Finding, and correcting, my mistakes

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ABSTRACT

Mistakes are inherent in the process of research but can illuminate it. Some of the author's mistakes have been false assumptions shared with others of his generation. His early work lacked a sufficiently sharp focus for him to be able to make any interesting mistakes. In 1967 he claimed that race was used as a role sign when he should have claimed that phenotypical differences were so used. He tended to take race as a synonym for colour, and failed to appreciate that a social construct could not be a basis for a general theory. His subsequent attempts to correct these mistakes are outlined.

KEY WORDS

collective action / methodological individualism / mistakes / Popper / race relations

S ir Karl Popper, one of my teachers, taught that we should learn from our mistakes, indeed 'that *all* our knowledge grows *only* through the correcting of our mistakes' (1969: ix; see also Agassi, 1968). In line with this doctrine, I have looked for the mistakes that I made in the study of ethnic and racial relations, and what I have done to correct them. The exercise has strengthened my belief that Popper's claim was either a pardonable exaggeration or depended on a restrictive conception of what constitutes knowledge. For in social science much can be learned from a data-gathering inquiry, like a social survey or a population census, or from attending a lecture. In the accumulation of new knowledge there are two phases. The first is inductive in character, gathering and sorting observations. The second phase, in which hypotheses about causal relations are put to the test, relies on deductive reasoning. Popper's doctrine applies best in the second phase.

Much research in contemporary social science is entangled in the transition from the first to the second phase. Not all inquiry makes this move. Information

collected for the purposes of public policy, like a population census, may remain descriptive.[1] When, however, the research worker perceives in the information an intellectual problem, something requiring explanation, there is an impulse to deductive reasoning.

Another of my teachers, Sir Raymond Firth, told me that Malinowski, his teacher, used to insist that 'without problems there are no facts'. Only when a scholar has decided on the problem can he or she decide which facts are or could be relevant. 'Science begins with problems, practical problems or theoretical problems', wrote Popper (1994a: 95–101). Yet the perception of a problem is no simple matter. The German expression *Problemstellung* is useful as denoting the recognition that something constitutes an intellectual problem; this recognition should include a formulation of the problem in such a way that it can be addressed, for, as others have said, a problem well stated is a problem half solved. Some of my mistakes have been false assumptions that I have shared with others of my generation, errors that can be identified only in retrospect. In trying to correct them I have learned things that, for me, constituted personal discoveries. Many of these were steps on the way towards the identification of causal relationships and the prospect that, one day, it may be possible to subject them to empirical testing.

Micro and Macro

My research career started in 1950, when I was engaged to undertake a study of 'the colonial stowaway', the young men from West Africa and the West Indies who had hidden in ships and, on arrival in Britain, been sentenced to imprisonment. On their release they had to find jobs and, in a period of acute housing shortage, somewhere to live. I persuaded my supervisor, Kenneth Little, head of the social anthropology department in the University of Edinburgh, to modify this plan so as to include a study of what was then known as the 'coloured quarter' in London's East End (Banton, 1955). This, basically descriptive, research occupied me for two years. Though I struggled to find a sharper focus for my work, I was unable to relate what I had learned from studies of black–white relations in North America to the circumstances that confronted me. I had not got far enough to be able to make any interesting mistakes.

My research in Sierra Leone, in 1952–3, was to be a study of migration from the rural hinterland into the city of Freetown and of the social life of the migrants there. Official record-keeping of migrants registering for work at the labour exchange did not prove a good basis for further work, so I concentrated more upon the institution of tribal administration and the network of voluntary associations. My chosen theoretical framework was that of structural-functionalism, which was founded upon a view of action as using scarce means to attain given ends but had in my, perhaps faulty, perception become a top-down theory of the social system. Accordingly, I wrote on such subjects as 'Adaptation and Integration in the Social System of Temne Immigrants in Freetown'. There

were failings that should be accounted blind spots rather than mistakes. For example, in my fieldwork I was told about the way in which different ethnic groups had competed in the building of mosques, but I did not properly record what I was told about this because I did not see religious ends as fitting within the social system. In truth, as others later demonstrated, mosque building was part of the competition between ethnic groups for social status within the urban system and was important to the growth in ethnic consciousness.

Why was my vision blinkered? As an undergraduate at the London School of Economics (LSE) I had been attracted by the bottom-up approach used in social anthropology and in the field studies of some United States sociologists. Yet most of the teaching in sociology was based on large-scale generalizations about society and relied upon a top-down approach. Students had at times to write about 'the individual and society', as if these were two separate levels of reality. This may explain why I could not take adequate account of bottom-up processes. I had read with some care Hayek's 1943 essay in which he contrasted methodological collectivism and methodological individualism, implying that a sociological methodology had to be either the one or the other, but I could not have digested its significance. For in both Stepney and Freetown I worked on the assumption that a sociologist was concerned with individuals only in so far as they were representative of groups. I wondered about the nature of these groups, but could get little guidance from the social science of the time.

When I returned to Edinburgh in 1954 I encountered others who held similar views of sociology. For example, the professor of psychology, James Drever, said on one occasion that 'when there's one person it's psychology; when there's two, it's sociology'. My dissatisfaction with any such demarcation was growing. This was a period in which I read Goffman's Chicago PhD thesis on inter-personal communication in the Shetland Isles, the pre-publication text of what became his *Presentation of Self in Everyday Life*, and a set of Everett Hughes's essays that Goffman had left in the Edinburgh Social Sciences Research Centre library. They all detected sociological issues in individual behaviour. In these years I followed closely the controversy about methodological individualism in *The British Journal for the Philosophy of Science*.

The study of ethnic and racial relations had started from a theory that held these relations to be determined by underlying biological differences, and in the criticism of that theory. The criticism was well developed by the time I became a student, but there had been no sustained attempt to develop alternative explanations of inter-group relations. None of the staff members at LSE at that time, as far as I know, were carrying out research (either empirical or conceptual) that obliged them to try to resolve what I later heard called the micro-macro problem. Though I did not then know it, Max Gluckman had offered a solution in 1940 in his analysis of a social situation in modern Zululand. He had shown how the macrocosm could be detected in the microcosm, yet his article did not come to my attention until nearly 10 years after I started research.

My book W*hite and Coloured* (1959) sought an explanation for discrimination on the part of white British people in their relations with non-white

people in Britain. As individuals, most white people professed to be favourably disposed towards those who were then called coloured colonials (and ex-colonials), but they believed that other whites were less well disposed than they were. Their dispositions to discriminate or not to discriminate were sometimes delicately balanced, while discrimination occurred more frequently in private than in public relationships. Psychological explanations could not account for such observations, hence the book's distinction between prejudice and discrimination as alternative *explananda*. This distinction was not then generally understood in Britain. To account for discrimination it was necessary, I held, to appreciate the social significance that, in the 1950s, attached to a dark skin colour. Men with darker skin were treated differently because they were deemed to be unfamiliar with the norms by which private relations were governed, and because (as Kenneth Little had earlier argued) a dark skin colour detracted from a man's prestige and that of whites who associated with him. Much of the book discussed inter-personal behaviour, seen in a framework constituted by British class structure and history. I argued that the economist, the psychologist, and the sociologist could answer the questions that an observation posed in the light of their discipline's theories. They dealt only with aspects of behaviour, unlike the historian or the philosopher. In continuing with this line of thought I recalled R.G. Collingwood's advocacy, in his autobiography, of 'the logic of question and answer' (see, additionally, Ritchie, 1948[1943]); it clarified issues of problem formulation and explanation.

White and Coloured cleared up several conceptual confusions, but my reasoning had still not advanced to a point at which any true mistake could be identified. Nor was there any in my descriptive research on police–public relations (1964). Continuing my interest in the micro-macro problem, I took Gluckman's technique as a model for the book *Racial Minorities* (1972a). This elaborated upon the question-and-answer approach in accounting for an historical event, the collapse of a pressure group called the Campaign Against Racial Discrimination (see 1972: 16–19).

Race Relations

There has been a tendency in recent sociology to examine theories in the abstract, without sufficiently considering the adequacy of the explanations of problems that can be derived from the theories. Inquiry must start with a question, even if further work may be needed to define that question clearly. It is in the process of trying to improve explanations that mistakes are made, and it is this process that obliges scientists to improve their conceptual armoury. There is a parallel here with legal reasoning. When a case comes to court, preliminary proceedings have determined the issues to be decided. If it goes to appeal, it may be on a single issue. The courts pronounce on one issue at a time and restrict their reasoning to what is necessary to resolve the issue. This fits with the view that a problem well defined is a problem half solved.

Perhaps the central element in the approach I have favoured is Popper's claim that, by seeking causal explanations, objective knowledge can be attained in the social sciences. The nature of this approach may be the better appreciated if it is contrasted with two others that, by implication, I rejected in *White and Coloured*. One is that of some philosophers, like Peter Winch and those who have taken their lead from Wittgenstein. This approach does not inform the search for new knowledge of a transcultural character. The other is the approach, often of Marxist inspiration, which maintains that the social scientist should seek an historical understanding of events. What is accepted as a satisfactory causal explanation will also rest upon subjective judgements but, because it focuses on the explanation of particular observations, the subjective element is more restricted. Popper (1945, vol. ii: 250–4) wrote of historical explanation as a form of causal explanation that rested upon some trivial universal law. After reading some of the literature on the causes of the US civil war, I concluded that it was better to use Popper's alternative formulation and speak of historical interpretation. The word 'explanation' can then be kept for causal explanation.

The most important characteristic of my book *Race Relations* (1967) was its assumption that there was, or could be, a global field of study, identifying features of social relations common to the USA, South Africa, Britain, and other countries in which social relations were at times distinguished by the use of race as a role sign. I presented the study of race relations as an applied social science, resembling criminology in being multi-disciplinary and in having a practical object. Criminology was concerned with the prevention of crime, race relations with the prevention of discrimination.

The book was well received because it was in tune with its times and with the expectations of my readers. Those expectations were about to change. The year 1960 had been called 'Africa Year' at the UN, and I had thought it was 'a turning point in the study of race relations'. Yet the US Civil Rights movement and the student disturbances of 1968 meant that for the study of race relations the later year became even more of a turning point. 'Race relations' had been regarded as a general name comprehending prejudice (the dimension of attitude), discrimination (the dimension of behaviour) and racism (the dimension of ideology). Since 1965 it had been the title for British anti-discrimination laws. Influenced by the Civil Rights movement many sociologists came to see this as an unsuitable name, distorting perception of the reality. They began to use 'racism' as the general name (the changes of vocabulary are reviewed in Banton, unpublished). Much sociology in the USA had, for a long time, concentrated on US society, taking the national values expressed in the constitution as a framework. In this respect there were parallels with the study of social policy in British universities. The effect of the Civil Rights movement was to strengthen this value orientation, so that many sociologists insisted that the study of ethnic and racial relations must be designed to contribute to an anti-racist politics. As they prioritized the study of more political problems, the intellectual current started to flow

along a new channel, while, more importantly for me, my own thinking also took a new course.

By 1967 I had reached a stage in which I could make identifiable mistakes. I had started from the premise (which I had learned at LSE, but which was accepted by the educated public, see Banton, 1983: 71–2) that 'race relations' was an appropriate name for a field of study. I accepted the formulation of the leading academic authority, Chicago's Robert E. Park. This, I now believe, was a mistake I shared with most of my generation. In 1939 Park had declared that race relations were not so much the relations that existed between individuals of different races as those between individuals conscious of these differences. This offered a justification for the belief that racial relations were a distinctive kind of relations. It legitimated the assumption widespread in the USA that blacks and whites were distinct races, and the 'one drop' rule that any ascertainable African ancestry assigned an individual to the black race.

Although I adopted the premise that racial relations were distinctive, I felt uneasy about it. None of the alternative expressions then available seemed any better. Reference to differences of colour had gone out of fashion. Trying to specify more closely what it was that individuals might be conscious of, I wrote in *Race Relations* (1967: 57) that:

> Differences of sex, age, race, and relations of consanguinity are used in social organizations as ways of dividing people up and allocating them positions in the division of labour. It would seem that societies can, in certain respects, be more effectively organized when people are conditioned to believe themselves more different from one another than biological and psychological tests would indicate … When racial differences are used as ways of dividing up a population … these outward differences serve as signs telling others the sorts of privileges and facilities to which the person in question is conventionally entitled.[2]

When the publisher invited me to prepare a second edition of *Race Relations*, I declined because I was dissatisfied with its eclecticism. It employed different theoretical approaches for the discussion of different regions, reflecting whatever approaches were current in sociological publications about them. Believing that I ought to seek a synoptic framework, I concluded that I had to go back in time and re-examine my assumptions before I would be able to move forward.

I began by reading further in the history of racial thought. This taught me that the idiom of race in both the English and French languages could be dated from the politics of the 17th century, and that the 'racializing of the West' had preceded the 'racializing of the world'. Racial ideologies were not a product of the colonial encounter, as many colleagues assumed. The theories propounded in the years 1848 to 1863 that were regarded as the source of 'scientific racism' merited particular attention. It came as a discovery to find that the key concept in these doctrines was not race but permanence of type, so that racial typology was a more accurate name for the body of doctrine.

In its time, racial typology offered a highly controversial explanation of the unequal development of societies and populations. Though the theory was

severely criticized, it exercised popular influence because scholars were unable to formulate equally simple and more persuasive explanations of the observations addressed. To do so was no easy task. Unsurprisingly, contemporary sociologists looked to other fields in the search for analogies that would give them a new perspective on the processes they wished to study. Herbert Spencer famously drew an analogy between the work of a panel beater and a social reformer before he turned to biology and wrote of society as a social organism. Such a vision, like the theories that were called Social Darwinist, tended to strengthen the supposition of in-built differences between racial groups. Park found better analogies in the study of ecology. In *The Idea of Race* (1977), I reviewed the history of some relevant concepts and distinguished between an idea of race and a concept of race. This was a distinction between what I now believe are better distinguished as *emic* and *etic* concepts (Lett, 1996). I wrote of racism as a political idea rather than a concept and revived a 19th-century concept of ethnogenesis. The discussion of the changing meanings of the word 'race' led on to my critique of presentism, and to the differentiation in *Racial Theories* (1987) of race as lineage, as type, as subspecies, as status, and as class. In 19th-century writing, race became a two-dimensional concept: race as type exemplified the horizontal dimension of classification; race as lineage and as subspecies exemplified the vertical dimension of inheritance.

Partly because 'race' had so many meanings, the word racism was used in different senses and for different purposes. For me, it simplified matters to rename the theories of 1848–63 as racial typology. This helped separate the history of racial theory from the possibly causal effects of one kind of theory, which was important because of the tendency in some quarters to blame writers like Gobineau for the political exploitation of racial ideas in the 20th century. Separation meant that I could more easily concentrate on the causes of discrimination.

Any assumption that racial relations are distinctive must rest on a conception of race. Pitt-Rivers (1970) observed 'One cannot avoid the suspicion that Banton's concept of race is simply the popular British concept shorn of its innuendos of prejudice and raised from the daily life of the British Isles to the status of a universal scientific category.' The criticism was justified: in much (though not all) of *Race Relations* I had indeed treated race as a synonym for colour, as Rex (1973: 185) and other critics also objected.[3] So 12 years later, I acknowledged that people perceive not racial differences, but differences of colour, hair form, underlying bone structure and so on. Phenotypical difference is a first order abstraction, race is a second order abstraction. It is phenotypical differences which are used as role signs (Banton, 1979: 130).[4] This was my first main mistake; the second lay in my attempt to devise a global classification of the relations in various parts of the world that were to be the objects of study. I recognized that in English-speaking countries the word race was used as what in the 1990s came to be called a social construct. Yet I failed to appreciate that such a construct, because it was time and culture bound, could never be a basis for a general theory. The exposition in my book was built round what I called

'six orders of race relations'. In each order the social system generated stimuli to social alignment on the basis of assignment to racial categories, but the differences between the kinds of category were minimized when they were grouped as 'racial'. The orders were given names familiar to my readers (institutionalized contact, acculturation, domination, paternalism, integration, and pluralism). The use of familiar names made the scheme more acceptable, but the attempt to characterize whole societies was misconceived. Societies are constantly changing and include many contradictory elements. If I was to devise a systematic classification, it would have to be based on an explicit theory; this would yield a matrix that might well incorporate some empty cells.

A Synoptic Theory

A clearer application of methodological individualism started with my essay on the migrant's choices. It sent me back to the reference to exchange theory in *Race Relations* (1967: 67), and led me, in 1976, to formulate what I called a rational choice theory of ethnic and racial relations. There were several steps en route. The folk concept of race had been the basis for what I had called racialization. My essay on folk and analytical concepts developed my argument that the language of social science has to be separated from the language of everyday life. At one point (1979: 135–6), I was emboldened to write that I did not 'think it irrelevant to remark that medical science would not have made the progress it has, had doctors accepted the patient's conception of his complaint as a definition of the disorder from which he was suffering'. Popular usage of words like 'race' and 'racism' corresponded to the patient's description of pain. The analyst needed a battery of analytical concepts to identify the causes of pain. By folk and analytical concepts I meant much the same as American anthropologists meant by the distinction between *emic* and *etic* constructs, though, because this distinction was not always drawn consistently, I did not use these expressions for another 24 years. I described 'race relations' and 'ethnicity' as folk concepts, necessary for some purposes (as in legislating against discrimination) but needing refinement for sociological purposes. My stress on a problem orientation was exemplified, also in 1979, by my attempt to use rational choice theory to account for findings on price discrimination in housing markets. Two articles in the same journal, one conceptual and the other empirical, made a pair.

   Racial and Ethnic Competition (1983) was the book I wrote instead of a second edition of *Race Relations*. In it I advanced a rational choice theory that was a theory of alternatives as well as choices. It employed rationality as a criterion of behaviour, not in order to differentiate a particular set of choices; it was a theory of aggregate behaviour (1983: 107). Seven years earlier I had concluded that if there were any theories of ethnic and racial relations they must be finite in number and that it should be possible to formulate each one as a set of propositions. In the 1983 book I set out seven theories (typological,

selectionist, ecological, Freudian, class, pluralist, and split labour market) as foils to my own proposed theory. It proved impossible to compare the explanatory power of the different theories because they were *explanantia* addressed to different *explananda*. Recognizing that much of the writing about racial relations had been at least in part historical, I drew a parallel with economic history. Economic theories can be refined, even tested, by their application to historical evidence, while the interpretation of the historical record can benefit from the identification of causal relationships in economic theory. This can be seen as an attempt to correct my second mistake. My proposed theory aspired to a status similar to that of micro-economic theory. It aimed to formulate propositions about the drawing of boundaries that could be checked against historical evidence, and to identify relationships that could inform historical studies in the way that economic theory can guide the writing of economic history (1983: 136–8).

Though *Racial and Ethnic Competition* was received respectfully, its impact has been limited. The selective marketing of new knowledge became evident as the generation of 1968 took charge of the teaching of ethnic and racial relations in an expanding system of higher education. The *soixante-huitards* (as this generation would be called in France) were most interested in policy issues, so the attention of a new generation of students was directed to particular sections of the literature.[5] In recent years – as can be seen from the pages of the journals *Ethnic and Racial Studies* and *Ethnicities*, and a reader entitled *Theories of Race and Racism* (Back and Solomos, 2000)[6] – the strongest trend has been that inspired by the Cultural Studies movement. Many of its exponents are content to work with what Pitt-Rivers called 'the popular British concept' of race and do not seek to elaborate any universal scientific categories that might be used in comparative analysis. They may not wish sociology to develop as a social science or consider this an important issue.

My attempts to challenge these trends had little effect. They included an analysis of racial discrimination as a public bad, an elementary application of game theory, and an outline of a theory of ethnic mobilization (Banton, 1985). To underpin my emphasis on problem orientation I drew attention to the different philosophies of definition associated with Kant-inspired and Hegel-inspired epistemologies. If explanation is the goal towards which sociologists should aspire, the epistemological issues cannot be ignored. Conceptual analysis should facilitate the development of theories that can be used to gather and account for empirical findings. In this process theories will be improved.

The expression 'rational choice theory' evoked hostility from many quarters, particularly from colleagues who had a different understanding of the name's import. I had my own reasons for making less use of this name when, from 1992 onwards, I found it preferable to write about collective action as a means for the attainment of goals that could not be attained by individual action. This featured in *Ethnic and Racial Consciousness* (Banton, 1997), a book that distinguished between the practical language of everyday life and the theoretical language needed for social science. It asserted that the presence of a

word in the practical language did not mean that there must be something in the natural world that corresponded to it. The description of groups as ethnic groups could mislead, for the addition of this adjective carried the implication that ethnic groups were a class with common features. I stressed the importance to group mobilization of group names, of what people called themselves, and of how names were negotiated in interpersonal relations. To call a group an ethnic group was to make a second order abstraction, while what passed for ethnic consciousness was identification with a group identified by a proper name. It was wrong to assume that, because academics classified groups as ethnic, their relations with other groups had a special character.[7] Interpersonal relations are often multi-dimensional; how behaviour is classified depends upon the purpose of the classification.

The contrasts between folk and analytical concepts, practical and theoretical language, *emic* and *etic* constructs, and ordinary and technical language, are different ways of presenting the same distinction. The distinction matters in this context because reliance on ordinary language makes it more difficult to identify sociological as opposed to social problems. For a sociological problem to be well stated, what appears as a big problem often has to be broken down so that its component parts can be isolated. Sometimes an opposite course has to be taken, and a smaller problem subsumed under a bigger one. The study of ethnic and racial relations provides examples of both. The 19th-century conception of race as resembling species was an over-generalization that has persisted as the curse of ethnic and racial studies. It subsumed in a single concept a multitude of inter-group differences that require separate examination. The significance attached to physical differences varied enormously, between regions, between the situations of everyday life, and over the course of time. To aggregate the many interpretations of group difference as interpretations of 'race' and to present racial consciousness as a common factor in varied situations (as Park had done) was to over-generalize, especially when suggesting that the significance attaching to a dark complexion in one society resembled that in another society. It created a false problem. For legal purposes it may be necessary to consider whether someone's action has been racially motivated, or to have been influenced by ideas of racial distinctiveness, but for sociological purposes different questions have to be asked about motivation. It suits individuals to invoke phenotypical categories in some circumstances but not in others. For both theoretical and practical purposes it may be as useful to understand when discrimination does not occur as when it does. Many differences are aggregated in macro categories like 'black' and 'white' and need to be broken down. For instance, in many countries the categories 'black' and 'white' have social class associations such that the over-generalization implicit in reference to racial differences can lead to an under-estimation of the significance of socio-economic differences.

Studies that are restricted to single countries will exemplify under-generalization if they fail to utilize comparative perspectives. So, too, may studies that ignore the commonalities between distinctions based on phenotype,

ethnic origins,[8] religion, and gender. Propositions about the significance of physical differences should be re-stated in general terms as part of the bigger problems of when, why, and how social differences are represented as natural differences. Social differences between men and women, and between blacks and whites, often show the results of a process by which social differences are made to appear natural differences. Just as the groups called 'races' were thought to be biological units, so, too, in some quarters, were nations. A leading Russian politician and authority on nationalities policy (V. Zorin) declared in 1999 that 'Any nation, any people, is a manifestation of nature, which must be respected, with which we must come to terms in the same way as we do with the sun, with the water, with the air …' (quoted Codagnone and Filippov, 2000: 265). The doctrines of gender, racial and national differences as manifestations of nature have helped conceal the instrumentality that underlies the shaping of groups.

Ethnic and racial studies as a global endeavour would do better to assume that there is nothing about ethnic and phenotypical differences that forces individuals to form groups on the basis of these characteristics. The common features shared with groups formed on the basis of differences of class, descent, language, and religion have to be identified and explained first. Then the features specific to ethnic and phenotypical differences can be compared with those specific to other forms of difference. As the problem of accounting for ethnic and racial relations is part of a bigger problem, it cannot be solved on its own. The correction of my generation's mistakes calls for a general theory, built from *etic* concepts, which sets out to explain the formation and dissolution of ethnic and racial groups as the outcome of goal-seeking, including the seeking of both shared goals and individual goals. Many of the ends of human action are implanted during socialization, being fashioned to promote cooperation with fellow members of family, ethnic, and other groups. They generate preferences for association with co-ethnics, but in daily life the value attached to association with co-ethnics varies with circumstances. Changes in relative values generate group formation and dissolution. Social institutions exert top-down influences upon individuals to make them cooperate in the attempt to secure shared goals; other goals can be attained by individual action. The existing theory of collective action has to be expanded to take better account of the structural constraints.

To assume that because there is a word in the ordinary language (like 'ethnicity') there must be a corresponding phenomenon, is, I contend, a mistake. Some of those who collaborated with Glazer and Moynihan in 1975 did not avoid this error when they asked new questions about whether the social significance attached to shared ethnic origin was primordial or circumstantial, although the pursuit of this question contributed signally to the growth of knowledge. Nevertheless it offered no satisfactory treatment of the elements in ethnic group formation that are shared with other kinds of group formation. From a sociological standpoint the critical questions are posed by the

importance individuals attach to shared ethnic origin relative to other values (such as shared class interest). Working with a student from Malaysia, I developed a technique for measuring this. It permitted the formulation of hypotheses, such as 'ethnic loyalty will be less salient in workplace relations compared with the domestic sphere' and about how differences of gender, religion and generation would influence expectations of behaviour in inter-ethnic relations. Several of the research worker's predictions were proven to be mistaken, but these were empirical errors, mistaken conceptions of others' attitudes. The research was not advanced enough to generate theoretically interesting mistakes (see Banton and Mansor, 1992, and some further findings reported in Banton, 2000).

Conclusion: Popper's Mistake

According to Popper's philosophy, the identification of mistakes helps us recognize what we have learned. Knowledge has grown because we have defined our problems better, found more convincing answers to our questions, and can move forward with a better sense of direction. One difficulty with this formulation lies in the use of the first person plural. Who are the 'we'? Those who study ethnic and racial relations seek to answer many different kinds of questions. They often insist, on political and moral as well as theoretical grounds, that their colleagues should address certain questions rather than others. Nevertheless, they have, from time to time, to accept the cogency of arguments advanced by others who set out from very different starting points. For example, I have accepted the cogency of the Marxist argument that many of the concepts used in this field are borrowed from ordinary language where they have been shaped by the structure, and class struggles, of the surrounding society (Banton, 2001: 183–4).

When asserting that science begins with problems, Popper failed to notice that there can be an important difference between practical and theoretical problems. In the practical language, folk concepts are fashioned to aid the solution of the problems of everyday life, and are subject to political distortion.[9] At one point Popper (1994b: 67) wrote 'Thus the very idea of knowledge involves, in principle, the possibility that it will turn out to have been a mistake, and therefore a case of ignorance.' The transition from ignorance to knowledge in a field like ethnic and racial studies depends upon a process of conceptual clarification in which folk concepts are cleaned up. The folk concept of race is not simply a mistake. It is a crude representation of phenotypical variation that serves certain everyday ends. These are distinct from the ends served by theoretical concepts, so the two kinds of concept are different in character. If there is this difference in the conceptualization of problems in respect of race, it is probably experienced in other fields of sociology also and is occluded by a Popperian dichotomy between ignorance and knowledge.

Popper's philosophy of science derived from his earlier studies of physics rather than his later interest in biological evolution. Post-Darwinian biology has been revolutionized by the genetical theory of natural selection that laid the foundation for a generalized analysis of processes of organic change. Yet this theory, by itself, is insufficient to account for the origin of species, to explain why particular species have developed, what characterizes them, the size of their populations, and so on. In response to such *explananda*, zoologists consider the resources available in particular environments, the competition from other species, and so on. Similarly, the study of ethnic and racial relations could take inspiration from a general theory in order to account for relevant features of the cultures and institutions prevailing in particular localities, allowing for the significance attached to shared experience, control of territory, and the global competition for resources.

The problems that directed my research in Stepney and Freetown were loosely defined and lacked the stimulus that can be provided by a comparative dimension. In *Race Relations* I attempted to develop a general theory that could help account for patterns of change in particular localities, equating race with phenotypical variation; I presented the orders of race relations as generating stimuli to social alignment in a top-down direction and failed to take sufficient account of corresponding bottom-up processes. My first mistake arose from a moral impatience with a conceptual challenge that demanded a wide-ranging deconstruction of the folk concept and an examination of the possibly legitimate uses of the word *race*. Other writers in the field have seemed to share this impatience, believing it unnecessary to differentiate the various meanings of the word when instead they could publicize their contribution to the diagnosis of a political challenge. My second mistake was an extension of the first one. In *Racial and Ethnic Competition* I tried to rectify these mistakes by representing ethnic and racial groups as created, maintained (and sometimes dissolved) by the actions of individuals in including some persons in their groups and excluding others. Seeking to lay foundations for a general theory, I emphasized the analytical and transcultural potential of concepts like inclusion and exclusion, mobilization, and preference for association with co-ethnics.

My mistakes belong in the transition from the phase in which social scientists can work with ordinary language concepts to the phase in which a technical language may make it possible for an empirical finding to falsify a theoretically significant hypothesis. That transition may be accelerated by social changes, for the concepts in the two languages can influence one another. The inadvisability of taking for granted the existence of racial and ethnic groups, as if they were natural phenomena, will become ever more apparent with the increasing numbers of persons of mixed ethnic origin (for whom the name 'multi-racial' is often preferred in the USA). Social scientists will have to pay more attention to the social significance attributed to differences in skin colour than the recent preoccupation with racism has permitted. This will stimulate them to develop a conceptual armoury that will supersede the conception of race that has defined so much of their 20th-century work.

Notes

1   If a demographer extrapolates from one census result to estimate later population figures, and a subsequent census shows them to be inaccurate, it should be possible to locate the mistaken assumption. I made a mistake of this kind in Table IV (p. 159) of *White and Coloured* (1959), regarding the size of the Indian minority population. I knew that most of the people in East London were (at the time) of Pakistani nationality. I assumed, without inquiring into it, that the ratio of Pakistanis to Indians in Britain was similar to that in East London, and therefore underestimated the Indian population.

2   Had I been writing this a decade later, I might have elaborated upon the parallels between the cultural use of sex differences and of so-called racial differences. Popular beliefs about the natural differences between men and women are very different in the Middle East and in North America, while beliefs about gender differences are very different in 21st-century Europe from what they were in the 19th century. Natural differences of sex are used as role signs and the manner of their use changes in association with other social changes. The main contemporary alternative solution to the problem of defining racial consciousness was also caught up in the use of popular consciousness to define a theoretical problem. John Rex (1970: 37–40) referred to 'cases in which the perception of physical differences does not lead to race-relations problems'. He opposed both the study of ideas in isolation and any view of them as 'mere epiphenomena arising from structural sources' (as in simple versions of Marxism), but made little allowance for bottom-up processes. Other writers, then and subsequently, have tried to evade the difficulties by writing of racial conflicts as if everyone could agree which these are.

3   *Race Relations* was the subject of a multiple review in *Current Anthropology* (1969) 10(2): 202–10.

4   Paul Gilroy (1998: 838–9) appears to repeat this mistake. When he writes about 'how we see "race"', about 'the fact of "race"' and the economy 'that reproduced "race"', he surely refers to phenotypical difference.

5   One element in the post-1968 outlook was the claim that the impossibility of isolating sociology from all political influence justified the orientation of social science to attain political objectives. That it was impossible to exclude all political influences did not mean, I considered, that it was not desirable to exclude them so far as was possible. While I am concerned here with mistakes in theoretical reasoning, it should not be thought that I ignored practical issues. In 1970–2 I participated in the work of a Home Office committee on police training in race relations and wrote a manual about this. My membership, from 1986 to 2001, of the United Nations Committee on the Elimination of Racial Discrimination enabled me to contribute to the development of international law regarding racial discrimination. Article 3 of the International Convention on the Elimination of All Forms of Racial Discrimination had, from 1970, been interpreted as directed against apartheid. In 1995 I persuaded the Committee to adopt what became its General Recommendation XIX; this stated, in part, that 'The reference to *apartheid* may have been directed exclusively to South Africa, but the article as adopted prohibits all forms of segregation in all States parties.' Had I not pressed for a broader interpretation of article 3, no other

member of the Committee would at this time have done so. I also drafted the Committee's statement for the second United Nations Conference on Human Settlements in 1996 (Habitat II) about racial segregation in housing as a breach of human rights. The Special Rapporteur on the Right to Adequate Housing of the Commission on Human Rights (Mr Miloon Kothari) later testified that he had been inspired by the Committee's General Recommendation XIX and that the 1996 statement 'had proved groundbreaking' (UN document CERD/C/SR.1533 paragraphs 33–61).

6   Despite the title, the editors do not identify any theory of race or any theory of racism.

7   In *Racial Consciousness* (1988: 9), I wrote that 'In one form [racial consciousness] is an individual's interpretation of how his or her life is affected by the way others assign him or her to a racial category. In another form, it is an individual's tendency to assign others to racial categories.' In the second edition (1997: 17–18), I dropped these formulations to write of group consciousness. I now contend that Park's axiom should be discarded.

8   The distinction between ethnic origin and ethnicity can be important. Those who write of ethnicity in the abstract usually generalize about the qualities shared by groups classified as ethnic in the ordinary language of particular societies at particular moments in time. The loose use of 'ethnicity', as in much popular writing about ethnic conflict, distracts attention from the other motivations underlying group formation.

9   For example, in recent years the mass media in Britain have frequently invoked ideas of race when presenting issues of immigration, asylum and community relations. This must influence one popular meaning of the word, but it is also used for quite other purposes, as for combating various forms of discrimination and as an implicit underpinning for accusations of racism.

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