

“Looking for It in Genetix”: Response to Comment

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WE thank the authors of this comment for engaging with our work (Jæger and Møllegaard 2022). The authors make a number of critical comments on our research objectives, indicators of cultural tastes, and methods and interpretations of empirical results. We address these comments in detail below. Our overall response is that the authors either misunderstand or misrepresent what we say and rehash a number of generic criticisms of twin studies. Moreover, the authors seem not be aware that many of the criticisms they raise (e.g., the assumed dichotomy between nature and nurture and the assumptions in twin studies) have been addressed in recent sociogenomics literature (e.g., Conley and Fletcher 2017; Harden 2021; Mills and Tropf 2020). As we see it, none of their criticisms challenge the empirical results we present.

We would like to offer a bit of context before responding to the authors' comments. Our article is part of a larger project that seeks to understand how family background shapes cultural tastes and participation. This is an important, yet understudied, topic in sociological research on cultural stratification. Our contribution is that we collected data on monozygotic (MZ) and dizygotic (DZ) twins to distinguish genetic and (two types of) environmental variation in cultural tastes and participation. To do this, we use the ACE model, the “workhorse method” in behavior genetics, that separates the total variance in an outcome of interest into variance components attributable to shared genes (A), shared environments (C), and unique environments (E). We use a standard implementation of the ACE model used in thousands of research articles (Polderman et al. 2015).

Research Objectives

The authors of the comment speculate on our objectives for carrying out this research. We did not go into this project with specific priors about the role of genetic and environmental factors in shaping cultural tastes or behaviors. Our article begins from the observation that existing research on cultural stratification provides no evidence on the relative role of genetic and environmental factors in shaping cultural tastes and participation (a few studies use data on siblings, but these studies cannot distinguish genetic and environmental influences). This is an obvious gap in the literature. In the absence of evidence from research in sociology, we draw on results from research outside sociology that includes outcomes that resemble or reflect cultural tastes (summarized in Table 1 in our article). This research suggests that genetic factors play a non-trivial role. As we see it, there is no reason to assume

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that cultural tastes and participation are inherently different from other types of outcomes sociologists typically care about (e.g., education, social interactions, and values), most of which have been shown to be shaped in part by genetic factors (Polderman et al. 2015). Thus, it makes sense that genetic factors would shape cultural tastes and participation. The results we present in the article, similar to those from other research that studies outcomes that resemble or reflect cultural tastes, are that genetic factors (A) and unique environments (E) explain most of the variance in cultural tastes and participation, whereas shared environments (C) explain only little. Interestingly, shared environments explain some of the variance in cultural *tastes* but almost none of the variance in cultural *participation*. Although these empirical results seem perplexing (also to us), an anonymous reviewer of our article made the comment that our results only come as a surprise to sociologists.

Empirical Indicators

The authors make several comments on our empirical indicators of cultural tastes and participation. We now respond to each comment.

First, the authors argue that our empirical indicators are crude. We agree in the sense that our indicators refer to general cultural activities and genres (e.g., we ask “How often do you go to the cinema?” and not “Which genres of movies do you watch at the cinema?”). We chose these indicators because they resemble those used in existing quantitative research on cultural tastes and participation. Because we use a research design not previously used in research on cultural stratification, we opted for empirical indicators of cultural tastes and participation comparable to those used in existing research. We believe this approach creates a clearer link between our results and those reported in existing research. Although crude, our indicators cover a broad set of cultural activities and genres (e.g., performing arts, music, and literature). This means that although our set indicators might be crude in terms of depth, they are not crude in terms of width.

Second, the authors argue that our indicators of highbrow, lowbrow, and popular tastes and participation are problematic because they pertain to cultural outings constrained by material conditions (e.g., cost, accessibility, and the presence of young children). It is correct that our indicators mostly refer to cultural outings. However, this is the standard approach in existing quantitative research. We do ask about cultural tastes, which are not constrained by material conditions to the same extent as cultural participation—and get very similar empirical results. Moreover, our empirical results do not change if MZ and DZ twins are constrained by material conditions to the same extent because these constraints are picked up by the MZ–DZ correlations.

Third, the authors argue that some of our indicators “are associated with the upper classes.” We do not know exactly what the authors mean by this, but we interpret it to suggest that our indicators and measurement of cultural tastes and participation somehow favor highbrow culture. This is not the case, and the principal component analyses (PCAs) we carry out based on these indicators identify three latent variables of which only one pertains to highbrow culture.

Fourth, the authors argue that our interpretation of the results from the PCAs is problematic because several indicators of cultural tastes and participation load on the same principal component. This is perfectly normal, and we get very similar results when running PCAs of, respectively, cultural tastes and cultural participation (cf. Table A2). We think the similar results for cultural tastes and cultural participation support the stability of the PCA results. Finally, the substantive content of the PCAs, in terms of patterns of cultural tastes and participation, is similar to what we find in other articles using different methods (Katz-Gerro and Jæger 2013; Klokke and Jæger 2022).

Fifth, the authors argue that our measures of omnivorousness in music and reading are narrow in the sense that they only capture omnivorousness *in volume* (i.e., total number of genres liked). This is correct, and we note this limitation in the article (P. 260 and note 3). That said, existing research has used the same approach. Moreover, there is no agreement in the literature on how to measure omnivorousness (de Vries and Reeves 2021).

Methods and Interpretations

The authors make several comments on our choice of (twin) method and interpretation of empirical results. Many of their comments do not refer to our specific analyses and results, but to twin research in general.

First, the authors state that our article is grounded in a “simplistic nature–nurture dichotomy, fueling unproductive oppositions between sociology and biology.” We do not recognize this interpretation of our research. In fact, throughout the article we draw on research in sociogenomics that challenges the (now rather obsolete) nature–nurture dichotomy. For example, in the introduction (P. 254) we make clear that “our research design does not assume biological determinism or that the results we present generalize beyond our specific case. Research shows that the impact of genes and environments varies across contexts and over time.”

Second, the authors argue that the concept of heritability is problematic *in general* because we cannot interpret heritability independently of environments. We agree completely. Throughout the article, we interpret our empirical results as pertaining to the specific context of Denmark and the particular (young) sample of twins for whom we have data. For example, in the hypothesis section (Pp. 258–9) we argue that heritability is likely to be higher in Denmark than elsewhere because the high level of income redistribution in Denmark suppresses inequality in family environments.

Third, the authors argue that departures from the additive ACE model, in particular gene–environment interactions (GxE), lead “researchers prone to interpret heritability to meaningless conclusions.” We agree that GxE represents an important challenge that must be addressed upfront. In the article, we discuss the general implications of GxE and present supplementary analyses that address GxE in our empirical case in the final discussion. Research in sociogenomics that addresses GxE argues that GxE represents an opportunity for sociology to add important nuance to naïve debates on nature versus nurture (e.g., Conley and Fletcher 2017). We agree.

Fourth, the authors argue that our statement that our “findings are difficult to reconcile with theories in sociology that emphasize the role of the family environment” mean that we supposedly “succumb to the exact same fallacy” (of interpreting results independently of environmental context and G×E). As we argue above, we do in fact interpret our results as specific to the Danish context.

Fifth, the authors argue that by conceptualizing the shared family environment (the C in the ACE model) as pertaining to *parental* influences, we ignore other shared environments such as friends, neighbors etc. It is correct that shared environmental influences beyond parents go into the C component. Yet, we find little evidence that C matters, so this point is of little empirical relevance. We agree with the authors that it would be interesting to distinguish the environmental impact of parents from that of others. However, this would require unique variation in “others” that is rarely available (say, if twins go to different schools or have different peer groups).

Sixth, the authors argue that twin research *in general* does not provide reliable estimates of heritability because this type of research relies on dubious assumptions. Their main conclusion seems to be that we should never use twin studies. We respectfully disagree. Twin research is a well-established scientific discipline, and we see no reason why sociologists should not use it. The assumptions underlying twin research have been studied for decades, and statistical and interpretational remedies for these assumptions are now well established in the literature. We discuss a number of assumptions in our article, including how they might affect our results.

Seventh, the authors conclude that because we argue that the so-called equal environments assumption (EEA) is unlikely to present a problem in our specific analysis, “One implicit assumption of such statements is that twins can be thought of as a universal scientific category, that is, as an invariant substance that can be captured and analyzed the same way we examine atoms and genes.” We cannot see how our discussion of EEA would lead to this conclusion. As stated above, we interpret our results as pertaining to a specific context and sample of twins.

Eighth, the authors argue that because decades of research has “confirmed the structuring role of the family environment in the formation of cultural tastes and educational outcomes,” we are obliged to provide extraordinarily strong evidence to support our conclusion that genetic factors also shape cultural tastes and participation. We think this statement is nonsensical, as no previous research has used a genetically informed design and thus has been able to distinguish genetic and environmental variation in cultural tastes and participation. With regard to educational outcomes, also mentioned by the authors, twin research finds that shared family environments have a considerable impact on educational outcomes (most research reports C estimates of around 0.30). Consequently, shared environments—in addition to shared genes—shape educational outcomes, as the authors argue. Our results (and results from twin research outside sociology that addresses outcomes that resemble or reflect cultural tastes) suggest that cultural tastes and participation might be different. We encourage future research that uncovers these differences.

In summary, we do not ask readers of *Sociological Science* to change their views of cultural stratification based solely on our article. We end the article by calling for more research to address this important issue. All we ask is that readers take note

as, to the best of our knowledge, our article is the first to use a genetically informed design to address the possibility that genetic factors, in addition to environmental ones, might shape cultural tastes and participation. Our results for cultural tastes and participation mirror results for other outcomes sociologists care about, all showing that genetic factors in part shape these outcomes. Consequently, our results really are not too surprising.

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