TRACING THE CONTOURS
Feminist Research and Feminist Objectivity

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Synopsis—This paper discusses Donna Haraway's 1988 arguments about feminist objectivity and situated knowledges. The author suggests that in order for feminist research to enhance feminist objectivity, as outlined by Haraway, it is necessary to delineate some principles or criteria according to which research could be evaluated. The author argues that the three elements which Haraway points to as being central to any discussion of feminist objectivity—accountability, positioning, and partiality—lead to three questions, which, in turn frame criteria and principles according to which research may be defined as implementing the goal of feminist objectivity. It is these three criteria—reinscription, micropolitics and difference—which are discussed and defined in this paper. The author uses the example of her recent research with young working class people in Britain to demonstrate how the criteria may be used to implement and enhance the projects for developing feminist objectivity.

The projects of feminist research are frequently thought of as having epistemological concerns at their centre (Le Doeuff, 1987), these concerns having posed challenges to the practices and theories of the human sciences within the academy. Many writers have been working on these issues in the past two decades (Stanley & Wise, 1979; Hartsock, 1983; Mies, 1983; Rose, 1983; Smith, 1987; Eichler, 1988; Stacey, 1988; Haraway, 1989; Hill Collins, 1990; Harding, 1991) and it is generally agreed upon that issues of objectivity and their relationship to 'science' are issues which are at the forefront of the projects of feminist research. Simultaneously, arguments which look critically at positivist approaches to knowledge have impinged upon the disciplines of sociology (Abbott & Wallace, 1990), history (Alonso, 1988; Passerini, 1987), and social psychology (Squire, 1989) to name but a few examples. These universes of discourse have been informed by feminist approaches, which means that questions are raised, for example, within psychology about the apparent objectivity of the experimental method (Wood Sherif, 1987). This, in turn,

has brought into focus the arguments about the limited value of quantitative analyses in providing insights into issues of human relationships (e.g., Griffin, 1985) and about power inequalities within the research process (Bhavnani, 1988). Such discussions frequently have focused on the *methods* deployed in the generation of insights in the human sciences. What often has flowed from these discussions are broader challenges which interrogate empiricism and positivism.

Such challenges mean that scientific activity as neutral and value free has gained academic credence (e.g., Rose & Rose, 1976) although not a widespread academic acceptance. If, however, it is accepted that scientific insights are social in origin, then these origins may be analysed by tracing the historical development of such insights. Thus, an historical approach can facilitate answers to questions such as *why* a particular issue is investigated at a particular point in time. For example, *why* was it that the end of the nineteenth and turn of the twentieth centuries saw considerable interest in relying on arguments about the brain size of black people and white women to explain apparent differences in cognitive abilities between these groups and white men (Griffiths & Saraga, 1979). An historical approach may also facilitate the posing of questions as to *how* such knowl-

edge is produced—that is, who produces it and how it becomes privileged. The logic of this argument is that an historical approach encourages questions to be raised about the political economy of knowledge production. In this way, an historical approach can eliminate the idea of total knowledges; thus, objectivity and truth come to be seen as concepts which are historically situated and situationally specific.

Knowledge production is, therefore, an historical process. My argument is that feminist epistemologies, in the process of continuing challenges against positivism, have always placed questions and issues about the historical relationships between science and society at the centre of our work. Feminist theorising has always argued that there is a necessity for scientific work to examine its practices, procedures, and theories through the use of historical insights, for it is these insights that bring into focus the ways in which knowledge production is a set of social, political, economic, and ideological processes (e.g., Acker, Barry, & Esseveld, 1983).

The arguments about the historicisation of knowledge, embraced by many writers (e.g., Bhaskar, 1989; Fraser, 1989) lead me to ask of feminist studies—is feminist work being developed with an adequate historical sense of differences amongst women?

The work in the United States of, for example, black writers such as Angela Y. Davis (1971, 1982) and Patricia Hill Collins (1990) and in Britain by writings such as those of Bryan, Dadzie, & Scafe (1985), Grewal, Kay, Landor, Lewis, & Parmar (1988), and Ware (1992) has shown that the histories of the feminist movement in both of those countries are fraught with racisms and exclusionary practices. These arguments continue by suggesting that an important consequence of these histories is that racialised, gendered, and class-based inequalities are embedded into the creation of knowledge. What often occurs in the process of presenting feminist arguments for the historicisation of knowledge is that the points about racisms, exclusion, and invisibility of women of colour become silenced (see Haraway, 1989, for an exception). Thus the questions that charges of exclusion and invisibility pose of feminist studies begin to disappear, the projects to create feminist knowledges become weak and

fragmented, and history gets reenacted. It is sometimes implied that inclusion of racism in feminist work can lead to fragmentation of feminist projects. My argument is that far from an analysis of racism leading to fragmentation, it is the process of not engaging with the consequences of racialised inequalities which weakens the projects of feminisms.

Thus, I argue that challenges from feminist writers and analysts to positivist approaches to knowledge raise an issue central to knowledge production, namely, that such production is an historical process. I suggest, however, that many of those working on feminist epistemologies have often developed inadequate arguments about such historicity by erasing, denying, ignoring, or tokenising the contradictory and conflicting interests which women may have—often seen most clearly in the writings of women from all over the world (see Bhavnani, 1992, for a discussion of these contradictions). Conflicting interests can also mean that different standpoints develop which are in sharp opposition to each other. For example, the history of white women's suffrage in the United States demonstrates conflicting interests in that such suffrage was often argued for at the expense of black suffrage (Davis, 1982). This way of writing history, that is, that conflicting interests amongst women are made *visible*, can lead to questions arising about objective knowledges. In other words, this approach can demonstrate that objective knowledges are situated and partial, not impartial or disembodied, and neither are they transcendent.

While the above arguments are not new, nor specific to feminist critiques of the social sciences, it is Donna Haraway (1988) who has recently recast and reframed them in her representation of feminist objectivity. Her sense of objectivity is in opposition to positivist discussions of this concept, and is also distant from the absolute relativism embodied in the view that all truths are equally valid. She says "Feminist objectivity means quite simply *situated knowledges*" (p. 581). In describing feminist attempts to grapple with discussions of truth, she convincingly points to "'our' problem [which] is how to have *simultaneously* an account of radical historical contingency for all knowledge claims" (p. 579), while still retaining a sense of the material or

'real' world. She suggests that feminists could view objectivity as a "particular and specific embodiment," rather than as a "false vision promising transcendence of all limits and responsibility" (Haraway, 1988, p. 582).

> Feminist objectivity is about limited location and situated knowledge, not about transcendence and splitting of subject and object. It allows us to become answerable for what we learn to see. (Haraway, 1988, p. 583)

She continues later by stating that

> We seek those ruled by partial sight and limited voice — not partiality for its own sake, but, rather, for the sake of the connections and unexpected openings (which) situated knowledges make possible. Situated knowledges are about communities not about isolated individuals. (Haraway, 1988, p. 590)

What may be derived from this is that partiality of vision need not be synonymous with partiality of theorising, and, indeed, may be desirable, for the partiality she discusses could lead to greater insight for feminist analyses. Thus, she argues that not only are positioning and partiality two key elements of feminist objectivity, but "becom[ing] answerable for what we learn to see" requires that a third element — accountability — also be present. The strength of this argument is that she *engages* with difference, and, indeed, uses difference as the springboard from which to transform feminist arguments about objectivity.

Her emphasis on accountability, positioning, and partiality is helpful, for this accentuation can permit a clearer approach to analysing and developing feminist insights into objectivity. Further, her argument that these elements, when emanating from feminist frameworks, can provide some dynamic and creative connections in the production of knowledges is both exciting and timely. If these elements do permit the development of creative and dynamic connections, then it is appropriate to pose the question: "by what means do these elements permit the making of such connections?". What are the implications to be drawn from the three elements —

accountability, positioning, and partiality — for the ways in which knowledge production can be faithful to the notion of "feminist objectivity" outlined by Haraway? What are the principles that flow from these elements and which, in turn, indicate criteria according to which research can be evaluated as 'feminist'?

This question is one which consistently haunts feminist researchers — especially those of us working within the social sciences — namely, is it possible to identify principles which could frame the development of criteria for the conduct, evaluation, and dissemination of feminist work in the social sciences? Is there, indeed, anything which is particular to feminist enquiry in the social sciences, after having specified that the main agent of the enquiry be a woman, or women? This question has been raised many times (recently by Jayaratne & Stewart, 1991) and yet it has still not been thoroughly interrogated. Most readers of this article would agree that research is not necessarily feminist if it is conducted by a woman, nor that the subjects of the enquiry be only women, but surely it is valid to state that the main agent of any research which claims to be feminist must be 'woman.' However, while that is a necessary condition for feminist work, it is not a sufficient one. It is necessary because 'feminist' is derived from 'feminism,' which is a political movement comprised of women, but it is not sufficient because there is a clear distinction between 'woman' and 'feminist.' Each category is not unitary nor singular, (see Sandoval, 1991) but neither are they collapsible into each other — for 'feminist' is an achieved status, or, more precisely, a continuous accomplishment.

If it is not sufficient that women are the key agents in any work which is defined as feminist, then how can one identify work as feminist in the sense of furthering the aims of feminist objectivity — that is, creating situated knowledges — as laid out by Haraway? I suggest that questions be developed, and principles delineated in order to set up markers against which any social scientific enquiry could be evaluated for its claim to be feminist.

The first principle which flows from Haraway's insistence on accountability as an element within feminist objectivity is that any study whose main agent is a woman/women and which claims a feminist framework

should not reproduce the researched in ways in which they are represented within dominant society—that is, the analyses can not be complicit with dominant representations which reinscribe inequality. In other words, the accountability of the research is not only to specific individuals, but also to the overall projects of feminisms. For example, feminist work often struggles to make the agency of women visible, while not presenting this agency as deviant (e.g., Essed, 1990). So, when people are in positions of structural subordination, research which is defined as feminist must, at the very least, reflect upon whether the analysis presented in the work reinscribes the researched into the dominant representations of powerlessness, into being viewed as without agency, into being defined as abnormal. The questions which flow from Haraway's first element are 'does this work/ analysis define the researched as either passive victims or as deviant?' 'Does it reinscribe the researched into prevailing representations?' If that is the case, as in, for example, some studies which have been published about South Asian women living in England, (see Brah, 1987, for a commentary about this; and see Brah & Shaw, 1992, for an example of a study which avoids such a trap) then it seems to me that regardless of whether the research focuses on women, or is conducted by women, or both, *it may not be defined as being informed by feminism*. If research is unable to achieve such a definition, then, it cannot implement the project of furthering feminist objectivity. I am not, for one second, suggesting that women researchers provide romanticised analyses of people who are frequently in positions of structural subordination. Rather, I am arguing that for feminist objectivity to be enhanced, and for knowledge production to be explicitly understood as an historical process, it is incumbent on women researchers to pose the above question of our/themselves, and to deal with it in the analysis. When this is done, then the work may be claimed as fashioning feminist objectivity.

The second question emerging from Haraway's arguments about positioning is whether the research report, however and wherever it is presented, discusses, or, at its most minimal, makes reference to the micropolitical processes which are in play during the conduct of

research. In short, the question is, how and to what extent does the research conduct, write-up, and dissemination deal with the micropolitics of the research encounter—what are the relationships of domination and subordination which the researcher has negotiated and what are the means through which they are discussed in the research report?

The third question, analogous to her element of partiality, is centred upon 'difference.' In what ways are questions of difference dealt with in the research study—in its design, conduct, write-up, and dissemination?

It is these three questions: Are the researched reinscribed into prevailing notions of powerlessness?, Are the micropolitics of the research relationships discussed?, and How are questions of difference engaged with?, which I suggest flow from Haraway's discussion of feminist objectivity, and which provide reference points through which principles may be delineated and thus, research projects evaluated. The three questions generate principles and criteria which permit the creation of sufficient conditions, beyond the necessary one that the main agent is a woman. It is the combination of these necessary and sufficient conditions which then can provide the framework for evaluating research as feminist.

I shall use three questions as reference points—reinscription, micropolitics, and difference—to frame the second section of my paper. This is an examination of a research study I conducted in Britain in 1984 and 1985, whose write-up was completed in early 1988 (Bhavnani, 1991). I shall use the lens of feminist objectivity combined with the questions raised above to comment on that work and to discuss in what ways it stands under the umbrella of feminist objectivity.

The research study explored the ways in which young, working class people in Britain discussed issues in the domain of the political. Much of the psychological work on young people in Britain has discussed youth as a homogeneous group, focusing on insights derived from biology and conventional psychology (e.g., Conger, 1973). Such work has therefore disguised the ways in which the transition between childhood and adulthood are social transitions (Bates et al., 1984). The transition is presented as a 'natural' one.

From this vantage point, when the political views of young people have been discussed, most often using survey methods, young people have been presented as either politically apathetic or politically rebellious (see, e.g., Furnham, 1985, for a review). The definitions which have been used to tap political views have tended to focus exclusively on the parliamentary process in Britain, as well as potential voting behaviour. Whilst not *all* work with young people has done this, as, for example, the work which came out of the Centre for Contemporary Cultural Studies at the University of Birmingham in England, (see, e.g., Hall & Jefferson, 1975) there is an overwhelming set of discourses about white young men in particular — much of this work had been done with men — suggesting that working class young men in Britain are not interested in politics. That is, the argument implies, young working class men are not political. In this way, the direction of such research ends up reproducing discourses which, in general, cast young working class men as social victims. The work from the Birmingham Centre, and their approach to young people, mostly men, relied on ethnographic and quasi-ethnographic methods. The use of ethnographic approaches, combined with feminist work with young women, as in for example, the work of Angela McRobbie (1982) and Christine Griffin (1985) demonstrated to me that it was possible to take the perspective of young people seriously within an academic research project. But I did not assume that I should automatically take the young people's perspective at face value.

The study did not define 'politics' only as the arena in which voting, political parties, and knowledge of the official political processes in Britain are discussed. Rather, it took as a starting point that politics is the means by which human beings regulate, attempt to regulate, and challenge, with a view to changing unequal power relationships. Such an open definition of politics required that I think hard about the topics to be discussed within the study, as well as the ways in which these topics would be discussed with young people. As a result, I conducted pilot work in Sheffield, a town in Northern England in which I used a formal questionnaire, with closed questions, as well as 'hanging out'[1] in the largest shopping mall in the centre of Middleton,[2] where youth often gather on weekday afternoons and on Saturdays. I also spent considerable time in youth centres in Middleton — where young people come to play pool, table tennis, listen to music, and talk. From the 4 months I spent immersed in this style of work, that is, a constant 7 days a week engagement with young people, I quickly saw that young women, black and white, were not very present in either the malls or the youth centres, except in the latter case, when there were 'girls' nights or special sessions set up for young women of South Asian origin. As I had wanted to discuss issues within the domain of the political with both young men and young women, I decided to move the main study to schools in Central and South Middleton. I first conducted single-sex group discussions with 90 16-year-olds in their final year of school in early 1985. The discussions covered a range of issues having "to do with society" as I explained it, and I used a frequency count of the topics which were raised by the school students to include in the individual interviews. I also wanted to discuss issues which are more frequently thought of as political, such as the parliamentary parties, and so I included this as well into the individual interviews.

The decision to move away from adopting quantitative analyses in this study was informed both by my personal history of work in developmental and social psychology and by my pilot work. The agenda for the open-ended individual interview schedule which I used was a negotiation between myself and the young people who were the potential interviewees of the study, and the use of open-ended interviews, based on prior group discussions, was a means whereby that negotiation could occur. There was no indication that the young people did not want to discuss the issues within the domain of the political which they and I had negotiated together, and so, suggestions of political apathy remain marginal for any analysis of this work. They also talked at length — as the thousands of pages of transcripts show.

In interviewed 72 young people in this way — half of whom were men. Approximately one-third of the interviewees were of Afro-Caribbean origin, one-third were of South Asian origin, and the remaining third was white. The topics covered in the individ-

ual interviews were derived from the group discussions and included employment, unemployment, training for young people, racism, democracy and voting, marriage and violence against women and children, and the miners' strike in Britain of 1984/1985. Sixty of the 72 interviewees were interviewed by me 6 months later—the issues being life since leaving school, employment, unemployment and training schemes, recent rebellions by youth in Britain, their futures, and party politics. In this way, these interviews about issues in the domain of the political were situated in the context of the movement of these young people from school to unemployment or the labour market. These 132 interviews, each of 40-45 minutes in length, were tape-recorded and then transcribed.

I have been reflecting on the points I made earlier about furthering the project of feminist objectivity because there have been times when I have discussed my research in public forums that I have been asked "But what has this project got to do with your interests in 'race' (for which read difference) and gender (for which read feminist studies)?" A question which requires specification of how feminism and difference are implicated within this enquiry is certainly a helpful question, for any response to it has to lay bare some of the assumptions, and hence the criteria according to which academic work may be claimed as feminist. The criteria are clearly not obvious in this study, for apart from my self-definition as a black feminist in Britain in the past two decades, there is apparently very little in any initial outline to indicate that the work is based on and aims to develop feminist work. The research explored issues in the domain of the political—not specifically a feminist preoccupation, nor, indeed, part of that common sense of "women's issues." I also interviewed both men and women. In what ways, therefore, can it be defined as a feminist enquiry, and, hence as something which could help in the elaboration of feminist objectivity?

My previous discussion suggested three questions—focusing on reinscription, micropolitics, and difference—which generate principles and criteria from which research conducted and written by women can be evaluated for claims to feminism. Below, I shall discuss my research according to these three

questions, and I shall also draw on insights from Haraway's suggestion that feminist objectivity provides unexpected openings and connections.

The first principle I discussed was that of reinscription. Let me point to the ways in which this research project may have been partly successful in avoiding reinscribing the researched as without agency, and as 'politically apathetic'—that is avoided a representation of young working class people as cultural dopes. I would not want to claim that it was totally successful, but I should like to present the reasons for my thinking that it was, partly, successful.

Many of the young people, when discussing party politics organised their arguments through the theme of intelligence ("don't think I'm a brain box" was the way in which one young woman said it). The suggestion was implied by many of the interviewees that their views about, for example, the Labour Party were not legitimate because the speaker did not see her/himself as intelligent. Thus, intelligence came to be seen as a necessary requirement for being able to comment on parliamentary parties. This link between democracy and 'intelligence' provided by the interviewees led to an unexpected opening. That is, the young people in this study were not necessarily politically apathetic, but rather, that these young people understood the playing out of democracy in relation to levels of intelligence, and thus defined themselves as not intelligent enough to present *legitimate* opinions about party politics. Not that there was no interest there—just an implicit and explicit wondering whether their views were legitimate. It is this link between democracy and intelligence which provides an unexpected opening.[3] The point for the present argument is that the young people in this particular research project were not politically apathetic or politically disinterested, but rather, that there were distinct and comprehensible reasons why they appeared to not discuss many issues in the domain of the political. In taking the stated views of young people seriously, but not necessarily at face value, the research did not reinscribe the researched into dominant representations—which suggests that young working class people are social victims. In avoiding such a reinscription, the study may lay claim to fur-

thering feminist objectivity, and therefore, suggest a more productive way of understanding the construction of politics by this group of young people.

The second question which I have suggested could be used to indicate whether the project of feminist objectivity is beginning to be implemented, is related to the micropolitics of the research process. Again, let me take my research as a case study through which I can show what I understand by this.

The power of the researcher in relation to the researched — a set of power relationships which are bounded by the imperatives of resource availability — can define the parameters of the theoretical framework, can control the design of the study, and can inform how the study is conducted, analysed, and written up. That is, the researcher is positioned in a particular relationship of power in relation to the researched. Frequently, research which has been influenced by the arguments of feminist writers such as Helen Roberts (1981) or Liz Stanley (1989) will note this positioning of the researcher. My argument, however, is that the micropolitics of the research situation need to be analysed and not only noted. For example, relationships within my study flowed from the socially ascribed characteristics, such as 'race,' gender, and class, as well as age, of the interviewer and interviewee. These socially ascribed characteristics carry hierarchical loadings of their own. Many times, the sensitive social scientist has tried to regulate this unevenness in the social characteristics by ensuring that women interview women and that black researchers interview black people. In fact, it may even have been expected that I would have designed a research study in which I set up such 'matching' — that is, that I only interviewed South Asian women, or black women or black and white women. I knew however, from the start of this study that I wanted to interview white men — because I wanted to see "what would happen." Rosenthal's 1966 work on the experimenter effect is often cited to justify the matching of researcher and researched, and work such as that by Zenie-Zigler (1988) is an example of a study which merely noted the position of the researcher in relation to the researched. I suggest that both matching and noting can take the gaze of the analyst and reader away from the micropoli-

tics of the research encounter. This is because the processes of matching and noting cannot explicitly take account of the power relationships between the researcher and the researched, and yet both processes imply that unevenness between the two sides in a research study has been dealt with. In the research study discussed in this paper, such matching was never present — because I was always a woman who was 15–20 years older than the interviewees. This age non-matching was frequently interwoven with matches or non-matches of culture, 'race,' and gender. For example, when interviewing young white men the frequently encountered imbalance of power between white men and black women was potentially both inverted and reproduced in the interviews. That is, when interviewing young white men, my role as student researcher, my age, and my assumed class affiliation may have been taken as sources of potential domination. However, my racialised and gendered ascriptions suggested the opposite. That is, in this instance, the interviewees and myself were inscribed within multifaceted power relations which had structural domination *and* structural subordination in play on both sides. This interplay of subordination and domination on the part of both interviewer and interviewee was a consistent feature of my study.

Let me compare this to a study for which I have considerable respect, but which, as I reflect upon it, I seem to want to add to. Paul Willis (1978) conducted an ethnographic study which deployed participant observation, discussion, and individual conversation with a group of young working class men in Britain in the mid 1970s. He wanted to analyse the ways in which, as they moved from school to the labour market, working class men obtained and stayed within 'working class jobs.' One part of his study showed that when the boys talked about their girlfriends, they discussed them in objectified and frequently very dismissive ways. This notion of how this group of young men discuss issues of heterosexual relationships forms part of some forms of academic conventional wisdom — white working class men are overwhelmingly and offensively sexist. None of the young men I interviewed talked about women in that way. Clearly, there is an important question to be dealt with here, which is why the young men

in my study did not talk to me in the way in which they talked with Willis.

The first answer which is often presented is that "well, you're a woman and they thought they should not be rude to you, the *woman* researcher." Such a comment implies that my interviews with these young men are not authentic—and thus that work such as that of Willis is authentic about young men. This notion, that *some* kinds of work are 'authentic,' and, therefore, by implication, others are inauthentic has been sufficiently discredited to make that kind of explanation unsatisfactory. What such a notion does is, however, to reinscribe a Willis-type study as 'natural,' as *the truth*. What I am saying is that Willis, a white man, becomes a marker for a universalistic insight whilst I, a black woman, become particularised. (See, e.g., Barbara Christian's 1987 discussion of a similar issue when 'race' and theory are under scrutiny.) In other words, I argue that the questions which may be addressed to me about my work also need to be put to Willis. When one begins to think of it in this way, then it is possible to analyse the micropolitics of the research process. That is, when the socially ascribed, hierarchically organised characteristics of the researcher and researched have structural domination and structural subordination in play on both sides in a manner which inverts the usually encountered imbalances, *this* set-up can provide an opening for an analysis of the micropolitics. I am not suggesting that men interviewing women is a consequence of my argument, for that is merely a replication of the most frequently encountered power imbalances in research studies. What I am suggesting is that an inversion of this 'normal' power imbalance in research studies—from the conception right through to the analysis—can permit a sharper analysis of the micropolitics of research, so that *feminist* objectivity can be implemented. So, any text which emerges in a research encounter cannot be taken for granted.

The third question which can be posed is to ask in what ways issues of difference are seen and dealt with explicitly. Ironically, this has been the one that has been the most difficult one for me to address explicitly in the context of this paper. I have again taken my lead from Donna Haraway's account of partiality, which she makes clear does not imply partiality of theorising. Many readers will be familiar with the argument, often, but not exclusively presented by women of colour that studies that have women as researchers and women as the researched group have ignored or glossed over differences amongst women (e.g., Bridenthal, Grossman, & Kaplan, 1984; Hewitt, 1985; Lazreg, 1990). This research study did point to many continuities of experience for the young people, who had shared experiences of their schools, their housing, and the relationship of their household to the local state. However, it is clear that there were also non-shared experiences and accounts such as those of racism, culture, and gender. If difference is understood as difference of interests within this study—that is that there are material reasons for the discontinuities of experience and identities put forward by the young people—then the ways in which the young people talked about racism provides an entry point for such a discussion. On the whole, the young white people in this study did not express explicit racism, although some of their comments were situated within discourses which can lead to a reproduction of racism. An example of this would be "I don't care what colour they are—they're just my friends." In general, however, most of the white interviewees claimed to be against racism by utilising the theme of "we're all humans aren't we?" This appeal to a common biology and naturalness led the white interviewees to suggest that racism was also 'natural.' For example, many white interviewees said "it's human nature" and, therefore, that little could be done to eliminate it by society.

In contrast, many young black people identified a number of the ways through which some of the contradictions of racism could be considered, thus indicating one type of strategy for challenging racism. Examples of the identification of contradictions were points such as "why do they want a tan when they criticise us because of our colour?" Or reference was made to the arranged marriage between Prince Charles and Diana as being publicly lauded, while official statements in Britain about arranged marriage within South Asian cultures condemned the practice. Some of the young women of South Asian origin, suggested that as marriage was a 'natural' consequence in their lives, then one could defend the *concept* of arranged marriage, because the logic was that "if you're going to get married anyway," an arranged mar-

riage was preferable to a love marriage. When asked why, one young woman said:

> **PN11**: Cos you don't have to go out looking for someone — I couldn't.
> **KKB**: You couldn't?
> **PN11**: No, must be like a hunter with a spear and net, hunting for a husband.

This strategy, of pointing to contradictions within racist arguments was one which was developed by the black interviewees. This is one way of beginning to examine difference — in the sense of pointing to the contradictions within racist arguments. However, the young black people also discussed racism through suggesting explicit strategies to tackle it. Such strategies included "I told my teacher," "I wanted to grab and choke them," and "I ignored them," this last implying that ignoring was one strategy out of a repertoire of strategies available to her. The interweaving of a concept of strategy into their discussions suggested that these speakers thought that racism could be altered by being challenged, either by pointing to the contradictions or be suggesting explicit means to tackle racism. In suggesting that patterns of racist behaviour can be altered and eliminated, there is a consequent implication that, therefore, racism is not natural. That is, it is implied that racism is a result of social definitions rather than a biological inevitability of "human nature."[4] It is this discontinuity of both identity and experience which can be generated if a sense of difference is built into the research process.

In conclusion, it is clear that feminist projects which trace the contours of feminist objectivity are a central means by which it may be possible to escape from the impasse over questions of 'objectivity,' and 'truth,' which at present hound many discussions of research practices. Further, I suggest that *feminist* objectivity, with its principles of accountability, partiality, and positioning leads to a set of questions, which, in turn, frame principles and criteria for the evaluation of research studies — criteria of whether or not subjects are reinscribed into powerlessness, of how the micropolitics of the research are discussed, and of how 'difference' is made integral to a research study.

The point can be made most concisely us-

ing the following extract from an interview conducted for the study:

> **KKB**: What's your ideal job?
> **PN60**: I'd like the job of the Queen.
> **KKB**: Why? What does she do?
> **PN60**: Well, put it like this, she gets paid for breaking bottles against ships and we get arrested for breaking bottles on the street!

ENDNOTES

1. This is a common technique used in participant observation and ethnographic research where the researcher spends time with the researched on their territory, sharing their work, leisure, or home environments.

2. This is the fictional name for the large town in the North of England where I conducted the study.

3. The concept of intelligence has been widely criticised for at least the past two decades as being a means by which economic, social, and racialised inequalities are both reproduced and therefore sustained (see Richardson, Spears, & Richards, 1972) — that is, that notions of intelligence can undermine the goals of a democracy. Clearly, a question which then can be raised, from the interviews with the young people, is what it is in official discourses about public-domain politics which legitimates this view.

4. One can ask why it was that the young black interviewees discussed racism with me in these ways, and why the white interviewees did so in a different way, and I have dealt with that type of argument in my discussion of Willis (1978).

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