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Advances in disentangling age, cohort, and time effects: No quadrature of the circle, but a help☆

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Abstract

Based on Schaie's (1965) general developmental model, various data-driven and theory-based approaches to the exploration and disentangling of age, cohort, and time effects on human behavior have emerged. This paper presents and discusses an advancement of data-driven interpretations that stresses parsimony when interpreting the results of sequential models. Second, a synthesis of data-driven and theory-based approaches examines the specific predictors of patterns of cross-sectional, longitudinal, and time-lag differences. This approach is exemplified with data from two cross-sectional samples. In 1991 and 1996, representative samples of 13- to 29-year-old Germans were interviewed orally. Parts of these samples were analyzed employing a time-sequential and a cross-sequential strategy (analyzed $N = 6105$). While the data-driven approach allowed for two alternative interpretations, the second approach revealed that parental emotional help for their children declined with age, partly due to the children leaving home. Help provided for parents generally increased with age, however, leaving home had the opposite effect so that overall, only small and inconsistent age increases in help for parents were found.

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Almost 40 years after Schaie's (1965) pioneering paper, it seems as if the issue of age, cohort, and period effects on human behavior has been settled. After a peak of publications on sequential models during the 1970s, the debate on sequential models, their variants, and on how to interpret the obtained results has ceased. Nowadays, the general impression seems to be that sequential models are the most favorable research strategy within developmental psychology and other social and behavioral sciences (Trautner, 1992). Sequential models appear, however, expensive, extremely time-consuming, and in the end useless for disentangling age, cohort, and time effects on human behavior.

However, as will be demonstrated in this paper, none of this is entirely true. This paper will show that misunderstandings continue to exist, for example, on the interpretation of interaction effects. Most importantly, new ways of data analyses will be presented that facilitate disentangling age, cohort, and time effects on the joint bases of theory and statistics.

Most of the previous attempts to disentangle the confound of age, cohort, and time effects may be subsumed under (a) the thesis of *data-driven interpretations*, and (b) the antithesis of *theory-based data analyses and interpretations*. We will present both consecutively and will then turn to (c) our synthesis of *simultaneous theory- and data-based analyses and interpretations*. We will illustrate an advancement of the thesis (a) and of the new synthesis (c) with survey data on emotional help adolescents and young adults received from and provided for their parents. The thesis of the data-driven approach consists of the systematic inspection of cross-sectional, longitudinal, and time-lag differences obtained from sequential plans analyzed with analyses of variance, and the search for the most parsimonious interpretation of findings. The synthesis of theory-based and data-driven approaches is a two-step procedure of first hypothesizing possible causal or at least predictive factors for age, cohort, and/or time effects and then testing whether these factors can explain cross-sectional, longitudinal, and time-lag differences.

Thesis: Data-driven interpretations

Schaie's general developmental model

Table 1 shows an illustration of the basic cross-sectional, longitudinal, and time-lag designs and how they are combined into Schaie's (1965) three sequential strategies for developmental research. Cross-sectional designs aim to vary age while keeping time constant. However, with age, also the cohort has to be varied, that is, cohort is confounded with age. Instead of saying that a cross-sectional design varies age, it is more accurate to say that the *cross-sectional factor* is varied. For example, in

Table 1
Schaie's three sequential strategies

Sequential strategy	Time of investigation	Birth cohort, if... 13 years old	18 years old	23 years old	28 years old
Time-sequential strategy (TS)	1986				
bold: time-lag design	1991	**1978**	1973	1968	
	1996	**1983**	1978	1973	
	2001				
Cross-sequential strategy (XS)	1986				
bold: cross-sectional design	1991	**1978**	**1973**	**1968**	
	1996		1978	1973	1968
	2001				
Cohort-sequential strategy (CS)	1986	**1973**			
bold: longitudinal design	1991	1978	**1973**		
	1996		1978	**1973**	
	2001			1978	

Note. Each sequential strategy combines two or more time-lag (TS), cross-sectional (XS), or longitudinal designs (CS), respectively. The resulting two-dimensional arrays vary the time-lag and cross-sectional factors (TS), the cross-sectional and longitudinal factors (XS), and the longitudinal and time-lag factors (CS), respectively.

Table 1, the cross-sectional factor of a study conducted in 1991 has the steps age 13/cohort 1978, age 18/cohort 1973, and age 23/cohort 1968. The effects of this cross-sectional factor on the dependent variables will be called *cross-sectional differences* throughout this paper. This is to underscore that the cross-sectional differences may reflect age effects, cohort effects, or both.

A time-lag design aims to investigate time effects on certain dependent variables with age being constant. Consequently, the subjects at different times of investigation stem from different cohorts. For example, 13-year-olds in 1991 and 1996 were born in 1978 and 1983 (see Table 1). That is, not time, but the *time-lag factor* is varied with the steps time 1991/cohort 1978 and time 1996/cohort 1983. The effects of the time-lag factor on the dependent variables will be referred to as *time-lag differences*. Finally, longitudinal designs follow the same cohort over age, simultaneously altering the time of investigation. Thus, not age is varied, but the *longitudinal factor* (see Table 1). For example, the 1973 cohort is investigated repeatedly, so that the steps of the longitudinal factor are age 13/time 1986, age 18/time 1991, and age 23/time 1996. Effects of this factor will be called *longitudinal differences*.

Schaie's three sequential strategies each combine two of the above-mentioned factors. The *time-sequential strategy* (TS) varies the cross-sectional and the time-lag factors. Again, it would not be accurate to say that the TS would vary age and time, because both are confounded with cohort in this strategy. Thus, the two factors of this strategy are more precisely labeled as cross-sectional and time-lag factors. In the *cross-sequential strategy* (XS), the cross-sectional and longitudinal factors are varied. Finally, the *cohort-sequential strategy* (CS) varies the longitudinal and time-lag factors. The time-lag and cross-sectional factors are always between-subject factors. The longitudinal factor can be either a within- or between-subject factor. The longitudinal factor is considered a between-subject factor if independent samples are drawn at different times of measurement.

At first glance, the CS may appear especially suited for developmental research, because it can be made of several longitudinal studies. But when there are m age groups and n cohorts, this strategy needs $m+n-1$ times of measurement. In Table 1, for example, three age groups and two cohorts result in four times of measurement. On the contrary, for the TS and XS, only two times of measurement are necessary. Furthermore, the longitudinal and time-lag differences measured by the CS can also be measured by the XS, and the TS, respectively. Therefore, Schaie (1965) called the combination of XS and TS the "most efficient design" for exploring age, cohort, and time effects.

Sample of the empirical example

We will illustrate the methodological discussions with empirical data. Table 2 shows the composition of the sample. In this particular study, Schaie's (1965) "most efficient design" of TS and XS was employed. Two independent cross-sectional samples were drawn in 1991 and 1996. In general, equal intervals between times of measurements, birth cohorts, and thus ages are preferred (e.g., Schaie, 1965; Trautner, 1992; Wohlwill, 1973/1977). Given the interval of 5 years between the 1991 and 1996 assessments, this would suggest that the sample was to be divided into subsamples with an age/cohort range of 5 years each. However, the large sample size ($N=6105$) allowed splitting these subsamples in halves so that age-groups with 2- to 3-year intervals were obtained. This fits possible changes in the dependent variables better than the somewhat long 5-year intervals. Because the sample was split into 2- to 3-year intervals, participants of the same birth cohorts in 1991 and 1996 were two age intervals older in 1996 than in 1991 (see the legend of Table 2). Or, in other words, participants of the same age at both occasions differed by two birth cohort intervals.

The sample was quite representative of German youth after unification. In 1989, the former so-called German Democratic Republic (East Germany) opened its borders to the Federal Republic of Germany (West Germany). In the following, a process of unifying both German states began that resulted in an almost complete

Table 2
Composition of the illustrative sample

Sequential strategy	Time of investigation	Number of participants at ages of...						
		13/14	15–17	18/19	20–22	23/24	25–27	28/29
Time-sequential strategy	1991	*520*	*647*	*489*	*800*	*563*		
	1996	357	644	*389*	*557*	*399*		
Cross-sequential strategy	1991	*520*	*647*	*489*	*800*	*563*		
	1996			*389*	*557*	*399*	423	317

Notes. Those subsamples set in italics were part of both sequential strategies. The 13/14-year-olds in 1991 stem from the same cohorts as the 18/19-year-olds in 1996. The 15- to 17-year-olds in 1991 stem from the same cohorts as the 20- to 22-year-olds in 1996, etc.

transfer of West German economical, political, and societal structures to East Germany and a dramatic increase of unemployment in East Germany. In 1991, about 35% of the participants came from East Germany while in 1996, slightly less than half of the participants came from East Germany. Participants who had moved between both parts of Germany after unification were excluded from the sample. Slightly more than half of the participants were female.

Hypothetical results and their interpretation

Fig. 1 shows various hypothetical single and combined effects of age, cohort, and time. Fig. 1 also describes whether the TS and XS would detect significant cross-sectional, time-lag, and longitudinal differences. Fig. 1C illustrates how both TS and XS compare the data cross-sectionally along the lines of the graph. The TS also does time-lag comparisons. These can be found in the graph as vertical comparisons between data points of different times at the same ages. Finally, the XS compares longitudinally between subjects of the same cohort. As the interval between the two times of measurement was 5 years, subjects in 1996 were 5 years older in 1996 than in 1991. In the shown example of lacking longitudinal effects, these are the horizontal comparisons.

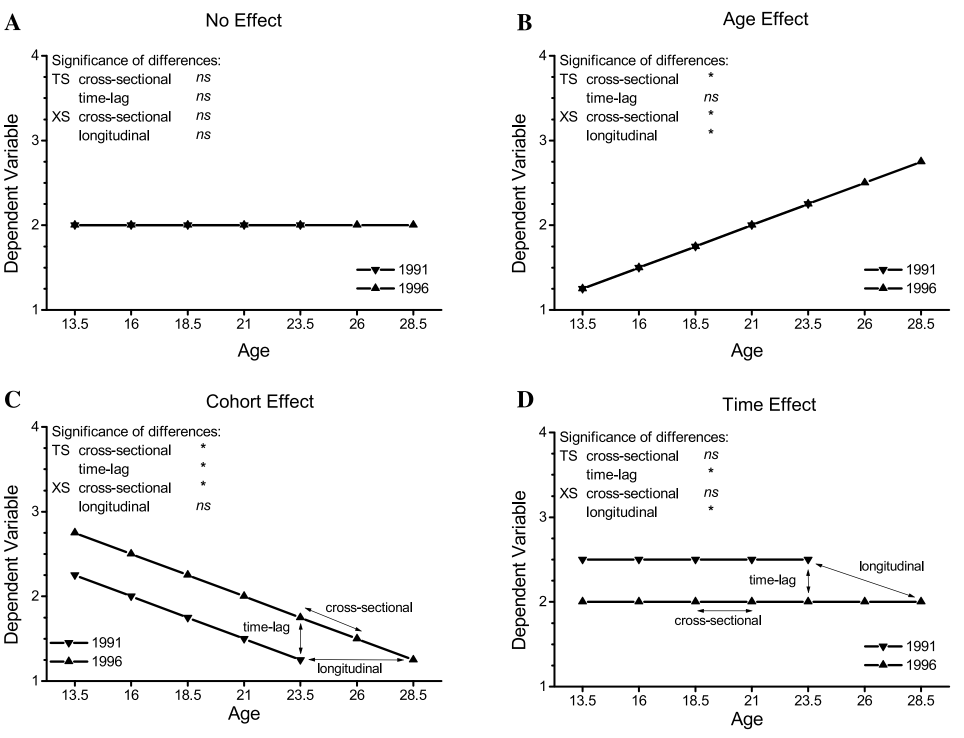

Fig. 1. (A–D) Illustration of hypothetical main effects of age, cohort, or time on a dependent variable. (E–H) Illustration of hypothetical combinations of main effects of age, cohort, or time on a dependent variable.

Fig. 1A depicts the simplest case of neither age, nor time or cohort effects. Accordingly, no significant differences can be detected by the TS and the XS. Fig. 1B is an example of a pure age effect. Both, the TS and XS detect cross-sectional differences. While the TS does not find any time-lag differences, the XS also establishes longitudinal differences. Fig. 1C shows a cohort effect. Again, both TS and XS find cross-sectional differences. This time, the TS also reveals time-lag differences because subjects of the same age, but who participate at different time-points, stem from different cohorts. The XS, on the contrary, cannot detect any other differences. The longitudinal differences are zero because the same cohorts are followed over time. The last pure effect, a time effect, is depicted in Fig. 1D. Neither the TS nor the XS shows cross-sectional differences within the two times, but both time-lag differences in the TS (same ages, but different times) and longitudinal differences in the XS (same cohorts over time) become statistically significant. In a nutshell, each of the age, cohort, and time effects results in a characteristic pattern of two out of the three possible cross-sectional, time-lag, and longitudinal differences.

What about combinations of age, cohort, and time effects? Figs. 1E–G depict the combined effects of age and cohort, age and time, and cohort and time. Figs. 1E–G can be derived from overlays of Figs. 1B and C, B and D, and C and D, respectively. Fig. 1E (age and cohort effects combined) is very similar to Fig. 1D (pure time effect).

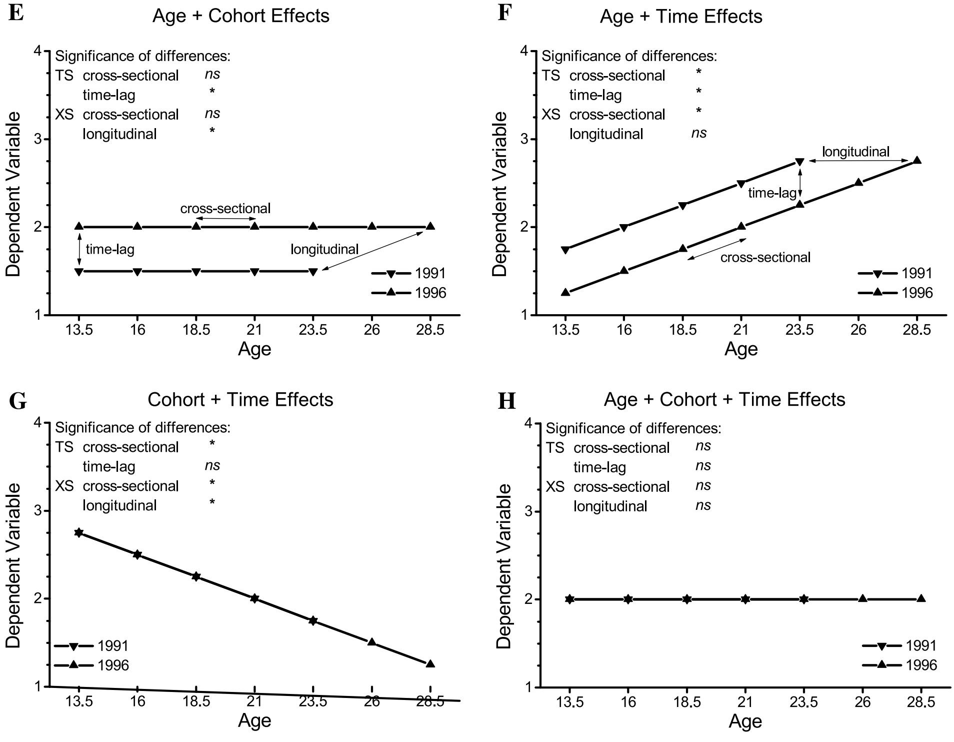

Fig. 1 (*continued*)

In fact, if the time effect was reversed so that the line for 1991 would be located below that for 1996, the result would be Fig. 1E. Likewise, Fig. 1F (combination of age and time effects) is similar to Fig. 1C (pure cohort effect). Furthermore, the cohort and time effects shown in Fig. 1G resemble the pure age effect of Fig. 1B. Accordingly, the patterns of significant findings of the TS and XS in Figs. 1E–G are equal to those of Figs. 1B–D. Finally, Fig. 1H shows a combination of age, cohort, and time effects, as can be deduced from overlaying Figs. 1B and G. The hypothetical data were chosen in a way that the combination of all effects sums to the same (zero) differences as those shown in Fig. 1A when none of the three effects of age, cohort, and time are present.[1]

Fig. 1 leads to several conclusions: (a) The same pattern of results can be due to different combined effects of age, cohort, and time. For the infinite number of possible combinations of linear age and cohort effects, a time effect can be found that nullifies the combined effect. Adding these combinations of age, cohort, and time effects to all parts of Fig. 1 does not change anything. It can be concluded that each empirical result can be due to an unlimited number of combinations of age, cohort, and time effects. (b) Therefore, from any empirical result, no unequivocal conclusions about age, cohort, and time effects can be drawn. (c) Figs. 1B and C show that the third confounded effect (age is confounded with cohort and time in the XS, cohort is confounded with age and time in the TS) does not cause interaction effects between cross-sectional and time-lag, or cross-sectional and longitudinal differences, respectively. As age, cohort, and time depend linearly on each other, there is no reason why the third confounded effect should cause (multiplicative) interaction effects of the other two. However, Schaie (1965) stated that the third confounded effect would do so. Although Adam (1978) has argued that the assumption of an interaction effect was due to a sign error in Schaie's (1965) original publication, and others have also tried to clarify this issue (Dowd, 1980; Lewis-Beck & Glenn, 1977; see also Baltes, 1968, pp. 159f.), the wrong notion of interaction effects caused by the third, confounded effect has been reiterated (Schaie, 1970, 1994; Trautner, 1992; Wohlwill, 1973/1977).

Schaie (1965, 1970) has repeatedly attempted to do the "quadrature of the circle" and to disentangle age, cohort, and time effects on the basis of empirical data. Both, his set of six "decision rules" (Schaie, 1965), and his method for comparing mean cross-sectional and longitudinal age-gradients (Schaie, 1970) were proven to be wrong (Adam, 1978). Finally, Schaie (1970, p. 489), developed the idea to "let the confound [between two of the factors of age, cohort, and time and the third confounded factor in each sequential strategy] become asymptotic." However, Schaie did not elaborate this method further, and to us it seems logically impossible that many confounded data can ever be less confounded than only a few. Thus, as shown in

[1] Figs. 1A–H show only possible examples of the effects. Other data would lead to different means (Figs. 1A–H), different slopes (Figs. 1B, C, and E–H), and different time-lag differences between the two lines (Figs. 1C–H). This would also alter the results of the statistical analyses. For example, the combination of age, cohort, and time effects does not necessarily lead to non-significant results of all analyses. Figs. 1A–H show the minimal number of statistical effects, given the respective age, cohort, and time influences.

Fig. 1, it is and will be impossible to find the sole "true" interpretation of data in terms of age, cohort, or time effects.

More constructive use of data-driven approaches

So, is it correct to state that sequential strategies cannot help in establishing age, cohort, and time effects? The answer has to be yes as long as one tries to find the one and only "truth" in the data. But this in fact reflects a naive empiricist view of the nineteenth century (Holzkamp, 1972). The most recent view on truth derivations argues that knowledge always stems from selective observations and cognitive reconstructions of them (Scarr, 1985). Thus, each empirical finding allows several interpretations. Hence, the apparent problem not to know how to interpret cross-sectional, time-lag, and longitudinal differences, is less unique than first appeared, but it is the general problem in empirical science, that findings are open to various interpretations. This, by the way, is mirrored in contemporary statistical techniques such as latent variable modeling (Borsboom, Mellenbergh, & van Heerden, 2003; MacCallum & Austin, 2000). For example, exploratory factor analyses allow for various decisions on the number of factors, orthogonal or oblique rotations, etc., each resulting in different findings that all are equally "true." (The analogy to factor analysis will be picked up in the next section.) Statistical tests generally test whether effects are significantly different from zero. If not, in most instances it is assumed that these effects do not exist (although the rejection of the alternative hypothesis does not prove the null hypothesis). This is because of the preference of parsimony: It is preferred to keep as few causal factors in the explanation as possible. Hence, structural equation models are even accepted if they are significantly different from the data, as long as the various goodness-of-fit indices (that in part explicitly take parsimony into account) appear appropriate.

Therefore, the question is not, "What is right?," but: "What is the optimal interpretation?" If there is no theory to derive testable hypotheses from, then the study is completely exploratory and the interpretation has to be solely data-driven. In this case, we recommend the most parsimonious explanation. For example, in a case as depicted in Figs. 1B and G, one would prefer the interpretation of the results as an age effect over the less parsimonious one of cohort plus time effects.

But what "is" an age effect? As Trautner (1992) pointed out, "age" is only a placeholder/proxy for the actual occurring influences like maturation. The same applies to cohort and time. If one leaves the meanings of age, cohort, and time completely open, there is no need to distinguish between the three, because, for example, environmental changes can be described both in terms of cohort and time effects. As time can be calculated from cohort and age (Baltes, 1968), Schaie and Baltes (1975) agreed that for descriptive purposes, only age and cohort effects need to be distinguished. For explanative purposes, however, they considered the distinction between age, cohort, and time as potentially useful. We share this view. Labels like "age," "cohort," and "time" lead a researcher to a specific association as well as the consideration of a specific explanation. For example, if one reveals an age effect, one will think of possible influences that have to do with individual development. If one describes the very same finding as cohort plus time effects, one will think of societal changes as causes. Our recommendation to chose the most parsimonious explanation thus needs an addendum: Parsimony does not only mean to prefer one main effect over two or three effects (e.g., age instead of cohort plus time). Parsimony also means that only a small number of actual causes "behind" the age, cohort, and time effects need to be assumed, and that these assumptions also fit previous empirical findings.

Empirical example: Emotional help between parents and adolescents

Aim

The study aimed to explore age, cohort, and time effects on emotional help adolescents and young adults received from, and provided for, their parents. This study serves as an example for a data-driven interpretation and theoretical considerations will be mentioned later. Because cohort and time effects may differ between the two parts of Germany, political region was included as an additional factor in the analyses. The measurement of emotional help is somewhat less than optimal because the research question was imposed on existing data.

Method

The participants were interviewed orally. Among a variety of other topics, the participants were asked: "How often did you do this in the past 12 months for your parents?" and "Which of these activities have your parents done for you?" Each question was followed by a list of 12 activities (seven in the 1996 survey). Most of these activities were household tasks such as dishwashing or cleaning up the room. Others were caring for children (siblings or grandchildren) or giving somebody a lift with the car. Among these activities, there were also two kinds of emotional help, "suggesting to begin something new" and "helping at problems by advice and deeds." The answer possibilities ranged from *never* (1) to *regularly* (4).

For each elicited emotional help, analyses of covariance (ANCOVAs) were computed with the cross-sectional factor, longitudinal (XS) or time-lag factor (TS), and political region (i.e., East or West Germany) as factors of interest, and gender and parental divorce (yes/no) as control factors. Each parent's level of education and size of community were introduced as covariates in order to control for possible sampling differences. The significance level of 5% was divided by the number of F tests for each ANCOVA in order to correct for chance capitalization. The results are only presented insofar as they are relevant for this methodological paper. The full results of this section have been published elsewhere (Masche, 1999).

Results

Help received from parents. For parental suggestions to begin something new, the time-sequential analysis (TS) revealed cross-sectional differences ($F(4, 5058) = 48.97$, adjusted $p < .05$), but no time-lag differences ($F(1, 5058) = .63$, *ns*). The cross-sequential analysis (XS) revealed significant cross-sectional ($F(4, 4810) = 33.00$, adjusted $p < .05$) and quasi-longitudinal differences ($F(1, 4810) = 49.47$, adjusted $p < .05$), and a significant interaction effect ($F(4, 4810) = 5.13$, adjusted $p < .05$). The left part of

Fig. 2 shows these results. Both cross-sectional gradients for parental suggestions show a decline. The gradient for 1996 is, however, less steep than the one for 1991, which is indicated in the significant cross-sectional by longitudinal interaction. Except this interaction, the pattern of results resembles that of Figs. 1B and G. These results suggest either an age decline or a cohort increase (later cohorts receiving more help) plus a time decline. The more parsimonious interpretation is that of an age decline. The interaction reflects that the age decline was somewhat more pronounced in 1991 than in 1996.

For advice and deeds, the results are even more clear-cut. They consist of the same main effects and no significant interaction. More specifically, the TS showed significant cross-sectional ($F(4, 5070) = 27.21$, adjusted $p < .05$) and insignificant time-lag differences ($F(1, 5070) = 4.62$, *ns*) while the XS led to significant cross-sectional ($F(4, 4823) = 12.85$, adjusted $p < .05$) and longitudinal ($F(1, 4823) = 60.04$, adjusted $p < .05$) differences. Thus, the overall conclusion is that the results for both kinds of parental emotional help can most parsimoniously be explained by age declines.

Help provided for parents. For suggestions to begin something new, no difference reached significance. That is, in the TS, cross-sectional ($F(4, 5061) = 1.47$, *ns*) and time-lag differences ($F(1, 5061) = 8.91$, *ns*), and in the XS, cross-sectional ($F(4, 4815) = .39$, *ns*) and longitudinal differences ($F(1, 4815) = 2.18$, *ns*) were not

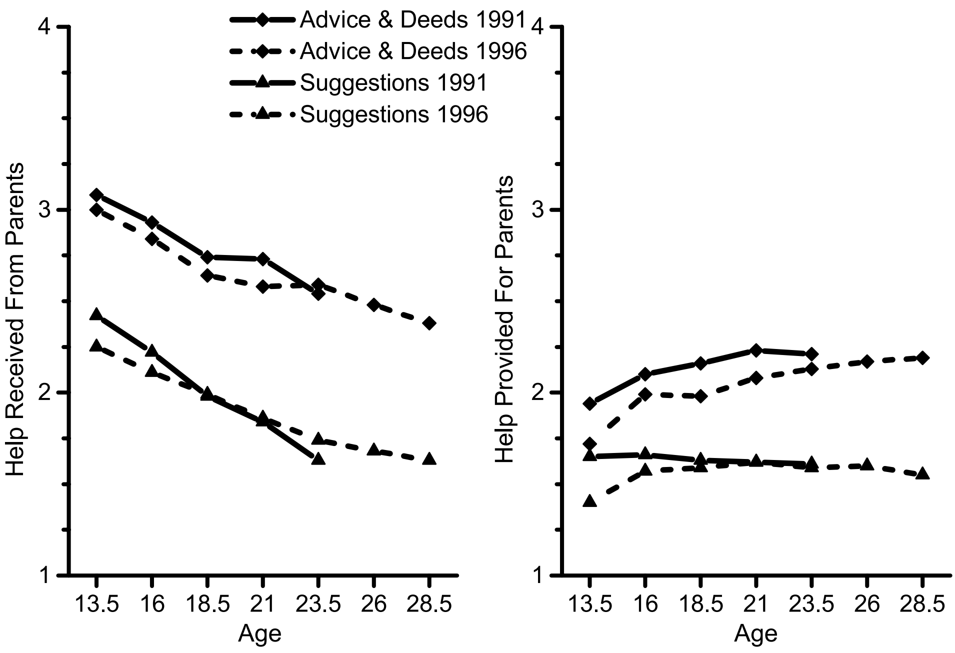

Fig. 2. Cross-sectional age gradients for emotional help as received from parents (left) and provided for parents (right).

significant. The right part of Fig. 2 shows that the cross-sectional age gradients for suggestions are more or less horizontal. These results are most easily explained as a lack of any age, cohort, or time effects.

Finally, for advice and deeds, the TS revealed both significant cross-sectional ($F(4, 5056) = 15.57$, adjusted $p < .05$) and time-lag differences ($F(1, 5056) = 25.46$, adjusted $p < .05$). The XS revealed significant cross-sectional ($F(4, 4809) = 8.17$, adjusted $p < .05$) but no longitudinal differences ($F(1, 4809) = .22$, *ns*). This is equivalent to the situation in Figs. 1C and F. This suggests that there is either a cohort decline (later cohorts providing less help) or an age increase in combination with a time decline. In addition, only in the TS, East German participants provided more advice and deeds to their parents than West Germans ($F(1, 5056) = 10.25$, adjusted $p < .05$). The XS did not reveal this East-West difference. As the TS was partly based on more recent cohorts than the XS, one may interpret this finding as an implicit political region by cohort interaction: The later-born adolescents in the East provided more advice to their parents than those in the West.

Discussion

The most parsimonious interpretation of these findings was an age decline in parental help (that was more prominent in 1991 than in 1996 for one of the two items), no changes in suggestions that adolescents and young adults made to their parents, and a cohort decline in advice and deeds given to parents. Later-born cohorts appeared to help their parents more in East than in West Germany. Less parsimonious interpretations would assume a cohort increase and a time decline for parental help, and for one of the two sorts of help provided for the parents an age increase and a time decline. So, what is the most parsimonious interpretation of all results together? One could assume that the same factors may affect the help provided for parents and received from them. As a sociologist, one may first try a social change interpretation. As the German unification occurred before the first measurement, unification may cause cohort effects. If one thus assumes cohort effects as the thread of the results, it is necessary to explain the additional time effects for the parental help. On the other hand, a developmental psychologist may assume that adolescents become more independent with age (Noom, 1999) and that the parent–adolescent relationship becomes less hierarchical and more peer-like (Youniss & Smollar, 1985). If age effects are the thread of interpretations, the time decline in advice for the parents needs further explanation. The interaction effect between cohort and age may be somewhat more easily explained by the sociological perspective that favors the cohort interpretation. In summary, these findings allow two alternative interpretations and, from data alone, it is hard to decide which one to prefer. Empirical results from other studies, however, allow for decisions based on parsimony.

We have demonstrated how the combination of cross-sequential and time-sequential strategies can be used effectively to explore possible age, cohort, and time effects. This one is the most efficient combination of any two sequential strategies, as it needs only two times of measurement.

Antithesis: Theory-based data analyses and interpretations

There have been various approaches to circumvent the confound of age, cohort, and time effects and thus ambiguities of interpretation. Probably the first of these attempts stemmed from Baltes (1968; Baltes & Nesselroade, 1972). He rejected the idea of analyzing three independent variables if one of them is totally determined by the other two. Thus, he restricted himself to analyses of age and cohort effects. However, as mentioned above, this restriction to two factors was meant to be useful for descriptive purposes only and not as an explanation of behavior (Baltes, 1968, pp. 157, 162; Schaie & Baltes, 1975).

Baltes' critique raises the question of the *meaning* of three effects if one is completely dependent on the other two. The meaning of these effects refers to an underlying theory of development. Schaie (1965, 1970) assumed age effects to be due to biological maturation, cohort effects to be caused by differences in the genetic structure or by different experiences prior to the first data collection, and time effects reflecting different environmental impacts at different times of measurement. On the basis of these notions, Schaie (1970, p. 494), hypothesized the development of crystallized intelligence as independent (!) of age. This was because Schaie assumed accumulating input from the environment as central to the development of crystallized intelligence—and this environmental effect belongs to time effects according to Schaie's view. Obviously, this is a very restricted understanding of development. As early as 1911, Stern assumed that inner developmental tendencies needed environmental influences in order to develop. In other words, Stern (1911) would count at least several of the environmental influences as age effects while Schaie (1970) regarded these as time effects.

From this, it becomes clear, how differently the "empty" (see Trautner, 1992) age, cohort, and time variables can be interpreted. Buss (1973) used this fact for eliminating time effects. He widened the interpretation of cohort effects so that they encompassed also cultural changes during lifetime between measurement occasions. Time effects were redefined as reflecting measurement or sampling errors only. If these are avoided or controlled, a two-dimensional age by cohort matrix is sufficient for the analysis of human development. Similarly but less radically, Jackson and Antonucci (1994) defined age as indexing factors related to organismic growth and change, cohort as indexing temporal differences in institutions and environmental influences, and time as indexing sociobiocultural influences. As these sociobiocultural influences in Jackson's and Antonucci's view affect age and cohort differences as well, time appears less interesting to investigate, again resulting in the analysis of an age by cohort matrix.

Labouvie and Nesselroade (1985; Labouvie, 1985) further developed the notion that the same factors cause age, cohort, and time effects. More explicitly than earlier authors, they drew attention to the fact that the temporal characteristics of an environmental antecedent and a behavioral outcome may differ. Because of this, "the same antecedent may generate (1) intraindividual change within a given cohort, (2) interindividual differences in such a change within a cohort, and (3) differences in change between cohorts" (Labouvie, 1985, p. 42). This implies that the same antecedent may cause age, cohort, and time effects. Furthermore, the temporal characteristics of both antecedents and outcomes can be expressed in terms of any two of age, cohort, and time. Like possible factor solutions in factor analysis, any representation is equally appropriate. But according to the theoretical orientation, one representation is preferred over other representations (Labouvie & Nesselroade, 1985). The authors plead for testing the effects of specific antecedents and mediators. Based on the antecedents and mediators under study, an appropriate representation in terms of age, cohort, or time should be chosen. Thus, while Schaie (1965, 1994) tried to set one of the three effects equal to zero on statistical grounds, Labouvie and Nesselroade (1985) suggest to do so on a theoretical basis.

Labouvie's and Nesselroade's plea for adapting the design to the specific antecedents and mediators under consideration, was developed further by Schaie (1986) and, for example, implemented by Menard (1992). Unlike the authors mentioned in the preceding paragraphs, they maintained the three-factorial model of age, cohort, and time. But they tried to replace one or more of the three effects by specific variables accounting for the effect. For example, in his investigation of illegal behavior in youth, Menard (1992) replaced cohort size for cohort and delinquent bonding and conventional bonding for age. Schaie (1986) provided an overview on how to replace age, cohort, and time with specific factors. Age could, for example, be replaced with non-calendar functional age. Cohort cannot only mean year of birth, but any population entering a specific environment at the same time. Apart from year of birth which is confounded with age and time, cohorts can be defined, for example, as the population of females having experienced menarche at the same time, or as the population of people having been married, or as those that have become unemployed or become infected by a specific disease at the same time. These examples illustrate biological or societal age-graded cohorts, history-graded cohorts, and non-normative cohorts. Dependent on the definition, the cohorts will be more or less independent from age and time. Concerning time, Schaie (1986) suggested first to consider which technological or attitudinal changes, changes in personal habits, or in knowledge diffused over the population may affect the behavior under investigation. The temporal characteristics of the environmental impact should also be taken into account according to Schaie. He suggests calculating an index of event density. The index of event density is the number of changes within a domain considered relevant for behavior. Calendar time should then be re-scaled so that event density is constant over "time." In other words, phases with many events are enlarged while others are reduced, thus dissolving the confound of time with age and cohort.

All these suggestions have in common that they draw attention to the fact that neither age (see Trautner, 1992) nor cohort or time are explanations of behavior. Instead, they are indexes for the causal factors. Thus, if one wants to turn from description to explanation of data, it is necessary to find factors that may be responsible for the age, cohort, or time effects found. However, all these approaches of theory-driven data analysis and interpretation are based on additional untestable assumptions. For example, as Labouvie and Nesselroade (1985) wrote, choosing a certain way of data description directs the interpretation in a specific direction. More importantly, these approaches make false exclusion errors (Kuhn, 2002). For example, by replacing cohort with number of births in a certain year (Menard, 1992), all other cohort influences are excluded and set to zero. If there are further causal factors related to cohort, these are falsely interpreted as age- and time-related, resulting in different causal interpretations.

Synthesis: Simultaneous theory and data-based analyses and interpretations

Rationale

So far, we have argued that data-driven approaches cannot provide any "truth" concerning age, cohort, and time effects, but if one restricts oneself to finding a parsimonious interpretation of data, a combination of time-sequential and cross-sequential strategies may work well, but does not always do so. Besides the "bottom-up" data-driven approaches, several authors have suggested "top-down" theory-based approaches. The common idea of these approaches is to eliminate one of the three confounded factors of age, cohort, and time by choosing specific causes that may account for one of them. However, the false exclusion of other causes linked to the excluded factor (e.g., cohort) may lead to wrong interpretations of resulting effects of the non-excluded factors (e.g., age and time). Thus, instead of excluding one factor a priori, the basic idea of the suggested synthesis of data- and theory-driven approaches is to test statistically whether any of the three can be replaced by specific antecedents or mediators while keeping all age, cohort, and time effects under investigation. This approach is data-driven insofar as the empirical results "decide" whether age, cohort, or time effects are revealed. It is also theory-based as possible antecedents or mediators are chosen on a theoretical basis.

More specifically, our suggested approach is the following: (a) Investigate a cross-sequential and a time-sequential sample (or any other combination of two sequential samples). (b) In a hierarchical regression analysis for each sequential sample, enter the respective cross-sectional, longitudinal, and time-lag factors first into the equations. (c) Enter one or more additional variables consecutively that you suppose to be responsible for the age, cohort, or time effects you want to explain. It is not necessary to explain all three, if you do not have any theoretical assumptions on their origin. (d) See whether the β weights for cross-sectional, longitudinal, and time-lag factors change significantly when each additional predictor is entered into the regression equations. Interpret changes similarly to the suggested way of a data-driven interpretation in the first part of the current paper. If, for example, the β of the cross-sectional factor in XS and TS as well as the β of the longitudinal factor in the XS is significantly reduced, but the time-lag β in the TS remains unaffected by a specific predictor, the conclusion would be that this predictor accounted for age effects. If before entering the predictor into the equations, the respective β weights were significant, the conclusion would be twofold: First, age effects were confirmed, and second, these age effects can be (partly or totally) explained by the predictor tested. In the same way, conclusions on cohort and time effects can be drawn. The following research example will show that even more conclusions can be delineated by this method.[2]

Continuation of the empirical example: Hierarchical regression analyses

Theory and methods

First, the general approach just outlined needed some minor additions: As differential cohort and time effects depending on the political region were likely, not only the cross-sectional factor and the longitudinal (XS) or the time-lag factor (TS) were entered into the equations, but also the political region (East vs West Germany) was entered in the first block of the hierarchical regression analyses. In the second block, the control variables gender, parental divorce, education of each parent, and size of community were entered. These variables were included to control for sampling differences.

Predictors of interest were entered into the regression equations at the third and consequent steps. Three predictors were aimed to explain age effects, and one predictor was considered potentially useful to explain cohort effects. The three predictors hypothesized to explain age effects covered various aspects of increasing autonomy. The first predictor was an index whether or not the respondents had already left the parental home. In earlier studies, leaving home (which is obviously age-related) was followed by somewhat more relaxed, harmonious, and possibly more mutual and egalitarian parent–child relationships (Papastefanou, 2000). It thus seems likely that after their children have left home, parents will provide less emotional help for them than before, while the children may provide more help, reflecting the increased mutuality between them and their parents.

The second predictor indexed whether or not the respondents had had a romantic relationship. Establishing intimate relationships outside the family of origin may limit the parents' opportunities to meet their children's emotional needs. The children may on the one hand concentrate on their romantic relationship and withdraw from help to their parents. On the other hand, the experience of this new type of relationship to a romantic partner may allow the adolescents and young adults to apply their newly acquired social skills to the parent–child relationship, leading to an increase of emotional help. Finally, the extent of parental knowledge (frequently called "parental monitoring") was chosen as a possible predictor. Parental knowledge is the degree to that the parents know of the whereabouts, thoughts, and

[2] This strategy of analysis is similar to basic tests of mediation models after Baron and Kenny (1986). However, Baron and Kenny's independent variable is replaced by the longitudinal, cross-sectional, and time-lag factors. And their mediator is replaced by the hypothesized predictors. Because of these replacements, Baron and Kenny's original interpretation cannot be applied here. Baron and Kenny's approach aims for establishing that the independent variable influences the dependent variable indirectly via the mediator. Here, the interpretation is that so far undetermined age (or cohort, or time) effects can be explained by specific predictors. Because of this change in interpretation, Baron and Kenny's procedure is altered in two details. First, the Sobel significance test of the indirect path from cause via mediator to outcome appears obsolete as an indirect path from, for example, age via an actual cause to an outcome does not make sense. Second, also the bivariate correlations between predictors ("mediators") and outcomes will be calculated. This is important if suppressor effects occur, as it will be illustrated in the example.

feelings of their children. In the present study, the participants were asked how frequently they told their parents where they spent their spare time and what they were occupied with. Parental knowledge is known to decrease with age (Masche, 1998; Masche & Senz, 2001), limiting the parents' possibilities to provide emotional help to their children.

The variable chosen as a potential predictor for cohort effects was the degree of parental transfer of cultural capital. How much parents care for their children's success at school, read or make music together with them may reflect societal changes in the importance of formal education, parental beliefs in their tasks in upbringing their children, etc. As in the former German Democratic Republic, public institutions took over much more of the educational tasks from the parents than in the Federal Republic of Germany and nowadays in the united Germany, cohort differences appear likely, and they may also be related to other forms of help provided by the parents. This, in turn, may also be related to the extent of help in the opposite direction.

Results and interpretation

In order to establish that a predictor explains an age effect on an outcome variable, it is necessary to show that the predictor is related to both age and the outcome. The relation to age implies that it is correlated with the cross-sectional factor in the TS and the XS, with the longitudinal factor in the XS, but not with the time-lag factor in the TS. Furthermore, the β weights of cross-sectional and longitudinal factors predicting the outcome have to be reduced after the hypothesized predictor has been entered into the regression equations. In the same way, it can be established whether predictors explain cohort or time effects.

The two indexes for leaving home and for having had a romantic relationship were clearly correlated to the cross-sectional (partial correlations controlling for gender, parental divorce, parental education, and size of community: $.34 \leqslant pr \leqslant .56$) and longitudinal factors ($.17 \leqslant pr \leqslant .31$, all ps $< .001$). They were also correlated to parental help ($|-.14| \leqslant pr \leqslant |-.25|$, $p < .001$). Having had a romantic relationship was also related to a somewhat greater extent of advice and deeds provided for parents ($.05 \leqslant pr \leqslant .08, p < .001$). Thus, the two developmental transitions of leaving home and of beginning a romantic relationship are candidates for explaining age effects on parental help and to a lesser degree partly to emotional help for the parents. The extent of cultural capital transferred from parents to their children was weakly correlated to the cross-sectional and time-lag factors ($.05 \leqslant |pr| \leqslant .09$, $p \leqslant 001$). It showed relationships with parental help ($.12 \leqslant pr \leqslant .26, p < .001$) and less close correlations with help provided for parents ($.04 \leqslant pr \leqslant .14$, $p < .01$). Thus, the transfer of cultural capital is a candidate for explaining cohort effects on help between parents and children. Parental knowledge was correlated with all kinds of help between parents and children ($.11 \leqslant pr \leqslant .43$, $p < .001$), but it showed at best very weak relationships to the cross-sectional factor ($.04 \leqslant |pr| \leqslant .08$, $p < .01$), the longitudinal factor ($|pr| = .02$, *ns*), and the time-lag factor ($pr = .03$, $p < .05$). Thus, parental knowledge proved to be an important predictor of emotional help between parents and their children, but it cannot explain age, cohort, or time effects.

Hierarchical regressions predicting the extent of emotional help were calculated next, as explained above. It is not so important whether the various predictors entered consecutively into the equations, each led to significantly more explained variance (actually, almost all predictors did so at the .001 significance level). What is of higher interest here, is whether the β weights of cross-sectional, longitudinal, and time-lag factors changed significantly after introducing additional predictors into the equations. It turned out that only the index for leaving home altered the β weights for cross-sectional and longitudinal effects. To begin with the help parents provided for their children, in the TS, the β weight for the cross-sectional factor influencing suggestions to begin something new changed from −.27 to−.20 ($z_{\text{change}} = -3.45, p < .01$) when the index for leaving home was entered. Likewise, in the XS, the βs for the cross-sectional and longitudinal factors changed from −.24 to −.16 ($z_{\text{change}} = -3.73$, $p < .01$) and from −.13 to −.08 ($z_{\text{change}} = -2.46, p < .05$), resp. Thus, exactly those βs that indicated an age effect (compare the pattern of significant effects in Fig. 1B) were reduced when leaving home was entered into the equations. The same applies to parental advice and deeds. The cross-sectional β was reduced from −.20 to −.12 ($z_{\text{change}} = -4.21, p < .01$) in the TS and from −.18 to −.09 in the XS ($z_{\text{change}} = -4.23$, $p < .01$). The longitudinal β was reduced from −.13 to −.07 ($z_{\text{change}} = -2.89, p < .01$). Thus, leaving home explained a substantial part of the age declines in both sorts of parental help to their children.

Also the βs for age effects on emotional help provided for the parents by the adolescents and young adults, changed when leaving home was entered into the regression equations. Unlike the preceding calculations, however, the βs did not reduce but increased. The β for the cross-sectional factor in advice and deeds to parents increased from .11 to .18 ($z_{\text{change}} = -3.43$, $p < .01$) in the TS and from .09 to .16 ($z_{\text{change}} = -3.34, p < .01$) in the XS. The β for the longitudinal factor increased from .01 to .05 ($z_{\text{change}} = -2.15, p < .05$). Concerning suggestions to parents to begin something new, the β changes partly failed to reach significance, but showed the same direction: The β for the cross-sectional factor increased from .03 to .07 ($z_{\text{change}} = -1.86, p < .10$) in the TS and from .01 to .05 ($z_{\text{change}} = -2.01, p < .05$) in the XS, and for the longitudinal factor in the XS from −.02 to .01 ($z_{\text{change}} = -1.38$, *ns*).

What does this mean? Remember that leaving home was closely correlated with the cross-sectional and longitudinal factors but not with the extent of help for the parents. Apparently, the increase in β weights is a typical example of suppressor effects. The index for leaving home shares irrelevant variance with the cross-sectional and longitudinal factors, which is suppressed in the hierarchical regression analysis. The increased βs reflect the relationship of age, except those aspects linked to leaving home, with help for parents. That is, those facets of age that have nothing to do with leaving home, predict an increase in emotional help for parents. This is illustrated in Fig. 3 for advice and deeds where the effects were more clear-cut. Leaving home implies lower levels of help for parents but both groups, those who were still living with their parents as well as those who had left home, showed an age increase in help for their parents. Before distinguishing these two groups, the age increase was less prominent (see the right part of Fig. 2) because with age, more and more children leave home, reducing the extent of help to the parents.

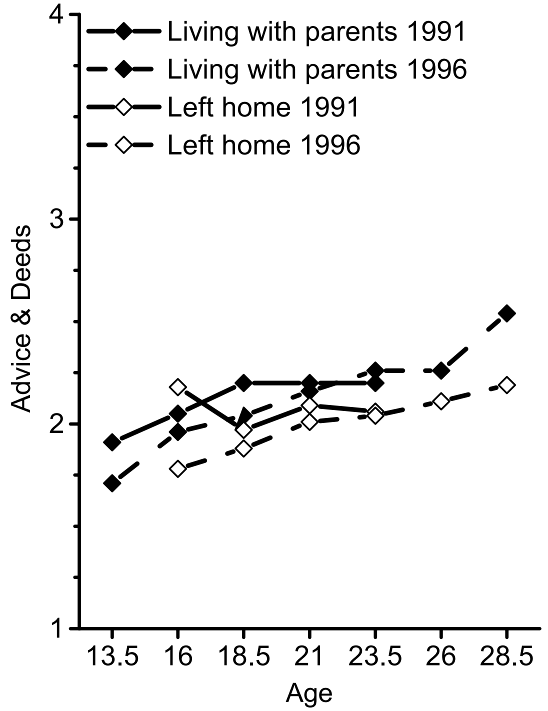

Fig. 3. Cross-sectional age gradients for advice and deeds provided for parents, separately for subjects who were still living with their parents and who had left home. (*Note*. No subjects had left home at age 13.5.)

In a nutshell, one of the variables hypothesized to explain age or cohort effects actually did so: Leaving home partly explained cross-sectional and longitudinal decreases in the extent of parental suggestions and advice and deeds to their children. For help in the opposite direction from children to their parents, suppressor effects occurred. These suppressor effects allowed a meaningful interpretation: Obviously, there occurred two opposite age trends, one negative age trend that was linked to leaving the parental home, and another positive age trend due to factors not empirically identified in this study. Taken together, always the β weights that changed in the hierarchical regressions reflect age effects. In the section on data-driven approaches, it had remained open whether the results should better be interpreted as age or cohort effects. The theory- and data-based hierarchical regression analyses now lend strong support to an age interpretation. These results support the notion of a less hierarchical and more autonomous parent–adolescent relationship with age (Noom, 1999; Youniss & Smollar, 1985). Adolescents (and young adults) engage in more mutual, peer-like relations to their parents, as became apparent in the increase in advice and deeds. However, one should not exaggerate this increase in mutuality. Other factors such as leaving the parental home operate in the opposite direction. Also a second study showed that still in late adolescence, most influences between parents and children remained hierarchical and were not mutual. They were intended to serve the adolescents' and not the parents' needs (Masche, 2003; Masche, Almagro Pulido, & Scheele-Heubach, 2003).

Discussion

In this paper, we have given an overview of the foundations of disentangling age, cohort, and time effects. We have shown that data-driven attempts to distinguish between age, cohort, and time effects on behavior in the sense that true facts are revealed have failed, and that they must fail. We suggested a data-driven method that finds the most parsimonious explanation of data. This was illustrated with an empirical study.

The antithesis to data-driven approaches is an approach based on theory-based analyses and interpretations. Although they contribute to the understanding of antecedents and mediators that stand behind the "empty" indexes of age, cohort, and time, they are all based on additional assumptions. These assumptions restrict the range of effects studied and thus may lead to misinterpretations if important effects are left out. Thus, we finally suggested a synthesis of data-driven and theory-based approaches. It is data-driven in the sense that none of the age, period, and time effects is a priori excluded from the analyses and that the interpretation follows the pattern of results found. At the same time, it is theory-based, because the predictors to explain age, cohort, or time effects have to be chosen out of the universe of possible variables on a theoretical basis. As the research example showed, this approach does not only allow replacing age, cohort, and time with meaningful variables but also to gain insights in which of the three are actually operating. Furthermore, it is even possible to find variables that suppress age, cohort, and time effects. As this approach needs only two times of measurement and even not necessarily longitudinal data, it is quite efficient for research.

Of course, as expressed in the title of this paper, no method can do the quadrature of the circle to provide the one and only answer to the question which of three effects is operating. As was shown, each result—even the result of no differences at all—can be interpreted as the combination of age, cohort, and time effects. The same rationale can also be applied to the regression analyses demonstrated in the last section. But the suggested principle of parsimony of interpretation is in line with the general belief that parsimonious models should be preferred (e.g., Pervin & John, 1997). And the principle of parsimony reminds us that we "do not discover scientific facts; we invent them" (Scarr, 1985, p. 499).

Another critique may refer to the validity of the dependent measures used in the research example. This, however, does not affect the general methodological discussion presented in this paper. Although we would agree that suggestions to begin something new are a somewhat rare form of emotional help and that "advice and deeds" is somewhat ambiguous as to whether emotional or rather practical help was measured, the concordance of results as well as a high correlation between the two items suggests that both tap the same dimension. This can be best described as "providing emotional help."

The methodological approach presented here can be combined with other suggestions made earlier. For example, instead of investigating one dependent variable at a time, by means of MANOVAs and canonical correlations, multivariate dependent variables could be investigated as suggested by Baltes (1968; Baltes & Nesselroade, 1970). Or, as Buss (1973) proposed, instead of mean values, heredity indices or factor covariances could be subjected to investigation.

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