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# Advances in Age–Period–Cohort Analysis

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Social indicators and demographic rates are often arrayed over time by age. The patterns of rates by age at one point in time may not reflect the effects associated with aging, which are more properly studied in cohorts. Cohort succession, aging, and period-specific historical events provide accounts of social and demographic change. Because cohort membership can be defined by age at a particular period, the statistical partitioning of age from period and cohort effects focuses attention on identifying restrictions. When applying statistical models to social data, identification issues are ubiquitous, so some of the debates that vexed the formative literature on age–period–cohort models can now be understood in a larger context. Four new articles on age–period–cohort modeling call attention to the multilevel nature of the problem and draw on advances in methods including nonparametric smoothing, fixed and random effects, and identification in structural or causal models.

***Keywords:*** *APC models; cohorts; identification strategies*

This issue of *Sociological Methods & Research* contains four state-of-the-art articles on the statistical analysis of age-specific data arrayed by periods (years) or, alternatively, by cohorts. This has always been the nub of the problem: Can you really distinguish one perspective from the other? This has been debated mostly from an algebraic perspective—the algebra of the generalized linear model. Sometimes it is fun to reconsider the problem solely from a visual perspective. One can plot the data in something like a three-dimensional space (albeit on the two dimensions of a page or a computer screen). Figure 1 is from Ploch and Hastings (1992), after Robinson (1988). It is most of the summary history of female labor force participation in the United States during the 20th century. The identification problem does

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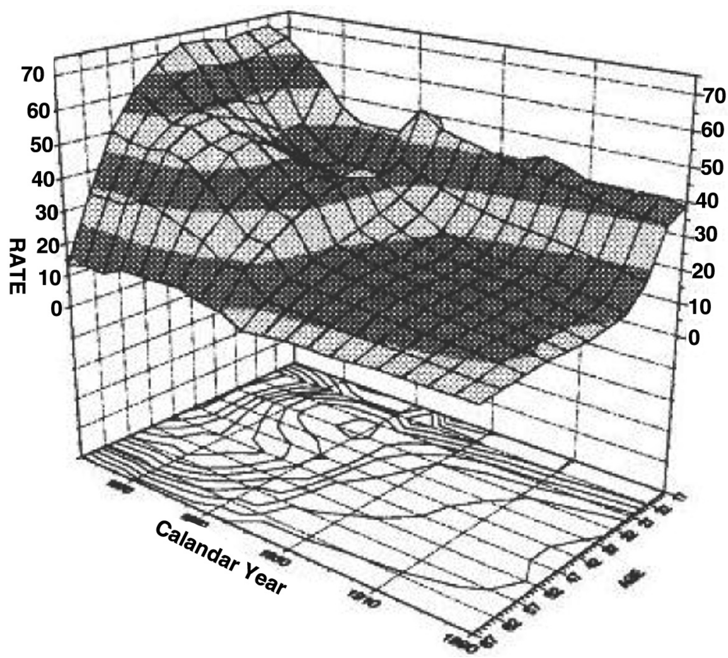
**Figure 1**
**Labor Force Participation Rates by Year, All U.S. Women, for Selected Years, 1890–1985**




Source: Ploch and Hastings (1992).

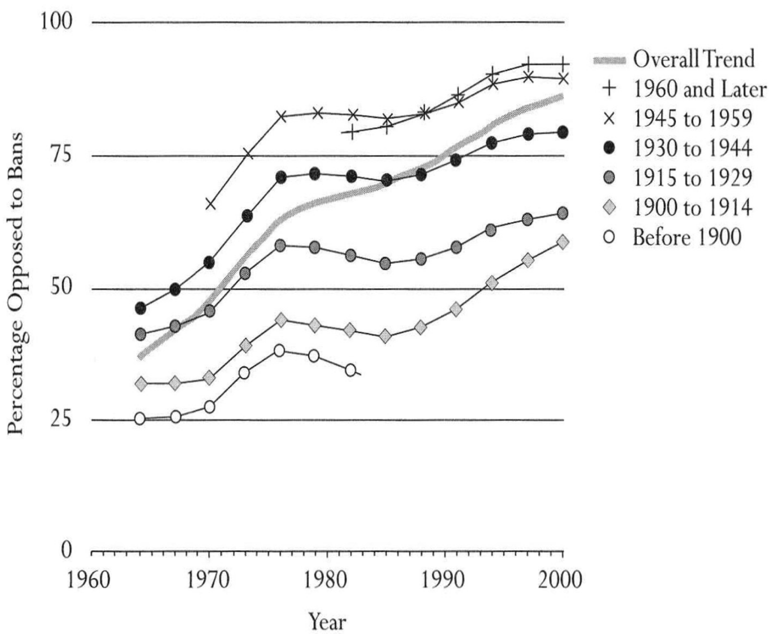
not go away (Kupper et al. 1985; Yang and Land 2008 [this issue]), but it does transmute, after a fashion: What defines the various peaks and valleys—the sinkhole, for example, in labor force participation rates at early ages in the 1950s?

The articles in this issue illustrate the various statistical approaches with reference to a variety of data. Some are overtly sociological, such as political alienation (Winship and Harding 2008 [this issue]; after Kahn and Mason 1987). Others are more strictly demographic or epidemiological—lung tuberculosis mortality in Taiwan, for example (Fu 2008 [this issue]), although even here one can imagine a place for the history of tobaccosis (Ravenholt 1990) and the sociological process by which the smoking habit and nicotine addiction are spread from place to place, from generation to generation, or from friend to friend (Smith 2004:117). I sometimes wonder at the enduring interest of age–period–cohort analysis to sociologists. Certainly, we have had an ongoing and colorful debate about the viability, plausibility, and wisdom of various omnibus solutions to the identification problem, but then something similar seems to have occurred among the epidemiologists and biostatisticians as well (Fu 2008; cf. Holford 1985; Kupper et al. 1985; Clayton and Schifflers 1987; Lee and Lin 1995).

One thing that has made these issues of so much interest to sociologists (and their fellow travelers, demographers) is that were I to tire of typing "age–period–cohort analysis," I might well shorten it to "cohort analysis," but never "age analysis" or "period analysis." We are mad for cohorts. The centrality of cohorts and cohort replacement for understanding social change via replacement was made so well by Ryder (1965) that I needn't repeat it here. Elsewhere (Smith 2004:111), I have commented on the durability of a great idea in the face of tiresome evidence to the contrary, in the case of variation over time in rates of fertility. It is only fair, therefore, to mention that there are times when cohort replacement is a very powerful phenomenon. In Figure 2, from Fischer and Hout (2006), there is a thick gray line indicating that the percentage of Americans opposed to bans on intermarriage increased substantially during the last third of the 20th century, from less than 40 percent to more than 80 percent. No single cohort changed anywhere near this much, but there were substantial differences between cohorts, and as younger cohorts with more tolerant views replaced less tolerant older generations, the overall opposition to such bans faded away. The power of cohort replacement is especially striking from the mid-1970s through the 1980s: This was a period when *within* cohorts the percentage opposed to such bans actually showed a slight decline, yet the overall percentage opposed continue to grow, albeit at a slower rate than previously.

This slowdown within cohorts is almost certainly a sign of the times, and not an age effect, since it was occurring at the same period of history for all cohorts, hence, men and women at a variety of ages. If there are age effects related to tolerance for intermarriage, they are not readily apparent in Figure 2. However, no one doubts that there are plenty of phenomena for which age effects are crucial: We grow older. Hormones come and go. Something bad seems to happen with our cells, year by year, day by day, although this is only the individual's perspective. I gather that we should take some consolation in the sensibility of this state of affairs from an evolutionary perspective: It's good for the species.

**Figure 2**
**Overall Trend Toward Greater Opposition to Bans on Intermarriage Reflects the Succession of Birth Cohorts**




Source: Fischer and Hout (2006).

Other things: First, we get smarter, and then we get stupider. For verbal acuity, the turning point appears to be about age 50 (Yang and Land 2008). But I assume that the same thing happens with mathematics as well. (Are we less good at linear algebra, or just less interested in it? Which comes first?) As we move from childhood to adolescence, we are better equipped for criminality and get more opportunity to do crimes, but then less, or maybe we have other things to do, or we are culled from the population (e.g., O'Brien, Hudson, and Stockard 2008 [this issue]). If we are in skilled labor, with low age substitutability, then our wages are in some sense held down when we are young and accruing specific capital; they rise thereafter, since surely a great deal of specific capital is required to, say, introduce a collection of articles. If there is less specific capital and greater age substitutability, expect a different age-earnings profile (Becker 1993).

Things change with age—no doubt about it. The real issue is "period change" versus "cohort change." Just when one seems to triumph, the other reasserts itself. Technical papers tend to get argued in technical terms—the desiderata are efficiency, consistency, unbiasedness, and identifiability. In technical reports, data help to illustrate or motivate the good features of a model. But at the end of the day, we need to consider whether the models comport usefully with our understandings of change—social, demographic, epidemiological, developmental, historical, and so forth.

## The Four Articles

In argumentation, your constraint is intelligent; the other guy's is his subjective opinion. Well, what might an intelligent constraint look like?

Fu (2008) starts with a sensible premise, that the typical structure of age–period–cohort data contains a lot of cohort overlap. Data are typically presented at intervals separated by, say, 5 years. So you have a bunch of observations for a 1-year interval, and then a bunch of other observations for another 1-year interval, 5 years later, from midyear to midyear. The Greatest Generation (Brokaw 2004) notwithstanding, cohorts are rarely sharply bounded substantively; they are the folks who show up in a given age group in a particular year. If observations are made 5 years apart, then age groups are typically 5 years in width, and this means that cohorts are too. When we estimate an effect for a given cohort, we are assigning this effect equally to what might instead be five separate 1-year cohorts. The original sin having been made, it is a small step to insisting that cohort effects be in some sense smooth, that neighbors be like neighbors. Polynomials for one dimension or another have long featured as potential solutions to the identification problem (e.g., Fienberg and Mason 1985): Just don't put in too many terms!

Fu (2008) goes this idea two steps better: First, Fu uses nonparametric spline smoothing instead of polynomials: Nice! Very flexible, very 21st century! Second, Fu establishes consistency in all three sets of effects via a two-step estimator. Estimate the age and period effects along with a nonparametric smoothing function for the cohort effects. You have to decide how many degrees of freedom for the smoothing function, but you can check the sensitivity of results to this choice. This model yields consistent estimates of age and period effects. Use these consistent estimates to pick a just-identifying constraint, say, on a pair of age groups, and then do a just-identified multiple classification analysis (Mason et al. 1973). After this second step, the cohort effects are consistently estimated too.

Consistent in what sense? We normally think that consistency is something that can be revealed with lots and lots of data, but there has always been a somewhat hidden two-level problem to age–period–cohort data. One is the old-fashioned estimation problem: Do we have enough observations in each cell to estimate consistently the corresponding rate or count in a population? The second is an issue that I have always thought (Smith, Mason, and Feinberg 1982) got short shrift with everyone focused on the identification problem: Do you have enough cells, typically enough periods of data? If you do, life is good. If not, the models are dominated by macro-level error in the corners of the table, that is, the cohorts for whom there are only one or two age groups and/or periods of observation. A statistical proof of consistency for an estimator may be thin gruel for an analyst with a limited number of periods of observation, and it will probably take more experience and more substantive knowledge, and less argumentation and assertion, to establish how big a table really needs to be for consistency to apply.

Yang and Land (2008) are foursquare on top of the multilevel issue: They treat the situation in which the analyst has the individual-level data underlying the rates or counts. These might be aggregated for purposes of summarization, but from an analytic standpoint, each micro unit (individual) can be categorized in terms of a person's age at the time of observation and, hence, the cohort. It can also be characterized by other individual-level characteristics that might reasonably be related to the response. Yang and Land are analyzing interesting data on verbal acuity from the General Social Survey. The question is whether verbal acuity is going down in the U.S. population and, if so, whether this is cohort replacement or something else. There are plenty of individual-level differences in verbal ability—educated people seem to have more of it, and so do women—and these effects can be estimated with micro-level data.

With Fu (2008), Yang and Land aver that cohorts are not likely finely segmented; instead, "meaningful cohorts often are considered to be of durations longer than single years" (2008:302). This said, the identification problem disappears. In a given year (observational period), respondents may be of (slightly) different ages but within the same 5-year cohort.

In fact, this definitional trick with respect to what constitutes a cohort would not have been needed. The age pattern of verbal acuity is captured by a simple polynomial—a quadratic, in fact. The pattern—you learn more and more words, and then you start forgetting them—is, as the authors note, “consistent with theory of cognitive growth” (Yang and Land 2008:321). The more faith we have in our theories, the less salient the identification problem.

But Yang and Land’s article (2008) is not about the identification problem. Rather, it is an inquiry into how the macro-level effects symbolized by periods or cohorts should be represented. Standard practice, multiple classification analysis (Mason et al. 1973), has been to use some sort of dummy variable coding, which, to tart it up a little, means a fixed effects model. Fixed effects models have some advantages above and beyond being easier to estimate with conventional software: They tend to give better results in small samples, where smallness is primarily with respect to the number of macro-level observations, and they do not require macro- and micro-level errors to be uncorrelated with one another. This latter assumption is one about which many econometricians are dubious, hence, the dominance of fixed effects models in the current social science literature.

And what is the competitor? A random effects model: There are a reasonable number of cohorts and periods, and these are in some sense a sample from a larger distribution of possible cohort and period effects where the distribution is not too nonnormal. Random effects models have a certain economy from the standpoint of parameterization. In the age–period–cohort case, they have a singular virtue: Whereas the fixed effects coefficients chew up whatever variance there is across cohorts and/or periods—an exhaustive set of dummy variables will do that!—the random effects model allows for the estimation of effects associated with macro variables measured for specific cohorts or years. From the beginning, critics of the age–period–cohort accounting framework (e.g., Markus 1983) pointed out that the real name of the game was not variance decomposition or reparameterization, but identifying substantive variables that operationalized the various effects. The random effects model is a good framework for doing this since it allows one to measure just how much of cohort effects are attributable to, say, cohort size. Yang and Land’s (2008) primary result—that for the verbal acuity data, and even in a situation of relatively few periods and cohorts, the random effects model compares favorably with the fixed effects counterpart—is therefore quite promising.

This use of a random effects representation of one or more of the canonical dimensions (age, period, and/or cohort) does not require micro-level data. It can also be used with traditional aggregated data, such as age–period-specific homicide or suicide rates, as per O’Brien et al. (2008).

Their age–period–cohort mixed model treats age and period as fixed effects—those ubiquitous dummies—and cohort as a random effect. As they note, one might just as well consider random age or period effects (Yang and Land, 2008, specify random effects for both period and cohort). But in this case the random effects cohort specification is motivated by an interest in the extent to which specific cohort characteristics—here, the proportion of nonmarital births in a cohort and the relative cohort size—explain the preponderance of the intercohort variation in both homicide offenses and suicide.

Winship and Harding (2008) are also interested in the extent to which measured variables can in some sense explain the age, period, and cohort effects from the accounting model. But whereas O'Brien et al. (2008) are essentially allowing "proxy variables" (e.g., cohort size) to soak up variance otherwise attributable to a random effect, Winship and Harding are posting full-fledged structural equation or path models, in which sets of measured variables (e.g., church attendance, education) intervene between age, period, and cohort (fixed) effects and, in this case, political alienation. Identification is achieved under some conditions described by Pearl (2000) in the framework of causal modeling. Strong features of this perspective include the following: (a) A given proxy variable can be associated with more than one of the dimensions of age, period, and cohort, and (b) unlike the traditional just-identified accounting model, the plausibility of alternative restrictions can be tested statistically.

There is something else fundamentally different about this article, and it is the idea that the original age, period, and cohort categories are the exogenous elements of a causal chain. I am dubious that we really want to think of these variables as causes since they do not satisfy the manipulability criterion (Holland 1986), and I think that the manipulability criterion has considerable value to us in the way we think about causes in the social world and design research (Smith 2003). But I am in the minority, and the argument that variables such as these can be causes (e.g., Winship and Sobel 2004) is generally accepted in practice. Fortunately, it does not appear that one needs to take a stance on the causal interpretation of these models to benefit from their use in establishing identified models and articulating variables associated with both the age–period–cohort accounting scheme and the response variable.

All four articles provide useful, innovative insight on what remains a canonical form of organizing data and thinking about the world of social and demographic change.

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