** https://doi.org/10.1037/met0000427

# Controlling for cohort effects in accelerated longitudinal designs using continuous- and discrete-time dynamic models

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This manuscript has been peer-reviewed and accepted for publication in the APA journal *Psychological Methods*. This paper is not the copy of record and may not exactly replicate the final, authoritative version of the article. The final article will be available, upon publication, via its DOI: 10.1037/met0000427

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Acknowledgements: This work was funded by the Ministry of Science and Innovation of Spain (ref. PID2019-107570GA-I00 / AEI / doi:10.13039/501100011033), granted to EE. Funding for data collection and curation was provided by the National Institutes of Health (National Institute of Neurological Disorders and Stroke (R01 NS057156) and the National Science Foundation (Grant BCS1558585), granted to SB and EF. The authors thank Prof. Michael D. Hunter for his assistance in the specification of the continuous time models.

Abstract

Accelerated longitudinal designs (ALDs) allow examining developmental changes over a period of time longer than the duration of the study. In ALDs, participants enter the study at different ages (i.e., different cohorts), and provide measures during a time frame shorter than the total study. They key assumption is that participants from the different cohorts come from the same population and, therefore, can be assumed to share the same general trajectory. The consequences of not meeting that assumption have not been examined systematically. In this paper, we propose an approach to detect and control for cohort differences in ALDs using Latent Change Score models in both discrete and continuous time. We evaluated the effectiveness of such a method through a Monte Carlo study. Our results indicate that, in a broad set of empirically relevant conditions, both LCS specifications can adequately estimate cohort effects ranging from very small to very large, with slightly better performance of the continuous-time version. Across all conditions, cohort effects on the asymptotic level ($d_{As}$) caused much larger bias than on the latent initial level ($d_0$). When cohort differences were present, including them in the model led to unbiased estimates. In contrast, not including them led to tenable results only when such differences were not large ($d_0 \leq 1$ and $d_{As} \leq 0.2$). Among the sampling schedules evaluated, those including at least three measurements per participant over 4 years or more led to the best performance. Based on our findings, we offer recommendations regarding study designs and data analysis.

Keywords: accelerated longitudinal design, state-space models, latent change score models, continuous time models

Translational Abstract

In this paper we propose an approach to identify and control for cohort effects in accelerated longitudinal designs. We use a simulation study to examine conditions related to sampling design, effect size of cohort effect, parameters affected by cohort effects, and modeling approach. Specifically, we extended a popular dynamic longitudinal model, the Latent Change Score (LCS) model, specified in discrete- and continuous-time. Our findings indicate that the proposed extension is effective for detecting and controlling for cohorts effects equivalent to those documented in the literature. Specifically, both discrete and continuous-time LCS specifications that included parameters to account for existing cohort effects were able to estimate such effects in all sampling conditions, particularly those with three or more measurements per person. However, when models did not include parameters specified to account for cohort effects, the parameter estimates were recovered with bias, which depended on the size of the existing cohort effects in the data. Finally, in situations when cohort effects did not exist in the data, the models that included parameters to account for cohort effects correctly estimated them as null in most scenarios. Based on these results, when examining ALD data in which researchers suspect there might be cohort effects, we recommend using models that include parameters to account for such effects, especially LCS models in continuous time.

Controlling for cohort effects in accelerated longitudinal designs by means of continuous- and discrete-time dynamic models

Accelerated longitudinal designs (ALD; Bell, 1953, 1954; S. C. Duncan et al., 1996), also called cohort-sequential (Nesselroade & Baltes, 1979), or cross-sequential designs (Schaie, 1965), are particularly useful yet underused designs for conducting longitudinal research. Their main purpose is to allow the researcher to examine the development of a process that unfolds over a long period of time, usually spanning years, in a much-reduced time frame. The key aspect for achieving such a purpose is the fact that different participants enter the study at different ages (i.e., come from different *age cohorts*). Each participant typically provides several repeated measures on the variables of interest during a time frame that covers only a fraction of the total period under study. Then, the features defining the general populational trajectory are estimated through the aggregation of the longitudinal information provided by the individuals, and the cross-sectional information provided by the cohorts (Bell, 1953, 1954).

In an ALD, none of the participants are followed during the complete time range of interest. Because of this, ALDs allow conducting longitudinal research for a fraction of the cost of a conventional study. Consider, for example, the large scale National Institutes of Health Magnetic Resonance Imaging (NIH MRI) study of normal brain development (Evans, 2006). This study examined the development of several cognitive abilities and brain features. Each participant was expected to provide three repeated measures, with an average interval of approximately two years between measures. The mean age at time 1 was 10.6 years, (*Sd* = 3.6). Therefore, participants with initial age equal to the sample mean were 10.6 years old at t1, and were measured again at ages

12.6 (t2) and 14.6 (t3), whereas participants one standard deviation above and below the mean were 14.2, 16.2, and 18.2 years old, and 7, 9, and 11 years old, respectively. The total duration of the study was 5 years, but the total age range spanned from 6 to 21 years, approximately. Figure 1 depicts an example of simulated trajectories in an ALD. The top panels in Figure 1 depict the latent (left) and manifest (right) individual trajectories if participants were followed during the whole age range. The bottom panels depict the information from such trajectories that would be available in an ALD spanning five years.

INSERT FIGURE 1 HERE

ALDs have been used successfully in different areas of research. For example, various studies have used data from ALDs to examine developmental changes in a range of cognitive abilities (McArdle et al., 2002), fluid reasoning (Ferrer, 2019; Ferrer et al., 2009; Green et al., 2017; Wendelken et al., 2017), memory (Fandakova et al., 2017), as well as developmental sequences linking brain structure and functioning to general cognitive ability (Estrada et al., 2019), among others. Other implementations of ALDs include the study of changes in adolescent alcohol use (T. E. Duncan et al., 1994), adolescent perceptions and values associated with English and math (Watt, 2008), or the use of homophobic epithets by adolescents (Poteat et al., 2012).

*Cohort equivalence*

The key assumption in an ALD is that the different cohorts included in the study come from the same population.[1] Such an assumption is typically termed *convergence* of the cohort-specific trajectories to the same general trajectory (Bell, 1953, 1954). However, in the context of statistical modeling, the term “convergence” also refers to

[1] ALD share other usual assumptions of between-person designs. For example, homogeneity of persons is also built into ALDs. That is, people within a cohort are randomly equivalent to one another.

finding the optimum set of values in an iterative process of model estimation. Therefore, in this manuscript we refer to this key assumption as *cohort equivalence*. Such an assumption implies that, if we could follow the youngest participants during the whole age range, their trajectories would be similar to those of the oldest participants. Similarly, if we had been able to measure the oldest individuals when they were young, they would have looked like the youngest cohort. If the age span of the study is not much longer than its actual time span, the equivalence assumption is reasonable. However, when the age span is much broader (e.g., in McArdle et al., 2002), the equivalence assumption may not be tenable. In fact, if there is a large age difference between the oldest and youngest cohorts, substantial differences between them are possible due to various factors, such as changes in the educational system, environmental resources, and historical events, to name a few. Although age is typically used to define cohort, as in birth cohort, it is these social and economic factors that are indeed responsible for differences in cohorts. Indeed, the term "cohort" can be used in reference to any group of individuals who share a defining feature. However, because developmental researchers are often interested in characterizing changes as a function of biological age, in this study we use "cohort" to refer to participants born in the same year (i.e., birth cohort), as is typically the case in ALDs.

Previous research has shown that, when the assumption of cohort equivalence is met and thus there are no cohort differences, the parameters of the generating process can be adequately recovered. For example, Estrada & Ferrer (2019) showed that, with sample sizes above 200 cases, the application of various ALD sampling schedules allowed recovering the parameters defining the population's trajectory with a latent change score model (LCS; McArdle, 2009). However, Estrada & Ferrer (2019) also found that, in the presence of cohort effects, several parameters defining key features of the population trajectory were recovered with substantial bias. This bias led to poor confidence interval coverage rates, and was found for parameters that differed across cohorts, and importantly, also for parameters that were cohort-invariant. That study, however, did not include a systematic examination of how cohort effects of different sizes, affecting different parts of the trajectory, impact the recovery of the generating parameters. The main goal of the present study is to propose an approach to detect and control for cohort differences in accelerated longitudinal designs. Specifically, we focus on two theoretically-informed types of cohort differences based on the developmental literature, in the context of cognitive development during childhood, adolescence, and early adulthood.

Most cognitive abilities show a fast growth during the first years of life followed by a gradual deceleration, until a maximum point somewhere between 20 and 30 years – the exact age depends on the specific ability and the specific individual (McArdle et al., 2002). After this point, some abilities such as processing speed, working memory capacity, fluid reasoning, slowly decrease (Ferrer & McArdle, 2004; Kail, 1991; Kail & Park, 1992; Kail & Salthouse, 1994; Salthouse & Kail, 1983), whereas others such as crystallized intelligence, continue to rise at a very low rate (McArdle et al., 2002).

Many ALDs focus on development from childhood to early adulthood. In this age range, all cognitive abilities show decelerated growth, which can be modeled as an exponential trajectory. In this context, cohorts can differ in at least two critical aspects: the initial mean and the maximum level. The first aspect, the initial mean, represents the average cognitive level when $t = 0$. Nonequivalence in the initial mean would imply systematic differences in ability at this timepoint between individuals born in different years. This is depicted in the left panels of Figure 2. The second critical aspect is the asymptotic or maximum level to which the mean trajectory tends. Non-equivalence here would imply systematic differences in the mean peak level of ability between different cohorts. This type of non-equivalence is depicted in the right panels of Figure 2.

INSERT FIGURE 2 HERE

The trajectories in Figure 2 represent higher average levels achieved by younger cohorts (i.e., individuals born later). In other words, younger individuals would achieve higher performance levels than older individuals. Effects along these lines have been reported in the literature of cognitive abilities. For example, it is well established that IQ scores have raised during the twentieth century (these gains are usually termed the "*Flynn Effect*"; Flynn, 1984; Nisbett et al., 2012; Trahan et al., 2014). A comprehensive meta-analysis reported a mean estimated gain of 2.31 IQ points per decade (Trahan et al., 2014). Using as a reference the standard deviation of any given cohort (15 IQ points), such an increase would imply a standardized difference of $d = 2.31/15 = 0.15$ per decade ($d = .015$ per year).

*Latent change score models for developmental research*

An increasingly large number of developmental studies, especially in the area of cognitive development have applied Latent Change Score models (LCS, also called latent difference score models, Ferrer & McArdle, 2003, 2010; McArdle, 2001, 2009; McArdle & Hamagami, 2001). LCS models represent the process of interest as a dynamical system in which the changes, instead of the levels, are the focus and are modeled as latent variables. A path diagram of a univariate LCS is depicted in the top left panel of Figure 3. LCS models allow examining lead-lag sequences between the different elements of a multivariate system, and capturing dynamical auto-regressive features that cannot be detected by means of other longitudinal models such as multilevel linear models.

At each repeated occasion $t$, a latent variable representing changes ($\Delta y_t$) in a latent process ($y_t$) is specified. Thus, at each occasion, the latent process is a function of the initial unobserved level, $y_{l0}$, plus the accumulation of changes up to that occasion. One general specification to capture such changes is the so-called *dual LCS* model, in which changes are a function of: (a) an additive linear effect captured by the latent variable $y_a$, and (b) a proportional effect from the latent level of the process at the previous occasion—captured by the self-feedback parameter β. The means of the latent initial level and slope ($\mu_0$ and $\mu_a$) capture, respectively, the mean level in the process at the first occasion, and the average additive component at every repeated occasion. The variances of these latent variables ($\sigma^2_0$ and $\sigma^2_a$) denote individual differences in such initial level and additive change. These two components are typically allowed to be correlated ($\sigma_{0,a}$ expressed as a covariance, or $\rho_{l0,a}$ as a correlation). The variance of the measurement error, or observed variability not due to the process, is captured by the parameter $\sigma^2_e$.

When modeling data on cognitive development or achievement from childhood to early adulthood, an overall positive trend—that is, growth— is typically found in the scores over time. In turn, the proportional effect β is negative, representing a damping effect—that is, the process tends to an equilibrium point or asymptote. The combined effect of the linear and proportional components of change leads to a nonlinear exponential trajectory with less overall gains over time. For more information on LCS models and their application to developmental change, see McArdle (2001), McArdle & Hamagami (2001), Ferrer & McArdle (2004, 2010), Ferrer et al. (2007), and Kievit et al. (2018).

Because the equation capturing the development is defined for the latent changes ($\Delta y$), instead of the levels ($y$), multivariate LCS models specify first-order ordinary difference equation systems in discrete time that allow capturing developmental trajectories with various functional shapes. Another interesting feature is the fact that they include a measurement structure that allows: a) partitioning the observed variance of each construct into measurement error variance and latent relevant variance, and b) specifying common latent constructs measured by multiple observed indicators (Widaman et al., 2010, see an LCS example in Estrada et al., 2019).

However, using LCS models for data obtained in an ALD entail one important problem: the standard LCS model is specified in discrete time (LCS-DT), and assumes that: a) all the intervals are constant between time points and participants (i.e, the interval between *t* and *t*-1 is always the same); and b) every measurement is taken at the exact same time for every participant. These conditions are almost never met in ALDs. Consider again the NIH MRI study of normal brain development as an illustrative example: even if this study was carefully planned and the time lags were very similar for most participants, the specific interval between any two given measurements varied widely across cases and measures, ranging from 314 to 1588 days. Furthermore, in any ALD, the exact time at which each individual is measured will always vary between individuals, even if they are in the same cohort. For example, if the relevant time metric is biological age, two participants from the same cohort, assessed in the same day, were likely born in different days, so the actual ages can be, say, 8.15 and 8.65 years, respectively.

In sum, the fact that the standard LCS model is defined in discrete time requires the researcher to assume constant time points and time lags, an assumption difficult to hold in empirical work. Previous research has shown that such an assumption leads to nontrivial overestimation of the measurement error variance in LCS-DT models applied to data from ALDs with nonconstant time lags (Estrada & Ferrer, 2019). This finding is consistent with previous studies on the effects of sampling-time variation on parameter estimation in latent growth curves (Miller & Ferrer, 2017). In the next section, we introduce continuous-time dynamic models as a solution to this problem.

*Modeling latent change in continuous time: State-space models*

In recent years, various authors have proposed the use of continuous time (CT) dynamic models for characterizing psychological processes that evolve over time (Boker, 2001; Boker et al., 2004; de Haan-Rietdijk et al., 2017; Deboeck & Preacher, 2016; Ji & Chow, 2019; Oravecz et al., 2009, 2011; Oud & Jansen, 2000; van Montfort et al., in press; Voelkle et al., 2012; Voelkle & Oud, 2012, 2015). These models typically use differential equations to describe the longitudinal trajectory of the variable of interest, which is assumed to unfold continuously. Every time-specific measure is considered a discrete realization of that continuous process. Such observed measures are typically linked to the latent continuous trajectory (and measurement error is partitioned out) by means of a measurement structure, similarly to confirmatory factor analysis.

Continuous-time models offer a number of important advantages, particularly in the context of ALDs. First, in ALDs, each individual provides only a few observations at discrete time points, and these time points differ across individuals. Because of this feature, CT models appear to be an optimal analytical choice for describing such trajectories. Second, CT models make it is easier to compare parameters that were estimated based on different time intervals. Indeed, CT models yield estimates that are independent of the time lag and can be transformed to any specific time interval (Voelkle et al., 2012; Voelkle & Oud, 2015). Third, most psychological processes are assumed to unfold in continuous time. Therefore, a CT model is proposed as a more theoretically accurate representation of those processes, as it explicitly captures this feature (Oud & Delsing, 2010). Fourth, the LCS-DT model described in the previous section is a specific case of an underlying CT model (Estrada & Ferrer, 2019). Such CT model contains the same information as the DT model and more. The former accounts for the order of measurement occasions, but also for the time interval between them. Therefore, in many scenarios, including the case of the LCS model discussed here, the parameters from the CT model usually contain all the necessary information to reconstruct the DT parameters for any specific time length, whereas the opposite is not true (Voelkle et al., 2012; Voelkle & Oud, 2015). Note, however, that not all DT models are the discretization of a CT model (Hamerle et al., 1991; He & Wang, 1989).

In this study, we apply a state-space model in continuous time (SSM-CT) to detect and control for cohort effects in ALDs. State-space models have been used in econometrics, engineering and psychology to describe dynamics in time series (Chow et al., 2010). They are composed of two main parts: the state and output equations. In this study, we use a specification including seven parameters, mathematically equivalent to the parameters in an LCS-DT model. The *state* (*or transition*) *equation* is a first-order ordinary differential equation describing change in a vector of latent variables for an infinitesimally short time interval (*dt*). Such a change is a function of the state of the latent vector at time *t* (i.e., the equation describes a continuous-time dynamic system),

$$\frac{d}{dt}\begin{bmatrix} y_{l,i} \\ y_{a,i} \end{bmatrix} = \begin{bmatrix} \beta & 1 \\ 0 & 0 \end{bmatrix}\begin{bmatrix} y_{l,i} \\ y_{a,i} \end{bmatrix}(t) \qquad [1]$$

where the $y_l$ is a latent variable representing the level of each individual *i* at time *t*, and $y_a$ is latent variable adding a constant magnitude to $y_l$. The parameters in the drift matrix (first matrix in the right-hand side of Equation 1) imply that $y_l$ changes as a function of itself (self-feedback β), and $y_a$ (with a loading of 1). In contrast, $y_a$ neither changes nor receives any influence from $y_l$, and thus is time-invariant. The role of $y_a$ is to allow changes in the mean trajectory of $y_l$ over time. Both latent variables allow between-individual variability (i.e., random effects), which are specified for $t$ = 0 as

$$\begin{bmatrix} y_{l0} \\ y_a \end{bmatrix} \sim N\left( \boldsymbol{\mu} = \begin{bmatrix} \mu_0 \\ \mu_a \end{bmatrix}, \boldsymbol{\Sigma} = \begin{bmatrix} \sigma_0^2 & \sigma_{0,a} \\ \sigma_{0,a} & \sigma_a^2 \end{bmatrix} \right). \qquad [2]$$

In a univariate system, the mean trajectory tends to an asymptote with value ($\mu_a$ / - β). Such asymptote is reached at $t = \infty$ with negative values of β, or $t = -\infty$ with positive values of β. Therefore, the parameter $\sigma^2_a$ can be interpreted as capturing individual differences in the asymptote (i.e., if $\sigma^2_a = 0$, all trajectories tend to the same asymptotic level).

The time-specific observations ($Y_t$) are linked to the latent level $y_t$ through the *output equation*, which is equivalent to a measurement model in dynamic or confirmatory factor analysis. In our case: $Y_{it} = y_{l,it} + e_i$, where the variable $e$ represents a measurement error, with mean 0 and time-invariant variance $\sigma^2_e$ (note that the latent variable $y_a$ is not linked to any observation). A diagram for this SSM-CT model is depicted in the bottom left panel of Figure 3.

This SSM-CT is an alternative parameterization to the standard LCS-DT described above and includes the same number of parameters. However, the interpretation of some of these parameters is different between the two models because of their different time metrics. In an LCS-DT, the parameters β, $\mu_a$, $\sigma^2_a$, and $\sigma_{l,a}$ are scaled for $\Delta t = 1$, whereas in an SSM-CT they are scaled for an infinitesimally short time interval ($dt$). Similarly, in an LCS-DT, the parameters $\mu_0$ and $\sigma^2_0$ refer to the first measurement occasion, whereas in an SSM-CT they refer to $t$ = 0, which is an arbitrary time point, and does not necessarily correspond to the first observed occasion. For further details on their mathematical relation, see Estrada & Ferrer (2019), Hunter (2018), Ji & Chow (2019), Voelkle and Oud (2015), or Voelkle et al. (2012).

*Purpose of the study*

To the best of our knowledge, no previous study has systematically examined the ability of dynamic models to identify and control for cohort differences in an ALD. This is critical, because the key assumption in ALDs is the absence of such cohort differences. If cohort effects exist, the trajectories of each cohort are not equivalent (i.e., in the original denomination by Bell, 1953, they do not "converge" into the same population trajectory).

Our goal is to propose an approach to detect and control for cohort differences in ALDs by means of LCS in discrete- and continuous-time. Specifically, we seek to examine: a) under which conditions of cohort effects is the equivalence assumption tenable and under which is not, and b) the performance of a methodological extension designed to detect cohort effects influencing two key model parameters: the initial level and asymptote.

## Methods

*Simulation procedure*

We generated repeated measures for a latent process *y* which unfolds in continuous time. The process is defined by the SSM-CT model described previously. The parameters of this model were chosen to represent the longitudinal development of a cognitive ability (i.e., reading ability) from childhood to early adulthood. These parameters are reported in Table 1, and were based on previous empirical studies (Ferrer et al., 2007, 2010; Shaywitz et al., 1990).

INSERT TABLE 1 HERE

The parameters in Table 1 were used for generating the data in all the simulation conditions. For each condition, different cohorts provided data for different parts of the trajectory. The generating parameters were constant across cohorts, except for the specific parameters described below. We manipulated three simulation factors: a) type and size of cohort effect, b) sampling schedule, and c) sample size.

a) Type and size of cohort effect. In the present study we examined cohort effects in favor of the younger cohort equivalent to those documented in the literature (i.e., at least the size of the Flynn effect). We included also larger effects to evaluate how extremely adverse conditions affect the recovery of the population trajectories. We generated nine different conditions. These are summarized in Table 2. The baseline condition implied equal parameters for all the cohorts in the study (i.e., no cohort effects). Four conditions generated cohort differences in the mean initial point (denoted by $d_0$), and four conditions in the mean asymptotic level ($d_{As}$). All the effect sizes were computed for a 13-year difference, the largest difference in our sampling schedules (see the following section).

In the presence of cohort effects on the *initial level*, the youngest cohort always had the same mean initial level ($\mu_0 = 10$), while the rest of cohorts had lower values (i.e., younger individuals entered the study with a higher average level). The effect size $d_0$ captures the mean initial difference between the oldest and youngest cohort, computed in the metric of the standard deviation in the initial point ($\sigma_0 = 5$, which was constant across cohorts). For example, an effect of $d_0 = 1$ implied that the $t = 0$ mean for the oldest cohort was 5 points below that of the youngest cohort ($d_0 = [(10 – 5) / 5] = 1$). In the remaining intermediate cohorts, the value of $\mu_0$ was proportionally scaled. Therefore, with $d_0$=1 and 11 cohorts, the $\mu_0$ values from the youngest to the oldest cohorts were {10, 9.5, 9, 8.5, 8, 7.5, 7, 6.5, 6, 5.5, 5}.

In the presence of cohort effects on the *asymptotic level*, the youngest cohort had a mean asymptotic level of $\mu_{As} = \mu_a$ / -β = 6 / -(-.2) = 30. The rest of cohorts had lower asymptotic levels. The cohort effect $d_{As}$ captured the mean difference between the oldest and youngest cohort, computed in the metric of the standard deviation in the asymptotic level ($\sigma_{As} = \sigma_a$ / -β = 1 / -(-.2) = 5). For example, an effect of $d_{As}$ = 1 implied that the asymptotic mean for the oldest cohort was 5 points below that of the youngest cohort ($d_{As}$ = [(30 – 25) / 5] = 1). In the remaining intermediate cohorts, the value of $\mu_{As}$ was proportionally scaled. Therefore, with $d_{As}$ =1 and eleven cohorts, the $\mu_{As}$ values from the youngest to the oldest cohorts were {30, 29.5, 29, 28.5, 28, 27.5, 27, 26.5, 26, 25.5, 25}, and the $\mu_a$ values for each cohort were {6, 5.9, 5.8, 5.7, 5.6, 5.5, 5.4, 5.3, 5.2, 5.1, 5}.

INSERT TABLE 2 HERE

b) Sampling schedule. Although many sampling schedules are possible within the ALD framework, we restricted our selection to ALDs that are feasible in a study spanning a maximum period of 5 years. Among them, we selected three schedules (designs D1, D2, and D3) shown to have the best cost-effective performance in previous studies (Estrada & Ferrer, 2019). These designs are summarized in Table 3.

INSERT TABLE 3 HERE

As a reference, we applied a “benchmark design” (D0), which included full trajectories for every individual of every cohort, measured once per year. This design includes 13 cohorts in which all participants are measured from ages 5 to 19. In other words, this is an “expanded”, rather than accelerated, longitudinal study ranging for a total of 28 years. Such design is unfeasible in real scenarios, but we included it here as a benchmark to examine the extent to which the cohort effects can be recovered when the full trajectories are available.

In all the designs, each participant was measured once during the corresponding year. The exact week of the year was chosen at random with equal probability for each of the 52 weeks in the year. This sampling procedure created random lags between and within individuals, to reproduce the varying time lags observed in empirical ALDs (Estrada & Ferrer, 2019).

c) Sample size. We generated samples of size 125, 250, and 500.

The combination of cohort effects, sampling schedules, and sample sizes led to 9 × 4 × 3 = 108 simulation conditions. For each of these conditions, we generated 100 replications.

*Data analysis: Extension of LCS-DT and SSM-CT models to account for cohort differences*

For each sample in each condition, we estimated two versions of the two statistical models described in our introduction (four models in total). Diagrams for these models are depicted in Figure 3.

INSERT FIGURE 3 HERE

a) Latent Change Score model in discrete time (LCS-DT). We implemented this model in SEM using *OpenMx* in *R* (RAM parameterization estimated with Full Information Maximum Likelihood; cf., Ghisletta & McArdle, 2012; Neale et al., 2016). This specification includes seven parameters and requires a discrete measurement occasion, and not actual age, as the underlying time metric. Because the exact time lag between measurements was different between and within cases, age bins were created in the center of each year –i.e., at ages 5.5, 6.5, 7.5, etc. In consequence, the variability in time lags between occasions is not specifically accounted for in the model, as it is the case in studies using empirical data. Using discrete measurement occasions entails creating very sparse data sets and very low coverage –or even zero– for some of the elements in the covariance matrix. A path diagram for this model is depicted in the top left panel of Figure 3.

b) Latent Change Score model in discrete time with cohort effects (LCS-DTc). We extended the standard LCS model described previously by introducing the cohort (i.e., year of birth) as an exogenous observed variable. The following additional equations were added to the model:

$$y_0 = \tau_0 + \gamma_0 \times cohort \quad [3]$$

$$y_a = \tau_a + \gamma_a \times cohort.$$

The youngest cohort, entering the study at age 5, was assigned a value of 0, the next cohort, entering the study at age 6, was assigned a value of 1, and so on. The latent initial level ($y_0$) and additive component ($y_a$) were regressed on this covariate[2] through the coefficients $\gamma_0$ and $\gamma_a$. These two additional parameters were intended to capture the corresponding linear cohort effects. Consequently, the parameters $\mu_0$ and $\mu_a$ are replaced by intercepts $\tau_0$ and $\tau_a$, and the variances $\sigma^2_0$ and $\sigma^2_a$ become residual variances after the cohort effect has been accounted for. A path diagram for this model is depicted in the top right panel of Figure 3.

c) State Space Model in continuous time (SSM-CT). This model is mathematically related to the standard LCS-DT and also includes seven parameters (see Table 1). It is composed of two equations[3]:

$$d\boldsymbol{y}_t / dt = \boldsymbol{A}\boldsymbol{y}_t + \boldsymbol{B}\boldsymbol{u}_t + \boldsymbol{q}_t \quad [4]$$

$$\boldsymbol{Y}_t = \boldsymbol{C}\boldsymbol{y}_t + \boldsymbol{D}\boldsymbol{u}_t + \boldsymbol{r}_t \quad [5]$$

[2] In our *OpenMx* specification, the covariates are included as *definition variables*.
[3] We adapt here the notation in Hunter (2018), also used in *OpenMx*. To highlight the similarities with the common LCS-DT specification, we changed the notation of the latent states (***y***) and observed variables (***Y***).

Equation 4 is a more compact expression of the *state equation* described previously as Equation 1[4]. It describes how the latent states change over time with a first-order linear differential equation: $\boldsymbol{y}_t$ is an $l \times 1$ vector of latent states, $t$ is time. The derivative of $\boldsymbol{y}_t$ with respect to $t$ is $d\boldsymbol{y}_t / dt$. $\boldsymbol{u}_t$ is an $m \times 1$ vector of covariates or exogenous variables, and $\boldsymbol{q}_t$ is an $l \times 1$ vector of dynamic noise with mean zero and covariance $\boldsymbol{Q}$. $\boldsymbol{A}$ is an $l \times l$ matrix of autoregressive dynamics –i.e., drift matrix–, and $\boldsymbol{B}$ is an $l \times m$ matrix of covariate effects on the latent states. Equation 5 is the *output equation*. It is identical to the measurement model in the SEM framework (Chow et al., 2010; Hunter, 2018) and describes how the latent states relate to the observed variables at a single point in time: $\boldsymbol{Y}_t$ is a $n \times 1$ vector of observed variables or outputs at time $t$, $\boldsymbol{r}_t$ is an $n \times 1$ vector of observation noise (or measurement error) with mean zero and covariance $\boldsymbol{R}$. $\boldsymbol{C}$ is an $n \times l$ matrix of factor loadings, and $\boldsymbol{D}$ is an $n \times m$ matrix of covariate effects on the observed variables. In this framework, the latent initial mean vector included in Equation 2 is noted as $\boldsymbol{x}_0$, and the latent initial covariance matrix is noted as $\boldsymbol{P}_0$ (Hunter, 2018). A path diagram for this model is depicted in the bottom left panel of Figure 3.

Estimation of the SSM-CT model is carried out through a set of recursive algorithms called hybrid Kalman Filter (Boker et al., 2018; Chow et al., 2010; Hunter, 2018). This procedure iterates in cycles of one prediction step (using a Kalman-Bucy filter) and one correction step (using a Kalman filter). For a given time $t$, the prediction step makes a forecast for (the factor scores in) the state vector $\boldsymbol{y}_t$ and the state covariance matrix, based on the initial state of the system at $t_0$, the time interval between $t$ and $t_0$, and the dynamics of the system ($\boldsymbol{A}$, $\boldsymbol{B}$, $\boldsymbol{Q}$ and $\boldsymbol{u}_t$). The update –or correction– step uses the observed data and the measurement model to correct the forecast from the

[4] This re-expression is possible because our model does not include time-varying covariates or dynamic noise.

previous step. The algorithm is designed to iteratively reduce the prediction error by adjusting the parameter estimates through Maximum Likelihood prediction error decomposition (for further details, see Boker et al., 2018; Chow et al., 2010; Hunter, 2018; Kalman, 1960; Kalman & Bucy, 1961).

We estimated this model using the functions *mxExpectationStateSpaceContinuousTime* and *mxFitFunctionML* in *OpenMx* (Boker et al., 2018; Hunter, 2018; Neale et al., 2016). In this implementation, a multiple group model is estimated in which each person is a group with a time series of observations, and all free parameters are constrained to be equal across "groups". This specification uses only the available data points and the exact time –i.e., age– at which they were measured.

d) State Space Model in continuous time with cohort effects (SSM-CTc). The standard SSM-CT described in the previous section was expanded by introducing the cohort (i.e., year of birth) as an exogenous observed variable. This covariate was scaled as explained for the LCS-DTc model, with a similar rationale. Two additional regression parameters $\gamma_0$ and $\gamma_a$ were specified to capture linear cohort effects on the initial and asymptotic latent levels, with the latter expressed in the latent additive component. This resulted in a model with nine parameters. In the *OpenMx* SSM specification, the $\boldsymbol{x}_0$ vector is defined to include the covariate in Equation 3 (*R* code for the models in this paper can be found in https://github.com/EduardoEstradaRs/PsychMeths2021-Cohort-effects-ALD-CT-DT). A path diagram for this model is depicted in the bottom right panel of Figure 3.

The time metric was the same for all DT and CT models: a one-point increase represented one year. After fitting all models to all samples, we obtained the parameter estimates and their standard errors. In any empirical situation, and especially when

alternative statistical models are being compared, it is highly recommended to compute various fit indices for the models and assess the extent to which the data are adequately represented by each of them. In our study, however, the fit indices most suitable for comparing our models – -2*LogL*, *AIC* and *BIC* – are completely dependent on the number of data points available in the dataset. Consequently, the CT and DT versions of each model are expected to achieve the exact same fit (as, in fact, they did). The variability –i.e., standard deviation– in model fit was also fairly similar between CT and DT versions of each model. Thus, these fit results are not reported. The standard error for the relative biases is available from the authors upon request. The *R* code for estimating the four models is available at:
https://github.com/EduardoEstradaRs/PsychMeths2021-Cohort-effects-ALD-CT-DT

## Results

The models converged for all samples and conditions. Based on the estimates and standard errors, we evaluated accuracy by computing the relative bias and 95% confidence interval (CI) coverage of the parameters. We evaluated estimation efficiency through the empirical standard deviation of the estimates. In addition, we also computed the relative bias of the standard errors. Due to the space restrictions, here we include only the results regarding relative bias, coverage, and estimation variability. Other relevant information can be found in the Supplementary Materials.

### *General estimation accuracy: Overview of Relative Bias and 95% CI coverage*

For each parameter in every condition, we examined estimation accuracy by computing the parameter's Relative Bias as $RB = (\bar{\theta}_{est} - \theta) / \theta$, where θ is the true parameter value and $\bar{\theta}_{est}$ is the average estimated value of the parameter across all replications in a given condition.[5] Values of *RB* closer to zero imply unbiased estimates, positive values imply overestimation, and negative values imply underestimation. Previous literature has considered estimates to be substantially biased when $|RB|>.10$ (Flora & Curran, 2004; Rhemtulla et al., 2012). Coverage was computed as the proportion of 95% confidence intervals around the estimated parameter value that include the true parameter value[6]. As such, 95% is the optimal value of coverage, whereas coverage below 90% is considered inadequate (Collins et al., 2001; Enders & Peugh, 2004).

First, we provide a general overview of the models' performance by reporting the Root Mean Square Relative Bias as $RMSRB = \sqrt{\sum_{k=1}^{K} RB_k^2 \big/ K}$ , where $K$ is the number of parameters in the model (either 7 for LCS-DT and SSM-CT, or 9 for LCS-DTc and SSM-CTc). These results are reported in the top section of Figure 4. The bottom section of Figure 4 depicts the mean coverage for each model, computed as mean(*coverage*) = $\sum_{k=1}^{K} coverage_k \big/ K$ . A table with the numerical results is included in the Supplementary materials.

INSERT FIGURE 4 HERE

The first finding worth noting in Figure 4 is that the models including cohort effects (SSM-CTc and LCS-DTc) showed good performance in all the conditions. In contrast, the models without parameters accounting for cohort effects (SSM-CT and

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[5] Due to the age binning and random time lags, when the LCS-DT models were applied, the first measurement occasion was at 5.5 years. Thus, the parameters related to the latent initial level were compared with the expected values for that age. They are reported in Table 1.

For $\gamma_0$ and $\gamma_a$, $\theta$ equals zero in the conditions where the corresponding cohort effects are null. In such conditions, computing *RB* would imply dividing by zero. Therefore, *RB* was computed in those conditions by dividing the absolute bias by the maximum value for $\theta$ in our study: the population value for $\gamma_0$ and $\gamma_a$ in the conditions $d_0 = 2.0$ and $d_{As} = 2.0$, respectively.

[6] Created using standard error estimates. These are Wald-type confidence intervals based on asymptotic normal theory, not profile likelihood confidence intervals.

LCS-DT) showed both substantive bias and poor coverage rates in conditions where such cohort effects were large. In particular, results were inadequate for these two models (either because *RMSRB* was above 0.1 or because the mean coverage was below 90%) in the conditions with $d_0 \geq 1$ and $d_{As} \geq .5$. Cohort effects on the asymptotic level caused substantially larger misspecifications than on the initial level. Continuous time models showed better performance than DT models. Indeed, SSM-CT achieved excellent performance in all conditions, except for a slightly high *RMSRB* in some conditions within design 1 (i.e., the ALD with lower data density). Sample size was not a relevant factor; samples of 125 cases led to similar results than *n*=250 and *n*=500.

The results in Figure 4 provide a general overview of the estimation accuracy under every condition. However, caution is warranted when interpreting these results because they hide relevant biases in some parameters. In the next section, we report the accuracy results for each parameter and focus on the comparison between the different sampling schedules.

*Accuracy of Parameters across designs*

**Design 0** includes yearly measures for each individual in every cohort across 15 years. This sampling schedule is impractical in applied settings, but it was included as a “benchmark” condition, so we could inspect the models’ performance under optimal data density. The most important finding regarding D0 is that the generating parameters can, in principle, be adequately recovered under optimal sampling conditions: The models including cohort effects showed minimal *RB* and excellent coverage for all parameters in all conditions. The only exception was a marked overestimation of the measurement error variance for LCS-DTc, leading to poor coverage rates. This overestimation was found also for LCS-DT, and is expected when a DT model is applied to a design with non-constant time lags (for details, see Estrada & Ferrer, 2019). The models not including cohort effects led to overestimation of $\sigma^2_0$ and $\sigma^2_a$, and simultaneous underestimation of $\mu_0$ and $\mu_a$, when the corresponding cohort effect was present. This is expected: when the between-cohort variability was not accounted for by the model, this effect was absorbed by the variance of the latent component affected by each type of cohort effect. Consistently, the mean differences between cohorts introduced a negative bias in the corresponding latent mean. Due to space restrictions, here we include figures for the three designs that are feasible in applied settings (D1, D2, and D3). All numerical results, and a figure for D0, are available in the Supplementary materials.

**Design 1** includes two measurement occasions per individual, separated two years apart. This is the design with the lowest data density. The results regarding *RB* and coverage for this design are depicted in Figure 5. The first notable finding here is a marked overestimation of the cohort effect on the initial level $\gamma_0$ and the variance of the latent component $\sigma^2_a$ in the models including cohort effects. For $\gamma_0$, overestimation may happen because the model has information about the initial level for only the youngest cohort. These biases decreased with larger sample size, especially for SSM-CTc. Models that do not include cohort effects were especially affected by the effect on the asymptotic level. In all the conditions, the coverage rates were excellent for the models that included cohort effects (probably due to large standard errors). In contrast, coverage rates were poor for the models not including cohort effects, particularly with $d_0 = 2$ and $d_{As} \geq .5$. As expected, larger cohort effects led to larger bias in SSM-CT and LCS-DT.

INSERT FIGURE 5 HERE (D1)

**Design 2** implies three measurement occasions per individual, separated two years apart each, thus spanning five years. The results regarding *RB* and coverage for

D2 are depicted in Figure 6. This design led to better results than D1, particularly for the models that include cohort effects. Importantly, the biases for $\gamma_0$ and $\sigma^2_a$ found in D1 were present in D2: $\sigma^2_a$ was accurately estimated, and overestimation of $\gamma_0$ was found only for LCS-DTc with $n$=125, but not for SSM-CTc. Coverage rates were excellent for SSM-CTc and LCS-DTc in all conditions. In contrast, the models not including cohort effects led to marked bias with $d_0$ = 2 and $d_{As} \geq .5$.

INSERT FIGURE 6 HERE (D2)

**Design 3** implies four measurement occasions per individual during four consecutive years. Relative to D2, each participant in this design provides more data points (4 instead of 3), but during a narrower age range (4 years instead of 5). Design 3 led to results very similar to D2. This implies that the additional measure included in D3 does not contribute to a substantial improvement in estimation accuracy. In fact, D3 led to slightly higher bias for most parameters in all conditions, and worse coverage rates for the measurement error variance in the DT models. Results from D3 available in the Supplementary Materials.

*Variability of the estimates*

We computed *SDRB* as $SD[(\theta_{est} - \theta)/ \theta]$.[7] This index allows expressing the estimation inefficiency in the same scale for all parameters, models and designs. This index is always positive, and values closer to zero imply less variability of the estimates in any given condition –i.e., more efficiency. We provide a general overview of the models' efficiency by reporting the mean *SDRB* for each model, computed as

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[7] When comparing different models, studies on incomplete data often use a measure of efficiency based on the ratio [variance(estimates from $model_0$) / variance (estimates from $model_1$)], where $model_0$ is the baseline model, expected to be more efficient –i.e., achieve lower variance of parameter estimates–, and $model_1$ is an alternative model expected to show higher variability –i.e., lower efficiency. Our Design 0 could be used as the reference condition, but because it had SDRB values below 0.1 in all conditions, presenting the values as a quotient would lead to artificially high values for the rest of designs. Therefore, we decided to present the raw values for each design and compare the variability with the true parameter value, which was invariant across conditions

mean(*SDRB*) = $\sum_{k=1}^{K} SDRB_k \big/ K$ . These results are reported in Figure 7. Results regarding specific parameters are reported in the Supplementary materials.

INSERT FIGURE 7 HERE

A surprising finding regarding *SDRB* is that sample size had little relevance for estimation efficiency. Of course, larger sample sizes led to lower *SDRB*, but the main factors affecting *SDRB* were the specific ALD and model. As expected, D0 was the most efficient sampling schedule, with mean values ranging approximately between .1 (models including cohort effects with $n$=125) and .025 (models not including cohort effects with $n$=500). Among the other three designs, D1 had a clearly worse performance in all conditions. D2 and D3 showed very similar efficiency, with marginally better performance of D2 with the models that include cohort effects. The models that do not include cohort effects showed better average performance than their alternatives. Estimation variability was fairly similar for different types and sizes of cohort effect.

## An Empirical Example: Development of Abstract Reasoning from Childhood to Early Adulthood

To illustrate the utility of the method proposed, we applied the four statistical models discussed previously (i.e., SSM-CT, and SSM-CT with cohort effects, LCS-DT, LCS-DT with cohort effects) to a data set from an accelerated longitudinal study.

### *Sample and measurement instruments*

For these analyses, we use data from the Neural Development of Reasoning Ability (NORA) study (Ferrer et al., 2009; Wendelken et al., 2011) a longitudinal research project designed to examine the development of fluid reasoning from childhood to adolescence. Data were collected using a cohort sequential design involving three waves of measurement with 201, 122 and 70 participants at the first, second and third occasion, respectively. At time 1, the participants ranged in age from 6.00 to 19.1; at time 2, the age range was 6.46 to 20.5; and at time 3 the age range was 7.75 to 21.0. Of the 201 participants, 94 (46.8%) were females and 107 (53.2%) were males. The interval between assessments ranged between 12 and 24 months.

As part of a battery of measures, at each occasion, participants completed the Matrix Reasoning subtest of the Wechsler Abbreviated Scale of Intelligence (WISC-R; Wechsler, 1981). This subtest measures the ability to select the geometric visual stimulus that accurately completes a series of stimuli that change along a particular dimension. Such change follows one or more abstract rules that the participant must infer. We refer to this variable as *abstract reasoning*.

Because this study was designed to study developmental changes in reasoning as a function of age, we defined cohorts based on biological age. We rescaled the year of birth to create a covariate ranging from 0 (younger participants, born later, and entering the study at age 6) to 13 (older participants, born earlier, and entering the study at age 19). Because the DT model requires age bins, we rounded the values to the closest integer.

*Results from DT and CT models*

Table 4 reports the parameter estimates from all four models. The fit statistics for SSM-CT with cohort effects were: -2*LL* = 2130.5, *AIC* = 1398.5; for SSM-CT: -2*LL* = 2143.9, *AIC* = 1409.9; for LCS-DT with cohort effects: -2*LL* = 2129.7, *AIC* = 1401.7; and for LCS-DT: -2*LL* = 2143.5, *AIC* = 1411.5. The parameter estimates were consistent in the DT and CT versions; the parameters not affected by the metrics of time had very similar values in the DT and CT versions of the same model. For example, the measurement error variance ranged between 8.19 (SSM-CT with cohort effects) and

8.60 (LCS-DT). Because of this consistency, we focus here on the CT models for interpreting the results.

The most important finding in Table 4 is a significant effect of the cohort covariate on the latent additive component, but not on the latent intercept. This finding was replicated in the CT ($\gamma_0$ = -1.46, $p$ = .078; $\gamma_a$ = -.125, $p$ < .001) and DT models ($\gamma_0$ = -1.19, $p$ = .145; $\gamma_a$ = -.101, $p$ = .002). The negative effect on the latent intercept implies that participants born earlier had higher reasoning ability at age 6. However, this effect was not statistically significant and cannot be assumed to exist in the population. Considering that the mean of the latent additive component was positive (in SSM-CT with cohorts, $\mu_a$ = 8.75), the negative effect on such component implies that, for any given time lag, participants born later receive a larger positive constant influence. This means that, if we were to follow the youngest cohort, in the long run they would reach the higher asymptotic level: $\mu_{Asymp} = \mu_a / (-\beta)$; in SSM-CT with cohorts, the youngest cohort is predicted to reach a mean level of 8.75 / .240 = 36.45, whereas the oldest cohort in the study is predicted to reach [8.75 + 13×(-.125)] / .240 = 29.68.

Taking as reference the residual variance in the latent additive component, in SSM-CT with cohorts $\sigma^2_a$ = .5, the difference between the youngest and oldest cohort can be expressed as Cohen's standardized difference $d = (\mu_{a,young} - \mu_{a,old}) / \sigma_a$ = (8.75 – 7.12) / .707 = 2.30. It is also possible to standardize the regression loading to obtain the Pearson's correlation coefficient between cohort and additive component: $r_{cohort,a} = \gamma_a \times (sd(\text{cohort}) / \sigma_a)$ = -.125 × (3.67 / .707) = -.65. Both estimates imply a very large effect size in favor of younger cohorts. Figure 8 depicts mean predicted trajectory by the SSM-CT without cohorts, and the cohort-specific means implied by the SSM-CT with cohort effects, along with the observed individual data.

Comparing the SSM-CT model with and without cohort effects, the inclusion of cohort effects on the latent intercept and additive component led to differences in the mean and variance of both parameters; $\mu_0$, $\mu_a$, $\sigma^2_0$, $\sigma^2_a$. In the model with cohort effects, both latent means were higher, as they capture the mean level for the youngest cohort, whereas in the model without cohort effects these parameters capture the means for the whole sample. Regarding the latent covariances, as expected, both had lower values in the model with cohort effects, as they represent residual variances, after considering the effect of the cohort. Interestingly, the variance of the additive component was not statistically different from zero in the models with cohort effects. In all cases, the latent covariance was not significant and lead to correlations very close to zero: no relation was found between the initial level and the additive component.

*Discussion of results from empirical data*

In this empirical example, we illustrated how the proposed methods can be applied to a data set from an accelerated longitudinal design. Specifically, we examined the development of abstract reasoning as a function of biological age, and examined the degree to which 13 different cohorts (age at t1 from 6 to 19 years) were equivalent in terms of their developmental trajectories. The most important finding was a significant effect of age cohort on the additive component. This effect was negative: participants born earlier, and who entered the study at older ages, were expected to reach lower asymptotic levels than younger participants. Although we did not examine possible reasons for this effect, one potential factor is the result of sampling. That is, younger children whose parents are able to bring them to the study, thus completing hours of cognitive and brain measurements are more likely to be on a more positive developmental trajectory.

Note that the development of abstract reasoning can be assumed to have exponential shape in the age range considered. However, this functional form is rather unreasonable across the whole life span (McArdle et al., 2002). In consequence, although the latent additive component is mathematically linked to the trajectory's asymptote, from a substantive standpoint it makes more sense to state that individuals born later would reach higher peak levels than their older peers.

## General Discussion

### *Summary of findings*

In this paper, we examined the ability of LCS models, defined in both continuous- and discrete-time, to detect and account for cohort differences in accelerated longitudinal designs. We also examined the extent to which cohort differences led to bias in the parameter estimates when such differences were not controlled for.

*When cohort effects were present* in the data and the models included parameters accounting for such effects (i.e., models LCS-DTc and SSM-CTc), these were adequately estimated in all conditions, particularly those with three or more measurements per individual. In other words, the cohort effects on the latent initial level and latent asymptotic level (i.e., additive component) were accurately estimated, and so were the rest of the parameters. However, when models not including cohort effects (i.e., LCS-DT and SSM-CT) were fitted to data from an ALD with cohort differences, the results were tenable only when such cohort differences were not large ($d_0 \leq 1$ and $d_{As} \leq 0.2$). In these conditions, most parameters were recovered with acceptable bias. With larger cohort effects, however, the estimates from LCS-DT and SSM-CT became untenable. Across all conditions, cohort effects on the asymptotic level ($d_{As}$) caused much larger bias than on the latent initial level ($d_0$).

*When cohort effects were not present* in the data, the models that included parameters to account for cohort effects correctly estimated them as null in most scenarios. In fact, these models showed better accuracy in almost every condition, without a substantial decrease in efficiency, than those models without parameters for cohort effects. Based on this finding, it appears reasonable to specify cohort effects in LCS models (either in discrete- or continuous-time) when suspecting such effects in data from ALDs. If cohort effects are present in the data, they will be correctly estimated in the model; if they are absent, they will be identified as such, without any bias and with only marginally larger standard errors.

Regarding the sampling schedule, Design 1 (two measures per participant, separated two years apart, spanning 3 years) provided clearly worse results than Designs 2 (three measures taken in alternate years, spanning 5 years) and Design 3 (four measures taken in consecutive years, spanning 4 years). In fact, Design 1 led to a marked overestimation of the cohort effect on the latent initial level, and the variance of the latent asymptotic level, regardless of the actual magnitude of cohort effects in the population, particularly for samples of 125 participants. Designs 2 and 3 led to excellent results across all the conditions. Both designs showed very similar performance, but Design 2 was slightly better in terms of accuracy and efficiency in most conditions. In other words, covering a wider time range with three measures per person is preferable over having a fourth measure. Based on these results and the fact that Design 2 requires one less measurement per participant, we recommend it over Design 3. Of course, this recommendation applies to the conditions simulated in this study, particularly when the researcher has reasons to assume that the model specified is correct.

Another interesting finding is that sample size had very little relevance regarding cohort effects, at least in the conditions examined in this study. As expected, the smaller sample size ($n$ = 125) led to less efficient estimation across all conditions. Furthermore, parameter biases were reduced with larger samples. However, and importantly, the specific research design, the model specified, and the type and size of the cohort effect, all had a much larger influence on the quality of the parameter estimates than sample size. Based on this, we hold that samples of 125 participants are enough for conducting ALDs in the presence of cohort effects, if the correct sampling schedule (e.g., Design 2) is applied.

*Theoretical and methodological considerations*

The present work extends previous attempts to control for cohort differences in accelerated longitudinal designs (cf., Miyazaki & Raudenbush, 2000). The procedure proposed here intends to extend modern dynamic models so that they can account for cohort effects. In particular, we proposed an extension of LCS models, both in discrete- and continuous-time. This family of models has become a standard tool in developmental research due to, among other features, its flexibility to identify lead-lag effects between different processes, detect sources of between- and within-individual variability, and include a measurement structure linking observed variables to latent constructs.

Equivalence between cohorts is a key aspect in ALDs. These designs are based on the assumption that individuals of different ages come from the same population, and it is possible to characterize a population trajectory through the aggregation of their individual information. However, if individuals born later differ from those born earlier (e.g., they enter the study at a higher initial level or they reach higher level when they exit), the equivalence assumption does not hold. The present work provides important insights into how the model parameters are affected when such an assumption is not tenable, and how cohort differences can be controlled for.

Cohorts can be defined in different ways depending on the context and research question. For example, in some studies there are variables that define cohorts clearly, such as school grade, or treatment received, among others. In contrast, the concept of cohort may be elusive in an ALD. In this study, we used biological age to define cohorts because a) this information is available in most ALDs, an b) age is very often the variable of interest on which to map developmental processes. However, our proposed approach can be readily applied to any cohort variable measured with an interval scale, as long as it is reasonable to apply such scale as the time metric for measuring both the age range of interest and the time range of the study.

Cohort differences have been widely documented in the literature on cognitive abilities. One famous example is the so-called Flynn effect. During the twentieth century, individuals born later showed systematic increases in cognitive function, particularly fluid reasoning (Flynn, 1984, 1987; Gerstorf et al., 2015; Nisbett et al., 2012; Pietschnig & Voracek, 2015). The smaller cohort effects in our study ($d_0$ and $d_{As}$ = 0.2) were specifically included to reflect empirical estimates of this effect (Trahan et al., 2014). Based on our findings (see Figures 4 to 7), we conclude that a) such "small" cohort differences are adequately recovered by the models including cohort effects (SSM-CTc and LCS-DTc), with a slightly better performance of the continuous-time version; b) models not controlling for cohort effects lead to very small biases when cohort differences affect the asymptotic level ($d_{As}$), and neglectable biases when they affect the initial level ($d_0$).

Across all our simulation conditions, cohort differences in the asymptotic level caused a larger misspecification than when differences affected the initial level (in the models not controlling for cohort effects). Relatedly, the parameter capturing cohort differences in the initial level was harder to estimate in some conditions. Both results are easier to interpret using Figure 2. Because the developmental process simulated here has a much faster growth in the early years, even large cohort differences in the initial point are compensated early on. Thus, cohort differences in the initial level are harder to estimate, but also cause a smaller bias in the parameter estimates.

When analyzing developmental data (obtained from an ALD or otherwise), a researcher can choose among various statistical models. In this study, we focused on LCS models for both theoretical and methodological reasons. From a statistical standpoint, LCS are well suited for characterizing the developmental trajectories of cognitive and other psychological variables, which can be assumed to have exponential shape during childhood and adolescence. From a theoretical perspective, the determinants of change included in a dual LCS model have a direct interpretation in terms of developmental features: the additive component is linked to the peak level and the self-feedback parameter indicates the rate of reduction between the initial and peak levels. For these reasons, and given our goal was not to compare different models, we used LCS models for generating the data and also to recover the information from such data. One consequence of this is that the fit indices for the model were uninformative because they were entirely dependent on the number of repeated measures in a given sampling schedule. However, when the generating process is unknown (as in empirical data sets) it is possible to compare different statistical models. In the context of ALD, different models could account for cohort effects differently. In such scenarios, fit indices are not a mere function of the number of repeated measures and can be used to compare among the different models.

*Limitations and future directions*

In this study, we focused on the development of a single construct over time. Systems including dynamic interrelations between two or more processes could lead to more complex trajectories in which, for example, there may not be asymptotic levels. Future research should investigate the ability of our approach to recover the trajectories in such multivariate systems.

Besides the initial and asymptotic levels, cohort differences could affect also the rate of change. Participants from different cohorts could show faster or slower maturation of the psychological process under study. In the LCS model, such variability would be captured by differences in the self-feedback parameter. However, when implementing LCS models in the standard SSM or SEM frameworks, estimating random effects in that parameter is not always possible. Future research should examine how to model this particular type of cohort effect.

In our analyses, we examined the recovery of key features of the population trajectory, including cohort effects, when such effects were positive: participants born later reached higher levels. We were interested in this type of effects because they capture relevant empirical phenomena (e.g., Flynn effect). It is unknown whether our results can be generalized to scenarios with cohort effects in opposing direction (i.e., older cohorts reaching higher levels).

In line with developmental research using ALDs, we used biological age to define cohorts. However, other sources of between-individual variability unaccounted for will affect the model estimates. Importantly, other definitions of cohort are also possible, including *study cohorts* (participants who entered the study in a particular year), *grade cohorts* (students in a particular grade), or *historical cohorts* (participants with data in a given year), among others. From a modeling standpoint, it is possible to include additional covariates for each of these types of cohorts. However, several of them can be severely confounded. Given the particular pattern of data missingness in ALDs, the separation of such different cohort effects may prove difficult, or even impossible. Future research should examine the degree to which such a separation is possible under different developmental scenarios.

Similarly, cohorts can be sometimes defined by non-monotonic categorical variables, for example, two different groups separated several decades apart, groups receiving different interventions, or students from different schools or countries, among others. In principle, such qualitatively different cohorts could be modeled using LCS models via a multiple-group specification. Another possibility would be to create dummy variables for each category, and estimate the regression coefficients for each of them. Future research should examine under what conditions these “qualitative” cohort effects can be identified and controlled for. One interesting feature of the multiple-group specification is that, in principle, it would make it possible to characterize cohort differences (either qualitative or quantitative) in any of the model parameters. For example, it would be possible to estimate a different self-feedback parameter for each cohort, and test whether the different parameters can be constrained to have the same value across groups. However, this strategy should be applied carefully in the context of ALDs. Because the sample is divided into several cohorts, each cohort may include only a few participants (e.g., 125 participants divided into 13 cohorts leads to 9-10 participants per cohort). Therefore, the cohort-specific estimate for a given parameter may be inaccurate and the method may not have sufficient power to detect cohort differences. Future research should investigate this strategy.

Other modeling approaches are available for detecting and controlling for cohort differences. For example, Driver & Voelkle (2018) developed a Bayesian framework for estimating hierarchical continuous-time models. In such a framework, it is possible to specify between-individual variability in any of the model parameters. In a similar vein, the recently developed Dynamic Structural Equation Modeling (DSEM;

Asparouhov et al., 2018; McNeish & Hamaker, 2020) allows random effects in any of the model parameters in a discrete-time framework. Both approaches hold great promise for detecting the type of cohort effects studied in this paper (i.e., cohort non-equivalence in the mean and variance of the latent intercept and additive component). Importantly, they may be useful for detecting cohort differences in other parts of the model not studied here, such as the rate of change. However, future research should examine the possibilities and limitations of applying these methods in the context of ALDs, with its unique features such as high percentage of data incompleteness, random time lags between observations, or particular shape of the developmental processes, among others.

*Conclusion*

We have shown that cohort effects can be adequately recovered in the context of ALDs. Using a simple extension of LCS models, such effects can be captured in dynamic models including latent variables in both discrete and continuous time. Based on our findings, and considering that that they apply to the conditions studied in our study, we offer the following recommendations:

1. Because cohort equivalence is a key assumption in ALDs that is typically unknown, researchers should consider the adequacy of including parameters for cohort effects in their models. Our findings show that, when cohort effects are linearly related to age and affect the initial and asymptotic levels, they will be adequately recovered by LCS models in CT and DT. Note, however, that other types of cohort effects may exist.
2. Design 1 (two measurement occasions) should be avoided because it leads to biased parameters in most conditions. If, however, this is the only option, using a State-Space model in continuous time with parameters for cohort

effects (bottom-right panel in Figure 3) is recommendable. The alternative LCS model in discrete time (top-right panel in Figure 3) will likely lead to overestimation of some parameters.

3. Designs 2 and 3 lead to similar results, with a slightly better performance of Design 2 in most scenarios. Therefore, if the researcher can assume that the generating model is adequately specified, and under the conditions simulated here, Design 2 is preferable because it achieves better results with one less measure per individual (three instead of four). Consequently, we recommend Design 2 as the most efficient sampling schedule.
4. Consistent with previous findings (Estrada & Ferrer, 2019), investing resources in covering a wider age range for each participant is preferable to increasing sample size: 125 participants appear to be enough, if the right sampling schedule is applied (e.g., Design 2).
5. Also consistent with previous research, we recommend using LCS in continuous-time, based on State-Space modeling, with parameters to account for potential cohort effects. Besides the advantages of CT modeling noted elsewhere (Oud & Delsing, 2010; Voelkle et al., 2012; Voelkle & Oud, 2015), particularly in the context of ALDs (Estrada & Ferrer, 2019), this model achieved higher accuracy than the discrete-time alternative, with only a relatively small decrease in efficiency.

We hope that the findings reported here, and the recommendations derived from them, will inform the design of future studies and facilitate an efficient use of the resources available for longitudinal research.

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Table 1. Baseline generating parameters

| Parameter | Value in DT ($\Delta t$=1) | Value in CT | Transformation |
|---|---|---|---|
| $\beta$ Self-feedback | -.181 | -.2 | $\beta_{DT} = e^{\beta_{CT} \cdot \Delta t} - 1$ |
| $\mu_0$ Initial mean | 11.795 | 10.0 | * |
| $\mu_a$ Additive component mean | 5.438 | 6.0 | $\mu_{a,DT} = \mu_{a,CT} \cdot \frac{e^{\beta_{CT} \cdot \Delta t} - 1}{\beta_{CT}}$ |
| $\sigma^2_0$ Initial variance | 24.766 | 25.0 | * |
| $\sigma^2_a$ Additive component variance | .821 | 1.0 | $\sigma^2_{a,DT} = \sigma^2_{a,CT} \cdot \left(\frac{e^{\beta_{CT} \cdot \Delta t} - 1}{\beta_{CT}}\right)^2$ |
| $\sigma_{0,a}$ Initial-Additive component covariance | 3.172 | 3.5 | $\sigma_{0,a,DT} = \sigma_{0,a,CT} \cdot \frac{e^{\beta_{CT} \cdot \Delta t} - 1}{\beta_{CT}}$ |
| $\sigma^2_e$ Measurement error variance | 2.000 | 2.0 | |
| Implied values | | | |
| $\mu_{As}$ Asymptotic level mean | | 30.0 | $\mu_{As} = \mu_a / -\beta$ |
| $\sigma^2_{As}$ Asymptotic level variance | | 25.0 | $\sigma^2_{As} = (\sigma_a / -\beta)^2$ |
| $\rho_{0,a}$ Initial-Additive component correlation | | .7 | $\rho_{0,a} = \sigma_{0,a} / (\sigma_0 \cdot \sigma_a)$ |

* In DT, the initial mean and variance refer to the first measurement occasion. In CT, they refer to $t$ = 0.

Table 2. Cohort effects in the study (in continuous time)

| | Generating parameters | | Expected estimates for cohort effects in the model | |
|---|---|---|---|---|
| Cohort effect | $\mu_0$ in the oldest cohort | $\mu_a$ in the oldest cohort | $\gamma_0$ | $\gamma_a$ |
| baseline | 10.0 | 6.0 | .000 | .000 |
| $d_0$ = .2 | 9.0 | 6.0 | -.083 | .000 |
| $d_0$ = .5 | 7.5 | 6.0 | -.208 | .000 |
| $d_0$ = 1.0 | 5.0 | 6.0 | -.417 | .000 |
| $d_0$ = 2.0 | 0.0 | 6.0 | -.833 | .000 |
| *dAs* = .2 | 10.0 | 5.8 | .000 | -.017 |
| *dAs* = .5 | 10.0 | 5.5 | .000 | -.042 |
| *dAs* = 1.0 | 10.0 | 5.0 | .000 | -.083 |
| *dAs* = 2.0 | 10.0 | 4.0 | .000 | -.167 |

Note: All the *d* values are computed for a 13-year difference, using the standard deviation of the first cohort as the standardizer for the mean difference.

Table 3. Sampling scheme for each accelerated longitudinal design. Shaded cells represent one measurement occasion for all the cohort members during the corresponding year.

| | DESIGN | Age in years: 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 | 13 | 14 | 15 | 16 | 17 | 18 | 19 | % of cases entering in each cohort |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| All cases measured at all occasions | **D0** | 13 cohorts ■ | ■ | ■ | ■ | ■ | ■ | ■ | ■ | ■ | ■ | ■ | ■ | ■ | ■ | ■ | 100 / 13 |
| **Two** occasions per case | **D1** | 13 cohorts | | | | | | | | | | | | | | | |
| | | ■ | | ■ | | | | | | | | | | | | | 13.3 |
| | | | ■ | | ■ | | | | | | | | | | | | 13.3 |
| | | | | ■ | | ■ | | | | | | | | | | | 6.7 |
| | | | | | ■ | | ■ | | | | | | | | | | 6.7 |
| | | | | | | ■ | | ■ | | | | | | | | | 6.7 |
| | | | | | | | ■ | | ■ | | | | | | | | 6.7 |
| | | | | | | | | ■ | | ■ | | | | | | | 6.7 |
| | | | | | | | | | ■ | | ■ | | | | | | 6.7 |
| | | | | | | | | | | ■ | | ■ | | | | | 6.7 |
| | | | | | | | | | | | ■ | | ■ | | | | 6.7 |
| | | | | | | | | | | | | ■ | | ■ | | | 6.7 |
| | | | | | | | | | | | | | ■ | | ■ | | 6.7 |
| | | | | | | | | | | | | | | ■ | | ■ | 6.7 |
| **Three** occasions per case | **D2** | 11 cohorts | | | | | | | | | | | | | | | |
| | | ■ | | ■ | | ■ | | | | | | | | | | | 18.8 |
| | | | ■ | | ■ | | ■ | | | | | | | | | | 18.8 |
| | | | | ■ | | ■ | | ■ | | | | | | | | | 9.4 |
| | | | | | ■ | | ■ | | ■ | | | | | | | | 9.4 |
| | | | | | | ■ | | ■ | | ■ | | | | | | | 6.3 |
| | | | | | | | ■ | | ■ | | ■ | | | | | | 6.3 |
| | | | | | | | | ■ | | ■ | | ■ | | | | | 6.3 |
| | | | | | | | | | ■ | | ■ | | ■ | | | | 6.3 |
| | | | | | | | | | | ■ | | ■ | | ■ | | | 6.3 |
| | | | | | | | | | | | ■ | | ■ | | ■ | | 6.3 |
| | | | | | | | | | | | | ■ | | ■ | | ■ | 6.3 |
| **Four** occasions per case | **D3** | 12 cohorts | | | | | | | | | | | | | | | |
| | | ■ | ■ | ■ | ■ | | | | | | | | | | | | 24.5 |
| | | | ■ | ■ | ■ | ■ | | | | | | | | | | | 12.2 |
| | | | | ■ | ■ | ■ | ■ | | | | | | | | | | 8.2 |
| | | | | | ■ | ■ | ■ | ■ | | | | | | | | | 6.1 |
| | | | | | | ■ | ■ | ■ | ■ | | | | | | | | 6.1 |
| | | | | | | | ■ | ■ | ■ | ■ | | | | | | | 6.1 |
| | | | | | | | | ■ | ■ | ■ | ■ | | | | | | 6.1 |
| | | | | | | | | | ■ | ■ | ■ | ■ | | | | | 6.1 |
| | | | | | | | | | | ■ | ■ | ■ | ■ | | | | 6.1 |
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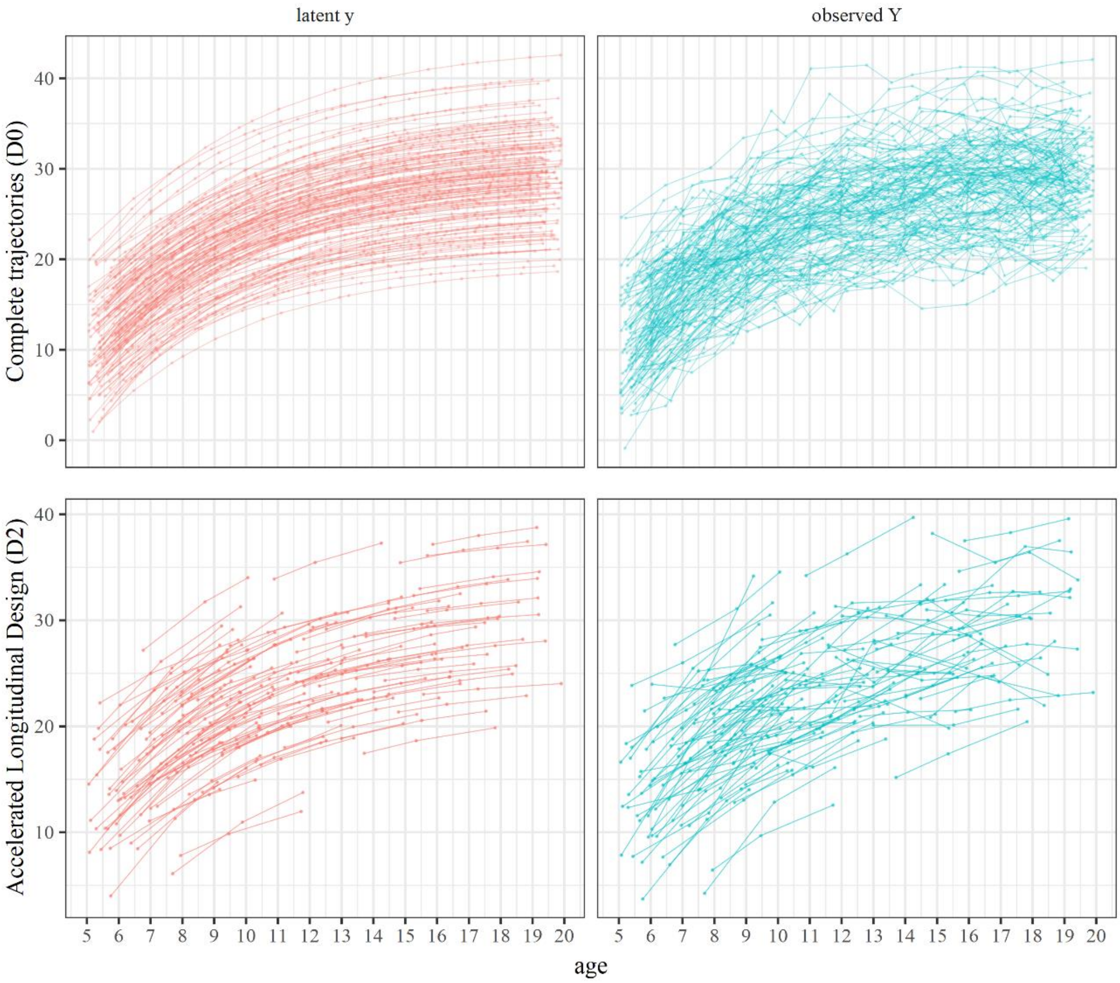
Table 4. Parameter estimates from all four models fitted to the NORA data

| SSM-CT - cohort effects | | | | | | SSM-CT | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| Parameter | *Estimate* | *S.E.* | *95% CI* | | *p* | *Estimate* | *S.E.* | *95% CI* | | *p* |
| Cohort → intercept $\gamma_0$ | -1.463 | .829 | -3.089 | .162 | .078 | - | | | | |
| Cohort → additive $\gamma_a$ | -.125 | .033 | -.189 | -.061 | <.001 | - | | | | |
| Self-feedback $\beta$ | -.240 | .054 | -.346 | -.133 | <.001 | -.362 | .041 | -.442 | -.282 | <.001 |
| Mean intercept $\mu_0$ | 11.115 | 1.465 | 8.244 | 13.987 | <.001 | 7.826 | 1.434 | 5.016 | 10.636 | <.001 |
| Mean additive $\mu_a$ | 8.747 | 1.267 | 6.263 | 11.230 | <.001 | 10.665 | 1.029 | 8.649 | 12.682 | <.001 |
| Variance intercept $\sigma^2_0$ | 69.459 | 20.162 | 29.941 | 108.976 | .001 | 67.084 | 20.603 | 26.702 | 107.466 | .001 |
| Variance additive $\sigma^2_a$ | .500 | .284 | -.056 | 1.056 | .078 | 1.245 | .493 | .279 | 2.212 | .012 |
| Covariance $\sigma_{0a}$ | -.758 | 2.103 | -4.880 | 3.365 | .719 | 2.926 | 2.326 | -1.633 | 7.485 | .208 |
| Variance error $\sigma^2_e$ | 8.190 | 1.099 | 6.037 | 10.344 | <.001 | 8.495 | 1.106 | 6.328 | 10.662 | <.001 |
| **LCS-DT - cohort effects** | | | | | | **LCS-DT** | | | | |
| Parameter | *Estimate* | *S.E.* | *95% CI* | | *p* | *Estimate* | *S.E.* | *95% CI* | | *p* |
| Cohort → intercept $\gamma_0$ | -1.188 | .814 | -2.784 | .409 | .145 | - | | | | |
| Cohort → additive $\gamma_a$ | -.101 | .033 | -.166 | -.036 | .002 | - | | | | |
| Self-feedback $\beta$ | -.206 | .048 | -.300 | -.113 | <.001 | -.293 | .028 | -.348 | -.237 | <.001 |
| Mean intercept $\mu_0$ | 11.373 | 1.428 | 8.574 | 14.172 | <.001 | 8.554 | 1.338 | 5.931 | 11.176 | <.001 |
| Mean additive $\mu_a$ | 7.467 | 1.021 | 5.466 | 9.467 | <.001 | 8.640 | .687 | 7.293 | 9.987 | <.001 |
| Variance intercept $\sigma^2_0$ | 63.270 | 18.622 | 26.770 | 99.769 | .001 | 59.739 | 18.575 | 23.332 | 96.146 | .001 |
| Variance additive $\sigma^2_a$ | .374 | .218 | -.053 | .802 | .086 | .861 | .324 | .226 | 1.496 | .008 |
| Covariance $\sigma_{0a}$ | -.104 | 2.005 | -4.035 | 3.826 | .959 | 2.646 | 1.702 | -.691 | 5.982 | .120 |
| Variance error $\sigma^2_e$ | 8.390 | 1.105 | 6.225 | 10.556 | <.001 | 8.595 | 1.091 | 6.456 | 10.734 | <.001 |

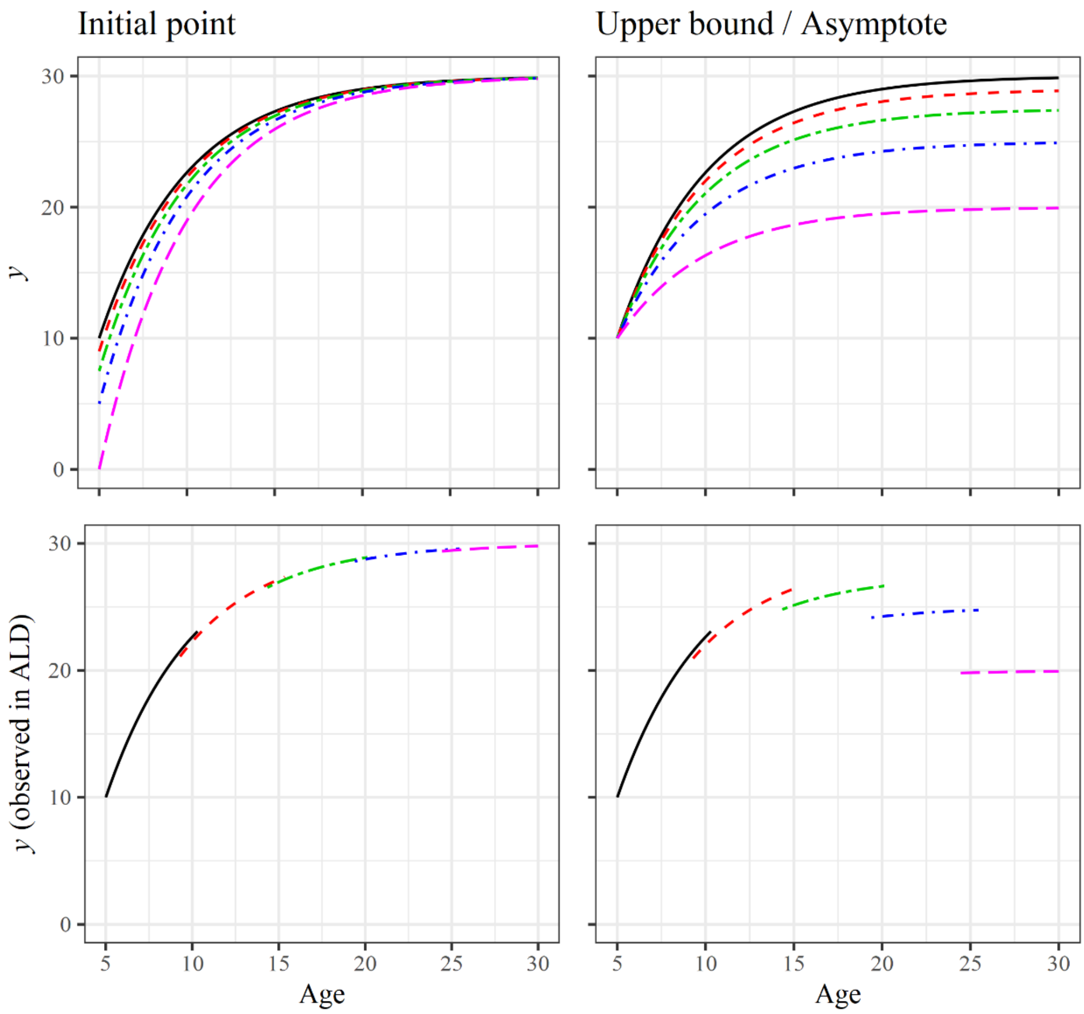
Note: SSM-CT = State-Space Model in continuous time. LCS-DT = Latent Change Score model in discrete time. S.E. = Standard Error. CI = Confidence interval

Figure 1. Example of trajectories in an Accelerated Longitudinal Design




Note: The top panels depict the latent (left) and manifest (right) individual trajectories if participants were followed during the whole age range. The bottom panels depict the information from such trajectories that would be available in an ALD spanning five years.

Figure 2. Type of cohort effects in a hypothetical 5-cohort ALD




Note: Each line represents the mean trajectory for a cohort. The black solid line represents the youngest cohort in the study.

Figure 3. Diagrams of the Discrete. (DT, top) and Continuous-time (CT, bottom) models applied

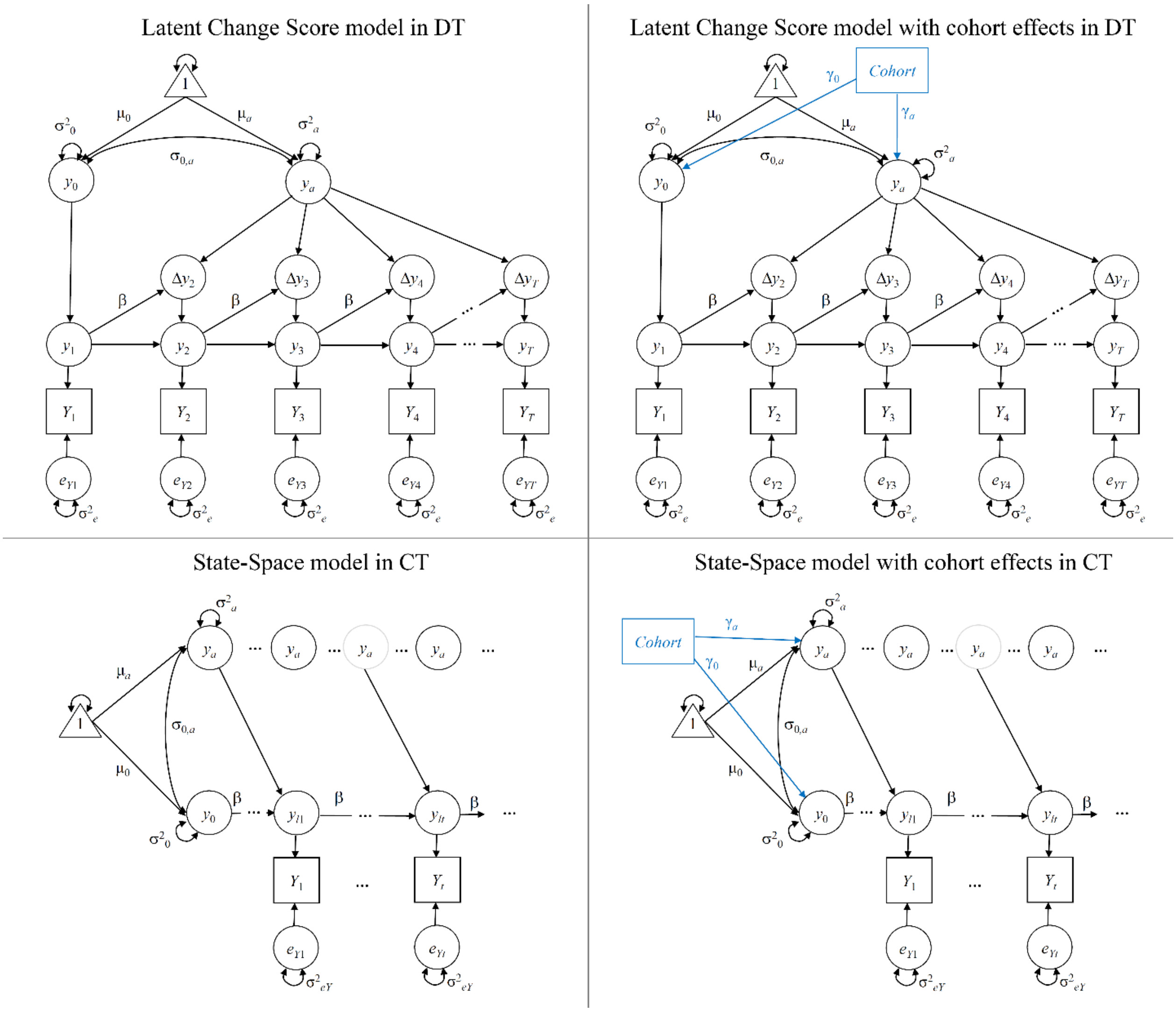


Figure 4. Root Mean Square of the Relative Bias (top) and Mean Coverage (bottom)

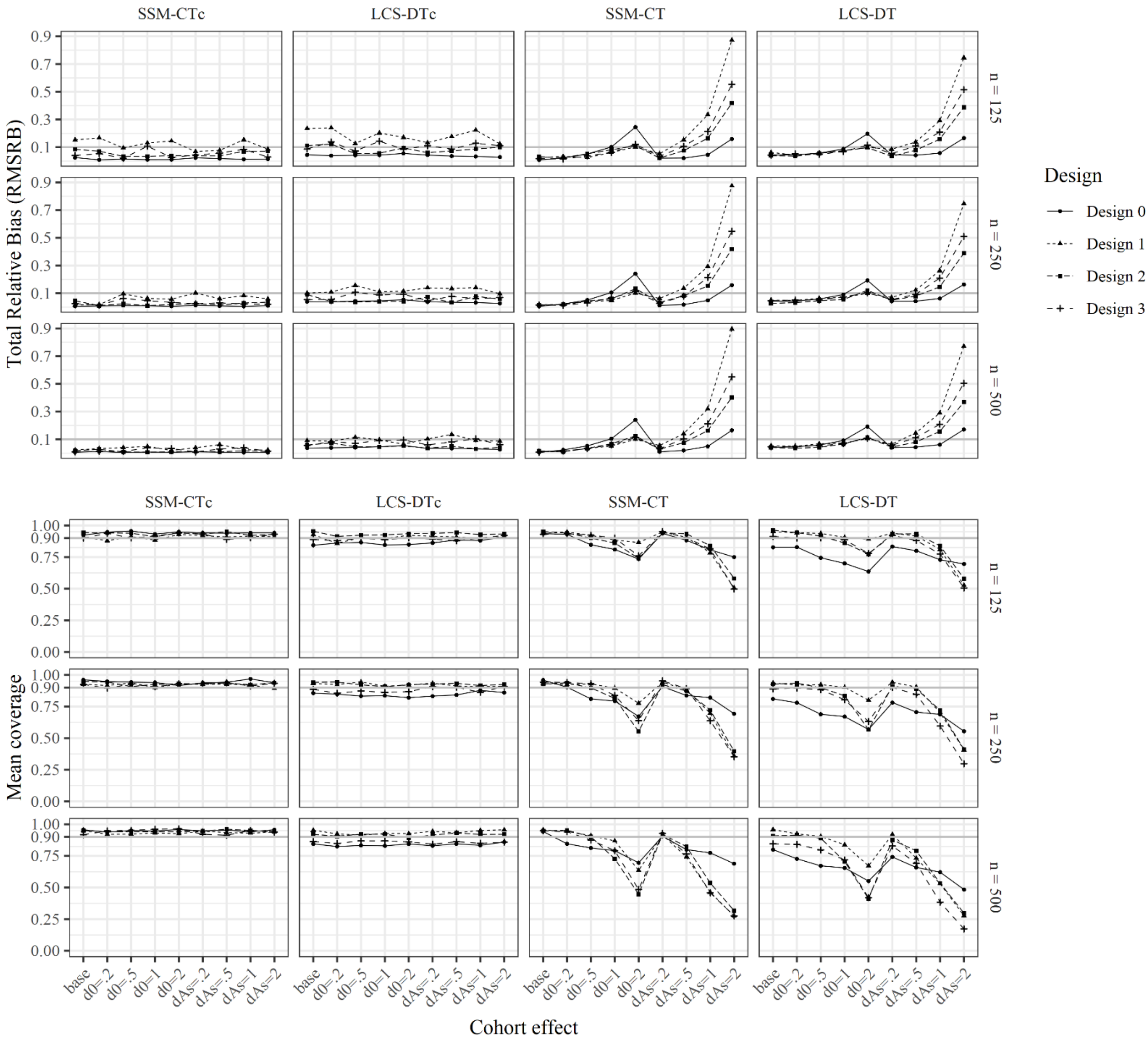


Figure 5. Design 1: Parameters' Relative Bias (top) and 95% CI Coverage (bottom) for the four models in all conditions

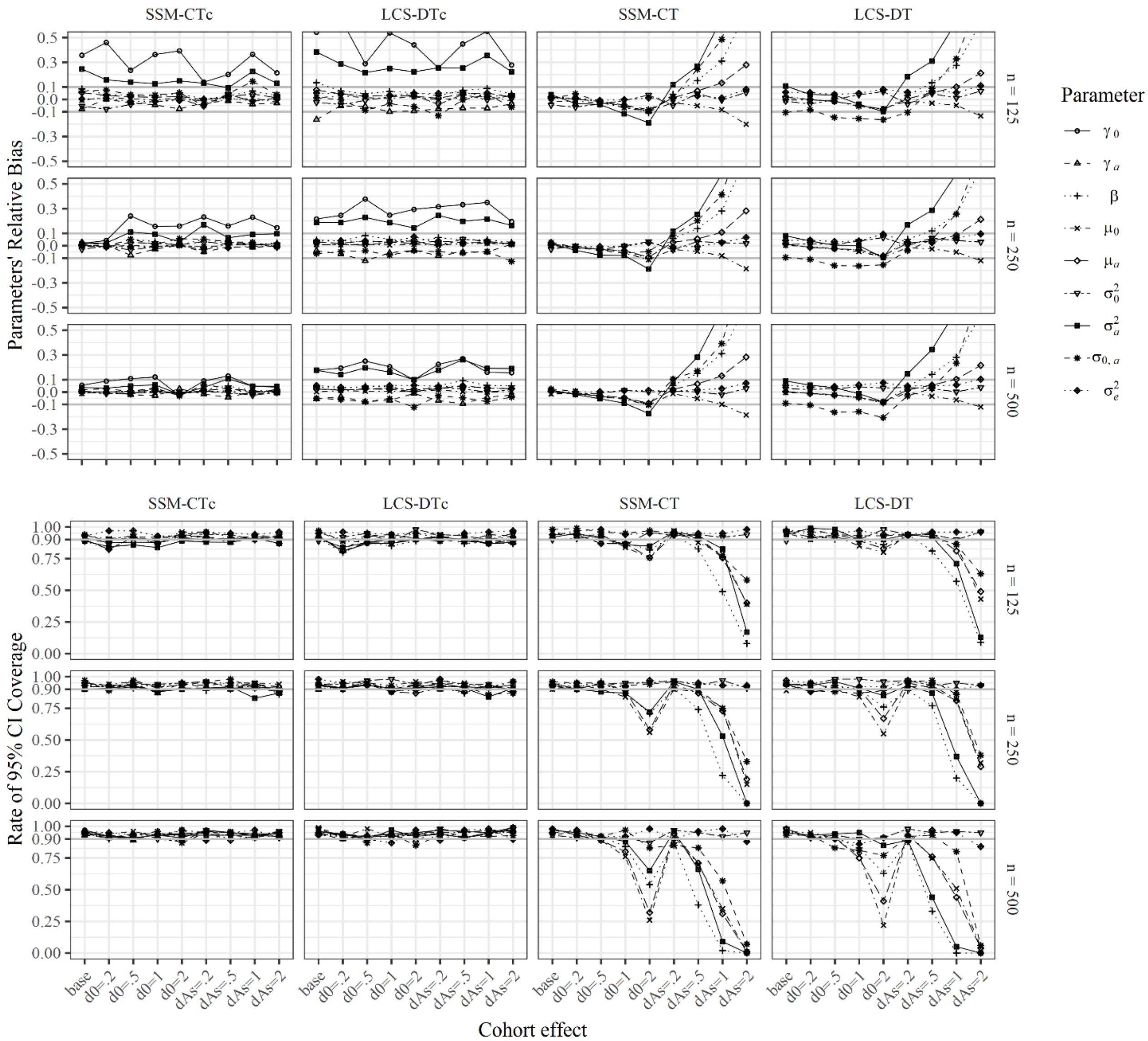


Figure 6. Design 2: Parameters' Relative Bias (top) and 95% CI Coverage (bottom) for the four models in all conditions

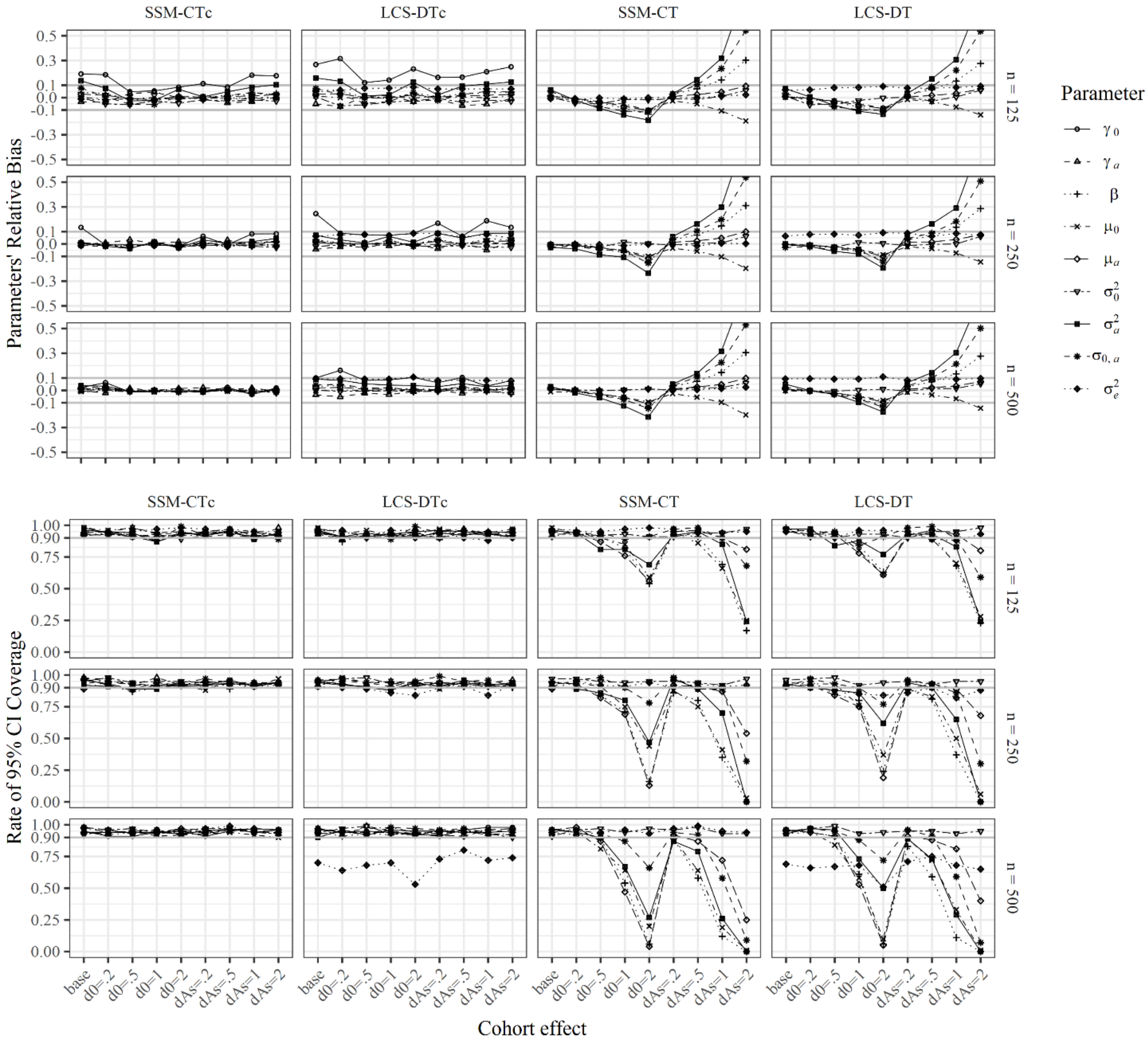


Figure 7. Mean SDRB of the four models in all conditions

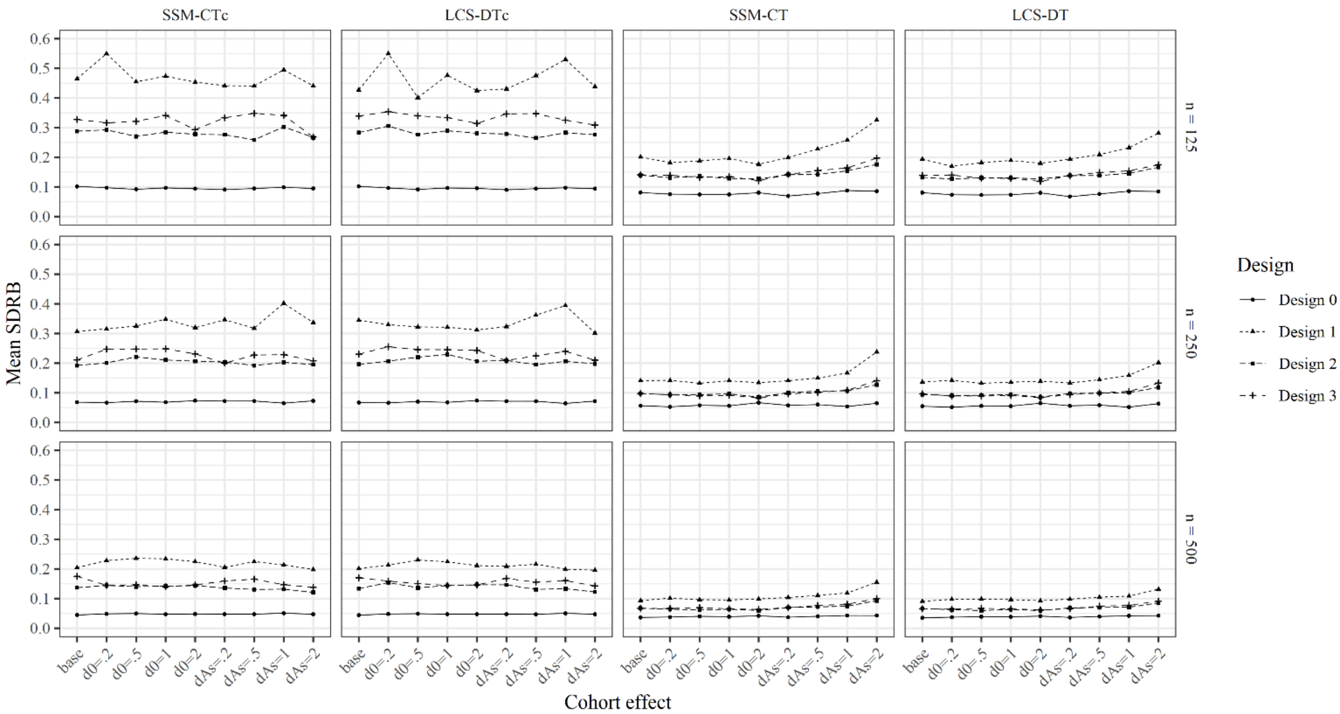
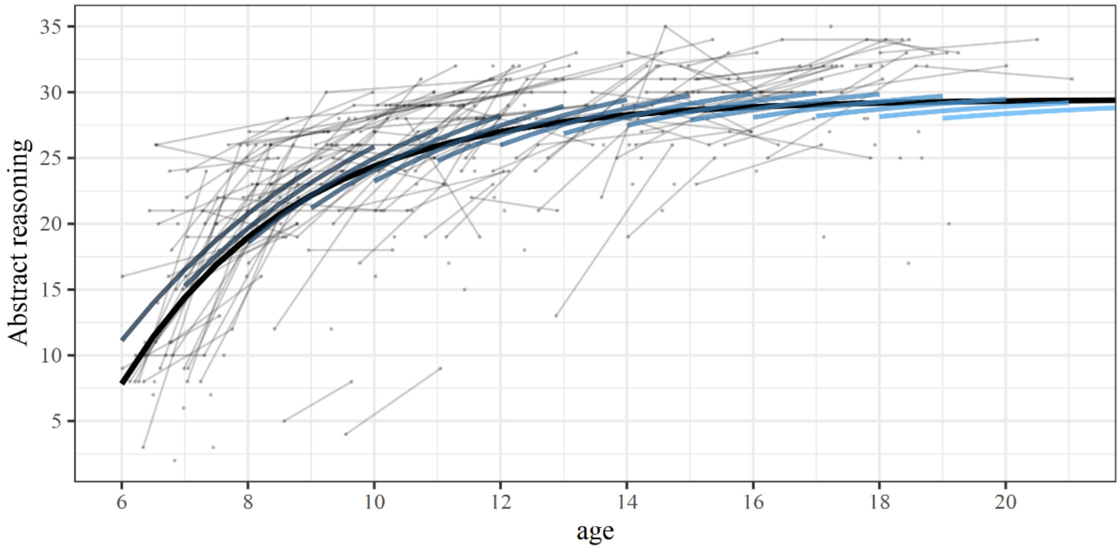


Figure 8. Developmental trajectories of abstract reasoning in an empirical sample




Note: Dots and thin lines represent observed individual data. The thick black line represent the mean trajectory implied by the SSM-CT model without cohort effects. The thick short lines represent the cohort-specific mean trajectories implied by the SSM-CT with cohort effects.