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Abstract

Participants in debates about developments in science and technology point to issues overlooked or downplayed by scientists—or, if the debate is among scientists themselves, by *other* scientists. Sometimes included among participants in debates are interpreters of science—sociologists, historians, philosophers, and scholars from other fields of Science and Technology Studies. Taking these scholars as the audience, this article asks what should we do if we identify a significant issue *not yet subject to debate*? In particular, what should we do when the overlooked issue is *conceptual*—a matter of how inquiry is framed—more than it is a matter of analyzing the evidence or applying the results? I address the title question in-principle, but my thinking is informed by the range of ways I have been working to influence research related to a specific case (summarized in an appendix). I do not argue for particular actions or provide a how-to guide; my goal rather is to promote more systematic attention to the mostly implicit models that scholars interpreting science have regarding their aspirations and strategies for influencing science.

Keywords: Conceptual systematization

Participants in debates about developments in science and technology point to issues overlooked or downplayed by scientists—or, if the debate is among scientists themselves, by *other* scientists. Anti-fluoridation campaigners noted the decline in cavities in communities with non-fluoridated water supplies (Martin 1991); AIDS activists questioned the ethics of protocols for clinical trials and Cumbrian sheep farmers pointed to possible sources of irradiation other than Chernobyl (Sismondo 2008); skeptics of associations of human cancers with cell phone use invoke the inaccuracy of self-reported phone use (Schmidt 2018); and so on. Sometimes included among participants in debates are interpreters of science—sociologists, historians, philosophers, and scholars from other fields of Science and Technology Studies (STS). Taking these scholars as the audience, this article asks what should we do if we identify a significant issue *not yet subject to debate*? In particular, what should we do when the overlooked issue is *conceptual*—a matter of how inquiry is framed—more than it is a matter of analyzing the evidence or applying the results?

There is, of course, no reason to expect that an overlooked issue raised by an STS scholar would be seen as a decisive contribution to science. After all, in establishing and contesting knowledge, scientists mobilize heterogeneous resources: equipment, experimental protocols, citations, the support of colleagues, the reputations of laboratories, metaphors, rhetorical devices, publicity, funding, and so on (Latour 1987; Law 1987; Clarke and Fujimura 1992, 4-5). Understanding that does not warrant dispensing with the *what to do* question. In this article, I address the title question in-principle, but my thinking is informed by the range of ways I have been working to influence research related to a specific case (which is summarized in the appendix). I do not argue for particular actions or provide a how-to guide; nor do I tackle the larger subject of researchers overlooking the ways their research is shaped by social context. Nevertheless, I hope that my consideration of what to do about overlooked conceptual issues stimulates more systematic attention to the mostly implicit models that STS scholars have regarding their aspirations and strategies for influencing science. This goal stands even if readers who consult the appendix deem not significant the specific case of an overlooked conceptual issue that has concerned me.

1. The in-principle question

Readers who have already viewed the appendix know the field and the overlooked issue that have informed my thinking. Please, however, put the specific case out of mind while I try to motivate the general question in the title. The first step is to establish that there are times when our work states or implies that conceptual issues have been overlooked by researchers. Let me start with the easy case in which the title question's "we" refers to contributors to philosophy of biology as the field has flourished in the English-speaking world over the last 35 years.

A longstanding emphasis in philosophy of biology has been on systemization of biologists' concepts. A well-known example of this line of work has been to systemize Darwin's argument for natural selection. The central logic laid out by Darwin (1859) in the first four chapters of *On the Origin of Species* is, as I would present it using some modern language (Taylor 2001), given in Table 1.

Table 1. A conceptual systemization of Darwin's argument for natural selection*

1. IF there is

a. Variation among organisms in traits,

b. Inheritance (or reproducibility), at least partially, of traits [Chapters 1 and 2], and

c. Hyperfecundity, so that not all can survive to reproduce,

THEN there will differential representation of variant traits in lineages of organisms over time, in other words, evolution (or "modification by descent"), and a struggle for existence [Chapter 3].

2. IF those that survive and reproduce are the ones most fit to their environment, i.e., survival (and reproduction) of the fitter, or "natural selection,"

THEN evolution will result in local improvement of adaptation to conditions of existence [Chapter 4].

This is not the place to try to influence philosophers to accept my account over others (see Taylor 2001). The point, whether conceptual systemization is of Darwin's argument or in some other realm of biology, is that we are saying or implying that we make systematic and clear what researchers had not—or more systematic and clearer. Or we extend their thinking further. In short, there are things that researchers have overlooked and they are significant enough for us to spend our time examining them.

A second step in motivating the general question in the title is to establish that most of us do something with our accounts of the issue. For a start, we use our systematizations to influence our *students*, on the premise that an economical account aids their understanding and recalling of the ideas. That modest action, in turn, has ramifications, which need not always be explicit. The systematization distracts students' attention from other aspects of the original, such as Darwin's metaphors and his extensive use of cases. After all, at 124 pages, the first four chapters have enormously more content than Table 1. Conceptual systemization thus guides students towards downplaying Darwin the rhetorician in favor of Darwin the theorist. If science is held to combine rational interpretation, empirical confirmation, and influencing an audience, conceptual systemization steers our students away from science and towards philosophy. That is, attention is not focused on the methodological and expository challenges of assembling evidence to demonstrate that some evolutionary change and the adaptation of the resulting traits to the environment were produced by a process of differential survival due to the effect of the trait.

Most of us also try to influence our *philosophical colleagues* to accept our systematization as better than those of others, given, say, the logical flaws or what they omit from the original scientific work. For example, Lewontin's (1970) well-known systemization of Darwin's argument adopts the modern redefinition of the term *fitness* as the contribution of parents to numbers in the next generation. My account in Table 1, however, preserves Darwin's sense of fit-ness "to the conditions of life" (Darwin 1859, 60).

The two kinds of actions—influencing students and colleagues—may seem modest—even if in the Darwin example we consider ramifications such as steering students' attention to rational interpretation over empirical confirmation and influencing an audience. The actions suffice, however, to make the point that most of us do do something with our accounts of overlooked issues. It follows conceptually that we might examine the “what to do?” question *more systematically*. Let me propose a list of options. (The order of options is not meant to convey priority.)

1. *Stay quiet*. We may have self-doubts given the numbers of researchers involved over any extended period and science’s self-correcting mechanisms. We may also make a pragmatic decision that the other options to follow are beyond our capacities.
2. *Convey the ideas to students*, perhaps, as mentioned above, because an economical account aids their understanding and recalling of the ideas, but perhaps also because we hope, through the young, to influence the next generation of research and debates.
3. *Submit our ideas to the journals in which the researchers publish* to see if we can get the ideas recognized or have our errors exposed by reviewers. This approach is, of course, ambitious given that we have not spent our careers assembling data in the field or lab, becoming fluent with the jargon and expository style, establishing collegial relationships that allow us to solicit critical comments to help fashion drafts that can get through positive reviews, and gaining the reputation that motivates readers to read our work.
4. *Build a collaboration with researchers* to undertake the research that has been overlooked or elicit their insights about why such research is not warranted.
5. *Submit our analysis of the overlooked issue to audiences of philosophers of science*, through conversations, talks, and publications, to see if we can get our ideas recognized or have our errors exposed by reviewers more interested in concepts than in method and empirical observation.
6. *Tease out the historical, sociological, political, cultural implications* of the issue that has been overlooked and try to interest researchers from the relevant fields of interpretation of science in exploring those implications.
7. *Try to get wider public debate going* by teasing out the political implications—if they exist—of the issue that has been overlooked.

This list matches the range of ways I have been working to influence research related to a specific case (summarized in the appendix). It seems unobjectionable to me, yet I understand that, for others, not all these categories of action spring to mind. To wit: On two occasions I solicited answers to the title question at the start of presentations to audiences of philosophers and historians of science. Of the responses written on the notecards I collected back from the audiences, almost all fell in category 4 above, that is, recommending interaction with the researchers or doing the research oneself (Taylor 2011). I cannot show that these audiences were typical, but the experience matches my observation that there are very few case studies or systematic treatment of this range of ways—from direct to backdoor or indirect—to influence researchers about what they have overlooked. I return to this issue in the discussion (Section 2).

Suppose the “we” in the title question were expanded so as to include interpreters of science from other STS fields. The seven kinds of actions would still apply, but, with respect to category 2, my emphasis on concepts may seem less meaningful to students not in philosophy. At the same time, given that conceptual issues often invite *methodological* changes, the overlooked conceptual issues could be made relevant to a wider range of students (Chang 2013). Of course, interpreters of science also imply (or even state explicitly) that researchers overlook the ways their research is shaped by social context. I suspect that different kinds of actions would likely be called for to influence researchers to self-consciously address their social situatedness (Taylor 2005, 135ff). Without discounting the importance of such actions, examining them is beyond the scope of this article.

2. Discussion

The in-principle treatment in the section 1 conveys that, when interpreters of science assert or imply that researchers have overlooked significant conceptual issues, there are a range of kinds of action that might conceivably and feasibly be taken. The article is implying, as a “meta” point, that this issue—the one in the title question—has been overlooked by researchers interpreting science. Let me qualify that last claim in a number of ways.

First, as noted earlier, scientists mobilize heterogeneous resources; interpreters of science may well be interested in studying and perhaps engaging around the ways research is shaped along dimensions other than the overlooked conceptual issue. Still, as suggested by actions #6 and #7 in my list, conceptual issues may provide points of entry to the social shaping of science.

Second, there are a few relevant case studies and systematic treatments that speak to influencing researchers—scientists as well as interpreters of science—about what they have overlooked. Stanford (2006) uses historical cases to draw the attention of philosophers of science to the possibility of unconceived alternatives. Chang (2013) reviews a number of “functions” that would be served if historians of science were to engage more with the content of scientific knowledge, being prepared to judge that researchers have made misjudgements. Harman and Dietrich (2013), with an intended audience of biologists as well as historians and philosophers of biology, assembles case studies of how 18 “outsider” scientists had transformative influence on fields of the life sciences that were not their original homes. The small size of this set of sources hardly diminishes my observation that there are very few case studies or systematic treatment of this range of ways—from direct to backdoor or indirect—to influence researchers about what they have overlooked.

Third, overlooked issues, by definition, do not constitute scientific or public *controversies* as sociologist of science, Martin (2014, 21) defines them: debates that “occur over an extended period or involve a lot of people.” Nevertheless, I see a parallel when Martin, writing to an audience of activists, notes that STS studies provide few insights about strategy to participants in controversies (p. 449ff). This conclusion affirms this article’s goal of promoting more systematic attention to the mostly implicit models that scholars interpreting science have regarding their aspirations and strategies for influencing science.

Finally, Martin’s observation applies just as well to this article itself, given that I establish the title question as an issue in principle but do not say what to do to influence interpreters of science to give more attention to the question, let alone provide guidance about what actions are most appropriate when they think that researchers have overlooked a significant issue, conceptual or other. I wonder whether addressing these issues requires influencing philosophers and other interpreters of science to self-consciously address their own social situatedness or, at least, for them to get drawn into some controversy about what to do. In the meantime, I hope this article suffices to make the point that overlooked issues exist and warrant our examining the what to do question more systematically.

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Appendix. The specific case of a conceptual issue overlooked: Quantitative genetics and the - possibility of underlying heterogeneity

The crux of the specific case is that throughout its 100-year history quantitative genetics seems to have overlooked the *implications* of *underlying heterogeneity*—although relatives may be similar for a given trait because they share more genes or environmental conditions than unrelated individuals, the genes and environmental conditions underlying the development of the trait *need not be the same from one set of relatives to another*. It is not the possibility of underlying heterogeneity by itself that makes it seem significant to me, but its implications for the analysis and interpretation of classical and modern quantitative genetics.

As laid out in Taylor (2014), claims that some human trait, say, IQ test score at age 18, shows high heritability derive from an analysis of data from relatives. For example, the average similarity of pairs of monozygotic (MZ) twins (who share all their genes) can be compared with the average similarity of pairs of dizygotic (DZ) twins (who do not share all their genes). If the former similarity exceeds the latter, it is reasonable to associate that with the greater genetic similarity of twins in a MZ pair (give or take possibly greater similarity in upbringing of twins in a MZ pair raised together). The more MZ similarity exceeds DZ similarity, the higher what quantitative genetics calls the *heritability* of the trait.

Researchers and commentators often describe heritability calculations as showing how much a trait is *heritable* or *genetic*. However, no genes or measurable genetic factors (such as alleles, tandem repeats, or chromosomal inversions) are examined in deriving heritability estimates, nor does the method of analysis suggest where to look for them. Moreover, even if the similarity between twins or a set of close relatives is associated with the similarity of yet-to-be-identified genetic factors, *the factors may not be the same from one set of relatives to the next, or from one environment to the next*. In other words, the underlying factors may be *heterogeneous*. It could be that pairs of alleles, say, AAbbcbDDee, subject to a sequence of environmental factors, say, FghiJ, are associated, all other things being equal, with the same outcomes as alleles aabbCCDDEE subject to a sequence of environmental factors FgHiJ (see Figure A.1 for the case of human twins where both members of each pair are raised in the same household).

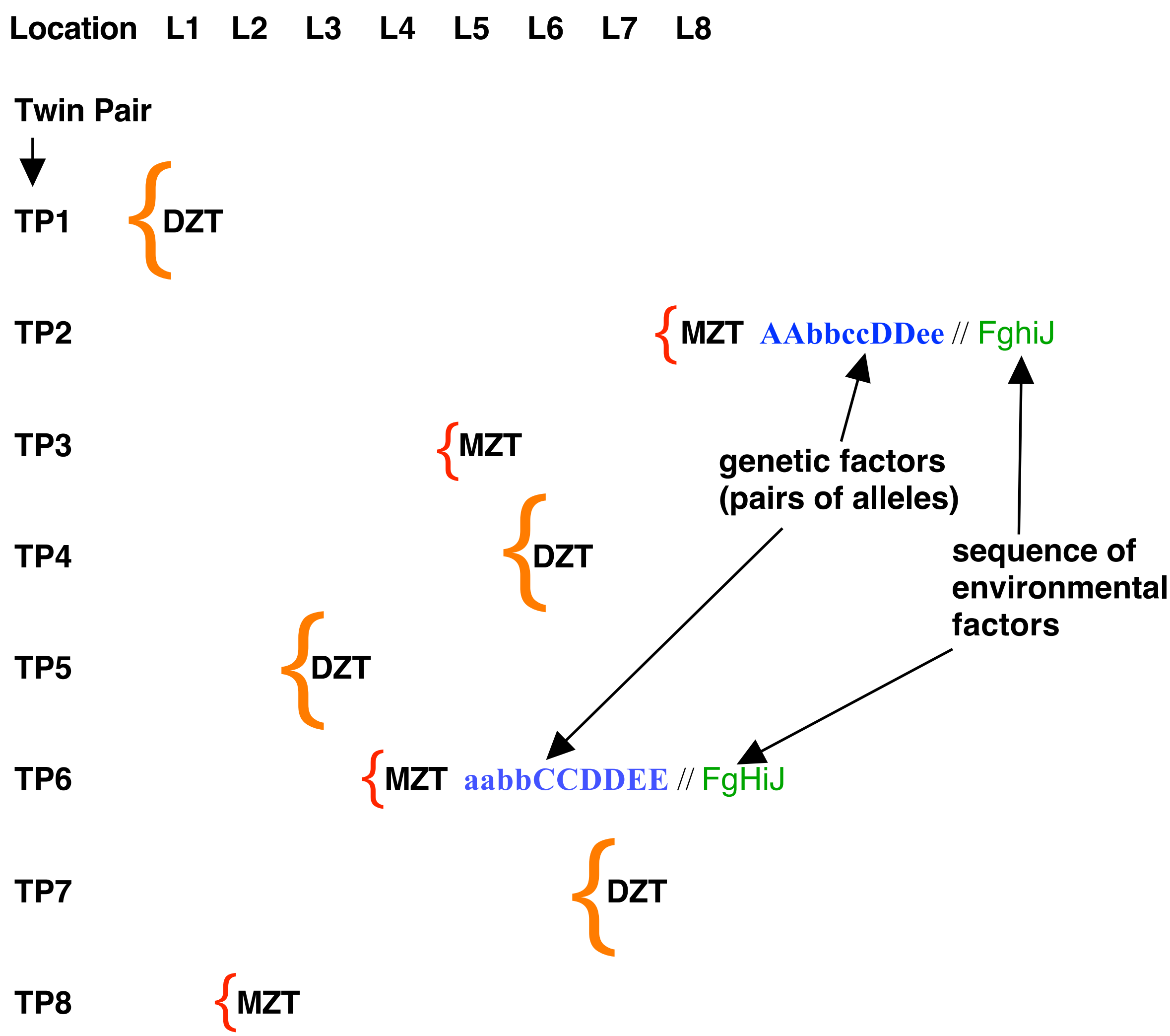

Figure A.1. Factors underlying a trait may be heterogeneous even when identical (monozygotic) twins raised together (MZT) are more similar than fraternal (dizygotic) twins raised together (DZT). The greater similarity is indicated here by smaller size of the curly brackets. The underlying factors for two MZT pairs are indicated by upper- and lowercase letters for pairs of alleles (A-E) and the environmental factors to which they are subject (F-J).

The gap between homogeneous and heterogeneous genetic and environmental factors influencing the development of a trait has not (yet) been recognized as a noteworthy concern by quantitative geneticists or by critical commentators on heritability research (e.g., Downes 2015 and references therein). To allow readers to appreciate why it concerns me, let me examine some implications. These implications may make more sense, however, if I first clarify some terminological issues that, in my experience, make it hard for people to visualize the difference between observable genetic factors and calculations based on observations of traits, such as heritability calculations.

Heritability sounds like *heritable*; it should not be surprising that popular accounts elide the distinction. (To cite a recent example: "a large study of twins has found links between genes and which university people go to"; Wilson 2018.) Researchers do not help either when they define heritability as the "contribution of *genetic differences* to observed differences among individuals" (Plomin et al. 1997, 83; my emphasis) or the "fraction of the variance of a phenotypic trait in a given population caused by (or attributable to) genetic differences" (Layzer 1974, 1259). Admittedly, such definitions do not strictly rule out the possibility that, from one set of twins to the next, *different* genes are linked to similar, say, university admission outcomes, but they do not draw attention to that idea.

To appreciate that "genetic" in the technical definition of heritability does not refer to genetic in the sense of stretches of the DNA in the genome, consider the partitioning of variation of a trait into fractions that is the foundation of classical quantitative genetics. This is a field that arose in agriculture, where multiple varieties of plants can be grown in repeated plots in many locations (Figure A.2). For a given trait, say, yield per unit area, the variation can be partitioned (through the statistical technique of Analysis of Variance [ANOVA] and its kin) into four components: a. between the means for each variety when averaged across locations (V_A, V_B, etc.); b. between the means for each location when averaged across varieties (L_1, L_2, etc.); c. between the means for each variety-location combination when averaged across plots (and after taking out a. and b.; not depicted); d. what is left over or *residual.* (Components c and d are combined in the curly brackets of Figure A.2.)

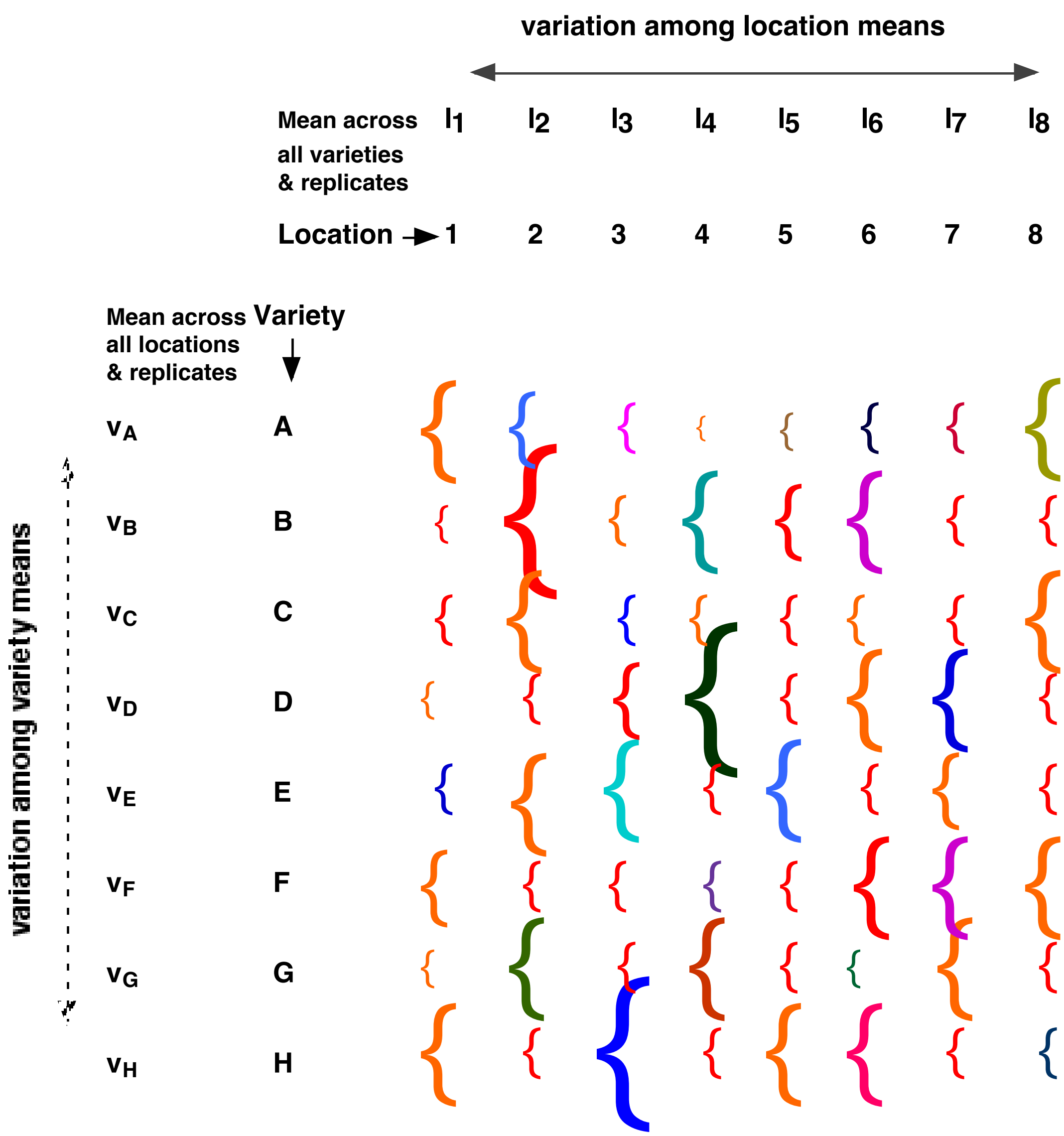

Figure A.2. Partitioning of variation in the ideal agricultural evaluation trial where each of a set of varieties is raised in each of set of locations, and there are two or more replicates in each variety-location combination. The variation among replicates within variety-location combinations is indicated by the size of the curly brackets. (The agricultural evaluation trial contrasts with Figure A.1 in which the replicates of any variety—twin pairs—are raised in only one location—household—per variety.)

Heritability, technically, is the ratio of the variation among variety means to the total variation for the trait (a ratio that is contingent on the specific set of varieties and locations). There is no conceptual or empirical connection between such a measure of variation in a trait and differences among individuals in their genes. The distinction becomes obscured, however, when the following moves are made: varieties are referred to as *genotypes*; the variation among the variety or genotypic means across locations is called *genotypic variance*; this term is shortened to *genetic variance*; and that quantity is interpreted, ambiguously, as the fraction of variation in a trait associated with "genetic differences" (Taylor 2014). The origin of these common moves can be traced to the models used by quantitative genetics to partition trait variation. In order to take different degrees of relatedness into account (e.g., MZ twins being more closely related than DZ twins), these models posit theoretical, idealized genes that have simple Mendelian inheritance and direct contributions to the trait. Yet, given that the partitioning is of variation in *traits*, it must be possible to partition variation without using models of unobservable genes and their hypothetical effects (Taylor 2012; Note that, although theory and simulation for gene-free analysis show that human heritability estimates are unreliable and typically overestimate the correct figures, this is a separate overlooked issue from the one raised in this appendix.) If such *gene-free* analysis were standard practice—it is not—it would be easier to keep sight of the distinctions that are summarized in Table A.1.

Table A.1. Three conceptually and empirically distinct senses of the term “genetic”

“Genetic”	**Area**	**Focus**	**What varies among subjects**
Sense 1	Quantitative genetics	trait	components of variance of trait, partitioned using ANOVA
Sense 2	Relatedness	variable part of genome	fraction of variable part of genome shared by relatives
Sense 3	Population and Molecular genetics	site(s) on chromosomes	heterozygosity at site(s)

The distinction between *observable genes* and calculations based on *observations of traits*, such as estimation of heritability, might break down if there were a gradient of measurable, albeit yet-to-be-identified factors running through the variety/genotype/twin-pair means in Figures A.1 and A.2. The existence of such a gradient, however, need not be the case, as is obvious when we think about, say, human height. That development of height occurs through pathways that involve diverse combinations of genetic and environmental factors—not a single gradient—makes intuitive sense when we note the different timing of growth (e.g., early spurt, late bloomer) and the make-up of the final height (e.g., long trunk, short legs versus short trunk, long legs).

If we keep firmly in mind the distinction between observable genes and calculations based on observations of traits, it is possible to delineate the approaches researchers can take on the basis of heritability estimates *when the underlying factors are unknown and possibly heterogeneous*. As noted in Taylor (2014), researchers might:

1. seek to *identify the specific genetic and environmental factors without reference to the trait’s heritability* or the other fractions of the total variance in the given trait;
2. take high heritability as a heuristic indicator that the trait is potentially worthwhile candidate for molecular research but *expect many fruitless molecular investigations*. Such investigations may bear fruit for that (unknown) fraction of high-heritability traits for which the underlying factors are *not* heterogeneous;
3. *restrict attention to variation within a set of relatives*. This path makes sense because, even if the underlying factors are not known, high heritability still means that if one twin develops a trait (e.g.,

type 1 diabetes) the other twin is more likely to as well. The second twin might be advised to take measures to reduce the health impact if and when the disease started to appear for that twin. However, notice that this path assumes that the timing of getting the condition differs from the first twin to the second; the factors influencing the timing could well be heterogeneous;

4. focus on *heritability as a fraction of the variation* and put aside any search for associated genes or environmental factors. This focus is useful in agricultural and laboratory breeding as a heuristic to predict advance under selective breeding. Ditto for evolutionary biology to the extent that it borrows the models and practices of artificial selection. (If the actual advance is less than predicted, one source of the discrepancy might be the underlying heterogeneity of genetic factors and their re-assortment through mating. Yet the discrepancy matters little because breeders can always compensate: they discard the undesired offspring, breed the desired ones, and continue. Of course, selective breeding is not an acceptable option for humans); and
5. *restrict the range of varieties or locations* (and thereby reduce any underlying heterogeneity). Agricultural researchers can do this in a number of ways: restrict the range of locations in which a variety is raised or grown; control environmental conditions, such as (for animals) the regimes of feeding and husbandry or (for plants) the application of fertilizer and irrigated water; or produce inbred lines and thereby eliminate the heterogeneity of genetic factors that characterize outbred varieties. In the study of human traits, it is not feasible to control the full range of relevant environmental conditions or to breed for genetic uniformity, but there are some ways to restrict the locations included, e.g., to include only families of low socioeconomic status (Turkheimer et al. 2003).

The set of five approaches above is quite circumscribed; this makes the implications of overlooked possibility of underlying heterogeneity significant, at least in my assessment. It diminishes the utility for medical research and potential treatment not only of the results of classical quantitative genetics but also of Genome-Wide Association studies (McCarthy et al. 2008). And it puts an exclamation point on the scientific consensus that most medically significant traits are associated with many genes of quite small effect. The possibility pushes back against the enthusiasm of researchers who accept "the estimation of genetic variance in populations [and want to move] to the detection and identification of variants that are associated with or directly cause variation” (Visscher et al. 2007).

Concrete examples under the seven categories of actions in section 1

Section 1 in the body of the article motivated the title question in principle: interpretation of science involves claims that researchers overlook significant issues and there are a range of kinds of action that we might take. Regarding the specific case of an overlooked conceptual issue described above, let me now list actions I took between 2004-14 (extending at times into broader concerns about nature-nurture debates) (Table A.2). This list should indicate that actions under each category proposed in section 1 are not simply conceivable in principle, but *feasible.* Making this point does not depend on my recounting the details of each action or soliciting readers’ assessments of my successes and failures. (Notes at the bottom of the table, however, provide relevant URLs for anyone wanting to follow up.)

Table A.2. Some actions I took between 2004-14 regarding the specific case described above

1. Stay quiet	Notwithstanding the actions summarized in the categories below, I did not center my research, presentations, and profile as a scholar on underlying heterogeneity. I made a pragmatic decision to keep the focus of my research on social epidemiological approaches that address the life course development of health and behavior (Taylor 2004, 2018).
2. Convey the ideas to students	Not much action: One session only in a semester-long doctoral course on Epidemiological thinking (Taylor 2018).[1]
3. Submit ideas to science journals	The article published as Taylor (2012) was the end result of a sequence of submissions to science journals of progressively less status. Along the way, implications were stripped out or toned down to get around reviewers who, without identifying errors, resisted the effort to revisit what they considered had already been critiqued or shown not to be a problem or was no longer of interest. (The experience matched that described in Myers 1986.) A major university press recommended publication in some alternative forum to get the ideas vetted by more scientists before the Press would send a book manuscript out for review. This advice led to the independent publishing of Taylor (2014) (which addressed more issues than underlying heterogeneity).
4. Build a collaboration with researchers	An NSF SGER (Small Grants for Exploratory Research) budgeted for visits with researchers. Some researchers were not open to visits; others were, but the visits were shorter than needed to builds collaborations.
5. Submit ideas to philosophy of science journals	Most of my efforts fit under this category, resulting in 9 articles and 5 commentaries.[2]
6. Tease out the historical, sociological, political, cultural implications	Session at joint meetings of STS societies, Vancouver 2006.[3] Visiting fellowship at KLI near Vienna 2008 & 2010. New England Workshops on Science and Social Change, at Woods Hole 2009 and Coimbra Portugal in 2012.[4] Planned blog of manuscripts and reviews (modeled on Myers 1986) [not undertaken]. "Why look at genes" series of blog posts, 2014[5]
7. Tease out the political implications	Joined and participated in Genes and Society Working Group.[6] Long interview with reporter planning a review for *Science* of developments in nature-nurture science [no article emerged]. Proposal for new book, *Troubled by Heterogeneity,* on heterogeneity more generally than with regard to heritability.[7]

Notes: 1. http://www.faculty.umb.edu/pjt/epi; 2. http://www.faculty.umb.edu/pjt/cv.pdf; 3. http://www.faculty.umb.edu/pjt/4S06.html; 4. http://www.stv.umb.edu/newssc09.html & http://www.stv.umb.edu/Coimbra12.html; 5. https://whystolookforgenes.wordpress.com; 6. http://www.genesandsociety.org/; 7. https://scholarworks.umb.edu/cct_sicw/7

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