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Eurythmics or Xenakis? Cultural Tastes (Are Not Made of Genes): Comment on Jæger and Møllegaard, "Where Do Cultural Tastes Come From? Genes, Environments, or Experiences"

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An article recently published in *Sociological Science* explored cultural tastes and practices in Denmark using a behavioral genetic lens (Jæger and Møllegaard 2022). Using data from monozygotic and dizygotic twins, the authors concluded that shared familial environments mattered less than genetic factors, thus questioning the soundness of established sociological theorizations of cultural inequalities, including Bourdieu's *Distinction* (P. 266). After a close methodological and conceptual examination, our main conclusion is that social scientists should lend little credence to the claims put forth by Jæger and Møllegaard, which fall short of the methodological and conceptual standards habitually upheld in both sociology and behavior genetics. This is true whether we selectively focus on the cultural aspects while ignoring the genetic dimension, or vice versa. Logically, their combination only brings additional uncertainties.

Before delving further into this, we would like to make it clear that our criticism is not rooted in some anti-biology sentiment, as proponents of so-called biosocial perspectives sometimes claim to avoid dealing with perfectly legitimate scientific issues. Genetics and biostatistics are established, complex disciplines that sociologists cannot reasonably expect to be proficient in simply by incorporating twin data into their statistical models. Hence, our response is the result of a collective work that brought together scholars with complementary areas of expertise, including sociology of science, sociology of culture, epistemology, (bio)statistics, and genetics. We firmly believe that the relationship between biological and social processes is a topic worth studying, but only in a careful, rigorous manner. It is acknowledged that social processes have an impact on the usual measures of the relative importance of genetic and environmental factors (Morris et al. 2020). Yet, it would appear that the analysis of variance still unconsciously structures the way of thinking of many researchers, who seem unable to formulate a sociological problem without considering the separation of causal factors (Abbott 1988). It is precisely because the article we comment upon is grounded on a simplistic nature–culture dichotomy, fueling unproductive oppositions between sociology and biology, that we decided to address its arguments.

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Crude Measures of Cultural Tastes and Practices

To investigate the link between genetics and cultural outcomes, Jæger and Møllegaard relied on survey data collected in Denmark among monozygotic and dizygotic same-sex twins. The respondents were asked four questions: two addressed cultural taste ("On a scale from 1 to 5, how interested would you say you are in the following activity?") and participation ("How often have you attended the following activity within the last 12 months?"); two concerned omnivorousness, one for music and the other for reading (Pp. 259–60). The indicators used to assess and measure the twins' cultural tastes and practices pose a series of problems. First, the highbrow/lowbrow/popular indicators are partially based on variables describing cultural outings. Although these variables are not illegitimate in research on cultural practices, they are poorly adapted to the objectification of highbrow versus lowbrow differences. Indeed, outings partly reflect tastes but are also strongly constrained by material conditions such as cost, accessibility of places, presence of young children, et cetera. Therefore, the role of cultural capital is largely masked by income, place of residence, or type of household. Moreover, we observe that half (6 out of 12) of the cultural outcomes are associated with the upper classes, for both tastes and participation, which raises some questions about the relationship between the three analytical categories (highbrow/lowbrow/popular). Given these limitations, one can only wonder why the authors did not instead rely on the more detailed and precise data that they gathered to measure omnivorousness in music and reading.

The interpretation of the axes of the principal component analyses (PCAs) also raises several issues (see Table A2 of Jæger and Møllegaard 2022). Empirical studies in the sociology of culture amply demonstrate that cinema and rock music can be considered middlebrow, or sometimes even highbrow, cultural outings (Christin 2012; Katz-Gerro 2002; Katz-Gerro and Meier Jæger 2015; Purhonen, Gronow, and Rahkonen 2011; Robette and Roueff 2014) but in no case strictly lowbrow practices as suggested by the authors' results and interpretations (Table A2). Of course, because cultural hierarchies diverge across time and place, it remains possible that these cultural practices occupy a different stratum in Denmark's legitimacy scale. Yet, data collected in the city of Aalborg in the 2000s would tend to contradict this interpretation as they align with the patterns observed elsewhere, as exemplified by the case of the hard-rock band D-A-D, which was at the time mainly valued among the intellectual segment of the population (Prieur, Rosenlund, and Skjott-Larsen 2008). Likewise, the first author's own research on cultural practices in Denmark has shown that cinema is very well represented among omnivorous, middlebrow, and lowbrow respondents, thus making it difficult to classify it as uniquely lowbrow (Katz-Gerro and Jæger 2013).

This problem mainly stems from the fact that the article fails to distinguish between the different cultural genres that populate apparently homogeneous categories. Lowbrow and highbrow participants might well go to the cinema often, but it is likely that they will not watch the same types of movies (Boyadjian 2021). Likewise, the authors' own results illustrate this limitation: the PCAs rank "musical" both as a highbrow and lowbrow taste and practice (see Table A2). It is thus highly problematic that heterogeneous practices such as "cinema," "play," or "musical" are used as reliable markers of cultural hierarchies and put on the same analytical level as more specific genres (classical music, rock/pop, techno).

A last set of problems relates to omnivorousness. As the authors point out, their article is only dealing with omnivorousness "in volume" (P. 260), also known as "voraciousness." However, most of the debates on omnivorousness that have been going on in the sociology of culture since Peterson's seminal work (Peterson 1992) concern tastes and practices that cross symbolic boundaries by combining different degrees of legitimacy, that is, omnivorousness "in composition" (see de Vries and Reeves [2022] for a recent overview). It is thus rather ambiguous to claim to contribute to debates on omnivorousness given the indicators that the authors mobilize. The only measure of omnivorousness is based on an additive scale that merely counts the number of genres read or listened to by the survey respondents (P. 260). Therefore, the findings are highly dependent on the nomenclature of genres adopted. It would be easy, for example, to propose more precise categories to measure lowbrow tastes and practices in order to make the corresponding social groups appear more omnivorous.

On the contrary, surveys of cultural practices and the nomenclatures commonly used tend to reflect a legitimacy bias. Following this inclination, the authors give a more detailed account of highbrow genres (e.g., distinguishing between classical and opera) or those in the process of being legitimized (rock music appears in several categories), whereas less valued tastes are reduced to coarse categories (e.g., electronic music or folk music). The problem is even more clear when it comes to measures of cultural taste and participation, which, for some unknown reason, are different from the ones used to gauge omnivorousness. When they investigate highbrow, lowbrow, and popular sensitivities, the authors again distinguish between classical music and opera, whereas the most popular genre (according to their PCAs; see Table A2) is an improbable, catch-all category entitled "techno/rap/dance/hip-hop."

Heritability Does Not Separate Genetic and Environmental Causality

The issues raised by the categorization and analysis of cultural tastes and practices are not the only ones to warrant a critical reading of this article. The genetic side too is riddled with problems and limitations. Many criticisms have long been raised about the concept of heritability itself, its misinterpretations, and its arduous estimation in twin studies (e.g., Fuentes and Bird 2022; Larregue 2018; Lewontin 1974; Robette, Génin, and Clerget-Darpoux 2022; Robette and Reeve 2022; Turkheimer 1998). For the sake of simplicity, we have summarized the main criticisms in the online supplement, where we also describe the mathematical model from which this measure is derived.

In the article, the results from the twin models are interpreted as follows (P. 265):

> Table 3 shows that, across all eight dependent variables, the A component (ranging from 0.30 to 0.63) is consistently larger than the C

> component (ranging from 0 to 0.33). Moreover, we find that shared environments have no discernible impact on cultural tastes and participation in four out of eight dependent variables (i.e., estimates of C are zero in the ACE models, which means that we report results from AE models). Substantively, these results suggest that shared genes, rather than shared environments, drive most of the total impact of family background on cultural tastes, participation, and omnivorousness in music and reading. At face value, these findings are difficult to reconcile with theories in sociology that emphasize the role of the family environment (Bourdieu 1977, 1990). Nonetheless, our findings align with research outside sociology on similar cultural traits, most of which also reports high estimates of A and low estimates of C (see Table 1).

Before delving further into the interpretation of these models, it is worthwhile to underline that a rapid glance at the confidence intervals should have led to a more careful reading of the results. For instance, if we look at popular tastes, the confidence intervals for the genetic component are $(0.03; 0.83)$ and $(0.02; 0.78)$ for the shared environment; when it comes to highbrow participation, they respectively are $(0.47; 0.70)$ and $(0.01; 0.63)$. Such wide gaps clearly weaken the authors' authoritative tone.

But the most important issue lies in the fact that the heritability of a trait does not give a measure of the relative significance of genetic and environmental factors (Lewontin 1974). Yet, such claims have become more and more numerous in the social sciences in recent decades (Bliss 2018; Larregue 2020; Panofsky 2014). It is possible that some social scientists are simply not aware that the notion of heritability has been repeatedly questioned in genetics (Lewontin 1974; Robette et al. 2022; Turkheimer 1998). One only has to open up an authoritative textbook to find an unambiguous formulation of the problem at stake: "A high heritability does not mean that a character is unaffected by the environment. Because genotype and environment interact to produce phenotype, no partition of variation into its genetic and environmental components can actually separate causes of variation" (Griffiths et al. 2004:659). A respected behavior geneticist echoes the same views: "the answer to the question of whether heritability reveals anything about biological or genetic etiology is an unequivocal 'no': Heritability reveals nothing about the difference between aphasic individuals and monks. Indeed, the heritability of religiosity is in all likelihood higher than the heritability of aphasia" (Turkheimer 1998:786).

A low heritability value can thus be due to the scarcity of variation in the genetic factors responsible for the trait; the classical—quasi-folkloric—example is the number of eyes and limbs that human beings possess, the variance of which is essentially attributable to environmental factors (such as traumatic events or fetal exposition to toxic compounds), meaning that the heritability is virtually null. However, in our species, the genetic basis for having two eyes and four limbs is rather firmly established. Conversely, high heritability can be attributed to the paucity of environmental variations. In a controlled environment (such as the environment of laboratory animals, or to a lesser extent the environment of farm animals), most of a trait variance can be attributed to genetic variance, and heritability estimates will be near to 1. Such traits with a high heritability can change dramatically when the environment is modified. The same phenomenon will occur even when the environment is not controlled, provided the environmental changes are large enough. The textbook example of a highly heritable trait in humans is height, which has notwithstanding increased by roughly two standard deviations within the last century—a large change usually attributed to modifications of the environment, the Korean case being particularly illuminating in this regard (Pak 2004).

Moreover, departure from the additive model, such as the presence of gene–environment interaction—which is, in our opinion, pervasive in the building of many traits and more particularly of behavioral traits—will lead researchers prone to misinterpret heritability to meaningless conclusions. For example, the heritability of tuberculosis has been estimated to values ranging from 0.50 to 0.70 (Abel et al. 2014). However, we know that the primary cause of tuberculosis is infection by *Mycobacterium tuberculosis*, that this high heritability estimate is attributable to the existence of genetic factors involved in the response to the infection, and that eradicating *M. tuberculosis* would lead to the extinction of the disease. Yet, had not Koch discovered the existence of this bacterium, these researchers would readily conclude that tuberculosis is a genetic disease. When Jæger and Møllegaard claim that "[their] findings are difficult to reconcile with theories in sociology that emphasize the role of the family environment" (P. 265), they succumb to the exact same fallacy.

Twin Studies Do Not Provide Reliable Estimates of Heritability

Another objection, although of lesser importance, to the use of heritability values obtained from twin studies is that these do not provide reliable estimates of heritability. First of all, it is important to recall that heritability estimates are most often deduced from observed correlations between monozygotic (MZ) and dizygotic (DZ) twins, instead of from the more readily available correlations between ordinary siblings, or between parents or offspring. The rationale behind this approach is related to the widely acknowledged presence of shared environmental factors among families. If there were no shared environment in families, heritability could be estimated, for example, as twice the correlation between a parent and its offspring. Note that the polygenic model was introduced by Fisher (1919) to show that the correlation between relatives can be modeled in the framework of Mendelian inheritance. Heritability is best understood as a summary of these correlations rather than through its contextless definition as a variance ratio.

Nevertheless, the shared environment in families induces a positive correlation between the family members' traits, which would artificially lead to high heritability estimates. It is certainly of prime importance for behavioral traits because the family nucleus is a prime area of socialization for individuals. Twin studies can be thought of as an attempt to correct this bias without actually measuring the environment—and thus to estimate what the correlation between relatives would be in the absence of shared environment. However, as we explain in the online supplement, such models need—besides the usual hypotheses of linearity and of absence of gene–environment interactions and correlations—two extra hypotheses to do so: first, that the dominance term in the model is null (the model is then known as the ACE model), and second, that the amount of shared environment between DZ twins and MZ twins is equal. Both hypotheses are equally fragile, and departure from both hypotheses lead to an overestimation of the heritability (cf. "Common environment and twin studies: the *ADCE* and *ACE* models" in the online supplement). We will here focus on the latter hypothesis, known as the equal environment assumption (EEA). This assumption is already dubious for traits with a biological nature, as even before birth the environment shared by MZ twins is more important: 70 to 75 percent of MZ twins are monochorionic (i.e., they share a single placenta), whereas DZ twins are always dichorionic, because they form from two distinct eggs (Hall 2003; Price 1950). But when it comes to behavioral traits, the EEA is even more questionable. It is undeniable that each twin's social dispositions are formed in close contact with the other, meaning that their relationship is an important part of the environment leading to the development of cultural tastes. Yet, the following hypothesis has, to the best of our knowledge, never been seriously investigated: in a situation where their friends and family will expect them to have similar tastes and behavior, and maybe also inevitably because of their physical resemblance, MZ twins will exhibit a stronger tendency to mimetism. It would not be surprising for this effect to be stronger for MZ than for DZ twins.

When such violations of EEA occur, the heritability estimates are biased upward, whereas shared environment estimates are biased downward. One can observe that the correlation between MZ twins is higher than the correlation between DZ twins, but it is impossible to decide what part of the difference is attributable to genetic factors and what part is due to an excess of shared environment in MZ twins, and in particular to twin mimetism. The only solution is then to postulate that one of the two factors is negligible and to attribute the excess of correlation observed in MZ twins to the second. Twin studies researchers have decided to neglect twin mimetism. When dealing with behavioral traits, this decision does not seem very judicious.

One recurrent answer to this line of criticism is that some empirical tests have lent credence to the soundness of the EEA. In their supplementary material, Jæger and Møllegaard thus write that "Since the EEA is central in twin models, it has been rigosously [sic] tested and found to be credible across a wide range of outcomes (e.g., Conley et al. 2013; Felson 2014). Consequently, we have no reason to assume that it presents a problem in our analysis" (Pp. S4–5). 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 or genes. The possibility that twins might not be raised similarly or occupy the same positions across social spaces—including countries, but also along class-gender-race lines—and periods is never contemplated, which should seem highly problematic to any sociologist. Because neither of the two references mobilized by the authors applies to their population of reference (i.e., a sample of Danish twins born in the years 1985 to 2000) or to the range of behaviors they are actually considering, their rebuttal can only be read as a convenient way to dismiss important criticisms of the EEA.

Genetic and Environmental Factors Are Hopelessly Intricate

Another element that is of prime importance for behavioral traits is the effect of assortative mating. We say that there is assortative mating for a trait when mates tend to be more similar than they would be if they were randomly selected. This phenomenon is observed for many traits, and in particular for behavioral traits. Assuming that the ACE model does hold, one of the consequences of assortative mating is that it creates a positive correlation between genetic loci involved in the building of the traits, resulting in a sizable increase of genetic variance (Crow and Felsenstein 1968). This is due to the fact that the genetic factors of the parents are correlated, inducing an intra-individual correlation in their offspring's genetic loci. Thus, selecting partners on the basis of one's similarity for a given trait modifies the value of the genetic variance for this trait (and, therefore, the total variance of the trait). Put differently, genetic variances are not biological, immutable values; they are affected by our behavior.

On the other hand, one can show in the ACE model that assortative mating leads to an underestimation of the heritability: Falconer's formula estimates $\left(1 - rh^2\right) h^2$ instead of h^2, where r is the correlation between the parent's phenotypes (see, for instance, Caballero 2020:6.1.3). But this formula is derived assuming there is no shared environment between the parents and the offspring—an assumption so widely recognized as dubious that twin studies were designed to get rid of the bias induced by the shared environment. It is therefore necessary to consider the case where there is both assortative mating and cultural transmission from parents to children (or any cause of environmental correlation). When a positive correlation between parents and their offspring's environment is introduced in the model, assortative mating induces a positive correlation between genetic and environmental factors, thus rendering impossible the partition of a given trait's variance into a cumulative sum of genetic and environmental variance: the gene–environment correlation creates an increase of trait variance that is neither genetic nor environmental. This issue of quantitative genetics is addressed in more detail in a technical note (Noûs and Perdry 2022). The main consequence of this is that the total variance of the trait can no longer be decomposed in a sum of a genetic and an environmental component, thus rendering the concept of heritability meaningless. Another consequence is that the correlation between family members is modified in a complex way when both of these phenomena are considered. To illustrate this last point, we have included a numerical application in the last section of the online supplement in which we consider the simple case of parent–offspring correlation.

To synthesize, even if one were to endorse the simplistic ACE model, it still would not be feasible to obtain a meaningful decomposition of the variance of a trait, let alone to interpret the observed correlations between family members while neglecting the complexity of mate choices and of cultural transmission.

Modeling the Shared Environment Is a Complex Task

In contrast, Jæger and Møllegaard consider that the shared environment simply corresponds to the familial background, uniformly shared by the members of the family. Hence, the first results section, which is transparently entitled "Total Impact of Family Background," entirely relies on the following assertion: "In the ACE model, we capture the total impact of family background via the combination of A (shared genes) and C (shared environments)" (P. 262). Because the ACE model does not allow us to identify empirically what these so-called shared environments really are, this statement is entirely speculative. For instance, it is likely that twins share not only genes and family but also friends, schools/universities, places of work, and so on. All of these social networks would fall under the shared environments category, and yet the authors are surprisingly silent about this matter.

This is especially problematic given the age composition of the sample. Respondents were born in the years 1985 to 2000 and the survey data were collected in 2019, which means that the younger respondents were approximately 19 at the time of the collection. The mean age of the respondents, however, was 25 (P. 259). At that time, most respondents would have left their parents' place: in the late 2000s the median age for leaving home among Danish youths was around 21, the dominant trajectory of young adults being oriented toward self-fulfillment and autonomy from the parents (Van de Velde 2008). Hence, we can estimate that at least half of the respondents had been living by themselves for four years when they answered the survey. What does it mean to capture the "family background" in such conditions?

Another potentially important limitation lies in the boundaries of the family itself. The article puts forth a limited conception of the familial sphere. Indeed, the authors' framing of the issues at stake lend central importance to parents while ignoring other potential contributors to twins' cultural tastes and practices. Again, this is crucial because a central argument of the article is that genes matter more than the family environment, which is said to contradict established sociological theories of cultural dispositions, including Bourdieu's *Distinction* (P. 266):

> A first takeaway from our analyses is that although family background is important, it appears to operate only to a limited extent via the family environment. This finding challenges theories arguing that the family environment is crucial for cultural socialization (Bourdieu 1979; Guhin, Calarco, and Miller-Idriss 2021; Jæger and Breen 2016). It also challenges the usual interpretation in empirical research that intergenerational correlations in cultural tastes and participation originate in the family environment (Kraaykamp and van Eijck 2010; Nagel 2010; Notten et al. 2012; van Hek and Kraaykamp 2015; Yaish and Katz-Gerro 2012). How can we reconcile our findings with existing research?

At first, this statement might seem quite logical and hard to contradict. Yet, the authors fail to mention that a family environment is not only made of parents and that there are good reasons to believe that other family members, including grandparents (Klokker and Jæger 2022; Møllegaard and Jæger 2015), might play a role in the cultural dispositions of the studied twins. We can only speculate as to why such crucial relationships were left out of this article when they are directly related to the genetic thesis put forth by the authors. Because it is through their parents' genes that children inherit a part of their grandparents' genetic material, the remaining statistical association is necessarily related to what the authors label environmental factors.[1]

To be sure, one could simply argue that these causal pathways are still captured in the shared environments (C) coefficient, because twins would likely equally spend time with their grandparents, for instance. Yet, remember that the authors found that the coefficient for shared environments was sometimes equal to 0, which they erroneously interpreted as demonstrating that only genes and so-called individual experiences mattered in cultural behavior. This was the case for four of the dependent variables: lowbrow cultural participation, popular cultural participation, music omnivorousness, and reading omnivorousness. Can it be, then, that cultural omnivorousness is only driven by genetic factors and entirely independent from the shared environment? Because decades of research have confirmed the structuring role of the family environment in the formation of cultural tastes and educational outcomes, Hume's principle that extraordinary claims require extraordinary evidence would dictate that the only-genes-matter thesis be subjected to more rigorous testing before being given any scientific credence.

Concluding Remarks: "Cognitive Skills" and the Naturalization of Social Hierarchies

To conclude our critical reading of this article, we would like to call into question its social implications. One key element of the theorization of social reproduction concerns the arbitrary nature of cultural hierarchies (Bourdieu 1979; Bourdieu and Passeron 1970), namely the fact that highbrow, middlebrow, and lowbrow genres are sociohistorical products and, as such, closely related to the symbolic struggles that social groups in engage with one another. This explains why Bourdieu's *Distinction* begins with a somewhat arid critique of Kantian aesthetics, which endeavored—if we are to follow the French sociologist's reading—to naturalize cultural hierarchies and justify them under the guise of a seemingly neutral, objective philosophy.

This crucial element was apparently lost in translation. In their discussion, the authors ask the following question: "how might genes shape the taste for highbrow culture such as opera and classical music?" (P. 266). The answer, which is purely conjectural, proposes the following causal path (P. 266):

> Imagine that parents have a genetic predisposition for high cognitive skills and patience, which they pass on to their children. Children who inherit this predisposition respond more favorably to parents' highbrow inputs than do children who do not possess this predisposition because acquiring highbrow culture requires cognitive skills, patience, and time (Ganzeboom 1982; Notten et al. 2015).

This view is in line with the first author's past research, where lowbrow cultural tastes were simply defined as those that did not require cognitive skills (the criteria used to arrive at that conclusion are of course unknown): "going to the cinema or to a rock concert are considered lowbrow activities, based on their wide availability, their relative inexpensiveness, and the fact that they do not require particular cognitive competency" (Katz-Gerro and Jæger 2013:247). Conversely, highbrow practices, such as listening to classical music, are deemed more complex and demanding.

What "cognitive skills" or "cognitive competency" are is not entirely clear. No definition is offered, and the overall argument put forth by the authors is meaningful insofar as we adhere to commonsensical notions about the level of "smartness" of different cultural practices. Hence, if we are to follow the authors' arguments, listening to Chopin's nocturnes is, for instance, intrinsically more cognitively demanding than watching the Danish television show *Borgen*. But how does one scientifically gauge the level of skills involved in these two activities? The answer, which is essentially tautological, ends up naturalizing cultural hierarchies: highbrow practices are highbrow because they require specific cognitive competency, and vice versa. Once this operation of naturalization is achieved, it becomes conceivable to attribute the unequal repartition of these skills to a gene-centric process that is almost entirely detached from the historical vicissitudes of social struggles.

Notes

1 One could also argue that part of the genetic effect does not transit through parents, which might be due to the effects of dominance, epistasis, or measurement error in the trait itself. Yet, this might need to be empirically documented.

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