

account of theory choice is rejected on rather dubious grounds. Ben-Ari takes 'incommensurability' to be synonymous with a lack of agreement or communication between communities and ignores Kuhn's semantic interpretation of this notion.

The author's polemic style seems to be even less charitable towards other opposing views. For example, the Strong Programme in the sociology of knowledge is caricatured by grouping it under the same heading as the 'post-modernists' who claim that 'the real world does not exist' (p. 118). Another example is creationism, which is the main target of Ben-Ari's critique of 'pseudo-science'. The author presents several disconnected and obscure claims which he attributes to creationists, with no attempt to reconstruct their arguments or classify different types of creationism.

Ben-Ari's defense of science has a lot in common with the kind of rhetoric used during the 'Science Wars' of the 1990s. Both parties in the Wars often claimed more territory than they could defend, a tactic which frequently resulted in the persuasion of the persuaded. In the background of this troublesome state of affairs was the assumption that buying into the scientific discourse meant accepting a wholesale bargain. Here lies a question for the philosophy of science: Does the task of remedying popular misconceptions about the workings of science warrant the affirmation of a unique and universal scientific method? I think there are good reasons to answer this question in the negative. Indeed, many such reasons are cited by Ben-Ari himself: Scientific communities thrive on internal debate and criticism; the quantitative methods of many sciences allow for precise predictions to be made and tested; scientific theories are coherent and concise and yet explanatorily powerful. None of these characteristics require viewing science as a unified project producing ahistorical and universal results. With a good understanding of such descriptive criteria, scientific literacy becomes a perfectly achievable goal.

Ben-Ari's argumentative strategy may obstruct the strength of the book, which lies in its approachable presentation of basic concepts from various scientific fields. Ben-Ari's interdisciplinary interests are also revealed in the biographical 'vignettes' that follow each chapter of the book. Some of these passages depict the life and work of a less well-known scientist such as Emmy Noether and Judah Folkman, and are written in an enjoyably humorous tone. Ben-Ari's educational goals might have been better served by focusing further on such anecdotes, rather than by attempting to make sweeping philosophical claims about science that will not withstand close examination.

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ELISABETH A. LLOYD, *The Case of the Female Orgasm: Bias in the Science of Evolution*, Cambridge, MA: Harvard University Press, 2005, 320 pp., \$27.95.

The existence of male orgasm has never posed a problem for evolutionary biologists, since its incidence with reproduction is clear enough. However, women can conceive perfectly well without orgasm, and most women, in fact, cannot orgasm from coitus alone. Why, then, are most women orgasmic (at least during non-coital sex)? Why has evolution preserved this trait, if it doesn't impact reproductive success? Note that the question is not why women experience sexual pleasure, as that certainly bears on reproductive success, but why women sometimes have orgasms, especially when those orgasms don't come from coitus.

Evolutionary biologists have proposed some 20 accounts to explain how female orgasm is adaptive. Elisabeth Lloyd, in her new book, carefully dismantles each one. Instead, she endorses Donald Symons's 'byproduct' account, introduced in his *Evolution of Human Sexuality* (1979). Like male nipples, female orgasm is an evolutionary byproduct of selection in the opposite sex, Symons argues. Sexual differentiation occurs rather late in embryonic development, well after the underlying structures (enervation, erectile tissues, and blood supply) of male orgasm and female nipples are formed. As a consequence, males get nipples and females get orgasms.

Symons's views have been widely ignored in the literature in favor of adaptationist accounts, however. For example, Desmond Morris claimed that orgasms cause women to become sleepy, which promotes maintaining a horizontal position for at least several minutes. This, Morris claimed, might increase the chances of fertilization. However, sex research shows that women, unlike men, become excited and aroused after orgasm, which (among other evidence) defeats Morris's account. Another account, initially proposed by Robert Smith, is the uterine upsuck hypothesis, according to which orgasm lowers the pressure inside the uterus, drawing sperm in. Again, Lloyd shows that the evidence simply isn't there. For each proposal, she shows the experimental evidence or statistical support is wanting. Fundamentally, all 20 accounts are proposed mechanisms by which female orgasm increases fitness, but Lloyd shows that there is no evidence that female orgasm is in any way associated with reproductive success.

Why, then, have adaptive accounts been so successful? This dynamic in many ways reflects the career of sociobiology. E.O. Wilson's *Sociobiology: A New Synthesis* sparked a firestorm when it was published in 1975. In the decade that followed, scientists and non-scientists debated the intellectual, moral, and political legitimacy of adaptationist explanations of human behavior. Trenchant criticisms from Ernst Mayr, Stephen Jay Gould, and Richard Lewontin convinced many evolutionary biologists (and, incidentally, many philosophers of biology) that sociobiology was hopelessly impoverished. Although the fundamental disagreements about method were never fully resolved, sociobiology remained marginalized during those years. The 'sociobiology wars' eventually receded from public view by the late 1980s, and sociobiology seemed to be well and truly dead.

In fact, the fortunes of sociobiology were just beginning to reverse. A political sea change in American universities isolated critics of sociobiology. In addition, sociobiology transformed itself into a new field with a new name, 'evolutionary psychology', absorbing some of the criticisms of sociobiology and side-stepping others. By the mid 1990s, evolutionary psychology achieved a certain respectability among psychologists and biologists, helped by advances in kin selection models and the popularity of books such as Robert Wright's *The Moral Animal: Why We Are the Way We Are: The New Science of Evolutionary Psychology* (1994).

Lloyd's criticisms of adaptationist accounts of female orgasm were first raised in the context of the original sociobiology wars of the 1980s. However, Lloyd's focus in her latest book on standards of evidence in human evolution remains timely. Lloyd fruitfully applies her prior work on evidence in sociobiology to evolutionary accounts of physiological traits. The issues are structurally identical: how should psychologists or sex researchers or indeed any scientist not trained in evolutionary biology construct and test evolutionary hypotheses? Cross-disciplinary science is always fraught, and Lloyd takes sex researchers to task for importing evolutionary theory without applying its methodological rigor. Lloyd is less concerned with adaptationism per se than its sloppy application. Ultimately, whether female orgasm is an adaptation is inconsequential to Lloyd. Thus, while she blames adaptationist bias for the breakdown in this

area of sex research, she shows only that the standards have been too lax. I'm not sure that she has adequately explained why this sloppiness has been tolerated for so long.

In addition to adaptationism, Lloyd blames androcentrism for the unpopularity of Symons's byproduct account. Sex researchers often assume that female sexuality is like male sexuality, despite the wealth of evidence to the contrary.

Lloyd's book provides a detailed look into science gone astray. It will be of interest to historians and philosophers of evolutionary biology, as well as scholars concerned with the role of values in science.

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BERND GAUSEMEIER, *Natürliche Ordnung und politische Allianzen. Biologische und biochemische Forschung an Kaiser-Wilhelm-Instituten 1933-1945*, Göttingen: Wallstein Verlag, 2005, 352 pp., € 27,00.

Bernd Gausemeier has written a dissertation on the research done in three institutes of the Kaiser-Wilhelm-Gesellschaft (KWG): in the KWI for Biochemistry by Adolf Butenandt, in the KWI for Biology by Alfred Kühn and Fritz von Wettstein and in the KWI for Brain research by Nikolai Timoféef-Ressovsky. Two aspects are dealt with extensively: the actual experiments done and the way funds for the research were raised.

The topics dealt with are not new. Thus the problem arose for Gausemeier how to deal with details already described by others. I will discuss a few examples. In 1933, the Institute for Biochemistry was headed by Carl Neuberg. He was fired for being a Jew in 1935. Butenandt became his successor in 1936. Gausemeier does not mention the remarkable fact found by Wolfgang Schieder, that Butenandt became a member of the Nazi Party (PG) almost the same day he became director of the KW-Institute. To become PG was apparently the price Butenandt had to pay for becoming director of the KWI.

Moreover, the research done in all three institutes has been described by Ute Deichmann in her books on biologists and biochemists in Nazi Germany. Gausemeier quotes her mainly where he thinks she is wrong. Deichmann compared the citations of papers written by emigrants and by those who stayed. To Gausemeier citations are misleading when one wants to show the influence of papers. Moreover, according to Gausemeier, the cases of fraud Deichmann mentions are not proven. The defense enzymes (Abwehrfermente) by Abderhalden are quoted by Gausemeier as real. The D-amino acids found in cancer tissue are wrong according to Gausemeier. He does not mention that they are the result of fraud by Hanni Erxleben, the technician of Fritz Kögl. This reminds one of the modern sociology of science: where there is no truth, there is no fraud.

The general claim of Gausemeier, that 1933-1945 research in the three institutes was first class, is not new but well documented. Moreover, there was no attempt by the Nazis to influence or determine the direction of the research done. It is not new that the research was very well funded by the KWG, the DFG (German research foundation) and the chemical industry (Schering and IG Farben). In particular the special unit which was created to investigate the functioning of viruses, especially the tobacco mosaic virus (TMV) was funded by IG Farben.