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Unpaid Work In New Zealand: What Can The Official Statistics Tell Us?

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Introduction

In the last decade, there has been massive restructuring in the economy of New Zealand with many of the changes beginning to contest the traditional roles of males and females in society. In particular, women in two parent families with dependent children have increasingly been moving into paid work. A significant number of men have moved out of paid work to unemployment, while another group of men have moved into part-time work (Davey and Callister, 1994). These patterns of paid work are well documented in official statistics.

But according to Australian research, the three largest industries in the economy are the everyday household activities of preparing meals, cleaning and laundry, and shopping. In addition, the volume of labour time used in the Australian household economy is currently almost 40 percent greater than all labour used in the formal economy (Ironmonger, 1994). What can be said about patterns of unpaid work in New Zealand, and in particular, changes which may have occurred over the last decade?

Overseas, time-use surveys are the tools most commonly used for gathering information on unpaid work. Statistics New Zealand recognise that they are 'the most effective way of obtaining data on the nature and extent of involvement in unpaid work...' (Statistics New Zealand , 1993:121) In 1990, the Department of Statistics carried out a pilot survey in order to test the methodology for a full survey. Although this provided a broad view of the division of household labour between men and women, the sample was too small to look at a range of variables such as age, ethnicity, family composition or labour force status (Department of Statistics, 1991). In addition, the response rate was low and the sample not well balanced (McKinlay, 1992). There are currently no official plans to carry out a full time-use survey. This would require the provision of additional funds by government.

In the absence of a time-use survey, Statistics New Zealand argue that the population census is a potential source of information (Statistics New Zealand, 1993). The Household Labour Force Survey also provides some very limited data (Callister, 1993). In this paper, we will examine the methodologies used in the census, look at the information provided, and briefly discuss the data in relation to findings from overseas and other New Zealand studies of unpaid work.

The Census

The census questions on unpaid work/activities have undergone a number of changes since 1971. In 1971, respondents could tick a box registering that they were a

'housewife', defined as undertaking unpaid domestic duties and not working for financial reward. In the 1976 census, the occupation became gender neutral with respondents asked what their principal work status was, with a census category 'household duties (unpaid)'. In 1981, the question was changed again. Those who worked less than 20 hours per week in paid work were directed towards a set of employment status choices which included 'household duties (unpaid)'. Prior to the 1986 census, Statistics New Zealand tested questions related specifically to work around the home. Examples given included housework, childcare, gardening and repairs. Some respondents experienced difficulty differentiating between hobbies and household work, such as gardening and sewing. A significant number had difficulty calculating their weekly hours. When respondents were asked to break down their time spent in specific categories, such as housework, gardening and home maintenance, further difficulties occurred. Many activities overlapped or were carried out simultaneously making time allocation difficult or inaccurate. According to Statistics New Zealand, there were also variations between respondents' estimates of the time involved. In the childcare category, some included all the time they were with the children, while others counted only the time they actually spent caring or supervising.

Because of these problems, Statistics New Zealand decided to include a simple question in the 1986 census, with the implication that its very presence recognised the importance of unpaid work (Statistics New Zealand, 1994). This question required all people aged 15 years and over to state their main work or activity, and the choices included home duties - looking after children,

home duties - not looking after children, paid work and unpaid work in a family business. The 1986 census also asked about hours of voluntary work carried out on a regular basis and a question regarding unpaid work in a family business. In the latter question, it is not clear what activities this includes. Some people may see looking after children at home on a farm in this category

In the 1991 census, the question was further modified. The 'Activities' question asked if respondents, no matter what their main work or activity status, if they did any of the following activities in the week before the census: looked after children at home; looked after other dependants at home; housework; attended full-time study or training course, took part in physical recreation or sport, and none of these activities. The question on voluntary work was modified by asking for the total hours of voluntary work and the type of work the person spent the most time on.

In this discussion, we will focus on the 1991 'Activities' question, and in particular, the responses regarding housework and looking after children. No time dimension is included in this question. Respondents could tick the box as to whether they looked after a child for five minutes or five days, whether they cooked just one meal or all the meals and cleaned the house, so the replies give no indication of intensity of involvement. Nevertheless, if respondents ticked the box, it may indicate that they put some value on this work.

It is not immediately clear what is covered in 'housework'. Most respondents may have seen housework as activities carried out within the house such

as cleaning, or washing clothes. However, the notes accompanying the census questionnaire state that 'housework also includes gardening, lawn mowing and general household maintenance'. Thus the wording of the question may have led to understating the amount of 'housework' men do. It is clear from the pilot time-use survey, and other studies of the division of household labour, that women tend to do work inside the house while men tend to do outside work (Fleming and Easting, 1994: 27).

'Looking after children at home' also lacks definition. No notes are provided for clarification. Ticking the box could cover a range of possibilities. It may be a father looking after his own child full-time during the week or keeping an eye on them after school; an older brother or sister looking after a younger sibling; or a grandparent doing some short term babysitting. The definition of children is left up to the respondent and it is likely that people will have differing views as to whether teenagers are 'children'. There may also be uncertainty whether playing with children, or taking them on visits, constitutes 'looking after'. There are also likely to be cultural/ethnic variations in the way people respond to this question.

Despite these problems, a major advantage of the census against an instrument such as a time-use survey is its wide coverage of the population. According to the Department of Statistics, the 'Activities' set of questions had an overall non-response rate of 3.2%, but the non-response was highest amongst Maori or Pacific Islanders, males and low income people. In addition, it is possible to relate the 'Activities' data to a wide range of variables, including labour force status, gender, age and ethnicity, and the characteristics of individual family

members, including ages of any dependent children. Given this coverage, and the lobbying to have questions on unpaid work added to the census, it is surprising that there has been very little analysis of this data source.

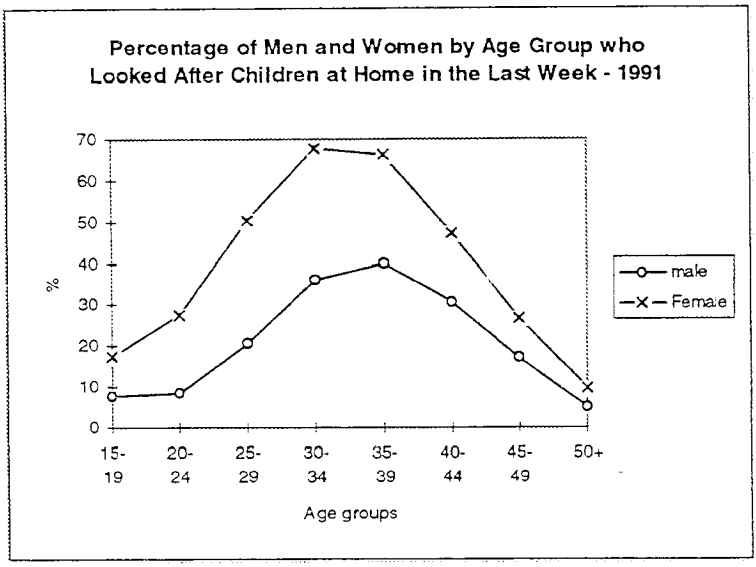
Data from the Census

(I) Work at Home - Differences by Age and Gender

In this section, the analysis begins with an overview of the whole population over the age of 15. It then narrows down to the 20-39 age group, a time when family formation and childrearing generally takes place. Specific attention is then brought to bear on both parents within two parent families with children under five. It is suggested that these families are where childcare will be at its peak as an unpaid activity.

The people most likely to be involved in childcare and housework are young adults in the main child-rearing years. (Figures 1 & 2). Consistent with time-use surveys throughout the world, women are more likely than men to undertake childcare at all ages (Gershuny and Robinson, 1988; Juster and Stafford, 1991). At the peak childrearing years, the male/female gap is the widest but tends to decline with age.

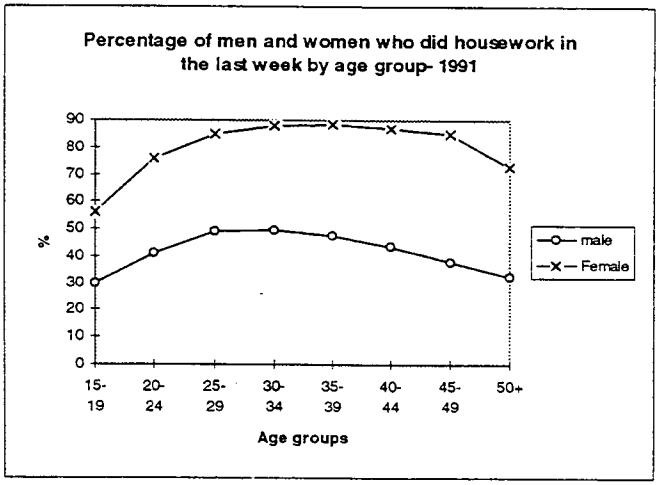
Figure 1



Source: Census, Statistics New Zealand

Figure two shows that women are also more likely than men to undertake housework at all ages. The patterns reported by teenagers suggest that sex-role stereotyping begins at an early age in New Zealand, well before young people set up their own homes. While men's participation peaks in the early twenties and then drops off, women continue to participate at a much higher rate. The trend lines continue to diverge up to age 50. Whether these differences imply a greater willingness by younger men to participate in housework, i.e a cohort effect, or whether the pattern reflects the amount of unpaid work to be done, combining both childcare and housework in the young adult age groups, remains a question to be investigated.

Figure 2



Source: Census, Statistics New Zealand

(ii) Labour Force Status

Regardless of their labour force status, women are more likely than men to be involved in both childcare and housework, according to the 1991 'Activities' question. In other words, even when both are in full-time paid work, women are more likely to undertake domestic responsibilities than men. This is true for all ages and ethnic groups.

These gender differences are especially marked in the main child-rearing age group - 20-39 years (Table 1). For childcare, the difference between male and female participation is narrowest for full-time workers who may well not have children of their own. It is widest for part-time workers, those seeking part-time work and the group not in the labour force. The percentage of men who had looked

after children varied little by labour force status. There is much more variation for women. Those working full-time or seeking full-time work had the lowest levels (closest to male rates), with figures around 70% for the other categories.

For housework, differences between male and female participation are wide regardless of labour force status. Male involvement in housework is higher than for childcare and varies more by labour force categories, but again the lowest rates are for men not in the labour force and the highest rates for unemployed males. In contrast, for women, involvement in housework is uniformly high, regardless of labour force status.

It is worth looking more closely at differences between men and women, aged 20 to 39, in the part-time, unemployed seeking part-time work and not in the labour force categories. Women not in the labour force are likely to be full-time homemakers and carers. Many women working part-time may also see unpaid work as their main activity, with paid work fitted in around their domestic responsibilities. The status of men who are classified as not in the labour force is not clear. A few may be full-time homemakers, but information presented here and elsewhere does not suggest that this as a widespread situation (Callister, 1994; Easting and Fleming, 1994: 28). It is more likely that most are discouraged workers, unemployed but not actively seeking work. Part-time work for men in this age group, which has increased over the last census decade (Callister and Davey, 1994), may also reflect the tight labour market. Men may be working part-time not from choice, but because this is the only paid work available. Thus 'not in the labour force' implies different roles for men and women. We suggest that the women are oriented mainly to unpaid work at home, whereas most of

the men are actually members of the paid workforce but currently unemployed or underemployed. The group cannot therefore be realistically seen as a single category when analysing trends in paid and unpaid work.

Table 1 Work at Home - Age Group 20-39 by Labour Force Status - 1991

% who indicated they participated in looking after children and housework in the last week

	Labour force status				
CHILDCARE	Full-time	Part-time	Unemployed & seeking full-time work*	Unemployed & seeking part-time work*	Not in labour force
Male	29	28	32	32	25
Female	39	68	55	74	70
HOUSE-WORK					
Male	44	44	50	51	35
Female	79	87	83	89	80

* Unemployed and seeking work using the 1991 definition

(iii) Ethnicity

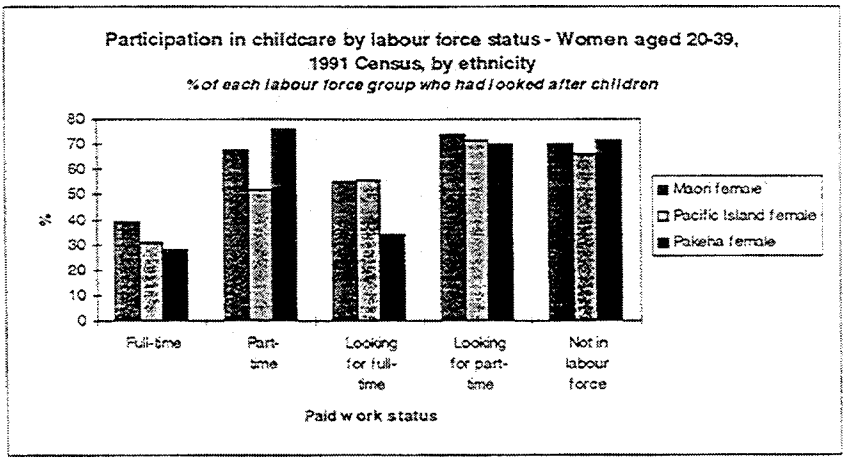
Taking the 20-39 age group as a whole, Maori men and women had slightly higher levels of involvement in childcare than either Pacific Islanders or Pakeha. The 1991 census levels were 60% for Maori women (52% for the other two groups) and 29% for Maori men (25% for the other two groups).

Figure 3 shows the percentage of women in each labour market category who looked after children. In all three groups, women working full-time, or looking for full-time work, were least likely to have looked after children, but the rate was lowest for Pakeha women. A high proportion of Pakeha women combined childcare with part-time paid work.

There is, however, little difference by ethnicity for the non-labour force and seeking part-time work categories. The higher levels of child-care involvement for Maori women may be associated with higher fertility levels and a lower average age of child-bearing. Age specific birth rates for the total population are highest in the 25-29 age group, but for Maori women peak in the 20-24 age group, with rates for women under 20 twice as high as those for the total (Department of Statistics, 1992).

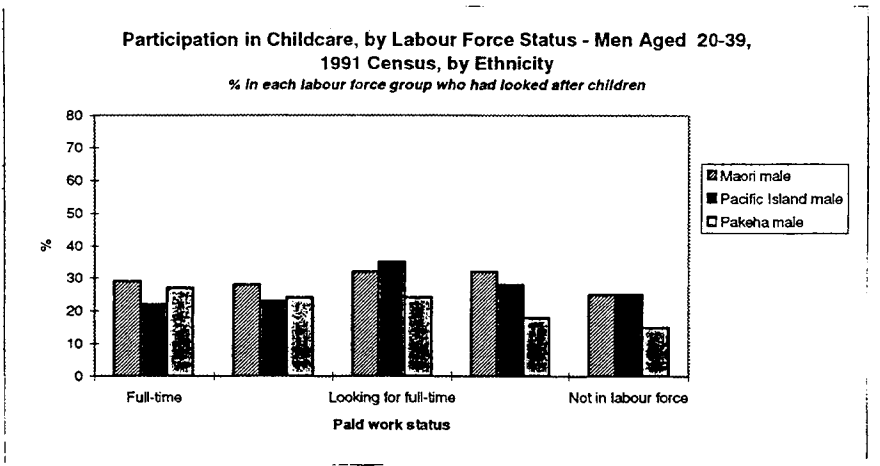
Male differences by ethnicity were much less marked. The higher rates for Maori and Pacific Island male involvement in childcare apply specifically to the unemployed and non-labour force groups (Figure 4). This may be because a higher proportion of Maori and Pacific Island men live in extended families.

Figure 3



Source: Statistics New Zealand

Figure 4



Source: Statistics New Zealand

For housework, it is the Pakeha group which tends to have the highest participation rates, followed by Maori and then Pacific Islanders (figures for full-time female workers aged 20-39 are Pakeha 84%, Maori 79%, Pacific Islander 62%)

(iv) Two Parent Families With a Dependent Child Under Five

Many adults, even in the main child-rearing age groups are not parents, although this does not preclude their involvement in the care of children in their households. However, in this section, we focus on two parent families with pre-school children, the extent to which fathers are involved in childcare, and the effects of labour force status, education and income levels.

Table 2 gives the percentages of fathers who had cared for children, according to the labour force status of the father and of the mother. It appears that where mothers are in the labour force, a higher percentage of fathers undertook childcare. This seems to imply greater sharing of this type of unpaid work when mothers enter paid work. However, where both partners are not in the labour force, the fathers' participation is lowest, at 51%. Given the availability of a female parent at home, half of the fathers may leave all of the childcare to her, even when there is a very young child in the family.

Table 2 Two Parent Families by Labour Force Status of Both Parents -Youngest Child Under Five Years of Age - 1991[1]

% of fathers who stated that they looked after children at home in the last week

	Fathers			
Mothers	Full-time	Part-time	Unemployed*	Not in labour force
Full-time	66	81	83	73
Part-time	70	72	86	70
Unemployed*	59	66	72	59
Not in labour force	56	58	70	51

* unemployed and seeking work (1986 definition)
Source: Census, Statistics New Zealand

The pattern is more complex when we introduce educational achievement by both parents. Table 3 shows the situation where both parents are in full-time paid work. Higher levels of fathers' involvement in childcare are associated with higher educational attainment by both fathers and mothers, peaking at 82% when both have university degrees, and with a low of 45% where both have no educational qualifications.

[1] The numbers in some of these cells are relatively small. The total numbers of fathers in each cell in tables 2-4 is therefore set out in the appendix.

Table 3 **Two Parent Families by Highest Educational Qualification of Both Parents - Both in Full-Time Paid Work - Youngest Child Under Five - 1991**

% of fathers who stated that they looked after children at home in the last week

	Qualification of mother				
Qualification of Father	University	Other tertiary	Total School	No Qualification*	Total
University	82	77	74	66	78
Other tertiary	77	71	67	64	69
Total school	76	69	65	60	66
No Qualification*	70	61	58	45	54
Total	79	70	65	54	66

* Includes still at school

Source: Census, Statistics New Zealand

The patterns are less clear when both parents are not in paid work (Table 4). However, in this situation also higher parental educational levels seem to be associated with higher levels of childcare involvement by fathers. The pattern is similar where the father works full-time and the mother works part-time or is not in paid work.

Table 4 Two Parent Families by Highest Educational Qualification of Both Parents - Neither Parent in Paid Work - Youngest Child Under Five - 1991

% of fathers who stated that they looked after children at home in the last week

	Qualification of mother				
Qualification of Father	University	Other tertiary	Total School	No Qualification*	Total
University	71	76	64	62	70
Other tertiary	76	68	68	62	66
Total school	76	69	65	65	66
No Qualification*	74	63	64	55	58
Total	73	67	65	57	61

* Includes still at school

Source: Census, Statistics New Zealand

Relative income levels are another factor. Again in two-parent families with pre-school children, the percentage of fathers who had looked after children was similar when both parents were in the same income group, or where the fathers' income was higher (63% and 64%, respectively). Where the mother earned more than the father, 68% of fathers had looked after children.[2]

It seems clear that educational qualifications of both mothers and fathers influence the likelihood that fathers will

[2] Mothers earned more than fathers in only 8.5% of these two parent families.

take up childcare. Differences by labour force status may be related to educational levels. However, relative incomes of mothers and fathers appear to have little effect on childcare levels for fathers.

Discussion

Novitz (1987) reviewed small scale studies on unpaid work in New Zealand undertaken in the 1970s and early 1980s. She suggests that while men may be spending more time with their children, and husbands of women in paid work do slightly more housework, the major responsibility for unpaid household work and childcare still rests with women, even when they are in full-time employment (Novitz, 1987). In their review of time-use surveys, Gershuny and Robinson (1988) controlled for socio-demographic changes, more women in paid work, more unemployed men, and smaller families. They, in contrast, argue that in both the USA and UK, women in the 1980s do substantially less housework than those in equivalent circumstances in the 1960s, and that men do a little more, although still much less than women.

The official statistics from the early 1990s on unpaid work at home in New Zealand do not indicate a significant involvement by men, in childcare in particular, despite decreases in male, and increases in female, labour force participation. Even given difficulties with the data, the patterns for men not in the labour force are consistent with overseas studies. These indicate that while there are isolated cases of unemployed men taking on a much greater share of domestic work, overall the uptake is not high, with little evidence of 'role reversal' (Lewis, 1986: Morris, 1990). Studies of unemployed men and their role in childcare in the UK suggest that when

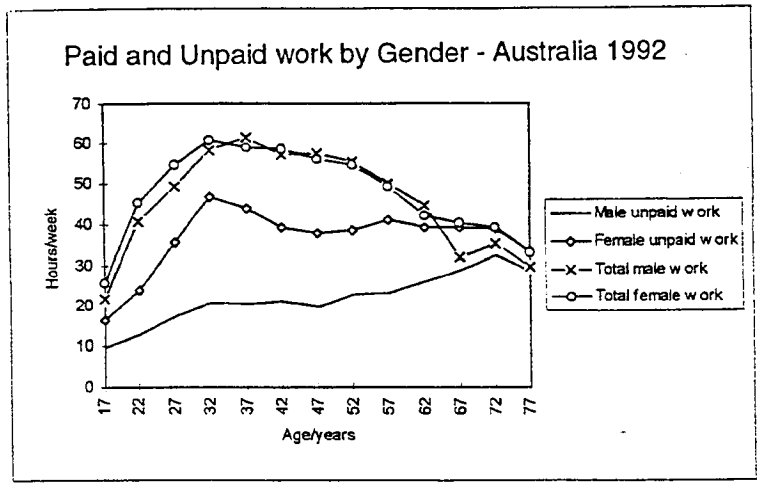
the man's partner is also at home he picks up little of the domestic work. Morris suggests that this is partly because working class men, who are the most likely to be unemployed, identify domestic labour as women's work. There is also a defensiveness from women who do not wish to give up this role.

The New Zealand data for families where both parents are in full-time paid work raise some questions about the applicability of overseas studies on the division of labour, A number of studies, based around families where both parents are in paid work (Brannan and Moss, 1990; Crosby, 1991; Hochschild, 1989; Pleck, 1985; Sandqvist, 1987) indicate that, over time, men in these families have increased their **relative share**[3] of unpaid work. While this may in fact be true for many New Zealand families in this group, the census data appear to indicate that in a third of families, the father undertakes no (or more likely very little) childcare work.

Where both parents are in full-time paid work, some women are facing a 'double shift', but it is difficult to assess the extent of the extra burden because full-time work for women in these situations typically involves fewer hours than for men (Davey and Callister, 1994). In some cases, women will be doing more total work than men. However, Australian data shows that if the total hours of paid and unpaid work for men and women are combined over the whole population, their overall work hours are very similar (Ironmonger, 1994) (see Figure 5).

[3] Relative to the mother's unpaid work within a particular household

Figure 5



Source: Ironmonger, 1994

Juster and Stafford (1991), when comparing international time-use studies, also found that in industrialised countries, men's and women's total work hours were, on average, very similar. If these findings are generally applicable, it means that in New Zealand there is likely to be balancing of total paid and unpaid work by men and women. So if, in future, over the whole of society, male unpaid work in the home is to increase, then their paid work hours need to decline. At the same time, women's paid work hours would need to increase. For paid work, this is not an unreasonable assumption. Over a long time period, male hours of paid work have been declining and female hours increasing. If this trend continues at its present rate, women and men will be working the same average hours of paid employment less than 30 years from now (Bittman, 1991).

But the need for men's unpaid work in the home to increase is based on the assumption that the amount of unpaid work in society stays static over time. Gershuny and Robinson (1988) argue that the total volume of domestic work has declined since the 1960s. Part of the reduction in unpaid work has come about through 'professionalisation' of childcare and housework (Jaggar, 1988). Hertz (1986), using American case studies, notes that where family resources are sufficient 'chequebook solutions' are used to resolve conflicts over childcare and housework. While this might at times appear to bring about a greater sharing of tasks in the home, on a wider basis, the employment of predominantly women workers in childcare and household services continues to uphold, and in many situations further entrench, traditional gender roles (Howard-Smith, 1993; Jensen, 1986; Wrigley, 1991). For example, in kindergartens and childcare centres, men represented a mere 2% of the labour force in 1993 (Ministry of Education, 1994).

In households that can afford it, therefore, unpaid work may be decreasing as it is shifted into the commercial sphere. However, a contrasting argument suggests that unpaid work commitments may be increasing in other circumstances. This has come about in countries where welfare state provisions are being retrenched and where responsibility for caring is being thrust back upon families and communities. Examples include the deinstitutionalisation of psychiatrically, intellectually and physically handicapped people; the promotion of 'community' care and moves to retain frail elderly people in their homes rather than in residential care. The assumption that families are able and willing to provide such care and

that it is acceptable to the receivers has been vigorously challenged (Mason & Finch, 1993).

To further illuminate these issues, regular surveys of both paid and unpaid work patterns need to be carried out. If greater equality in unpaid work is seen as desirable, then researchers need to design methodologies for data collection and analysis which ask not only 'who is responsible for (most of) the family labour?' but also 'for which activities and under which conditions can or will family tasks be redivided between partners?' (Van Dongen et al, 1993: 1).

Conclusion

Official sources of data on unpaid work in New Zealand have major limitations. But what we can deduce from them is consistent with the findings of overseas time-use surveys. In particular, gender is still the major factor influencing who carries out unpaid work in New Zealand homes. In all age and ethnic groups, and regardless of their involvement in paid work, women still are more likely to participate in childcare and housework than men. Women also dominate the workforce in paid childcare and domestic occupations. Signs of movement away from these traditional roles are slight, and the implications of some policy changes are to increase the demand for unpaid work by women.

We conclude that looking at unpaid work on its own is potentially as unrealistic as analysing paid work in isolation. The demands made upon women for unpaid work can only decrease if men reduce their hours of paid work and are willing to take on more domestic responsibilities, or if the work is further 'professionalised'. The latter may improve the balance of paid and unpaid work at home, but generally has

the result of transferring the job to other women. So over the whole of society, sex role stereotyping in housework and caring work remains strong.

Unpaid work is a major component of the total amount of work which needs to be done to make society function. If the important interactions between paid and unpaid work which have been explored in this paper are to be clarified, the collectors of official statistics need to move away from their heavy concentration on paid work. Statistics New Zealand have begun this with their inclusion of questions relating to unpaid work in recent census collections. More thought needs to be applied to improving existing questions, as we have indicated. However, ultimately there is no substitute for a full-scale time-use survey if we wish to find out more about paid and unpaid work by men and women in New Zealand, and the factors which influence their participation.

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Appendix

Table 2a Two Parent Families by Labour Force Status of Both Parents -Youngest Child Under Five Years of Age - 1991

	Fathers			
Mothers	Full-time	Part-time	Unemployed*	Not in labour force
Full-time	21,507	951	822	1,476
Part-time	31,125	1,125	888	810
Unemployed*	4,431	231	1,863	651
Not in labour force	61,683	2,202	7,854	8,589

* unemployed and seeking work (1986 definition)

Source: Census, Statistics New Zealand

Table 3a Two Parent Families By Highest Educational Qualification Of Both Parents - Both In Full-Time Paid Work - Youngest Child Under Five - 1991

	Qualification of mother			
Qualification of Father	University	Other tertiary	Total School	No Qualification*
University	1,419	1,056	501	105
Other tertiary	663	3,243	2,568	1,275
Total school	405	1,668	2,208	819
No Qualification*	162	1,335	1,854	2,091

* Includes still at school

Source: Census, Statistics New Zealand

Table 4a **Two Parent Families By Highest Educational Qualification Of Both Parents - Neither Parent In Paid Work - Youngest Child Under Five - 1991**

Total fathers

	Qualification of mother			
Qualification of Father	University	Other tertiary	Total School	No Qualification*
University	237	240	174	78
Other tertiary	123	1,389	1,524	1,680
Total school	87	687	1,476	1,140
No Qualification*	57	1,167	2,163	6,165

* Includes still at school

Source: Census, Statistics New Zealand

Poverty in New Zealand: 1981-1993

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Introduction[2]

After around two decades of modern poverty research in New Zealand which has focused on poverty at a point in time, it is now possible to provide estimates of the changing numbers of poor over time.

The overall conclusion may not seem surprising. If the standard poverty level of the Royal Commission on Social Security (the BDL) is used, the proportion of the poor in the population as a whole hardly changed between the March year 1981 and 1990. In the following three years to March 1993 year (the latest currently available), the number of the poor rose sharply. This conclusion is not particularly dependent on the level used to measure poverty, if it is set in constant price terms. However, this paper shows that if the line is based on a population mean or median income, the interpretation is much more problematic.

Because there is an evident shift in the level of poverty in the 1990s, the paper concludes by tracing the factors which caused these changes. The conclusion that the benefit cuts seem to have been the main contributing factor will not be

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2 I am grateful to comments from two referees and Sally Jackman

surprising, but the research also identifies a labour market deterioration before the benefit cuts.

Before providing the new results the paper provides a brief review of poverty research, to place those results into a context and development.

Poverty Research in New Zealand

Modern poverty research in New Zealand began in the early 1970s with the survey of large families in Hamilton by Peter Cuttance (1974), the Department of Social Welfare's Survey of the Aged (1975), and my work using a diverse set of sources and approaches (Easton 1973a, 1973b, 1973c, 1976). Exact precedence is unimportant, the publication dates need not reflect when the work was done, nor its results disseminated to fellow researchers. What is relevant is the development of a number of research studies at about the same time.

That the research began then does not mean there was no poverty before the 1970s, nor that it increased markedly about that time. We simply do not know. There has never been the data bases to estimate the magnitude of poverty earlier - indeed the possibilities of a quantitative tracing of poverty through time only really begin in the 1980s. However, it seems likely that there was considerable poverty, in the modern sense, before the research began.

The research seems to have been precipitated by three major factors. First, research overseas - especially in Australia, Britain, and the USA - percolated to New Zealand. Second, there was a sharp deterioration in New

Zealand's economic performance from late 1966, which lasted about a decade and added to the financial pressures of many families. Third, the Royal Commission on Social Security (RCSS), established in the wake of economic stagnation and reporting in 1972, focused attention on income adequacy questions. Certainly the RCSS's report has been a seminal document for all subsequent thinking on poverty issues.

A fourth factor which facilitated research was the establishment by the Department of Statistics (now known as Statistics New Zealand) of a regular household economic survey, called Household Economic Survey (HES) today, but previously known as the Household Expenditure and Income Survey (HEIS). Easton's (1976) review paper used the first available survey of 1973/4 to provide national estimates of aggregate poverty. Initially the income side of the survey was not very reliable, but by the early 1980s, there had been sufficient development to give confidence in the HES income statistics.

This critical data base was supplemented by various special surveys which provided information on which the HES did not (and often could not) cover. The statistical quality of the surveys varied from the rigor of the Department of Social Welfare's survey of the Aged and the Christchurch Child Development Study's (1978, 1980a, 1980b) longitudinal investigation of a birth cohort, to ad hoc surveys of little professional competence which nevertheless sometimes gave useful insights (Easton, 1986).

By the late 1980s, the foundations of the research program had been set down (Easton, 1991). Subsequent work has

been to build on the paradigm, and there have been few new innovations.

The Poverty Research Paradigm

Much poverty research, especially that oriented towards estimating the size and composition of the poor, may be thought of as identifying and measuring a particular characteristic of a population (usually income) distribution. We are familiar with the notion of a mean of a distribution as a measure of the overall population - so faced with a statement like 'real disposable per capita income rose 1 percent this year', most people accept without much reflection that this is a statement about the average. However, the mean is only one measure of a statistical distribution which, while popular, may not always be the most appropriate. In particular, the middle of a distribution is not relevant to examine when considering poverty.

There are a number of other ways of describing the distribution of a population, including the lorentz curve, the gini coefficient, and the coefficient of variation (Easton, 1983). Each has its strengths, and weaknesses, including the difficulty of computation and the understandability of subtle statistical concepts by the general population. Instead, poverty research in New Zealand has tended to focus on the number (or proportion) of the population below a particular income level called the 'poverty line'. A variety of poverty lines have been proposed, but the basic idea of counting the number at the bottom of the distribution has been a central theme of almost all of New Zealand's quantitative poverty studies.

The intention of the exercise is not to conclude that those who are a dollar below the poverty line (whatever that is) are terribly badly off, and those who are a dollar above are in a satisfactory life style, although this is an often unfortunate misinterpretation of the gee-whiz approach. Rather the single figure is intended to give some order of magnitude, and perhaps guidance to policy directions.[3]

The Population Distribution: Disposable Household Income Adjusted for Household Composition.

At an early stage, the accepted population distribution was disposable household income adjusted for household composition. Disposable income is total household income from market and benefit sources (but rarely inter-household private transfers), less income taxation and other compulsory outgoings. The income is then adjusted for the household composition. At the simplest level, this might be thought of as placing income on a per capita basis so that households with more people have their disposable income scaled down on pro rata basis. In practice, a more sophisticated procedure based on household equivalence scales is used, which allows for economies of scale in household spending, and the lesser needs of children. More is said about equivalence scales below.

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3 Another measure, until recently difficult to calculate, is the 'income gap' which is the amount of income necessary to bring those up to the income of the poverty line. Thus it gives some notion of the distribution of the hardship within the poor. We shall see that there are problems in the interpretation of the measure. It is likely to remain a much less important measure in the foreseeable future.

In the 1970s, the calculation of the resulting distribution was very ad hoc, but the Statistics New Zealand ASSET model of the household means, today that each sample household in the HES sample can be operated on separately.

A couple of alternative population measures have been household expenditure rather than income, and to deduct housing expenditure from disposable income. Deducting housing expenditure from income generates a mongrel concept, signalling the crudity of the method. The problem of housing in poverty studies has been long recognised - essentially the problem is that many people have an implicit addition to their income from a favoured housing situation, such as a mortgage free home - but the deduction method is embarrassingly coarse.[4]

Determining the Poverty Line

Initially a number of possible poverty lines definitions were explored, but by the end of the 1970s, the most commonly used one was the RCSS BDL, or the Royal Commission on Social Security Benefit Datum Line. Originally described as the Pensioner Datum Line (PDL), it had been the level set by the 1972 Royal Commission for a couple on the 'pension' (i.e. aged benefit), or other social security benefits, adjusted in line with changes in prices.

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[4] A related problem is that the household equivalence scales have to be adjusted if housing expenditure is removed

Behind this pure dollar rate was some subtle argument, which has not always been recognised in subsequent developments. The 1972 Royal Commission, which devoted considerable space to discussing various aspects of poverty, argued that the aims of the social security system was 'to ensure, within limitations which may be imposed by physical or other disabilities, that everyone is able to enjoy a standard of living much like that of the rest of the community, and thus is able to feel a sense of *participation in and belonging to* the community' (Royal Commission on Social Security: 1972:65 original's italics). This objective was a major consideration when they set their benefit levels. Thus the BDL sets a poverty line which implies a definition that a typical person with that income is able to participate in and belong to their community.

How did the 1972 Royal Commission decide its recommended poverty line was adequate for the purpose? It heard a lot of (typically qualitative) evidence from submissions, and, on this basis, made an informed judgement. Now that judgement could be wrong - certainly, it was not precise. However, the evidence of the systematic and ad hoc surveys is that the BDL appears to be near a material standard of living which enables the average couple to fulfil the Royal Commission objective. The true figure, assuming 'true' is a meaningful notion in this context, might be 10 percent higher or lower, but the available evidence suggests that the Royal Commission made a reasonable stab at the level (Easton, 1986).

There have been three major proposals for an alternative poverty line.[5] The first argues that the notion of participating in and belonging to involves behaviour and material consumption relative to the population as a whole. As the average level of material welfare changes, so should the poverty line. The adjustment is easy enough if an economy is growing (or declining) uniformly, but suppose there is a sharp contraction which lowers average real incomes by 10 percent say. Does the poverty line go down by this amount too? Easton (1980) exhibits angst over this problem - perhaps not as much as he would have had he known what was going to happen to average incomes in the 1980s (see below). But as best as one can interpret him from hindsight, his position was that the poor should not be expected to carry the full burden of adjustment of short term swings in material prosperity (especially in a downward direction), but if there is a secular change (including declines) in average real incomes, the poverty line would probably be expected to adjust in parallel over time (unless there was a change in social attitudes).

The second alternative, proposed by two Treasury official, is to relate the poverty line to a food entitlement (Brashares and Aynsley, 1990; Brashares, 1993). A detailed critique is published elsewhere (Easton, 1995), but for this paper's purposes we note the exercise was almost entirely bereft of any empirical input, and certainly made no attempt to incorporate the behaviour of the poor in any assessment of the measurement of their deprivation. It chose a not entirely

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5 One option which has not been explored is a poverty line based on the rate of national superannuation. it could be argued that an adequate minimum income for the elderly should apply to the entire community. (See Easton 1980.)

arbitrary bundle of food,[6] which it costed, and then multiplied up by a parameter (of 4, with alternatives of 3 and 5) which was arbitrarily chosen by the researchers. More fundamentally, it is far from clear what was the underlying notion of poverty.

The third alternative proposal involved using a particular proportion of a median income level (Stephens, 1994). We shall be examining this notion in greater detail below, and will find it gives paradoxical outcomes. Unfortunately, the underlying conceptual framework is as enigmatic as that of the Treasury officials'.

The choice of the median appears to arise because it is used in international poverty comparisons. However, the international academic studies do not chose the median because of any inherent merit, but because it is a practical solution to a problem of standardisation of cross-national comparisons. It would be foolish to hang a nation's social policy on some decisions of a few foreign academics struggling with a very different issue.

Stephens (1994) seems to acknowledge this, because rather than using the 50 percent proportion which the international studies do, he argues that the poverty line should be 60 percent of the median. The choice of ratio is no less arbitrary than that of Brashares and Aynsley. Indeed, Treasury officials could be entitled to argue that if an international criteria is to be used, than so should the

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6 Although less than that which would be fed to prisoners. While it is not obvious why in welfare terms beneficiaries should be less well fed than those incarcerated, it is to be noted the latter are more likely to riot

international parameter, and that the 50 percent is more appropriate. Either choice is subjective, containing little reference to the living circumstances of the poor. That Stephens uses expressions like 'preferred', without any criteria of preference, and in the passive voice to disguise who is preferring, adds to the general picture of capriciousness of the approach[7]

A project involving 'focus groups' was introduced to give some empirical content to a poverty line. Unfortunately, the material has still not been written up, so comment depends on fragmentary reportage. Apparently a small number of selected families are asked how much they think is necessary to maintain an adequate standard of living. We are told that their conclusion is not very different from Stephen's preferred poverty line.

Supposing this is an accurate account of the work, there are two points to be made. The first is that the procedure is dependent upon small samples. Unfortunately, large sample studies from overseas show that those in different material circumstances give different answers. Regrettably, the focus group approach uses such small samples that the sort of evaluative procedures for their more efficient overseas counterparts are not available.

Secondly, and even more fundamentally, the focus groups are not asked to provide an estimate based on some proportion of a median defined income, the notion that Stephens is proposing. Instead they give estimates of

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7 There is an interesting parallel between the poverty lines in that Stephens omits housing expenditure and Brashares and Aynsley omit everything except food expenditure.

absolute expenditure (and not income), which is then compared to the outcome of the median income notion. We are told that in 1990/1, their selection was similar to the 60 percent level. Now this level changes in later years, and it is not clear at all that the focus groups would change their assessments of needs by the same amount. For instance, in 1991/2, the median fell by about 8 percent compared to a year earlier (see below). It seems likely that the focus groups in 1991/2 would chose the previous real outlays, rather than adjust their assessments down too, so the relativity becomes 60*1.08 = 65 percent of the median. Thus the calibration applies at best for a single year. The procedure thus fails miserably to provide a regular updating of the poverty line.

The Data Base

As already explained, the usual data base by which poverty numbers are calculated comes from the HES, after transforming unit household records. Usually the transformation involves calculating the disposable income, and adjusting the total for household composition by using a household equivalence scale.

The most readily accessible data is that published by Mowbray (1993) which provides adjusted household disposable income for seven of the ten years from 1981/2 to 1990/1[8] In addition Mary Mowbray has kindly made

[8] Because slightly different equivalence scales are used in the first three years of the data and the last three years, it has been necessary to meld the series, using the 1987/8 year data which is calculated using both scales. The splice is

available her estimates for 1991/2 and 1992/3, based on the latest HES available. The Mowbray data is presented as the mean equivalent disposable income in each income decile. Figure 1 shows the income shares of the ten deciles. It is based on appendix Table I[9]

Unfortunately, the middle deciles are rather crowded and it is difficult to identify patterns within them. However, there are two obvious effects. Income in the highest decile increases relative to the rest after 1988 because of the income tax cuts on upper incomes in the late 1980s, while the Income in the lowest decile bumps around due to losses from the self employed. This phenomenon is returned to later.

Inspection of Appendix Table I enables a quantitative assessment of the changes. Over the 13 year period, the income share of the top decile increased by around 4 percentage points. This was at the expense of the bottom seven deciles. The big losers were those in the 3rd to 8th decile (above the groups which are typically treated as the poor) who each lost at least .5 percentage points. However, the 9th decile, while experiencing a cut in its income share of around .3 percentage points, experienced a proportional cut of around 5 percent.[10]

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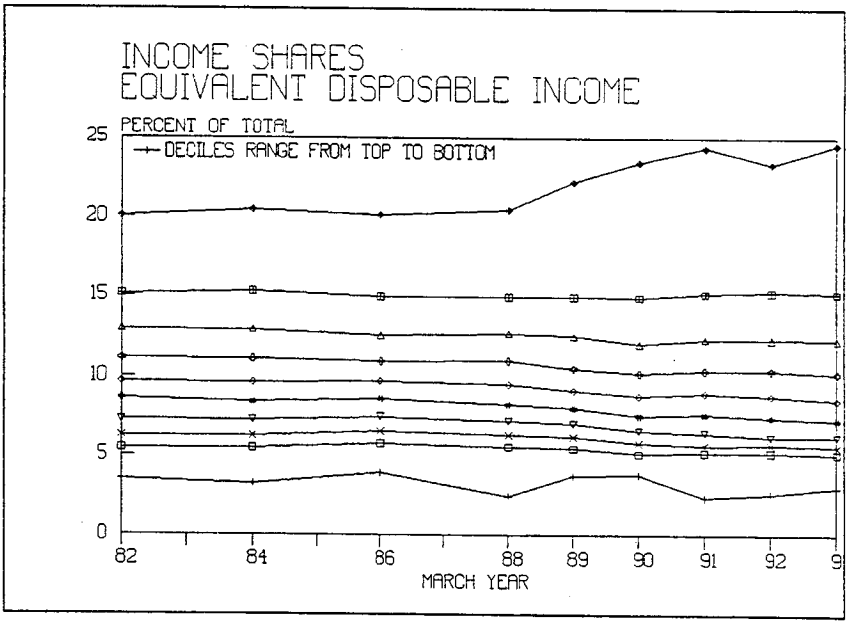
small, and does not obviously alter the conclusions presented here.

9 Because the mean real adjusted disposable income changes little over the period, the graph is also similar to that for average incomes.

10 The Gini coefficients - a measure of overall inequality - show the same story, increasing from an average of .291 in the 1981/2 to 1987/8 period, to .304 in 1990/91 to 1992/93. A shift of .013 may seem small, but Easton (1993) points out

Figure 1



Data from Appendix Table I

These changes should not surprise us. The substantial tax increases given to high incomes in the late 1980s, meant that others' disposable income relativity would suffer. However,we need a finer method than decile shares to explore the actual impact on the incomes of the poor.

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that under certain assumptions, a shift of that magnitude would represent roughly a 10 percent increase in the coefficient of variation of a log normal distribution.

Before further using the data,a couple of caveats should be noted. The first is that like most work in recent years, the data uses a household equivalence scale due to Jensen (1988). Although fashionable like some of the poverty lines we have examined, the Jensen scale is based upon a priori theorisation, with little empirical content. There are some five New Zealand household equivalence scales, three of which are based upon empirical evidence of sorts (Easton 1994). It is noticeable that the Jensen scale 1988 is the one with the strongest economies of scale, that is the one which thinks household costs rise most slowly as the size of the household increases, and the second lowest weighting for children. These are a priori assumptions of the Jensen scale, sometimes justified by crude comparisons overseas (e.g. Brashares, 1993). However, when Easton (1973b) made a careful comparison between New Zealand and New York, and found quite different economies of scale effects.

Suppose the Jensen economies of scale assumption is significantly wrong (as the empirically, based scales suggest). Results based on it will be distorted. The likely effect will be to underestimate the degree of poverty in large households. especially those with children. Thus we should be cautious when interpreting the poverty estimates - they are probably conservative.[11] The development of empirically based authoritative accepted equivalence scales is one of the priorities if poverty studies are to progress.

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[11] At least with regard to those based on an absolute poverty line. Those dependent on the mean or median are further complicated by the reference point itself being partly based on the Jensen scale.

The second issue is that the Mowbray figures, like many others, report households, not people in households. There is a tendency for the poorest to be in large households. Thus in 1992/3, the 10 percent of poorest households contain 11.4 percent of all people, and the twenty percent of poorest households contain 21.3 percent of people. Where appropriate, the figures reported here are for people rather than households.[12]

Poverty Lines: 1981/2 to 1992/3

The easiest poverty line to understand is those which are constant throughout the period. The first is the 1972 Royal Commission's BDL which was equivalent to $14050 p.a. for a couple in 1991/92 prices.[13] It should be noted that the BDL is not the same thing as the actual benefit level, although for many years they were closely linked. In addition, we have provided estimates for the Treasury level, labelled as TDL, which comes to about $11050 p.a. Futher, an upper level, labelled as UDL ($17050 p.a.), an equivalent amount above the BDL, is also shown.

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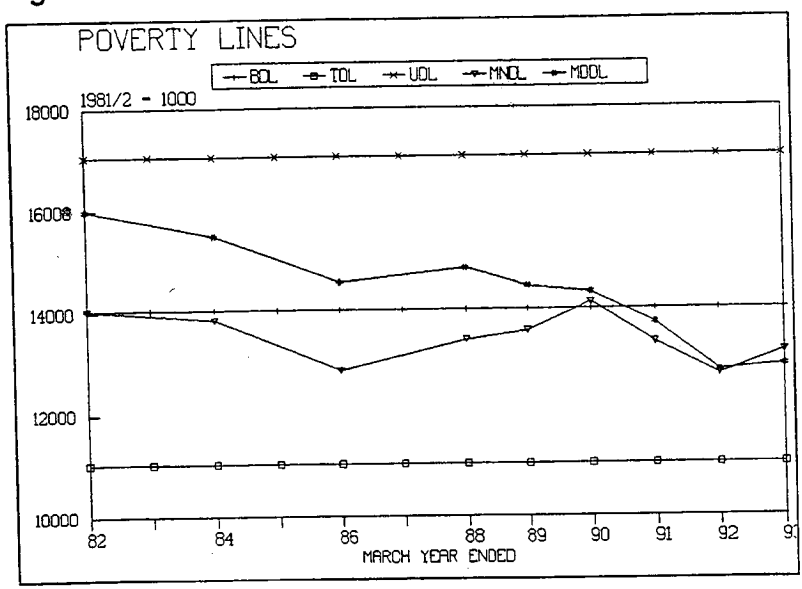
12 A final difficulty with the figures, which reflects more upon the method to be used here than the underlying data, is that the method assumes the distribution is somewhat smoother than it is. Statistical distributions of this nature are usually smooth, but the impact of benefits can mean that there are sharp changes in the distribution, as when a large number of people on the minimum benefit cut into the distribution.

13 All the following calculations use 1991/2 prices, those being the ones used in the Mowbray data base. Each poverty level refers to the required annual income of a couple. Other household compositions can be adjusted from this by using the (Jensen) equivalence scale.

The second group of poverty lines arise from adjusting the BDL for changes in disposable income. On earlier occasions, Easton (1980, 1990) adjusted by real per capita private disposable incomes . However, between 1983/4 and 1992/3, this measure fell .7 percent[14] while mean household disposable incomes fell by 4.8 percent in constant prices, according to the Mowbray figures. That represents an annual difference of .44 percent a year, which might be due to error and definitional differences, but may also reflect changing household composition. Perhaps it hardly needs worrying about. Even so, it is incorporated in the calculations.

Figure 2



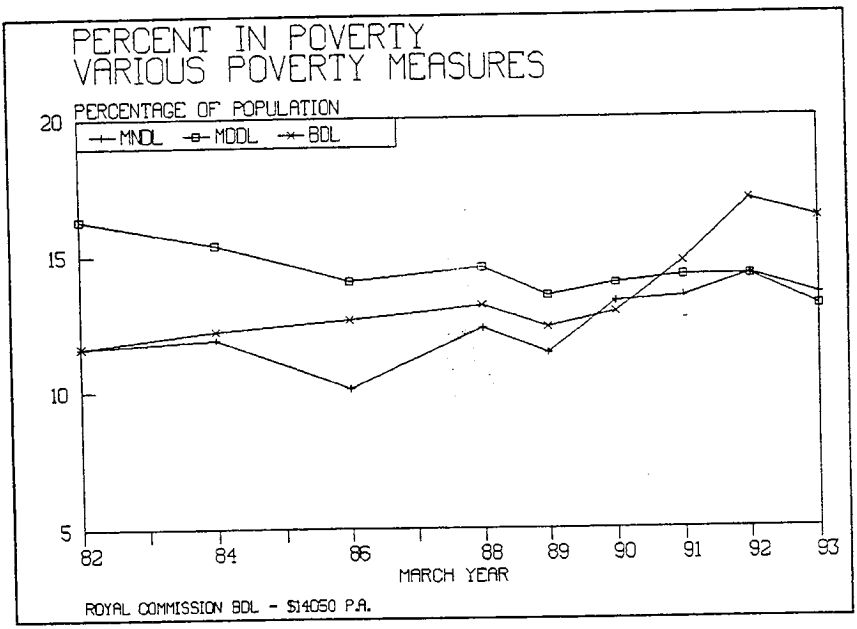
[14] There is no comparable official figure for 1981/2.

Being unable to find a suitable disposable income series before 1983/4, the calculations use a real private consumption expenditure as a proxy. It rose 4.5 percent in per capita terms between 1971/2 when the Royal Commission set the BDL and 1981/2. After adjusting for the .44 percent p.a. effect, the poverty line would have been about the same level as the BDL in 1981/2. It has a different path after, so we call this level indexed to the mean equivalent (disposable) income MNDL. As Figure 2 shows, it tended to deteriorate through the 1980s and early 1990s, a decrease of 6.0 percent in total, or an average of .6 percent a year. This reflects the falling levels of income over the period, especially after 1987/8.

The third poverty measure uses the median income as the reference point, taking Stephen's proposed 60 percent as the relevant proportion. We call this MDDL. As shown in Figure 2, it has experienced an even greater decline than MNDL, a total of 19.2 percent or 1.9 percent a year over the eleven years. This reflects a changing income distribution, in which the income share of middle income households are falling relative to top income households in a general period of stagnation. The main cause of the shift was the increasingly favourable income tax treatment on upper incomes at the expense of those in the middle.

Figure 3 shows the estimates of the headcount poverty for the three central poverty measures. They show quite different patterns. The estimates based on the BDL - that is a constant real poverty level - conform most closely to the conventional wisdom, rising mildly through the 1980s, and leaping in the 1990s. We shall be considering them in further detail in the next section.

Figure 3



The MNDL - which allows for changes in average incomes - shows a general drift downward of poverty numbers in the early 1980s, as average incomes fell, but the real value of benefit levels were maintained and unemployment fell slightly. The reversal in the unemployment trend in the late 1980s (with some weakening of real benefit levels towards the end) meant poverty numbers on this measure increased mildly, and they increased again in the early 1990s, although not as dramatically as for the BDL.

The third measure, the MDDL, gives an almost entirely counter intuitive pattern of headcount poverty. It shows the proportion declining from 15.0 percent in 1981/2 to 11.7 percent in 1992/3, with no obvious rise in poverty in the early 1990s. This conclusion is an artefact of the falling level of the MDDL over time. Basically, the index is falling faster than poverty is rising.

To understand why this has happened, consider the following thought experiment, not unrelated to what actually occurred over the period. Take an income distribution and calculate the headcount poverty based on some proportion of the median as the measure. Now suppose the government taxes those on middle incomes, and transfers the revenue gains to the rich in lower taxes. The mean income will not change, but the median (which is based solely on the distribution of middle incomes) will fall. As a result, the MDDL will fall, and so the numbers in poverty will fall. Paradoxically, a transfer of income from the middle income to the rich (with no change in the incomes of the poor) reduces the degree of poverty in the community on this measure. This outcome is so illogical as to suggest that poverty levels based on median incomes as reference points are almost completely useless.[15] It is difficult to see any merit in the measure, other than for those who think reducing taxes on the rich is socially beneficial.

In regard to the MDDL, this writer confesses to being sympathetic to relating the poverty line to changes in

[15] Indeed it suggests that their use for international comparisons may be totally misconceived too.

average incomes, since the 'participating in and belonging to' notion depends on some overall community standard. Indeed, an important shift in thinking on poverty, officially initiated by the 1972 Royal Commission, was from an absolute notion of poverty to a relative one. However, I remain perplexed on how to incorporate changes in average real income levels into the analysis. Here I want to suggest a tentative strategy. Recall that attempts to calibrate the BDL left a margin of error of about 10 percent. Mindful of this, I suggest that the poverty level be a constant real value one until the mean disposable incomes move 10 percent above (or below) the level at which the level was calibrated.

Thus we are still in a situation where the concept of a constant real value suffices as the best measure we have of poverty level, given the limited changes to average real income over the last two decades.

Poverty Numbers Since 1981/2

However, I have never been committed to the precise BDL level: hence the searching for evidence to verify its appropriateness. It is now possible to offer poverty estimates based on different constant real price levels. The following analysis provides estimates for seven constant real value poverty levels for the period from 1981/2 to 1992/3. The middle level is the BDL at $14050 p.a. for a married couple, and with three levels above and three below, at $1000 p.a. steps. The upper level is the UDL and the bottom level is the Treasury TDL.

The resulting estimates are shown in Figure 4 and Table 1. The pattern is similar for the seven levels. Basically, the proportion of the poor did not change much between 1981/2 and 1989/90. After that, it rose sharply, typically by between a quarter and a half. The pattern is not simple. The lowest increase is for the BDL (26.2 percent) while the extremes of the TDL experience a 55.1 percent increase and the UDL 37.4 percent increase.

Figure 4

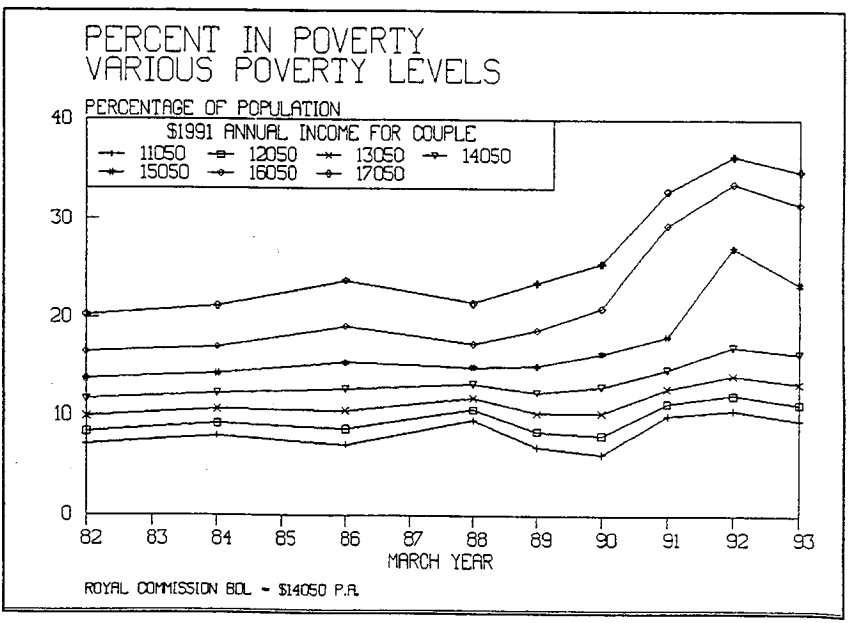


Table 1: CHANGES IN POVERTY PROPORTIONS

POVERTY LINE $1991/2	11050	12050	13050	14050 (BDL)	15050	16050	17050
			PROPORTION				
1981/82	7.1	8.4	9.9	11.6	13.7	16.4	20.2
1989/90	6.2	8.0	10.2	12.9	16.3	21.0	25.4
1992/3	9.5	11.2	13.2	16.3	23.3	31.5	34.9
% INCREASE 89/90 TO 92/3	55.1	39.3	29.7	26.2	42.5	50.3	37.4

This means that there were complicated processes affecting the poor in the late 1980s and early 1990s. An evident effect was the benefit cuts which occurred in early 1991, typically on 1 April 1991 (and hence first impacting in the 1991/2 year).

Another complication is that careful examination of the graph shows that there were some deterioration in the incomes of those in the disposable equivalent income brackets between $14050 and $16050 p.a. in the two years before the cuts occurred. The most likely explanation is that the deterioration of the labour market in that period was impacting on labour earnings (including supplementary earning for beneficiaries).

Table 2: Two Estimates of Poverty in 1989/90 and 1991/2[16]

NUMBERS OF POOR	Easton (1993)	This Paper
1989/90	355,000-360,000	430,000
1991/92	485,000-540,000	593,000
CHANGE	35%-52%	35.3%

Before leaving these figures, it is appropriate to briefly revisit an earlier estimate of the change in poverty over the cuts period. Using the equivalent disposable income distribution derived by Brashares and Aynsley (1990), and adjusting for subsequent distributional change as reported in the HES, I found that poverty defined by the BDL measure increased by around 43 percent, with a range of 35 to 52 percent. The new figures suggest that the increase was 35.3 percent at the bottom of the earlier estimate. This seems to arise because I had slightly underestimated the numbers of poor (Table 2). Readers are left to judge whether the earlier estimate was misleading. Here I note that the figures were not robust to a percentage point, but the ad hoc procedure was reasonably satisfactory at giving an order of magnitude[17]

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16 The population increased by 2.4 percent over the two year period.

17 Part of the problem may be that the two distributions I used were different (with the Treasury based one a little higher - and to the right - of the Mowbray one).

Who are The Poor?

As interesting as gee-whiz figures may be, of greater interest is the composition of the people who are in the poorest part of the distribution. When considering the issues of the composition of the poor, there are two separate questions: which are the largest groups among the poor, and which groups are more likely to be poor. The difference is that the second question may be answered in terms of groups who are small in the community, even though they may not make up a large proportion of the total poor.

Mowbray has supplied figures by household type by equivalent disposable income decile for 1992/3. The answers to the two questions are as follows:

Table 3: Who are the Poor?

The largest groups in the bottom deciles are (in order):

Two adult households with three or more children
(about 27% of total)
One adult households with one or more children
(about 19%)
Two adult households with two children (about 16%)
(no other group above 10 percent)

The groups with the largest proportions in the bottom quintile are (in order)

One adult households with one or more children
(about 43 percent of the group)
Two adult households with three or more children
(about 28 percent)
(no other group over 25 percent).[18]

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[18] There are two anomalies.

In summary, the largest group of the poor remain children and their parents. Almost 29 percent of all children are in the bottom quintile (in contrast to a fifth of the population). The relative proportion in the lowest decile is even higher.

In contrast, the rich are adults only households, who make up 76 percent of the topmost quintile, despite being only 41 percent of the population.

Allowing that these figures may be conservative, because of the equivalence scale used, we must conclude that the situation first identified two decades ago - of children being the greatest reason for poverty - remains true to this day. This is despite various family assistance measures (which have not been large in actual expenditure outlays; St John, 1994). Probably the most effective means of dealing with family poverty is still the simple strategy of increasing family assistance.

Seventeen percent of single adult households (who are not superannuitants) were in the lowest decile, but only 5 percent in the next. Thus there appear to be a group of single people who are poverty stricken, and who are quite unlike the rest. One might conjecture that these are unemployed and others in similar work situations who are not eligible for benefits, plus students

While only 13 percent of national superannuitants are in the bottom decile, the proportion who are is skewed towards single households, who tend to be in the top part of the second to bottom decile, and in the third decile.

The Income Gap

The 'income gap' or 'income deficit' is the amount that is required to bring the poor up to the poverty line. It measures the intensity of the poverty, because it is larger when the lower the average income of those below the poverty line.[19] Table 4 gives the income gap for the BDL for each year, and for the seven poverty levels in 1992/93. There is a number of ways of representing the measure. Here we report it as a proportion of the poverty level.

Table 4: INCOME GAPS AS PERCENTAGE OF POVERTY INCOME

Income Gap for BDL (by years)

81/82	83/84	85/86	87/88	88/89	89/90	90/91	91/92	92/93
26.4	31.3	23.2	44.0	22.9	18.6	40.9	34.8	29.5

Income Gap for different poverty lines (1992/3)

$11050	$12050	$13050	$14050	$15050	$16050	$10750
32.6	32.3	31.5	29.5	23.4	20.8	23.4

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19 It is not, however, a measure with direct relevance to policy. It does not tell us how much it would cost to bring everybody up to the poverty line, since the implicit marginal tax rate is an impractical 100 percent.

Disappointingly, the data does not make a lot of sense, leaping about in unpredictable ways. Inspection shows this is related to those years in which households with income losses (typically from self employment) are larger than usual. The instability, which is partly sampling error but also may reflect the volatility of self employed incomes, could be eliminated by clipping unwanted observations from the bottom. Such an approach leaves considerable room for arbitrary decisions, and the statistical properties of the clipped series are far from clear.

The data seems to suggest that the size of the income gap seems to have gone up in the early 1990s, relative to the 1980s. Thus there are not only more poor, but the hardship of the poor has been greater.

Conclusion

Readers are unlikely to be surprised that poverty and hardship rose, on any reasonable definition, in the 1980s - the statistics confirm the anecdotal evidence and survey (e.g. Dalziel, 1993; List, *et al* 1992; NZCCSS, 1992 People's Select Committee, 1992). They may be surprised that the distributional structure was so stable through most of the 1980s. The most evident impact was the 1988 tax cuts, and there is a hint of the effect of labour market deterioration shortly after. But in summary, there is little evidence from the available data that market changes had a major influence on the distribution of household income over the period. Distributional policies involving taxation and benefit levels, and even demographic change, seem to have had more effect.

The study has cleared away some misleading developments in thinking about poverty. Median-based poverty levels seem useless for tracing poverty over time. Hopefully too, a little progress has been made on the thorny question of indexing the Royal Commission BDL for changes in real incomes: it proves broadly unnecessary over the last two decades, but at some stage, the question of recalibration has to be addressed. Nor should some of the quantitative innovations developed in this paper be ignored.

Problems remain, even confining the concern to the measurement of the size of the poor, and leave definitions of the poverty line and the actual experiences of the poor as unresolved. The equivalence scale is still not acceptable, and problems like housing outlays and the losses of the self employed have still to be addressed with more finesse than they have in the past. Probably the different price experiences for different parts of the income distribution and household types becomes more significant as social welfare moves to a more targeted approach for user charges.

In policy terms, we know that benefits underpin the bottom of the income distribution, that our poor are children and their parents, and that the aged are not our worst off. We knew this all two decades ago. Hopefully, over the next few years, policy will develop in manner which will mean such insights need not remain research results with so little practical policy input.

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Appendix Table I
Household Income Shares by Deciles 1981-82 to 1992-93
Equivalent Disposable Income

	81-82	83-84	85-86	87-88	88-89	89-90	90-91	91-92	92-93
Top	20.1	20.5	20.1	20.8	22.3	23.9	24.8	24.0	25.1
2nd	15.1	15.3	14.9	15.2	15.0	15.2	15.4	15.7	15.5
3rd	13.0	12.9	12.5	12.9	12.6	12.3	12.6	12.7	12.6
4th	11.2	11.1	10.9	11.2	10.6	10.4	10.6	10.7	10.4
5th	9.7	9.6	9.6	9.7	9.1	9.0	9.1	9.0	8.7
6th	8.6	8.4	8.5	8.4	8.0	7.7	7.7	7.6	7.4
7th	7.2	7.2	7.4	7.3	7.0	6.7	6.5	6.5	6.3
8th	6.2	6.2	6.5	6.4	6.2	5.9	5.7	5.8	5.7
9th	5.4	5.5	5.7	5.6	5.4	5.2	5.3	5.3	5.2
Bottom	3.5	3.2	3.9	2.5	3.7	3.8	2.4	2.7	3.1
Mean $1991	29205	28809	27623	27965	28325	29471	27829	26497	27437
Gini Coeffi-ient	.291	.293	.288	.294	.294	.299	.305	.303	.305

Notes:

Data from Mowbray (1993:18), plus later years provided by M. Mowbray. The author is grateful for assistance from Vasanatha Krishna and Mary Mowbray.

To calculate actual income levels (in $1991) multiply income share by year mean, and divide by 10.

The Ecological Significance of Bourdieu's `Field' and `Habitus': Towards an Integration of Ecology and Practice Theory.[1]

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Abstract

This paper proposes a theoretical approach to the sociological investigation of relationships between the social and natural environments. This framework integrates ecological concepts about the relationships between organism and environment advanced by Lewontin with Bourdieu's theory of social practice. It is argued that because the social and natural environments are brought into relationship by the social and economic actions of

[1] This paper has been developed from preparatory work for an empirical study of relationships between the social and physical environments in an atoll society. The field work for this study has now been completed, but analysis of the data is not sufficiently advanced for the application of the approach outlined in this paper to be discussed in depth. However, the theoretical approach outlined here was central to the conceptualisation of the research problem and guided the acquisition of data in the field. A considerable amount of the data gathered was obtained for the purpose of charting, in detail, the social and physical spaces (fields) of one village to support the analysis of the relationship between them.

My acknowledgements are due to Margaret Denton of the Sociology Department at Massey University for her extremely helpful comments and suggestions on earlier drafts of this paper.

human beings, an analysis of social practice is essential to sociological approaches to the question. Bourdieu's theory of social practice transcends the traditional structure-agency divide by conceptualising the generation of social practice in terms of a dialectical relationship between structure and agency. The ecological framework advanced by Lewontin conceptualises a dialectical relationship between organism and environment in which organisms are actively involved in the construction and rearrangement of their environments. The integration of these approaches is accompanied by the application of a relational concept of space to the task of conceptualising the networks of relationships which constitute the social and natural environments.

Introduction

The problematic character of the relationship between human beings and the natural[2] environment is widely acknowledged and has become the focus of attention from diverse disciplinary perspectives. This paper focuses upon this postulated relationship, the character of which is taken to be reciprocal or dialectical because social and economic activity impacts upon the natural environment to bring about changes which in turn impact upon the social environment. The former impacts result from the actions of human beings, while the effects of the latter impact are manifested in human action.

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2 The terms 'natural' and 'nature' are used here simply to refer to the physical environment which provides both the physical space within which the activities associated with the social environment take place and the material resources upon which those activities depend

Because the relationship between the social and natural environments is substantially maintained through the actions of people, it is appropriate that a sociological study of the question should focus upon social practice as the site of the motive force maintaining the relationship between the two. The crucial sociological issue underlying this is associated with the question of the means by which social practice is changed or reproduced in a context of changing objective conditions and the most appropriate sociological approach to this question is within the framework of a theory of practice.

The growing consciousness of environmental issues has been associated with the growing social movement of environmentalism, a phenomenon which is of obvious interest to sociologists. However, this paper is proposing an environmental sociology rather than a sociology of environmentalism. In developing this approach, the paper proposes an integration of Bourdieu's (1977; 1990a) theory of social practice and ecological concepts derived from Lewontin (1982) and Ingold (1986; 1987; 1989; 1990 and 1992). The central postulate of the approach proposed is the inseparability of the concepts of environment and organism; that, as Lewontin (1982: 160-163) argues, the two are dialectically related and one cannot be defined without reference to the other. It will be shown that the ecological postulate of the interpenetration and mutualism of environment and organism is 'mirrored' in Bourdieu's representation of field (social environment) and habitus (social body) as being mutually constitutive because '[t]he body is in the social world but the social world is also in the body (Bourdieu, 1990:190).'

This paper is in six parts, the first of which briefly discusses the ontological basis of the approach being presented and the nature of the theoretical object with which it is concerned. Part two outlines the elements of practice theory and ecological thought which are later combined in Part six. Parts three and four discuss Bourdieu's theory of social practice and the concepts of Field and Habitus. Part five discusses the concept of relative space, its relationship to Bourdieu's concept of social space, and its use as a means of conceptualising the social and natural environments and their relationships to each other. Part six discusses the integration of practice theory and ecology.

1. The Theoretical Object

Central to the development of the approach advanced in this paper is the view propounded by Bachelard (1991:26) that 'the order of the given world [is both inseparable from and irreducible to] the method we use to describe it'. For example, a particular aspect of physical reality, a piece of wood, can be represented in diverse ways when it is viewed as a physical object, or a cultural object. As a physical object it can be subjected to different types of analysis: it can be registered in passing as a familiar object given to immediate perception; at a higher level of abstraction its gross dimensions can be measured and its weight and colour noted; at a more fine-grained level of analysis its chemical and molecular structure can be analyzed; and at a finer-grained level still, the chemical and molecular constituents can be subjected to analysis of atomic structure. As a cultural object, the piece of wood might be identified with the tree from which it was wrought and the uses to which it might be put defined in terms of the social

and economic significance of that tree. It might be identified as a natural object by some, while others might recognise it as an artefact. In these scenarios, the object is apprehended in different ways according to the perspective from which it is viewed and the means of analysis employed. The more fine-grained the analysis, the higher the degree of abstraction and the gross physical object given to immediate perception becomes, at the level of atomic resolution, a three-(or more)-dimensional space structured by the network of relationships between fields of atomic force.[3] As a cultural object, the piece of wood can be analysed in terms of its use and significance in the networks of relationships and meaning which constitute social reality. Which of these objects is the real object? It is contended that none of them is 'real' in terms of any ontological finality; they are all theoretical objects, whether explicitly and consciously constructed or not.

While the objects given to immediate perception are usually taken for granted and rendered meaningful to those perceiving them, in terms of unconscious schemes of perception inculcated during their socialisation, the objects which are not given to immediate perception can only be apprehended as a result of the exercise of what Bachelard terms 'a rational force immanent in the mind' (Bachelard, 1991:54). For example, the physical sciences number among their objects sub-atomic 'particles' whose existence were predicted on the basis of theoretical expectations prior to the availability of the means to detect them and provide empirical verification of their existence. Such objects are clearly not given to immediate perception, but have become

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[3] Concepts which are themselves constructions of theoretical labour

known as a result of sustained engagement between the real and the rational. Bachelard continues:

> Whereas reason was, in the early days of science, formed in the image of the world, now, in modern science, the aim of mental activity is to construct a world in the image of reason (Bachelard, 1991:54).

This rational construct represents 'a second-order realism, realism reacting against everyday reality and contesting immediate experience' (Bachelard, 1991:49). Bachelard rejects both realism and rationalism as dichotomous philosophical alternatives, arguing that the acquisition of knowledge of reality is associated with the opposition of mind and reality in a process such that:

> ...between them there are constant reactions, reactions that give rise to reciprocal resonance. At every single moment therefore, a renewed given world is offered to our mind (Bachelard, 1991:26).

Bachelard describes his own approach as 'applied rationalism' on the grounds that, as he puts it ' the epistemological *vector*.... goes from the rational to the real, and not the reverse...' (Bachelard, 1991:48). This theme of Bachelard's is developed and applied in the field of social science by Bourdieu, whose relational ontology significantly underpins the theoretical basis of the approach which is outlined in this paper.

Sharing Bachelard's rejection of a first-order realism, Bourdieu is further influenced by the critique of Aristotelian substantialism (Bourdieu and Wacquant, 1992:97), which

forms part of Cassirer's critique of both substantialist and nominalist ideas about the nature of concepts (Cassirer, 1923 and 1957:298-299). In the course of this critique, Cassirer distinguishes between *'thing-concepts* and *relation-concepts'* (Cassirer, 1923:9) and argues against the relegation of the category of relation to a secondary position by the privileging of thing-concepts which is associated with the substantialist equation of concepts with 'real' things. Cassirer argues that the development of modern science and mathematics has been accompanied by a move away from a focus upon things to a focus upon relationships and the development of the view that things, rather than existing as discrete entities, are defined in their relationships with other things (see also Cassirer, 1957:473-479)[4].

Bourdieu's application of a relational ontology to his theoretical approach is made clear when he says:

> I could twist Hegel's famous formula and say that *the real is the relational*: what exists in the social world are relations -- not interactions between agents or intersubjective ties between individuals, but objective relations which exist 'independently of individual consciousness and will,' as Marx said (Bourdieu and Wacquant, 1992:97).

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[4] This type of relational thinking is also central to Arne Naess's articulation of deep ecology which rejects 'the man-in-environment image in favour of *the relational, total-field image'* in which organisms are envisaged as 'knots in the field of intrinsic relations.' Intrinsic relations are such that any two things so related are each defined in terms of their relationship. If the relationship is dissolved, the two things are no longer the same things they were when related (Naess, 1989:28.

This relational ontology is central to Bourdieu's concepts of field and habitus which are discussed later.

Thus the suggestion, central to this paper, of two environments, the social and the natural, being brought into relationship by the social actions of human beings, must be seen as a theoretical construct.[5] To speak in terms of the social and the natural being brought into relationship by the actions of human beings is to speak conceptually and analytically within an ontology of relationships rather than 'real' objects.

2. Practice Theory and Ecology

The focus of this paper is the integration of practice theory within a broader ecological framework. Ecology is concerned with the interactive relationships between organisms and their environments (Gibson, 1979:8; Hardesty, 1977:290; Steward, 1955:30; Vayda and Rappaport, 1968:477) and this seems an appropriate place

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5 The empirical study based upon the theoretical framework proposed in this paper is conducted in terms of two spaces, or environments, the physical and the social, and the relationships between them. Initial consideration of the fieldwork data shows very clearly that the physical space is structured in terms of genealogy, generation and gender, the same factors which predominantly structure the social space of the community under study. The physical space is consequently highly socialised, while at the same time giving substance to the factors structuring the social space. Thus to speak of two environments is analytically convenient, a theoretical construct which should not be construed as 'real.'

to begin the formulation because central to the question of people-planet relationships are human beings and their relationships to two environments, the natural, and the social, which are themselves brought into relationship through their mutual relationship with people. It is these relationships which are the object of this paper and attention will be paid to developing an ecological framework which attempts to conceptualise a relationship between the human subject and what Ingold (1990:224) terms 'the continuum of organic life' in which a mutualism of person and environment is recognised (Ingold, 1992:40).

Practice theory, as it has developed since the early 1980s, is concerned with conceptualising practice, which is essentially 'human action in the world' (Sahlins, 1981:6), in terms of an integration of the concepts of agency and structure (Ortner, 1984:145), the relationship between which Giddens (1979) has termed one of the central problems of social theory. It is Bourdieu's formulation of practice theory which most influences the approach taken in this study. It is explicitly sociological and theorises the generation of practice as the synthesis of a dialectical interplay between objective social structures and subjective, cognitive structures at the individual level. Bourdieu's theory of practice is therefore concerned primarily with the social environment, although it does assume that climatic and ecological conditions represent a baseline for human life and the construction of social life (Bourdieu, 1977:116).

The corollary of the establishment of sociology as a discipline distinct from psychology and biology, both of which share with sociology the study of human beings, has been its focus upon collective phenomena external to the individual and the explanation of social behaviour and

phenomena in terms of social facts rather than biological facts. However, the bracketing of the biological by sociologists has been regularly identified as a problem by those attempting to apply sociological analysis to understanding relationships between society and nature because the natural realm has been so clearly associated with the biological sciences (for example, see Catton: 1972; Catton and Dunlap: 1978a; 1978b; 1980; Dunlap: 1980; Dunlap and Catton: 1979; 1983; Benton: 1991; 1992; 1994; Crook: 1980; 1987 1989; Dickens: 1992; Ingold: 1986; 1987; 1989; 1990; 1992).

The contemporary convergence of environmental concerns with sociological analysis has its origins with the pioneering environmental sociologists, Catton and Dunlap, who vigorously opposed the exclusion of biology from mainstream sociology on the grounds that its inclusion was necessary if sociological analysis was to be able to come to terms with the natural environment upon which the social environment depends. Other recent formulations by social scientists to effect such an integration have been proposed by Benton, Crook, Dickens and Ingold, while working from the 'opposite' direction has been the biologist Robert Hinde (1987, 1991).

Central to such syntheses is a departure from the neo-Darwinian emphasis upon genes as the mechanism underlying the evolutionary adaptation of organisms to their environments, to an approach which conceptualises the development of organisms taking place in the context of an interaction between inherited particulars and environmental influences (Goodwin, 1984). It is this movement away from biological particulars (the gene) towards an holistic account of organisms in relationship with their environments which

provides a conceptual meeting ground for ecology and practice theory because it facilitates the development of a single conceptual framework within which organism and social agent, natural environment and social environment can all be incorporated. Consequently, the approach adopted here is not to attempt to give equal weight to social and biological explanation, but rather to incorporate consideration of the natural environment in company with the social environment through the use of a single conceptual framework.

The reintegration of aspects of reality which conventional disciplinary boundaries have differentiated poses conceptual problems associated with the need to rework concepts drawn from the disciplines concerned to form a new conceptual framework. It is with such a work of reconceptualisation that this paper is concerned.

3. Social Practice

Bourdieu's approach to theorising social practice is applicable to the focus of this approach because it constitutes social reality in terms of a dialectical relationship between objective structures and subjective (cognitive) structures in a way compatible with the organism-environment dialectic proposed by Lewontin and Ingold. The objective structures include both social structures and material conditions of existence, which include the natural environment. The mechanism by which these two apparently incommensurate elements are brought into dialectical relationship is represented by the concept of habitus, a concept which provides the framework within which a theory of social practice is constructed and

reconstitutes the objective and the subjective as elements in a distinct social ontology. Bourdieu defines habitus as:

> ...systems of durable transposable *dispositions*, structured structures predisposed to function as structuring structures, that is, as principles of the generation and structuring of practices (Bourdieu, 1977:72).[6]

In this dialectical relationship, the dispositions constituting habitus are formed in response to the conditioning of the 'material conditions of existence' upon the individual (Bourdieu, 1977:85) and in habitus the objective social structures become embodied in the individual. In this relationship, objective structures assign limits 'to the habitus's operations of invention' by virtue of the habitus 'being objectively adjusted to the particular conditions in which it is constituted', and as a result of which 'the habitus engenders all the thoughts, all the perceptions, and all the actions consistent with those conditions, and no others' (Bourdieu, 1977:95). But within those limits, habitus embodies 'an endless capacity to engender products -- thoughts, perceptions, expressions, actions' (Bourdieu, 1977:95). Habitus thus represents the embodiment of both conditioning and creativity. Bourdieu describes habitus as a 'paradoxical product [which] is difficult to conceive, even

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[6] Bourdieu's concept of habitus is very similar to Searle's concept of background, which Searle describes thus:

> ... the background consists of mental capacities, dispositions, stances, ways of behaving, know-how, savoir faire, etc., all of which can only be manifest when there are some intentional phenomena, such as an intentional action, a perception, a thought, etc (Searle, 1992:146).

inconceivable, ... so long as one remains locked in the dilemma of determinism and freedom, conditioning and creativity' (Bourdieu, 1977:95). The habitus therefore represents a 'conditioned and conditional freedom... [which] is as remote from a creation of unpredictable novelty as it is from a simple mechanical reproduction of the initial conditionings' (Bourdieu, 1977:95).

In its application to the individual, habitus can be likened to a social or cultural garment clothing the biological individual, and forming an interface between the individual and the objective structures which constitute the wider social realm. The dispositions constituting habitus represent the product of the socialisation of the individual in the matrix of the objective social structures, such as language and economy which are themselves the products of collective history (Bourdieu, 1977:85). However, to say that the dispositions which constitute habitus are the results of socialisation is not to identify the habitus itself with socialisation. The dispositions represent a resource upon which agents draw (with varying degrees of self-awareness) as they negotiate the social world. Thus the strategies which agents devise are in no way directly determined by the agents' socialisation but can, rather, be likened to bespoke creations cut from cloth which is, however, given by socialisation. These dispositions represent a past surviving in the present which, by their durability, they carry forward into the future. Thus, in Bourdieu's framework, individual agents are not engaged in constant 'unprecedented confrontation' with the world, but rather engage with the objective structures of the social world girded, as it were, by 'systems of durable transposable *dispositions*' (Bourdieu, 1977:72), or habitus. The acquisition of habitus by individuals occurs in 'a chronologically ordered series of

structuring determinations', such that 'the habitus acquired in the family underlies the structuring of the school experience' and so on (Bourdieu, 1977:86). The structuring associated with people's earliest experiences is dominant in the integration of individual agents who share a 'statistically common' exposure to particular conditions (Bourdieu, 1977:86-87).

The objective framework within which social action occurs is conceptualised by Bourdieu in terms of 'social space' and the field, which is discussed below. The social space is so constructed that agents, groups or institutions within it are more similar the closer together they are. This contrasts with geographical space, in which agents who are socially distant can be in close physical proximity. For example, in what Bourdieu terms a strategy of condescension, an agent occupying a high position can interact directly with an agent occupying a low position and symbolically deny the social distance between them. However, this visible lack of distance does not negate the structural distance in social space because the immediately visible relationships between agents can conceal the objective relationships between the positions they occupy and the objective relations manifested in this structural social distance are irreducible to the physical interactions which occur in geographical space (Bourdieu, 1985:16 and 1990:127-128). The point of this conceptualisation of social reality is to transcend phenomenal reality and construct a theoretical object in terms of what Bachelard (1991:49) calls 'a second-order realism ... reacting against everyday reality and contesting immediate experience'.

It is within the framework of dispositions and subjective, cognitive structures constituting habitus that social action

occurs. This action occurs not as the detemporalised form of execution in which objectivism constitutes it, but rather in the form of strategies pursued by agents over time (Bourdieu, 1977:5-9). So agents, even when acting within strong formal constraints, have the capacity to engage strategically in social interaction through their ability to determine the timing of their actions and responses in an interaction comprised of moves which might, in other respects, be predetermined.

4. The Field

The key concept which can be applied to all of the elements of reality which are brought into relationship by this study is the 'field'. Essentially a field, as the concept is used in this study, and in science generally, is a network of forces defined in terms of a particular type of 'space' which is itself defined in terms of the relationships between the elements pertaining to the field. In Bourdieu's use of the concept, a particular social field is defined in terms of social space, in which the positions of the agents, groups and institutions pertaining to the space are defined in terms of the social distance between them.

A particular field represents a field of forces, semi-autonomous of other fields, and the site of struggle for position within it. A field is therefore defined by a particular system of objective power relations between positions in the field. Positions within the field are determined by the types of symbolic capital available to agents, because it is the composition of this that determines which of the symbolic points may be occupied in the system of power relations.

However, the positions are not fixed because all are, in principle, contestable.

The concept of capital is drawn by analogy from the discipline of economics, but is not used in an economistic sense. Capital is considered to be of different kinds associated with different social fields. The varieties of capital often discussed by Bourdieu include: economic, social, cultural, symbolic, scientific and juridical. A particular field is defined in terms of the types of capital at stake in the network of power relations which structure the field. So, for example, in the religious field, symbolic capital is at a premium, but its acquisition can be facilitated by the possession of other forms of capital such as cultural capital in the form of academic qualifications and social capital. Within the economic field, the principal form of capital is economic capital, the acquisition of which is facilitated by the possession of cultural and social capital. In any field, then, a number of capitals are involved and the composition of an individual's 'portfolio' of capitals determines the positions open to that individual within the power relations of the field.

The relationship between agents and the social field to which they are attuned, as proposed by Bourdieu, is consistent with the ecological postulate of the interpenetration and mutualism of organism and environment. The natural environment can itself be conceived, in field terms, as a number of spaces of resources, each space or field representing the effective environment of a particular organism or species, person or social group. The biological individual can be conceptualised as a morphogenetic field comprised of a network of neighbouring parts arranged to form a specific

relational structure (Goodwin, 1984:228-240). Finally, the social individual, defined in terms of habitus (Bourdieu) or background (Searle), has a relational field structure comprised of mental dispositions, intentions and thought.

5. Space

Bourdieu's concept of social space is related in this approach to the implicit spaciality associated with the ecological conception of the organism surrounded by its environment and developed as a means of representing environments. Thus the natural environment is conceptualised in terms of physical or geographical space. The social environment is also conceptualised as a space, using Bourdieu's concept of social space, a space whose dimensions represent social relationships in terms of social distance. The relationships between the social and physical environments are therefore conceptualised in terms of relationships between, or an articulation of, two different spaces. Central to this articulation is a third space, the mental space, represented by Bourdieu's concept of habitus.[7] Defined as a structure of dispositions or orientations to action acquired by people during their socialization, habitus represents the embodiment of the structures of the social space within the person and expresses a mutualism of person and social space

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[7] Lefebvre (1991:11) also postulates three spaces or fields: firstly, 'the *physical* -- nature, the cosmos; secondly, the *mental*, including logical and formal abstractions; and, thirdly, the *social*.' He is concerned 'to discover or construct a theoretical unity between 'fields' which are apprehended separately'.

(environment) comparable to the mutualism of organism and natural environment (space) postulated by ecologists .

Here space is defined in the relational sense developed by Leibniz who 'rejected Newton's theory of an absolute space on the ground that space is nothing but a network of relations among coexisting things' (Jammer, 1954:48). Defined in this relational sense, the concept of space can be applied to any network of relationships, whether these are realised in physical (geographical) space or not. In fact, Leibniz himself illustrated his relational theory of space with the example of a genealogical line as a mentally constructed space of genealogical relationships (Leibniz, 1969:704).

The application of the concept of space within the social sciences is most clearly associated with human geography where it is applied to questions of location in physical space and spatial relationships in physical space. Attempts to apply space as a concept in urban sociology have, according to Prior (1988:86-87), emphasised relationships 'between buildings, settlements, landholdings and the like -- upon landscapes' resulting in a sociological geography rather than a sociology of space. Hillier and Hanson (1984:x) note the conceptual difficulty in relating society to physical space which results from the treatment of society as an a-spatial, abstract domain, and space as a physical domain. Soja (1989) also speaks in terms of social, mental and physical spaces, arguing that although these can be conceptualised separately this does not mean that they are autonomous, because they overlap and interrelate, and he argues that:

> Defining these interconnections remains one of the most formidable challenges to contemporary

> social theory, especially since the historical debate has been monopolized by the physical-mental dualism almost to the exclusion of social space (Soja, 1989:120).

By defining spatial order as 'restrictions on random process', Hillier and Hanson (1984:xi) provide a way of conceptualising both the natural and the social realms in spatial terms because just as the structure of physical space places constraints on the possibilities for action, so too does the structure of social space place constraints upon the possibilities for social action. The mental space, defined, in terms of habitus, as a structure of dispositions, can also be understood as imposing a restriction upon random process, in the sense that it embodies the structures of the objective social space, providing a conceptual framework for individual social agents in their engagement with the social realm and setting the parameters within which acceptable social action is located.[8]

The effects of social space can be just as tangible as those of physical space. Physical space is experienced immediately through the physical senses, so that an attribute of physical space such as distance can be apprehended through sight or muscular fatigue. Social space is experienced through habitus and the perception of social distance is a feeling induced by the recognition of outward signs of social differentiation. Just as physical distance is relative to its context and the means available

[8] This is not to suggest that agents are constrained by habitus to only engage in socially acceptable action. Clearly this is not the case; people can and do breach conventions; but in any particular hierarchy of social conventions some are more likely to be breached than others

for its traversing, so too is social distance relative and context sensitive in the sense that its ease or difficulty of being traversed varies according to the possession of social capitals which afford the means for doing so.

The spatial representation of the social realm can be illustrated with an example from the fieldwork based upon this approach. One of the principal dimensions of the social space of the community studied is the genealogical field. This field consists of positions defined by descent lines. These positions are distributed in a three-dimensional space the form of which is determined by the connections which exist between these descent lines. Whether or not two or more descent lines are connected depends upon whether or not one or more people can claim descent from them all. The spatial pattern that emerges is one of a number of zones beginning with a dense central region comprised of about one quarter of the adult population sharing descent from one descent line. These people are all connected to other descent lines which occupy the second zone in the genealogical space. There are people who claim descent from descent lines located in the second zone and the third zone, but not the first zone, and so on. The further a zone is from the centre the fewer are the number of kin based relationships enjoyed by the people associated with that zone. A person's position within this space is important because it indicates both the range of social resources which are potentially available to them and their relationship to the physical space in terms of residence location and land use rights, both of which are determined by descent. The shape of the genealogical space is therefore an important determiant of the socially sanctioned possibilities for social action which are open to any particular person.

6. Ecology and Practice: An Integration

Fundamental to an ecological approach is the concept of a dialectical relationship between organism and environment. An environment is literally 'that which surrounds' (Ingold, 1992:40) and as such is inconceivable without the existence of an active organism to be surrounded. If, as is being argued here, organism and environment are dialectically related and one cannot be defined without reference to the other (Lewontin, 1982:160-163), then the immediate environment of a particular organism must be defined to include those objects and forces used and harnessed by the organism in pursuit of a project which it embodies. The materials which comprise the environment of a particular organism do not in themselves constitute an environment, being, rather, the raw materials with which the organism constructs its own environment. Kaplan and Manners (1972:79) acknowledge this distinction by referring to the environment per se and the effective environment. The environment per se is the total stock of resources, materials, objects and entities comprising the global natural environment or the global social environment, while the effective environment is comprised of the resources, materials, objects and entities which the organism directly uses and is in relationship with. Similarly, Lewontin (1982:160) defines an environment as 'nature organised by an organism' in the sense that organisms manipulate the objects comprising their environments, they move things around to suit their purposes, internalise parts of their environment by eating them and replacing them with bodily waste-products, for example.

Developing the concepts of environment per se and effective environment provides a way of integrating ecology

and practice theory. The totality of objects comprising the natural environment per se represent a stock of resources upon which organisms draw in order for each to construct its effective environment. The environment per se is therefore host to a plurality of effective environments drawing upon a common pool of resources. Developments in one effective environment can impact upon another effective environment negatively by denying that environment's occupants access to customary resources. An example of this, in the global context, is the phenomenon of rising sea levels. This phenomenon, which results at least partly from activity within the effective environments of industrialised countries, impacts upon the effective environments of people who are geographically and culturally distanced from the source of change. In this case social and economic power is associated with an ability to manipulate and change the effective environments of others.

Similarly, the social world is host to a diversity of social environments, or fields. In Bourdieu's conception of habitus and field, the relationship between the two is very similar to the relationship of interpenetration and mutuality between organism and environment postulated by Ingold (1992:40) and Lewontin (1982:160). As Bourdieu (1990:190) puts it: 'The body is in the social world but the social world is also in the body' because the social body, habitus, and the social field are mutually constitutive. Consequently social reality exists both within agents, in habitus, and outside them, in fields (Bourdieu, 1990:127).

Of course not all social realities (fields) are compatible with all habitus because agents receive their primary socialisation in different social fields and the extent to which

particular agents will be attuned to a particular field will depend upon the stage in the process of their acquisition of habitus at which they became exposed to the field's conditioning influence. When agents operate within a social field to which their habitus is attuned they are like fish in water, operating in an effective environment or field of which their command is practical and unconscious. When, on the other hand, agents encounter a field to which their habitus is not attuned, they are like fish out of water, in an alien and, for them, non-effective environment. In this situation, the agent can only gain a command of the field which is conscious, because the field's characteristics must be consciously acquired, because they have not been inculcated during the agent's primary socialisation.

In terms of the integration of ecology and practice theory which is being attempted here, the concepts of environment and field can be interchanged and it is possible to interpret Bourdieu's framework in an explicitly ecological way. At the same time, the ecological frameworks advocated by Ingold and Lewontin are oriented towards practice in the sense that they conceptualise organisms/people engaging directly with their environments in the course of their practical action in the world. Ingold, for example, drawing upon the ideas of Gibson (1979), conceptualises the relationship between organism and environment in terms of effectivities and affordances. In this context, an effectivity is the action capability of an organism and an affordance is the possibility for action offered by objects in the environment (Ingold, 1992:52). The objects comprising the environment include those pertaining to the non-living or a-biotic world, the world of other species and the world of conspecifics; the affordance offered by any of these objects varies according to the project of the organism (Ingold, 1987:2). Applying this

conceptualisation to the social realm agents will recognise as social conspecifics those agents who share with them a particular social field. The affordances offered by these conspecifics will be of a different kind from those offered by agents who are non-conspecific due to their association with 'foreign' fields.[9]

Clearly different agents have greater and lesser effectivities within their fields and environments, while different fields have greater and lesser degrees of power to project their influence upon other fields and environments. Consequently the analysis of any particular instance of interaction between environment and field must be conducted in terms of which environment(s) and which field(s), and whose environment(s) and whose field(s), are involved. Such an approach offers the analytical advantage of incorporating natural and social environments within a single conceptual framework, while at the same time acknowledging a plurality of environments articulated through the actions of the agents to which they are host.

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[9] In the study based upon this approach, the 'environmental' characteristics of the genealogical field are revealed in the way people distinguish clearly between those people with whom they share descent and those with whom they do not. The former are clearly conspecifics from whom a fellow conspecific can expect support in the range of activities of which community social and economic activity is comprised.

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On Linking Sociology and Epidemiology: Some Observations on and a Proposal for 'Critical Epidemiology'

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Abstract

This paper considers a recent proposal by Arnoux and Grace for moving from 'physical' to 'critical epidemiology'. Although critiques of epidemiology are now common, this proposal is new in that it suggests epidemiology needs to incorporate analysis of the discursive realm. However, an evaluation of Arnoux and Grace's proposal finds it lacking in substantive methodological detail, and suffering from imprecision in the use of the term 'discourse'. In order to show that social science has an important contribution to the topics that epidemiology analyses, we discuss some possible inputs from Latour, Sacks and others, and then provide a case study of a major New Zealand accident, the 1984 ICI fire.

At a very general level, sociological and epidemiological approaches to health and illness share a common focus. That is, the way that illness, disease, accidents and injury are unevenly distributed in society. But just as neighbours often squabble about whether or not a shared fence needs painting, repair, or replacement, there are serious borderline disputes between epidemiology and sociology. Our impression is that epidemiologists are mostly happy with the way they approach the study of health and illness. From their side the fence, it looks fine. However, the view from

the other side is not so rosy: it is suggested that the epidemiologists' side of the fence is in fact poorly maintained, and it is to sociologists that epidemiologists should turn for advice on fence maintenance. In short, sociologists have argued that epidemiology works with an impoverished version of the social nature of health and illness.[1]

This paper is concerned in the first instance with one such argument from the sociologists' side of the fence. Specifically, we will consider a recent paper by Arnoux and Grace - 'Method and Practice of Critical Epidemiology' (1994) - which argues for a movement 'from physical to critical epidemiology'. Below, we will describe what this movement is seen to entail, offer some critical observations on it, and then via an introduction of some new material, offer some fruitful avenues for the development of critical epidemiology.

I. Arnoux and Grace's Argument

Two background details are useful to begin with. First, Arnoux and Grace's paper is published in a book - *Social Dimensions of Health and Disease: New Zealand Perspectives* (Spicer, Trlin and Walton, 1994) - which aims

1 More specifically, it has been argued that epidemiology has: constructed a risk epidemic (Skolbekken, 1995); described the distribution of health and illness but neglected causal connections to social inequalities; blamed the victim; medicalised rather than socialised prevention; focused attention on the behaviour of individuals rather than the quality of the social environment; and encouraged conservative policy changes (Krieger, 1994; Niehoff & Schneider, 1993).

to identify and debate fundamental issues on social aspects of health and disease in New Zealand. Second, their contribution is placed within a section which deals with 'Issues in Social Research Methodology', thus giving their paper a methodological slant. These background details are important because as will be seen below, the general form of our criticism is that Arnoux and Grace provide little in the way of practical research guidelines, and consequently we do not see how their version of critical epidemiology fosters the interdisciplinary endeavour which appears to be a main aim of the book and their paper.

Moving to content, Arnoux and Grace's argument is stimulated by their involvement in a research team contracted by the Accident Compensation Corporation to undertake epidemiological research on unintentional injury[2] in New Zealand. Literature reviewing and fieldwork convinced the researchers that there were 'numerous and substantial problems arising from both the theory and practice of contemporary epidemiology in the area of occupational health, and more specifically, injury' (Arnoux and Grace, 1994:260). Basically, the main source of these problems is found in the old debate about positivism: epidemiology is underpinned by the positivist tradition, but because accidents and injury prevention necessarily involve human agents, the methods of the natural sciences cannot be adopted outright in epidemiology. Certainly, epidemiology is concerned with the **social** determinants of disease and injury, but for Arnoux and Grace, epidemiology goes nowhere near far enough in considering the complex social web enveloping health and illness. Thus, the case is made for epidemiology to take on board the 'critical

2. Note that Arnoux and Grace use the term 'unintentional injury' because it is ACC's preferred term. For the sake of brevity, in this paper we prefer to use the term 'accident'.

epistemology of social sciences' (Arnoux and Grace, 1994:261).

Currently, most sociology undergraduates display a 'general horror of what they call 'positivism'' (Silverman, 1989:217), and there is nothing new in the tenor of Arnoux and Grace's comments on positivism. However, perhaps more unknown here (possibly owing to its publication in French) is their use of Latouche's terms, *objectivity* and *objectality*. Objectivity remains the search for valid and reliable knowledge, but the term objectality is coined to suggest that attempts at objectivity are actually reifications which do injustices to the real experience people have of injury, disease and so on. Arnoux and Grace suggest that epidemiology is embroiled in the tension between objectivity and objectality:

> Epidemiology is generally defined as the study of the distribution and determinants of health, disease and injury. This definition potentially positions epidemiologists as mediators between people and managing institutions. In practice they are most often involved in the tasks of surveillance that institutions need to manage the collective health of populations. While, individually and as a profession, epidemiologists may seek to work *objectively*, as agents of health management institutions they are socially positioned to work *objectally* (Arnoux and Grace, 1994:262, original emphasis).[3]

In essence, what Arnoux and Grace claim is that it is simply inappropriate for epidemiology to use natural science

3. Arnoux and Grace use a lot of italic emphasis, and from this point on, all quotes reproduce the original emphasis, unless stated otherwise.

methods for research into the social processes and interactions that constitute disease and injury. The correlate of this is that the social science emphasis on the **purposive** character of social life must be incorporated into studies of disease and injury.

From Physical to Critical Epidemiology

After having made these initial points, a very important step then occurs in Arnoux and Grace's argument. We find that the social science emphasis on the purposive character of social life boils down to a focus on the **discursive** realm:

> In a social context, for example in a factory, down a mine or quarry, in a shop or office, or at home, causality involves people's perceptions, understandings,motivations and communications. All involve *words*. All involve *discourse*. In this realm causality is more effectively defined in terms of *understanding social processes* through dialogue with the persons involved and through an analysis of their discourses (Arnoux and Grace, 1994:.265)

Again, this is familiar territory to the sociologist, but despite the pervasive nature of such an argument, Arnoux and Grace claim that they 'have not yet found a coherent approach to the discursive component of injury causation in the epidemiology literature' (Arnoux and Grace, 1994:267). Their attempt to fill this gap is labelled 'critical epidemiology', which is to be contrasted with 'physical epidemiology' (ie. traditional epidemiology).

The important point about discourse that Arnoux and Grace then note, is that it is not to be treated uncritically, not to be merely recorded and reported verbatim. This is because there are no 'pure facts': the world is known through

language, theories, presuppositions, and so on, thus making understanding and interpretation the key to social science inquiry. Thus hermeneutics becomes central to critical epidemiology, as is clearly seen in the three key stages to such work:

1. Preliminary qualitative hermeneutic studies of the concerns and views expressed by people about their disease (such as actual disease or accident and injury processes).
2. Quantitative information and data gathering, and analysis.
3. Hermeneutic interpretation of both qualitative and quantitative data. (Arnoux and Grace, 1994:267-268)

Clearly, Arnoux and Grace do not reject analysis via quantitative means, but equally as clearly, hermeneutic analysis of the discursive realm is absolutely central to their plan for critical epidemiology.

The final section of their paper is devoted to 'critical outcomes', and to structure their approach, Arnoux and Grace delve into the etymology of the word epidemiology. They suggest three main concerns needing to be addressed: '*epi* - the problem, *demos* - the people, *logos* - the understanding of what is happening and of what can be done about it' (Arnoux and Grace, 1994:270). All three involve critical dimensions. Firstly, in characterising the problem, we have the issue of whose perspective is to be taken: unions, insurance companies, and management groups have very different concerns about accidents and injury prevention. Consequently, critical epidemiology needs an hermeneutic understanding of different people's experience of the problem, and an analysis of institutional

context. Secondly, under *demos,* we have a focus on the people who suffer the disease or injury. Here the point is to move from treating people as a **population** at risk, an objectal approach, to a concern with understanding the actual people involved and their discourses: 'The purpose of accident reporting, for example, has little or nothing to do with understanding the people involved in accidents. In either case discourse is central' (Amoux and Grace, 1994: 271). Thirdly, under *logos* we get to an issue central for epidemiology - causation: '*Logos* refers to the understanding of the causes or determinants of the problem' (Amoux and Grace, 1994: 271). Here the argument is difficult to summarise, but essentially what Amoux and Grace suggest is that a critical understanding is arrived at by confronting all the various elements of evidence in a **dialectical** process. This is seen as a major point of contrast to physical epidemiology, and one which is crucial for the socio-political goals of critical epidemiology:

> While the manipulation of numbers to characterise accidents or disease pertains to formal logic ..., this is only a *moment* in the analytic process. It is necessary to move on from there to the broader dialectics of occurrence and prevention, the realm of causes, decisions, choices and actions leading to accident, injury or diseases, and ensuing from them. ... The starting and ending points of critical epidemiology are inherently: (a) hermeneutic, focusing on, and critically interpreting, discourses referring to both *epi-* and *demos* ... and (b) dialectical, focusing on the contradictions and conflicts expressed in these discourses (Amoux and Grace, 1994: 272).

Thus, it is the focus on both, hermeneutic analysis, and dialectical analysis of contradictions and conflicts, that

physical epidemiology lacks, and that critical epidemiology aims to incorporate.

Two final points should be noted before critical comment is offered. Firstly, Arnoux and Grace are not denying that physical epidemiology constitutes a valid and practically useful approach to the study of health and illness in society. They are, however, reporting their experience that it is 'inadequate in social settings where discursive aspects prevail, such as with unintentional injury' (Arnoux and Grace, 1994: 272). Secondly, the framework they provide for critical epidemiology is seen as being applicable beyond the confines of accidents. That is, they are making some claims about the general utility of critical epidemiology for research into the broader topic of health and illness. Certainly they are in no doubt that their programme has practical applicability, as is seen in their concluding paragraph:

> The ultimate objective then is not only to find physical points to intervene and break causal processes or to place a protective barrier, but more centrally and importantly to transform or replace the processes leading to injury or disease in the first place. This critical approach provides a firm basis for the development of prevention work because it is grounded in the experience of the people directly concerned with the problem (Arnoux and Grace, 1994: 273).

II. Critical Observations

Firstly, it should be made clear that we have no argument with Arnoux and Grace's reiteration of the critique of positivism: there is little doubt that there are extreme

difficulties in applying the methods of the natural sciences to the objects of inquiry of the social sciences. Linked to this, we are also in agreement that it is an important endeavour to fill-out the social focus of epidemiology. But, as will be made clear below, we are not so sure about Arnoux and Grace's specific proposals on how to do this, for, when interrogated, critical epidemiology looks to be in need of further development.

A first quick point of critique can be developed from the immediately preceding quotation. Note how the final sentence invokes the central importance of capturing the 'experience of the people directly concerned with the problem' - an invocation which is frequent in Arnoux and Grace's argument. This is surprising for Arnoux and Grace themselves note that there are no 'pure facts' (see above) - should this not be extended to argue that there is also no such thing as 'pure experience'? This is, of course, the upshot of the now widely publicised critique of 'authentic experience' associated with the postmodernist turn in sociology and social theory. In short, the notion that individuals possess privileged experience or an authentic voice has fallen out of favour to be replaced with an argument that we account for experience and give voice using socially available vocabularies. As we will make clear below, taking this point seriously does not mean throwing out what people say about accidents, but it does necessitate a change of analytic focus.

But our main misgiving with Arnoux and Grace concerns their use of the term 'discourse'. The general argument that discourse is a crucial part of social life is accepted, but a more specific problem arises in that Arnoux and Grace are very hazy about what they actually mean by discourse. Much like Kuhn's term 'paradigm', which at one count was used in 21 different senses (Masterman, 1970:61), the

meanings of Arnoux and Grace's term 'discourse' proliferate alarmingly. Without much effort, five distinct usages can be identified:

- Use 1: discourse equals communication via words ('causality involve's people's perceptions, understandings, motivations and communication. All involve *words*. All involve *discourse*' (p.265).)
- Use 2: discourse is the specifics of talk ('the sociological literature which argues that *discourse*, the specifics of what people have to say about any matter, and *dialogue*, talking with people about what they have to say, are central to any research or investigative process (p.267).)
- Use 3: discourse is an environment of values 'the problem-solving environment is *value laden*. This is the realm social scientists call the discursive', p.266).
- Use 4: discourse is a causal component of injury ('the discursive component of injury causation', p.267); Critical epidemiology makes distinctions between various types of causation ... and between physical and discursive aspects of causation', p.269).
- Use 5: discourses make reality knowable/data are a metadiscourse ('The reality of accidents, health, disease or injury is not directly knowable. It is only knowable through discourses about them and through the critical analysis of these discourses. Data should not be taken at face value as expressing positively the 'reality' - they quantify a metadiscourse about it', p.272).

Now it is quite possible that as a theoretical concept, 'discourse' could be defined to include all of the above

elements, but then the problem is that empirically it really does become very troublesome - a catchall-phrase that covers everything and nothing at the same time. Here, it must be stressed that Arnoux and Grace are arguing that epidemiologists who are analysing the social processes of injury and disease must incorporate the discursive, and can do this by taking on what social scientists know about this realm. But our fear is that if epidemiologists look to critical epidemiology for advice, they are likely to become lost and confused. As it stands, Arnoux and Grace's argument provides no uniform use of the term 'discourse', nor any real guidelines as to how one would go about analysing accidents, injury and disease in terms of any of the versions of 'discourse' provided above.

The main trouble here is that in suggesting a move from physical to critical epidemiology, Arnoux and Grace have bought some unnecessary baggage: the rationalist focus on causation. If one wishes to focus on discourse, it is just not necessary to be limited by a concern for causation; and further, the term 'discourse' can be replaced by much more specific terms which have the effect of considerably tightening up social analysis of accidents, injury and disease. One major framework we favour is a focus on 'sociologics'. Below, we briefly describe this framework, then follow with some research examples, and conclude with a case study of a major New Zealand accident.

From Discourse To Sociologics

While the use of the term 'sociologics' is not uncommon and its lineage is unsure, our usage of the term is stimulated in large part by the seemingly unconnected work of Bruno Latour and Harvey Sacks. Latour's main field of study is the sociology of science, which to us seems particularly relevant to the topic of linking sociology and epidemiology.

This is because when sociologists study science a contrast is very often made between the lay world of beliefs, and the scientific world of knowledge. On one hand, science claims to produce objective knowledge, while on the other hand, sociologists studying science (generally) take a relativist position and argue that science is imbricated with beliefs (just like the lay world). Substitute epidemiology for science and we see the relevance for our current discussion. Latour's forceful argument (see Latour, 1987) is that while the relativist argument is a strong one, sociologists must go beyond just showing clashes of beliefs. But how is this to be done? Simply by realising that science consists of chains of associations and dissociations Thus studying science involves looking for the strengths and lengths of linkages, and the nature of obstacles to them. By doing this, the knowledge/belief binary is dispensed with as:

> Each of these chains [scientific/lay] is *logical*, that is, it goes from one point to the other, but some chains do not associate as many elements or do not lead to the same displacements. In effect, we have moved from questions about *logic* (is it a straight or distorted path?) to **sociologics** (is it a weaker or stronger association?) (Latour, 1987: 201-202).

While this is an extreme gloss of Latour's argument, the point we are making from it is simple: critical epidemiology should take an approach where none of the people involved in accounting for accidents and injuries are treated as ever thinking either logically or illogically, but simply sociologically. That is, accidents are accounted for by making associations and dissociations; accounts are done together using socially situated practical reasoning. Building upon this, Harvey Sacks' contribution to our proposal can now be noted, as his work, or developments of it, are crucial

for approaching accounting work and practical sociological reasoning.

Harvey Sacks founded Conversation Analysis, which has now developed into an international research programme.[4] Amongst the work done by conversation analysts are some studies very relevant to accidents, in particular a number of studies on emergency calls for help. These studies make the simple point that the activity of calling for and receiving help is largely carried out through talk, which is itself a socially organised structure. But like other social structures, talk is characterised by multiplicity and variability, a point that has potentially important consequences for it suggests that the simple task of describing an accident is 'vulnerable':

> Persons describe events and activities to one another all the time. Among such descriptions are those about which something might or must be done. The work of description can thus be integral to another task, namely complaining. If this complaining ought to summon forth a remedy, or alternatively, some counter to the complaint, either of these might involve questioning the depicted problem itself. In regard to the latter, it may be observed that both the nature of the problem and the speed with which the remedy is offered are often conditional on the outcome of such questioning, that is, on the surviving shape of the complained about matter.

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4. Sacks was killed in an automobile accident in 1975 and a great deal of his most influential work was published posthumously, including the recently released *Lectures on Conversation* (Sacks 1992). This publication contains an introduction by Emanuel Schegloff which provides an excellent overview of Sacks' insights, some of which were taken up by later conversation analysts.

> ... Along with the indication of 'on whose behalf' the description *cum* complaint is delivered, the display of the see-ability and hear-ability of the trouble is what we have termed the caller's stance, and how it is managed in the call can increase or reduce the vulnerability of description (Whalen and Zimmerman, 1990:484-485).

The fact that sociologics enables a 'complaint', 'hoax', or 'real' emergency call to be distinguished is highlighted in one particularly telling case study. Whalen, Zimmerman and Whalen (1988) show how a call to a fire department for an ambulance went wrong in a systematic and orderly way with fatal consequences. The call was switched to a nurse who rigidly followed the department's protocol for assessing the worthiness of the call, the 'caller's stance' was questioned, the caller became irate with an argument developing, and subsequently a woman died while waiting for an ambulance. The point to be taken is that at a basic level, conversations consist of chains of associations - they are essentially sequential networks - and it is by these chains that descriptions of accidents and what ought to be done about them get accomplished.

Now, in contrast to sociological research on accidents that attempts to find the privileged speakers on accidents, a view based on sociologics is not so much interested in the content of what is said, but how people involved in accidents can get others to **feel as they do** - how do they make dissenters feel lonely (Latour, 1987: 59). This is not an easy task as is highlighted in Harvey Sacks' classic paper 'On Doing 'Being Ordinary' (Sacks, 1984). Sacks uses the notion of 'entitlement' to analyse a conversation where one person reports seeing the end results of a car accident - a 'most gosh awful **wreck**' (Sacks, 1984: 424) -

while stuck in a traffic jam. He suggests that the person giving an account of the wreck has become entitled to an experience, quite a different entitlement to anyone else hearing the story. Along with the rights to tell the story, go the rights to have a feeling about it - you may feel awful, cry, feel faint the rest of the day, and so on. This may seem perfectly trivial to many, but Sacks makes a very powerful sociological observation from it:

> Now one reason I raise this whole business, ... is that we could at least imagine a society in which those having experienced something, having seen and felt for it, could preserve not merely the knowledge of it, but the feelings for it, by telling others. ...
>
> That is to say, if I tell you something that you come to think is so, you are entitled to have it. ... But the stock of experiences is an altogether differently constructed thing. ...
>
> Now that fact obviously matters a good deal, in all sorts of ways. Among the ways it comes to matter is that if having an experience is a basis for being aroused to do something about the sorts of things it is an instance of; for example, the state of freeways, the state of automobiles [the cause of accidents] ... then plainly the basis for getting things done is radically weakened where those who receive your story cannot feel as you are entitled to feel (Sacks, 1984:425-426).

One very common technique for mitigating this problem of getting others to feel as we do, is by the use of what Latour calls 'black boxes'. Latour borrows this term from cybemeticians, who when confronted by a piece of machinery or a set of commands too complex, simply draw black boxes around them - they then need know nothing

more than inputs and outputs as the content no longer needs to be reconsidered (Latour, 1987:2-3).[5] The point is that individual actors or social groups grow stronger by stacking more and more black boxes, with the result that anyone 'obstinate enough to dissent, will be confronted with facts so old and so unanimously accepted that in order to go on doubting he or she will be *left alone* (Latour, 1987: 59). However, it is important to note that black boxes never remain fully closed or properly fastened - they can leak and they can be opened. Thus, getting others to feel as we do about accidents can be seen as a dynamic social process involving attempts to stack black boxes (well known facts/what everyone knows), while at the same time attempting to open other black boxes stacked from opposing viewpoints.

So far, we have noted the relevance of conversation analysis and a Latourian network analysis to investigating the sociologics of accidents. It should also be noted that while we have some misgivings about Amoux and Grace's talk of a 'discursive aspect of causation', we are not against discourse analysis per se. That is, where discourse analysis is taken to mean the analytic focus on locating particular utterances/sentences within overall sequences of utterances/sentences, and in showing the uses to which language can be put (Silverman, 1993: 120-124).[6] But

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5. Note that what is put into black boxes can be both social - ways of human association - and technical - associations of materials. See Callon and Latour (1981) for an earlier and detailed formulation of the black box concept.

6. Latour's work clearly displays many of the features of discourse analysis, being concerned for example with how scientific papers utilise rhetorical forms to convince readers.

what has not been noted so far is the relevance of ethnographic approaches to the sociologics of accidents, and to show that such an approach is important. Below we briefly mention the work of Baccus.

Baccus (1986) studied attempts to regulate 'multipiece truck wheel accidents'. These are not accidents that occur to people while driving trucks, but accidents that occur to mechanics in the process of truck wheel changing: the accident potential of multipiece truck wheels occurs in the process of re-assembly and inflation, where if the parts are incorrectly seated, they can literally explode causing death or serious injury. Because of this potential, a manufacturer's recommendation is 'NEVER [red caps] hit tire or rim with hammer [black small case letters]' (Baccus, 1986:47). However, ethnographic analysis led Baccus to believe that such a dangerous procedure had a common and compelling situated logic:

> The overriding concern of the workman *throughout* the airing process is the seating of the ring and he 'keeps in touch' with that seating process by contact with the ring(s) using a long handled tool, with either a hammer or a tool with a head which can serve as a hammer. The design features invite the knocking down of the lock ring to keep it under the rim gutter and a hammer device is chosen to accomplish this. That this activity continues over the course of the airing implies that this piece of embodied logic, the very thing that should *not* be going on, is, nevertheless, the very thing that is so compelling. And compelling in a 'logical' sense, given the understanding of how situated logics work. (Baccus, 1986: 48, original emphasis)

Thus, Baccus emphasised that the design features of multipiece truck wheels combined with mechanics' situated logics resulted in demonstrably dangerous workplace practices. Very simply, this study exemplifies how ethnographic investigations of local sociologics can make important critical contributions to the epidemiological endeavour.[7]

III. Case Study: the ICI Fire and Related Events

Attention is now turned to a more extended discussion of a major New Zealand accident - the 1984 ICI fire in Auckland.[8] We will firstly describe the ICI fire and events surrounding it, and then formulate our analysis as a proposal for critical epidemiology.

7. At this point, it should be noted that we have made no attempt to review the sociological literature on accidents and injuries. Our feeling is that this would unnecessarily complicate the form of this paper. While Arnoux and Grace are probably right when they claim that there is not a large sociological literature explicitly on accidents and injury, obviously other relevant material can be discussed. Some examples include: Douglas (1986), and Beck (1992) on risk; Foucault-inspired studies of governmentality by Hacking, Ewald, Defert and Castel (see Burchell, Gordon and Miller, 1991); the political economy approach in medical sociology (eg. Bammer and Martin, 1992; Willis, 1994); and studies of the transition from accident to emergency medical treatment (eg. Hughes, 1980; 1988). And, of course, more directly there is Dwyer's (1991; 1992) work which does feature New Zealand material. However, with our focus on discussing Arnoux and Grace and offering our own new proposal for critical epidemiology there simply is not space for a more extended literature review.

8. This discussion is an abbreviated and reshaped version of a lengthier unpublished paper by Dew (1993).

On 21 December 1984, there was a large fire at the ICI Riverview warehouse in Mt Wellington, Auckland. The New Zealand Fire Service extinguished the fire with a total of 340 firefighters being involved, either in the actual firefighting or in the subsequent clean-up of the site and firefighting equipment (Elias et al., 1990:1). One ICI employee (a cleaner) was severely burnt while attempting to put out the fire with an extinguisher - he died three months later in hospital. While thirtyfour firefighters sustained immediate injuries from the fire in the form of skin rashes, the number of ill-effects from the fire grew with time: by 1985, 60 personnel had been examined for various ailments, predominantly skin rashes and nausea; and by 1990, 214 firefighters reported health problems related to the ICI fire. Further, six firefighters were retired on medical grounds due to illness attributed to attendance at the ICI fire, and in December 1988, one firefighter died of leukaemia (Elias et al., 1990:2).

As is often the case with major accidents, the ICI fire was investigated by various bodies who produced several reports. In the process of report-compilation a complex set of interactions was set into play: the Firefighter's Union, the Fire Service Commission, the Department of Health, the Accident Compensation Corporation, the media, the medical profession and unorthodox medical practice were all involved in debating what ill-effects the firefighters actually suffered, and what caused these ill-effects. In short, we have a controversy.

Dispute Over Ill-Effects and Causes

In agreement with Amoux and Grace, the situation is one of competing discourses about the accident, its cause and consequences. However, in contrast to Amoux and Grace, we do not believe that by somehow collecting together

these different discourses and submitting them to an 'hermeneutic dialectical' analysis we can arrive at a more comprehensive understanding of the situation. What we propose is that two crucial rules of method be utilised here.[9] First, since the settlement of a controversy produces facts (knowledge about Nature), we do not use facts to explain how and why a controversy has been settled. Second, since the settlement of a controversy produces stability in Society, we cannot use Society to explain how and why a controversy has been settled. Thus, on one hand, we make no appeal to factual knowledge about chemicals and their combustion, nor to known evaluations of types of medical treatment in our analysis of the controversy. In this way, the task is to show how the various parties go about putting things in black boxes and building their constructions more and more solidly - it is only after the controversy has been settled that Nature lies behind the facts; it is never behind facts in the making. On the other hand, the second rule suggests that we avoid appealing to any gross structural features of society to explain the settlement of the ICI fire controversy. A clear candidate here would be the suggestion that as the controversy is situated within a Capitalist Society, analysis should focus on class-based inequalities: the controversy pits workers and firemen against a multinational company and the 'ideological state apparatus' of medicine (Waitzkin, 1979).

Grounded in these two points, we are then relatively free to pursue the parties to the controversy as they go about working out what happened and why, building alliances with others, putting things in black boxes, and attempting to take things out of other black boxes. We will see that there are situations where there is no agreement on what can be

9. These are lifted and slightly reworded from Latour (1987).

called suitable evidence in the ICI fire controversy: what is suitable for one institution is not for another, what is beyond doubt for one is not for another. Further, the events of the ICI fire show how different individuals positioned within institutions have different ideas of cause, and hence there is great difficulty in getting beyond competing interpretations to some definitive and comprehensive causal explanation: all explanations are embedded within socially provided and sustained vocabularies, hence providing any attempt at an overall explanation with a problem of incommensurable viewpoints. We do not have the space for a full-fledged analysis of this aspect of the ICI fire, but the following points illustrate how both the causation and characterisation of injury were disputed in the ICI fire.

Firstly, there were conflicting reports about knowledge and equipment use. The Fire Service claimed that all personnel involved in fighting the fire 'wore breathing apparatus at all times when fighting the fire' (NZ Fire Service Commission, 1985:7). This claim was contradicted by another report stating that few of the firefighters wore protective breathing apparatus throughout their exposure (Glass, 1985:25). In terms of knowledge, ICI claimed that verbal information about the chemicals on the site was given at the time of the fire. The Elias Commission's final report to the Minister of Health concurs with this position concluding that at all times knowledge of the chemicals involved was 'available to the fire service...for the purpose of effective firefighting, appropriate safety management and decontamination' (Elias et al., 1990:56). The Firefighter's Union denied this and claimed that a full list of chemicals was not available to the officers fighting the fire (Summers, 1990:7), and consequently the procedures followed could not be based on the knowledge of what the firefighters were dealing with, as claimed by the Elias Commission.

A second point of dispute developed over whether another threat from the fire was dioxin produced from 2-4-5T combustion. The Fire Service was advised by ICI after the fire that burning 2-4-5T does not produce dioxin (NZ Fire Service Commission, 1985:35) - a claim directly contradicted in the Glass (1985:9) report. This matter is also complicated by the fact that dioxin may arise in the combustion of chlorine (Commission for the Environment, 1985) and chlorine was stored in the ICI warehouse. Exposure to dioxin can cause a skin condition known as chloracne, severe cases of which can last for 15 years. Dr Grieg, a dermatologist who saw the firefighters, diagnosed the skin condition as folliculitis and not chloracne. Consequently, it was assumed that dioxin was not absorbed by the firefighters. After the fire, there were fears about the effects of dioxin, but a diagnosis of folliculitis, as opposed to chloracne, supported the contention that the firefighters were not suffering from dioxin poisoning. If Dr Grieg had made an initial diagnosis of chloracne, different alliances could have been forged and the overall outcome may well have been different. Clearly, this highlights the importance of diagnosis, which as the sociology of medicine has shown is a highly variable socially constituted process (eg. see Freund & McGuire, 1991:ch. 9 & 10).

Also complicating matters, was the important factor of time-elapse. A report (Commission for the Environment, 1985) incorporating the DSIR analysis of swab samples taken from buildings in the path of the smoke, showed no evidence of 2-4-5T or dioxin. Disputing this, the Firefighter's Union claimed that the DSIR dioxin tests, which were taken over 24 hours after the fire, were useless as dioxin is broken down and disappears within 24 hours after soil contact (Summers, 1990:18). However, the Elias Commission ignored this and accepted that the DSIR

findings indicated no evidence that firefighters were exposed to dioxin (Elias et al., 1990:14). There were some tests carried out in Sweden of tissue analysis for chemical poisoning, but due to machinery failure, a test for TCDD (dioxin) was not carried out (Elias et al., 1990:14). We see here how questionable assumptions in one report (from the DSIR) become support for another later report (the Elias Commission) with the original assumptions being glossed over - clearly, black boxes have been stacked to strengthen a particular construction.

This dispute over the possible presence of dioxin, and over the diagnosis of dioxin poisoning, became an important element in the eventual dismissal of the firefighters' claims that the problems they suffered resulted from chemical contamination. The difficulty of making any kind of definitive diagnosis by referring to the known chemicals at the fire appears to have been ignored by the Elias Commission report, which dismissed chemical intoxication as a source of the firefighters' troubles and opted in favour of a psychological explanation. In its investigation, the Elias Commission appointed a psychiatric specialist and ultimately concluded that although the firefighters definitely faced exposure to toxins at the time of the fire, the symptoms suffered by the firefighters were due to stress and anxiety (Elias et al., 1990:9).

This psychological explanation was further bolstered by disputes about the involvement of an unconventional medical specialist - Dr Tizard - who treated many ICI firefighters for chemical poisoning. Dr Tizard's diagnostic method is known as electro-acupuncture by Voll (EAV), and his treatment methods consisted of vitamin C injections, homoeopathic preparations, and the use of hyperbaric oxygen. The Firefighter's Union had arranged appointments for many of its members interested in Dr

Tizard's treatment, and the Accident Compensation Corporation was contacted and agreed to the fees for diagnosis and treatment (Elias et al., 1990:68). About the same time, a task force was set up by the Department of Health to investigate prior claims by Dr Tizard about the prevalence of chemical poisoning amongst his other patients. On this issue, the task force concluded that they could not support his claims, but interestingly, this separate report was ultimately used by the ACC as a basis for refusing to pay for any firefighters to be treated by Dr Tizard. The New Zealand Professional Firefighter's Union was upset about this move, responding that the task force had at no time shown 'even the slightest interest in those individuals' treated by Dr Tizard (Summers, 1986:43). The Union conducted its own survey and found that firefighters reported greater satisfaction and better results from Dr Tizard's treatments than they did from conventional methods. Ultimately, the Firefighter's Union stated that it had 'absolutely no faith left in the Department of Health and submits that the Task Force report is of no value' (Summers, 1986:45).

It should be noted that in contrast to the Firefighters' conclusion, the far more influential Elias Commission report assumed the value of the Task Force findings in its report, parroting its opinion of Dr Tizard and his methods, and concluded that the ill-health conditions of the ICI firefighters were due to stress and anxiety, and not to chemical poisoning. In addition, in its report, the Elias Commission extensively quoted statements made by some firefighters about the anxiety they had felt since Dr Tizard's diagnosis of chemical poisoning was made (Elias et al., 1990:21), the implication being that the stress and anxiety that the Commission found was at least in part due to Dr Tizard's involvement.

The controversy ultimately came to revolve around the problems of chemical pollutants and the way in which medical science can be employed to determine satisfactory outcomes for major institutions. Matters of disputation in the ICI events include the determination of the amount and type of exposure, the conflicting scientific evidence concerning the effects of exposure on humans, and in some of the disorders, the time lag between exposure and consequence. In the case of the firefighters, the poor recognition of their claims should not be viewed as due to a **lack** of scientific support: scientific claims are always contestable, and as we have stressed, it is more useful to argue that the facts are arrived at **after** the settlement of the controversy. What seems to have hampered the firefighters was a complicated and unintended outcome of their attempts at alliance building. They did believe that their ill-effects were due to chemicals, but equally they also believed that Dr Tizard's unconventional treatment was efficacious; unfortunately for the firefighters, this turned out to be an easily opened black box with the ultimate outcome being the suggestion that Dr Tizard's treatment was itself only a part of the real cause of the firefighters' problems - stress.

Further, the association with Dr Tizard drew the firefighters into a another overlapping controversy - the extent of chemical poisoning in New Zealand - which was a construction facing an extremely well established alliance built upon many black boxes. That is, the medical establishment who, allied with the Department of Health, were able to undermine the position of one of their members who was not only using unorthodox therapies, but loudly proclaiming that there was a great undiagnosed health problem in New Zealand that conventional therapy was unable to remedy. The diagnosis of stress vindicated the position taken by the Task Force and the Department of

Health in relation to Dr Tizard. If the Elias Commission had pronounced a chemical connection with the firefighters' symptoms, then the Task Force's findings would have been called into question. Further, the ACC also benefitted from the findings of the Task Force and of the Elias Commission as they did not have to pay out for symptoms caused by stress. The ACC would have had to pay if chemical poisoning was given as the cause for the symptoms, and if this had been the conclusion, then a precedent would have been established for future compensation claims for chemical poisoning.

Psychologists and psychiatrists also benefited from these events as, to the relief of established bureaucracies, they extended their domain of activity. The multinational chemical company, ICI, was absolved from all responsibility and blame, even though at least one death could be attributed to their negligence, and the firefighters' problems were a consequence of putting out the ICI fire. Indeed, the main losers were the firefighters who were deprived of compensation for Dr Tizard's treatment, which they found practically effective despite claims to the contrary. Additionally, after the controversy, they found themselves increasingly subjected to medical surveillance, particularly an expansion of medical evaluation to include stress and psychological factors. This could have led to firefighters losing employment, or at least feeling threatened in their work. In short, debates about, and through, sociologics have very real consequences.

Conclusion

While this paper has covered a large amount of material, it can be briefly concluded. We have described Amoux and Grace's critique of physical epidemiology and their favoured

move to critical epidemiology, and while we are in agreement that there is a need for social science input into the study of accidents and injury, Arnoux and Grace's proposals were found lacking in substance.

To summarise our objections, it is useful to return once more to the final statement from Arnoux and Grace:

> The ultimate objective then is not only to find physical points to intervene and break causal processes or to place a protective barrier, but more centrally and importantly to transform or replace the processes leading to injury or disease in the first place. This critical approach provides a firm basis for the development of prevention work because it is grounded in the experience of the people directly concerned with the problem (Arnoux and Grace, 1994: 273)

It is indeed a laudable and sensible aim to 'transform or replace the processes leading to injury or disease in the first place', and our discussion above is perfectly consistent with this position. However, the whole emphasis of Arnoux and Grace on collecting different discourses of those 'directly concerned' and submitting them to 'hermeneutic dialectical' analysis, we find very doubtful. There are good reasons for arguing that direct groundings in authentic experience do not exist; this does not have to be some kind of postmodernist nihilism; rather, there is simply a strong case for seeing accounts of accidents and injury as 'always and already social', and as we have suggested, a matter of sociologics. And, from our brief discussion of Sacks' work and its development in conversation analysis, we can also argue that the 'people directly concerned with the problem' are themselves actively engaged in working out what

happened.[10] This is what needs to become a topic of investigation: not the way that different accounts can be added together to 'adequate the real' (McHoul, 1982), but how people put together acceptable versions of events. Thus, we feel that any attempt to 'add up' discourses to gain a more comprehensive viewpoint of **causation** is doomed to failure. Paradoxically, Amoux and Grace's critique of the positivism in physical epidemiology seems to have led them into another 'view from nowhere' (Nagel,1986), this time one based supposedly on discursive/qualitative methodology.

In contrast, through adopting Latour's rules of method, we argued that inquiry in this area can be reformulated to ask: 'How do participants to accidents, in concert with others, go about accounting for accidents, and in that process what associations, dissociations and alliances do they form?'. Basically, that is our proposal for critical epidemiology. A crucial thing to note about it is that

> The main difficulty in mapping the system of heterogenous associations is in **not** making any additional assumption about how real they are. This does not mean that they are fictitious but simply that they resist certain trials - and that other trials could break them apart (Latour, 1987: 205).

Whether or not epidemiology can live with (or even next door to) such a view is a moot point, as it clearly poses difficulties for any commitment to causal explanation. Nevertheless, at the very least, our proposal is sociological,

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10. For a more detailed theoretical discussion of this point, see Sharrock and Anderson (1982).

it can be critical, and we believe it can contribute to the epidemiological endeavour.

To conclude, it should be noted that while we have criticised Arnoux and Grace for failing to make the methods of critical epidemiology clear, our own proposal for critical epidemiology contains **nothing** new in terms of methods. In our view, existing methods are perfectly adequate for building critical epidemiology, and above, we have noted the utility of at least, conversation analysis, discourse analysis, ethnography, and case studies. What conclusions are derived from these methods is obviously an open matter, but we hope that our outline of the utility of investigating sociologics provides a convincing option for the future pursuit of critical epidemiology.

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REVIEWS

Peggy Koopman-Boyden (ed), **New Zealand's Ageing Society: The Implications**, Daphne Brassell Associates Press, Wellington, 1993.

Reviewed by Paul Perry
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The ageing of society is a reality that New Zealand has been struggling to come to terms with, at best with limited success. The difficulties of today will only intensify as the baby boom generation enters old age during the first two decades of the next century. Peggy Koopman-Boyden suggests that the end of the twentieth century may be a transitional period where the focus on youth shifts to the concerns of older people.

This 250 page book of readings has the goal of providing the first multidisciplinary view of New Zealand's elderly. It approaches this goal through 10 essays by experts from the fields of medicine, politics, Maori studies, sociology, geography, and economics. The three parts of the book are concerned with putting ageing in context, the implications for policy, and a view of the elderly themselves. Drawing on such a variety of expertise inevitably makes it difficult to achieve a full sense of coherence with respect to theoretical concerns or style, but it does offer the advantage of an informed overview in many areas of interest.

In the first chapter, Koopman-Boyden briefly reviews many theories of ageing, from biology, psychology, anthropology, and sociology. There is little direct linkage to New Zealand, and by definition, there is little room for depth, but this is a very useful guide for the uninitiated, or those with knowledge in only one discipline. One of her important conclusions is that age has yet to be deemed a significant factor in explaining social behaviour, but it most certainly deserves to stand alongside the likes of gender, ethnicity and social class.

A wealth of demographic description is found in the chapter by Brian Heenan. The facts never stop coming. I was particularly struck by the uneven geographical distribution of the elderly, as well as the fact that only nine percent of those 65 years or more do **not** live in a private dwelling. This chapter is a very valuable resource, but at times I felt a desire for a little more analysis and interpretation.

John Campbell's medical overview of the changes in physiology and health status of the elderly is another valuable resource. Trends suggest that the elderly can expect more years free from disease, but also a longer period of physical dependency at the end. A theme found here, and repeated throughout the volume, is one of inadequate resources and the need to plan for the increased demands on health and welfare services.

How the interests of the elderly have become strongly tied to the state is examined by sociologist Kay Saville-Smith. The historical development of that linkage is not always easy to follow, but the conclusion finally brings an analytical overview with a clearer sense of the trends,

including increased state involvement, increased state encouragement of the voluntary sector, and more recently, increased encouragement of private, profit-making care.

A richly detailed description and history of housing, housing policy, and its implications for the elderly, comes from David Thorns. I learned a great deal, but throughout I felt a desire for more analysis and interpretation, which finally came in the last four pages. Among the essential conclusions is the continuing need for state assistance for the elderly with housing. Market forces serve only the needs of the more affluent elderly, leaving the asset-poor in a poverty trap if the state is not there to assist.

The chapters by Susan St. John (an economist) and Terri Green, while very informative, were the most difficult ones for me, for rather different reasons. St. John provides a recent history of pension provisions, and systematically examines six possible responses to the economic problems of ageing. Elements of the discussion were a little hard to follow, given the language and assumptions about society that economists tend to use, although the basic message is clear. There is no easy answer, but things will be less bleak if the economy improves, and if a consensus can be reached on what society should provide for older people. Green focuses on assistance and care for the disabled elderly. There is much to be learned here, but in my view, the excessive detail and dated data (rarely later than the mid-1980s) detracts from the piece. To her credit, she does make some clear recommendations about how to cope with what will be an alarmingly great increase in the numbers of disabled elderly.

Ruth Bonita looks at older women and skilfully blends a great many descriptive facts with a good critical interpretation. The interface of ageism and sexism, demographics and their consequences, reasons for living longer, caregiving, income, health, the health system, and quality of life are all examined. She sees the future as difficult to predict. The number and proportion of older women will be increasing, especially at older ages, but this must be balanced against a cohort that is better educated, more assertive, and wanting a greater say in society.

Opening the subject of the Maori aged to debate is the contribution of Roger Maaka. Discussion is centred on the roles and status of kaumatua, rather than other aspects of Maori ageing that are so often hidden within the picture of mainstream society. Maaka tells us how, unlike the Pakeha elderly, many kaumatua become far busier than they have ever been before in their lives. We also learn that government has a fundamental misunderstanding of kaumatua leadership, which in fact has very little executive power.

In the final chapter, Stephen Levine and Nigel Roberts provide an interpretative look at politics and the elderly, largely since the 1984 election. Among the evident trends is a growing activism by the elderly, while major politicians have become younger, and a tendency for superannuation (seen as a `right', not a welfare benefit) to emerge as an important political issue whenever it appears to be threatened. A suggestion for the future urges the elderly to find a way to align their goals with other major sectors of society.

Overall, this book succeeds as a broad multidisciplinary look at ageing in New Zealand. A wide variety of topics are covered by highly knowledgeable people. There is much to be learned from each chapter. Nevertheless, as I read through many (but not all) of the chapters, I felt there was room for a fuller discussion in certain ways. It would be good if there was **more** critical interpretation of the rich base of facts and description, tied in to more consideration of likely needs and developments in the future, and what this means in the current ideological/political context of New Zealand. Indeed, most of the authors seemed to be trying to maintain a high degree of value neutrality, in a realm that really cannot help but be rather ideological and highly politicised.

I hesitate to raise a number of small matters, but a book that breaks new ground and covers a wide spectrum would benefit from an index. Many of the chapters also rely on data that goes no further than 1986. I fully recognise the problems of getting up to date information, but such data makes a book published in 1993 seem fairly dated by 1995.

This is a book that is largely accessible to the general reader, while bringing together a great deal of descriptive information that is useful to the academic or social policy specialist. For a course on ageing in New Zealand, it would be a natural choice.

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Patrick Day, **The Radio Years: A History of Broadcasting in New Zealand (Volume 1)**, Auckland, Auckland University Press, 1994.

Reviewed by Brennon Wood
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The historical sweep of *Radio Years*, ranging from the 1890s to the 1960s, brings firmly into view the technological and institutional arrangements too often neglected by the myopic presentism of much textual analysis. To be sure, broadcasting is a field of rapid and often bewildering change, its products as ubiquitous as they are fleeting. But that means we must develop our historical imaginations. All the more so given that these are in important ways *still* radio years. Without some understanding of the past, how can we make sense of the most recent legislation, which simultaneously sanctions selling off the state's commercial wing and establishes public radio as an independent, chartered organisation (Maharey, 1995)? While `old-fashioned' radio has often been ignored in media studies, there are signs of a growing interest in the medium (cf. Wilson, 1994). Day's book is undoubtedly the most significant contribution to date.

Who can question the value of stories about ancestors? Picture Shelley the Controller storming the studio and smashing a Vera Lynn record over his knee. Or Prime Minister Savage in his shirt-sleeves, lying on the floor listening to the wrestling. Day's history offers intriguing and unanswered questions. Why was it, for example, that controversial discussions developed further in

`women's sessions' than in the officially sanctioned talk shows? Day sharpens our sense of the contemporary by highlighting differences with days gone by. Listening to radio used to be a subtle art - synchronise three knobs, vary the brightness of filaments, tune two coils, adjust two condensors and, if that wasn't enough, dress up in a headpiece fitted to a horn. How curious were the 1930s, when concern that `excessive broadcasting' would drive record sales down meant new songs could be played only once in the first week of release, and no more than twice a week for the following six months.

The shock of the old lies just as much in continuities with the present. How interesting that prior to state monopolisation in the 1930s, the national service was a private company under contract to the government. `Shortland Street' first entered our broadcasting vocabulary in 1935. The artifices of `actuality' have also been surprisingly long-standing. 2YA's `live' report of the first trans-Tasman fight in 1928 was delivered from the studio, the announcer's vivid make-believe descriptions reinforced by a special-effects record. And note the longevity of issues dominating the broadcasting debate today. Arguments over `popularity versus quality' and the importing of foreign programmes got underway in the 1920s and 1930s. These years also saw the beginning of payments to rugby unions for the coverage of matches.

The Radio Years offers many such absorbing by-ways to students of broadcasting. Of course, a general text cannot satisfy specialist interests. Then again, that can be as much a value as a weakness. The broad historical sweep rightly poses the problem of a synthetic

interpretation. Ultimately, just what did the radio years amount to? In a nutshell, from the 1890s to the 1960s, public and commercial broadcasting developed as a department of the state. Day's book clearly reveals the character and shortcomings of this institutional form. From very early on, `the official view' was that radio should be organised nationally. We should not forget the ambitiousness of this task. As a radio audience, the South Island's west coast was part of Australia until the 1930s (p.133). Equally, we should remember how this task was carried out.

In practice, developing radio as a medium of national cohesion meant using it `to unite the country in an understanding and acceptance of its government's policies and actions' (p.314). Throughout the radio years, broadcasting was subordinated to governments of the day. While the state promoted universal reception, it enforced `a deliberate restriction of access to radio transmitters' (p.247). This allowed for detailed and extensive personal domination by Ministers, and especially by Prime Ministers. Opposition politicians were routinely denied the microphones held by government power. Their silence, along with the ever-renewed ban on `controversial' material, meant that broadcasting emerged with no significant news and current affairs programming. Labour's first Prime Minister `never relinquished his personal control of the radio services' (p.271). The Official News Service he established in 1937 was a powerless drone. For the next 25 years, bulletins were prepared either in the Prime Minister's Office or, under National, in the Tourism and Publicity Department. This refusal to countenance `any change which might threaten the practice of using radio as a

presenter and supporter of government policy' was one of the factors which stalled the arrival of television, first discussed as early as 1928 (p.313).

Such were the limits on broadcasting as a site of nationhood. The state's non-commercial radio cultivated a national identity defined in `high culture' terms, preoccupied with classics and devoid of independent political commentary (p.246). The YA announcers spoke proper; they wore dinner jackets and evening gowns. In contrast, the state's commercial stations were smaller, their programmes combining a local orientation with international popular music. They were always favoured by the majority of listeners (p.319). Savage's heart wasn't with the highbrows either. He too endorsed that `populist combination of commercial and state-controlled broadcasting that was distinctive of New Zealand radio' (p.278). This `populist' tradition of personal domination denied any need for either professional journalism or diversity of viewpoints. In 1949, the principal of Victoria University College, disturbed by the banning of a school broadcast on evolution, described radio as having a `totalitarian character' (p.292). Day similarly concludes that `the decision, upheld by successive governments, not to allow New Zealand broadcasting the right to report and discuss freely, emasculated the medium for the entire period covered by this volume' (p.319).

The legacy of the First Labour Government, picked up and extended by National, severely restricted the democratic integrity of broadcasting. Radio's non-commercial public was politically uninformed and detached from the majority of the population. This familiar Pakeha identity hegemonised a national terrain

weakened by the devaluing of popular culture. It is hard not to read the current situation as an outcome of this background. The radio years prepared for, and so could but feebly resist, the privatised, depoliticised national identity so vigorously promoted by contemporary media. Post-1960s broadcasting is certainly different. Day's book, however, suggests it has not changed beyond recognition. The history sheds light on a number of current issues. How else, for example, would it be possible for TVNZ to commission accountants to recommend, `ignoring the social and political considerations', further cuts to its already skeletal news service (Drinnan, 1995)?

This book has its shortcomings. At the outset, Day claims this is `a history not just of the growth of broadcasting but of the changing nature of the society that adopted the medium' (p.5). The latter theme comes through only indirectly. This is very much an official chronicle, replete with descriptions of technology and élite personalities. It is perhaps surprising to find a sociologist practising such traditional historiography. That said, those whose specialist and totalising concerns run deeper and wider will find *The Radio Years* indispensable. The book is sure to become the standard reference for many years to come.

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Robyn Munford and Mary Nash (eds), **Social Work in Action**, Dunmore Press, Palmerston North, 1994.

Reviewed by
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He iti he pounamu
It is small but precious

A paucity of indigenous material on social and community work practice has been addressed with the arrival of this book. It allows a range of social service practitioners and theorists an opportunity to articulate their theoretical frameworks in various fields and models of practice in Aotearoa/New Zealand social and community work.

This book, from editors Robyn Munford and Mary Nash, follows their contribution in conjunction with Celia Briar to an analysis of social policy from womens' perspectives in *Superwoman Where Are You?* (1992). This was achieved by allowing a number of authors to address a

range of issues - a smorgasbord of ideas prompting thoughts about less mainstream views in the area of social policy. This latest book adopts the same approach with respect to social and community work practice.

The book is structured into four main sections; (i) setting the scene, (ii) cultural insights, (iii) selection of approaches including radical, Christian, psychodynamic and feminist approaches and (iv) critical perspectives and reflections. The selection of the authors reflects what the editors consider 'exciting, innovative and worthwhile in this country at the present time'.

The rationale for providing this smorgasbord of approaches is described as follows: 'There has to be different ways of working with people, for some methods will suit one group, while other ways of doing things will be more effective in some cases' (p.17).

Responsibilities to Maori as tangata whenua is reiterated by various authors throughout the book and space is provided to articulate and legitimise indigenous ways of working. We also hear the voices of Tangata Ata Motu (namely Samoan and Tongan) articulating principles and approaches to social and community work which can only enhance our understanding of our professional obligations as practitioners in social work and community work in this country.

This book is clearly designed as a text for students of social and community work programmes with a number of study questions and reading lists at the end of most chapters to prompt further work and thought.

It begins with an interview between Mervyn Hancock and the editors. Merv has been instrumental in the development of social work as a profession within Aotearoa for many years and continues to argue the need for the retention of core social work values. Merv reminds us that as practitioners in social and community work, we are there to help people and that we must retain 'a level of optimism that social workers can continue to challenge and bring about change' (p.13).

The first three chapters provide a context for understanding how social and community work has developed as a profession within Aotearoa/New Zealand and identifies the significant shift in the focus of practice as a result of governmental policy changes, to the current emphasis on voluntary work and residual models of social work.

The New Zealand Association of Social Workers (NZASW) recently dedicated their January 1995 publication *Social Work Review* to Tangata Whenua perspectives of practice in a special edition called 'Te Komako' and it provides an excellent adjunct to the section in this book on 'Cultural Insights'.

The authors in this section favour a structural analysis approach which identifies the broader historical, socio-political and economic context of social ills experienced by those whom practitioners work with. However, case studies and life examples are used to highlight the principles of what is culturally appropriate. The fact that there are so few publications in this area make it a very valuable section and essential reading for those in training.

The ideas contained in the 'Just Therapy' approach argues for the expression of spirituality, addressing the 'cultural, gender and socio-economic context of therapy' (p.116) by working with people with 'problem-centred patterns' (pp.116-117) and weaving 'new threads of meaning and possibility that give new colour and textures' (p.117).

Leland Riwhiu's article links mythology with a Maori re-definition of social work practice, firmly placing the experience of Maori within a historical and political framework of the Treaty and the consequences for the way in which relations between Maori and Tauiwi are structured.

Rachael Selby writes of her experiences of as a way of highlighting the opportunities within whanau for meeting our needs as Maori. She recognises, however, the impact of dislocation on this whanau structure: 'For many of our whanau members urbanisation and the effects of colonisation have rocked the foundations for them and they have lost contact with the marae and the whanau' (p.148-149).

Lita Foliaki provides an intelligent critique of a contentious issue in the Pacific Island community - the punishment of children. She identifies the tensions inherent in being a Pacific Island social worker, namely as a Tongan, and well understands the cultural implications of her work as a statutory agent while personally committing herself to empowerment as articulated by Freire: 'One of the most important tasks of community work is the development of educational programmes which starts from the experiences of the

people and leads to an understanding of society and their particular position within it' (p.165).

A contribution by Ann Opie on assessing older clients and their caregivers, Sue Townshend on working with the deaf community and Raylee Kane's contribution on abortion counselling are all long overdue. There are also valuable contributions on foster care, working with people with disabilities and their families, precipitating disclosure in cases of child sexual abuse, working with families and models of practice which include feminist Christian, psychodynamic and radical approaches.

This is a section which is in danger of jumping around too much because of the vast array of organisational settings, client groups and approaches. However, each of the authors in this section have provided essential insights into their fields of practice and models of working.

There are common threads through all of the works such as the importance of understanding who you are and the values, beliefs and analysis which inform your practice and the link between the personal and political. Critical theories, including a structural analysis approach, are to be found throughout the work of the various authors. The interplay of practice and policy is best spelt out in Mike O'Brien's work on 'The Case of the Neglected Triplets: Policy, Class, Poverty'. He says: 'In creating and shaping the context it is social policy that forms the fundamental barriers surrounding the nature and form of practice ...I am wanting to emphasise the ways in which policy is embedded in practice, and practice is embedded in policy' (p.394).

Mike also summarises another theme common to all authors in this book - the contexualising of the experience of people with whom social and community workers work. 'Social work takes place within a particular historical, economic, political, ideological and cultural contexts' (p.394).

He goes on to argue that: 'It is social policy that provides the framework for the critical understanding of these contexts, the issues that arise from them, and their implications for practice' (p.394) in order to demonstrate his argument that policy and practice are inevitably inter-connected. He continues with another common theme 'Social work is informed by analysis. That analysis has to place the person/group/family/community/issue within its environmental location' (p.394), and goes on to argue something which I strongly believe - that '"environmental location" has to incorporate such dimensions as class, gender, ethnicity - the social institutions which daily affect people's lives. Social policy has a particular interest in and concern with those institutions as well as the wider social forces that create, maintain and change those institutions' (p.394).

The aim of the book is spelt out in the blurb on the back: 'to present a diverse range of contemporary social work practice' by having practitioners and educators make 'transparent the theoretical beliefs which inform their work' and illustrate this from practice. I think the book achieves this aim admirably. It contains a lot of information and ideas, all of which are long overdue in this form. I anticipate that this book will become essential reading for all students, practitioners, educators and policy makers in the social services. It

should also be useful to those generally interested in the social sciences, race relations and the role of professions in this society today.

Robyn and Mary have provided a forum which allows new voices to be heard in the literature on social and community work practice by allowing each author to tell their story. This is consistent with their work on 'A Feminist Contribution to Social Work' which emphasises the therapeutic value in the dynamic of listening and telling (p.238). Stafford describes this as: 'A story saves a life a little at a time by making us see and hear and taste our lives and dreams more deeply. A story does not rescue life a the end, heroically, but all along the road, continually. I do not make the story; the story makes me'.

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Anne Opie, **Beyond Good Intentions: Support Work with Older People,** Wellington, The Institute of Policy Studies, 1995.

Reviewed by
Cynthia Watkins, Department of Social Policy and Social Work, Massey University

Anne Opie has established a specialist niche with her research in the domain of caregiving of dementia sufferers. Her latest book *Beyond Good Intentions* explores this interest using a different perspective from her previous books, *Caring Alone and There's Nobody*

There. She examines the effectiveness of social work services for caregivers and people with dementia.

Caring for dementia sufferers is one of the most difficult areas of work undertaken and social workers require a wide range of skills when working in this field - individual counselling, bereavement counselling, family therapy, advocacy skills, group work and community development - as well as an extensive knowledge of available resources. Social workers are required to provide practical assistance and emotional support.

To investigate the competency of social work practice in this area, Anne Opie has based her observations on a qualitative research project involving four sites of social work practice with older people and has interviewed 130 workers in this area of health care - social workers, managers of health services, other health personnel and caregivers. The interviews provide insight into individual proficiency as well as an understanding of the context of structure, policy and practices which guide and constrain social work practice.

Anne Opie's research identifies considerable differences in the quality and professionalism of social work practice in this field with older, qualified and experienced social workers providing the most effective and higher quality of practice. However, Anne Opie recognises that the level of practice is defined not only by the competency of the individual practitioner but also by the status of the social work profession, the organisational structure, health policies, the target population and the level of resources.

This particular area of social work involves a socially devalued client group with limited power - this research contributes valuable feedback on the provision of support in the community related to a client group with limited means of commanding their own resources. Constructive suggestions for improvements in the quality of social work practice are recommended. Anne Opie identifies areas where social work practice with older people could be improved including, for example, the more specialised training programmes for this field of work, provision of supervision for social workers, quality assurance programmes to ensure consistent effective practice, and a recognition of the complex nature of this type of social work. Anne Opie also promotes a broader organisational effectiveness with a proposal for the development of a national dementia strategy, identifies the need for differentiation between support for acutely and chronically ill older people and their caregivers, and calls for the recognition of the social and economic contribution of caregivers.

The complete title, *Beyond Good Intentions: Support Work with Older People,* is rather misleading with the focus of the book on the support of caregivers and older people with dementia. Although the wider issues raised could equally apply to social work practice in other fields, the social work practice and client emphasis is related to cargiving and work with older people with dementia. There are many other aspects of social work practice in support work with older people and Anne Opie does not concentrate on these other components.

The fieldwork which provides the basis of the research was undertaken in 1992. The effects of the health

reforms since that date have highlighted many of the points raised in *Beyond Good Intentions*. Social work practice in medical and community settings now concentrates on faster discharges from hospital and maintenance in the community. The increasing emphasis on the practical rather than the psychological and emotional needs of older clients has influenced the role of the social worker and the focus is now on assessment and brokerage for other services.

Beyond Good Intentions provides useful information and is a reference not only for social workers but also for the policymakers who structure the shape the shape of social work practice relating to the community care of older people with dementia. Anne Opie has a way of presenting her findings in a clear and concise manner. Her use of direct quotations and the descriptions of actual situations personalises the research and the data becomes ultimately an interesting read.

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Carl Davidson and Marianne Bray, **Women and Part Time Work in New Zealand**, Christchurch, New Zealand Institute for Social Research and Development, 1994.

Reviewed by
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University of Auckland

Women and Part Time Work in New Zealand is a welcome, thoroughly researched, contribution to the study and discussions of work in New Zealand in the 1990s. Carl Davidson and Marianne Bray have undertaken a qualitative empirical research project on questions concerning part-time work and women at a time when the focus of many research agendas on work in New Zealand is dominated by matters relating to the Employment Contracts Act 1991, or the applications of various new technologies, both material and discursive (eg Information Technology, job redesign, Total Quality Management programmes, labour market reforms). It is refreshing to see some current core empirical work conducted on questions often forgotten since the 1970s and 1980s, when the conditions and experiences of women's work mattered somewhat more visibly to trade unionists and policy makers, and there was at least more public discussion of gender and work. It is no surprise that the research was funded by the Suffrage Centennial Year Trust which recognises the continuing importance of these issues.

The authors present their research and its findings in report form rather than in the form of a more scholarly treatise that locates their work in a wider sociological tradition that draws on extensive literature and contemporary international debates. For the purposes of this report, and its hoped-for primary readership, this may not be a weakness. But for readers other than policy makers or fact-seekers, the lack of wider contextual and theoretical discussion and the retention of unexamined, conventional understandings of work, employment, and 'part-time' in the report render it immediately limited. (There is a growing body of international literature that questions the primacy of work and its conventional meanings in social organisation in the imminent future eg Aronowitz, 1994; Casey, 1995; Gorz, 1989; Rifkin, 1995). None the less, the report's genre of research conducted in the modernist social science tradition continues to have relevance today, and continues to provide social analysts with important empirical data about the lived, everyday experiences of contemporary social life. In the case of this report, the wealth of factual information on women and part-time work is impressive and the rigour with which the research appears to have been conducted is commendable.

The authors provide a useful 8 page summary of the book at the outset. They also indicate and helpfully discuss the reasons for their selection of 4 sectors: retail, service, education, and health as the primary targets of their study. Women have long been well-represented in these sectors, and as the authors point out, where women comprise the majority of full-time employees, employers are more likely to manage fluctuations in demand for labour by employing part-time workers, who

in turn tend to be women (p.29). I would like to have seen a self-reflexive declaration early in the report of the authors' orientations to the subject matter and the exclusivity of their focus on the experiences of women part-time workers. The absence of such declarations leads the authors to proffer numerous statements of the like: 'Part time work is commonly perceived as problematic because it is associated with low rates of pay, minimum employment conditions, and little employment security' (p.90). This perception may indeed be the case among women part-time workers, but other parties to the relation of part-time work may have conflicting perceptions of its associations - a point that requires at least some commentary rather than simply implied refutation.

The book is organised in a straightforward style with each of the 7 chapters offering a conclusion in summary form of the main points reported and discussed. These summaries will be useful for some readers but the material is by and large repeated in the general summary at the beginning of the book and could have been omitted. An alternative may have been to suggest some further reading for readers wishing to explore more extensively matters not fully discussed in the text.

Much of the book is devoted to describing the contexts and 'realities' of part-time work (Chapters 1-5). Chapter 6 undertakes a promising discussion of the 'implications of working part-time' implicitly for women workers - other stockholders in this matter are not mentioned - but to my mind this chapter does not discuss or interpret the research findings to the extent that it could have, even in a report-style book. The reporting of rich informant data

is well done, and more of this material would have been welcome in this chapter. At the same time, a number of important implications are sketched out, but a more comprehensive, analytical discussion of these matters and their wider implications is not undertaken. For example, the authors omit to undertake a discussion, in view of their findings, of one of the serious questions they criticise others for neglecting (p.13) of the division of labour between women and men in contemporary New Zealand conditions.

As a result, the books findings, as Sonia Davies notes in her preface, present no surprise. Most of the women informants reported dissatisfaction with their part-time employment; most preferred more hours of work, better pay and conditions, and improved opportunities for advancement and participation in their employing organisation. They fear marginalisation, eroding conditions and casualisation. The authors interpretative comments implicate the Employment Contracts Act 1991 and similar deregulation legislation as key factors in the casualisation and intensification of work, particularly traditional women's work. While the data reported indicate the outcomes of these recent developments since the 1980s in terms of the experiences of women workers, no effort to socially analyse these outcomes or to locate them amid broader social, economic, and technological change is offered in this book.

The book offers sound empirical data that describes and documents current experiential realities of women and part-time work, but it falls short of the sociological project to analyse, interpret and theorise these conditions. For

readers seeking this level of scholarship, the book may well be disappointing.

But I would emphasise and endorse the importance and usefulness of the information gathered and reported in this small book to a more general readership. Politicians, business leaders, employers, trade unionists, and social policy makers could be richly informed by this book. At the very least, the research findings reported here remind us of the very real concerns about women's unequal experiences in the workplace and the likelihood of those gaps widening under social and economic policies designed for short term sectorial gain at the expense of longer term and wider social interests.

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Jane Ritchie and James Ritchie, **Violence in New Zealand** (2nd Edition), Wellington, Huia Publishers, Daphne Brasell Associates Press, 1993.

Reviewed by
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According to the authors of the second edition of *Violence in New Zealand,* the main text remains unchanged 'because, as a review it stands' (p.viii). What has been added is an epilogue which briefly reviews the last five years and goes on to outline a violence reduction programme.

As a review of violence in New Zealand, this book is comprehensive. The book follows a well structured path that begins by reviewing the nature of violence. Within this section, the chapters deal with the difficulties of defining violence and examine the major physiological and psychological theories used to explain violent behaviour. The next section comprises two chapters dealing with the patterns of violence experienced in New Zealand. The first chapter examines different aspects of domestic violence, whilst the next chapter deals with violence in the wider society. The bulk of the book is taken up in the next two sections. The first is concerned with the origins of the particular patterns of violence found in New Zealand and the second examines some factors which contribute to and reinforce these patterns.

The final section of the book points to remedies that may be taken to decrease the violence in our society. Included here are recommendations for change in social

policy, race relations and a proposed violence reduction programme covering all levels of society. These recommendations are reiterated and a more detailed reduction programme is presented in the epilogue which, as already noted, begins with a brief overview of violence in New Zealand over the past five years.

Most of the chapters within this book can be read as separate analyses in their own right. Each deals in some detail with different aspects of violence, concluding with recommendations or critical comment. As such, for those interested only in certain aspects of violence, this style makes the book a useful resource.

The approach adopted can clearly be found in chapter four, 'Violence at Home', which gives a comprehensive overview of domestic violence within New Zealand. The chapter examines the issues of corporal punishment, spousal abuse, domestic homicide, marital rape and child sexual abuse. Each of these issues is treated in a similar manner. First, an attempt is made to gauge how much of a particular type of domestic violence is experienced in New Zealand by using existing local studies (which the authors note are very few). Then reasons for such violence are examined, along with a discussion of the effects of mediating factors. This is followed with an examination of how successful community interventions are in trying to curb that aspect of domestic violence. The long term effects of the type of violence in question are then discussed. The chapter concludes by noting some of the moves being made within the community to decrease the level of domestic violence as a whole and then suggests actions that still need to be taken if it is to be eliminated. Throughout

there is a large amount of evidence that not only strongly supports the authors' arguments but is also an excellent resource for those who wish to conduct further study on a particular aspect of domestic violence.

Although each chapter is a self-contained unit, they all add to and support the central thesis that violence in New Zealand is not due to a breakdown of social patterns but 'that violence is part of what we are, an expression of deep and extensive cultural patterns' (p.vii). This is illustrated in the third section of the book, 'origins of New Zealand Violence', where it is argued that socialisation into the acceptance of violence as a 'natural' part of our society begins early for many individuals through their experience of corporal punishment at the hands of their parents. Examining various parts of New Zealand society, the Ritchies illustrate that the right to use force as a measure of social control is an integral part of our culture and is reinforced throughout society. For example, the whole New Zealand character is built on an image of physically tough males. The ideal New Zealand character is the 'resourceful Kiwi bloke', 'the wiry little bugger' who can 'hang on in there' (p.91). This image is strengthened in the glorification of the exploits of the ANZACs at Gallipoli and the anti-heroes of New Zealand folklore like Stanley Graham, the mass murderer, and George Wilder, the great prison escapee. Although these latter men were criminals, the Ritchies state that:

> This image of the outsider battling authority and, at least for a time, winning, connects powerfully with New Zealand attitudes to authority and the fragile controls that curb male aggression (p.94).

It is male aggression and 'the persistence of male role-derived notions of power and privilege to engage in violent acts' (p.viii) that is, according to the authors, the reason for the continuation of the patterns of violence experienced in New Zealand. These male patterns of violence contribute to and reinforce societal attitudes to sport, guns, sex and violence, media and alcohol.

The suggested remedy needed to overcome violence in our society is found in the last section of the book, inclusive of the epilogue. These suggestions focus on more equal social policies and the serious redress of wrongs by Pakeha against Maori, including the adequate provision of resources for Maori. But the most important ingredient is that 'men must change' (p.viii). The pattern of violence in New Zealand is male and therefore it is they who must change. Attitudes must be altered and this will have financial and personal costs for every New Zealander.

While I agree that violence is an integral part of our culture, I would argue that the authors have over-emphasised the part of males in some of the patterns found. For the most part, the book portrays women as the victims rather than the perpetrators of violence, although in the 'Preface', the authors note that 'women bully sometimes too, throw things, inflict abuse' (p.viii). Overseas research (Steinmetz, Gelles and Strauss, 1988) suggests that the word 'sometimes' is misleading, as the findings indicate that women are as likely as men to use violence in domestic disputes and are actually more likely to be the perpetrators of child abuse. Although a similar study has not been conducted in New Zealand, these findings suggest that both men and

women may be equally responsible for some of the patterns of violence found in New Zealand. Whilst men are the largest perpetrators of violence and women its most affected victims, it is the violent actions of both men and women in the home that continue to perpetuate the cycle of violence in New Zealand society. It is my feeling that the emphasis on the 'maleness' of New Zealand violence within this book distorts reality, especially in regard to domestic violence where both men and women must change.

Despite this criticism I found the book to be interesting and its style and presentation makes it available to a wide audience.

References

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Erik Olssen, **Building the New World: Work, Politics and Society in Caversham 1880s - 1920s**, Auckland, Auckland University Press, 1995.

Reviewed by
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This handsomely produced book is a much awaited introduction to the research on the social history of Caversham that Erik Olssen, Tom Brooking and their associates have been working on for more than twenty years. In this collection of essays, Olssen weaves together an impressive range of research materials to provide a richly textured portrait of life in the Dunedin suburb during those decades that bridge the turn of the century.

In the Introduction Olssen, sketches his view of the labour process, of skilled work and social class. He also sets out his views on structure and agency, the links between social being and social consciousness, and how these in turn reflect and are reflected by signs and meanings (p.14). The language in the last sentence reveals that the author is, albeit warily, prepared to present his theoretical credentials openly and therefore slide across that always arbitrary boundary between history and sociology.

The book is organised thematically. After being generally introduced to Caversham and The Flat, geographically and historically, the reader is taken through essays on

the handicraft, building and metal trades within the suburb. Many of these trades were and remain male preserves, but there is close attention to the gendering of labour, particularly in the realm of womens' skilled work. Ethnic divisions are also alluded to, with mention of the Chinese workers who served as convenient pariahs when economic conditions deteriorated. Thus we get a fascinating analysis of the interconnections between different forms of the labour process, the subtle and less subtle gradations of skill, and the complexities of patriarchal and racist exclusion.

These chapters draw on a wide set of sources, whose individual imperfections are acknowledged, but together provide the most in-depth historical analysis of a 'community' yet provided in the New Zealand literature. Olssen and his research assistants have produced a quantitative picture of social trends based on street directories, electoral registers, factory statistics, census data and other published and unpublished secondary works. Union and voluntary association minutes, local newspapers and political tracts, are also drawn on. Biographical and autobiographical vignettes, of prominent and less famous locals, are used imaginatively to humanise the statistical portrait of local lives. The memoirs of a Mrs Lee and Mrs Brown, for example, are drawn on extensively in the chapter on skilled women workers, but they also reappear throughout the book. Sam Listers' prolific penmanship in *The Otago Workman* serves much the same purpose in the political chapters that follow. Oral histories were also drawn upon, although to what degree is not entirely clear. The outstanding photos and maps used throughout the book are a further source of illumination

and interpretation. These illustrations are not mere enhancements to the text, but are an integral part of the analysis. They are a methodological device that adds a further dimension to our understanding of social context and place, most pleasingly exemplified in the case study of the Hillside Workshops (presented in Chapter Six).

The penultimate two chapters underline Olssen's conviction that social and political history are intimately wedded. The fine grain of electoral, union and lodge politics in Caversham and its surroundings are examined on a local and national canvas. In particular, the ties between the sites of daily experience - the factory, shop, home, the neighbourhood - and the production and reproduction of meanings and values, are well demonstrated. The author describes how words like 'labour' and 'class' promoted different political representations of work and society. The meanings attached to these symbols shifted in accord with economic boom and bust, and when jobs and skill differentials were threatened by technological change and the changing shape of the labour force. The trend was from apathy to Liberalism to Socialism (with its own local and national flavours), and possibly, back to apathy. But no neat linear progression is provided because the author is well attuned to the complexity of sectional interests that muddied the waters of any fundamental change in capital/labour relations, or the relations between men and women.

The final essay, entitled Social Structure, was of particular interest to me. Here Olssen explores the meanings of social class in Caversham. What was distinctive, he asks, about the locality, New Zealand, and

the global conditions in which both were embedded, that fostered perceptions (or the lack of them) of social lives lived in class terms? If class was used as a lens through which one viewed the world, the vision was intermittent and frequently cloudy. Why? Olssen's answer is not particularly original, but is very eloquently stated. He looks at the subtleties of interpersonal relations between employers, managers and workers, often influenced by the reality or myth of social mobility. The mesh of interdependencies and contradictions of rights and duties in family, trade, and workshop presented further contradictions. The way in which 'community', locality, patriarchy and ethnicity sometimes gelled with class imagery and behaviour, but more often created points of friction, or provided a glass on the workings of capital and labour, also played a part. Olssen makes much, perhaps too much, of the levelling experience of the shared voyage out in steerage. This experience, compounded by a constant tendency to compare Old and New worlds, he argues, elevated images of relative self-improvement and the undermining of patterns of deference. Broader changes in technology, skill differentials, and mass education, promoted occupational differentiation that further confused what was already a highly attenuated link between the harsh realities of British class and the often, but not always, subtler tones of local status distinction. Equality of regard, 'the fair go' and the scorning of pretension, masked or removed class distinction, while the contradictions with gender and ethnic inequalities were effectively neutralised by conceptions of 'natural' distinction - male/female - local/foreigner.

There are weaknesses in Olssen's book. He often displays a lack of surety with theoretical concepts and is rather too inclined to assume that theories and theorists can be trundled in and out to suit empirical circumstances with apparent disregard for overarching incompatibilities between different world views. For example, the rather tired juxtaposition of Marxian and Weberian possibilities could have been partly eliminated if he had been more aware of recent sociological work - Crompton, Sayer, Waters, Ohlin Wright for example - who have covered this ground with greater rigour and imagination than some of the rather outdated sources drawn upon. Equally, the constant allusion to structure and agency is rather mechanical, despite Olssen's rightful insistence on seeing the world in dynamic fashion. But most striking is a curious lack of engagement with much of the New Zealand literature beyond the narrow confines of Otago, and history in a disciplinary sense. There is some mild sparring with Fairburn, and other historians get less shrift in the footnotes, but much sociological writing on gender, class and community is surprisingly ignored. Bedggood, Wilkes, Willmott, Hall and Thorns, James and Saville-Smith, to name but a few, get nary a mention.

These caveats aside, this is a splendid book that provides a wealth of data and argument that specialists and general readers alike will learn from and quarrel with. As Olssen concludes about his city and its suburbs, and one never loses sight of the fact that he has long been one of its citizens: 'We are as we find ourselves because that world once existed' (p.261). *Building The New World* brings us another, highly readable, step nearer towards a deeper understanding of that premise.

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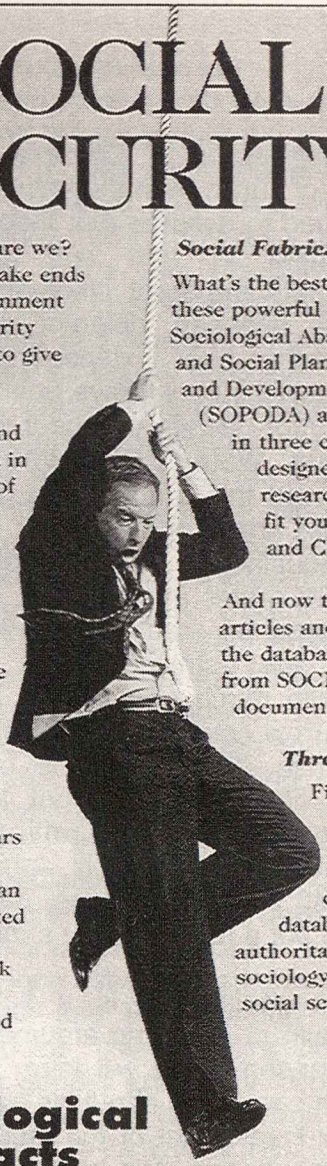

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