## Biology and Homosexuality

Simon LeVay's fascinating report of brain differences between heterosexual and homosexual men (Reports, 30 Aug., p. 1034) has elicited in the ensuing debate some subtle but significant misstatements about biology's role in behavior that cannot help but fuel further misperceptions about homosexuality. They read something like this: Biology was never thought to play a role in homosexuality, so LeVay's evidence in support of a biological substrate for homosexuality is surprising and adds a new element to the debate. It is also provocative because it suggests that homosexuality is innate and resistant to change. All in all, a biological basis for homosexuality is an "explosive notion." These sentiments were widely expressed in the news coverage of LeVay's paper, including Marcia Barinaga's article "Is homosexuality biological?" (News & Comment, 30 Aug., p. 956), and surprisingly appear to reflect the views of both scientists and science writers.

First, the empirical question never was, "Is homosexuality biological?" LeVay's findings are important scientifically because they provide a critical "first handle" on what may be different biologically about heterosexual and homosexual men. He was successful in part because his hypothesis was not open-ended, but focused on the nature of a specific potential brain substrate. However, evidence for a biological basis for homosexuality is hardly news, because this proposition was never seriously in doubt, at least as an issue of natural science. This is because the biological basis of behavior is a *premise* for psychobiology. Put another way, as Hebb pointed out more than 40 years ago [see the preface to his *Organization of Behavior* (*1*)], we should not treat our ignorance of the nature of biology's role in psychological functioning as evidence that biology in fact has no role.

Second, it is still all too common to see early experience, social learning, or choice pitted against biology, but these are false dichotomies. This is because the brain has been shown or is assumed to be the underlying mechanism in these processes. Several decades of empirical work have shown that the brain is a product of early experience, social environment, and genetic instructions. So, it manifests the workings of both nurture and nature. Moreover, while the effects of both nurture and nature on the brain and behavior can be enduring and resistant to change, they need not be inexorable, on all occasions or for all individuals. LeVay's findings may affirm a role for deterministic forces in sexual orientation, but they do not preclude either homosexuals or heterosexuals from grappling with the course of their own development. Choice may be a forceful biological process in its own right.

THOMAS A. SCHOENFELD
*Departments of Psychology* and *Biology,*
*Frances L. Hiatt School of Psychology,*
*Clark University,*
*Worcester, MA 01610*

REFERENCES

1. D. O. Hebb, *Organization of Behavior; A Neuropsychological Theory* (Wiley, New York, 1949).

LeVay's effort to correlate biological variables with the sexual orientation of individuals is flawed by the manner in which he creates bipolar categories of "heterosexual" and "homosexual" men. While he recognizes the diversity of human sexual behavior, LeVay's findings are confounded by these simplistic and unreal classifications. The behavioral continuum of males involved in homosexual, bisexual, or heterosexual activity has not been considered in LeVay's experimental design. Kinsey, for example, used a seven-point scale of sexual behavior (*1*). LeVay, however, includes in his classification of "homosexual" men all men who have had sexual encounters with men irrespective of the number of sexual encounters with women. The result is a misclassification bias where, for example, men who are self-reported bisexuals are classified as homosexual, implying a unity of sexual behavior that does not exist for men who have sex with men.

The appearance of LeVay's paper highlights a serious issue in science public policy. Should such a study, based on a questionable design, with subjects drawn from a small, highly selected, and nonrepresentative sample, receive the kind of international attention and credibility that publication in a journal with the stature of *Science* lends? Granted that research on human sexual behavior has become an imperative as a result of the AIDS pandemic, but does the need for data support publication of such preliminary results? Those on either side of the political spectrum with not-so-hidden agendas are likely to be hungrily awaiting data of this kind. Scientists researching (and journals publishing) in such highly controversial areas must recognize the greater sociocultural (and in this case epidemiological) milieu in which they exist and exercise greater restraint in the rush to publish and publicize.

JOSEPH M. CARRIER
GEORGE GELLERT
*Orange County Health Care Agency,*
*515 North Sycamore,*
*Santa Ana, CA 92701*

REFERENCES

1. A. C. Kinsey, W. B. Pomeroy, C. E. Martin, *Sexual Behavior in the Human Male* (Saunders, Philadelphia, PA, 1948).

*Response*: I completely agree with Schoenfeld's point. Biology is the study of life and as such encompasses all possible mechanisms for the determination of sexual orientation short of the supernatural. It is unfortunate that the word is often misused (and I was guilty of this in the abstract of my paper) to refer selectively to those aspects of life that biologists understand.

Carrier and Gellert criticize me for not giving due weight to the diversity that exists in sexual affect and behavior. As I stressed in the paper, I was limited in what I knew about these men's sex histories to what was noted in their medical records. The only realistic way to correlate such diversity with brain structure is to study structures that can be imaged in the living brain. This is not true of INAH 3 (the interstitial nuclei of the anterior hypothalamus 3), but since there are a number of larger brain structures that have been reported as gender-dimorphic, there may exist the opportunity for addressing these issues. I may well have oversimplified the problem in my study, but sometimes such oversimplification is necessary to make progress in a novel field.

I would also like to suggest a moratorium on the use of the word 'agenda' in any context unconnected with committee meetings.

SIMON LEVAY
*Salk Institute for Biological Studies,*
*Post Office Box 85800,*
*San Diego, CA 92186–5800*

REFERENCES

1. J. A. Cherry and M. J. Baum, *Brain Res.* **522**, 191 (1990).

## Artificial Heart Development: A Long Effort

The article "Artificial heart—the beat goes on" by Eliot Marshall (News & Comment, 2 Aug., p. 500) reviews some recent experience with circulatory assist devices, but does not give a very good picture of the long, detailed, multidisciplinary effort that

was necessary for the development of such devices.

The first biomedical engineering program at the Pennsylvania State University began in 1970 with a team (made up of a surgeon, an engineer, and a veterinarian) that has worked without interruption for 21 years. Its goal was to create an electrical-powered heart that could be recharged as needed through the unbroken skin without wires. Extensive basic research on cardiovascular and circulatory pathophysiology was required as new artificial materials were developed and tried.

In 1971 the first left ventricular assist device was implanted in a calf, and in 1976 an improved device was implanted in a human patient. Since then the Penn State device has been used in more than 225 patients worldwide and has saved more than 100 lives. In 1990 the assist device was declared a mechanical engineering breakthrough and an international historic landmark by the American Society of Mechanical Engineers.

It took 12 years to develop the pneumatic-powered artificial heart and implant it successfully in a human patient. The Penn State heart still is the only one approved for human use by the Food and Drug Administration.

It may be ten more years before the team feels it is safe to apply for human use of the electric-powered heart. To be sure and safe takes more time than might be gathered from Marshall's article.

GEORGE T. HARRELL*
*2010 Eastridge Road,*
*Timonium, MD 21093*

*Vice President for Medical Sciences Emeritus, College of Medicine, University Hospital, Pennsylvania State University, Hershey, PA.

*Erratum*: Figure 2, part B (p. 100) in the report "Regulation of transendothelial neutrophil migration by endogenous interleukin-8" by A. R. Huber *et al.* (4 Oct., p. 99) was printed incorrectly. The correct figure (parts A, B, and C) appears below.