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For Charlotte

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Preface

My aim in this book is to provide an intelligible overview of the field of sex differences in cognition for the educated nonspecialist. The book is not intended as an exhaustive reference work on the subject, though I believe it presents all the major findings. Some readers may not find it necessary to read the background contained in chapters 2 and 3 before moving on to the rest of the book. An elementary review of the standard procedures used in dealing with numbers is provided in the appendix. Readers unfamiliar with concepts like standard deviation, effect sizes, correlation, and so on, may consult this appendix. Chapter 12, on body asymmetry, can be skipped without losing any essential information about sex differences in cognition.

The reader will find that much of the human research referred to in this book was conducted in my own laboratory at the University of Western Ontario. I have certainly not intended to omit important work from other labs, and have not, I hope, done so. But we always know more about the research we are personally involved in, so the tendency to feel a sense of familiarity and security with the results is inevitably greater. Relying heavily on this work has also allowed me to provide more personal commentary about the studies than I could have done with others' research.

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It is a pleasure to acknowledge my many collaborators in this research, former graduate students or post-doctoral fellows. They include Elizabeth

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Sex and Cognition

1
Introduction

When science ignores facts in favour of ideology . . . it ceases to be science and becomes propaganda for a dogma.
Kenneth Hilborn, 1996

This book will describe the major differences between men and women in cognitive or problem-solving abilities. It will also discuss the possible biological contributions to such differences. Many social scientists claim that it is unnecessary to invoke biological factors at all in explaining how people come to differ in their cognitive patterns. They prefer to attribute the variation that we see in human beings to variations in environmental, including socialization, influences. An extreme version of this position insists that unless a cognitive function or the relevant brain system shows sex differences at birth, we can never infer that it was not environmentally determined. For example, it has been suggested that, since newborn babies can neither do math nor hit a target accurately, we may not infer that these behaviors are influenced by biological factors (Foss 1996).

Let's consider how we would understand the development of a physiological function like lactation—the production of milk by the mammary glands—within such a framework. Lactation normally does not occur in children of either sex. Nor does it occur in adult males; but it begins in adult females after the birth of a child. Must we then conclude that it is determined by the different social environments that males and females experience? Hardly! It is equally fallacious, therefore, to claim that behavioral sex differences that make their appearance after childhood must be solely, or even primarily, due to gendered socialization.

Similarly, some writers insist that ability differences between men and

women must be considered experientially determined unless we can demonstrate unequivocally that they are not. For example, "I impose the highest standards of *proof* . . . on claims about biological inequality." (Fausto-Sterling 1992) This is surely a very strange approach. It implies that we may not entertain biological explanations of human behavior until we have ruled out all plausible socialization mechanisms. To illustrate how strange this viewpoint is, we might turn the argument around and say we will not accept a difference between the sexes as environmentally influenced unless we can rule out the possibility that the differences are congenital. This is as logically acceptable as the first position, and equally flawed. The aim of scientific research is not (or should not be) to uphold or deny any particular social or political ideology. Rather, the aim in science must be to find the truest explanation we can; that is, the explanation that best fits all the current facts, regardless of current dogma.

If we want to develop an accurate account of how people's problem-solving behaviors originate, we cannot, a priori, willfully exclude any potential source of variation across individuals. It is not only unjustified scientifically to take such a biased view of how behaviors are determined, it is contrary to common sense. The business of science is to find out how the world really works, not how it ought to work according to some wishful schema or other. Scientific explanations change as more information comes in, but at any one point in time a scientific analysis attempts to encompass *all* relevant facts. In human cognition, this must include data from biologically relevant sources such as hormonal and brain research and studies in nonhuman species.

This does not require us to give equal consideration to all possible interpretations of a given finding. For example, an isolated fact, such as that women in one study remembered landmarks on a particular learned route better than men did, could be due to any number of factors. It could be due to women's better overall memory for objects (though this explanation can be ruled out by giving a separate item-memory test and seeing if the general memory accounts for the better landmark memory). It could also be due to the fact that particular items on the route—a red schoolhouse, a specific tree—hold greater interest for women; but if this sex difference holds up across several studies in which the landmarks differ, this is an unlikely explanation. Other possibilities may come to mind. The

most probable explanation, however, in the absence of contradictory data, is that this is part of a general mammalian pattern that is also seen in rodents. That is, it is probably due to a strategy, favored by females, to use unique items in their environment to find their way.

Egalitarian Ideology

The bias against biological explanation seems to have arisen from egalitarian ideologies that confuse the Western concept of equal treatment before the law—the societal application of the idea that "all men are created equal"—with the claim that all people are in fact equal. People are not born equal in strength, health, temperament, or intelligence. This is simply a fact of life no sensible person can deny. We have chosen a system of governance which has decided that *despite such inequalities* each individual shall have an equal right to just treatment before the law, as well as equal opportunity.

Egalitarian ideology, however, often goes beyond this and insists that all people would be equal if they had equal environmental stimulation. That is, if upbringing, nurturance, exposure to education, opportunity, and so forth were exactly the same for everyone (a situation in practice impossible to achieve, hence impossible to test), we would all be equally endowed. Apart from the appalling dullness our lives would suffer, this is such nonsense that it is difficult to see how anyone can maintain it with a straight face. Most of us have grown up with brothers and sisters who have shared most aspects of our environment, yet we turned out quite different. We all have friends with backgrounds similar to our own, yet here the diversity in temperament, skills, and ultimate occupation is often greater. A social scientist flatly unwilling to entertain the idea that there are important biological contributions to the variations we see in cognitive pattern from one person to another has stopped being a scientist and has become an ideologue.

It has been suggested that a distinguishing characteristic of ideology is its commitment to a position, regardless of evidence (Hilborn 1996). Sexism, racism, and egalitarianism can all be considered ideologies to the extent that they are commitments to a system of beliefs *without empirical support*.

Nature/Nurture

What form, then, should scientific explanations of differences between the sexes take? It has become apparent over the years that the nature/nurture argument in its polarized form is an unprofitable one, since genetic predispositions cannot operate in a vacuum and environments must have a genetic code to work on. In other words, there can be no explanation that entirely rules out either environmental or genetic influences. In fact, even the basic dichotomy implied here is too simple. Factors such as sex hormones, variation in which is indirectly due to genetic factors, may in some sense be considered as environmental in nature. The potential complexity of such determinants of behavior is indicated by the fact that the amount and type of sex hormones can vary significantly depending on other prenatal influences present—stress, general health, nutrition, and so forth.

Insofar as natural selection arises out of the past environment of a species, any species' genetic characteristics are inevitably constrained by such environments. Indeed, one might expect the genetic code to develop the typical phenotype only in supportive environments, that is, environments similar to those in which the genetic code evolved. Some general characteristics of the external world, such as the circadian cycle and the dimensionality of space, have remained the same for millions of years (Shepard 1987). These characteristics may therefore in some way be encoded in the genetic makeup of most living organisms. Different genetic characteristics would be required of animals who are mobile on land, as compared to water, and so on. In other words, we should expect a match between the genetic code of a species and its past environment.

So the activation of genes is often environmentally determined. Yet for any influence of the environment to have a lasting effect, there must be a permanent physiological change of some kind.

The *Oxford Dictionary of Biology* defines biology as "the study of living organisms, which includes their structure, . . . functioning, origin and evolution." So when we talk of biological influences we are referring to a very broad spectrum of factors including, for example, our evolutionary legacy as social beings. Many people assume that to label a

process *biological* rules out substantial modification to that process in the course of one's life, but this is incorrect. For example, if a certain level of androgens (masculinizing sex hormones) early in life is optimal for organizing the brain for certain spatial functions in later life, we should not assume that this influence is fixed or unalterable. Two individuals with the same degree of exposure to androgens prenatally may experience many other interacting "biological" influences that might make their performance on a spatial test different. In fact, the idea that any two individuals will have exactly the same degree of exposure to androgens is a situation in itself almost impossible to envision, since, apart from identical twins, the rest of the hormonal and nutritional milieu may be quite different. The fact that the spatial ability of adult men can vary with the changing testosterone levels across different seasons and times of day also clearly tells us that biological systems are not immutable.

However, the changeability of biological processes and the fact that we cannot at this stage of knowledge specify exactly how in the nervous system they have their effect on cognition are not excuses for dismissing them as unimportant factors. Critics of the theory that sex hormones influence spatial ability may, for example, chip away at each study that bears on this issue. They may point out uncontrolled variables in one study or may offer plausible alternative explanations in another study. Such critics often use the word *prove*, suggesting that a particular study, because of some limitation (and no study on human beings can ever be perfect), does not prove a hypothesis. Quite true, no one study is likely to sufficiently support a hypothesis to the point where we accept it wholly. Scientists do not expect to prove a position, they expect either to disprove it or to find sufficient and wide-ranging evidence for it, so that it becomes more and more plausible and alternative explanations become less and less likely. Human behavioral science, especially, must operate by looking at the cumulative evidence, not just at one study.

Another important criterion for accepting one hypothesis as more plausible than another, is how well the hypothesis fits with facts in other related fields of science, such as physiology, neuroscience, or evolutionary biology. If two hypotheses both appear able to account for substantial

parts of the data, but one is also consistent with other bodies of pertinent facts, we are more likely to accept the explanation that has a broader base of support.

For example, research suggests that men and women differ in the degree to which they are distressed by sexual, as compared to emotional infidelity in their partners (Buss et al. 1996). Men, on average, are more affected by evidence of sexual infidelity and women more by emotional infidelity. Both socialization and sociobiological explanations of these facts have been proposed. The former stress the societal influences on different attitudes in men and women; the latter employ evolutionary concepts. On balance, because the evolutionary explanations are more broad-based, involving reference to the differing certainties of maternity and paternity (a woman always knows she is the parent) and the differing degree of parental investment between males and females (women invest more in their offspring), we would be inclined to accept the evolutionary explanation over the socialization one. Ultimately, of course, the deciding criterion is evidence.

Similarly, in the field of ability differences, the large sex difference in throwing accuracy has been attributed by some to the differing sports histories of men and women. Others, myself included, have suggested that this sex difference probably arises in some way from the division of labor between men and women over our long evolutionary history, during which men were the hunters and defenders. Even apart from the fact that the sex difference survives statistical correction for sports history, we would be inclined to give greater weight to the evolutionary explanation, because it fits better with knowledge of men's and women's different roles in those societies relatively unaffected by modern technology. We also have comparative data suggesting that even in a related primate, the chimpanzee, males throw objects much more frequently than females do.

We have mentioned that a common fallacy in research on human individual differences, including sex differences, is the tendency to impute such outcomes to different experiences. While we would not deny that experience contributes something to individual differences, we must be cautious in inferring that the experience *determines* the abilities. It may be the other way around. That is, we all choose our activities to some

extent, and we generally tend to choose those that we are good at, with the result that we also gain more experience with those activities. So it isn't necessarily true that men are better at certain spatiomotor tasks because they have had experience at sports. Rather, they may choose certain sports because they have the appropriate abilities. This need not be consciously done—the positive reinforcement we get from doing something well, and the praise that ensues, may simply make such activities more attractive and more likely to occur. This phenomenon, whereby people end up in activities or occupations in a nonrandom or self-directed way, is called *self-selection*.

Different Criteria for Research on Sex Differences?

Some feminist critics have argued that cognitive and brain differences between men and women should be ignored, because there is a great deal of overlap between the two groups (Fausto-Sterling 1992: 251, Favreau 1993). It is certainly true that, in the larger context of comparison with other species, the similarities between men and women far outweigh the differences. However, if we were to adopt the criterion of *no overlap* between groups as a requirement for accepting a difference, we would find almost no behavioral data in any field to be acceptable. For example, if we were studying the effects of aging on memory, there would always be overlap in the scores of older and younger people. Yet we infer with some confidence that memory is, on average, not as good in the elderly. Even factors that have dramatic effects, such as damage to the brain, will often show a fair amount of overlap in the performance of those with and without brain damage. Why should sex differences be treated differently from other kinds of data?

In answer, it has been suggested that the more serious the consequences of accepting a hypothesis, the higher the level of supporting evidence should be before we accept it (Foss 1996). By "serious consequence" is presumably meant some possible application of a finding. There are strong reasons why such an approach is dangerous. First, it is difficult to think of any finding in behavioral science that will have absolutely no application in the larger world. The history of science makes it clear that it is impossible to predict which basic research will generate "useful"

applications. Even in labs devoted to esoteric studies in cognitive psychology, a finding that recognition or recall of certain stimuli is better under one condition of presentation than another might well have application in education, or in some field of human engineering. It might, for example, suggest certain optimal ways of arranging work stations.

Moreover, if by application we mean *social* application, such an approach would require that a researcher decide whether his or her study has some *social consequence*, a term that has different meanings to different people. Are we then to have a research commissariat to decide whether a particular finding has sufficient social consequence to warrant a different level of evidence? Is the researcher then expected to apply a different statistical test or a differential weight of evidence, depending on whether the answer to the question of ultimate social importance is yes or no? To some extent, unfortunately, this already happens in the editorial screening of papers submitted to journals. I know of more than one occasion when a paper (not my own) on a controversial topic was either rejected on that basis or subjected to more intense scrutiny than would operate for other subjects.

This is objectionable and absurd. The rules of evidence and the stringency of statistical evaluation must be the same for all findings. Facts are neutral. We can't allow ourselves to get into a situation in which we say, "People won't like this finding, so acceptance must be at a more stringent level, whereas this other finding, which people will approve of, can be accepted more readily"! Or, "This is a finding that won't upset anyone, so I'm willing to generalize from it, but this other finding may be unpopular, so I need more evidence to support it before reporting it." Fortunately, science is a self-correcting discipline, with each successive finding modifying or refining earlier ideas. So even though as fallible human beings we sometimes make mistakes of inference, the truth ultimately will out.

Further Reading

Buss D.M., Larsen R.J. & Westen D. (1996) Sex differences in jealousy. *Psychological Science*, 7, 373–375.

Caplan P.J. & Caplan J.B. (1994) *Thinking critically about research on sex and gender*. New York: HarperCollins.

Fausto-Sterling A. (1992) *Myths of gender*. New York: Basic Books. (An avowedly feminist writer, Fausto-Sterling talks about the necessity for higher standards of "proof" for sex differences on page 11; and about the extent of overlap between the sexes as a basis for ignoring most sex differences, on page 251.)

Favreau O.E. (1993) Do the Ns justify the means? Null hypothesis testing applied to sex and other differences. *Canadian Psychology*, *34*, 64–77.

Foss J.E. (1996) Is there a natural sexual inequality of intellect? A reply to Kimura. *Hypatia*, *11*, 24–46.

Hilborn K.H.W. (1996) *The quest for "equality."* Toronto: C-FAR.

Shepard R.N. (1987) Evolution of a mesh between principles of the mind and regularities of the world. In J. Dupre (Ed.), *The latest on the best: essays on evolution and optimality*. Cambridge Mass: MIT Press, pp. 251-275.