

players with all the best tunes.

The great difficulty here is that those who work in peace education serving an “unrealistic” approach are unlikely to get any short-term gratification from what they are doing. So it will be harder to undertake, or get others to undertake, than most kinds of peace education serving “realistic” approaches. And it will be harder for those who have already undertaken it, to keep going. The end is so distant, so much an outside possibility. Can we be sure that in leaving established opinions, we are following peace? This makes us dependent in practice on fanatics of some sort, on the less critical at any rate, for peace education. The problem then arises of informing the fanatical (the other fanatics) and directing fanaticism away from futile or even harmful ways in the meantime. I do not really know how to address that problem. Perhaps the universe had better be benign rather than not, after all, if we are to have the remotest possibility of peace.

- 1 This objection follows one made by Mr Timothy Curtis, of Preston, while an ancestor of the present paper was benefitting from an airing at the Human Sciences Seminar, directed by Dr Wolfe Mays at Manchester Polytechnic. Some considerations put afterwards by Mr David Melling of the Polytechnic, I hope to take up in another place.
- 2 This has debts to points made by Mrs Ulrike Hill, of Manchester, and Miss Pat Collins, of Plymouth, which I hope to take further elsewhere.

Cf. Eliot's:

Or the purpose is beyond the end you figured,  
And is altered in fulfilment. (*Little Gidding*.)

## **Barry Barnes, The Sociology of Relevance, and the Relevance of Sociology**

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I thought of putting something in the title about theology. Better not. Juxtaposing theology and any single social science might recall the sermon in *The Way of All Flesh* in which geology was shown first to be totally without significance for theology and then to be positive evidence for the literal truth of Genesis. If theology is to be explained away by sociology, that shows insufferable presumption. If theology could be helped by sociology, which would mean that theologians could or should learn something of it, then this

would seem to burden yet further theological curricula. If sociology has no special relevance, but yet is an honourable way of earning a living, both sociologists and theologians should continue to cultivate their separate gardens; but is this not the old idea that something could be true in theology and false in philosophy, and vice-versa, which so annoyed Aquinas?

Fundamentally, the objection to the involvement of sociology in theology seems to be that sociology developed to study things going wrong. There can be a sociology of error; can there be a sociology of truth? The best way of answering this seems to be to look at the work of the American historian of science, T S Kuhn, as presented by the Edinburgh sociologist, Barry Barnes.

Barry Barnes was originally trained as a natural scientist but subsequently developed an interest in sociology, and has a Readership in the Science Studies Unit of Edinburgh University. Thomas Kuhn has written detailed studies of individual scientists, notably Copernicus and Max Planck, but his best known book is *The Structure of Scientific Revolutions* (1962),<sup>1</sup> which seeks to discern general patterns in the growth of scientific knowledge. I shall try and see Kuhn through Barry Barnes's not altogether uncritical eyes; but this will mean looking at his *Interests and the Growth of Knowledge* (1977),<sup>2</sup> where Kuhn's appearance is a very modest one, as well as his *T S Kuhn and Social Science* (1982).<sup>3</sup> If a case can be made for showing that sociology is relevant not only to our avoiding error but also to our finding truth in the physical sciences, then there seems a possibility that it can help us in theology.

*Interests and the Growth of Knowledge* is a book aimed at "the intelligent undergraduate" and, indeed, anyone who has heard of the sociology of knowledge and would like to know more about it. Barry Barnes lays his hand on the table early in Chapter I. He sketches out a common view of knowledge:

"knowledge is best achieved by disinterested individuals, passively perceiving some aspect of reality, and generating verbal descriptions to correspond to it. Such descriptions, where valid, match reality, just as a picture may match in appearance some aspect of the reality it is designed to represent. Invalid descriptions, on the other hand, distort reality and fail to show a correspondence when compared to it; often they are the product of social interests which make it advantageous to misrepresent reality, or social restrictions upon the investigation of reality which make accurate perceptions of it impossible."<sup>4</sup>

Against this "contemplative" view he sketches an alternative view in which knowledge is both active and social. The world is known through the representations of it created by, accepted by, and

communicated by particular social groups in order to further their interests, which are bound to include prediction of, and control over, nature. This “social activity” understanding of knowledge is carefully distinguished from idealism since in it socially-created knowledge is always open to revision by feedbacks from extra-social reality.

Interestingly, of the five writers whom Barry Barnes considers in this first chapter, the two whose approach he finds satisfactory are art historians, W M Ivins and E H Gombrich, while the three he finds unsatisfactory as being stuck in shifting sand between the “contemplative” and the “social activity” views of knowledge, Jurgen Habermas, Karl Mannheim, and George Lukacs, are thinkers influenced by both the Hegelian strand in Marx and the neo-Kantianism of such late nineteenth century figures as Dilthey.

Barry Barnes finds that for the Lukacs of *History and Class Consciousness* human consciousness would ideally reflect reality were it not distorted by the pressure of class interests. Not only does this show the continuing, if unconscious influence of the “contemplative” view but it also ignores the historical nature of knowledge. We can only know the world as much as we do because of the resources accumulated over time in a particular society. While Karl Mannheim was programmatically committed to the “social activity” view of knowledge, he did not in practice develop this idea; indeed, he commended mathematics and “pure” science by reason of their freedom from social interests, a freedom which Barry Barnes and his Edinburgh associates have called most convincingly into question. Habermas<sup>5</sup> divides up the various forms of knowledge into three main groups, each determined by a knowledge constitutive interest. The knowledge constitutive interest which has created the natural and technical sciences is one in prediction and control; the “historical-hermeneutical” sciences rejoice in the possession of an emancipatory knowledge constitutive interest, by reason of their self-reflective nature. While Barry Barnes is fully entitled to argue, as he does, that Habermas does not understand how natural scientists do their work, he develops a positive counter-argument that all knowledge is instrumental, that is, concerned with prediction and control, and also normative, and therefore essential to the building of community and the task of self-understanding.

In Chapter II Barry Barnes considers ideology. He argues that Marx was right to claim that capitalist ideology distorted the competent work of Adam Smith and David Ricardo. But could a bad ideology produce good work? And could the good work continue if the bad ideology were withdrawn? Barry Barnes offers a yes to both questions, giving as example the case of Karl Pearson, whose

views on imperialism and controlled breeding for the lower classes horrify us today. Yet his work on statistics, which was very closely linked to his social and political views, continues to be very significant for present-day statisticians. Even if scepticism about statistics is permissible, geometry is surely sacred, in its Platonically pure abstraction. But the survival of the traditional proofs of the Euclidean theorems has been due not simply to their absolute logical compulsion (of which responsible mathematicians tend to be rather doubtful) but rather to their value as a means of transmitting the attitudes and beliefs which characterise mathematicians.

Barry Barnes then considers "The problem of Imputation". Lukacs faced the problem that people did not always do what it is in their class interest to do, but his solution, that ideal class-consciousness will only prevail after the triumph of Communism, dodges the question of how it operates here and now. Another writer, L S Feuer, who argues that all ideologies are embraced by individuals through perceived self-interest, seems mainly of interest as presenting an opposite, although not totally dissimilar, view to that of Lukacs. Neither seriously considers how beliefs actually develop in a particular social formation. Barry Barnes gives two examples of this kind of work; a study which he and Donald MacKenzie undertook on the reception of Mendelian genetics by British scientists,<sup>6</sup> and Lucien Goldmann's theory<sup>7</sup> of the link between Jansenism and the social situation of the legal nobility in 17th century France. While Barnes is deeply impressed by Goldmann's approach, he feels that the links between Jansenist theology, the various social strategies of the legal nobility, and the self-questioning of Pascal's *Pensées* is insufficiently developed.

The last chapter cautiously suggests that a satisfactory sociology of knowledge would be broadly Marxist, while avoiding the errors of Lukacsian imputation on the one hand, determination by purely technical factors on the other. The closing recommendation "to seek to express 'the intention of a good life' in well-informed and well-constructed activity"<sup>8</sup> is surely unobjectionable.

*T S Kuhn and Social Science* has the merits of the earlier book, notably its clarity and freedom from jargon. It is concentrated on Kuhn himself but throws a good deal of light on the development of the sociology of science at the present time. Little biographical information is given about Kuhn, and we are told practically nothing of his work on Max Planck. The Kuhn of this book is the Kuhn of *The Structure of Scientific Revolutions*.

The first chapter sets this book in its context. Kuhn himself quotes three influences on his work, that of Ludwig Fleck, a German medical scientist who wrote *Genesis and Development of a Scientific Fact*,<sup>9</sup> Jean Piaget the Swiss psychologist who studied

the intellectual development of children, and Ludwig Wittgenstein for his work on the way ideas are made use of within particular communities. However, intellectually respectable ancestors did not save Kuhn's book from giving offence. *The Structure of Scientific Revolutions* marked a decisive break with the image of the scientist as the heroic individual whose creative genius leads him to defy ignorance and prejudice and, by pure reasoning, reveal unguessed truths. Kuhn's scientist, on the contrary, is an individual whose training has been in obedience to authority and in respect for tradition; by it the scientist is admitted to a community of people who work closely together and who accept that authority can and should supervise the execution of their work and criticise any failure to adhere to accepted orthodoxies. The same description, if applied to ballet dancers, commandos, and Dominicans, would have been accepted as strictly factual; but applied to scientists it seemed to challenge the rationality of those whom Western society had put forward as the guardians and witnesses of its own claim to rationality. Kuhn was simply looking at science as a cultural activity, which had the normal features of any other aspect of culture, a historical tradition, a system of socialization, shared activities, institutional norms, boundaries and taboos, rewards and sanctions.

This emphasis on the cultural and social nature of science is clarified in Chapter II. We are told of the training of scientists "its most evidently distinctive feature is the extent to which it relies upon textbooks: the accepted terminology of a field, its methods, its findings, its favoured mode of perceptions, are all conveyed through their use. And the credibility of all these components of scientific culture depends not upon the indications of experience lying behind the exposition of the text, but upon the authority of the teacher, and the institutional apparatus which supports it".<sup>10</sup> If we ask what this text-book training gives, Kuhn's answer is: paradigms. Barry Barnes tells us that "a paradigm is an existing scientific achievement, a specific concrete *problem-solution* which has gained universal acceptance throughout a scientific field as a valid procedure, and as a model of valid procedure for pedagogic use. Carnet's cycle has been adopted for use as a paradigm in this sense; so has Mendel's experimental work on inheritance in peas, Bohr's on the electronic orbits of the Hydrogen atom, Crick's and Watson's on DNA".<sup>11</sup> Indeed, we are told on the next page that scientific knowledge is essentially a repertoire of paradigms. Without this repertoire, scientists would have no shared body of knowledge nor methods of research. Any established set of beliefs (not only in science) by the mere fact of its belonging to a community presents some kind of obstacle to critical examination or a search for

alternatives.

Having made this point, Barry Barnes offers a blast against “Manichaeism mythology”, by which he means the belief that authority and society are constantly at war with reason and creativity. In point of fact, most of our knowledge is acquired by listening to authority, and much of the rest of it is acquired by finding analogies to what we know already, that is, by a paradigmatic method. But what we learn from authority has to relate to experience at some point – Kuhn gives the example of a boy out for a walk with his father, who shows him a duck, a goose, and a swan; the boy learns simultaneously from authority and from his own observation. Likewise, when confronted by a new item of knowledge, we strive to place it in the categories which we have acquired by our exposure to an existing tradition. One need only recall the ancient *Punch* joke of a rural station master reflecting on the designation appropriate for a travelling tortoise, “This ’ere tortoise ain’t a dog or a cat nor a horse, its a bloomin’ insect”. But if our systems of classification are cultural constructs, not just reflections of the world-as-it-is, then this leads us towards what Barry Barnes calls “finitism”, the view that each use of a classification operates in a specific setting, and may not be transferable to all other settings. Kuhn’s analysis of changes in the understanding of “speed” in Aristotle, the late medieval theorists, and Galileo is seen as an example of finitism in practice. Philosophers who reject finitism seem to reject with it any significance for the sociology of knowledge.

Chapter III begins with a consideration of the nature of “discovery”. In the myth of heroic, individualistic, science, discoveries are the scientist’s trophies. Kuhn, understandably in view of his understanding of science as a social activity with its own history, suggests that “discovery” be thought of as a process, rather than as an event. The question of discoveries leads us to Kuhn’s distinction between normal science and scientific revolutions; normal science is when scientific research is being done according to an established paradigm, a scientific revolution is characterised by the collapse of an established paradigm and attempts to introduce a new paradigm.

“Normal science” is not just a period when nothing exciting is happening, and measuring and deduction from rules take the place of creative thought. Normal science requires invention and imagination. New discoveries in normal science are validated ultimately not by chemical or mechanical tests but by the consensus of the scientific community. Scientists of equal competence may sincerely disagree on the evidence of validity of a new discovery. The growth in certainty of a scientific community is a social process and can be legitimately studied sociologically.

Scientific revolutions begin within the detection of an anomaly that cannot be accounted for within the dominant paradigm. The accumulation of anomalies is likely to produce increasing dissatisfaction among scientists in a particular field of science and to searches for alternative paradigms which would remove the anomalies. A scientific revolution is characterised by discontinuities in research and the growth of knowledge, resulting in new modes of cognition, inference and explanation. Kuhn gives as examples of scientific revolutions the discovery of oxygen and the adoption of Daltonian atomism in chemistry, the transition from Aristotelian to classical mechanics and then to quantum mechanics in physics. Barry Barnes, while noting that for Kuhn scientific revolutions are to be expected, suggests that he has not clearly defined the magnitude of change required for a new direction of research to be classified as a revolution. "They include changes in the common culture of the educated elite of the whole of Europe, and esoteric modifications in the problem-solutions of small groups of highly specialized professionals."<sup>1 2</sup> In fact, Kuhn's account of normal science seems more value than his sketches of scientific revolutions because it shows more clearly the social, historical, and paradigmatic nature of scientific research. The chapter closes with a brisk refutation of the criticisms brought by Sir Karl Popper and Imre Lakatos.

The fourth chapter is entitled "Evaluation" – a well-argued polemic against the idea that it is ever possible to step outside the historically and socially conditioned world of the scientific community and find some absolute logic which can, like the blindfolded goddess of justice, make an unerring and presuppositionless decision. Or, to put it differently, the evidence is never strong enough to free human beings from the responsibility of an interpretative judgment. In his polemic, Barry Barnes is happily led to elucidate the term "conceptual fabric". This he defines as "a structure made up of generalisations which connect concepts into a single integrated whole". The hypotheses which compose theories in the natural sciences are not independent of each other, nor are they acquired step by step. They are necessarily related to each other, so that good evidence for one will raise the credibility of the others linked to it. On the other hand, the refutation of one hypothesis will not necessarily bring the rest of the conceptual fabric crashing down, since, for example, it may show that, while a particular law is valid, the range of categories to which it applies is not as wide as was at first thought.

Is not the repetition of a scientific experiment by another researcher sufficient to test it adequately? Barry Barnes negates this by referring to a study by H M Collins<sup>1 3</sup> on a reported case of the detection of gravity waves. Here, scientists wishing to repeat the

original experiment insisted on doing so with improved equipment, partly because their historical knowledge of their science told them of errors arising from faulty equipment, partly because the culture of science stresses the importance of adding to existing knowledge, and partly because of that element of personal ambition which all professions regard as legitimate.

Nor is it possible to say, without reference to cultural and historical boundaries, where science ends and non-science begins. The rejection of parapsychology has been shown by more than one researcher to have taken place despite care by parapsychologists to be faithful to such scientific principles as rigorous statistical testing. Barry Barnes comments, "Not just the insufficiency, but the sheer carelessness of the arguments – is very striking".<sup>14</sup>

In the last chapter, Barry Barnes relates Kuhn's theories to contemporary work in the sociology of science, such as Brian Wynne's intriguing correlation between conservative political ideology, interest in psychical research, and belief in the existence of the ether among late-Victorian Cambridge physicists.<sup>15</sup> The point is hammered home that social and ideological interests do not harm inductive research but rather structure it. The final assessment of Kuhn mentions him in the same breath with Marx and Durkheim; but attention is also drawn to the limitations of those gentlemen. Kuhn's paradigms must also be submitted to culturally defined evaluation.

Has Kuhn, as presented by Barry Barnes, anything to say to theologians? It could be argued that his work is positively reassuring for believers since it shows that the sociology of knowledge is not tied to a crude reductionism which explains everything away as so much "alienation" or "projection". If social interests, far from harming, structure, and even promote the advance of scientific knowledge, then the not infrequent suspicion that Catholic theologians, having their conclusions fixed beforehand by ecclesiastical authority, must be guilty of objective dishonesty is in its turn thrown open to serious questioning.

A more significant objection to the use of the methods of the sociology of science in theology lies in contrasting attitudes to change. From the seventeenth century onwards, natural science has been committed to the increase in the amount of scientific knowledge; Catholic theology from the time of Irenaeus – indeed, from the time of the later books of the New Testament – has been profoundly conservative in its insistence that it is holding fast to the doctrines already known and taught by the apostles. Even Newman's *Essay on the Development of Christian Doctrine*, far from glorying in innovations in Catholic teaching, is intended to show that it has not significantly changed, but only expressed ideas with



greater clarity and specificity. But if Kuhn and the contemporary sociologists of science are doing useful work in studying how science actually develops, then the study of the social mechanisms by which an unchanging body of doctrine maintains itself in situations of cultural and institutional change should be very fascinating. If we look for an interpretation of the Church's understanding of the stability of her doctrine, we could surely find many elements in Newman's work; the dialectic, for instance, between the conserving, boundary-setting role of the Pope and the bishops, the debates and questions of the theologians, and the position of the faithful, apparently passive transmitters of tradition, but who, by their witnessing to that tradition, can and perhaps should be consulted on matters of doctrine.

And what do theologians do for anomalies? Or, to put it differently, what are the catalysts of innovation in a system that prides itself on continuity? For Newman, it was the heresies which imposed clearer thinking and more exact definitions. But if for him a heresy was a challenge to a sleepy-minded orthodoxy, it could also be an unsatisfactory preliminary sketch of a position which in its developed form would be found orthodox.

Newman does not, so far as I am aware, consider in any detail the problem of what we call cultural translation as a source of theological enrichment, although he was certainly aware of the issue. Another source of the restatement of Catholic doctrine which, so far as I know, has been very little considered by historians of theology is surely the changing social, as distinct from, cultural, base of Church supporters.<sup>16</sup> The great theologians of the fourth and fifth centuries were predominantly bishops, faced with a variety of problems, particularly those of their flock. The friars, linked to the new towns and the new universities, produced the theology of thirteenth century Europe. The theologians of the Counter-Reformation were predominantly Jesuits because the Jesuits were present on the new frontiers of the sixteenth century. The theology of Congar or de Lubac was written with a wider audience in mind than the seminarians who provided text-book theologians with their captive readership. In other words, a theologian surely intends his/her theology at a particular target and this targeting must surely affect the form of the work.

Clearly again, there are other social factors. David Edge and Michael Mulkey have shown in *Astronomy Transformed* (1976) how the growth of radio astronomy was affected by the relations between the scientists involved. Presumably the nature of contacts – or the nature of the lack of them – between theologians is relevant again for the shaping of theology. Would it be better to mass theologians together as at Louvain or Oxford so that ideas

may circulate among them, or spread them out to Maynooths and Ushaws so that they may relate to local parochial communities? And do these desirable things happen at Louvain and Oxford, at Maynooth and Ushaw? And if so, how? And if not, why?

May Barry Barnes forgive me. I have used his excellent survey of the sociology of scientific knowledge as a stalking-horse for quite a different purpose. But he has surely shown that knowledge inspired by the love of truth has no cause to fear scrutiny of its ties to human hopes and fears.

- 1 Usually, the second edition, 1970, University of Chicago Press, is quoted.
- 2 Routledge & Kegan Paul, London, pp x + 109, £4.75.
- 3 Macmillan, pp xiv + 135, £3.95.
- 4 *Interests and the Growth of Knowledge*, p 1.
- 5 *Knowledge and Human Interests* (English translation), London, 1972.
- 6 D A Mackenzie and S B Barnes, "Biometrician v Mendelian: a controversy and its explanation". *Kolner Zeitschrift fur Soziologie*, vol 18 (1975), pp 165-96.
- 7 Lucian Goldmann, *The Hidden God* (English translation), London, 1964.
- 8 *Interests and the Growth of Knowledge*, p 86.
- 9 First published in 1935, English translation in 1979.
- 10 *T S Kuhn and Social Science*, p 16.
- 11 *T S Kuhn and Social Science*, p 17.
- 12 *T S Kuhn and Social Science*, p 56.
- 13 H M Collins, "The Seven Sexes: a Study in the Sociology of a Phenomenon, or the Replication of Experiments in Physics" in Barry Barnes and David Edge (editors), *Science in Context*, Open University Press, 1982.
- 14 *T S Kuhn and Social Science*, p 92.
- 15 Brian Wynne, "Physics and Psychics; Science, Symbolic Action and Social Control in Late Victorian England" in Barry Barnes and Steven Shapin, *Natural Order*, (1979), reprinted in Barnes and Edge, *Science in Context*.
- 16 For instance, consider the difference between the dependence of the Baroque Church on the Catholic monarchies, and the alliance of the nineteenth century Church with the peasants and petty bourgeoisie. Historians who with naive cynicism ascribe the rise of Ultramontanism to power-hungry Vatican officials should try to apply the method of Marx in *The Eighteenth Brumaire of Louis Napoleon*.