

Letters

MIXED FEELINGS

I write once again with a mixture of admiration and puzzlement at the *ABT* writings of Paul R. Gastonguay, most recently "A Sociobiology of Man," in the November 1975 issue (*ABT* 37[8]:481). I have considerable regard for Gastonguay's lecturing and writing style, plus his imaginative attempts to unify diverse subject matters. Yet his messages are often hampered by what appear to be attempts to twist the state of current biological knowledge into a shape consonant with some prior philosophical prejudice, rather than vice versa. Thus his interesting discussions of evolution become needlessly but hopelessly teleological; it is as if he were enough of a disciple of Teilhard de Chardin to admire—as do we all—his obvious courage and depth of thought, yet not enough of a disciple to realize that, scientifically speaking, Teilhard was simply wrong. As George Gaylord Simpson put it in 1962:

Those tendencies [that is, reductionism] were unquestionably salutary in some respects. They have helped to eliminate the last vestiges of pre-Darwinian teleology from biology. They have also helped to counteract vitalistic, metaphysical, and mystical ideas which, whatever one may think of them in their own sphere, are completely stultifying as principles of scientific explanation. Here, however, the reductionist tendency has been two-edged. By seeming to negate the very possibility of scientific explanation of purposive aspects of life, it has encouraged some biologists, who insist that such aspects nevertheless exist, to seek explanations quite outside the legitimate field of science. Naming of names is perhaps invidious, but to show that I am here setting up no straw man I will just mention Teilhard de Chardin and Sinnott.

Gastonguay compares the thinking of Lorenz and Montagu on the origin of human aggression (which are clearly contradictory), states his own belief "that it is both"(!), and then for much of the rest of the paper, right down to the opening sentence of his conclusion, comes clearly and decisively down on the side of Lorenz. I think it important to point out here that Lorenz received his Nobel prize less for his ideas that were right than for those he put forth that stimulated research on the part of younger workers that showed him to be wrong.

Finally, Gastonguay admits "the fact that we are evolved animals," yet wants to "determine the proportion of animal and of human in [man]"—as if such a thing were either biologically sensible or scientifically possible! Similarly, I think it illogical to pose such questions as "Which sexual practices are human and which are animal?" Since he admits we are all to be

found within the boundaries of the set called "animals" then any human sexual practices must necessarily also fall both within our own subset and the larger one. And, at a time when we are trying to increase people's tolerance and sensitivity to persons not sharing our own sexual persuasions, it is doubly unfortunate to lump together, as Gastonguay does, "Abortion, euthanasia, homosexuality, death and dying." From previous writings, *ABT* readers know he does not approve of abortion and probably not of euthanasia. Death and dying, though natural processes, are not generally greeted with enthusiasm by most persons! Thus the unhappy result is to convey to the reader a negative subjective opinion of homosexuality that could contribute still further to the ignorance and prejudices that surround it.

Reference

SIMPSON, G. G. 1962. *Biology and the nature of science*. Dedication lecture, Lapham Hall, University of Wisconsin, Milwaukee, 17 Feb. Published by the College of Letters and Science

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Paul R. Gastonguay comments:

Jeff Baker seems unaware that "the state of current biological knowledge" includes attempts to quantify social structuring and development. I am merely providing a framework from which to begin inquiries into such quantification. These efforts are not based on any "prior philosophical prejudice" of which I am aware.

Besides inferring prejudices to which I never refer, Baker also presumes I impose a Teilhardian purpose to my framework. Admittedly, I am guilty of *retrospective* teleology, by which one determines the advantage of those traits which survive in the sieve of natural selection, but I never infer that the paths described in my article are predetermined.

As human behavior is becoming increasingly quantifiable, we will indeed be able to differentiate between animal and human behaviors, as we can between animal and human anatomical and physiological traits. Hence, it certainly will become "scientifically possible" to determine the proportion of *animal* and *human* in man. I also reject the notion that all human sexual practices are not only human but animal as well. I would like to think that my psychosexual drive is one step removed from the chimp's.

Finally, I did not lump together abortion, euthanasia, and so on because I disapprove of all of them. Instead, I clearly indicated that the one thing they have in common is their biological perspective.

All in all, I wish Baker would comment more on the substantive issues of the article: Are sociobiologists such as E. O. Wilson justified in claiming that sociobiology is a science, that social instincts, territorial types, role differentiation, information flow patterns, and so on can be measured and quantified? And am I correct in assuming that evolution is an additive process in which an advanced stage has some vestiges of previous stages, as in my comparison of the three inputs to the human person and to the cell?

N.S.F. FUNDING: WHO IS ACCOUNTABLE ?

In “The 9.2 Million Dollar Silence” (*ABT* 37[7]:438), William V. Mayer raises the question, “Why has Congress refused to provide needed funding to update American teachers in current content and modern methodology?” The arguments he puts forth to support his questionable point of view are based on emotions and offer little to demonstrate that his assumptions have much merit.

Both the House and the Senate now specifically forbid funding instructional improvement implementation programs, but their refusal of funds for these programs was based on other factors besides poorly selected “titles for projects” and “playful political publicity prods,” as Mayer states. Such observations have no more merit than the argument that the MACOS program teaches 10-year-olds about wife-swapping, cannibalism, and infanticide. What the Subcommittee on Science, Research, and Technology was seeking in reviewing NSF programs in precollege science education during the past year was an assessment of the peer review process as it has evolved in federal research support programs. Possibly certain officials of NSF may have demonstrated “a disenchantment with education” during these hearings by specifically not calling witnesses to put on the record how the peer review process has been applied in the area of educational research. In fact, when members of the subcommittee requested information from NSF to clear up this matter they were initially refused it because, they were told, they would not be able to understand the details. It was only the threatened withholding of the implementation funds that finally motivated NSF persons to release the information.

Mayer’s accusation that review members appointed by Representative Olin D. Teague (D-Tex.) “had no meaningful background in either curriculum development or implementation and dissemination” is hardly relevant. How many of the principal investigators, directors, and coordinators developing and disseminating NSF-funded education programs have ever had

meaningful experiences in these areas? How many of the persons making up the peer review groups assessing education proposals have this background? How many of those monitoring these programs for NSF have had valid experience in these areas?

And then, following these stages, how many NSF-funded programs are being underwritten *because* the commercial sector saw no need for the materials, when, in fact, there is no need existing? In many of the NSF-supported programs, the cronies responsible for selling (or is it disseminating?) the program were responsible for all three phases of the project’s development—establishing the need without objectivity, developing the materials without documentation as to their values, and propagandizing the project to the educational community without audits or controls that are meaningful to the program.

The argument put forth by Mayer that “1,700 schools in 47 states” have selected the MACOS program does not mean that a need has been filled. It could very well mean that a multimillion dollar sales campaign financed by NSF funds pressured districts into buying a program for which no need actually existed. It has only been in the heat of belated controversy that several of the NSF-funded programs, such as MACOS, are attempting to establish cases for their projects.

Mayer goes on to say that science knowledge in American schools and in the adult population is declining “because such a negligible amount of money was spent to prepare teachers to handle these new scientific materials,” and he cites the figures of \$55,300,000 available to NSF for precollege educational purposes in 1968 as opposed to \$5,500,000 today. However, the trend in declining test scores began about 12 years ago—before the decrements in NSF funds existed.

Why don’t we educators direct more efforts toward systematically assembling materials to produce solutions of a practical nature that might be useful in mitigating this trend? We should do this before we make representations to Congress and the tax-paying public that are more vocal than substantial. Certainly we need the continued support of the public, but we also need actions that will alter ineffective programs if we are to prevent further erosion of federal support for science and science education. The question that we in science education and those officials of NSF with responsibilities in the area of implementation have failed to answer to the satisfaction of the members of Congress is, “Are the current implementation activities conducted by NSF the most sensible and practical way to validate needed curricular changes?”

The present procedures for effecting reforms in science teaching were initiated during a period quite unlike the social context in which education finds itself today. Maybe the procedures by which new programs are orchestrated into our educational systems could be updated, improved, and strengthened by considering alternative approaches rather than taking pen in hand