effectiveness of the scientific lobby; the commercial ties of scientists in the recombinant DNA field; the control of the risk assessment process; the ability of scientists to contain the issues to laboratory biohazards; the role of environmental groups; and the politics of the Recombinant DNA Advisory Committee.

We shall await another collection of papers to bring the issues up to date and address these critical factors. □

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Synthetic biology

Richard Roblin

A Double Image of the Double Helix: The Recombinant DNA Debate. By Clifford Grobstein. Pp. 177. (Freeman: San Francisco and Reading, UK, 1979.) Hardback £6.90; \$11; paperback £3.50; \$5.95.

THE tone of books about the recombinant DNA policy debate has both reflected and fueled the intensity of public concern. As the debate grew heated during 1976–78, books by journalists and science-writers vividly detailed the early history of the controversy, and discussed possible biohazards. In the quieter atmosphere of 1979, Clifford Grobstein has contributed a balanced, analytical view of the American sector of the recombinant DNA landscape which emphasises the science and public policy issues thrown up by the turmoil. In doing so, he has written an excellent primer for the next stage of discussion about recombinant DNA policy issues.

Grobstein's book will be read with profit by a variety of readers. Here, interested members of the public will find an outline of the relevant science, a history of the controversy, and thought-provoking discussions of exploration, risk and research regulation. Scientists should find interesting Grobstein's interpretations of the social and political context of their research, even though they may not like the world which he describes. Bureaucrats and legislators should ponder Grobstein's comments on the incompleteness and instability of current public policy on recombinant DNA research. In Grobstein's opinion, failure to extend application of the NIH Recombinant DNA Research Guidelines to industrial as well as academic research risks undercutting their effectiveness.

One of the major sections of the book deals with the issues surrounding regulation of basic research. Grobstein finds such regulation generally distasteful, and articulates several requirements for sound regulation of research. He views continued research and expansion of human knowledge as "essential to the public interest", and would restrict these

only when "there is compelling evidence of clear and present danger." However reassuringly familiar this criterion sounds to American ears, how would it apply in the recombinant DNA case? The conjectural nature of recombinant DNA biohazards remains the central intellectual dilemma of recombinant DNA policy. Except for intentionally malevolent applications, there never was, and may never be, compelling evidence of clear and present danger in the use of recombinant DNA techniques. What policy to follow given this uncertainty remains the central issue of the recombinant DNA debate. Does this mean that Grobstein believes the guidelines approach and the concomitant public and legislative furore to be an example of unsound regulation? He doesn't say, and I believe he would have made his book better if he had been more specific in this and several other places.

One thorny problem associated with the recombinant DNA debate is the role of the public in policy formation. If there are conjectured risks to public well-being because of the use of recombinant DNA techniques, then should the public have a say in how these techniques are to be used? Grobstein points out the tendency to apply criteria and procedures developed for regulation of human experimentation to recombinant DNA research. This analogy led some to suggest that appropriate policy for recombinant DNA research required something like a public 'informed consent' before proceeding with widespread use of the technique. Grobstein raises, but does not directly answer, the question of whether this extension of the principles of risk-benefit calculation and informed consent is appropriate for research in general (or recombinant DNA in particular). My own feelings are that these criteria are not appropriate where risks are conjectural, because meaningful risk-benefit estimations cannot be made. In addition, why single out the area of recombinant DNA research, when public "informed consent" has not been seriously urged in other research situations (for example, deepspace exploration) where risks could also be conjectured?

Inevitably, in an area as fast moving as recombinant DNA research, the pace of new discoveries outstrips the rate at which books can be conceived, written and published about them. Thus, Grobstein mentions only in a footnote the recently discovery that the DNA sequences which

code for several eukaryotic viral and cellular proteins are not all adjacent to one another as they are in bacterial DNA. The discovery of 'split genes' and RNA splicing in higher eukaryotic cells, and the lack of evidence for RNA splicing mechanisms of comparable specificity in *E. coli*, have led many scientists to conclude that split eukaryotic viral and cellular genes will not be correctly translated into functional proteins in *E. coli*. If many eukaryotic genes are split and cannot produce functional proteins in *E. coli*, then the chance of producing pathogenic *E. coli* through 'shotgun' cloning of eukaryotic DNA segments is reduced still further. These arguments were partly responsible for the considerable reduction in containment for such cloning experiments in the December 1978 revision of the NIH Guidelines. It is unfortunate that Grobstein was unable to include the impact of this new information in his discussion of potential risks in recombinant DNA research.

Grobstein ends his book with a suggestion that a presidential study commission be appointed to work toward a comprehensive assessment of issues generated by the recombinant DNA debate. He feels that this would create a needed arena "in which all issues can be examined and which gives full access to all interested parties and experts". However, since Grobstein's book was written, there have been significant changes in both the content and administration of American recombinant DNA policy. Estimates of the possible risks of recombinant DNA experimentation have dropped further. Accordingly, containment requirements for several types of experiments have been reduced, and significant supervisory authority over recombinant DNA experiments has been delegated to local Institutional Biosafety Committees. The NIH Recombinant DNA Advisory Committee (RAC) has been enlarged to 25 members, thus expanding its expertise and diversifying it politically. Meetings of the RAC have become a national forum for debating and voting to recommend changes in the NIH Guidelines, the chief ingredient in current American recombinant DNA research policy. RAC procedures, which provide for prior publication of proposals to be discussed and publicly accessible meetings, have opened a permanent channel for public input into decision-making on recombinant DNA research policy. In addition, the major scientific and procedural objections of critics like Sinsheimer, Wald and King have either been accommodated or been found to be generally unpersuasive. In the light of these recent developments, one wonders whether Grobstein would continue to advocate a presidential study commission.

Given the current trend, we are at the beginning of a period in which the NIH Guidelines will be partially or completely

dismantled. Some scientists believe that it will be possible to stuff the genie of regulation of recombinant DNA research back into the bottle. If so, what kind of a legacy will the recombinant DNA debate leave? Perhaps it is too early to clearly assess this question, but the following thoughts come to mind. The development and evolution of guidelines for recombinant DNA research provides a case-study in innovative formation of a policy to cope with uncertainties generated by a major expansion in human knowledge and power. Early discussion of the possibilities in public, a continuation of cautious research coupled with risk-assessment studies, and built-in mechanisms for modifying regulations in the light of new

knowledge may prove to be useful principles in analogous future situations. In addition, the recombinant DNA debate has conveyed to a wide audience the vast potential of the 'synthetic biology' opened up by recombinant DNA techniques. Thoughtful consideration of the ends to which this potential is being and could be applied should commence, as concern over potential biohazards diminishes. □

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Drives and action patterns

John R. Krebs

The Origins and Rise of Ethology: The Science of the Natural Behaviour of Animals. By W. H. Thorpe. Pp. 174. (Heinemann Educational: London, UK; Praeger/Holt, Rhinehart and Winston/CBS: New York, 1979.) £5.90.

THIS book is an extended version of the first Niko Tinbergen lecture, given by Professor Thorpe to the Association for the Study of Animal Behaviour in 1974. He describes how ethology started, what it was like in the middle of the twentieth century and what it is about today. The middle section of the book, which is full of anecdotes and personal recollections of Professor Thorpe, is to my mind the best.

The early history of ethology is traced by Thorpe to John Ray, the seventeenth century British naturalist who, among many other things, did experiments on clutch size in birds. The subject in its present day sense (the naturalistic study of animal behaviour; not the "science of character-formation" as in the term used by J.S. Mill) emerged in France in the eighteenth and early nineteenth centuries through the writings of C.G. Leroy, J.B. Lamarck and E. Geoffroy-Saint-Hilaire. (Some readers may be amused to know that Leroy was gamekeeper of the area in France called Marly — virtually the same name as that given to the wood near Oxford in which R.A. Hinde carried out his thesis research nearly 200 years later.) Although Britain had a strong tradition of country naturalists in the eighteenth and nineteenth centuries, Thorpe rightly emphasises D. Spalding as the key British figure in the early development of ethology. Spalding pioneered experimental studies of behaviour development and had a great

impact on Lloyd Morgan, who was influential in espousing a scientific, reductionist approach to the study of animal behaviour. Among early American students of behaviour, Thorpe mentions particularly the contributions of Whitman, Craig, Lashley and Wheeler; perhaps he should also have included W.C. Allee whose book *The Social Life of Animals* foreshadowed much that is current in ethological thinking.

From this straightforward historical account, Thorpe moves to the period with which he is familiar from personal experience. In the 1930s and 1940s ethology became firmly established as a discipline distinct from other approaches to the study of behaviour, mainly through the work of Lorenz and Tinbergen. The account of this period is very revealing to those who have seen only the later years of the era of these great men. Today, Lorenz's theories are viewed as somewhat old-fashioned and naive, but as Thorpe stresses "The very fact that Lorenz's early papers now appear so outdated is a tribute to their effectiveness . . . It is the impetus which they gave to experimental work which has,

in a sense, rendered them so quickly out of date, although they are still of course extremely well worth reading".

According to Thorpe, the central problem facing ethologists in the 1930s was to elucidate the nature of instinctive behaviour. Although it was widely recognised that animals perform complicated "goal oriented" sequences of behaviour, such as nest-building and courting, without any particular training, there was no conceptual framework within which to analyse and dissect these instinctive behaviours. Lorenz provided the framework in a series of papers published in the decade 1931-41. Instincts were viewed as being made up of simple stereotyped, inborn movements (fixed action patterns) linked together by more flexible and modifiable goal oriented behaviour (appetitive behaviour). Both of these were thought to be driven by "specific drives" (physiological mechanisms). Usually the fixed action patterns were released by a specific stimulus (releaser) and they were consummatory in the sense of dissipating the specific drive for that appetitive behaviour and fixed action pattern sequence. Internal drive was considered to then gradually build up until a fixed action pattern could discharge it again. If no suitable stimulus appeared, the action might go off "in a vacuum". Professor Thorpe stresses that this theoretical framework was valuable because it immediately led people to investigate in detail the nature of instinctive behaviour. For example, physiological and endocrinological studies of drives, perceptual studies of releasers, and genetic studies of fixed action patterns were set up to test Lorenz's ideas.

While providing a lucid account of Lorenzian theory, Professor Thorpe also tells us many anecdotes from the immediate post-War years. Stories are told of how Lorenz nearly went to Cambridge

Niko Tinbergen and Konrad Lorenz in Cambridge, 1959