

Changes in dental status and dental care utilization in the Swedish population over three decades: age, period, or cohort effects?

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Abstract – Objectives: In cross-sectional data, cohort effects give a false impression of changes in edentulousness over age and time. This study, therefore, corroborated cross-sectional data with corresponding longitudinal analyses. Complete or partial edentulousness and yearly dental care utilization were studied from 1968 to 2002. **Methods:** A nationally representative Swedish panel study allowed repeated cross-sectional comparisons of ages 18–75 (5 waves $n \approx 5000$), and ages 77+ at later waves (2 waves $n \approx 500$). Cross-sectional 10-year age group differences in 5 waves, time-lag differences between waves for age groups, and within-cohort differences between waves for 10-year birth cohorts were examined. **Results:** Regular over time analysis revealed large decreases in edentulousness between waves. While cross-sectional differences indicated an exponential curve-linear age-dependency, longitudinal differences revealed little decline, contradicting cross-sectional results. Following the cohorts showed little change within cohorts but large differences between cohorts. Care utilization increased between 1968 and 2002 in older age groups but decreased somewhat in younger ones. In 1968, utilization was highly age-related with lower utilization rates among older age groups. By 2000/2002, this had almost reversed. However, all cohorts had aged with little change in their rates of care utilization, while at the same time great between-cohort differences were demonstrated. **Conclusions:** For both edentulousness and dental care utilization, almost all variation originated before 1968. These cohort effects are probably the consequences of changes in dentistry going back several decades and they demonstrate the early socialization of health behavior.

Key words: age; cohort; edentulous; health care; period; Sweden

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Many studies have described large decreases in edentulousness over recent decades; some of these studies have also described concurrent increases in health care utilization (1–8). The results of many studies that set out to describe trends over time, i.e. changes in age-specific prevalence, are however likely to be confounded by cohort effects.

A Norwegian study tried to identify the effects of cohort, period, and age on dental health (9). This

study found cohort effects, as well as age and period effects. Similarly, a Spanish study inferred that changes in dental care utilization were chiefly the consequences of cohort effects (10).

As studies of trends over time confound period effects with cohort effects, this implies that trends may sometimes be misinterpreted, i.e., changes may be interpreted as originating in the studied time rather than as reflecting historical change.

In the Swedish welfare state, dental health has been the target of a multitude of interventions. Period changes linked to these interventions are therefore of considerable interest.

For example, at the end of 1960s, researchers found large socioeconomic differences in dental health (11, 12), provoking a national debate that resulted in the initiation of a national dental health insurance program in 1974. This insurance program was later, in the 1980s and 1990s, successively abandoned. The program greatly reduced the cost of dental care for adults. With this program, the government aimed to create greater social class equity in dental health; it also had the goal of generally promoting better dental health in the Swedish population. By the turn of the new century, the poorer dental health status of older people was recognized (1) and the government initiated a high-cost protection program for people above age 65. Nonetheless, the reintroduction of the national dental health insurance program for people of all ages is a recurring issue, especially at election times.

Large investments have also been made in young people. For example, dental health care for children and adolescents has been free of charge since 1960s. It is, therefore, likely that cohort effects in, for example, dental care utilization exist in Sweden. On the contrary, it is assumed that governmental reforms such as the general dental health insurance program, now abandoned, or the high-cost protection for the elderly, do indeed change people's behavior – even in older age groups. If such reforms have had any effect, these should be reflected in period effects in dental care utilization as well as in dental health status.

Given that regular analysis of over-time trends confound period effects with cohort effects, many governmental decisions may have been based on poor empirical evidence. To our knowledge, no studies have corroborated cross-sectional findings with corresponding longitudinal data.

Age, period and cohort effects cannot be observed directly. Cross-sectional and longitudinal surveys can describe development over time, but the different effects confound each other in analyses. There are three basic survey designs for examining development (13–15). Firstly, there is cross-sectional analysis of a phenomenon by age. This will show age effects as well as cohort effects. In single cross-sectional analyses, the two are confounded. Secondly, repeated cross-sectional surveys may be used for time-lag analysis (16),

i.e. the comparison of two or more measurements of the same age groups over a period of time. This analysis of the over-time trend will show both period effects (that influenced results during the studied period) and cohort effects (that may have exerted their influence long before the study began). Thirdly, there are the longitudinal analyses that follow a single cohort over a period of time. These will show how the cohort is aging (age effects) as well as period effects (factors that may be influential during the studied period).

Period effects refer to specific events that have had the potential to bring about change in the entire population during a specific time period, i.e. the period under study. These can include dramatic events and changes in the physical and social environment such as famine or wars or more subtle changes, such as changes in the social insurance system or in health care. Period effects can result from anything that affects the physical or social environment or people's behavior.

A cohort refers to a group of people sharing similar social and cultural circumstances during a longer or shorter time period, such as childhood or young adulthood (17). Most studies use the concept of birth cohorts, e.g. all the persons born in a specific year or a five- or ten-year time span. Cohort effects refer to factors that have influenced a specific cohort, i.e., changes between one cohort and the next. Whereas period effects refer to events and influences during a studied time period, cohorts may differ from each other because different factors influenced them previous to the study period. Cohort effects arise from historical differences, e.g. in the social or physical environment during childhood or young adulthood, or from differences in the structure or size of different cohorts, e.g., baby-boomers differ from previous and later cohorts partly because of their greater numbers.

Age effects are because of biological, psychological and behavioral processes and occurrences connected with growing up and becoming older. Age effects are usually seen as indicators of aging, which are biological processes leading to impaired capacity and disability. Some changes are not caused by aging *per se*, but are because of the accumulated response to prolonged environmental exposure. It should be noted that exposure may be so correlated with the passing of time that exposure effects cannot empirically be separated from age effects. It is not easy to empirically identify aging (18), but it is important to realize that age effects do

identify similarities in aging across generations and cultures.

The difference between period and cohort effects may at times seem arbitrary. Essentially, the distinction is methodological; period effects refer to events that took place during the study period, i.e. between baseline and follow-up of a specific study, whereas, cohort effects refer to historical differences, i.e. differences already present at baseline. Cohort and period effects may result from the same kinds of factors or causal mechanisms. Yet, it is necessary to differentiate between cohort and period effects to place events and mechanisms within a time frame. It is important to understand this time frame when investigating explanatory factors or planning interventions.

It may be difficult to distinguish age, period and cohort effects, as it can only be performed given certain assumptions (13, 19). However in certain cases, interpretations are more or less clear (15). For example, if longitudinal and cross-sectional descriptions of the age-relatedness of a health condition agree, the most likely explanation is that a sole age effect that has been present, i.e. that period and cohort effects are negligible. In short, to be able to identify which effects have been in operation, we need at least to complement cross-sectional descriptions with corresponding longitudinal analyses.

This study aims to study cross-sectional and longitudinal data for dental status and dental care utilization: Do cross-sectional and longitudinal data give the same picture of the changes over time? Are there any clear age, period, or cohort effects?

Materials and methods

The Level of Living Surveys, LLS (20), were conducted in 1968, 1974, 1981, 1991 and 2000. In 1968, the sample consisted of approximately 6 000 randomly selected persons between the ages of 15 and 75. When the survey was repeated in 1974, everyone from the 1968 sample who was living in Sweden and less than 76 years old was included. In 1974 and at each successive wave, a sample of younger persons and newly arrived immigrants were added to maintain representativity. The ceiling of 75 years was maintained but the lower age limit was raised from 15 to 18 in 1991. Thus, all waves were nationally representative of the adult population at the time of the survey. The surveys were carried out in collaboration with Statistics Sweden, and professional interviewers conducted

the interviews. Between 1968 and 2000, the response rate fell from 90.6% to 76.6%. The number of respondents fell from 5 654 to 5 126.

The SWEOLD study originates from the LLS. It consists of two cross-sectionally nationally representative surveys of people aged between 77 and 98 living in Sweden interviewed in 1992 and 2002 (21). Response rates were 95.4% and 88.5% (22). The two waves of this study contained all those persons aged 77+ who were eligible for at least one LLS. They were interviewed in their homes or institutions and subjects who could not be interviewed directly (11.9% and 12.8%) were interviewed by proxy.

The interviews were about people's general circumstances in LLS and in SWEOLD, and their working life in particular in LLS; both also included questions about dental health. Dental health was measured by the same question in all surveys: Which of the following statements best describes your teeth? The respondent was shown a card with the response alternatives: 1. No teeth or roots only; 2. Dentures, complete or partial; 3. Own teeth, in poor condition: many missing etc.; 4. Own teeth: many fillings, crowns or bridges; 5. Own teeth, in good condition; no or few fillings. 'Completely or partially edentulous' refers to response alternatives 1–3. While response alternatives number 1 and 3 through 5 remained intact during the years of the surveys, the earlier LLS had a greater number of response alternatives concerning different kinds of dentures. These response alternatives of earlier years could be combined to give '2. Dentures, complete or partial', the only response alternative for dentures in LLS 1991, 2000, and in SWEOLD.

Health care utilization was measured by the question: Have you visited a dentist during the last 12 months? Yes/No.

Analysis

Our approach to the identification of age, period and cohort effects is based on Palmore's (15) analysis in which the three separate levels of analysis are delineated: measurable differences, effects, and underlying causes. In short, there are essentially two constellations in the three measurable differences by means of which it is possible to ascertain which effects have been in operation. Firstly, when no effects are present then there are no measurable differences, neither cross-sectional, longitudinal or time-lag differences. Secondly, when only one effect is operating, two of the

measurable differences will agree and the third will be zero. For example, an age effect but no period or cohort effects will make cross-sectional and longitudinal descriptions of age differences similar and time-lag differences negligible. Similarly, a period effect suggests negligible cross-sectional differences, and similar time-lag and longitudinal differences, whereas a cohort effect suggests negligible longitudinal differences, and similar cross-sectional and time-lag differences.

To complicate things, the above two constellations may also arise from a combination of several effects (i.e. two or more effects where some cancel each other out), but in lack of outside verification the most parsimonious solution should be the simplest one, i.e. the one with fewest effects (15). Unfortunately, Palmore's suggestion for a solution offers no identification, i.e. separation, when there are two or three effects. This is the case when all three measurable differences are present.

We studied cross-sectional 10-year age group differences in 1968, 1974, 1981, 1991/1992 and 2000/2002, time-lag differences between waves for 10-year age groups, and within-cohort differences between waves for 10-year birth cohort panels followed over time.

Identification of similar patterns necessitates alternative descriptions of these differences. For example, longitudinal differences may be distributed either over years, i.e. calendar time, or over age. As all three measurable differences have alternatives, a total of six different graphic descriptions are possible, i.e. for each of the separate differences, over age, time period, and cohort, there is a description based on two of the three differences (cross-sectional, time-lag, and longitudinal).

As a result of high mortality in the oldest ages, two measurements of oldest 10-year cohorts were ignored in the last waves. A few measurements were also ignored because of the irregular intervals between waves, which truncated some cohorts in some waves. The irregular intervals also made it necessary to estimate many of the breakpoints for the graphs of the longitudinal cohort differences. These estimates were calculated on the basis of the graphs describing the longitudinal period differences.

Results

Completely or partially edentulousness

Figure 1 shows the percentage of people who were completely or partially edentulous. Age differences

are on the first row, period differences (over calendar time) on the second, and cohort differences on the third. Figure 1a and e shows the cross-sectional differences, Fig. 1c and f the time-lag differences, and Fig. 1b and d the longitudinal differences (the headings show the effects that the curves confound).

Figure 1a shows the cross-sectional age differences. The curves compare the rates of people completely or partially edentulous between the different age groups for the different waves of the survey. The rates become successively higher for successively older age groups, indicating a curvilinear relationship for each wave. In all waves, the percentage of edentulous people increases with age.

Figure 1b shows the longitudinal age differences. The curves follow the cohorts over age. For example, the 1955–1964 cohort may be followed from an average age of 21–31 years, and then on to age 40. Between the last two steps in age, the rate of completely or partially edentulous people increased marginally for this cohort. The curves for successively older cohorts begin at successively older ages, and older cohorts begin with higher rates of edentulousness. Two things stand out. Firstly, older cohorts have a higher percentage of edentulous people. Secondly, the rate of edentulous people in the cohorts seems to have stayed more or less the same over age as the curves are parallel and run horizontal at different levels. If anything, rates increased somewhat and then foremost for the 1905–1914 and 1915–1924 cohorts. Consequently, in comparison to the earlier curves in Fig. 1a, edentulousness does not show any clear age dependency longitudinally.

Figure 1c shows the time-lag period differences. The curves compare rates in the same age groups across the waves of the survey. The figure shows how the percentage of edentulous people fell between 1968 and 2000/2002 in all age groups. The largest decrease (in percentage point decline) occurred in the older age groups with the highest rates of edentulousness.

Figure 1d shows the longitudinal period differences. The differences are the same as in Fig. 1b but distributed over time instead of over age. The overall picture indicates that the rates of edentulousness that the cohorts started out with in 1968 changed little over the time period until the 2000/2002 follow-up. If anything, rates of edentulousness increased somewhat in most the cohorts followed between 1968 and 2000/2002. This increase

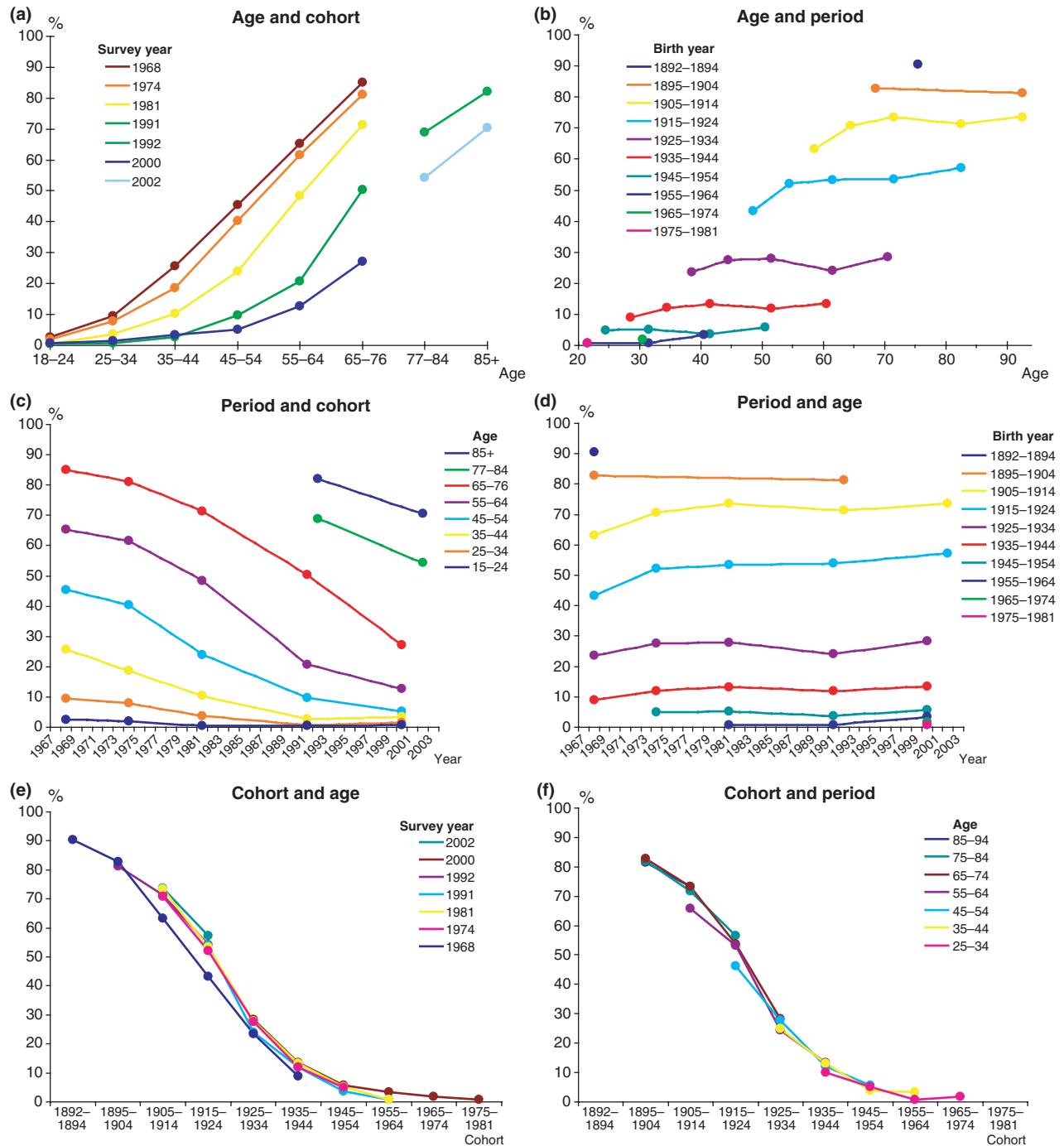


Fig. 1. Percentage of edentulous people, cross-sectional, time-lag, and longitudinal analysis of age, period, and cohort patterns.

occurred foremost between 1968 and 1981 (rates for the 1915-1924 cohort also increased somewhat later).

It is possible to compare the changes between any pair of waves, i.e. 1968-1974 with 1974-1981 etc. A comparison between the two later time periods (1981-1991 and 1991-2000) indicates that the increase in edentulousness was somewhat more pronounced in the last period. In comparison

to the earliest period (1968-1974), the increases in the middle periods (1974-1981 and 1981-1991) may also have been somewhat less pronounced than earlier.

Figure 1e shows the cross-sectional cohort differences. The differences are more or less the same as in Fig. 1a, but reversed and distributed over cohorts instead of over age. The curves compare the rates of edentulousness between the different

10-year birth cohorts within the different waves. Successively later cohorts have rates of edentulousness that are lower. This pattern is seen for all waves. Together the curves of the different waves form a more or less consistent curve-linear cohort dependency.

Figure 1f shows the time-lag cohort differences and enables us to compare cohorts' prevalence rates at a similar age. At each age, successively later cohorts show lower rates. This pattern is seen for all ages. Together the curves for the different ages form a curve-linear cohort dependency, more or less coinciding with the cohort dependency shown by the earlier cross-sectional cohort differences.

Health care utilization

Figure 2 shows the figures for health care utilization. Figure 2a shows the cross-sectional age differences. The curves show that health care utilization was age-related in 1968, with successively older age groups having successively lower utilization rates. In 1974 and 1981, utilization was still age-related, but less so. In 1991, the straight linear age-relatedness is no longer apparent. Instead there is a curve-linear pattern, with utilization rates peaking in middle age. By 2000, the age-relatedness from 1968 almost reversed its linear pattern, with successively older age groups exhibiting successively higher utilization rates.

Figure 2b shows the longitudinal age differences. The curves follow the cohorts over age. Between young adulthood and middle age, the 1925–1934, 1935–1944, and 1945–1954 cohorts increased their utilization rates somewhat. Furthermore, in older age groups, the 1895–1904 cohort shows an increase in utilization rates. While the 1915–1924 cohort indicates an initial increase and a later decrease, the 1955–1964 and 1905–1914 cohorts do not indicate any change. Generally, in comparison to Fig. 1a, the curves' changes were small and the change was not consistent over cohorts. The cohorts' curves also were on very different levels and therefore the figure does not show a uniform and clear age pattern.

Figure 2c shows the time-lag period differences. The curves show large increases in utilization rates between 1968 and 2000/2002 in the older age groups. In younger age groups, utilization rates increased less, and in the two youngest age groups there was even a fall in rates between 1991 and 2000.

Figure 2d shows the longitudinal period differences. Between 1968 and 2000/2002, the different

cohorts show utilization rates that either did not change or rates that increased somewhat. The increase is noticeable chiefly for the 1925–1934, 1935–1944, and 1945–1954 cohorts (from 1974 for the 1945–1954 cohort). While the rise was noticeable up to 1991, between 1991/1992 and 2000/2002, there was either no change or a decrease (for the 1915–1924 cohort).

Figure 2e shows the cross-sectional cohort differences. The graphs show a curve-linear pattern with higher utilization rates for later cohorts. Rates peak for the 1945–1954 cohort and then become successively lower for later cohorts.

Figure 2f shows the time-lag cohort differences. Successively later cohorts show successively higher utilization rates, irrespective of age. Rates peak for 1945–1954 cohort, and then become successively lower for later cohorts. Together the graphs for the different ages demonstrate a curve-linear cohort dependency, which more or less coincides with the cohort dependency shown by the earlier cross-sectional cohort differences.

Age, period, and cohort effects

The cohort effect component appears in cross-sectional cohort differences (Fig. 2e) and in time-lag cohort differences (Fig. 2f). If the other two components, age and period, are negligible cross-sectional and time-lag graphs will show the same trends. Cross-sectional and time-lag trends in cohort differences did largely agree for both edentulousness and dental care. This suggests that both health status and care were subject mainly to cohort effects.

Discussion

At large, cohort effects seem to have dominated dental status as well as dental care utilization meaning that different cohorts with very different health statuses and utilization patterns have moved through time with their patterns relatively unchanged. The movements of these cohort patterns through time have caused large changes in age-specific prevalence, as well as in age-related patterns. The dominating presence of cohort effects in these areas may be inferred by the specific constellation of the empirical differences (15). In both areas, cross-sectional and time-lag analyses demonstrate large differences, whereas longitudinal analysis indicates no or only small changes.

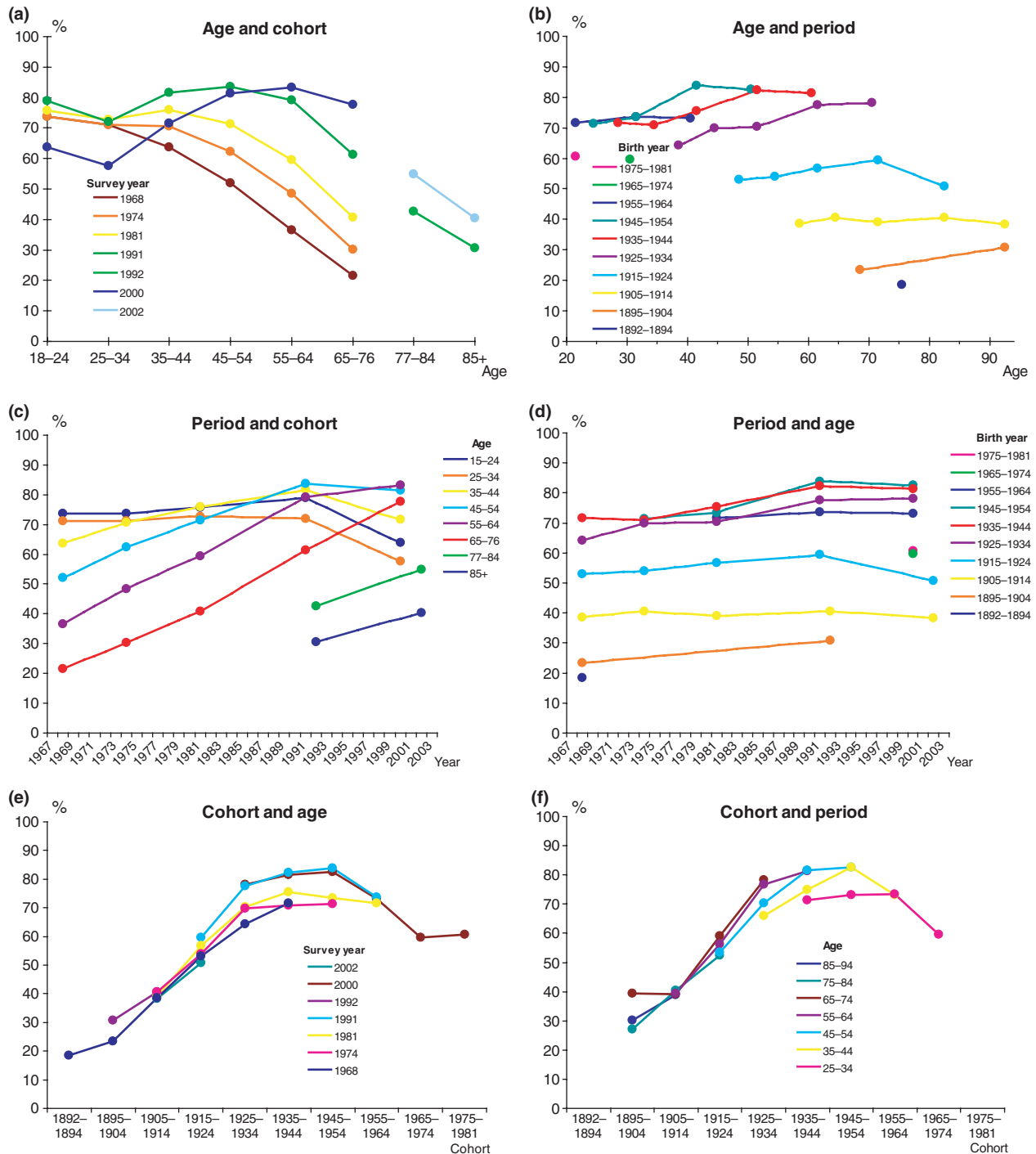


Fig. 2. Percentage utilizing dental care last year, cross-sectional, time-lag, and longitudinal analysis of age, period, and cohort patterns.

We have earlier presented the data for dental status in a paper concerning age changes (23). Here, the same results were used to illustrate the false impression of age changes that cohort effects may give rise to. Disability (i.e. walking limitations), in contrast to complete or partial edentulousness, exhibits a pronounced age effect, i.e.

similar cross-sectional and longitudinal patterns of age-relatedness (23).

Differences in dental care utilization between cohorts followed a curve-linear pattern; rates increased for successively later cohorts, peaked for the 1945–1954 cohort and then became successively lower for later cohorts. In contrast, rates of

completely or partially edentulous people only became successively lower for later cohorts in a curve-linear fashion. All cohorts had aged with little change, simultaneously exhibiting these large between-cohort differences.

The cohorts' movement through time caused the age pattern in health care utilization to reverse. In 1968, it was more common for younger people than older people to have visited a dentist during the previous 12 months. By 2000, it was more common for elderly people than younger people to have visited a dentist.

Older cohorts with 'poor' dental care habits had been replaced by younger cohorts with 'better' habits. This may imply that better health care habits are something one is socialized into one's formative years, and these habits have then followed the cohorts through life. Cohorts born after the 1945–1954 cohort, moreover, learnt to visit a dentist seldom, possibly because of their better health status during childhood and teenage years.

The over-time changes in dental status were caused by the forward movement through time of cohorts with varying degrees of edentulousness. Because successively later cohorts had lower rates of edentulousness, this movement only created successive decreases in age-specific rates over time. This pattern also implies that no major period changes really occurred, indicating, for example, a similar incidence throughout the period. The cohort effect probably reflects successive changes in dentistry which took place several decades ago. Extraction seems to have been a widely-used method of curing dental problems earlier in the 20th century. If dentists gradually substituted pulling out teeth with other cures (that allowed people to keep their teeth) during the decades preceding the baseline in 1968, this would have created the gradual decrease in edentulousness for later cohorts.

Time-lag period differences, in contrast to longitudinal ones, indicate a more or less continuous improvement in dental status between 1968 and 2002, visible for all age groups. It seems more than plausible that this fall in edentulousness sometimes have been concluded to originate in studied time, i.e. have been miss-specified as a period effect. A great many interventions have been carried out in the field of dental care and the changes over time linked to these interventions have been of great interest. For example, there was the introduction of the general insurance program in 1974. Because

prevalence comparisons over time confound period and cohort effects, many governmental decisions may have been based on poor empirical evidence. Our results show that cohort effects totally obscure the period changes that occurred.

Besides the dominating cohort effects, it is still possible that minor changes were because of period effects. One way to get around this confounding cohort effect is to compare the longitudinal age effect across periods. A comparison of longitudinal period differences may indicate that the two time periods after the introduction of the program in 1974, the 1974–1981 and 1981–1991 time periods, were more beneficial to the population's dental health status than the time period before. The rate of edentulous people even went down in some cohorts in the latter time period. It is, moreover, noteworthy that the last time period 1991–2000 does not seem to have been as beneficial as the two middle periods.

For dental care, similarly, the longitudinal differences may indicate a different development during the last period than during earlier ones. Utilization rates even went down for some cohorts, which was in contrast to earlier periods when rates only demonstrated increases.

If the general insurance program was of any effect, this should be evident in these longitudinal patterns. Although the pattern in dental care utilization gives no clear support, the pattern in edentulousness suggests that the program might have contributed to improve people's dental status. It is, however, important to note that the longitudinal changes are not anywhere close to the changes indicated by 'usual' repeated cross-sectional comparison.

The decrease in utilization rates seen in the longitudinal pattern during the last decades might also be because of policy changes, e.g. lately people have been recommended check-ups every second year rather than yearly.

Inconveniently, it is not possible to disentangle of age, period and cohort by empirical analysis alone (13, 19). For example, dental status could also reflect the fact that period effects have successfully countered the age effects. Although this represents a less parsimonious solution, it cannot be ruled out. This is an example of the identification problem, common to all approaches that try to identify effects. The important message here is that it is not possible to ascertain by empirical evidence only whether the change can be ascribed to cohort effects, i.e. effects of occur-

rences before baseline, or to period effects, i.e. occurrences between baseline and follow-up (1968–2002). This is highly relevant in view of the considerable governmental efforts that have been made in the field of dental care.

In the local Norwegian study of edentulousness, there was evidence of the presence of all three effects (9). The Norwegian findings may also imply that age effects in fact do exist, but that they were successfully countered throughout the period under study in Sweden. A better understanding of conditions and effects across countries is needed to enable us to evaluate changes more accurately.

Beside the identification problem, there are other problems that may make evaluation tricky, e.g. problems associated with measuring, nonresponse, and representativity. For example, the increase in nonresponse in later waves of surveys could possibly skew the picture of the population changes. The increase was largely because of an increase in people who refused to participate (20). Nonresponse was furthermore age-related, with nonresponders tending to be older. Changes in attrition, for example loss to follow-up because of death, can also influence results. Differential loss to follow-up suggested a slight underestimation of edentulousness in later waves (24). Whether dental health status and care are related to mortality risk is not clear, but the influence of changes in selective mortality on the results is not likely to have been substantial. Opposing patterns of effects in the same data but for a different outcome (i.e. an age effect in walking difficulties) suggest that our findings are robust (23).

Trend surveillance for edentulousness has to take considerable cohort effects into account. Large cohort effects demonstrate the importance of early-life factors for health in later life.

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