Change in American Attitudes to Homosexuality

The Liberalization of American Attitudes to Homosexuality and the Impact of Age, Period, and Cohort Effects

David Ekstam, Uppsala University, Sweden

Prior analyses of age, period, and cohort effects in American attitudes to homosexuality have resulted in conflicting findings. I show that this is due to insufficient attention to the statistical identification problem facing such analyses. By means of more than four decades worth of survey data and two attitudinal measures taping social tolerance of homosexuality, I demonstrate that the conflicting results of prior research can be explained by differences in the implicit and unsubstantiated assumptions made to ensure model identification. To make up for the lack of attention to these assumptions in prior work, I discuss which age, period, and cohort effects we might expect to see based on prior knowledge about the case at hand, socialization theory, and research on how aging affects outgroup attitudes. On that basis, I also discuss which conclusions about age, period, and cohort effects we can actually draw in the case at hand. On a more general level, this article joins a growing literature that cautions against age-period-cohort analysis that does not give sufficient attention to theoretical expectations and side information when making the identifying assumptions on which the analysis must unavoidably rest.

Introduction

Over the past three decades, the average American has become dramatically more likely to endorse gay rights and to view homosexuality as ethical (Pampel 2016; Schwadel and Garneau 2014; Twenge, Sherman, and Wells 2016). This swift cultural transformation is surprising, especially since persistence, rather than change, tends to characterize political orientations over the individual lifespan (Alwin, Cohen, and Newcomb 1991; Jennings and Markus 1984; Stoker and Jennings 2008). The case has consequently been subject to a large number of age-period-cohort (APC) studies attempting to decompose this change into

I would especially like to thank Anders Westholm, Gina Gustavsson, and James L. Gibson for helpful comments and advice. Direct correspondence to David Ekstam, Department of Government, Uppsala University, Box 514–751 20 Uppsala, Sweden; Tel: +46 76 281 11 81; e-mail: david.ekstam@statsvet.uu.se.

© The Author(s) 2021. Published by Oxford University Press on behalf of the University of North Carolina at Chapel Hill. This is an Open Access article distributed under the terms of the Creative Commons Attribution License (http://creativecommons.org/licenses/by/4.0/), which permits unrestricted reuse, distribution, and reproduction in any medium, provided the original work is properly cited.
period and cohort effects while statistically isolating or otherwise accounting for age effects (e.g., Andersen and Fetner 2008; Baunach 2011; Keleher and Smith 2012; Pampel 2016; Schwadel and Garneau 2014; Sherkat et al. 2011; Twenge, Sherman, and Wells 2016). However, these studies have come to widely different conclusions.

I address these conflicting findings, arguing that cross-study differences principally are methodological and a token of insufficient attention to just how sensitive. APC analysis is to model specification. This model dependence originates from the fact that age, period, and cohort are not logically independent of one another \( (\text{period} - \text{cohort} = \text{age}) \), which is known as the APC identification problem (Mason et al. 1973). This renders estimation of effects associated with the three variables impossible unless a constraint on at least one of them is imposed (Bell and Jones 2013; Glenn 2005). However, since it is the constraint that determines the solution among the infinite number of possible solutions in this scenario, it follows that results produced by an APC model will be meaningful only if the constraint(s) used can be justified theoretically (Glenn 2005; Luo and Hodges 2016; O’Brien 2017). This issue has been overlooked in previous research on American attitudes to homosexuality and constraints have instead been chosen without any explicit theoretical justification given.

More specifically, I try to contribute to the research field on attitudinal change toward homosexuality in four different ways. First, I review the various analytical constraints that have been used but not made explicit in prior research. I additionally discuss the validity of these constraints and the associated assumptions in light of what we know about the general attitudinal development, socialization processes of group-centric attitudes, as well as how aging affects such attitudes. Second, I fit the various models employed by prior studies to 26 pooled waves (1973–2016) of the General Social Survey (NORC 2018) and a dependent variable tapping social tolerance of homosexuality, keeping sample and operationalization constant across models. In so doing, I show that the conflicting findings of previous research essentially are a product of different studies using different analytical constraints in order to ensure model identification. Depending on the constraint, cohort effects are either estimated to follow a strongly positive trend or no trend at all, and age effects are either estimated to be negative, absent, or even positive. Parameter estimates for period effects differ substantially across models as well.

Third, in spite of this constraint dependency, I provide robust evidence of substantial interest to the field on socialization and attitudinal change toward homosexuality. Across all models and regardless of what underlying trend in the cohort effects that is estimated, cohorts born in the period 1942–1951 rank higher in tolerance compared to adjacent cohorts, and cohorts born in the period 1957–1966 deviate in the opposite direction compared to adjacent cohorts. These findings make a lot of sense from an impressionable-years perspective on political socialization (Sears 1975; Stoker and Jennings 2008), as the 1942–1951 cohorts grew up during the counterculture era of the 1960s and 1970s and experienced the emergence of homosexuality as a salient political issue as young adults, whereas the 1958–1967 cohorts came of age during the peak of the
HIV/AIDS crisis and the generally more conservative zeitgeist of the 1980s. This suggests that early life socialization can have a significant and lasting effect on how one views homosexuality throughout the lifespan. In terms methodology, these findings also demonstrate that discrete variation around linear variation (trends) in age, period, and cohort can be reliably estimated by purely statistical means, even if the linear variation cannot (Glenn 2005, 14).

Finally, I weigh in on the more general methodological debate about APC analysis. In recent years, this debate has primarily focused on whether so-called hierarchical APC estimation with cross-classified random effects modeling (HAPC-CCREM) (Yang and Land 2006, 2008, 2013) “solves” the identification problem (Reither et al. 2015a, 2015b; Yang and Land 2006) or not (Bell and Jones 2014, 2015, 2018; Luo and Hodges 2020; O’Brien 2017). The analyses of this paper lend support to O’Brien’s (2017) argument that HAPC-CCREM models intrinsically shrink linear effects in one of the two variables that are specified as random due to the fitting function of mixed effects models. If correct, this means that mixed effects APC models impose a “silent” constraint that has been overlooked in most previous research. While this does not mean that the HAPC-CCREM approach is without merit, it does mean that the shrinkage constraint must be theoretically justified in order to support any results produced by the approach.

A Cultural Transformation

In the United States, homosexuality as a salient social and political issue first emerged in the wake of the Stonewall riots in 1969 and the subsequent mobilization of the American gay movement (Andersen 2006). By the end of the 1970s, many states had passed legislation that strengthened gay rights and Pride festivals had been held in most big cities (Epstein 1999). Notwithstanding this initial progress, the 1970s also saw strong countermobilization from conservative groups (Fetner 2008) and the zeitgeist concerning homosexuality became markedly hostile during the 1980s following the outbreak of HIV/AIDS (Marcus 1992; Ruel and Campbell 2006). However, since the early 1990s, social tolerance of homosexuality has steadily increased (see figure 1). For example, while only 60 percent of the American adult population toward the end of the 1980s were willing to allow homosexuals working as teachers, that number had grown to almost 90 percent in 2016. In prior research, this rapid cultural transformation has been attributed to a greater media visibility of homosexuals (Schiappa, Gregg, and Hewes 2006), changing elite signals (Brewer 2003), pro-gay social movement campaigns (Epstein 1999), and to more general structural shifts such as declining religiosity (Sherkat et al. 2011), increasing levels of education (Schwadel and Garneau 2014), and an individualization and detraditionalization of American society (Inglehart and Welzel 2005).

However, a controversy dividing prior work on the topic concerns the extent to which the increase in tolerance over the past few decades reflects period effects as opposed to cohort effects (e.g., Andersen and Fetner 2008; Baunach 2011; Pampel 2016; Schwadel and Garneau 2014; ScherKat et al. 2011; Treas
Figure 1. Development of social tolerance of homosexuality, 1973–2016. Notes: Trends represent weighted means for each measuring period. All items are binary (yes or no) except for the “are homosexual sexual relations wrong?” item, which is scored: (1) always wrong, (2) almost always wrong, (3) only sometimes wrong, and (4) not wrong at all (rescaled 0–1).

Source: General Social Survey (NORC 2018).

2002; Twenge, Sherman, and Wells 2016). A period effect is attitudinal variation in the aggregate resulting from intraindividual change, the latter being caused by specific events or a more general transformation of the social or political environment. A cohort effect, by contrast, is attitudinal variation in the aggregate resulting from incoming generations being different from outgoing ones due to differences in early-life socialization (Glenn 2005). Cohort effects thus imply that attitudinal shifts on the aggregate level can occur through the process of generational replacement even if no individual-level attitudinal change occurs during the period (Firebaugh 1989). Accordingly, the overall development shown in figure 1 can either reflect pure period effects, pure cohort effects, or a combination of period and cohort effects.

Predominantly, prior studies of American public opinion on homosexuality report liberal trends in both period and cohort effects (Andersen and Fetner 2008; Baunach 2011; Keleher and Smith 2012; Lewis and Gossett 2008; Pampel 2016; Ruel and Campbell 2006; Sherkat et al. 2011; Treas 2002). For example, generational replacement (cohort effects) is estimated to account for 54 percent of the total increase in moral acceptance of homosexuality from 1973 to 2014 (Pampel 2016), 35 percent of the increase in same-sex marriage approval from 1988 to 2008 (Baunach 2011), and 21 percent of the decrease in negative feelings
toward homosexuals from 1988 to 2008 (Keleher and Smith 2012). According to these studies then, attitudes have become more liberal not only as a result of period effects affecting all cohorts but also due to the continuous exit of relatively intolerant old cohorts and the entry of more tolerant new ones.

However, the picture of this “silent revolution” driven by generational replacement has been questioned by some recent studies (Schwadel and Garneau 2014; Twenge, Carter, and Campbell 2015; Twenge, Sherman, and Wells 2016). Looking at the willingness of the public to grant homosexuals various civil rights, Schwadel and Garneau (2014) fail to find any trend in the cohort effects and instead attribute change between 1974 and 2010 to period effects. Largely the same results are reported by Twenge, Sherman, and Wells (2016) for an indicator that taps social tolerance of homosexuality over the period 1972–2012 and by Twenge, Carter, and Campbell (2015) for an indicator of generalized tolerance over the period 1972–2014. In other words, these studies maintain that the rapid liberalization of public opinion on homosexuality over the past few decades almost entirely has been driven by period effects.

The APC Identification Problem

While differences in operationalization and longitudinal scope between studies may partly explain the divergent findings described above, there is also reason to believe that a more fundamental issue is at play, namely the so-called APC identification problem (Mason et al. 1973). The APC identification problem is due to the fact that period and cohort effects are confounded by age effects in repeated cross-sectional data (Bell and Jones 2013; Glenn 2005). An age effect is attitudinal variation across age groups (independent of cohort effects) that may be brought about by, for example, accumulation of social experience, neurological aging, or age-graded changes in social roles (Yang and Land 2008). While age effects under normal circumstances do not shift attitudes on the aggregate level, they confound period and cohort effects whenever a relationship exists between age and the dependent variable.1 The reason is that any comparison of two cohorts at a given point in time is by logical necessity a comparison of age groups and, furthermore, that any comparison of two cohorts over time so as to keep age constant is by logical necessity also a comparison of time periods.

Ideally, one would solve this issue by estimating the effect of age, period, and cohort simultaneously in a regression model. However, since each variable is an exact linear function of the other two (period − cohort = age), this cannot be accomplished, as keeping two of the variables constant per definition also keeps the third variable constant (Bell and Jones 2013; Glenn 2005). Age, period, and cohort effects can of course simultaneously be estimated if some kind of constraint is imposed on one of the effects, which breaks the linear dependence in the statistical model, and several different types of constraints have been suggested over the years.
The inevitable and often overlooked problem with this approach, however, is that it is the constraint that determines the solution that best fits the data out of the infinite set of possible solutions of age, period, and cohort effects combinations that exist due to the fact that the three variables are not mathematically independent of one another (Bell and Jones 2013; Glenn 2005; Luo and Hodges 2016). Due to this fact, it also follows that imposing an incorrect constraint on one of the effects will lead to incorrect estimation of all three effects. As shown by Rodgers (1982), if the true age effect $b_a$ is incorrectly estimated as $b_a + v$, the true cohort effect $b_c$ will be estimated as $b_c + v$, and the true period effect $b_p$ will be estimated as $b_p - v$.2 Basically, if the linear effect of one variable is changed, the linear effect of the other variables will also have to change in order to reduce the residuals and make the solution a good fitting solution.

In sum, different constraints can produce different results and the results will be meaningful only if the constraints are valid (Bell and Jones 2015; Glenn 2005). This state of affairs is often overlooked in applied research and prior studies of American attitudes to homosexuality do not constitute an exception. Few of the studies that I cite in this article offer more than a short paragraph on the identification problem, and not a single one of them provides any explicit motivation, theoretical or empirical, of why a given constraint is chosen.

**Analytic Constraints Used in Prior Research**

Predominantly, prior studies on American attitudes to homosexuality have estimated period and cohort effects (specified as two sets of dummy variables) by simply omitting terms for age (Andersen and Fetner 2008; Baunach 2011; Keleher and Smith 2012; Lewis and Gossett 2008; Ruel and Campbell 2006; Sherkat et al. 2011; Treas 2002). Notably, these studies all estimate that both period and cohort effects account for the liberal shift in public opinion in recent time. However, while omitting age effects obviously breaks the linear dependency in the statistical model, it also translates into the constraint that age effects are absent in the data generating process. An attempt to justify this assumption is not provided in the aforementioned studies and, for reasons explained, the results will be biased if the assumption is incorrect. To what extent it is reasonable to expect age effects to be absent in regard to attitudes on homosexuality is discussed in a latter section of this article.

Another group of studies has taken a more intricate approach (Schwadel and Garneau 2014; Twenge, Carter, and Campbell 2015; Twenge, Sherman, and Wells 2016). Instead of omitting one of the variables, these studies have circumvented the identification problem by grouping one of the variables into multiyear units, for example, by using five-year cohorts (e.g., Schwadel and Garneau 2014).3 This breaks the linear dependency in the statistical model (since keeping two variables constant no longer keeps the third constant) and implies the assumption that the cohorts grouped together have exactly the same effect on the dependent variable. As pointed out by Yang and Land (2006, 2008, 2013), this kind of model can either be specified with age as a fixed effect and period and cohort as cross-classified random effects (HAPC-CCREM) or with
fixed effects for all three variables (HAPC-CCFEM). Generally, HAPC-CCREM performs better than HAPC-CCFEM with regard to model fit (Yang and Land 2008, 2013) and is the more widely used specification as illustrated by the fact that it is used by all the aforementioned studies.4 Treating period and cohort as random effects also makes a lot of sense conceptually, as both can be regarded as higher-level contextual variables (within which individuals are nested) and as samples of contexts drawn from an infinite population of contexts. By contrast, age is better conceptualized as an individual-level attribute and is therefore more appropriately modeled as a fixed effect.5

Despite its conceptual appeal and statistical efficiency, the HAPC-CCREM approach has been subject to criticism in a series of simulation studies (Bell and Jones 2014, 2015, 2018; Luo and Hodges 2016; O’Brien 2017).6 Most importantly, it has been shown that mixed-effects APC models intrinsically impose a silent constraint that the linear effect (trend) of one of the two variables that are specified as random is shrunken to near zero (O’Brien 2017, also see Luo and Hodges 2020).7 According to O’Brien (2017, 2598), the variable specified as random that is affected by shrinkage is the one that decreases the likelihood function the most if its linear effect is shrunk. However, insofar as a true linear cohort effect exists, the variable affected by shrinkage in the HAPC-CCREM specification will in this case most likely be cohort since the correlation between age and cohort will be much stronger than the correlation between age and period.8

Since linear APC effects are interdependent (Rodgers 1982), shrinking one of the effects will also affect the estimation of the other two effects. In the scenario of a true positive cohort effect, a true negative age effect, and a true positive period effect, a HAPC-CCREM model should shrink the positive cohort effect ($b_c - v$), inflate the negative age effect clockwise ($b_a - v$), and inflate the positive period effect ($b_p + v$) (see figure 2). Consistent with this, all of the studies of social tolerance of homosexuality using the HAPC-CCREM approach estimate a very strong positive trend in the period effects, a very strong negative age effect, but no trend in the cohort effects (Schwadel and Garneau 2014; Twenge, Carter, and Campbell 2015; Twenge, Sherman, and Wells 2016).

**Figure 2.** Expected misestimation of linear APC effects in mixed-effects APC models depending on the choice of fixed effect.
A peculiar variation of the HAPC-CCREM approach is employed by Pampel (2016), who instead specifies cohort as the fixed effect and age and period as the cross-classified random effects. While Pampel (2016) cites Yang and Land (2006), the latter have never advocated specifying cohort as the (single) fixed effect and Pampel provides no theoretical or empirical justification for this modification. Given that shrinkage of the random effects occurs, it is furthermore likely that this specification imposes a completely different constraint than the one imposed in the conventional HAPC-CCREM specification. Namely, in the scenario previously described, specifying cohort as the fixed effect should thus cause the model to shrink the negative age effect \((b_a + \nu)\), inflate the positive cohort effect \((b_c + \nu)\), and deflate the positive period effect \((b_p - \nu)\), thus effectively producing a mirror image of the results produced by the HAPC-CCREM model (see figure 2). In line with this argument, Pampel (2016) reports strong liberal trends in both the period and cohort effects but a negligible effect of age, results that stand in stark contrast to those reported by Twenge, Sherman, and Wells (2016) who basically analyze the same data but use the conventional HAPC-CCREM specification.

In summary, prior APC studies of American public opinion on homosexuality have produced widely different results, but they have also imposed widely different constraints in order to circumvent the identification problem inherent in APC analysis. As such, it is possible that the divergence in constraints explains the divergence in results. Before examining this possibility by fitting the various models used in earlier studies to empirical data, I will in the following two sections discuss how age, period, and cohort effects are likely to appear in the case of American public opinion on homosexuality. This discussion will help in evaluating the validity of constraints and the results they produce.

**Period Effects or Cohort Effects?**

In order to outline expectations in regard to period and cohort effects, it is necessary to consider whether susceptibility to attitude change is in some way conditioned by age. Namely, if susceptibility remains on a constant level across the individual lifespan, change-inducing events will influence all age groups equally and therefore show up as pure period effects. However, if susceptibility decreases sharply after a period of plasticity from about 18 to about 30 years of age, as the impressionable-years theory on political socialization holds (Sears 1975, Sears and Levy 2003), change-inducing events will also leave cohort effects in their wake, as cohorts being young at the time will be influenced the most. While no study to my knowledge has been conducted on how susceptibility to attitude change develops across the lifespan in regard to attitudes toward homosexuality, longitudinal research on other sociopolitical attitudes generally support the impressionable-years theory (e.g., Alwin, Cohen, and Newcomb 1991; Hatemi et al. 2009; Jennings and Markus 1984; Krosnick and Alwin 1989; Stoker and Jennings 2008). The theory is predicated on the notion that the individual typically leaves the parental home without a strong self-conception and that the early adult years are marked by a trial-and-error process of forming
a coherent social and political identity, a process during which the individual is assumed to be highly sensitive to environmental cues and open to attitude change (Sears 1975). However, once an identity has crystallized toward the end of early adulthood, that identity is, according to the theory, set to function as a lens through which information is processed and interpreted, thereby making attitude change less likely during middle and late adulthood (Sears and Levy 2003). Moreover, once an identity is crystallized, the individual is likely to start to self-select out of environments that are incongruent with that identity, which decreases the prospect of attitude change during later life stages even further (Caspi and Roberts 2001).

On the basis of the impressionable-years theory, it is thus reasonable to expect that both period and cohort effects have contributed to the increase in tolerance over the past three decades—contrary to the findings of studies that have taken the HAPC-CCREM approach (Schwadel and Garneau 2014; Twenge, Carter, and Campbell 2015; Twenge, Sherman, and Wells 2016). More precisely, it is reasonable to expect a liberal trend in period effects starting in 1969 (following the Stonewall Riots and the subsequent mobilization of the American gay movement) and a corresponding liberal trend in cohort effects starting with cohorts born in the early 1940s (cohorts that still were in their formative years during these events). However, it is also reasonable to expect a temporary interruption in the underlying period trend during the 1980s (following the outbreak of HIV/AIDS and the emergence of organized antigay movements) and, correspondingly, a temporary interruption in the cohort trend with cohorts born toward the end of the 1950s or in the first half of the 1960s.

**What about Age Effects?**

As already mentioned, a common constraint used in previous studies is that attitudes toward homosexuality are unrelated to age (e.g., Andersen and Fetner 2008; Baunach 2011). At first glance, that assumption is surprising since aging is commonly believed to induce social conservatism. However, direct evidence on how attitudes are affected by age is hard to come by. While it is easy to show that older adults hold more negative attitudes to homosexuality than young adults at any point in time, these age differences might of course reflect cohort effects. Nevertheless, there is some empirical evidence of more reliable nature that suggests a negative relationship between aging and social tolerance of homosexuality, at least during late adulthood.

The arguably most convincing evidence for a negative aging-tolerance relationship comes from research that has linked age-related cognitive decline to negative outgroup attitudes. Like any organ, the brain is in general negatively affected by aging during later life stages, and it is well documented that older adults tend to be impaired in cognitive tasks that require inhibition of information and executive control compared to younger adults (Salthouse and Miles 2002). This age-related decline in regulatory and inhibitory functions has in turn been found to mediate age differences in outgroup prejudice based on a variety of analytic techniques (e.g., Gonsalkorale, Sherman, and Klauer 2009;
Payne 2005; Radvansky, Copeland, and von Hippel 2010). For example, using functional magnetic resonance imaging, it has been shown that older adults with relatively well-preserved regulatory functions are able to suppress automatically triggered negative bias when presented with pictures of individuals belonging to stigmatized social groups, whereas older adults with impaired regulatory functions are not (Cassidy, Lee, and Krendl 2016; Krendl, Heatherton, and Kensinger 2009). In other words, even if neurological aging may not induce negative outgroup attitudes per se, neurological aging may reduce the ability to suppress negative bias already in place and therefore prompt more intolerant explicit attitudes (Gonsalkorale, Sherman, and Klauer 2009).

Evidence for a negative age-tolerance relationship also comes from longitudinal research on mean-level change in personality traits. Among personality traits, Openness to Experience is a powerful negative predictor of antigay attitudes (Shackelford and Besser 2007) and Openness has been found to decline during late adulthood (Roberts, Walton, and Viechtbauer 2006; Specht, Egloff, Schmukle 2011; Srivastava, Gosling, and Potter 2003). Similarly, Social Dominance Orientation, which is a powerful positive predictor of antigay attitudes (Pratto et al. 1994), has been found to increase across the individual lifespan (Roberts, Walton, and Viechtbauer 2006). Thus, assuming that personality is relatively insensitive to period effects, aging might induce more negative outgroup attitudes due to an increasingly “conservative” personality with age (Cornelis et al. 2009).

It is furthermore possible to conceive of a negative link between age and tolerance of homosexuality during late adulthood from a sociological perspective on aging. According to contact theory, intergroup contact is under some circumstances likely to reduce outgroup prejudice due, among other things, to gains in knowledge and reduced anxiety (Allport 1954). In line with contact theory, having friends or acquaintances that are homosexual is a powerful negative predictor of antigay attitudes (Herek and Capitanio 1996; Lewis and Gossett 2008). Since old age typically brings about some degree of social isolation, and since social selectivity tends to increase during adulthood (Carstensen 1995; Carstensen, Fung, and Charles 2003), it is conceivable that the frequency of meaningful contact with gays and lesbians decreases with age during later life stages for the average heterosexual person, and this might prompt more discriminatory attitudes.

By way of summary, there are theoretical considerations as well as empirical evidence that neurological, psychological, and sociological aging reduce social tolerance of homosexuality during late adulthood. Admittedly, the evidence for a negative age-tolerance relationship are fragmentary, and the effect is unlikely to be very strong, especially in the case of the rather crude attitudinal indicators that typically are included in long series of repeated cross-sectional data. Nevertheless, the assumption that age effects are completely absent, which has been made in some previous studies (e.g., Andersen and Fetner 2008; Ruel and Campbell 2006), is probably incorrect.
Data

In order to show in a systematic fashion that different constraints produce different results and that the divergent findings of prior studies do not simply reflect other differences of method (e.g., differences between the data sets and measures used), the various models employed by those studies are fitted to 26 pooled waves (1973–2016) of the GGS. While the GSS includes several questions tapping antigay attitudes, the measure most commonly used in prior research is the so-called homosex item (e.g., Pampel 2016; Twenge, Sherman, and Wells 2016). The question asked in this case is whether “sexual relations between two adults of the same sex is wrong or not,” scored: (1) always wrong, (2) almost always wrong, (3) only sometimes wrong, and (4) not wrong at all. This item (rescaled 0–1) will constitute the dependent variable, treated as continuous, in all analyses presented in the main body of the text. In order to keep the terminology in line with most prior studies, I will refer to this item as a measure of social tolerance of homosexuality. Additionally, analyses will be made using three binary items (combined into an additive index, rescaled 0–1) that ask the respondent whether homosexuals should be granted various civil rights or not. These items constitute the dependent variable in Ruel and Campbell (2006) and Schwadel and Garneau (2014). I will refer to this index as the civil rights index, and results from models using it as the dependent variable are presented in the online Supplementary Material.

The main independent variables of interest are of course age (18–89), period (1973–2016), and cohort (1892–1995). Age and period are measured as years of age and year of study, respectively. Following prior research (e.g., Schwadel and Garneau 2014), cohort is coded as five-year categorical units except for in one model, which uses the unmodified cohort variable. Additionally, in order to account for compositional differences across age, period, and cohort as well as to make models more comparable to those used in prior studies, covariates for education, sex, race, region of living, immigration background, marital status, and parent are included in all models. See Table 1 for descriptive statistics.

Statistical Models

Initially, the three different models used in prior studies will be fitted to the data. Model 1 uses the HAPC-CCREM specification (developed by Yang and Land 2006, 2008, 2013; used by e.g., Schwadel and Garneau 2014; Twenge, Sherman, and Wells 2016), in which period and cohort are specified as cross-classified random effects and age is specified as a fixed effect (with a linear and quadratic term). Model 1 can thus be written as

\[ Y_i = \beta_0 + \beta_1 A_i + \beta_2 A_i^2 + \delta Z + \kappa_c + \pi_p + \epsilon_i \]  

where \( Y_i \) is the social tolerance of homosexuality of individual \( i \), \( \beta_0 \) is the intercept (grand mean), \( \beta_1 \) and \( \beta_2 \) are the fixed effects for age and age-squared, \( \delta \) is a row vector of coefficients and \( Z \) is a column vector of variables for the additional
fixed effects (e.g., sex and race), $\kappa_c$ is the random effects for cohort ($C = 21$), $\pi_p$ is the random effects for period ($P = 26$), and $\varepsilon_i$ is the residual for respondent $i$. In other words, $\kappa_c$ represents the residual associated with cohort $c$, to which respondent $i$ belongs, and $\pi_p$ represents the residual associated with period $p$, at which respondent $i$ was observed.

Model 2 is identical to Model 1 save for the fact that the positions of age and cohort are switched so that age is the random effects (cross-classified with the random effects for period) and cohort is the fixed effects (specified as discrete). While this specification is not advocated by Yang and Land (2006, 2008, 2013), it is the specification used by Pampel (2016). Model 2 can be written as

$$Y_i = \beta_0 + \beta_1 C + \delta Z + \alpha_a + \pi_p + \varepsilon_i$$

where $\beta_1$ represents the fixed effects of cohort (a set of 20 dummy variables with associated coefficients), $\alpha_a$ is the random effects for age ($A = 71$), and $\pi_p$ again is the random effects for period.

In principle, both Model 1 and Model 2 are identified by the use of five-year categorical cohorts. However, as already mentioned, these two models should also impose the constraint that the linear effect of one of the variables specified as random is shrunk to near zero (O’Brien 2017). Given the expected age, period, and cohort effects, Model 1 should shrink the linear effect of cohort, whereas Model 2 should shrink the linear effect of age. Since slopes in an APC model are interdependent (Rogers 1982), Model 1 should in this case also overestimate the negative effect of age and the positive effect of period, whereas Model 2 should overestimate the positive effect of cohort but underestimate the positive effect of period. In other words, Model 1 and Model 2 should roughly be the mirror image of one another in regard to estimated age, period, and cohort effects (see figure 2).

Compared to the previous models, Model 3 is more straightforward: period and cohort are specified as two sets of dummy variables but terms for age are omitted. This model can be written as

$$Y_i = \beta_0 + \beta_1 C + \beta_2 P + \delta Z + \varepsilon_i$$

where $\beta_2 P$ represents the fixed effect for period (a set of 25 dummy variables with associated coefficients). This is the specification used by for example Andersen and Fetner (2008), and it imposes the constraint that no relationship exists between age and the dependent variable. Given the expected age, period, and cohort effects, this model should overestimate the positive effect of period but underestimate the positive effect of cohort. Furthermore, insofar that Model 2 shrinks the linear effect of age to near zero, Model 3 should produce very similar estimates for period and cohort effects as Model 2.

Two additional models, not used in prior research on attitudes to homosexuality, will be fitted to the data. In Model 4, age and cohort are specified as fixed effects, but period is specified as a random effect.

$$Y_i = \beta_0 + \beta_1 A_i + \beta_2 A_i^2 + \beta_3 C + \delta Z + \pi_p + \varepsilon_i$$
This kind of mixed effects model has been used elsewhere by Bell (2014) and should, given shrinkage of the random effects, estimate a much flatter period trend than the other models.

Model 5, finally, uses CCFEM (Yang and Land 2006, 2008, 2013) in which age, period, and cohort are all specified as fixed effects. However, instead of using the five-year categorical cohorts for identification, Model 5 uses a modified age variable that breaks down the age range into two groups: those aged 18–65 and those aged 66–89. This allows us to measure period and cohort as single birth and survey years, respectively. Model 5 can be written as

$$Y_i = \beta_0 + \beta_1 A + \beta_2 C + \beta_3 P + \delta Z + \varepsilon_i$$ (5)

where $\beta_1 A$ represents the modified age variable and its associated coefficient. This specification imposes the rather straightforward constraint that age effects
only manifest themselves as a difference between the elderly and younger age groups. While this assumption is unlikely to be exactly correct, there are reasons to believe—based on the previous discussion of age effects—that it is approximately correct and it should therefore produce approximately correct estimates of age, period, and cohort effects.  

The analyses were conducted by means of STATA 15 using ordinary least squares estimation for the fixed-effects models and full maximum likelihood estimation for the mixed effects models.

## Results

Starting with Model 1 (HAPC-CCREM) and Model 2 (mixed effects, cohort as fixed), estimated effects of age, period, and cohort from these models are presented in figures 3–5. As expected, the two models produce widely different estimates for age and cohort effects (compare with figure 2). Whereas Model 1 estimates no linear cohort effect but a very strong, negative age effect, Model 2 estimates a strong, positive cohort effect but no linear age effect. Thus, although differing only in the choice of effect specified as fixed, these two models tell completely opposite stories of how age and cohort are related to social tolerance of homosexuality. This explains why Twenge, Sherman, and Wells (2016), who use Model 1, and Pampel (2016), who uses Model 2, come to very different conclusions with regard to cohort effects using the same dependent variable as in this article.

The divergence between the results of Model 1 and Model 2 is a consequence of the shrinkage discussed by O’Brien (2017) and Luo and Hodges (2020): a

---

**Figure 3. Estimated period effects, Models 1–3.**

---
positive effect of cohort is shrunken and shifted to a negative effect of age in Model 1, whereas a negative effect of age is shrunken and shifted to a positive cohort effect in Model 2. This interpretation is supported by the fact that Model 3, which uses fixed effects but omits age terms, produces essentially the same estimates for cohort and period as Model 2 (see figures 3 and 4). Seeing
that results are unchanged if the civil rights index is used as the dependent variable (see figures S1–S3 in the online Supplementary Material), this explains why Schwadel and Garneau (2014), who use Model 1, and Ruel and Campbell (2006), who use Model 3, come to very different conclusions with regard to cohort effects when analyzing the same outcome variable.

Obviously, at least one of these three models produces very incorrect estimates. While Model 1 estimates the expected negative age-tolerance relationship, the estimated effect of age is arguably unrealistically strong and the model does, furthermore, not estimate the theoretically plausible positive trend in cohort. By contrast, Model 2 and Model 3 estimate the expected positive cohort trend but do not estimate (or account for, in the case of Model 3) the theoretically plausible negative age effect. It is noteworthy, however, that estimates of period effects are relatively similar across the models, even if Model 1, as expected (see figure 2), estimates a much steeper overall trend than the other models. According to each of these models, social tolerance of homosexuality increased, net of generational replacement, between 1973 and 1976; decreased between 1976 and 1990; and increased again between 1990 and 2016. Overall, the estimated period effects are in line with expectations.

Moving on to Model 4 (mixed effects, only period as random) and Model 5 (HAPC-CCFEM, age grouped), estimated effects of age, period, and cohort from these models are presented in figures 6–8. Evidently, these models also produce widely different parameter estimates. Most notably, Model 4 estimates a much flatter period trend than all other models. Again, this is a result of shrinkage of the random effects in mixed-effects APC models (Luo and Hodges 2020; O’Brien 2017). As a consequence, Model 4 also estimates a stronger positive cohort trend than the other models, as comparatively less temporal variation is assigned to period effects. Furthermore, in contrast to the other models, Model 4 estimates a positive age effect that levels off during late adulthood. This strange result is difficult to explain, but it shows just how sensitive APC analysis is to model specification. Model 5, which uses the modified age variable in order to enable identification, produces more theoretically plausible results, however. While the difference is small, Model 5 estimates a steeper period trend and a flatter cohort trend compared to Model 2 and Model 3. The reason for this is of course that Model 5 also estimates a weak but nevertheless meaningful negative effect of age. That is, once age differences are accounted for, differences across cohorts become smaller, which in turn assigns more temporal variation to period.

In summary, estimated effects for age, period, and cohort vary significantly across models. This is simply a result of different models imposing different constraints. Which of them that is most correct is not self-evident. In terms of model selection statistics, AIC and BIC give contractionary information, with the AIC favoring Model 3 and the BIC favoring Model 1 (see table 2). This is unsurprising, for while model selection statistics can discriminate between models on the basis of statistical parsimony and how accurately they estimate discrete effects of age, period, and cohort, such statistics cannot tell whether a model actually estimates the correct combinations of linear effects (Bell and Jones 2015, 5). The model that produces the most plausible estimates must
instead be selected on the basis of theoretical and empirical side information. Model 5 (HAPC-CCFEM, with age grouped) is the model that imposes the most theoretically informed constraint and is also the model that produces the most theoretically plausible results: a weak negative effect of age and strong positive
Figure 8. Estimated period effects, Models 4 and 5.

Table 2. Model Selection Statistics

| Model                  | AIC          | BIC          | Log-likelihood | df. | Observations |
|------------------------|--------------|--------------|----------------|-----|--------------|
| Model 1 (HAPC-CCREM)   | 32,564.97    | 32,768.16    | −16,258.48     | 24  | 35,114       |
| Model 2 (fixed effects, cohort as fixed) | 32,562.16 | 32,917.75 | −16,239.08 | 42  | 35,114       |
| Model 3 (mixed effects, no age terms) | 32,487.47 | 33,029.31 | −16,179.73 | 64  | 35,114       |
| Model 4 (mixed effects, only period as random) | 32,519.62 | 32,883.68 | −16,216.81 | 43  | 35,114       |
| Model 5 (HAPC-CCFEM, age grouped) | 32,559.74 | 33,812.76 | −16,131.87 | 148 | 35,114       |

Note: Entries correspond to models using the full set of covariates (see Table S1 in the online supplementary material).

effects of both period and cohort. In the end, inferences about linear effects of age, period, and cohort must nevertheless remain cautious.

Estimates of discrete (nonlinear) effects of age, period, and cohort are much less model dependent, however. For example, when the unmodified homosex item constitutes the dependent variable, all models estimate a statistically significant ($p < .05$) negative period effect in 2004. While speculative, this interruption of the underlying positive trend may reflect the strong Republican push for a constitutional banning of same-sex marriages during the period preceding the 2004 presidential election (Lewis 2005). Similarly, all models estimate cohorts
born 1942–1951 to deviate upwards from the underlying trend and cohorts born 1957–1966 to deviate downwards from the underlying trend. While these deviations are not statistically significant across the board, something is clearly different about these cohorts controlling for the linear trend in cohort effects and compositional differences in sociodemographic variables (e.g., sex, education, and marital status).23 These deviations align with expectations based on the impressionable-years theory: whereas the former cohorts grew up during the counterculture era and experienced the Stonewall riots as young adults, the latter cohorts came of age during the midst of the HIV/AIDS crisis and the generally more conservative 1980s. Insofar as these discrete cohort effects in fact are in fact results of early-life socialization, this constitutes evidence that susceptibility to attitude change declines during adulthood with respect to social tolerance of homosexuality. By extension, this suggests that Model 1 (HAPC-CCREM)—as well as previous studies taking the same approach (Schwadel and Garneau 2014; Twenge, Sherman, and Wells 2016)—underestimates the liberal trend in the cohort effects, and consequently, the impact generational replacement has had on shifting American public opinion on homosexuality over the past decades.

Conclusion

Shifts in public opinion often prompt the question of whether period or cohort effects account for the observed change and countless APC studies exist on a wide range of attitudes and behaviors. The question is important since its answer allows for a better understanding of how public opinion changes and even the ability to predict future change when cohort effects are present. However, the question is also notoriously difficult to answer confidently due to the fact that age, period, and cohort effects are inherently confounded, which forces the introduction of model constraints in order to ensure that the statistical model is identified. Prior APC research on change in American public opinion on homosexuality has overlooked this difficulty and paid too little attention to the fact that the results of such analyses will always be contingent on the particular constraints imposed in order to ensure identification.

In this article, I have reviewed the constraints that have been used in previous APC research on American public opinion on homosexuality (e.g., Andersen and Fetner 2008; Pampel 2016; Schwadel and Garneau 2014; Twenge, Sherman, and Wells 2016). I have argued that these constraints rest on unrealistic assumptions about the way age, period, and cohort effects play out in the case at hand. Fitting the various models employed by prior studies to 26 waves of pooled data from the GSS with two different indicators of social tolerance of homosexuality as the dependent variables, I also show that the conflicting findings of prior studies largely reflect differences in the constraints employed. This does not mean that the findings of previous work are necessarily wrong. But without any theoretical or empirical justification of the constraints imposed, they should be taken cautiously.
In future studies, researchers would do well to choose constraints based upon theory or prior knowledge about how at least one of the temporal variables relates to the given phenomenon and case under study as well as clearly state the constraints and the theoretical assumptions they translate into. With regard to future APC research of attitudes toward homosexuality, further advancement of research in cognitive aging in relation to inhibition of prejudice is in particular likely to help researchers in selecting identifying constraints.

**Supplementary Material**

Supplementary material is available at *Social Forces* online, [http://sf.oxfordjournals.org/](http://sf.oxfordjournals.org/).

**About the Author**

David Ekstam is a PhD student at the Department of Government at Uppsala University, Sweden. His research focuses on long-term change in outgroup attitudes, with a special interest in generational replacement, age effects, and the mechanisms that drive attitude formation on the individual level.

**Notes**

1. Age effects do not cause shifts in attitudes on the aggregate level as long as the age composition of the public is stable over time.
2. This holds exactly for the traditional fixed-effects APC model (i.e., Mason et al. 1973) and it holds approximately for the models discussed in this article (see Rodgers 1982).
3. Grouping of cohort years is also done by the studies that omit age terms (e.g., Andersen and Fetner 2008; Ruel and Campbell 2006).
4. According to Yang and Land (2008, 2013), HAPC-CCREM tends to produce better model fit since it estimates two variance components for period and cohort (which, respectively, represents the distribution of the random effects for period and cohort) rather than unique coefficients for each period and cohort unit. The HAPC-CCREM specification also allows for random variation associated with the individual cohort and period effects. If such variation exists, HAPC-CCREM will tend to produce more precise parameter estimates (smaller standard errors) than HAPC-CCFEM (Yang and Land 2013, 202).
5. Even if it is also possible to conceptualize age (life stages) as a contextual variable, a conventional age range cannot be considered as a sample drawn from an infinite population and is therefore, according to general guidelines in the multilevel methodology literature, more appropriately modeled as a fixed effect (Rabe-Hesketh and Skrondal 2012, 96–97).
6. The criticism of the HAPC-CCREM approach has in turn been challenged by other simulation studies (Reither et al. 2015a, 2015b).
7. According to O’Brien 2017, 2594, this occurs due to the fitting function of mixed-effects models, which jointly minimizes the discrepancy between observed and predicted values and the variance of the random effects.

8. Given a trend in the cohort effects, age and cohort will be correlated because older cohorts will have an on average higher age than newer cohorts in repeated cross-sectional data.

9. Inference is similarly ambiguous in panel data, since such data confound age effects with period effects.

10. Since 1975, the GSS is based on a nationally representative full-probability household sample. This means that persons living in large households have a lower probability of being included in the sample, and the GSS administrators therefore recommend employing sampling weights. It is however not clear how to model sampling weights in multilevel APC models (weighting is not used by e.g., Pampel 2016; Twenge, Sherman, and Wells 2016). After omitting observations due to the oversampling of blacks in surveys conducted 1982–1987, the analyses are therefore made without weights. Running nonhierarchical models with and without weights suggests that the impact is trivial, however. For more information about the GSS, see Smith et al. (2017).

11. The three binary items ask whether a male homosexual: (1) should be allowed to make public speeches in the respondent’s community or not, (2) should be allowed to teach at colleges or universities or not, and (3) should have the pro-gay book he has written removed from the respondent’s public library or not. The index consisting of these items has an acceptable alpha of 0.8 in the pooled data.

12. The cohort range in the raw data is 1883–1998 but is capped 1892–1995 due to few observations at the tail ends (n < 30). This cap does not substantially change the results.

13. The use of five-year cohort units alongside single-year age and period units does in itself impose a constraint in the statistical model that can affect results (Luo and Hodges 2016). All models are therefore also run with the unmodified cohort variable as well as with cohort measured as three-year units and as ten-year units. However, these modifications do not substantially alter cross-model differences. (for models using he unmodified cohort variable, see Figure S5, S6, and S7 in the online supplementary material).

14. Running models with only age, period, and cohort does not substantially alter the discrepancy across models showed in Result section in this article.

15. The grouping of age is not conventional practice but it follows essentially the same logic as the grouping of cohorts and it is consistent with the HAPC-CCFEM guidelines given by Yang and Land (2006, 2008, 2013).

16. Another way to impose the constraint that age effects are negative during late adulthood would be to use effect coding and the novel “s-constraint” method (O’Brien 2020).

17. Regression output from all models is presented in table S1 in the online Supplementary Material.
18. A fixed-effects model without cohort terms produces almost identical estimates for age effects as Model 1 (see figure S4 in the online Supplementary Material).

19. Averaged across all cohorts and time periods, tolerance of homosexuality decreases more than 70 percent across the adult lifespan according to Model 1.

20. The shrinkage of the random effects of period in Model 4 is especially evident when single-year cohorts are used or when the civil rights index constitutes the dependent variable (see figures S3 and S7 in the online Supplementary Material).

21. As pointed out by one reviewer, the disagreement between the AIC and the BIC is due to the large sample size \( n = 35,114 \) and the fact that the penalty applied in the BIC (but not in the AIC) increases with the sample size. The BIC consequently favors Model 1 (HAPC-C CREM), which is the smallest of the models in terms of degrees of freedom, whereas the AIC favors a slightly larger model (Model 3). However, this does not imply that Model 1 (or Model 3) more correctly estimates the true linear age, period, and cohort effects than any of the other models.

22. If Model 5 is correct, future generational replacement will continue to shift public opinion toward greater tolerance of homosexuality.

23. When the homosex item constitutes the dependent variable, cohorts born 1947–1951 rank significantly higher \((p < .1)\) in tolerance compared to cohorts born 1957–1961 according to Model 1 and Model 3. According to Model 5, cohorts born 1947, 1948, 1950, and 1951 rank significantly higher \((p < .1)\) in tolerance compared to cohorts born 1957 and 1958.

References

Allport, Gordon W. 1954. The Nature of Prejudice. Cambridge, MA: Addison-Wesley.

Alwin, Duane F., Ronald L. Cohen and Theodore M. Newcomb 1991. Political Attitudes over the Life Span: The Bennington Women After Fifty Years. Madison: University of Wisconsin Press.

Andersen, Ellen A. 2006. Out of the Closets and into the Courts: Legal Opportunity Structure and Gay Rights Litigation. Ann Arbor: University of Michigan Press.

Andersen, Robert and Tina Fetner 2008. “Cohort Differences in Tolerance of Homosexuality: Attitudinal Change in Canada and the United States, 1981–2000.” Public Opinion Quarterly 72:311–30.

Baunach, Dawn M. 2011. “Decomposing trends in Attitudes toward Gay Marriage, 1988–2006.” Social Science Quarterly 92:346–63.

Bell, Andrew 2014. “Life-Course and Cohort Trajectories of Mental Health in the UK, 1991–2008–A Multilevel Age–Period–Cohort Analysis.” Social Science & Medicine 120:21–30.

Bell, Andrew and Kelvyn Jones 2013. “The Impossibility of Separating Age, Period and Cohort Effects.” Social Science & Medicine 93:163–5.

Bell, Andrew and Kelvyn Jones 2014. “Another ‘Futile Quest’? A Simulation Study of Yang and Land’s Hierarchical Age-Period-Cohort Model.” Demographic Research 30:333–60.

Bell, Andrew and Kelvyn Jones 2015. “Should Age-Period-Cohort Analysts Accept Innovation without Scrutiny? A Response to Reither, Masters, Yang, Powers, Zheng and Land.” Social Science & Medicine 128:331–3.
Bell, Andrew and Kelvyn Jones 2018. “The Hierarchical Age-Period-Cohort Model: Why Does it Find the Results that it Finds?” Quality & Quantity 52:783–99.

Brewer, Paul R. 2003. “The Shifting Foundations of Public Opinion about Gay Rights.” Journal of Politics 65:1208–20.

Carstensen, Laura L. 1995. “Evidence for a Life-Span Theory of Socioemotional Selectivity.” Current Directions in Psychological Science 4:151–6.

Carstensen, Laura L., Helene H. Fung and Susan T. Charles 2003. “Socioemotional Selectivity Theory and the Regulation of Emotion in the Second Half of Life.” Motivation and Emotion 27:103–23.

Casp, Avshalom and Brent W. Roberts 2001. “Personality Development across the Life Course: The Argument for Change and Continuity.” Psychological Inquiry 12:49–66.

Cassidy, Brittany S., Eunice J. Lee and Anne C. Krendl 2016. “Age and Executive Ability Impact the Neural Correlates of Race Perception.” Social Cognition and Affective Neuroscience 11:1752–61.

Cornelis, Ilse, Alain Van Hiel, Arne Roets and Malgorzata Kossowska 2009. “Age Differences in Conservatism: Evidence on the Mediating Effects of Personality and Cognitive Style.” Journal of Personality 77:51–88.

Epstein, Steven 1999. “Gay and Lesbian Movements in the United States: Dilemmas of Identity, Diversity, and Political Strategy”. In The Global Emergence of Gay and Lesbian Politics: National Imprints of a World-Wide Movement, edited by Adam, B.D., Duyvendak, J.W., Krouwel, A., pp. 30–90. Philadelphia: Temple University Press.

Fetner, Tina 2008. How the Religious Right Shaped Lesbian and Gay Activism. Minneapolis: University of Minnesota Press.

Firebaugh, Glenn 1989. “Methods for Estimating Cohort Replacement Effects.” Sociological Methodology 19:243–62.

Glenn, Norval D. 2005. Cohort Analysis, Quantitative Applications in the Social Sciences. Beverly Hills: Sage Publications.

Gonsalkorale, Karen, Jeffrey W. Sherman and Karl C. Klauer 2009. “Aging and Prejudice: Diminished Regulation of Automatic Race Bias among Older Adults.” Journal of Experimental Social Psychology 45:410–4.

Hatem, Peter K., Carolyn L. Funk, Sarah E. Medland, Hermine M. Maes, Judy L. Silberg, Nicholas G. Martin and Lindon J. Eaves 2009. “Genetic and Environmental Transmission of Political Attitudes Over a Life Time.” Journal of Politics 71:1141–56.

Herek, Gregory M. and John P. Capitanio 1996. “Some of my best friends”: Intergroup contact, concealable stigma, and heterosexuals’ attitudes toward gay men and lesbians.” Personality and Social Psychology Bulletin 22:412–24.

Inglehart, Ronald F. and Christian Welzel 2005. Modernization, Cultural Change, and Democracy: The Human Development Sequence. New York: Cambridge University Press.

Jennings, M. Kent and Gregory B. Markus 1984. “Partisan Orientations over the Long Haul: Results from the Three-Wave Political Socialization Panel Study.” American Political Science Review 78:1000–18.

Keleher, Alison and Eric R. Smith 2012. “Growing Support for Gay and Lesbian Equality Since 1990.” Journal of Homosexuality 59:1307–26.

Krosnick, Jon A. and Duane F. Alwin 1989. “Aging and Susceptibility to Attitude Change.” Journal of Personality and Social Psychology 57:416–25.

Krendl, Anne C., Todd F. Heatherton and Elizabeth A. Kensinger 2009. “Aging Minds and Twisting Attitudes: An fMRI Investigation of Age Differences in Inhibiting Prejudice.” Psychology of Aging 24:530–41.

Lewis, Gregory B. 2005. “Same-Sex Marriage and the 2004 Presidential Election.” Political Science & Politics 38:195–9.
Lewis, Gregory B. and Charles W. Gossett 2008. “Changing Public Opinion on Same-Sex Marriage: The Case of California.” Political Policy 36:4–30.

Luo, Lying and James S. Hodges 2016. “Block Constraints in Age–Period–Cohort Models with Unequal-Width Intervals.” Sociological Methods & Research 45:700–26.

Luo, Lying and James S. Hodges 2020. “Constraints in Random Effects Age-Period-Cohort Models.” Sociological Methodology 50:276–317.

Marcus, Erik 1992. The Struggle for Lesbian and Gay Rights. New York: HarperCollins.

Mason, Karen O., William M. Mason, H.H. Winsborough and W. Kenneth Poole 1973. “Some Methodological Issues in Cohort Analysis of Archival Data.” American Sociological Review 38:242–58.

NORC. 2018. “General Social Survey”. Accessed January 5, 2018. www.gss.norc.org.

O’Brien, Robert M. 2017. “Mixed Models, Linear Dependency, and Identification in Age-Period-Cohort Models.” Statistics in Medicine 36:2590–600.

O’Brien, Robert M. 2020. “Using Old Results to Produce New Solutions in Age–Period–Cohort Multiple Classification Models.” Quality & Quantity 54:111–24.

Pampel, Fred C. 2016. “Cohort Changes in the Social Distribution of Tolerant Sexual Attitudes.” Social Forces 95:753–77.

Payne, B. Keith 2005. “Conceptualizing Control in Social Cognition: How Executive Functioning Modulates the Expression of Automatic Stereotyping.” Journal of Personality and Social Psychology 89:488–503.

Pratto, Felicia, Jim Sidanius, Lisa M. Stallworth and Bertram F. Malle 1994. “Social Dominance Orientation: A Personality Variable Predicting Social and Political Attitudes.” Journal of Personality and Social Psychology 67:741–63.

Rabe-Hesketh, Sophia, and Anders Skrondal. 2012. Multilevel and Longitudinal Modeling Using Stata. Vol. 1, Continuous Responses. College Station: Stata Press Publication.

Radvansky, Gabriel A., David E. Copeland and William von Hippel 2010. “Stereotype Activation, Inhibition, and Aging.” Journal of Experimental Social Psychology 46:51–60.

Reither, Eric N., Ryan K. Masters, Y. Claire Yang, Daniel A. Powers, Hui Zheng and Kenneth C. Land 2015. “Should Age-Period-Cohort Studies Return to the Methodologies of the 1970s?” Social Science & Medicine 128:356–65.

Reither, Eric N., Kenneth C. Land, Sun Y. Jeon, Daniel A. Powers, Ryan K. Masters, Hui Zheng, Melissa A. Hardy, Katherine M. Keyes, Qiang Fu, Heidi Hanson, Ken R. Smith, Rebecca L. Utz and Y. Claire Yang 2015. “Clarifying Hierarchical Age–Period–Cohort Models: A Rejoinder to Bell and Jones.” Social Science & Medicine 145:125–8.

Roberts, Brent W., Kate E. Walton and Wolfgang Viechtbauer 2006. “Patterns of Mean-Level Change in Personality Traits across the Life Course: A Meta-Analysis of Longitudinal Studies.” Psychological Bulletin 132:1–25.

Rodgers, Willard L. 1982. “Estimable Functions of Age, Period, and Cohort Effects.” American Sociological Review 47:774–87.

Ruel, Erin and Richard T. Campbell 2006. “Homophobia and HIV/AIDS: Attitude Change in the Face of an Epidemic.” Social Forces 84:2167–78.

Salthouse, Timothy A. and James D. Miles 2002. “Aging and Time-Sharing Aspects of Executive Control.” Memory and Cognition 30:572–82.

Schiappa, Edward, Peter B. Gregg and Dean E. Hewes 2006. “Can One TV Show Make a Difference? A Will & Grace and the Parasocial Contact Hypothesis.” Journal of Homosexuality 51:15–37.

Schwadel, Philip and Christopher R.H. Garneau 2014. “An Age–Period–Cohort Analysis of Political Tolerance in the United States.” Sociological Quarterly 55:421–52.
Sears, David O. 1975. “Political Socialization”. In Handbook of Political Science, edited by Greenstein, Fred I., Polsby, Nelson W., pp. 93–153. Reading: Addison-Wesley.

Sears, David O. and Sheri Levy 2003. “Childhood and Adult Political Development”. In Oxford Handbook of Political Psychology, edited by Sears, David O., Huddy, Leonie, Jervis, Robert, pp. 60–109. Oxford, England: Oxford University Press.

Shackelford, Todd K. and Avi Besser 2007. “Predicting Attitudes toward Homosexuality: Insights from Personality Psychology.” Individual Differences Research 5:106–44.

Sherkat, Darren E., Melissa Powell-Williams, Gregory Maddox and Kylan M. De Vries 2011. “Religion, Politics, and Support for Same-Sex Marriage in the United States, 1988–2008.” Social Science Research 40:167–80.

Smith, Tom W., Michael Davenport, Jeremy Freese and Michael Hout 2017. General Social Surveys, 1972–2016: Cumulative Codebook. NORC: University of Chicago.

Specht, Jule, Boris Egloff and Stefan C. Schmukle 2011. “Stability and Change of Personality across the Life Course: The Impact of Age and Major Life Events on Mean-Level and Rank-Order Stability of the Big Five.” Journal of Personality and Social Psychology 101:862–82.

Srivastava, Sanjay, Oliver P. John, Samuel D. Gosling and Jeff Potter 2003. “Development of Personality in Early and Middle Adulthood: Set like Plaster or Persistent Change?” Journal of Personality and Social Psychology 84:1041–53.

Stoker, Laura and M. Kent Jennings 2008. “Of Time and the Development of Partisan Polarization.” American Journal of Political Science 52:619–35.

Treas, Judith 2002. “How Cohorts, Education, and Ideology Shaped a New Sexual Revolution on American Attitudes toward Nonmarital Sex, 1972–1998.” Sociological Perspectives 45:267–83.

Twenge, Jean M., Nathan T. Carter and Keith W. Campbell 2015. “Time Period, Generational, and Age Differences in Tolerance for Controversial Beliefs and Lifestyles in the United States, 1972–2012.” Social Forces 94:379–99.

Twenge, Jean M., Ryne A. Sherman and Brooke E. Wells 2016. “Changes in American Adults’ Reported Same-Sex Sexual Experiences and Attitudes, 1973–2014.” Archives Sexual Behavior 45:1713–30.

Yang, Yang and Kenneth C. Land 2006. “A Mixed Models Approach to the Age-Period-Cohort Analysis of Repeated Cross-Section Surveys, with an Application to Data on Trends in Verbal Test Scores.” Sociological Methodology 36:75–97.

Yang, Yang and Kenneth C. Land 2008. “Age–Period–Cohort Analysis of Repeated Cross-Section Surveys: Fixed or Random Effects?” Sociological Methods & Research 36:297–326.

Yang, Yang and Kenneth C. Land 2013. Age-Period-Cohort Analysis: New Models, Methods, and Empirical Applications. Boca Raton: CRC Press.