

Quantifying Stability and Change in Personal Culture

Using Panel Data

Abstract

Does personal culture—a person’s attitudes, beliefs, values, and practices—change over the life course, or is it largely fixed by adulthood? Recent work has reached substantially different conclusions on this question. We argue that this disagreement is an unintended consequence of the “tournament of models” approach researchers have used. We introduce a new measure for quantifying the proportion of systematic variance in panel data attributable to intrapersonal change versus stable interpersonal differences. Applying this measure to 609 items from seven surveys in five countries, we find that although intrapersonal change is common, it is generally small in magnitude. We also demonstrate that this measure is useful for comparing social groups by showing that intrapersonal change is less common among U.S. college graduates than among those without a college degree. Overall, we believe this new measure will be a useful methodological tool and help reconcile an important theoretical debate.

Introduction

Does personal culture—a person’s attitudes, beliefs, values, and practices—change over the life course, or is it largely fixed by adulthood? This question underlies an important contemporary debate in sociology (Kiley and Vaisey 2020; Lersch 2023; Lizardo 2017) and has deep roots in seemingly contradictory theoretical perspectives. For example, pragmatist

theories of action claim that changes in social environments cause people to adapt their views and make new cultural meanings (Gross 2009; Swidler 2001), while Bourdieusian practice theories argue that the “past conditions of production” leave a mark on people’s personal culture that lasts throughout their lives (Bourdieu 1990). Models of social influence assume that people adapt their culture in the face of new information (Goldberg and Stein 2018), while the emphasis on cohort effects in models of aggregate social and cultural change requires them to be open to change while young but become fairly resistant to it as they age (Ryder 1965). Finally, life course theories posit important changes over time as people advance through important transitions in their lives (Bardi et al. 2009; Elder, Johnson, and Crosnoe 2003).

Because all of these perspectives have some empirical support, the current debate is not about whether the processes they posit exist at all, but about their relative contribution to explaining the cultural differences we see in the world. Nevertheless, researchers have struggled to reach a consensus on the importance of cultural change during adulthood. Over the span of a few years, most apparent changes in adults’ survey responses appear to be transitory, with little evidence of persistent change (Kiley and Vaisey 2020; Vaisey and Kiley 2021). This suggests that change during adulthood may not be a major factor in explaining contemporary cultural divides. By contrast, when we consider a longer time horizon, there is evidence that adults make at least some persistent changes (Lersch 2023).

These seemingly inconsistent findings are partly due to the fact that researchers have taken a “tournament of models” approach to adjudicate different theoretical perspectives (Lersch 2023: p. 228). That is, researchers have relied on model selection criteria—primarily the Bayesian Information Criterion (Raftery 1995)—to sort variables into the “no change” or “any change” piles. The size of these piles then serves as the primary evidence for the truth of the theory (Kiley and Vaisey 2020; Lersch 2023; Vaisey and Kiley 2021).

We argue, however, that asking *whether* people change on an item is an unintended source

of confusion. Being able to detect *any* change on an item depends not only on sample populations, measurement, statistical methods, and power, but also on definitions of what counts as “change.” These differences can limit the possibility of agreement on the assignment of a variable to the right pile. Moreover, asking whether there is evidence of change reduces to, “Is there *any* evidence of change in this item?” But such binary questions are ill-suited to address the core of the theoretical debate: determining *how much* the process of intrapersonal change contributes to cultural differences.

Of course neither pre-adult socialization nor the contemporary social context alone can explain the full range of cultural differences. The relevant question is about the *relative* contributions of these processes. In the current debate, for a single construct (e.g., support for gay rights), model comparison approaches can only help decide *whether* intrapersonal change is happening, not about how much. This also prevents investigating under what conditions, across which groups, and in what domains we might see differences in the the amount of intrapersonal change. These questions are about *degree*, not existence. Taking this debate forward therefore requires precise measurement rather than declarations of victory for a particular perspective.

In this paper, we move beyond the “tournament of models” approach and introduce a new method for quantifying the relative contributions of interpersonal differences and intrapersonal change for a single item. We use 609 items from seven panel surveys from five countries to quantify the proportion of variance in that is attributable to systematic intrapersonal change over the study period rather than to stable interpersonal differences.

We observe similar results across all datasets despite their varied social contexts and duration. In general, we find that intrapersonal change accounts for a small amount of variance in personal culture survey items. Of course, any question about whether a given amount of intrapersonal change is “meaningful” or “important” is a substantive and theoretical. Nevertheless, on many measures (some observed for over a decade), we do not see enough

change to believe that intrapersonal change processes play a substantial role in explaining the differences we observe between adults. Notable, interesting, and important exceptions exist, but they are departures from the overall pattern.

In addition to advancing a theoretical debate, our method can also help address specific substantive questions. To illustrate how it might be used by a researcher with a specific topical interest, we investigate differences in the relative importance of intrapersonal change for people with and without a college degree. We find a smaller amount of systematic change among college-educated respondents, suggesting that college crystallizes one’s personal culture rather than fostering an openness to new information. This offers a new empirical basis to theorize about the role of education in influencing personal culture.

Taken together, our goal in this paper is to advance—and, hopefully, transcend—debates about *whether* adults change. People do change, at least a little, on most things. Our method can help quantify exactly *how much*.

Background

Stability and Change in Personal Culture

Recent debates about whether adults undergo intrapersonal cultural change emerged in part because theories of cultural change at the aggregate level tend to implicitly invoke one of two models of individual behavior. The first, what Kiley and Vaisey (2020) call a “Settled Dispositions Model” (SDM), assumes that peoples’ personal culture is relatively fixed by the time they are adults. While they might make temporary changes in their declarative culture in reaction to their environments, this model assumes that people return to a settled baseline over a short period of time. This model underlies theories of cultural change that suggest people are imprinted by early socialization experiences such as the “past conditions of production” a Bourdieusian practice theory (Bourdieu 1990), cohort replacement theories

of aggregate change (Mannheim 1952; Ryder 1965), and control theories in social psychology (Robinson 2007; Smith-Lovin and Heise 1988).

The second model summarized by Kiley and Vaisey (2020), an “Active Updating Model” (AUM), posits that people continually update their personal culture as they move through life. This model suggests people change their personal culture as they adapt and make new meanings when encountering new social environments, discourses, and information (Gross 2009; Swidler 2001). This model underlies, among others, theories of cultural diffusion (Christakis and Fowler 2010), attitude alignment (DellaPosta 2020), and polarization (Bail et al. 2018). It is also implicit in most studies that ask whether specific experiences, changes in social roles, or political events, affect personal culture (Gelman and Margalit 2021; Slothuus and Bisgaard 2021; Visser and Mirabile 2004).

There is no reason to believe that only one of these two models is “correct” at all times and in all places. A population observed over some period of time contains a mix of people who are changing and people who are not. Instead, different perspectives argue that each of these ideal-typical models is more operative at different times, for different people, and for different elements of personal culture. For example, adolescence and early adulthood is typically viewed as a “formative period” for personal cultural development and thus characterized by higher rates of active updating, while middle age and later life are potentially characterized more by settled dispositions (Alwin and Krosnick 1991; Eaton et al. 2009; Krosnick and Alwin 1989). Similarly, salient issues, such as views around gay rights in the 2010s; issues that see substantial elite realignments, such as views around the Vietnam conflict in the 1970s (Zaller 1992); and novel issues of public opinion, such as views around vaccines during the Covid-19 pandemic (Scoville et al. 2022), might be characterized by active updating, while established issues of low salience are characterized by stability.

Empirically comparing which of these models better fit a broad range of questions from the General Social Survey’s rotating panels, Kiley and Vaisey (2020) found limited evidence of

durable change. While the majority of items did prefer the AUM, the amount of durable change detected on these items was small. A substantial minority of questions (39 percent) favored the SDM, meaning they were more consistent with zero durable change. Questions with more evidence of durable change included salient issues like gay marriage and questions tapping “public” statements or behaviors such as partisan identification and religious service attendance. There was also more evidence of durable change among early adults (people ages 18-30) than among the rest of the population. Overall, the researchers concluded that “results ultimately suggest that real, persistent attitude change is an uncommon phenomenon among adults” (Kiley and Vaisey 2020: p. 500; see also Vaisey and Kiley 2021). This lack of durable change is consistent with other recent findings that cohort replacement plays a somewhat larger role than period effects in explaining differences in personal culture (Vaisey and Lizardo 2016).

On the other hand, the claim that change is a relatively infrequent phenomenon has been difficult to square with research identifying durable change as a result of social experiences across a number of cultural dimensions, such as morality (Bročić and Miles 2021), trust (Mewes et al. 2021), and concerns about immigration (Kratz 2021). Similarly, there are longitudinal studies showing that cues from political elites can change an individual’s position on specific policy issues (Slothuus and Bisgaard 2021; Zaller 1992) and that changes in their close contacts and acquaintances can change individuals’ attitudes on group-related politics (DellaPosta 2018; Gelman and Margalit 2021). Given how often we observe people change their personal culture, it is hard to accept that adults do not change.

Drawing on these findings, Lersch (2023) challenged the SDM and AUM as a “needless dichotomy,” proposing the “Life Course Adaption Model” (LCAM) as an alternative. This model draws on the life course perspective to model personal culture as a different linear trajectory over the duration of a panel for each respondent. In doing this, Lersch rectified two shortcomings of the AUM and SDM. First, the AUM, as described by Kiley and Vaisey, posits that changes follow a Markov process where responses at time t are a function of

responses at time $t - 1$ but not earlier time points. However, earlier life experiences can (directly and indirectly) mold personal culture when transitioning to new social roles or into new environments, even if their initial impact is delayed. For example, childhood events might influence views on family structures later when individuals form their own families. Therefore, the LCAM considers influences from earlier than just $t - 1$ on responses at time t . Second, Kiley and Vaisey’s analysis of the AUM and SDM is based on three-wave panel data over four years, which might not be intensive or extensive enough to adjudicate the two models. Lersch evaluates the LCAM against the AUM and SDM using panel data spanning a wider duration (from 3 to 36 years) and more waves (3 to 18), offering a better chance to observe durable change.

When Lersch compared the LCAM to the AUM and SDM on data from five countries, 297 of the 428 questions he analyzed preferred the LCAM, suggesting that we observe some linear change for adults on most items. The SDM was preferred on 112 items, and the rest did not yield conclusive results. No questions favored the AUM. He concludes that “new experiences over the life course [...] can persistently move individuals’ personal culture in novel directions” (Lersch 2023: p. 243-244).

Despite differences in how these researchers interpret their findings, the empirical results are not far apart. Kiley and Vaisey (2020) found that the majority of items they tested favored the AUM, meaning there was evidence of durable change among adults, a pattern consistent with Lersch’s results. Also Lersch (2023) found evidence of durable change on most items, but these changes were small. On average, people changed only about .07 standard deviations over 10 years. This is consistent with Kiley and Vaisey’s finding that durable change is often small. Moreover, a quarter of the items Lersch studied still preferred the SDM, suggesting that even with different assumptions about change and more extensive data, many questions still are consistent with a model that allows zero intrapersonal change.

Other studies are compatible with these findings as well. For example, Brocic and Miles

(2021) estimate that completing graduate degrees in humanities, arts, and social sciences shift peoples' moral relativism only about .2 standard deviations on average compared to people with no college degree, and this is the largest effect they identify. And studies of aggregate change show that, even on items where cohort effects explain more variance than period effects, there is always evidence that some people change over time (Vaisey and Lizardo 2016). In other words, despite different interpretations, the results of previous work are in many ways highly consonant.

Quantification, Not Adjudication

To this point, the theoretical debate has been about whether people *ever* change. As we argued above, this is rarely the right question since the answer is almost always “yes.” Over sufficient time and in a large enough sample, researchers will likely observe some evidence for durable individual change. A lack of evidence for change may be due to a poor survey question, a low resolution in response options, or that the question simply was not asked for long enough or to enough people. Conversely, finding evidence for some change tells us only that it was “not zero.” From that alone, we learn little about how much intrapersonal change has happened in a population.

A more theoretically productive approach begins with a model assuming that during an observed time period people might remain stable, might change a little, or might undergo significant shifts in their personal culture. Lersch's LCAM does this by modeling each individual as following a personal linear trajectory. But rather than debating whether this model fits a particular data set better than a model that assumes that people never change, it would be better to quantify these individual changes and compare them to the stable differences that exist between people. In other words, the focus should shift from asking, “do people change?” to asking, “what are the relative contributions of change and pre-existing differences for explaining variance in personal culture?”

We propose an approach—which we formalize below—that offers such a quantification. Our measure relates two variance components that each reflect a combination of relevant theoretical processes. The first component, stable interpersonal differences, reflects the accumulated experiences of people prior to entering the panel survey. Lersch (2023: 24) calls this “early imprinting.” While commonly associated with experiences during a formative period that result in settled dispositions, this variation could also reflect experiences that happened at any time as long as they predate the panel and consistently affect subsequent responses. For instance, for those people entering the panel post-retirement, this “imprinting” might reflect this pivotal life transition. Consequently, this component also reflects variation in individuals’ social roles or statuses at the start of the panel that were important in shaping their dispositions.

The second component, the amount of intrapersonal linear change people make during the panel, captures durable changes in personal culture over time. This captures the set of processes collectively called “persistent change” or “adaption” by Lersch and “active updating” by Kiley and Vaisey. Lersch attributes these changes to social triggers such as moving into a new environment or adopting new social roles (although he does not measure these directly). They might also reflect the diffusion of new cultural forms across social networks, cues from political elites or otherwise culturally influential leaders, the emergence of issues in politics or culture, or large-scale social shifts.

A third component, which we might call “residual variance” or “fluctuation,” accounts for the remainder of variance in peoples’ responses. These non-durable changes emerge for a variety of reasons. For example, people might not have a clear disposition on a particular item as it is asked. Instead, they might internalize a broad set of considerations and construct an opinion in the context of the survey interview, with different considerations coming to the forefront of their cognition during each interview (Feldman and Zaller 1992; Tourangeau, Rips, and Rasinski 2000; Zaller 1992). This variance can also include measurement error, such as misinterpreted questions, erroneous response selections, or responses getting coded

incorrectly. Although this third component at least partially reflects important processes of personal culture, it does not directly touch on the ongoing debate outlined here. Because of this, we quantify this component but focus principally on the other two.

At a theoretical level, the coexistence of the sets of processes captured in the two main components, interpersonal differences and intrapersonal change, is undeniable, and they are linked in many ways. Any intrapersonal change during one's life will likely manifest as interpersonal differences by the time people enter a panel survey. Furthermore, unless people are entirely socialized early on and never deviate from these dispositions, we expect observing intrapersonal change in a segment of the population when surveyed over time.

Furthermore, these components are not necessarily useful in isolation or without context. What is important for the theoretical debate is the relative contribution of these two components in explaining cultural differences in a population. In other words, when we look at a population of adults over some time frame, is there enough intrapersonal change relative to interpersonal difference to view it as an important process for why people differ from each other? Only through quantifying the two components we can get closer to a true answer to this question.

Yet the true utility of the proposed quantification goes beyond settling past debates; it allows researchers to ask new questions. For example, classifying survey items based on whether they show durable change overlooks possibly important differences among those that do show such change. By quantifying the relative contributions of interpersonal differences and intrapersonal change, researchers can gauge their relative importance in explaining differences among survey items, between groups within a given population, across time, and across societies.

Expectations

By shifting the question and adjusting the approach, we expect consistent patterns across datasets. First, because previous work found evidence of durable change on most measures

of personal culture, we expect that we will find some intrapersonal change in most items. But because these earlier studies also found many questions (between 25 and 35 percent) lacking evidence of *any* durable change, we expect considerable variation in the proportion of variance accounted for by intrapersonal change, with some questions indicating essentially none at all.

Second, we expect the variance attributable to intrapersonal change to be modest compared to pre-existing interpersonal differences. This expectation should be uncontroversial, regardless of the theoretical process assumed to be most relevant for a particular question. Panel surveys do not encompass individuals' entire lifetimes. Thus, even if intrapersonal change during adulthood is the major driver of cultural differences, the change detected in a panel covering 20 years probably will not fully account for the entire variance of interpersonal differences.

Our expectations so far largely echo what was observed in previous studies. However, theoretical perspectives differ in how much change they expect, enabling quantification to provide a new perspective on existing questions. "Formative period" and cohort theories, which emphasize early-life socialization as a strong influence on adult personal culture, imply low amounts of intrapersonal change, even when we account for the duration of observation. Conversely, other theoretical perspectives emphasizing the importance of people's contemporaneous social environment might expect a larger degree of intrapersonal change, even in a relatively short panel.

Third, we expect the variance explained by each process—interpersonal differences and intrapersonal change—to depend on specific attributes of the question and panel. First, the higher the response resolution for a given question and the larger the sample, the greater the variance attributed to intrapersonal change should be, as smaller changes in intrapersonal change will be easier to detect. Second, life course theories posit that social transitions drive changes in personal culture. Since the probability that any person experiences such a transition goes up the longer we observe them, we expect panel duration to be positively

related to the proportion of variation explained by intrapersonal change.

Analytic Strategy

Data

We use data from seven nationally representative panel surveys from Australia, Germany, Great Britain, Switzerland, and the United States (summarized in Table 1), combining all the data files used in previous work (Kiley and Vaisey 2020; Lersch 2023).¹ These studies cover a long period of time (the range of surveys spans from 1968 to 2021), with 609 personal culture items capturing attitudes, beliefs, values, self-assessments, self-descriptions, and behaviors (Alwin 2007). We restricted the sample such that individuals between the ages of 18 and 79 are included without further elimination, and in all surveys, we used all possible cases for which respondents provided responses. In the end, the analyses that follow rely on a cross-national sample with a cross-domain set of items to capture a broad range of individual personal culture. Supplemental Materials A documents the list of all variables used in the upcoming analyses.

Table 1: The Description of the Data Sources

Country	Survey	Period	Outcomes
Australia	Household, Income and Labor Dynamics in Australia (HILDA)	2001-2021	30
Germany	Socio-Economic Panel Study (SOEP)	1984-2020	122
Great Britian	British Household Panel Survey (BHPS)	1991-2008	83
Great Britian	Understanding Society/UK Household Longitudinal Study (UKHLS)	2009-2020	66
Switzerland	Swiss Household Panel (SHP)	1999-2019	77
United States	General Social Survey (GSS)	2006-2012	183
United States	Panel Study of Income Dynamics (PSID)	1968-2019	48

Life Course Adaption Model

The estimation procedure begins with with Lersch’s (2023) Life Course Adaption Model, which formalizes survey responses at time t as a function of individual-level random intercepts and slopes for survey age. In some contexts, this model is called a mixed-effects growth curve model. This model assumes a set of propositions about change that reflect the theoretical debate to this point. First, consistent with the settled disposition model, it assumes that people start the survey with cultural differences, modeled as random intercepts for each respondent. Second, it assumes that people change over time, taking the form of random slopes for each respondent as a linear function of time. Third, it assumes that people deviate around this baseline randomly over time, reflecting “fluctuation” or short-term non-persistent change. Formally, this can be written as

$$y_{it} = \beta_0 + \alpha_{0i} + (\beta_1 + \alpha_{1i})\text{year}_{it} + \epsilon_{it}$$

$$\alpha_{0i} \sim \mathcal{N}(0, \tau_0^2)$$

$$\alpha_{1i} \sim \mathcal{N}(0, \tau_1^2)$$

$$\epsilon_{it} \sim \mathcal{N}(0, \sigma^2)$$

where β_0 is the average intercept, α_{0i} is the random intercept component for individuals, β_1 is the average yearly change in the outcome, α_{1i} is the random slope component for individuals, and ϵ_{it} is the random error term that captures transitory disturbances. The α_{0i} and α_{1i} terms are also allowed to covary.

Analysis Steps

As a first step in our analysis, we fit this LCAM to each of the 609 measures of personal culture outlined above. We then use these models to derive two measures of the components

we outlined above: pre-existing interpersonal differences and intrapersonal change over time.

We estimate these using a form of variance decomposition. First, to measure stable interpersonal differences, we calculate $V(D)$ according to the equation below, where \widetilde{year}_i is the midpoint of the observed years for each respondent for that particular item.²

$$V(D) = 1 - \frac{\sum_{i=1}^N \sum_{t=1}^T [y_{it} - (\hat{\beta}_0 + \hat{\alpha}_{0i} + (\hat{\beta}_1 + \hat{\alpha}_{1i})\widetilde{year}_i)]^2}{\sum_{i=1}^N \sum_{t=1}^T [y_{it} - \bar{y}_{it}]^2}$$

This gives us the proportion of total variance in an item attributable to stable interpersonal differences.

To calculate the second component, which is the variance attributable to systematic intrapersonal change, we calculate $V(C)$ as follows:

$$V(C) = 1 - \frac{\sum_{i=1}^N \sum_{t=1}^T [y_{it} - (\hat{\beta}_0 + \hat{\alpha}_{0i} + (\hat{\beta}_1 + \hat{\alpha}_{1i})year_{it})]^2}{\sum_{i=1}^N \sum_{t=1}^T [y_{it} - \bar{y}_{it}]^2} - V(D)$$

This is the incremental proportion of variance accounted for when we allow the model predictions to change over time for each person.

The third component, which is the proportion of variance attributable to measurement error or transient fluctuations, is what's left, such that these three numbers sum to 1. As we discussed above, this component is not of core interest here.

Together, $V(D)$ and $V(C)$ reflect the total systematic variance of the outcome that the LCAM can attribute to either differences between people or linear intrapersonal change over the course of the panel. Our principal measure of interest is the proportion of systematic variance accounted for by intrapersonal change, which we calculate as

$$\omega = \frac{V(C)}{V(C) + V(D)}$$

We can think of ω as a type of Intraclass Correlation Coefficient focused only on the systematic

components of the model while ignoring residual variance that might be attributable to measurement error or non-persisting change (see also the variance decomposition of period and cohort by Vaisey and Lizardo 2016). The higher this proportion is, the more systematic variance in responses is attributable to intrapersonal change during the panel. From now on, we use ω to stand for *the proportion of systematic variance attributable to intrapersonal change*.

Because this quotient summarizes variance proportions and therefore has no natural referent, it is hard to say at which values we observe “a lot” of intrapersonal change. But summarizing questions in this way allows us to compare the relative importance of our two broad processes across a range of questions that might have different levels of non-systematic variance. This gives us a sense of the relative prevalence of these two processes for each question, which is not achievable under the tournament of models approach. This also allows us to compare which kinds of questions show more or less intrapersonal change relative to interpersonal differences. While the tournament of models approach allowed researchers to say whether questions in general showed *any* evidence of updating, this approach allows us to make more specific claims about the prevalence of these theoretical processes for particular questions relative to each other.

As a second step in our analysis, we use linear regression to conduct a meta-analysis, modeling ω as a function of features of the question and panel. This allows us to explore how these features are associated with the degree of intrapersonal change. These covariates include the number of response options (measured as 2, 3-5, and more than 5), the specific survey, the log of the number of participants, the number of waves observed, the time period (i.e., decade) the question was first asked, and the total duration of time the question was asked.

Results

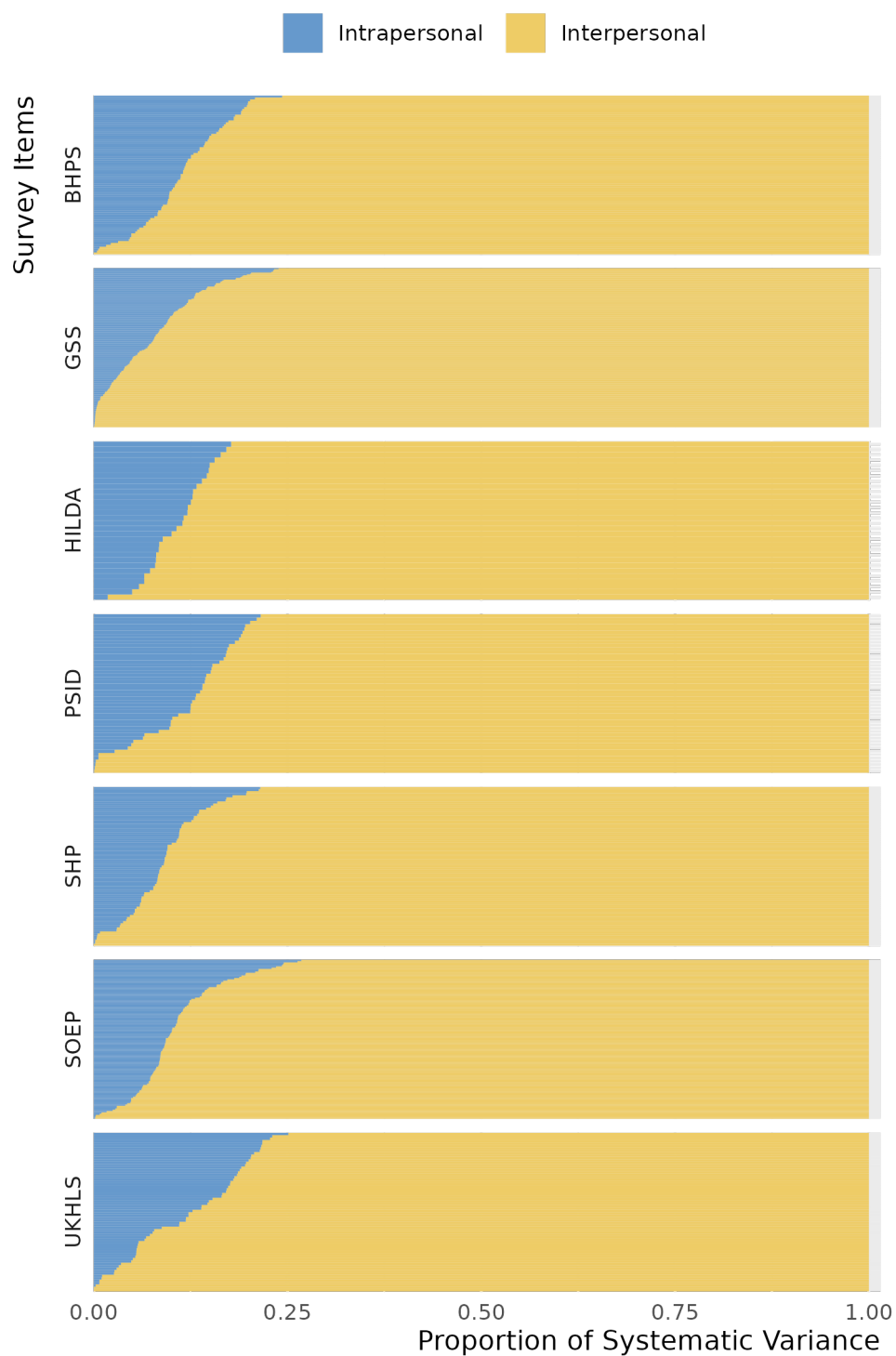
Variance Decomposition

Figure 1 plots the proportion of systematic variance attributable to interpersonal differences and intrapersonal change for each of the 609 questions, plotted separately by panel. To broadly summarize the results, all panels show a similar range of the proportion of systematic variance attributable to intrapersonal change. Across panels, the median value of ω is 0.094 (mean of 0.098), with an interquartile range of 0.053 to 0.139. All panels include questions where ω is essentially 0, and all panels include questions with ω values greater than 0.20. The maximum ω value is 0.26.

Across all questions, interpersonal differences account for a much larger share of the systematic variance in responses than intrapersonal change. Again, this is to be expected. Interpersonal differences capture not just pre-adult socialization, but all accumulated experiences up to the start of the panel that might influence personal culture.

To the extent that there are differences across the panels, the PSID has the highest ω values with mean .120 and median .135. While we cannot disentangle features of the sample from features of the questions asked to each sample, the specific samples for many PSID questions have lower average ages than those from other panels. To the extent that younger respondents might be more likely to make durable changes of opinion, these higher estimates of intrapersonal change might reflect the distinct age profile of respondents in this sample. At the other end, the GSS has the lowest range of ω with mean .073 and median .069. This potentially reflects the fact that the GSS observes people for a shorter duration, on average, than the other panels. If, consistent with life course adaption theories, people are more likely to make significant cultural changes the longer we observe them, then duration likely affects the range of ω (a point we explore more below). However, the GSS results are still consistent with results from the other panels.

Figure 1: Proportions of Systematic Variance in Personal Culture



Notes: The figure shows ω and $1 - \omega$ as the proportion of systematic variance attributable to intrapersonal change and interpersonal differences. See Supplemental Materials A for the full set of item values.

While there are some differences between panels, these differences are small compared to the differences within panels. For about 6 percent of items, ω is greater than 0.20. These questions tend to ask about objectively changing external referents (e.g., confidence in specific government leaders or political parties), life satisfaction, or current financial position. At the other end, questions about religious identification, views on gender roles, and support for civil liberties tend to have very low estimates of intrapersonal change.

In contrast to the tournament of models approach, quantifying change this way allows us to explore variation in the relative importance of intrapersonal change across questions that all show evidence of change. For example, Kiley and Vaisey (2020) found that confidence in the press and confidence in religious leaders were both characterized by active updating. Our results show that intrapersonal change is much more important for explaining variance in confidence in the press (0.164) than confidence in religion (0.049), even though both are updating.

Appendix A shows the distribution of $V(D)$ and $V(C)$ across panels, and Supplemental Materials A presents the estimated proportion of variance attributable to interpersonal differences and intrapersonal change, the estimated values of ω , and the proportion of residual variance for each question. Interpersonal differences are almost always the largest component of the total variance and tend to account for between 55 and 70 percent of total variance, while intrapersonal change is always the smallest, typically accounting for between 3 and 8 percent of total variance. Residual variance tends to account for between 22 and 37 percent of variance, though on several questions residual variance is greater than 50 percent. This might indicate survey items with low reliability or ones that capture genuinely rapid fluctuations.

As we noted above, the substantive importance of a given amount of intrapersonal change depends on a range of factors, including how long the panel runs, whether assumptions about linear change hold, and whether the panel is capturing a distinctly turbulent period or a distinctly stable period for the relevant item. However, if we assume that the period under

observation is “typical” for a question—not a time of extremely heightened (or lowered) sensitivity or change—then it does not seem realistic for that intrapersonal change accounts for a large share of the cultural differences we see in the world.

Meta-Analysis

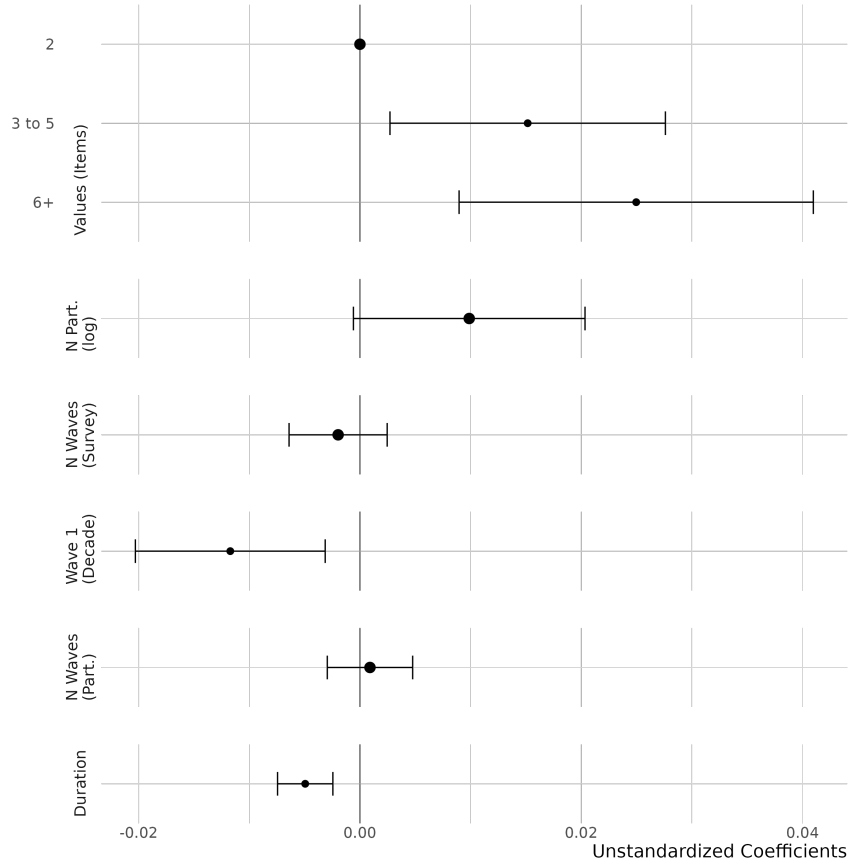
Figure 2 plots the results from a linear regression of ω as a function of question, panel, and sample features. These models also include fixed effects for panels and topics, so coefficients reflect the association within a panel and topical domain.

Figure 2 shows that the more response options respondents are given and the larger the sample, the larger the values of ω . We interpret these coefficients as suggesting greater resolution on a question makes it easier to detect and model change. The earlier a question was asked, even net of how long it was been asked, the lower ω .

Although we did not state expectations for how question content would relate to ω , some of the associations between question structure, panel duration, and panels themselves might be driven by differences in the topics addressed by each panel. To address this, we followed Hout et al. (2016) and Lersch (2023) in coding each question as falling into one of nine different content categories and included these as indicator variables in the meta analysis. Supplemental Materials B presents the estimates for these content indicators. They show some associations with ω , with questions about subjective SES; social life, social cohesion, and trust; environment and climate; and health and morale showing larger ω values than questions about religion and spirituality; politics, government and the economy; and gender and family.

The most notable result in the meta analysis is the negative coefficient attached to the duration of years covered by the question. The longer a question is observed, the less is the value of ω . Theories of personal cultural change that link changes in personal culture to social experiences, including the LCAM, suggest that the longer we observe respondents, the

Figure 2: Regression Model Estimating ω



Notes: The model estimates the proportion of systematic variance attributable to intrapersonal change. It includes the log of the number of participants, the number of waves the variable was asked (capped at $t = 10$ for each item), the date of the first wave (measured as the year minus 1968 divided by 10), number of waves the question was asked averaged across participants, duration in years per item averaged across participants, and panel and topic fixed effects. Coefficients are estimated from suppressed intercept model based on predictions at participants' wave mid-point. Survey indicators and item topics not shown. See Supplemental Materials B for the full set of coefficients.

more likely people are to undergo potentially transformative experiences and therefore the more variance would be attributable to intrapersonal change. Finding a negative coefficient here seems to challenge that assumption.

As a further test of this finding, we compared the ω values when using the full duration of a panel compared to when we dropped the final wave and therefore reduced the total duration of observation for the question. If the coefficient reflects a true negative effect of duration on ω , we should see that same effect within questions. Results from this analysis are presented in Appendix B and contradict the coefficient from the regression model; with a few exceptions, we found that the longer we observe the same question, the higher ω is. We interpret this combination of findings as suggesting that the kinds of questions asked for a longer time period tend to demonstrate less intrapersonal change than questions asked over shorter periods, rather than a true function of time. This supports the conclusion that the GSS shows less intrapersonal change because of its shorter duration.

College and Change in Political Culture

These results mostly re-frame and align previous findings, but the value of our approach lies in its ability to extend the debate to a broader set of theoretical questions. To show this potential, we turn to a specific empirical example: the relative importance of intrapersonal change and interpersonal difference for explaining cultural variation by level of education.

Previous work has established a positive relationship between education and attitude stability, especially on issues related to American politics. This stability is often attributed to education facilitating “chronic information” – a general understanding of and attention to the domain of American politics, including the positions held by major parties and political figures and how issues relate to one another at a logical or socio-logical level (Alvarez and Brehm 2002; Boutyline and Vaisey 2017; Zaller 1992). These perspectives argue that because college graduates have more knowledge of American politics, they are better able to consistently

connect the considerations in their cognition with the answer choices they are presented with in a survey.

This work has tended to focus on the fact that college-educated Americans give responses that are less likely to be affected by measurement error or short-term influences than the rest of the population (Alwin 2007; Zaller 1992). But this focus on the non-systematic or residual component of variance across groups lumps the two systematic forms of difference together. That is, it obscures the fact that the amount of interpersonal differences and intrapersonal change might also differ across these groups.

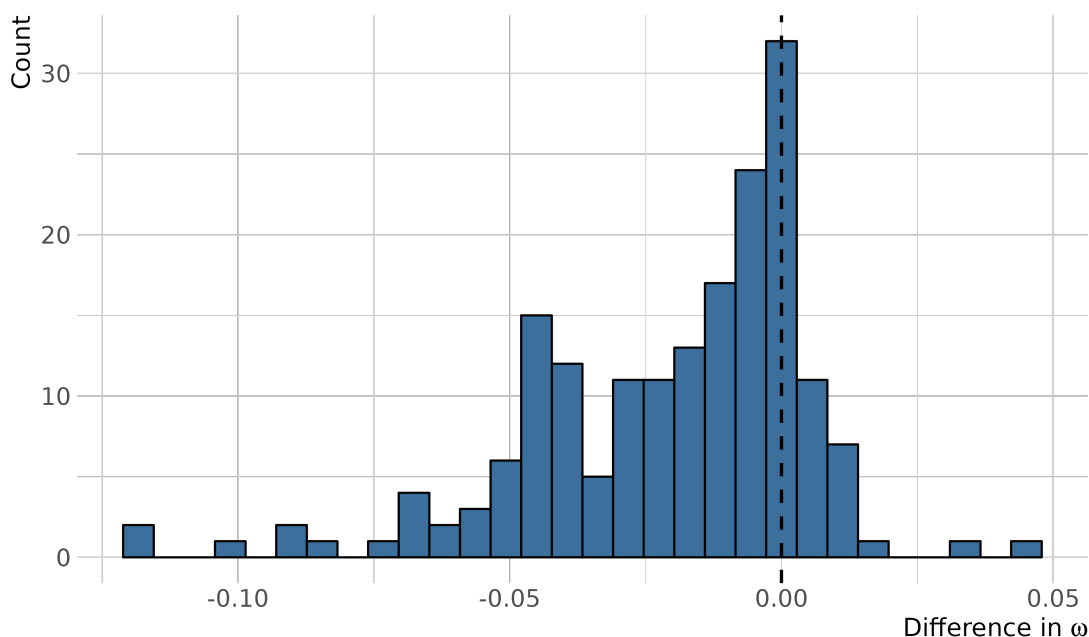
There are theoretical reasons to believe that education might be associated with either more or less intrapersonal change. On one hand, because college graduates are more connected to mainstream discourse and elite signals, they might be more likely to make durable changes in response to the emergence of new information, new issues, or political realignments, while those without chronic information might display more variance around an unchanging baseline (Zaller 1992). Conversely, it could be that the observed stability of the college educated reflects the fact that they have already formed durable opinions and are relatively closed off to new information. If this is true, college could be understood as a formative experience that solidifies some dimensions of personal culture. Perhaps for those who do not attend college, later life experiences might prove more important in forming or changing personal culture, as these experiences potentially provide information that college-educated peers have already received.

To compare these competing propositions, we calculate ω values separately for people with at least a bachelor's degree and people with less than a bachelor's degree at wave 1³ of the three General Social Survey's panels. We focus on the GSS because it contains the largest number of questions tapping general political dispositions, which is the domain where education has proven particularly relevant for understanding attitude stability. The GSS also covers a turbulent window of American politics from 2006 to 2014. This window covers the start of

the Great Recession, debates about federal intervention in and regulation of Wall Street, the election of Barack Obama as the first black U.S. president, debates about the role of the federal government in the health care sector, the emergence of the Tea Party, and political realignment and clarification on the issue of gay marriage, among other topics.

Figure 3 plots the distribution of differences in ω values between people with at least a bachelor's degree and people with less than a bachelor's degree at wave 1 of the panel for 183 GSS items. Values greater than 0 indicate that intrapersonal change accounts for more systematic variance among college graduates than among those without a college degree, while values less than 0 indicate the opposite.

Figure 3: Difference in ω Across College Graduates and Non-Graduates



Notes: The figure shows the difference in ω values across college graduates and college non-graduates. Values above (below) 0 means that those with college degree have higher (lower) variance of intrapersonal change. The dashed red line marks 0 difference.

There is a clear pattern in Figure 3: for more than 80 percent of these GSS items, intrapersonal change is a larger component of systematic variance for people without a college degree.

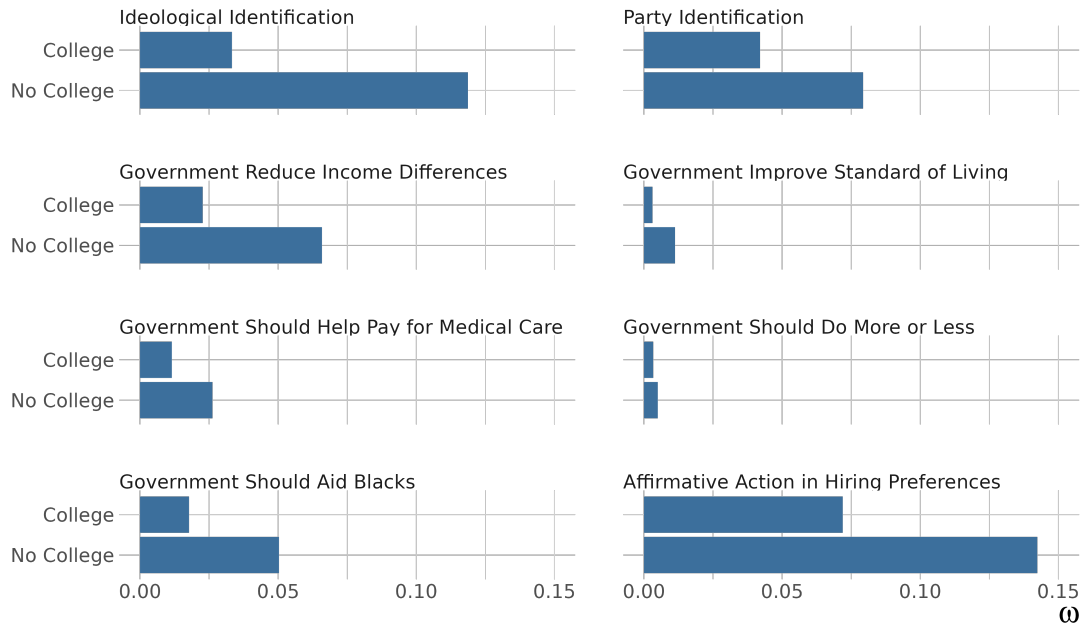
While most of these differences are small in absolute terms (less than 2 percentage points), several are greater than 5 percentage points. Given the distribution observed in Figure 1 showing that the systematic variance attributable to intrapersonal change averages around 0.09, a 5 percentage point difference between groups is quite substantial.

To more clearly illustrate some of these differences, we highlight eight questions designed to tap general political dispositions: partisan identification (Democrat vs. Republican) and ideological identification (liberal vs. conservative) on seven-point scales; four questions about the government’s role in improving the condition of the poor, paying people’s medical bills, giving special treatment to black people, and doing things that private businesses could do, measured on five-point scales; a question about whether the government should do more to reduce income differences, measured on a seven-point scale; and one question about whether black people should be given preferences in hiring, measured on a five-point scale. We present estimates of ω for these eight questions, for both education groups, in Figure 4.

On all eight questions presented in Figure 4, ω values are smaller for people with a college degree, meaning intrapersonal change is less common among them. There are also large differences in ω values across questions for both groups. For example, on the question of whether the government should try to solve more problems or leave those problems to be solved by private businesses (“government do more or less”), less than one percent of the systematic variance is attributable to intrapersonal change for both groups. In other words, while people might vacillate on this question at random (37 percent of variance is residual for this question), there is functionally no evidence that people make systematic changes of opinion on this issue during the GSS panel.

In contrast, partisan identification and political ideology both show larger values of ω than most other questions, as well as a larger absolute difference by degree status. Compared to the other questions, intrapersonal change plays a much larger role in accounting for partisan identification and political ideology. And this is particularly true for respondents who do not

Figure 4: Difference in ω Across College Graduates and Non-Graduates on Political Culture



Notes: ω values across college graduates and college non-graduates on 8 political culture items from the General Social Survey (2006-2014).

have a college degree; the ω value for ideological identification among non-college educated respondents is almost four times that of college-educated respondents.

It is worth pointing out that this meaningful difference in ω values across education groups and across questions would not have been detectable using previous methods. For partisan identification and political ideology, both college-educated respondents and people with less than college degree would likely favor the AUM or LCAM over the SDM because these questions both show evidence of some members of the population making *some* intrapersonal change. In other words, the tournament of models obscures the fact that the relative importance of intrapersonal change differs across these two groups, that intrapersonal change explains more systematic variance for ideological identification and partisan identification for people without a college degree, and that there appears to be more durable change on questions of affirmative action than on questions of government aid to black Americans.

We believe these patterns shed new light on the mechanisms underlying differences in attitudes and behavior across groups. Something about college attendance seems to crystallize personal culture. While these results should not be interpreted as causal effects of attending college – they are potentially confounded by age, social class, race, gender, and other factors that explain selection into higher education – they open up a set of new questions and dynamics to explore.

Discussion and Conclusion

The objective of this paper is to intervene in the recent debate on whether people change their personal culture – their attitudes, beliefs, values, practices, and dispositions – as they move through their adult life. Instead of falling back on a “tournament of models” approach (Lersch 2023: p. 228) to proclaim victory for a particular answer to this question, our goal is to refocus the debate to the theoretically more productive question of the relative importance of intrapersonal change for explaining differences in the personal culture. We propose a new approach that quantifies the amount of observed systematic variance that is attributable to either interpersonal differences at baseline or intrapersonal change over the duration of a panel.

Applying our proposed measure, ω , to 609 survey items from all of the panel datasets previously discussed in this debate revealed a consistent pattern. Nearly all questions show evidence of people making some durable, intrapersonal change over time. Some show notably high amounts of intrapersonal change. For some questions about life satisfaction or views on government benefits, it seems plausible that differences in adult experiences predominantly account for observed differences between people.

However, intrapersonal change is often substantially less pronounced than interpersonal differences, accounting for less than 10 percent of systematic variance on average across all datasets. On numerous questions, like those on civil liberties, abortion, generalized trust,

and civic duty, the systematic variance attributable to intrapersonal change is essentially zero. For these questions, it seems there is not enough cultural change during adulthood to warrant attributing the differences we observe to experiences and social transitions; instead, the primary source of the observed differences appears to stem from experiences in childhood, adolescence, and early adulthood.

More than adjudicating past positions in this debate, the approach we presented in this paper will enable scholars to explore more precisely the relative contributions of change and stability in explaining variation in personal culture, as well as how these contributions might differ across populations. To illustrate this, we showed how the amount of cultural difference explained by intrapersonal change varies substantially by survey item but also by individuals' characteristics such as their education. While age and other factors related to college completion might confound this pattern, it suggests that college completion may crystallize personal culture to an extent that renders later adult experiences on political dispositions less influential.

Limitations

Although we believe it is a major advance on previous work, the approach we outline here has important limitations. We allocate all systematic variance to one of two sets of theoretical processes: intrapersonal change and interpersonal differences at baseline. Our approach does not quantify the proportion of people who “change,” nor can we be sure that the amount of interpersonal change we detect is driven by many people making small changes or a few people making large changes.

Our results and interpretations also hinge on how we have defined change. As does the LCAM, our approach treats cultural trajectories as varying linear slopes for each respondent, thus assuming that change is a linear function of time. This assumption simplifies reality in which change likely also takes non-linear and discontinuous forms. People might jump from

one “stable” disposition to another or experience a “turning point” in their lives that upsets an otherwise stable trajectory. In parts, our assumption of linearity is a limitation of the data, as most questions are only observed for three to six waves. Panels with more waves might allow researchers to loosen this assumption to test alternative, more flexible models of change.

Similarly, our approach assumes that durable change is unidirectional. This is a sensible assumption on short panels where classifying change that lasts less than two years as durable seems unreasonable. Practically, it means that the variance produced by people making durable changes in their cultural dispositions to then return to a previous state later in life is classified as residual variance, rather than intrapersonal change. Longer panels and more flexible definitions of “change” might allow us to account for such trajectories.

Finally, we had chosen to examine the broadest array of measures of personal culture available to us, ranging from religious beliefs and core values, to policy preferences, and even to the importance of different features when buying a new car across five countries (Australia, Germany, Switzerland, the United Kingdom and the United States). Nevertheless, our findings remain limited to the kinds of questions that are asked in panel surveys and in the contexts they were administered, reflecting issues of general (national) politics, gender roles, immigration and race relations, and general well being. Although we have no reason to believe results to be different, our findings do not directly speak to other dimensions of culture such as artistic tastes, leisure activities, and time use.

Implications for Cultural Sociology

Despite these limitations, we believe our method and findings have important implications for social science research. Sociologists interested in understanding cultural differences have largely asked about the *existence* of cultural change in adults (e.g., Kiley and Vaisey 2020; Lersch 2023; Vaisey and Kiley 2021). Yet this approach has inadvertently limited the debate.

In any population, *some* degree of adult cultural change is inevitable. Although ideal types like the settled dispositions and active updating models are useful, no theoretical perspective would expect either early life socialization or adult intrapersonal change to be the sole source of one’s personal culture. Our results reinforce this point, showing the relevance of both factors and allowing their precise quantification.

This and other recent findings (Quinn et al. 2023; Stewart and Berkman 2023) suggest that it is theoretically more productive to measure the relative importance of these two components in concrete, substantive cases. Drawing a unified conclusion from survey items measuring various cultural forms on different scales and across different time frames is challenging. Nevertheless, the general pattern suggests that, for most items, intrapersonal change in adulthood is not the primary reason we see the differences we see between people in the world.

We are not claiming that adults remain static or that their changes are inconsequential. Even quantitatively minor shifts, such as a 2 percentage-point change in support for gay marriage, can have massive ramifications. While the majority may remain consistent in their views, understanding the underlying mechanisms of even such numerically minor shifts remains a crucial task for the sociology of culture.

Our findings suggest that understanding variation in personal culture requires examining the conditions and experiences of early life. While sociological research often focuses on transitions between social roles, changes in social networks, and the experience of organizational environments, these factors seem to account for a smaller proportion of adult differences than early-life experience. Simply put, we need more work on early-life socialization (Guhin, Calarco, and Miller-Idriss 2020).

This conclusion aligns with a range of recent causal inference work suggesting that selection effects, rather than treatment effects, predominantly account for personal cultural differences among individuals in varied social roles and positions (Campbell and Horowitz 2016; Wodtke

2018). While some people clearly change as they transition into new roles or environments, this change seems to be insufficient in magnitude and duration to explain what are often pronounced differences among people in diverse roles. These and our results suggest that when observing differences in personal culture across social roles, such as parenthood, education, or professional authority (Longest, Hitlin, and Vaisey 2013), or across occupations (Weeden and Grusky 2005), selection likely plays a large role in explaining these difference, though exceptions always exist.

Our analysis does not provide an answer as to why intrapersonal change seems to have limited impact on understanding cultural differences among adults. The situations that promote durable change in personal culture might simply be rare during adulthood. Alternatively, it is possible that adults do encounter opportunities, necessities, and incentives for change, but their ability or willingness to change decreases. All the more it is important to research when and how social situations can provoke durable change in adults.

Related to this, our results regarding education and political views suggest that the importance of processes that lead to such change can vary by group. Aligning with life course theories, it seems the significance of experiences for cultural change may be contingent on other, prior experiences. For example, factors that shape cultural dispositions may likely differ for college and non-college graduates; the latter might be more profoundly influenced by mid-life experiences than the former in this regard. In trying to understand differences in personal culture, sociologists should therefore pay more attention to the heterogeneous effects that various factors including social events, encounters, and situations can have.

Our findings also have implications for understanding cultural change at the aggregate level. Given that intrapersonal change accounts for a relatively small amount of variance among adults, many forms of cultural change at the aggregate level are necessarily more likely driven cohort replacement than by contemporaneous social conditions (Underwood et al. 2022; Vaisey and Lizardo 2016). This likely holds true for cultural change at the macro

societal level as well as the micro level such as within organizations, political parties, and professions. Our findings also indicate that the impact of formative experiences, as opposed to contemporaneous social factors, on cultural change is contingent upon an individuals' level of education. The weight of these two factors might thus change as the prominence of education shifts in the life course. If scholars find similar differences across other social categorizations like class or race, it might necessitate a more thorough integration of demographic processes into the analysis of cultural change at the population level.

Implications for Survey Research

Our findings underscore the value of extended panel surveys to advance theories of culture. On average, non-systematic fluctuations in responses account for more than four times as much variance as intrapersonal change, measured as linear change. Differentiating between the two is therefore crucial for understanding cultural differences and cannot be done with cross-sectional data. Instead we should extend panels beyond two waves to gain more leverage to understand when and in what form personal culture changes.

A key insight from our meta-analysis is that an increase in response resolution (the range of response options survey respondents are given) correlates with an increased share of systematic variance explained by intrapersonal change. This may reflect specific issues being assessed by survey items using different scales. Alternatively, it might imply that when intrapersonal changes occurs, they are often subtle with answers moving from “agree” to “strongly agree,” rather than from “strongly agree” to “strongly disagree.” Such subtle changes require finer response options to be detected.

Perhaps most important, we need more panel studies on youth. It is very likely that the majority of adult differences in personal culture are rooted in different experiences before age 18. By empirically constraining our analyses to adult experiences, social scientists may inadvertently concentrate on topics and questions that, while important, might not be able

to help explain major cultural differences in adult populations.

Conclusion

We believe the approach we outlined here can push past the “needless dichotomy” implicit in the question of whether people change or not (Lersch 2023). Characterizing questions as displaying change or not can only take researchers so far, but the question of whether some questions demonstrate more change than others, or whether some groups are characterized by more stability than others, has the potential to weigh in on a broader range of theoretical debates. We hope researchers find our approach useful as they investigate these questions.

Notes

¹For more information on these data sources, see (Goebel et al. 2019; Income Dynamics 2013; Smith et al. 2022; Summerfield et al. 2011; Taylor 1996; University of Essex and Research 2019; Voorpostel et al. 2016).

²We use the model estimate from the midpoint year, rather than the value at the first wave, because this is the best measure of “baseline” available under the assumption that the person does not change. The first wave measure alone contains an unknown amount of measurement error and transient fluctuations.

³A small number of respondents report different highest degrees at wave 1, wave 2, and wave 3. Some of this is due to measurement error, and some of it is due to a small number of people obtaining a higher degree during the four years of the panel. Estimating the panel with highest at wave 1 or highest degree reported across the panel produces functionally identical results.

Appendix

Appendix A: The Distribution of Variation Components

Figure A1 shows the distribution of $V(D)$ and $V(C)$ across all panels.

Appendix B: ω Values When the Last Wave Removed

Figure A2 presents the alternative strategy we used to understand the effects of duration on intraindividual change. First, we restricted the sample such that we have at least 4 waves of observations for all participants. This reduced the number of items to 249. We then fitted the Life Course Adaption models to this sample. Second, we removed the last wave from all the observations, and refit the same models once more. In the final step, we calculated the ω values for each item. The Figure C1 shows a scatterplot for these two sets of observations.

References

- Alvarez, R. Michael, and John Brehm. 2002. *Hard Choices, Easy Answers: Values, Information, and American Public Opinion*. Princeton, N.J: Princeton University Press.
- Alwin, Duane F. 2007. *Margins of Error: A Study of Reliability in Survey Measurement*. Hoboken, N.J.: John Wiley & Sons.
- Alwin, Duane F., and Jon A. Krosnick. 1991. "Aging, Cohorts, and the Stability of Sociopolitical Orientations Over the Life Span." *American Journal of Sociology* 97(1):169–95.
- Bail, Christopher A., Lisa P. Argyle, Taylor W. Brown, John P. Bumpus, Haohan Chen, M. B. Fallin Hunzaker, Jaemin Lee, Marcus Mann, Friedolin Merhout, and Alexander Volfovsky. 2018. "Exposure to Opposing Views on Social Media Can Increase Political Polarization." *Proceedings of the National Academy of Sciences* 115(37):9216–21. doi:

Figure A1: The Distribution of $V(D)$ and $V(C)$

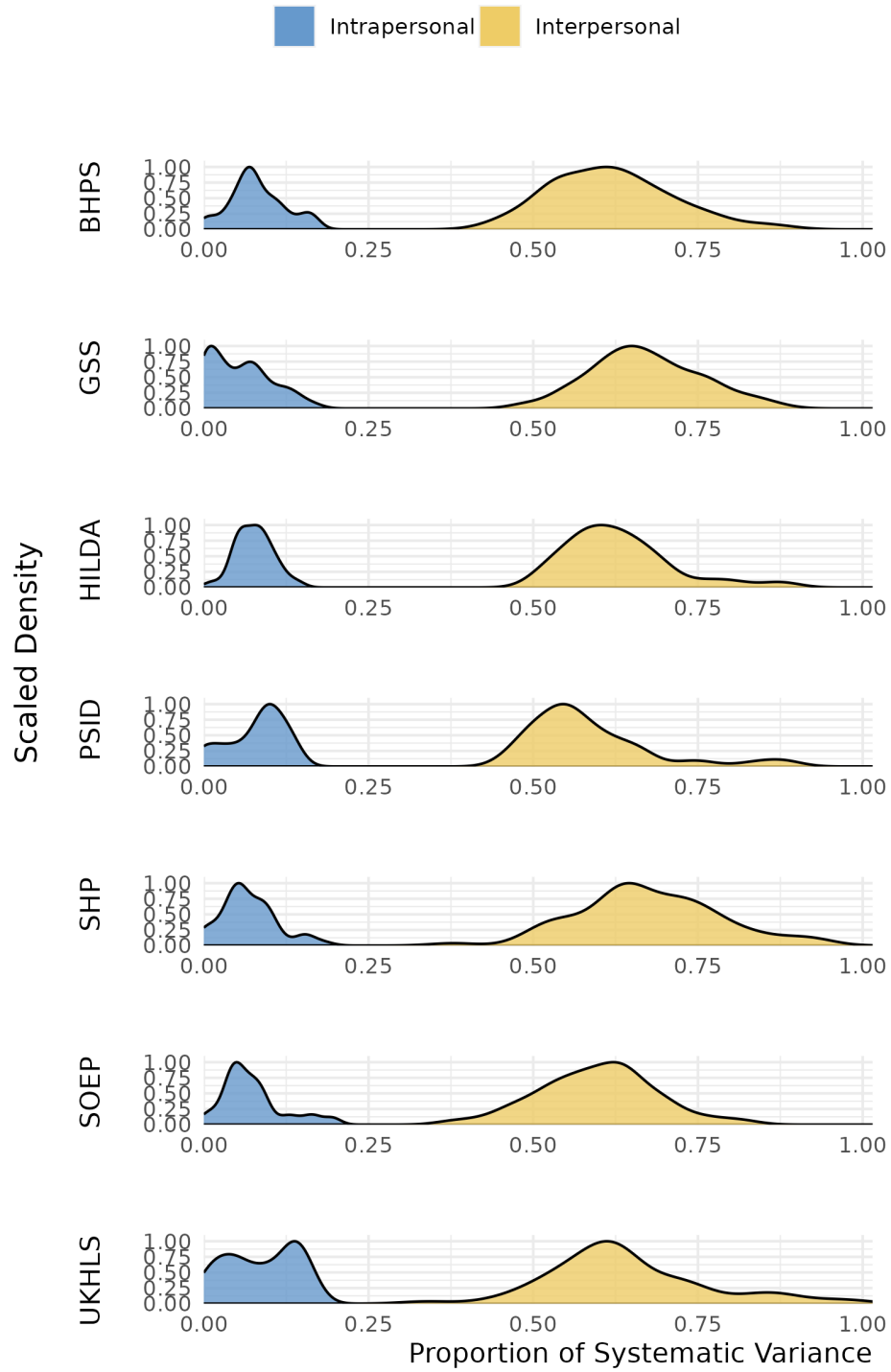
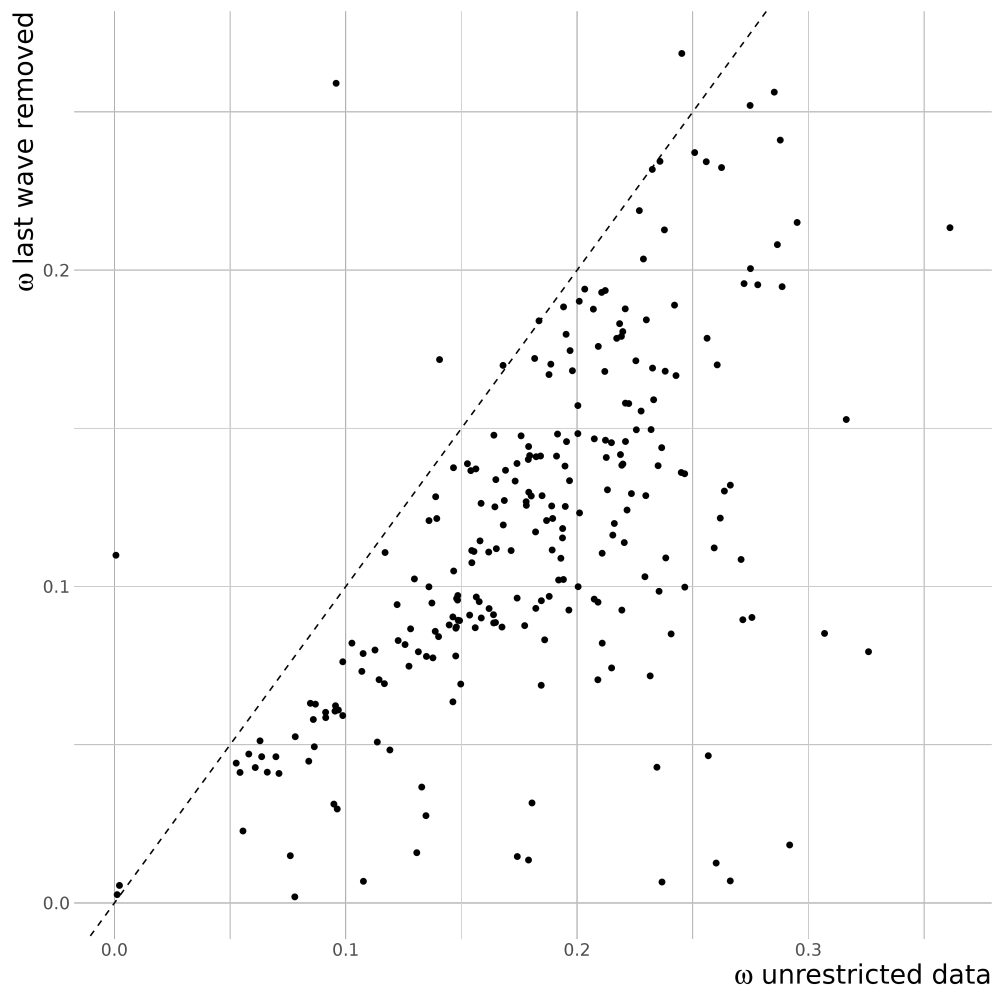


Figure A2: Difference in ω Across Two Sample Specifications



10.1073/pnas.1804840115.

Bardi, Anat, Julie Anne Lee, Nadi Hofmann-Towfigh, and Geoffrey Soutar. 2009. "The Structure of Intraindividual Value Change." *Journal of Personality and Social Psychology* 97(5):913–29.

Bourdieu, Pierre. 1990. *The Logic of Practice*. Stanford, Calif. : Stanford University Press, 1990.

Boutyline, Andrei, and Stephen Vaisey. 2017. "Belief Network Analysis: A Relational Approach to Understanding the Structure of Attitudes." *American Journal of Sociology*

- 122(5):1371–1447. doi: 10.1086/691274.
- Bročić, Miloš, and Andrew Miles. 2021. “College and the ‘Culture War’: Assessing Higher Education’s Influence on Moral Attitudes.” *American Sociological Review* 00031224211041094. doi: 10.1177/00031224211041094.
- Campbell, Colin, and Jonathan Horowitz. 2016. “Does College Influence Sociopolitical Attitudes?” *Sociology of Education* 89(1):40–58. doi: 10.1177/0038040715617224.
- Christakis, Nicholas, and James Fowler. 2010. *Connected: The Amazing Power of Social Networks and How They Shape Our Lives*. HarperCollins Publishers.
- DellaPosta, Daniel. 2018. “Gay Acquaintanceship and Attitudes Toward Homosexuality: A Conservative Test.” *Socius* 4:2378023118798959. doi: 10.1177/2378023118798959.
- DellaPosta, Daniel. 2020. “Pluralistic Collapse: The ‘Oil Spill’ Model of Mass Opinion Polarization.” *American Sociological Review* 85(3):507–36. doi: 10.1177/0003122420922989.
- Eaton, Asia A., Penny S. Visser, Jon A. Krosnick, and Sowmya Anand. 2009. “Social Power and Attitude Strength Over the Life Course.” *Personality and Social Psychology Bulletin* 35(12):1646–60. doi: 10.1177/0146167209349114.
- Elder, Glen H., Monica Kirkpatrick Johnson, and Robert Crosnoe. 2003. “The Emergence and Development of Life Course Theory.” Pp. 3–19 in *Handbook of the Life Course, Handbooks of Sociology and Social Research*, edited by J. T. Mortimer and M. J. Shanahan. Boston, MA: Springer US.
- Feldman, Stanley, and John Zaller. 1992. “The Political Culture of Ambivalence: Ideological Responses to the Welfare State.” *American Journal of Political Science* 36(1):268–307. doi: 10.2307/2111433.
- Gelman, Andrew, and Yotam Margalit. 2021. “Social Penumbras Predict Political Attitudes.” *Proceedings of the National Academy of Sciences* 118(6):e2019375118. doi: 10.1073/pnas.2019375118.
- Goebel, Jan, Markus M. Grabka, Stefan Liebig, Martin Kroh, David Richter, Carsten Schröder, and Jürgen Schupp. 2019. “The German Socio-Economic Panel (SOEP).”

- Jahrbücher für Nationalökonomie Und Statistik* 239(2):345–60.
- Goldberg, Amir, and Sarah K. Stein. 2018. “Beyond Social Contagion: Associative Diffusion and the Emergence of Cultural Variation.” *American Sociological Review* 83(5):897–932. doi: 10.1177/0003122418797576.
- Gross, Neil. 2009. “A Pragmatist Theory of Social Mechanisms.” *American Sociological Review* 74(3):358–79. doi: 10.1177/000312240907400302.
- Guhin, Jeffrey, Jessica McCrory Calarco, and Cynthia Miller-Idriss. 2020. *Whatever Happened to Socialization?* preprint. SocArXiv. doi: 10.31235/osf.io/zp2wy.
- Hout, Michael, and Orestes P. Hastings. 2016. “Reliability of the Core Items in the General Social Survey: Estimates from the Three-Wave Panels, 2006–2014.” *Sociological Science* 3:971–1002. doi: 10.15195/v3.a43.
- Income Dynamics, Panel Study of. 2013. “Public Use Dataset.” *The Survey Research Center Institute for Social Research, University of Michigan*.
- Kiley, Kevin, and Stephen Vaisey. 2020. “Measuring Stability and Change in Personal Culture Using Panel Data.” *American Sociological Review* 85(3):477–506. doi: 10.1177/0003122420921538.
- Kratz, Fabian. 2021. “Do Concerns about Immigration Change After Adolescence? How Education and Critical Life Events Affect Concerns about Immigration.” *European Sociological Review* 37(6):987–1003.
- Krosnick, Jon, and Duane F. Alwin. 1989. “Aging and Susceptibility to Attitude Change.” *Journal of Personality and Social Psychology* 57:416–25. doi: 10.1037//0022-3514.57.3.416.
- Lersch, Philipp M. 2023. “Change in Personal Culture over the Life Course.” *American Sociological Review* 88(2):220–51. doi: 10.1177/00031224231156456.
- Lizardo, Omar. 2017. “Improving Cultural Analysis: Considering Personal Culture in Its Declarative and Nondeclarative Modes.” *American Sociological Review* 82(1):88–115. doi: 10.1177/0003122416675175.
- Longest, K. C., S. Hitlin, and S. Vaisey. 2013. “Position and Disposition: The Contextual

- Development of Human Values.” *Social Forces* 91(4):1499–1528. doi: 10.1093/sf/sot045.
- Mannheim, Karl. 1952. “The Problem of Generations.” Pp. 276–322 in *Essays on the Sociology of Knowledge*. New York: Oxford University Press.
- Mewes, Jan, Malcolm Fairbrother, Giuseppe Nicola Giordano, Cary Wu, and Rima Wilkes. 2021. “Experiences Matter: A Longitudinal Study of Individual-Level Sources of Declining Social Trust in the United States.” *Social Science Research* 95:102537.
- Quinn, Joseph M., Robert E. Freeland, Kimberly B. Rogers, Jesse Hoey, and Lynn Smith-Lovin. 2023. “How Cultural Meanings of Occupations in the u.s. Changed During the Covid-19 Pandemic.” *American Behavioral Scientist* 67(1):125–47.
- Raftery, Adrian E. 1995. “Bayesian Model Selection in Social Research.” *Sociological Methodology* 25:111–63. doi: 10.2307/271063.
- Robinson, Dawn T. 2007. “Control Theories in Sociology.” *Annual Review of Sociology* 33(1):157–74. doi: 10.1146/annurev.soc.32.061604.123110.
- Ryder, Norman B. 1965. “The Cohort as a Concept in the Study of Social Change.” *American Sociological Review* 30(6):843–61. doi: 10.2307/2090964.
- Scoville, Caleb, Andrew McCumber, Razvan Amironesei, and June Jeon. 2022. “Mask Refusal Backlash: The Politicization of Face Masks in the American Public Sphere During the Early Stages of the COVID-19 Pandemic.” *Socius: Sociological Research for a Dynamic World* 8:22.
- Slothuus, Rune, and Martin Bisgaard. 2021. “How Political Parties Shape Public Opinion in the Real World.” *American Journal of Political Science* 65(4):896–911. doi: 10.1111/AJPS.12550.
- Smith, Tom W., Michael Davern, Jeremy Freese, and Stephen L. Morgan. 2022. “General Social Surveys, 1972-2022: Cumulative Codebook / Principal Investigator, Tom w. Smith; Co-Principal Investigators, Michael Davern, Jeremy Freese and Stephen l. Morgan.” *Chicago: NORC*.
- Smith-Lovin, Lynn, and David R. Heise. 1988. *Analyzing Social Interaction: Advances in*

- Affect Control Theory*. New York: Gordon; Breach Sciences Publishers.
- Stewart, Evan, and Diane Berkman. 2023. “Shifting or Settled? Tracking Racial Animus During COVID-19.” *Social Psychology Quarterly* 86(3):312–33.
- Summerfield, Michelle, Ross Dunn, Simon Freidin, Markus Hahn, Peter Ittak, Milica Kecmanovic, Ning Li, Ninette Macalalad, Nicole Watson, Roger Wilkins, and others. 2011. “HILDA User Manual – Release 10.” *Melbourne Institute of Applied Economic and Social Research, University of Melbourne*.
- Swidler, Ann. 2001. *Talk of Love: How Culture Matters*. Chicago: University of Chicago Press.
- Taylor, Marcia Freed. 1996. “British Household Panel Survey User Manual: Volume a- Introduction, Technical Report and Appendices.”
- Tourangeau, Roger, Lance J. Rips, and Kenneth A. Rasinski. 2000. *The Psychology of Survey Response*. Cambridge, U.K. ; New York: Cambridge University Press.
- Underwood, Ted, Kevin Kiley, Wenyi Shang, and Stephen Vaisey. 2022. “Cohort Succession Explains Most Change in Literary Culture.” *Sociological Science* 9:184–205.
- University of Essex, Institute for Social, and Economic Research. 2019. “Understanding Society: Waves 1–9, 2009–2018 and Harmonised BHPS: Waves 1–18, 1991–2009.” *UK Data Service*.
- Vaisey, Stephen, and Kevin Kiley. 2021. “A Model-Based Method for Detecting Persistent Cultural Change Using Panel Data.” *Sociological Science* 8:83–95. doi: 10.15195/v8.a5.
- Vaisey, Stephen, and Omar Lizardo. 2016. “Cultural Fragmentation or Acquired Dispositions? A New Approach to Accounting for Patterns of Cultural Change.” *Socius* 2:2378023116669726. doi: 10.1177/2378023116669726.
- Visser, Penny S., and Robert R. Mirabile. 2004. “Attitudes in the Social Context: The Impact of Social Network Composition on Individual-Level Attitude Strength.” *Journal of Personality and Social Psychology* 779–95.
- Voorpostel, Marieke, Robin Tillmann, Florence Lebert, Ursina Kuhn, Oliver Lipps, Valérie-

- Anne Ryser, and B. Wernli. 2016. "Swiss Household Panel User Guide (1999–2015)." *Lausanne: FORS*.
- Weeden, Kim A., and David B. Grusky. 2005. "The Case for a New Class Map." *American Journal of Sociology* 111(1):141–212. doi: 10.1086/428815.
- Wodtke, Geoffrey T. 2018. "The Effects of Education on Beliefs about Racial Inequality." *Social Psychology Quarterly* 81(4):273–94. doi: 10.1177/0190272518804145.
- Zaller, John. 1992. *The Nature and Origins of Mass Opinion*. Cambridge: Cambridge University Press.