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# CRIMINOLOGY

## MATHEMATICAL PROOF IS NOT MINUTIAE AND IRREDUCIBLE COMPLEXITY IS NOT A THEORY: A FINAL RESPONSE TO BURT AND SIMONS AND A CALL TO CRIMINOLOGISTS\*

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In their initial article, Burt and Simons (2014) laid out a range of specific criticisms of twin-based research. They argued that violations of the statistical assumptions that underpin this line of work result in upwardly biased estimates of heritability and downwardly biased estimates of environmental effects. According to Burt and Simons, the findings generated from behavioral genetic approaches are “biologically nonsensical” and are “misguided.” Indeed, Burt and Simons stated, without qualification, that an entire class of statistical models used to analyze twin data are “seriously flawed.” Because behavioral genetic designs suffer from “fatal flaws,” they argued, twin-based research designs and their associated statistical methods should be put to an “end in criminology.”

We examined their assertions carefully. We found that Burt and Simons (2014) cherry-picked the studies they cited in support of their position regarding the biasing impact of the equal environments assumption (EEA), something they openly admit in the online supporting information of their rejoinder (Burt and Simons, 2015, this issue). Specifically, they state, “we focused on articles that showed that the EEA is not a valid assumption” to support their claim that heritability estimates from behavioral genetic studies are chronically overestimated as a result of violations of this assumption. In fact, they relied primarily on a single source (Joseph, 1998, 2004, 2006, 2010) for their critique. Notably, we found that they failed to cite the vast majority of the more than 60 studies that tested for violations of the EEA—a body of work that is inconsistent with their claims.

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In addition, as we noted in our comments on the original article by Burt and Simons (2014), they misquoted and mischaracterized scholars, a trend that they continue in their rejoinder (Burt and Simons, 2015). A simple example occurs in the first sentence of their rejoinder, where they allege that they follow in the footsteps of “renowned behavioral geneticists (e.g., Rutter, 1997; Turkheimer, 2011a)” and call for an end to heritability studies. We encourage readers to interpret Rutter and Turkheimer’s behavioral genetic scholarship, including their continued publication of twin-based studies, for themselves (Rutter, 2006; Turkheimer, 2011b; Turkheimer, Pettersson, and Horn, 2014).

We also mathematically tested the assertions made by Burt and Simons (2014) that violations of different modeling assumptions invalidated twin-based results. Using simulated data, we demonstrated unequivocally that the mathematical framework employed in the analysis of twin and extended twin studies is sound. Yes, violations of assumptions can produce biases, but in most instances, these biases cancel each other out. In other instances, they could slightly inflate or slightly deflate genetic and environmental parameter estimates, but these effects are too small to affect the general conclusions from these studies. Unfortunately, our mathematical proofs had no discernable effect on the opinions of Burt and Simons. They reacted (Burt and Simons, 2015) by accusing us of “wrangling over the methodological assumptions and statistical minutiae of the twin-study model,” which they said “is a waste of scientific energy and attention”. Regarding the validity of the EEA, one of the core criticisms leveled against twin studies in their original article (2014), Burt and Simons concluded in the online supplement to their rejoinder (2015) that we are going to have to “agree to disagree.”

We are perplexed by their response. The essence of their initial argument was methodological and statistical, so we showed—methodologically and statistically—that their assertions were mathematically overstated, if not incorrect. There is no room for subjective opinion or agreements to disagree: There is only algebra.

Having moved away from their earlier mathematical assertions, Burt and Simons (2015) now pivot to the position that the “fatal flaw” in heritability studies is conceptual, not mathematical (the subtitle of their rejoinder is “The Fatal Flaw Is Conceptual”). They even chastised us for interpreting the “crux” of their case as being methodological when it was really conceptual. This is news to us. We focused on the methodological aspects of their initial article (2014) because it began with 20 pages of discussion regarding the supposed methodological shortcomings of heritability studies and was followed by just 2 pages on their view of a conceptual problem. Their rejoinder and online supplement, moreover, repeatedly emphasizes the methodological flaws of heritability studies. Furthermore, Simons has noted that, “For years the two of us have fretted about *the methodological soundness of heritability studies* of crime. . . . The major point that I want to make (and we also make it in the paper) is that *we are critiquing a method* and not a particular group of scientists” (Simons, personal communication, January, 24, 2014, emphasis added). Given these facts, we considered it a safe assumption that the thrust of their critique was methodological and statistical.

Nonetheless, their revised emphasis on the conceptual position views all human phenotypes as the result of gene–environment covariance, which includes epigenetics (note, we addressed epigenetics in Barnes et al. 2014: 615–616) gene  $\times$  environment interaction (G $\times$ E), and gene–environment correlation ( $r$ GE). Because of these additional sources of variance, Burt and Simons (2014) claim that it is impossible to disentangle genetic from environmental influences.

This is, admittedly, an appealing and seductive notion. Yet the position of Burt and Simons (2014) is again contravened by evidence obtained by means of the “minutiae” of the algebra for twin-based models. We do not have space here for a full discussion of the mathematical properties of GxEs and *r*GEs and how they affect heritability estimates, but interested readers can see Coventry and Keller (2005), Keller and Coventry (2005), and Purcell (2002), or for a more brief discussion, the online supplement for Barnes et al. (2014). As these sources attest, GxEs and *r*GEs do complicate twin-based research.

Fortunately, GxEs do not *invalidate* the results of twin-based methodologies even though GxEs and *r*GEs probably have some impact on all three components of the ACE model. Therefore, critics of twin-based designs must specify *how*, *when*, *where*, and for *whom* these gene–environment relationships matter. They are called on to do so because GxEs between additive genetic components (*A*) and shared environmental effects (*C*) will operate to inflate heritability estimates, but interactions between *A* and nonshared environmental factors (*E*) will inflate the nonshared environmental estimate (Purcell, 2002). Thus, GxE interactions are *equally likely* to inflate estimates of heritability and the environmental components. The hard work still yet to be done within criminology consists of understanding when *A* and *C* interact and when *A* and *E* interact. Moreover, *r*GEs affect the parameters in different ways, meaning that future criminological research needs to identify the scenarios that can lead *A* and *E* to correlate. Research designs that can “pull apart” these influences exist and are utilized in other disciplines (Johnson, 2007; Keller, Medland, and Duncan, 2010; Purcell, 2002). If Burt and Simons are truly concerned about the biasing impact of GxEs, then they should support their claims by closely assessing the mathematical “minutiae” involved with the estimation of these models and provide evidence of the extent to which these influences affect parameter estimates.

Meanwhile, we can safely assume that GxEs and *r*GEs play a role in the development of nearly all complex behavioral outcomes, including antisocial behaviors. Supportive evidence exists for GxEs and *r*GEs on various types of antisocial behaviors (Beaver, 2013). In light of the available evidence, how do these findings impact the current debate? We offer three thoughts.

First, as we stated previously, Burt and Simons (2014, 2015) gloss over the inherent difficulties that accompany GxE research. This kind of research is fraught with many unique problems, which we have confronted as researchers. Primary among these problems is the nonreplication of GxE interactions (Duncan and Keller, 2011). Nonreplication strongly suggests that some of the most widely cited examples of GxE interaction are methodological and statistical artifacts (Duncan and Keller, 2011; Ioannidis, 2005). Recent research by Belsky et al. (2014), for example, failed to find evidence of an interaction between 12 measured genes and a measure of early maternal sensitivity. This is not to say GxE studies should be ended, which seems to be the strategy *du jour* of Burt and Simons, but that a host of practical issues will need to be addressed by scholars who are willing to do the dirty work of examining “minutiae.”

Second, we are surprised that Burt and Simons (2015) contend that we did not address their conceptual argument that genes and environments are so intimately tied together that it is impossible to separate their effects. In fact, we fully addressed the errors in the logic of their conceptual argument (Barnes et al., 2014: 28) by discussing the “damned rectangle” example that has been used for decades to debunk the outdated myth that variance cannot be separated into genetic and environmental components (Harris, 2006; Plomin et al., 2013). We will, nonetheless, address this conceptual argument again.

Yes, genes and environments do interact in the *colloquial* sense. Indeed, it is impossible to separate a person from his or her environment (genes) to study the “pure” influence of genes (environment). The two are inexorably linked in a tangled causal network that consists of mediators, moderators, and feedback loops. But the question remains whether scholars can separate sources of variance into genetic and environmental components. That is what matters for heritability studies. The answer—which has been known for at least 30 years—is a resounding “yes.” As Duncan (2014) explained:

Throughout the history of GxE research, the question of whether or not GxE effects are separable from genetic and environmental main effects has been asked on many occasions. The answer is yes (though it is not necessarily intuitive); GxE effects are meaningfully and actually separable from genetic and environmental effects. Plomin and colleagues explained this elegantly in 1977, making the point that “interactionism,” which they define as the idea that “environmental and genetic threads in the fabric of behavior are so tightly interwoven that they are indistinguishable,” is simply false *at the population level*. To be clear, it is true that—for an individual—genetic effects cannot be expressed in the absence of an environmental context just as environmental effects necessarily manifest themselves in the context of an organism’s genome. However, *at a population level*, it is possible to distinguish genetic from environmental effects. (p. 262; emphasis added)

To echo Duncan’s (2014) point, genetic and environmental sources of variance can be disentangled, and GxEs can be modeled, at the *population* level, which is precisely the goal of heritability studies. The argument by [Burt and Simons’ \(2014, 2015\)](#) that genes and the environment are inseparable in explaining human behavior rests on the erroneous belief that statistical models are designed to explain behavioral outcomes for specific *individuals*. Of course, statistical models are designed to partition and explain *sample variance*, not individual behavior. In partitioning variance, it is possible to separate genetic and environmental influences (see [Barnes et al., 2014](#)).

Our third thought on the importance of GxEs and rGEs concerns their impact on non-biosocial research. If it were true, as [Burt and Simons \(2014, 2015\)](#) claim, that genetic and environmental effects are inseparable, then what would that imply for contemporary sociological studies that do not use genetically sensitive research designs? Employing the logic of Burt and Simons would necessitate the conclusion that the results of these studies are uninterpretable because it would be impossible to separate the impact of the environment from the impact of genetic factors (not to mention their statistical interactions and correlations). Even purely environmental influences would turn out to be too complicated to study. Moreover, humans are always part of nested or overlapping groups such as families, schools, and neighborhoods. If we were to follow Burt and Simons’s argument to its logical extension, it would be futile to try to separate contextual influences from those individual traits. Despite this logical conclusion, Burt and Simons reassure criminologists conducting environmental-centric research that there is no need to alter their methods or to adjust their conclusions—only heritability studies must be ceased. Perhaps we should take the advice of Burt and Simons and “call the whole thing off.”

The position that developmental influences are too complex to be disentangled reminds us of the argument advanced by advocates of the intelligent design theory of the origin of species. The argument is that biological systems such as the human eye are irreducibly

complex. Behe (1996), for example, argued that complex organic systems are composed of highly interdependent parts and that removal of any part makes the system useless. Arguments like this are designed to stifle research by making people think, “It’s all a mystery!” But undaunted researchers have gone on to show—for example, by tracing the evolution of the eye—that apparent mysteries can be solved with the methods of science.

These same methods of science can also be used to test the statements made by [Burt and Simons \(2014, 2015\)](#) about the irreducible complexity of human traits and behavior. These methods have shown that individual differences in some behavioral characteristics—for example, executive functioning—are influenced primarily by differences in genes ([Thompson et al., 2001](#)). For other characteristics, such as many personality traits, differences in genes and environments make roughly equal contributions to the variance (Bouchard and Loehlin, 2001). Whereas other developmental outcomes, such as the language that a child speaks (as well as the child’s accent), are solely the product of environmental inputs (Harris, 2006), although the ability to speak any language at all is a built-in characteristic of our species ([Pinker, 2002](#)). The degree of specificity that emerges from behavioral genetic studies would be lost if researchers were scared off by the admonitions of Burt and Simons. The fact is behavioral geneticists can and do separate genetic from environmental influences. Human development is complex, but it is not impossibly complex.

We now turn our attention to addressing the broader readership of *Criminology*. Let us step back and look clearly at what Burt and Simons (2014, 2015) have said and where their vision of the future of criminology would lead us. They made the claim that an entire class of statistical models, as well as the findings generated from thousands of studies based on these models, is “fatally flawed.” A claim this bold requires that the burden of proof be shifted to those making the claim. The standard of evidence should be substantial. Have Burt and Simons met that standard?

By any reasonable measure, they have not. [Burt and Simons \(2014, 2015\)](#) have never worked with twin data, and they show no signs of being familiar with the large and substantial body of work that supports the use of these behavioral genetic methods. And yet they claim to have invalidated the work of statisticians and behavioral geneticists around the world.

What would happen if the readers of this journal believed the assertions made by Burt and Simons (2014, 2015)? Although they do not explicitly call for a ban on twin-based research, they have created the intellectual justifications that would effectively lead to such a ban. Indeed, their original article (2014) is already being cited as evidence by referees in the peer-review process that twin-based designs are inherently flawed and therefore such studies should not be considered for publication. Before our discipline moves toward forbidding certain kinds of research, we ask that you consider three points. First, read the evidence for yourself. Read the studies Burt and Simons did not tell you about and ask yourself whether Burt and Simons were straightforward in their selection and description of the literature in both their article (2014) and their response to our critique (2015). Second, could any methodology or statistical model, including the ones you use, make it through the type of inquisition carried out by Burt and Simons where evidence is not necessary and selective citation is allowed? When we showed that their claims could be disproved mathematically, they took mathematics off the table by invoking irreducible complexity. Third, consider the broader ramifications for science if, in fact, a ban takes

place. Twin-based research will no longer be published in criminology, although it would be published in just about every other discipline. This result might appeal to some, but those with a commitment to open scientific investigation are likely to find this contrary to the canons of science.

Consider, too, the linguistic gymnastics used by Burt and Simons (2014, 2015) to claim that nothing positive “at the present time” has come out of behavioral genetic research. Nothing, even presently? Findings from behavioral genetics ultimately led to the acceptance and more humane treatments of individual differences in human development, including those concerning addiction and mental illness. Heritability studies also provided us the evidence that certain behavioral pathologies were genetically based and were not the products of deficient maternal behavior. Moreover, heritability studies are now being used to assess variation in the volume and density of brain structures—structures often implicated in human aggression. More broadly speaking, a biosocial framework—a framework informed by the findings of heritability studies—was used to build the Nurse-Family Partnership by David L. [Olds \(2007\)](#). Biosocial data have been used to justify expensive environmental efforts to remove lead from buildings—buildings primarily occupied by poor, inner-city youth. The U.S. Supreme Court relied heavily on biosocial evidence in their *Roper v Simmons* decision to abolish capital punishment for juvenile offenders. Nonetheless, Burt and Simons responded by stating that a belief in Santa Claus may bring joy to people but the belief is still wrong—a clever yet diversionary response to a serious discussion.

Before we close, we must address the decision of Burt and Simons (2015) to inject the historical sins of biological theorizing, including eugenics, the use of lobotomies, and racism into this debate. Unfortunately, this is not the first time that Simons has implied that there is a racial subtext to the motives of those with whom he disagrees. More than 35 years ago, [Simons \(1978\)](#) critiqued Hirschi and Hindelang’s work on IQ and delinquency. In their rejoinder, Hirschi and Hindelang (1978) responded to Simons with many of the same criticisms we have leveled against Burt and Simons, including selectively reading the literature, relying heavily on a single source, and mischaracterizing research findings. The similarities are, indeed, uncanny. What is also instructive is that 35 years ago, Simons equated heritability to racism and he inferred that Hirschi and Hindelang’s work would sponsor racism. We see a similar pattern here.

Calls to end research should, we suggest, be greeted with the utmost skepticism. It is one thing to analyze research models and methodologies critically—it is entirely another to call for their removal from a discipline. Indeed, publishing calls to “end research”—something we have never seen in a respected academic journal—should happen only when scholars can mathematically prove that a method is flawed. This standard, of course, is difficult to achieve precisely because it rejects arguments by analogy and prioritizes actual evidence. Clearly, [Burt and Simons \(2014, 2015\)](#) did not reach this standard and have done nothing more than create an elaborate ruse. We are thus left with the fact that an article was published in our flagship journal calling for an end to research based on “conceptual” not mathematical criticisms. This precedent is dangerous.

In this context, criminologists should treat the claims made by Burt and Simons (2014, 2015) with substantial skepticism. Given its statistical and conceptual flaws, their article should not be cited by responsible scholars as “proving” that heritability studies are inherently inaccurate. We also do not recommend that our article (2014) and commentary (2015) be treated as offering the last words that criminologists should read on this topic.



Ultimately, the debate over heritability estimates is not between Burt and Simons and us, but between Burt and Simons and the large number of scholars across fields who use behavioral genetic designs. We believe that this scholarship supports the positions we have articulated. We now invite you to evaluate the evidence and to decide for yourself.

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