REVIEWS

SOCIOBIOLOGY

D. Barash, Sociobiology and Behaviour, Heinemann, 1979, £3.95 pb
T. Clutton-Brock & P. Harvey, Readings in Sociobiology, W.H. Freeman, 1979, £4.95 pb
W. Mackensie, Biological Issues in Politics, Manchester UP, 1978, £3.95 hc
M. Midgley, Beast and Man, Harvester, 1979, £7.50 hc
M. Ruse, Sociobiology: Sense or Nonsense? D. Reidel, 1978, no price
E.O. Wilson, On Human Nature, Harvard UP, 1978, £7.85 hc

Given its current cachet, it is not surprising how many books have been appearing on sociobiology. This is a recent selection that either tackle it directly or circle around it. Some are for, some against, some dubious; some useful, some plain bad. It is a common practice, when reviewing groups of books, to make a few general comments and then to look at the peculiarities of each in turn. I want to reverse this procedure, situating each in turn before I consider some issues common to them all.

Wilson's book is significant since it is the completion of his 'trology', dealing first with insect societies (his specialism), then with the general sociobiological thesis, and now finally with extrapolations to human beings. Many commentators have already noted how muted this third book is when compared with the brash claims of 'Sociobiology - the New Synthesis'. Gone are the vast claims about evolutionary ethics (which always displayed more ignorance than ideology). Gone are the most explicit assertions about the disappearance of other disciplines into sociobiology. But what remains is still hard-core.

A glance at some of the chapter headings reveals this: 'Aggression', 'Altruism', 'Religion'. Curious how hot the controversy between group-selectionists (ethologists) and individual selectionists (sociobiologists) is, since when it comes down to identifying main empirical issues, main units, and proximate causes, they look so much alike. For these are exactly Lorenz's concerns, for example.

In general, the arguments are really bad. Consider, for example, Wilson's reconstruction of the 'origins of homosexuality'. It is, of course, prima facie paradoxical on their account, because reduced chances of procreation should have meant that - if genetically determined - gayness should have been selected out millions of years ago. Ah no, says E.O., because 'maybe' homosexuals passed on their genes by helping relatives with shared genes to survive. This is a nice example of what various people have called 'Just-so' stories that make the theory indefeasible. If Wilson's case is to hold, he must also make the following claims: first, that historically gays did behave in this way (of which there is no evidence); second, that the reasons for this behaviour are biologically given. And the two aspects must be genetically linked - to be gay must be genetically associated with 'helping gene-relatives'. But in that case, why the hell are gays not determined to this behaviour today? No doubt an 'answer' will be constructed, and the Just-so story will roll on.

David Barash's book is another popularisation (something of which sociogiologists are very fond, Dawkins, for example, writing a very crude account of his own book in Vogue). As with Wilson's book, the gap between the careful recounting of animal studies, and the last-chapter extrapolations to human behaviour is incredibly wide. However, I wouldn't want to suggest a split such that animals are left to the sociogiologists, while we hang on desperately to the humans. For consider his handling of animal intelligence. He cites (p48) the experimental evidence on the selective breeding of maze-bright and maze-dull rats, as part of his general evidence for a genetic component in the quantity of intelligence. But then he notes the further experiment in which maze-bright and maze-dull rats were then tested on slightly different mazes, and all difference in speed of learning disappeared. Barash is perplexed: 'the implications of this finding are obscure' (p49). But to me they are crystal-clear, and a vital refutation of sociobiological assumptions and methods. Am I odd in finding them so obvious? For doesn't it show that what was being genetically selected for was not maze-brightness at all, but some accidental advantage for that particular maze?

Barash's book is particularly interesting, in my opinion, for its handling of the charges of 'ideology' against the sociobiologists. Right at the beginning (p7), he agrees that past complaints against the political uses of Darwinism have been quite justified when one considers the way in which a notion of 'natural competition' became an ideological justification for laissez-faire capitalism.

But by the end of his book he gets very upset about charges of racism against his fellow-theorisers since he insists they are asserting the 'unity of mankind'. Yet less than 30 pages later, he is arguing:

> Genetic relatedness often declines dramatically beyond the boundaries of a social group and, significantly, aggressiveness increases in turn. Hostility towards outsiders is characteristic of both human and non-human animals. Physical similarity is also a function of genetic relatedness, and human racial prejudice, directed against individuals that look different, could have its roots in this tendency to distinguish in-group from out-group.... Clearly, this suggestion of a possible evolutionary basis for human racial prejudice is not intended to legitimise it, just to indicate why it may occur. (pp310-11)

It is very curious, and needs exploring, why the sociobiologists cannot see that this is exactly what the critics see as racist in their theory: the 'location of a genetic encoding for xenophobia'. The fact that they so commonly defend themselves against any such charge by using a fact/value distinction, I shall return to later.

Michael Ruse, a philosopher of biology, has written a deeply disappointing book. To be fair, the first third is very useful. It is a clear, careful exposition of what sociobiology is all about. In particular, he describes very well the range of writers to apply the theory to human society, from the hardliners like Alexander and Trivers, to the extremely cautious Maynard Smith.

But then he passes to what is effectively a defence of sociobiology against 'unfair criticisms'. And from here on, its quality of exposition and arguments falls dramatically. He maligns or misunderstands opponents, as when he cites as an aim of critics to stop research in the field. And like the sociobiologists themselves, he cannot understand the charge of racism except as a claim that the theory must covertly be caliming superiority/inferiority.

The key to the weakness of Ruse's book is in his claim that the meaning of the theory of evolution is obvious. The only alternative way of applying it to human beings that he can conceive is so naively put that it must cast doubt on the whole book's credentials. Having repeatedly referred to 'cultural components' as possible additions to genetic processes, without ever asking what sort of explanation a cultural account is, he asks:

> Could one bring up humans to have absolutely no aggressive tendencies, no interest in sex, no feeling for children, and no willingness at all to relate altruistically to others? This is what an extreme culturalist position would seem to imply. (p156)

I don't see why it has to imply this at all, even if we accepted the alternative as 'extreme culturalism'. But be that as it may, when Ruse wants to enter notes of caution about too easy an application of genetic explanations, he himself falls back on just such a view of culture, as the opposite of genetics:

> ... our genes might drive us towards maximising our own individual reproduction, but this is not to deny that through our culture we might decide to limit reproduction for the good of the group. (pp84-85)

But as many of us have been trying to point out, that leaves a quite irresoluble dualism of genes and culture. How is the culture supposed to establish itself in opposition to the genetic drives that Ruse describes? Overall, I don't feel that his book takes us any deeper into the theory, even in his defence of it.

Mary Midgley's book, however, is very important. It seems to me to come from the heart of what is best in British philosophy's current attempts to escape the restrictions of its immediate analytic past. Without doubt it is going to be widely read and used in teaching. For it is the best that liberal philosophy is capable of producing on sociobiology at the present time.

And in truth some bits are very good. She has a very fine discussion of 'beastliness', the tendency to regard animals as inherently brutal and dangerous. It is delightfully written, in sparkling prose. There is also a particularly good discussion of the idea of a direction to evolution, and of the concepts of 'higher' and 'lower'. But it is remarkably thin on alternative conceptions to sociobiology. And so, even in the sort of descriptive clarification at which her book is best, one is apt to come across nasty little slides into theory, such as:

> It is one of Lorenz's more interesting suggestions that only creatures capable of aggression towards their own kind are capable of affection. (pp47-48)

But this is not simply an 'interesting suggestion', but a heavily charged hypothesis about the relation between primary drives and their possible ritual redirection. It is a central theoretical proposition of both ethology and sociobiology.

Her book is mainly concerned to agree with the fundamental demand of the sociobiologists that humans be seen as continuous with other species; we are animals. Much of her argument here is to good effect. And indeed she is very good at demolishing the more absurd claims of Wilson et al. I liked in particular her destruction (pp169-72) of Wilson's claim that, come sociobiology's full development, subjects such as sociology etc would all become branches of neurology. She puts her objections wittily and prettily.

But in a curious way she still concedes much ground to them without admitting that she is doing so. For a start, in common with so many, she can conceive of no alternatives other than some form of genetic determination, or a 'blank tablet'. Then in her discussion of culture she can write: 'How far possessiveness and exclusiveness have innate, as well as outer, sources is a factual question' (p287). Given the long debates, particularly on IQ, about whether this way of phrasing the question is meaningful, that is a remarkable unargued gift to those she claims to evaluate. Indeed, the book is marred repeatedly by such concessions so that when she presents her own account - in so far as one can be disentangled - it is very questionable. It is based on a distinction between 'open' and 'closed' instincts. A closed instinct is one whose direction, object and pattern of activity are closely prescribed genetically; an open instinct is closer to being generalised 'interest' and motivational source.

But why continue to call them instincts at all? Her implicit answer seems to be based simply on the need for continuity of account. Humans are, after all, evolved animals. True - but that doesn't prescribe what the continuity consists in. And using the concept of an instinct carries an implication with it - that all instinctual behaviour is moulded and governed by the requirements of 'survival'. And Midgley accepts this, if we can take as evidence her tendency to describe human activities (eg p307) as having 'obvious survival value' - as though that were a sufficient explanation.

This is an important book, and in some respects a good one. But it is careless, taking over ideas and evidence uncritically (see, for example, her use of Shepher's dubious evidence on incest from the kibbutzim). And it is very weak in its theory of the significance of biology. We need to do better.

The Clutton-Brock and Harvey 'reader' is a pretty technical affair, bringing together some of the crucial articles that began sociobiology as a distinctive reinterpretation of Darwinism. Despite its technicalities, RP readers interested in the theory ought to read it. For several reasons: first, it is important that we avoid simplistic charges of ideology against these theorists. We must be clear that sociobiology arose out of a theoretical controversy within evolutionary biology. If we want to see it as an ideological development - as I do - we need an account of its ideological significance that can encompass that fact.

Secondly, some of the key concepts of sociobiology are here formulated very clearly and concisely. In particular I would pick out Maynard Smith's concept of the Evolutionarily Stable Strategy (ESS), an essential tool in the theory for modelling the interaction of individual gene strategies. To date, critics of sociobiology, both inside and outside biology, have tended to fire their guns at the theory in general. I believe that it is time we turned our attention to some specific concepts of theirs - and the ESS concept would be an excellent starting point. Its similarities with Hobbes' conception of politics as the mediator of individual egoisms, and with Smith's 'hidden hand' should be enough to get us worrying. But we need biological, philosophical and mathematical investigations of it. For the concept depends on the idea that in a society of animals there is a calculable balance of individual strategies (hawkish, doveish, cheats, etc) which would be stable and would result in the genetic maintenance of those strategies; and I suspect very strongly that it will only hold as long as arbitrary values can be assigned to the advantages conferred by the various strategies. Just as Lewontin has demonstrated that group selection could only occur under specialised circumstances, so I suspect can an ESS.

Thirdly, of course, all such arguments are only worthwhile if one is committed to Darwinism as a general programme, and to the need to save Darwinism from the sociobiologists. I believe that we have to be. But if RP readers are certain they are competent in this respect to withstand the sociobiological claim that they are straightforward Darwinists, I suggest they test themselves on Hamilton's 'Geometry for the Selfish Herd', or Trivers' 'The Evolution of Reciprocal Altruism', or Maynard Smith's 'Evolution and the Theory of Games'.

Finally Mackenzie's book, a published version of some lectures he gave, is an odd, stimulating and irritating set of pieces. At one level, it reeks of a certain style of lecturing: patronising, built on quirks and personal reminiscences, loosely structured onto apparently random references. I find this a pity when the lecture or essay, in the hands of earlier thinkers, was a chance for tight argument and the condensation of a thesis (see some of Kant's essays for brilliant uses of the form). This seems in part rambling old man stuff.

At another level, however, it tempts. Mackenzie breezily drops in a distinction between adaptiveness and adaptability - which, when one thinks about it, contains the germ of a real theoretical breakthrough. For adaptability implies that an organism cannot be defined by a repertoire of behaviours in relation to some fairly fixed 'natural environment'. You won't find this spelt out by Mackenzie who uses the distinction for different purposes:

> A man might be adaptable individually, but his conduct might not be for the species adaptive, either genetically or socially. In other words, the adaptable person may be a chameleon or conformist, but social adaptivity is a matter not of individual change but of social change, and that depends, other things being equal, on the presence of an adequate supply of creative non-conformists. (p60)

And thus we pass from a biological concept into some fairly trite political arguments. One senses that Mackenzie would like to be regarded as one of those non-conformists; and the effort to put his individual touch and a particular form of 'relevance' into the lectures prevents real development of ideas.

This book won't stay in print long, but a few people will be sparked off - either by irritation, or by spotting those old implicit ideas - to do something a bit more thorough than this 'speech-day' stuff.

Looking back on my comments on the particular books, I find I have been very critical of all of them. Nor do I want to change that in retrospect. With the exception of the Reader (which has other purposes, and a specialised audience), I find it significant that they are all lazily theorised. And the issues are far too important for that.

For a long time philosophy - especially in Britain - has managed to talk about humans as though Darwin had never written. The philosophy of biology has been seen as one of those sidelines that philosophers with odd interests might go off into. When a theory such as sociobiology emerges, and starts making big claims about ethics, epistemology, mind and behaviour (to name but a few), there is almost an embarrassed silence. Some take over, or are taken over by, the theory, accepting it with an incredible degree of uncriticism. But once accepted, those central concepts have a tendency to carry dangerous implications which ought to be noted. Consider Wilson, quite logically drawing a conclusion from his version of the implications of Darwinism for man: '... the intellect was not constructed to understand atoms or even to understand itself but to promote the survival of human genes' (p2). This is a correct claim if sociobiology is a starter. Its consequences are vast. We can't allow them to pass unnoticed.

In exactly the same vein, we can't allow to pass the use - repeated with boring predictability in every book that is pro sociobiology - of a fact/value distinction. There are a host of reasons for denying this, and we will need to spell them out, and out loud. There is the fact that organising concepts such as 'selfishness' and 'aggression' are drawn originally from political and moral affairs, and can be shown to carry still the tincture of that origin. There is the fact that a theory of fundamental motivation is being suggested; and therefore we could not have any reason to oppose the demands of our genes. And there is the fact that in this approach there is a project of proving behaviour innate; all significant human behaviour is swallowed up in this project, leaving no basis from which we could reject at the 'value'-level what is supposedly proven at the factual level.

We also need to point out that the charge of implicit racism against sociobiology is directed precisely against its 'empirical' claim that xenophobia is empirically encoded. And we can't do that without challenging the claim to be empirical. In the same way, Barash's claim that sociobiology is only sexist 'if sexism is recognition of male-female differences; however it does not imply that either sex is better' (p283) needs meeting with proof that such an assertion of difference is not simply a matter of evidence. It is a question of whether 'masculinity' and 'femininity' are meaningful concepts at a biological level, and of the ideological derivation of the sociobiological definitions of these.

All of which leaves an awful lot of work to be done. Sociobiology is a big challenge to us. It challenges us philosophically to develop our conception of Darwinism, a non-reductionist version. It challenges us politically, through its renewed scientific validation of racism (which the NF has now taken up - see Spearhead this year), sexism (which the women's magazines have been taking up), etc. And it challenges us to develop and work with a view of ideology that actually can cope with the diverse layers of sociobiology, on the one hand within the abstractions of population genetics, and on the other hand within the political discussion of immigration, housework, and the market economy. To date, I don't think we have done very well in answering these challenges.

Martin Barker

LYSENKO

D. Lecourt, Proletarian Science? The Case of Lysenko, New Left Books, 1977, £5.75

The history and significance of Lysenkoism has rightly become the crucial test-bed for all attempts at Marxist/Materialist accounts of science. The scandal of Lysenko's rise to power, the subsequent suppression of research in genetics and related fields, and imposition of his preposterous agricultural methods throughout the Soviet Union, has become a cautionary tale employed with great success by Western anti-communists: the philosophy of Marxism and the practice of socialism are incompatible with scientific liberty and objectivity, in short, with science itself.

For Marxists, apart from some tiny factions who still, apparently, advocate and propagate Lysenkoism (1), the problem is a far more serious and difficult one. That Lysenkoism was a catastrophic aberration seems indisputable. That there are themes, doctrines and aspirations in Lysenkoism which are widely held by socialists who would share this judgement is also not seriously disputable (not, that is, by anyone who has taken the trouble to read Lysenko). To what extent are those themes, doctrines and aspirations themselves implicated in the Lysenko catastrophe? What remains of dialectical materialism if the lessons of the Lysenko scandal are taken to heart (and head)? How far was the rise of Lysenko and the imposition of his doctrine a function of economic, ideological and political imperatives of the Soviet regime of the period, and the doctrine itself a 'Stalinist' travesty of the dialectical materialist philosophical legacy? What are the lessons of this episode for our understanding of the relationships between scientific research and the construction of socialism?

Lecourt's text is one of a very small number of serious attempts by Marxists to come to grips with these problems. After a preliminary discussion of the reception in France of Lysenko's famous report to the August 1948 Lenin Academy of Agricultural Sciences, and some analysis of the doctrine expounded in that report, Lecourt goes on to provide an

1 Some small Maoist groups mentioned in Bob Young, 'Getting Started on Lysenkoism', Radical Science Journal 6/7, 1978, pp81-105

analysis of the history of the development and consolidation of Lysenkoism in the USSR. In this Lecourt relies largely, as he concedes, on materials derived from Medvedev, Joravsky, and Graham (2). He divides the 'pre-history' of Lysenkoism into three periods. The first period, 1927-29, saw Lysenko becoming well-known as the promulgator of a small number of agricultural techniques. Most notable of these was the practice of transforming winter into spring varieties of crop plants by subjecting soaked seeds to low temperatures. 'Vernalisation', as the technique was called, attracted immediate official attention and was rapidly imposed on state farms over wide areas of the USSR.

From 1929 to 1935 Lysenko advanced theoretical explanations of and generalisations from his initial techniques, as well as those of Michurin, the revered Russian horticulturalist whose follower Lysenko now claimed to be. Michurin's work on hybridisation, and on 'vegetative crossing' through grafting became, together with the vernalisation techniques, the basis for an alternative theory of

2 Z.A.Medvedev, The Rise and Fall of T.D.Lysenko, Anchor, pb.1971
D.Joravsky, The Lysenko Affair, Cambridge, Mass., 1970
L.R.Graham, Science and Philosophy in the Soviet Union, London, 1971

heredity, one counterposed to the Mendelist genetics which predominated in the academic research centres of the USSR. Central to this new notion of 'heredity' was the theory of the so-called 'phasic development' of plants. Plant development takes place in a series of phases, there being, proper to each phase of development, a definite required constellation of environmental circumstances. If this is present, then the plant develops into its next phase. If, however, the plant is confronted with conditions alien to its heredity requirements, it will deviate from its normal course of development. According to Lysenko this, together with hybridisation and other techniques, are methods of 'destabilising' the heredity of plants. The off-spring of forms so treated are peculiarly malleable, and, if produced under conditions to which adapted strains are required for successive generations, their heredity can be 'fixed' at will. The incompatibility with the orthodox genetic ideas of the period, especially as distorted by Lysenko, is clear: 'vegetable hybridisation' refutes any notion of a mysterious heredity substance located in the sex-cells, and, most important, the environmental induction of directional changes in organisms, which are then transmitted to future generations, is asserted: the 'inheritance of acquired characteristics'.

The third period in the rise of Lysenko (1935-48) dates from the beginnings of the association between Lysenko and I.I. Prezent. Possibly under Prezent's influence, Lysenko's techniques and hypotheses become organised and unified under the doctrine of dialectical materialism. Lysenko's Michurinist theory of heredity is based on the practical experience of plant and stock-breeders (as, in the favoured version of Darwin's theory, were the materialist elements in Darwin), a science growing out of, and contributing directly to practice, the agricultural practice of the developing socialist society. Dialectical and materialist in its philosophical foundations, Lysenko's Michurinist teachings constitute the emergence of a new, proletarian science, irreconcilably opposed to the metaphysical (particulate heredity, independent of environment) and idealist (immortality of the germ-plasm), in short, bourgeois science of Mendelian genetics, whose ideological solidarity with racism and imperialism was undeniable. Once the link had been made between this bourgeois science, with its complete practical bankruptcy in the face of the Soviet agricultural crisis, and 'Trotskyite and other Double-Dealers' currently under notice of 'liquidation' by Stalin, (3) the way towards the suppression of genetics and its researchers was clear. The suppression gained momentum from 1936 until, when, in 1948, Lysenko's positions were officially consecrated, the few geneticists still prepared to fight for their positions at the sessions of the Lenin Academy either recanted or submitted to give Lysenko's report a unanimous vote of support.

Lecourt, in attempting to reveal a 'material base' underlying the astonishing rise of Lysenko, and the undiscriminating zeal with which the Soviet authorities seized upon, generalised and imposed his recipes, challenges the claims of Medvedev and Joravsky concerning the effectiveness of the Lysenkoist-Michurinist techniques, as distinct from the question of the truth or falsity of their supposed theoretical explanation. Certainly Lecourt is right to argue that it cannot be inferred from the theoretical falsity of the Lysenkoist doctrine that the techniques from which it was elaborated were ineffective. But it is of the nature of the case that there is little by way of direct, reliable and checkable evidence as to the effectiveness of these techniques in the Soviet Union during the earlier period of Lysenko's rise (Lecourt concedes that later on, caught up in the imperatives of the Stalinist political system, there was no alternative open to Lysenko and his associates but extensive fraud and fanciful invention of results). Lecourt's view that the early techniques, especially, were effective, at least under the limited conditions of their initial application, rests upon the grounds, first, that Lysenko's detractors themselves admit the effectiveness of these techniques, but deny Lysenko's originality, and, second, that the geneticists at the 1948 sessions do not challenge the results claimed by the Lysenkoists, though they do not hesitate to be scathing in other aspects of their critique of Lysenkoism. Beyond these rather weak (though, perhaps, the best available) arguments, Lecourt's case carries the rather circular source of its conviction in the presumption that something must have accounted for the immense enthusiasm for these techniques and their promulgator, and what else if not their success?

But even the success of the techniques alone, if that could be established, would not account for the haste with which the authorities proceeded to impose the Lysenko-Michurinist doctrine and practice after 1935. Lecourt's answer to this question lies in an analysis of the 'economistic-technicist' agricultural policy of the Stalinist state. Collectivisation was imposed forcibly as a way of increasing agricultural production, and of exacting from the peasantry a greater 'tribute' to the development of heavy industry. Collectivisation would make possible new technological developments and a vast expansion of agricultural productivity. When the series of bad harvests in the early thirties came to be analyses in this perspective they could be seen only as the outcome of the contradiction between the socialisation of agriculture and the continued use of agronomic theory and technology derived from the capitalist countries. A new agronomic science and technical base, appropriate to the new and revolutionary social relations of Soviet agriculture, was required. In addition, the colossal violence and repression of the collectivisation relied, argues Lecourt, on the forthcoming technological revolution to bring about an ideological revolution among the peasantry:

> This was perhaps the ultimate hidden motor of Lysenkoism, what gave it its strength and guaranteed its support: it had appeared at the right moment in response to a problem and a demand produced by a 'technicist' economic conception and practice of the construction of socialism. (4)

But overdetermining the role of Lysenkoism as the imaginary solution to the technical problems of Soviet agriculture, was its role as the 'ideological cement' of the social stratum of experts, administrators and technicians thrown up by the Stalinist agricultural programme.

Most of the rest of Lecourt's book is devoted to a

3 Lecourt, p49

4 Lecourt, p75

painstaking analysis of the main features of this 'ideological cement', in particular the logical articulation of Stalin's version of 'diamat' with Lysenkoist biology, and its functionality in relation to the imperatives of the Stalinist political system. It would be impossible to convey in the short space of a review the sophistication and brilliance of Lecourt's analysis in these chapters (4 and 5), so a rather crude and simplified outline must suffice. Lecourt argues that the 'Mendelism' attacked by Lysenko was a caricature. In fact, a reduction of the work of Mendel and his successors to the work of the 19th-century biologist August Weismann. This reduction and critique was effected by Lysenko in the name of Darwin. But Darwin's work, too, is subjected to a historical falsification. Darwin's work has a materialist, scientific core - the conception of evolution by selection - combined with an idealist ideological element - the concept of a struggle for existence - derived from Darwin's reading of the bourgeois ideologist Malthus. The Weismann/Mendel/Morgan tradition in biology elaborates the idealist, bourgeois side of Darwin's work, whilst the Michurinist/Lysenkoist tradition inherits the true, scientific and materialist content of Darwin's work.

These systematic falsifications are, Lecourt agrees, not independent of one another, but constitute a theoretical web whereby finalist, teleological conceptions which Darwin's conception of natural selection had replaced could be reintroduced into the theory of nature and history. The very concept of natural 'selection', which Darwin himself recognised to be metaphorical, is taken by Lysenko to have a literal theoretical meaning. If organisms can adapt to their environments, and pass this adaptation to their offspring, and this is the mechanism of evolution, then it is Nature itself which exercises real choices in effecting directional organic development. Once the secret of this is understood, then the mechanisms can be made available for human selection, and hence voluntary direction of organic life.

Lysenkoism as a biological doctrine is, then constituted by a finalist, teleological philosophy of nature. This finalism finds its systematic elaboration and rationalisation in the official philosophy of the Soviet State - in dialectical materialism. Lecourt attempts to show that Stalin's 'ontological' version of diamat involves a commitment to a finalist theory of natural and human history, an evolutionism, which in its application to human history is also 'technicist'. It was Stalinist 'technicism' in the construction of socialism in agriculture which made Lysenkoism necessary, and it was the finalist evolutionism of Stalin's version of diamat which came to provide the philosophical basis for Lysenkoism. A further consonance between diamat and Lysenko's biology is in the notion of the interconnectedness of nature and its processes: the idea of environmental conditions affecting the 'nature' of an organism. The Michurinist teaching is surely compatible with this aspect of dialectics, in contrast to the geneticists' alleged isolation of their 'hereditary substance' from environmental influences.

From this homogeneity it was but a short logical step to the presentation of the new science of heredity as an application of dialectical materialism. Further, since the latter philosophy is the world-outlook of the proletariat, the biology which is derived from it must be the first of a new category of revolutionary theoretical innovations: a proletarian science. The correlative judgement of the science of genetics need not be spelled out. Its 'administrative' consequences are all too clear. From 1948, the doctrine of the 'two sciences', bourgeois and proletarian, became the official doctrine of the Soviet state, and under that banner was unleashed an 'ideological class struggle' against all forms 'of bourgeois objectivism and cosmopolitanism'(5) which had consequences far beyond the boundaries of academic research in genetics.

Was this, Lecourt asks, an inevitable, logical outcome of dialectical materialist theses? If so, why were these conclusions drawn and these events unleashed now, in 1948, and not, say, ten years previously, when Stalin's Dialectical and Historical Materialism appeared? Lecourt's view is that the implications of a philosophical system are not necessarily drawn from it immediately. What determines when, and which of its possible implications are derived from it, and given practical shape, is something external to a philosophical discourse, not its own internal 'dynamic'. In this case the external determinants are the practices and requirements of the Stalinist state system. 1948 marks the consecration of the amalgam of Lysenkoism, diamat, and the 'theory' of the two sciences as the Soviet state ideological system: an ideological ensemble addressed to a specific social stratum - the 'intelligentsia' - which both participated in and served the power of the authorities. The 'ideological class struggle' unleashed among this stratum was to mobilise it in the interests of the domination of the state over the masses of the people.

It is Lecourt's provocative conclusion that this integral relation between Lysenkoism, the Stalinist state apparatus and the social structure it sustains explains the persistence of Lysenkoist doctrine (and, indeed, the revival of Lysenko's personal fortunes) well after Stalin's death, as well as the official silence of Soviet and orthodox communist philosophers concerning the whole episode: their refusal to investigate and to analyse its causes. As Althusser puts it in his laudatory introduction to the book: 'The history of the causes of Lysenkoism continues'. (6)

If some of Lecourt's arguments seem tendentious, if some of his explanations seem vague, schematic, or insufficiently supported by evidence (and, to this reader, some do), then this is readily conceded by the author, and its reasons should be evident to the reader. Despite these, perhaps unavoidable, weaknesses we have a courageous, penetrating, and serious analysis of one of the most challenging episodes in the history of socialism - challenging, that is, to those who have committed themselves to playing whatever part they can in the future of that history and who know that to do so they must understand and learn from its past.

Perhaps deliberately, Lecourt does not explicitly answer the questions as to the implications of the Lysenko disaster for present socialist practice which I posed at the beginning of this review (in

5 Lecourt, p114
6 Lecourt, p16

truth, he doesn't explicitly pose them either, but a concern with them is present in every page of the book). To what extent was the rise of Lysenko and the imposition of his doctrine a function of the economic (technical), ideological, and political imperatives of the Stalinist regime? Sometimes, at least as regards the last phase, Lecourt seems to come close to suggesting: 'wholly so'. '[It] was for no reason inside Lysenko's theory', we are told 'that it attained its universal destiny in 1948'. If one wants to explain this whole complex process, 'one cannot pronounce in terms of error and truth'. (7) It is almost as if the doctrinal content and epistemological status of Lysenkoism were irrelevant to its appropriation by external forces to serve purposes necessary to the Soviet state. This is a danger for those who share Lecourt's commitment to a 'materialist' (careful!) treatment of ideology as a reality, inscribed in social practices and rituals. To analyse historical processes, as Lecourt has done, with the help of such a conception of ideology, to give due weight to the determinants and effects of its reality, as does Lecourt, is not necessarily to be committed to the denial that this reality may be assessed as true or false, nor yet that its truth or falsity may be an essential question in the understanding of its determinants and its effects.

To fail to recognise this would, in the case of the present study, be to fail to pose the question of the complicity of the orthodox Marxist philosophy, dialectical materialism, as well as the biological doctrines of Lysenko, in the whole tragic episode. Not to connect Lysenkoism with the requirements of the Soviet State and to reduce it to those requirements, both, paradoxically, have the same effect: they leave intact and unexamined the practice and content of Marxist philosophy.

Fortunately Lecourt does not, in general, fall into this error, some of his less qualified assertions notwithstanding. The whole analytical procedure by which Lecourt seeks to demonstrate the internal connections of Lysenkoist biology, diamat, and successive sets of requirements imposed by the authorities would be irrelevant if that were his true position. It nevertheless remains the case that, with one qualification, Lecourt does not explicitly confront the questions: How far was Marxist philosophy itself implicated? What remains of Marxist philosophy if we learn the lessons of Lysenkoism?, what are the proper relations between philosophy and science, and between these and politics in the struggle for socialism?

The qualification concerns Lecourt's attempt to contrast Lenin's use of the dialectic with Stalin's 'ontological' doctrine of dialectical materialism. The argument is not entirely clear, but the point seems to be that whereas Lenin (and Marx, and Mao - but not always Engels!) used the principles of the dialectic as so many instruments of ideological struggle, means of opposing and dispersing dogmatism of one form or another, in Stalin dialectics becomes transposed from its role as a guide to thought into Nature itself, as its law and immanent form of motion. The implication, never explicitly stated, is that it is the ontologising of diamat that bears the responsibility for the catastrophe of the formation and availability of Lysenkoism for appropriation by the Soviet authorities. But this clearly will not do. Diamat is a dialectical and materialist philosophy. That is to say, it contains not only a logic and epistemology but also a philosophical ontology: it is only this which marks it off as a 'materialism' at all. Lecourt's apparent temptation, to reduce diamat to a heuristic, the status of a list of warnings for the thinker (a temptation shared, for example, by Lewontin and Levins in their work on Lysenko(8)) simply will not serve the purpose. Either this heuristic has no rational foundation (it is derived from authority, by revelation), in which case it is no less dogmatic than its ontological version, or it has a rational foundation. If the latter is the case, then it is hard to see how ontological presuppositions as to the nature of the world and the real conditions of possibility of our knowing it can fail to figure prominently in any such rational foundation.

Indeed, it is hard to see why Lecourt is so concerned to demarcate Stalin's ontological version of diamat as the culprit, from the philosophical work of Marx, Lenin and Mao, since he endorses the judgement made, ten years previously, by Louis Althusser, that 'Marxist Philosophy ... has still largely to be constituted'(9). This goes, too, presumably, for Marx, Lenin, Mao, and others as yet unmentionable, even by Lecourt? It is clear, at any rate, that the whole of the 'classical' practice of Marxist philosophy has to be called into question - not just that of Stalin. And an importantly relevant fact about that tradition is that it takes as one of its central points of departure, in Engels, a teleological evolutionary biology - that of Haeckel. The essential features of Lysenkoism as a biological doctrine, as distinct from a set of techniques, is already present in the tradition of diamat as constituted in Engels' later writings. There, too, in essence, is Lysenko's 'falsification' of Darwinism, the replacement of natural selection by the inheritance of acquired characteristics, and evolution as an essentially directional progress from lower to higher forms (10).

But even this may not turn out to be the fundamental question. At several points in his text Lecourt recognises that liberal denunciations of two specific 'external' interventions have a point, but in each case he hastens on, as if to suggest that the point has only a conditional validity, or that it isn't the essential point. The 'external' interventions at issue are: (1) the interventions of a specific philosophy, dialectical materialism, as an instrument which settles a debate in a specific scientific domain, the theory of heredity, and (2) the intervention of a specific political apparatus (Stalinist State) to 'settle' the disputes of biologists and philosophers alike. To what extent is it legitimate for philosophy (any philosophy) to declare itself arbiter on scientific questions (any scientific questions)? Lecourt seems to reject the legitimacy of this intervention in the Lysenko episode, but in doing so he makes it himself. 'Finalism and science are incompatible': this is the philosophical premise of Lecourt's critique of Lysenkoism.

7 Lecourt, p120

8 R. Lewontin and R. Lewin, 'The Problem of Lysenkoism', in H. & S. Rose (eds), The Radicalisation of Science, London, 1976

9 Cited in Lecourt, p104

10 For a more extended argument to this effect, see T. Benton, 'Natural Science and Cultural Struggle', in J. Mepham & D. H. Ruben (eds), Issues in Marxist Philosophy, Vol. II

And what of the legitimacy of the imposition of political requirements, not just in the funding and institutionalisation of scientific research, but in the very theoretical categories of scientific discourse itself? As Lecourt himself recognises, but fails to analyse, the recurring theme of Lysenko and his followers, their proud boast, and their key argument against the Mendelists, is the relation of their work to the requirements of practice. The requirement that nature must be transformed finds its illusory satisfaction in a theory which inscribes the satisfaction of that requirement in organic nature itself (nature can be transformed in accordance with human will). Mendelism, which recognised mechanisms of heredity whose accessibility to human techniques of directional transformation seemed negligible in the relevant future, was characterised as a doctrine of fatalist passivity in the face of external nature.

Seen in this light, it is the anthropomorphic reduction of the natural to the human practised by Lysenko, not the scientific realism of the geneticists, which most deserves the epithet 'idealism'. It is an idealism born of the intervention of external requirements into the very constituting categories of a 'scientific' discourse. Such is generally the way when science is denied its conceptual and methodological autonomy from politics. No doubt many comrades will see in this the 'theoreticism' which Bob Young, among others, wishes to see rejected among the responses to the Lysenko episode (11). Against this I would assert, though I have not the space to argue for it, that only theoretical discourse constituted independently of external exigencies can adequately serve practical needs: science is not wishful thinking, and wishful thinking never serves practice well.

11 Bob Young, op cit., pp93-94

This too, though, is a philosophical intervention into scientific terrain. It stands against other such philosophical interventions. Whilst philosophy certainly stands in need of the ultimate credentials it used to claim in relation to the special sciences, it must also be conceded that science is by no means always 'alright as it is'. Perhaps the 'liberalism' of Mao's 'Let a hundred Flowers Bloom', banal though it is, is the only answer we have. What at any rate should be clear is that although there can be no certainty of a true outcome when discourse confronts discourse, there can be certainty that when discourse confronts the inquisition, the bonfire, the censor or the mental asylum, truth will not be the outcome. It should not be supposed, however, by those for whom the question of Stalinism is simply a matter of ethical abhorrence, that in the construction of socialism under the conditions faced by the Soviet people such a road would have been easy to follow: while the Soviet people starved and were slaughtered in war, the geneticists studied - fruit flies. A demagogue's paradise!

Ted Benton

MARX

Richard E. Olsen, Karl Marx, Boston, Twayne, 1978, $10.95 hc

This book has the admirable aim of providing a sympathetic interpretation of Marx's work while avoiding jumpting to premature conclusions or forcing Marx into a partisan framework. Although disagreement with such diverse figures as Marcuse and Althusser is registered, the book is introductory rather than innovatory. Each of the seven chapters attempts to deal with isolatable general aspects of Marx's theories. They are arranged to facilitate a progression from the more accessible to the more complex of these aspects.

After a largely biographical first chapter Olsen moves on to Marx's view of history. Drawing mainly from the Grundrisse he argues that Marx sees history as a movement immanent in societies but does not exclude the role of accident. This mixture of immanent laws and 'accidents' or countervailing factors is a main theme in the remaining chapters. The attempt in this interpretation to make Marx acceptable also weakens the challenge offered by Marx's theories.

The key chapter, on Marx's methodology and dialectic, is unfortunately uneven. Olsen approaches some of the main issues, for instance, the differing interpretations of the concept of labour in Marx and Hegel, but fails to develop their significance. A discussion of the starting-points of analysis and presentation in Marx's work, especially in the context of the critique of political economy, would have been helpful and would have brought out some of the difficulties in Olsen's own method of presentation. This chapter is also marked by Olsen's interpretation of Marx's work as a 'social science' which is only externally related to human purposiveness. In Marx's work, writes Olsen, 'We simply have a social world scientifically understood; purpose enters into the picture only in terms of application of this understanding.'

Nevertheless there are a number of useful introductions to some contentious issues; the problem of periodisation in Marx's view of history, the continuity of the concept of alienation, the transformation of values into prices, the immiseration of the proletariat, and the falling rate of profit. Anyone already acquainted with Marx will find nothing new in this book, though much that would have benefited from a consideration of Rosdolsky's work. It may, however, serve as an introduction to Marx for anyone wishing to start with some of the more hotly debated issues.

Pete Stirk

PROGRESS IN SCIENCE

L. Laudan, Progress and its Problems, London, Routledge & Kegan Paul, 1977, £5.95 hb

This is an interesting and important book which deserves to be widely read and discussed. It involves a sustained polemical assault on those philosophers who believe that rational progress in science is achieved by accepting or rejecting theories by appeals to the facts. According to Laudan it is not factual adequacy but problem-solving effectiveness which is the prime motor of scientific growth.

Laudan's thesis is developed in part 1 of his book, and applied to the history and sociology of ideas in part 2. He makes many valuable points in the latter section, particularly about rationality and the sociology of knowledge. However, in this review I shall concentrate on describing and briefly criticising the argument in part 1.

Problems are the focus of Laudan's philosophy, and he identifies two broad classes of them: the empirical and the conceptual. An example of the former would be why the leaves of trees are green - a well-known fact which only became a problem at a specific stage of scientific development. Conceptual problems are more fundamental, and involve issues like the possibility of there being action-at-a-distance, whether or not matter is to be identified with space, and if the universe reveals evidence of design by a supreme intelligence.

Laudan rightly points out that philosophers have paid far too little attention to the role of problems in science. What is more, their empiricist leanings have led them virtually to ignore conceptual problems. Yet if anything these have played a more important role in the history of science than empirical problems. According to Laudan, if there is continuity in the historical record it is at the level of the latter. Discontinuities in conceptual articulation and development are quite prevalent, however. Moreover, whereas empirical inadequacy is routinely tolerated by the scientific community, perceived conceptual deficiency is not, and can readily lead to outright theory rejection.

Reflecting this emphasis on conceptual change in science, Laudan takes care to distinguish what he calls research traditions (RTs) from scientific theories. Marxism and the mechanical philosophy are typical RTs. They specify what the world is made of, how those entities interact, and what methods should be used to study them. RTs sponsor theories which are, however, separable from them. The same theory can be accommodated within more than one tradition and theories are far more easily jettisoned than the global frameworks which underpin them.

Laudan's concept of an RT obviously owes much to Kuhn's paradigms and to Lakatos' research programmes. However, contra-Kuhn he believes that its fundamentals are continually challenged. He also argues that the hard core is softer than Lakatos suggested, and that it undergoes historical development. Typically, the 'essence' of contemporary Marxism, says Laudan, is not what it was at the turn of the century; it has evolved and has been modified over time.

The unit of appraisal in Laudan's scheme is the RT. The strategy of appraisal is a mini-max one: to progress, maximize the empirical problem-solving adequacy of an RT, and minimize the conceptual and anomalous problems with which it is confronted. Anomalous problems are not simply unsolved problems. An RT can be confronted with a host of unsolved problems but these do little to impugn its credentials unless or until they are solved by its competitors. Thereupon they become anomalies for the RT in question. An anomalous problem for a particular RT is one which it has not solved, but which has been solved by (one of) its competitors.

That granted, the assessment of progress in Laudan's philosophy is essentially context-dependent and temporal. It involves an evaluation of how a problem-solving entity (an RT) has performed over time, and by comparison with its rivals. His concept of rationality is parasitic on this concept of progress: it is rational to accept those RTs which are efficient problem-solvers. The usual procedure of defining progress in terms of rationality is thus inverted. conventionally progress depends on reason, which dispels the mists of prejudice and mystification. Close attention to the facts allegedly ensures the objectivity and rationality of our beliefs, and enables us to draw nearer to the truth, i.e. to progress. Laudan insists, however, that we have no way of knowing whether or not science is true or even probable, or whether it is drawing closer to the truth. The link between reason and truth is thus snapped. Progress is now characterised pragmatically as increasing problem-solving effectiveness, and reason is defined in terms of it.

One of Laudan's chief concerns is to develop a concept of reason which is sufficiently rich to assess as rational (at least) certain key episodes in the history of science. Typically, he suggests that by about 1800 it was rational to accept Newtonian mechanics in preference to Aristotelian mechanics. Starting from this 'pre-analytic intuition' we need to explore the cluster of considerations which Newtonians advanced in favour of their views at that time. It will emerge that their reasons for being Newtonians embraced both empirical and conceptual considerations. 'Internalist' history focuses only on the former. However, in England in the late 17th century, Newtonians believed their theory had solved empirical problems and that it was methodologically sound, as well as being an antidote to atheism and to 'left-wing' political views. Methodologically speaking, such considerations must also be built into our assessment of why it was rational to adopt Newtonianism by about 1800. Laudan's approach specifically makes allowance for this.

We are on treacherous ground here. Laudan proposes that, as a working hypothesis, we should assume that the supporters of Newtonianism in 1800 were behaving rationally. We are, at least to begin with, to take their reasons as good reasons for accepting that world view. But were they? Was it rational in or before 1800 to espouse a physical theory partly because it fitted in with one's 'conservative' religious and political views? This surely needs to be argued for, not assumed. And usually considerations of truth and justice are brought to bear in such arguments, and form an integral part of an assessment of the rationality or otherwise of people's behaviour. Laudan specifically eschews this option: he stresses that determinations of truth and falsity are irrelevant to the acceptability or the persuitability (in their embryonic phase) of theories and RTs. Having thus jettisoned truth (and, presumably, justice), he lands up espousing an essentially technocratic concept of rationality as problem-solving effectiveness. This can, of course, be used to justify the most heinous crimes.

Although I am most unhappy with Laudan's proposal, it would I think be churlish to end on so negative a note. Laudan is struggling against an arrogant tradition in Western thought which takes our science and the culture which has fostered it to be at the pinnacle of human achievement. It is essential that this view be fought against, and one way of doing so is to point out that there can be different conceptions of rationality from our own. However, no sooner is this done than one tends to slide almost inexorably into (what I take to be) the pitfalls of relativism. If I was clearer in my own mind how to handle the issue of relativism I dare say I could write a more trenchant critique of Laudan's position.

John Krige

WOMEN AND POWER

Carol and Barry Smart (eds.), Women, Sexuality and Social Control, Routledge & Kegan Paul, 1978, 121pp, £2.95
Eileen M. Byrne, Women and Education, Tavistock, 1978, 285pp, £3.95

Both of these books discuss issues of vital importance to feminist theory, but are predominantly valuable for their presentation of specific examples of how women's subordination is ideologically reinforced. This empirical approach is not, however, accompanied by a serious discussion of the more interesting theoretical questions that are raised. This is particularly unfortunate in the case of the Smarts' collection of essays, where, for example (as the title indicates), the editors fall pray to the amorphous functionalist notion of 'social control', which seems to imply an apparently agentless and mechanical reproduction of power structures.

Two of the articles in this edition present a potentially useful approach to the means by which women are forced into domestic labour. The Smarts distinguish between public and private spheres in claiming that women's oppression is maintained largely at the private level, because, since the industrial revolution, domestic labour has appeared to lack the surplus-value characteristic of more public forms of commodity production. While admitting that women are equally exploited in public life (by legislation, at work), they emphasize the specifically covert forms of oppression which tend to 'privatise' and hence disguise the nature of domination, and which make the victims themselves feel that their problems lie in their own personal lives.

Tove Stang Dahl and Annika Snare's 'The Coercion of Privacy' tries to take this discussion beyond the largely economistic boundaries which have prevailed in the last decade of socialist feminist theory. They argue that domestic labour is not compared with 'free-market' labour because the former has historically become 'invisible'. The reproduction of labour power occurs in a quasi-feudal framework, because mutual rights and duties are presumed, i.e. work for support and protection. The plight of the last serfs is maintained by the ideology of the privatised home as the ultimate refuge of non-competitive virtues. The home is the man's castle but the woman's prison, because for her such 'privacy' is not a respite from the capitalist world but a means of furtively denying the value of her labour.

Mary McIntosh attacks the underlying assumption often prevailing in empirical studies on male/female differences, that men 'demand' sex and women 'supply' it. She discusses three approaches to sexual behaviour, but, in the middle of a plea for cultural relativism in such explanations, she rather arbitrarily opts for a Freudian perspective. She thus fails to explain the relation of cultural differences to Oedipal development, and cannot account for variations in the experience and expression of sexuality in societies which are not patriarchal or based on the nuclear family.

Julia and Herman Schwendinger's account of their own work on rape is interesting reading because it tries to base theoretical analyses upon active participation in the Women's Movement, using a Marxian concept of praxis. The other articles in the book are straightforward empirical studies, e.g. on rape reports in the press, on working-class teenagers' perception of sexuality etc, and are useful in showing the role of ideology in various specific fields.

Byrne's book, at least, does not lack theoretical clarity; the problem is rather that its assumptions are those of 19th-century liberalism. She discusses only the practical organisation of curricula and claims that the problem of discriminatory education would be solved if women and men received the same training for all occupations. This requires that 'the leadership of education' be 'convinced' that 'male/female' does not equal 'vocational/domestic', and that positive discrimination be introduced through government intervention. Her book may be useful in reminding teachers of their daily sexism, but for those who seek to go beyond merely promoting women from the industrial reserve army to the front line it has little to commend itself.

Christine Lattek

SOCIOSOMA

Richard Totman, The Social Causes of Illness, Souvenir, 1979, £5.95 hc £4.25 pb

The concept of 'mental illness' has long been regarded as problematic, and critics of orthodox psychiatry, such as Szasz and Laing, have argued that the 'medical' model of what is known as mental illness is both theoretically inadequate and socially oppressive.

'Physical illness', however, has generally been regarded as relatively unproblematic, and it is this which Richard Totman wants to question. He argues that the deep-rooted idea that physical illness is something which has purely physical causes, and can be treated solely by methods such as surgery and medication, is not a satisfactory one, and is, in a sense, a modern aberration. (Theorists such as Freud, who have suggested that bodily illnesses or symptoms may have other than organic causes, have been out of the mainstream of modern thought about bodily illness.) It is this 'medical' model of illness, Totman argues, which more than anything else has been responsible for the depersonalization and alienation often involved in modern medical practice; for the almost esoteric cult of the 'expert' in the medical profession; and for the tendency to rely exclusively on surgical or physico-chemical treatments or palliatives.

The idea that 'stress' may make people ill has become a popular commonplace; but, Totman argues, it is inadequate, and in any case it is rarely accorded more than lip-service in the medical profession. He says that while in some cases 'physical' factors, such as the presence of a virus in an epidemic, may bear the predominant role in causing illness, it is nevertheless possible to identify predispositions to illness, including things like cancer, which are not based on physical factors as normally understood, but on the life situation of the person who is at risk.

Human beings, he says, live in a framework of social meanings; of 'structures' of knowledge, and sets of 'rules' which are the basis for selecting actions. Within these rules and structures, we are able to act purposefully, and receive confirmation from those around us. He also uses cognitive dissonance theory to suggest that we have a need and a tendency to justify or rationalise whatever we do in accordance with our predominant rules and structures. We have a need for purposeful action within an acceptable framework, and for consistency within that framework. Thus, he argues:

> A person's resistance to disease remains high provided that his attitudes, beliefs and values are sufficiently compromising, and providing that he is continually involved.

A person is at risk, however, if his life situation changes in such a way that he is disoriented, unable to pursue accustomed goals, with the old 'rules' no longer applicable; and he is more likely to be at risk if his attitudes are rigid, or his previous goals too narrowly limited to a single framework of action. Thus, in certain circumstances, bereavement, retirement, change of job or life style etc may put a person at risk.

Totman recognizes that it is not possible to prove a thesis of this sort by any sort of conclusive empirical test, but the evidence that he presents for the relation between illness and life situation is cumulatively impressive, and the critique of the depersonalization of medicine very welcome. His book stops short of asking, however, whether there may not be certain types of 'life situation' built into the very nature of current industrial capitalism, which tend to put people at risk. Although Totman's view of human needs incorporates both the need for commitment and involvement and the need for flexibility, adjustment and compromise, I think that he tends to stress the latter rather more than the former. It may well be, however, that the possibilities of serious and purposeful involvement are chronically restricted for many people, given the current nature of much industrial work, for example, or attitudes towards 'housework' and the role of women. And it may be that flexibility and adjustment have limits which are not due merely to personal rigidity or inadequacy. While it is obviously true that one needs to help individuals in their current situation, and they cannot wait for social change, it is a pity if talk of adjustment, compromise and flexibility blur the more general sorts of social question that need asking.

Jean Grimshaw

UTOPIANS

B. Goodwin, Social Science and Utopia, Harvester, 1978, £11.50 hc

This is a book with an important theme trying to get out. Its aim is to explore the structure of the ideal societies proposed by the 19th-century utopians. Centrally, Goodwin tackles Godwin, Owen, Fourier and Saint-Simon, and tries to identify their common starting-points and their subsequent divergences. This is a good project, since so many studies have viewed these thinkers only historically, and then in a narrow sense of their being precursors of us wonderful people who came later.

The sad thing, to my mind, is that in the end Barbara Goodwin is limited in this important project by the very tradition she has sought to escape. I'll try to illustrate what I mean. Goodwin traces the structure of their thought via their conception of the evils to be removed, via questions of agency, and so on. She examines their fundamental assumptions about human nature, the derived methods of control and cohesion, and the central values to be realised. These are all the right questions to ask, in my opinion.

But she answers them in an oddly thin way, such that interesting conclusions are completely missed. Take, for example, her discussion of reason (pp48-54). She notes that the utopians draw on a tradition in which 'reason is triumphant'. After a brief description of the way reason is the ultimate test for all these thinkers, she throws up against them a distinction between logical and critical rationality, and charges them with conflating the two. She then counterposes to them what she calls a social scientific (Durkheimian) view that 'reason bears an essentially socialising character'. Quite apart from my doubts about this source and validity of that distinction, and my suspicions about what the Durkheimian view of reason implies, it does seem to me that her 'rush to judgement' has caused her to miss such a lot.

In two main ways. First, she isn't historical enough. The concept of reason with which Godwin and Owen operated derived from the classic empiricist tradition, within which there is a problematic relation between reason and the passions. In Hume, for example, it was crucial to his theory of knowledge and society (expressed, for example, in his essay 'Of The Original Contract') that 'reason is, and ought only to be, the slave of the passions'. In Bentham's optimistic utilitarianism, reason is the capacity that resolves an otherwise impossible tension between pain and pleasure determining what we do, and determining what we ought to do. A use of the historical context of this sort could have revealed so much about both the theoretical and the practical characters of their concept of reason. Instead, Goodwin blames them for conflating teaching and preaching in their idea of reason. But she doesn't tell us how or why they did it.

Secondly, she isn't conceptual enough. She has a tendency to take on trust, without argument, certain current trends of thought, and counterpose them to the utopians. Thus the already mentioned separation of teaching and preaching. But since she is eager, rightly, to show the practical consequences of the utopians' central concepts, she ought to be willing at least to raise the question of the implications of her own counterposed views.

She doesn't. In fact the book is marred by the use of organising concepts which are conceptually and politically naive. Thus: 'Malleability is a second order human characteristic, vacuous in itself' (p60). Tell that to B.F.Skinner. Or: 'Philosophical psychology identifies need and greed as two constituents of human nature which set limits to the form which an ideal society can take.' (p70). Just whose philosophical psychology is that?

More systematically, this weakness reveals itself in her final judgement that either a view of society is to derived from a theory of human nature, or 'humane social values' (p195) will have to mediate between the individual and social forms. But it is only on certain very specific theories of human nature that derivations can be based. For good or bad, a construction of an utopian project from the writings of the early Marx would not produce a derived blueprint. But the theory of human nature would still be essential to the shaping of the new society.

What I'm arguing is that Goodwin has set herself a really interesting task, but denied herself the tools to complete it. She discusses, for example, the centrality of 'work' in their accounts of human nature, and dares to ask the big question: 'whether the utopians' designation of work as the central social activity represents a capitulation to the incipient capitalist work ethic' (p118). But she doesn't even attempt to answer it.

There are at least two ways in which this might have been done. First, one must consider the evidence of the influence of these writers; but Goodwin, fairly enough, is trying not to repeat the work done on the radical uses of the utopians. Or, one could examine the place of the concept of 'work' in their systematic structure. Given the nature of her book, I had hoped for this. But Goodwin explicitly disavows this possibility; she argues at the end that the use of a notion of human nature 'detracts from scientificity' since all one can do is look for consistency, and then examine the basic assumptions.

I don't agree at all. I think the significance of the utopians' use of 'work' is that, in the best of them, it is made an end-in-itself, and therefore becomes a measure against which society can be judged. (On the same grounds, John Locke must be considered historically more significant than Thomas Hobbes. Hobbes sees work as a consequence of the problems of men's selfishness and insecurity. Locke poses work as man's essential property. It's true that thereby Locke makes himself far more internally inconsistent as a theorist, but those very inconsistencies are revealing of the real political problems of liberalism. Therefore, it is not just a question of consistency.)

But quite apart from my personal reaction that one can go much further, I am left with the embarrassing question: if using 'human nature' as a data-base is so objectionable, why write the book? Especially since, she concludes:

> The now controversial principle of basing social theory on ideas of human nature, with the resulting intrusion of subjectivism, will not itself be challenged, as that was their chosen method, and a common one at the time. (p186)

This reveals, I think, all the problems. Despite her disclaimers, she has treated the utopians primarily as 'precursors to our better achievements'. On her premises, they are not very interesting precursors. And they are judged to have failed by a conception of science that is itself not opened to question. (See p175).

Her book contains a 'book' that is fascinating, and some of the material she draws together begins the job to be done, despite her method. Regularly, though, she seems to me to fall at the fence. A final example of it, that may indeed be the most important: without doubt, a prime point of contact between the structure of the utopians' theories and political practice is in the 'problem of the educators'. Who embodies 'reason triumphant', and how do they intervene? Given the fact that Marx's famous quotation was in large measure directed at Owen, it has some significance. But Goodwin, although mentioning it in several places, never explores the real impact of this problem either for their theories or for their historical significance.
What a pity.

Martin Barker