



# Sociological Explanation <sup>1</sup>

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This was very much a *piece d'occasion*. The Department of Sociology had been established in 1964, and I was appointed to the Chair in the following year, so the '*occasion*' was, for the three people who made up the department's staff then, more than usually fraught – or, as I began by saying, 'daunting'. Hence its rather manifesto-like character, and the sustained argument, mostly by example, that sociology is properly a critical, assumption-testing, discipline.

Having to give an inaugural lecture is a rather daunting affair though, I am sure, a salutary one. Luckily, there is always tradition to sustain one and to afford some guidance. There are, one finds, models, or types, of inaugural lectures. I cannot claim to be a connoisseur, but, judging from a small and heavily biased sample, they seem to fall into three groups. There are those, to begin with, which announce new departures for a subject, new horizons, recent territorial acquisitions in teaching or research, perhaps a reformed constitution: they are, in short, manifestos - delivered, of course, modestly, even diffidently sometimes, and with proper deference to neighbours and previous tenants, but manifestos nevertheless; muted manifestos. The second kind defines itself more precisely. There is hardly a single field of scholarship or science in which the contribution of Scotland, of this university itself, has not been extensive and weighty - even, at times, momentous; very few branches of learning in which it is not possible to point to a noble and inspiring tradition of intellectual endeavour. There is special propriety on the occasion of an inaugural lecture, then, in recalling - invoking - the achievements of predecessors, of the giants on whose shoulders we presume to stand; there is a special propriety in setting oneself the aim, not unduly modest, either, of continuing or reviving the traditions they formed. And for those who invest in this kind of inaugural, there is the very large bonus to collect from the rich deposits of portable quotations which lie embedded in so much of Scottish intellectual history, with its unique and rewarding blend of wit and sententiousness, of high thinking and low living. Inaugurals of this kind are known to the trade as Scotch, or Upper Library, jobs.

Third, and last, is the guided tour through the main thoroughfares of a new and unfamiliar subject. Less striking in its appeal than the first, less elegant in manner than the second, more pedestrian by definition of course than either, the guided tour runs the twin hazards of losing half one's audience by boring them with what is already distressingly familiar stuff, and the other half by hurrying them through the more complicated or remote precincts.

These risks I have to ask you to face with me, however, because this is the form and pattern I want to adopt for this lecture. I do so not because sociology is new or unfamiliar

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<sup>1</sup> Inaugural lecture given to the University of Edinburgh in 1965.

- for me to think so would be presumptuous - but because it has seemed to me a subject more than usually susceptible to misconception and misconstruction.

There is, I shall argue, a special reason for this. All branches of knowledge, scientific and other, are concerned with description as well as with explanation, have their substantive content as well as their methodology, are fact-finding, diagnostic or taxonomic activities as well as theoretical and model-building activities. It is indeed by their descriptive activities, their substantive area of study, that specialist studies are known to the non-specialist public. Sociology is no exception, and it is because of this, I believe, that the misconceptions have arisen. For the misconceptions, such as they are, relate to what is publicly known about the descriptive activities of sociology.

The title I have chosen for this lecture, therefore, while not deliberately misleading, is rather elliptical. I shall have to deal with sociology in its descriptive aspects, and, moreover, to try to show how both as description and as explanation sociology is always a critical activity. In considering sociological explanation, furthermore, I shall not seek either to present you with a review of the methods of research used in sociology or of customary procedures in analysing research data - which would be very tedious and exceedingly inappropriate. I shall also steer very clear of the ground which has been ploughed so heavily in recent years by British and American philosophers. My references will be to empirical sociology rather than to what is commonly designated by sociologists as social theory. My object is to try to point out by example and to explain as best I can what is distinctive about sociology in its approach to its subject matter. And I shall do this cumulatively, adding items to the account as we go.

Let me begin by taking it as common and undisputed ground that we tend to live more and more in a world of organised, departmentalised, bodies of knowledge; and that this is not a matter merely of the exigencies of university curricula, or of the shortness of life and the accumulation of knowledge which forces increasing specialisation on us. Intellectual life, scholarship and science are subject increasingly to the principles which govern the division of labour in the rest of civilised existence. We have become acutely aware of the cultural divisions which can grow up as consequence, and, in time, as reinforcement, of specialisation, and there is an increasing number of enthusiastic or conscience-stricken attempts to bridge the gaps. But there are other consequences which we are perhaps less conscious of. Among them is the odd tendency for the world in which we live, the environment of physical matter, of natural circumstance, and of events, to shape itself and to become organised after the same pattern of specialisms, and in their terms. History is, of course, both the past and the study of the past - of course; more particularly it is the body of recorded and ascertainable facts about the past which is regarded by historians as relevant to historical studies. Law has the same familiar and entirely unambiguous ambiguity in common usage; it is both the body of law and the study of law. And it is difficult to think of a time or a possible circumstance in which it might have made sense in either case to regard the subject-matter in any different way from the study of it. But it also makes equal sense to talk of chemistry and physics in the same way; and there was certainly a time when even quite civilised people did not. For us there is a chemical world and a physical world: the chemistry of aircraft engines or

their physics, the chemistry or the physics of the human body, are terms in general currency. More significantly, during the past few generations new disciplines have acted on the world and on circumstances in the same fashion. Instead of enumerating all the particulars of forms of livelihood, standard of living, division of labour, system of exchange, modes and rates of capital formation, range of products, and so on, it is meaningful, acceptable and common usage to speak of 'the economy'.

There is a specific reference here to those actions, events and objects which are the relevant objects of study to economists. And the reference is really quite specific. It is not uncommon, for instance, to find in accounts and explanations of movements in prices, or of fluctuations in consumption, allusions to 'non-economic' variables, so-called, which nevertheless do effect changes in 'the economy'. Psychology has acted as an organising principle in a similar fashion, so that the special attributes of individual attainment, emotional response, mental experience and development which have become appropriate for psychological study now make up a recognised and recognisable sector of the world as we experience it. We can speak meaningfully of the psychology of a person and mean something different from what we mean when we speak of 'a person'.

In all these instances, a science or a discipline has come to achieve so established a recognition as a map of a segment or a set of elements in the world of common experience that it serves as a handy way of discriminating the world of common experience itself. It is one of the ways in which the world becomes a manageable place to live in. Most of us, after all, do seem to think most easily of the world itself as a map. But the process by which economics maps into 'the economy' or by which chemistry maps into 'the chemistry' of our bodies tends for the most part to be taken for granted or completely elided.

Organising our experience of the world in this fashion, convenient, customary and unexceptionable as it is for the layman, is often unwelcome and embarrassing to the specialist himself. Every decade produces its fresh crop of new specialisms which transcend the boundaries of disciplines almost as soon as they are firmly established in the public mind. But the point of this excursion into the higher generalities is to underline what I am sure you have run ahead of me to perceive, namely, that there is no segment or set of elements in the world of common experience which is organised in this way by sociology. We cannot speak of the sociology of Scotland as one can of the Scottish economy, nor of the sociology of children as one can of child psychology. Interestingly enough, substantive fields of sociology, many of them at least, go by titles like the sociology of education, the sociology of law, the sociology of politics, the sociology of medicine. In all these cases, the substantive area of study is defined by another discipline. The mapping has been done by it, not by sociology.

It is for this reason that this guided tour is taking place under the advertised announcement of 'sociological explanation'. For the substantive areas of sociological studies are composed out of the way in which sociology operates upon previously organised bodies of knowledge, not, let me hasten to add, only and merely upon bodies of scientific and academic knowledge but also upon systems of belief, and codes of accepted

practice. Sociology operates in and upon these fields in quite specific directions and in quite specific ways. It does so by questioning assumptions which seem to be made by people, and especially by people in authority in education, law, politics and so forth, about the behaviour of people. These assumptions are sometimes explicit in the form of expressed statements, more often implicit in the form of preference orderings or concealed value-judgements, but they are all formulated within what I can best call the territorial boundaries of each system of organised knowledge and practice; they are assumptions to the effect that the human behaviour visible to the educationist, the lawyer, the politician and so on, is ordered sufficiently for their purposes according to the principles and the vocabulary of ideas developed within the educational system, the law, political science, and so on. Let me try to make this rather opaque pronouncement clearer by instances of what I mean.

I can begin by what will be for many of you very familiar ground. The 1944 Education Act for England and Wales, like the later Scottish Act, was designed to provide for more education at the secondary level, for different kinds of secondary education, and altogether to ensure that opportunities for educational and thus occupational and social advancement would be equally accessible to all children. There is no reason at all to question the sincerity of those who framed and later administered the Act. Indeed, the strength of the point I wish to make lies in the very genuineness of the attempt to reduce to vanishing point the inequities which had been built into the educational system maintained by the state. Within the perfectly valid frame of reference adopted by legislators, administrators and their advisers, the system of selection for secondary education was as psychologically sophisticated and as fair as one could possibly expect. Certainly, so far as I know, no fairer system of selection has since been devised. Yet a series of studies carried out by Professor Himmelweit and her colleagues during the 1950s demonstrated conclusively that equality of opportunity had certainly not been achieved. These studies were not, of course, concerned in the least with the techniques of selection themselves, the apparatus of tests, their administration, the impartiality of teachers and educationists - anything but. The inquiries were directed towards bringing to light considerations and factors affecting educational performance which, familiar as they are to all of us now, had simply not been taken into account in the design of the new educational system; it is not that the structure of families and their material and social circumstances were thought of as not affecting the school life and career of the child - of course they were. But those factors had not been treated as affecting attainment in the ways and to the extent they were now shown to do.

During the 1960s the considerations or assumptions treated as external to the frame of reference of education, or disregarded entirely, have been added to by educational sociologists. Educational performance is now being related to organisational features of the school system, to the institutional character of the class-room situation, to the particular difficulties and anomalies of the teacher's role, and the structure of the teaching profession. Those later researches, like the earlier, are directed towards eliciting considerations and determining factors which, previously lying outside the technical scope of the educational system, are nevertheless relevant to the educational process and should henceforward be taken into account.

There is an important sense, therefore, in which educational sociology is tributary to the theory and practice of education.

I suggested that the clear definition of the boundary of a field of scholarship or science and the coherence and homogeneity of the kind of facts regarded as lying within it - the fact that we can talk about education as a body of knowledge, and as an administrative system, and as a developmental process of a special kind - that all this comes from the existence of a publicly accepted frame of reference and a particular coinage of ideas and beliefs which is in good currency. The frame of reference changes, of course, and so does the body of ideas, aspirations and values accepted as good currency. The main tradition of sociological writing in the field of education - a tradition which stretches through the work of Durkheim (himself, incidentally, a Professor of Education), Weber, and Mannheim (who also, when he came to this country occupied a chair of Education) - this main tradition bears on the way in which ideas and beliefs about the purpose, the appropriate administration and the nature of education have changed in response to changing and emerging needs in society. The actual causes of change, as Mannheim said, are motivated acts, but the motives themselves are shaped by changes in social conditions. And these changes occur at an accelerating pace under industrialism, which throws new burdens on educational institutions - the progressive burdens of mass instruction, promotion of scientific and technological progress, occupational recruitment and now, it seems, social selection - for, in the case of the great majority of people in this country, the place they are going to occupy in the social system and the class structure is settled before they are twenty years old.

'Under conditions of advanced industrialism', as Mrs Floud and Dr Halsey have said, 'the economy becomes increasingly dominated by the institutions of research and technological innovation ... So that the educational system comes to occupy a strategic place as a central determinant of the economic, political, social and cultural character of society'.<sup>i</sup> On this larger scale, as well as in the study of educational opportunity and educability, the role of educational sociology is to examine, to question, to raise doubts about, to criticise the assumptions on which current policy, current theory and current practice are based.

The essentially critical function of sociology at this level is just as clearly present in political sociology. I have to insist on this critical function in the case of this field of studies, because the rendering of the purpose of sociological explanation in this context that I want to put forward is not widely current. In particular, it is very different from that advanced by Martin Lipset and Reinhard Bendix, who are individually two of the most distinguished contributors to this field of sociology, and who, in combination, carry a very formidable - a papal- weight of authority. They write: 'Like political science, political sociology is concerned with the distribution and exercise of power in society. Unlike political science, it is not concerned with the institutional provisions for that distribution and exercise, but takes these as given. Thus political science starts with the state and examines how it affects society, while political sociology starts with society and examines how it affects the state: i.e., the formal institutions for the distribution and exercise of power.'<sup>ii</sup>

We all, as academic teachers and students, deal in over-simplifications and learn to live with those of other people. But this attempt to dichotomise the study of political science and political sociology by polarising them, so to speak, on different points of origin is more than a pardonable oversimplification. It seems to me false as to the facts, possibly with regard to political science, certainly with regard to political sociology, which began with the attempt to measure the extent to which political institutions of a particular kind - namely, political party machines - can and do influence the behaviour of people in society. It is categorically false; the two kinds of study do not occupy two halves of the same football pitch or defend two goals, one labelled 'state' and the other labelled 'society', and advance towards the other; they are different kinds of game, played on different pitches. And the statement is, I believe, false as to the relationship between the two studies. Political sociology is not just the study of the same substantive field as political science but from a different angle of approach. It is tributary to - or, if you like, parasitic upon - political science, in the same way as educational sociology is upon education - parasitic, in the sense in which criticism is parasitic.

There are several fairly distinct divisions of activity in political sociology. The best known is the study of voting behaviour which effectively begins, despite Andre Siegfried's notable work completed before the First World War, with Lazarsfeld, Berelson and Gaudet's panel study of the American presidential election of 1940.<sup>iii</sup> What Lazarsfeld and his research team did was to interview a sample of 3000 electors in a part of Ohio at the beginning of the election campaign, and to interview sections of the main sample at regular intervals up to the presidential election in November. The research design was concerned specifically with estimating the actual influence exerted on voting by the campaigns of the two parties throughout the whole six months preceding the election. From this, and subsequent studies in America, Britain and elsewhere, we have gained an increasingly vivid and detailed picture of how much voting is a matter of habit, how little rational choice seems to enter in, how far political allegiances are formed virtually in childhood, how few voters change that allegiance in normal election circumstances. We are getting to know more about the influence of demographic factors and about the curiously overlooked part played by religious affiliation in certain countries. None of this work, or of other work on party organisation or pressure groups, on the nature and social function of ideology contributes anything to the solution of the major issues of political principle or of political organisation. It is not an approach to the field of study of political science from another point of departure. But it does affect very much the terms in which these issues are to be debated, and the limits of the considerations which must henceforward be regarded as pertinent to political studies. W. G. Runciman has remarked that 'Lazarsfeld's work has placed an important limit on the scope of *a priori* theorising about democracy; and it has done so by producing sociological evidence directly relevant to the tenets of political theory. It is not evidence which necessarily supports a left-wing or a right-wing view; but it is important precisely because any theory of democracy, whether left or right, must take account of it.'<sup>iv</sup> I would, while supporting this, also say that the importance of the sociological work in this field lies not in its limiting the scope of *a priori* theorising but in extending it - of pointing to considerations which political studies must take into account beyond those which were previously seen as 'politically relevant'.

There are two corollaries to which I think I can now point as proceeding from what I have said so far. The first I have already suggested - which is that the relationship of sociology to these and other fields of substantive study is tributary. Sociologists, more than most scientists perhaps, admit the force of the injunction to forget their past, since much of what of value there is in it has been incorporated and has taken root in other disciplines. Secondly, while the direction and purpose of sociological kinds of explanation has been to amend and supplement the kinds of evidence and consideration lying within substantive areas of organised knowledge mapped by other established disciplines, the outlines of what might turn out to be a substantive area peculiar to sociology are perhaps becoming perceptible. In the cases I have mentioned, sociology has not only pointed to uniformities and variations in performance and in choice which are inexplicable in terms of the existing rationale of education or of politics, but has identified the external factors in terms of unwitting regularities among groups and categories of individuals, of latent controls and limitations of action, of conventions and observances which hardly can be said to rise to the surface of articulate expression. We are, in fact, dealing with the institutional framework of social behaviour, the implicit, unthinking and unarticulated code of norms which govern or influence individual conduct. Vilhelm Aubert's study of the judiciary in Norway,<sup>v</sup> when it was first published, evoked violent reactions among the legal profession precisely because it pointed to the fact that, in giving sentence, judges appeared to be following a tacit code which contravened the explicit code of equality before the law. We are, in this country, aware of the embarrassing variations in the practices followed in different magistrates' courts in giving sentence - the large discrepancies in the penalties exacted for identical infringements of the law in apparently very similar circumstances. These variations have been the subject of a good deal of discussion and criticism in recent years, and, indeed, investigations have been undertaken to establish just how far the inconsistencies range. But the presumption in this connection, so far as this country is concerned is, I believe, that the inconsistencies are just that - that the natural range of variation which must occur because of differences in temperament, idiosyncratic interpretation of the law, uncontrollable prejudice against persons, and so on, is perhaps wider than it should be. What Aubert did was to scrutinise and compare the sentences and the utterances of judges (senior as well as junior) in giving sentence, relate those to the recorded circumstances of the cases on which sentence was pronounced, and demonstrate that the variation in sentencing behaviour correlated extremely closely with the social class of the accused person. Not an astonishing conclusion, perhaps, but interesting. Interesting, because the correlation bespeaks a rule, a normative principle influencing the sentences given, which is certainly external to the principles which overtly apply to the behaviour of judges, and even contravenes those principles. Other studies, notably the Chicago studies of the conduct of arbitration cases by lawyers, point to the existence of rather more complicated normative principles which seem, in the same latent, unwitting fashion, to distort or contravene the principles of action which prevail, and which are - I must emphasise - honestly maintained, within the system of law itself.

I have, so far, kept to what I have thought might be more familiar ground for this explanation of sociological explanation, largely because I hoped in doing so to make clear the way in which sociological explanation is shaped by its special purposes. I

want now to discuss rather more closely the essentially critical, assumption-testing nature of sociological investigation. I can, I think, bring most light to bear by recounting some research experience of my own in industrial organisations.

Most empirical studies of organisations depend a good deal on interviews with managers. One begins these interviews conventionally with questions about the particular job one's respondent does, and how it fits in with other people's and with other departments. The next step is to examine the discrepancies between the picture one gets from different respondents of the organisation in which they all work. There always are discrepancies, of course. But the question presented by these inconsistencies is not 'Which version is right?' but 'How do these differences arise? How is it that these different versions of the same set of circumstances and actions have arisen in the minds of people who have to co-operate with each other in the very circumstances they view so differently?' The need to account for these differences marks the first stage beyond narrative description.

Some years ago, at the outset of one such inquiry, I encountered a major difficulty even before reaching this first stage when comparison becomes feasible. The firm was in a very rapidly expanding and technologically advanced industry. A whole series of interviews with managers followed a rather disconcerting pattern. After listening to my account of myself and of what I was interested in finding out, they would say, in answer to my first question, 'Well, to make all this clear, I'd better start from the beginning', and then proceed to give me an account of their careers in the firm. This account would be lucid, well-organised, and informative, but would stop short at some time beforehand - when, in fact, they had arrived at their present position. I would then ask again what they were in fact doing now, what the different functions were that they carried out, whom they saw in connection with them, and so on. After a pause, they would then go on to explain, equally lucidly, how they and their department would operate when the present crisis was past, or the very big job they were rushing through was completed or when the reorganisation I had doubtless heard about had been carried through, and they could all settle down to work to a plan. After a succession of such interviews I was fairly certain that I had encountered the sociologist's poor substitute for the natural scientist's 'discovery' - the feeling that what had looked like good common sense ground (and what could be more common sense than that managers know what jobs they are supposed to be doing?) was turning into rather liquid assumptions.

Luckily, the managers who had provided me with this experience found my reaction, when I was sure enough of myself to tell them, as interesting as I did, and agreed, four of them in one department, to carry out an experiment. This consisted merely in each keeping a detailed record over a period of five weeks of how he spent his working time, whom he met, what problems he was concerned with, whether he issued instructions, whether he gave, exchanged, or received information, and so on.

I should like to dwell on this account of the genesis of a particular piece of research a little. Like any other kind of inquiry which has a history and an establishment, sociology seems at anyone time to be pursuing not so much the right kind of knowledge

as the right kind of questions, not definitive information but fresh hypotheses. Anyone who has done research in any field will testify to the truth of Agnes Arber's remark that the difficulty in most scientific work lies in framing the questions rather than in finding the answers. What is not so often insisted upon is that questions do not suggest themselves or rise at the bidding of the specialist student with a little time on his hands. They arise from doubt. Doubt, in turn, arises from the existence of an alternative where none was previously suggested; it arises from a discrepancy between facts, or between accepted interpretations, or between intended and achieved results. In this particular case, it arose from doubt as to whether what everybody regarded as an abnormal departure from the pattern of activities as they should be was not in fact the normal condition of things.

Let me go on to say a little more about the research project which followed. The four people who carried this through did so in quite exemplary manner, swamped me with thousands of record forms and launched a research project which kept me, and a hundred other managers in a number of different industrial concerns, fully occupied at intervals over the next two years. There is one aspect of the results of this first, pilot, study which I want to mention here. I extracted all the record forms on which the departmental manager and one or other of his subordinates had recorded meeting each other. There were 240 of these. In 165 of them, the departmental manager had noted that he was giving a subordinate instructions or decisions; when one turned to the records made by the subordinates of the same episodes, only 84 of them indicated that they were receiving instructions or decisions. In fact, then, half the time, what the manager thought he was giving as instructions or decisions was being treated as advice or simply information.

This result, which I talked over at some length with the people who had done the recording, is open to a number of interpretations, all of them throwing some light, I think, not only on what we may call the pathology of the systems of world in which equality is their prescriptive right as citizens yet accommodate themselves to the working necessities of subordination and inferior status. But for my present purpose, what I want to underline is the way in which the rules of the game which was actually being played between these four people - all of them young, intelligent, hard-working, ambitious - were in fact unrecognised by them. There were many other ways in which the same suggestion made itself felt - that organisations are made to work very often by the unwitting observance by their members of rules of the game which are not only different from the formal articulated body of rules but are not realised in anything like explicit form by the players themselves. The management of this department, for example, when they were asked at the end of this five-week period - when they had been composing almost minute-by-minute records of their activities - how much of their time was spent on all matters directly related to production, gave roughly well over half of their combined time as the answer. And in this they were, they thought, being conservative; after all, they *were* running a production department. In fact, they spent less than a third. In other companies, estimates of how the whole management group's time was spent - given after each individual member had spent several weeks in unusually close attention to just this - were even more wildly out. These results, incidentally, have inclined me to attach rather

less than full objective validity to the figures published in one of the appendices to the Robbins Report (which are based on an inquiry conducted by postal questionnaire) into the way in which university staff distribute their time among their various activities.

I have used a miniature, perhaps trivial, illustration to demonstrate the widespread and pervasive tendency for human action to proceed in a *context* of thought and belief and intention very largely at variance with the manifest import of the actions themselves. In his 1961 Trevelyan lectures, E. H. Carr argues that what the historian is called upon to do is to investigate what lies behind the act. It was, he went on to suggest, a serious error to assume, as Collingwood had, that this meant the investigation of the thought and purposes of the individual actor. These may, said Carr, be quite irrelevant. 'The relations of individuals to one another in society and the social forces which act through them produce from the actions of individuals results often at variance with, and sometimes opposite to, the results they themselves intended.'<sup>vi</sup>

Sociology also has been described - by Karl Popper among others - as concerned, in the way E. H. Carr suggests, with the unanticipated consequences of human action. There are innumerable examples of this in the field of administrative action and planning. I can take one from near home. In 1954, a group of professional people working in Pilton, a large Edinburgh ward which is almost wholly made up of municipal housing estates, asked the Department of Social Study to carry out a survey which would help clarify some of what they saw as the social problems of the area. Most of these, at the time, had to do with juvenile delinquency and with a whole series of related difficulties to do with the unruliness of children and adolescents and their hostility to ordinary controls. As part of the preliminaries to the survey, which was carried out by graduate students in the department, I looked at the make-up of the population of the ward - which, even at that time, numbered some 28,000 people - about the same size population as Stirling. There were three noteworthy features. First, there was a marked preponderance of young people. Virtually one in four of the population was between ten and twenty years old; this compared with one in eight for Edinburgh as a whole, and one in eleven for the Central wards. In some parts of Pilton, this disproportionate number of children was even higher - in one section, over half the population was composed of school children and older teenagers. There was also a corresponding numerical deficiency of people between twenty-five and thirty-five years old - the most active section of the mature adult population, and there were very few old people.

Now it seemed to me then, and I still believe, that the implications of this state of affairs are quite obvious. The social control and social education of children is immeasurably more difficult in a population with mature adults so heavily outnumbered. The mere thickness on the ground of young children and adolescents will tend to make them a much more powerful force in any community than normally, will reinforce any resistance to adult control from inside or outside a community, and will tend to make adults look for their own entertainment and recreation away from the area. The incidence of unacceptable forms of individual and group activity among children and adolescents will appear to be much higher than in other districts of the City. The forms of activity at any given time, and the choice of companionships open to the individual child will be

much more diverse than usual. Child and adolescent society will tend, therefore, to be more self-sufficient.

I think it is reasonable to conclude that the 'youth problem' of Pilton at the same time was largely demographic in character. And the population structure which produced a kind of dislocation in the normal system of relationships between adult and children, and in the behaviour of children, was the direct consequence of a housing policy which, in Edinburgh as everywhere else, filled large housing estates built in the 1930s with young families. From ten to fifteen years later, the population of course consisted largely of the middle-aged and the adolescent, and there appeared the sudden growth of delinquency rates in suburban areas which was a notable feature of so many English and Scottish cities. It is as though society played confidence tricks on itself.

On a larger scale, society seems to play not confidence tricks so much as self-confidence tricks on itself. These are a familiar element in social history. It took an immense amount of painstaking effort over many years to prove that a third of the working-class population of London was living in poverty; more years of work still, by Rowntree, to prove that the vast majority of families able to afford less food, clothing and warmth than on the most spartan of reckonings constituted bare subsistence level, had not been plunged into distress through some moral obliquity or defect of character, but through pressure of circumstances which they had no possible means of controlling. The astonishing feature of the *Our Towns* report on the condition of children evacuated from city slums in 1940 was not the squalor and unseemliness of the children but the blank ignorance of all other sections of society about them and the circumstances of urban life which had produced them. Within the last few weeks, Professor Townsend's survey of the millions of families in Britain living at or below the subsistence level represented by national assistance has come, again as a shock. The results of Harrington's survey of the incidence of poverty in the United States three years ago came as a shock. Now, they are the stock-in-trade of the weekend political speaker.

The traditional role of descriptive sociology, in this country and elsewhere, has largely been to point out what is immediately obvious to everybody as soon as the task of collecting and presenting the facts has been done. In this sociology performs its familiar tributary function, this time in the formation and development of public opinion and common knowledge. In its other, more specialised, task of searching for explanations of behaviour, sociology often seems even more directly concerned with the obvious. A little while ago, I said that if one could point to a substantive area which constituted the field of study for sociology, it would be the institutional norms which seem to govern action in the sense of providing navigational rules for decision and action, or limits and constants which the behaviour of people seems to observe. But there exists already an enormous fund of knowledge - common knowledge based on common experience and commonsense - about the characteristic patterns of behaviour which can be observed among different groups of people and in different kinds of situation. Many years ago, Paul Lazarsfeld wrote a lengthy review of the first two volumes to be published on the studies conducted during the Second World War into the morale of American troops and the reactions of conscripted men to army life. He lists a number of conclusions, and

suggests that most people would dismiss them as familiar, or as so obvious that there was no point at all in examining them. For example: better-educated men show more psycho-neurotic symptoms during training than those with less education - (the mental stability of the intellectual compared with the psychological resilience or impassivity of the ordinary man has often been commented upon). Second, men from rural backgrounds were usually in better spirits during their army life than men brought up in the city. Third, troops from the Southern states were better able to stand up to the climate in the hot Pacific Islands than Northerners. Fourth, white privates were more eager for promotion than Negroes. One can add a fifth, equally obvious: officers and men in units where promotion was most frequent and rapid were more satisfied with their present positions and prospects than were people in units where there were least chances of promotion.

Lazarsfeld remarks: We have in these examples a sample list of the simplest kind of interrelationships which provide the bricks from which an empirical social science can be built. But why, since they are so obvious, is so much money and energy given to establish such findings? Would it not be wiser to take them for granted and proceed directly to a more sophisticated type of analysis? This might be so except for one interesting point about the list. Every one of these statements is the direct opposite of what was actually found. Poorly educated soldiers were more neurotic than those with higher education; southerners showed no greater ability than northerners to adjust to a tropical climate; Negroes were more eager for promotion than whites; and so on.<sup>vii</sup>

In this last instance, as in all the others, sociology defines itself as a critical activity. The purpose of sociology is to achieve an understanding of social behaviour and social institutions which is different from that current among the people through whose conduct the institutions exist; an understanding which is not merely different but new and better. The practice of sociology is criticism. It exists to criticise claims about the value of achievement and to question assumptions about the meaning of conduct. It is the business of sociologists to conduct a critical debate with the public about its equipment of social institutions.

This purpose of critical understanding is more important now than it has ever been. Sociology, like other social sciences, is the creature of the new human situation which industrialism has brought about. It emerged, tentatively at first, as the need grew to understand, mitigate and possibly even control the transformations which individual lives and the social order continually undergo. As it has developed, it has become clothed with more and more of the objectivity and methodology of the natural sciences, and has become infused with more of their spirit of inquiry and discovery as ends in themselves; but like other social sciences, its character has nevertheless remained basically ideographic. All the social sciences are, I believe, governed by the need to understand and to represent in adequate terms the nature of individual personality and mental experience, or the relationship of individuals to each other, or the varieties of economic and political institutions and relationships, or the social order itself.

The new impetus which has been given in our generation to the pace of scientific and technological development and to industrial and economic change all over the world gives a new urgency to these studies.

In many ways, the pressing need to know more about human behaviour in all its context – a need which has found increasingly popular expression during this century – is a manifestation of the disparity between man's understanding and control of nature and his insight into and command over his own conduct and his own affairs. Traditional wisdom, the oversight of the 'intelligent amateur', and the accumulation of experience over a lifetime, which served earlier generations are now insufficient when we are so promptly confronted with the direct and the indirect, the projected and the unanticipated, consequences of discoveries and decisions. Earlier generations, however fast they saw their world changing, were at least persuaded that certain traditional institutions and values were immutable, and even that the passage of time alone might solve major difficulties and problems.

Time, indeed, was seen in the nineteenth century as on the side of man. Now, it seems, time is against us. More accurately, perhaps, if more prosaically, the difference lies in the sheer multiplicity and technical difficulty of the factors entering into the decision-making process. The point here is that we are in a fundamentally different situation from that obtaining when piecemeal changes could be made in social, economic or political systems as and when it seemed best, and when institutions could be discarded or replaced without much regard being paid to the social fabric of which they formed part. Decisions, planning and action in scientific, educational, economic and social affairs must now take cognisance of an ever-increasing span of considerations if they are both to be effective and not do more harm than good. Similar circumstances obtain for public and private corporations; and the concurrent growth of studies of decision-making in economics, sociology and psychology is again a manifestation of the way in which development in the social sciences reflects the emergent needs of society.

It is not fortuitous that all societies, whatever their political character or stage of economic development, have realised the need for some form of planning. 'Planning' in fact, is a word of dubious relevance to what is happening, if it is read in its traditional sense of producing a design which future actions, at set times, will convert into a finished construction in complete accordance with it. It is much more a matter of deciding the direction and goals of activity, or setting the upper and lower constraints to the amounts and to the kinds of activity which are pertinent to the achievement of the goals. This new connotation places much more emphasis on selecting the sets of relevant variables and on understanding and controlling them and the factors which affect them. Planning, in short, has become a complicated process of social cybernetics, into which psychological, social, geographic, economic and educational factors enter, and a process which has to be implemented in terms of organisational and administrative expertise compared with which our existing procedures are but primitive craft skills.

The demands which present social needs are putting on the social sciences are already enormous. I am convinced that a far greater volume of demands and needs are present in

latent form, or are building up. They are being expressed in a bewildering variety of forms. These demands are altogether out of proportion to the present capabilities and resources of the social sciences. If they are to come within measurable distance of an adequate response to the need which society has to them, positive and substantial efforts must be made to foster their development. These efforts are now, I believe, visible in a number of countries in Europe. They appear to be imminent in Britain.

I began this lecture by observing that sociology was not a new discipline. This is true, but it is, in one sense at least, new to this University. It has been born at a time when the demands on it, as on other social sciences, are growing, and at a time also when the character of the discipline itself is changing out of recognition. Sociology in Edinburgh looks forward to a strenuous but, I hope, an adventurous and lusty infancy.

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- <sup>i</sup> Jean Floud and A. Halsey, 'the Sociology of Education', *Current Sociology*, vol. VII, no. 3, 1958, p. 169.
  - <sup>ii</sup> Reinahard Bendix and Seymore M. Lipset, 'Political Socioiology', *Current Sociology*, vol. VI, no. 2, 1957, p. 87.
  - <sup>iii</sup> Paul F. Lazarsfeld, B. Berelson and H. Gaudet, *The People's Choice*, Columbia University Press, 2<sup>nd</sup> ed. 1948.
  - <sup>iv</sup> W.G. Runciman, 'Sociological Evidence and Political Theory', in Peter Laslett and W.G. Runciman (eds), *Philosophy, Politics and Society*, Basil Blackwell, 1962, pp. 42-3.
  - <sup>v</sup> Vilhelm Aubert, *Sociology of Law* (Chapter 6, 'Law Courts and the Class Structure'), Institute for Social Research, Oslo, 1964 (mimeographic).
  - <sup>vi</sup> E.H. Carr, *What is History?*, Penguin Books, 1964, p. 52.
  - <sup>vii</sup> Paul F. Lazarsfeld, 'The American Soldier – An Expository Review', *Public Opinion Quarterly*, 1949, p. 380.