Heritability and Genetic Causation

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The method in human genetics of ascribing causal responsibility to genotype by the use of heritability estimates has been heavily criticized over the years. It has been argued that these estimates are rarely valid and do not trace genetic causation. Recent contributions strike back at this criticism. I present and discuss two opposing views on these matters represented by Richard Lewontin and Neven Sesardic. I suggest that the conflicting perspectives are based in differing concepts of genetic causation and differing motivations and contexts of discussion. I use the distinction between structuring and triggering causes to clarify the basis for the opposing views.

1. Introduction. The interactionist consensus of nature and nurture acknowledges that causal influences from genotype and environment are both necessary but separately insufficient in causing human development and behavior.[1] This position is now considered a truism, and there are hardly any accounts disputing it. However, the nature-nurture controversy is not resolved by accepting this rather trivial notion of interactionism. There is still discussion as to whether it is possible to separate environ-

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1. Following Carey's (2003) terminology I here talk about interaction in a *loose* sense, meaning that both genes and environment contribute to traits. This is different from interaction in a *strict* sense, which is equivalent to statistical interaction. Even if genes and environment always can be said to interact in a loose sense, they may not always interact statistically. I will discuss statistical interaction as nonadditivity in a later section.

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mental and genetic causes and quantitatively estimate the relative genetic influence on phenotypic differences in specific traits. A method of measuring the strength of genetic influence in behavioral genetics is heritability analysis, a statistical method based on a linear analysis of variance, which has been discussed and criticized for several decades.[2]

Personality traits, cognitive performance, alcoholism, and mental disorders like schizophrenia are some traits that have been subject for heritability studies in human populations. In the 1970s and 1980s several philosophers and scientists criticized hereditarianism, particularly the article on heritability of IQ by Arthur Jenssen (1969). Substantial elements of the criticism concerned the methodological and conceptual grounds for heritability analysis. In the article "The Analysis of Variance and the Analysis of Causes," Richard Lewontin ([1974] 1976) aimed to show that the analysis of variance fails to trace genetic causation, and his arguments became an important basis for an antihereditarian consensus in the philosophy of science. This conceptually based criticism has rarely been challenged, although some attempts have been made, more recently by Neven Sesardic (1993, 2000, 2003), who has set out to disrupt the antihereditarian consensus.

The aim of the present study is to examine and evaluate parts of Sesardic's attempt to strike back at antihereditarianism. I focus on two disagreements between Lewontin and Sesardic (introduced in Sesardic 1993). One is whether heritability estimates actually trace genetic causation, and the other is whether heritability estimates are (or can be) useful at all. I will argue that the former disagreement has a basis in different conceptions of genetic causation, and that the latter has its source in different motivations and contexts of discussion. To illuminate the two conceptions of genetic causation present in the discussion, I use the distinction between structuring and triggering causes.[3] I will argue that heritability analysis can trace genetic causation based on the use of a local notion of causation. But even if the analysis has this capacity, I aim to show that heritability results have limited implications, and that the method lacks the support to withstand other important parts of the traditional conceptual and methodological criticism.

2. Heritability and the Analysis of Variance. Heritability analysis is based on the Analysis of Variance (ANOVA), and the goal is to estimate how

2. Heritability analysis is still in use, but new and powerful techniques in genetics demand other statistical methods, as for instance QTL-analysis (Quantitative Trait Loci Analysis) in gene mapping.

3. The terminology of structuring and triggering causes is borrowed from Dretske (1995).

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much of the variance in a phenotypic trait can be attributed to genetic variance. Variance is a measure of the degree to which the scores of a trait are dispersed away from the mean. For an estimate of high variance there are more individual phenotypic differences for the trait in question than for an estimate of low variance. One key premise for heritability measurements is to assume that phenotypic variance ($\mathbf{V_P}$) of a trait in a population may be expressed by one component of genetic variance ($\mathbf{V_G}$) and one of environmental variance ($\mathbf{V_E}$), so that

$$\mathbf{V_P} = \mathbf{V_G} + \mathbf{V_E}.$$

Genotype-environment correlations and interactions are assumed to be nonexistent or minimal. These assumptions are required to solve the equations for the interesting parameters.[4] The heritability coefficient, $\mathbf{H^2}$, is given by the ratio of the total genetic variance to the phenotypic variance:

$$\mathbf{H^2} = \mathbf{V_G}/\mathbf{V_P}.$$

This measure is called broad heritability (Lush 1943).[5] Heritability is understood as the proportion of phenotypic variance ascribable to genetic variance, or in other words, as the extent to which genetic differences contribute to differences in a phenotypic trait.

A heritability measure close to 1.0 for a trait indicates that almost all phenotypic variance in the population results from variance in genotypes and not from environmental variance (Griffiths et al. 1996, 832). However, one cannot determine from the heritability analysis alone whether the high heritability estimate results from a low sensitivity of the trait to changes in environment or from a high similarity of environments in relevant conditions for the trait.

A heritability measure close to 0 for a trait indicates that almost all phenotypic variance is due to environmental variance and that genetic differences hardly contribute to phenotypic differences in the trait. The reason could be that genotypes are very sensitive to environmental influence for the trait in question. It cannot be ruled out, though, that genes

4. For details on the assumptions and equations of heritability analysis and the method of twin studies see e.g., Carey 2003.

5. There is also a measure called narrow heritability, *h*, that is only based on additive genetic variance. Broad heritability is based on total genetic variation, which is the sum of additive variance, dominance variance and epistatic variance. When there is dominance and epistatic variance, some alleles or genes will suppress or interact with the effect of others. The measure of broad heritability is used in relation to psychological traits, because the total genetic contribution to these traits is of interest. However, in breeding of domestic animals, narrow heritability is more useful, because this measure is taken to give the best indication of what traits would be worth trying to breed.

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influencing the trait are fixed in the population, that is; all individuals in the measured population have identical genotypes influencing the trait in question. To control for this, other methods must be used.

The analysis of variance cannot alone be expected to trace causes; it only traces correlations. However, when the analysis is used in a previously established causal context, it is normally expected to give some causal information. As I will discuss in later sections, Lewontin and Sesardic seem to disagree about the causal context of heritability analysis.

3. The Locality Objection. Lewontin points to that heritability analysis gives information about a particular population in a certain set of environments. In a different population or in a different environment the results might be different. These spatiotemporal limitations make the analysis local and unable to ground general statements about causal relations. From the locality objection[6] Lewontin aims to establish two claims; since heritability analysis is a local analysis,[7] (1) it does not reflect genetic causation, and (2) it is useless in the pursuit of finding an "index of efficacy of environmental or clinical intervention in altering the trait either in individuals or in the population as a whole" (Lewontin [1974] 1976, 179). Both claims will be more fully explained in the following.

Lewontin holds that the Norm of Reaction analysis is a far better alternative than heritability analysis in the assessment of relative contributions of genes and environment (Lewontin [1974] 1976, 184). In an analysis of the Norm of Reaction, a table or graph is presented showing phenotypes that would result from the development of chosen genotypes in a large range of environments. Each line in a Norm of Reaction graph represents data from different genotypes. If the lines are parallel (and not horizontal) there is an effect of both environmental and genotypic variation, but there is no genotype/environment statistical interaction. In this situation there is a perfect additive relation between genotype and environment, which means that the differences between genotypes are the same in all environments. Thus, the change in phenotypic outcome in different environments can be predicted from the genotype. In situations of additivity, heritability estimates are no longer just local. The result from one environment can be extrapolated to other environments.

The interesting cases, at least to Lewontin, are when the norms of reaction lines are not parallel and when they cross. In these situations there is a statistical interaction between genotypes and environment, and

6. Following the terminology of Sesardic (1993).

7. Except from the case of additivity, to which I will return.

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the relationship between them is nonadditive. The change in phenotype for one genotype cannot be predicted from another genotype.

The assumption of additivity (or approximate additivity) is necessary for heritability analysis to be relevant outside the population and environment sampled. Lewontin holds the assumption of additivity to be wrong in most cases. He argues that additivity of phenotypic traits is probably rare in nature (Lewontin [1974] 1976, 191), and points to classical Norm of Reaction analyses conducted for larval viability in *Drosophila* (Dobzhansky and Spassky 1944) and growth in clonal plants (Clausen, Kek, and Heisey 1940). These experiments show a large amount of nonadditivity in the measured traits. It is very difficult to obtain Norm of Reaction data for complex traits in humans; however, Lewontin finds it plausible to assume that human behavior cannot be expected to show more additivity than the simpler traits in *Drosophila* and clonal plants.

I then interpret Lewontin's argument to go as follows:

> P1. Additivity of environmental and genetic causes of traits is a major assumption for analysis of variance to measure the strength of genetic causation.
>
> P2. Additivity is rare.
>
> C. Heritability Analysis rarely reflects genetic causation.

Even if this is the key argument of the article, Lewontin's main message seems not to be how the analysis of variance fails to reflect causation. His main concern is rather what he calls the chief programmatic fallacy (Lewontin [1974] 1976, 179). The fallacy is, as already mentioned, to assume that heritability estimates give an index of the efficacy of environmental intervention in a trait. It has been argued that when there is high heritability of a trait there is not much use in trying to change it environmentally. A more concrete example is that since IQ seems to have high heritability it has been argued that there is not much use in trying to change IQ scores by investing large amounts of money in education for groups with lower IQ rates. These arguments are Lewontin's main target, and he aims to cement the view that a measure of high heritability will not have implications for this level of environmental interventions. The locality of heritability analysis makes the heritability estimates susceptible to change if relevant environmental parameters change. This means that if a trait is measured to have high heritability in one environment, the trait may have lower heritability in another. Since the outcome of the analysis is dependent on the environmental distribution, it seems difficult to draw any conclusions about the efficacy of environmental interventions from the estimate of heritability; a high estimate of heritability will not imply that environmental interventions are ineffective.

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Lewontin's criticism against heritability analysis for being local can also be interpreted somewhat differently. I think it is reasonable to suggest that a view of causes as tightly connected to laws and global or general relations is close to a widespread use in everyday language about science. When the public is presented with scientific results telling that IQ is genetically caused, it might easily be taken to mean something like a law-like relationship and not a local result as is the case using the analysis of variance. If this is the situation, it is reasonable for Lewontin to criticize the analysis of variance for not reflecting universalizability, as the results would often be interpreted as universal by policymakers.

Neven Sesardic (1993) aims to undermine Lewontin's main points. He holds that (1) additivity is not necessary for tracing causation by the analysis of variance; to him a local analysis is still a causal analysis. He also argues that (2) additivity is not needed to make extrapolations from one environment to another as long as they are close. Additionally, he holds that (3) additivity is not that rare in nature, which implies that heritability analysis on several occasions can ground more general causal claims. And finally, (4) he finds heritability analysis to be very useful in different types of research. I will discuss these claims in the following.

4. Two Modes of Causation. Considering the first of Sesardic's claims, I find him to make a convincing case that heritability analysis actually can trace genetic causation, but this in a framework of local causation. What is needed to make a local causal claim is to show that a difference in one factor is making a difference in another factor in a specific context. This is a minimal causal claim, nonetheless it is causal. Lewontin demands more to make a causal claim. He requires that a difference in one factor has similar effects in several situations. He might not request a global or law-like relationship, but at least a degree of generality that secure the causal claim to be valid in other relevant contexts. This kind of generality is needed to make extrapolations, but it may not be necessary to make a causal claim.

Some of the divergence in Lewontin's and Sesardic's views of genetic causation can be illuminated by invoking the distinction between structuring and triggering causes. Fred Dretske (1995) gives an analysis of structuring and triggering causes that I draw from, but I do not use the terms in exactly the same manner as Dretske.

One example, explaining the suggested distinction, is the scenario where a computer operator moves the cursor on a screen by pressing a key on the keyboard (Dretske 1995, 121–122). In this situation the pressure on the key is the triggering cause of cursor movement. But there are several other relevant conditions that could serve as a causal explanation of that same event. Examples of such conditions are the actual electrical con-

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nections in the computer (hardware) and programming (software). However, in the present scenario, hardware and software are thought of as background conditions or part of normal circumstances, although highly causally relevant for the outcome of the key pressing event. These can be thought of as structuring causes. The triggering cause is in normal circumstances the cause we ask for in a defined causal context, while the structuring causes are all the relevant causal factors thought of as background conditions in the same causal context.

The causes asked for in heritability analysis are genetic differences, and can be thought of as triggering causes in this context. When heritability analysis is conducted in a chosen set of environments there are important background conditions that are viewed as 'normal circumstances'. These are, among others, (1) the diverse influences of the environments chosen, and (2) the underlying multiple causal pathways inside the organisms and between the organism and its surroundings, including complex feedback systems and multilevel mechanisms. These conditions are causally relevant for the outcome of heritability analyses, however, they are not the causes asked for. They are structuring causes in the context of heritability analysis.

If a heritability analysis results in a high heritability estimate for a trait, the variation in genotype is interpreted to be an important causal factor given certain background conditions, expecting that if the conditions changes, the effect could be different. However, the success of the analysis is based on that background conditions do not change to a significant extent. Lewontin emphasizes what happens when background conditions *do* change. He focuses on the importance of differing environments and on the complex multiple causal pathways that are not allowed a role in heritability analysis. He calls attention to the structuring causes and the possibility that they are plastic and changing. Sesardic steers the attention away from the potential plasticity of structuring causes. He focuses on the possibility of tracing triggering causes by assuming a degree of fixation of other causal pathways, a widely used way of tracing causes in scientific research. Thus, Sesardic downplays the structuring causes while Lewontin emphasizes them.

5. Additivity and Extrapolation. As stated, Sesardic seems to agree with Lewontin that heritability analyses often should be considered local, but Sesardic also holds that as long as there is a strong prevalence of the environments measured "genetic differences will tend to manifest themselves consistently in phenotypic differences" (Sesardic 1993, 402–403). He argues that when making local causal inferences, it is not essential, not even desirable, to infer general or global causal relations over the whole environmental range. He finds it impossible and uninteresting to

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assess all possible environments, and argues that it is the effect of genotype given the dominant environments that is interesting. Sesardic seems to argue that as long as environments are close, additivity is not a necessary precondition to make extrapolations from one environment to another: "the less a new population differs from the original one, the more reasonable it would be to expect similar heritability values" (Sesardic 1993, 404). I think this claim has difficulties. One reason is that it is probably very hard, if not impossible, to judge whether complex environments influencing complex behavioral traits actually are close or not. Environments influencing for instance IQ could change substantially from family to family even within the same socioeconomic group. Seemingly small differences in environments could turn out to have large effects on the heritability of phenotypic traits. The great difficulties in controlling for environments in complex behavioral traits also make it hard to tell whether the environmental range one is sampling from is small or large. Therefore, when Sesardic claims that it is only the prevalent environment that is interesting, I find him to underestimate the actual complexity of environments and unpredictability of the effects that seemingly small environmental changes could have.

The case would be different if an additive relation between genotypes and environments was established for a trait. In cases of additivity, heritability estimates indicate a general relationship between genotypes and traits, which can be extrapolated to other environments. Sesardic tries to establish that additivity is common in complex behavioral traits, but he is not able to present empirical evidence. He rather shifts the burden of proof by citing several authors stating that it is difficult to find evidence for nonadditivity in the behavioral domain (e.g., Plomin 1986, 108; McGue 1989, 507). However, several of the authors he refers to in order to back up his point do not argue beyond that interactions are ignored due to the traditional and problematic assumption that interactions are ignorable in heritability analysis (see Sesardic 1993, 407). Sesardic also cites several people that have been much criticized for ignoring the possibility of statistical interactions (e.g., Jensen 1981, 124).

It is very difficult to judge on an empirical basis if additivity is rare or common in nature, since experiments addressing complex behavioral traits are extremely difficult to conduct. However, as mentioned, several experiments on more simple traits in plants and animals show that many traits are nonadditive. It is still an open question if one can infer from these experiments that similar nonadditvity exists in behavioral traits in humans. It is often assumed that the more complex a trait is, the more interactions one can expect to find (see e.g., Lush 1994, 158). Assuming this, the generality of heritability measurements can be expected to decrease when the complexity of the traits in question increase. But as for

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the opposing claims from Sesardic, these are speculations that are very hard to support empirically.

6. Differing Motivations and Contexts of Discussion. I suggest that the disagreement about the usefulness of heritability analysis has an important basis in different contexts and agendas for discussion. It is an important aim for Lewontin to establish that heritability estimates are useless as a scientific basis for relocating social policy resources, since it is mistaken to assume that traits are not responsive to environmental interventions if heritability estimates are high (Lewontin [1974] 1976, 179). An intensely debated example is the question whether it is any use in policies aiming to improve intelligence in populations of lower performance if IQ is measured to have a high heritability (see e.g., Block and Dworkin 1976). Lewontin's strong assertions about the uselessness of heritability analysis need to be evaluated on the background of the IQ-case, as he, well argued, finds heritability measurements to have very limited applications in this situation. I find it reasonable for Lewontin to emphasize structuring causes in this case, as the background conditions for IQ heritability analysis seem to be very plastic and therefore limiting for the success of the heritability analysis (see e.g., Turkheimer et al. 2003). As long as additivity is not firmly established, heritability cannot be taken as a measure of how effective certain social policies will be.

Sesardic strongly opposes that heritability analysis is useless on a general basis. He points to important applications in cancer research, where it can be used to single out what factors are causally relevant in cancer development. He holds that the analysis of variance cannot provide the full causal story about how, and under what conditions, the cancer develops. But he certainly finds that at least some causal information can be drawn from the analysis; it can for instance help to find what factors are causally relevant for further research (Sesardic 1993, 405). When it comes to cancer research, I find it more justified to focus on genetic variation as a triggering cause, because the goal of the investigation is to single out causal factors for further analysis and not to find a basis for developing social policies. Thus, possible plasticity in background conditions may not matter to the same extent, and the goal of analysis is not as ambitious as for the IQ case. The results are taken to be local, but will still give an indication of what factors are important in relevant environments. The plasticity of background conditions in cancer research could also be expected, in some cases, to be less than for the IQ case, as environmental factors to a greater extent than the IQ example may be controlled for. It is probably more difficult to control for the microenvironmental and macroenvironmental factors in a family, a neighborhood or a society that influence complex behavioral traits like IQ than for some

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of the environmental factors that can have a huge impact on cancer development, for instance hormone intake, smoking, pollution and radiation. Still, the case of cancer research is immensely complicated, so these suggestions remain bare speculations.

I suspect that Lewontin could acknowledge the use of heritability analysis in less ambitious contexts than the IQ case. When Lewontin claims that heritability analysis is totally useless, I think it is reasonable to interpret him as still speaking in the context of IQ and social policy changes, and that he might be open to Sesardic's suggestions of other applications set in totally different contexts.

7. Disturbing a Consensus? Is Sesardic disturbing the interactionist and antihereditarian consensus that is taking place among many social scientists and philosophers of science? I think Sesardic is right in claiming that, although a local analysis, heritability analysis can trace genetic causation. Canceling out common causes and only take into account those causes that make a difference on a certain background is a common and very important method of singling out causes in science (see e.g., Lush 1994, 159). Thus, even if it is difficult to obtain high quality conditions for empirical research on heritability, it is not in principle impossible to obtain causal information from heritability analysis. But this is causal information of very limited application unless additivity is common in nature, and is additivity common in nature? This is an empirical question on which there are few good results, but at least empirical research indicates that several simple traits in plants and animals are nonadditive.

Lewontin makes his case very general against the analysis of variance, however, his conclusions seems to depend on the setting of IQ measurements and policy changes. Even if he judges heritability analysis to be useless, I suspect that he would agree with Sesardic that it might be useful in some research contexts. After considering Sesardic's counterarguments I still find that Lewontin has demonstrated important methodological limitations of heritability analysis, however Lewontin's statements that the method is totally useless and without the capacity to trace genetic causation do not stand.

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