(as well as for social companionship) within my definition, and I think I also know well the scientific or archival function that undisturbed areas must serve for future ecologists. Presumably Brower reads "multiple use" of wilderness preserves, a slogan of some of his opponents, and he knows, as I do, that by that self-contradictory philosophy Mount McKinley National Park will suffer the fate of Walden Pond. But as a historian of environments, I also know that *absolute* freedom from human disturbance has been unattainable since the Neolithic age began. I am just optimistic enough about human character to suspect that future generations will find their Waldens in places as tame as Thoreau's Walden must have seemed to John Muir. The melancholy fact is that most of them will have to.

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Imprinting

It is regrettable that the term *imprinting* appears headed for the same semantic limbo that *instinct, innate,* and similar, once useful, terms attained some years earlier. This trend toward confusion has certainly not been retarded by the two most recent publications dealing with the subject of imprinting (*1, 2*).

Imprinting has generally been regarded as a somewhat distinctive form of learning (*3*). Its primary characteristics appear to be a restriction of its occurrence to a fixed and relatively brief period in the life of an organism, the absence of overt reenforcement apart from that provided by the subject's response, and a relative stability of the preference that develops for the imprinted surrogate. Hess (*4*) has adumbrated some additional characteristics, though the significance of some of these (for example, differential effects of drugs) is questionable.

Now it should be made clear that all but possibly one of these characters is common to forms of learning that have not, in the past, been considered instances of imprinting. Latent learning characteristically may occur in the absence of overt reenforcement; single-trial conditioning is also not unknown (*5*), nor is the stability of the imprinted response as irreversible as was originally supposed by Lorenz (*6*). The only factor in regard to which one can still assert the uniqueness of imprinting is its temporal fixity: if exposure to a surrogate does not occur within a limited period during the development of the organism—the critical period—the preference for that surrogate does not

develop. If one argues that the concept of imprinting does not involve the notion of a critical period, limited in time, one can no longer defend the view that there is anything unique about imprinting as a learning process.

I have argued elsewhere (*7*) for the view that intermediate processes link imprinting to conventional types of learning. However, since we do know that some kinds of responses can be established only by exposure to the relevant stimuli during a specific and brief period in the organism's life, and that the response is linked to that stimulus in the absence of overt reenforcement, it does make sense to regard this type of learning as moderately distinct and to call it imprinting.

The papers originally cited, therefore, are deemed misleading on the following grounds.

1) Gray's (*1*) periods of exposure to the model extended for intervals of 24 hours and to ages of up to 5 days after hatching. How he can still assert that he has disproved James's contentions (*8*) when James adhered to our more precise definition of imprinting is difficult to understand. Under normal conditions, one might expect the result obtained by Gray to be attainable at *any* period in the life of the chick. In contrast, James's results can be expected only from chicks of a specific age, the critical period.

2) Moltz's (*2*) efforts to redefine "imprinting" operationally are manifestly pointless. When he ignores his own dicta and continues, in his discussion, to use *imprinting* in a manner differing from his own definition, he compounds confusion.

Finally, I wish to assert that much of the dissatisfaction with the critical-period criterion for the occurrence of imprinting has been assuaged. The difficulty has generally lain in the fact that no two workers could agree on the temporal definition of the critical period. It has recently been suggested (*9*) and demonstrated (*10*) that this has been due to age determinations having been based on the event of hatching (in birds, at least), an event notoriously susceptible to environmental influences. When age determinations are based upon developmental age—that is, time elapsed since the onset of blastulation—no such major discrepancies appear. Thus, it appears entirely reasonable and empirically valid to define imprinting as a rapid form of learning limited in its occurrence to specific developmental stages. That, after all, was what Lorenz (*11*) originally stated.

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References and Notes

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2. H. Moltz, *Psychol. Bull.* **57**, 291 (1960).
3. W. H. Thorpe, *Learning and Instinct in Animals* (Methuen, London, 1956).
4. E. H. Hess, *Science* **130**, 133 (1959).
5. D. K. Adams, personal communication.
6. G. Gottlieb, *Behaviour*, in press.
7. P. H. Klopfer, *Ecology* **40**, 90 (1959).
8. H. James, *Can. J. Psychol.* **13**, 59 (1959).
9. P. H. Klopfer, *Wilson Bull.* **71**, 262 (1959).
10. G. Gottlieb, *J. Comp. Physiol. Psychol.*, in press; P. H. Klopfer and G. Gottlieb, *ibid.*, in press.
11. K. Lorenz, *J. Ornithol.* **80**, 50 (1932).

I should like to observe that, besides being a capable investigator, Klopfer has an ability to identify the important points at issue in the theoretical treatment of imprinting. But I cannot agree with some of his criticisms.

First, he seems to object to the fact that I did not refer to a critical period in my most recent article on the subject of imprinting (*1*). The puzzling feature of the objection is that Klopfer did not mention any of the three earlier articles on imprinting of which I was author or co-author, wherein criticality *was* discussed (*2,3*). His apparent inclusion of me among those who do not give proper attention to criticality might therefore be construed as something less than correct. The reason I neglected criticality in the article in question was the absence of appositeness.

My report on imprinting to motion-

less objects was aimed at James's (*4*) discussion of retinal flicker in imprinting, a discussion which emanated from what I felt to be a misinterpretation of otherwise perfectly valid data. I have tried, and failed, to understand why Klopfer does not believe my experiment to be a test of James's interpretation. Since my first independent group was started at the age at which James began his dependent groups (experimental and control), Klopfer seems to be saying that I cannot adduce imprinting because I did use independent groups. If my inference is correct, then Klopfer does not consistently hold to his listing of one of the primary characteristics of imprinting as learning in a "fixed and relatively brief period." My period appeared to be as fixed as James's and was even briefer so far as range of age on exposure was concerned; if Klopfer implies that my experiment is invalid because I used a total exposure time per subject of 24 hours whereas James used but a fraction of this, then I haven't the faintest idea what to say except that Klopfer has one opinion and I have another.

Second, the hypothesis which Klopfer advances to account for the differences in critical-period topography from experimenter to experimenter is ingenious and, to a zoologist, undoubtedly plausible. I myself doubt that variation in the onset of blastulation would explain anything more than the subject variability demonstrated when a group is exposed under a certain condition to a certain model, and even here I doubt that the hypothesis is sufficient. This is not the place to present experimental data, but perhaps I may say that I have evidence indicating it is differences in the *models* which produce some, and perhaps most, of the discrepancies in the topography of the critical period.

This finding indicates to me at least, a genetic coding in the animal for reactivity to characteristics of the biologically natural social companion, which we may have been approximating in various degrees with this and that different kind of model. I have evidence that releasers can play a role in the responsiveness of chicks to novel objects, a more significant role than contemporary researchers may be prepared to accept (*5*). I wonder if Klopfer's hypothesis, whatever its validity, does not encourage disdain of the fact that developmental level of behavior can be identified only through behavioral research. We are not yet in that ideal (and perhaps mythical) stage of science where physiological events can predict behavioral events previously unknown. Until that time, and provided Klopfer's hypothesis is not absolutely valid, I suspect that dating the age of a subject from birth (or hatching) will work as handily as dating from blastulation.

Third, as a zoologist Klopfer ought to realize that much of the reason for dispute about imprinting is that theorists refuse to consider a functional theory of imprinting. It is not that such a theory is nonexistent, because several years ago I published my opinion that the function of the process of imprinting is the establishment of a social bond between the young and its parents, whether in animals or man (*3*). The study of imprinting is the analytical investigation of this process, with emphasis on the behavior of the young. By seizing upon the more romantic elements of imprinting in birds, such as the rapid learning evidenced in some cases and the ability of any researcher to become a Pied Piper of sorts, investigators and theorists alike have ignored the most elementary of all questions in the delineation of a behavioral process: what the process does for the species to help it survive and procreate. Behaviorists may yet regret the day they forgot their Darwin.

Fourth, while Klopfer's allusion to the history of the study of imprinting is by way of being an expository device, I should like to submit a few words about this history, if the rather inadequate knowledge now commonly met with can be called history (*6*). Imprinting was discovered by Spalding, who was also, as nearly as I can determine, the man who first isolated critical periods (*7*). William James gave us our first systematic definition of criticality in behavior and first stated the opinion that the process we now call imprinting is ended by the onset of the fear period (*8*). That aspect of imprinting theory accredited to Lorenz (*9*)—that imprinting involves a rapid learning of the first moving object that the hatchling sees—was previously stated in its essentials by Heinroth (*10*), who apparently mixed long-known research facts with the quite peculiar learning theory of the German philosopher Hermann Samuel Reimarus (*11*). It was Reimarus the Cartesian who originated the conception that lower animals learn what they need to learn in a rapid manner to complement their instincts (compare Lorenz's similar conception in regard to imprinting); from Reimarus's point of view this rapid learning was possible because animals cannot learn very much.

While Lorenz should be given all possible credit for emphasizing the importance of imprinting, he cannot be given credit for a theory the basic outlines of which are not his. Nor should he be given credit for inventing the term *imprinting*, which is a translation of the German term *einzuprägen* used by Heinroth (*10*), which in turn bears strong resemblance to the term *stamping-in* frequently employed by Douglas Spalding (*7*).

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1. P. H. Gray, *Science* **132**, 1834 (1960).
2. ——— and K. I. Howard, *Perceptual Motor Skills* **7**, 301 (1957); D. M. Baer and P. H. Gray, *ibid.* **10**, 171 (1960).
3. P. H. Gray, *J. Psychol.* **46**, 155 (1958).
4. H. James, *Can. J. Psychol.* **13**, 59 (1959).
5. P. H. Gray, *J. Comp. Physiol. Psychol.*, in press.
6. For example, H. Moltz [*Psychol. Bull.* **57**, 291 (1960)] has reviewed the history of imprinting with practically no consideration of the long list of researchers, from Spalding onward, who investigated the phenomenon of imprinting without express use of that rubric. The names of some of the men who so contributed to our knowledge of this behavioral process would, I feel sure, be not a little surprising to the nonhistorian.
7. D. A. Spalding, *Macmillan's Mag.* **27**, 282 (1873).
8. W. James, *Principles of Psychology* (Holt, New York, 1890).
9. K. Z. Lorenz, *Auk* **54**, 245 (1937).
10. O. Heinroth, *Verhandl. intern. ornithol. Kong. 5. Kong. 1910* (1911), vol. 5, p. 589.
11. The writings of H. S. Reimarus are not generally available, but a critical discussion of his theory may be found in the English translation by his contemporary, C. G. Leroy [*The Intelligence and Perfectibility of Animals from a Philosophic Point of View with a Few Letters on Man* (Chapman and Hall, London, 1870)]. There is also the more sympathetic discussion in G. S. Brett, *A History of Psychology: Medieval and Early Modern Period* (Allen and Unwin, London, 1921).

Klopfer states that my "efforts to redefine 'imprinting' operationally are manifestly pointless." Considering his emphasis on the critical period, I suspect that his dissatisfaction stemmed

from the belief that I ignored the "temporal fixity" of imprinting, a characteristic which he regards (and rightly so) as unique. The following definition, contained in the article to which Klopfer refers, makes it evident that I did no such thing: "Thus, imprinting will be defined as the procedure of visually presenting to an animal a large moving object *during the first several hours of its life* under conditions that insure that the object is not associated with such conventional reinforcing agents as food and water" (italics added).

Klopfer also states that I ignored my "own dicta" and that I thereby compounded confusion. I must admit that I am unceratin as to what he intended to convey. To which dicta (or even dictum) is Klopfer referring? What is the nature of the confusion? To what extent have I compounded it?

In conclusion, may I say that it does not appear unreasonable to expect a scientist to be explicit when criticizing the work of another and to offer at least some evidence in substantiation of a sweeping dismissal.

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Handling Scientific Information

In a recent issue of *Science* [**132**, 1922 (1960)], Helen Brownson, in the article "Research on handling scientific information," makes the following statement: ". . . the essential problem of applying machines to the handling of scientific information on a large scale has yet to be solved. This unsolved problem has to do with means of analyzing the subject content, meaning, and relevance of documents for mechanized handling. Research directed toward this end is making progress but is still in its infancy."

What Helen Brownson calls *the* unsolved problem is really a pseudo-problem which cannot delineate or define a fruitful field for research. In *The Mathematical Theory of Communication*, by Shannon and Weaver, the following two statements appear: (i) "The semantic aspects of communication are irrelevant to the engineering aspects." (ii) "This does not mean that the engineering aspects are necessarily irrelevant to the semantic aspects."

If one properly understands these two statements, one can also understand why mechanized systems and coding can contribute to the semantic aspects of information storage and retrieval systems and why semantic considerations cannot contribute to the solution of problems of mechanization (engineering aspects). Suppose one wished to develop a high-fidelity system