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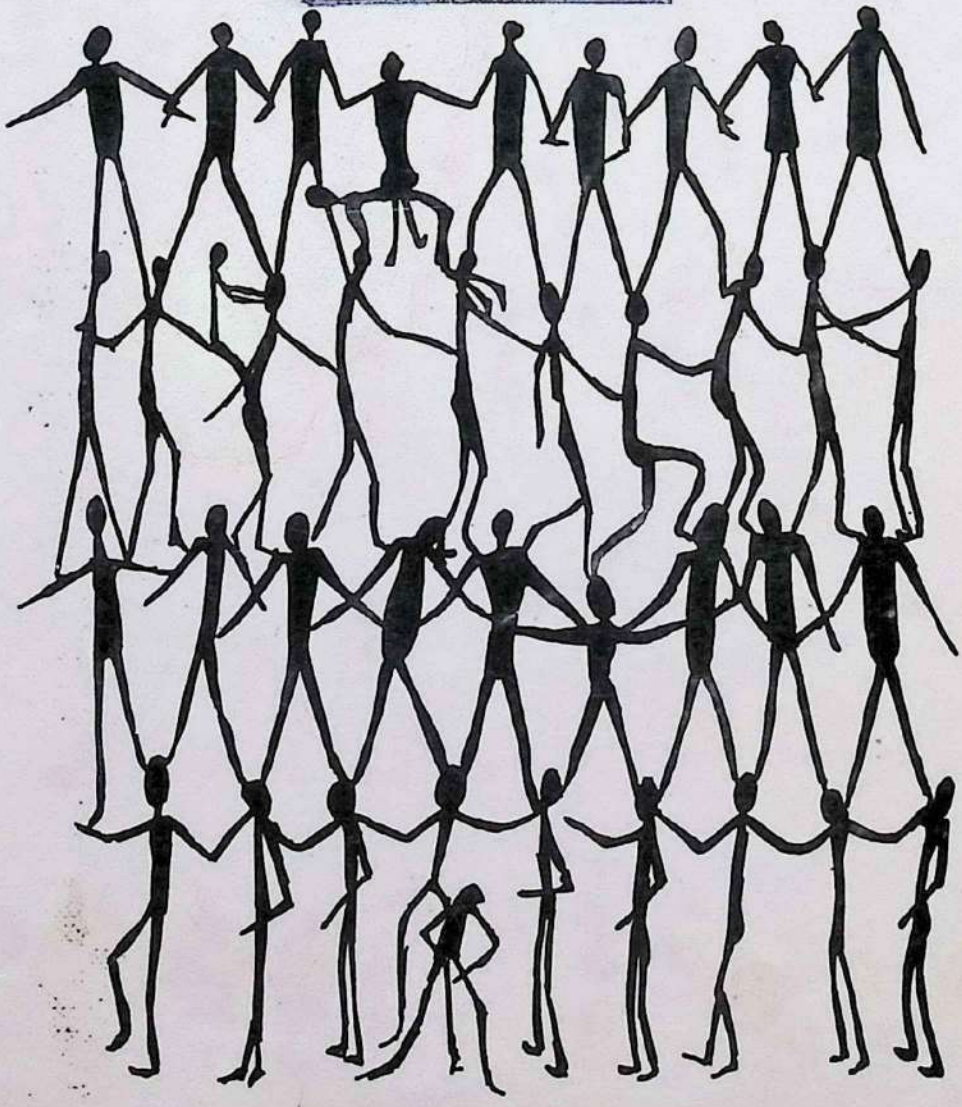

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****Erratum Vol. 5 No. 1****

1. contents page of the last issue (V.5 No.1), in reviews section, Peter Reid should be Peter Read.
2. p.71, last line, should read - 'He shows that *none* of the parties....'

Society and culture in New Zealand: an outburst for 1990

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Introduction

Throughout 1990 New Zealand has been extolled as a "bi-cultural society". The meaning of the term "bi-culturalism" has been conveyed in expensive media images but left ill-defined in serious argument. Kawharu's (1989) edited collection, easily the most impressive of several recent volumes on the Treaty of Waitangi, focusses on somewhat different issues. Mulgan's (1989) discussion, although not aimed at an academic audience, makes a valuable contribution and the tendency to dismiss Vasil's (1988) work should be resisted, but much of the literature is disappointing. Spoonley (1989:99) in his "keywords" paper has little to say - unless "the distribution of resources between Maori and Pakeha, more or less equally" is regarded as a useful definition - and even Walker (1990) leaves the concept vague, although in such a generally thorough work one might have every reason to expect something more concrete. Perhaps the failure to introduce any precision into the term (which is, after all, charateristic of *political* argument) may concern only a few academic pedants, but if that is so I confess myself among them. The slogan obviously informs us that New Zealand is a society in which two main cultures, the Maori and the Pakeha, co-exist. Yet in what sense are we to understand the term "culture"? If "culture" is used in its ordinary sociological sense to mean the socially transmitted elements of a society - its language, history, art, and so on - then "bi-cultural society" can be given a more or less straightforward interpretation. There is the set of Maori cultural artifacts and practices produced by Maori people and the parallel set produced by Pakeha people. However, anthropologists generally use the term "culture" where sociologists prefer "society" and in that sense the term "bi-cultural society" would mean "bi-social society" or, since some re-phrasing seems necessary, a society composed of two principal sub-societies.

But there is more to this than pedantry. If New Zealand is understood as a "bi-cultural society" in the first, sociological, sense then attention is directed towards the analysis of Maori culture and Pakeha culture and, what is more, to culture conceived as a reified idealist abstraction. This gives rise to various problems of which only the most troublesome need be mentioned here. First,

Maori culture tends to be interpreted as an ethnic culture in a sense that cannot be applied without absurd distortions to "Pakeha culture" (the very concept of which is highly dubious) and, secondly, the existence of a distinctive New Zealand culture (in this sense as some set of practices characteristic of a social identity forged in New Zealand) is completely neglected. There is another difficulty, too. If the concept of "culture" is used in its limited sociological sense to refer to the culture of a society then the cultural productions of members of that group must be regarded as instances of its culture. It follows that a Maori rock band is producing Maori culture. While this might be acceptable to some cultural theorists it is probably less acceptable to those who prefer to recognise as Maori cultural productions only those which conform to established traditional standards whether they are produced by people who happen to be Maori or not. That question (which, being couched in the concepts of an idealist view of culture, is inadequately expressed) will be taken up later, but it is exactly this area which is contested in the debate about ethnic culture. In this paper I will attempt to resolve these and other difficulties by adopting a systems approach to society outlined by Bunge (1979). The article is an attempt to introduce and work with Bunge's framework, it provides an opportunity to make a plea for the sociological investigation of class and ethnically positioned communities in New Zealand, and (hence the "outburst") it allows certain professional frustrations affecting me as a sociologist of education to be expressed. I fear that this indulgence will affect its reception, since careless people will certainly regard me as a "new times" convert to the "new right"; but that is a calculated risk. They are wrong.

A Systems Theory Model of Society

Mario Bunge's work is well-known to professional philosophers but has so far made little impact on the wider scholarly community. In these circumstances it may be appropriate to offer a brief explanation of the use here of Bunge's conceptual framework which, after all, has been produced by a philosopher not a sociologist or anthropologist. In fact, the precise conceptual analysis of a philosophy supported by formal logic has been sorely needed in this area and Bunge is particularly well placed to provide it.[1] While his analysis of society

1 Bunge's systematic and materialist philosophy often filters through at second remove. A most useful recent book by Liston (1988), for example, cites the social systems theorist Sztompka (1974) as a major influence, and in Sztompka's highly technical work, we find acknowledged *his* important debt to Bunge. The general neglect of Bunge's work would make a story in itself. He was professor of theoretical physics at Buenos Aries and moved in the early 1960s to become foundation professor of the philosophy of science at McGill, Canada. He is thus something of an outsider. His preference to develop his own system with virtually no reference (and that usually not flattering) to other philosophers together

comprises but one chapter in a seven volume treatise devoted to the development of a fully integrated materialist philosophy, that analysis is securely grounded. Bunge offers a form of systems theory (or rather an analysis in terms of systems and their functions for he is at pains to distance himself from what is known as general systems theory) in some superficial respects reminiscent of Althusser's, (1977) whose political, economic and cultural spheres have received some attention; but Bunge's materialism is fundamentally different from Althusserian structuralism. His approach should also be distinguished from Archer's (1988) attempt to develop a systems model of culture and agency.[2] With these remarks his basic concepts may be outlined.

A society is a system composed of people bonded by social relations. All human societies have four sub-systems; a kinship system, an economic system, a political system and a cultural system. The kinship system is concerned with reproduction and is understood as fundamental. The remaining three components are regarded as overlapping spheres of practice. The economic system is concerned with the production of goods and services; the political system with regulation and management; and the cultural system with the production and transmission of knowledge. Each of these overlapping spheres can be divided into sectors, the economic system, for example, is readily

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with the mathematicised exposition of his work must also militate against its widespread acceptance. His uncompromising materialism and contempt for all forms of idealism does not recommend it to most contemporary intellectuals either. None of this, of course, detracts from Bunge's status as one of the world's leading philosophers.

2 Archer proposes an extremely extended and non-materialist interpretation of a cultural system: "At any time a Cultural System is constituted by the corpus of existing intelligibilia - by all things capable of being grasped, deciphered, understood or known by someone". (p. 104) She continues, "By definition the cultural intelligibilia form a system, for all items must be expressed in a common language (or be translatable in principle) since this is a precondition of their being intelligible". (p. 104) Not only is this view all-embracing but it is fundamentally idealist: "This ... stream of intelligibilia escape their progenitors and acquire autonomy as denizens of World Three, after which ... they act back on subsequent generations of people". (p. 144) Popper's World Three concept is specifically criticised by Bunge as inadequate and misleading. By contrast Bunge maintains "a culture is a concrete system composed of living rational beings engaged in activities of various kinds, some of which go far beyond the calls of biological need, and all of which are social because they draw from tradition and influence social behaviour". (p. 212) He insists that culture lives in the brains of those who care about it and who cultivate it and roundly declares that a book not read is just a lump of matter. To Bunge a cultural system is a concrete system and should not be conceptualised as a body of meanings, values, rules or, indeed, "intelligibilia". A cultural system is concrete because it is composed of concrete things interacting in concrete ways.

separated into sectors of production, distribution, marketing, and so on. Bunge recognises that modern nations are often super-systems composed of all the kinship, economic, cultural and political systems located in different regions and which co-exist and interact within a territory. Many modern nations are composed of several societies each of which contributes its own kinship, economic, political and cultural system to the whole. These systems are coupled to each other, with a degree of strength that varies, to form a national economy, a national cultural system and a national political system. To analyse New Zealand as being composed in this fashion of two main societies, the Maori and the Pakeha, may seem attractive, but I will argue that such an analysis would be unconvincing. On reflection it quickly becomes apparent that New Zealand can only be regarded as a single rather well integrated society rather than a national super-system like the USSR, federal Canada or the Balkan patchwork called Yugoslavia. It is not like Northern Ireland either. If New Zealand is theorised as a society composed of two principal sub-societies on this model then what might be thought the clear realities of a systematic economic, political and even cultural integration into a single society in this country are overlooked. It may be sensible to say that there are two peoples in New Zealand, each with its own kinship system, but there is only one national society. There is a single economy, a single political system and, in general, a single cultural system. These statements are not intended to be contentious. Some economic resources are in Maori collective ownership but it is impossible to speak of a Maori economy in contemporary New Zealand. A Maori political system makes a little more sense and, indeed, one can be recognised, but it is important to understand the extent to which the Maori political system is a sub-system of the New Zealand political system. Despite the renewed legislative activity of the last six years under a Labour administration the Maori apparatus of government is relatively weak and lacks any constitutional independence. There is no separate Maori judicial system and proposals for one have been rejected. When separate administrative systems do emerge spontaneously (as, for example, in the Department of Social Welfare) we have observed that they are quickly closed down. It is not sensible to think of Maori and Pakeha as separate societies each with parallel political systems which are both sub-systems of the New Zealand political system: there is no Pakeha political system just as there is no Pakeha economy. In the same way the New Zealand cultural system with its various sectors, education, scientific and technological research, the arts, and so on, is most adequately conceptualised as a single system. It strains common sense as well

as the framework of this analysis to speak, for example, of parallel Maori and Pakeha educational systems. To a considerable extent the Maori cultural system, in particular, must be recognised as being embedded within the general New Zealand system.

New Zealand should be regarded as a systems-integrated society composed largely of two peoples with distinct historical origins one of which, the Maori, continues to maintain institutions that constitute it as a socio-system, a partial society at least, but that there is no sensible parallel entity we can identify as Pakeha society except in an informal sense. Not only do many New Zealanders of European descent not recognise themselves as Pakeha (and often object to the term on grounds that would be respected if they were an ethnic minority) but, more importantly for sociological analysis, they rarely organise themselves as an ethnic community for economic or political purposes. The only major national exception to this (and it is anomalous) is the Crown in its status as signatory to the Treaty of Waitangi (Sharp, 1970). And when Pakeha organise for cultural purposes those are very limited and specifically ethnic purposes. Moreover, they organise not as "Pakeha", who cannot be regarded as an "ethnic" group in any serious sense, but as Scots, Dutch, Yugoslavs, and so on. It is significant that the various attempts to explore the notion of "Pakeha culture" generally employ a concept of culture quite at variance with that employed in discussions of Maori culture. Spoonley (1988:46), for example, asserts that Pakeha do not sound or act like or share the same values as Europeans, North Americans and Australians, and must be seen as "having their own way of living - or culture". If this quasi-anthropological concept of culture as a "way of life" (which is not at all the concept preferred by Mahuta and Ritchie (1988) in their discussion of "authentic" Maori culture, as will be shown) really was utilised in the study of the Maori community we might be less ignorant about contemporary Maori "ways of living" than we now are. New Zealand, then, should be recognised as a single society wherein live two relatively distinct peoples. This situation, which is by no means uncommon in the world, is the result in our case of the colonisation of New Zealand by Europeans and the subsequent destruction of Maori society in the full sense of a self-sufficient political, economic and cultural system. The society which eventually replaced the indigenous society, however, was a New Zealand society not a "settler society" a "Pakeha society" or a "European society".

Of course, it should be understood that the analysis is concerned with the *development* of New Zealand society. For this reason the question whether

New Zealand became an integrated society in 1840, 1852, 1867 or some other date is probably not one that can be determined by reference to precise sociological criteria. In the early days it certainly made sense to speak of "settler" society and Maori society because it is possible to delineate distinct settler and Maori kinship systems, economies, political organisations and cultures (institutions for the production and transmission of knowledge) which were for a time not at all coupled, and then for a period only loosely, by a central, integrated and integrating, national state apparatus and the growth of institutions within civil society which rejected the principle of ethnic separatism. New Zealand did not take the path of separate development, this is not New Caledonia, and as a result we now live in a country which should be recognised as a genuinely post-colonialist society.

From the very beginning of the new society in this country Maori were not fully excluded from the political system, the economic system or the cultural system. It goes without saying that the bases of their former independence as a society, particularly in the economic and political spheres and to only a slighter lesser extent in the cultural sphere, were systematically dismantled by the new system and that system was fundamentally modelled on European systems (the British system to be more precise), but it is nevertheless the case that the new society was a society of New Zealand citizens of both races. (Incidentally blanket objections to this word and its physicalist connotations, when we hear the terms "Caucasian" and "Polynesian" in crime reports every day, are unwarranted - I note that Walker (1990) is happy to refer to the Maori race). There are obvious political reasons why some Maori (and others) should wish to refer to New Zealand society in the past and today as "Pakeha society" but they are factually mistaken. A society is composed of people and New Zealand society always was and has increasingly become a society of Maori and Pakeha. This is not to deny that New Zealand law has not at times been framed overtly in the interests of the Pakeha majority (especially where the stake is land), but that does not affect the substantive conclusion. It is meaningful to speak of Maori society (if we remember that partial society is a more precise term) in a way that is not exactly paralleled by talk of Pakeha society because the Maori do maintain organisations which constitute them as a distinct community in New Zealand. It is perfectly possible to conceive of New Zealand as a Maori society (had the population ratios been reversed at the turn of the century every comparable case suggests that New Zealand would have been decolonised well before now), however, it does not by any means follow

that since New Zealand is not a Maori society it is a Pakeha society. Ever since 1840 New Zealand has been developed as a systems-integrated society of Maori and Pakeha, albeit one in which the specific interests of Maoridom (particularly its attempts to reconstruct its economic, political and cultural systems) have been suppressed under Pakeha hegemony. This must be acknowledged, but it is difficult to imagine how things could have been essentially different *given colonisation* which the Maori, had they been able, would undoubtedly have brought to an end well before they were outnumbered.[3] European settlers took control of this country but, at the same time, they tried - and with a fundamental degree of success - to create a society in which all citizens of the Crown would enjoy equal rights. That may not be what the Maori who signed the Treaty of Waitangi expected and it is certainly not compatible with some contemporary concepts of "partnership", but it should be recognised for the achievement it was.

Culture and "Ethnic Culture"

Based on its kinship system (iwi, hapu, whanau) the Maori community, although not constituted as a society in the full sense of a self-sufficient social organisation, exists as an almost fully integrated sub-system of New Zealand society. This kinship system is much weaker than it must once have been but it is far from dead. Although with no viable economic system the Maori political

3 I am not so bold as to offer myself as an apologist for colonisation. But I do argue that New Zealand should be recognised as a post-colonialist society and it is in that context that discussions about "indigenous peoples' rights" and "reparations" should be conducted. That citizens of one ethnic origin benefit uniquely from the collective ownership of ancestral property in a way that those of another origin does not seems symmetrical to many non-Maori. The extent to which such misgivings are derived from a concept of individual equality (for much the same reasons that many people favour heavy death duties), rather than based on ignorance and racism, may be underestimated. Heavy-handed "anti-racist" propaganda will do little to win acceptance for the organisational forms and structures appropriate to this society (Gilroy, 1990). Moreover, disinclination to accept uncritically every stance of the race relations industry does not constitute "racism" - the so-called "modern" definition of which ("power plus prejudice") is too absurd to be treated seriously, particularly when it is interpreted to mean that "Maori cannot be racist". In the theory of many academics who subscribe to this position, discourse itself is held to be a form of power, and it follows that nice observations to the 'European peasantry crawling disease-ridden off wooden tubs, together with other noxious animals' (letter in the *Dominion*, 6 October 1990), and such like, are derogatory and racist irrespective of the ethnicity of the writer. Being abused as 'noxious animals' is mild for the genre, and what many New Zealanders of European descent find most objectionable are not the increasingly frequent insults of this sort but the apparent willingness of race relations apologists to countenance them by means of a theory which disregards their character when, and only when, they are directed at the Pakeha.

system based on the Marae and about to be strengthened if the proposal to institute iwi authorities proceeds has a significant role as a sub-system of the New Zealand political system. The Maori cultural system is particularly evident. Maori is an official language (although its maintenance is poorly supported), the educational system has an expanding Maori sector and is especially well developed at pre-school level, Maori arts are extremely important and integrated into the national system of cultural production and conservation, and Maori cultural traditions for the ritual ordering of social relations are sustained and recognised. It is pointless to force a similar analysis of Pakeha society. The settlers brought their kinship system with them and their descendants continue to maintain it, their language is the principal language of the nation, but beyond that the effort to establish parallels breaks down.

There is no Pakeha cultural system, in education, in the arts, in science and technology, or in any other area of cultural production and dissemination because the national institutions developed in this country to serve those functions were always intended to meet the needs of all citizens. Thus, while it makes sense to regard Te Kohanga Reo as a Maori institution within the New Zealand educational system it does not make sense to regard Kindergarten and Playcentre as Pakeha institutions and not because they have between them many more Maori children than Te Kohanga Reo but because they are general institutions of New Zealand society. Mulgan (1989:123) has also realised this and, as he observes, mentioning Te Kohanga Reo, "[i]nstances of monoculturally Pakeha institutions are less easily identified." This is only partly because they would be condemned as racist. The point is that many areas of social analysis the opposition is not Maori institution / Pakeha institution but Maori institution / general New Zealand institution.[4] Of course, all Maori institutions are necessarily New Zealand institutions (leaving aside the Sydney Maori Club perhaps), but they are not general institutions of New Zealand society but specifically intended to promote the specific interests of one of the peoples of this country. It is perfectly understandable that Maori should form such institutions since the economic, political and cultural systems developed as national institutions by New Zealand society do not always serve

4 In this respect New Zealand is quite different from Israel, for example, a society permeated at all levels by separate Jewish and Palestinian institutions, indeed Israel is hardly a fully integrated society at all but a Jewish state in which co-exist, somewhat uneasily, the civil societies of two distinct peoples.

Maori interests as well as they serve Pakeha interests. This is not altogether surprising remembering New Zealand's colonial history but it is still not an adequate reason to regard all institutions not under Maori control as Pakeha institutions.

I propose, then, to regard Maori as a distinctive New Zealand people whose institutions constitute a socio-system which has the form of a partial society (and is a sub-system of New Zealand society) and to regard the Pakeha (by which term I understand the descendants of European settlers and recent immigrants from Europe) as members of a New Zealand society developed as an integrated society following the Treaty of Waitangi and, far more significantly, the 1852 Constitution Act which established representative democracy (and a large degree of self-government) in New Zealand. (McIntyre and Gardner, 1971) In this sociological analysis the term "Maori culture" refers to the sphere of formal cultural production and embraces Maori language, arts and crafts and music, Marae protocol, educational and training activities, and so on. The analysis also recognises kinship practices, Maori controlled economic activities (Maori fishing, tourist ventures, land operations, and so on), and Maori political organisations as institutions of the Maori socio-system. (In the standard usage of anthropology this entire constellation of institutions and practices is regarded as Maori culture.) More will be said about the informal, life-style, practices of Maori people.

Maori are the descendants of a people subjected to colonisation (and they had little choice in the matter) who have maintained certain social institutions and practices in the face of casual indifference or actual opposition from the settlers and their descendants. Many of these practices have, therefore, acquired an altogether new layer of meaning for Maori. They are no longer simply the way Maori life is lived but the signs by which Maori demonstrate to themselves and to the Pakeha that Maori life *is* lived. The maintenance of traditional ceremonial practices and the continued production and display of traditional (and therefore "authentic") Maori arts necessarily takes place in a transformed social context in which an element of self-conscious tradition-maintenance and identity-creation is ever-present. Such elements have become so important, in fact, that the concept of "ethnic culture" has been introduced in an attempt to reflect their character. The Maori renaissance (an appropriate name since like the great European renaissance we are witnessing the creation of new cultural forms and practices rather than the re-birth of dead ones) is

essentially and necessarily concerned with the reactivation, recultivation and regeneration of "authenticity" in Maori cultural practice. Hence, Mahuta and Ritchie (1988:30) define "authentic" Maori culture as: "the culture that has sustained Maori difference over time, made meaningful the persistence of the language, the social forms such as the tangi, the cultural manifestations in arts and oral literature and the sense that these things are ineradicably indigenous and will prevail." Incidentally, the first use of the word "culture" in this extract may mean "society" but it may be used in an idealist sense and mean that "Maori ideas and values" have sustained the language, social forms and arts: it is not surprising that there is so much confusion. "Ethnic culture", in any case, is a less than helpful term. If it means that set of organisations involved in the production of cultural performances and artifacts which contribute specifically to the maintenance and celebration of the collective identity of a people as a group with a certain origin and history then it may be accepted, but any suggestion that Maori culture is an ethnic culture as if there were no other elements to Maori culture or as if all Maori culture were necessarily the expression of an ethnic culture in that sense, is quite insupportable.

This error is very common. The term "ethnic group" generally denotes a community that identifies itself as such through beliefs about its origin and descent. In the nineteenth century "nation" had much the same meaning but nation has become more closely identified with the state (precisely because of the success of nationalism) and "ethnic group" is now more generally used to refer to national groups who have never achieved (or who have lost) statehood, or who have migrated (voluntarily or otherwise) at some stage of their history from their original homeland and now live within the boundaries of another national territory and perhaps under the hegemony of another national people. But confusions arise as a consequence of the idealist tendency to construct the object of social and anthropological study as "culture" rather than the organised practices of communities and social groups. This can plainly be seen in the definitions of culture offered by Metge (1976:9), "a system of symbols and meanings in terms of which a particular group of people make sense of their world, communicate with each other, and plan and live their lives" and Bullivant (1981:2) "a perceptual template ... inherited from one's group [with which one] makes sense of the world out there." Once culture is defined in this way then the task of the cultural anthropologist becomes that of examining the "system of symbols and meanings" and their "perceptual template" as the "culture of the group". It is inevitable given the multi-referent character of

"culture" that there will be some confusion about Maori culture, or rather the referent of the term "Maori culture". In the hands of a Metge or a Bullivant "ethnic culture" means that set of beliefs which construct and signify a collective identity based on origin. If "culture" is defined in idealist terms as "symbols and meanings" and if ethnicity is defined as possessing a set of symbols and meanings that confer collective identity of an appropriate kind then "ethnic culture" (indeed, the concept of culture itself) is hopelessly trivialised. It means that "Scottish culture" is understood as stories of Bannockburn and Prestonpans, Burns' night and bagpipes, Harry Lauder and the Loch Ness monster... This is absurd but as no one is likely to take it seriously at least it is harmless. When this sort of thinking infests the discussion of Maori culture, where for some reason this laundry list approach is taken seriously as, for example, in the Department of Education's (1988) statement on the school curriculum, real damage is likely to be done.

The argument that there is no Maori culture in New Zealand is quite commonly expressed (and not only by "rednecks", see Ritchie, 1963, for example), but that is not the view taken in this paper. The Maori cultural system is constituted by the set of organisations involved in the production of Maori cultural goods and services (arts and crafts, books, films, dramatic productions, educational knowledge, genealogies, and so on). Organisations can be regarded as part of the Maori cultural system if the goods and services they produce are concerned to reflect on and contribute to traditional or contemporary Maori life. In this system theory mode of analysis a television company thus becomes part of, or acts within, the Maori cultural system when it produces a drama that reflects on Maori life. A Maori rock band that reflects contemporary Maori life is also part of that cultural system. This makes it particularly clear that the organisations of cultural production in this country are highly integrated at the level of New Zealand society. It is important to realise that Bunge's analysis requires the rejection of the view that the set of goods and services produced by such organisations should be regarded as Maori culture itself. Culture is not a set of performances and artifacts but a living system of cultural producers. The cultural system produces cultural products not a reified "culture", in just the same way that the economic system, the economy, produces material goods and personal services not "economy". This analogy exposes very clearly how the popular tendency to reify culture rests on faulty conceptual foundations.

The concept of "lived culture" has been introduced by those working in the field of cultural studies (Clarke, Critcher and Johnson, 1980) in order to support the study of the informal practices of daily life in communities. If cultural anthropologists or sociologists influenced by cultural studies were to seek to learn about the contemporary way of life of Maori people, the way Maori live now in the day-to-day worlds of Otara and Porirua and Ruatoria, they would be interested in the ordinary practices and ideas and values of families in those communities. This usage presents no serious difficulties for the analytical framework employed in this paper since "lived culture" can readily be translated as "informal cultural practices" of a certain kind. Communities, understood as groups of people living together, engage in work and play, rear their children, care for their aged, all of which and more we can call practices, and the set of those practices can be regarded as their informal culture in a loose anthropological sense. Such ethnographers might be concerned with the practices of a community that seem to be central elements of its life-style (child-rearing, division of domestic labour, recreation, and so on) and with the ideas and habits of thought which support those practices. The family can be regarded as part of the informal cultural system (it produces socialised individuals) and certain of its practices can thus be regarded as cultural performances (teaching a child one's language is as much a cultural activity as writing a book). If we are interested in ideas and values (models of society, constructions of identity, concepts of morality, and so on) then these products of the informal cultural system can also be studied, not as the reified "culture" of the community, still less as the "mental" generators of behaviour, but just as a certain type of cultural product with, of course, their own influence on social practice.

There arises, however, a nice problem of theory. How can the nature of the cultural practices of a community be expressed by an appropriate concept? So-called lived cultures, as the distinctive way of life characteristic of a community or group, can be identified in purely nominal terms ("hippy culture", "bohemian culture"), in terms of the causal conditions which supposedly give rise to that way of life ("culture of poverty", "culture of oppression") or in terms which attempt to express its central psycho-social character ("respectable", "rough", "fatalistic"). Where an actual community demonstrates cultural practices that can be characterised, for example, as much by their distinctive relations to economic production as by the traditions of their ethnic group (such as those studied by Hohepa, 1970, and Kawharu,

1975) it is extremely difficult to determine whether the way of life produced by that community and recorded by the ethnographer should be categorised as the practices of community related by bonds of class (occupation, income and material conditions of life) or by bonds of ethnicity (origin, language and certain traditional practices). Where the general object of such research is with the psycho-social characteristics of the community's practices in the area of socialisation (the usual concern of educationally focussed research) the designation of those elements of its practice as being derived from ethnic relations or class relations is arbitrary and purely nominal. It was Ritchie's (1963) half understanding of this which misled him to suppose that in communities of the type he studied there was no Maori culture (in the "authentic" sense) but only a class culture (in the "life-style" sense) of "deprivation". The conceptual framework of this paper does not encourage the identification of the informal or "lived" cultural practices of Maori communities as "Maori culture" which has been given a distinct meaning as the cultural system developed by Maori within the New Zealand cultural system. That is not to say that the situation is entirely unproblematic, as I will show.

Cultural Change, "Acculturation" and Bi-culturalism

The origin of cultural practices is a fascinating topic and the study of cultural diffusion and acculturation is a major area of anthropological investigations. However, the attempt to trace the origin of the various practices of an ethnically identified community in order to determine whether or not they can be recognised as linked to its tradition for the purposes of assessing the degree to which that community is practicing its "own" culture is thoroughly misguided. Of course, the loss of certain cultural practices, the ability to speak a language, for example, is invariably a cause of concern and sorrow to those with deep feelings of attachment to those practices. We are all the losers when a language dies. But if a community, because of cultural contact, adopts and adapts new practices and abandons or transforms some of its old ones, those practices become its own cultural practices. A society that comes first to acquire competency in the language of another group and eventually to adopt that language largely in place of its original tongue makes that language its own.[5] But there is no need to be over-concerned with the question of whether

5 Under Milk Wood is no less a Welsh play and The Matriarch no less a Maori novel for being written in English. Needless to say, a society does not adopt the language of another society unless it is culturally dominated by that society, although the principal mechanism of language collapse is not so much the apparatus of cultural domination such as schools and newspapers but inter-marriage between monoglot speakers of the dominant language and bi-lingual speakers of the dominated language within a society

this or that practice of a community or a family should be recognised as a Maori practice or not. Since the (formal) Maori cultural system has been defined as constituted by the set of organisations involved in the production of Maori cultural goods and services there is no difficulty in extending that definition to the informal Maori cultural system which includes families and their sphere of cultural production (language usage, child-rearing practices, sexual division of labour, and so on) in a broad sense. If certain practices have been widely adopted by contemporary Maori families then those practices must be recognised as having become part of the repertoire of Maori social behaviour. This can hardly be denied since it is widely acknowledged that a great number of what are now regarded as central elements of Maori traditional practice have, in fact, been adopted and adapted by the Maori in the post-contact period. What is more contentious, but none the less real for that, is the fact that many family and community practices have been adopted, over many generations, specifically from contact with the Pakeha working class - and that is exactly why many Maori scholars (see Rangihau, 1975) are reluctant to accept them as part of the "authentic" Maori way of life or as Maori practices at all. One can sympathise with their difficulty but the force of the analysis (to say nothing of the actual behaviour of thousands of indisputably Maori families) is against them. The way a people live may not constitute their *culture*, but it is still their way of life. In the realm of politics disputes over what practices are to be recognised as Maori cultural practices in this informal sense, are determined by the fiat of Maori authority, as we saw in the Carrington Hospital dispute, but from a purely intellectual point of view this resolves nothing.[6] Webster (1989) has discussed the difficulties inherent in the "expert definition" of Maori culture in a most illuminating essay and one can only hope that his brave refusal to defer to the tenets of the new cultural ideology of "blood and soil", which is indeed pseudo-scientific and fundamentally racist in character (Neumann, 1942), will be followed more widely.

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which gives little or no institutional recognition to that language. Under such structural conditions many bi-lingual speakers come to see little point in making the effort to transmit their first language to their offspring.

6 A dispute in 1989 concerning the running of the Maori psychiatric unit at Carrington Hospital, Auckland, was investigated by a body including Hiwi Tauroa, a Maori elder and former race relations conciliator, which found that the practices of the unit, alleged to be Maori cultural practices by the head of the unit Titewhai Harawera, were not Maori cultural practices.

This is not the place for an extended discussion of "class culture". An influential body of work in cultural studies attempts to show how the "lived" cultural practices of working class communities is causally generated by specifically capitalist relations of production. It is not necessary to accept this thesis in order to recognise that studies of working class communities, or groups within working class communities, have made a valuable contribution to our understanding of ordinary life in contemporary society. In this country both class and ethnicity have a powerful influence on social opportunity. It is time to recognise, I suggest, certain characteristic practices of Anglo-Maori working class life and, in particular, of its young people.[7] It is well known that the great majority of employed Maori are occupied in manual occupations and that a substantial proportion of them are unskilled. It is equally well known that only a small minority speak Maori to any extent and it is likely that only a minority have any significant formal contact with the collective organisations of Maoridom (some data are given in Nash, Harker and Charters, 1990a). The family and community practices of the way of life, the "lived culture" of this group is treated with pointed neglect by contemporary sociologists and anthropologists who refuse to acknowledge either the concept of the Anglo-Maori working class or the reality of its practice (see Webster, 1989). It is left to drama and literature to reflect and comment on the nature of the "lived culture" of a what seems to be a large sector or fraction of the Anglo-Maori working class. Patricia Cooke (1990) thus reviews *Whatungarongaro*, a play about street kids by the Maori drama group He Ara Hou, as a "statement about New Zealand today" that "pulls no punches about the shortcomings of the Maori family it shows us" and Kathryn Rountree (1990) sees a "raw and convincing" dialect in Alan Duff's *Once Were Warriors*, a novel about "kids living in pub car-parks, in wrecked cars, in court, and dying from solvent abuse, in gang fights, in a house fire, [and] from suicide". Moreover, strenuous attempts are being made by agencies of various kinds (the concern of women's groups with male violence, alcohol advisory services with drunkenness, anti-tobacco campaigns targeting young Maori women, and so on) to interrupt

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7 "Anglo-Maori working class culture" refers to the sets of practices I understand as the informal culture of Anglo-Maori working class people. "Anglo-Maori" does not refer to the genealogical descent of individuals but simply applies a correct label - correct because it recognises in these practices acculturation to the structure of class relations in New Zealand - to the character of the informal cultural practices of working class Maori. Note that I do not refer to *the* Anglo-Maori working class sub-culture since it is possible that more than one more or less discrete set of practices, or "life-styles" might be described by a competent ethnographic study.

those elements of Anglo-Maori working class practices (practices not exclusive to this section of the population, not definitive of its way of life, but according to all observers except most sociologists nevertheless common within it) that are transparently detrimental to the political, educational and social progress of the whole Maori community. I do not suggest, of course, that the cultural practices of the Anglo-Maori working class should be analysed without the closest attention being paid to the structural conditions which gave rise to this class fraction, indeed, such an analysis is essential if we are to understand the character of its cultural productions. What I object to is the politically motivated denial that this class fraction with certain characteristic practices exists at all and the consequent inability of social scientists to recognise influences which, at this level, do contribute to success and failure at school.

For a decade teachers have been introduced to the concept of "Maori knowledge codes" (although no one knows exactly what qualities of what set of practices the term refers to) and led to believe that the major reason for the under-achievement of Maori pupils rests in the school system. It is has become commonplace for writers in this area to assert that educational failure is created by the schools and that no progress has been made in recent times. Kaâi-Oldman (1988), for example, reports that in 1960 0.5 percent of Maori and 3.78 percent of non-Maori reached sixth form and, in the teeth of evidence to the contrary given in her own article, nevertheless suggests that the gap is "intractable".[8] The same story is told in the press almost everyday. Thus, Jim Perry (1990), a primary school principal, asserts in a recent newspaper that since 1968 "there has been a marked and noticeable decline in the success rate of Maori children through the schooling system." The facts are that in 1989

8 These figures, 0.5 and 3.78, are given by Hunn (1960:24) as the 1958 percentages of the two "racial elements" in the 12-18 age group in Form VI. Walker (1980:213) misrepresents these data when he states that, "Only 0.5 per cent of Maori children reached the sixth form compared with 3.78 per cent of Pakehas." Kaâi-Oldman has reproduced Walker's error and also failed to realise that the number of pupils in the upper and lower sixth forms is not the same as (and is lower than) the number who "reached the sixth form". Moreover, the percentage of the 12-18 age group in Form VI is not the appropriate base for comparison with contemporary annual statistics. Harker (1970:143) calculates that in 1963 1.8 per cent of Maori and 8.6 per cent of non-Maori pupils reached the lower sixth, and comparisons between current sixth form enrolments and those of thirty years ago should use his figures. Actually, the percentage of pupils reaching *university* was greater than 3.78 percent of the year group in the late fifties and early sixties.

25.5 percent of Maori attained Sixth Form Certificate compared with 60.3 percent of non-Maori. Readers may be left to make the calculations for themselves but they will not find any evidence of a "widening gap"; there is room for improvement, to be sure, but that improvement will be based on an acceleration of existing trends which show Maori educational performance to be catching up with that of non-Maori. More than anyone in New Zealand I have drawn attention to the phenomenon of credential inflation, but even I do not suppose that pupils leaving school today with four years of secondary education actually know less than those who left a generation ago with only two (or occasionally three). As to the theory that Maori educational progress is retarded by school practices it can only be said that the evidence in favour of it is extremely thin, and that the evidence against it (the less than impressive performance of the established Maori integrated and private schools and the fact that socio-economic variables are more highly correlated with educational performance in the case of Maori than non-Maori) should at least suggest that it cannot be entirely sound. Mahuta and Ritchie (1988:31) state that Maori parents expect schools to teach their children the "skills of literacy, numeracy, and learning to learn" so that they are equipped "to search for knowledge, to enquire and master all the technology from pens and pencils, through books and libraries to electronic media." These writers then go on to liken the school to a factory (!) turning out shoddy goods (without any mention, needless to say, of the quality of the input - its their analogy, not mine). This doctrine is extremely naive and I challenge those who advance it to explain why it is that among Pakeha parents there is a gap of 5 or 6 IQ points between the five-year old children of the upper-middle class (doctors, lawyers, secondary teachers) and the lower-middle class (social workers, primary teachers, journalists). These data are reported by McGee and Silva (1982) and strongly indicate that the cognitive skills necessary to high performance (in the areas that Mahuta and Ritchie endorse) in the educational system are extremely sensitive to early socialisation, so sensitive that a difference of about a third of a standard deviation on an individually administered IQ test is apparent within the Pakeha middle class itself, and before their children have set foot in school. What is more, when their children do attend school they do not regard it as a "factory" either but closely monitor their children's progress and do not hesitate to use all the family resources, of income, literate knowledge and social contacts, to ensure that they maintain the level of attainment regarded by the middle class as satisfactory. Critics might also like to explain why it is that the way of life of the Anglo-Maori working class, which we have seen to be a matter of

concern in several specific respects, is supposed to have no effect whatsoever on the educational performance of its children. Serious research into why thousands of Maori children fail in school when they should succeed waits to be carried out and it is far from obvious that the new dogma helps to establish a climate in which such research can be carried out with an appropriate degree of scientific objectivity. Peters and Marshall's (1990:56) assertion, for example, that they "found no evidence for to sustain the view that peer pressure was a major factor in school learning" seems to be refuted by their own data for they quote (ibid., p. 54) a Maori student saying "[Its] hard staying at school, especially when you see your (younger) mates getting dole money ... and spending most of it on drugs and beer." Perhaps Peters and Marshall have a different concept of "peer pressure" from the rest of us. The circularity of their general argument, incidentally, has a disingenous appeal all of its own: the relative failure of Maori children in the educational system is clear evidence of institutional racism and because of this institutional racism many Maori children fail. Leaving aside academic courtesies, however, the propaganda in this area has become a disgrace and a great hindrance to the advancement of a reliable description and analysis of the full causes of Maori underachievement in the educational system. This is an urgent social and political issue and I am entirely conscious of the considerations which lead many Pakeha social scientists to adopt the position they do, but there are other views and those who believe it their duty to study social structures and social practices throughout New Zealand society with their own more formal traditions of evidence and logical rules may, in the long run, prove to be not only sounder scientists but sounder politicians in the struggle for social justice.

Too many social scientists in New Zealand have lost their professional nerve in an excess of cultural and epistemological relativism.[9] The sentiment that gives rise to this intellectual stance is a generous one, of course, and it is understandable that many intelligent and decent-minded young middle-class

9 The conventionally adopted relativism about knowledge is simply astonishing. It seems that we have forgotten the Nazi's celebration of "Aryan" science (although it as well for the world that they decided to learn nothing from "Jewish science" and so lost most of their nuclear physicists), and even the revelations of the Gulag have made no difference. Solzhenitsyn (1974:100) tells us that Soviet prosecutors, adopting 'the most flexible dialectics' and holding that 'it is never possible for mortal men to establish absolute truth' reached the conclusion that 'as evidence is always relative' there was little need to leave the office to seek any. Relativism about knowledge is always a doctrine that serves the interests of tyranny - large and small.

Pakeha suddenly confronted with the reality of their history and the glaring current social and racial inequalities in New Zealand should think themselves obliged to demonstrate the fullest respect for the Maori experience and "perspective" and dedicate their own efforts to the cause of anti-racism and bi-culturalism. This is not a response that should be criticised, but misgivings about "blame the victim" theories have inhibited a whole generation of young scholars from studying (or even adequately conceptualising) social processes of profound importance. The consequences of this subservient and in certain cases pusillanimous attitude deserve to be carefully assessed. Is it not possible that for every student who accepts the standard race-relations line there are two or three others who learn only to bite their tongues and, having being presented with no serious analysis whatsoever (and no one will convince me that this whole area is not an appalling intellectual muddle), merely revert to uneducated prejudice once freed from the institutional pressures of the academic environment? Indeed, might it not be the case that the very popularity of certain well-publicised national figures willing to articulate views contrary to the received wisdom of those practicing being Pakeha, arises to some extent from the fact that they have been left important truths to tell?[10] There is certainly evidence (Massey and Jesson, 1990) that so-called "Cultural Equity" courses are perceived as one-sided propaganda and resented by many students compelled to study them.

The question of who counts as Maori is no less contested than the question of what counts as a Maori cultural practice and is an even more taboo subject. Before the arrival of the Europeans Maori could have had little need to concern themselves with such an issue. But after some 200 years of inter-marriage between Maori and Pakeha the matter is no longer clear cut. The usual bureaucratic convention adopted in New Zealand is to determine ethnicity by self-ascription and this leads to the mistaken view that ethnic identity is a matter of individual choice. A number of individuals do, in fact, have this choice because they would be accepted as Maori by Maori and Pakeha by Pakeha, but ethnicity is nevertheless ascribed by the collective and can become a contested issue. In law Maori are those who can claim Maori descent. In practice Maori people seem content to recognise anyone as Maori provided (a) they are of Maori descent and (b) where the line of descent is weak they are

10 For the enlightenment of any readers who might peruse this article in decades to come I refer here especially to Sir Robert Jones (rich property developer) and Mr. Winston Peters (Minister of Maori Affairs).

able to demonstrate a significant degree of involvement with Maori organisations. Pakeha generally ascribe Maori ethnicity on the basis of physical characteristics and may be reluctant to allow claims to Maori ethnicity by people who are predominantly (and sometimes overwhelmingly) of European descent. Notwithstanding the letter of the law there can be little doubt that most Pakeha regard an individual of, say, 1/64 Maori descent[11] as Pakeha not Maori and should "positive discrimination" practices (especially quota places in higher education) ever come to privilege such people it is most unlikely that those practices would be accepted without protest. Perhaps we should not be so keen to dismiss evidence (Sowell, 1984) that "positive discrimination" in the form of quotas feeds public resentment and acts to favour individuals from minority communities who are already well resourced.

There is a great objection in the race relations literature to the "fractionalisation of Maori identity" and an attempt to declare such counting as pernicious. It is argued that people should be identified as "Maori" rather than "quarter Maori" or whatever, but that is not the point. The point is that there must be a sensible limit in practice to the degree of descent beyond which the

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11 Mitchell and Mitchell (1989) cite a case where this degree of descent (which means that a great, great grandparent was of one eighth Maori descent) is offered in all seriousness. The boys and girls in the Mitchells' sample of very able Maori children were (quite apart from being overwhelmingly from non-manual backgrounds) typically from homes where one parent was of full European descent and the other of half or three-quarters European descent. Their comment that "Some are not particularly Maori by upbringing, appearance or outlook" (p. 120) thus fails to astonish. What is astonishing is the failure in race-relations circles to understand why most Pakeha, wrongly no doubt, reject the right of such people to adopt the voice of the colonised and the oppressed. If someone partly of Maori descent identifies him- or herself as Maori then it is "racist", according to a certain body of opinion, for anyone (or maybe only for Pakeha) to question that. Yet ethnicity is ascribed by the collective and many New Zealanders of European descent suppose themselves to have as much right as any other people to decide who is "one of us" (although the issue generally arises only in the context of reservations about the "right to speak" which rarely reach the public arena). Ethnic politics, like all politics, is divisive and it makes necessary bureaucratic mechanisms for the resolution of contested claims to ethnic identity (for example, that implied in the statement "Mr X is not known to me as a Tuhoe" made by one Maori speaker to another during the course of a television debate). This is not a pleasant business. Nazi Germany began with ethnic certificates and progressed to more visible signs, while in South Africa racial classification was long determined by a particularly despised state office: the proposed iwi authorities are likely to be faced with a similar task. Consciousness of this inherently divisive aspect of ethnic politics is the principal reason, in fact, why many New Zealanders prefer to identify themselves as "New Zealanders" or "Kiwis" rather than as "Maori New Zealanders" or "Pakeha New Zealanders".

assertion of Maori identity becomes a mere affectation. The Maori community is based on kinship and descent and that being so the fraction of descent is not irrelevant, perhaps 1/8 is just acceptable but is 1/64? Pool and Sceats (1990:47) report that "there will only be as many [Maori] 0-4 year olds in 2011 as in 1981", but if the rate of inter-marriage between Maori and Pakeha continues at its current level (about 70 percent according to a North Island survey reported in Nash, Harker and Charters, 1990b) and all those of Maori descent are classified as Maori then most of the population will be Maori within a couple of generations: but most of them will also have about 15 European great-great-great grandparents. Perhaps such thinking informs Spoonley's (1990:48) assertion that "about one-third of those entering primary school are Maori."[12] But were this weak basis for self-identification to be accepted it would transform the entire structure of meaning now associated with Maori identity and have untold consequences for Maori organisations. Far from being irrelevant the high rates of inter-marriage and the "fractionalisation" of Maori identity is probably the dominant factor informing Pakeha public sentiment in matters to do with race relations in New Zealand. Metge (1970:111) declared: "In recent years, the proportion of Maori who have married non-Maoris has increased significantly. The families founded as a result cannot be regarded as Maori families." If that is still the received wisdom then more than two-thirds of the children who are regarded by themselves their families and their teachers as Maori children are not being brought up in Maori families: some urgent re-thinking is obviously necessary.

After all, what is "bi-culturalism"? In this analysis New Zealand is an integrated society in which two ethnic communities live and work together. If *that* is what "bi-cultural" means then New Zealand society has long been a bi-cultural society. But, of course, the principal social institutions of that society (political, economic and cultural) are almost invariably derived from the practice of the descendants of the colonists and settlers and not from the original people. So long as New Zealand remains a representative parliamentary democracy with an independent judicial system and a modern capitalist economy based on the division of labour and the application of

12 This figure is repeated so often in certain publications that it might well qualify for Big Lie status. According to the Department of Education (1988) there were 50056 five year olds at school in July 1987 of whom 9326 were Maori, so 18.6 per cent of those entering primary school are Maori: for reasons best known to himself Spoonley, who cites no source, has inflated the official figure by almost double.

scientific knowledge that will continue to be the case. Those to whom "bi-culturalism" means the development of separate civic institutions for the government and administration of Maori and Pakeha citizens need to state their aims with more precision and be more willing to consider the possibility that their opponents may be inspired by sentiments other than racism. There are many principled objections to the extension of the doctrine of separate development (not the least of which is the essentially non-democratic character of appointed Maori authorities which has the consequence that it is impossible to determine what proportion of the Maori community actually shares the views and endorses the policies of its representatives) and these should be allowed recognition. This is not only a matter for political concern (is "tino rangatiratanga" an essential component of "bi-culturalism"?) but raises fundamental sociological issues. The thrust of social relations since the collapse of feudalism has been away from traditional, status assigned, authority towards modern, status achieved, authority. How tribal authorities can be expected to exert social and political control over dispersed populations which must always have the option of declining to recognise them and which (even in the current class structuring of the Maori community) have access to an alternative status hierarchy, has not been adequately discussed. The entire basis of traditional Maori authority will come under enormous strain once a sizable middle class emerges to replace the current elite structures. This is to say nothing of the impact modern thinking (and do not the *avant garde* talk of *post-modern*?) may be expected to have, and has already had, on traditional conceptual patterns and structures of thought. There are already people who think that professional counselling (pay-by-the-hour "caring") can be linked to a pre-modern concept like "aroha".

Taking the narrower interpretation of culture as the production and transmission of knowledge specifically concerned with the ethnic traditions and concerns of a social group then the advocacy of "bi-culturalism" might signify that New Zealand society should give equal weight and respect to the culture of Maori and Pakeha. I have no criticisms to make of that position other than to reiterate an earlier point that the concepts of Maori and Pakeha "ethnic culture" in this sense are not exactly parallel. A great deal of what is legitimated under the rubric of "bi-culturalism" in the educational curriculum, the history of Maori and Pakeha in New Zealand (before and after contact), the study and practice of New Zealand arts and crafts which must obviously recognise the Maori tradition, the local literature, and so on, should really need

no such legitimation since these areas are quite clearly components of a general New Zealand culture. To the extent that "bi-culturalism" (no matter how inadequately defined) has encouraged these highly desirable developments it has contributed positively to New Zealand society. At least ten percent of all Maori pupils are now being educated in bi-lingual schools, classes or units and this may be justified in terms of the ordinary civic right of a social group to educate its children in its preferred language. If this initiative, which has been pressed on the educational system by the Maori community, leads to increased educational equality there will be even more reason to welcome it. The second half of Hirsch and Scott's (1988) collection contains some impressive and heartening reports of successful educational practice and it is difficult to believe that it rests on the worthless theory presented in the first half. There are always grounds to interrogate vague and ambiguous concepts and the rhetoric of "bi-culturalism" should not be exempted out of some suspicion that political and educational expediency might thereby be served better. I will not assume that the tendency of some Pakeha to adopt random practices from what they suppose to be the Maori tradition and arbitrary lexical items from the Maori language (presumably as symbolic demonstrations of their personal commitment to a "bi-cultural" society in some sense) is in any way definitive of what "bi-culturalism" means. There can be few more certain ways for Pakeha to strip a cultural practice of its traditional positioning and dignity than to transform "Knees Up Mother Brown" into a "waiata". Sociologists with a desire to know exactly what sort of society "bi-cultural" New Zealand is becoming have no shortage of material and no shortage of work before them.

Conclusion

This paper will undoubtedly be regarded as provocative (but for every restrained outburst of this kind in response to muddled propaganda there are hundreds of others of another kind altogether) and if it offends certain taken-for-granted susceptibilities (and I shall be disappointed if it does not), then so much the better if that is what it takes to generate an academic debate on realist foundations about the nature of contemporary New Zealand society. The analysis of these fundamental concepts of our discipline, "society" and "culture" is so neglected that I make no excuse for this essay in the application of Bunge's systematic approach to the examination of our social system. There is a tiresome amount of rhetoric in this field which will not stand up to five minutes serious analysis and those responsible for it should be challenged: I

have challenged a few of them. "Race relations" is not my academic speciality (and so I need not apologise for my incomplete knowledge of specialist literature), but the sociology of education is, and in that area it is becoming difficult to carry out research and even more difficult to teach in the climate of politically motivated half-truths encouraged by academics in the field of race-relations who ought to know better. New Zealand is a highly integrated society composed (in the main) of two peoples with distinct origins neither of whom should be regarded as being organised as a separate society in the full sense of the term. The Maori form a socio-system, which has some of the features of a full society, but the Pakeha do not, except in respect of their kinship system and some limited and informal cultural practices.

Two hundred years ago there were, indeed, in this land distinct European and Maori societies in which were practised very different ways of life and it was sensible then to regard "literacy, numeracy", "pens and pencils" and "books and libraries" as distinctive practices of a transplanted European cultural system but it is not sensible now (indeed, even to hint that this might still be the case to a discernible extent is to invite the charge of racism). When individuals moved, more or less permanently, from one society to another they were readily identifiable (as "Pakeha Maori" in the case of Europeans), but that term has not been used in popular speech this century (not in the same sense, anyway). To affect that this state of affairs still exists is to deny the reality of an integrated New Zealand society. Societies change. What was so is no longer so. When teachers in New Zealand require their Sixth Form students to study *Macbeth* they are not working within a "Pakeha cultural system" to transmit "Pakeha knowledge"; they are working within an institution of the New Zealand cultural system to transmit knowledge within the tradition of English literature and the study of English literature is part of being educated in a language which is, needless to say, a New Zealand language - and the major one at that. But on the other hand, when a school teaches Maori language it may be regarded as part of the Maori cultural system in that respect. This lack of symmetry has been noted before and rests in the fact that while there can be recognised a traditional "authentic" Maori culture, in the sense that Mahuta and Ritchie propose, the attempt to construct a "Pakeha" equivalent simply cannot be sustained (it is possible to speak of "high culture", and that might be the right parallel in some respects, but even then it cannot be denied that opera, to select an obviously telling example, is part of *New Zealand* "high culture" rather than *Pakeha* "high culture"). Certainly, the

educational system transmits the knowledge and its associated technology (Mahuta and Ritchie's "literacy, numeracy", "pens and pencils", "books and libraries") that the Europeans brought to New Zealand, taught to the indigenous people in the only way they knew how (they are not my ancestors those people but if they were I would honour them), and have continued to develop with them ever since but, for the last time, no one should want to refer to that knowledge (in idealist usage) or the practices that generate it (in materialist usage), as "Pakeha" culture.

Cultural production in New Zealand should be analysed with regard to the organisations of our integrated society and recognition given to sub-systems of the Maori socio-system. If and when ethnographic attention is paid to communities identifiable by their class location and ethnic origin their informal cultural practices, their way of life, should be analysed within an adequate socio-cultural framework. The purpose of philosophy, Marx said, should be to change the world: but the conclusion he drew from that was not to abandon philosophy for propaganda but to practice philosophy (and economics and history and sociology) with more precision and rigour.

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Postmodernism and the Death of Vinyl : Reflections on the Sociology of Rock

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Academic analysis of popular music and its associated manifestations was initially slow to develop. Even the increasingly popular field of media studies tended to concentrate its attention on the visual media, particularly television, and neglected popular music. True, there were notable exceptions, with the work of Simon Frith particularly prominent. (Frith, 1978, 1983; see also Gillet, 1983). Now, however, there is a veritable flood of material, as well as a marked increase in the number of courses either directly on popular music, or on it as an aspect of popular culture, media studies, etc. The new prominence of the sociology of rock reflects recognition of rock/pop music's centrality as a global cultural phenomenon, associated with a multi-billion dollar industry, and a many faceted pop-youth culture reaching out into every aspect of style. This emerging literary explosion takes a number of forms and approaches the topic from a range of perspectives, including political economy, cultural studies, feminist studies, and media studies, this last with its own rich theoretical mix of film theory, psychoanalysis, feminism, literary theory etc.

The intention of the article is to introduce and critically overview this recent writing on popular music, with particular regard to its topics of study, its methodological and theoretical preferences, and the issues and arguments it has generated. This general discussion provides a basis for consideration of the sparse New Zealand writing in the area, and some suggestions for further work in this country.

Texts, critics, and cultural capital

The starting point of much media analysis, and that adopted here, is the study of texts. In the case of pop music this is usually fairly ephemeral in nature, through the review columns of the music press, newspapers and magazines. Such columnists function as important arbiters of taste. Rock critics construct their own version of the traditional high-low culture split, usually around notions of artistic integrity, authenticity, and the nature of commercialism. The best of such critics - and their associated magazines - have published

collections of their reviews. Frequently, as with **The Rolling Stone Record Guide**, and **Christgau's Guide to Rock Albums of the Seventies**, these become bibles in the field, establishing dominant orthodoxies as to the relative value of various styles (genres) and pantheons of artists.

A major contribution to this group of studies is American critic Dave Marsh's **The Heart of Rock and Soul, The 1001 Greatest Singles Ever Made** (1989). Previously there was a dominant tendency to accord value to albums as opposed to singles. But as Marsh observes, singles are the essence of rock and roll. After all, no one goes around humming albums, and while the singles market is no longer the primary one for the music industry (a point we shall return to later), it remains the dominant feature of radio airplay lists. Accordingly, in most cases "record production, promotion, and marketing is entirely determined by the search for and exploitation of potential hit singles". (Marsh, 1989 : xii). Utilizing singles as the frame of reference for rock history also alters the relative value of particular styles of music. Suddenly album-oriented "progressive" music (non Rhythm and Blues), which has produced few hit singles yet dominates critical discussion, is pushed to one side.

Yet Marsh's approach has its own heroes and villains. For him, the sixties are indisputably the most creative period of rock and roll - just under half of the singles included are from that decade. Amongst his top choices, it is not until number 24 (Springsteen's Born to Run) that we see a post 1970 single, The antipathy to punk is clear, though several postpunk "New Wave" performers are prominent. (Elvis Costello has 5 entries). There is also a predilection for American performers; English bands with considerable singles commercial success, such as the Troggs and the Small Faces, do not rate a mention. Indeed, it is a fascinating exercise in trivia to compare the American chart rankings of Marsh's selections with their English equivalents, (see Gambacinni et. al., 1987), an exercise which shows the commercial failure of many of Marsh's selections on the other side of the Atlantic. (the exercise works equally well in reverse).

A similar gatekeeping role is played by the leading histories of popular music. Preeminent among these is the Rolling Stone History of Rock and Roll, **Rock of Ages** (Ward et al, 1986). Here there is both a nationalistic flavour and some aesthetic discrimination at work. Only British bands/performers who had an impact on the United States charts are mentioned, though occasionally there is

a gesture towards those who, despite their lack of sales success, influenced American styles (e.g. the Searchers). This is a history of rock and roll as it happened in America. Furthermore, there is a high-low culture distinction at work within the idiom itself. There is a great deal on bands seen as avant guarde, such as the Fugs and the Quicksilver Messenger Service (who? Read the book!), but Paul Revere and the Raiders (Just Like Me. Kicks, Hungry) and Tommy James and the Shondells (Hanky Panky, Crimson and Clover, I Think We're Alone Now) barely rate a mention; instead they are dismissed as "teeny bopper", juvenile fodder, unfit to join the pantheon of "authentic" performers. Yet which of these groups still get airplay, and sell respectable amounts of "greatest hits" packages.? Are all their admirers duped consumers, or does their music have a timeless quality which the Rolling Stone writers, in their reach for a high art approach to rock, have somehow overlooked ?

All this is to simply make the point that rock critics, like Marsh, and their histories of rock, are playing a key role in defining the reference points, the highs and lows in the development of rock. They imbue particular products with meaning and value, and even their internecine arguments strengthen an artist's or record's claim to being part of a selective tradition. The consumers of rock music themselves frequently reflect (even if only to reject) such categorizations. The above paragraph is more than an arcane reflection on the two volumes, it also serves to demonstrate my own cultural capital in the field. As Bourdieu has observed, "nothing more clearly affirms one's class, nothing more infallibly classifies, than tastes in music." (1986 :18). There is a history of studies exploring the relationship between youth's tastes in music and factors such as class background, ethnicity, gender, location, and attitudes towards school. (Roe, 1983; Shepherd, 1986; Roe, 1990).

Trondman (1990) has usefully combined such an approach with an examination of how rock functions as a form of cultural capital. Following Bourdieu, he sees taste as "both conceived and maintained in the symbolic struggle between the classes" (p.71), with music a crucial dimension in creating a sense of social distance between various groups of youth in Sweden. Rock is regarded, in common with many of the critics, as split between two major genres : a mature "artistic" rock, and a commercial "idol rock". In such a distinction, one form of rock - the mature - is identified with what Bourdieu refers to as legitimate culture, while the other expresses distance from legitimate culture. The adherents of the former are, logically, found primarily

among university students and graduates, people who have good prospects of becoming part of the legitimate culture, and there is an emphasis on music that satisfies demands of "intellectuality", "aesthetic appeal", and "association with tradition". Trondham teases out the more precise functioning of this core distinction, utilizing data from a large-scale Swedish survey. He concludes that :

> those who have an avid interest in the "right" kind of rock can develop their taste into a "learned discourse" or "scholastic jargon" in the periphery of legitimate culture. For some, this form of assimilation can offer a port of entry into legitimate culture and preparatory schooling in the tradition of assimilation. (p.81)

This involves a process whereby the individual, in acquiring a taste for particular artists, both discovers the "history" and assimilates a selective tradition. He or she is then able to knowledgeably discuss artists, records, styles, trends, recording companies, literature etc. Of course, this can occur with music which is not part of the selected tradition. In this case, it nevertheless serves a similar function, to distance its adherents from that tradition, and to assert their own, oppositional stance; this is the pattern with many youth subcultures, though it must also be recognized that these styles are frequently coopted by the "mainstream". (Hebdige, 1979; Roe, 1983).

Political Economy : Music as a Business

Popular music, like other mass consumed forms of contemporary culture, is produced by large scale commercial enterprises for a mass audience. As most analysts have appreciated, this situation means that consumer tastes are shaped through the production and promotion of particular artists and styles rather than others. Sanjek has contributed a massive empirically rich study to what is an honourable tradition of work in this vein. (Denisoff, 1986; Frith, 1983) The third volume (Sanjek, 1988) deals with the period from 1900 to 1984; its coverage includes the effects of technological changes, shifting business practices (e.g. discounting, returns, and rack sales), radio airplay policies, music publishing, licensing arrangements, and tie-ins with film and television. The volume is a mine of information :

> Michael Jackson's cross-over LP "Thriller" was already the best selling solo album in history, with world-wide sales of 25 million so far. It provided CBS records with $120 million in a twelve month period and lifted its net income by one quarter over 1982, to $187 million. Out of royalties of two dollars from "Thriller's five dollar wholesale price, Jackson invested $1.2 million in the

> production of a fourteen minute videocassette of the LP, 300,000 copies of which were sold at $29.98 each in the first three months. He then went on to work on a one hour documentary, The Making of Thriller. It, too, sold for $29.98, and was available for bidding by the television networks. It was anticipated that Michael Jackson Inc., a holding company, would pay its board chairman in excess of $50 million by the end of 1984. (Sanjek, 1988 : 647)

Awesome as such statistics are, they of course tell us nothing about the *nature* of Michael Jackson's appeal. But that is not Sanjek's brief. What he does provide is a comprehensive and seemingly exhaustive, chronological account of the way the American music industry works. Others can build on this information in their discussions of radio airplay policies (Barnes, 1988), their analysis of the Thriller video (Mercer, 1988), and so on.

In incorporating such knowledge of the industry into sociological analysis of it, Frith's work (1983) remains central. It is now usefully complimented by Fink (1989), who provides similar material to Sanjek, but with a more contemporary focus, and by Wallis and Malm (1990), who provide an overview of global developments. In particular, they note "a growing antagonism between the globalization of culture on one hand, and the attempt to preserve cultural identity and protect national needs on the other." (p.16). Their on-going study has particular relevance to small countries like New Zealand , where local artists and music industries face an uncertain future, with both threats and opportunities present in the continued internationalization of pop/rock music.

Postmodern Pop

There is a thesis currently popular with academic commentators on pop/rock music that sees such music, once associated with youthful rebellion and political activism, as now thoroughly commercialized and incorporated into the postmodernist capitalist order, What was once revolt has become style. In his collected essays, **Music For Pleasure**, (1988), and in his contribution to his edited volume **Facing The Music** (Frith, 1988a), Simon Frith suggests the arrival of a curious entity, postmodernist pop, a value free zone where aesthetic judgements are outweighed by whether a band can get its video on MTV and its picture in **Smash Hits**. One reviewer suggested that even this bleak analysis was insufficient : "What is missing - except in the closing essay on MTV - is a sense of the sheer scale and ruthlessness of today's conglomerates, their marketing techniques concentrated upon a vulnerable teenage market." (Davis, 1988). Like Frith, Dave Hill's analysis of a number of major contemporary pop stars, **Designer Boys and Material Girls**, echoes Adorno's critique of

popular music as manufactured mass culture. Appropriately subtitled Manufacturing the 1980's Pop Dream, Hill's study of performers such as Michael Jackson, Duran Duran, Wham, and Madonna, argues that "this fresh species of genuinely talented practitioners are ready and willing to manufacture themselves" (his emphasis), and that "never before have commerce and creativity so happily held hands." (Hill, 1986 : 9). While both Frith and Hill concede that rock has always been about more than just music, they argue that it has never been more so than it is today, increasingly subverted to the services of commercialism.

This pessimistic view of rock absorbed into a postmodernist aesthetic is based on several, interrelated arguments : the decline of the youth market; the subversion of pop/rock music into a broader media spectrum, including the new music/fashion press, advertising and MTV; and the impact of new technologies, most notably the death of vinyl and the market emergence of the compact disc. Taken together, these are seen as invalidating most of the assumptions upon which rock culture has previously rested :

> Commercial popular music no longer depends on the sale of records; it can no longer be understood in terms of a fixed sound object; it is no longer made in terms of a particular sort of audience, rebellious youth. In short, the rock system of music making no longer determines industry activity. (Frith, 1988a :129).

Let us unpick these arguments. The decline of the youth market can be seen in the demographics of the eighties. In July of 1983 the number of Americans over the age of 65 surpassed the number of teenagers. This 65 plus group will continue to grow, and the United States Census Bureau projects that by the turn of the century it will account for one seventh of the population. At the same time, the post war baby boomers are growing up . About a third of all Americans - some 76 million people - were born between 1946 and 1964. This generational group, seen by demographers as a pig moving through a python, has dominated American culture for the past four decades: "At each stage of their lives, the needs and desires of the baby boomers have become the dominant concerns of American business and popular culture. "(Dychtwald, 1989 : 18). These trends mean a concomitant decline in the importance of youth as a social and political force and a decline in their importance as a market target group. (Similar demographic trends are evident in New Zealand.) Frith observes that this means that the traditional rock consumer - the rebellious teenager - is no longer central to the music industry : "the 1980's

marked the rise of yuppie rock, in which the ideology of teen is translated into an affluent adult life style." (Frith, 1988a : 127).

This shift in market composition is reflected in the contemporary recycling of nostalgia, with radio stations devoted to "golden oldies", and the ubiquity of classic songs in advertising, (for example, "I Heard It Through The Grapevine" to promote Levi jeans). For the flagging record industry, it means a plethora of 1960's reissues, compilations and remastered tracks for the bourgeoning compact disc market. The notion of "golden oldie" itself is very much a constructed one (with the gatekeeper critics discussed earlier playing a central part), worthy of further investigation.

But are these trends really so negative for the contemporary state of rock music ? While it is perhaps true that for older listeners the consumption of music is a largely private affair, (with what that implies for the vitality of live performances, concerts and the club scene, the traditional route into success in the music industry), it does not mean that these older consumers have no interest in new styles of pop music. Rather, they represent a maturing of taste, not in the sense of greater sophistication but through their ability to relate to the tradition of pop music. The yuppies represent, therefore, a new market segment for the industry, one which compliments rather than replaces the old teenage market. The original consumers have merely aged. Critic Marsh observes that, for him, "rock and soul based music has become more sustaining, not less, as I've aged. That there are parts of rock that appeal most readily to teens is undeniable, as is the fact that some of the best of it quite consciously aimed at that audience from the start. But I did not die before I got old and neither did the music..." (Marsh, 1989 : xxiii. The last comment is a reference to a line from the Who's "My Generation" - "hope I die before I get old."). Amen to that. The point is that while historically rock was associated with young people, who were the first to appreciate that it had a serious message to convey, the appeal and message of the music go beyond youth per se. It is also a mistake to emphasise, as many rock critics do,the rebellious aspect of rock. Reliance on that alone would have seen the music long-relegated to the status of a passing fad, a point we shall return to.

The view of the subversion of pop/rock to the imperatives of business/commercialism ignores the point that rock music has become a hugely diverse art form. Alongside the evolution of commercial "mainstream"

rock has been the parallel growth of "alternative" labels (such as Slash, Alternative Tentacles) and artists. Student radio stations have played a key role in promoting these, while a number of groups/performers have "crossed over" to achieve commercial success (for example, REM, Sinead O'Connor).

The decline of rock's traditional youth market is insufficient to signal the death of rock, but, the critics argue, in conjunction with other developments threaten its vital signs. Prominent among such developments are rock's increasing association with advertising. The use of classic songs as commercial backing tracks to sell a huge variety of products has become highly organized and has assumed a taken for granted status. An advert. in a British trade journal proudly noted : "SBK Songs now have the rights to the entire CBS songs catalogue. That's over 150,000 titles available for use in advertising. Anything from Lennon and McCartney to the Wombles. There is no middleman. We work directly with your agency. " (Cited Frith, 1988a : 89).

MTV : the Viper in the Nest ?

The ubiquity of music video is now a major cultural phenomenon, influencing feature films, television shows, advertising and the very nature of the music industry. The "MTV" television channel is Big Business, earning enormous sums from advertising, and spawning many imitators - for example, Canada's "Much Music" and Europe's "Music Box". In 1986 MTV reached nearly 20 million American homes and was watched regularly by 85% of 18 to 34 year olds. (Kaplan, 1987). There is a well-proven correlation between heavy rotation on MTV and increased record sales. Music video is dear to the heart of the postmodernist cultural critics, enamoured with its blurring of fantasy and reality, its collapse of the artistic into the commercial. (Goodwin, 1987). Music critics are rather less enthused : "Never before have commerce and creativity so happily held hands. The most ubiquitous, expensive and absolutely crucial example is the promotional video." (Hill, 1986:9). While music videos can be considered as pioneers of video expression, it is the visuals which dominate, and the music arguably becomes the sound track to an essentially visual experience. (Aufderheide, 1986; Shuker, 1989). For performers, this reinforces the increasing preoccupation with visual style.

The Music Press : Gatekeeping and Lifestyle

Pop/rock magazines don't simply examine and promote the music. As our initial discussion indicated, in the process they contribute to the delineation of

what counts as taste in the field, constructing audiences/consumers. Increasingly, they are also purveyors of style. Those aimed at younger people - the fanzines for "teenyboppers" - promote vicarious identification with the pop stars. By exposing the stars as people (at least in the official version of their personnas), they make them accessible to adolescent fantasies. This is the culture of the bedroom (McRobbie and Garber, 1976), replete with pin ups. At another level are the older, established rock magazines - New Musical Express, Melody Maker, and Rolling Stone -still emphasising a tradition of critical rock journalism in which their writers act as the gatekeepers for the tradition. They are, however, losing ground in the magazine wars to the new kids on the block :

> The enormous space in magazines like The Face, Elle, and Blitz given over to images and illustrations means that the printed word is pushed to the sidelines. There are few sustained reviews or critiques. (of current rock culture or its music). A grounded analysis of art and design objects is abandoned in favour of a celebration of them in terms of lifestyle and consumerism...You are what you buy or aspire to buy. (McRobbie, 1988 : xiv).

There are no sustained studies of the music press. Frith (1983; 1988) and McRobbie's edited volume (McRobbie, 1988) include some useful analysis, while i-D magazines encyclopadia of the '80's (Godfrey, 1990) presents an insider's appraisal, in the process reflecting the very point McRobbie is making above. Such insightful decoding of the magazines needs to be tested with the views of the consumers themselves, particularly the social functions they serve for their varied readership, and the segmented nature of that readership.

Technology and the Death of Vinyl

The continued rise in the market share for the compact disc, along with the continued expansion of cassette tape sales (including the tape single), threaten to relegate vinyl - the LP and the 45 - to the status of quaint historical artifacts. Not only are the record companies availing themselves of the availability of new (and arguably superior) technologies, they are promoting these new technologies to offset a decline in profitability during the 1980's. (see Frith, 1988a : 92-3; Sanjek, 1988 : chapter 37; Fink, 1989 : 23-6). The confirmed record purchaser can be targeted to upgrade (the word itself is a marketing ploy) their audio equipment, and, while they are at it, replace those old tapes and records with compact disc rereleases. Much of this has been at the expense of the traditional format for rock music - the single (45)- which had already

taken a battering in the early 80's. In 1986, in Britain for the first time since 1974, not one single release went "platinum" (sold a million copies), and single sales were at their lowest since 1977. Since the single's traditional rationale has been partly as a form of promotion and market research, a tool for building up an artist's visibility and promoting their subsequent album, it would appear that the single is no longer pre-eminent in creating audiences. (Frith, 1988a : 99). This, argues Frith, lessens the record companies historical dependence and responsiveness to the youth market. This, however, is to too readily assume that the youth market are not majors purchasers of the new formats.

Commerce, Style, and Auteurs

The picture of an authentic rock culture subverted by commercialism is too simplistic. Rock has always been about two things : style, the links with subcultures, advertising, fashion and the avant guarde; and autuers working within a business - the creative tension between art and commerce. This postulates a Gramscian view, with culture as a site of struggle, in place of the deterministic postmodernist analysis currently being advanced by Frith et al..

Underpinning the sense of a present crisis in rock culture as it is subsumed by postmodernism, is a particular view of that culture's past. While writers such as Frith are obviously conscious that the music business has always been just that, a business, their analysis ultimately depends on a romantic version of the history of rock. The scenario is that rock was born closely associated with rebellion in the 50's, a connection that resurfaces from time to time (the Woodstock generation and the counterculture of the late 1960's; punk); generally, however, rock has been incorporated - homogenized, constrained, and commercialized. The older rock critics - Marsh, Christgau, Frith, Marcus - obviously feel a sense of loss. For them, contemporary rock has lost its importance as a cultural force, with the decline of the traditional youth market and its collapse into style. Certainly, as our previous discussion shows, this view is not without substance, but, as has already been indicated, it has serious flaws.

The rebellious teenager has never been rock's central market figure, but only one part of what has always been a highly segmented - and frequently musically conservative - youth audience. In 1969, the year of the Woodstock Festival and the high point of the counterculture music scene (the San

Francisco sound, etc), the biggest selling single in the USA was the Archies "Sugar, Sugar", a classic "bubblegum" record with an insidious hook. The very label "bubblegum", of course, suggests the music's disposable quality and is indicative of serious critic's hostility to it. To echo our earlier discussion of Marsh's singles compendium, while such sixties critics promulgated a belief in rock as the cutting edge of popular/youth culture, epitomized by the serious auteur, the charts and popular consumption show a very different picture.

The on-going tension between art and commerce in popular music is evident in the very terms "Pop" and "rock". I have used them somewhat interchangeably here, regarding both as commercially produced music for simultaneous consumption by a mass market. But beyond similarities of production and consumption, aesthetic distinctions are generally attached to the two labels. "Pop implies a very different set of values to rock. Pop makes no bones about being mainstream. It accepts and embraces the requirement to be instantly pleasing and to make a pretty picture of itself. Rock on the other hand, liked to think it was somehow more profound, non-conformist, self-directed and intelligent." (Hill, 1986 : 8). One aspect of the postmodern view I do accept is that this distinction is no longer valid; I would also argue, however, that it never really was to any significant extent. As much as anything else, "rock" has been a marketing device. Even rock's frequent refusal to admit to commodity status, and its attempt to position itself as somehow above the manufacturing process, all too easily become marketing ploys - "the Revolution is on CBS" slogan of the late sixties being perhaps the best example.

Similarly the notion of authenticity. A recent compilation, "The Cream of Eric Clapton" portrays the guitarist as creating "music with which he feels comfortable, to the left of mainstream pop"; and refers to "his honesty to pure music", for which he has "made no compromise". All well and good, but while establishing Eric's cultural credentials as an authentic artist, above purely commercial considerations, this pitch is serving to define the guitarist's target audience, indeed, to construct that audience: those who feel comfortable (like Eric) with Clapton's auteurist style, and who can achieve emotional and intellectual satisfaction in their position vis-a-vis this artist's music. Authenticity is a marketable commodity.

This is not to deny the validity of the notion of the auteur in rock. All rock performers are, obviously, concerned about the nature of their product and its

presentation, (even if this may take on a cynical view of the market.) Arguably, auteurs are concerned to make more of a personal statement through their music, and more preoccupied with its creative aspects. This is frequently evident in biographies and autobiographies : "The fact is that all the recording science and technology in the world is no substitute for real feeling. Music is about feeling...if the song isn't about anything that anybody gives a damn about, there's nothing you can do. All the technique that exists can't make it any good...all the production values you add won't do anything except make it glossy." (Crosby and Gottleib, 1988 : 309). Most biographical studies of rock performers fall into the quickly produced, lots of pictures and little supportive text variety, reflecting the rapidly shifting nature of stardom. However, the best of them offer invaluable personal trajectories of the theoretical issues traversed here - especially the nature of the music industry - as well as key questions we have not dealt with, such as the nature of stardom, and its personal costs : Ritz (1986) on Marvin Gaye; Wiener (1984) on Lennon; and Geldof's autobiography, Is That It ? (Geldof, 1986). At their very best, they situate the subject within the rock tradition, teasing out diversity and contradiction, assimilation and synthesis : Murray (1989) on Jimi Hendrix and post war pop; Marsh (1983; 1987) on the Who and Springsteen.

It is necessary to rather labour the point that rock has always been about style, and its relationship to commerce and creativity (see Melley, 1970), and, in particular, youth subcultures (Hebdige, 1979). Two brief examples must suffice here. Firstly, the influence of the British art school environment and mentality on the development of art-rock, punk and new wave, all styles considered "progressive" within the accepted history of rock. Frith and Horne (1987) show that the multi-media innovations associated with these styles of music (the fashions, the graphic arts, the use of elaborate light shows in live performances) provided an acceptable resolution of the recurring conflict between creativity and commerciality by collapsing the boundary between pop and art; hence "art into pop". Secondly, the much vaunted cultural revolt of punk in the late 1970's was in fact centred around a retro consciousness, with an emphasis on the recycling of previous musical traditions (most notably 60's "garage bands", and American protopunks of the early 70's - the Velvet Underground, New York Dolls, etc.), and a similar recycling of post war subcultural fashion (Hebdige, 1979). Instead of trashing the past, punk in a very real sense celebrated it, though this celebration was in a highly selective fashion. Contemporary punk/thrash music works in a very similar fashion, still

reacting against the dominant trends. The very names of many of the bands act as symbolic markers of their non-commercial orientation (Butthole Surfers, Dead Kennedies, Circle Jerks).

Stranded in Paradise : Kiwi Sociology of Rock

In New Zealand, not only is the general field of popular culture largely undeveloped (exceptions include Perry, 1989; Shuker and Openshaw, 1991; Lealand, 1988), but the sociological study of pop/rock music is almost totally neglected. There is some intelligent and provocative journalism in magazines such as **Rip It Up, The Listener,** and **Broadsheet,** though constrained by space and immediacy this generally confines itself to the analysis of particular records, styles or performers. As with the overseas critics discussed earlier, there are assumptions of authenticity and cultural value frequently at work here.

The fullest academic analysis of rock music in New Zealand is Lealand's examination of the dominance of American popular culture, including American pop music (Lealand, 1988). In his preliminary throat-clearing to this discussion, Lealand provides thoughtful appraisals of the interrelationship between pop as a commercial product and its consumers, the difficulties of evaluating styles and tastes in pop, and the question of rock's association with dissent and rebellion. His major focus includes indispensable data on market shares, 1957-1984 (clearly an update is needed), a consideration of radio airplay policies and the issue of a quota for "New Zealand" music, and a fascinating discussion of why American rock/pop icons Springsteen and Madonna should enjoy such levels of popularity here. We also have some preliminary analysis of patterns of adolescent consumption of pop music (Shuker, 1990), a thoughtful appraisal of issues of political economy, nationalism and cultural identity (Maharey, 1985), and a brief introduction to the impact of rock video (Shuker, 1989) . Interestingly, in the area where one might have expected to see greater weight accorded to pop/rock music, the study of youth subcultures, it remains absent. Nor do the review columns of the "sociological" journals indicate much interest in the area. While Willis's **Learning to Labour** is accorded classic status, his equally interesting **Profane Culture** is ignored. Frith's efforts are passed by, as indeed are most of the works discussed here. (For exceptions, see Shuker, 1987).

I have used as my heading here, the title of John Dix's impressive labour of love, **Stranded in Paradise. New Zealand Rock'n'Roll 1955-1988** (Dix,

1987), because while Dix's substantial volume does not have any pretensions towards being a sociological account of the development of pop/rock music in this country, it nonetheless provides a partial empirical base for such an account. He fully describes general trends, and provides extended case studies of their leading exemplars, relying heavily on personal accounts from the musicians. Along the way, there is useful light shed on the development of the local recording industry, the process of building a following (an audience), and the perils of the rock lifestyle (Dragon). There is also a compendium of chart listings and the various music awards. Dix writes as a fan, a very knowledgeable fan. His general thesis is that indigenous rock music must be regarded as a valid and evolving art form, and it is at this point that there is a clear link with considerations of cultural imperialism and quotas for New Zealand music.

The sociology of rock, then, remains a field ripe for exploration in New Zealand. Both the local and overseas literature suggest potential topics, methodologies, and theories to test out. We still know very little about how local youth actively interpret and collectively construct their sense of music-their particular cultural capitals, and the social functions of their preferred musical styles. We know even less about the functioning of the local recording industry, the development and operation of "independents" such as Flying Nun, Propellor, and Pagan, and the distribution and marketing role of the majors. Nor (Dix aside) do we have any extended examinations of particular performers, their reinterpretations of musical styles, and their working lives within the constraints of the pub rock and campus circuit and the limitations of a small indigenous market. Finally, the whole question of a quota for local music and cultural nationalism cries out for a fuller analysis.

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On Comparing Resolute Thinkers: II

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> **Against mediators:-** Those who want to mediate between two resolute thinkers show that they are mediocre; they lack eyes for seeing what is unique. Seeing things as similar and making things the same is the sign of weak eyes. (Nietzsche, 1974:212, par. 228)

We must be equally mindful of Neitzsche's aphorism as we now examine the role of comparers of resolute thinkers as mediators. For whereas the first three problems of comparing such thinkers centred on the thinkers themselves in respect of the similarities *and* differences in the periods to which they belong, their social positions, the traditions in which they work(ed) and their discourses, it falls upon the comparer to see and show these differences and similarities. Accordingly, the three remaining problems concern comparers in their approaches to the thinkers.

IV. The Comparer as an Interpreter of Selected Thinkers

Although in the first article reference was indeed made to the interests and values and to the choices and purposes of comparers, we must now offer a sustained analysis of what Hegel calls 'the *agent* of Comparison'. (Hegel, 1975:169, my italics)

For a start, comparers, like the thinkers themselves, occupy social positions and exist in a certain situation. So how much consideration, if any, should they give to these concerns of the sociology of knowledge? Judging by various works of comparison, neither the authors nor the publishers find their social positions of much importance. For all that we find are brief statements in the prefaces and, yes, on the jackets of the books about the authors' academic - and this is what they almost always are - advisers, background and status. And, notwithstanding the fact that even the members of this 'unanchored, *relatively* classless stratum', as Mannheim (1936:137) calls it, raise their socio-*political* heads above the *everyday* party conflict, there is no real need for comparers to say anything more about these *group* bases of their knowledge. Bearing in mind how the narrator of that inimitable work, Tristram Shandy has no qualms about informing the reader that 'nothing which has touched me will be thought trifling in its nature, or tedious in its telling' (Sterne, 1983:10), the

absurd, though perhaps literary, outcome of a neurotic concern with such bases would be the enterprise of the comparers of 'the lives and opinions' of the thinkers giving so many asides about their *own* positions and situations that the comparisons themselves are never really undertaken!

But this is not to say that comparers should simply leave it to posterity to decide whether their *oeuvre* - like that of the comparer of the 'systems' of Democritus and Epicurus (see Marx, 1975a:25-207) - is so important as to merit investigation of their social positions and situations. No, they must be sufficiently reflexive and reflective to have knowledge of their positions and to be aware of how the latter will infiltrate into their researches. Now, there has been a long-standing debate on whether one position will allow, at particular junctures, a 'better' vantage point on history than other positions. Just to cite a few of the candidates on offer, we have Lukács (1971) singling out the position of the proletariat, Mannheim (1936:137) emphasizing that of Alfred Weber's '"socially unattached intelligentsia"', and Gramsci (1971:330-335) stressing that of the 'organic intellectuals' of the working class. And there is indeed the empirical possibility that one position may have this advantage, even in regard to the comparison of thinkers. Yet the danger that a charged identification with one position could lead to the production of 'useful' rather than 'true' - to use that opposition and approval of the latter notion we find in Marx (1976:97) - interpretations makes it necessary to continue to allow for and insist upon recognition of and checks to infiltration from positions - in this instance, those of comparers. In short, and both mindful and critical of how Nietzsche himself (1968:272-276, paras. 493-507) concentrates solely on the facticity and usefulness of perspectives in providing interpretations, comparers must endeavour to *understand* their perspectives, the concept of which, according to Mannheim (1936:244), 'signifies the manner in which one views an object, what one perceives in it, and how one construes it in his thinking'.

Is it necessary, though, for comparers to *record* any of these aspects of their perspectives? We certainly find reports of this or that dimension in the prefaces and introductions to a number of comparisons. And this is one occasion where the practise shows what must be done. Peter Munz and Paul Mattick are thus providing the reader with interesting and relevant personal details about the 'manner' in which they viewed Popper and Wittgenstein, and Marx and Keynes, when the one admits that the 'shape' of his 'argument' is 'almost wholly dependent on the fact' that he was a student of both philosophers

(Munz, 1985:x), and when the other states that although he has *'witnessed'* the post-war 'period of "unprecedented prosperity"', he has also *'experienced* the Great Depression between the two world wars'. (Mattick, 1969:vii, my italics) Furthermore, readers are being given essential information about what each comparer 'perceives' in her situation when Ruth Bevan (1973:11 & 13) argues that 'we need a "politics of reality" founded upon a political science' and that 'the modern relevance' of Burke and Marx is anchored, for example, in their pursuance of 'a historical-empirical method of social analysis which to them is "scientific" and hence "realistic"', and when Miriam Glucksmann (1974:xiii) declares that 'it was in the context of confusion in Britain about the difference between structural and structuralist thought, and the lack of attention paid to Althusser, in contrast to Lévi-Strauss, that I decided to compare these two theorists, and to tackle the issue of the homogeneity and heterogeneity of structuralism'. Finally, as regards 'construal', both David Rubinstein (1981:1) and Tom Goff (1980:viii & 21) rightly inform the reader at the outset of their respective comparisons of Marx and Wittgenstein and of Marx and Mead that they are aiming to offer a 'synthesis' of their ideas. All in all, then, these six comparers illustrate how the backgrounds, contexts and purposes of comparisons can and should be recorded, but without, be it noted, their being mistakenly treated as, in Gunnar Myrdal's language, 'valuations' which have been 'introduced' as explicit 'value *premises*' (see Myrdal, 1958:52 & 155, my italics) from which interpretations of one or both of the resolute thinkers will be *deduced*.

Yet in interpreting the thinkers comparers will still face a hermeneutic version of the problem of situational-relativity. To begin with, it is precisely because we are dealing with an *historical* agent of comparison that perspectives on the discourses of the thinkers do and will change. Speaking of major literary texts, George Steiner (1978:158) expresses the personal dimension of this historicity when he argues that 'our own sight-lines to the work change with different personal circumstances, with age, and in relation to the open-ended aggregate of whatever else we have read or experienced'. Reflecting upon the nature of understanding, Hans-Georg Gadamer (1979:261, 487 & 273) draws out the ontological significance of this historicity in his claims that we are bound to tradition, which is both our 'precondition' and what we 'produce', that this evolving tradition 'supports both the transmitted text and its interpreter', and that in the latter seeking to interpret the former there is the 'fusion' of the 'horizons' of the present and past. In describing this 'fusion of the horizons of

understanding' as 'what mediates between the text and its interpreter' (Gadamer, 1979:340), we can say that he is disclosing the not so strange 'Strange Loop'[1] of the interpreter facing the textual edifices of traditions which are indeed at the 'same' time bases of his or her perspective.[2] But the punch-line of the problem follows from Steiner's image of both the act of reading *and* the text being in 'motion' (1978:158) and from Gadamer's insistence that because 'there emerge continually new sources of understanding' (1979:265-266) interpretation 'is always on the way'. (Gadamer, 1981:105) For if these claims are true, then on what grounds can comparers take issue with either one thinker's interpretation of the other thinker (Weber's of Marx, for instance) or any other interpretations of the thinkers?

Even Gadamer and Steiner imply that there are such grounds, however. Part of the former's **Truth and Method** is directed against Emilio Betti's 'canon of the hermeneutical autonomy of the object', according to which

> meaning-full forms have to be regarded as autonomous, and have to be understood in accordance with their own logic of development, their intended connections, and in their necessity, coherence and conclusiveness; they should be judged in relation to the standards immanent in *the original intention*: the intention, that is, which the created forms should correspond to *from the point of view of the author* and his formative impulse in the course of the creative process; it follows that they must not be judged in terms of their suitability for any other external purpose that may seem relevant to the interpreter. (Betti in Bleicher, 1980:58, my italics)

Although Gadamer's belief that 'the meaning of a text goes beyond its author' leads him to be rightly critical of Betti's reduction of interpretation to what he, Gadamer calls 'psychic particularity', he does allow that 'understanding is not *merely* a reproductive, but always a productive attitude *as well*'. (Gadamer, 1979:264 & 466, my italics) It is this former attitude which is well exemplified

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1 As noted in the first article, according to Douglas R. Hofstadter (1980:10), 'the "Strange Loop" phenomenon occurs whenever, by moving upwards (or downwards) through the levels of some hierarchical system, we unexpectedly find ourselves right back where we started'.

2 Indeed, Gadamer (1979:293-294) himself makes the point: 'The way in which the interpreter belongs to his text is like the way in which the vanishing point belongs to the perspective of a picture. It is not a matter of looking for this vanishing point and adopting it as one's standpoint. The interpreter similarly finds his point of view already given, and does not choose it arbitrarily'.

when Ringer (1986:151) reports how 'some of the great modernists were not only my "objects of study" [in his The Decline of the German Mandarins], but also my colleagues and mentors in the interpretation of their own culture. I thought *with* them, not only *about* them'. Insofar as a conflict of interpretations concerns intentions, one ground for resolving the dispute is thus concrete evidence regarding the thinker's intention(s) in writing the 'created forms'. Yet it remains true that irrespective of 'the original intention', the meaning of a text will not only not necessarily 'correspond' to it but also always go 'beyond' it. For example, although the weight of evidence is that Marx never intended to support technological determinism, his statement that 'the hand-mill gives you society with the feudal lord; the steam-mill, society with the industrial capitalist' (Marx, 1955:95) lends itself to what we saw in the first article is Weber's reading of Marx as a technological determinist. But if this illustrates a productive attitude, our next necessary questions must be: is there any limit to it? Can it have a completely free reign? Even Gadamer, though, implicitly replies in the negative. 'Fresh sources of error', he thus affirms, are 'constantly excluded, so that the *true* meaning has filtered out of it all kinds of things that obscure it'. (Gadamer, 1979:265, my italics) Similarly, Steiner (1978:158) admits that literary texts have elicited 'misreadings'. And as soon as one introduces these notions of 'error' and 'misreading', then one must presuppose as a second ground of interpretation texts-in-themselves as *language* which will resist and even exclude certain interpretations. One of the bars to a reading of Marx as an out and out technological or economic determinist is thus the very language - note especially the words 'social' and 'appropriate' - of the opening sentence of that oft-quoted summary statement of the 'general conclusion' and 'guiding principle' of his studies: 'In the social production of their existence, men inevitably enter into definite relations, which are independent of their will, namely relations of production appropriate to a given stage in the development of their material forces of production' (Marx, 1971:20), which statement undoubtedly presupposes his understanding of the 'social' as 'the co-operation of several individuals, no matter under what conditions, in what manner and to what end. It follows from this that a certain mode of production, or industrial stage, is always *combined* with a certain *mode of co-operation, or social stage*, and *this mode of co-operation is itself a "productive force"*'. (Marx and Engels, 1965:41 my italics) Leaving aside this illustration, though, the grounds of interpretation are, and the 'autonomy of the object' embraces, both intentions *and* language.

It could be said that we have allowed for a textual realism against a hermeneutic version of Mannheim's relationism. But if the latter and its

implicit realism is evidenced in Gadamer's claim that 'the *discovery* of the true meaning of a text or a work of art is never finished; it is in fact an infinite *process*' (Gadamer, 1979:265, my italics), then the relativistic or Nietzschean perspectivistic alternative to this relationism is Jacques Derrida's talk of 'the *absence* of an author', of 'meaning' being 'a *function* of *play*', and therefore of centre-less texts with, yes, 'a *finite* language' permitting 'infinite *substitutions*'. (Derrida, 1978:287, 260 & 289, my italics) To be sure, our arguments for realism in the first article stand against this radical relativism or perspectivism, which even denies that 'anything like perception exists'. (Derrida as quoted in White, 1978:280) Be that as it may, Derrida's notion of deconstruction, of 'an active interpretation', of a 'Nietzschean *affirmation*' of 'the play of the world' (Derrida, 1978:292) does help to illuminate the problem of the selection of thinkers to compare.

We can even begin our renewed examination of this problem by asking rhetorically, are the various persons whose lives and works have been compared waiting and calling to be compared as 'resolute thinkers'? If we abided by Nietzsche's own conception of 'thinkers' as those 'few' who (like Newton) 'think day and night and do not even notice this any longer, as those who live in a blacksmith's shop no longer hear the noise' (Nietzsche, 1974:344 fn. 155), then there have indeed been precious few of these. Other of his remarks, moreover, allow us to claim that his very expression deconstructs itself as an identificatory label. For in saying (1974:108, par. 42, my italics save for 'their') that with some people, including 'artists *and* contemplative men of *all* kinds', their '*idleness* is *resolute*' and that 'they actually require a lot of *boredom* if *their* work is to succeed', he is recognizing that we cannot have persons who are continually firm of purpose and thinking day and night. Although it admittedly involved 'an active interpretation' on our part or, to leave the reader out of this, my part, it was for this reason that 'resolute thinkers' have been regarded as those who, after the 'disagreeable "windless calm"' (Nietzsche, 1974:108, par. 42) of boredom or the pleasing turbulent movement of experience, have been determined in the application of their powerful minds and have made a significant contribution to human thought.

Even though it may be allowed that this expression is thus applicable to the thinkers that are referred to in this paper, has any 'play' in their selection for comparison been one of 'the *world*'? In other words, have not the interests and values of comparers been such as to confine their attention to Western

civilization and to male thinkers? Notwithstanding the references in the first article to Chinese civilization and to the work of Needham, the former may well not be a restriction but simply a consequence of my ignorance of comparisons of the thinkers of other civilizations. The latter limitation, however, warrants more comment. For the numerous obstacles faced by women throughout recorded history have meant that proportionately fewer of them have been able to make a significant contribution to human thought. In the light of this generalization, it is interesting to note that there was some justification for Martin Green (1974:xi) to start his comparison of the von Richthofen sisters, Else and Frieda with the claim that they were both 'passionate' and 'brilliant' and then to eclipse them somewhat when he began to discuss and compare the biographies and writings of two of the men in their lives, Weber and D.H. Lawrence. Yet even with women who have made a remarkable contribution to human thought we do not find that they have been presented in dramatic confrontations with female or male thinkers of a similar stature working in similar fields. For instance, we have Robert Wallace's comparison of Jane Austen and Mozart (see Wallace, 1983), but not, to the best of my knowledge, one of Austen and, say, Emile Brontë. Similarly, although the fact that Sidney and Beatrice Webb were, as the latter termed it (see Hamilton, 1933:1-2)[3], a 'double star personality' means that they cannot be compared, at least not at the level of their main intellectual offerings, neither this 'personality' nor Beatrice has been compared with other thinkers[4]. And even though there is a juxtaposition of the biographies of Eleanor Marx, Rosa Luxemburg and Angelica Balabanoff (see Florence, 1975), we do not have - again, as far as I know - a comparison of Luxemburg and, say, Lenin, which would surely prove a dramatic and worthwhile undertaking.

Related to these restrictions is an issue which Derrida (1978:292) broaches in his arresting claim: 'Being must be conceived as presence or absence on the basis of the possibility of play and not the other way round'. Now, we can certainly admit that a comparison of resolute thinkers presupposes a play of contrasts. After all, a Luxemburg and a Lenin, a Marie Curie and an Einstein differ markedly from Everywoman and Everyman. Moreover, the comparison

3 Incidentally, whereas Hamilton's title follows the practise of the Webbs to refere to themselves as 'Sidney and Beatrice', Lisanne Radice (1984) has chosen a different syntax : her work is entitled Beatrice and Sidney Webb : Fabian Socialists!

4 Though Green (1974:271-273) does draw attention to some similarities between Beatrice and Else von Richthofen.

of a figure like Marx not only with Weber, Keynes, Burke, Wittgenstein and Mead - all of which we have already mentioned - but also with Mill, Durkheim, Vico, Freud and Teilhard (for the latter, see Lischer, 1979) would seem to illustrate the possibility of a lot of 'play' on a 'bottomless chessboard' - the phrase is Derrida's (see Hoy, 1978:78) - of thinkers. And it cannot be denied that this range of comparison is testimony to a variety of contemporary interests and values and thus to these comparisons satisfying the *cultural relevance* requirement of comparison. Someone could even find it relevant to compare Marx and, say, Wilhelm Weitling. But rather than any of these actual or possible comparisons giving credence to Derrida's opposition to the kind of interpretation which 'dreams of deciphering a truth or *an origin which escapes play* and the order of the sign' (Derrida, 1978:292, my italics), they indicate, or would indicate, that the very range, nature, influence and quality of Marx's work make for the centrality of him and his discourses to one historical period and to one or more traditions of thought. Instead of these examples pointing to the need for a notion of centre-*less* traditions, then, at least one of the subjects of them lends support to our conception of *historically significant* comparisons of thinkers.

But to which objective tradition(s) will they and their discourses be regarded as central? It is generally conceded that Marx is central to the tradition(s) of Marxism. And it may well be conceded that 'the historical significance of Marxism is ... overwhelmingly greater than that of sociology or economics'. (Therborn, 1976:38) These are two of the main reasons why this resolute thinker has attracted so many comparers. Yet the work of one of the latter may be such as to treat Marx as peripheral even to the contemporary tradition of sociological thought. This is clearly the case in Talcott Parsons' **The Structure of Social Action**. Like us, he (1968:697-698) insists that the 'facts' concerning 'the published works' of certain writers 'do not tell their own story; they must be cross-examined. They must be carefully analyzed, systematized, compared and interpreted'. But his cross-examination concerns the works of Marshall, Pareto, Durkheim and Weber; the influence of Marx, he believes (1968:xiv), belongs 'properly on the wings rather than at the core [of this 'line of development in sociological theory']'. Now, although this concentration on the former writers and playing down of the latter's work can be seen as a good example of Derrida's claim that 'the presence of an element is always a signifying and substitutive reference inscribed in a system of differences and the movement of a chain' (Derrida, 1978:292), the fact that we are

emphasizing interpretation as *work* on a *finite* language rather than as a 'play' allowing '*infinite* substitutions' enables us to conclude that, and to focus upon how, the comparer's 'system', like that of any thinker, is not beyond criticism. Partly in critical response to Parsons' 'system of differences', we therefore have Marx being rightly brought out from the (left) wings when the work of Giddens (1971:vii) shows Durkheim, Weber *and* Marx as having 'established the principal frames of reference of modern sociology', and when Jeffrey C. Alexander (1982, Volume 2:7) compares Durkheim *and* Marx as 'the founding fathers' of sociology.

That criticism must not come to an end even with these comparisons, though, can be illustrated by briefly considering Alexander's interpretation and thus selection of Marx and Durkheim. He openly admits that he will 'evaluate and criticize' these two thinkers 'from the perspective of a multidimensional theory' (Alexander, 1982, Volume 2:7), according to which action must be conceived 'as in part voluntary, in part determined'. (Alexander, 1982, Volume 1:67) Indeed, it is precisely because Durkheim and Marx are supposed to have mistakenly engaged in one-dimensional theorizing that they are brought together in one volume, whereas Alexander discusses Weber and Parsons in two other volumes on the ground that they made serious efforts to theorize from 'a multidimensional premise'. (See Alexander, 1983, and Alexander, 1984) But one fundamental question is whether Marx adopted a one-dimensional view of action. For Alexander (1982, Volume 2:179, my italics), the answer is clear-cut: Marx came to assume 'the *complete* instrumentality of action'. He is thus said to use in the Preface to **A Contribution to the Critique of Political Economy** a 'base-superstructure model ... designed to translate his presuppositional logic into a model of a two-tiered *system*, with the *sole determining* power allocated to the bottom level'. (Alexander, 1982, Volume 2:180, my italics) Contrary to this reading, however, even the initial work[5] on the finite language of this text shows that Marx actually allows for voluntarism in his (1971:20-21, my italics) statements that 'definite *forms* of *social* consciousness' 'correspond' to, rather than are determined by, 'the economic structure of society' (on which does indeed arise 'a legal and

5 That is, the initial work on the/an English translation of it. For prior to this there is the task of reading and translating the German text, which work mayor may not be performed by the comparer himself or herself. Steiner is one comparer who had to rely on translations. Yet although he (mistakenly) regards (1980:44) translations as 'more or less flagrant modes of betrayal', he still (somewhat inconsistently) allows that his comparison of Tolstoy and Dostoevsky may be of value, albeit of 'restricted value'.

political superstructure') and that 'the mode of production of material life *conditions* the *general* process of social, political and intellectual life'[6]. Bearing in mind our earlier quotations from this work and The German Ideology, we can say that what he articulates in the former is in fact a material *and* social forces of production-structure-superstructure-*social* consciousness model of a 'tiered system' in a dialectical *process* that permits 'independent contributions' (see Alexander, 1982, Volume 1:67) from the top to the bottom levels. Given that this model indicates how even the older Marx proceeds from 'a multidimensional premise', and if we accept as valid Alexander's construal of Durkheim and Weber, then there is a good warrant for comparing, not Marx and Durkheim, but Marx and Weber.

Yet there was nothing wrong in Alexander introducing a standard (multidimensionality) in his selection of the founding fathers and pre-eminent progeny of sociology. For comparers cannot know the presence of the 'being' of the significance of the thinkers' contributions without the work of constructing and applying standards in interpreting their discourses. Differences between the 'being' of contributions to the arts, philosophy, socio-historical sciences and natural sciences mean that such standards will vary. That there are differences between contributions to the arts and natural sciences, for instance, is illustrated when Steiner (1980:7) quite rightly finds it necessary to follow his declaration that Tolstoy and Dostoevsky are 'the two greatest of novelists' with the admissions that this kind of proposition 'cannot be demonstrated' and 'is not subject to rational proof', whereas Aaron Lerner (1973:11) can base his contention that Newton and Einstein were 'the outstanding scientists of their day and perhaps of all time' on their formulation of 'laws of nature' which 'have enabled other scientists to make accurate predictions about the universe'. The latter claim certainly presupposes standards like universality of explanation and accuracy of prediction. But even as regards the arts, good or bad, better or worse, aesthetic standards can be offered about the degree to which contributions to one genre are achievements. These standards are implicit in Steiner's reference to, and discussion of, how

6 The statement following this one has misled many of Marx's interpreters, for in declaring that 'it is not the consciousness of men that determines their existence, but their social existence that determines their consciousness', he is simply reminding himself of his opposition to Young Hegelian idealism. The statement neither refers to the determination of '*social* consciousness nor gainsays the contention of Marx's Economic and Philosophic Manuscripts of 1844 (1970:113) that human social existence presupposes and involves consciousness.

his superb novelists 'excel in comprehensiveness of vision and force of execution'. (Steiner, 1980:8) Whatever the kind of standard, though, a comparer's conjectural (fore-) knowledge of which resolute thinkers have best met one or more of them can and does help to regulate his or her selection of the thinkers.

The upshot of this line of argument is that although we have brought out how the agents of comparison can and must engage in that 'conversation' which Richard Rorty (1979:318) sees as the hallmark of hermeneutics, we have allowed that this colloquy among comparers of thinkers can be epistemological. But rather than following Rorty (1979:316, my italics) in regarding the latter as proceeding on 'the assumption that *all* contributions to a given discourse are *commensurable*', which notion, we saw in the first article, involves the supply of 'rules' which will secure 'rational agreement' on '*every* point where statements [in various discourses] seem to conflict', this is a use of 'epistemology' as a reference to standards which will either merely promote rational agreement as to why comparers disagree on the interpretation and thus selection of the thinkers or, applying particularly to scientific thinkers, show how more rational agreement can be reached on these two tasks.

Are we therefore able to leave behind hermeneutic interpretation and move on to an epistemology that promises rational agreement? Paul Feyerabend and Rorty would deny this. As was pointed out in the first article, the former (1982:67-69) allows for comparison but comparison as (mere) 'preferences' for various criteria in choosing between theories. More tellingly, Rorty appropriates to hermeneutics all concern with apparent or actual *differences* between the discourses of two resolute thinkers and with attempting to move beyond them when he (1979:346, 343 & 384) distinguishes between epistemology as 'discourse about normal [or commensurable] discourse' and hermeneutics as both 'discourse about as yet incommensurable discourses' and 'the inchoate questioning out of which inquiries - new normal discourses - may (or may not) emerge'. Rather than refer to *differences* between the 'being' of contributions to various endeavours, moreover, he (1987:51) allows and hopes for the gradual fading away of 'the oppositions between the humanities, the arts and the sciences'. Just how far, if at all, one must follow Feyerabend and Rorty in these claims must thus be addressed as we turn to explore the crucial problem of -

V. The Method of Comparison

From Hegel onwards strong reservations have been expressed about this method. In the Science of Logic the trouble is said to hinge on how 'the comparer goes from likeness to unlikeness and from this back to likeness, and therefore lets the one vanish in the other and is, in fact, *the negative unity of both*. This unity, in the first instance, lies beyond the compared and also beyond the moments of the comparison as a subjective act falling outside them'. (Hegel, 1969:421) Instead of drawing this 'external comparison' between any 'categories', Hegel (1969:435-436, my italics) urges that 'they must be considered in themselves', as 'they are in *and* for themselves'. Similarly, although the Hegel (1975:170, my italics) of the Logic concedes that the results of comparison are 'indispensable', they are still claimed to be 'labours only *preliminary* to truly intelligent cognition'. As to what one modern philosopher understands by the latter, we have Gadamer (1979:206, my italics) making the Hegelian point that 'the essence of comparison presupposes the freedom of the knowing subjectivity, which is in control of both members of the comparison' and then adding: 'It makes things contemporary as a matter of course. Hence we must doubt whether the method of comparison really satisfies *the idea of historical knowledge*'.

Now, we have already rightly moved away from a merely 'subjective' comparer to his or her '*knowing* subjectivity' and then to the sorts of ('truly intelligent') 'control' that can be exerted by the *community* of knowing comparers *and* by the 'being' of the contributions of the compared thinkers. But do the latter have to be considered as they are 'in and for themselves'? In other words, to what extent, if at all, is Gadamer's doubt warranted in respect of the method of comparing resolute thinkers? Lest, however, we accept uncritically the presupposition behind his doubt, we must also ask whether 'the idea of historical knowledge' has to be satisfied by all of the following five methods that are implied by our analysis of the previous problems and at least the first three of which are, in effect, used by all comparers of thinkers.

(1) *The Regressive Method.* Sartre (1963:152 & 154) argues that we need a 'progressive-regressive method' precisely because 'the movement of comprehension' of 'the meaning of any human conduct' is 'simultaneously progressive (toward the objective result) and regressive (I go back toward the original condition)'. In the case of a study of Flaubert, he (1963:146) contends that the second 'movement'

would involve research into the following 'hierarchy of heterogeneous significations: Madame Bovary, Flaubert's "femininity", his childhood in a hospital building, existing contradictions in the contemporary petite bourgeoisie, the evolution of the family, of property, etc'. Guided by their problems and intellectual standpoints, comparers of resolute thinkers have gone back to research these and/or other sorts of 'original condition'. The range is from Helen Davis's attempt 'to approach the problems of both Nietzsche and Tolstoy through their own lives and personalities' (Davis, 1971:264) to Graeme Duncan's sketch of the historical and theoretical 'background' to the ideas of Marx and Mill. (See Duncan, 1973:Chapter 1) Given some of the arguments and examples in the first article, though, we can say that the regressive method allows comparers to obtain and present historical knowledge not just of these conditions but also of those more developed and immediate conditions of each thinker's social position and situation. And it is only by having this knowledge of thinkers as they are '*in* themselves' that those inevitable and necessary *group* bases of their thought and knowledge can be comprehended.

But is the effectivity of the compound of conditions such that they can never be truly '*for* themselves'? A positive answer could well be derived from Mannheim's (other) definition of 'the "perspective" of a thinker' as 'the subject's whole mode of conceiving things as *determined* by his historical and social setting'. (Mannheim, 1936:239, my italics) However, we can give a negative answer as regards the relatively open intellectual life that resolute thinking presupposes and thereby continue to maintain our conception of dialectical interactions between thinkers and their social positions by linking the regressive method to the notion of *predisposition*. Following the definition in the Oxford English Dictionary of 'predisposition' as 'the condition of being predisposed or inclined beforehand (*to* something or *to do* something); a previous inclination or favourable state of mind', we can therefore use this notion to refer to how the regressive method enables the disclosure of conditions that *incline* rather than determine thinkers to act or think in a certain way or direction.

(2) *The Progressive Method*. Whereas the concept of predisposition involves what can be described as an inclining forward toward from

conditions, that of *'project'*, which is fundamental to Sartre's progressive 'movement of comprehension', presupposes and, given predisposing rather than determining conditions, *can* presuppose a "'wrenching away from toward ...'". (Sartre, 1963:147) In other words, the project of, for instance, 'the author' of Madame Bovary to escape from the petite bourgeoisie 'has *a meaning*, it is not the simple negativity of flight; by it a man aims at the production of himself in the world as a certain objective totality'. (Sartre, 1963:147) Accordingly, even though comparers rarely use the term 'project' - Annie Osborn (1964:102 & 117) just happens to refer to Rousseau's and Burke's 'projects'[7], they do and must use the progressive method to recover the thinkers' primary aims[8]. Armed with this historical knowledge, they will then be able to understand not merely that, say, neither Wilhelm Wolff nor the First International scripted Capital and neither Else von Richthofen nor the German Democratic Party wrote Economy and Society, but also, and more positively, thinkers like Marx and Weber as *lively* authors who produced their own discourses[9].

Agreement with Sartre and Betti on the need to allow for 'the author' does not, however, entail the conclusion that use of the first two methods is the only way to interpret the thinkers' discourses. Our cue for introducing what is the main way of interpreting the latter is Sartre's own admission that, because 'the project is in danger of being deviated ... by the collective instruments; thus the terminal objectification *perhaps* does not correspond exactly to the original choice', we 'must take up the regressive analysis again, making a still closer study of the instrumental field so as to determine the possible deviations'. (Sartre, 1963:148, my italics) But what is required as well as this 'return to biography' (1963:148) is a method that is oriented toward the language of the discourses - a language which, in its delivered or inscribed totality, I have argued *never* corresponds 'exactly' to the author's 'original' choices or intentions.

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7 Though her use of the term may have been prompted by her (1964:90) reference to Rousseau's Projet pour l'education de M. de Sainte-Marie.

8 Similarly, Quentin Skinner (1969:48-49) argues that 'the appropriate method by which to study the history of ideas' involves 'the recovery of intentions'.

9 In declaring (1977:148, my italics) that 'the birth of the reader *must* be at the cost of *the death of the Author*', Roland Barthes is not only mistaken about the status of the author but also exemplifies that unfortunate recent largely French zero-sum or either/or mode of thinking.

(3) *The Hermeneutic Method.* To be sure, given that hermeneutics is concerned with all sorts of meaning, this method accompanies the regressive method, which is directed toward, amongst other things, the predisposing nature of meaning-full traditions; and, *if* taken as not being restricted to the meanings of texts as totalities, even embraces the progressive method, which is oriented toward meaningful projects. Nevertheless, it differs from the former in that it enables a study of the authors' discourses, especially textual ones, by using both analysis and synthesis in the respective, yet interdependent, tasks of dissecting and reconstructing the discourses and the traditions of thought to which they respond and contribute.

Central to the hermeneutic method, moreover, are not the concepts of predisposition and project, but rather at least three other notions. Two of them have already been articulated. For Althusser (1969:67 fn. 30), the *problematic* of a theory is 'the objective internal reference system of its particular themes, the system of questions commanding the answers given'. Whereas Althusser thus anchors a problematic in 'questions', R.G. Collingwood (1940:Chapter 4) seizes upon the 'logical efficacy' of a *presupposition* in the sense that it is a logically prior supposition that permits a certain question to be posed. But rather than stopping at Collingwood's discussion of 'on presupposing', we need to reflect upon 'on preconceiving' and supplement the concept of presupposition by articulating that of *preconception* as the amalgam of hidden or overt, known or unrecognised opinions that precede and underlie suppositions. Even now we have Duncan (1973:9 & *passim*) being alert to Marx's and Mill's preconceptions, Alexander (1982, Volume 2:xix & *passim*) examining the 'general presuppositions' of Marx's and Durkheim's arguments, and Glucksmann (1974:13 & *passim*) studying the relationship between Lévi-Strauss and Althusser at the 'level' of their problematics. But whereas Althusser (1970:317) refers to 'a symptomatic reading' of, and Glucksmann (1974:3 & 58) to a 'morphological' approach to, problematics, it is the hermeneutic method that enables the detection and description of the problematics *and* presuppositions *and* preconceptions (and so on) of discourses.

Clearly, then, this method, unlike the first two methods, is primarily concerned with texts. But it should not be thought that this shift of

focus away from the contexts of conditions and intentions means either that the hermeneutic method fails to satisfy 'the idea of historical knowledge' or that such knowledge does not have to be provided. On the contrary, all of the 'in themselves' features of texts are, as Collingwood (1940:66) says of absolute presuppositions, 'historical facts' and a knowledge of many, if not all, of them is essential in any comparison of thinkers.

(4) *The Methods of Criticism*. One reason why such knowledge is necessary is stated by Marx (1965:575) when, in opposition to Grün's exposition of Fourier's 'system' by way of little more than quotations from Fourier's works, he insists: 'It is only possible to criticize such constructions (and this applies also to the Hegelian method) by demonstrating how they are made and thereby proving oneself master of them'. True, some comparers offer little, if any, criticism of the thinkers' discourses. There is not much criticism in Green's comparison of the von Richthofen sisters[10] and none in Lerner's of Newton and Einstein. Most comparers, however, can be seen as applying one or both of the following methods of what Weber (1949:52, my italics) calls '*scientific* criticism':

(a) *Dialectical Criticism*. According to him (1949:54, my italics save for 'consistency'), 'the scientific treatment of value-judgements may not only understand and empathically analyze (*nacherleben*) the desired ends and the ideals which underlie them; it can also "judge" them *critically*. This criticism can of course have only a *dialectical* character, i.e., it can be no more than a formal logical judgement of historically given value-judgements and ideas, a testing of the ideals according to the postulate of the internal *consistency* of the desired end'. An example of such criticism is J.G. Merquior's account of Rousseau's political philosophy, the '*true* paradox' of which is, he claims (1980:86), that Rousseau was 'a backward-looking anarchist, profoundly at variance with the course of social history', who nonetheless 'founded modern democratism, and

10 There is rare (personal) criticism of the following kind: 'Lawrence is often shrill and foolish about public facts, Weber is often dull and conventional about the inner life, even his own'. (1974:147)

thus the modern principle of legitimacy'. On the other hand, comparers like Alexander (1982, Volume 2:300) and Glucksmann (1974:120) are concerned less with 'ends' and more with providing 'formal logical judgements' about the 'strains' and 'internal contradictions' in, for instance, Marx's presuppositions and Althusser's problematic.

(b) *Technical Criticism.* Weber (1949:52-53) argues that 'inasmuch as we are able to determine (within the present limits of our knowledge) which means for the achievement of a proposed end are appropriate or inappropriate, we can in this way estimate the chances of attaining a certain end by certain available means. In this way we can indirectly criticize the setting of the end itself as practically meaningful (on the basis of the existing historical situation) or as meaningless with reference to existing conditions'. It could thus be asked how 'practically meaningful' it was for Tolstoy to 'attempt to make of the spiritual realm of Christ a kingdom of this earth' (Tolstoy as quoted in Steiner, 1980:258) and, yes, even for the Einstein (1960:157) facing the tradition of quantum theory to call for 'pursuing to the end the path of the relativistic field theory'. More indirect actual examples of such criticism are Merquior (1980:73) stating that Rousseau's theory of legitimacy has '*historical validity*' in the sense of 'the suitability of the principle of democratic participation to the kind of society where we came to live', and Duncan (1973:199 & 206) drawing upon 'past history and present tendencies' to support his contention that Marx's vision of communism 'appears to be unrealisable in its fullness'.

Although these are indeed methods of *scientific* criticism, do they meet 'the idea of historical knowledge'? Dialectical criticism does so because consistencies and inconsistencies in the discourses of resolute thinkers are 'historical facts' - they are so even if the thinkers themselves were not aware of the incoherence or coherence of their discourses - and it enables their discovery. Furthermore, just as historians do in their studies of ideologies and mentalities, so must ambitious

comparers provide this kind of knowledge of the thinkers' discourses. As for technical criticism, it can be treated as an historical method for finding out *why* certain ends were attained and others not attained. However, it does not inform researchers *what* the ends, theories and so on were, and it goes beyond historical knowledge *per se* when, and insofar as, it criticizes the discourses in the light of the standards and content of the *comparers'* knowledge, which, at least as regards Merquior's and Duncan's explicit claims, is of both the thinker's age *and* the contemporary one.

One way to avoid this latter consequence would be for the comparer to rest content with simply recording any technical criticism one thinker offered of the other. A comparer of Marx and Weber could thus cite the latter's criticism of the revolutionary 'hopes' of The Communist Manifesto for 'the collapse of bourgeois society' (see Weber, 1971:208-219) and of the belief that one could have a socialist economic system in which 'the mass of men' would orient themselves to the 'interests' of others on 'purely ideological grounds'. (Weber, 1978:203) Another way of avoiding it is suggested by Joseph Agassi's view that 'serious studies in the history of science demand comparison *and* judgement of theories against a given historical background rather than against the standard of the up-to-date text book. (Agassi, 1963:41, my italics) Just as he does with the proponents and opponents of phlogistonism, then, comparers of resolute thinkers could 'assess' the ideas or techniques of the younger or later thinker by comparing them with their 'background and predecessors' rather than with contemporary standards and developments. By doing so, they would be able to dispel any Gadamerian doubt they may have had about the historicity of comparison and thereby to join Agasi (1963:41) in declaring: 'There can be no greater praise of the comparative method than to say it is genuinely historical'.

(5) *The Methods of Critique*. Either of those ways of keeping the comparative method historical can even be used to the extent that one of the thinkers employed one or both of these methods of critique. In

discussing the latter on the basis of Weber's and Marx's ideas, it can be pointed out that the neglected level on which they themselves can *and* must be compared concerns their conceptions of and engagement in criticism *and* critique. For there are clearly some affinities and many differences in:

(i) Weber (1949:105-106, my italics) assuming that 'the setting' of a 'problem varies with 'the content of culture itself', that 'the attempt to order reality analytically through the construction of concepts' depends on that setting and that 'the dissolution of the analytical constructs so constructed' occurs 'through the expansion and shift of the scientific horizon', and then eliding all three assumptions when he asserts that 'the greatest advances in the sphere of the social sciences are substantively tied up with the shift in practical cultural problems and take the *guise* of *a critique of concept-construction*', and

(ii) Marx (1971:21) focusing upon 'social order[s]' and their 'problem[s]', writing of 'the working-up of observation and conception into concepts' (Marx, 1973:101), and looking upon Capital as 'a *critique of the economic categories*, or ... the system of bourgeois economy critically presented. It is a presentation of the system and simultaneously, through this presentation, a criticism of it'. (Marx and Engels, 1975b:96)

Although such contrasts intimate how Marx's critique *qua* scientific theory includes a grounded ('technical') criticism, the difference we need to single out concerns how his complementary project of offering a 'critique and history of political economy and of socialism as a whole' (Marx and Engels, 1975b:97) and its realisation in the Theories of Surplus Value shows that he, unlike Weber, expressly allows for an independently significant *critique of theories*. My claim is thus that whereas concept- (and category-) construction is indeed part and parcel of the production of theories, the latter involve more than the former inasmuch as they provide explanations rather than descriptions and, if scientific, contain hypotheses. Bearing this distinction in mind, we can then identify:

(a) *Conceptual Critiques*. These conceptual exercises involve assessing the usefulness of concepts and, less frequently, refining, replacing or supplementing them. Given that concepts may be either unrelated to *or* part of theories, the critiques can be of both concept-constructions *and* theories. What is more, they can advance philosophy and all of the sciences. It would be open, then, for a comparer of Marx and Weber who wanted to use a purely historical approach to present and then judge (in the way Agassi describes) Weber's critique of Marx's distinction between 'factory' and 'manufactory' (see Weber, 1927:162-163) and of his failure to distinguish between phenomena of an economic, '"economically relevant" and "economically conditioned"'kind[11]. (See Weber, 1949:65)

(b) *Theoretical Critiques*. With these it is a matter of theories being applied as arguments against and/or in tests of either concept-constructions or theories. From this it follows that there are two kinds of critique and two objects for them, and, contrary to Weber, that 'the greatest advances' in all of the sciences and even in philosophy involve not only critiques of concept-construction but also theoretical critiques of both the latter *and* theories. In the effort to remain historical, a comparer of Marx and Mill could thus discuss the one's theoretical critiques of the other's concepts and theory of political economy (see Marx, 1972:190-236), and even a comparer of Marx and Weber could cite and, if he or she wished, 'judge' (as Löwith did) those lectures of Weber's on the sociology of religion and of the state to which he gave the title: 'A *Positive* Critique of the Materialistic View of History'. (See Marianne Weber, 1975:604, my italics)

Strategies of this kind for keeping the comparative method purely historical are unduly restrictive, however. To be sure, everything we have said so far about this method satisfies 'the idea of historical knowledge' and the knowledge provided by its component methods and their accompanying strategies must be

11 Although Marx did not make this essential threefold distinction, it can and must be related to his material and social forces of production - (economic) structure-superstructure-social consciousness model of social formations.

known by the agents of comparison. Comparers of resolute thinkers, in other words, both must *and* can have *strong* eyes for seeing what is similar *and* different about the thinkers, and what the one may have said about the other's discourses. Revision of Nietzsche's aphorism can be continued by pointing out that all such comparers are mediators in the sense that they 'mediate' or 'form a connecting link' between the thinkers. (For this and all subsequent definitions of 'mediate', see the Oxford English Dictionary) It was argued in the first article that comparers, in forming this link, need not be, and have not been, limited to reporting merely what the younger or later thinker thought of the older or earlier thinker's discourses. Indeed, it was contended that a comparer who wanted to offer a *fair* 'judgement' of the latter in the light of the former's either explicit criticisms and critiques *or* discourses as a whole, which may well contain additional *implicit* criticisms and critiques, would have to ensure that the older or earlier thinker's conceptions and standards of critique (and criticism) are used to weigh up the merits of the discourses of the other thinker. If a comparer is governed by a standard that is supplementary to that of historical knowledge, then, the method of comparison does not have to be limited to providing the history of one thinker's unidirectional, or even of both thinkers' mutual, critiques and criticisms. Whereas Hofstadter (1979:689-690) identifies the 'Strange Loop' of Escher's 'Drawing Hands', which depicts a left hand drawing a right hand and, at the same time, the right hand drawing the left hand, a comparer can present a *non*-historical 'Strange Loop' of the thinkers marking each other's discourses.

Our previous analyses and examples show that there is no reason, moreover, why users of the methods of criticism and of critique cannot take *contemporary* developments and standards, ideas and techniques, as their *main* point of departure[12]. Knowledge of this kind could obviously inform the comparer's technical criticism of both or one of the thinkers (Duncan's of Marx, for instance) and it can even aid dialectical criticisms by facilitating the offering of penetrating formal logical judgements about the internal consistency of the thinkers' discourses (for example, Merquior's of Rousseau's). But in the case of the methods of critique we can be mindful of

12 In other words, while Agassi (1963:41) and Needham (1959:150) or see my first article: 4 fn.3) are correct to claim that it is not worthwhile to assess, respectively, phlogistonism and old Chinese mathematics in the light of modern chemistry and mathematics, the contemporaneity or greater proximity of thinkers *and* their comparers can make it appropriate, even pressing, to subject the former to the latter's yardsticks.

Victoria Bonnell's useful distinction between 'the mediation of history by theory and the mediation of history by concepts' - the one involving the formulation of 'theoretical statements or models' and the other using 'concepts rather than theories or models' as 'the principal heuristic device' for 'the selection, organisation and interpretation of empirical material' (Bonnell, 1980:157, 162 & 166) - and refer to *the mediation of resolute thinkers by*:

theoretical critiques, as with Alexander's project to 'evaluate and criticize' Marx and Durkheim 'from the perspective of a multidimensional theory';

conceptual critiques, an illustration being P.Q. Hirst's (Althusserian) 'conceptual reading' of Bernard and Durkheim and his 'critique' of the latter on the basis of the former's and more modern 'epistemological concepts' (Hirst, 1975:9 & 12); or

conceptual and theoretical critiques, among which can be included Munz's (Popperian) critique of Wittgenstein and Mattick's Marxian critique of Keynes.

The very intent *and* content of these various critiques shows just how erroneous is Glucksmann's rather odd claim that 'as a general principle [*sic*] there seems *little* to gain by criticizing one theoretical framework from the point of view of another: one is left with an *inevitable* eclecticism and reductionism ...'. (Glucksmann, 1974:120, my italics) For each of them provides non-eclectic and non-reductive advances in the assessment, clarification and/or formulation of concepts and theories. Both allegedly inevitable consequences can be avoided even if the comparers and their critique(s) 'mediate' between the thinkers in the (further) sense of forming 'a transitional stage between one thing and another'. For example, John B. Thompson (1981:3, 216 & *passim*) is not only well aware of the dangers of 'intellectual eclecticism' in his comparison of Ricoeur and Habermas against the background of ordinary language philosophy but also lets his critiques of these thinkers and of this tradition 'prepare the way' for his non-reductive outline of 'the contours of a critical and rationally justified theory for the interpretation of action'.

Eclecticism and reductionism are, however, characteristic of those *starting* points for some critiques that are offered by comparers who 'mediate' in the yet further sense of interceding or intervening 'for the purpose of reconciling'

the resolute thinkers. Now, it is undoubtedly these mediators who are *the* target for Nietzsche's aphorism. As Goff and Rubinstein are the only comparers we have mentioned so far who are mediators of this kind, does this mean that we should hearken to Nietzsche and dismiss them as 'mediocre'? Well, the relative degree of uniqueness of Marx, Mead and Wittgenstein means that it was a mistake for them to talk, even elliptically, of a 'synthesis' of Marx and Mead (see Goff, 1980:2 & 22[13]), and Marx and Wittgenstein (see Rubinstein, 1981:1 & 90) - as if the totalities of their discourses could be synthesized! Yet *if* there *are* points of 'compatibility' (Goff, 1980:87-91) or 'substantial parallels' (Rubinstein, 1981:1 & 181) between some of their ideas, which similarities Goff and Rubinstein make the basis for their critiques of contemporary views on the sociology of knowledge and the character of social scientific explanation, then these comparers illustrate how, once one rejects the notion of absolutely 'unique' resolute thinkers, there is no *a priori* reason why mediators should not both see *and* discover that one or more significant 'things' about the thinkers are similar, and make them into a point of departure for critiques of other discourses.

Instantiated by such critiques is the fact that all comparers of resolute thinkers 'mediate' between them in the (final) sense that their comparative method is 'the intermediary or medium concerned in bringing about (a result)'. These results range from the regressive, progressive and hermeneutic methods enabling the relating of the thinkers to their conditions, the recovery of their intentions and the reconstruction and dissection of their discourses to the methods of criticism and of critique permitting the evaluation of the coherence and practical meaningfulness of their viewpoints and discourses, the assessment of the usefulness and adequacy of their discourses and theories, *and* the provision of further critiques, concepts and theories.

Their production brings us back to Rorty's distinctions in his intriguing chapter, 'From Epistemology to Hermeneutics'. (Rorty, 1979:Chapter 7) We have already noted that the epistemology he desires to leave behind 'proceeds on the assumption that all contributions to a given discourse are commensurable', which assumption derives from 'the notion that the objects to be confronted by the mind, or the rules which constrain inquiry, are common to all discourse, or at least to every discourse on a given topic'. (Rorty,

13 But on another occasion he makes it clear that the synthesis is of 'relevant *elements* of their perspectives'. (21, my italics)

1979:315-316) Opposed to this notion is hermeneutics as 'an expression of hope that the cultural space left by the demise of [foundational] epistemology will not be filled - that our culture should become one in which the demand for constraint and confrontation is no longer felt'. (1979:315) However, when Rorty goes on to view epistemology as '*discourse* about normal [or commensurable] discourse', and hermeneutics as 'discourse about abnormal [or incommensurable] discourse', he allows that 'the two do not compete, but rather help each other out'. (1979:346, my italics) Now, if epistemology is seen as 'the search for the immutable structures *within* which knowledge, life and culture *must* be *contained* - structures set by the privileged representations which it studies' (1979:163, my italics), then it is indeed true and well that it is dead. Against *this* belief, it is necessary to stress continued conversation. But if we understand epistemology as a study of an evolving mind, of an improving technology and of testable theories all in confrontation with structures which may be either immutable (for example, those of the historical past and of the universe) or mutable (aspects of society and nature, for instance), then to report its 'demise' would be, as Mark Twain said of his 'death', an exaggeration. My claim is therefore that use of the methods of criticism and of critique illustrates the possibility of a movement *from* hermeneutics *to* epistemology. For the point about them - above all, theoretical critiques - is that they are the standard *and* rule governed approaches which enable comparers to derive new commensurable or rationally agreed upon discourses from their comparisons.

Such results will, however, be precluded insofar as thinkers are treated purely as *artists*. This does indeed run counter to Rorty's pragmatism, which 'views science as one genre of literature - or, put the other way round, literature and the arts as inquiries, on the *same* footing as scientific inquiries'. (Rorty, 1982:xliii, my italics) To be sure, the arts, which for us include literature, can 'help ethics do its [job]', as Rorty (1982:xliii) terms it, and there is an artistic and a rhetorical dimension to science. Comparers could thus approach **War and Peace**, **Crime and Punishment**, Newton's **Principia** and Einstein's papers with these considerations in mind. Yet it is because the arts, unlike science, have their primary 'footing' in non-cognitive and aesthetic standards that Steiner's point about how it 'cannot be demonstrated' that Tolstoy and Dostoevsky are 'the two greatest of novelists' can be extended to how one cannot *demonstrate* not only which are the greatest artists but also which of those selected as such one *must* choose.

This is not to argue that the aesthetic standards of the arts could not influence a comparer's choice of resolute *philosophers*. One reason why Davis (1971:6 & 8) compares Tolstoy and Nietzsche as 'notable' advocates of, respectively, pacificism and militarism is because they were 'creative artists of the very first rank'. But such standards need not enter the choice of philosophers (Kant and Hegel were not creative artists though they certainly merit comparison) and they are irrelevant to a choice between their discourses (we should not prefer Plato's to Aristotle's because the former are artistic and the latter prosaic). For what is relevant to the latter choice are such standards as category-appropriateness, logical consistency and systematic relatedness, and the use of methods like citing 'counter-examples to demonstrate a flaw in a thesis', showing that 'the theory is inconsistent', and revealing '*petitio principii*, vicious infinite regression and circularities'. (Smith, 1988:258-267) Moreover, contrary to the tenor of Steiner's claim that the 'choice' between Tolstoy and Dostoevsky as, in effect, both novelists *and* philosophers 'foreshadows what existentialists would call *un engagement*; it *commits* the *imagination* to one or the other of two radically opposed interpretations of man's fate, of the historical future and of the mystery of God' (Steiner, 1980:11, my italics), the centrality of *critical reason* to philosophy means that comparers can, do and must use such standards and methods in choosing between philosophical arguments, including those that support imaginative commitments to different world views. One result of comparisons in which there are conceptual and theoretical critiques of one or both of the philosophies can thus be 'inquiries - new normal discourses' in the not unimportant senses that (more) 'rational agreement *can* be reached' (Rorty, 1979:316, my italics) over where they agree and differ (as with Fell's [1979] final confrontation of Heidegger and Sartre), over the flaws in them ('sociologising philosophy' as 'represented' by Wittgenstein and others must thus try to answer Munz's critique of it) and over where to seek the solutions to problems (Thompson's [1981] study of Ricoeur and Habermas has furthered the debate on the interpretation of action).

This brings us to resolute *scientists* and their theories, which have been *the* focus in most of the debate concerning incommensurability and comparison. As was pointed out in the first article, even though Kuhn and Feyerabend introduced the notions of incommensurable viewpoints and incommensurable theories, they still allow that comparisons of such viewpoints and theories can be undertaken. Indeed, Harold I. Brown (1983:23, my italics) goes so far as to claim that they were 'trying' to make the point that 'incommensurable theories

can be genuine competitors capable of being compared in a *rational* and *objective* fashion'. Yet the degree to which, if at all, Feyerabend, for one, accepts *this* kind of comparison is open to doubt. For he (1978:69, my italics) insists that the necessary 'transition to criteria not involving content ... turns theory choice *from* a "rational" and "objective" routine *into* a complex decision involving *conflicting* preferences and *propaganda* will play a *major* role in it, as it does in all cases involving *arbitrary* elements'. As for Rorty, although he (1987:40) allows for 'rationality' in the sense of 'reasonable', his call (1987:50, my italics) is for 'only the most *tenuous* and *cursory* formulations of criteria for changing our beliefs, only the *loosest* and *most* flexible standards'.

But what are the criteria or standards that can be employed in the comparisons of, in mediating between, scientific theories? Feyerabend himself (1978:68 fn. 119) includes among those *not* involving content:

(i) linearity, coherence, 'number of facts predicted' and 'conformity with basic theory (relativistic invariance; agreement with basic quantum laws) or with metaphysical principles (such as Einstein's "principle of reality")'.

Realist philosophers, on the other hand, include both these kinds of standard *and* those involving content. We thus have cited and discussed such standards as:

(ii) empirical confirmation, logical fertility, extensibility, multiple connection, simplicity and causality (see Carl R. Kordig, 1971:107-111);
(iii) observational success, observational nesting, fertility, track record, inter-theory support, smoothness, internal consistency, compatibility with well-grounded metaphysical beliefs, and simplicity (see W. H. Newton-Smith, 1981:223-232); and
(iv) logical structure, metaphysical compatibility, existential success and 'explanatory and classification schemes that encompass a greater variety of phenomena'. (See Rom Harré, 1986:230-231).

Although it is not possible to go into any detail about these variously different, identical or similar criteria or standards, it can be admitted that their overall

number and even the characteristics of some of them - above all, that of metaphysical compatibility - leave an opening for (Feyerabend's) propaganda to play a role in theory choice. Yet the history of science provides no grounds for claiming that it 'will' or has to play a 'major' part in the choice between theories. Indeed, the existence of a scientific community, Lysenko's promotion of Lamarckism and the Nazi notion of 'Jewish physics' illustrate why it is both meaningful and necessary to allow for the declension: we are scientists, you are an ideologist and they are out and out propagandists. Even more important is the fact that the criteria or standards that are accepted and applied by the scientific community are neither Feyerabend's 'arbitrary elements' nor Rorty's 'tenuous and cursory formulations'. For they range from coherence/logical fertility/internal consistency/logical structure, which is integral to dialectical criticism, philosophy and science, to, say, simplicity, multiple connection and metaphysical compatibility, which are involved in conceptual critiques, philosophy and science, and to causality, extensibility, number of facts predicted and empirical confirmation/observational success/existential success, which are relevant to technical criticism and crucial to theoretical critiques and science. To be sure, even in respect of the latter, individual scientists may be 'flexible', as Rorty terms it, in the application of standards. For instance, in response to Walter Kaufmann's paper of 1906 announcing that 'the measurement results' of his experiment were 'not compatible' with Lorentz-Einsteinian relativity theory, Einstein fell back on the criterion of extensibility. (See Gerald Holton, 1973:235) Although this exemplifies why there is no *short* run 'ultimate test' (Newton-Smith, 1981:223) of empirical confirmation/observational success/existential success, this criterion or standard has a greater 'order of strength' (Harré, 1986:230) in 'the long run', to add Newton-Smith's qualification (1981:224), than not only coherence and metaphysical compatibility but also extensibility and the like[14].

In fine, a comparer of resolute scientists such as Newton and Einstein can accept Brown's conclusion that 'the admitted difficulties of comparing and choosing between incommensurable alternatives do not provide any evidence against the claim that, from time to time, such comparisons and choices *must* be made', and could therefore at least discuss - though Lerner does not - how, for instance, the test of measuring the deflection of light by the sun both can

14 As Harré (1986:230) expresses his criterion of existential success, 'we prefer a theory-family which has promoted successful "search and find/fail to find" programmes over one which has not'.

and did 'provide relevant [confirmational/observational/existential] evidence for *mediating*' the dispute between Newtonian physics and general relativity[15] (Brown, 1978:22-23, my italics)

But what of the *socio-historical* sciences? Can and do comparers offer 'rational and objective' mediations between the theories of resolute thinkers who have contributed to them? Foreclosing this possibility is certainly a consequence of some arguments that are even more radically relativistic than Feyerabend's. Consider, for example, Richard Ashcraft's contention, which he advances in his confrontation of Marx and Weber, that 'the adequacy of any explanation of how or why the social system functions as it does - whatever elements it may share in common with its rivals - will in the end depend upon its relationship to the socially formed interests of a particular group'. (Ashcraft, 1972:165-166) Another comparer of Marx and Webter takes this logic one stage further, albeit minus the Marxian stress upon 'interests'. For Löwith (1982:105) believes that 'not only Marx but also Weber cannot be refuted on the basis of so-called "facts", but only in that "struggle of the gods" [to which Weber refers], of fundamental and consistent standpoints, even though the struggle is carried on with the means of science'. Now, if comparers want to treat the thinkers simply as political figures with *Weltanschauungen*, then they can indeed battle against one or both of them on the basis of their interests and standpoints and even by using scientific knowledge. Nor have we failed to recognise that these bases of comparers and other investigators not only help them to focus upon varied objects but also may allow some of them to see more things than others.

Yet that neither the explanatory adequacy nor the refutation of the thinkers' *scientific* theories is dependent upon the interests and standpoints of the inquirers is indicated by Marx and Weber themselves. Marx (1969:119) was thus vehemently opposed to those, like Malthus, whom he thought seek 'to *accommodate* science to a viewpoint which is derived not from science itself (however erroneous it may be) but from *outside, from alien, external interests*'. And lest it be objected that, for him, the interests of the working class could be

15 This also illustrates the general point that whereas, for Feyerabend (1975:223, my italics), 'the *content* classes of certain theories are incomparable in the sense that none of the usual logical relations (inclusion, exclusion, overlap) can be said to hold between them', the content of competing theories can be shown, if not by the theories themselves then by the mediator's theory, to involve levels of areas of object overlap/invariance.

derived from his scientific theory, it must be remembered that even *if* he accepted this, he was still firm of the belief (1976:97) that science itself is compromised, nay, eliminated whenever consideration of that which is 'useful' to, say, a class replaces or has priority over that concerning whether theorems are 'true'. Similarly, Weber not only treated the 'struggle of the gods' as inherent in *non*-scientific criticism[16] but also explicitly envisages the time when research 'will consider the analysis of the data as an end in itself. It will *discontinue* assessing the value of the individual facts in terms of their relationships to ultimate *value*-ideas'. (Weber, 1949:112, my italics) Although it is problematic what viewpoints can be 'derived' from science and necessary to be critical of a positivistic emphasis upon 'data' and 'individual facts', this is another instance where one must let Marx's and Weber's epistemological and, yes, *moral* arguments inform one's approach to the comparison of these and other socio-historical scientists. For they intimate that neither refutation nor 'adequacy [of explanation] *is* a *relational* term' (Ashcraft, 1972:165, my italics) or *should* be a 'relational' term in the sense of being based on the interests and standpoints of individuals or groups. Using Rorty's language, the fallacy is that whereas Ashcraft and Löwith tacitly presuppose that one can 'rationally agree' on what the group interests and basic standpoints are, if adequacy of explanation and refutation are presumed to be based on the inquirer's own interests and standpoint, then theory choice would have to be conducted by directing discussion away from any 'conflict' between the thinkers' 'statements' first to their interests and standpoints then to the comparer's then to those of any critic of the comparer's theory choice and so on in a vicious infinite regression. Contrary to this, the adequacy of claims as to the explanatory power or refutation of the thinkers' theories necessitates, as even Mannheim's relationsism implicitly allows (see my first article and Mannheim, 1936:254-256), a concern with the relationships between the theories and the object(s) of inquiry.

From this it should not be inferred that theory choice in the socio-historical sciences, any more than in the natural sciences, immediately involves the standards of causality, extensibility, number of facts predicted and empirical

16 According to Weber (1949:60), 'every meaningful *value-judgement* about someone else's *aspirations* must be a criticism from the standpoint of own's own *Weltanschauung;* it must be a struggle against *another's* ideals from the standpoint of one's *own*'. In terms of logic, this claim involves one (Nietzschean) 'must' too many, that is, the 'criticism' neither entails nor implies the 'struggle'.

confirmation/observational success/existential success. For a start, some comparers may quite simply not be interested in the explanatory power or refutation of the theories but rather in the epistemologies and methodologies that inform and underpin them. Jürgen Kocka (1985:135), for example, offers a 'confrontation of Marx's and Weber's philosophies of science and methodological conceptions'. Although this is a worthy comparison in several respects, including its adherence to our injunction to apply each thinker's conceptions and standards of critique to the other's ideas, his attempted 'partial mediation' (1985:135 & 158) of Marx and Weber may well not meet with full rational agreement if, like this writer, one judges that he has neglected those very aspects of their thought that we have been reflecting upon and, as a result, that he is mistaken in identifying (1985:135) the initial positions of Marx and Weber as, respectively, 'authoritarian dogmatism' and 'non-committal decisionism'.

More significantly, to admit, as we have done, that epistemologies and methodologies 'inform and underpin' theories, and that concept- (and category-) construction is integral to the production of theories, means both having to comprehend each theory in the light of its concepts, a methodology and an epistemology, and drawing upon all those other standards to which reference has been made even if, be it noted, the thinkers did not (always) employ them. That the former is required is illustrated by our stress upon preconceptions, presuppositions and problematics, and by Duncan (1973:14) pointing out that 'although particular testable assertions can be drawn out from the theories [of Marx and Mill], in general the parts can be understood only in relation to the[ir] larger conceptual framework'. That the latter injunction is necessary is exemplified by Marx's declaration that 'the task' for a 'historical, social science' is 'the grasping of real relations' and not 'the dialectic balancing of concepts'. (Marx, 1973:90 & 106) For this must be corrected to '*the* task ...', thereby not excluding the need for 'the [coherent] dialectic balancing of [essential or useful] concepts'. In other words, the socio-historical sciences, like aspects of the natural sciences, are not exclusive of the need for hermeneutics, philosophical work, non-empirical standards and dialectical criticism.

Granted, too, that even as regards the issue of whether and, if so, how and to what extent theories have grasped or explained 'real relations', the mediation between them in the sense of the production of *commensurable* results is

always less common and more problematic, even in the medium term, in the socio-historical sciences than in the natural sciences. One reason for this is that changes in history resulting from people drawing and acting upon practical conclusions from socio-historical theories render more of the latter than natural scientific theories of ideological relevance. Another reason is that the complexity, diversity and variability of the objects of the socio-historical sciences require that *the* method of the natural sciences, the hypothetico-deductive/deductive-nomological one must be replaced, or at least supplemented, by the less rigorous 'hypothetico-suggestive' (Ernan McMullin, 1980:83) and statistical-probabilistic (see Carl G. Hempel, 1959:350) approaches[17]. Indeed, just as Hilary Putnam (1983:189-190) quite rightly points out that it is 'simply unreasonable' to view 'philosophical truth' as 'publicly demonstrable as scientific truth', so is it the case that 'truth' and even plausibility are not as 'publicly demonstrable' in the socio-historical sciences as in the natural sciences.

But still 'publicly demonstrable'! Notwithstanding the comparative rarity in the former sciences of eventual compelling decisions of a kind like that in favour of general relativity as against Newtonian physics, and even if there are more disagreements in them than in the latter sciences over attempted mediations between the thinkers' epistemologies, methodologies and conceptual frameworks, the comparer's choice concerning the explanatory power or refutation of the theories is not a more pertinent province for either Feyerabend's (mere) 'preferences' for different (non-content referring) criteria or Rorty's hermeneutics to the exclusion of epistemology (save as discourse). Rather can and must the choice be based on, firstly, conceptual and theoretical critiques. Without these it is impossible to find out whether the concepts of the theories are useful or essential or neither, and whether the hypotheses and explanations are adequate or not. Precisely because they are integral to theoretical critiques and concern the object(s) of inquiry, there can and must indeed be, secondly, application of those 'stronger' and specifically scientific epistemological standards of causality, extensibility, number of facts predicted and empirical confirmation/observational success/existential success.

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17 In other words, the usual goal cannot be for *deductions* from statements asserting initial conditions *and* universal hypotheses, but rather for, respectively, 'tentative analogies, possible consequences' in the realm of history and 'probability hypotheses' concerning the occurrence of events (and the like).

More 'rational and objective' mediations between socio-historical theories are not only theoretically possible but also being more frequently striven for by those (still) minority of comparers who engage in both philosophical *and* scientific work. Testimony to this include Mattick's comparison of Marx and Keynes, Duncan's of Marx and Mill and a few recent comparisons of Marx and Weber. However, that debate must needs be continued can be exemplified by briefly commenting on what some comparers have argued about Marx and/or Weber. For instance, that an adequate interpretation of a thinker's 'conceptual framework' is a necessary (but not sufficient) condition for an adequate test of his or her theory is illustrated by Duncan's conclusion as to Marx's 'failure' at the level of his 'economic theory'. (Duncan, 1973:302) For one of its premises is the mistaken one that, for Marx, everyone is 'subjected' to a 'confining and rigid economic system'. In other words, there is no allowance for what we have seen is Marx's far more extensive and flexible material and social forces of production-structure-superstructure-social consciousness model of social formations. But that progress is being made both in the interpretation of Marx and in the movement toward the application of 'stronger' standards to his theories is shown by Robert Hanneman and Randall Collins' 'stimulation model' of Marx's conception of 'a *dynamic* relationship' between the economy *and* the state, which stimulation, even though 'not designed to test' a theory, 'shows what a theory is capable of. It shows whether, starting at a given point and following certain processes, one can arrive at a given outcome'. (Hanneman and Collins, 1987:96 & 98, my italics).

Although Weber did not synthesize his 'type concepts' and knowledge of 'generalised uniformities of empirical process' (Weber, 1978:19) into a theory of social formations capable of being operationalised in this way, comparers of Marx and Weber can seek to choose between their ideas at either the broader level of their accounts of the emergence and development of capitalism or the more particular level of their analyses of the events and trends in individual countries. As regards the former, all we have are predominantly dialectical criticisms and conceptual critiques that *culminate* in such claims as 'Marx's causal factors were retained in Weber's account but were transformed by being given new roles' in a more comprehensive 'story of the rise of rational organisation and the inexorability of its dominance of the modern life-world' (Stephen P. Turner, 1985:185), and as, on the one hand, 'without the base of Marxist conceptualizations, Weberian class theory lacks even an agenda' and,

on the other, Weberian 'concepts of class closure and the institutionalization of class interests' must be employed as 'theoretical tools' within Marxism. (Morton G. Wenger, 1987:63-64) But at the latter level there is an endeavour that *sets out* to 'test the logic of the theory of history by analyzing and comparing Marx's historical writings on revolutions in France, particularly in **The Eighteenth Brumaire of Louis Bonaparte** (1852), and Weber's writings on the Russian Revolution, especially in **Zur Lage der bürgerlichen Demokratie in Russland** (1906)'. (Lawrence A. Scaff and Thomas Clay Arnold, 1985:192) However, the trouble with this test is that it not only proceeds on the erroneous assumptions that Marx believes in 'the evolutionary laws of motion', 'the historical laws of class struggle' and 'the mechanistic laws of motion'[18] (1985:194, 207 & 210) but also is not of what could well be mediated between, namely, Weber's *and* Marx's writings on changes in Russian society. Indeed, the lesson of this example and some others is that although not every facet of the method of comparison either has to satisfy Gadamer's 'idea of historical knowledge' or makes 'things contemporary *as a matter of course*', the testing of socio-historical theories quite obviously requires historical knowledge and must be of theories that are designed to explain objects that are more or less 'contemporary'.

VI. The Form of the Comparison

Emphasising the importance of content in this way and others is one good reason for taking issue with the one-sided character of Graf Yorck's assertion: 'Comparison is always aesthetic, it is always concerned with the form'. (Quoted in Gadamer, 1979:206) On the other hand, our admission that all comparers of resolute thinkers are mediators in the sense that they *'form* a connecting link' between them means that it is necessary to single out the form of the comparison as our final problem. For what we find - and will always find - is that the comparer's purpose, the field(s) in which the thinkers excelled and the number of methods that the comparer chooses to integrate into the text of the comparison will all condition the employment of forms of comparison that have varying advantages and disadvantages.

Comparers who are concerned with offering confrontations of philosophical ideas and who find it unnecessary to introduce the regressive method into their

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18 Whereas the authors treat these conceptions as held by the Marx of 1852[], even the Marx of Capital is more modest and thinks in very different terms. for he (1976:92, my italics) seeks 'to reveal the *economic* law of motion of *modern society*'.

texts may thus choose the *dialogue* form. Nowadays, of course, this need not be the 'mere' literary technique of a Plato or a Hume; dialogues between philosophers can be broadcast and/or printed. Those between A. J. Ayer and Arne Naess, Karl Popper and John Eccles, Noam Chomsky and Foucault and Leszek Kolakowski and Henri Lefebvre, for instance, were chaired and reflected upon by Fons Elders (1974). But that the absence of such dialogues is no barrier to the use of the dialogue form is well testified by Maurice Cranston's Political Dialogues. For although his dialogues are '"imaginary"' in the sense that they are 'constructions' for which he is responsible, 'they are built on the real beliefs and words of the men who figure in them'. (Cranston, 1968:ix) As for the strengths of this form, when he suggests that it 'seems to be a natural manner in which philosophical ideas can be articulated and explored' (1968:ix) he is intimating how it can render comparisons both direct and dramatic. Yet the trouble with the dialogue form is that it does not allow the inclusion into the text of some of the sorts of knowledge and standards to which reference was made in the last section. Admittedly, a comparer using this form could reply, 'my readers, many of whom will be well educated, do not need my presence as a historian who takes them back to the thinkers' original conditions or as a critic who presents and dissects their systems'. But this would be to forget that even though the dialogues may well not be 'mere fabrications' (Cranston, 1968:ix), they still involve the *retreat* and *not* the *absence* of the comparer as mediator. In one of Cranston's dialogues, for example, it is not only Marx's 'real beliefs' but also Cranston's beliefs about his beliefs that are used to build Marx's replies *and* non-replies to Bakunin's arguments. More importantly, that presumption ignores how even the knowledgeable may need to be reminded of old findings or informed of new discoveries. Mindful of this, practically all comparers rightly incorporate into their texts what they see as the most relevant results of their employment of the regressive, progressive and hermeneutic methods.

One form in which this is done can be described as *continuous* comparison. The form itself involves a clearly 'present' comparer comparing the thinkers throughout the text; there is relatively little separate discussion of them. It may be thought that this is a form found only in papers (such as a few of those in Giorgio Tagliacozzo's Vico and Marx: Affinities and Contrasts)[19]. Yet this would be mistaken. Against a background of sketches and even 'Annals' of

19 For instance, those by Terence Ball (1983) and Donald Phillip Verene (1983).

their times, Green's book (1974:Part 1) provides a number of continuous comparisons of the families, lives and minds of Frieda and Else von Richthofen and of Lawrence and Weber. But the prime illustration of the use of this form is Bevan's Marx and Burke, which is largely continuous comparison of Burke and Marx in chapters dealing with their *Weltanschauungen* and 'historical empiricism'. Now, although this form involves the advantage of a direct comparison of the thinkers, Bevan herself is drawing attention to its weaknesses when she admits that her thinkers 'lived in very different times' (Bevan, 1973:171), when she refers to 'many Burkes and many Marxs' (13) and when she sometimes finds it necessary to devote several pages to an exclusive interpretation of one of the thinker's central ideas (for instance, 86-95 on Marx's conception of social change, and 116-122 on Burke's arguments concerning society and representation).

Faced with these sorts of problem, most comparers decide to adopt as their primary form *partition* comparison. Just as Theda Skocpol and Margaret Somers (1980:178) point out that the guiding concern in 'comparative history as the contrast of contexts' is 'that the historical integrity of each case as a whole is carefully respected', so is this form a matter of 'present' comparers trying to preserve the contextual *and* textual integrity of each thinker's historical situation, social position and discourses - whether these be artistic, philosophic or scientific - by at least the first three methods being used in sections of chapters, in chapters, in parts of books or even in books that focus primarily upon one of the thinkers. Exemplifying these possibilities, we have Lerner (1973) devoting one section of his chapters to Newton and another to Einstein, Osborn (1964:ix) manifesting her concern with 'the historical development' of the thought of Rousseau and Burke and with the need to set out their 'fundamental principles' by having some chapters on each of them[20], Giddens (1971:viii) following through his conviction that attention to 'the social and historical "rooting"' of Marx, Durkheim and Weber is a requirement for an 'adequate interpretation of their writings' by devoting parts of his work to each of these thinkers, and, as we have already had occasion to note, Alexander finding it necessary to have volumes on Marx and Durkheim, Weber and Parsons.

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20 Although C.P. Courtney is equally cognisant of such considerations, the form of his work on Montesquieu and Burke differs from Osborn's comparison precisely because he is less concerned with Montesquieu than with 'the origin of Burke's ideas' and 'the influence of Montesquieu on Burke'. (Courtney, 1963:xiv)

Partition by no means exhausts these comparisons, however. If it did, drawing out the similarities and differences between the thinkers *would* be left entirely to the reader. Needless to say, comparers are not prepared to abnegate their intellectual *raison d'être* in this way. Accordingly, continuous comparison is introduced. Lerner (1973) does so at the end of all but one chapter and in his final chapter; Lev Shestov (1969) uses it in the closing sections of his essays on Tolstoy and Nietzsche, and Dostoevsky and Nietzsche; Steiner (1980) and Osborn (1964) employ it in places in their earlier chapters and in their concluding chapters; W. von Leyden (1982) includes it in both of the parts that make up his comparison of Hobbes and Locke; Duncan (1973) and Giddens (1971) incorporate it in the last parts of their books; and Alexander introduces it in all three of his aforementioned volumes.

So far we have been primarily concerned with the relationship between form and the integration into the text of the regressive, progressive and hermeneutic methods. But what of the methods of criticism and of critique? Well, each of the forms allows their integration. Dialogue can do so through the inclusion of the criticisms and/or critiques that the thinkers gave, or could in principle have given, of one another's ideas. Continuous comparison enables comparers like Bevan who have a critical and theoretical intention to run together their evaluations and *both* thinkers' actual or possible mutual criticisms and critiques. Of course, the rationale of partition precludes the latter; comparers are limited to offering their dialectical and technical criticisms, and their conceptual and theoretical critiques of each thinker's discourses taken *separately*. However, the point about the combination of partition and continuous comparison is that it permits the running together of thinker, *critical* mediator and thinker. To be sure, Lerner's comparison again illustrates how that which is allowed may not be taken up. And Steiner's concluding chapter and even Shestov's closing sections lend very little support to Adorno's contention that the essay is the 'critical form *par excellence*'. (Quoted in Gillian Rose, 1978:15) Yet those like Duncan and Giddens who want to construct an 'intellectual bridge' between the thinkers' 'systems' and to evaluate the flow of philosophical and/or scientific goods crossing it both must and do include *critical* continuous comparison.

Other critical mediators not only utilise the second and third forms but also seek to add to the quantity and quality of intellectual goods as they either move

on to articulate their own 'systems' or depart from their own 'systems'[21]. An inkling of this endeavour can be gained by simply outlining the succession of chapters, parts or volumes in a number of comparisons. Goff (1980) moves from the chapter, 'The Critique of the Sociology of Knowledge' via ones on Marx and Mead to the chapters, 'Marx and Mead: Towards a Critical Sociology of Knowledge' and 'Conclusion: The Critical Perspective'. Mattick (1969) begins with the chapters, 'The Keynesian Revolution' and 'Marx and Keynes' and then provides many chapters on Marx's political economy and a Marxian analysis of various contemporary events and trends. Munz (1985) proceeds from chapters on the historicity and conditions of knowledge to those on Kuhn's and Rorty's (Wittgensteinian) ideas and then to ones on (Popperian) evolutionary epistemology and the evolution of evolution. Fell (1979) opens with the chapter, 'The Problem of Phenomenological Ontology', has three parts dealing with Heidegger's and Sartre's early and later ontologies, and a fourth part which both confronts their thought and outlines 'the legacy of the confrontation for a future phenomenological ontology'. (1979:361) Thompson (1981) follows his 'Thematic Exposition' of Wittgenstein and ordinary language philosophy, of Ricouer and hermeneutic phenomenology and of Habermas and critical social theory with a 'Constructive Critique' part which contains three chapters, each of which discusses a problem that had been broached in the first parts and ends with the respective section, 'Towards a Theory of Action', 'Towards a Methodology of Social Science' and 'Towards a Theory of Reference and Truth'. Finally, Alexander's four volume study (1982, 1983 & 1984) is distinctive in that he presents his 'multidimensional theory' in the first volume and then employs it in the others as the standard by which to assess the writings of Marx, Durkheim, Weber and Parsons, and to develop 'a theoretical logic for sociology'. (Alexander, 1982, Volume 1:123 & 126)

The question then arises, do these six comparisons have an additional form? And one label that springs to mind is 'dialectical'. Now, there is indeed a comparison, Reuben Osborn's Freud and Marx with the subtitle A Dialectical Study, which was no doubt chosen because the book was seen as establishing 'the dialectical unity' of the Marxian and Freudian 'dialectical'

21 Rubinstein is one comparer who certainly moves on to outline his own metatheory (in opposition to 'objectivism' and 'subjectivism' in social science), but who proceeds to it on thebasis of little or no criticism or critique of either of his thinkers. (See Rubinstein, 1981: Part 2)

approaches to 'the dialectical character' of, respectively, historical and mental life. (Osborn, 1937:25, 213 & 241) Rather than take this insufficiently critical book as our benchmark for the *dialectical* form of comparison, though, let us at least be stirred to appreciating what it involves by citing Hegel's conception of 'the Dialectical principle' as 'the indwelling tendency' for thought to negate 'the one-sidedness and limitation of the predicates of understanding' and, therefore, as 'the life and soul of scientific progress'. (Hegel, 1975:116) For this form is one where the comparer's deconstructive *and* constructive purposes lead him or her to engage in criticisms and critiques of one or both or all of the thinkers in a way which starts with a philosophic or scientific problem and then involves striving to solve it either from the outset of the work or in closing sections, chapters or parts.

Understood thus, it is not necessarily associated with the dialectical things that other writers have discussed. For instance, although all of the authors of those six comparisons use dialectical criticism, none of them incorporates their comparative method into that astounding Lukácsian 'dialectical method' which is 'concerned *always* with the same problem: knowledge of the historical process in its *entirety*' (Lukács, 1971:34, my italics) and only Mattick uses 'the dialectical method' in Marx's less ambitious sense of one that, amongst other things, illuminates 'the *special* laws' that regulate 'a *given* social organism'. (Marx, 1976:192, my italics[22]) Nor does the dialectical form have to be a result of 'dialectical thinking', which Fredric Jameson (1971:240, my italics) defines as 'the attempt to think about a given object on one level, and *at the same time* to observe our own thought processes as we do so'. But just because such observations would in fact occur before or after thought about the object does not mean that the comparer cannot *write* what Jameson (1971:53) calls 'dialectical sentences', the property of which is that 'self-consciousness' is 'inscribed' in them. Granted, such sentences can be employed in all forms save the dialogue one and must not be too numerous lest the comparer embark upon the Tristram Shandy venture of being too concerned with self to the neglect of what is being thought about. Moreover, given that comparisons using the dialectical form are likely to be historically significant ones - particularly in the sense that the thinkers contribute, even if from opposite standpoints, to the same tradition, for us it will not be the case that the 'strength' of such sentences '*increases* proportionately as the realities linked are *distant* and

22 This is Marx quoting and endorsing parts of I. Kaufman's critical review of the first edition of Capital.

distinct from each other'. (Jameson, 1971:53-54, my italics) Yet if dialectical sentences are seen as the way in which the comparer as interpreter records the role of his or her preconceptions, presuppositions and problematics in the selection and treatment of the thinkers, then they are especially necessary in that form, the dialectical form in which the comparer is most preoccupied with producing commensurable results, with furthering a tradition of thought.

It cannot be denied that the dialectical form of comparison, even more so than the partition and continuous comparisons forms, is an instance of (chosen) content determining the selection of methods and form. For if the comparer as mediator seeks to discover things about the processes of thought and of history, then this will demand use of the methods of criticism and of critique, and of the dialectical form. Those methods we have been able to identify and examine through what turned out to be our own very selective and tentative comparison of Marx and Weber. That form has its strength in clear and distinct consideration of a problem and a solution to it, and has its basic weakness in a mistaken formulation of, or solution to, the problem.

No existing comparison of resolute thinkers allows us to follow the Hegel of the Aesthetics (1975:440) and speak of it as content and form going 'steadily hand in hand in their advance to perfection'! Nevertheless, the content and sometimes the forms of all such comparisons have, albeit to varying degrees, advanced our knowledge of the processes of thought and even of history. Indeed, it was this contribution that prompted this inquiry into the problems of comparing resolute thinkers. As for the latter's content and form, they are, of course, such that the solutions to each of the six problems fall within the main sections of the overall paper. Yet one general conclusion must be drawn. Given the advance represented by comparisons of resolute thinkers, and if our solutions to the problems of comparing them are sound, then there are two decisive respects in which we *can* be, yes, *for* mediators.

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DEBATE: A comment on William Tunmer's review essay (see Vol.4(1)) on Mark Olssen (ed) *Mental Testing in New Zealand*

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The recent Review Essay by Tunmer (1989) includes a selective and generally uncritical synopsis of aspects of *Mental Testing in New Zealand*, Olssen (1988). While a brief discussion of text bias is included, the focus is on an interpretation of differences in mental test scores as a function of Matthew effects in reading, after Stanovich (1986). A brief overview of the *Test of Scholastic Abilities* (TOSCA), NZCER (1981), is included, but this is secondary to the review of emergent literacy and a posture for the moral "high ground", with a tale about the responsibilities of gun manufacturers, which presumably is meant to be a message for the TOSCA publishers.

The main points to be developed in this Comment on Tunmer's Review Essay are that the relationships between "emergent literacy" and TOSCA are not necessarily uni-directional as suggested, and that there is far greater similarity between theory in cognitive psychology and what I take the ill-defined term "mental test theory" to mean. This would be readily apparent to readers if mental test theory and cognitive theory were judged by the same techniques, evidence and standards. As will be demonstrated, the inconsistent use of evidence to show the alleged superiority of theory from cognitive psychology over theory from psychological measurement, is a major criticism. Another point of criticism is the attempt to establish a direct link, no matter how tenuous, between early intelligence tests and TOSCA, but this particular theme adds nothing to the argument that sees TOSCA-performance as a function of Matthew effects. The third major criticism is a legacy from *Mental Testing in New Zealand*, particularly contributions from Olssen, Codd, Dioriro and Ballard, and this is a tendency to attribute to the TOSCA manual, interpretations, views and a rationale that differ from the original, and could barely be justified by any reasonable interpretation of the full text. This feature adds little to the Matthew thesis, which is the main contribution of the Review Essay, and if anything, detracts from its structure and argument.

It would be wrong to give the impression that the Review Essay has only negative features. On the whole, the treatment of the Matthew effects in reading is timely, valuable and informative, if a little lop-sided in stressing quantity (Nagy and Anderson 1984) over quality of associated verbal interactions. Nor can the irony of appeals to undefined terms e.g. "metalinguistic abilities", "verbal processing skills", "advantageous early reading experience", "small differences in reading ability" and "reciprocally facilitating relationships" (p.71), pass without comment. This is especially cavalier given the criticism of "mental test theory" (p.67-69).

Before further elaborating on the points of contention, it might be helpful if, as a co-author of TOSCA, I state clearly my views on some fundamental aspects of this test. Much of this will stand in stark opposition to Olssen (1988). TOSCA is *not* a measure of inherited potential, it is *not* a modern day embodiment of eugenics, it owes nothing to Galton, Goddard, or Thompson. TOSCA is *not* a measure of general intellectual ability, it does *not* report an IQ, is *not* an instrument of oppression, nor is it biased for individuals by virtue of their ethnicity, gender, social class or age, but it does differentiate between those who master its content to varying degrees, and it did show marked inequalities within its school-age standardization sample. These inequalities were *not* evenly distributed across this sample, were *not* simply a function of TOSCA's construction, nor is TOSCA per se, responsible for the distribution of knowledge, skills, abilities or other cognitive or conative influences reflected in its scores. It is suggested that it is the demonstration of these inequalities that has motivated critics like Olssen (1988), not their existence, as they have been recognised decades before TOSCA (Shuker p.104) and will long out-last this test, which despite Nash's 1985 plea for withdrawal, (Olssen 1988, p.14) is still seen by many New Zealand schools as useful.

Following on these various disclaimers in respect of TOSCA, a fair question would be what in fact are its major characteristics? TOSCA is designed to measure a sample of cognitive skills and knowledge likely to underpin the academic side of our school curriculum, and thus it is referred to as a test of scholastic abilities. Recent factor analytic studies by St George and Chapman (1984) and Reid and Gilmore (1989) strengthen this interpretation. TOSCA provides a snapshot of present functioning in respect of its content, is positively related to other measures of academic achievement, shows stable and enduring qualities for groups as measured by predictive validity studies

Reid and Gilmore (1989) and is valid for its legitimate uses, and reliable (Reid, Jackson, Gilmore, Croft, 1981). The skills and abilities sampled by TOSCA are most certainly learned and are not fixed or immutable. TOSCA performance will be influenced by a host of learned skills and in turn, will influence the learning of additional skills. To cling to the notion that TOSCA somehow causes educational "success" or "failure", when undeniably the relationship is circular and reciprocal, would be the ultimate excess of a doctrinaire approach to educational measurement.

The 'snapshot' obtained by TOSCA in 30 minutes contributes information of a known quality to a composite picture of a school learner. If acted on professionally, this information should contribute to greater success in school learning, however success is defined. This point which is of prime educational importance has been lost sight of by Olssen et al (1988), as they fight their way through a theoretical mire of their own making, showing little understanding of, or concern for, the practice of education today. To some extent at least, one might reasonably have expected the Essay Review to have been alert to this.

Mental Test Theory, Cognitive Psychology and Matthew Effects

The review Essay does attempt to delineate the field of mental testing, as a definition of any consequence had been ignored by Olssen (1988) as editor of the text in question. Unfortunately this leads the Review Essay into difficulties as an attempt is then made to illustrate the "critical difference" between mental test theory and cognitive psychology by stating on p.64 that we *know* (my italics) by theory and research that reading comprehension and listening comprehension are causally related, but it is *assumed* (his italics) that general mental ability is causally related to academic achievement. It is not a major criticism to point out that research has also indicated a causal relationship between listening comprehension and reading comprehension and that theory in this area is in its infancy when laid alongside measurement theory. However, it is a major criticism to point out that the Review Essay adopts inconsistent standards regarding the nature and usefulness of hypothetical constructs, the adequacy of definitions, use and interpretation of correlation coefficients and the general role of theory in measurement and cognitive psychology. What is subjected to criticism in "mental test theory" (undefined) is held up as a cornerstone of cognitive psychology! A dollar each way may be a good ploy at the races, but in this forum it pays no dividends.

This inconsistency is further illustrated by the Review Essay's discussion of correlations and the use of mental test data to bolster the "Matthew effects" interpretation. It is legitimate to point out difficulties with correlations, but the essay could have gone on to remind readers, including Ballard as quoted (p.69) that a numerical relationship as represented by a correlation, does not necessarily signify a causal relationship, and that a predictive validity coefficient is an indication of a test's characteristics over time, not an index of how well a test might or might not predict an individual's later performance.

It seems pertinent to remind readers that the index of predictive validity "which fails to show anything", (Ballard p.69), is of course a correlation coefficient, which when applied in cognitive psychology or the Matthew effects, somehow becomes a legitimate measure. Furthermore, given the weakness of "mental test theory" as outlined, how can evidence from WISC-R and WPPSI, undeniably two "mental tests", be relied upon to give theoretical support to Matthew effects interpretations?

The interpretation given to Bishop and Butterworth (1980 p.72), provides a case in point. The Review Essay states *"the children who subsequently became poor readers were not at the time particularly weak in the verbal subtests of the WPPSI."* Bishop and Butterworth could also be interpreted as meaning that the *WPPSI* was either: (i) an unreliable measure at age four; or, (ii) its predictive validity was so suspect that its measurement of verbal skills at age four was poorly related to the measurement of those skills at age eight; or, (iii) the measurement of non-verbal skills at age eight was also unreliable.

Also Neale, McKay and Thompson (1979) suggest that receptive verbal skills predict later reading better than do expressive verbal skills. The *WPPSI* verbal subtests include both expressive and receptive skills, so a portion of the test is unlikely to have predictive validity in this setting anyway.

Likewise on p.73 following a discussion of Nagy and Anderson (1984) the Review Essay states that "Consistent with this suggestion is the finding that the *strength of the correlation* (my italics) between verbal intelligence and reading ability steadily increases up through the grade level." A more plausible interpretation of this strengthening correlation is likely to be found in the increasing reliability of the tests used, rather than a necessary increase in the relationship between the skills being measured. A simple research technique to

be applied to questions of this type is to correct the tests for unreliability, and check the ensuing correlations for statistical significance. However, as noted previously, we still have the situation where allegedly weak tests from both a theoretical and technical stance, are now providing evidence to bolster a theory attempting to show the weaknesses of mental tests in general and TOSCA in particular. Stanovich's "double whammy" (p.67) must now have conquered new dimensions.

It would be noted also, that aspects of the descriptive phenomena called the Matthew effects, as related to Nagy and Anderson (1984) pertain almost exclusively to 'reading mileage', i.e., number of words exposed to, without reference to the quality of teacher/pupil interaction associated with that exposure, or the relationships between the exposure and other areas of the school curriculum. It cannot be claimed with any seriousness that amount of print exposed to *per se*, irrespective of the cognitive and lexical qualities of this reading will *by itself*, account for differences in TOSCA performance. There is ample evidence in the TOSCA Manual to indicate that around 55 percent of the variance in TOSCA and *PAT Reading Comprehension* for example, is common. There is nothing new in demonstrating the relationship between reading comprehension and tests of scholastic abilities, when reading is undeniably a major scholastic skill. There is little point in asserting that TOSCA differences reflect differences in "literacy skills" rather than differences in "basic learning capacity", when the authors of TOSCA have never claimed that this is what the test measures. The difficulty faced by the TOSCA authors is that they are being criticised in part, because TOSCA correlates so well with measures of literacy achievement. Given the central role of literacy skills in schools, criticism would be more vehement, and justifiably so, if TOSCA related poorly to other measures of literacy.

My final point here relates to the discussion of "mental test theory", which remains throughout the essay, and the book surveyed in the essay, as a largely ambiguous and undefined term. Whether the reference is to writing on the structure of human abilities, measurement theory and psychometric methods, achievement test theory and construction, contemporary views on intelligence, historical views on the nature and development of intelligence, or scaling and technical problems in measurement and so on, is never made clear in either source. The position adopted in the Review Essay is that "mental test theory" is devoid of "explanatory power", "vague and poorly defined" and somehow

"inferior" to theory from cognitive psychology. To dismiss decades of writing and empirical work from veritable giants of psychology like Binet, Spearman, Terman, Thurstone, Hebb, Vernon, Thorndike, Guildford, Anastasi, Fergusson and more recently Sternberg, suggests little understanding of writing on intelligence and the contribution of this work to psychology. Then again, perhaps this approach creates a false distinction. This is not to suggest that theory or even definitions of intelligence are entirely satisfactory, but they are no less useful than in other areas of cognitive psychology, even minor ones like sentence comprehension. Tuddenham (1962) sums up thus. "Until now intelligence tests have proven extremely useful in the absence of a satisfactory theory of definition of intelligence... However fundamental progress will almost surely depend upon providing our tools with a more solid theoretical foundation."

The Essay Review's treatment of the origins of group testing is a definite improvement on the original text in that it is shorter, more objective and lacks the factual inaccuracy of the latter (for example, Olssen p.36, "...mass testing of 1.7 million men for the armed forces in World War 1"). According to Yerkes (1921) who directed the project, the figure was 1,750,000 between September 1917 and January 1919. Readers interested in scholarly treatment of the origins of group mental ability testing are referred to Tuddenham (1962). It will be clear that the origins of group testing differ markedly from Olssen's account, or the Review Essay's summary of this.

Mental Testing in New Zealand

At a more general level it is disappointing that Tunmer does not review thoroughly the book in question. A full and critical review, while a lengthy undertaking, has yet to be presented. Furthermore, important weaknesses have also been overlooked by Snook (1989) in his review. Only Haig (1988) has been alert to these in his earlier incisive and telling review.

With the exception of Flynn, McKenzie, Shuker, Nash and Codd mostly, the contributions cited in the Review Essay are either inconsequential, or palpably misleading. For example Ballard (p.220), asserts that "Reid et al (1981) simply *deem* (p.4) that their test is a measure of important abilities". What p.4 actually says on this point is, "...the test measures those verbal and numerical reasoning abilities deemed to be requisites of success in academic aspects..." A clear difference to Ballard's interpretation I contend. Diorio, in his attempt to

make an original point is even more prone to misinterpret. The TOSCA Manual (p.7) states he says, "It is unwise... to place great emphasis on the importance or nature of the test", which after Diorio's discussion (p.200) becomes "The TOSCA authors believe... that concealing the significance of tests from pupils is a justifiable way of assisting pupils to obtain their highest possible scores". McCreanor has attempted to base his discussion of bias on data gathered for his Master's thesis, and this is in his favour, but out of step with the data-free approach of many other contributors. However his analysis cannot be taken seriously when it is based on the most fundamental of all flawed statistical procedures, the averaging of percentile ranks and these from *different* Parts and Forms of TOSCA! Percentile ranks are derived scores which do not form an equal interval scale, hence adding and averaging are likely to distort the relationship between the original raw scores from which the percentiles were derived.

The strongest criticism must be directed at Olssen's own work when it claims that "...mental testing in educational contexts is primarily a political affair", and later that "...mental testing in education is not based upon neutral criteria but rather upon a definite social and moral view of the way human beings are and of the way that society ought to be", (p.29). What are mental tests in education? How is this politics manifested? What are neutral criteria? How do these articulate with Olssen's criteria? What other enterprise within education is not based on a view of what society ought to be? Are Olssen's interpretations not also driven by some belief about education and society? Who is to say that those who see a place for educational testing share common views about the nature of education and society? Questions of this type may be readily asked of Olssen and I believe, demonstrate the weaknesses in much of his account.

It may be readily demonstrated too that as a text *Mental Testing in New Zealand* contains numerous errors of fact plus conceptual inadequacies, and is based on criteria that certainly are not neutral. A reviewer with some knowledge of the field would have been quick to highlight some of these. In addition too, there are thoughtful and thought-provoking comments from Nash among others, now that he has abandoned his most strident tones.

The final point here is illustrative of the Review Essay's uncritical acceptance of much of *Mental Testing in New Zealand*. Early on the essay says that the

book, "...arose out of the controversy surrounding the publication of the Test of Scholastic Abilities (TOSCA) by NZCER..." in 1981. The "controversy" real or imagined, ideological or substantive, political and not educational, was clearly localized in the Education Departments of Massey and Otago universities and NZCER, and apart from no more than eight comments to correspondence columns over four months, was the subject of some six articles to *NZJES* from November 1983-1985, with all but two being written by staff at the previously mentioned institutions. The publication of TOSCA was clearly not surrounded by controversy of a size and scope that the critics imagine, given that it took some three years for the first substantive written comment, and a little over seven years to this latest effort.

A Ricochet from 'A Parting Shot'

In the process of rejecting totally the validity of the Review Essay's final piece of moralizing, it is tempting to ask what if any educational significance is likely to come out of the book *Mental Testing in New Zealand*. It is all very well to hide behind something called "theory", and convince colleagues that the various writings on TOSCA have been valuable "commentary". The likely reality is that these commentaries address issues and personal ideologies that are far removed from the day-to-day issues of educational practice. Critics in time will come to realize that TOSCA is here to stay for as long as schools find it a valid and useful test. The real issues are not *whether* TOSCA should be available, but *why* it may be used, *when* it is used and *how* it might be used in the best possible way. These are important issues for most teachers confronting the necessity and reality of educational assessment.

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A Reply to Croft: Ability Test Construction as Naive Retrograde Empiricism

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The major problem with Croft's commentary is that he should have begun his discussion at precisely the point that he ended it. In his next to last sentence he states that, "The real issues are not *whether* TOSCA should be available, but *why* it may be used, *when* it is used and *how* it might be used in the best possible way" (p.8). The difficulty here is that the answer to the question of whether the TOSCA should be available depends crucially on whether the test developers have provided a convincing case for its use. Although Croft asserts that the TOSCA "is valid for its legitimate uses", at no point in his commentary does he provide any indication as to exactly what these "legitimate" uses are. It is a generally accepted dictum that educational tests are useful only if they inform instruction. The critical question, then, is whether the TOSCA is, in fact, useful in making instructional decisions.

In a recently reported study on the factor structure of TOSCA, St. George, Chapman and Lamboume (1990) concluded that instruction would be better informed by other tests. Two factor analyses were undertaken in their study. The first was with TOSCA scores and results from other ability and achievement measures, and the second was with TOSCA items. Although there was strong evidence of a general "verbal-educational" factor in students' performance on the TOSCA, there was no evidence of item clustering

according to the test-development item categories presented by the test developers. On the basis of these findings, St.George et al. concluded that unless the general verbal-educational nature of TOSCA results can be shown to have educational utility, "there are compelling reasons for favouring the use of more content-specific educational achievement measures in both the placement of pupils, and the organisation of instruction" (p.35). Such a conclusion is not surprising because the TOSCA was clearly not designed as a diagnostic instrument to help teachers determine the *specific* educational needs of children. Instead, it is a rather anachronistic test that stands against the current trend towards criterion-referenced and curriculum-based assessment.

Although the findings of the St.George et al. (1990) study suggest that the scores from the TOSCA are of little or no instructional value, the major theme running through the Olssen (1988) volume is that the use of the TOSCA can actually be *harmful*. Croft may be right when he emphatically asserts that "the TOSCA is *not* an instrument of oppression" (just as a handgun in a shop is not a murder weapon). But this assertion does not preclude the possibility that the TOSCA can be *used* as an instrument of oppression (just as a handgun can be purchased and *used* to commit a murder). The argument presented in the Olssen volume, in summary form, is this. If the TOSCA measures differences in *knowledge attainment* rather than "the pupil's *capacity* to cope with the abstract manipulation of the verbal and numerical symbol systems of mainstream New Zealand society" (Reid et al., 1981, p.4, emphasis added), then using TOSCA results for pupil placement would discriminate negatively against students who have had unequal access to the skills and knowledge necessary for succeeding in school. Placing already disadvantaged students into low "capacity" streams would only compound the situation, since capable but poor or culturally different children who are misclassified to lower streams would be further deprived of the chance to do well in school. The use of a test like the TOSCA could also lead to self-fulfilling expectations about what children are capable of achieving, especially if it is incorrectly assumed by teachers and school administrators that the TOSCA measures some underlying "capacity" for learning that determines ultimate level of academic attainment.

The major weakness in the argument presented in the Olssen volume, however, is the assumption that the TOSCA measures differences in knowledge attainment. As the developers of the TOSCA point out, the "vast majority" of test items do not assess skills that are *directly* taught to children (Reid et al.,

p.4). For example, it is not a part of the regular school curriculum that children are taught to solve problems involving "disemvowelled" words, anagrams, words with scrambled letter order, sentences with scrambled word order, sentences written backwards, run-on sentences, etc.

Nevertheless, solving such problems may require component skills that are acquired or improved as a *consequence* of learning other skills that are directly taught in school. Thus, the major aim of the review essay by Tunmer (1989) was to draw attention to recent research that documents the consequential nature of achievement-related skill acquisition that follows from learning to read. Examples of some of the positive "spinoff" effects of literacy achievement are increased vocabulary, syntactic knowledge, general knowledge, metalinguistic abilities, speed of verbal encoding and semantic memory access, ability to generate orthographic images, knowledge of the orthographic cipher, ability to form and maintain a phonemic code in short-term memory, and ability to monitor comprehension processes.

Several specific examples were given in the review essay to demonstrate how the positive spinoff effects of reading achievement can improve performance on the TOSCA. For example, the TOSCA includes several items that measure vocabulary knowledge. But research indicates that the major determinant of vocabulary growth during the school years is amount of free reading, not "capacity" to cope with abstract symbol manipulation. Since skilled readers not only read more than less skilled readers but also tend to read more difficult materials, their vocabulary knowledge will be generally greater as a result. If the TOSCA measures vocabulary growth, and if reading achievement is the primary factor influencing vocabulary growth during the school years, then we would expect the TOSCA to be positively correlated with both vocabulary and reading achievement. Consistent with this suggestion, St. George et al. (1990) found that TOSCA correlated more highly with PAT Vocabulary ($\underline{r} = .78$) and PAT Reading Comprehension ($\underline{r} = .76$) than with any of the other achievement and ability tests included in their test battery.

As a consequence of Matthew effects in literacy, the time limit on the TOSCA also greatly advantages better readers. The skilled readers' superior speed of verbal encoding and semantic memory access, and their superior ability to maintain verbal material in working memory, enables them to process printed information more quickly and efficiently than poor readers. This, in turn, frees

up more cognitive resources for allocation to higher order cognitive functions, such as solving verbal and numerical reasoning problems presented in *linguistic* form. It is therefore not surprising that the TOSCA correlates so highly with achievement tests in which *verbal* skills are involved (see St.George & Chapman, 1983; St.George et al., 1990, for supportive evidence).

Although the TOSCA is not a *direct* measure of academic attainment, it is clear that performance on the test depends greatly on the spinoff skills and effects of literacy achievement. This analysis provides a straight-forward explanation for why the factor analytic study of TOSCA by St.George et al. (1990) yielded a general verbal-educational factor but failed to provide any evidence in support of discrete structural components tapping the test-development item categories. Performance differences on the TOSCA are largely a *reflection* of differences in the spinoff skills of *prior* academic achievement, skills which themselves are essentially *verbal* in nature.

Recently reported research is consistent with the view that performance differences on general verbal ability tests like the TOSCA are largely a consequence of Matthew effects in literacy achievement. Research by Juel (1988), for example, indicates that reading achievement itself is a major factor influencing growth in listening comprehension skills among disadvantaged children with below average school language and listening comprehension abilities at school entry. This finding suggests that listening comprehension ability is both a cause and a consequence of reading achievement, a phenomenon referred to as reciprocal causation. In another study that supports the concept of Matthew effects in literacy, Anderson, Wilson, and Fielding (1988) found that out-of-school reading was the best predictor of reading gains between Grades 2 and 5.

The Issue of Causality

Croft disputes the claim that performance differences on the TOSCA are largely a consequence of literacy achievement. Instead, he argues that the TOSCA measures abilities that are, in fact, *causally* related to academic attainment. He states that "TOSCA is designed to measure a sample of cognitive skills and knowledge likely to *underpin* the academic side of our school curriculum" (emphasis added). Clearly, the notion of "underlying skills and knowledge" is being used as a general hypothetical construct to "explain" individual differences in academic achievement. The developers of TOSCA

claim strong support for their hypothesis when they state that the test measures "those verbal and numerical reasoning abilities which *have been found to be requisites for success in school work*" (Reid et al., p.4, emphasis added).

In his review essay Tunmer (1989) argues that such a claim cannot be justified on either theoretical or empirical grounds. He contrasts the general approach taken by the developers of TOSCA with the kind of theory construction that normally occurs in cognitive psychology, citing as an example the theory and research that has established a *causal* connection between listening comprehension ability and reading achievement. Croft, however, rejects this example and argues that theoretical developments in the areas of listening and reading comprehension are "in their infancy" when laid alongside mental test theory.

That this claim is blatantly false can be easily demonstrated by examining separately the theory and research supporting a causal relation between listening comprehension and reading achievement, and the theory and research supporting a causal relation between TOSCA performance and academic attainment. Consider first the theoretical arguments linking listening comprehension ability to reading achievement. Reading has been defined as the translation from print to a form of code from which the reader can already derive meaning (Venezky, 1976). Children who lack proficiency in the language being read should therefore encounter reading difficulties. Accordingly, deficiencies in the different levels of language functioning should result in different kinds of reading difficulty. For example, children who are unable to discriminate between different types of phonemes should encounter difficulty in analysing speech and relating it to print. That is, they should have difficulty in learning to decode words.

Children with deficient morphophonemic rule knowledge would not be able to take advantage of this knowledge in breaking the orthographic code of English. For example, the letter s represents regular noun plural inflection, even though it is not always realised as the /s/ phoneme, as is true of words like *dogs* and *cars*, in which the final sound is /z/. However, for beginning readers with morphophonemic rule knowledge it is not necessary to learn the exceptions on a case-by-case basis. In acquiring spoken English, these children unconsciously learn a phonological rule that specifies that plural inflection is realised as /s/ when it follows a voiceless stop consonant, as in *cats*, and as /z/ when it follows a voiced phoneme, as in *dogs*.

Children with poorly developed lexical representations should have difficulty in comprehending words after they have been recognised. Such a deficiency should also limit the development of their decoding skills. When beginning readers apply their incomplete knowledge of grapheme-phoneme correspondences, possibly in combination with their knowledge of the constraints of sentential context, to unfamiliar words, the result will often be close enough to the correct phonological form that they can correctly identify the word and thus increase their knowledge of grapheme-phoneme correspondences. However, this can occur only if the unfamiliar word is in their *listening* vocabulary.

Deficiencies in syntactic and discourse knowledge should also affect reading comprehension. Children who are deficient in syntactic knowledge should have difficulty in understanding sentential relations in written sentences. And children who have problems in integrating the propositions underlying sentences into larger sets of interrelated propositions through the application of pragmatic and inferential rules should have difficulty in comprehending and recalling stories.

There are four major sources of evidence in support of these claims. First, an enormous amount of research has shown that children who suffer impairment in one or more of the various components of language functioning (e.g., speech perception, morphophonemic rule knowledge, lexical knowledge, syntactic knowledge, discourse knowledge) are much more likely to encounter reading problems than children with normal language comprehension skills (see Mann, 1986, for a review). Second, research on non-native speakers indicates that reading achievement (in English) is a function of English oral language comprehension ability, as measured by tests that tap the various components of language functioning. Matluck and Tunmer (1979), for example, found that a monotonically increasing parabolic function provided the best fit of their data, indicating that children must attain a threshold level of competence in the language being read before they can progress in reading.

Third, several studies have reported strong correlations between listening comprehension ability and reading achievement. However, it is important to recognise that correlational data is of no value in establishing a causal relationship in the absence of *a defensible theoretical framework*. With respect to listening comprehension ability and reading achievement, Gough and

Tunmer (1986) have proposed a model of the *proximal* causes of reading performance differences, sometimes referred to as the Simple View (Hoover & Gough, 1990; Juel, 1988; Juel, Griffith & Gough, 1986; Tunmer & Hoover, in press). The model proposes that differences in reading comprehension are a function of the product of two factors, decoding (i.e. word recognition) and listening comprehension; that is, $\underline{R} = \underline{D} \times \underline{C}$, where $\underline{R}$ = reading comprehension, $\underline{D}$ = decoding, and $\underline{C}$ = listening comprehension. Each of these factors is assumed to be necessary, but not sufficient, for success in reading. For example, if decoding ability is high but listening comprehension is quite low, the child will be a very poor reader (i.e. if $\underline{D} = 1.0$, where 1.0 is perfection, and $\underline{C} = 0$, then $\underline{R} = 0$).

The model yields three predictions. First, it predicts that the inclusion of the product of decoding and listening comprehension in the regression equation for reading comprehension should account for a significantly greater amount of variance than the linear combination of the two variables alone. This prediction was confirmed in a recently reported longitudinal study by Hoover and Gough (1990) which showed that the linear combination of decoding and listening comprehension, and the product of these two variables, accounted for almost 90% of the variance in reading comprehension performance by fourth grade.

The results further revealed developmental changes in the relative contributions of decoding and listening comprehension to the variance in reading comprehension, with decoding accounting for more of the variance in the lower grades. This finding is related to a second prediction of the model, which is that beginning readers should be able to read as well as they can listen provided that inadequate decoding skills are not holding them back. That is, at increasing levels of decoding skill there should be positive slope values between listening and reading comprehension of increasing magnitudes. In addition, the intercept values for the slopes should all be zero because reading comprehension should be zero if listening comprehension is zero irrespective of the level of decoding skill. These predictions were also confirmed.

A third prediction of the model is that within the population of poor readers, decoding and listening comprehension should be negatively correlated. If reading comprehension is the product of decoding and listening comprehension, then to achieve a low score on reading comprehension, a child

who performs at a high level on decoding must achieve a low score on listening comprehension and vice versa. Thus, for increasing sample reductions based on decreasing reading comprehension skill, correlations between decoding and listening comprehension should go from positive to negative. This prediction was also confirmed.

The fourth, and perhaps most important, source of evidence supporting a causal relation between listening comprehension and reading achievement comes from intervention studies. Several studies using a variety of different training procedures have demonstrated that improvements in listening comprehension lead to improvements in reading comprehension (see Hoover & Gough, 1990).

Let us now examine the theoretical arguments in support of a causal relation between TOSCA performance and academic attainment. Given Croft's claim that theoretical developments in the areas of listening and reading comprehension are "in their infancy" compared to what has occurred in mental test theory, we would expect that there would be much to say. However, there is *nothing* to say. The developers of TOSCA frankly admit that they were unable to find a suitable theoretical framework to serve as the basis for developing their test: "The search yielded no clear-cut theoretical formulation which the test developers judged to be entirely suitable" (Reid et al., 1981, pp.4-5). This, of course, is not surprising since it is very difficult to imagine how solving "disemvowelled" words, hidden words, run-on sentences, anagrams. etc. is related to learning anything.

The only evidence that the developers of TOSCA offer in support of a causal connection between TOSCA performance and academic attainment is concurrent and predictive correlations. The difficulty here, of course, is that such correlations do not preclude the possibility that some third factor, such as the spinoff effects of *prior* academic achievement, is responsible for a spurious correlation between TOSCA performance and school achievement. To provide convincing evidence of a causal relation, the developers of TOSCA would need to conduct an intervention study. But such a study would be a complete waste of time. Nobody, including the developers of the TOSCA, would seriously believe that training school children to solve anagrams, run-on sentences, hidden words, etc. would have any effect whatsoever on academic attainment. The developers of TOSCA defend their total lack of a defensible

theory by stating that their approach "rested on pragmatic considerations and empirical evidence" (Reid et al., p.5). But as we have seen, the pragmatic considerations are highly questionable and the empirical evidence, in the absence of a defensible theoretical framework, is uninterpretable. Such are the consequences of naive, retrograde, empiricism.

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REVIEWS

Political Issues in New Zealand Education (Second Edition), Dunmore Press, Palmerston North, 1990. Edited by John Codd, Richard Harker, Roy Nash.

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I read *Political Issues* while I was in Ohio recently attending the National Women's Studies Association annual conference and visiting colleagues. The surrounding rhetoric was radical feminist on the one hand, and postmodernist on the other. Unsurprisingly, my reading of the text was affected by the discourses around me in that wonderfully contradictory place that is the United States.

You might wonder why I was reading this book in such a setting. Well, partly in order to review it and partly to check it out as a potential student resource for my on-going University course in the sociology of education. The stage two students had found the first edition worthwhile, and I thought this would be a useful update. There are several points in its favour as a text: it is written in accessible language, it is based within the New Zealand educational context, and its chapters take a critical ideological position. These things are not often found together in one place!

The important Introductory chapter (which the students usually read to get a quick fix) reproduces some of the text from the first edition and includes an account of the recent restructuring of schooling. The chapter gives a very good introductory account of the relationship between education, the state, the economy and the reproduction of class in New Zealand. The editors' theoretical framing of their project is, to my mind, excellent : the essays in the book "share a political-cultural problematic which begins with the systems of the state, the economy and the family, and the structures of class, ethnicity and gender. They ask what effects, intended and unintended, are produced by the education system in relation to social structures and political action" (p.8). The book almost lives up to its promises. But who are speaking? There is a significant partial absence of women and Maori, as I discuss below.

The chapters in Part One : Education, Inequality and Cultural Reproduction are very good introductory pieces on the complex ways in which schooling in New Zealand still works to perpetuate class, race and gender inequalities. **The Part Two : Policies and Practices** papers on educational policies also work very well for students, introducing them to excellent discussions on 'discourse' and the state (by John Codd), equal employment policies in action (Wanda Korndorffer), the political dangers for Maori of taha Maori programmes in school (Graham Smith) among others. **Part Three : Contested Issues** seems a bit of a grab-bag with an assortment of papers on such issues as the 'back to basics' arguments mixed in with historical accounts of moral panic in New Zealand and of the Proficiency Examination. Hardly the hot educational topics of the early 90s. Maybe I'm just a fashion-follower, but I would have thought Maori education, Pacific Island education, special education, Boards of Trustees' work, teacher union issues, even the politics of research in education, might have been a bit more 'contested' - and, dare I say it, important - than those topics selected.

Overall, I liked the text, and I think the students will find bits of it useful. But I was also left with a nagging concern about what the book's silences might also be teaching our students.

Part of what's lacking is a sense of excitement and of tentativeness - a self-mocking almost, which allows an engaged and enjoyable reading. Exactly the same complaint can be levelled at *New Zealand Education Policy Today* and *Towards Successful Schooling*, other recent books in the same area. New Zealand sociology of education is certainly alive and well, judging by our book publication rate - which is something to celebrate. But we are (on the whole) a worthy and dull lot. We take ourselves ever so seriously, and don't seem able to be self-critical, or critical of our favourite left theorists. We are also a rather exclusive little band; we have not done well at collecting and publishing the varied voices of those working in education as teachers, students, parents, unionists, principals and so on. And Maori and women's voices are still under-represented. All those writing in PINZE are academics, nearly half the chapters come from Massey University, and the vast majority of the authors are Pakeha male (only one is Maori).

Maybe the domination by the sonorous academic voice is unsurprising: this is after all a university text, located in the academic market-place, written by and

for academics and their students. What is more surprising at a time when postmodernist discourses are raging in social theory is the book's unself-critical tone. It is a time when marxism has been de-centred, when claims to totality and certainty are undercut, when we authors are self-conscious about our partial constructions. Things have improved from the first edition where the editors' introduction stated that theirs was 'a correct theoretical model of schooling...[which]...can explain every currently contested issue in NZ education'! (p.19) These dubious phrases were left out of the second edition thank goodness, but the over-riding tone is still one of certainty and correctness (explicitly so in the case of Nash p.129). This tone not only generates a false authority, but also makes such texts very hard to engage with. One is invited to read and accept - or reject, perhaps - but not to enter into a shared exploration of the complexities of educational issues. It is the tone of the academic I suppose, our students are to listen to us, and believe.

This issue of knowledge and power is mentioned in the text - once - but is not turned upon the text itself. John Codd discusses how Foucault's analysis of power and knowledge can be usefully applied to the deconstruction of education policy, suggesting that policy texts can be read as 'ideologically constructed products of political forces' (p.147); he also points out that the discursive effects of policy documents are often ignored by policy analysts. (p.148) The same could be said of curriculum documents such as this book. Especially as it is a student text, and therefore in a central position in the construction of legitimated knowledge it could have been an ideal opportunity for education students to be exposed to the issue of power and knowledge production through a political analysis/deconstruction of the very text they are reading.

The text as 'ideological construction' struck me most forceably in the invisibility of the feminist and Maori struggles in the otherwise excellent account of the recent history of the educational restructuring in NZ in the Introductory chapter. While the Introduction tackles the relationship between education, the state, the economy and the reproduction of class in NZ, there is a virtual silence on women and Maori. We are told that the left has been too 'airy', *never* based in a 'concretely referenced discussion of political transformation'. (p.19) Okay, but what about other critical historical players (many of whom would count themselves as on the 'left')? Maori and feminist struggles over the educational changes have had significant effects on the

equity provisions of the Charters and other aspects of education reform. Even if they turn out to be less effective than many of us would like, the public and accountable equity statements in the reforms did not simply appear out of a misguided liberal benevolence on behalf of the blokes in Treasury or in the Government machinery. While Maori suggestions for the Charter text were repeatedly watered-down, it was in the context of a partly successful struggle by Maori and feminists that the text of the Charter developed. Feminists were also successful in the significant gains made in the early childhood area. While much of the input of the male left has been via scholarly publications, those radicals working in direct contact with the State were very often Maori and feminists. Feminist educators, recognising the need for on-going central policy direction, have struggled hard to influence that as well as attempting to influence practice at the local level of teacher practice through booklets such as *Countering Sexism in Education*, and active organisations such as Feminist Teachers.

We don't know much about feminist influences; there is an 'awesome silence' regarding gender initiatives in education. As in other countries, there has been little attempt to map, analyse and theorise the general territory or specific characteristics of gender equity work in and for NZ schools. We do not have any easily accessible written history or analysis of the work of women's groups within political parties, school teachers, teacher unions or others in areas such as special education and early childhood. As Jane Kenway (1990) says in reference to the Australian context, it is also true here that we do not know how women's lobbying activities have been negotiated within women's groups and mixed groups, or about how they have articulated with either popular educational or policy discourses, or with contemporary social and economic circumstances (p.40).

As far as I could see from the bibliography, no female or Maori voice informed the Introduction. (Indeed, Maori are mentioned only three or four times, always in oddly negative contexts: in relation to the low number of Maori at University, the potential sexism of school boards, and the passive acceptance of the educational reforms.)

There are three chapters (out of seventeen) which discuss women's/girls' education. Sue Middleton's very useful chapter (Family strategies of cultural reproduction: case studies in the schooling of girls) is reprinted from the first

edition. She locates the school and family experiences of three feminist educators within their historical/political context, indicating how cultural capital is created and transmitted; how the intersection of class, race and gender are lived is usefully illustrated in the words of 'real people'. Elsewhere the text stays out of the lived sites of the family and the classroom, and the lives of 'real people'; as I said, few voices and thoughts of others than academics are heard here.

Anne-Marie O'Neill (Gender and education: Structural Inequality for women) provides an excellent update of Allanah Ryan's chapter in the first edition. Along with Middleton's, this piece is a particularly useful resource for undergrad teaching, not only because it summarises quantitative data on girls' curriculum choices and achievement patterns with attention to the situation of Maori girls, but also because it includes a useful theoretical analysis. O'Neill indicates that at least at secondary school level, girls tend to do somewhat better than boys; the question of girls' 'disadvantage' in education must be focussed not on achievement but on cultural reproduction. That is, it must focus on the processes through which girls (and boys) become 'gendered subjects'. Not just the ways in which schools "ideologically condition girls to accept entry into specific subject areas and occupational locations on the basis of their gender and class background" (p.82) but the various and contradictory cultural processes infused in everyday life in schools through which girls' class- and ethnic- based senses of being female are (re)produced. O'Neill points to the possibilities in poststructuralist analysis for understanding the crucial nexus between school, home and work; how these sites produce and maintain each other and how women are positioned within them.

Wanda Korndorffer's chapter (Equal employment opportunities in the universities: From policy to practice) is also extremely useful - and ought to be recommended reading for all actively involved in EEO initiatives in NZ universities. She documents practices and possibilities of EEO policy, as well as discusses difficulties and contradictions in areas such as definitions of merit.

While gender issues are mentioned in passing elsewhere (although this is not evident in the index), women's voices are only really heard in these chapters. Maori voices are also hardly heard. Graham Smith, the only Maori author, counterposes the Pakeha state initiatives in Maori curriculum with those programmes initiated and developed by Maori in Maori interests (kura kaupapa

Maori). These are not an attempt to 'help Maori kids to do better' in mainstream schools, which is the assimilative aim of state initiatives, but a political struggle in the cultural production of Maori survival and change. Many other chapters - albeit from a Pakeha viewpoint - include some reference to Maori educational issues.

Despite its lack of multiple voices, as I said, I did enjoy the book. My responses here are not an attempt to engage in a fashionable scolding of the white Marxist boys (again). If I am scolding anything - and that is what reviews often do - it is the lust for authoritative accounts which characterises most academic texts. Textbooks make an ideal site for reflexive accounts which are explicitly conscious of how they establish their own authority, of *who* they allow to speak and who they silence, and therefore what can be said...and ultimately what is thought and acted on. This is a political/educational issue which deserves a place in any contemporary sociological and educational text.

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Public and Private Lives
by Peter Dwyer (Melbourne, Longman Cheshire)

Reviewed by Nicola Armstrong,
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Peter Dwyer's book Public and Private Lives purports to be a "careful" analysis of Australian society which examines two themes: the relationship between the individual and society, and the process of social change. The book is designed as an introductory sociology text that "...builds on the readers own experience" (vii, original emphasis) and aims to 'bridge the gap' (ibid) between the 'amateur and the professional'. By rejecting approaches to

Sociology which introduce 'founding fathers', 'research methods' and 'basic concepts' (p.vi), Dwyer attempts to create a readable account which begins with the "everyday relationships of people in Australian society" (vi).

Dwyer's book suffers however, from the very critique he directs at other writers. If conventional sociology books appear to him too 'abstract', concerned 'more with theory than with experience' (vii), his own book suffers from a retreat from theorisation, is overly descriptive and draws upon on a aged literature focused primarily on the sixties and seventies.

While laudably clear and methodic, Dwyer fails to make the sociological connections between the research data he describes. For example, in the first section, Dwyer details four community studies (Martin, 1967 and 1970; Wadsworth, 1976; Bryson and Thompson, 1972 and Montague, 1981) which analyse (particularly) the impact of class relations on interactions between families and within neighbourhoods and communities. While potentially interesting, Dwyer fails to make explicit the *sociological connections* between the research projects he describes, leaving readers to draw what analytic clues they can from the text.

Dwyer's discussion of the "Australian national character" is similarly problematic, in that it fails to sociologically interpret in any depth the stereotype it describes. In this section Dwyer draws heavily on the work of Conway (a 'thesis' developed "...in the late 1960s and reasserted in the mid 1980s" p.33) which discusses the "...soul of the Australian" as "...starved captive in a dungeon created by generations of either not caring or dreading to show care" (Conway, 1985 in Dwyer: 1989:34). The problem, according to Conway, is that the sons of Australia lack "...a tender virile sire" (p.33) to identify with, then must resort instead to 'shallow associations with their peers' to 'air those unrealised fantasies of manhood which the domestic rut at home has now made impossible", (ibid:33). In the vacuum created by this default in "husbandly and fatherly tenderness", Australian mothers have 'seized' upon making the home their 'sure stronghold'. Conway writes:

> "It is in the intense mastery of her children she (the Australian mother) was to discharge the overt love and the latent hostility that gross defects in male sensitivity had allowed to accumulate for so long. In this she had not only the thrust provided by her own unconscious frustrations, but often the memory of the harsh lot of her mother and grandmother before her".
> (Conway, 1985:44 cited in Dwyer, 1989:33)

Although I found Conway's diatribe fascinating at one level, I was concerned that Dwyer failed to dismantle sociologically the ideological content of this sexist and moralistic book. The reader is left with an analysis as shallow as the Australian character it purports to describe. What is the point, sociologically speaking, of using work which Dwyer himself describes as a "...web of petulant impressions" (ibid:34) based as they are on a series of simplistic and bigoted stereotypes?

Perhaps the point could have been to use these stereotypes to facilitate a critique of unified constructions of sexuality, using the work of sociologists such as Macquarie University's Bob Connell. Connell's work on the construction of "hegemonic masculinity" and "emphasised femininity" (Connell, 1987:186) would provide an excellent lever with which to topple Conway's journalistic account. Not only does Dwyer miss an opportunity to provide an interesting sociological critique, he fails to take responsibility for refuting a clearly bigoted, anachronistic and destructive account of the Australian people and their cultures.

The major problem, then, with Dwyer's account is that it does not locate the micro-politics of the individual, family, neighbourhood or community in the *context* of the structural social relations of Australian society. An example of this is the account of James' (1981) research on the New England area of New South Wales. James suggests that wives "...*indirectly* influence the public advancement of their husband", through a process of "...vicarious fulfilment for themselves through the success of their husbands in their careers and of their children in schooling" (Dwyer, 1989:21). Dwyer argues that James's work could be seen as "...proof that although women 'know their place' they seek ways to achieve 'vicarious success' by influencing the public career of their husbands" (ibid).

Although he acknowledges James's interpretation is couched in terms of 'manipulative' women, Dwyer fails to place James's analysis in the context of wider social relations, other than a rather cryptic reference to the ways in which men and women "...negotiate the link or balance between the public and the domestic" (p.22). What is clearly needed at this juncture is a sociological analysis of the wider context of the sexual division of labour and the social and historical construction of gender relations in Australian society. Indeed, despite its clear application to Dwyers account, an analysis of the sexual

division of labour receives barely a mention, other than a brief citation in the final chapter referring to 'recent' work by Rosaldo (1978) and O'Brien (1981).

The rather weak analysis of gender is coupled by an almost complete lack of attention to ethnic relations, in terms of Dwyers description of the social composition of communities and domestic life. Indeed the research Dwyer cites on the organisation of domestic life renders invisible the experience of all but Anglo-Australians, although it is noted that "ethnicity" does "make a difference" (p.23) to the patterns of social interaction in particular regions or communities. What these differences *are* is left, we presume, to our own sociological imaginations.

The final chapter of Dwyer's book did provide an interesting overview of economic and political change in Australia, which raised the pertinent dichotomy of social versus economic 'man' and the thorny problem of what Australians can 'expect' by virtue of being members of their society. At this point Dwyer could have referred to an increasing literature from the Left concerning the rights associated with 'citizenship', in particular the 'New Times' analysis associated with British journals such as *Marxism Today*. Instead Dwyer appeals to a questionable form of 'social morality' which supposedly provides a universal bond between individuals and society. As sociologists, we must at the very least be sceptical of claims to a universal social morality, when we know that such claims silence and exclude the *diversity* of the social relations which we seek to investigate. Rather than appeal to universal morality, Dwyer could have appealed to the collectivity of our lived experiences as the means by which the individual is bound to others within the social world. That is, people do not live and work in families, communities, factories, trade unions, feminist organisations or tribes *as individuals, they work and live together*. If we are interested in turning students onto sociology, surely the means to do this is by the good example of insightful sociological analysis of the social nature of the world we live in, not spurious appeals to shared ideals of the 'good society'.

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Paul F. Green, (editor) **Studies in New Zealand Social Problems**, Palmerston North: The Dunmore Press, 1990 ($34.95 pp inc. GST).

Jim Consedine, **A Poison in the Bloodstream**, Whatamango Bay, Queen Charlotte Sound, Cape Catley Ltd., 1990 ($20.00).

Reviewed by Pat Shannon,
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Shortly after receiving a review copy of the Green book on Social Problems I happened to notice a three line review of it in our local newspaper. The reviewer restricted himself to two comments:

1. the book contained a strange assortment of problems;
2. the editor's claim that we had too few books on social problems was false - we had been deluged with them.

These comments, in the context of Green's introduction to the "Social Problems" book, where he emphasised the distinctiveness of a sociological understanding of social problems and his carefully legitimated hierarchy of problems treated - (national, inequality, crime and deviance and institutional problems in that order) intrigued me. It seemed to capture the essence of the questions which arise from such a book. The other reviewer was correct. We have been deluged by whole lots of books about social problems. The crop varies widely in quality. Some authors seem more concerned to work out their own problems - particularly about young people - is there a message there? In others however we have sane, balanced and committed people with considerable experience and a passionate commitment to change. Jim

Consedine is one of these. Do sociologists have a distinctive contribution to make here and is it as valuable as that of such a committed involved person?

To test this I decided to do a joint comparative review of the Green book with Consedine's book on the prisons, also given to me for review. It seemed a straight contest between a sociological book by various experts, inherently (?) fragmented in perspective and diverse in content and a passionate, committed holistic book by a committed insider. The comparison had the added advantage of comparing an 'obvious', conventionally defined social problem - crime and imprisonment - with what sociologists deem to be interesting problems.

Perhaps my comparison can start there. Green defends his choice of topics on the grounds that they are 'problems of society' rather than just conventionally regarded as social problems. So problems of national growth (population) development (energy policy) and autonomy (nuclear free policy) are treated in Part one, leading to problems to do with inequality (discrimination against the unemployed, sexism and racism) in Part two, crime and deviance (violence, drug abuse, religious groups) in Part three and, finally problems of institutional arenas (education and equality, work and technology and medical imperialism) in Part four. All of these would normally be recognised as areas of problems, if not problems themselves, by many people besides sociologists and sociologists themselves would see many other problems (social or otherwise) to be analysed. So what is the strength of the overall collection beyond the fact that it might indicate the purely personal interests of the authors Green invited to contribute? Does such a categorisation have any advantages over Consedine's approach?

A point is that the Green text allows connection to be drawn. The selection of problems enables a coverage of both the conventional and unconventional to highlight both the arbitrary nature of problem selection and the structured connections between seemingly disparate phenomena such as violence, unemployment, (blamed on the victims), inequality in education, racism (as result) and sustainable development and population changes.

Paradoxically, it does exactly what Consedine has done, when he works from the imprisonment of offenders who are poor and unemployed, (and the ignoring of their victims) to inequality under the law, biculturalism (as response), institutionalised evil, prisons as solutions and the humane

development of a positive policy. The differences do not seem considerable. While Consedine is more restricted in his focus on one problem (imprisonment), he certainly is not more restricted in his attention to structured issues of power, ideology and all the social process and social structural issues which are supposedly the strengths of sociologists.

Consedine is of course, passionate, committed and goes on about spirituality quite a bit - so maybe it is a dispassionate value-free approach which is the contribution of sociologists. However, as Green admits, while some do attempt it (fruitlessly) others make no such attempt and are clearly based on strong value commitments. There are more facts/figures in Green but that only makes it more boring to read than Consedine (I had to force myself to read Green's book over weeks (!) while Consedine was not easily put down). (The list of criteria for difference could go on - but I find few of the possibilities convincing. In fact the books are extremely similar in important respects.) The strengths and advantages of Consedine's "lay" book are its holistic, committed, value-based, structurally and culturally sensitive approach and positive proposals for change. The papers in the Green collection, somewhat paradoxically, are far more descriptive, less given to explanation and thus with few policy proposals. Even as a sociologist, it seems to me that Consedine wins, certainly for the lay reader - or even an academic 'interest only' reader. Where Green has strength is as a text for long-suffering students. Its strength here is its diversity of approaches and viewpoints. The introduction by Green (albeit a bit more laboured and turgid than necessary) highlights the importance of theory and values for all analysis. It does therefore provide a good basis from which to not only analyse the issues the various authors raise but also one from which their positions can be critically examined.

In this sense the book provides good raw material for such analysis. However, perhaps inevitably, it is uneven. The chapters that attracted me most were those on energy policy by Read and education by Harker - however those were ones where familiarity did not dull appreciation. On the positive side all the chapters give valuable examples of sociological description and its insights. There seems to be very little about the analysis which is indigenous (as claimed on the back of the cover) beyond its setting and this applies as much to the chapter on racism. We do not therefore have in this book on Aotearoa/New Zealand perspective on social problems, but we do have an interesting collection by local sociological writers. At its best it is exciting and stimulating (Read on an

alternative energy policy, Harker on education and equality) but at other times it fails to meet the challenge of informed lay analysis (Sandra Coney is another lay example who has discussed some issues as well as they are treated here).

In sum, this book will inform and educate rather than persuade and captivate but it certainly does provide a basis from which to build. It will be useful for student courses on local social problems and certainly provides good solid under pinning for the growing lay realisation of the structural causes of problems. However a word of warning. It is weak on policy. We do perhaps need to be careful here. If lay sociologists can outdo the professionals someone may decide to lay off the professionals. It is surely insufficient merely to seek to understand the world, we must also seek to change it if we wish to write about social problems.

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On chalk and cheese: a reply to Shannon

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It is always difficult to respond to criticism and my first decision was to let the Shannon review stand without reply. Being philosophical I thought 'any publicity is good publicity'. The trouble, of course, is that the review reflects unfairly on the work of many others. So as editor, I decided to risk a response on behalf of the remaining contributors. The Studies are far from perfect and certainly not exhaustive but hopefully we can revise and expand the set as the needs of current and potential users are discovered. The selection of Studies was compiled to fill a long standing need for a New Zealand collection covering a wide range of issues. I'm sure Shannon would agree that there has long been such a need. It is therefore disconcerting to see him repeat the error of a relatively ill-informed journalist conceming the deluge of books on social problems. There are certainly specialised collections available on issues such as deviance (Hill *et al*, 1983), education (Codd *et al*, 1990), environment (Hay *et al*, 1989) and numerous 'holistic' treatises on single issues or particular problem areas. The fact remains, however, that the Studies constitute the only current collection to cover such a broad area of social problems. Surely there is room for both broad collections and narrowly focused books whatever our

pedagogical preferences might be. There was an empty niche in the academic market and Dunmore selected Studies to fill it.

There are more important points and real issues raised in Shannon's review as with the claim that the collection does not reflect an 'indigenous' analysis. It is true that a Maori perspective is not and certainly should be included in such a collection. But if the reviewer is looking for a uniquely Aotearoa/New Zealand perspective that is unlike American, Australian or British approaches then we ought to know more about his criteria. Is New Zealand not to be seen as a part of the world-system? Is its dominant culture not a variant of Western European culture and are its racist and sexist institutions not comparable to those in other English speaking colonies? I would love to debate this one; for with the important exception of the Maori perspective, there is an awful lot of nonsense about concerning the unique qualities of New Zealand society.

As in the opening statement, Shannon's line of argument seems strange coming from a social scientist. His statements are often contradicted by the facts and seem strangely at odds with the main thrust of the Studies. For example, he suggests that this diverse collection is made more boring than Consedine's book because the chapters are so descriptive and reliant on facts and figures. Is this a complaint about boring social science? There are a total of four tables and six figures spread through only four of the thirteen chapters (half in the population chapter) and two in the preferred Harker chapter. Perhaps Shannon was so bored by the turgid and laboured introduction that he repeatedly fell asleep or failed to complete his reading? One can only wonder about the closing assertion that the collection is 'weak on policy' and the implied suggestion that the contributors lack passionate commitments. In fact only one chapter excludes policy preferences, but that is hardly a shortcoming, given the authors' commitments to ethical neutrality. All of the other chapters explicitly provide policy options and in most cases considerable space was devoted to the consideration of various alternatives. In every chapter (including the value-free one) the relevant policy decisions made in fact were subjected to critical analysis.

I share the reviewer's approval of the Read and Harker efforts, but I cannot for the life of me explain his failure to acknowledge the value based policy commitments on equity issues and against racism and sexism made by Spoonley and West Newman. What about the commitment to public health

programmes made by Hannifin and White and their opposition to the conventional strategies of criminalising and medicalising our problems? These are not isolated cases:

* Clement's commitment to the peace movement and his analysis of how to spread our nuclear free policy to others cannot be dismissed as descriptive sociology.
* Hill, Couchman and Gidlow critically analyse employment and labour force policies and then explicitly lay out the value bases for an active labour market policy by government.

These chapters do not exhaust their topics and undoubtedly any of them could be transformed into the basis for a more detailed 'holistic' and in-depth treatment by some other specialists. The only difference that I can find between the value commitments and policy discussions which distinguish my contributors from Consedine and Coney is that most (not all) of them are academics and social scientists rather than journalists or priests. I can only conclude that Shannon finds fault with their theoretical frameworks and policy judgements, but if so, the criteria for his judgements are not at all clear.

Having opened the review with a spurious contest between a collection of sociologists and a committed involved person like Jim Consedine, Shannon closes it with an untenable distinction between professionals and lay people. Jim Consedine and Sandra Coney, his exemplars, are not academic social scientists to be sure, but they are most certainly professionals. They both possess the educational backgrounds, occupational and research skills and experiences that qualify them as professional experts in their respective fields. If anything there are more amateurs among the academics than there are among the priests and journalists who devote their lives to research and direct involvement in prison life or women's health issues. If there is a distinction to be made between academic and non-academic investigators and social problem analysts, it would have more to do with the environments in which they work, than in their commitments to solving problems. Frankly the image of professional versus lay person should be abandoned as an outdated and inappropriate extension of the medieval distinction between the 'clerics' and the 'laity'. In an age where priests are simply another class of white collar workers, it seems at once erroneous and arrogant to regard academics or social scientists as modern day clerics who should possess a monopoly of professional expertise.

What an academic environment *should* provide is an assurance that certain systematic procedures and clear standards have been adhered to. "It is this conscious reliance on theory, as well as on the painstaking assessment of various official, media and scientific sources of data, that makes the social scientist's ... contribution to the public debate" a credible one (Green, 1990: 9). If Sandra Coney's and Jim Consedine's passionately committed efforts as social activists, journalists and freelance investigators have been constrained by the same standards of objectivity then they deserve the same respect. But the fact remains that they do not work in such an environment and the constraints they work under do not lend their writings the same degree of scientific objectivity and credibility (Corbett, 1990). If that makes science boring and activism more interesting and compelling for some social scientists then perhaps they should change their field. My own belief is that good social science and responsible social activism can and should be compatible and complementary. I know that many of the authors in these Studies find no great difficulty incorporating both into their lifestyles.

There is no lack of 'passionate commitment', 'structural or cultural sensitivity' or professional expertise that would distinguish Jim Consedine or Sandra Coney from the authors collected in these Studies. There are no grounds for inferring that they are more or less passionately committed or involved. They work in different environments which provide distinctive constraints and inducements. These are the sorts of differences that might have been legitimately considered in such a comparative review. Shannon, however, seems to have thrashed through a comparison of chalk and cheese.

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