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Contested Inheritance:
The Emergence of Social Science Research in New Zealand

A thesis presented in partial fulfilment of the requirements
for the degree of Doctor of Philosophy in Social Policy at
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Abstract

The substantive task of the thesis is to explore a dimension of social change – the emergence of social science research within New Zealand by 1984. The thesis begins by asking questions about the status of any account – as description, understanding, or explanation. In the first instance, these questions are discussed within positivist and anti-positivist traditions. Following on from these, the work examines a series of post-positivist approaches that focus on method, general theory, and metatheorising.

Many accounts of social science development emphasise the role of a rational social science idea, others stress contextual considerations. In finding these wanting, the thesis argues for the incorporation of culture, structure and agency, and discusses attempts at resolving these within the work of Anthony Giddens and Andrew Abbott.

The final part of the theoretical discussion explores the potential of Critical Realism for causal analysis. Within this tradition, Margaret Archer's work is particularly useful with its commitment to robust notions of agency, culture and structure, and emphasising their interplay through time. With some modest revision to attune her position to the demands of practice, the thesis suggests 'Retrospective Narrative Realism' that incorporates ontological insights and the two practical moves of analysis and narrative. The thesis argues that these moves must be explicit, using the analytic to create hypothesis that are then tested via narratives that link emerging structural and cultural forms.

The thesis uses this approach for the substantive task of writing a theoretically informed account of social science research. It works within the spheres of State, University and broader social spaces, centring interests and the role of conditioned interaction. The account is presented within four phases: *the inheritance* – research by the State for the purposes of the State; *in search of independent means* – research by the State of broader social forms; *alternative benefactors* – research of social issues by broader social and University interests; *on the brink of bankruptcy* – a questioning of the State and social forms by social and University interests. Over these four periods it is possible to trace the emergence of social research, then social science, and then specific disciplines.

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Section I

The Task

Chapter One

Introduction

The Background

The roots and boundaries of academic disciplines have intrigued me for some time. In part this is a reflection of my own convoluted disciplinary passage through studying history and political philosophy, prior to undertaking empirical social research within the field and discipline of social policy. Being a relative latecomer to social policy, I was curious about its subject matter, concepts and methods. Frequently it seemed little more than an emended social administration, its subject matter centring on State welfare institutions, and remaining somewhat parasitic on the concepts and methods of other disciplines – particularly sociology, anthropology and economics.

The lack of theoretical awareness of social policy signalled that it had not quite shaken off the social administration and ‘jobbing research’ influences. Added to this was also its relatively short historical memory and a-temporality, with time amounting to a specialism – the history of welfare as a separate area of study. Indeed, while social policy analysis aspired to provide evidence for betterment towards justice, equality, and freedom, there was less awareness of how the past shaped the present and continued to shape the future.

My interest in exploring the development of social policy was clear but settling on an object of study proved more difficult. Whether viewed as a field of inquiry or as a discipline, social administration and social policy were closely associated with a *mélange* of developments around social research activity, social science, and a broader range of individual disciplines. While I wanted to acknowledge the intertwining of social policy with these disciplines, taking them all into consideration threatened to deliver separate chapters on politics, sociology, anthropology and so on. The resulting work would have been long and laborious but, more seriously, limited in its explanatory task. Thus in conceptualising the task, care was taken to avoid essentialism or the gathering of a plethora of disembodied facts, whilst retaining overlaps that might exist. There also needed to be recognition that some social science disciplines share more of a family resemblance than

do others. There also had to be a broader focus on sites of knowledge, including the State and extra-social spaces and their interrelations, with social science research standing at an intersection of the educational/intellectual, economic, social, and political, and therefore necessitating the investigation of the State, public service *and* University. These musings led to the study focusing on the development of empirical social research and social science activities within New Zealand State and society. The earlier part of analysis is tied to Statecraft, social research and a more unitary notion of *social science*, while as the account progresses, the focus shifts to individual *social sciences*. Individual social sciences that are central to the account include those contemporarily recognised as social administration, social policy, sociology and social work, but also reaching into the realms of anthropology, psychology, and economics. To avoid cumbersome reference to social research/ social science, the term 'social science research' is used throughout the study to signal the broad area of inquiry.

Exploring these social science research developments appeared a huge task. To ensure the reach did not exceed the grasp of inquiry, I turned my attention to how sociology and history could furnish theoretical and explanatory resources to explore change over time. A number of alternatives presented themselves in terms of methodological imperatives: description versus explanation; causal explanation versus interpretivist understanding; science versus relativist perspectivism; socially conditioned actors versus behaviouralism; narrative versus analysis; in time versus through time, and so on. A range of questions had to be tackled for the account to be written, and these questions became a sextant to keep the study on track and prevent it becoming a morass of historical facts.

It was how accounts of the past married process and evidence that gave the project greater theoretical grounding, acting as a brake upon my enthusiasm to map the history of social science research and disciplines and avoiding the problem of the work becoming chronology or reportage. While clearly the past mattered, it was why and how it mattered that became central, and there was a re-focusing from surface events to underlying mechanisms. Theoretical explorations can also become all-consuming and throughout the study theoretical exegeses and polemic were kept in check by the practical task of making sense of social science research.

Looking across a number of countries, it was apparent that the conditioning impact of structures and cultures differed in every setting. Although this particular account focuses upon New Zealand, in its drafting there is an implicit cross-national perspective that arises from the broader experience of Western countries, and from having some in-depth knowledge of both the British and New Zealand scenes. In writing the account, the nature of colonisation was examined and a complex set of dynamics unpacked – including relations with Britain and, later, the United States. A series of six cycles was perceived, leading to a decision to focus the account upon the earlier four cycles, concluding discussion at 1984. Specifying stages was not enough in itself, it also required that the reasons for transition from one cycle to another be clarified.

Given the discussion traverses terms of social research, social science and individual social sciences, some clarification of those terms is useful. Oberschall (1972a, p. 2) provides a neat starting point:

What is meant by social research? For our purposes it consists of broadly systematic empirical investigation by means of social surveys, field observations, and the secondary analysis of demographic, socioeconomic and institutional data.

Social research focused on the collection of empirical material stands apart from a more philosophical and reflective tradition of social diagnosis. Social science has a similar interest in studying the social, political and economic world, and pays greater attention to the conceptual and defining terms – for which it then goes in search of empirical evidence. Aside from this unified social science field, individual social science disciplines such as sociology, political science, economics, anthropology, and perhaps social policy, identify specific subjects and fields for inquiry, continue conceptual clarification, and develop more overarching theoretical commitments which inform methodological and technical matters. Disciplines are constructed, with groups of adherents identifying theoretical and conceptual resources and making claims that this generates autonomous knowledge. Such demarcated disciplines select particular subjects for their investigation – the individual, the political, the economic, the local, the social and so on. There are inevitably overlaps and ongoing skirmishes into the territories of other disciplines.

The Study Material

Data was obtained from primary and secondary sources. Primary data included many central Government records and University of New Zealand archives held at the National Archives in Wellington, and private papers and collections held at the Alexander Turnbull and Hocken Libraries in Wellington and Dunedin. Also consulted was social science research literature from across the period, held in journals, books and grey literature at the National Library, Wellington. The secondary literature includes writing around politics, economics, Universities and Government science. I was also fortunate to have useful discussions with some key individual players from the 1960s and 1970s, and whilst these discussions were helpful in allowing me to gain orientation and clarify my ideas, they are not included as primary data as such.

On a technical note, Chapters Two to Nine use a Harvard referencing system and occasional endnotes to clarify particular points. The presentation of analysis in Chapters Ten to Thirteen places all references and additional information in its endnotes. Whilst the chapters of analysis could have retained the earlier system of referencing, it would have been to the detriment of the explanatory exposition. Chapters Ten to Thirteen aim to present in the clearest form possible the fusing of the analytical and the narrative and this is aided by a stylistic approach that brings the players and the broad cultural and structural constraints to the fore.

Structure of the study

Frequently in reporting research, there is a tendency for *post-hoc* rationalisation – the report is how the researcher/writer *wishes* the project had unfolded and been experienced. Whilst not denying some elements of overlap in the research stages, the sequence of thesis chapters is an accurate portrayal of how the research progressed theoretically and practically, moving from an initial problematic, through methodological and conceptual considerations, to the analytical resolution of this problematic.

Chapters Two to Four are labelled ‘The Task’ and allow the research problem and focus to be outlined more fully. As Chapter Two suggests, in writing an account of social science research, there was a period of groundwork that involved scouting ahead, gaining a feel for the possible task, and an early immersion in the primary and secondary material. As this

immersion continued, and the data threatened to overwhelm, there was a lurking question of what would happen to this stockpile once it was amassed. Could a set of synopses constitute a description or explanation and, if it could, what would be the status of the final synthesis? Thus began a series of incursions into the nature of knowledge (Chapter Three) and exploration of different views around the possibility and desirability of explanation. These debates suggested key polarities of explanation versus understanding, and narrative versus analysis. The appropriateness of these choices hinged upon their conceptualisation of 'science', and Chapter Four moved to examine ways in which a traditional view of science activity and the dichotomies it invoked could be surpassed.

Chapters Five to Seven are effectively 'The Tools' of the study. Chapter Five highlights evolutionary, structural and ideational approaches to accounting for social science research. These approaches point to an internal dynamic of social science research and one cluster of them traces the contours of a rational social science idea. A second set of approaches points more to material considerations and contextual dimensions as being crucial to grasping social science research. In the case of structural interpretations, social science research arises from external consideration, outside pressures and tensions. Each of the views outlined in Chapter Five attempted to shortcut the explanatory circuit, offering their own keys to unlocking change. In these accounts agents were by-passed, either made mere conduits for ideas or relegated to bearers of forces. Chapter Six explores different ways by which individuals could be introduced into the account, whether through forms of individualism, or by reconnecting individual action to the fact of social existence. While it is tempting to see the best way forward as simply adding the insights of Chapters Five and Six together to produce a working methodology, this should be resisted and resolutions sought that explore the relationships or interrelationships of agency and structure, and better situate the role of culture. Chapter Seven explores some of these workings in detail, focusing particularly on the work of Anthony Giddens and Andrew Abbott.

'Sharpening the Tools' best reflects the focus of Chapters Eight and Nine. The Critical Realist (CR) tradition discussed in Chapter Eight offers routes to resolving various explanatory dilemmas that have been encountered thus far. The chapter explores the roots and routes of Critical Realism and highlights some of its problematic claims and weaknesses. Chapter Nine draws upon the work of two Critical Realist writers, Margaret

Archer and Christopher Lloyd, who have attempted to develop a more viable historical sociology. In introducing the insights of CR for historical sociology, Chapter Nine suggests some additions and re-workings of the insights of Archer for historical practice. The practice of conducting historical research suggests two particular dynamics: first, the historical account is assembled through a move of retroductive realism, which is in essence a process of 'backward mapping'. This allows mechanisms to be identified, which are then assembled and illuminated within a narrative exposition. There is discussion of how these two processes are achieved in a transparent manner and are made to deliver causal explanation. While the narrative presents the account from the 19th century to 1984, analysis works in reverse by carving out four discrete cycles. Chapter Nine offers some further reflections upon this process and how it must be aligned with a commitment to fallibility within accounts. To ensure that processes of analysis and narrative presentation are both recognised and clear, the approach developed here is labelled as *Retroductive Narrative Realism*. It is heavily indebted to the work of Margaret Archer, whilst also incorporating the methodological insights of Andrew Abbott.

The final section is termed 'Getting the Job Done' and consists of the analytic and narrative presentation of Chapters Ten to Thirteen. In achieving its explanatory aim, the analysis identified mechanisms and emergent properties through a process of backward mapping. These mechanisms and properties are then linked and 'tested' within the narrative in their exposition. Whilst the analyst moves backwards in their mapping, the narrator reverses this process. Retroductive Narrative Realism offers an account that is transparent, causal, fallible but grounded. The result is the explanatory contribution presented in Chapters Ten to Thirteen.

Chapter Two

Contested Inheritance:

Depicting the growth of social science research

Introduction

There is no conceptually informed account of the emergence of social science research in New Zealand. This chapter traces the parameters of that which is to be explained, focusing upon the emergence of sites of social science research and the forms it took. This preliminary 'reportage' (Runciman, 1983) suggests initial fusion in the overlap of sites and actors and, later, fission via organisational specialisation and disciplinary configuration within the social sciences.

An overview must avoid 'encyclopaedism' (Rothblatt and Wittröck, 1993, p. 2) and guard against factual 'reportage' that seeks to speak for itself as some narcissistic epistemology (cf. Fischer, 1971). The methodological encounter is 'bracketed' until later chapters; here the emphasis is upon the 'empirical' and the actual. The aim is to present a sharp contrast between the current chapter and the later theoretically informed analysis that emerges through the sieve of ontology and epistemology.

An account must also avoid contemporary definitions of social science research directing investigation and which has a requirement that the object of investigation must be explicitly labelled as 'social research' or 'social science' in order to come under the terms of any study.¹ A further danger is viewing social science research as a timeless practice so beginning instead a lengthy historical journey into the distant past of Greek antiquity. Social science research is a product of 'modernity' and its practices have intensified with 'late modernity'. Its development is intimately associated with the emergence of the nation-state, and in this regard, social science research in New Zealand is no different.

Discussion begins with early statistical developments of the State, highlights the growth of welfare professionals and the attempted institutionalisation of social research within

Government Departments and the University of New Zealand during the inter-war period. It goes on to highlight the post-war emergence of research activities within the burgeoning systems of State and University and traces the ascendancy of social science research in its diverse forms after 1945.

State Reconnaissance

Amassing routine data on population size and structure and on economic conditions is synonymous with the emergence of nation states. This relationship is evident for the older Europe of Britain, Prussia and France, the older colonies of the United States and Canada, and those lands subject to 19th- century colonisation including South Africa, Australia and New Zealand. As Desrosières (1991) acknowledges ‘... the setting up of systems of statistical recording goes hand in hand with *the construction of the State*’ (p. 197) (also Kalpagam, 2000).

In New Zealand, the colonial State has since its early days (1840–70) collected statistics in the hope of persuading potential migrants to settle here (Oliver, 1977; see also Fairburn, 1989). New Zealand – as untamed but plentiful, having a low population, and with low incidences of illness and disease – contrasted within the industrialised and famine-ridden homes of origin. Collation of information contributed to the establishment and expansion of the State.² Statistics satisfied the State’s increasing desire to ‘know’ its population reflected in the Blue Books of 1840–51 that presented figures for population, trade and agriculture, and the development after 1851 of the Census.³

Over time the State sought to ‘know’ further characteristics of its population, households, and workers, and the Census captured the colony’s changing economic, social and political context (Thorns and Sedgwick, 1997). As the State assumed greater responsibility for intervening in economic affairs, it collected data on national income, employment, prices, and unions. From the 1870s onwards there was the collection of economic data on bankruptcies, exports, imports, unions and strikes. As *statecraft* expanded so the State began also to track departmental expenditure, employees and activities.

Public sector growth and the development of the Census contributed to the institutionalisation of data collection and statistics. The difficulties faced by the 1890 Royal Commission on Sweating in accumulating reliable data led to the Department of Labour (established in 1894) being given the task of collecting statistics on labour conditions.⁴ In its Annual Report of 1894, the Department of Labour acknowledged shortcomings in the collection of industrial statistics. Calling for increased legal powers to collect such statistics, it warned that 'New Zealand will be unable to compete with other and better-informed countries if she remains in the dark as to the cost of production and the sources of supply among her own people' (AJHR, 1893, H-10, p. 8).

By the early 20th century the level and quality of routine data gathering was still perceived as relatively inadequate. The 1912 Education Commission, for example, commented upon the 'paucity of statistical information relevant to the questions' (AJHR, 1912, H-18, p. ix). Elsewhere, Section 15 of the 1900 Maori Council Act aimed 'to collect and tabulate facts and statistics' about Maori mortality, health and improvements, but little was undertaken until the 1920s (Dow, 1999). State sector reform saw the first governmental statistician appointed under the Census and Statistics Act (1910) and in 1915 the Census and Statistical Office was established, becoming a separate Statistics Department in 1936. The growth of statistics and social statistics was embroiled in a symbiotic relationship whereby State growth created the need for more information, and improved information in turn facilitated growth and intervention.

Complementing *routine* aggregation of demographic and economic information were *one-off* activities of State reconnaissance. Increased labour market regulation and provision of pensions required greater prognosis and diagnosis of particular social ills. Social investigation helped furnish an information base for the Old Age Pensions Act of 1898 (Morris, 1952), while in 1893 the Department of Labour sought to 'follow the lead of some European countries by obtaining information on working class expenditure around food, clothing, lodging and expenses'. The aim was to understand variations in the rise and fall of wages and almost 800 schedules were distributed by inspectors of factories, labour

bureau agents, and trades-union secretaries to working class men who were expected to make *bona fide* returns (AJHR, 1893, H-10, pp. 40–50).⁵

Although the Department of Labour's Annual Report of 1894 drew attention to the attempts of private societies and trades unions to identify their 'standing ground' around economic issues, including prices of raw materials and trade flows, there was relatively little interest in welfare 'facts' outside the State apparatus. In contrast with Britain where a statistical movement developed outside, and autonomous to, the State and established Universities (Manicas, 1987), developments within New Zealand were primarily State-driven.

Royal Commissions marked further State attempts to collect systematic information around matters of particular concern. A close relationship exists in New Zealand between early State-sponsored inquiry and the use of Royal Commissions (Robertson and Hughes, 1982; cf. also Bulmer, 1980). Early Commissions consulted with 'experts', collected a range of research materials, and encouraged the development of statistical capacity. The 1912 Education Commission⁶ took voluminous evidence on costs, administration, powers and rights, expenditure, function, and possible improvements within education. Similarly, in the deliberations of the Cost of Living Commission, a major cost-of-living study undertaken in 1910–11 and survey of seasonal fluctuations in manufacturing employment were used.⁷

'Proto social scientists'

Early reformers and researchers existed *within* the New Zealand State, including individuals such as Edward Tregear, Duncan MacGregor, George Hogben, John Beck, and Truby King, whom Oliver (1977) labels 'proto social scientists and researchers'. One of New Zealand's best-known reformers, Edward Tregear, is also one of its best-known early researchers (Howe, 1991). During time as Secretary of Labour and Chief Inspector of Factories, he produced statistics on rents that prompted new house building between 1905 and 1910. Tregear also gathered statistics on the condition of labour, utilising local agents to report shortages in the labour market in an attempt to fine-tune intervention (pp. 76–7). Similarly, Duncan MacGregor as Inspector-General of Hospitals visited homes of the poor

and from 1886 submitted reports to Parliament on the causes and solutions of pauperisation that were derived from statistics and field visits. For Tennant (1989), ‘MacGregor became in his primitive and prejudiced way, one of New Zealand’s earliest social researchers’ (p. 45). George Hogben’s first annual report as Inspector-General of Education was an exhaustive survey of the condition of education (AJHR, 1901, E-12; Webb, 1940). During the Royal Commission on Education in 1911, he toured for three months, taking evidence from major centres, and used his appointment on the Commission as an opportunity to inspect schools (Roth, 1952, p. 98). John Beck’s administrative activism emerged as early as 1907 when he toured children’s courts and child welfare institutions with a view to improving conditions, and his welfare investigations continued into the 1920s and 1930s (Dalley, 1998). Finally, Truby King was active in encouraging health research activity within the Department of Health.

Although strictly senior ‘administrators’, such individuals during this period had multiple roles that included investigative – and often policy – influence. Investigations were driven in part by political and public concerns and their activities stimulated the development of departmental functions.⁸ Such administrative pioneers are to New Zealand’s social research tradition what Booth and Rowntree are to Britain’s, although the latter were individuals with social conscience, administrative interest, *and* private means.

The growth in numbers of professionals

In addition to centralised State functions – the Census, the development of routine statistical collection, and the work of key departmental administrators – a growth in the number of welfare professionals contributed to early reconnaissance. The development of health, education and social service activities meant professionals were increasingly intervening, policing, and disciplining individuals, families and communities. Exponential growth of the professions promoted the investigative activities of public health officers, sanitary inspectors, school medical officers, probation officers, child welfare officers, to say nothing of teachers, nurses, and medical practitioners. Whilst these groups lacked any formal instruction in research, increased surveillance of clients and patients was fed back to central Departments, and further routine monitoring was appended to increasingly detailed Departmental reports (and Royal Commission submissions) submitted to the

House of Representatives.⁹ The submission of annual Departmental reports of information and 'facts' to inform and change practice was particularly evident in the Health Department which developed a picture of children's health status during the inter-war years (Sutch, 1966, pp. 152–3; Dow, 1999). Within public health, individuals such as H.B. Turbott became well known,¹⁰ and research by School Medical Officers during the 1930s explored the nutritional value of milk and the susceptibility of Maori to certain diseases.

Medical research work was consolidated under the auspices of the Otago University Faculty of Medicine. In 1925 the Government provided a grant for polio research, and later on funding was provided for cancer research and, on the initiative of the Ministry of Health, for research into rheumatism and chronic arthritis. Based at the University at Otago were a number of nutrition researchers who were involved in the work of the Nutrition Research Unit¹¹ and the University received Carnegie Corporation funds from 1929 onwards to support its Home Science Project. The New Zealand Health Act 1920 had recognised the responsibility of Government to support medical research (Hodge, 1987; Dow, 1999) and the formation of the New Zealand Medical Research Council (MRC) in 1937 put medical research on a more coordinated footing. The MRC remained a separate Departmental committee under the Minister of Health until 1951, when reorganisation granted the Council greater power (NZMRC, 1951; Dow, 1995, 1999).

The growth of the State encouraged more regular and timely information flows. As the role of the State in economic and social affairs became increasingly complex in administrative systems and policy-making intentions, one-off Royal Commissions were insufficient (Thorns and Sedgwick, 1997; also Tennant, 1989). From the 1930s, the State began to view the contribution of research and information more strategically. The Social Science Research Bureau was a short-lived venture into specialised social research activities. Established under the auspices of the Department for Scientific and Industrial Research (DSIR), 'to assist Government by providing the necessary factual bases for policy measures of a social nature' (AJHR, H-34, 1937, p. 4), its role included the coordination and stimulation of research, networking, publication, and initiating research.¹² From the outset there was lack of clarity of function as well as a chronic lack of resources¹³ and a solitary survey of the living standards of dairy farmers was all that emerged. The

Bureau fizzled out more through lack of interest than as a result of *prima facie* opposition (Robb, 1987).¹⁴

The University of New Zealand

Up to this point little has been said about the contribution of the University of New Zealand (UNZ) to social research activities and thus far it is the State which has occupied the lead role. Fledgling activities that did exist within the UNZ centred on developments in psychology and anthropology, and were funded in the first instance by the Carnegie Corporation rather than by the State.

During the 1920s James Shelley and Clarence Beeby pioneered experimental techniques in the field of psychology with child guidance and vocational guidance. As well as undertaking educational and vocational testing of children, they conducted research in industrial psychology, including time-and-motion studies and work-flow investigations (Winterbourn, 1950; Beeby, 1988; Carter, 1993).¹⁵ Industrial psychology attracted some Government interest and the DSIR in 1930 commissioned Shelley and Beeby to explore the effect of workplace changes on factory productivity. The Department of Labour also held out hope of clarifying causes of work absence, and particularly of identifying a cause-and-effect relationship. Economic depression heightened interest in how psychology and vocational guidance could assist individuals within the labour market, and a series of psychological tools were developed to assist empirical investigation around causes of 'vocational maladjustment'.¹⁶ Initially based in Colleges, psychological services were provided by State agencies after 1945 (Winterbourn, [1953] 1979).

The perceived value of industrial psychology led to the establishment of a quinquennial Fellowship of Social Relations in Industry at Victoria University College in 1941 funded by a private Hamilton businessman. The appointee was A.E. Hare and his research on Work Councils and work conditions encouraged L.S. Hearnshaw at Victoria to survey munitions factories in a bid to improve efficiency of war production. The latter, in turn, led to War Cabinet approving an Industrial Psychology Division within the DSIR to investigate workplace issues.¹⁷

There is a New Zealand tradition of anthropological investigation of the South Pacific and of studying Maori.¹⁸ Many early 'social' inquiries were associated with activities of the learned societies (Sutherland 1946; Fleming, 1987; Galbreath, 1998; Smith, 1999). With the development of colonial administrative and educational systems, such activities became institutionalised through the learned societies and networks and, later, academic disciplines. Systematic study began with the establishment of the Ethnological Society in London 1843. The inaugural meeting of the New Zealand Society in 1851 was followed by the formal incorporation of local societies into the New Zealand Institute in 1868 (Sutherland, 1946; Fleming, 1987). The Polynesian Society, established in 1892, sought 'to promote the study of the anthropology, ethnology, philology, history and antiquities of the Polynesian races'. As Barrowman (1999) suggests:

Wellington had been the centre of the colony's emerging indigenous intellectual life, of an historical and ethnological scholarship which was 'amateur' only in the sense that it remained outside the university (p. 38).

Whilst some advocated more attention be paid to economic and social problems, *Proceedings of the New Zealand Institute* suggest that relatively little attention was paid to the social dimensions of Colonial New Zealand.¹⁹ Two of the original fellows of the New Zealand Institute (in 1919), Elsdon Best and Stevenson Percy Smith, were anthropologists, illustrating the human sciences' emphasis upon anthropology during this time. Within the learned societies, non-professional scientists without formal training made a major contribution to activities and inquiries. The UNZ played a relatively small part in studying Maori culture and communities and most anthropologists pursuing Maori issues were based in Museums (Peter Buck and H.D. Skinner) or Government Departments (Edward Tregear and Duncan MacGregor).

By 1945 Maori research had experienced four overlapping phases: the colonial adventurer; the colonial collector; the social paternalist; and the welfare reformer. In its earliest form, inquiry sought to document the exotic and the different, aiming at a wider London-based readership. As Sutherland (1946) noted:

From the days of the first contact of Europeans with the inhabitants of New Zealand there are on record observations of native life and custom as well as general impressions of the people themselves (p. 71).

In its second phase, study sought to capture and preserve fragments of Maori culture, given its projected demise. An earlier fascination gave way to the more active role of collector, classifier and compiler, with an emphasis on both physical and social anthropology.²⁰ As late as 1945 one Professor of Anatomy expressed the view:

Anthropology of the Maori race is totally inadequate. I feel that we are neglecting a great responsibility in not accumulating complete records of the anthropological characteristics of pure-blooded Maoris while they still exist.²¹

A third development during the 1920s and 1930s emphasised transition and aimed to document the misfit between Maori and the 'modern world'. It sought to resolve perceived tensions around identity, loyalty, and activities of work and home that developed following urbanisation. A fourth phase, 'welfare reformism' consolidated after the 1930s and 1940s, driven by urbanisation and increased State administrative and welfare activity. It explored the interface of the 'native people' with the role of the welfare-providing State. So, for example, economist Horace Belshaw in 1935 asked about the relationship of Labour's housing and public policy to Maori²² and Ernest Beaglehole spoke of 'applied anthropology' and how 'findings may be applied to the always difficult task of wisely governing a native people' (1937, p. 166). There was also New Zealand's colonial role in Samoa and the reputation of enlightenment it sought to foster in dealings with indigenous peoples of the South Pacific. Investigative tools of social science research were seen as contributing to this. Psychology increasingly became the dominant research perspective for research within Maori communities – the Beagleholes dubbed their approach 'psychoanthropology' (1946) and Hearnshaw ([1965] 1979) reflects on the inter-war developments in 'ethno-psychology' (cf. also Sutherland, 1946).

University-based research interest around Maori began to increase from the 1920s and 1930s under disciplines of history, education and psychology.²³ There were the beginnings of academic writing, including Firth's study of the economic role of Maori (1929),

Sutherland's review of the Maori situation (1935), and a greater professionalisation of research 'on' Maori. H.B. Turbott's work provided a model for a variety of social/health surveys of Maori communities carried out by students at Otago University from the 1930s (Dow, 1999). The relationship of Maori and policy was also the focus of a series of New Zealand Council for Educational Research (NZCER) studies (McQueen, 1945; Beaglehole and Beaglehole, 1946). Founded in 1934 with support of the Carnegie Corporation, the NZCER is New Zealand's most enduring social research institution.²⁴ In addition to sponsoring the NZCER, Carnegie activities included rural adult education, library funding, museums, arts, rural sociology, and visitors grants (McQueen, 1942; Beeby, 1988; Alcorn, 1992; Carter, 1993).²⁵ The Council became a centre for research activity, a stimulus to such activity, and the route through which a range of other applications and projects were channelled (Alcorn, 1992, p. 16, p. 23).

Prior to the establishment of the NZCER, societal studies consisted primarily of institutional descriptions and historical reviews and there was a dearth of empirical investigation (Mules and Butchers, 1936; updated by McQueen, 1947). Empirical studies conducted by the NZCER include those of vocational guidance, school-work transition, apprenticeship, and rural life, and the Council fostered institutional discussions, policy surveys, psychological surveys, and service reviews. The best known was *Littledene* (Somerset, 1938), which collected various data including interviews with farmers and institutional records. The Feilding Community Centre 'experiment' run by the Somersets, the Rangiora school 'experiment' in curriculum and school organisation, and the Agricultural High School at Feilding, were also investigated (Strachan, 1938; Wild, 1938; Campbell, 1945). *Vocations for Maori Youth* was commissioned by the Vocational Guidance Association and defined by its author as one dimension of a 'Maori problem' (McQueen, 1945). Studies involved developing professional guidance centres and roles, and helped shape the personal characteristics seen as desirable in professional guidance officers (McQueen, 1940, 1945; McQueen et al., 1941). Projects also focused on a range of transitions – between school and work, between educational institutions, and the transition to University. Most of these early researches were undertaken by teachers or practitioners rather than dedicated 'professional' researchers.

Post-War Expansion of the Universities

After 1945 there was considerable change in the extent, nature and organisation of social research activity within the UNZ. During this period disciplinary configurations are increasingly delineated within the academy – public administration, sociology, anthropology and social administration. The public sector also exhibits growing interest in analysing changing social structures and in investigating the causes and solutions of ‘social problems’. Social research capacity develops rapidly during this period and there are also increased debates about Government support of University research activities.

It was only in the post-war period that funding for University research received more attention with the Senate establishing the University Research Committee in 1946 to distribute a £10,000 Government grant. Research in the social sciences made only small demands on the University Research Committee and the major source of funding for social science (in addition to internally generated resources) was the Carnegie Corporation which donated \$60,000 for the five years after 1953 (Parton, 1979). As a result of Carnegie funding, over twenty projects were soon underway with Victoria and Auckland Colleges receiving the highest proportion of funding.²⁶ A number of topics focused on Maori communities including Joan Metge’s work on rural and urban based Maori (1959); James Ritchie’s on technological change and Maori communities (1955); John McCreary and John Rangihau’s study of contemporary Tuhoe (McCreary and Rangihau, 1958).

There was continued interest in the role of psychology in exploring interpersonal and social problems and Carnegie funded Beaglehole’s epidemiological survey of Maori mental disorder and his work on personality structure. Developments in the field of psychology remained concentrated around Canterbury and Victoria with their legacies of the work of Shelley, Beeby, Hunter and Beaglehole. In the 1950s and 1960s Ernest Beaglehole, the first professor of Psychology at Victoria, embarked upon numerous studies, situating psychology at the interface of anthropology and sociology. Of 24 studies published by 1960, around half were on Maori communities, and Beaglehole built around him a range of ‘social psychologists’: McCreary, Congalton, Robb, Brown and Spinley. Sharing an empirical preoccupation they marked the first major concentration of academic social science research (Ritchie, 1979).

Further Carnegie-funded work included stratification investigating social and cultural assimilation of working adolescents conducted by Somerset; older people (McCreary and Somerset, 1955); divorce (Nixon, 1954). *Hydrotown* explored the community that sprang up around the construction of the Clutha hydro-electric project (Campbell, 1957) and the Hawera Survey advised how to spend resources within the area (Congalton, 1954).²⁷

While public administration had gained a foothold in the public service during the 1930s, it grew considerably during the 1940s at Victoria College and was later also assisted in the 1950s by Carnegie funds for studies of political parties and administration. The School of Social Science established at Victoria after 1950 brought together a range of social scientists – Marsh, Minn, McCreary, and Robb – and received substantial Carnegie support for research work. David Marsh embarked on a School research programme that reflected the growing popularity of the survey, including studies of the ‘aged’ in four main centres 1951–54, a study of Child Welfare foster homes, and a study of recidivism in the Children’s Court (McCreary, 1969). Many of the studies utilised cadets enrolled in the social work programme (Vandenberg et al., 1965). Calls were made for policy to be established on a sounder basis, including improved information on social needs (Green et al., 1954; Scott, 1955a) and the existing range of research activities were criticised as overly descriptive and too wide-ranging (Scott, 1959). Despite some cooperation between Government Departments and the Colleges, the latter remained critical of the lack of government-funded research on social problems, and of the failure to explore the consequences of existing policies (Marsh, 1950; Ferguson, 1955; Scott, 1955b, 1959; Polaschek, 1958).

With the disestablishment of the UNZ in 1961, a University Grants Committee became the intermediary between Government and the new Universities. The 1960s saw demand for University study increase, and of particular note was the growing popularity of social sciences including sociology, political science and anthropology. Social science was bolstered by the introduction of sociology teaching at Canterbury and Victoria by the end of the 1950s, and expansion continued apace into the 1970s. More specialised social science departments were established or gained independence from original host

departments. For example, at Massey University during the 1970s new programmes were introduced in social work and social policy, and the total social science staff jumped from 39 to 85 between 1970–74. This growth included the establishment of a Chair in Sociology. At Canterbury, sociology separated from psychology in 1970 and a Chair of Sociology was established the following year. Similarly, a Chair of Sociology was also appointed at Auckland. In addition to the growth of enrolments, restructured teaching departments, and new appointments, there were discussions around the establishment of dedicated research centres. At Victoria, for example, an Industrial Relation Research Centre and Institute for Criminal Research were introduced, and hopes were expressed for a survey research centre.

Research activities blossomed during the 1970s. As well as continuing fascination around acculturation, integration, and socialisation of Maori, there was work on racial attitudes and migration (Forster and Ramsey, 1969; Webb and Collette, 1973). Work continued around New Zealand's social stratification and occupational mobility (Vellekoop, 1969; Webb and Collette 1973), and a range of demographic and family-related studies, including work on marriage patterns and child rearing began (see Baldock and Lally, 1974, for summary). Inquiry developed in a range of what were to become sub-fields of sociology – education, religion, and sport; as well social welfare and administration (Palmer, 1977; cf. also Oram, 1969; Trlin, 1977).

Despite University expansion and the continuation of a social administrative tradition in some quarters, calls for more policy-oriented research met a disappointing response (NZ Planning Council, 1982, p. 50). Although funding priorities for academic social science included policy and evaluative research, there appeared to be a gulf between the academic and policy worlds.

Knowledge For Policy in the Post-War Period

A comment on public sector change completes the brief sketch of social science/ research developments to 1984, setting the basis for the later *explanatory* task. In the immediate post-war period, social problems were perceived as becoming more complex – which encouraged Ministers and public servants to draw on resources beyond political and

administrative *savoir-faire*. Proffering satisfactory advice and making appropriate decisions were seen to require public servants and Ministers to rise above day-to-day Departmental affairs (Baker, 1957). The seeds of social science,²⁸ blown in from the United States and Britain, began to take root in the mindset and organisation of Government. Initially centring upon collecting baseline 'social facts', research was expected to provide an understanding of the size, nature and perceived causes of problems. Welfare studies utilised existing administrative data, and also embarked upon the beginnings of a social survey tradition. The guiding principle was one of personal pathology – research on 'social problems' was synonymous with individuals and families, focusing variously on work motivation, behavioural impacts on health, and frameworks drawing heavily upon deficit theory.

Throughout the 1950s and 1960s, Government research activity remained a relatively piecemeal affair. Embryonic research functions were gradually added to those information and statistical functions that already existed within the Departments of Justice, Labour and Education. The 1958 Joint Committee on Youth Offending drew together a range of departmental interests in seeking to understand the causes and cures of juvenile offending, and was serviced by a research unit.²⁹ By the end of the 1960s there were research components within Education,³⁰ Justice, and Labour.³¹ Despite the vision of establishing 'causal links' (for matters such as offending, indigence, and educational failure) – the reality was of a handful of staff engaged in descriptive surveys (MacKay, 1975).

The 1960s heralded a fundamental re-examination of institutions and impetus to devise policy on a more informed and strategic basis. Central was the desire to make better use of research knowledge in policy formulation. A range of interrelated debates evolved from the late 1960s around the funding, organisation and utilisation of social research. The 1975 Statistics Act strengthened mechanisms for inter-departmental coordination of statistics and there was increased use of social indicators (Wood, 1975, p. 160; Thorns and Sedgwick, 1997, p. 123).

The National Development Conference of 1969 recommended that the National Research Advisory Council (NRAC), which had previously paid scant attention to social research,

should review Governmental social research capacity.³² The result was the Gibson Report (1970) covering central government and city councils, but excluding university activities. The report recommended that the Council develop a social science arm to foster development of research activity.

A cacophony of voices added to the debate for the reorganisation of social science within Government and the Universities during the 1970s (Shields, 1974; Fougere and Orbell, 1974). There was growing political and legislative interest in the role and most efficient organisation of social research and policy evaluation, from senior public servants, politicians and political parties. Explicit recognition of the value of social research was embedded in the Department of Social Welfare Act 1971 (section 4). The Social Council highlighted the importance of evaluating systems and exploring the effectiveness of spending on particular interventions (Social Council, 1973; Social Development Council, 1975). During this period, distinctions between public servants charged with research and a policy function became cemented. Distinctions were also increasingly made between different types of social research activity – statistics, the ability of social science to explore longer-term issues, and evaluating the effectiveness of existing and recently introduced programmes (MacKay, 1975).

The Task Force on Economic and Social Planning (1976), part of a consultative trend begun during the late 1960s, highlighted the dearth of social research and stressed the need for both longer-term research activity and the more immediate evaluation of programmes. The Task Force provided the impetus for establishing the New Zealand Planning Council (NZPC) and Commission for the Future. The former continued to advocate for the increased use of social science and evaluation.

Social Impact Assessment (SIA) which emerged after 1973 as a result of the requirement that environmental reports should include social components was frequently used to assist the planning of 'Think Big' type projects (Ministry of Works 1983; Ministry of Works 1987). There were by the early 1980s some explicit pushes towards programme evaluation with a Social Programme Evaluation Unit within the Department of Social Welfare, and the State Services Commission (SSC) tentatively encouraging such activity across

Government agencies (Benton, 1983).³³ Responses from 12 of the 16 Government Departments surveyed around social science capacity suggested there were 121 permanent staff working in the field of social science (Keir, 1982). By the 1980s a high tide of social research activity was beginning to ebb – the NZPC and Commission for the Future were to fall during the *de-quangoisation* that emerged during the mid-1980s. The role of social science, social research and programme evaluation in re-shaping society was soon to be displaced by a strong ideological current that swept through the nation's institutions.

Summary

The chapter has depicted a number of developments that had taken place by 1984, sketching what is to be explained in later chapters. Viewing the changes as a whole, more detailed attention is required in terms of addressing why the scale of activity increased in particular institutional domains and who was responsible for this. Similarly, why particular organisational responses of the public sector, including specialisation, occurred and how this happened merits greater exploration. Further questions also include why the organisational response of the University system occurred in the ways it did; what explains the emergence of social science disciplines during the period; and how the professionalisation of research capacity took place and who had interests in such development. The chapter that follows begins a series of incursions into more theoretical territory. It explores the possibility and desirability of explanation, drawing a sharp distinction between explanation and the 'reportage' with which the current chapter has been concerned.

Chapter Two Notes

¹ Winch (1958) cites Gotthold Ephraim Lessing as a foreword 'the same [moral] actions do not always have the same names, and it is unjust to give any action a different name from that which it used to bear in its own time and amongst its own people'.

² Statistics were used in early publications: Earp (1849) *Handbook for Intending Emigrants to the Southern Settlements of New Zealand*; Clayden (1886) *A Popular Handbook to New Zealand: The Rise and Progress of Australia, Tasmania and New Zealand* (1857).

³ Lord John Russell wrote to Governor Hobson, 'you will receive through one of our Secretaries of State a book of table in blank commonly called the Blue Book, to be annually filled up with certain returns relative to the revenue and expenditure, militia, schools, cause of exchange, imports and exports, agricultural produce and other matters in the said Blue Book, more particularly specified to the state of our sociology...' (correspondence presented to the House of Commons on the 14th April 1840).

⁴ 'For the purpose of obtaining reliable data as to the social and economic condition of industry throughout the colony, Government are requested to take steps as soon as possible for the establishment of Bureaux of Statistics in this colony' (AJHR, 1890, H-5, paragraph 29, p. vi).

⁵ The report highlights the French roots of the survey and subsequent systematisation by Le Play and Ducpétiaux, and further activities within Germany and Great Britain.

⁶ Full title 'The Commission to Inquire into and Report on Certain Matters Relating to the System of Education in New Zealand' (AJHR, 1912, E).

⁷ 'Inquiry into Cost of Living in New Zealand 1910-11' was authored by J.W. Collins, who as Editor of the Journal of the Department of Labour also published volumes of national and comparative data. His study drew on 69 family expenditure budgets from across the four major cities during October 1910-11. The Commission also took evidence from a small number of University academics from mathematics and economics. The early role of economists with the Royal Commissions is traced in Fleming (1989). See also Belshaw (1936) for the use of Commissions during the late 1920s.

⁸ In commenting on policy making in New Zealand, Lang (1951) notes how powerful individuals such as Tregear blurred the distinction between policy making and administration.

⁹ Cf. Turbott (1940, 1984). Also, Turbott, H.B. (1929) AJHR, H-31, p. 23; Turbott, H.B. (1930) AHJR, H-31, pp. 89-90; Turbott, H.B. (1931) AJHR, H-31, pp. 17-8, pp. 36-8. Cited in Dow (1999).

¹⁰ Individuals include Elizabeth Gregory, Elizabeth Wilson, Muriel Bell, E.G. McLaughlin and W.E. Henley.

¹¹ See also 'General Committee Minutes of Social Science Research Bureau' (SSRB, 9/59, Box 17, National Archives).

¹² A report by Kolb to the DSIR Minister Sullivan dated 3 June 1938, notes confusion around the Bureau's function (SSRB, W3432, 6/15, Box 11, National Archives).

¹³ It is noteworthy that discussion during the 1940s does not attribute the Bureau's demise to Machiavellian motives (see for example Jacoby, 1949, p. 331). The conspiracy theory appears to have more recent origins.

¹⁴ It is important to not overemphasise Shelley's role in educational research. C.E. Beeby's comments on Shelley suggest:

Nor did he make much effort to see New Zealand schools for himself. For his criticism he relied on his English experience and on the accounts brought in by his students. He rarely visited a school except to make a prize-giving address or to read poetry to a sixth form (1992, p. 48).

¹⁵ 'Industrial psychology sets out to make a scientific study of matters connected with welfare and efficiency of the human being in industry' (AJHR 1944, H-34, p. 14).

¹⁶ Kerry Howe (1991) suggests New Zealand has two substantive socio-intellectual traditions: interpreting Maori/Polynesian culture and administering the paternal state (p. 203).

¹⁷ Reviewing the *Transactions and Proceedings of the New Zealand Institute and the Royal Society of New Zealand* from 1869 would suggest: Adams, C.E. (1897) 'An Investigation in rates of mortality in New Zealand during 1881-91'; Newman, A. K. (1882) 'A study of Causes Leading to the Extinction of Maori'; (1883) 'Is New Zealand a Healthy country? An Inquiry with Statistics'; Segar, W. (1901) 'Population of New Zealand, Comparison of the Age Distribution of the Population of the Four Chief Provincial Districts of New Zealand' and his 'Statistics of Insanity, Cancer and Phthisis in New Zealand' (1909).

¹⁸ See Newman, A.K (1886) 'Study of the causes leading to the extinction of Maori'; Walsh, P. (1908) 'The Passing of the Maori: Inquiry into the principal causes of the decay of the race'. *Transactions & Proceedings*.

¹⁹ Response to the questionnaire distributed by the UNZ Chancellor David Smith around the role of the University and research (David Smith Collection, Letter dated 28 July 1945, Alexander Turnbull Library).

²⁰ (SSRB, W 3432, 9198, Box 17, National Archives).

²¹ Taken from a review of Masters thesis within the UNZ (1905–1948). Although early social science doctorates were rare, those that were submitted pre-1945 were addressed at Maori issues (including studies by Turbott; Beaglehole; Skinner, McQueen and Duff)(National Archives).

²² The Carnegie foundation was established by Andrew Carnegie, a steel magnate who in 1911 gave US\$100 million to establish the Carnegie Corporation of New York. The NZCER was given a grant of \$17,500 for each of five years, extended for a further five years in 1939 (see *Dominion*, 20 May 1939). The NZCER was given statutory recognition under the NZCER Act 1945 and Carnegie funding was replaced by State support.

²³ Of £9, 300 distributed in 1954/5, Victoria received 45% and Auckland 31%. Of 22 projects, seven were at Auckland, nine at Victoria, four at Canterbury, and two at Otago (UNZ, Senate Minutes, 1955, National Archives).

²⁴ Social Science Research Committee 10th October 1958 (UNZ H 2-1, Minutes, 1958, National Archives).

²⁵ Thinking particularly of the sociologically influenced social survey, psychological measurement techniques, and anthropologically influenced community studies.

²⁶ Reports of the Joint Committee of Youth Offenders included: 'Delinquent Generations in New Zealand' (1963); 'A Study Comparing Maori and non-Maori Appearing in the Children's Court in 1960' (1963); 'A Study of Vandalism' (1963); and a project around shoplifting (1958) (CSW, 20.1, 20.2, National Archives).

²⁷ By 1968 the Child Welfare Division within Education had undertaken some 40 projects (e.g. a follow-up study of residents of Boy's Homes and Special Schools; a follow up of referral and first offenders; an evaluation of home status placements. Its staff was small – one senior and two research officers, one of whom was shared with an inter-departmental Joint Committee on Youth Offending (National Archives, CSW, 8/10/33 Submission of Child Welfare Division to NRAC, 1970).

²⁸ Health stood apart (independent funding for medical research had existed since 1951), and from the 1960s a research unit within the Department of Health explored funding formulas.

²⁹ National Development Conference Recommendation 617 'That the National Research Advisory Council report on the adequacy of social science research services especially in respect of defining the problems concerned with the recruitment and retention of a skilled labour force.'

³⁰ Even outside the social policy arena, agencies – including the New Zealand Forest Service, Ministry of Agriculture and Fisheries – were attempting to evaluate the social effects of their programmes (NZ Planning Council, 1982, p. 22).

Chapter Three

The Emergence of Social Science: Explanation within positivist and anti-positivist frameworks

Introduction

The broad intent of the thesis is to explore the emergence of a tradition of social science research in New Zealand by 1984. The previous chapter brought into sharp relief the contrast between the 19th century and the situation by 1984. On the one hand, the early picture highlighted a dearth of social science research and how its activities were confined to particular locations – notably central government statistical functions and the NZCER. By 1984 there had been exponential growth in the amount of research activity, the emergence of particular organisational forms, and fluctuations in the perceptions of what social research could achieve. These shifts are summarised below.

Scale of activity: The situation in the early 20th century was one of relatively little social science research activity being undertaken. The University tradition was one of introspection. Within the State, the dominant thread was policy action as opposed to research reflection. By 1984 the volume of activity had intensified and extended across both State and University. The number of individuals employed within the social science sector and projects devoted to empirical work grew, and levels of funding increased. There were attempts to introduce more coherent organisation of social science within the State, and vigorous debates about social research and social sciences' role in contributing to policy and knowledge of society.

Organisational response of the public sector: At the beginning of the period there was no explicit social science research role within the public sector, and relatively little consideration of whether or how social science research could contribute to decision making. By the close of the period there existed distinct social research and social science sections within individual State Departments and specialist roles within the public service. Contracting relationships between Departments and external providers were emerging (notably, Universities were providing both programme/policy-focused work and blue-skies

type activity). Discussion centred on how the State could best encourage these developments.

Organisational response of the University: At the commencement of the account, the University of New Zealand had, at best, an embryonic social science interest with few dedicated staff, an underdeveloped curriculum, and little research activity. By 1984 empirical social science was embedded as a tradition of scholarship. Within the University *system* there was the growth of institutions, a burgeoning of social science departments, a greater number of staff appointments, and a willingness to look for State funding to bolster research ambition.

Disciplinary developments: It is difficult to identify distinct branches of social science inquiry in the form of academic disciplines at the beginning of the period. Within the University system, the social sciences were a relatively monolithic group of disciplinary interests. Over time, however, social science became increasingly fragmented and specialised terrains emerged or were demarcated within the academic system: anthropology, sociology, social policy, social work, psychology, politics, and public administration.

Professionalisation of social science research capacity: The early 'research' role that developed within and outside the State was a non-specialist, amateur role. Individuals occupied multiple positions including those of administrator and policy adviser. By the end of the period, 'research' was identifiable as a specialist role within the public service, and a core role for University social scientists.

These five changes map the parameters of social science development. The table below summarises the changes that occurred within social science research up to 1984. Such a mapping highlights two interrelated developments: the growth of particular intellectual/cultural ideas; and the institutionalisation of these ideas, procedures and practices within the context of disciplines, Universities, and the public sector. It is these twin considerations that are the object of the explanatory account.

Table 3.1: Dimensions of change 1920 to 1984

Dimension of change	Situation in 1920	Situation by 1984: activity of Government and University
Scale of activity	relatively little empirical social science activity	increased volume of research
Organisational response (public sector)	little public service capacity; little incorporation of research process into policy system	distinct public service role and research capacity within Departments; desire to incorporate research into decision making
Organisational response (University system)	virtually no financial support, activity or interest in empirical dimensions	considerable activity, interest in empirical activity within University system; debate around funding
Disciplinary development	social science disciplines as relatively unformed/unitary	social science disciplines somewhat variegated – moves to Departmental research programmes
Professionalisation of research capacity	embryonic research role with amateur status (both inside/outside Government)	development of ‘researchers’ and dimensions of professionalisation within Government and University

‘Reportage’

The basic question arises: how can we best *explain* or *understand* such developments? In Chapter Two the material was presented as ‘reportage’, to use a term coined by Runciman (1983) who distinguishes among primary understanding (reportage), secondary understanding (a concern for causes), tertiary understanding (focus on understanding participants’ interpretations) and the notion of evaluation. This fourfold distinction of *what, why, what like, how good/bad* he sees as applicable to making sense of actions and institutions.

Reportage¹ is concerned with getting the story out, with the aim of avoiding pre-emption of explanation; i.e. to present a story that would not be likely to be called into question by a rival observer as an inaccurate portrayal of context (Runciman, 1983, p. 60, p. 95). Pivotal within Runciman’s account of ‘reportage’ are ideas of the ‘rival observer’ and the ‘recording angel’. The latter is a hypothetical recorder of all actions who has access to the intentions of agents. The angel observes and catalogues all events. (A similar capacity to list all events appears in Walter Benjamin’s notion of the ‘Chronicler’ ([1955] 1968,

p. 254)). As Runciman writes:

Reportage is the stage at which the facts are stated, the observation listed or the story told. It cannot be the first stage of all, either in logic or in practice, once it is acknowledged that 'facts' can never be defined independently of presupposition imposed on their observations by even the most clear-eyed and least prejudiced observer (p. 42).

Whilst recognising the conceptual and practical difficulties of such a task, the reportage of the previous chapter was undertaken in part to alert the reader to the substantive issues that lie at the centre of this socio-historical inquiry. For Runciman, reportage is a prelude to the other dimensions of his framework – the start of the journey rather than the terminus:

... in social science, there is no purpose to be served by sequences of reports apart from the contribution which they make to the explanation and/or description and/or evaluation of some sequences of human behaviour (p. 102).

We should similarly dismiss any crude Baconianism that has the researcher as chronicler ferreting for 'facts', which are subsequently ordered and offered *as* an explanatory or interpretive account. Thus there is an orientation to the tasks that plausibly lie beyond, rather than as synonymous with, reportage – explanation and understanding, however conceived. There is an expanse of ground to cover before we arrive at this 'destination explanation', but what is explicitly signalled here is that any explanatory account must venture beyond 'reportage'. A second underlying theme of discussion concerns whether the emergence of social science research is best approached by traditional historical investigation, or whether it lends itself to a sociological mode of inquiry. Should narratives and a tendency to frame *what*-type questions be emphasised? Alternatively, is it fruitful to pursue a more sociological interest in explanation, perhaps with causal analysis? Do narratives preclude causal accounts and can thick narrative description alone be the sole carrier of explanatory adequacy? How these questions are subsequently addressed will condition how or whether an explanatory account is produced. The onus is to provide a considered defence of any theoretical resources that are marshalled in allowing the account to progress.

Exploring the emergence of social science is a daunting task – perhaps introducing dense literatures drawn from cross-national contexts, a range of methodological perspectives, and multiple institutional ambits. Writers have surveyed the scene and cut paths through the hardy terrain of social science development. Their coverage extends to:

the growth of the university system (Kerr, 1963; Halsey and Trow, 1971; Geiger, 1984; Bulmer, 1987a; Rothblatt and Wittröck, 1993; Wittröck, 1993; Soffer, 1994);

the development of academic disciplines (Campbell, 1969; Oberschall, 1972a & b; Abrams, 1981; Soffer, 1982; Bulmer, 1985; Manicas, 1987, 1991; Becher, 1989; Platt, 1991; Wagner et al., 1991);

the growth of government social science capacity (Cole, 1972; Bulmer 1987b & c; Blume, 1987; Skocpol, 1987; Walker, 1987; Nowotny, 1991; Davidson, 1991; Deleon, 1991);

the utilisation of social science within policy advice (Weiss, 1977, 1986; Berger, 1980; Bulmer, 1986abcd & e; Bulmer, 1987b & c; Cornish and Clarke, 1987; Thomas, 1987; Smith, 1987).

It is also helpful to identify the domestic literature. It addresses questions of University growth, disciplinary development and Government social science capacity within New Zealand (Beaglehole, 1949; Morrell, 1969; McCreary 1971a & b; Wood, 1975; Beeby, 1979; Parton, 1979; Ritchie, 1979; Condliffe, 1981; Sinclair, 1983; Beeby, 1992; Robb, 1996; Thompson, 1996; Barrowman, 1999).

Despite the substantive richness of these contributions across a range of institutional spheres, there is less direction and guidance in gaining theoretical orientations for the task ahead. In the first instance, documenting and explaining change are key tasks for history and sociology, and it is these forms of inquiry that offer signposts and well-trodden tracks for discussion.

Since the 1970s one development has been the challenge to dichotomising historical method as primarily storytelling centred on events, and sociology as quantitatively focussed investigation. There is the rise of social science history (exemplified in New

Zealand with Miles Fairburn's falsificationist approach to history (cf. 1989; 1999)), and historical sociology's wish to theorise change over time (cf. Stedman Jones, 1976; Tilly, 1981; Skocpol, 1984a & b; Giddens, 1984; Archer, 1995; McDonald, 1996a & b; Sewell, 1996; Somers, 1998; Calhoun, 1998; Abbott, 2001). The thesis is sympathetic to the view that both sociology and history and their engagement with change, stasis and time provide the conceptual resources upon which a viable historical sociology must draw. The challenge is to determine how these resources are fused, enmeshed and related.

A constituent framework must incorporate detailed historical and institutional accounts. To fully account for the growth and expansion of social science research it must examine sites of knowledge creation and transmission, and the various relationships between these sites: the University, State agencies, as well as developments within the broader social and cultural fabric. Before expanding upon this view, it must be established that there is a theoretical case to answer – namely that reportage is inadequate – and, assuming such a case can be made, we must then specify how explanation is best achieved.

In Search of an Explanation

At the core of disciplines of sociology and history lie explicit, and frequently implicit, assumptions as to the *sorts* of explanation they are able to offer, and the *power* of these explanatory statements. The notion of explanation introduces a plethora of terms that are at once philosophical, scientific and technical and, herein, perhaps lies the dilemma. Their labelling as such enables sociological and historical enterprises frequently to evade epistemological and ontological debates, pushing ahead with the 'real' business of finding out about society and history as the present and the past respectively. Explanation soon becomes taken for granted, and the terms with which it associates – clarify, describe, prove, demonstrate, elucidate, illustrate, expound, cause – gather their own momentum as they become integrated into common-sense portrayals of the business of the disciplines. As some historical methodologists note (Lloyd, 1986, 1993; Fairburn, 1999) historians' frameworks frequently remain unacceptably tacit. This may be hidden within encyclopaedism where layers of 'fact' are heaped onto the methodological platter, where the devil is seen as residing in the detail, and little detail is spared as the text groans under the weight of endnotes. Under an encyclopaedic view, an account of social science development must necessarily contain details of sites of knowledge production – the four

University Colleges, each individual academic Department, Governmental departments, external bodies and organisations such as Learned Societies, Fabian Societies and trades unions. Adopting this 'paradigm' has the implication that historical sociology becomes concerned with temporality solely to demarcate two points in time within which to situate facts. Analysis therefore becomes akin to some mammoth filing cabinet in which these facts are ordered and sorted, giving not so much a *historical* sociology as a *retrospective actology*, by which is meant a focus on surface events *in time*, rather than aetiology *through time*. Abrams identifies the historian's tendency to 'bury[ing] the principles of explanation that underpin their work beneath the rhetoric of a story' (1982, p. 196) rather than making methodology explicit. But a theoretically grounded notion of what explanation is and how it should best proceed deserves fuller attention. For this thesis, explanation lies at the heart of sociology, and explanatory success is the pulse that signals continuing life. Given the centrality (and the magnitude) of this claim, the task of ensuing chapters is to clarify the possibility of explanation within the human sciences, to explore the status of human sciences' claims, and to identify an explanatory process capable of delivering upon such promises.

The term 'explanation' retains a particular plasticity, a malleability whereby its (alleged) properties side-step more complicated issues. A hypothetical example may illustrate the numerous ways 'explanation' is used. If one returns home one summer evening, having consumed a large part of a bottle of vintage malt whisky, it comes as no surprise to be asked to 'give an explanation'. The focus of this explanation would be the causes contributing to my present state of unsteadiness, an overall euphoria, perhaps a desire to sing, dance, and reminisce; or alternatively of moroseness, tetchiness, even drifting into deep slumber. If required to provide an 'explanation', my first response could be that the liquid imbibed contained alcohol, which has been metabolised, resulting in a number of physiological changes.

As part of an introductory lecture to biology or chemistry this response would be suitable. But for the context in which an explanation is being required (nay – demanded) the response will simply not do. Neither will appending further details of the metabolising process – alcohol dehydrogenase within the liver, the conversion of acetaldehyde to acetate by aldehyde dehydrogenases and so on – assist clarification. Regardless of how precisely

the terms are presented, how deeply mechanisms are delved into, or what supporting evidence is introduced to bolster the 'explanation', the track is simply irrelevant in this particular instance. Neither molecular biology nor the *Lancet* will help here. Invoking laws of physiology will not suffice because a different sort of explanation is demanded; one that draws on different sorts of information. At this point a reader's retort may be to agree that explanation is defined in very different ways – with the previous example simply illustrating the extreme elasticity of the concept 'explanation'. This, at least in part, is undoubtedly true. But the problem for sociologists and historians is that this looseness of usage goes unrecognised, and the term 'explanation' is deployed as if it were uncomplicated and uncontested.²

A second line of explanation may be developed, drawing upon social, as opposed to physiological/biological, considerations. This social reasoning could itself take a number of forms. So a riposte to the request for an explanation might be to list the establishments visited, provide details of particular brands held at licensed establishments, account for who was present within these locations, and so on. Such detail enters into the terrain of 'reportage' – but the issue is that irrespective of how detailed this account becomes, or how accurately the route is recounted, it is doubtful that this would be acceptable as a satisfactory explanation (cf. Runciman, 1983, p. 149). Filling out the context is not what is required; neither is the questioner assuaged by simply verifying the chain of events (although this may be a necessary albeit not sufficient condition for a more robust version of explanation). Quite simply, the answer does not lie in reportage becoming *verbiage*.

A behaviourist view would see all behaviours as a series of responses to stimuli provided by the environment, and interaction with that environment. Responses are thus triggered by occurrences in the environment that are empirical and readily observable but which ignore meaning. While relaxing the stimulus-response interpretation allows dispositions to be attributed to individuals, and these dispositions to be subsequently acted upon by the external events, it still results in a problematic view of social causation.

Discussion so far has focused on physical and external processes. Beyond physical processes are internal/mental reasons as opposed to mechanisms that are purely physical (i.e. 'I chose' or 'I wanted to continue to consume', rather than 'my arm lifted the glass to

my mouth thereby beginning the metabolising process'). Thus, motives, goals, and rationales would be central to this sort of explanation, rather than a series of behavioural responses on the body or our dispositions. Action is distanced from behaviour because the former is seen as intentionally rather than externally caused. This points to questions of *how* and *why*, involving internal states, with intentional agents having to choose in varieties of circumstances and where contingencies rather than regularities are of interest (cf. Hall, 1999, p. 161). Drawing upon these explanatory resources discounts explanations of the chain of events as things that 'just happened'.³ Instead there is a belief that some pre-existing rationale could be uncovered which led to the physical effect of imbibing, and ultimately, inebriation. So, this could be pitched in terms of celebrating a friend's significant birthday, or to alleviate a stressful day. Finally, an explanatory line of defence could be one that focuses on the pressures to consume alcohol that result from societies and social classes, including workplace hierarchy, comradeship and masculinity. Thus occurrences were to some considerable extent beyond my own choice

The (rather hackneyed) point being made is that explanation is a slippery concept. In everyday life we may allow this slipperiness to be used to our advantage (although the price here would be one of domestic dishonesty). Explanation must not be used in such a slippery manner in our intellectual pursuits, and *qua* historical sociologists it is central to grasp and elucidate what is meant by explanation. Avoidance or unbridled pragmatism are intellectually dishonest options. The centrality of explanation to history and sociology and the 'essentially contested' nature of such a concept are recognised. What is starkly apparent is that a division of labour frequently emerges *within* as well as *across* the various social science disciplines. While philosophers and methodologists discuss such issues and argue for particular distinctions and approaches, practising social scientists push ahead with either fine-grained or macroscopic endeavours. It is not difficult for the hairline fracture between theoreticians and practitioners of various disciplines (including history and sociology) to become a yawning gap under institutional and disciplinary pressures (such as the push to specialisation, specialised journals and more focused teaching responsibilities).

My own intention is to avoid such chasms, drawing from both theoretical and empirical sources in developing an account of social science research development. A first task is to

clarify the various approaches taken to explanation within the social sciences. Sound arguments must be advanced to *support* any particular perspective chosen, avoiding methodology emerging by elimination or 'default setting'. As Lloyd (1986) suggests, philosophy should attempt to suggest the preferred structure of explanation – having a dual descriptive/prescriptive role (p. 26).⁴ Already my theoretical colours are being fashioned and later incursions into rival territory will require them to be fully hoisted. First explanatory frameworks associated with positivism and anti-positivism are discussed. In examining explanation as traditionally conceived within the domain of natural science, notions of cause, law, and generalisation are despatched. The question then addressed is whether social science explanation must be exchanged for understanding.

Explanation in Science: The covering law

Social science has historically taken its lead from the natural sciences, with advancement in the latter setting the gold standard for social science to emulate. Within the broad sweep of the Enlightenment, natural science was seen as having power and explanatory potential, allowing greater access to underlying physical mechanisms and underpinning the increasing mastery of them. Within natural science the 'covering law theory' was presented as the key to scientific explanation. In terms of this theory, any full explanation must contain the statement of a law. The covering law theory:

... requires that every full explanation include essentially at least one law or lawlike generalization, whose role in the explanation is to 'cover' the particular event being explained (Ruben, 1990, p. 97).

This Humean consideration sees a particular event as being explained when it is brought under the generalisation of a law whereby:

It becomes an instance of a general rule stating that, given the presence of certain conditions, events similar to the one to be explained will occur (Gardiner, 1952, p. 1).

Humean constant conjunctions are presented as the solution to the problem of explanation. But fuller scientific explanation may wish to emphasise the 'because' component of any explanatory sentence (given X then Y, *because...*). Thus a particular case must satisfy a causal law for explanation to occur but goes beyond mere correlation between X and Y. Rather, satisfactory explanation involves addressing the 'why' question – which may lead to a higher generalisation being made that explains a sub-law within a system (e.g. we may

understand how Kepler's predictions are seen to be *explained* from within Newtonian physics).

In the covering law interpretation, explanation in natural science may occur deductively or via inductive probability. Carl Hempel ([1962] 1993) identifies two covering law approaches to scientific explanation: the *deductive-nomological*, and the *inductive-statistical*. More simply put, these are akin to reading off 'events'/'instances' from a master compendium of laws which cover every case (the deductive trait), or adding to the compendium given the re-occurrence of particular 'events' and instances and attaching probabilities to these events (the statistical probabilistic version). In terms of the covering law interpretation, any 'full' explanation must include a law, defined as something true, quantified, and generalisable. The emphasis is therefore upon explaining external entities and mechanisms, and is unrelated to personal opinions. While there is some recognition that natural science itself inevitably has some social dimensions these are not seen to contaminate the knowledge that is produced. Under this interpretation, covering laws and our knowledge of them can be insulated from, and remain untainted by, the processes of how scientists go about uncovering them. Nature is seen to be independent from the ideas we hold about nature.

The covering law approach of explanation has a close, often overlapping, relationship with prediction, providing a symmetry that allows us to:

deduce[s] from a given formula taken in conjunction with certain data an infinitely wide range of values (Gardiner, 1952, p. 21).

Under this view, natural science searches for constant conjunctions and these are seen as exhausted by atomistic events. Scientists passively receive constant conjunctions and these are imprinted upon their sensory apparatus. The unity of scientific method suggests that the logical properties of what constitutes an explanation are constant throughout natural and social science. This assumption is J.S. Mill's perspective in *A System of Logic*, and the position of Carl Hempel (although their ontological and epistemological premises, and therefore their understandings of what this unity consists of, differ). This assumption is, of course, also the caricatured positivist position that late 20th- century social scientists railed against, and the one that many continue to pillory.

In terms of covering law explanations, an iron-cast historical materialism could arguably fit such a mould, with modes of production completely determining the direction of subsequent economic and political development. Gaining access to the particular laws of production would enable us at once to describe, explain and predict. Under this interpretation, individual actors are absent from the stage except when they 'act' as mannequins channelling the underlying forces as individuals become mere bearers of these modes.⁵ Much of Popperian social philosophy is levelled against the attempts of 'historicists' to introduce covering law notions into social science (a charge that is also aimed at, amongst others, materialist Marxist scholars). Historicists, Popper argues, assume that social goals and aims are given by the spirit of an age or stage of economic development. Popper's charge is that the historicist fails to understand the distinction between a trend and a law: a trend depends upon initial conditions which are subject to change, thereby changing the trend. He allows the 'logic of situations' where, given a general background of attitudes and beliefs, some paths are more likely to be followed than others. But under his own methodological individualism such trends are always open to reversal and only conditional trends, labelled by Popper as 'ad hoc laws', fit in with his tentative approach to society and science. Sociological 'laws' under this view are either mistaken, or are in fact quite trivial.⁶

Despite the suggestion that explanation is crucial to social science, the usefulness of a covering law approach is already crumbling.⁷ Explanation is not some search for timeless laws that run through the period in question. A difficulty appears to be the different material that physical and social science must 'explain'. One option would be to concede the covering law definition to natural science and devise an alternative better suited to the material of the human sciences. As Weber suggested in *Wirtschaft und Gesellschaft* (1947, p. 99):

We apply the term 'adequacy at the level of meaning' to the subjective interpretation of a coherent course of conduct when and in so far as according to our habitual modes of thought and feeling, its component parts, taken in their mutual relations, are recognized to constitute a typical complex of meaning. It is more common to say 'correct'. The interpretation of a sequence of events will, on the other hand, be *causally* adequate in so far as, according to established generalizations from

experience, there is a probability that it will always occur in the same way
(italics in original).

The debate about the appropriate relationship between the natural and the social sciences continues to stalk the halls of philosophy of (social) science. The social sciences, in dealing with a social world, encounter a 'material' radically different to that of natural sciences (put clearly by Chalmers (1999) as 'rocks do not talk back to you; human beings do'). Given the startling achievements of natural science, there have, since the 19th century, been varying attempts by social science to forge methodological and philosophical alliances with natural science. Yet, the attempt to replicate natural science has:

left us with a problem. After all, human meaning and consciousness are central features of the social world. As such, some reference to the 'inner mental states' of human beings is required in order to understand, as opposed to explain, social relations (Williams and May, 1996, p. 183).

With talk of the diverging subject matter of natural and social sciences, and with the association of causal laws with the former, is social science relegated to a less universal and rigorous branch of science? Does the vision of explanatory social science disappear when grappling with its complex subject matter? Must the social sciences (including sociology/history) eschew their grandiose aims of explanation (let alone laws and generalisations), perhaps settling for descriptions or interpretations of human/social behaviour?⁸ In highlighting differences between the natural material/social science material, it is important to avoid overplaying what is evidently a sociological truism – that the world consists of people.⁹ Although subjective meaning is a constituent feature of human behaviour, it is less clear what the implications of this are for the logic of explanation (Runciman, 1983, p. 15).

Does the work of the human sciences, including history and sociology, result in a different kind of knowledge from that gained by studying the natural sciences? A range of arguments has typically distinguished history from natural science – history is in the past and cannot be known outside experience; history is unique and unclassifiable (although clearly uniqueness describes only some features, not others); historical events have

richness and complexity; and history concerns action, statements and thoughts rather than dead matter. Yet Gardiner (1952) argues:

‘Thoughts’ and ‘plans’ are not substitutes for ‘causes’ which operate on a different sphere of existence... The two forms of explanation are different and it is a mistake to try and conflate them (p. 51).

Do dual notions of explanation result within human and natural spheres? A strong thread of hermeneutic theorising of human action that traces back to Dilthey makes *understanding* the aim of human sciences, leaving natural sciences to concern themselves with causal explanation. As Delanty (1997) writes:

The hermeneutic task is to reconstruct the relationship of individual units of meaning to a meaningful context. To do this, observation is not enough since meaning cannot be subject to simple explanation and description, which characterizes positivism (pp. 42–3).

Anti-naturalists including Winch, Louch, Taylor, and Searle rule out the possibility of causal explanation, emphasising instead the place of *meaning*, *significance* and *intentionality*. The separation becomes clear: explanation is about causes and observation; understanding about meaning and interpretation – i.e. whether we understand the world from *within*, or explain the world from *without* (cf. Hollis, 1994). Explanation becomes *de jure* the preserve of natural science, while the hermeneutic lineage *prizes* thick descriptive accounts and interpretation over a thinner analytical explanation.

The Role of Social Theory

Positions of positivism and anti-positivism signal diverging roles for social theory. These are captured in the categorisation of theory’s overlapping roles: knowledge/truth, affective, reflective, and normative (Craib, 1992a).

Knowledge/truth: The cognitive dimension presents social theory as a precursor to attaining knowledge and truth – the search for an essence of how things *are* or *warranted claims* of what they might be. This desire may be stated in numerous ways – as the pursuit of knowledge, objectivity, Truth or truth. Despite its timeless quality, the search to unearth the philosophers’ stone of unitary truth has seemingly revealed only the weevily soil of contested truth claims. The search for truth as a unitary, incontestable product or outcome

has increasingly led to the recognition of spatial and temporal contingency. The relationship of the social/scientist to the physical and social worlds is mediated by human and cultural practices thereby always presenting a filtered perspective. There is a growing consensus that scientific – and with it any possibility of social – knowledge is a culturally, spatially and temporally located practice (cf. Kuhn, 1962; Bhaskar, 1975, 1979; Feyerabend, 1975; Mackenzie, 1981; Somers, 1996, 1998).

The precise implication of these positions for social science activity is less clear. Does an awareness of historical and geographical location result in an inability to make judgements amongst a raft of knowledge claims, allowing each claim (or game) to be as valid as the next? The impact of spatial/temporal location on the search for knowledge encourages a greater focus on process and transparency – clarifying assumptions, conceptual apparatus, and arguments that make sets of knowledge claims ‘claimable’ and an emphasis on fallibility instead of on dogmatism. In part, these distinctions are encompassed within the catch-all cry for ‘reflexivity’. How this reflexivity is to be weighed and the specific implication for social science is a recurring theme of discussion.

Utility: theorising may be given a practical function or usefulness. Theory is seen to produce some instrumental outcomes, being used ‘for something’, rather than to explain something existing in the world. Instrumentalism frequently presents its ambitions as those of working unit theory – with little requirement to delve further into the broader presuppositional framework. Adams (1989) points to this distinction between knowledge/utility when he suggests the questions ‘is it true?’ and ‘does it work?’ are the distinction between theoretical knowledge and practice knowledge:

The point of scientific theory is to state a truth whose usefulness is neither here nor there; and the point of a technical theory is utility, over which questions of truth and falsity do not arise because their function is not to explain anything but to guide our action (p. 35).

One implication of an instrumental slant on theory is the concern with outputs, allowing right or useful answers to be reached but for wrong reasons, or for reasons of which we are not aware (cf. Sayer, 1992).

Security: Previous discussion suggests theory has external or instrumental use, either producing something (knowledge), or allowing the achievement of aims (application). But theory and theorising may also be viewed from a more psychological perspective of meeting individual and group needs. Seen in this light, theory has intensely personal facets as well as public dimensions. Theory may provide both ontological security and the opportunity to project our own experiences. Gouldner (1970), for example, writes:

Social theorizing, then, is often a search for the meaning of the personally real, that which is already assumed to be known through personal experience. Basing itself on the imputed reality of the ordinarily experienced, much of theory work begins within an effort to make sense of one's experiences (p. 484).

Thus theorising is a strategy that allows individuals to position themselves in the world, contributing to security via explanation, understanding, prediction, and, perhaps, control of the social world.

Normative: Theory may also fulfil a more normative role, reflecting implicit assumptions about how the world *ought* to be. In this way, theory becomes the 'carrier' of description of how the world is, and prescriptions of how it should be. These positive/normative notions are frequently fused – allowing theory to posit a world that both is and should be (cf. Adams, 1989; Croissant, 1998). Given that normative considerations are value considerations, how and where do values enter the theorising process and how are values recognised and subject to reflection and revision? There is a tendency for ontological 'descriptions' to have implications downward for theories (and prescriptive consequences). Descriptions soon become unit theory prescriptions, and vice versa.

These roles are helpful in contemplating the shape of particular theoretical traditions. What shape do these positivist/empiricist and anti-positivist alignments give social theory? During the later 20th century, positivism became a pejorative term and as a caricatured position continues to provide ammunition for critics. Key positivistic notions included: facts given directly to careful, unprejudiced observations via the senses; facts as prior to and independent of theory; facts as constituting a firm and reliable basis for scientific knowledge; and real science as empirical science promising cumulative knowledge

(Alexander, 1982; Chalmers, 1999). This 'received view' of positivism routed philosophy – science and the accumulation of facts told us all that we needed to know (Munz, 1984, p. 3) and for such an empiricism the first rule of philosophy is that there is no philosophy (Nola and Sankey, 2000). The deep streams of positivism that flowed through 19th- century social science continued to be drawn up into the 20th century. Skinnerian behaviouralism saw the objective of (social) science as the discovery of universal laws of human behaviour, prizing the observed and avoiding reference to inferred 'inner-man' (1948, 1971). For the logical positivism of Neurath (1973) and Carnap (1966), what could be known to the senses became the criteria of meaningfulness, the physicalist approach to society suggesting that:

In a strictly scientific sociology one can describe only the behaviour of men at a certain time, their habits, their ways of life, their processes of production, etc., in order then to raise the question of how new habits arise by the joint effect of these observed habits and other circumstances. There are Protestants but there is no Protestantism (Neurath, 1973, p. 358).

Positivism also irrigated much of post-war social theory, including varieties of logical empiricism within individualistic explanation, as well as flowing deep into the roots of structural and materialist functionalism that flourished during this period. It contributed to the strongly quantitative tradition that flowered throughout social science, an investigative influence that spread from Faculty to Government Department as State intervention blossomed. The positivist belief of replicating natural science in pursuit of social knowledge suggests a foundational starting point that may be visualised as strong non-contestable foundations producing knowledge about the social world. In this view, the heuristic shape of theorising is a 'truth triangle' with immovable foundations that legitimate epistemological and methodological stances, allowing substantive knowledge to be crowned. Within the walls of the triangle lie the epistemological resources that allow truth to be discovered or uncovered. The base contains fundamental commitments and aligns these – via epistemological imperatives of what counts as evidence – to the process of finding facts and laws. Levels of theoretical activity are interlaced, with broader assumptions having implications 'upwards' for unit level theories, and conversely unit level theories have implications 'downwards' in their implicit ontological and epistemological statements. These upward and downward flows, 'couple' levels of

theorising – limiting the theoretical assumptions and outcomes that may be fastened at upper and lower levels. Positivistic social science upholds the existence of one truth triangle containing epistemological resources and methodologies to approximate social knowledge. Moreover, the triangle does not reflect nor refract; rather its perspicuous qualities provide a ‘window’ to knowledge via sensory perception. The ‘truth triangle’ provides a view of all aspects of the world that we can reliably know.¹⁰

The truth triangle presents the relationship of science, knowledge and the world as fixity, plane and solid foundation. The posited truth triangle allows unobstructed viewing – essentially ontology and epistemology become conflated – the window of epistemology is so clear as to allow direct access to knowledge. To expand the metaphor, the window – being so fully open – literally disappears from view. But if science and social science do not provide such a ‘window’ and direct access to knowledge, then the metaphor requires re-adjustment. In re-adjusting it, a wedge is driven between epistemology and ontology, for far from being translucent, the window is in fact coloured by values and commitments diffusing from foundations – captured perfectly in Wagner’s (1992) comment, ‘All knowledge is filtered through the lens of a perspective. We cannot see without it’ (p. 210).

The latter argument results in the view that the world is opalescent; that, at best, knowledge is accumulated through a glass darkly. If knowledge is filtered prior to reaching us, attention is re-focused upon issues of epistemology. But we must not allow the (relatively uncontentious) acknowledgment that all knowledge is filtered to simply dissolve all matters ontological (cf. Archer, 2000a). Recognising epistemological authority does not necessarily result in dismissing the reality of an existing external world.

Reacting to the positivist hegemony of the post-War period and collecting behind the standard bearer of contextualism, critiques drew upon a pre-existing human science perspective within social thought (cf. McLennan, 1995, 2001). Reflecting a series of hermeneutic, Wittgensteinian, and phenomenological interests, they re-emphasised the subjective, local and micro perspective’s, moving away from aspirations of generalised knowledge.¹¹ A family of positions – perspectivist, conventionalist, constructivist, and contextualist – challenged the positivist image of knowing and being. These attacks

excavated the foundations of the truth triangle, undermining its claims to scientific knowledge, and denying the possibility of universal knowledge. In its place understandings were advanced that allowed a number of self-referential systems, with internal assumptions and values supporting truth claims. The result was a relativist drift – multiple ‘truth triangles’ on the sea of relativism. Each triangle or belief system acts as an orienting system and the triangle becomes a particular prism that *colours* how we see the world (e.g. perspectivist); or truth is created *within* the walls of the triangle (e.g. conventionalist). Other anti-positivist’s sought to pull down the shutters on viewing the world, inverting our gaze from any external referent, while others opened up multiple windows on the world, each as subjective and valid as the next. In juxtaposing these positivist and anti-positivist positions the ‘epistemological dilemma’ is encountered whereby:

Either knowledge of the world is unrelated to the social position and intellectual interests of the knower, in which case general theory and universal knowledge are viable, or knowledge is affected by its relation to the knower, in which case relativistic and particularistic knowledge can be the only result (Alexander, 1992, p. 323).

What is in dispute here is the precise nature of reflexivity. Positivism denies reflexivity any place within social science – the researcher becomes concerned with the application of logical rules, and with adding to the stocks of established knowledge. There are, however, versions of reflexivity which acknowledge the shaping of knowledge by its relation to the knower and also avoid relativistic and particularistic knowledge. In ruling out an Archimedean standpoint, and conceding the knower’s values and positions are coloured, social knowledge, criticism and credibility are thereby retained. If each standpoint occupies its own truth space (with its own foundations, assumptions and values) full-blown relativism becomes celebrated, and within this relativist ocean:

truth is not some mysterious, timeless quality, but a social status conferred on familiar ideas, and hence varies from status system to status system, that is, from society to society (Jarvie, 1996, p. 305).

The leap is too quickly made from recognising spatial/temporal contingency to the implication that all views are equally true. This leap embraces the relativistic fallacy and abandons the possibility of judgement and thereby a commitment to social and political

action. The switch occurs from a tenable epistemological dilemma (in effect grasping the importance of our social/historical position and of fallibility) to the epistemological fallacy – that all knowledge is equally viable. Thus whilst the formation, expression and legitimation of our beliefs are in important respects socio-culturally relative, the conclusion is reached whereby:

meaning and validity of claims about the world are also, and in every important respect, culturally or perspectively relative, to the extent that cross-contextual assertions about the nature of the ‘world’, ‘truth’ ‘facts’ and the like are inherently unsustainable (McLennan, 2001, p. 87).

Social theorising has been blighted with the choice of positivism or anti-positivism, but such choice rests upon an erroneous view of the nature of science. Single or multiple ‘triangles’ must be replaced by an approach which incorporates movement and change, offering conditional but ongoing possibilities of social knowledge.

Summary

This chapter has introduced the thorny notion of explanation, presenting a view of explanation as covering law derived from science, as well as criticisms unleashed from a hermeneutic tradition. Both positivist and anti-positivist positions present a stark choice, premised on unsustainable views of science. Positivism’s concern with surface events and regularity does not concur with the recognised practice of natural science and fails to incorporate the place of meaningful action in social life. On the other hand, many anti-positivist positions whilst introducing important hermeneutic insights, offer no viable account of scientific activity and forsake explanation. Both must be surpassed if the possibility of *causal explanation within the human sciences* is to be retained. The chapter that follows takes some tentative steps towards such explanatory intent, exploring a range of positions that have sought to move beyond the positivist/anti-positivist dichotomy.

Chapter Three Notes

¹ Levels of historical explanation are also distinguished by Watkins who cites Walsh's notion of colligation or chronicle, 'explaining an event by tracing its intrinsic relations to other events and locating it in its historical context' ([1952] 1973a, pp. 154–5).

² This amnesia afflicts the range of social sciences. I am mindful of Trigg's (1985) statement that 'Empirical social science must start from a properly articulated philosophical base if it is to be successful. The philosophy of the social sciences cannot be an optional activity for those reluctant to get on with the 'real' empirical work. It is the indispensable starting-point for all social science' (p. 205).

³ Donald Davidson defends a position of the *rationalisation* of action being a species of causal explanation (1996). His argument is that 'wanting' is a genus including all pro-attitudes towards an action. Thus, when asked *why*, 'for no reason' means for no further reason besides action being done intentionally – i.e. wanting to do it. The notion that wanting must enter into every full statement for action is an Aristotelian one.

⁴ Lloyd (1986) provides a useful scorecard for such philosophical exploration, suggesting that philosophy is concerned with *structure, content, coherence, influence* and *effectiveness* of methodologies and explanations. See also Alan Ryan's (1973) questioning of whether philosophy of science is a descriptive or normative concern.

⁵ Note also the parallels here with Tony Lawson's (1997) view of a similar channelling that occurs *vis-à-vis* economic 'theory' which removes choice from individuals.

⁶ His falsificationist scientific method suggests that we aim for 'powerful' and useful theories with wide explanatory capacity (see Chapter Four). Popper is not alone in suggesting that social science generalisations are frequently fairly imprecise and concerned with particular situations than generalisations, i.e. putting emphasis on initial conditions. Gardiner (1952), for example, notes:

... history provides us with no system of precise correlation. Generalizations, in so far as they are enunciated by historians (and this is comparatively rare), are of an essentially loose and 'porous' nature (p. 93).

⁷ However, notions of generalisation and covering laws continue to be influential in empirical research. Covering law notions have also been recentred in recent Rational Choice accounts of explanatory social science (see Kiser and Hechter, 1991).

⁸ The allusion here is to the 19th- century German distinction between *erklären* (causal explanation) and *verstehen* (interpretive understanding).

⁹ This sociological truism has been used to bolster various erroneous positions within sociology, economics and political philosophy. Methodological individualists draw particular inferences from this dubious 'fact'.

¹⁰ The metaphor has parallels with Rorty's metaphor of the 'mirror'. For Rorty (1979), philosophy has traditionally been viewed as the discoverer of truth, with language having the ability to describe or 'mirror' the world. His own position denies essences and objectivity, regarding language as serving a more pragmatic dimension. See also McLennan's distinctions (1995, p. 61).

¹¹ McLennan (1995) refers to this as the 'underside' of the Enlightenment tradition (p. 57).

Chapter Four
Post-Positivist Developments: Method, general
theory and metatheorising

Introduction

The previous chapter offered two paradigms for making sense of social life, and suggested difficulties in the routes they advocate for social investigation. Even though discussion has predominantly confined itself to methodological cover, ontological shapes have begun to press through the methodological fabrics laid upon them. The aim of this chapter is to begin to signal more fully a shift to what has been termed ‘the furniture of the world’ (Bunge, 1977). In the previous chapter, positivism (perhaps more accurately labelled ‘logical empiricism’) presented a view of the world as unitary and external, and society as subject to laws parallel to those of natural science. Anti-positivist stances suggest the world is an idealist *construction* of our activities or *reflection* of various positions. Underpinning the many deficiencies of these two camps is their inability to conceptualise the nature of *being* and *knowing*.

The purpose of this chapter is to explore a third, broad set of post-positivist positions¹ focusing on three particular approaches that develop method, general theory and metatheorising respectively. Post-positivist approaches, whilst acknowledging the insights of naturalism and hermeneutics, defend the possibility of social knowledge. They seek to stem the philosophical solipsism that flows from the spatial and temporal contingency of social life without embracing a moribund empiricism. This chapter explores how these three approaches are ultimately unsuccessful and how criticisms may be bridged.

The ‘Post’ in Post-Positivism

The challenge is to sketch a viable position for socio-historical inquiry that retains any insights provided by positivism and anti-positivism but which addresses their own failings. Debates about the logic, structure and legitimation of social theory would appear to reassert themselves (Alexander, 1982; McLennan, 1996; Nola and Sankey, 2000). The precise nature of this post-positivism or post-empiricism is at issue. Previous chapters

discussed positivism and anti-positivist critiques in their various micro, contextual and perspectivist forms. Alongside these, post-positivism signals best those positions that, while acknowledging a range of contextual critiques, retain some (positivistic) vision of a cumulative, rational and general knowledge. Whilst advancing criticisms of positivism, broad post-positivist positions stop short of untrammelled relativism in what they propose as the aims and value of social inquiry. Of particular interest is how such approaches avoid the strong pulls exerted by positivist and anti-positivist alignments. In staking out the post-positivistic tradition of Popperians and meta-theorists it is shown how they drew strength from challenges levelled against the 'standard' view of science – covering laws, emphasis on observation and facts, and the absence of theory (cf. Holmwood, 1996, p. 41). As Peter Manicas (1987) writes:

Every key tenet in this 'standard view' has been either abandoned, liberalized to the point of triviality, or thoroughly undermined (p. 243).

A revised model of natural science, and by association the anti-positivist image of social science, were driven by a series of historical studies of science displacing the emphasis on logical principles (Lloyd, 1986).

The critique of natural science was wide-ranging and addressed scientific aims, approach, observational context, and logical status. The mission of science as producing covering laws that made explanation and prediction possible was assailed. Such Hempelian notions failed to provide explanatory linkages – instead its approach provided descriptions of correlations that existed between events, and at best 'explanation sketches'. If positivism aimed at the pursuit of general laws, then the failure to elucidate causal mechanisms undermined the explanatory promise of social science (the 'because' dimension). The unwillingness of some approaches that utilise general laws to be challenged via evidence (such as materialist history) led critics to relegate such activities to the realm of non-science. The perceived development of closed practices and systems was the very antithesis of what social scientific activity should promote (Popper, 1945, 1957).

Within natural science the phenomenalist programme of observation was increasingly dethroned (albeit not abandoned); black holes and quarks became constituent parts of scientific knowledge despite their apparent unobservable nature. The positivist programme was irrevocably tarnished. As Gellner (1985) notes, 'They [positivists] revere observable

facts, and these are but the surfaces of things' (p. 5). The stratified view of scientific activity underpinning the influential philosophy of Bhaskar suggested the view of science premised on observables was one-dimensional and erroneous. Many philosophers (including Popper (1960, 1969); Kuhn (1962); Quine (1964, 1969); Hanson (1965); Bhaskar (1975, 1979); and Feyerabend (1975)) categorically rejected observation as a theoretically-neutral endeavour. Rather, theories played a crucial role in shaping our observations and observational activity is reliant on *our* prior theoretical framework. Positivist demarcations of 'the' world and our thoughts about it are replaced by an awareness of the conceptual apparatus utilised in the process of seeing.

Whereas the model of natural science maintained that evidence/facts were able to prove or verify theories, revised orthodoxy presented theory as being radically underdetermined by evidence. The notion of verified theories being added – perhaps once and for all – to stocks of social knowledge was seen as illusory. From one side, Popper's falsificationsism launched an onslaught on verification and corroboration. A second line of attack on the role of observation/evidence in settling scientific disputes emerged from Duhem's and Quine's work whereby observational apparatus may be at fault, and indeterminacy of meaning and translation received fuller philosophical attention. Duhem's (1954) holistic account of theory testing advanced that theories are accepted from part of a network – unlike the positivist assumption that the world is 'grainy' (cf. Gellner, 1985). Similarly, for Quine 'our statements about the external world face the tribunal of sense experience not individually but only as a corporate body' (1964, p. 141). The notion of observation as trumping theory is revised by being reversed; theories may be retained despite them coming into conflict with observational statements, i.e. the test situation may be at fault. As Quine (1964) notes: 'Any statement can be held to be true come what may, if we make drastic enough adjustments elsewhere in the system' (p. xx).² Meaning, explanation and truth are relative to a linguistic and conceptual edifice, or as Quine writes 'Epistemology merges with psychology, as well as linguistics' (1969, pp. 89–90).

Increasingly recognised within the philosophy of science was the metaphysical basis of science (Lloyd, 1986) and that theory, metaphors, analogies and models were key to understanding. From the 1960s and 1970s onwards the extra-scientific basis for knowledge was frequently reduced to social interests, and even unbounded relativism,

within the varying interpretations of Kuhn (1962, 1970), Feyerabend, (1975), Bloor (1976) and Barnes (1977). Thus the things we believe about the social world are shaped not only by evidence but also by interests within professional and broader social settings. The philosophy of knowledge is reduced to the sociology of knowledge – personal, professional, social and cultural factors and interests – and science as an axiomatic practice is de-legitimised (Nola and Sankey, 2000). Science becomes no different to other spheres of cultural and political knowledge. Scientific activities and the boundary between science and non-science were presented as dependent on various social interests (Kuhn, 1962; Feyerabend, 1975)³ and philosophy and epistemology could gain no foothold on such shifting sands of social interest and power. While the response of some was to shift the emphasis of *a priori* rules to adhering to more pragmatic principles and problem solving (Miller, 1987; Somers, 1998), for others the focus reverted to the sociological exploration of communities, traditions and schools.

The weight of these criticisms of ‘science’ results in little of the gold-standard positivist scientific view being retained. For social science to resemble natural science, a prerequisite became the appropriate re-conceptualisation of scientific activities. While a number of responses veered towards relativism, others sought to maintain the universalistic line by wholesale revisions to positivist/logical empiricist assumptions. It is to these post-positivist developments that discussion now turns.

Post-Positivism as a Warrant⁴ for Arresting Falsehood

A major influence was Popper’s challenge to the notion of science progressing inductively as verification. Up to the 1930s, the Humean inductive observation was received orthodoxy but Popper challenged the traditional conception of scientific method whereby: observation/experience → inductive generalisation → hypothesis formulation → attempted verification of hypothesis → proof or disproof → knowledge (cf. *Logic of Scientific Discovery* (1960) and *Conjecture and Refutation* (1969)). Dismissing as absurd the notion that we engage in straightforward observation of the world, Popper saw a frame of reference as the starting place, allowing investigators to move from problems and theories to observational activities. *Contra*-Mannheim, he denied the possible self-objectivity of the scientist, believing that we can rely neither on our senses nor on our intellect to guarantee

the truthfulness of theories (Popper, 1945). Popper tussles with the question that is of enduring post-positivistic interest:

How can we admit that our knowledge is a human – all too human – affair, without at the same time implying that it is all individual whim and arbitrariness? (1969, p. 16).

For Popper, given that the validity of knowledge cannot be traced to its source, the answer lies with falsification and the public character of science. It is not from *where* knowledge emanates but *how* it is subject to a series of rigorous tests that is of interest, thereby splitting the context of discovery and justification. As Jarvie (1999) notes, the difference between science and magic lies not in their substantive claims but in their stance toward claims (p. 74)(cf. also Somers, 1998). Popper's non-justificationist philosophy cautions that observation may present only verifications within experimental activities, whereas only the falsity of a theory may be inferred from empirical evidence, and such inference is deductive (Jarvie, 1999, p. 52). Theories must be refutable and empirical tests set out to falsify our starting hunches. Despite never having absolute and final refutation of any given theory, testing should be disciplined and systematic, and a belief is held rationally if it has withstood criticism. The truth of a statement is not constituted by the decision to accept it as truth (Miller, 1999, p. 65) – audiences are all too frequently bamboozled by the trickery of magicians and conjurers and broader social communities are no different. Popper's position retains a regulative vision of truth within critical rationalism in terms of which only objective truth allows talk of error (cf. Popper, 1969; Susser, 1989). If science searches for truth, then the truth sought is truth about the real world. This understanding of truth must be independent of the criteria we use to classify it (Munz, 1984, p. 228; Miller, 1999). Instrumentalism, Popper (1969) notes, is unable to account for scientists' interests in truth and falsity. The ability of explanatory theories to survive tests presents the possibility of (fallible) knowledge and avoids relativistic conclusions. Procedural maxims are replaced by a flexible methodology and the shift is from authoritative rules or communities to an activity best secured within an Open Society. Of important note is Popper's minimal ontology – the world is the sort of world which would falsify untrue statements (Munz, 1984, p. 241).

A central target for Popper's criticism is the *Myth of the Framework* and the relativism he believes it generates:

A rational and fruitful discussion is impossible unless the participants share a common framework of basic assumptions or at least, unless they have agreed on such a framework for the purpose of the discussion (Popper, 1976a, p. 24).

The myth suggests that not only do we operate within the framework of our theories, language and traditions but we can never break out of them. The spawning of frameworks, incommensurable worlds, and multiple language games is seen as a degenerative path for the philosophy of science. Popper – in common with many post-positivist thinkers – argues that a difficulty is not an impossibility. 'Framework, like languages, may be barriers; but a foreign framework, just like a foreign language, is no absolute barrier' (p. 45). We must not confuse relativism with the true insight of fallibility.

The views of a 'younger' and 'older' Popper – shifting from the 'concrete' to the 'comparative' – can be identified in the move from initially dismissing falsified theories (sequentially and singularly) to a later emphasis upon the merits of a critical stance and the weighing of competing theories. The later Popper avoids more absolute notions and measures of falsification and progress towards truth, switching to progress towards truth *relative* to other competing theories. During both of these phases he advances the possibility of knowledge that is cumulative, transcultural and testable.

Popper's legacy is evident across a broad range of range of social science disciplines. Even limiting his influence to New Zealand, his work has enduring influence in public policy analysis (Williams, 1980; Easton, 1997; Swann, 1998, 1999), and historical method (Munz, 1984, 1993; Fairburn, 1989, 1999). Peter Munz's anti-positivism and anti-relativism has a clear Popperian lineage. Upholding a commitment to a universal rationality, he dismisses incommensurability, suggesting that a number of theory-transcendent facts and observations may be shared across several theories. More broadly, he advocates a form of evolutionary selectionism that develops a strand of Popper's later philosophy. As Munz (1984) notes, 'We count as knowledge every theory which is left standing when all conceivable criticism has been temporarily exhausted' (p. 49) (also, Munz, 1993; Jarvie, 1996; Weinert, 1996). Miles Fairburn's *Ideal Society and its Enemies*

(1989) contains a range of interpretations (i.e. rival theories) that seek to explain 19th-century New Zealand society, along with his own theory that the period 1850–1900 was characterised by high mobility and atomisation. The work is underpinned by a Popperian logic and elsewhere Fairburn outlines these methodological commitments in greater detail (Fairburn, 1999). He distinguishes between knowledge being shaped and knowledge being determined, and while historical explanation acknowledges a place for hermeneutics, this does not preclude approximating the best explanation and cumulative historical knowledge. Hermeneutics must be underwritten, and theories are to be presented with explanatory power, reliability (plausibility, credibility, probability), and originality.⁵

Some educational inquiry has utilised Popperian insights (Swann, 1998, 1999) and adopted his position of warranted beliefs, rejecting notions of knowledge erected on secure rational or empirical foundations (Phillips and Burbules, 2000, p. 29). Once again, this work stresses the impossibility of simple observation, stressing fallibility and the importance of scrutinising claims. Acknowledging that every inquirer adopts a framework, perspective, or point of view – so retaining a weaker constructivism of power, politics, or ideology – allows knowledge to be shaped by data and rational considerations:

it does not entail that we have to give up the idea that evidence is pertinent to our judgements about the truth or warrantability of our conjectures (Phillips and Burbules, 2000, p. 31).

Thus inquiry can be rational and cumulative and truth is retained as a regulative ideal through the process of peer review. Evidence is central in considering the warrantability of theories, allowing for a pluralism of methods (for a good account of this, see Phillips 1992, Chapters 4, 5 and 8).

Popper and those working within his tradition recognise the importance of conceptual frameworks and theories, but maintain the distinction between fact and theory. Falsification holds the view that concepts are meaningless unless they can be operationalised and measured, differing from positivism only in how this was to be achieved. Essentially there are hard falsifiers that will enable the testing of our theories. Yet as Putnam (1974, p. 228) notes ‘... falsification in science is no more conclusive than verification’. If falsification is to adjudicate appropriately, there must be a closed system – something that social life renders problematic. The presuppositional apparatus of the

Popperian position comes ready assembled – there is one tradition that we are implored to work within, a set of conceptual apparatus that we disregard at our peril. The emphasis upon empirical dimensions and neglect of causal mechanisms remains an unresolved problem with Popper's work and his embrace of an individualised methodology is difficult to sustain given social complexity.

Post-Positivism as the Necessity of General Theory

For sociologist Jonathan Turner, positivistic theory which takes an abstract rather than a particular historical or empirical case remains a possibility and such theory can be tested against facts, allowing the investigator to explore causality and illuminate operative mechanisms (1992, p. 172). In tune with Turner is Wallace's (1992) emphasis upon 'concept standardization' and the importance of shared concepts and common rules. Thus some of the weaknesses of the Hempelian model of causation are attended to, leading potentially to a 'social physics' of testable laws (cf. Turner, J., 1990). But the second variant of post-positivism discussed here (taking Alexander, Colomy, Wagner and Berger as its exemplars) stands apart from Turner by distinguishing a propositional space aside from a general level of theory.⁶ This post-positivism begins not from the Popperian methodological 'warrant', but seeks instead to grasp the nature of social theory and theorising (see Wagner and Berger, 1985; Colomy, 1990a & b, 1991, 1992) arguing for a post-positivistic social science that takes the logic of general theory as its point of departure.

Jeffrey Alexander writes:

In order to defend the project of general theorizing, it must be accepted that sociological argument need not have immediate explanatory pay off to be scientifically significant (1982, p. 65).

Whilst not demanding immediate pay-off, significant debt is still incurred, and theorising is akin to a 'promissory note', a down-payment to secure the continuing sociological project. Unlike the positivist disavowal of theory and the Popperian tendency to compact discussion within methodology, broader theoretical reflection provides the basis for the possibility of social knowledge. Alexander's theoretical reconstruction begins by reinterpreting conventional understandings of scientific processes to eclipse the 'dangerous' dichotomy of positivism and 'human sciences' (1982, 1995, p. 90; also

Colomy, 1991). The possibility of general theory precludes its dissolution into the knower and, alternatively, its absolution as unrelated to the knower ('the epistemological dilemma') and suggests, 'Theoretical knowledge can never be anything other than socially rooted efforts of historical agents' (Alexander, 1995, p. 91). Whilst for Popperians the practice of science is *re-wrought* using familiar empiricist materials, in Alexander's scheme it is *re-thought* with science having both a metaphysical and an empirical element. These two dimensions, their interdependence and two-directionality, Alexander argues, have been overlooked by those seeking to understand natural and social science. He writes:

I am articulating the kind of framework that would make it possible to be 'realistic' about empirical truth without abandoning the commitment to relativism imposed by historically-specific cultural frameworks (Alexander, 1982, p. 128).

The introduction of such additional conceptual materials clashes with Popperian claims about the strict role of empirical referents within scientific statements. In Alexander's view, social science consists not of observation with crucial empirical tests (whether verification or falsification), nor of simple methodological refinement or quantitative fine-tuning. The fixation of logical positivism for stripping, cleaning and reassembling an observational language is also dismissed. Instead, the task is a theoretical, discursive or presuppositional one that promises to deliver:

universal criteria of evaluation, judgement, and, indeed, explanation vis-à-vis the social world (1982, p. 18).

He sees no conflict between acknowledging the theoretical *a priori* and the search for objectivity and rational intellectual standards (1982, p. 113).

Such a reorientation, argues Alexander, avoids the pitfalls afflicting 20th- century sociology – a crisis of confidence, the burgeoning of paradigms, and the confinement to activity within sub-fields and corresponding neglect of theory. While positivism split the empirical and non-empirical, Alexander identifies all data as theoretically informed and deprives empirical commitment of a solely experimental basis. The key becomes grasping the non-empirical and generalised elements of theory and the independent contribution they make. This interpenetration of theory and data has the consequence that fundamental shifts in scientific belief occur when empirical changes are matched by alternative

theoretical commitments. But such gains are not solely empirical, and general theoretical elaboration may be dogmatic and horizontal. Thus for his post-positivism:

all scientific development is a two-tiered process, propelled as much by theoretical as by empirical argument (Alexander, 1982, p. 30; italics in original).

Although frameworks are themselves open to dispute, it is a task of sociology to justify the choice of standards for providing 'post-positivistic standards of knowledge' (1982, p. 114). The theoretical domain is not merely a legitimate domain but is the *legitimising* domain. It stands apart from a positivist position:

Since non-positivist arguments about generalized logic ultimately rest not upon 'automatic' processes of proof but upon contingent processes of persuasion, objectivity can occur only to the degree there is agreement among the contending parties that their disputes shall be judged according to the same higher standard (1982, p. 115).

These generalised commitments result from empirical, cognitive and moral commitments and preferences, and must stand outside the theories they attempt to judge. In clarifying the importance and range of presuppositions, considerable space is devoted to advancing his own persuasions, squared by his stance that Kuhnian incommensurability is overstated and distorted communication can be overcome. His interest in presuppositions that create a general framework leads him to identify action and order as 'the true presuppositions of sociological debate' (1982, p. 65).

After 1985, self-conscious 'neo-functionalism' emphasises patterned departures, contingency, conflict and collective behaviour, against functionalist master-trends towards specialised institutions. This neo-functionalism is defined as a discourse and a research program (Alexander, 1985), and seen as explaining particular facets of the social world allowing the accumulation of knowledge within the confines of discourse:

By generalized discourse, we refer to discussions that argue about presuppositions, about ontology and epistemology, about the ideological and metaphysical implications of sociological argument, and about its broad historical grounding (Alexander and Colomy, 1998a, p. 64).

Elsewhere he expands upon his view of discourse:

Discourse becomes as important a disciplinary activity as explanation. Discourse is general and foundational. It aims at thematizing the standards of validity that are immanent in the very practice of social science. Responding to the lack of disciplinary confidence in empirical mirroring, theoretical discourse aims to gain provisional acceptance on the basis of universal argument. It is, therefore, the very impossibility of establishing permanent foundations that makes foundationalism in the social sciences so critical (1995, p. 123).

Later writings emphasise the role of scholarly traditions and schools in shaping any presuppositional/discursive frameworks. These sociological traditions provide units for assessing cumulative social scientific knowledge (Alexander and Colomy, 1998a & b). Social science flows within schools and traditions and each discourse contains distinctive criteria for truth. The success of some schools is not based primarily on theoretical effectiveness of positions nor on objective empirical scope (Alexander and Colomy, 1998b, p. 36) but upon discursive movements over time. The competition of traditions provides the possibility of relative and comparative progress:

Instead of speaking about theoretical or empirical progress per se, one must speak of relative explanatory and theoretical success vis-à-vis one's own tradition or competing ones... By making such comparisons we can measure social scientific progress although, to be sure, this is progress in a postpositivistic sense (Alexander and Colomy, 1998b, p. 63, p. 77)(also Colomy, 1991).

Social science experiences routine presuppositional disagreement and the relationship of theory and facts makes social science essentially contestable. These frameworks are not subject to empirical test but are presuppositions, definitions, models and meta-methodological imperatives. 'Discursive argument and the rational criteria they imply are only subjectively compelling' (Alexander, 1998, p. 174).

Forsaking social science as a mirror to reality does not mean that the project of rationality is abandoned. 'Present reason' embedded in social practices offers a 'hermeneutically rooted version of universality' (Alexander, 1995, p. 113) which allows the construction of frameworks to interpret the world. Also, the lack of permanent standards of rationality does not equate to arbitrariness. As Alexander writes, 'Human beings create the world view that allows them to conceive of this world, but they do not create this world as such' (1995, p. 96). He continues, 'General theory is not something that simply reflects the objective world out there' (p. 97) and goes on to suggest, 'Whether impersonal worlds are acknowledged to exist is the first criterion for universality. Whether practitioners feel themselves bound to these frameworks point to the second' (p. 114). Reaching consensus – shared disciplinary matrices of rationalist notions of evidence and logic – remains a possibility:

To engage in foundationalism is to put forward general theoretical arguments, to create criteria for truthfulness that are so universally compelling that they produce agreement about validity claims between practitioners within a field (1995, p. 122).

Alexander's recognition of the discursive and choice of many or any positions is tempered by his agenda of persuasion. There is an apparent Janus-faced nature in Alexander's work. Two voices echo throughout – Alexander the presuppositionalist outlining the tradition-based activity within which we engage and how traditions judge our endeavours. Hark also Alexander the neo-functionalist developing interpretations within his own neo-functionalist discourse and outlining the substantive presuppositions that provide a basis for ongoing activity.⁷ It is unclear which of these positions best profiles his post-positivist commitment and Alexander the neo-functionalist cannot resist intruding into debates about the nature of presuppositions/discourses, and his neo-functionalist position frequently decrees the presuppositional 'options'. Thus while Alexander argues that knowledge is created within discursive spaces, and such discursive spaces are subjectively compelling, an underlying thrust of his work is that certain presuppositions are simply not up to the task that they face. While earlier work (1982) defends a particular pair of presuppositions, he shifts to a Kuhnian emphasis upon social rather than theoretical conflict.⁸

The 'presuppositional choice' that allows ongoing work within schools and traditions is more the identification of the presuppositional basis – of which neo-functionalism is not one contender but the outright champion. Alexander does highlight the necessity of theoretical presuppositions – the absence of which simply makes activities cease to be social theory and become something else altogether (such as a novel or a poem). But it appears that once discursive spaces are opened he is unsure how to best close these and still retain a regulative view of 'knowledge'. Alexander's criteria for persuasiveness present some difficulties:

persuasiveness is based on such qualities as logical coherence, expansiveness of scope, interpretive insights, value relevance, rhetorical force, beauty, and texture of argument (1998, p. 169).

Defending his choice is an act of persuasion which, if undertaken in a rationalist vein, promises to overcome distorted communication. But if frameworks are held for empirical, cognitive and moral considerations and persuasiveness attributed to any number of these, how is 'persuasiveness' to be calibrated? While all of these considerations may sit in judgement, some must be elevated to a higher court of appeal – otherwise why are we to adopt the neo-functionalism of Alexander? He points to tradition as a framework for interpretation with progress occurring within these traditions, but excludes positivism and relativism which could arguably also be held for a number of 'empirical, cognitive and moral considerations'.

A thread runs between Alexander's work and David Wagner and Joseph Berger's discussion of cumulative knowledge (Wagner and Berger, 1985; Wagner, 1992). Wagner and Berger (1985) distinguish different types of theoretical work: orienting strategies, unit theories, and research programmes. *Orienting strategies* are essentially theories about theory, addressing questions about the nature of theory – its content, connections between theories, and how theory should be studied. Such meta-theoretical dimensions are ontological and epistemological statements that direct where and how our investigation should take place. These basic categories construct an image of the world (the 'furniture'), with direct implications for how we then set about gaining knowledge of it. At the core of such orienting devices are values and goals, shaping the foundational concepts which facilitate description/analysis, and presenting benchmarks or evaluative criteria that allow us to judge the social world. Thus orienting strategies:

... are statements about values..., not statements about facts... Such prescriptive arguments are largely nonempirical, and conflicts between them are often unresolvable (Wagner and Berger, 1985, p. 701).

These perspectives facilitate the accumulation of knowledge or, more accurately, allow *knowledge-claims* to be made. Second are what Wagner and Berger refer to as *unit theories* whereby theoretical statements are made arising from meta-level endeavours, and these are the resulting theoretical products to be evaluated (Wagner and Berger, 1985, p. 703). This level of theorising outlines concepts and relationships, and promotes empirical testing. On the basis of such testing, unit theories are seen to be supported more or less well than rival unit theories. Thirdly, there are what they label *theoretical research programs*, focusing on the context of interrelated theories within which unit theoretical work occurs:

Basically, a theoretical research program is a set of interrelated theories, together with research relevant to evaluating them (p. 705).

The suggestion is (*pace* Alexander's presuppositions/ discourses) that orienting strategies are the broadest form of reference – a directive and mediating strategy upon which our observations ultimately rest. These orienting strategies are evaluated directly and indirectly (Wagner, 1992, p. 211) and allow for unit theory to be developed *within* them and to be subject to empirical test (Wagner and Berger, 1985). Despite the absence of bedrock foundationalism, theorising is presented as fruitful and rational, and theories may develop cumulatively within the confines of programmes.

This typology outlines a demarcation between the theorising of theory, and the more 'underlabouring' role of unit theory construction and testing. It suggests we can both *do* meaningful social science (unlike some extreme versions of anti-positivism) and *theorise the doing* of social science (unlike positivism). While the view of orienting levels as a value position makes arbitrating 'rationally' amongst differences that emerge at this level difficult, the possibility is not discounted:

Orienting strategies *are* evaluated, both directly and indirectly. On the one more direct level, we assess the originality, clarity, and internal consistency of the theorist's vision... (Wagner, 1992, p. 211).

But once again it is unclear how criteria of judgement are adopted in order to rule out certain orienting strategies, and as in the case of Alexander's work, how traditions containing abstract and concrete components are to be ruled upon.

Metatheorising

One suggestion made by Colomy (1991) is that the 'adjudicative metatheorizing' of George Ritzer can contribute towards 'reasoned judgement about the analytic merit of competing traditions' (p. 279). It is helpful to explore the work of Ritzer – both the claims made by Ritzer himself and the evaluative strategy of Ma that Colomy sees as flowing from it.

Ritzer stresses the necessity of social theory for explanatory activity and the importance of systematic study of social theory. He emphasises that metatheorising occurs *after* the creation of theory and that it takes that theory as the focus of study (2001, p. 15). He identifies three approaches to metatheory:

- studying theory to gain a better understanding of it (Mu)
- studying theory as a prelude to theory development (Mp)
- studying theory to produce a new overarching theoretical perspective or metatheory (Mo).

He is critical of theorising prior to the development of theory that attempts to lay down its prerequisites (which he calls Om). For Ritzer, abstract philosophy is exorcised from social theory by first understanding relevant theories and then using such understanding to develop either new theory or an overarching perspective.

His early work on sociology as a 'multi-paradigmatic science' drew upon Kuhnian insights. A paradigm '*subsumes, defines, and interrelates the exemplars, theories, and methods and instruments that exist within it*' (Ritzer, [1975] 1980, p. 60; italics in original). He innovates that paradigms can be identified *within* a discipline and outlines three sociological rivals – social facts, social definitions, and social behaviour. Illuminatingly, in the five years between the original and revised editions of *Sociology: A Multiple Paradigm Science*, Ritzer added his own 'integrated sociological paradigm' to

address the micro-macro, and objective-subjective deficiencies of rivals.⁹ This move from description to prescription is accompanied by caution that:

Such a paradigm would not replace existing paradigms, but rather is designed to supplement them by dealing with issues that are beyond their scope... the choice of the paradigm depends on the kind of question being asked (2001, p. 79, p. 95).

Ritzer foresees gains from discussing various paradigms but precludes convergence toward one complete paradigm: 'each of the paradigms is, in itself, incomplete and incapable of adequately explaining any social phenomenon' ([1975] 1980, p. 212) and adequate explanation requires the range of sociological paradigms.

In his later work, the earlier restraint in identifying a limited number of paradigms gives way to fully-fledged eclecticism, as paradigms are displaced by fragmented positions in a development that is seemingly both descriptive and prescriptive. The sociological world of multiple paradigms (from which we choose and then defend) has been replaced:

today's theorists need not be constrained by any theoretical perspective... They can put together a perspective of their own based on ideas drawn eclectically from many theories... as they move from issue to issue, they can utilize different theoretical perspectives... theorists simply have a lot more lenses at their disposal and with which to examine the social world. They are going to see a lot more things and they are going to be able to look at those things in many different ways and from many different angles (2001, pp. 147–8).

Ritzer presents this fragmentation as exhilarating and disconcerting, 'a new world of theory which is free, confusing, chaotic, and lacking in anything that a thinker can hold onto as a base or as an incontrovertible truth' (2001, p. 151). We are exhorted to abandon the goal of knowledge for new 'insights' and explanation is swapped for the 'playfulness' of theory:

The measure of theory today must be the creation of new insights and new ideas rather than advancing some theoretical perspective. These new insights and ideas do *not* necessarily represent advancement. They are simply new, different, useful for a time, and ultimately to be altered or

discarded for newer and more useful ideas and insights' (2001, p. 151; italics in original).

With talk of 'ransacking' postmodern social theories for new metatheorising resources, his eclecticism is given full flight. Everything is up for grabs – 'Playing with the limits of rationality and readability might lead to some creative new metatheoretical work' (p. 156) intertextuality, discourse and deconstruction become the new routes.¹⁰ There would seem to be little future for theory in knowledge generation, explanation and prediction. Despite his stance that no arbitration is possible amongst paradigms, he actually devises his own, originally arguing that this integrated paradigm offered more grasp on levels of social reality. The assumption of theoretical deficit is used to develop his own paradigm and he works with explanatory intent (and ontology), something he later abandons when viewing metatheory as offering insight but not explanation. Arguably, metatheorising is itself of little use if it leads only to deconstruction. While metatheorising is distinguished from the descriptive/prescriptive 'philosophical sociology' (Weinstein and Weinstein, 1992, p. 136) at dispute is the nature of this reflectivity or 'hypereflectivity' (Weinstein and Weinstein, 1992, p. 140).

Colomy (1991) recognises that Ritzer does frequently adjudicate and invites him to go beyond understanding social theories to exploring and evaluating the broader traditions, his hope being that an 'independent sociological space is created for producing universalistic standards and for applying them in a dispassionate manner' (p. 279).

The Nature of Reflexivity

This section pulls together a broader theme that runs through the post-positivism of Popper and Alexander, and Ritzer's metatheorising. Previous discussion traversed a number of issues about the nature of reflexivity, airing a number of fallibility, critical, dogmatic, sceptic, and pluralist considerations. As McLennan (1995) writes:

In what is often characterised as our 'post-positivist' methodological condition today, it is increasingly accepted both that a diversity of valid philosophies, paradigms, general theories, approaches to research, and value orientations exist, and that it is neither possible nor desirable to say definitively which amongst these are more objective, true, scientific, or

essential. That broad outlook is what I am calling ‘methodological pluralism’ (p. 57).

At the heart of the response to the positivist bogeyman is the notion of methodological pluralism and the nature of reflexivity. McLennan identifies three variants of reflexivity: *Liberal-normative* where we recognise that we might err or be one-sided; *sociologistic reflexivity* – whereby all positions are a product of time, place and culture; *rhetorical reflexivity* or ‘rhetorical reductionism’ where all claims are about persuasion (2002, p. 495). Any of these would dispel the positivist promise of trans-cultural and trans-historical knowledge through the gathering of facts. But the continued re-thatching of straw-men has prevented clarification of the many strands strewn within the reflexivity camp. Drawing upon McLennan’s distinctions sheds light on these issues.

In the post-positivism of the Popperanians, reflexivity is conceded in a fallible, liberal-normative vein. Any knowledge is tenuous under conditions of falsification but the possibility of universal knowledge that is not confined to a language community or paradigm is upheld. Thus there is criticism within the day-to-day activity of marshalling evidence and making warrantable claims. All warrantable claims are laid open to discussion, whilst the framework of research itself is subject to a measure of criticism. But this criticism does not become a position of Criticism. Despite the questioning of claims, a problem arises when this *attitude* of criticism becomes the *logic* of scepticism with a tightly circumscribed method. Thus a Critical position directs us to be always and everywhere sceptical, thereby preventing substantive debates. Under Criticism, only adopting the position of our opponents – the ultra sceptics – prevents interminable descent. As Munz (1984) notes:

one behaves as if there were guidelines for criticism and as if criticism meant simply a standard scrutiny of one’s opponents. If all standards are met, it is assumed, criticism must stop. But all guidelines are nothing but disguised attempts to define correct knowledge in advance (pp. 50–1).

This position is what Munz calls dogmatic scepticism – ‘the dogmatic opposite of dogmatism’ (1984, p. 54). It denies the possibility of sociological knowledge. Such issues are accentuated by those that use necessity of language and representation and rhetoric to

disarm their opponents (cf. Edwards et al., 1995; O'Neill, 1995). At issue is what reflexivity is claiming for itself because if it constitutes a position then this is clearly inconsistent and guilty of the 'performative contradiction' (cf. Hall, 1999). Many proponents of the sociology of scientific knowledge suggest that no status or privilege is being claimed by adopting the reflexive stance – the focus is instead upon how writers persuade us of their positions. But as Fuchs (1992) suggests:

reflexive self-reference oscillates between self-exemplification and self-contradiction or between tautology and paradox (p. 156).

In contrast, the position adopted here is that there is place to stand and engage in a realist discourse which is about making arguments and having a point.¹¹ While the notion of liberal-reflexivity can be accommodated within the Popperian position and avoids the strong position of rhetorical reflexivity ('hyper-reflexivity') whereby criticism becomes Criticism, the position fails to fully acknowledge sociologistic reflexivity. In turn, the notion of sociologistic reflexivity (awareness of our own position and site) when taken to extremes dispels the possibility of explanation and theoretical commitment, impaling discussion on its particularism.

What are coming sharply into view are the different meanings of reflexivity and distinct inclusionary and exclusionary brands. At issue is whether reflexivity can be extended indefinitely before it turns back upon itself and becomes a position – a dogmatic position – that is universalistic in its denial of universalistic sociology. The notion of sociologistic reflexivity clarifies the positions of Alexander, Colomy, Wagner and Ritzer. They recognise there is no 'view from nowhere' but then have an inability to close the Pandora's Box and get a view from somewhere. We have a mass of paradigms, positions, orienting devices but appear to lack ability or an appropriate measure to arbitrate amongst them. Whilst Alexander, Colomy and Wagner retain hope of arbitration (without clear guidance on how), Ritzer abandons the very notion of arbitration itself. But:

What kind of motivation for social explanation is it that begins from the acceptance of incompatible and multiple different accounts in principle?
(McLennan, 2002, p. 496).

Some boundaries must be placed upon reflexivity and a measure of exclusionary reflexivity is unavoidable. Rhetorical reflexivity denies that support we give a particular

perspective can be attributed to its explanatory appeal (McLennan, 1995, p. 56). Social theory as an *explanatory* exercise must continue to hold 'epistemological ambition' (McLennan, 1996). The force of strong reflexivity denies this ambition, whilst Popperian ambition forecloses discussion far too early.

At issue is not whether approaches are reflexive or not but rather the precise nature and strength of this reflexivity and how we continue our search for explanation. The requirement is one of 'trying to impose *some* critical limits upon the range of acceptable strong perspectives' (McLennan, 1995, p. 53). A major challenge is to recognise the ground that may be conceded to reflexivity without sinking the explanatory project. In epistemological terms, our social situatedness is undeniable but does not condemn us to particularistic knowledge. Given that reflexivity must maintain epistemological ambition, a broader distinction between metatheorising and philosophical sociology is coming into view. The approach to theorising that Ritzer was keen to discard (his 'Om') must be retained, for failure to do so excludes broader philosophical questions about the nature and possibility of knowledge. The distinction drawn by Bhaskar between philosophy and *a priori* forms of reasoning is instructive here with only the latter making claims to unrevisable certainty (see Collier, 1994, p. 25).

Later chapters argue that Critical Realism can incorporate insights of reflexivity and uphold robust explanation with depth and memory. Critical Realism also shares a number of post-positivistic 'manifesto commitments':

- the avoidance of positivism, relativism and irrationalism
- the potential of reason
- anti-foundationalism of both the senses and reasons
- science/ social science as ongoing, arduous activity.

An undergirding strength of Critical Realism is its willingness to consider the interrelationship of matters ontological-methodological-practical. Any stance around the nature of *being* has implications for what are taken to be valid epistemological claims (knowing) and how it is possible to set about creating practical knowledge. Whatever starting point is adopted in sociology, there is no shirking ontological commitments and this is an oversight of positivism, anti-positivism and post-positivist variants. As Bhaskar

(1989) notes 'Without some prior specification of an object of inquiry, any discourse on method is bound to be more or less arbitrary' (p. 69).

A starting point is to re-conceptualise theorising as a flexible rather than foundational concern, but to guard against reflexivity that denies criteria of purpose and judgement. Central to the task of sociology are three types of theorising – ontological, methodological and practical. Although not deterministic, such relationships serve to broadly condition choice elsewhere in the theoretical system, and whilst some theorists seek to establish explicit connections among the ontological-methodological-practical components of social theorising, others work solely within one domain. But regardless of whether *explicit* connections are established, theorising in a particular way in any one of these domains has implications 'downwards' and 'upwards' (i.e. from ontological to methodological and practical and *vice versa*). Robust social theorising must seek to connect across all three domains. Unlike positivist edifices, the Critical Realist position has built in flexibility allowing a series of patching and realignments within all three domains, and fallibility within the ontological, epistemological and practical. Unlike unbounded relativism, Critical Realism has possibilities of (indirectly) assessing the fertility of each theoretical vision. The consistency with which secure connections are made across ontological-methodological-practical components suggests a further route towards the critique of particular positions, including whether defensible assumptions are used to fasten dimensions and whether these can be upheld against their fuller picture of theorising. Emphasis on secure rather than frayed or steadfast connections avoids both unbridled instrumentalism and inflexible system building.

Distinguishing the role of *being* as aside from that of *knowing* recognises that the former has a *conditioning* effect. This ontological dimension bears upon the real shape of social science theorising. These considerations are revisited in Chapter Eight. The focus of the thesis now shifts to how agency, culture and structure are advanced within explanatory frameworks.

Summary

The purpose of this chapter has been to explore some broad positions beyond positivism and anti-positivism. The approaches discussed so far within this third alignment show a reluctance to engage with matters ontological. A further aim of discussion was to acknowledge the importance of reflexivity and to ask about its location within any theoretical system. Popperian falsifiability, Alexander's multidimensionality, and the methodological relationism of Ritzer are beyond repair, given their disavowal of matters ontological. Also, signalled within this chapter are the shared manifesto commitments of post-positivists and Critical Realists and this receives more attention in Section III. The following chapters (Five, Six and Seven) however address how attempts have been made to account for social science development.

Chapter Four Notes

¹ A number of writers suggest a three-way division of: empiricist/positivist; anti-positivist (holist/conventionalist/relativist); and post-positivist/critical realist (e.g.. Lloyd, 1986; Layder, 1994, p. 73). A slightly different split suggested by Munz (1984) consists of positivism, contextualist/relativist, and his own position of evolutionary epistemology.

² See also his essay, 'Speaking of Objects' (1969), on the necessity for apparatus of identity and quantification.

³ Gellner (1985) makes an interesting distinction between philosophical and social views of science. For the former: 'A solitary Robinson Crusoe could, according to such a theory, practice science. Given resources, longevity and ability, no achievement of science as we know it would, "in principle", be beyond his power' (p. 107). The social view is that society is a precondition for scientific activity – sometimes a particular type of society (ie. rational, open).

⁴ The notion of 'warranted assertability' rather than truth owes its origins to John Dewey (cf. Phillips, 1992).

⁵ In this light he shares Popper's granular vision of the world. Thus as Gellner (1985) notes: 'Popper does not exclude imagination and empathy, let alone theoretical daring, from the social sciences. He merely does not allow hermeneutics to overrule facts' (p. 62).

⁶ Jonathan Turner also considers abstract theorising to detract from the real work facing sociologists (1990). But as Bryant (1995) notes, 'the metatheory which [Turner] condemns in others has its justification when he himself practices it (p. 14).

⁷ I concur with Ritzer on this point (2001, p. 148).

⁸ Or the earlier Kuhn (1962) at least where his discussion of paradigm shifts is seen as the result of political considerations. By 1970 Kuhn talked about 'good reasons', included in which were 'accuracy, scope, simplicity, fruitfulness' (1970, p. 261).

⁹ But it should be noted that while avoiding any realist commitment he maintained that they are for heuristic purposes 'levels are either theoretical or metatheoretical (Mu) tools; they are *not* ontological realities' (Ritzer, 2001, p. 81; italics in original).

¹⁰ Weinstein and Weinstein (1992) provide the best definition of metatheory, 'Metatheory is the rejection of attempts to provide foundations for theory and the permission to do without such foundations (p. 145).

¹¹ Shared by a number of writers including Fuchs, McLennan, O'Neill, Bryant, Stones, Giddens, and the Critical Realist school, to name only some.

Section II

The Tools

Chapter Five

Explaining Change: From internalism to externalism

Introduction

Previous chapters discussed positivism, anti-positivism and post-positivism, commenting on the status of social knowledge. This chapter explores the overall focus of inquiry, asking whether elucidating a practice such as social science research requires an internalist (intellectual) or externalist (contextual) emphasis. In exploring these two approaches the chapter examines what are termed here 'the advancement of knowledge' and 'causal structuralist' approaches. In light of the critique of these the chapter begins to explicate the particular tools and concepts that may be required to advance an account of social science research.

The Advancement of Knowledge

Discussion begins with a series of 'explanations' of social science that emphasise the internal characteristics of social science, frequently looking to the unfolding of the idea over many hundreds of years. The 'advancement of knowledge' adopts the axiom that there is an intrinsic quality within the idea of social science research that explains its trajectory and development. The emergence of social science research is often attributed to its mimicry of natural sciences, and how the rationality that is carried by both can scythe a path through confusing natural and social worlds. For social science research, therefore, the purported strength and logic of its ideas causes them to diffuse through modern societies and nation states, assisting human societies to understand and exert control upon their social world.

After the Middle Ages, the notion of progress began to incorporate the realm of ideas and human knowledge as being incremental, cumulative growth through the ages (Sztompka, 1993, p. 25). Progress moves beyond relying on the supernatural or providential to explain change, locating it instead in natural realms of potentialities unfolding. As an explanation of social science emergence such a view is situated within the stream of intellectual developments of the Enlightenment from the late 16th and early 17th centuries, representing

a shift in world-view and a challenge to intellectual, religious, political, and economic authorities. Numerous traditions were forged in the intellectual melting pot of the Enlightenment: Bacon and Hume stressed the empirical, the role of experience, and the place of induction; Descartes and followers continued the Platonic tradition of logical deduction from self-evident principles.¹

The repercussions of the Enlightenment were a catalyst to an institutional reconfiguration: democracy replaces autocracy, the Regent's court giving way to the elected chamber; the elevation of political economy; astronomy is displaced by science, and mysticism by reasoning; and, later, the Church is challenged by the prestige of the University. Even later still, disciplines become the new schisms of science. The overshadowing institutional and organisational development for all of these changes is the emergence of the nation state, a complex interconnection of institutions and administrative relationships, located at the centre of a particular geographical area, and reaching out into the newly found fabric of society (see for example, Giddens, 1995, p. xvii). What is evident in an internalist-focused account is the consolidation of particular intellectual traditions and the emergence of ideas driving the development of institutions. The role of agency is marginalised to those who harbour and labour great ideas and context is ignored (cf. Kelley, 2002). The position resembles a philosophical exercise in the history of ideas, and frequently underpins the liberal tradition of explaining scientific and social change.

The march, step, leap or do-se-do of reason

Elaborating the work of Gellner (1964) and Sztompka (1993) suggests at least four ways of applying internalism to understand the emergence of social science research. These are visions tracing evolutionary emergence of rational social science; Comtean stages; Popper's Manichean distinction of societies that are rational (Open) and irrational (Closed); and, more recently, Abbott's notion of cyclical or fractal change. These are sketched below as suggestions of how social science may be explained.²

First, from an evolutionary perspective, faith in the Enlightenment results in history over the *longue durée* becoming the march of progress towards the modern – and rational – present. The elevation of science – and later social science – mark the retreat from the use of 'traditional' resources to gain orientation within the physical and social worlds. Modern

societies are liberated from ancient and medieval constraints of astrology, mysticism, tradition, and religion, which are superseded by 'explanations' stressing the role of science, universal accounts, empirical debate, and driven by an underpinning commitment to rationality. This shift underpins the notion that the world is ours to investigate and explore, rather than one whose meaning is simply communicated or revealed. Delineations are drawn between 'rational', 'non-rational', and 'irrational' ways of understanding and knowing the world (Adams, 1989 and 1998; Hughes, 1990; Sztompka, 1993; Delanty, 1997; Chalmers, 1999; Smith, 1999).

At the root of the Enlightenment were radical changes in the view of the world and the belief that science offered an appropriate methodology to better understand this physical (and, later, social) world. Science was perceived by practitioners as the study of existing reality which transcends the discourse of science thereby allowing it to make comment on the 'real' world. Rather than truth being revealed, under this new science it would be discovered by human action and investigation with the intellectual and experimental resources at hand to steel us to the task. The Copernican revolution centred the role of the individual and knowledge discovery, and introduced notions of change and progress as the result.

Does the explanation of social science development stop here? Can empirical social science be viewed as an idea whose time had come in the evolutionary development of breaking free from earlier ignorance? Is there some inevitability in the emergence or delivery to human consciousness of social science, perhaps reflecting the progress, advancement, or high mark of human capacities to live together in civil society? Since the Enlightenment, and particularly since the work of Condorcet, social sciences have sought to establish a relationship between the gradual accumulation of knowledge and social change, with an emphasis on moral progress. This, as Bohman (1991) succinctly notes, translates to the aphorism 'what is later is better'. Taken to the extreme, such a teleological view of history becomes Whiggish triumphalism (cf. Butterfield, 1959). Gellner (1964) notes that with progress, time ceases to be morally neutral: 'Life has come to be lived *on an upward slope*.... Its solution is endogenous' (p. 4). The ensuing vision of society has been dominated by a liberal tradition of tolerance, emphasising the place of the

individual *vis-à-vis* particular institutions and facilitating a more humane and dynamic society.

Second, a Comtean perspective of the ‘stepping’ of reason sought to bridge the methodological chasms between science and society. He offered laws governing the progress of the human mind as it moved through theological, metaphysical, and positive stages. For Comte, the only form of knowledge was that supported by empirical evidence, whilst underlying the development of society, was the evolution of the human mind. In his idealist conception of stagist change, positive science was the pinnacle of human thought and development ([1855] 1974).

A third explanation for the rise of social science identifies a ‘leap’ from darkness into light signalled by the Enlightenment. This tradition equates civilisation with the elevation of the individual and sees rationality as offering the capacity to determine how society should be organised. Unlike the evolutionary and stagist view, the victory of social science is seen as neither inevitable nor linear. The best exponent of this position, Karl Popper, argues that the move to a position of what he terms ‘critical rationalism’ marks the high-water mark of human faculty but may be reversible.³ Critical rationalism provides the rationale for translating his fallible scientific method to social interventions. To approximate truth, one must lay oneself open to being proven wrong, and aim for increasing ‘verisimilitude’ – powerful explanatory theories that have laid themselves open to falsification (Gellner, 1974; Popper and Eccles, 1974; Freeman, 1975, Gray, 1976).

This rationality principle is presented as a law of social science that should not be tested, and indeed cannot be proven. Rather, the ‘decision’ to adopt it is a moral one, ‘I am a rationalist because I see in the attitude of reasonableness the only alternative to violence’ (Popper, 1974, p. 337). But taking this leap of faith in the rational is for Popper what distinguishes *Open* and *Closed Societies* (cf. Susser, 1989). Although the process is not necessarily linear, any move away from rationalism (i.e. the belief ‘I may be wrong and you may be right, but only by discussion will we get closer to the truth’), has disastrous, almost unthinkable, consequences, ‘If we turn back, then we must go the whole way – we must return to the beasts’ (1945, Vol I, pp. 200–1).

The purpose of rehearsing these debates about the Enlightenment, the role of science, and the relation to society, is to present various endogenous explanations of the rise of social science (stagist, gradual, Manichean). These positions, albeit with varying emphasis, suggest that the emergence and intrinsic rationality of scientific procedures 'explain' the subsequent diffusion of social science research through societies. Social science research is a function of ideas, belief and revelation, while material contexts – political, social, economic – are downplayed. These backdrops are not seen to impinge on the real activity occurring centre stage as social science (as a form of rationality) is revealed to us. Human actors are nowhere to be seen.

This internalist theme is not confined to the history of ideas but has also a contemporary expression in the recent work of Andrew Abbott (2001). The argument that social science ideas are constantly being reinvented is the (reinvented) message of *The Chaos of Disciplines* where he develops an account of social science development, asking:

How does social science change? In its complex history some see an immanent trajectory, others see local practices. Some see political domination, others see internal competition (p. 3).

Abbott goes on to develop an 'internalist' account of change that centres on the notion that the subset of a larger unit can contain a scaled-down version of structures and processes in that larger unit. He rejects explanations that wed knowledge to power, and also dismisses what he calls the 'variable approach' to understanding change in disciplinary configurations. (Thus a Foucaultian 'archaeology', where an anonymous discourse develops in a style for which nobody argues, and functionalist explanation are roundly dismissed.) His interest is both the cultural structure of our intellectual life, and the settlement of the social structure. He draws upon 'self-similarity', a notion which has some resemblances to Parson's AGIL model and four functions of adaption, goal attainment, integration, and latency, but suggests his own developments are more self-conscious, empirically grounded and dynamic than Parsons'. At the core of Abbott's argument are 'fractal distinctions'. Stated simply: 'A lineage starts, then splits again, then splits again' (p. 11). In moving away from any metaphor of cumulation, the implication of his theorising for social science development is that:

intellectual life in social sciences is organized around perennial debates that produce proliferating lineage with the peculiar properties of self-similarly, self-replication, and rootlessness (p. 121).

Abbott concludes that social science's external and internal structures are produced by the same mechanism, that of self-similarity (p. 10). He is at pains to distinguish his arguments from those of simple differentiation, arguing that fractal cycles provide a model for perpetual discovery, whereby one side 'ingests' the concepts of the other. Why this cannot be explained by a Hegelian dialectic (perhaps utilising a variety developed by Gouldner) is not totally clear. Nevertheless, Abbott writes:

Under a Hegelian model, any unified version of social science would call its opposite into existence, and the two would then be synthesized into a new social science transcending the earlier version (p. 15).

Whilst emphasising the internal conditions of development, he suggests the interplay between the cultural and structural – pointing to institutions and contexts that allow settlement of a particular disciplinary terrain. He suggests that 'the heart of the disciplinary system is a stable social structure between disciplines and mutable cultural structures within them' (p. 148). This work of Abbott, creative as it may be, generalises a theory from some social and cultural patterns, whilst failing to fully acknowledge the role of agency and the place of power and interests.

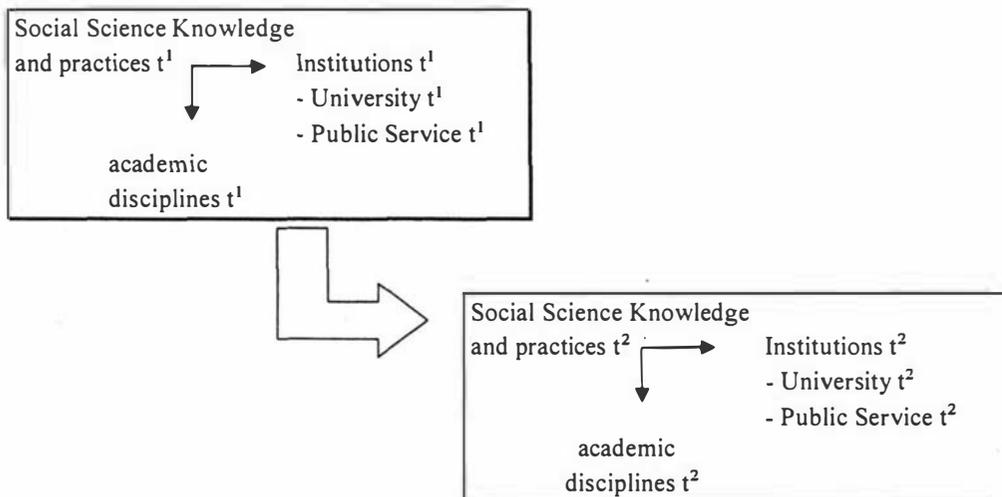
Advancement of Knowledge: A critique

Is social science written into the script of the Western world because of an intrinsic nature and does it unfold, typically in the teleology of the modern? Is social science and its institutionalisation inevitable with rational social science driving institutionalisation? Is a rational social science simply a choice between good and evil (Popper) or are contemporary disciplines best understood as a continued re-invoking of a good idea (Abbott)? Perspectives that suggest the march, step, leap or do-se-do of reason prove susceptible on a number of fronts. First, a reading that suggests that the history of social science is a series of ideas taking hold, and being replaced or fused by others over time is not in itself an explanation. Rather, the 'interpretation' is either *reportage* of the very phenomena it is attempting to interpret, or alternatively, it fails because it does not suitably elucidate any mechanism that drives the process. To suggest what *is*, is not to account for

how and *why*. Reportage is an inadequate basis for historical sociology and does not suggest how things change, nor why – to use Archer’s phrase (1988) – there is ‘cultural embroidery rather than syncretism’ (p. 158).

Second, the approach is wont to separate the cultural dimension of change (i.e. ideas and practices) from the institutional context in which they develop. According to this particular account, institutions are some ‘reflection’ or appendage of ideational developments. The diagram below illustrates how in each epoch social science research is seen as giving rise to a particular institutional configuration. The flow is in one direction – *from* knowledge and practices *to* institutions.

Figure 5.1: Knowledge growth and the institutional mirror



The ‘explanation’ short-changes the reader by failing to provide an understanding of the institutional context within which social science research changes when arguably the institutional context has significant bearing upon this development. In rejecting the above diagram, institutions, roles and social context are not some appendage to embellish any ideational account; rather they constitute the very mediums through which an understanding of change in social science research is possible. Assuming social scientific research ideas have their own trajectory, which are then mirrored in the rise of Universities and other ‘monuments’ of knowledge production within the rational/modern society, is inadequate because it relegates and defuses the various interests, conflicts, tensions and

compromises taking place *across*, and *within*, such sites at particular points in time. It is these very relations that require attention and this requires us to have an understanding of structure, as well as more sophisticated tools around culture.

The third limitation centres on its inability to account for the form that social science ideas take in different places at different points in time. Crucially, in a universalist sweep these accounts fail to provide insight into why and how cross-national developments differ. It points away from purposive, detailed studies that should lie at the centre of the historical-sociological enterprise. In failing to account for differences across time and space there is a danger of ignoring the very variations that require clarification to achieve adequate grasp of social science research development (cf. Sztompka, 1993, p. 145). Hence the framework is misguided, because ideas and practices do not simply develop, unfold, and diffuse through institutions, and neither is it the task of historical sociology to catalogue such a process. The framework suffers from myopia – explaining social science research developments by focusing on the institutionalisation of social science research within one or other domains and failing to view a broader, more complex, environment. Focusing solely on the State or the University neglects other social, economic and political relationships, and dispenses with an investigation of the various institutional configurations. Any account must attend to institutional shape and form, illuminating how the institutional and the ideational/cultural interact. The institutionalisation of social science is not always in academic forms; it may also be within political life, professions, educational practice, and State policy (Wagner and Wittröck, 1991a). Moreover, specific dimensions of this configuration have competing rationales, with diverging expectations underpinning University systems across different countries, for example. Within nation states it is possible to discern multiple traditions of ‘the’ University, and an institutional configuration that focuses on the rise of the University (or Universities) is liable to present modern University systems as somehow inevitable and conjoint with the emergence of social science research. Given that there is no trajectory or common driver for social science research, a framework must facilitate accounts that have texture and depth and be capable of explaining the nuances and intricacies within a particular time and place.⁴ Hence what is required is a historicising of the account. The historical sociologist must be prepared to roll up their sleeves, forsake the notion of a broader script or plot, and write (and unearth) their own account.

Fourth, an account that emphasises the ‘movement of ideas’ pays inadequate attention to agency. In its grand sweep, the meta-narrative carries all before it. At best there is a place for brilliant individuals making discoveries and innovations; at worst individuals are bystanders or media through which ideas unfold.⁵ There is no developed role for human agency, nor for a structural context, and nothing of the relationship that holds between them. Time is transmogrified into an agent. The ensuing account relegates group struggle, conflict, compromise and action. Both agency *and* structure fall from view which is problematic on an ontological and methodological level and empirically at odds with the development of social scientific research practice.

Finally, the emphasis on the ‘unfolding’ of social science research has particular implications for analysing the emergence and constitution of academic disciplines. One consequence is to depict the natural emergence of social science, and within that, the natural segmentation of disciplinary ideas. Yet fields of knowledge and disciplines are themselves contested domains, brought into being by the specific involvement of actors and structural circumstances. Social science as a collection of disciplines was not predetermined: each sub-discipline has a heritage, and experienced birth pangs of delivery. A theoretical framework must be capable of clarifying this which requires us to put actors, interests and structures ‘back in’.

Summary

A number of the tensions that emerge when distinguishing between internalism and externalism are captured by the discussions of Hacking (1991) and Kelley (2002). Thus internalist history becomes ‘tracing ideas in terms of an inner dynamic...’ with actors shaped solely by individual psychology (Kelley, p. 2). Such narratives may be contrasted with an externalism that ‘place ideas in the context of their own particular time, place and environment’ (p. 2). The distinction is thus about whether ideas and discoveries are the focus, or whether greater attention is given to the milieu of political, economic, social and cultural environments. For example, what conditions shape the reception of certain ideas, and why are these ideas ripe for promulgation and institutionalisation rather than falling by the wayside? Internalism will not deliver an adequate account of social science research development and endogenous approaches at best offer a philosophical discussion that outlines the contours of particular ideas and highlights their similarity or difference to

earlier ideas. The fundamental weakness is that a framework – and the accounts to which it gives rise – cannot be pitched solely on the terrain of intellectual history. It is necessary to look beyond accounts that emphasise a dominant drive of social science to simply unfold, whether in a movement of rationality, Reason, Open Societies or fractal distinctions. A framework must acknowledge that human conduct is conditioned by – and in turn conditions – cultural and structural dimensions.

Causal Structuralist Frameworks

The previous array of accounts suggested rational social science research was something of a non-question. Competing frameworks that have sought to introduce external considerations into the explanation of social change include causal structural and materialist frameworks. Approaches elevating structural considerations within explanation developed and can be explored through diverging schools of thought; via Marx, economic determinists, as well as those who since the beginning of the 20th century have drawn upon biological analogies, including Talcott Parsons. Whilst the term ‘functionalism’ has ambiguity and relative elasticity it broadly argues that circumstances, situations and interactions produce patterns of institutions. Here the functionalist net is cast very wide, including those modernisers who find their apogee in the form of Fukuyama’s declaration of the ‘end of history’ (Bell, 1962; Fukuyama, 1992; Levy, [1966] 1996).⁶ Functionalist forms span whole literatures of post-War ‘mainstream’, Marxist, and neo-Marxist, sociology.

The functionalist agenda: evolutionary roots

The roots of functionalism lie in an evolutionary analogy drawing upon the biological organism, in an attempt to understand society and social change whereby change centres upon evolution and adaption.⁷ These influences can be traced through a range of 19th-century thinkers including Comte, Marx and Mill (Manicas, 1987; Sztompka, 1993). A strong link exists between these theoretical developments around evolution and a belief in progress. Peter Manicas captures this 19th-century fixation:

the idea of a ‘generalizing’ ‘functionalist’ and deterministic and a historical social science owes much to the naturalistic philosophies of progress (1987, p. 53).

Those advocating evolutionary schemes in the social sciences suggest human societies develop from relatively simple to more complex forms of organisation. It views the maturity of society as similar to the growth of an organism, connecting the biological and social worlds. Under the scheme there is a directionality of social forms; the unlikelihood of regression, and the positive possibility of progression (Wright, 1983; Giddens, 1984, Sztompka, 1993).

A major point of disagreement is around the driver of adaptation (wholly structural or also cultural), and the place of human agency within social change. In the (non-materialist) work of Herbert Spencer, the most ardent social exponent of Darwinian evolutionary concepts, agency disappears altogether and purposeful, meaningful and directional social change (Holmwood and O'Malley, 2003, p. 45) is introduced in place of opportunistic selection. Marx has been widely interpreted as vacillating about the role of agency, with differing interpretations being read into the 'young' or 'older' Marx. The distinction is reflected for example in Fleischer's (1973) division of Marx's anthropological, pragmatological and nomological periods. The resurrection of the 'young' or 'humanist' Marx is presented in contrast to 'older' 'materialist' Marx of *Das Capital* where he writes that once societies had identified laws 'it can neither leap over them nor decree the abolition of its national phase of development' (cf. also, Elster (1986); Lloyd (1986, 1991); Sztompka (1993); Noble (2000)).

An evolutionary approach to explaining change and social science is T.H. Marshall's threefold distinction between civil, political and social rights ([1964]1973). For Marshall, citizenship rights unfolded in a sequence, whereby British citizens gained their civil rights between 1650 and 1832, their political rights between 1832 and 1918, and their social rights thereafter. The latter involved the institutionalisation of the welfare state and the development of social science research. His evolutionary perspective is tempered by the fact that war and the Beveridge report had quickened the process of social rights into (velvet) revolutionary change. The welfare state raised society onto a higher moral plane with a heightened sense of community and increased living standards. The development of social science research for the welfare society may be situated within this evolutionary Marshallian framework. The deficiencies of such a notion mirror those discussed earlier: the framework ignores contingency and contextuality, fails to acknowledge diverse paths

of change and multiple causes, does not account fully for exogenous considerations, and downplays human capacity.

Parsonian functionalism

The writings and influence of Durkheim, mediated through British anthropological influences of Radcliffe-Brown and Malinowski, set the stage for developments in functionalist theory in the post-1945 period. Both Radcliffe-Brown and Malinowski argued against the notion that practices spread across societies in some form of diffusion following discovery or emergence (Baert, 1998). Talcott Parsons drew functionalist theory into the theoretical mainstream, and it contributed, in the United States particularly, to a polarisation of systems sociology and problem-oriented empirical research (Baert, 1998, p. 99)(see also Mills, 1959; Gouldner, 1970; Skocpol, 1984a). Although Parsons is open to a range of interpretations (Alexander, 1985, p. 10; Sciulli and Gerstein, 1985), one broad reading suggests that people internalise shared central values and norms of society within the need-disposition of the personality structure. In meeting these needs individuals unknowingly contribute to meeting the ongoing needs/function of the system: adaption; goal attainment; integration; latency. The result is that Parsonian functionalism has implications that include:

- societies conceptualised as entities with self-forming, self-regulating processes (drawing on the biological metaphor);
- roles and institutions exist because they supply necessary conditions, and the key is to understand social functions;
- specialised institutions develop to supply either the means/end for solving internal/external problems;
- individuals occupy social roles, of which they are not fully conscious, operating as 'functionaries';
- functional prerequisites need to be fulfilled for society to survive (social needs as relating to the whole), with the social system manifesting a 'hidden teleology' of social needs;
- the persistence of practices because of their effects (which are often unintended) which secure equilibrium or integration of the social system;

- models of social change that incorporate notions of ‘structural strain’ and ‘differentiation’ of system parts and complexity – paralleling the new species within biological determinism (Giddens, 1979; 1996; Wallace, cited in Cohen, 1989; Colomy, 1992; Turner, S., 1992; Baert, 1998; Noble, 2000).

The Parsonian vision is one of human society evolving from the primitive to the modern with the separation /autonomy of institutional spheres, and increasing functional specialisation and interdependence resulting in improved effectiveness and efficiency (Smelser, 1959; Seidman, 1998). Functionalism came to dominate Anglophone sociology, and drove theoretical perspectives in New Zealand once sociology gained an institutional footing in the 1950s and 1960s (cf. Forster, 1969; Webb and Collette, 1973; Baldock and Lally, 1974).⁸

How does functionalism ‘explain’ the emergence of social science research? Frequently, functional-evolutionary determinism occurs where there is an unsubstantiated jump from *reportage* to *explanation*. Take for example the relatively uncontroversial statement (from someone who, it should be noted, is not a functionalist):

The coming of modernity in the emerging industrial civilization and nation states of the nineteenth century entailed new forms of understanding of social reality (Wittröck, 1991, p. 333).

In the above statement ‘entailed’ serves reportage/factual purposes rather than explanatory ones. It posits a chronological statement along the lines of ‘social science research came into being after industrialisation had taken place’. The functionalist obfuscation occurs when these two events are yoked together and a necessary or logical relationship is posited rather than something that is temporal/contingent. Such enthymemic reasoning for example occurs when Daniel Lerner explores the emergence of Western social science research and functional requirements:

Social science developed largely as a way of perceiving, evaluating, and correcting the frictions and tensions generated by the high rate of individual mobility and institutional change in modernizing society (1973, p. 7).

In Lerner’s interpretation, mobility and change generate a raft of problems calling for new ways of conceptualisation, theorisation and explanation. At the centre of such analysis lie

functionalist notions of 'strain' and differentiation. Strains in society lead to speculation being replaced by empirical inquiry and a result is the emergence of social science research.

Modernisation – what Noble (2000) dubs 'neo-evolutionism' – continues the functionalist-evolutionist theme, suggesting there is one history and that it is universalistic, deterministic, and closed. Modernisation is encapsulated in Levy's ([1966] 1996) *magnum opus*, as well as in post-industrial theory and 'end of ideology' type debates (e.g. Bell, 1962). In Levy's work, the distinction is drawn between modernised and non-modernised societies – a distinction based on the 'use of inanimate sources of power and/or tools' (p. 11) and his perspective emerges strongly:

All of the present relatively nonmodernized societies will change in the direction of greater modernization. They will change in that direction regardless of whether their members wish it or whether the member of some other society or societies wish to force such changes upon them (p. 31).

Modernisation tends to dichotomise societies into traditional/modern – with the traditional seen as cyclical and stable before they reach the point of take-off (Adam, 1995, p. 29; Knöbl, 2003). The approach assumes a deviance or lag approach to explaining variation. As an explanatory format, it would suggest that the practices of social science research are associated with modernised societies (and colonised societies such as New Zealand), and on the horizon for traditional societies 'whether they like it or not' (cf. Parker, 2000, p. 19).

Some problems of functionalism

The most direct way to dismiss functionalist accounts of social change is to approximate Gellner's (1964) argument that history is not some 'world-growth story', and that any attempt to fit change into such a framework is inadequate given empirical, logical and moral counter-arguments (see also Sztompka, 1993). However, the roots of functionalism reach deeply into explanatory social theory and the subsequent accounts they generated. As the standard-bearer of post-War theory, functionalism merits more detailed discussion. Anthony Giddens has waged war against the post-War functional orthodoxy, expressing a desire to remove the language of functionalism from the terrain of social theory. Promoting his own structuration theory as a 'non-functionalist manifesto' (1979, p. 7), he aspires to eliminating the 'pernicious' influence of functionalism from the contemporary

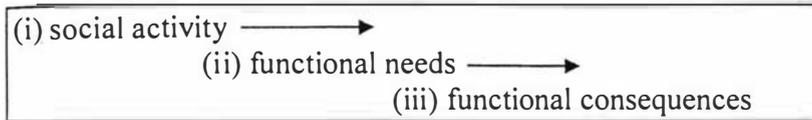
scene (1984, p. xxxi). His position is both avowedly anti-functionalist and anti-evolutionary, eschewing all forms of teleology, as well as forms of evolutionary mechanism that, although not teleological, draw upon the language of adaptation and mutation ([1981] 1995, p. ix, p. 15). Four criticisms levelled by Giddens help expose the explanatory limitations of functionalism.⁹ Discussion is underpinned by a useful distinction drawn by Cohen between functionalism-as-resource and functionalism-as-topic (Cohen, 1989). Functionalism-as-resource focuses on outlining a set of functional needs that exist within a system; functionalism-as-topic emphasises that a functional system must meet an unspecified set of functional needs, although there is less concern with outlining what these may be. Drawing on this insight of Cohen, functionalism-as-topic is explored given it is presupposed by functionalism-as-resource.

Life as agency

At root Giddens' attack on functionalism is ontological rather than methodological, asserting that needs must be ascribed to agents and the latter are the hub of social theory. Functionalist (and structural) approaches are said to be 'strong on structure, weak on action' (Giddens, [1976] 1993; see also Thompson, 1989, p. 58). Giddens thus introduces the insights of phenomenology which he believes must be taken seriously – albeit not *too* seriously – when theorising and in seeking to transcend the agency/structure dualism (see also Giddens, 1996, p. 88 for his view of purposive action). Causal and non-causal structuralism (as with pure forms of evolutionism) dispense with the active subject altogether (cf. also Hechter, 1992).

Functionalism, Giddens argues, reduces human agency to the internalisation of values (needs and desires), which may ultimately result in individuals being viewed as 'cultural dopes' (cf. discussion of Parson's actors, 1979, p. 52; also Archer, 1988, 1995). Giddens outlines three instances where functionalist reasoning is used, but where it is dispensable once an appropriate account of agency is developed: the implicit counterfactual (that sets up a relation that calls for explanation rather than explaining it); feedback processes; and the notion of 'conspiracy' (Giddens, 1984, pp. 294–7).

Implicit counterfactuals: reference to the counterfactual is an acknowledgment of Cohen's (1978) argument for 'consequence laws', which may be expressed diagrammatically in the relationship:



Cohen writes, 'In establishing "consequence laws" we set up generalizations to the effect that whenever a given social item is functional for another, the first social item is found to exist'. But this, argues Giddens, is not really an explanation because it fails to supply the *mechanism* for change. The burden is upon functionalism to illuminate the mechanism involved, whereby a functional need leads to a particular consequence. This, he argues, functionalism is unable to achieve:

functionalism is a social theory in which the teleology of the capital term function is either redundant or falsely applied (1996, p. 91).

Thus consequence laws should be replaced by counterfactual reasoning whereby the functional need (ii) (or 'system need') is a counterfactual, referring to what would have to be the case for a particular consequence (iii) (or system) to come about, be maintained, or be transformed. The link between (ii) and (iii) is therefore about identifying the *conditions* that have to be met if the consequences are to come about. Therefore, 'has to' is not a property or a need of the system. Hence it is not an explanation, provides no causal links, and the notion of homeostasis is redundant.¹⁰ Seen in this light, the relationship becomes contingent and empirical, rather than being a logical one.

Feedback: a key functionalist tenet is the argument that the system 'repays' or eliminates dysfunctions. In Giddens' view, however, the notion of feedback for functional needs is simply erroneous. Although not denying that 'payback' occurs within systems, it is powerful agents that do this, so he argues. Thus a system's need presupposes actors' wants (1996, p. 93). Elsewhere he writes, '...the only teleology involved in social systems is that comprised within the conduct of social actors' (1979, p. 112). If societies do not have needs other than those of individuals, then notions of 'adaptation', 'need' and other such biological metaphors are superfluous and mischievous. The result is:

Not even the most deeply sedimented institutional features of societies come about, persist, or disappear, because these societies need them to do so. They come about *historically*, as a result of concrete conditions that have in every case to be directly analysed; the same holds for their persistence or their dissolution ([1981] 1995, p. 18; italics in original).

Moreover, functional reasoning is also tautological, with the definition of survival prerequisites being made in terms of the qualities of the survival system e.g. selective adaptation. Stated simply – what is, must be, and it merely identifies the object to be explained, as the earlier criticism of Daniel Lerner amply illustrates.

Conspiracy: given the focus on agency, events happen because someone designs them and carries them out. Giddens' emphasis on agency accommodates unforeseen consequences – plans may be foiled and the complexity of social life, combined with the actor reflexivity and awareness, dilute conspiratorial notions of particular practices and activities. Therefore individuals and groups may make history, but not all of the circumstances or events are of their own choosing.

Life is constituted

A second criticism is functionalism's inability to treat social life as 'actively constituted' through the doings of its members, and thus to recognise the recursive nature of social life. There is a consequent failure to deal adequately with time, and a disregard for how social life is produced and reproduced through time. Functionalism marginalises diachronic theorising and elevates the synchronic (Döbert, 1981; Sztompka, 1993), with the implication that:

The characteristic view of the synchronic/diachronic distinction is that to study a social system synchronically is take a sort of 'timeless snapshot' of it. Abstracting from time, we can identify functional relations, how the various contributing elements of a social system are connected with one another. When we study change diachronically, on the other hand, we analyse how they change over time. But the result of this is an elementary, though very consequential, error: *time becomes identified with social change* (Giddens, [1981] 1995, p. 17; italics in original).

Typically, functionalists leave events in time to historians and sociologists are left to pick up structural dimensions of social systems without acknowledgment of the temporal depth of these.

Life is power

There is the tendency of functionalism to treat power as a secondary consideration – almost as an accessory of social theory. Giddens sets himself the task of recentring power within social theory and analysis (albeit in a very different manner to Foucault or a generation of neo-Marxists). Interest in the manifestation of power leads him to develop an analysis of authoritative and allocative resources, locating power within both agency and structure.

Life as negotiated

Finally, functionalism does not acknowledge the negotiated character of norms, and the diverging and conflicting interests of society (Giddens, [1976] 1993, 1996; see 1984, Chapter 1 for his ontological critique of functionalism). The orthodox functionalist emphasis of empirical/normative consensus is one that does not allow us to build the existence of conflict into our overall framework.

Together such criticisms suggest serious deficiencies in the functionalist explanation of the rise of social science research, and indeed of functionalist developments *within* social science more broadly. Accepting functionalist reasoning results in societal conditions (e.g. urbanisation or industrialisation) being seen to provide the catalyst to the metamorphosis of social science research. As Wittröck et al. (1991) comment:

[the functional-evolutionary understanding follows] a clear causal assumption that social science knowledge develops in response to the functional imperatives inherent in a particular pattern of social interaction (p. 69).

Thus, under a functionalist schema, the historical sociologist rolls up their sleeves but little ink is spilt in the pursuit of historical practice. Instead there is a linearity whereby institutions and ideas are the result of societal pressures (or 'needs'). Arguably, however, social science research and modern institutions shape each other 'in mutual interplay' (Giddens, 1991a, p. xiii). Hence, social science research and its institutions are not simply a product of the environment and circumstances, but actually serve to act upon that

environment, in turn conditioning and shaping the environment. With respect to the welfare state he argues, for example:

The circumstances which originally prompted the emergence of welfare systems were thoroughly permeated by, yet also provided a basic prompting to, innovative developments in social science (Giddens, 1991a, p. xiv).

Giddens acknowledges that functionalism, unlike action theories, can accommodate unintended consequences and can analyse institutional and large-scale progress more easily than can micro-perspectives (1979, p. 7; 1995, p. 16). Yet functionalism fails to include contingency, agency, and multiple causality. It is an enduring strength of Giddens' project that he identifies these deficiencies and sets to them with such gusto.

Giddens is unequivocal in his rejection of all types of evolutionary history whether gradual or stagist (1984, [1981] 1995, p. 72). In discussing historical materialism he observes connections between functionalist modes of reasoning and Marxist views of progression, stagist, and endogenous change ([1981] 1995, p. 15; see also p. 27, p. 223, p. 230). Giddens critiques historical materialism believing the search for a single set of explanatory mechanisms, idealist or materialist, is doomed (1995). Thus particular attention is devoted to that brand of 'evolutionism', in terms of which societies move through an irreversible series of stages (tribal, feudal, Asiatic, capitalistic, and communist). Given that there is no universal history of change, invoking a universal periodisation of social development is misguided. Specific criteria (e.g. complexity or increasing mode of production) are erroneously held up as the motor driving the direction and stages (1984, p. xxvii–xxix), and Marxism portrayed by Giddens, in common with other functionalist writings, is over deterministic. Institutions do not work behind the backs of social actors who are uninvolved in their production and reproduction. Elsewhere Giddens writes:

In explaining social change no single sovereign mechanism can be specified; there are no keys that will unlock the mysteries of human social development, reducing them to a unitary formula, or that will account for a major transition between societal types in such a way either (1984, p. 243).

Summary

Over-playing materialism and structure as the antidote to explanations that defend the 'growth of reason' proves problematic. Macro functional explanations do not account for why differences emerge in institutionalised forms, ignore micro-macro links, and render agency impotent in particular situations. It is necessary look to the broader intellectual, institutional and political 'constellations' which may influence developments, and within these constellations to outline local and contingent resources (Wagner and Wittröck, 1991b). Developments are partial, and are continually hampered and reversed; breaks are never clear, uncomplicated, or immune to revision or reversal.

Following attacks on functionalism and modernisation during the 1970s, a number of paths were open. These included a focus on micro-perspectives, theories embracing a more cultural turn, and sophisticated interpretations of materialism whereby 'history is seen as produced by the complex interplay of human activities and structural conditions' (Sztompka, 1993, p. 173). The reconceptualisation of individual and social action produced responses that were variously Marxist, Weberian, and economic. As well as bringing actors back in to explanatory frameworks, there was also a greater willingness to explore cultural and structural context. While some simply reified and unified culture, swapping it for structure as the explanatory driving force, or alternatively viewed culture in the mirror of structure, others sought to capture historicity and institutionalism within social life, paying attention to context, process and temporality. A *mélange* of promising debates developed around agency, culture, structure and their interrelationship, and these are the focus of the next two chapters.

Chapter Five Notes

¹ Here it is worth pointing to the discord between British and Continental traditions of philosophy. The British empiricist tradition (Bacon, Locke, Berkley, Hume and Mill) believed that the ultimate source of all our knowledge is observation. The Continental tradition (Descartes, Spinoza, Leibniz) emphasised the intuition of clear and distinct ideas.

² Gellner makes the valuable distinction between progress theories that are episodic and evolutionary. The episodic ones are a sole transition that may be stretched over centuries. Evolutionary change on the other hand is about an ongoing 'world growth story' – with emphasis placed upon the journey rather than the arrival (1964, p. 9).

³ To clarify, I am arguing that Popper's understanding of rationality can be used to justify the emergence and existence of social science research. The previous chapter explored the status of the knowledge claims that resulted from using the fallible social science methodology that was seen by Popper as the only defensible version of 'rationality'.

⁴ The march of ideas contains an inherent narrative rather than merely listing the emergence of particular ideas at specific times. As Sztompka (1993) notes, this may involve 'the working out of a transcendental meaningful telos *in* history, as expected by metanarratives of philosophical history' (p. 87).

⁵ As Sztompka (1993) neatly puts it, 'It is the time that gives birth to the great individuals' (p. 264).

⁶ The field of inquiry could easily fragment into a diverse range of systems theory and neo-functionalism responses of which Luhmann's structural-functionalism is one.

⁷ Within this section there is some inevitable overlap of materialist evolutionary accounts with the idealist evolution introduced earlier in the chapter (cf. Granovetter, 1979).

⁸ Baldock and Lally (1974) note, 'Implicit functionalism and unbridled eclecticism would seem to be the most common orientations detectable in the studies we have analyzed' (p. 278). Functionalist influences remained strong within British and New Zealand anthropology (see Piddington 1950, 1957).

⁹ Discussion here draws heavily on Giddens' critique of functionalism and historical materialism (Giddens, [1976] 1993, [1981] 1995, 1984; Giddens and Pierson, 1998). In addition to Giddens, Archer provides another scholarly *entrée* into discussion (1988, 1995).

¹⁰ 'A homeostatic process is merely a set of causes and effects unless it can be said to be operating with some "end in view" or to fulfil some need' (Giddens, 1996, p. 92).

Chapter Six

Putting Actors Back In

Introduction

One of the major criticisms has concerned the expunging of actors from explanatory frameworks, be it frameworks that emphasise the evolution of ideas or forms of structuralism. This chapter explores how actors are best reintroduced into the discussion and a human face placed upon history. The chapter profiles individualism and social actors to illustrate the potential weaknesses that must be superseded in a viable account. In the first section, *methodological individualism* and *rational choice theory* explore a tradition that conceives of atomised individuals and their interrelationship as the explanatory matrix. Second, it explores understandings that are delivered through a social action tradition – whereby actors are social actors and *people in society*.¹ Satisfactory incorporation of agency into a framework is a necessary – although by itself not sufficient – requirement for explanation. It is not just *whether* actors are reintroduced into the discussion but *how* this occurs, and the implications of this for structure and culture within any chosen framework.

False Humanism, Erroneous Economism

The reply to the question: ‘Are concepts in social science analysable to interests and activities of individuals?’ – appears relatively simple – ‘Of course!’. Few contemporary social scientists in search of explanation would reduce all to structures or evolutionary ideals and deny *some* measure of individual capacity in their explanatory scheme. But the individualist also answers affirmatively the more focused question ‘Are concepts in social science analysable *solely* to the interests and activities of individuals?’ A strongly individualist lineage (traceable through Mill, Popper, Hayek and Watkins) puts individuals at the centre of explanatory frameworks and has been highly influential post-1945. It is necessary to explore and counter some of the arguments they advance.

For Hayek ([1943] 1973) an interest in studying ‘social phenomena’ (whether classes, nations, societies or economies) does not mean it is appropriate to start from direct observation of these. The fallacy of ‘misplaced concreteness’ is avoided under Hayekian social science by introspection – exploring man’s actions and their unintended and undesigned consequences (the ‘inside’ as opposed to the ‘outside’ (p. 44)). Inquiry focuses on the attitudes of individuals webbed together in systems of relationships, and how such attitudes form ‘recurrent, recognizable and familiar elements of the structure’ (p. 35). Social science methodology consists of careful reflection upon these attitudes, which allows the effects of actions in combination to be seen (p. 41). Collective terms or wholes come via the mind and are constructed as mental models, having properties only from their construction. Brodbeck ([1954] 1973a) captures this well when she notes how Hayek distinguishes between ideas that are *constitutive* of social phenomena and those which are *about* these phenomena – between ideas that cause and ideas that explain social structure (p. 105). Thus collective terms cannot be used in explanations because they are themselves theories – ‘They do not stand for definite things or classes of things’ (Hayek, [1943] 1973, p. 46). The sociologists’ error is to use these wholes as explanatory – and historians having recognised these wholes make the mistake of searching for their laws of development. Under Hayek’s schema, the whole must instead be ‘built-up’ from considering the individual parts:

... rebuilding from the directly known elements the complex and unique structures which we find in the world, and...tracing from the changes in the relations between the elements and changes in the wholes...(Hayek, [1943] 1973, p. 62).

Although all social phenomena have physical properties, Hayek’s ‘subjectivism’ denies there is any unity of scientific method. (To return to the example of Chapter Three – in explaining the consumption of malt whisky the raising of the arm tells us far less than a description of a person’s state of mind and mood.) Hayekian methodological individualism is literally that – how to set about gathering data about individuals and then constituting wholes (Scott, [1961] 1973).

Popper, in contrast, is a methodological individualist on epistemological grounds, retaining only a minimal ontology. Whilst Hayek begins with individualism and uses it to explain collectivities, Popper finishes with individualism; whatever method is used 'we should never be satisfied by explanation in terms of so-called 'collectives' (1945 II, p. 98). Structures are no more than recurrent patterns of action and the aim of social science is to break structures down into their constituent parts (Porpora, [1989] 1998). Popper rejects explanations that are holistic, historicist and law-ridden (see also Chapter Three). For him, social explanation does not unearth an inner mechanism of social progress towards a predetermined destiny, and theoretical history is replaced by an empiricist focus on individuals making and remaking their destinies in response to the problems they face in social life (Popper, 1957). Assuming society has moved into the light of the Open Society, social analysis is undertaken in a piecemeal and fallible fashion, with unintended consequences acting as a brake upon widespread holistic change.

Similarly, working within the individualist tradition forged by Popper is J.W.N. Watkins who rests his argument on two principles – the individualist constraint on explanation, and the alteration principle (Miller, 1996)(or what Kincaid (1996) calls 'exhaustion' and 'determination' respectively). First, social science explanation inevitably rests on individual foundations:

- social processes and events should be explained by being deduced from
- (a) principles governing the behaviour of participating individuals and
- (b) descriptions of their situations (Watkins, [1952] 1973a, p. 149).

Social science thus explores dispositions to allow the identification of how in various situations they lead to 'principles of social behaviour' (Watkins, [1952] 1973a, pp. 156–7, p. 165). Two sorts of explanation may thus be given by methodological individualists: explanation *in principle* involving typical dispositions being bracketed or bundled together; alternatively, more detailed explanations may be compiled, which involves filling in the descriptive fine-grained details of general dispositions. While explanations in principle are sometimes merely a shorthand way of explaining events, 'rock-bottom explanations' of large-scale social phenomena must, however, consist of individual dispositions and beliefs and interrelationship of individuals.

Given that individuals may remain anonymous with only typical dispositions attributed to them, explanation is not necessarily reduced to compiling a series of biographies. Rather, the skill of the social scientist is in identifying dispositions and constructing models which will illuminate how in particular situations they generate regularity ([1957] 1973b, p. 177). Under Watkins' schema, the practising historian explores behaviour that is assumed to be purposeful but not necessarily fully rational ([1952] 1973a, p. 163). For Watkins, the holist errs in seeking direct access to some overarching structure and behaviour, when such institutions (or situations or events) as 'State' and 'Capitalism' are the result of individual dispositions, situations, beliefs, and physical resources and environment (Watkins, [1957] 1973b, pp. 167–8). Thus:

society is not some unimagined sort of organism, but really consists only of people who behave fairly intelligibly and who influence each other directly and mediately, in fairly comprehensible ways ([1952] 1973a, p. 153).

Social scientists do not therefore have 'direct access' to an overall structure of interacting individuals and the measurable phenomena is the interaction of individuals (Watkins, [1952] 1973a, p. 150, p. 165). The alteration principle – his second principle – locates change and causal efficacy in human agency. Thus humans make their own history and they make it in the circumstances that are of their own choosing – subject to the availability of perfect information. Watkins concedes that individuals can be frustrated by the actions of other individuals but denies 'that an individual is ever frustrated, manipulated or destroyed or borne along by irreducible sociological or historical *laws* ([1957] 1973b, p. 176, footnote 8; italics in original).

The mantle of individualism has been taken by an economic humanism of rational choice theory (RCT), which imports economics into the realm of social science and history. Economics is used to inform the model of man that explains the full range of human endeavours. Under this 'sociological rational choice' (Hechter, 1992), there is a particular reading of neo-classical theory, emphasising instrumental rationality and a hierarchy of preferences for the calculating and utility-maximising individual (p. 368)(see also Kiser and Hechter, 1991; Kiser, 1996). Differences do exist between individualists and rational choice theorists, and among the latter themselves, on assumptions of dispositions and individual capacity (Zey, 1992; Zafirovski, 1999; Sil, 2000). But there is the:

Fundamental proposition that social analysis must first and foremost attend to the individual, whether it is to his/her actions, material interests, emotional gratification, or cognitive dispositions (Sil, 2000, p. 363).

Rational choice theory has meant an individualist renaissance within historical explanation (cf. Coleman, 1990; Kiser and Hechter, 1991; Hechter, 1992; Kiser, 1996). As a collection of positions there are common weaknesses in all forms of individualism that cast doubt on individualism's claims to be a framework for historical explanation.

A critique

Unnecessary dichotomy: Many individualist writers present anything that is not individualism as holism, and scapegoat the latter to bolster their own atomistic arguments. Popper, for example, offers little explicit defence of methodological individualism – focusing instead on demolishing the historicist case (Ackermann, 1976, pp. 167–8). Similarly, Hechter (1992) argues that structuralists presume that different individuals will act uniformly to the same structural conditions (p. 370). Proponents offer a short menu of individualism/RCT or holism, either removing a collectivist option or denying it a fair appraisal. Hayek, for example, outlines that many 'holists' argue that social phenomena can be understood only by considering the totality of everything. While this may be an account of some holistic positions, it does not devalue more sophisticated collectivist arguments, nor commit the investigator to methodological individualism (cf. Lukes, 1996). Watkins' view that 'holism is well-nigh equivalent to historicism', even if it were plausible, does not alone entail individualism ([1957] 1973b, p. 168). And whilst the point is not to throw structuralism a lifeline, there is a reluctance to contemplate more subtle positions that lie both between, and outside, individualist and holist camps. At times, overstatement renders a bizarre individualist standpoint, as Brodbeck's comment on Hayek's overstated view suggests:

It is one thing to hold that for every name there must be a corresponding individual thing, for example, that the state is literally a superperson, or, perhaps, a person among persons. It is quite another thing to believe that such terms refer to nothing at all (Brodbeck, [1952] 1973a, p. 106)(see also Ackermann, 1976, p. 168, for a parallel view of Popper).

Writing in a similarly 'super-' vein, Watkins suggests that sociological holism entails that a superhuman agent or factors are at work. This is wide of the target, and group minds, superhuman agents and such, receive more attention than is rightly due them. Frequently the methodological individualist position is sugared with a set of truisms – 'Society consists of people. Groups consist of people. Institutions consist of people plus rule and roles' (cf. Lukes, 1996) and so on.²

Eliminating sociality: The individualist position engages in a stark reductionism. Popper and Watkins' individualism reduces the social dimensions of life to the activities and interests of individuals. In reducing society to dispositions, Watkins for example states:

Society is a system of unobservable relationships between individuals whose interaction produces certain measurable sociological phenomena. We can apprehend an unobservable social system only by reconstructing it theoretically from what is known of individual dispositions, beliefs and relationships ([1952] 1973a, p. 165).

Such reductionism is not self-evident despite individualist views to the contrary (cf. Sayer, 1992, p. 118) and difficulties arise in reducing social life to individuals whether for descriptive or explanatory purposes.³ Despite attempts to dissolve society there are concepts that stubbornly remain – undefinable group properties over and above the individuals within any group (Brodbeck, [1958] 1973b, p. 290). Gellner ([1956] 1973) draws upon the distinction and suggests individuals may adopt a holist conception in description but will stop short of endowing abstraction with flesh and power – and therefore *explanatory power*:

History is *about* chaps. It does not follow that its explanations are always in terms of chaps. Societies are what people do, but social scientists are no biographer grand série (p. 263; italics in original).

Working with separate descriptive and explanatory notions (as Gellner and Brodbeck would seem to suggest) is problematic given the bond between social ontology and explanatory methodology; *how* something is described influences the explanatory dimension (Archer, 1995, p. 20).

Conceptualising the individual and their dispositions in isolation from society entails a number of inherent difficulties. Mandelbaum's ([1955] 1973) notion of social facts suggests, *contra* individualism:

one cannot understand the actions of human beings as members of a society unless one assumes there is a group of facts which I shall term 'societal facts'... (p. 223).

Societal concepts are used in attempting to understand aspects of individual behaviour and 'sociological concepts cannot be translated into psychological concepts *without remainder*' (p. 227; italics in original). A difficulty arises if these social facts are seen to build social structures in a law-like manner. Being human is an inherently social consideration because humans are born into societies and their thought and action are conditioned by societies' pre-existing individuals. A specific individual acquires a societally oriented pattern of behaviour from individuals who were themselves born into a society that was independent of them (p. 230). Rational choice theorists continue in the belief that the individual is the 'elementary unit of analysis' (Hechter, 1992; also Kiser, 1996). Others would question its assumption that social structure can be reduced without remainder (Zey, 1992; Archer, 2000b; Parker et al., 2003).

Whilst RCT accounts may pay lip service to structure (and culture), these dimensions are not attributed with the same significance that is given to instrumental action or rational individuals. Thus:

The ontological primacy of individuals makes their response to the situation theoretically more significant than the factors defining the situation in any general causal explanation (Sil, 2000, p. 362).

Variants of RCT ignore the context in which choosing, action and activity are being tackled. But as Parker et al. (2003) write: 'Rationality is only one mode of relating to an environment. It is not the environment itself' (p. 35). Yet under RCT rationality becomes the explanatory *modus operandi*, thus by-passing the fact that the context and sociality of life result in individuals being partly dependent variables.

Methodological individualists have countered the claim of irreducible social components by pursuing the argument *ad infinitum*, into the 'Garden of Eden', where, finally, individuals are cornered who are not born into a pre-existing society. But rather than

vindicating individualism, such an argument contains flaws reminiscent of John Rawls' invitation to adopt an Original Position (Rawls, 1971). A foundational Original Position is impossible because fully divesting ourselves of human social characteristics leaves nothing meaningful to bargain with. In the case of individualism/RCT, doing so results in vessels rather than human beings. To admit of this social component does not entail that all agency is what Archer would call 'the gift of society' (cf. Archer, 2000c). Moreover, the social inheritance also points to the variability of these structural and social dimensions. The argument advocating a re-setting of the clock is problematic given that we are locked into particular social and historical forms. Nor does admitting this preclude social theorising and lead to a hyper-relativised view of social knowledge. Given the sorts of *people* we broadly are, and that the range of dispositions to be found in people of particular social and historical contexts are socially induced dispositions (Mandelbaum, [1955] 1973, p. 284) – it is problematic and unnecessary to venture into Watkins' territory of dispositions of anonymous persons. Further, attacking holism does not undermine a viable form of sociological emergence (sentiments that emerge out of social intercourse of human beings) within sociological explanation. Gellner ([1956] 1973) notes that:

The real oddity of the reductionist position is that it seems to preclude *a priori* the possibility of human dispositions being the dependent variable in an historical explanation – when in fact they often or always are – and secondly to preclude the possibility of causes, in the sense of initial conditions, being a complex fact which is not desirable in terms of the characteristics of its constituent parts alone – which in turn seems often to be the case (p. 254).

The rational choosing individual working within the constraints of imperfect information and complexity fares no better. Given the individual make-up of stable and unchanging preferences there is weight to criticisms that the approach is essentialist (Cruickshank, 2000; Archer and Trotter, 2000). The thinnest reading of RCT rationality produces a single principle of cost-benefit analysis, but it is also difficult to simply append actors as emotional beings who make a full range of human choices (Zafirovski, 1999). Under a thicker version of rationality economic man is left clutching emotional baggage with a view that somehow his baggage can be weighed and travel separately from the agent. Thus efforts include the addition of non-rational variables – values, beliefs etc. which under

RCT become an instrumental means to an end. Emotions, however, constitute a dimension of who we are (cf. Archer, 2000b & c).

Constraint on agency: A vaunted strength of methodological individualism is that it passes the power and capacity for change over to individuals, rescuing the future from the entrapment of cultural and structural rhythms. Individuals become makers not takers of the future, and power resides in nations, localities and communities who, through collective action, re-fashion the future. As Watkins ([1957] 1973b) notes:

no social tendency exists which could not be altered if the individuals concerned both wanted to alter it and possessed the appropriate information (pp. 168–9).

As a political mantra for post-war society this is understandable. But as a theoretical framework there are *major* difficulties with this view that parallel Keynes' statement about equilibrium, that 'in the long run we are all dead'. We do not live in the long run conceived as an unbroken trajectory of time stretching out and with sights focused on such horizons, but in a series of short runs. There are always constraints that impinge upon us *here, now* – and those that we can remove are in turn constrained by the resources and awareness of the possibility of collective action that exist in the short and medium terms. It may be that in principle all social tendencies can be removed in the very, very long run; and that is all to the good, but is the concern of generations to come – and should be left there. (The issue also being that future generations will face identical conceptual issues of time but differing substantive ones.) At root a methodological individualist/RCT weakness is the inability to conceptualise time appropriately because in overlaying time frames the long run obscures the series of shorter frames and the constraint of the past within the present. In part, such difficulties arise from a failure to recognise that emergent structures have powers which both constrain and enable, but emergent structures are castigated by individualists as bearing the sins of holism, 'Conceiv[ing] of a social whole as something which endures through generations of men' (Watkins, [1957] 1973b, p. 171). There are, however, things that constrain, things of which actors are unaware *here* and *now*, and things that can only be changed beyond the short term, perhaps over a series of generations.

Throughout methodological individualism/RCT there is a failure to adequately conceptualise time. The suggestion is either to re-set time, or time is over run by suggesting the boundless and endless possibilities of long-run change. Reaching forwards and craning backwards is of little use when addressing practical and analytical tasks that literally face us.⁴ At issue are questions of historicity, temporality and process. In seeking to understand society, we are inevitably interested in the nature of society that exists at present or has existed in the past. It is possible to avoid re-setting time by having a suitably theorised notion of temporality and process.

Methodological individualism/RCT lacks recourse to viable temporal resources and, indeed, because of its stunted notions of time and society it is problematic to view it as a historical sociology at all. Methodological individualism/ RCT remains unaware of the theoretical and empirical possibilities of social science and its potential for diachronic social theory remains limited because:

[Methodological individualism] is concerned solely with the actions of individuals within a given sociocultural context and is incapable of recognizing as a theoretical problem the question of how the situation came to be as it is (Goldstein, [1956] 1973a, p. 271).

The methodological individualist takes the institutional framework as given, and focuses upon the individual, asking questions within a particular context rather than undertaking a diachronic exploration of how the context came to be (cf. Goldstein, [1956] 1973a, p. 274). This obscures the very things – preconditions, context of history, legal and institutional framework, the distribution of power and privilege (resources and knowledge) – that should ignite interest and be a focus and constituent of historical sociology.

While some versions of RCT have developed broader notions of social life, its account is still not a persuasive one. Thus it sees all action that achieves ends defined as ‘rational’, and considers that causal mechanisms emerge *a priori* from general theory that guides research (cf. Kiser and Hechter, 1991). Whilst action is ostensibly studied to observe instrumental rational pursuit of utility maximisation, the theory is used to guide research and complete research when the empirical evidence is lacking (Somers, 1998). As she writes:

Rather than hypothesized, discovered, problematized, deduced, induced, or generalized, the explanatory context of this law comes ready-made, causality is inscribed exogenously (p. 751).

What inevitably emerges is a covering law of explanation (cf. Kiser and Hechter, 1991). This parsimonious response neglects meaning, the social context, and the series of events through which something comes into being – considerations that should lie at the heart of a historical sociology (Calhoun, 1998; Sil, 2000).⁵ The attempt to incorporate narrative notwithstanding (Kiser, 1996), RCT fails to recognise meaningful human behaviour and withholds objective and causal constraint from structure.

In brief, the methodological individualist/RCT framework offers little to the explanatory task. The ontological thesis that all social entities are reducible without remainder to logical compounds of individuals is true but trivial (Little, 1991; Lukes, 1996; Archer, 1988, 1995). Although the methodological individualist position recognises that events can be unintended (cf. Watkins, [1952] 1973a, p. 165), its denial of ‘emergent’ properties is a fundamental weakness. Undoubtedly a series of methodological individualist interpretations exists from the very strong (the starting point for much methodological individualism) to the very weak (cf. Kincaid, 1996). The diluted version suggests ‘some reference to individuals is a necessary condition for any full explanation of social phenomena’. Returning to the question asked at the beginning of this chapter, if individualists are simply arguing that societies neither exist nor act independently of individuals, then there is no disagreement. But a reductionism and explanation that is *solely* individual is untenable because there are relations that cannot be dissolved to individuals.

Stripped of sociality, groups of individuals cannot do the methodological work expected of them. A place for actors must be retained – actors are part of explanations, sometimes even distinct individuals described in all their biographical glory. But it is circuitous and misdescribed to consider such a stance individualist; it is better to refer to appropriate explanation as being both peopled and structured. In summary, within the individualist/RCT account there are individuals, but no people; moreover, there are few interactions, legacies, influences, or, seemingly, little conflict. The account denies

structure and culture explanatory value, furnishing sociology with an underdeveloped notion of explanation.

Acting Up: Social actors, people and rule followers

'Social actors' and 'people' are in contrast to previous 'individuals' or 'rational choosing individuals'. Whereas previous methodological positions reduce activities to individual dispositions, action or utility, action stances move away from atomised individuals being the explanatory or interpretive currency. Instead, focus is on the interactions, production and negotiation of social life *among* individuals. Thus whilst it is always possible to be a solo actor, in recounting any scenario there is typically a supporting cast and *always* some backdrop of social custom, belief and tradition. Campbell (1996) summarises such approaches as social situationalist where action becomes *social* action, or what Ritzer (2001) refers to as the social definition paradigm.

Below a series of traditions are lightly sketched within the context of social action. Their starting point is frequently Weberian and anti-positivistic. Further influences include those of Wittgenstein, Husserl and the broader hermeneutic tradition of social analysis as making sense or *understanding* rather than causal explanation. This traces back to Wilhelm Dilthey's argument that the proper aim of the human sciences:

In the realm of human affairs [understanding] corresponds to what we designate as 'explanation' in the realm of knowledge of nature... understanding is the domain of all who are actively involved in human affairs, and differs from explanation by participating in life, which is possible only on the basis of life ([1883] 1989, p. 439).

As suggested in Chapter Three, positivist explanation of social life posits the discovery of regularities and social laws utilising procedures and methods from natural science. In contrast, social action approaches are inductive rather than deductive and argue that the social world is not one in its wholeness, and a reality does not exist (either in individuals or in structures) external to consciousness and awaiting discovery. Instead, the emphasis is upon the process of social life, the meanings attributed to particular situations through the ongoing interaction of individuals. Shared meanings may allow agreement on the intersubjective 'truth' of particular matters, whilst immersion and empathy, particularly captured in the notion of *verstehen*, are essential.

The *symbolic interactionism* of Herbert Blumer (1969) emphasised the importance of action in human group life and collective behaviour. In criticising the positivist tradition within sociology, he sought to replace the measurement of a supposed objective social structure with a focus instead upon action and interaction and as interpreting particular social situations. Society and structures become no more (and no less) than people 'doing things together' (Sztompka, 1993; Layder, 1994; King, 1999a). Joint action can become orderly, giving rise to institutions and structures, thereby constructing the social realm. Thus for Blumer:

Human society is to be seen as consisting of acting people, and the life of a society is to be seen as consisting of their actions (1969, p. 85).

Given that we 'make' rather than 'take' roles or act upon dispositions, the aim of social science becomes that of exploring interaction and the meanings, symbols and languages that allow human beings to actively create their social worlds. For Blumer this requires the close study of the social world (e.g. processes of 'negotiation' and 'labelling'), something quite at odds with the survey empiricism, abstract Parsonian system-building or macro historical work that had held sway.

Whilst exhibiting some affinities with symbolic interactionism, in other respects Erving Goffman's sociology has its own peculiarities, particularly the vivid use of metaphor and richness of *Encounter*, *Asylums* and *Stigma* (1961, 1962, 1963). Again, there is a particular emphasis upon face-to-face encounters, and the processes that allow the ongoing accomplishment of social order, including the use of rules, obligations and tacit knowledge (Williams, 1998). For Goffman, action towards the environment is on the basis of the meaning attributed to it, and his work emphasises individuals' ability to reflect on action and manipulate environments.

Alfred Schutz-inspired phenomenology emphasised common sense and the taken for granted stocks of knowledge upon which we draw when acting in the social world (1970). In common with symbolic interactionism it explores everyday activities and interactions. In place of a behaviouralist interest in laws of human action the emphasis is upon the subjective framework of an actor perspective and uncovering what the social world *means* for individuals. Meaning arises from the world as day-to-day activities take place, with individuals trying to make sense of, and come to terms with, the world around them and

in so doing they draw upon stocks of knowledge and typifications. The primary task of analysis is not to uncover social structure, but to explore how the world is experienced by individuals and:

to describe the process of meaning establishment and meaning interpretation as these are carried out by individuals living in the social world (Schutz, 1973, p. 219).

Garfinkel's ethnomethodology (1984) also prizes the pattern of daily interaction, the day-to-day routine of everyday life, and the way actors use common-sense knowledge in the local production of social order. The object is:

To treat practical activities, practical circumstances, and practical sociological reasoning as topics of empirical study, [and by] paying attention to the most commonplace activities of daily life... (p. 1).

In making common-sense visible, it shows how people draw upon background understandings and implicit rules, namely, 'How people analyze, understand and act in the social world' (Heritage, 1998, p. 175). The approach suggests that people make joint sense of the world together, using background knowledge to pad out the full meaning of what people say and do. These daily interactions create order but presuppose a range of dimensions that are taken for granted. Meaning must be continually worked at and 'action and meaning are always tied to particular contexts and cannot be understood from an external or objective standpoint' (Layder, 1994, p. 83). Unique events are explored in their naturalistic setting, interaction is examined and the role of language is frequently emphasised through conversation analysis.

Finally, working within the Wittgensteinian tradition of rule-following, Peter Winch's *The Idea of a Social Science* (1958) argues against basing social science on the methods of natural science and makes a sustained argument about the impossibility of explanation:

the notion of a human society involves a scheme of concepts which is logically incompatible with the kinds of explanation offered in the natural sciences (1958, p. 72).

For Winch social relations are unsuitable for formulating generalisations and scientific theories. He rejects argument for the unity of method (including J.S. Mill's view that understanding social institutions involves observing behavioural regularities of participants

and creating generalisations) and argues that to be comparable to natural science, the concepts/criteria according to which the sociologist judges that within two situations the same things have happened must be understood in relation to the rules governing sociological investigation. Winch's view is that: 'sociological knowledge is the kind of knowledge possessed in implicit and partial form by the members of society rendered explicit and complete' (Macintyre, 1973, p. 16). Unlike natural science, social science however has two sets of rules, i.e. *what* the sociologist is studying as well as *his study* of it are human activities. Human decisions cannot be determined by antecedent conditions and therefore for Winch:

When we speak of the possibility of scientific prediction of social developments of this sort, we literally do not understand what we are saying. We cannot understand it, because it has no sense (p. 94).

Such social things in Winch's understanding are unintelligible outside the mode in which they are being considered. Given his position that there is no distinction between the world and language (i.e. that reality is structured by language), Winch strictly circumscribes the limits of possible comparisons – activities must be understood in their own terms. The sociologist's task becomes conceptual not empirical – to elaborate various forms of life, describing and recognising social rules. Social science and the study of society are thus more akin to understanding a language than to understanding a machine.

These social paradigms (excepting Winch's) place particular focus on empirical activity, emphasising the social actor and context for social analysis. Whereas previous criticisms noted how methodological individualism and RCT afforded little social basis to individual identity and activity, social action remedy this deficit with an empirical focus upon the doings of individuals within relationships. However, the position of the social actor is achieved at too high a price.

Critique

Missing structures: In the first instance, structure 'goes missing in action'. Social action approaches offer an inadequate account of structure and agency. Positions fail to acknowledge that social structures are both the pre-existing context within which action takes place, and a constraint upon that action (they impact, block or restrict agency)(cf.

Layder, 1994). Social action approaches must be capable of incorporating (to use Goffman's dramaturgical metaphor) that the agent works the stage but has little choice of background scenery, and that performance is hampered and assisted by props that litter the stage. It is thus a mistake to underplay the fact that micro-interactions take place within an existing social structure and are shaped by that structure (Lloyd, 1986, p. 188). Within these approaches, structure is engulfed by cultural form, and it is difficult to see why one cultural repertoire is performed rather than another (Porpora, 2002).

Despite the ostensible process emphasis of social action, time remains underdeveloped and the past as informing and sedimented within the present is not fully acknowledged. Such action or micro-positions are seemingly indifferent to structure – and the opportunity to conceptualise an appropriate link between action and structure is lost. How do the micro, face-to-face, everyday encounters relate to structural contexts, institutions, and power? (Layder, 1994, p. 74). The insight of symbolic interactionism – that no individual is separate from society, and that we are born into formed societies – whilst an improvement on methodological individualism, remains underdeveloped and structurally impotent. So although it avoids reification, this is at the price of failing to conceptualise agency and structure and denying that there are existing real structures prior to mental processes with causal efficacy.

The 'bulging' social actor: The flip-side of paying inadequate attention to structure is the inappropriate conceptualisation of action. The 'social situationalist' positions work from shared assumptions that all meaning is necessarily social, and linguistic constructivism takes communicative acts as paradigmatic of all human conduct. The focus on meaning, communication and language serves to eliminate subject location in the consciousness of individual actors. In the words of Campbell, there is now nothing 'under the skull' (1996, p. 43; also Archer, 2000c). Campbell accuses such approaches of replacing 'motive' by 'reasons' and in doing so:

social situationalism actually denies the very principle which lies at the heart of the interpretive action tradition... action is voluntary, willed behaviour and not simply behaviour which is 'meaningful' (pp. 147–8).

Little is said about the specific types of action that occur in cultural settings. Humans are denied creative power and never presented as using their meaning-creating capability to actually accomplish an action, transcend or transform a situation:

the individual human being... has been completely dissolved away, stripped of both mind and body, now no more than a ghost in the social machine (Campbell, 1996, p. 151).

Denying explanation: Within the social action approach there is an emphasis upon interpretation or description as against causal explanation (Layder, 1994; Campbell, 1996). If sociology consists of actors constantly making and remaking their worlds then it is soon reduced to descriptions and trivia (Turner, J. 1992). The appropriateness of an interpretive or micro-tradition is seen to go beyond simply a heuristic device within social science. As Alan Ryan notes (1973):

It is easy to picture Verstehen as simply a heuristic device whereby we put ourselves in another man's shoes in order to inspire hypotheses about his behaviour, the scientific side of our activities then being confined to testing these hypothesis (p. 7).

This failure to grasp the heuristic of empathy was identified by Carl Hempel as the flaw in Collingwood's *Idea of History* (1946) which advocated history as an imaginative encounter of re-enactment with understanding rather than explanation. Actions for Collingwood were expressions of human thought. Unlike nature which concerns itself with the outside of an event, for the historian it is integral to get inside the event 'to think himself into this action, to discern the thought of its agent' (Collingwood, 1946, p. 168). It is unclear what a social science based purely on a hermeneutic or interpretative perspective would resemble. One writer ponders:

Would it offer explanations, generalizations and models? Or would it simply be a collection of concrete hermeneutical readings of different societies? Would any notion of law or regularity emerge? Should causation have a place in such a science? Strict adherence to the interpretic dogma would seem to lead to a form of social analysis that is highly descriptive and not at all explanatory (Little, 1991, p. 86).

Giving up the hard currency of explanation for the lavish description of ethnomethodology, the lived experience of phenomenology, or Collingwood's empathy, is to travel with debased coinage. Abandoning the explanatory project and conceding to social situationalist narrative or interpretivism has grave implications for historical sociology.

Further implicated in social situationalism is a relativistic linguistic turn exemplified by Winch's language games where there is no appeal beyond forms of life, neither to an external reality nor to independent criteria of rationality (Hollis, 1994, p. 157). Quite simply there are different ways of seeing the world given our social contexts. Denying the key constituents of social theorising (nature of reality, rationality, the possibility of explanation) Winch pushes relentlessly to the terminus of multiple language games. The position that is remaindered is deficient, and, in explanatory terms, worthless:

true explanation in terms of reasons must entail some account of the causal background; it is also true that a causal account of action will require a corresponding account of the intentions, motives, and reasons involved (Macintyre, 1973, p. 28).

While many social action traditions have led to detailed empirical programmes focusing upon actors and activities, and approaches such as symbolic interactionism have been absorbed into the sociological mainstream, they do not provide an appropriate basis for an explanatory, causal sociology.

Re-conceptualising Structures and Embracing the Cultural Turn

Discussion in this chapter and the previous one suggest a range of difficulties emerging from notions of evolutionism, structuralism, individualism and social action. Within these approaches are contained widely different notions of structure and agency. For methodological individualists and symbolic interactionists, structure is merely the pattern that results from individual behaviours. Hence categories such as economy or class are always produced by and reducible to individuals and do not themselves have causal efficacy (Porpora, [1989] 1998). In turn, some structuralists identify structure as law-like relationships among social facts with autonomous existence and ability to constrain and direct action, thereby reducing individuals to mere bearers of structures. Aside from these polarised views of society, some theorists working within network theory and structuration theories have begun to develop alternative conceptions of social structure (and its

reproduction) that draw upon structure as social relationships among social positions, and structure as rules and resources respectively. These will be discussed in subsequent chapters.

A further theoretical development with important consequences for the field of historical sociology has been the attention given to culture, and its relationship with material structure and agents. Whilst some early anthropological works presented culture as a series of learned behaviours and as a whole corpus of practice, beliefs and customs that united a society (Archer, 1988; Sewell, 1999), there has been stress on the competing and cross-cutting dimensions of culture. Whilst the outcome of these intersecting influences is complex, for some theorists culture is best viewed as autonomous and with causal efficacy, rather than simply a reflection of class, nation, or colonisation, and retaining a part in explanation rather than an archaeology of discursive practices. The emphasis has gone from reading a unitary and coherent cultural code of a particular society and interaction, or from seeing culture as the product of particular sites (media and art). It is now viewed as complex, multiple, changeable and beyond national boundaries (Sewell, 1999; Spillman, 2002), all of which has consequences for explanatory frameworks.

Compared with material social structures (wealth, resources and role) culture introduces something more 'fuzzy' to the debate – norms, identities and symbols (Sil, 2000). This cultural influence has been taken to have subjective and objective considerations. While some proponents of culture have pushed towards wider shores of relativism/anti-positivism, emphasising the discursive aspects of culture, this has failed to recognise:

the (potentially) *autonomous causal significance of cultural or political discourses* in shaping the complex event sequences that it examines (Emirbeyer and Goodwin, 1994, p. 1436; italics added).

Still others have taken the causal significance of culture as licence to present culture as a causal variable. But in making culture independent of the social and institutional setting, culturalism substitutes for a structuralism, whereby culture engulfs structure (see Sewell, 1992, 1999).

There is an alternative conceptualisation of culture that neither reifies it, nor turns it into another variable. Thus cultures block possibilities for action and prevent things from being

articulated in a public discourse or from being favourably interpreted. But cultures also enable. The role of the historical sociologist is to explore how currents of cultural and political discourse are drawn upon. In so doing, a more robust notion of culture is suggested whereby cultural discourses, narrative and idioms:

have emergent properties – and internal logic and organization of their own that requires that they be conceptualized as ‘cultural structures’ analytically separate from social structure (Emirbeyer and Goodwin, 1994, p. 1438).

In this light attention turns to how cultural structures pattern action with different interests refracted through the cultural lens, and with structural influences both enabling and constraining. Of particular interest are the limits on cultural variety and how reproduction occurs with groups framing choices and interaction in given situations and making sense of roles in particular environments (Spillman, 2002). Thus human agency both reproduces and innovates upon the cultural corpus, while this innovation is not itself unbounded. The aim of the thesis, therefore, is for culture without culturalism, retaining an historical and comparative perspective.

Summary

Taken as a whole, this chapter has focused on approaches that re-centre actors within social theorising. Two particular ways of doing this were illustrated: methodological individualism/RCT, and social action approaches. Major criticisms were levelled at both sets of attempts to advance a viable account of agency and the implications this had for explanation. Missing is the capacity to link the social individuals with the intellectual and institutional context, thereby reintroducing a politically structured and cultured domain. An adequate account must include *structure, culture and agency*, and arguably it is here that some of the most promising recent work has been conducted in historical sociology. William Outhwaite (1998) makes the comment:

The real opposition in contemporary social theory is between a somewhat ecumenical centre and the empiricists, functionalists, systems theorists, rational choice theorists and perhaps poststructuralists outside (p. 34).

It is time to look more closely into the ecumenical centre of social theory, at approaches that attempt to relate agency, culture and structure within social theorising.

Chapter Six Notes

¹ Some writers use the term Individualism to subsume both methodological individualist and interpretivist approaches of Blumer, Goffman and Garfinkel (cf. King, 1999a, Archer, 2000a).

² Susan James (1984, p. 20) also argues that wrong inferences are drawn from this truism. She suggests that individualism is accepted because it coincides with intuitions (p. 8). Bhaskar (1989) has also drawn attention to the intuitive hub of methodological individualism.

³ This distinction between descriptive emergence (undefinable group concepts) and explanatory emergence (laws of social science as not those of individuals) is Brodbeck's ([1958] 1973b, p. 309). The point is also developed by Goldstein ([1952] 1973b).

⁴ Thus Archer's critique discussed later is partially correct in noting it is a 'sociology of the present tense' (Archer, 1995, p. 86). I would agree that the major issue is the failure of individualists to theorise temporality.

⁵ Some developments of RCT have begun to introduce narratives of choice situations.

Chapter Seven

Explaining Change: Culture, Structure and Agency

Introduction

Taken together, previous chapters point to four dimensions that a workable framework for the substantive task must address. First, whether their substantive focus allows both intellectual and contextual changes to be explored, and whether explanation satisfactorily resolves the relationship *between* ideas and institutions. Second, how attention is given to the diachronic as opposed to a series of synchronic snapshots. Third, whether frameworks incorporate culture, structure and agency, and successfully conceptualise their interplay. Finally, the extent to which frameworks generate contingent rather than teleological explanations.

Either cognitive or context change: The first consideration is whether the overall thrust of a framework is to explain the growth of ideas or institutionalisation, and how this is achieved. Two ways of conceptualising the emergence of particular practices or disciplines exist: intellectual/cultural considerations, and, alternatively, focusing upon their context. Within this contextualism, there arises a range of questions about the weight and interrelationships of considerations. Is the emergence of ideas and institutionalisation wholly ideational, or driven by material conditions? To what extent do frameworks stress exogenous (external) and endogenous (internal) drivers of change? Some frameworks allow for accounts of change that focus on both the emergence of ideas and their context of development.

Synchronic/diachronic: Whilst a number of previous predominantly structuralist forms produce 'frozen' snapshots, frameworks must be cognisant of time and incorporate 'temporal vitality'. This requires a dynamic account of both the reproduced nature of social life and – something that is often disregarded by a number of social action approaches – the *longue durée* of institutional time. A framework must include a theorisation of change mechanisms, exploring why change occurs from one period to another, rather than merely describing actions or discourses in isolated frames.

Culture, structure and agency: Frameworks are variously reductionist in emphasising structures, cultural determinants or the voluntarist role of agency, when accounting for change. But in taking such stances they leave individuals with no history, society or culture, or alternatively 'drain[s] such relationships of their active, subjective dimensions and their cultural contents and meanings (Emirbeyer and Goodwin, 1994, p. 1428). Frameworks that explicitly add or combine and seek to retain both agency and structures merit some discussion.

Teleology/ contingency: A distinction exists between approaches entrenching change in teleology (leaning towards essentialism), and those recognising the place of contingency. The historical/sociological projects these give rise to differ – on the one hand determining the underlying influence, motor, or perhaps even laws, of social development, and on the other, unpacking and tracing contingency. Any framework must be capable of acknowledging the historical dimensions of social existence and institutions.

The range of frameworks discussed to this point have overall analytical 'stances' shaped by philosophical underpinnings and assumptions of the nature of social life. A recurring theme of previous discussion, and the central pillar of the summary below, concerns the relationship of culture, structure and agency. This triadic relationship must be appropriately theorised, and success in so doing holds the key to the successful incorporation of time and contingency within frameworks.

Table 7.1: Summary of theoretical approaches

	Emphasis on cognitive or institutional change and explanation	Synchronic or diachronic emphasis	Agency, culture or structural emphasis	Implied teleological emphasis	Summary of mode
Evolutionary and stagist	Cognitive. Institutions follow ideas	Diachronic	Both actors and structures absent	Life on an 'upward slope'	<i>Whiggish history of ideas</i>
Structural/Functional	Conditions shape institutions	Synchronic	Structures	Yes	<i>Systems</i>
Methodological Individualism/ RCT	Institutions follow choices, preference and dispositions	Synchronic 'sociology of the present tense'	Individual Actors or rational agents	None	<i>Economic humanist/ rationalist</i>
Hermeneutic	Cognitive. Meanings	Synchronic	Social actors	None	<i>Social actors</i>

At the heart of these discussions lies the question of how best to view social life, the nature of structure and culture, and whether agents are best viewed as *homo economicus* or *homo sociologicus*.

Resolving the Agency + Culture + Structure Equation

Summarising post-war debates around agency, culture and structure suggests sociological method formed around axes of individual/structural explanation of social life, whilst the concept of culture remained confined to anthropology. The anthropological notion of culture was viewed as monolithic and as binding the members of a society together. From the 1970s there were increased efforts within sociology to take the notion of culture seriously, although with little agreement on the precise implications of this. Discussion emerged about the place of, and how to forge a working relationship among, culture, structure, and agency. The purpose of this chapter is to explore these developments and to appraise the success of some of these endeavours.

The literature would suggest five approaches to the Culture + Structure + Agency equation. Methodological individualists conceive the resolution as more of a division than addition with structure and culture reduced to individuals. Similarly, structuralists would invert the calculation and proceed to dissolve agency and culture into structure. These are both problematic and as previous chapters address these in detail, little further is said about

them here. Discussed instead are three alternatives to the agency-culture-structure equation that see some merit in explanations containing agency, culture and structure. It is suggested that only one of the three approaches offers a fully theorised solution to this apparently straightforward, but nonetheless troubling, equation.

A practical pragmatism

Many sociologists and historians tackle the calculation by adding a fraction of agency, a measure of structure, or a dash of culture to produce a more satisfactory explanation. On the surface, what we have is a willingness to countenance methodological pragmatism with agency, culture and structure perceived as useful for description and/or explanation. Hence the first solution suggests something akin to a methodological *pluralism*. In this light, even defenders of methodological individualism concur, for example, that reduction is messy and difficult to achieve in detail, and to counter this they attribute some descriptive and pragmatic value to collectivist terms. Yet in the final push for explanation such descriptive pluralism gives way to reductionist individualism. Despite reference to collectivist terms by individualists – and individual terms by collectivists – in the final explanatory analysis one trumps the other.

For example, Susan James (1984) concludes that individualist and holist approaches are separate projects driven simply by competing value bases (p. 177) giving rise to the question of which form of explanation is more appropriate given the particular circumstances in question rather than which is ‘right’ (p. 155). For James:

These two projects – on the one hand a wish to investigate the constraining influence of social wholes, and on the other a desire to understand the shaping power of individuals – answer to different sorts of concern with the social world (p. 156).

In her view it is interests that drive the explanatory endeavour and reasons that lead to the adoption of individual or holist tools, and any problems raised by such Gestalt switches, are not addressed in her work:

Holists and individualists may both be said to have an overarching interest in understanding the mechanisms which operate in the social world, but beneath this unifying umbrella their eyes are fixed on different features on

the landscape, and their respective theories are to be judged in relation to the task they set themselves (p. 178).

Whether the social telescope or the individual microscope is utilised (cf. Hollis, 1994), offering a bird's eye or worm's eye view, one is soon pushed aside and, depending on preference or overt values, individual, culture or structure are *methodologically relegated*.

A pluralism in principle

The second solution to the equation, *pluralism in principle*, sums agency, culture and structure as a matter of general working principle – and they are seen as always and everywhere required for adequate explanation. Proponents argue that all explanations have *some* structural and individual and intellectual components, and that satisfactory explanation must invoke these. Despite generous helpings being heaped upon the methodological platter there is little regard to the theoretical palatability of combining these ingredients and the rationale for adding these continues to be theoretically underdeveloped. The second position resolves culture-structure-agency by ensuring they are together *methodologically elevated*.

Historians and historical sociologists adopting this approach include E.P. Thompson, who attempts to fuse the individual, cultural and structural to avoid reductionism and reification (cf. Thompson, 1995, p. 56). Thompson tells us *that* agency, culture and structure should be taken together but not *why* or *how* this is so. Similar endeavours are evident within sociology where a revised neofunctionalism, for example, claims the agency and structure puzzle as its very own (Colomy, 1985, 1992; Alexander, 1998, p. 163). Despite the broad revisionist and reconstructionist sweep, and the introduction of agency, construction, reconstruction and contingency (Alexander, 1985, 1998; Eisenstadt, 1985; Colomy, 1990a & b, 1992), neofunctionalism has a familiar pallor, retaining terms such as 'differentiation' and 'complex' (e.g. Eisenstadt, 1985, p. 103). 'Structural voluntarism' retains distinctive 'homeostatic' and 'adaptive' tendencies, and while imploring the combination of agency and structure offers less guidance as to why or how this should be done (Colomy, 1985). For example:

institutional development is partially shaped, but not fully explained, by such external and constraining social conditions as the predominant economic system (and the tensions it produces), prevailing political

structures and the distribution of power, hegemonic cultural codes, and the pattern of solidarity relations. According to this view, analysis of these conditions must be supplemented with examination of how concrete, collective, and individual actors, conditioned as they are by these encompassing structural parameters, contribute to the differentiation process (Colomy, 1985, p. 135).

Proponents of rational choice have also suggested that their developments are capable of accommodating structural and institutional constraints, as well as individuals who have their own values, preferences and utilities (Hechter, 1992, p. 369; Kiser, 1996, p. 260). While not denying that there are social and historical constraints on individuals, these constraints are still translated into distinctive choice situations which despite severing interests and pecuniary advantage, in the final analysis leave fundamental rational choice principles unscathed. Such qualification of positions (by rational choice theorists, structuralists and culturalists alike) is more about warding off criticism than establishing a solid bridgehead (Sil, 2000).

Other writers have engaged with questions around the linking of individuals-cultures-structures, causing them to reflect upon the importance of process and the relationship of sociology and history (Skocpol, 1979; Tilly, 1981; Elias, 1987; Sztompka, 1993). Some roots for this debate can be found in the work of Braudel (1980) who suggested that actors 'make history, but history bears them along' (p. 67), and recognised that historical time 'cannot envisage life as a mechanism that can be stopped at leisure in order to reveal a frozen image' (p. 48). Similarly, for Tilly (1981) the matter is:

to situate social processes in time and space. The work requires a permanent encounter of sociology and history (p. 52).

It is the promise of process that offers much to the potential resolution of this agency-structure-culture dilemma.

Against 'addition' of agency and structure

The third, more promising route to tackling the equation begins with exploring the interrelationship individuals, culture and structure have. Typically discussion begins from ontological roots of how to conceptualise practice. Perspectives thus resist simply summing the influences of individual, culture and structure, and instead explore their

interplay or *multiplication*. This is a theoretical and ontologically informed route, not chosen for its utility, nor as a methodological mantra to chant upon encountering the diverse social reality. This gives rise to a range of positions including structurationist and morphogenesis (both discussed below), whereby individual-culture-structure are conceived as *ontologically related*.

This third position, of adopting a *theoretical* grounding base for reconciling agency, culture and structure, is elaborated below. Many commentators would agree that such a reconciliation is a – perhaps even *the* – pressing question of sociology (Abrams, 1980; James, 1984; Hollis, 1984; Lloyd, 1986, 1993; Fielding, 1988; Thompson, 1989; Ritzer, 1990; Bryant and Jary 1991; Holmwood and Stewart, 1991; Bryant, 1995).¹

An appropriate conceptualisation of the relationship of structure, culture and agency has been hailed as the *tertium quid*, steering a path between deficiencies of individualism and collectivism, idealism and materialism. The solution reformulates the equation, moving discussion from the epistemological to the ontological, asking not ‘how do we explain social life?’ but rather more fundamentally ‘of what does social life consist?’ In Philip Abrams’ (1980) discussion of the agency-structure relationship:

It is the problem of finding a way of accounting for human experience which recognizes simultaneously and in equal measure that history and society are made by constant, more or less purposeful, individual action and that individual action, however purposeful, is made by history and society (p. 7).

Abrams outlines that accounts of individual consciousness must be constrained within an adequate explanatory framework. His strength lay in the bridge made between those adopting structuration as a methodological axiom (i.e. it will give better explanation) and those adopting it as an ontological axiom (i.e. it is *how* social life is constituted). A series of influential contemporary theorists and philosophers including Bhaskar, Abbott, Giddens, Bourdieu and Archer have addressed this question. The thesis explores three writers who have tackled this issue: Anthony Giddens, Andrew Abbott, and Margaret Archer, all of whom have interests in developing historical analysis.

Giddens' Structuration

The figure of Anthony Giddens towers over contemporary social theory; the detailed attention to his theory of social life in this section is a measure of the claims that are made for and the considerable acclaim that has met his writings. Giddens' project goes beyond merely criticising the staples of post-war sociology of functionalism and historical materialism; more ambitiously he aims at a more constructive and original basis for social theory,² including resolving the agency-structure dualism and with it a range of further dilemmas (micro-macro; objectivism-subjectivism; statics-dynamics) that had transfixed social science. Structuration was unveiled in *New Rules of Sociological Method* (first published in 1976), further developed in *Central Problems* (1979), and most comprehensively outlined in *The Constitution of Society* (1984). The resulting structuration theory instead of advancing one side of the dualism more forcefully, or adding a *souçon* of structure to agency-rich explanations (or vice versa), sought to revise the terms of the debate, conceptualising agency-structure not as dualism but as a 'duality of structure' (1984, pp. xx–xxi). For Giddens, structuration theory:

offers a conceptual scheme that allows me to understand both how actors are at the same time the creators of social systems yet created by them... it is an attempt to provide the conceptual means of analyzing the often delicate and subtle interlacings of reflexively organised action and institutional constraint (Giddens, 1991b, p. 204).

In *New Rules of Sociological Method* ([1976] 1993) he suggests interpretivist sociologies are 'strong on action, but weak on structure', whilst functionalist and structural approaches are 'strong on structure, weak on action'. In Giddens' view, neither structure nor agency has an independent existence – it is their relationship within *social practices* that promises to unlock theoretical development. In his reconceptualisation the knowledgeable ability of agents and recursiveness of social life are central to understanding the production and reproduction of social life (1984, p. 2). Following a fourfold exposition of structuration's interlocking themes of knowledgeable agents, structure, temporal vitality, and analytical validity, a critique is offered.

Knowledgeable agents

Given that the agency-structure dualism was to be replaced by an emphasis upon reproduced practices, a fundamental task was to conceptualise agent knowledgeability as an appropriate basis for action. Giddens attributes three dimensions to this knowledgeability: *practical consciousness* – knowledge tacitly held by agents; *discursive consciousness* – which arises when we reflect on action; and the *unconscious* – knowledge of which the agent is not aware. These levels of awareness and consciousness vary from human to human. Whereas a number of action theories such as ethnomethodology deal with the production of social life, they fail to fully address reproduction (Giddens, 1979). Moreover, such theories avoid theorising the unconscious and have little interest in the unintended consequences of intentional action. Because motivation of action may in part be unconscious, there is the possibility of unacknowledged conditions of action, and also the importance of accommodating unintended consequences (cf. Cohen, 1989). Thus:

The *durée* of day-to-day life occurs as a flow of intentional action. However, acts have unintended consequences; and ... unintended consequences may systematically feed back to be the unacknowledged conditions of further acts (Giddens, 1984, p. 8).

The reflexive monitoring of action is the process via which agents participate in the ongoing flow of social life. As Kasperson (2000) notes, intentionality is seen as a process rather than a concrete cause of a given action (p. 37), and this process dimension encompassing both the production and reproduction of social life is crucial to grasping Giddens' structuration theory (p. 117). Action and structure are both retained as constituents of ongoing social practices:

The production or constitution of society is a skilled accomplishment of its members, but one that does not take place under conditions that are either wholly intended or wholly comprehended by them (Giddens, [1976] 1993, p. 108).

Structure

In contrast to conventional usage, Giddens' conceptualisation of structure is stripped of descriptive connotations. Thus structures are not institutions; neither are they to be confused with the structural properties of social systems. Instead, structures are viewed as rules and resources that are the medium via which agents act: 'structure is both the medium

and outcomes of the reproduction of practices' (1979, p. 5). Agents draw upon constraining and enabling rules and resources in the achievement of social action. This view of structure is worth quoting at length:

Structure, as recursively organized sets of rules and resources, is out of space and time, save in its instantiations and co-ordination as memory traces, and is marked by an 'absence of the subject'. The social systems in which structure is recursively implicated, on the contrary, comprise the situated activities of human agents, reproduced across time and space. Analysing the structuration of social systems means studying the modes in which such systems, grounded in the knowledgeable activities of situated actors who draw upon rules and resources in the diversity of action contexts, are produced and reproduced in structure (1984, p. 25; underlined in original).

Structure exists as traces in the memories of agents ('memory traces') who utilise rules and resources, and exists only at the moment when the rules and resources are being employed in activities ('instantiation'). Thus structure takes on a virtual quality, 'There is no structure, in human life, apart from the continuity of processes of structuration' (Giddens, 1996, p. 101).

Temporal vitality

Unlike structural functionalism's equation of structure with system, within structuration theory systems are a pattern of social relationships that exist across time and space. They are reproduced relations between actors or collectivities, organised as regular social practices (1979, p. 66). In human society Giddens perceives no place for 'structure' or 'system'. Systems therefore:

...have structures, or more accurately, have structural properties; they are not structures in themselves. Structures are necessarily (logically) properties of systems or collectivities, and *are characterised by the 'absence of a subject'*. To study the structuration of a social system is to study the ways in which that system, via the application of generative rules and resources, and in the context of unintended outcomes, is produced and reproduced in interaction (1979, p. 66; italics in original).

Human interaction draws upon structural dimensions of signification (meaning), authoritative/allocative forms of domination (power), and legitimation (normative sanctioning) ([1981] 1995, p. 46). Structures do not exist in time-space, except in the moments of the constitution of social systems. Structural properties are institutionalised features of social systems, stretching across time and space. Regularised practices structured by rules and resources are 'deeply layered' in time and space, stretching over decades and large domains – the most deeply layered are institutions which can be analysed according to their modality and changes in the composition (1979, p. 65). Given such a view of structure, those aspects of society with more durable and 'observable' (real) existence:

are visible patterns of social relations that have become a routine feature of society by being continually reproduced in people's behaviour. Social systems refer to reproduced practices while institutions refer to reproduced rules and resources (Layder, 1994; p. 140).

At the heart of Giddens' project is the desire to fully integrate temporality within the duality of structure. He is not alone in noting the absence of sociological theorising of time/space (cf. Abbott, 1990; Adam, 1990, 1995; Urry, 1996). Structuration's interest in time is certainly encouraging given earlier criticism of some frameworks' (e.g. individualism and functionalism) inattention to time. Previous chapters noted how ethnomethodology/action theorists focused upon the day-to-day doings, and forms of evolutionary explanation alluded to the very long run. It is Giddens' view that these time frames could be reconciled via social practices – effectively bridging time from Schutz's *durée* to Braudel's *longue durée* (cf. Baert, 1998, p. 100). Such an approach avoids the functionalist error of dichotomising dynamics and statics, because:

on the practical level there simply is no way in which a 'static' analysis can actually be carried out; the study of social activity involves the elapse of time, just as that activity itself does (Giddens, 1979, pp. 198–9).

Nor can time simply be introduced to an analytical framework for this introduces the risk of committing the error of identifying time with social change ([1981] 1995, p. 17). From this theorisation Giddens develops a more unified mandate for social science where time

cannot be set aside or annexed by history; temporality remains crucial for social theory and promises to clarify the relationship of sociology and history (1979, pp. 7–8).

In a number of places, Giddens attributes to structuration the capacity to stimulate empirical work and practical social theorising. *The Constitution of Society* declares its intentions, 'Structuration theory will not be of much value if it does not help to illuminate problems of empirical research' (1984, p. xxix). He offers a checklist of which parts most impinge on empirical work including bounded human knowledgeability, the recursive nature of social life and double hermeneutic; social identities as virtual markers of structure; the importance of structural principles; power as a primary concept; and the contingent nature of constraint. Giddens suggests these ontological resources are synthesising concepts and allow fully grounded research practice (1984, p. 326). In moving from *how* to know social reality to the *existing* reality, Giddens develops a set of concepts which describe the social reality. While some question whether the ontological focus of structuration theory sets the limits to its usefulness (Gregson, 1989), a body of empirical research has emerged drawing upon the 'ontology of potentials' (Bryant and Jary, 2001). Giddens himself comments however, somewhat disapprovingly, upon the tendency of researchers to import his concepts *en bloc* into their work (Giddens, 1989, p. 294; 1991b).

Analytical validity

A process of 'methodological bracketing' allows examination of the interplay and recursiveness of social life within the duality of structure (1979, p. 80). Institutional analysis and strategic conduct thus become analytical (i.e. methodological) frames within the (ontological) duality of structure. For Cohen (1989):

Bracketing procedures simply suspend attention to certain facets of the subject-matter at hand *pro tempore* in order to clarify and analyse a particular aspect or dimension of this subject-matter that is of special interest (p. 67).

Hence social study explores the meanings and the worlds of actors, and examines the pre-existing social institutions. Within structuration, the human motor creates meaning within the production of social life, while the account of reproduction also allows some routine background humming.

In denying that social change searches for a single set of mechanisms, Giddens' social science becomes irremediably historical. While the concept of social practices and reproduction is not itself explanatory, empirical opportunities exist through analysts exploring contingency. The future is open, with unintended/intended consequences being the result of agents reflexively drawing upon structures (cf. Parker, 2000). The practice of historical sociology is made messy, but perhaps reassuringly so. Giddens' own analysis of social change would appear to be the exemplar of what is possible under structuration theory, presenting a series of structural principles – modes of institutional articulation, episodic characterisations, time-space edges, world-time – as tasters of analytical possibilities.

At this point, structurationist stock would appear to be soaring, trading on its ability to conceptualise agency, structure and time, and promising to deliver adequate analytical opportunities. In recognising reflexive agents, retaining structure, and elevating temporality structuration appears to offer a basis for historical sociology.

Structuration theory: a critique

A major weakness is soon revealed in structuration's conceptualisation of agency, notably the over-playing of agency at the expense of structure (Archer, 1982, 1995; Callinicos, 1984, 1987; Layder, 1998). The notion that agency/structure are bound together in social practices raises difficulties – difficulties that cannot be rectified within the edifice of structuration by placing a little more emphasis upon structure or a little more weight on agency. The over-active agent results from inadequately theorising agency – and by running together ideas of action and person, the result is that no one fails to be an agent. The conception of agency is not only overly generous but is also 'out of time'. Callinicos (1984, 1987) highlights that the knowledgeable of human beings and resistance is a-historical, seen as a general property of human agents. This knowledgeable is built into the original position that Giddens establishes, rather than recognising that the scope for agency varies. For Callinicos (1987):

Philosophical invocation of subjects' capacity to resist rather than for a historical examination of the variable conditions of action is indicative of his privileging of the pole of agency as against that of structure (p. 140).

In certain places within Giddens' work, the emphasis is upon transformation as against constraint (cf. Archer, 1995). However, a number of writers (e.g. Callinicos, Archer, Layder) would begin from how little control we have of our circumstances. Similarly, in Boyne's interpretation (1991, p. 55) power-knowledge shapes the concrete people that human agents are and social subjects are made with practical skills which result from historical processes over which they have little degree of control. Nor must we travel the full Foucaultian mile to concede Boyne a point here, we may simply ask of structuration: what are the boundary conditions and the circumstances that shape but do not determine? And may not structural constraint upon the individual be so great that that the option of agency is sometimes effectively dissolved?

Structuration's overemphasis upon agency leaves little space for a robust notion of structure. But a second difficulty emerges from the way Giddens engages with structures, giving rise to will-o'-the-wisp notions of 'memory traces' and 'instantiation'. There is a sense in which the problem of structure as something real and firmly embedded in modern social theory is simply side-stepped and something else introduced in its place. But as Layder (1994) notes of structure:

It is too closely bound up with the conventional notion of social structure as the wider context of social behaviour for it to be totally redefined (p. 141).

Giddens seeks to leave behind the problem of structure as we would traditionally recognise it. As Porpora ([1989] 1998) writes, 'the way Giddens has shifted the meaning of structure goes largely unnoticed' (p. 354). While for Giddens structure is rules and resources, and structure the cause and effect of actors' behaviour, it denies that relationships have causal effects. That structure shows resilience serves both as a useful analogy and an analytical lesson. Agency and structure are not so much untied nor the equation solved as the issues dissolved into the murkiness of social practices. The result, as Archer (1995) notes, is to 'flatten' the ontological depth of the social world (p. 94). The reality of structures seems to become dependent upon their instantiation. Yet structural characteristics and psychological characteristics would seem to defy compression into 'social practices'. Difficulties with structure are illustrated in Cohen's (1989) comment:

routinised practices effectively serve as the vehicles through which systems are reproduced, vehicles which are activated by agents through their

participation in day-to-day life. Hence agents reproduce the organisation of systems, but systems do not reproduce the organisation of agents nor do they activate the practices in which agents engage (p. 123).

But in some measure the point is that they surely do. To admit this is not to succumb to ruthless determinism nor reification. Giddens' talk of individuals as knowledgeable agents monitoring their action (unconscious, practical consciousness, and discursive consciousness) overdoes the enabling character of structure. The orientation of power towards agency (as amplified in Cohen's (1989) commentary), though laudable, is ultimately unhelpful for social analysis. Furthermore, structuration ignores culture – including people's histories, habits, customs and such (Meštrović, 1998). While for Archer (1988) under structuration 'the entire cultural corpus constitutes either order or chaos' (p. 100), William Sewell (1992) attempts to rescue Giddens' duality by arguing for resources being the effects of schemas (rules) so that they are 'manifestations and consequences of the enactment of cultural schemas' (p. 11), and this has the consequence of culture engulfing structure. Arguably the central place occupied by social practices restricts the development of a more viable position around agency, structure and culture, and a reconceptualisation of their interrelations is required. That emphasis on transformation is misplaced is also an underlying thrust of the attack by Meštrović (1998). Recognising constraints does not preclude the possibility of change and Giddens appears to present the either-or choice of transformation versus stasis. But in seeking to better negotiate the strictures of structure we are well advised to have regard for Rousseau's maxim – 'It is better to know one's chains for what they are than to deck them in flowers'.

To cap these criticisms of agency and structure, the conceptualisation of time is also found wanting, which serves to withhold adequate analytical purchase from structuration. As Layder (1998) suggests, combining agency and structure via social practices has the implication that:

The interaction of these two domains signifies a melding of very different time frames: that of the unfolding of situated activity – and the grand sweep of history as it manifests itself in the emergent institutional frames of whole societies. In this sense there is a convergence and continuous dialectic between objective, intersubjective and subjective facets of social life (p. 88).

Yet within structuration theory the bundling together of social practices serves to ‘sink’ rather than ‘link’ (Archer, 1995, p. 65), thereby obscuring forms and effects that are played out *over, in and through* time. The dissolution results in agency, culture and structure being denied recognisably distinct characters and properties and the consequence is a major analytical limitation. Such notions are at the heart of Archer’s powerful critique of Giddens’ elision ‘in the middle’ which results in the suppression of time that our analysis can never break into.³ Although Giddens sought to avoid upwards conflation of methodological individualism, and the downwards conflation of reification, he falls prey to central conflation (see diagram below), the consequences of which are that:

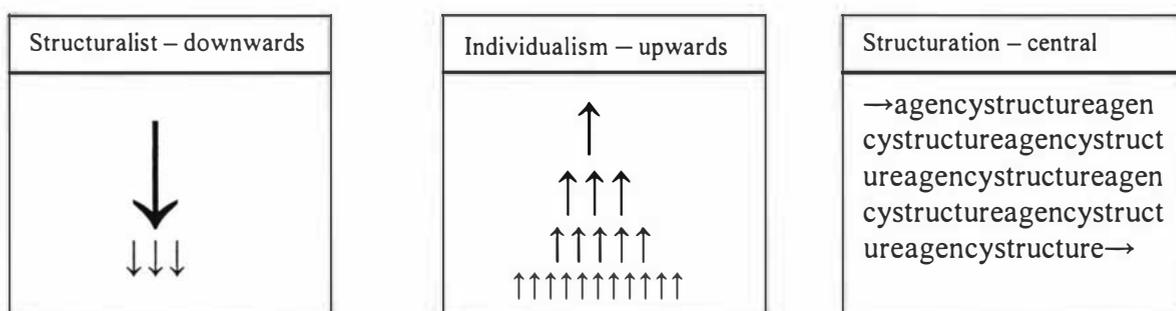
Rather than transcending the voluntarism/determinism dichotomy, the two sides of the ‘duality of structure’ embody them respectively: they are simply clamped together in a conceptual vice (Archer, 1982, p. 460).

Again, Giddens’ notion of social practices lies at the root of a number of objections to structuration theory. Giddens’ account is unable to account for the *degrees* of freedom and constraint which are entailed by social structure. Parker (2000) asks of Giddens’ agency and powers:

Does the distribution and specificity of kinds of power in institutional contexts of situated strategic action constrain it in a more determinate fashion than ‘enablement’? Does power always, evenhandedly, enable and constrain as the duality relation demands? (p. 63).

Structuration provides few answers to questions of this sort, questions that are of central interest to the historical sociologist. A stronger account will be one that separates culture, structure and agency, allowing exploration of their interplay over time, and answering ‘when’-type questions.

Figure 7.1: Three views of conflation:



Given this mounting criticism of agency, structure and time, it is unsurprising that structuration theory has analytical shortfalls. While Giddens' willingness to tackle the enduring quandaries of social theory within the duality of structure strikes a chord, and his analysis is frequently beguiling (world-time, time-edges, etc.), this analysis ultimately rests upon precarious foundations. His main analytic approach via methodological bracketing merely reconstitutes the dualism (cf. Layder, 1994, p. 143; 1998), while analysis of social practices veers between visions of routinisation (recursiveness) and radical social change (transformation):

Although the 'duality of structure' spans both images, it provides no analytical grip on *which* is likely to prevail under what conditions or circumstances. The theory of 'structuration' remains fundamentally non-propositional (Archer, 1982, p. 459; italics in original).

Under the terms of structuration theory we can never say when, what or why – yet these are the very things that are constitutive of historical sociology. Given that culture, structure and agents are analytically and temporally distinct (i.e. they belong to different dimensions of social reality) they cannot be reduced (thereby eliminating upwards and downwards conflation). Nor is it possible to view structure-culture and agency at the same time:

By enjoining the examination of a single process in the present tense, issues surrounding the relative independence of the components have been eliminated at a stroke (Archer, 1995, pp. 93–4).

It is here that discussion parts company with Giddens and structuration.

Narrative Positivism

The work of Andrew Abbott, and particularly his notion of narrative positivism, provides a valuable contribution to debates around agency and structure, and emphasises the importance of process within sociological activity. Much of Abbott's work has dealt with methodological tools for making sense of change over time, focusing particularly on the development of formalist methods. Matching his methodological endeavours has been substantive analysis around the development of professions, the Chicago School of sociology, and disciplines (1988a, 1999, 2001).

At the heart of Abbott's work since the early 1980s has been the critique of a sociological approach he labels the 'general linear model'. This model makes assumptions about how and why events work, dissolving explanation into a clutch of variables, with associations among these abstract variables presented as causes. This positivistic and standard quantitative approach to sociology is the post-war orthodoxy of 'Standard American Sociology'. This, so Abbott argues, is a stilted way of imagining the social process (1988b, 1990, 1992) and must be replaced/displaced by centring narrative within sociological analysis:

Social reality happens in sequences of actions located within constraining or enabling structures. It is a matter of particular social actors, in particular social places, at particular social times (1992, p. 428).

In line with Giddens, Abbott retains a strong focus on process, 'the social world is made up of subjects who participate in events' (1990, p. 141), and the use of stories or 'narrative positivism' retains a robust view of social action. These narratives are not simply equated with forms of interpretivism or biographical approaches to social analysis, and Abbott seeks to retain a purposeful sociology that moves beyond simply advocating thicker description of the social world.

Whereas variable/causal accounts view interaction as secondary, his own framework elevates interaction into a crucial role in outlining how and why events happen (Abbott, 1995). This important distinction can be juxtaposed with alternative frameworks that are offered for sociological, anthropological and historical explanation. Thus structuralist and positivist approaches that dominated sociology in the post-war period sought to freeze the social world, abstracting variables and cross-tabulating them to suggest a series of enduring relationships. Anthropological approaches, on the other hand, either froze time into an indivisible cultural block, allowing only thick description, or contributed a story with a central narrative but remained mute on causation and explanation. In Abbott's work, the centrality of interaction results in time not simply being part of the context or providing the beginning and end point for narration, but rather becoming part of the overall *logic* of explanation. Given the emphasis upon many narratives that constitute the events, interaction is a central component to allow these processes to run at different speeds and in different sequences. This differs from some versions of narrative that deal with frames of time by simply identifying the long, medium, and short terms in the manner of Russian

dolls – with the short term inserted into the medium term, which is itself inserted into the longer term and so on. Abbott, however, points to contingency as crucial to the task of explaining event narratives, thus allowing entity change to be incorporated into analysis – over time names may stay the same but may denote very different things.

Abbott has explored the possibility of using a range of innovative approaches including sequence analysis to further elucidate the temporal and process nature of social life. In places he introduces a welcome transparency into sociological analysis, such as with his discussion of colligation around levels of analysis or domains, the substantive stories at each of these levels, and the formal qualities of each event (Abbott, 1984). He aims to introduce clear and repeatable procedures for the undertaking of analysis, and because it seeks to remove the veil from stentorian narrative, his aspiration is to be applauded. Drawbacks of his work include the presentation of causes as reification, and the fact that he draws too sharp a distinction between the variables and narrative approach. Elsewhere, however, his work exhibits ecumenicalism – openness to narrative *and* causal ways of seeing the world. While he suggests that both the variables approach and narratives make assumptions about social reality (ontology, structure, time, cause) he does not explore the relationship of narrative to cause. Such ambiguity is reflected in the comment of Kiser (1996) that Abbott ‘is interested only in describing narrative sequences and not in discovering causal relations between events in the sequence’ (p. 252).

Summary

The purpose of this chapter has been to explore possibilities for resolving the agency and structure dualism. Whilst a range of theorists have identified agency, culture and structure as important, attempts to theorise their interrelations have more recent origins. What is the final tally on the structuration and narrative positivist scorecards? Previous discussion has suggested that structuration has many limitations. Layder (1994) writes:

Although it is true that structure and action are implicated in each other, the real question is, 'in what sense or in what way are they implicated?'

(p. 148).

It is the contention of this chapter that structuration cannot conceptualise these relations, and that a satisfactory account must conceptualise agency, culture and structure and explore their *interplay*. Narrative positivism, whilst promising much for a working sociology, does not adequately address this conceptualisation and leaves the issue of causality unclear. From the chapter has emerged the importance of resolving dimensions of agency, culture and structure through process; the next chapter maintains this focus but begins to address the matter of causality.

Chapter Seven Notes

¹ Lloyd (1986, 1991) also outlines a range of non-reductionist Marxists writers who draw upon structurationist principles: Marxist sociocultural history, totalising Marxist history and Marxist historical sociologists.

² Lloyd (1993) argues, *contra* Giddens, that an approach resembling structuration can be traced back to the Enlightenment and runs throughout the work of Marx, Weber, Simmel, and Piaget. Craib (1992b) also suggests that Giddens' work is a less radical break with conventional sociology than is frequently presented – hence his labelling of structuration theory as the 'theoretical omelette' (p. 20).

³ Expressed succinctly as: structuration 'is ever a process and never a product' (Archer, 1982, p. 457).

Section III

Sharpening the Tools

Chapter Eight

Critical Realism: For ontology, towards explanation

Introduction

Previous chapters sought to establish the nature of the research task and introduce notions of explanation, positivism and anti-positivism, and highlight a series of post-positivist stances (Chapters Two, Three and Four). Chapter Five discussed the internalist/externalist distinction and the consequences of this for making sense of social science research development. Notions of individualism and social agency were highlighted and the interrelationship of agency-culture-structure was situated within contemporary sociological writing (Chapters Six and Seven).

A series of interrelated debates have emerged around the practice of science, the status of scientific knowledge, and the implications of both of these for the possibilities of social understanding. Additional issues remain unresolved still, including how culture-structure-agency may be incorporated within frameworks, and whether a narrative or causal mode of explanation best suits the practice of historical inquiry. Whilst discussions of Giddens' structuration created some insights, and Abbott's narrative positivism signalled some promising developments, they are themselves unable to furnish a basis for a workable historical sociology. This chapter invokes a Critical Realist tradition whose ontological premises and view of causality attend to a number of the weaknesses of positivist and anti-positivist approaches.

The Roots of Critical Realism

Critical Realism starts from a philosophical engagement with practice and seeks to avoid both the *a priori* reasoning and the epistemological dilemma that bedevil social science. The project is an ambitious one, promising the possibility of judgement and explanation, and the resolution of agency-structure through emphasising interplay through time, and addressing both subjectivism and objectivism. Such aims parallel those of Giddens, and indeed Critical Realists are contemporaneous travellers searching for a sounder conceptualisation of social science to aid substantive inquiry.

The roots of Critical Realism (hereafter CR) lie in Kantian transcendentalism and the work of Roy Bhaskar (1975, 1979). In developing an alignment that he argues is beyond positivism and hermeneutics, Bhaskar has had a profound influence on the human and social sciences, setting in train vigorous theoretical debates and providing pointers for empirical investigation.¹ Underpinning his work is the move from the solely epistemological domain to the ontological (the latter defined in Quine's pithy question 'what is there?'(1964)), the non-foundationalist *philosophy* beginning not from '*what grounds do we have for believing X or Y*', or '*should we opt for a foundationalism of rationalism or empiricism*', but instead asking '*What must reality be like to make the existence of science possible?*' (Bhaskar, 1979). Through his discussion of experimentation, Bhaskar revises the account of natural scientific practice and its relationship to existing reality and our knowledge of it. Failure to conceptualise science appropriately leads to problems, such as the Winchian distinction between naturalism/anti-naturalism that is wholly dependent on empiricist theories of existence and causality (1979, p. 2; see also Chapter Six). A number of traditions, including those within positivism, post-positivism and idealism, are seen to hold erroneous views of science. Concluding that only realism can intelligibly account for scientific practices, Bhaskar introduces a distinction between the *transitive* and *intransitive* objects of science. Within the intransitive domain, reality exists – but our knowledge of it is conceptually mediated, fallible and theory-laden (the transitive domain). He notes:

intransitive objects of knowledge are in general invariant to our knowledge of them: they are the real things and structures, mechanisms and processes, events and possibilities of the world; and for the most part they are quite independent of us (Bhaskar, 1975, p. 22).

A wedge is driven between the challenge to positivism and the Hobson's choice of hermeneutics. Whilst positivism claimed to deliver the 'facts' of reality, it compacted the transitive and intransitive domains and ignored the mediation of language. Conceding that language mediates reality does not imply that language creates reality (so-called 'Super Idealism') – for realism a reality *exists* outside language and has causal efficacy. That intransitive objects can only be referred to by transitive discourse does not collapse the intransitive into the purely discursive realm. Bhaskar positions himself against both the

positivist preoccupation with observation and Humean constant conjunctions, *and* an anti-positivist (more relativist) constructivist tradition, remaining equidistant from Annalistic, as well as more theoretically anarchistic, post-modern, dalliance. In resisting the epistemic fallacy a clear delineation of *being* and *knowing* is drawn:

Knowledge follows existence, in logic and in time; and any philosophical position which explicitly or implicitly denies this has got things upside down (Bhaskar, 1975, p. 39).

Although our knowledge of this external world remains fallible, theories are not equally fallible and judgemental rationality is retained. At any point in time scientific theories may be wrong but are the best held about reality for the present, and ongoing work may allow their replacement. Succinctly summarised, the overall project is 'to combine and reconcile ontological realism, epistemological relativism and judgemental rationality' (Bhaskar, 1998, p. xi).

Bhaskar clarifies experimentation by distinguishing three, frequently non-aligned, levels of reality: empirical (consisting of observations and experiences); actual (made up of events); and the real (constituted by mechanisms). Positivism errs in pursuing constant conjunctions at the level of the empirical, whilst a shallow realism is limited by its concerns with the actual. But for Bhaskar the world consists of mechanisms not events. Despite social/scientific knowledge being a social practice and embedded in linguistic constructions, real mechanisms have an existence independent of language and conceptual appropriation. Things have unexercised powers, powers that are exercised unrealised, and powers that are realised unperceived (Bhaskar, 1975, p. 32). His stratification allows for a world of events that are without experience – a series of events that our knowledge apparatus prevents us from grasping at this present geo-historical moment, with the real not always revealed at the level of the phenomenal.

That mechanisms exist can be established by philosophical activity; the task of specific scientific practice is to discover *which* exist, *when*, *where* and *why*. Bhaskar's realism presents the possibility of cumulative knowledge development – reasserting the possibility of naturalism and, as we shall see, a truly *social science* (cf. Sayer, 1992). But within natural science, uncovering explanatory mechanisms relies heavily on experimental closure

and this is something ultimately denied to social science. More work is required around the transcendental move to ontology and the methodological imperatives in order to clarify the possibility of a realist social science.

The reach of realism within the social sciences

Armed with insights drawn from Bhaskar's philosophy, a realist social science takes similar transcendental steps to argue that certain things are necessary for the existence of social life and knowledge, including human intentionality, culture, structure and their interplay. As Benton ([1981] 1998) suggests, 'There seems to be no room for social science as a distinct cognitive practice with distinct methods, and autonomous theory, as is the case with the natural sciences' (p. 301), so reflection begins with the preconditions of social life. In maintaining the distinction between ontology and epistemology, what 'is' places limits on how we can conceive it and ontological regulation undermines conventionalist notions of truth (Archer, 1998a; Parker et al., 2003). In developing 'what is there', CR postulates a stratified reality of both individuals and structures, neither of which is reducible and whose relationship is explored over time. For Lloyd (1986) this results in 'a model of humans as agential and social beings, who structure the world through intentional action and have their action structured by the social world' (p. 10) (see also Archer, 1995; Lawson, 1997). This was also the central thrust of the 'duality of structure' developed by Giddens, but the major difference is the CR focus upon the relations between people and the properties of emergent social systems which are seen to have causal efficacy (cf. Porpora, 2002). The realist explanatory project then sets about establishing knowledge of these relations, drawing upon epistemological resources to provide analytical purchase via *sight* and *insight*.

Whereas the success of scientific explanation and prediction lies in the ability of experimentation to achieve closure, the social system is considered an open system (cf. Bhaskar, 1979; Sayer, 1992). Given that society is peopled, social science investigation pursues causal *and* interpretative explanation rather than rich description, generalisation, prediction, or Popperian falsification. Identifying mechanisms and their subsequent interplay in concrete situations is not some search for Hempelian laws because powers and liabilities exist whether they are exercised or not. Instead, there is necessity in the world and causal powers may be attributed as tendencies rather than seen as fixed essences.

Causal statements are about what an object is and the things it can do by virtue of its nature (Danermark et al., 2001, p. 55).² Within this open realm of the social world, numerous mechanisms may operate to co-determine change (cf. Collier, 1994, p. 161). While constant conjunctions and laws of social change paid no regard to generative mechanisms, under the CR schema they are the *raison d'être* of inquiry into social change.

Adopting hermeneutic insights, CR recognises social phenomena (action, texts and institutions) as concept-dependent. Thus the world is meaningful – if cheques had a different meaning or none at all they would cease to be the causal mechanism that they are. Such concepts and social relations (of individuals and institutions) constitute the social world. Structures are not solely interpretations because they may escape the understandings of actors, and actors are not given a blank *tabula rasa* but draw upon pre-existing symbols and norms in the conduct of social life.

Despite the world being hermeneutic and historical, the possibility of causal analysis still remains. Social science is not solely contemplative or conceptual, but mandates the discovery of generative mechanisms of the social world, with social ontology preceding the practical 'work'. Positivism's observational criteria for determining laws – constant conjunctions identified through empirical method – provides the foil for realist causality, with Humean *veracity* replaced by human *capacity*. Having distinguished the stratified reality of the empirical/actual/real, CR refuses to cede knowledge to solely observational referents; research as a staunchly theoretical enterprise attempts to guard against empirical (i.e. concrete) research sliding into empiricism (Sayer, 1992, p. 120). Structures, whilst non-observable, are nonetheless real, and in separating the real from the conceptual object, an epistemologically privileged observation language is ruled out. But observations are theory-laden not theory-determined, creating the possibility of judgemental social science. Thus, CR is unwilling to de-couple empirical referents altogether, maintaining that we can arbitrate knowledge claims through holding ideas of what exists in the world against the world itself.

The terrain of CR is rich for the historical and human sciences, although its soil requires further tilling to allow the substantive historical sociology of later chapters to gain a firmer footing. Many criticism have been levelled at the lack of a practically worked-through CR

agenda (see Pratt, 1995; Zeuner, 1999), and there is no consensus around the role of the intransitive and its implications for the pursuit of truth. Some Critical Realists maintain the importance of a correspondence view of truth (Layder, 1994; Potter, 1999); others suggest this be replaced by the notion of 'practical adequacy' (Sayer, 1992); and still others opt for a 'regulative' view of truth (Manicas, 1987; Outhwaite, 1987).

Major criticisms of CR have focused upon its supposed naïve correspondence theory of truth whereby truthful theories are an imprint of a given reality. Such a notion, it is argued, is in tension with attempts to uphold an epistemological relativism (Fay, 1990). An alternative CR strategy, of replacing Correspondence with *truth as an unobtainable idea* and something never known to be approximated, is said to be problematic because it removes ontology from the terms of the debate altogether (Fay, 1990, p. 35). The development of theories, so Fay argues (p. 37), should be viewed instead as a cartographic activity:

in mapmaking there is no sense that the terrain is itself *already* mapped, or that the job of the scientist is to *discover* this pre-existing map (italics in original).

Under Fay's metaphor, a variety of maps are constructed because there are many ways of describing the terrain, various modes of representation and multiple uses to which they can be put. Whilst judgement is still retained, maps are better conceived as more/less useful, clarifying/confusing rather than arbitrating on truth. Such arguments, however, downplay the structured reality, and whilst conceding that we do draw mental maps there are some relatively enduring features that investigators find themselves up against. In rejecting Fay's idealism, the case is made for reality being retained as a regulative ideal that conditions the sorts of statement that can be made.

Debates continue around CR method/s for identifying social structures and their causal influence (Layder, 1994; Archer, 1995; Lawson, 1997; Nash, 1999; Danermark et al., 2001; Kemp and Holmwood, 2003). How can the move from positing mechanisms to 'verification' be negotiated if CR is not to be merely imagining and positing a clutch of mechanisms (cf. Edwards et al., 1995; Potter et al., 1999)? There is merit in addressing: 'What, *in practice*, is so different about research informed by critical realism?' (Pratt, 1995, p. 67; italics added).

The 'Undeniability' of Ontology

Before an ostensibly methodological encounter takes place, some digression into the more fundamental moves of CR is required because here its approach is distinguished from positions identified previously. As outlined, transcendental analysis explores what in the nature of the social world makes the social world an object of possible knowledge prior to identifying how investigators come to know these characteristics. Once the ontological features have been identified, investigators are despatched to the epistemological/methodological to uncover mechanisms; consequently ontology precedes epistemology in the order of activities and remains separate from it. This initial sketch of the ontological presupposes epistemological and conceptual apparatus, yet realism fails to recognise – or requires momentary suspension of – the linguistic/conceptual nature of discourses when postulating the ontological:

all descriptions of the objects of our knowledge (that is, *what* we know), are *already* ineradicably bound up with specific preconceptions about the nature of our knowledge, that is, how we come to know the things we know (the objects of our knowledge)...an ontology of the social can never be an uncontestable and transcendently given thing; it is always as it were, 'overdetermined' by a theoretical discourse and an epistemology (Layder, 1990, p. 31, p. 62; italics in original).

A similar criticism is levelled by Norman Stockman, that realism does not attend to the 'constitution of domains of knowledge by the synthetic activity of the knowing subject' (1984, p. 117). Whereas prior understandings undergirding any conceptual framework require elucidation and justification, realism attempts to foreclose such discussion by arguing that only its concepts reflect the structure of reality (pp. 215–30).

This assertion of ontological privilege is said to leave out the constructional role of prior theoretical discourses in establishing ontological models (Layder, 1990, p. 120). For example, Bhaskar is criticised for smuggling a substantive (and refutable) characterisation into intentionality (Benton, [1981] 1998, p. 303). Traditions clash violently on the constituents of social reality (for example, among forms of realism, and between traditions of phenomenology, functionalism and Critical Realism – within realism Bhaskar and Harré differ profoundly in their schema of social constitution)(cf. Lewis, 2000). Many take realist disagreement about the nature of reality to mean there is no practical dividend from

adopting a realist position (Fay, 1990; Hall, 1999). Others are led to defend a more pragmatic realism (Outhwaite, 1987) that avoids metaphysical assumptions, but in many respects skirts close to a conventionalism.

While charged with the ‘ontic fallacy’ – the reduction of knowledge of being to being – a realist defence is that an implicit or explicit ontology is always required in our activities (cf. Bhaskar, 1979; Archer, 1995, 1998a & b). Here there are shades of what has been termed an ‘undeniability device’ (McLennan, 2001, p.98), but if there is an alternative to ontology, or indeed an alternative ontology, such a case has yet to be persuasively made by forms of empiricism or idealism. CR can thus be seen as levelling an immanent critique against these approaches, and if ontology is in some measure constitutive of theoretical activities, then philosophy can play a regulatory role in filtering out unsustainable accounts of what society must be (Sayer, 1992; Potter, 1999, p. 221). Chapters Five, Six and Seven attempted this – the question ‘what must society be like?’ reverberating throughout the discussion.

Recognising the centrality of ontological commitment to every theoretical schema does not preclude ongoing and sustained encounters; neither should it license a dogmatic imposition of any interpretation, as some critics would contend. In refusing to hold a roundly discredited form of empiricism, developing ontology is a theoretically underpinned interpretative exercise of ‘what is there’ that simply refuses to fold everything into the discursive realm, retaining some things as external to the discourse. The implications, as Patomäki and Wight (2000) write, are crucial because:

if discourses construct the objects to which the discourses refer, then the discourse itself can never be wrong about the existence of the object, in any meaningful or methodologically interesting way. Nor can an alternative discourse possibly critique another discourse, since the objects of a given discourse exist if the discourse says they exist

(p. 217).

Above this somewhat *minimal* and *mobile* element (McLennan, 2001, p. 94) of ontological ‘faith’, scope is retained for ongoing debate and conceptual development.

CR is aware of such criticisms and has tackled them head-on. Bhaskar holds that the premises and conclusions of philosophical arguments remain contingent facts, with synthetic *a priori* truths at best established in a geo-historical moment. He writes 'For philosophy gets going always (and only) on the basis of prior conceptualizations of historical practice, that is, of specific ideas of determinate social forms' (1989, p. 14). So whereas *a priori* forms of reasoning contribute to rigid system-building, the transcendental philosophy of CR does not (cf. Collier, 1994, p. 25). Similarly, Tony Lawson writing in the field of economics notes that his own transcendental move is 'fallible and corrigible' (1997, p. 271). The possibility for ontological revision in light of reflection and endeavours, which in turn regulates methodological and practical dimensions, remains. Whilst some see this ontological move as privileging the expert-philosopher (Tie, 1999, p. 61), there is, at best, an ontological confidence rather than intransigence concerning what is there, and ongoing possibilities to revise ontological conclusions in light of debate and criticism.

A strength of CR is its willingness to venture beyond mere cataloguing of positions and to make some sort of *commitment* around the aims and approach of social science. That some measure of commitment is required for realism may be attacked as a weak basis upon which to ground claims. But this diluted claim that *some* ontology is required, and that we can *debate* what that may be – provides the possibility of ongoing discussion, and of claims to knowledge. Quite simply, this is enough to get activity going and to keep it moving. Without such an initial ontological 'move', the rationale for inquiry evaporates because the explanatory scaffold is removed from things as having powers, and happenings as being contingent occurrences, with all reduced instead to discourse and the 'ephemeral play of linguistic tropes' (Tie, 1999, p. 94). In refusing to concede that the structured world is made up of indelible signifiers, texts and discursive articulations, possibilities and opportunities for explanatory historical sociology are retained.

Developed CR positions outline a detailed ontology that promotes ongoing dialogue and criticism. This may be contrasted with the tendency, for example within economics, to describe practices, rhetoric or the context of practices, but to resist offering criticism (Lawson, 1997, p. 43). Similarly in the work of Hall (1999), various approaches to socio-

historical inquiry are outlined but their status remains frustratingly indeterminate. Lloyd (1986) expresses the view – and it is one with which I would wish to align – that:

philosophy is not interested only in categorising, describing and analysing the methodologies and explanatory procedures of empirical discourses and philosophy. It has inherently the task of evaluating the ontological and epistemological foundation which exist within discourses (p. 33).

I have a great deal of sympathy with this view and would resist what is dubbed ‘reflexive methodologism’ (cf. McLennan, 2002), an approach which impales us upon endless classification. In contrast to such reflexive shuffling, we can make stronger claims for our activities and profit from attempts at explanatory work. The starting point is the willingness of CR to retain an ‘ontological hand’ and to continue to play for the strongest position possible. At root it is underscored by a commitment to a practical social theorising, having a point, making assertions, taking the ‘commas’ out (O’Neill, 1995).

Some CR writers defend their position by viewing it as a common-sense and intuitive philosophy:

Realism is, then, a common sense ontology, in the sense it takes seriously the existence of things, structures and mechanisms revealed by the sciences at different levels of reality (Outhwaite, 1987, p. 19).

A similar fanfare to the ‘common man’ introduces Collier (1994), and is evident in Manicas: ‘Common sense surely presupposes the existence of objects which exist independent of experience, and it is realist in believing that the nature of such objects can be known’ (1987, p. 247) (and Calhoun, 1996, p. 847; Nash, 1999). Although wary of the quagmire that is ‘common-sense’, arguably the transphenomenality of CR makes it diametrically opposed to ‘common sense’, and stronger connections exist between common sense and some variant of logical empiricism, for example. For others, perceived strengths of CR are its moves *beyond* common-sense and intuition, promising ‘scientific’ forms of explanation (Lloyd, 1986, p. 36, p. 21; Sayer, 1992). It is problematic to maintain that CR is both common-sense and capable of delivering the fruit of ‘scientific understanding’ without weakening the realist case.

With augmentation of ontology *provisionally* completed, CR moves to tackle how we might come to know and make specific claims about the existence of the real. A robust

CR stands apart from the naïve realist view that investigation can unearth the world once-and-for-all in the manner of correspondence-as-closure. A more sophisticated realism retains the goal of explanation and seeks to identify a real world of things and happenings, retaining an attitude of epistemological humility. There will be more to say in the next chapter about ontological outcomes, and how such epistemological humility can be easily overlooked to the overall detriment of CR historiography.

CR in 'Practice'

What are the practical implications of CR for the substantive task of exploring, perhaps 'explaining', social science research in New Zealand? To make sense of this social, political and intellectual change it is necessary to avoid narratives that simply list 'one damn event after another' or, alternatively, to avoid sinking into causal factors within a broader covering law approach. CR offers pathways to revisit thorny questions of agency-structure, causality, and historicity. The discussion of Chapters Three to Seven signalled such concerns that needed to be raised prior to the practical activity getting going. So far in this chapter, brief encounters with CR have helped sketch the possibility of explanation; some finer strokes of methodological principle must now be placed upon this philosophical canvas.

Unlike the objects of natural science, realists argue that structures, the objects of social science, are discontinuous and *relatively* less enduring in comparison with natural science. While they exist, they nonetheless still exert systematic causal effect on action. So far the thesis has highlighted a number of inadequate conceptions of structure, including viewing it as a pattern created by individuals or law-like social facts (Chapter Six), or as 'rules and resources' (Chapter Seven) (cf. Porpora, [1989] 1998, 2002). Under realism, structures are conceived differently and pose a challenge for the social scientist because they are in open *systems*, but not out in the open and perceptual. As Bhaskar (1989, p. 2) writes:

Structures are not spontaneously apparent in the observable pattern of events; they can only be identified through the practical and theoretical work of the social sciences.

While social structure is unobservable, it is nonetheless real because of its ability to shape observable human behaviour. Grasping structures becomes an ongoing challenge for fallible social science and investigation requires a '*historically specific*' set of ideas,

techniques and skills' (Collier, 1994, p. 54). Such investigative activity focuses upon the dynamic of social structure, replacing a descriptive social science that merely catalogues existing structures in a series of synchronic snapshots.

Beyond the first transcendental 'move', CR suggests 'practical' steps: positing the existence of particular mechanisms; collecting information for and against their existence; and 'validating' particular mechanisms. Yet those sceptical of the claims made by CR take these steps as vagueness, and an outcome of realism's metaphysical posturing which denies realism the sort of decisive tests that can occur in natural science. So added to the cacophony of ontology's, critics contend, is further empirical/theoretical noise which makes it nigh impossible to arbitrate amongst explanatory theories (Bryant, 1995, p. 88) (see also Hall, 1999, p. 48; Tie, 1999, p. 71; Kemp and Holmwood, 2003). Yet it is undeniable that most of the interesting discoveries of natural science are themselves unperceivable and require the development of perceptual apparatus and/or conditions of closure (see also Bhaskar, 1989, p. 82).

Some CR interjections recognise the importance of attending to these more practical, methodological considerations. As Lloyd poses 'How can we know that our explanations correspond with the reality of historical social processes if we are denied experimental and engineering tests?' (1986, p. 157). The resistance of social systems to internal and external closure and the tendential power of mechanisms make it difficult to isolate mechanisms and so provides, at best, a science that – while explanatory – is non-predictive. Instead of induction or deduction, differential conditions are required for positing mechanisms, particularly if processes of social change involve several mechanisms, and if the same mechanism produces different outcomes dependent upon its precise spatio-temporal relation to other objects. Given the contingency of causal powers ever being activated (Sayer, 1992, p. 107; Sayer, 2000, p. 15), and the impossibility of 'immaculate perception' (Outhwaite, 1987, p. 28), conceptualisation and abstraction are advanced as proxies for experimental conditions akin to thought experiments (Sayer, 1992; Nash, 1999; Danermark et al., 2001). Conceptualisation searches for natural necessity – the necessary properties making social structure what it is.

Despite recognition that CR is philosophy and not a method, and previous criticism notwithstanding, there appears some measure of accord about a process of realist inquiry:

1. The explanatory process begins with a description of some phenomenon/pattern, which may involve a 'thickish description'. In identifying substantive issues we are typically drawn to some contrastive or unusual issue that we believe requires explanation (Lawson, 1997). Here there are dangers of being drawn to the different or unusual because continuity and stability are themselves important tasks for explanation.³ This first step is likely to involve many commonplace concepts of everyday life and incorporates the interpretations of actors who may be involved.

2. *Analytical resolution* involves the separation or dissolution of the complex to explore the mechanism or series of mechanisms that exist. Whilst actor interpretations provide a (hermeneutic) starting point (Manicas, 1987, p. 268; Outhwaite, 1987, p. 55), actors themselves may not perceive the operation of some deeper structures. Many phenomena will already be identified because of the concept-dependent nature of social activities under certain descriptions and because of real definitions preceding causal hypotheses. Although conceptualisation must be grounded in everyday knowledge, this must be surpassed at a more general level. Without this prior theory we would have 'an inchoate mass of social phenomena' to attempt to sort and define (Bhaskar, 1989, p. 85). En route the material may be redescribed under a new schema of concepts and context (so-called abduction).

3. *Retroduction* designates the structure, mechanisms or agents that are to some degree responsible for some social process. Abstraction consists in specifying the internal relations without which the thing would not be the thing it is (Sayer, 1992). The investigator advances from one consideration (empirical observation of events) to arrive at something different (a conceptualisation of transfactual conditions) (Sayer, 1992, p. 96). Here conceptualisation must not be confused with classification because there is no attempt to 'slice' reality into categories. The step of retroduction is likely to 'operate under a logic of analogy or metaphor and to draw heavily on the investigator's perspective, beliefs and experience' (Lawson, 1997, p. 212). A range of prompts may assist the identification of causal impacts

(counterfactual thinking, social experiment, extreme case, comparison of different cases). Once mechanisms are postulated, a dynamic causal picture is created by returning to the concrete to explore their operation under particular conditions.

4. Discussion moves to how mechanisms are manifest under particular conditions. As Sayer (2000) writes, this involves ‘theoretically informed (and informative) narratives which make their aetiologies explicit and give appropriate weight to the synchronic or configurational and the episodic’ (p. 145).

Two types of realist explanatory projects may be identified – positing underlying mechanisms that may or may not ‘shine through’ (the abstract); and a focus upon a concrete phenomenon of unique interests in the interplay of mechanisms (Lloyd, 1993, p. 4; Lawson, 1997; Sayer, 2000, p. 14). Manicas labels these ‘understanding structure and explaining events (1987, p. 278) with the latter entailing:

drawing upon antecedently established knowledge of relatively enduring structures and mechanisms (rather than revealing them), and investigating the manner of their joint articulation in the production of the novel event in question (Manicas, 1987, p. 164; italics in original).

The thesis suggests that historical explanation requires that these two processes be appropriately conceptualised and united in order to constitute any particular historical account.

Emergent difficulties of realism

In listing the methodological steps of realism the question around how different theories and abstractions are compared was bracketed. This withholding was a deliberate move – a performative one – for it marks a distinction between concerns that are central to the CR theorist, and those of the CR methodologist or practising historical sociologist. A frequently overlooked dimension is how CR practice goes beyond merely echoing CR philosophy to deliver practical theorising and explanation. A number of criticisms have coalesced around this point of methodology and, given the substantive aim of the thesis, it receives further attention here.

CR theory suggests that conceptualisation and theoretical explanations are fallible but not equally so, because causal mechanisms can be assessed and legitimised empirically (Lawson, 1997, p. 196, p. 221). Theory is thus more than a purely heuristic exercise and knowledge remains an ongoing possibility. But the two criteria science has for ascribing reality – the perceptual and the causal – both create some difficulties for CR. Criticisms are levelled for example that CR social science becomes unduly reliant on empirical arbitration of falsification and verification, somewhat problematic given Critical Realists' commitment to open systems and undermining CR's own criticisms of empiricism (Suchting, 1991; Baert, 1998). The second criterion for ascribing reality, that of effects, in Saunder's view, 'produces no more than self-confirming tautologies' (1983, in Sayer, 1992, pp. 217–8; also Nash, 1999).

Regardless of whether practical activity provides *sight* or *insight* of mechanisms, knowledge of the real is always conceptually mediated helping to focus upon the content of such concepts within the research process (Danermark et al., 2001, p. 15). But difficulty remains about the 'validation' of CR concepts and accounts. As Nash (1999) notes:

If the concepts of science are to refer to what exists, then if what exists depends on the concepts that describe it, the entire realist project in sociology appears to be in jeopardy (p. 448).

If mechanisms are concept-dependent and have a raft of possible interpretations, then searching for and verifying mechanisms is of a very different nature to the search for physical mechanisms. Whilst the CR is attuned to the importance of conceptualisation (Nash, 1999, p. 449), little guidance is offered to the practising social scientist on how labels are placed upon reality and mechanisms presented before the courts of sociological and historical knowledge.

For realism, the interpretive view of social phenomena as concept-dependent and inherently meaningful does not necessarily rule out causal explanation (Sayer, 2000, p.118). As Danermark et al. (2001) suggest, that social phenomena are concept-dependent does not equate to the social world existing only as mental constructions in people's minds (pp. 34–5). To move beyond conceptualising and positing mechanisms does require more attention to how they are subsequently evaluated. One suggestion is that they be examined 'with reference to how fruitful they are as interpretive frameworks, and to what degree

they generate new and deeper knowledge of social structures' (Danermark et al., 2001, p. 138). But does CR concede too much to the imagination with its denial of decisive tests? Kemp and Holmwood (2003) question the scientific aspirations of Bhaskar's realism, arguing that alternatives to experimentation (explanation, conceptualisation, abstraction, agent 'insight') are all problematic. In their view therefore, CR is prone to trip at every 'practical step'. That social inquiry *should* search for structures does not equate to our *having* methods to identify those structures; to invert Bhaskar, you cannot get an 'is' from an 'ought'! CR appears as an overactive, and somewhat qualitative, imagination, and not conducive to arbitrating among various accounts of structure (cf. Kemp and Holmwood, 2003, p. 172). Research does seem to stall with a clutch of mechanisms, whilst establishing knowledge of causality demands some measure of arbitration.

Some such criticisms presuppose a view of science that is itself problematic. By returning to a consistent message of realism – clarify thy object – it is possible to advance an alternative route for how realism may develop. Bhaskar's qualified anti-positivist naturalism 'does not deny that there are important differences in these methods, grounded in the real differences that exist in their subject matter' (1989, p. 67). But, he argues, these are not so problematic as to deny the *possibility* of knowledge. Kemp and Holmwood's criticisms are hitched to 'decisive tests', the existence of which are also problematic for natural science, where there are more enduring objects of inquiry, but where every experiment involves *some* inferential leap. If all that is being argued is that we cannot know mechanisms with certainty, then this misses the whole realist thrust because the perspective develops practical accounts that have stronger grounds for claims to knowledge than competitors. Bhaskar is clear that science is both logical and social:

So science must be seen as a social process, irreducible to an individual acquisition, whose aim is the production of the knowledge of mechanisms of the production of phenomena in nature, the intransitive objects of inquiry (1989, p. 18).

Regardless of whether activity is empirical or abstract this social dimension cannot be ignored. CR is undaunted by the absence of decisive tests and rejects the implication that this curtails the possibility of knowledge (see, for example, Bhaskar, 1989, p. 83). The realist acceptance of fallibility allows plausibility and insight with reference to the object to assume a weaker function of validation.

A further criticism of realism is that its explanatory system relies on existing knowledge of structures to give explanation, making CR circular: explanatory success relies on prior explanatory success, and so on, *ad infinitum*. Indisputably, there is no readily available 'vault' of structures upon which to draw to fund explanatory activities, but this does not prevent our drawing upon theories that have presented some measure of robustness. As Bhaskar (1989) writes:

in science the raw materials used in the construction of new theories are established results, half-forgotten ideas, the stock of available paradigms and models, methods and techniques of inquiry; so that the scientific innovator comes to appear in retrospect as a kind of cognitive *bricoleur* (p. 78).

Having to establish each and every mechanism results in a *reductio ad absurdum*. In taking some structures for an initial footing, these are not simply assumed and reproduced. They are handled critically and assessed on their ability to withstand alternative interpretations. Hence there is the possibility of their replacement prior to being added to the conceptual structure, akin to Neurath's notion of replacing planks on a boat (cf. Sayer, 1992, p. 81). Nor does the replacement of individual planks preclude the possibility of assembling an almost new raft of explanatory ideas. Although agreeing that 'untested conceptualisation of structure' should be swapped for 'rigorous testing of hypothesis about structures operating in the social world' (Kemp and Holmwood, 2003, p. 182), we should take care to ensure that this does not become an opportunity for sceptic or ultra-sceptic tendencies. Despite this, the necessity of providing clearer routes or principles for practice is upheld, and the following chapter explores in further detail how the realist move in historical sociology may be achieved.

Summary

The chapter suggests a number of strengths of the realist position: its willingness to tackle the constitution of the social world, engage objectivist and subjectivist positions, and to incorporate process. While the aspiration and ontology are persuasive, more attention needs to be paid to the concerns that lie at the centre of practising historical sociology. There still remains a number of clarifications to be made around the relationship of CR and historical sociology. The following chapter attempts this in light of the work of Christopher Lloyd and Margaret Archer. It explores in greater detail the interrelationship

of agency, structure and culture, and more particularly how the practical task – devising ‘explanatory histories of emergence’ that are both analytic and narrative – may progress. Within Archer’s work there are opportunities for the practical tasks of historical sociology, with analytic retrodiction followed by narrative exposition offering possibilities for causal explanatory insights.

Chapter Eight Notes

¹ For example, the work of Andrew Sayer, Derek Layder, William Outhwaite, Tony Lawson, Christopher Lloyd, Andrew Collier, John Parker, Garry Potter, Peter Manicas, and Margaret Archer. A number of realist positions are reflected in debates in the *Journal for the Theory of Social Behaviour*.

² 'In the concept of tendency, the concept of power is thus literally dynamized or set in motion' (Bhaskar, 1975, p. 50).

³ For example, the ongoing contribution of Eisenstadt's *Political Systems of Empire* (1963) is its emphasis on stability.

Chapter Nine

Realising Realism within Historical Sociology

Introduction

Taking ontological preconditions as the point of departure, the chapter explores the role of CR in the practice of historical sociology. To this end, it examines the work of Margaret Archer and, to a lesser extent, that of Christopher Lloyd, in further detail. Both Archer and Lloyd seek non-reductionist sociology, developed from ontological premises, that retains causal explanatory potential. Their common message is that sociology is historical and empirical (Archer, 1979, 1995), and history is sociological and theoretical (Lloyd, 1986, 1993). Acknowledging their debt to Bhaskar's transcendental realism they develop 'morphogenetic social theory' founded on social ontology, and 'methodological structurism' respectively. The chapter argues that Archer's work, albeit with some modest additions to incorporate the concerns of the practising historical sociologist, offers some considerable insights and opportunities for the practical task ahead.

Morphogenesis and Methodological Structurism

Central to their schema's resolution of agency-structure is the necessity of upholding robust versions of both sides of the dualism. Reflecting upon the nature of the social world tells us that objects exist with particular properties, thereby allowing them to be studied as objects of knowledge.¹ Archer's trinity of works around culture, structure and agency (1988, 1995, 2000c), chart a course for a realist social theorising. Exploring the nature of social being is necessary, she suggests, because it regulates methodology and practical endeavours, 'what social reality is held to be cannot but influence how society is studied' (1995, p. 13). Whilst she recognises the ontological interaction of culture, structure and agency within the flow of social life, analytical dualism allows her to separate people and parts for subsequent empirical profit. The interplay of these is explored through morphogenesis, recognising the stratified nature of reality and ceding autonomy to all of these levels. Through strict adherence to these principles, errors of upward, downward, and central conflation are avoided (Archer, 1982, 1988, 1995). Her coda is summarised as:

Structure and agency can only be linked by examining the *interplay between them over time*, and [recognising] that without the proper incorporation of time, the problem of structure and agency can never be satisfactorily resolved (1995, p. 65; italics in original).

In theorising these interrelations, she focuses upon why and how a relationship exists, rather than simply asserting its existence (Parker, 2000, p. 72). In contrast to a number of methodological realists (e.g. Healy, 1998; Sibeon's anti-reductionist epistemology, 1999), Archer does not shirk making an ontological 'move' and the commitments it implies (cf. Chapter Eight). She suggests that the ontological-methodological-practical constitutes a further, mutually regulative, trinity within her work (1995, pp. 5–7, p. 20, p. 57).

Christopher Lloyd's 'methodological structurism' (1986, 1991, 1993) similarly theorises the constituents of society and their temporality, responding to Bhaskar's question 'what properties do societies and people possess that might make them objects for knowledge?' (Bhaskar, 1979, p. 13). As comrade-in-arms with Archer's attacks on methodological individualism and relativism² he develops a:

realist and structurist answer to the questions: what must the world in general be like for us not only to perceive the world successfully and so live our lives but also to develop and apply truthful explanations of the world (Lloyd, 1986, p. 107).

Archer and Lloyd concur that the answer lies in the depth of the social world with culture, structure and agency as autonomous and having causal efficacy. The realist search is for relatively enduring social entities and mechanisms – social relational structures – and to determine how these tendencies are played out:

Social realists believe that there are, *in principle*, discoverable real properties of *social structures* which are necessary (but not sufficient) for deciding theoretical and explanatory disputes about both action and social change (Lloyd, 1986, p. 97; italics in original).

Whilst in the flow of everyday life these properties may defy awareness, they can be uncovered through rigorous social scientific analysis. This view goes beyond the collectivist 'societal facts' because realism is not stymied by the causality of observable

events that ignores emergent properties unknown to sense data (Lloyd, 1993; Archer, 1995). Such 'generative mechanisms' are influential and isolatable in their own right and may be explored alongside the intervening influences of people. The task of the historical sociologist is to clarify which structures exist and whether, how, when and where they are brought into play. This propositional thrust contrasts with Giddens' structuration, whereby change is 'ever a process and never a product' (Archer, 1982, p. 457), and where duality of structure melds agency and structure together, thus preventing their investigation.

Christopher Lloyd's work has a somewhat ambiguous relationship to Giddens'. While Lloyd (1986) identifies methodological structurism, influenced by Bhaskar, as the route to historical explanation (emphasising non-reductionism, autonomous and stratified reality), he explores structurationist approaches – including Giddens'. By his 1991 article, Lloyd had absorbed Giddens' notion of social practices, arguing that the autonomy of levels and exploration of change over time can be tackled within such a framework: 'Structure and action, then, are not the poles of society but two moments in a dialectical society' (1991, unpaginated). Lloyd's adoption of Giddens' methodological bracketing is similarly evident: 'it is possible to construct a social methodology that emphasises one or the other for explanatory purposes' (1993, p. 48). Given the discussion of Chapter Seven, which identified structuration's incapacity to furnish explanation, the Giddensian embrace is perplexing and contradicts Lloyd's wish to uphold objective structures and autonomous levels. In more recent work (Lloyd, 2000), he abandons social practices and the virtuality of structures, rejecting Giddens' structuration, and siding with Archer's dualism. Arguably this move is an understandable one in light of his expressed aims and the thrust of much of his writing around 'methodological structurism'. The following discussion focuses primarily upon Archer's work given the consistent line it has maintained, and the opportunities it presents for historical analysis.

Agency, culture and structure

Archer's work has fully theorised notions of agency, culture and structure which together form the ontology of her schema. This includes a stratified view of agency that recognises prior structural conditioning, but also incorporates the existence of personal individual differences (unlike the accounts of individuals/social actors in Chapter Six). The operation

of institutions shapes roles, and in turn exerts influence on positions, whilst at the same time remaining open to possibilities of actor influence and reshaping. The perspective has the potential to explore, on the one hand, the extent to which influence is exerted by institutions and roles, and on the other, how voluntaristic re-shaping results from actor intentionality.

Her distinction between Primary Agents and Corporate Agents emphasises the organisation of social interests, whereby Corporate Agents have emergent powers of promotive organisation and articulation of their interests. These interests do not remain static but are regrouped as a result of renewed resources, increased membership, and the obtaining of greater ideological supports during phases of interaction.

Earlier chapters argued for a robust conceptualisation of culture within any explanatory framework. Archer's work shows considerable innovation in its theorisation of culture, and the distinction between cultural system and socio-cultural interaction. Stronger claims are invoked than culture simply occupying 'some place' within the explanatory framework. Instead, at any point in time the cultural system has autonomous properties that, although a product of human activity, 'escape its makers' to present situational logics and costs for actors. Ideas, beliefs, and theories have a role to play, and (unlike the theories of Chapter Five) the focus is upon *particular* ideas that are held and find support at *particular* points (cf. Archer, 1988). Because culture is a force in its own right, and not simply a mirror, the position does not 'rule[s] out a conflict of ideas which gets underway independently of material interests' (1995, p. 305). The approach does not therefore prejudge the role of culture but leaves its particular influence to be resolved by specific inquiries.

The conceptualisation of structures within Archer's work is in sharp contrast to Giddens' 'rules and resources', that lie outside time and space and are reliant on agency for their instantiation. His work has no place for objective social relationships and denies their emergence, autonomy, pre-existence and causal efficacy – conditions that are central to Archer's schema where structure shapes the practices of groups in different ways and cannot be collapsed into social practices (Archer, 1995, p. 97, p. 116, p. 132). Giddens' 'realism' – the view that real causal mechanisms operate in the world – remains intersubjective rather than objective (Porpora, [1989] 1998). In contrast, Archer's non-

reductionist ontology defends the existence of objective structures and subjective agency, using emergent properties (at levels of agency, culture and structure) to avoid reificationist metaphysical encounters.

Such a move is frequently challenged on the basis that structure is a heuristic device, but certainly not an ontological distinction (see King 1999a & b; also Chapter Six). In King's view, objective social structure reifies social relations and is unnecessary because it is better accommodated within an interpretivist framework. Thus the past, understood as social relations between individuals, has an impact on the present through interpretations of living individuals (1999a, p. 205). King accuses Archer of converting 'the temporal priority of the people's actions in the past into the ontological priority and autonomy of structure' (p. 211), and he dissolves emergentism into other people, other practices, other times. A heuristic film of 'structure' can therefore always be punctured allowing access to individuals. In tracing these *relations among individuals*, the interpretivist, so King argues, takes constraining structures and objective aspects into account, which are dependent on past structures.³

Archer concedes the relational nature of emergents with structures arising from the activities of (past) individuals and exercising effects through the activities of (present) individuals (Archer, 2000a, p. 465). Thus society is a series of relational encounters where some combinations with internal and necessary relations generate causal powers. The system is a 'carrier' of the past's influence and individuals alone cannot account for it (Parker, 2000, p. 75). In countering the interpretivist view, Archer writes:

Because of the temporal priority, relative autonomy and causal efficacy of structural properties and their influential power, there never is and never can be a tabula rasa from which to conduct interpersonal negotiation (2000a, p. 469).⁴

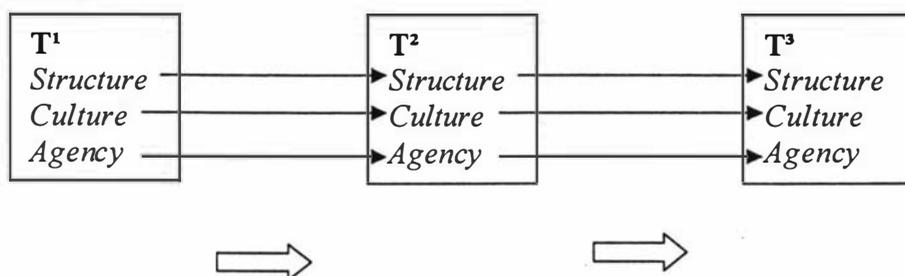
Some suggest Archer advances social structures whose present existence is entirely independent of the people who constitute society (Healy, 1998, p. 518). But Archer's view: structural conditioning → interaction → elaboration, is shorthand for the two dimensions of activity dependence of structures, namely that (i) structures emerge from activities of past individuals; (ii) they exert effects through activities of present individuals

(Archer, 2000a). Restating the importance of temporal priority prevents misunderstandings: structure (and culture) shapes and conditions, creating situational logics for interaction, which in turn shape structure. Opportunity costs are associated with situational logics – bargaining power, negotiating strategies, vested interests and so on. Structural conditioning occurs prior to interaction and cannot be wished away, although with agency’s transformative capacity it may dissolve over successive interactions.

Attempts to dislodge relatively enduring structures of relations and their temporal priority results in forms of idealism whereby: ‘If everyone woke up tomorrow firmly convinced that private property was evil, and communicated that conviction, it would very quickly cease to exist’ (Mann, D., 2002, p. 8). For Mann, social explanation involves intention, meaning and structure, and a given phenomenological moment can be explored synchronically, incorporating the three elements, or diachronically within one level of analysis (p. 57). Expressing his work diagrammatically would suggest that he deprives action and structure of their crucial temporality, resulting in either:

- bundles of influences (Box T¹ ⇨ Box T² ⇨ and so on); or
- an attempt to separate out levels of analysis by disregarding their interplay (Structure T¹ → Structure T² → Structure T³).

Figure 9.1: Conflation or synchronic theorising



Under such a schema, interplay and change over time cannot be appropriately dealt with, and it is in comparison with such approaches that Archer’s work has evident strengths.

Against charges of reification it is important to reiterate that social structures *are* the result of individuals. There is, however, greater clarity about *whose* actions constitute social structure at particular points in time, and awareness of the ever-structured circumstance in

which actors operate. Whilst efficient cause is retained with people, the causal efficacy of structures (and culture) results in them being real and provides the key to their substantiation (cf. Lewis, 2000). In focusing away from the empirical (institutional patterns, social organisations, and economic classes) attention moves to the relational nature of social structure. People slot into positions and the action of these position-holders is directed towards occupants of other positions (cf. Lawson, 1997, p. 164). Whilst not all relationships give rise to emergents, those that do have internal and necessary relations that generate their internal/external causal powers. The focus is upon social positions, bureaucracy, resources and interests, and cultural dimensions of belief, concept, symbol and paradigm.

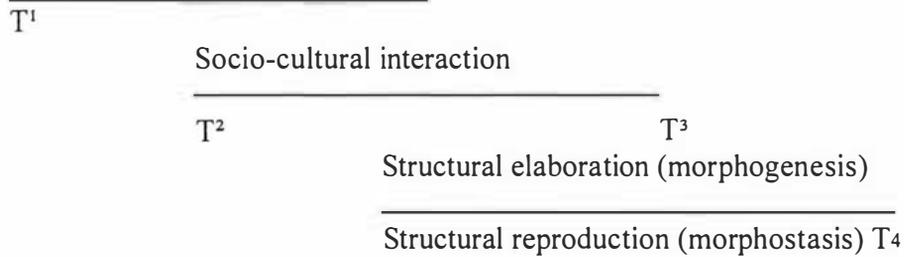
Structure and culture present limits to the situations that agents can encounter. Involuntary placement in a prior distribution of roles and resources gives rise to vested interests and entails different situational logics and strategic guidance. As these become played out through exchange and power (themselves relational properties) of bargaining, resources (wealth, sanctions and expertise), and negotiating strength, so the system is elaborated. The rates of exchange of these resources are socially determined through interaction and will vary over time; thus, for example, 'expertise' will be accorded greater value at some points than at others (cf. Archer, 1995).

Archer's temporality

Situating Archer's work within the broader context of CR, her real innovation has been the introduction and fuller elucidation of temporality. Thus culture, structure and agency are ontologically autonomous although interrelated in social life, and through analytical dualism the temporal conditions of their existence and relations can be explored. This simple, yet powerful, contribution is highlighted in the diagram that is reproduced below and which is a useful contrast with Figure 9.1 that represents Mann's work.

Figure 9.2: Analytical dualism and the basic cycle (Archer, 1995, p. 157)

Structural conditioning



A processual focus is upheld, 'linking not sinking' (1995, p. 65), that addresses the question 'whose activities are responsible for what and when?' Structures can be identified because unlike structures in conflationary approaches, they are irreducible, autonomous, and relatively enduring (1995, p. 168). The morphogenetic framework of systems provides for dynamic micro-macro interactions through 'analytical histories of emergence' via structural, cultural and agential interplay over time (p. 194).

Given that these dimensions operate over different periods of time (unlike the continuous flow of Giddens or Mann) the analyst has an opportunity to periodise them. Conditioning, interaction and elaboration are continuous through time but as an analytical tool, dualism is used to allow the investigator to cut into reality and project cycles forwards and backwards:

Although structure and agency are at work continuously in society, the analytical elements consist in breaking up these flows into intervals determined by the problem in hand: given any problem and accompanying periodization...(Archer, 1995, p. 168).

Archer illustrates her methodology with cycles of educational development (cf. 1979), and resting on twin pillars of analytical dualism and morphogenesis makes an important contribution to historical sociology. Her position contrasts with that of Lloyd (1986, 1993), who, while similarly upholding the importance of analytical dualism and commitment 'to explain the origins of the real structures of the world and their

transformations' (p. 4), distinguishes between studying events and structures, concentrating on either empirical (historical) or theoretical (sociological) respectively:

It has to be possible both to study structures relatively separately from the actions and events that they determine... without ever losing sight of the influence of the others (Lloyd, 1993, pp. 48–9).

Although recognising that the presentation of structural history takes a partly narrative form, this schism has the potential for work that is hypothesis-rich but explanatorily poor. To move towards 'exacting explanations' requires that attention be given to the situational logic, temporal sequence, and form of narrative, and these are the very virtues of morphogenesis. Whilst Archer's analytical histories of emergence have explanatory potential, Lloyd's work slides towards the positing of structures, and talk of using '*generalisations* about the *dispositions*, *powers* and *processes* of persons and social structures' appears to downplay the role of narrative. Whilst advancing structures as hypothesis is an acceptable first step, only when emergent structure and culture as hypothesis are tested by being spun within an 'analytical narrative' is explanatory value added. In developing such social accounts it is necessary 'to expand, or "unpack" them, by detailing the social processes that constitute the effective mechanisms' (Nash, 1999, pp. 454–5).

Putting concepts into a dynamic process exerts some regulatory effect upon them and avoids the error of structural analysis which abstracts but fails to explain origins (Sayer, 1992, p. 97). Further, an emphasis on interplay *and* causality avoids equating temporal succession with explanation – the prime error of story-telling narratists (cf. Sayer, 1992, p. 260).

For CR, cause/effect and multiple causality are central to its explanatory social science. Taken together, the analytic and the narrative help weave plots of how situational logics face actors, and how these are subsequently resolved through time. Some suggest this retention of Newtonian cause and effect is at the cost of ignoring simultaneity and connection, and downplays complexity. Certainly, the framework remains wedded to a systems perspective rather than a complexity/chaos approach (Adam, 1995; Byrne, 1998; Urry, 1999).

Analytical emergence contains the kernel of a position that may withstand many criticisms levelled against CR and offers a fruitful path for historical sociology. It is preferable to an unsatisfactory notion of 'crisis' making things clearer at certain points in time (see Benton, [1981] 1998, p. 309; Kemp and Holmwood, 2003). It is also an antidote to the criticism discussed in the previous chapters that CR posits 'dispositions' and 'powers' without spelling out how these are explanatorily appropriate within specific instances (cf. Nash, 1999). Not content with vague postulation, morphogenesis (or the revised variant discussed below) moves to 'test' analytical hypotheses in the form of causal narratives which explore interplay and emergence. Archer (1995, p. 175) writes:

The morphogenetic approach makes no leap from the real to the actual but rather dwells on the ground between them by analysing the generative mechanisms potentially emanating from structures (and cultures) as emergent properties and their reception by people.

The remainder of this chapter is concerned with pursuing the opportunities presented for historical sociology by morphogenesis.

Appending Morphogenesis: Towards 'Retroductive Narrative Realism'

Chapter One noted a three-way distinction of theoretical *exegesis*, rubbing shoulders with alternative positions and frameworks in *critique*, allowing the *practical* activity of historical writing. This tripartite task has been, and will continue to be, the overarching guide to the thesis. But whilst there has been some empirical historical work (e.g. Archer, 1979), as it stands morphogenesis is an explanatory format still in need of greater empirical substantiation (cf. Zeuner, 1999). There are unanswered questions around how historical accounts are reached under morphogenesis, and this is symptomatic of a broader fault line (discussed in the previous chapter) that runs through CR philosophy and surfaces in the practice of historical sociology.

Although CR is not a method, further attention must be given to the nature of practice for CR to underpin a viable historical sociology (cf. Pawson, 2000, pp. 284–5). Failure to do so will result in two discrete projects, CR philosophy and historical sociology, with the latter paying lip service to CR but never fully cognisant of what it is doing. The interjection of Steinmetz (1998, p. 171) may be seen in this light, because, for him:

most historical researchers, whatever their self-description, are critical realists... and [that] this stance is the most defensible one of the social science in general on ontological and epistemological grounds (see also Isaac, 1991; Chapter Three).

It is important to resist the call of common-sense to ground realist historical sociology because it elides a series of moves that should be transparent and clearly spelled out. In too many places CR is unable to get 'off the philosophical page' (Pawson, 2000). Questions still remain around how best to bridge the distinction between CR philosophy and historical sociological method, how transitive accounts are constructed, and how accounts are critiqued and arbitrated. Whilst Archer's work is central to my endeavours, the practical work also incorporates a number of more explicit methodological insights including those of Miller (1987), Abbott (1990, 1992), Fairburn (1999) and Gerring (2003).

Succinctly stated, my overall position is that given an immanent critique, some ontology is required, and grounds are advanced for what this may be. While willing to debate ontology, there is a refusal to surrender the practical side of the project and pursuit of substantive conclusions. The substantive conclusions that are offered in Section Four are not put forward on a 'take-it-or-leave-it basis' but are guided by a series of principles and are subject to procedures that serve to arbitrate. Central to my argument is that, in developing substantive accounts, greater attention must be paid to the historical sociologist as analyst *and* narrator (cf. Keane's discussion of culture, 1991).

Kaul (2003, p. 715) asks a crucial question of CR that drives to the heart of its practical value:

who performs these moves and uncovers these causal mechanisms, thus explaining the surface phenomena? In passive 'scientific' fashion, the critical realist theorist (as methodologist) is missing from the picture.

Her point, that the transcendental stance frequently wishes to avoid being sullied by the practice of social science, is an important one. While Kaul overdoes such criticism, in effect threatening to overlook some key CR insights, she is correct to suggest that further work is necessary to address epistemological and methodological challenges. Given that CR is clear about *why* such work is being undertaken (to produce fallible knowledge), to

Kaul's question of *who* is doing the work, should also be added the question of *how* such work is undertaken.

In tackling practical work, the historical sociologist faces inevitable complexity. It will be rare to find cases where the corpus of a social system (institutions, roles and distributions) or cultural system (beliefs, discourses, and ideas) are completely aligned, giving one emergent core of complementarity or incompatibility. Not all agents are involved in every sphere, while some interests will cross-cut a number of spheres. Facing such complexity, the strength of Archer's contribution is its centring of process, with the separation of people and parts and their interplay over time within a series of cycles. Thus the analyst cuts into the complex social world to identify and trace these cycles, and then proceeds to present them as narratives. However, historical accounts in the completed narrative presentation frequently overlook the work that goes on during their construction. As well as recognising the practical historical activity that is *processual* and involves tracing cycles through time within the narrative exposition, there needs also to be greater recognition of the *precessional* practice of putting up intransitive theories or hypotheses, and the ways in which the analytic and narrative are linked.

Two practical moves are being delineated: first a form of retroduction or backward mapping that elucidates mechanisms through constructing theories. The historical practitioner identifies a particular point in time or activity that they wish to illuminate and then sets about working backwards through time, seeking to uncover the structural and cultural dimensions that condition through their interrelations. While this may take a number of iterations, once completed it generates a series of developed hypotheses. The approach is then reversed into a processional one whereby hypotheses of structure and cultural mechanisms are 'tested' through the possibility, plausibility and strength of narrative exposition. An approach that recognises both of these within historical sociology and seeks to outline them transparently is signalled as Retroductive Narrative Realism. The practitioner works backward through time in their analytic framing and then reverts to a processional move in the narrative 'testing'. Awareness of these two moves and greater transparency in how they are presented allows some opportunities for arbitration among alternative accounts of change. Retroduction builds a model – positing hypotheses – which if existed and acted in the hypothesised way would account for phenomena:

Retroduction is about advancing from one thing (empirical observations of events) and arriving at something different (a conceptualization of transfactual conditions) (Danermark et al., 2001, p. 96).

It involves trying to get knowledge of what internal relations make x what it is, with the abstraction of transfactual conditions or mechanisms that must hold (e.g. class, patriarchy). The conceptualisation sheds light on what are observables and offers explanation of the mechanism that lies behind the social process.

Archer notes how realism allows substantive disagreement over which structures, generative mechanism, and types of interplay exist within the world, 'Transitive, corrigible narrative is the methodological hallmark of morphogenetic realism' (1995, p. 294). There is value in obtaining further clarity about how this occurs. Although the past has happened and been uniquely caused, our knowledge of the past changes, and so requires recognition that theories generated are historically contingent, for 'it is within the power of the human mind to revisit past events, to reinvent them, create alternative versions...' (Adam, 1995, p. 18). Opportunities always exist for re-examining provisional knowledge: new archives may be uncovered; alternative ways of conceptualising issues may develop with the dawning of a new epoch, and so on. While none of this is denied by CR accounts, the question of when explanation is 'good enough' and the place of reflexivity within historical activity would benefit from further attention.

Exacting explanations

The writing of the substantial account within Section IV is informed by the underpinnings of ontological realism, epistemological relativism, and judgemental rationality: knowledge is historically determined but grounds and approaches exist for arbitrating among knowledge-claims. This section alludes to what some of these grounds may be. However, given the ontological roots for CR historiography there are prior ontological considerations of culture, agency, and structure, with social action structured by objective structures, but agents as the only efficient cause. Research is further underpinned by a commitment to entities that are not immediately accessible to observation, as well as irreducibility, and the interplay of agency, culture and structure over time. Such ontological moves, while necessary, are not sufficient and must be accompanied by two methodological moves

around putting up hypotheses (an analytical move), and subsequently offering some form of ‘testing’ (the narrative move).

The analytical move

This methodological move is best summarised as ‘what are you arguing’ (cf. Gerring, 2003). It is about the forming of causal propositions, hypotheses or theories, and is essentially a *precessional*, i.e. retroductive, move. It involves a series of thought exercises and experiments in the search to suggest causal mechanisms.

First, it will necessarily involve the initial conceptualisation of a particular problem or question, and the meaningful and acceptable nature of these initial definitions and specifications. There needs to be clarity about that which is to be explained, and whether the continuous and complex social process that is selected is a worthwhile one for historical inquiry. For example, in the practical work that follows, a decision was made to explore ‘social science research’ rather than some broader or narrower concept (such as ‘intellectual activity’ or the academic discipline of ‘social policy’). Grounds must be given for some things being excluded from inquiry, and for a particular question being delineated within social processes. During the practical work that follows, such delineation is bound up in an argument that the causal stories of social science research differ from those that may be told around learned societies, individual academic departments or government departments.

Within Section IV that follows, once definitions have been clarified, three institutional fields of the educational, political, and socio-economic are suggested for inquiry. The reader must ask whether these fields and their interrelations are plausible, and whether evidence that accrues in support has some purchase on the world. Explanation is here being signalled as partly empirical, partly logical and partly social (Lloyd, 1986, p. 131). Analysis clarifies institutions and relations, separating those that are perceived as significant. The diagrams that bridge the four analysis chapters of Section IV are intended to outline clearly and transparently these structures and their interrelations. The analyst must be willing to leave some things out, and to foreground others within the structural and cultural mapping that occurs.

Investigation never begins from a *carte blanche*. Explanations are demanded by people with particular levels of pre-understanding and interests (Sayer, 1992), or as Gerring (2003) notes, 'Causal arguments rest delicately upon a skein of truisms and theories' (p. 16). The theories that are suggested may be explored for their resonance with what is known. While some may fit with the taken-for-granted ('common-sense'!) appeals, others will require more theoretical consideration. Within different fields there will be tentative knowledge held – for example within education, socio-economics, and politics. These preconceptions should not always be upheld and should be open to new knowledge, but neither is the intention always to look to overturn these. Thus sometimes an explanation may appear obvious because it resonates with our commonplace understandings. But at other times it is the obvious that is made to appear dubious, and new grounds are brought in to replace a provisionally held theory. Within the precessional, the research question and the structural and cultural relations that are advanced must be important and insightful.

The commitment to searching for deeper mechanisms is the difference between statements such as, 'The growth of government and University research led to the growth of social science research' and accounts that focus on how the research grew, why it grew, and the ways it grew. The former question is the sort Chapter Two found problematic for historical sociology. Thus the analytic move must be about mechanisms, saying that some things matter and others do not, giving the narrative a focus upon which it can then interweave.

This first move, at root, is a comparative one – many sets of mechanisms are contemplated and linked, perhaps generating a series of clear and understandable hypotheses. The foundational suppositions of the theory, the base of experience referred to as supporting the theory, and the chain of argument behind the theory must all be as clear as possible. While diagrams within the analysis attempt to illustrate the causal mechanisms, such sketches leave open the possibility of imagining a clutch of mechanisms. In the analytic move the explanatory motor is idling, and awaits the narrative exposition to drive explanation forwards.

The narrative move

Narratives that link through cycles are the second, necessary, methodological component of Retroductive Narrative Realism. Arguments defending narrative within historical sociology have been part of a broader renaissance of narrative within the field. What all narrative views share is that narrative is more than simply accumulating facts, 'The exhaustive representation of recounting of "reality" is not the objective or practice of any empirical science' (Bryant, 1994, p. 5). There is frequently less clarity about how narrative fits within a broader theoretical and methodological schema and Retroductive Narrative Realism must be aware of such shortcomings. One major strength of Andrew Abbott's early work is its attention to conceptualisation and abstraction, along with his recognition that 'social life happens in stories, that the order of events influences their ultimate outcome' (1984, p. 192). He emphasises levels of analysis, substantive stories, and the particular qualities and approaches of each story. Abbott talks to practical historical sociology and implicit in his work are the two moves, analytic/narrative, being signalled here. That Abbott's work lacks the robust version of causality and realist underpinning does not detract from his valuable contribution around methodology.

The core dimensions of narrative are those of item selection, temporal order and making links between events, occurrences or interactions (cf. Maines, 1993). Narratives are more than simply filling in the background context to enrich the account, and while never a story of absolutely everything, are also beyond a simple chronology of ordered events. It is the role of narratives in providing linkages that is crucial, and it is against such requirements that the quality of a narrative must be judged. Narratives must trace connections and attempt to introduce a dynamism into the account, offering compelling 'micropathways' (Little, 1991), which must always be peopled and interest-ridden. While this role may lose some of the literary sheen of narrative, the persuasiveness of narrative becomes more than some literary or rhetorical device. Accounts of social change must also refrain from cloaking the narrative with patterns of human action, whereby the narrative exposition becomes one that contains rational switchmen (for example Kiser and Hechter, 1991; Kiser, 1996). Thus narrative is about how people act given their interests and places within social structure, not about how they *must* act.

The practitioner may take numerous iterations to complete the narration process, and there is a constant precessional and processional movement within historical practice. This movement is temporarily suspended with the presentation of a coherent and evidentially supported narrative account that interweaves the people and the parts, and is anticipated to withstand critique. When the problem of devising such a narrative proves insurmountable, this triggers a return to the precessional methodological step and a reconsideration of structural and cultural relations. Developing the narrative causes the researcher to pay attention to a range of narrative possibilities, their consideration and elimination. Questions that may be asked of the narrative include whether it suggests a greater number and stronger links than do alternative versions. The narrative moves beyond actor interpretations and their own words in providing linkages.

In assembling a narrative, there is use of empirical evidence, including the breadth and extent of evidence saturation. The empirical material must be credible; there must, for example, be a wide search of sources, a search for rival sources of evidence, and the *possibility* of evidence that would falsify theories (Fairburn, 1999; Gerring 2003). Data accumulation is a good thing, but the theoretical nature of the analytic research move prevents a slide into empiricism. Richard Miller's (1987) view of explanation as causal, comparative and historical is useful, 'Typically, if not always, an explanation is defeated by an argument that some rival is superior' (p. 52) and 'fair causal comparison of a hypothesis with its current rivals' (p. 155). Exacting explanations uphold both the analytic and the narrative, offering transparency and looking for alternative explanations. Confirmation can only ever be tentative.

Summary

An account must not lose sight of its causal commitment: that is the search is for causal mechanisms rather than a covering law, functional or teleological explanation (cf. Bunge, 1959). To do this requires two practical moves within Retroductive Narrative Realism: the analytic one of achieving clarity over conceptualisation, and how, where, and why the analyst is cutting into continuous social processes. There is clarity about the structural and cultural dimensions and relations that are being placed in the foreground, and a commitment to 'enchainment', to narrative as moving beyond describing periods or frames, but also to how these frames themselves change.

In constructing the narrative, there are similar sets of principles: considering and eliminating alternative accounts within the construction of narrative; transparency and the possibility of re-tracing steps; the centring of interests; and arbitrating using empirical evidence, logic and social considerations.

Working with a CR commitment and drawing upon these two procedures of analysis and narrative, guides the practical account that follows in Section IV. Theories should be open to critical examination in both analytic and narrative forms, but when put through robust procedures they are not then offered on a take-it-or-leave-it basis. Instead, they are the best claims to knowledge currently available of particular dimensions of social change. And the tentative knowledge awaits replacement by theories that, while developed in a similar manner, offer greater explanatory potential compared with those they are displacing.

Chapter Nine Notes

¹ Their views are distinguished from the weaker argument that agency, culture and structure have *some* explanatory contribution to make.

² See also Archer (1998a), for an identical summary of positions.

³ He levels identical criticisms against Bhaskar (King, 1999b).

⁴ There are parallels to Rawlsian theorising that allows a starting point where there are no interests and no identities, something Susan Moller Okin (1989) takes severely to task.

Section IV

Getting the Job Done

Chapter Ten The Inheritance

Introduction

Section IV of the thesis proceeds from ontological and methodological considerations to discussion situated on the terrain of substantive analysis. It moves beyond recognising ontological regulation and the requirement that agential, structural and cultural interplay be constitutive of social explanation. The substantive explanation is the result of endeavours grounded in a commitment to ‘Retroductive Narrative Realism’ that allows both precessional (analytic) and processional (narrative) activity. In the first instance a series of intermediary ‘tools’ are developed as a result of this precessional activity. These result from situating social research at the intersection of educational, political, and socio-economic domains.

By interrogating these domains key concepts are devised. These include a view of the State that is inherently temporal and peopled – the State is never simply a bundle of economic or political processes. Thus the ‘Colonial state’, ‘welfare intervention State’ or ‘planning and knowledge State’ are not references to what the State does or must do in going about its business. Instead, such labels are intended to pinpoint interests within the State and highlight the agendas they seek to advance. For example, ‘Colonial State’ highlights the grafting of particular interests and aspirations in the 19th century historical setting of the South Pacific.

Notions of group ‘interests’, including prestige, resources, and autonomy are central to the account. The interests that are held by social groups are multiple and serve to create tensions and facilitate alliances across groups. There is, therefore, a range of trading currencies which groups may seek to accumulate and it is never enough to advance a gold standard explanation of interests always being ‘power’ or ‘income’. Understanding how different group interests coincide or conflict (e.g. interests across the public service, politics, education and social welfare) allows the account to explore the appropriate place of ‘reform coalitions’. Similarly, academic disciplines are first defined through their

shared and diverging interests rather than by focusing on ideational and conceptual apparatus.

The colonial, Dominion and postcolonial 'contexts' become important tools. Appropriately conceptualised they point to changes of structure and culture, and with the centring of diverse group interests change through time can be explained. 'State interests' are detailed including specific political interests, Departmental concerns, public servants, divisions and alliances within the public service, teachers, and so on.

Understanding the University as a Colonial, Dominion, national, and then more global institution with the relationships these involve is pivotal. It requires a conceptualisation of the academic and intellectual that reflects how the institution is embedded *vis-à-vis* local pressures and resources and wider trans-national flows. Similarly, the University itself is also constituted by the interests of teachers, Colleges, the Senate and disciplines.

In knitting together these concepts the substantive narrative is able to complete the explanation of the emergence of social science within New Zealand. It works within the spheres of State, University and broader social spaces, centring interests and the role of conditioned interaction to produce causal explanation. The 'development of research' is a concept pared or colligated from the stream of social life, and the *explicandum* includes an increased volume of activity within both State and University, changing notions of what constitutes research and research use, and the emergence of a specialist research role. Interests lies in the mechanisms that are abstracted from this stream of life together with actor interpretations. Four periods are identified by way of abstraction and precessional analysis.¹

The account is presented within four phases: first, *The Inheritance* outlines research by the State for the purposes of the State (Chapter Ten). It explores why and how research in its early guise was tailored to activities of statecraft. Second, *In Search of Independent Means* (Chapter Eleven) discusses the widening of research bases to incorporate research by State Departments of broader of social interaction that involve it looking outside its own administrative apparatus. Third, the chapter around *Alternative Benefactors* (Chapter Twelve) explores the beginnings of research undertaken by broader social and University

interests around social, cultural, economic and political life. Finally, *On the Brink of Bankruptcy* (Chapter Thirteen) highlights the questioning of a State-driven agenda of empiricism and fact-finding by interests within emerging social movements and a more assertive academia. Over these four periods it is possible to trace the emergence of social research, then social science, and then specific disciplines.

The chapters of analysis illustrate how structural and cultural conditioning, and subsequent actor interaction, give rise to differential outcomes depending on forms of conditioning and how actors interpret the context. The account presented in Chapters Ten through to Thirteen contains exemplars of both stasis and elaboration, a reminder that historical sociology does not solely unravel complex change but must also account for continuities in patterns, and that making sense of stasis is as important as elucidating change.

In reintroducing the methodological insights of previous chapters, analysis avoids focusing upon individual pioneers, and structural principles or evolutionary sweep. These twin errors of, on the one hand, actualism, and abstraction on the other, give way to a more dynamic and industrious historical account that incorporates constraint, enablement, action and, potentially, explanatory depth. Following abstraction these mechanisms are then presented in the process of subsequent interaction. The first chapter in Section IV addresses the period up to the 1920s. The introduction of narrative avoids equating 'what came next' with causality, and the sharp contrast of these chapters with the 'reportage' described in Chapter Two should be very evident.

Colonial New Zealand as Structured Context

The beginnings of the explanation lie in the broader setting of Colonial New Zealand and the contexts that were shaped for and by actors. An attempt is made to isolate the structural/cultural relations of social science research activity. How contemporary actors respond to and mediate these contexts is the second part of the cycle, and stasis/elaboration the *dénouement* prior to the commencement of a new cycle. The constraints and opportunities presented by colonisation, diaspora, and burgeoning State activity constitute the first phase of inquiry. Consequently, the question is: why and in what ways did the colonial inheritance condition the context and subsequent emergence of social science research?

Given the resources that are required for social science research, there are three possible bases from which it can develop: State bureaucracy, academia, and other areas of civil society including associations, reform movements, and wealthy individuals. A recurring analytical theme is one of State dominance of empirical activities and how social, political and educational conditioning mitigated a more developed range of social science research activities up to the First World War. Thus, State interests were more powerful when compared to other interests. As suggested in Chapter Two, embryonic social science research activity by the early 20th century was conducted in the context of occasional Royal Commissions, undertaken by the statistical bureaucracy, and carried out by a small number of senior public servants.² The calling of the University of New Zealand was to be the provider of examinations and this was a calling that echoed around the corridors of the four teaching Colleges. There was little extra-State activity – higher education was underdeveloped and social interests failed to initiate a reform movement and to provide a locus for research activity – unlike the situation in Britain and the United States during the 19th century.

The actualised picture is one of marginal State developments and stunted social science research activity within the University, but a fuller account consists of revealing mechanisms and their operation. By considering how cultural and structural emergents synchronised and entailed restricted or broadened opportunities for actors, it is possible to avoid a picture situated at the level of Humean events. Explanation is irreducible to narrative biographical chatter or analytical didacticism of structural variables; both modes

must instead be fused into a more fluent exposition. This is the familiar refrain of Margaret Archer highlighted in Chapter Nine. To her insights we must add further clarity around how this *explanatory conversation* is achieved: how are concepts defined as particularly useful and how is the transitive nature of the account balanced with surety of being a more reliable or truer account than the next? These were the questions that were addressed in Chapter Nine and such suggestions are implemented within the practice of the next four explanatory chapters.

Whilst the experience of colonisation provides the point of departure for New Zealand's socio-historical writing, there is no agreement around its precise implications for later developments. The starting point of this analysis is to invoke a structured context of colonisation to ask about: effects of scale, roles, positions, relations of educational, political and economic systems. In conceptualising the colonial, a three-fold framework whereby colonisation is viewed as *spatial*, *temporal* and *process* is suggested as a route to identifying mechanisms. Colonisation is a spatial phenomenon with settlement establishing satellite territories that are wrenched from the Mother country but weaned on its continuing cultural and structural influences. Relocation produces tensions between what is left behind – in the English and Scottish tradition, stratification, cultural idiom and influence – and what is recalled in the context of building anew. Even when severing the umbilical cord of sovereignty, the 'dutiful daughter'³ continues to be nourished through the norms, values and institutions of the Mother country. Such traditions are absorbed by the Colonial State and society and serve to nurture the growing economic, political and social body.

Colonisation's spatiality is captured beautifully with the naming of *New Zealand* and the terms 'antipode' and 'antipodean'. The latter, defined as 'being at polar extremes' and 'on the opposite side of world' defines communities geographically through emphasising where such communities are not located. Thus as shorthand for place, antipodean becomes synonymous with *isolation*, with being the England and Scotland of the South Pacific, with (dis)location.

In relation to social relations and resource distribution, colonisation is both enacted within a particular antipodean place and during a particular temporal moment following arrival.

Settlement and confiscation, development of infrastructure, and construction of institutions occur upon a landscape without a Western Enlightenment tradition, thereby necessitating a continuous grafting by settlers between ideas/aspirations and local conditions. As a consequence, analysis must explore the particular experience conjoint with cross-national events and influences that include the experience of war, the growing contribution of science, and global economic depression. Such grafting points to colonisation as an on-going *process* that occurs *through* rather than simply 'in' time, incorporating ideological (religious, linguistic, educational) as well as a material and institutional envelopment. In emphasising that colonisation must be viewed through, rather than in, time, it becomes possible to discount colonisation as simply another variable to be factored into explaining social science research and which somehow adds an 'extra dimension' to the experience of non-colonised Western nations. Instead, clusters of institutions and emergent relationships develop from the economic, political and social residues of colonisation, and such relationships lie at the centre of investigation. Influences are both structural and cultural, including social roles and positions within society, and ideas that developed as a result of settler antipodean location and innovation. Maintaining spatiality and temporality as core considerations becomes an important means of alluding to generative mechanisms. This chapter explores how colonial (dis)location conditioned contexts and activities, helping to elevate the State within the new society. It discusses the conditioning effects of the Colonial inheritance and its playing out via economic, political and social group interests.

The founding of the State

The State is a body with centralised legal, economic and political resources, steered by elected parties, and staffed by a bureaucracy. The colonial state undertakes a range of interventions that involve it reaching out into what were relatively empty spaces of civil society, and as a consequence it towered above the late 19th- and 20th- century landscape. As an illustration, New Zealand's domestic history is frequently mapped onto the contours of State activity and the State becomes the lens through which events are classified and time periodised, eliding history with State history.⁴ Indisputably the New Zealand State played a significant role in forging the nation in the crucible of post-settlement activity. As the sole institution that emerged as truly national (following the reduction of provincial power in 1876),⁵ it overarched more limited organisation of family, church, trade union, and voluntary association.⁶ The early State took responsibility for easing the passage of

settlers, ensuring lands for settlement and jobs for workers, thus was a template of State intervention inscribed upon political, economic and social life.⁷

Institutions that in countries elsewhere rivalled the State – church, trade unions, middle-class reform organisations – lacked resources within the settler context of New Zealand. The settler Churches had limited opportunity to tax the souls of the living and to accumulate wealth on the scale of established European orders. That the colony was in the early throes of capitalism, and a constricted industrialisation, served to weaken trade union organisation and labourers turned instead to the New Zealand Company for employment opportunities.⁸ Although associations of Friendly Societies covered one-fifth of males by 1919, organisations were small and, taken together, lacked political sway.⁹ The underdevelopment of capitalism meant fewer resources were available to support philanthropy and, as suggested by the experience of the 1885 Charitable Aid and Hospitals Act, many voluntary organisations required State support to remain afloat.¹⁰ But whilst Colonial New Zealand was a relatively ‘flat society’ with limited stratification (beyond that of Maori and Pakeha) it was not necessarily cohesive.¹¹ It was in these circumstances that the State stitched together a thin patchwork of institutions, working to overcome the prevailing provincialism. Add to this the relatively small population of New Zealand and limits created by the effects of scale, and it is understandable that the State became the broader social and economic thread that held the fabric together.

From the outset, the State was intertwined in the practice and experience of settlement. Abolishing provincial powers led to Government Departments administering functions on a national scale, and Vogel’s expansion of railways, bridges and infrastructure was a result of private sector inability to meet demand. Unlike Britain, where the State was associated with class and religious-based sectional interests and scarred from Poor Law Reform, Corn Law repeal and struggles for public education, the Colonial State was embraced by those seeking opportunities in the South Pacific.¹² To State responsibilities by the end of the 19th century were added regulation of working conditions and economic activity – including arbitration, public works and labour bureaus, tariffs, and an expanded role around economic development and expansion.¹³

A growing State role required an administrative structure that would administer and enforce, and here the public sector played supporting cast to the State's lead. By 1904, nearly 10% of all earners were on the public payroll; the 1880 Royal Commission recorded 10,853 public servants, a figure which had risen to 22,739 by 1904. Between 1867 and 1912 central Government employees increased 13.5-fold, while the population increased only 5-fold.¹⁴ Of this increase in Government employees, 74% were connected to commercial activity, such as the expansion of railways, both passenger and freight, and other commercial departments. Traditional social functions of Government (including courts and police) accounted for 12% of the increase and 6% were involved in 'new welfare' fields such as pensions.¹⁵ The eighteen Government Offices of 1868 had become 84 by 1912, and an increase of perceived and exercised responsibilities encouraged greater role specialisation – from 74 job titles in 1868 to 137 by 1912.¹⁶ One consequence was that New Zealand soon had too many administrative structures:

At no stage was much thought given to organisation: the emphasis has been on getting a job done, rather than on building up a logical administrative structure.¹⁷

In Britain the Northcote Trevelyan Commission (1853)¹⁸ created an Administrative Class and set the direction of the civil service until into the 1960s. In effect, the grade of entry determined career, and entry grade was correlated to educational opportunity and therefore parentage and class. In contrast, New Zealand's service remained generalist in outlook and ostensibly meritocratic, shunning any separate administrative class.¹⁹ The 1886 Civil Service Reform Act introduced competitive examinations in the Colony arranged by the Education Department and this arrangement continued until 1946. Rapid public sector growth by the early 1900s had also created anomalies whereby certain 'temporary' recruits had retained the right to promotion without having to pass internal examinations. The 1912 Public Service Act put greater emphasis on a career service and a diversified public service, and while specialist expertise was accepted under the 'Professional Division' there was no provision for generalist arts graduates. From 1918 the matriculation exam of the University of New Zealand (UNZ) was substituted for the Public Service Senior examination as the general 'promotion' examination for the Clerical Division. Provision was made after 1913 for public servants to be granted time off to attend University lectures and to undertake part-time study, particularly in the fields of accounting, law, and

Brothers, Charles

commerce which predominated within the Clerical Division until 1960. This created alarm within the service that it heralded a preference for a more advanced qualification. Not only was a significant State role emerging in New Zealand but it was also one with politico-administrative characteristics (recruitment, role, organisation) that set it apart from the British ethos of 'a station and its duties', German and French State organisation, and the decentralised system of the United States.

The State intervention that thrived from the later 19th century was based on a pragmatism rather than an ideology and certainly not on any weighted scientific analysis. During his visit in 1907, Sidney Webb noted the almost complete absence of organised study of social problems in New Zealand.²⁰ Such a criticism pinpoints the tradition of many State Departments to eschew reflection, opting instead for direct intervention and practice. As one commentator notes, 'statistics were a mere adjunct to the process of social engineering' with information utilised for day-to-day administration rather than for more sustained social and economic analysis and reform.²¹ Arguably, data collection, research activities and the broad analysis of social structure presumes an intellectual and professional class with interests in social policy and reform and a willingness to act on such information. It is difficult to identify groups with these interests in New Zealand at the century's turn, either on the demand side (politicians, public servants), or on the supply side (University or reform bodies such as charitable organisations or Fabian societies). The New Zealand reputation as a social laboratory frequently muddies the water and is best read as a comment upon the innovative and conjectural nature of intervention and not as alluding to rigorous hypothesis-testing. Thus:

Experiments there have been in abundance; but laboratory conditions are almost wholly absent. New Zealand's state experiments are in the main the result not of a scientific and enquiring spirit, but of a deep-seated disrespect for authority, tradition, and the experience of the past.²²

To this point the focus has been on the resources and role sets associated with the early New Zealand State. Focusing upon the State is not enough, and given that power is a relational concept, the State must be seen alongside other spheres.

The University of New Zealand

In some ways, the discussion of the UNZ is a continuation of the discussion in the previous section. The State was instrumental in shaping the New Zealand University system. In most countries University development after the 19th century is the result of broader civil society and social groupings but New Zealand's colonial inheritance presented a set of conditions that distinguished it. In the early years of its life, the New Zealand University 'was almost an abstraction'²³ because the University that resulted from the 1874 University Act undertook no teaching and hosted no students, and was far from a Humboldtian vision. Instead the University arranged examinations for the four provincial teaching Colleges that were in existence by the century's close. The Colleges themselves resembled teachers' institutions or civil service training bodies. The creation of a single University in any one city went against the contemporary provincialism that was strong during the 19th century. Otago came closest to grasping the University mantle in 1869, but the establishment of Canterbury College in 1873 shifted the balance.²⁴ Over time, dominance of Otago and Canterbury was increasingly challenged by the growth of Auckland and Victoria Colleges and broader North Island interests and strength. These internal relations – the separation of University and College, and dispersal of academic power through inter-collegiate rivalry – acted as important restrictions on the development of the University system overall. With the University of New Zealand restricted to an examining role, and teaching the responsibility of the Colleges, a vacuum emerged in the lobbying of central government. Pressure to secure adequate State support for teaching and research was further diluted in the inter-provincial haggling of Colleges. Nor was this provincialism fully harnessed by the Colleges themselves (through local benefaction and provincial interests) and used to assert provincial power *vis-à-vis* the State. Thus whilst provincial power was eliminated in the 1870s, a lingering *provincialism* proved more resistant to change and acted as a brake on the development of the University of New Zealand.

A social and economic context relatively devoid of an embedded intelligentsia or class structure presented significant structural challenges to the University of New Zealand. This is not to deny a significant amount of discussion and debate throughout New Zealand society at the turn of the 20th century in which a number of British, European and American visitors participated. Such grass-roots intellectualism was conducted within local societies and mechanics institutes, but without there being a perceived need to

monumentalise such developments within a University institution. By contrast, 19th-century English provincial universities of Liverpool, Manchester and Leeds were associated with industrialisation and the desire of the middle class to consolidate its position and to support their aspirations with financial backing. There was sponsorship by local industry (Sheffield, Reading, Manchester), funding by firms (Bristol, Reading) and industrial support for a national concern (Newcastle).²⁵ Higher education in the United States emerged against a background of a minimalist State, vigorous capitalism and the fortunes of magnates such as Rockefeller, Carnegie, Cornell and Hopkins,²⁶ and consequently alongside the tradition of American Colleges as teaching universities meeting the needs of certification and accreditation, private funds allowed the development of elite research universities. The UNZ was deprived of these resources and its construction upon a settler landscape nurtured a restricted set of developments that – as the usurping of Otago University's autonomy illustrates – were soon overshadowed by the demands of the Colonial State.

The University and its Colleges were reliant upon the patronage of the Colonial State for both legislative foundation and financial security. Fundamentally this began through the endowment of Colleges with confiscated Maori land, and for Auckland University College (1882) and Victoria College (1898) there was continued reliance on State benefaction for early development.²⁷ But Otago and Canterbury also looked to government funding for continued survival, something encouraged by the failure of Colleges and the University to secure large private endowments. In his speech of 1910, the Chancellor noted 'every year in America there are millions of pounds given for university purposes. The amount we have got for university purposes from our wealthy men is very small'.²⁸ The absence of established charities, organisations and philanthropy within New Zealand proved ongoing blocks to growth.²⁹ Many formative associations had their relatively limited means stretched by the demands of health and welfare provision (see endnote 9). But there were also competitors from within education: the failure of Canterbury College to secure bequests is attributed – at least in part – to secondary schools such as Christ's College absorbing much of the available goodwill and finance for education.³⁰ The separation of Lincoln College in 1896 reduced the rural interest in Canterbury College still further.³¹ Similarly, in the first thirty years of its life, Auckland University College received only two major benefactions³² and:

The attitude of Aucklanders seemed to be that further government assistance to the College was necessary before private citizens could be expected to provide money. Business wealth in Auckland did not seem to be of the kind from which education benefactions were made.³³

Although Otago University had greater measure of success in the run-up to the First World War, expansion of the University system after 1919 was based upon the State continuing as the main funding source for all Colleges.³⁴ Nor were alumni well placed to fill the gap of largesse; their small numbers and poorly paid occupations as teachers and public servants meant they were both organisationally and financially weak.³⁵ In effect therefore, the history of the early University is written into the institution's lack of history. Groups holding a concentration of resources were not a constituency of support for the University and Colleges. A majority of the colonial wealth lay with agricultural interests, while professions and manufacturing interests, more ideationally inclined to support a University, were prevented from doing so by the limited means they had available for such purposes.³⁶ Those better placed in the distribution of resources (the so-called 'New Rich') were likely to look beyond the colonial system and to send their sons to English and Scottish Universities and their girls to London.³⁷

Lack of external funds, low State funding and a commitment to widely accessible University education all added up to a system adept at providing quantity rather than quality.³⁸ As a proportion of its population, New Zealand had three times as many students as England, and twice as many as Australia.³⁹ Comparisons of spending showed New Zealand to be relatively disadvantaged⁴⁰ and undergraduate numbers increased five-fold whilst staff increased only three-fold between 1900 and 1925.⁴¹ The University system was thus bending under its own egalitarian vision of ensuring access and opportunity but was incapable of matching such commitment with adequate resources.

It is crucial to highlight the resources and their distribution. These structural limitations had implications for academic roles within the University and Colleges which was to have a major consequence for the timing and extent of social science research. Teaching loads were heavy and large classes became compounded by the breadth of subject fields teachers were expected to cover. Many students were enrolled part-time, and to cater for them,

lectures were held early and late in the day and on Saturdays, and a tradition of extramural University study emerged to meet the demands of part-time students.⁴² Writing in 1929, a Professor of English conceded there was much to the charge of New Zealand University education being 'glorified night schools'.⁴³ As a result, teaching and preparation overshadowed research, and education services were tailored to a series of training needs, particularly to ensuring adequately prepared teachers and public servants.⁴⁴ The age of recruitment to the public service was low – sixteen or seventeen being the norm – and many recruits undertook part-time and evening University study.⁴⁵ The role of examinations, encouraged by meritocratic recruitment to the public service, became deeply embedded within New Zealand educational practice and institutions. Recruitment and position were thus determined not by family name, religion or school history, but instead by the role examinations played in sorting and ordering individuals.⁴⁶ The emphasis on examinations – and the relative openness of the University in terms of class, gender, and religion – provide some support to the argument that the colony provided opportunities for upward mobility.⁴⁷

High staff-student ratios meant lectures remained the staple University fare and rote learning dove-tailed with the poorly resourced libraries that existed up to the First World War.⁴⁸ During his visit in 1898, Sidney Webb commented on the poor health of the public libraries and noted in particular the very poor Otago University library.⁴⁹ The proper resourcing of libraries received little attention up until the time when some improvement began in the 1920s and 1930s.⁵⁰ College teaching catered to the needs of examinations, thereby reinforcing a reliance on textbook learning and stifling teaching freedom and creativity. The impact of the examination system on the quality of University life was recognised by the 1925 Royal Commission:

The general impression left on our minds is that the New Zealand University offers unrivalled facilities for gaining a university degree but that it is less successful in providing university education.⁵¹

The generic nature of the early BA – which was a Scottish rather than English influence – encouraged breadth as the key to success 'offering a teachers not a scholar's degree'.⁵² The range of study contrasts sharply with the experience of the United States, where an in-depth focus on more limited areas emerged and disciplinary specialisms were being sedimented by the early 20th century.

As a consequence of the continuing legacy of colonialism, a University ideal was adapted to New Zealand's own local material circumstances. A direct marker of the colonial inheritance was the early University syllabus that was imposed by a lay Senate and marked in Britain until 1930. The Colleges themselves were also staffed by predominantly Oxbridge and Edinburgh-educated academics. An identical story of staffing and colonial influences holds across all Colleges⁵³ and the leadership and inherited academic culture of the University were strongly British until the post-1945 period.

At the turn of the 20th century, University research received little attention and remained grossly under-funded.⁵⁴ In 1908 George Hogben, Inspector General of Education, introduced annual post-graduate research scholarships to encourage investigations 'likely to be of economic value to industries of the Dominion' although such isolated attempts to foster research had little overall impact on a system where resources, function, and patterns of study restricted developments.⁵⁵ Teaching remained the primary focus and there was little attempt to raise the profile of either pure or applied research activities.

In summary, analysing the structural considerations would suggest that circumscribed social and political spaces provide a restricted set of intellectual roles that were confined primarily within, or in the service of, the State. Within the State itself there was a relative lack of resources to expand roles. The University remained focused on teaching with few opportunities to foster research development through its own staff activities given the demands upon them. Nor was there collaboration of the University with broader social agendas, as occurred, for example, in Britain and the United States.

Colonial New Zealand as Cultural Context

To structural resources and roles must be added reflection upon the contemporary collection of cultural ideas, beliefs, values, traditions and theories. These cultural properties, while not attributable to contemporary colonial actors, serve to impact upon their activities. A second major part of unpicking the colonial context involves considering how the roles of the State and University were perceived, as well as looking at prevailing views of equality, security, experts, and expertise. These properties impede actors' possibilities and prevent grievances and demands from being articulated in broader discourse and from being interpreted in a way that might foster material support.

Conversely, properties will also enable action, allowing claims to be expressed in discourses favourable to them gaining support. It is for the investigator to determine where the balance of restriction/enablement lies.

Previous discussion pointed to the towering influence of the Colonial State, but normative ascription also existed with the State accepted as lead partner by interest groups. The State was ‘the hyphen that unites’ in the pragmatic State-socialism that emerged after the 1890s. The expectation of intervention was driven by settlement having blurred individual and central responsibility and settlers had few qualms about looking to Government for assistance in times of hardship and Depression, or prosperity and growth.⁵⁶ In part this resulted from Wakefield’s brokerage, the payment of fares, and early manpower planning establishing a form of contract between settler and Company, a contract that was replicated in a range of social and economic spheres. This carried a structural price of further magnifying State intervention and provision, and such a legacy carried further ideational costs, and so it continued with recurring and spiralling ideational debt drawn against the State, including the perceived obligation to provide land and develop infrastructure. A contemporary observer wrote:

To doubt the power of the State is very rare in the Colonies, and when a colonial finds himself face to face with some difficulty, it is almost always to the State that he first appeals. To what else indeed should he turn? In the early days of a colony there is usually little co-operation between the immigrants; the Government is often the only bond which unites them, and some time is necessary before naturel [sic] groupings are formed.⁵⁷

The suggestion is that intervention was welcomed across the population and little ideational opposition formed to the centralised and expansionist activities of the ‘State Laboratory’. Alternative ideational bases upon which to organise economy and society, such as *laissez-faire* or communitarianism, found few takers across material interest groups – whether manufacturers, labourers, professionals or those in agriculture.⁵⁸ In threading an institutional network the State cross-stitched, appealing to a range of groups, embroidering egalitarianism and the promise of prosperity and security into the cultural design. Thus a range of values, attitudes and aspirations were perceived as not only compatible with, but also *obtainable* through, State activity and intervention. By the time of the Liberal

Government of the 1890s, a shift was underway where few felt the need to justify actions, regarding State growth as a distinctively New Zealand way of dealing with social problems:

Pragmatism *was* ideology. Just as a Frenchman was ‘instinctively’ ‘left’ or ‘right’, so a New Zealander was ‘instinctively’ State-Socialist.⁵⁹

Thus State activity was underpinned by being accorded full support at the ideational level, drawing together ideas that included prosperity, egalitarianism, enterprise and security.

The settler culture that emerged within New Zealand placed less emphasis on theoretical and abstract ideas, exhibiting a pride in pragmatic achievement under specific circumstances.⁶⁰ The 1925 Royal Commission attributed the University’s emphasis on examinations and absence of research as symptomatic of colonisation and settler preference for action over reflection. An experimental ‘what works’ belief in State activity and a distinctly settler can-do emphasis were ideational strands that helped condition the extent and shape of research activities. The State doing something was preferable to inactivity and this attitude impacted on how reflection, contemplation and investigation were only marginally incorporated into the political and decision-making machinery. Siegfried talked about how ‘they propose simple solutions for the most complex problems with an astonishing audacity’,⁶¹ while Martin writes that the:

familiar trait of New Zealand governance was that broad social concerns rapidly became translated into practical state intervention virtually by-passing the sophisticated social and economic research analysis undertaken in other countries.⁶²

Whilst reforms of the 1890s are represented as ‘State-Socialism’, architects of change rarely drew inspiration from principles or blueprints, responses instead arose from tackling a series of practical problems.⁶³ This pragmatic approach to politics and statecraft was compatible with a further cultural ‘strain of anti-intellectualism in the New Zealand character, an undervaluing of brains in comparison with brawn’.⁶⁴

The beliefs the broader populace held of the University of New Zealand provide further insight into the emerging ‘cultural body’ arising from the dynamics of settlement.⁶⁵ The emphasis upon action was reinforced by the absence of a broader push from within civil

society for a set of intellectual roles that would allow a greater measure of abstraction and knowledge-creation.

The experience of settlers within their countries of origin had given rise to a suspicion of elitist and class-based institutions and, for many, University carried connotations of Anglican privilege. Mistrust of higher education existed because most settlers and their predecessors as working men had been excluded from British provision. The experience was carried through generations of settlers and infused into the broader cultural context. It was clear that any University institution established within the Colonial setting would be tailored to Colonial needs, while loftier ideals of a scholarly community, elite research knowledge or cultural transmission, would be subverted to the instrumental concerns of the State. Hence the role of the University within the spatial and temporal confines of settlement was that of a teaching and examining body with direct and practical purpose. Underpinning the role of the University of New Zealand was an egalitarianism and meritocracy. An emphasis on lay knowledge and practical know-how encouraged research to be seen as an aside to the 'real' job of getting things done; engagement was preferable to detachment.⁶⁶ The space that could be occupied by an intellectual cadre became squeezed from both the structural and cultural sides, and filling the intellectual vacuum in place of the specialist, scholar or academic was the intrepid explorer and the amateur collector. The civic reformer on the model of Britain or the United States, in New Zealand was confined to the State apparatus. As Barrowman writes, scholarship:

Was 'amateur' only in the sense that it remained outside the university. The journalist-historian rather than the professor of history was the type of the colonial intellectual.⁶⁷

While Government interest in employing natural scientists stretched to the 1850s, it was lay rather than salaried science that for many years dominated. The development of physical science was strongly instrumental and Government support for it was underpinned by economic expectations.⁶⁸ Thus it was practical knowledge rather than abstract codification or introspection that was valued. Intellectual activities of civil society (such as the learned societies) frequently overlapped with dominant individuals based within the State bureaucracy. Neither did a broader vision of professorial expertise, knowledge generation and cultural transmission gain support from outside the University. There were

rivalries between the embryonic University system and the more democratised learned societies, while state-sponsored activities served to dampen challenges that were levelled from extra-social bases. There were tensions from the 1920s onwards for example between the State's DSIR and the New Zealand Institute (later to be the Royal Society).⁶⁹

Similarly, a broader notion of cultural transmission or ideal of knowledge production was not forthcoming from fledgling professional groups within Colonial society. Public servants and teachers were already served (albeit not generously) by the existing system and its underpinning ethic of egalitarianism. For these groups the system offered a secure footing onto the occupational ladder, and allowed them to clamber steadily up it with hierarchy and roles added as their climb continued. There was little support for, and a great deal of hostility against, more grandiose visions of intellectual life as the transmission of culture, civic duty and knowledge generation. Equally, the older professions of medicine or law when transplanted to the Colonial context were also unable to sketch alternative intellectual designs for the University. Within the University system professional schools of medicine and dentistry occupied a purely vocational realm with teaching an end in itself.⁷⁰ And while professional schools as a whole were better served in terms of resources and broader views of their role, special schools focused upon raising the value of their own intellectual capital and strengthening their own positions, rather than reshaping a broader University vision.

Aside from the place of education and the place of an intelligentsia, what can be said of specific social science concerns during this period? Arguably, notions of social science and branches of specialist activity were not fully distilled into a body of theory, techniques and practices during this period. There was by the early 20th century no cohesive body of social science ideas that would attract support from particular interests. Concepts, approaches and techniques of social inquiry remained relatively undeveloped and still embedded within philosophical and speculative traditions of thought rather than the new science of society.

Taken together, a series of ideational configurations gelled: pragmatism, an emphasis on action, and a strain of anti-intellectualism towards the roles of State and University. Such ideational concerns were broadly complementary with the structural dominance of the

State in social and economic life. These structural and ideational considerations presented no major contradictions that could not be addressed at a level of socio-cultural interaction. There was no additional cultural fund of ideas that advocated the value of social science research and fostered the support of material groups to challenge the dominance of the State. The empirical revolution, and particularly the development of causal and statistical developments, had yet to find their way into the cultural system. Nor did significant material groups develop ideational underpinning and support for a research University or a reformism that would result in social interests pursuing statistical activity and social investigation. The structural domain exhibited little contradiction and few sharp edges, while the cultural dimensions solidified to align with a State-dominated *status quo*. Taken together, such structural and cultural complementarity resulted in objective activity situations for social actors.

There were plenty of pulls for State activity and few blocks; conversely, for the University of New Zealand, and other latent social science research institutions within society, there existed few pulls and significant blocks. The lack of alternative actors, or barriers to encroachment after State intervention developed, becomes apparent.⁷¹ Support for an enlarged State role came from across ideational arenas and few ideas were diametrically opposed. The University was hamstrung and in the circuitous position that activity of knowledge-creation needed to secure value and prestige but in this environment it was difficult to gain initial leverage and an 'intelligentsia' found little support and few resources to encourage it.

Interaction and the Emergence of State Research Activities

From colonisation emerged a number of necessarily linked structural and cultural dimensions, creating negative feedback loops that dampened certain public-sector developments whilst encouraging others and stifling University and society. To this point attention has been focused upon social mechanisms and tendential live wires (of powers and forces). Missing has been a study of how structural and cultural conditioning influences and emergents are received in social interaction (i.e. how explanatory circuits are completed and mechanisms 'earthed'). This section on interaction allows exploration of how stasis is played out *through* groups thereby avoiding reification, and allows people to

become active 'makers' of structure and culture as well as its recipients. The remaining part of this chapter clarifies the role of interaction for explanation.

How did these structural and cultural dimensions condition agent responses and what outcomes followed the resulting interaction? To reiterate, the structural and cultural dimensions were identified above; this social context is not synonymous with what contemporary actors think and perceive because obstructions (and enablements) experienced by organisational actors may be the result of systemic incompatibilities (or compatibilities) of which they are unaware. So any explanatory account cannot simply be what actors perceived was the case. These non-observational relational dimensions move instead to their emergent powers of necessity/contingency, creating various situational logics.

The social context impedes and enables, ruling out unbounded change. Actors also contribute to the reworking and re-making of culture and structure. How and why this occurs involves a cast including: politicians; members of the University Senate; professionals within the State apparatus (public servants; teachers); professional groupings outside the State with some autonomy and power (medical interests); groupings of academics; and potential interests in broader civil society.

The interest of politicians and senior public servants in the development of social science research was confined to accumulating facts and descriptions that would ease the spread of their own activities. The focus was on Departmental size and spending along with one-off Royal Commissions and such research activity was contained within the State apparatus. The rationale for social science research was to assist decision making and the incorporation of social science research into the activities of the State was restricted to the activities of State employees. An expanded UNZ role was not seen as a constituent of or contributor to good Government and intervention, such as offering new techniques of intervention or educating a governing class.

The University interest is divided into various groups and bodies: the Senate, the Colleges as institutional players, academics within each College, and the broader collection of academics as a whole. Academic interests within the Colleges sought to expand the

University role including calling for a change to University function, greater resourcing, and autonomy – dimensions that would directly benefit their own position. But academic demands could soon be extinguished and ‘academics’ (who were in effect College teachers) were politically weak and socially and politically marginal. Under the 1874 University Act, the early Senate was a lay Senate⁷² and some two-thirds of the Senate (the seat of academic governance) was in the hands of lay members. There was no effective hub of representation for academic interests – representation by a lay Senate and appealing to a State funder relatively unsympathetic to their demands did not allow them to gain traction. Indeed, it remained the paradox that teaching staff were not, technically speaking, members of the University at all. That political machination was relatively unfavourable is best illustrated by the thwarting of the University Reform movement before the First World War. Among other things, this movement sought to introduce internal examination.⁷³ Neither did academic influence flourish within individual Colleges, with only the Otago University College Council containing professors amongst its members. As well as such external obstacles, the academic community – somewhat of an oxymoron even then – had various lines of internal division. As one commentator noted of the University Reform movement:

The Senate had no need to divide and conquer. Division of academic opinion was inherent in the diversity of ideal, experience and history of the four Colleges whose identity it controlled.⁷⁴

Aside from academic rivalries within Colleges, there were also those that existed across Colleges. The Otago professors, for example, opposed reform and while the more radical Victoria College took the vanguard on early calls for change, it was in perhaps the weakest position of all the Colleges. The experience of settlement had prevented one or two centres becoming dominant and the compression of University development fuelled struggles between institutions. The establishment of Otago’s Medical School created resentment in the Provinces and Colleges that coveted such a school of their own. It was a similar story in other prestigious disciplines; Canterbury College, pre-First World War, for example, resisted Auckland’s attempts to introduce an engineering school.

The reliance of the University and Colleges upon the State during this time created a relationship verging upon indenture. The State and the UNZ/Colleges were locked in a series of one-sided exchange relationships, whereby teaching activities and the University

vision were subordinate to priorities and funding set by the State. Within the arrangements that existed between the State and University, research knowledge was viewed as relatively unimportant and there was little (in terms of structural or cultural considerations) to convince politicians or senior public servants otherwise. The flow of services from the UNZ/Colleges to the State focused upon addressing a series of needs for State employees of public servants and teachers, while the financial resources and legitimacy ceded by the State provided few opportunities for the UNZ/Colleges to mount a consolidated structural or cultural challenge. For example, decisions of the 1912 annual conference of the professors, which was an attempt to raise the power of academic staff, were simply overruled by the Senate on matters such as external examinations.⁷⁵ By 1914 the Senate was paralleled by the introduction of a Board of Studies which granted teaching staff a place in the processes of University government. The Senate had changed little, however, and between 1914 and 1925, despite ongoing skirmishes between Senate and the Board, academics were unable to assert their voice. A worsening staff-student ratio did nothing to ease the problems of academics.⁷⁶

Aside from government grants and direct Government funds, alternative funding routes open to Colleges and academics also include the 'sale' or 'gifting' of academic services and actual or in-kind internal funds.⁷⁷ But just as the UNZ found little financial support from the State to enlarge aspirations, neither could it look to external supports – professions, industrial and manufacturing interests, or a middle-class social reform movement. There were few gifts and even fewer opportunities to sell its 'expertise'. Both ideational and structural supports for academic 'expertise' were limited, which strictly circumscribed the efforts of academics and the Colleges to develop greater autonomy.

With social roles confined predominantly within the State bureaucracy, the amount of leverage to be gained by academics and Colleges was restrained. In the period up to the First World War, the academic community was unable to raise the level of services it offered: politically there was little bargaining power and a lack of autonomy, while the microscope effect of small numbers accentuated the splits at College and faculty level.⁷⁸ Restricted bargaining power and lack of internal/external resources meant instrumental activities (e.g. by individual academics conducting research) were relatively few. Nor could academics fully articulate a vision, and they remained unable to gain a discursive

and ideological platform while groups with political and economic power continued to view the University as an irrelevancy. Academic staff lacked broader allies in the educational system with few supportive groups or recipients of University services pursuing change. Within society, groups were heavily dependent upon the State and resources flowed from the State to the University in an a-symmetrical relationship. Many professions (e.g. law and medicine) were able to extract considerable benefit under the existing relationships they maintained with the University via the State rather than through class relations (as per the British experience), or market relationship (as in the United States). The quasi-State activities of public service and teaching similarly received an adequate dividend from the existing delivery of services. Overlapping roles helped disperse opposition: amateur interests of public servants and those within the learned societies meant that many had an interest in maintaining the *status quo* and withholding intellectual authority from a University institution.

Neither ideational support nor resources was forthcoming from a broader public constituency or social reform movement. No material groups developed a competing ideology or assembled resources to give social science research a boost. In Britain a range of private philanthropic interests filled the gap when institutional bases (such as political party, State, voluntary organisations, Universities) failed to develop research and investigative activities. New Zealand had no similar comparative development, and instead it was in the work of individuals such as Tregear and Hogben where initiatives developed.⁷⁹ Only after 1945 were groups of academics able to exact independent access to resources and legitimacy and so begin a shift in the development of social science research.

Summary

In the period up to 1918, resources to underpin social science research within University or society may have elaborated the role and scale of research activity but there was also a lack of demand for such activities. The State served its own needs by developing statistical capacity, while social science research was not considered a constituent of the policy-making process. New Zealand's distinctive spatial and temporal experience of colonisation sets it apart from other countries including Canada and the United States. Colonisation in New Zealand gave rise to a narrow social structure, centralised State,

weakened University, and a distinctive set of relations. Internal-driven change as a response to social interests was unlikely, while State interests were also unconvinced of the usefulness of an expanded University.

As the first part of the account draws to a close, there is by 1914 little activity outside the State. Activity has taken place in the development of a statistics, but not a social science, sociology or social research. Because social research activity is an inherently decentralised system of exchange, there was some small-scale social research activity. Social science research requires relatively low investment and can be conducted by individuals up to the point that training is scientised and professional closure introduced. But social science research must also be valued – attributed with having purpose and thus accumulating ideational support. Gaining ideational support for social research is an important route through which research is validated or invalidated. By the close of the period, the changes that are evident are research *from* the State conducting research that is primarily *of* the State. The social science research that exists is best understood as fact-finding and a State-driven empiricism, contributing to Statecraft.⁸⁰ The University of New Zealand remained on the periphery.

To fulfil the promise of dynamic explanation it is necessary to explore how and why shifts took place through time, rather than simply develop further snapshots. Structure and culture do not stay the same but neither do they change without cause. Nor do groups themselves remain unchanged. It will be the complex interplay between these dimensions that begins to elucidate the emergence of social science research within New Zealand. The discussion so far provides a series of possibilities in the next stage of social science research development: the context facing the State could change; groups with potential benefit from University social science could coalesce, or academics could begin to develop some autonomy. Structural and cultural properties are only relatively enduring, and actors do regroup over time. It is not, however, the purpose of explanation to concern itself with possibilities but rather to show what occurred, why and how, tracing implications for subsequent phases. Developments following one cycle serve to structurally condition the subsequent cycle. It is towards this end that Chapter Eleven now turns.

Chapter Ten Notes

¹ As outlined in Chapter One, the presentation of analysis in Chapters Ten to Thirteen places all references and additional information in its endnotes.

² And even the amount of State activity should not be overestimated. Miles Fairburn (1989) notes a 'general smugness' in the view of New Zealand contributing to a virtual absence (bar Royal Commissions) of official investigation into working conditions, strikes, and the housing, health, and income maintenance of the poor.

³ Hancock (1942, p. 148).

⁴ For Wilkes (1993), New Zealand history has four periods: 1840–1890 the Minimalist State; 1890–1935 the Pre-Fordist State; 1935–1984 the Fordist State; post-1984 the Post-Fordist State. Almost identically for Thorns and Sedgwick (1997) periodisation consists of: 1840–1890 the Colonial State; 1890–1935 the Liberal State; 1935–1984 the Welfare State; post-1984 the Neo-Liberal State.

⁵ 'With the New Zealand provinces, as with jealous and ill-natured individuals, the same unfortunate rule is found to exist; and it would be easier to mix oil with water than to adduce the spirits in one province to unite with those of another, although the want of unity might be injurious to all' (*Rise and Progress of Australia, Tasmania and New Zealand*, 1857, p. 241).

⁶ Fairburn (1989) notes New Zealand's social organisation was 'deficient' which led to the rapid growth of the State and an attachment to family.

⁷ See Sinclair (1983).

⁸ Tennant (1989).

⁹ See Thomson (1998, p. 50).

¹⁰ Tennant (1989); Martin (1996).

¹¹ Miles Fairburn (1989) develops a thesis about the limited existence of hierarchical employer/ squire relationships with workers, the absence of class divisions, and limited, community-based support. The result was social atomisation and an expanded State role.

¹² Earp (1849) writes 'In our colonies are neither Poor Laws, want of employment, low wages, nor any of the causes which at home depress the energies and embitter the existence of the laboring man' (p. 5).

¹³ As Milne notes 'Because of the absence of large private firms and the weakness of local government, the State stands out like a Colossus' (1957, p. 4).

¹⁴ Ewing (1979, pp. 17–8).

¹⁵ Ewing (1979, pp. 17–8).

¹⁶ Henderson (1990). Instead of new work being conducted under existing Departments, new ones were created (Polaschek, 1958).

¹⁷ Polaschek (1958, p. 3).

¹⁸ *Northcote Trevelyan Report on the Organisation of Permanent Civil Service* (18th November, 1853).

¹⁹ Henderson (1990).

²⁰ Sidney Webb commented on the lack of economic and social study at the 'time when the Seddon government was legislating with cheerful confidence' (Campbell, 1941, p. 118).

²¹ Martin (1996, p. 12; also p. 76).

²² Webb (1940, p. 25).

²³ Beaglehole, J. (1949, p. 132).

²⁴ Gardner (1973).

²⁵ Archer (1979).

²⁶ Geiger (1984, 1993); Skocpol (1987); Manicas (1991).

²⁷ Smith (1999).

²⁸ Reported in 'A Word to the Rich; too many examinations', *The Dominion*, 1 July 1910.

²⁹ Campbell (1947); Gardner (1973); Parton (1979); Tennant (1989).

³⁰ Gardner (1973, pp. 146–7).

³¹ Gardner (1973).

³² Wallace (1973, p. 185).

³³ Wallace (1973, p. 176).

³⁴ Morrell (1969).

³⁵ Bloomfield (1984) gives numbers enrolled at the University:

1880: 211 students	1890: 596 students	1900: 805 students	1910: 1,862 students	1920: 3,822 students
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Compare this with the United States' 154,300 undergraduates in 1890, and 582,000 by 1920 (Manicas, 1991)

³⁶ McAloon (1996), using probate records to identify the wealthy in Canterbury and Otago from 1890, suggests the balance was heavily in favour of agricultural interests. Adjusting the cut-offs for 'wealth' 1890–1918 he identifies occupations of wealthy in Canterbury as being: 56.2% in farming; 12.7% in mercantile; 6.1 % in manufacturing; 6.9% in professions. Within Otago: 39.3% farming; 20.8% mercantile; 10.9% manufacturing; 10.4% professions. These probate statistics tend to favour the elderly population, and say more about retaining wealth than actual processes of giving but provide a useful indicator of where potential resources lay.

³⁷ Wallace (1973). This view is supported by Gardner (1973) who provides evidence to suggest: '... that Canterbury's social and professional leaders tended to regard Canterbury College as an inferior alternative in university education, which they would avoid if they could' (p. 138).

In 1909, 48 New Zealanders passed through medical examinations at Edinburgh University, half of the total number who were graduating from the Medical School in Dunedin. 'It seems a reasonable presumption that there is some want of confidence in the possibility of obtaining an adequate medical training in New Zealand' (University Reform Association, 1911, pp. 15–6; also p. 64).

³⁸ Wood (1946); Morrell (1969).

³⁹ Campbell (1947, p. 236).

⁴⁰ Average spending per student in 1935 for New Zealand was £32; Wales £90; Canada £80; England (excluding Oxford and Cambridge) £70; Sydney and Melbourne £70–80; Adelaide £60 (Beardsley, 1973).

⁴¹ Parton (1979).

⁴² Campbell (1941) notes half of University students were studying part-time and 1 in 10 were extramural students.

⁴³ Arnold Wall, *The Press* (30 March 1929).

⁴⁴ Wood (1946); Campbell (1947); Morrell (1969). Sidney Webb, visiting in 1898, noted that nearly all the students were actual or prospective teachers, with little interest in scholarship (Webb, [1898] 1959).

⁴⁵ Lipson (1946); Wood (1946).

⁴⁶ 'In a young democracy, where promotion for the most part was not settled by privilege of rank or station there must be some means of ticketing and sorting, and this was found from quite early days in progressive examinations starting in the primary schools and culminating in the highest degree and professional qualifications' (Wood, 1946, p. 128).

⁴⁷ See Gardner (1979) for observations on women in the early University. But as a broader observation of social life, Fairburn (1989) notes that 'educational qualifications were seldom listed among the many prerequisites for colonial success. Conventional education was no use for getting ahead and therefore attitudes towards it were somewhat negative or indifferent' (p. 55). Indeed, within the public service, qualifications served a highly instrumental purpose and were utilised at the lower cadet level rather than to select an elite administrative class.

⁴⁸ Clarence Beeby, reflecting upon his philosophy class of 1920, suggests 'Every week or fortnight an essay was set on a topic covered by the lectures, and we were expected to give a paraphrase of the appropriate sections in the lecture or in our single textbook' (1992, p. 52).

⁴⁹ Webb ([1898] 1959).

⁵⁰ Beaglehole, J. (1937); Morrell (1969); Beardsley (1973).

⁵¹ Royal Commission on University Education in New Zealand 1925.

⁵² Beardsley (1973, p. 96).

⁵³ See Sinclair (1983) on Auckland; Morrell (1969) on Otago; Gardner (1973) on Canterbury; Beaglehole, E. (1946), Beaglehole, J. (1949) and Barrowman (1999) on Victoria.

⁵⁴ University Reform Association (1911); Beaglehole, E. (1946); Sutherland (1946).

⁵⁵ University Reform Association (1911).

⁵⁶ Bassett (1998, p. 31).

⁵⁷ Siegfried ([1914] 1982, p. 54).

⁵⁸ As Emirbeyer and Goodwin (1994) ask: 'What role did political and ideological and cultural discourse play in sustaining or even expanding relations of solidarity?' (p. 1430).

⁵⁹ Hamer (1988, p. xxxv; italics in original).

⁶⁰ Downie Stewart ([1914], 1982) saw New Zealanders as 'cynical and practical opportunists'; see also Bassett (1997) and Turner (1999).

⁶¹ Siegfried (1914, p. 53).

⁶² Martin (1996, p. 76).

⁶³ See for example Bassett (1998) for the view that there was little consistent ideology underpinning changes but 'a willingness to experiment with the State's powers'. For Condliffe ([1936] 1959) developments are better understood as a form of opportunism rather than ideology or even true experimentation 'It had little to do with socialism. Reeves's phrase, "colonial governmentalism", is a truer description of the Liberal-Labour experiments than "state" socialism or Metins "socialism sans doctrines". It was "etatism" rather than socialism' (p. 183).

⁶⁴ Morrell (1969). Reeves (cited in Fleming, 1987) describes how the rise of anti-intellectualism coincided with growing prosperity 1898–21.

⁶⁵ 'A history of this University, therefore, is a study not altogether of education, though the bounds of education are wide; it is also a study of colonial history, a study of political science, *a portrait of the colonial mind*' (Beaglehole, J. 1937, p. ix; italics added). I owe the term 'cultural body' to Turner (1999).

⁶⁶ Beaglehole, J. (1937); Wood (1946).

⁶⁷ Barrowman (1999, p. 38).

⁶⁸ Galbreath (1998).

⁶⁹ Fleming (1987, p. 59).

⁷⁰ Parry (1975, p. 33).

⁷¹ 'In New Zealand, there are, so to speak, no obstacles, as a wave spreading easily over a sandy beach, the influence of the State makes itself felt up to the very doors of private life' (Siegfried, [1914] 1982, p. 55).

⁷² Parton (1979).

⁷³ Arnold Wall 'A Plea for a system of internal examination in the University of New Zealand' (1908) cited in Wall (1965).

⁷⁴ Parton (1979, p. 30).

⁷⁵ Parton (1979, p. 31).

⁷⁶ Parton (1979) notes this ratio went from 16:1 in 1900 to 24:1 in 1925.

⁷⁷ Geuna (1999).

⁷⁸ There were disputes and rivalries from within University Colleges (e.g. between special schools and broader arts and science faculties (cf. Parry, 1975, p. 10).

⁷⁹ Compare this with the traditions of Britain and the US. Within Britain (Engels', *Condition of the Working Class in Britain*, [1844], 1993; Booth, 1890; Rowntree, (undated); within the US, Jane Addam's 'Hull House Maps and Papers' [1895], 1970; and DuBois, 'The Philadelphia Negro', [1899], 1975).

⁸⁰ Wittröck et al. (1991) make a similar point in an international context, that despite social questions being raised towards the end of the 19th century and groups of intellectuals promoting and pursuing social research and attempting to establish a science of society and politics, there was (bar economics) very little institutional consolidation.

figure 10.1

The Inheritance

(19th Century)

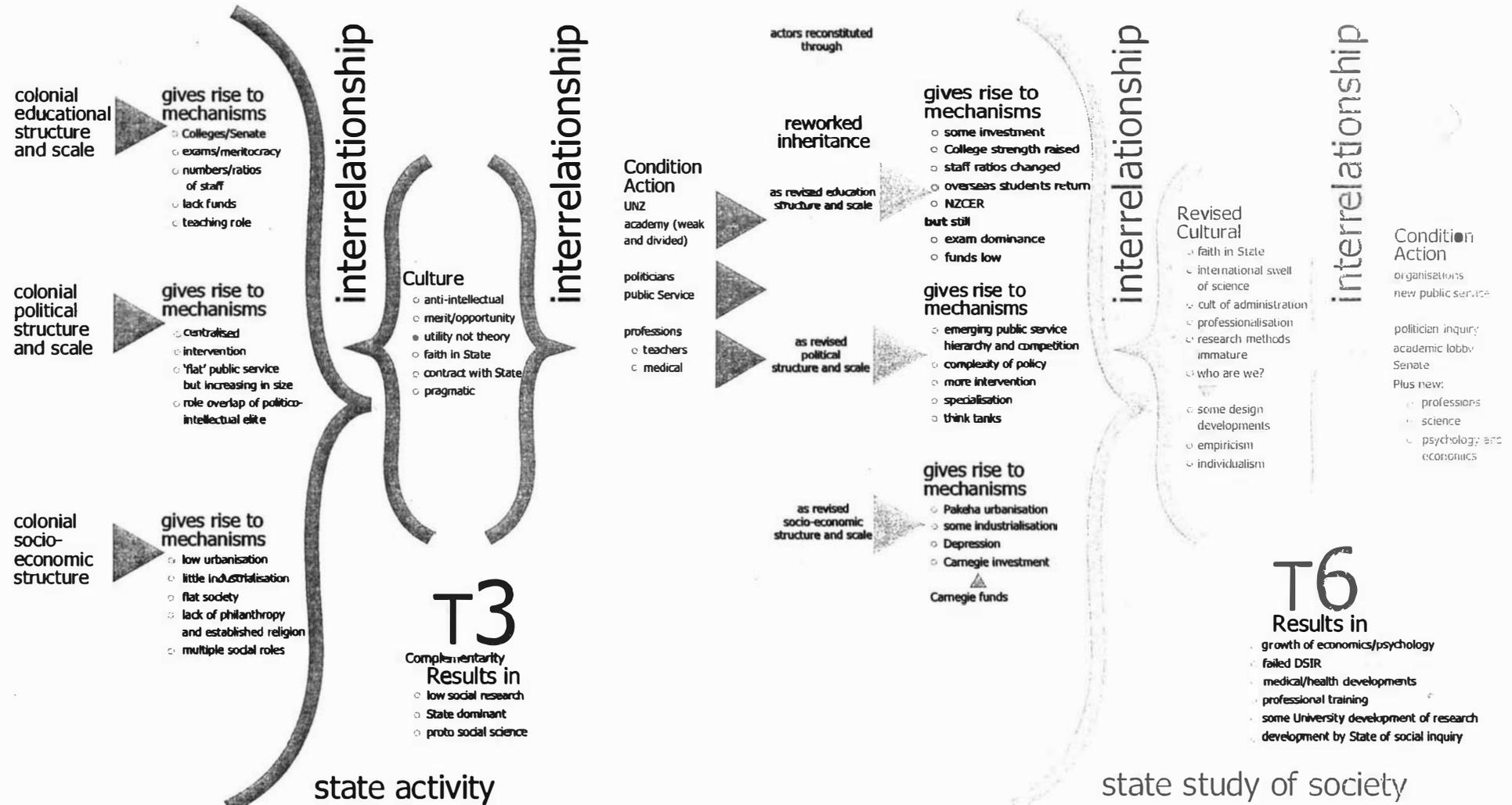
In Search of Independent Means (Inter-War)

T1

T2

T4

T5



Chapter Eleven

In Search of Independent Means

Introduction

The context of colonisation provides the structural and cultural conditioning for agents during the 19th Century and beyond, reaching into spheres of politics, economy, education and society. Social science research stands at the intersection of these spheres and is conditioned by emerging relationships. While such relations condition, they do not determine, and without the interjections of groups and their attempts to uphold interests, conditioning alone is unable to provide an account of change and robust explanation. Earlier chapters made a commitment to explain both stasis and change and this commitment which requires the account to go beyond a series of contextual snapshots and the reactions they engendered. ‘Joined-up’ explanation pinpoints how change *becomes* stasis, and stasis *becomes* change. To illuminate these transformations involves grasping how structural and cultural contexts are themselves subject to revision and, similarly, how social groups become organised and reorganised over time, with new groups emerging to advance the cause of social science research. The spatial, temporal and process dimensions of colonisation are ongoing concerns, with such experiences continuously weaved into ongoing developments of social science research up to 1945. During the inter-war period, alternative roles and positions emerge within public and welfare bureaucracies. Added to this is a growth of financial and other resources available within the Colleges, and a swelling tide of social science ideas from Britain and the United States that promised intellectual and practical returns. These developments of social science concepts must be considered alongside the value shifts that were underway for the broad fabric of polity, economy and society and within education. This new structural and cultural conditioning, and subsequent interaction, allows the next phase of social science research to be explored.

By 1918 the major source of research activity was the State collection of routine monitoring statistics, and the establishment of Royal Commissions for issues of pressing concern. During the inter-war period, however, politicians and public servants as the

makers and implementers of policy identified potential benefits from social science activities and lent their support to its organisational development and educational promotion. State Departments sought to incorporate the insights of social science research into their activities, and a by-product of this attention to social science research was an impetus to College-based activities. In the 1920s and 1930s there began a period of academic consolidation, and rapprochement between academic and State interests, which were both cut short by the outbreak of war. This chapter traces these inter-war developments and unearths the dynamics that challenged the melding of structural and cultural considerations that had previously encouraged stasis.

In addition to increased political and bureaucratic interests in social science research, the inter-war years were marked by the growth in numbers of welfare professionals that championed a greater role for social science research. This had consequences for the UNZ because many professional groups, across education, public services and social welfare, identified their own interests as benefiting from greater University input into preparation and ongoing training activities. The UNZ response to these incentives, both within its teaching of social science and in embryonic research activities, contributed to a growing academic autonomy through the accumulation of resource and ideational premiums. While the chapter begins to identify interests in favour of social science research, the story from here onwards is not some 'research growth-story'. Social science research still remains embryonic because of structurally and culturally conditioned situations.

Shifting Structural Setting: Expansion of the policy space

Following the First World War, a series of changes occurred within the policy context and these changes increased the likelihood of social science research taking root in New Zealand. The nation emerged from the War with a series of pressing social and economic issues that were seen to require attention. These included poor health, inadequate housing and unemployment that intensified during the Depression after the late 1920s. Whilst it is unnecessary to explain every detail of these socio-economic difficulties given that they belong to a different problematic, the upshot was that economic and social issues became of particular concern to politicians and senior administrators, prompting political and voter acceptance of increased intervention. During this time the population was also becoming

increasingly urbanised and the growth of cities presented particular opportunities for the insights offered by social science.

Some 1930s' imagery paints life in New Zealand as a rural idyll, perhaps best portrayed as the society of Somerset's 1938 study. In contrast to this mythologising, *Littledene* is best read as an epitaph to rural life, albeit one that is regularly revisited as a halcyon age. Whilst avoiding large-scale industrialisation, during this period Pakeha New Zealand was on its way to becoming an urban population with a corresponding reshaping of the scope of society.¹ These changes brought uncertainty in their wake around aspects of work and community life that presented opportunities for investigation of the likely outcomes and problems generated by upheavals. There is a parallel with developments in Britain, France, Germany and the United States, where disruption helped breed social research activity through either a social reform movement or, alternatively, a State agenda:

The development of the later eighteenth and early nineteenth centuries thus transformed the familiar scene of the city into a strange jungle, which cried out for exploration and safaris – what was Charles Booth's study but that? – aimed at discovering not the secrets of Africa but the mysteries of London.²

Given a sweeping international recession, the New Zealand State found itself facing new, and increasingly complex, policy demands.³ These encouraged a search for intellectual and practical guides that could help make sense of change and confusion. One consequence of the combination of the colonial social structure, derivative New Zealand political culture, and cultural settler pragmatism, was that this sense-making task was not transferred to a wide-ranging reformist movement or ideological vision. The legacy of colonisation, and the State intervention it bolstered, sustained the expectation that 'problems' would be addressed under the State's mantle rather than through the market, trades unions, religious organisations, or broader civic movement. Although intervention in New Zealand was traditionally shaped by practicality and exigency rather than by blueprint or ideology, the Depression encouraged a shift in political perceptions with new visions and values entering the policy debate. Alongside this infusion of political values was the understanding that complex problems demanded more comprehensive solutions and analysis, that went beyond traditional political and administrative *savoir faire* to incorporate a new expertise and scientific approach.⁴ Scale and complexity were seen to

require social, economic and physical planning, as well as a move to recognise and appraise the particular size, location and incidence of problems.⁵ The strategic implications for decision making were considerable, because until this time the primary role of senior public servants had been to implement Ministerial policy. Less attention had been given to policy development, planning or research, and while some Departmental heads from the late 19th century onwards had blurred that boundary, this was perhaps more a consequence of personalities than a reflection of shifting role requirements.

The Depression produced pressures to do things differently and Coates' decision to draw upon advice including the Public Service Commission (PSC), Commissions and Committees (most notably the 'Brains Trust' which included University-trained economists W.B. Sutch, Horace Belshaw and Campbell) signalled change was afoot.⁶ There was a shift in what was perceived as the appropriate way to conduct and organise policy making. The State was therefore undergoing major change and reorganisation. An expanding role for senior public servants saw the intertwining of advice, interpretation and execution of policy.⁷ As the PSC recognised, 'every public servant in the Dominion must realise that the problem of administration of government is becoming much more complex than it was formerly'.⁸ Hence the fear of Depression encouraged a reconsideration of policy making and opportunities for experimentation, and although the outcome was not solely research activity (advisory committees were also viewed as a substitute for research),⁹ there were immediate spin-offs for social science research and greater opportunities for it to gain traction. Thus new demands upon politicians and senior administrators presented *opportunities* for 'experts' and expertise, including specialisation within the public sector (and the contribution of teachers based in the Colleges). Since the 19th century there had always been a role for specialist public servants drawing upon the knowledge of particular professions (law, accountancy, engineers, surveyors, etc). This inter-war period witnessed moves to reconfigure an overall *policy process* underpinned by social science concepts that went beyond simply specialist *inputs* to a process.

Restructuring the public service

Opportunities for social science research knowledge to make a contribution occurred in two ways: in the role of social science research in strategic policy development; second in the form of the University offering training to State employees, and equipping them with

'scientific' techniques for improved administration and intervention. Either of these routes would help raise the stock of University prestige. There was the expectation that the strategic use of social science research would provide 'facts' and a 'knowledge base' upon which to base more effective and efficient economic and social intervention, while the instruction in new techniques would create a more scientifically informed State workforce.

In addition to the political and public service desires to improve the quality of strategic decision making, further environmental changes contributed to organisational shifts and a re-grouped public service with new interests in supporting social science research. Changes in the staffing of the public service – which was a result of the Depression – including greater competition for public service positions, created a context of increased insecurity and saw an alignment of senior interests within the service. Greater competition as the result of external economic events contributed to the University entrance examination being substituted for the Public Service Entrance examination as the minimum qualification for entry into the Clerical Division. Moreover:

Preference was given to those with partially completed professional qualifications, especially in accountancy or law, a credit pass in the University Scholarship examination, or University Entrance plus the Higher Learning Certificate, in that order before the University Entrance alone were considered.¹⁰

Later, increased intervention by the 1935 Labour Government also fuelled a rapid increase in the number and functions of the public service.¹¹ Between 1935 and 1937, permanent staff increased by 22%, and this encouraged moves by senior servants to differentiate themselves as an elite professional grouping, and to foster the development of a public administration culture and 'consciousness'.¹²

The challenges to hierarchy and group identity fostered a cadre of officials who had interests in securing training and status. One measure, and a subsequent vehicle for promoting these interests, was the Public Administration Society (later the New Zealand Institute of Public Administration), formed in 1935 to promote the civil service and the work of other public servants as recognised professions, and to encourage the study of public administration.¹³ The Institute pursued the trappings of professional status: restricting membership, establishing a journal, creating two study groups and a bi-annual

research prize. Although members of the Colleges were eligible for membership, and public servants of less than five years' standing or lower than Class VI were eligible for associate membership, the Institute was dominated by senior public servants drawn from across 28 Government Departments. The first annual meeting emphasised the importance of research and establishing 'a science of administration'.¹⁴ Thus was a catalyst gathering for social science research *within* government (in the training of public servants) rather than solely *for* government (in the utilisation of strategic advice) contributing to a powerful momentum. The Institute viewed social science research as enabling senior servants to be better public administrators, but closely aligned to this was that the organisation would also be a route to raising their own professional status and rewards.

Apart from the fledgling UNZ there existed few potential sources of legitimacy which the public service could tap, if it is remembered that European systems of hierarchy, class, or professional organisation had only a limited grounding in New Zealand. Colleges were encouraged to promulgate an academic knowledge base that would accredit, but which would also allow a more specialist form of expertise to be developed. As a direct result of this aspirant professionalism, the University now had a future ally. The Institute was pivotal in lobbying for a two-year full-time Diploma of Public Administration (established in 1940 but suspended in wartime), which included political science, economics and public administration, and was restricted to state servants. Institute activities also included monthly lectures and study circle meetings, and its members were invited by academics at Victoria College to attend public lectures on commerce and economics. At Wellington, given the concentration of State activities within the capital, there were developments of mutual interest to senior public servants and teaching staff of the College.

The inter-war University

During the inter-war period there was some redistribution in College resources and role sets that began to reshape fresh opportunities for academics. Up to the First World War, the academic community was weak relative to the State. It lacked resources, had limited bargaining power, and the public, political and professional perceptions of its role did little to enhance academic or University status. Discussion in the previous chapter highlighted the disadvantaged employment conditions of College teachers, with prodigious teaching schedules restricting opportunities for research activities. Although not all types of

research activity are capital-intensive – introspection and reflection do not require any elaborate equipment – all social research is somewhat labour-intensive in that it requires individuals to plan, conduct and write up investigative activities. During the period there were incremental additions to the numbers of staff employed within particular teaching areas, and in some spheres new teaching activities were introduced (e.g. anthropology from 1919 at Otago University). The staffing of Departments with an additional lecturer or two released individuals from administrative burdens and allowed teaching activities to be shared among colleagues, thereby freeing some time for research. Thus added to academic motive for research were increased opportunities during this period with a change to institutional roles.

Between 1925 and 1945, full-time College teaching staff positions increased from 166 to 249. While senior posts continued to reflect the colonial inheritance, three-quarters of University teachers were home-grown – a situation which potentially encouraged awareness of local issues. Among academic staff there was an increase in higher qualifications, usually involving an element of original research, and this led to raised expectations of scholarship among their holders. Individuals were also returning from overseas with doctorates, and a handful of doctorates were granted through the UNZ.¹⁵ Within the Colleges of the 1920s the role of teaching staff was increasingly perceived to include a research dimension, even if practical circumstances prevented the vision from prospering. As John Beaglehole writes:

From the 1920s academic talk was more and more in terms of research as the twin sister of teaching; more and more it became the habit to ask applicants for university positions what books they had written, what original research they had done.¹⁶

Despite such developments the University remained a generalist institution with high student/teacher ratios¹⁷ and continued to undervalue research.¹⁸ Apart from a few scholarships, the overarching University structure had done little to stimulate research funding, with research denied the necessary funding and, worse still, even actively discouraged.¹⁹ Throughout the 1930s and 1940s, academic voices were being raised against poor research conditions.²⁰ At Canterbury College a number of staff endorsed

Research and the University (1946), which emphasised the research function of the University:

We regard research and teaching not as separate functions of a University teacher, but as complementary parts of a single University.²¹

But the lay Senate retained control of University affairs and remained an impediment to academic development. Despite changes suggested by the 1925 Royal Commission on University Reform and the introduction of a new Council, it continued much like the previous Senate, even reverting to being renamed the Senate in 1929.

College rolls remained reliant on State employees. Of 3,850 students in 1925, 32% were studying full time and 14% were at teaching college. Of this total, 54% were in employment at the same time as pursuing studies, and half of these were employed in a public service capacity. In total, the State supported 41% of all students, as well as providing many bursaries to full-time students. The University and Colleges continued a reliance upon State finance with public funds providing 64% of University income in 1929–30. In England the comparative figure was 46% (including local rates), and 35% if only national funds were considered.²² Thus public employees and public funding remained the mainstay of the inter-war University system.

Despite resources and role sets stacked heavily against individual academics pursuing research, investigative activities did increase as the inter-war years passed. These instrumental activities contributed both to raising academic autonomy, and to re-shaping the view of what academic roles could, and should, be in an emerging Dominion context. Such internal initiatives of the Colleges raised the trading value of research and began to stimulate alternative sources of demand for University services beyond the teaching and credentialling of State employees.

However, there were still considerable countervailing conditions during this period, and academics were unable to revise significantly the balance of teaching and research scholarship demanded by the role. Academics did not reconstitute their relations with the UNZ, nor the terms upon which they traded with the State as its paymaster. Teaching remained the *raison d'être* for academics, and State employees the major recipients of their services. The University ideal remained bonded to the teacher or trainer, rather than

encompassing a broader view of knowledge or expertise. For academics and Colleges to move beyond such indenture required them to produce, and be seen to produce, stocks of expertise which could provide additional leverage and secure wider alliances with other groups who valued such products. This would require shifts in expectations of College teachers, moving beyond instruction to encompass holders of University positions being seen as experts and being involved in forms of knowledge-production.

Neither the Senate nor the Colleges were well placed to provide or generate additional funds during the recession of the inter-war period. In terms of external funds, a major financial injection for education and social science was received in the mid-1930s from the American Carnegie Corporation. Such support, however, did not flow directly into the Colleges, and only donations to libraries were aimed at the Colleges themselves.²³ While many of the Carnegie visitor grants for travel to the United States went to College academics, recipients also included those within the teaching and public service professions. The New Zealand Council for Educational Research (NZCER), established in 1934 outside the Colleges, became a stimulus to research activity and a route through which other applications and projects (e.g. adult education, museums, arts, and rural sociology) were channelled.²⁴ The NZCER undertook a range of projects on vocational guidance, school/work transition, apprenticeship, and rural life, fostering reviews and surveys of policy, institutions and services (Chapter Two provides more detail), offering the opportunity for studies attuned to local conditions.²⁵ At its inception, the NZCER sought to pursue the 'facts' and saw its role as collecting facts scientifically, and making judgements on the basis of them. Its research activities in large part were educational and psychological, but it also funded projects that were more broadly 'social'. Whilst there was fluidity among High Schools, Colleges and the community, and between administrators and teachers during this inter-war period, Colleges were relative outliers to these research developments and most early projects were undertaken by teachers and educational practitioners and not academic staff.

One implication of the NZCER was to further divert interest and resources away from the Colleges. State Departments looked to the newly established NZCER rather than the College system as a site of expertise. For example, the Education Department sought an 'independent' view from the NZCER on the Feilding Community Centre 'experiment',²⁶

similarly, McQueen's study of vocational guidance began as a request from the Minister of Education, as did Beeby's survey of the Intermediate School.²⁷ Thus while in some ways the impact of Carnegie on social science research was a catalyst, during the inter-war period in the Colleges at least it also acted as a brake on research developments.

Shifting Cultural Settings: Science, intervention and conceptual transformation

After the First World War One there was a set of changes in the ideational context of New Zealand society, economy and politics, and these changes allow an understanding of how the claims of social science research were both resisted and advanced. In part, the experience of the Great War helped unleash a range of new ideas, values and beliefs into society. An international phenomenon was the growing interest and hopes held out for science and applied technology, a sentiment echoed in Britain, the United States, Australia, Canada, and South Africa. From within these countries came increased political and public service interest in the role that scientific research could play. In 1916 Britain established its Department for Scientific and Industrial Research, and the British influence was felt when the 1925 Royal Commission (Heath Report) in New Zealand examined the organisation of agricultural science. Heath suggested that research should be kept separate from the executive and administrative function of Government, and in 1926 a DSIR was replicated in the Dominion.²⁸ Applied science became a legitimate part of Government activities and moved from mainly being controlled by 'amateur' bodies such as the New Zealand Institute and local societies. From 1935 the Labour Government encouraged the DSIR to do more research itself, and between 1936 and 1939 a number of important DSIR divisions were established.²⁹ While scientific practice became separated from overt political influence, it focused on applied work closely tied to direct benefits rather than pursuing pure research. Improved funding for natural science did not benefit the University science system and this remained relatively understaffed and continued to be viewed with suspicion.³⁰ Natural science is illustrative of the challenges faced by the University system within New Zealand at this time, and for the University to receive more support it would have to attempt to foster research with practical value. As well as perceptions among the elite, there was the parallel task of raising the public profile of science:

any scheme for Government organization was likely to fall short of the best results unless the national attitude to scientific investigation could be

greatly improved and the schools and colleges, the Workers' Educational Association, the museums and public libraries, the scientific societies, the cinematograph and the Press were indicated as the agencies by which this education of the public might be carried out.³¹

Along with increased support for natural and physical science by the State there was also increased acceptance that areas of health and education should be subject to the edicts of the 'new scientificity'. Although the role of Otago Medical School remained primarily vocational during the inter-war years, a small amount of research did get underway.³² Medicine and psychology fitted into an emerging paradigm of scientific knowledge that allowed practical interventions and facilitated greater control of the physical and human world.

These international and domestic tides presented an opportune context for social science research, for if the human sciences could align themselves with such accepted views, it would provide additional support. In Britain, for example, calls were made to 'establish a new scientific body with adequate endowment from the State, which will do for human social research what the Medical Research Council does for public health...'.³³ Such a view was also behind the establishment in New Zealand of a Social Science Research Bureau during the 1930s.

Psychology manifested a growing presence within the welfare arena where individuals' difficulties were reconceptualised as environmental rather than the result of hereditary factors.³⁴ There was a growing interest in concepts and techniques that promised to address individual and social ills, including psychology, traditionally housed in academic departments of philosophy but emerging as a separate force during the inter-war period. The growing emphasis of psychology on empirical concerns put it increasingly at odds with the tradition of introspection,³⁵ and the influence of experimental psychology from the 1920s and 1930s found support in fields of public health, education, child welfare, vocational guidance services, and studies of industry. Such a shifting theoretical paradigm generated interest in psychological concepts and also encouraged State intervention aimed at ameliorating particular conditions – thus to the will for intervention was added a 'scientific' way of going about it through individuals being identified, diagnosed, cured, and slotted back into society. Underlying this State activity was the belief that problems

were located within individual pathology and that individuals could be reformed and fitted to existing circumstances. Psychology soon became the dominant way of explaining juvenile delinquency, attributing it to socio-economic factors and ‘socio-psychology’³⁶ was also influential in rehabilitation, child welfare, and educational services. Many NZCER studies of the 1930s bore a strong psychological imprint and psychological concepts also began to find their way into lay discourse.³⁷ Technical innovations underway during the inter-war period included the development of statistical sampling techniques and survey approaches to the study of local communities. Interest in empirical inquiries was also encouraged by the study of community life – influenced by an American tradition of immersion and rich description. Somerset’s *Littledene* (1938), an investigation of ‘all the social factors that go towards the making of a typical New Zealand rural community’ was a highly accessible *entrée* into ‘scientific study’ of social life and contributed to the popularisation of research.³⁸

In the post-1918 period, the discipline of economics continued to clarify its techniques and continued with expansion in both University and public life. While not yet occupying the status of master science, its attempts to align with the models and techniques of hard science gave it a number of supporters. As the Professor of Economics at Victoria noted:

The supreme test of the completeness and maturity of science is its ability to serve as a basis of prediction. Economics has not yet reached this stage of development, but is moving in that direction.³⁹

To an established deductive form of economic study was now added the focus upon the collection of empirical evidence, giving – ostensibly – hypotheses with deductive verifications. This was potentially an appealing combination, offering the advocates of economics powerful analytical and conceptual tools. These tools and the simplicity and elegance of their mathematical formulae appealed to State interests, and compared favourably with the non-scientific approaches of other contemporary social sciences.⁴⁰

The study of society in the First World War period consisted of a *mélange* of history, economics and education, with considerable overlap amongst these areas of inquiry. A number of fields (psychology, anthropology, political science, public administration and economics) made significant progress during the inter-war years in clarifying concepts, techniques and topics for study, and it made more sense to talk about discrete social

sciences during the period. But despite neo-classical economics and classical sociology failing to provide the diagnosis of the prevailing economic and social crisis of the 1930s, neither did an emerging social science research provide the fully developed language, body of concepts, and experimental techniques that were necessary for entry into the 'new science'.⁴¹

Many of these ideas and concepts of society and economy found expression within the teaching activities of the UNZ, where a Diploma in Social Science was introduced in 1920. This course lacked empirical focus and had petered out by the mid-1930s. Examiner reports suggest that 'sociological' progress was impeded by the reliance on book learning and an inability to see concepts as having purchase on everyday New Zealand society and polity:

Most candidates were more successful in answering questions dealing with the book work than they were in answering questions that required the application of sociological principles to the social problems of the day. Some competence in this aspect of the work ought to be compulsory if the Diploma in Social Science is to have any real significance.⁴²

A more developed notion of what 'social science' was, and claims as to its practical applicability and contribution, began to drift in from the United States and Britain during the late 1920s. In part this was the consequence of the growth of social science methods and texts finding their way into the international market, and also reflected the development of particular Schools of interest, including those based around Chicago and the London School of Economics.⁴³ Postgraduate students continued to study in Britain during the 1920s and 1930s and it was through them and new staff appointments from Britain that such social science ideas were further reinforced within domestic academic circles. During the 1930s, cross-Atlantic flows and visitor grants to United States also blossomed and visitors included Hunter, Hight, J. Beaglehole, E. Beaglehole, Shelley, Smith, Campbell, Somerset, Wild and Strachan.⁴⁴ An emerging body of 'social' concepts, carried within books and by individuals moving between New Zealand and overseas, became ripe for application. Yet despite this international flow of ideas, it is important to recognise that:

Cultural forces, however international they may be still have to contend with the established structures of the different national systems of education and the vested interests associated with their maintenance.⁴⁵

The Depression of the 1930s not only generated structural consequence but also helped birth a series of innovative ideas that led to possibilities for, and conditioned the emergence of, social science research. Depression shook Governments, State Departments and populations and a political consequence was the election of the Labour Government in 1935. More broadly, the Depression cultivated the belief that the traditional relationship with Britain was of little help in addressing the problems that had to be faced. When George Bernard Shaw visited New Zealand in 1934, he was surprised at people's habit of still referring to England as "Home",⁴⁶ but a change was underway during the inter-war period, in that by then the majority of New Zealanders had been born in New Zealand. This domestication was paralleled in other institutional settings; for example, the Colleges became increasingly staffed by individuals who had some New Zealand qualification. A number of projects and studies emerged around New Zealand identity and making sense of 'who we are'. The 1930s saw the beginnings of a cultural nationalism, a development that coincided with the looming centenary celebrations of 1940. A series of studies marking New Zealand Centenary celebrations illustrate the desire to trace the shape and identity of New Zealand. As one commentator noted:

The overwhelming image implanted in the reader's mind is one of the inexorable tide of progress that had occurred in the Dominion in the preceding one hundred years.⁴⁷

Of the 13 scholarly studies commissioned in 1937 on facets of New Zealand life, only two were from individuals based within the UNZ. Five were from full-time public servants, and three from journalists.⁴⁸ This itself provides evidence that individual College staff were not seen to hold a monopoly on expertise.

The mid-1930s was also a time of optimism and idealism around the moral vision of education and great expectations of what State intervention could achieve in this and other spheres.⁴⁹ Increased idealism and expectations were connected to the hopes held out that scientific techniques would deliver social improvements. This hope, in turn, was connected to the continued erosion of amateurism and the elevated claims of

professionalism and expertise across a number of spheres. MacMillan Brown's calls in the 1920s for professional learning and techniques to administrate Pacific interests, and the focus directed towards more effectively and efficiently administering the Maori population are exemplars of this shift. The attempt to demarcate a sphere of expertise was underpinned by the advocacy of academic credentialling.

Summarising the cultural fund, there was a series of ideas on the rise including specialisation and professionalisation. While faith in the State remained high, its claims were seen as best mediated through a growing cadre of administrators who were adequately equipped with social scientific concepts and understandings. While public views of the University remained relatively lukewarm, the Colleges were beginning to stake claims to expertise and counter notions of the University as an instrumental and degree-granting body.

Interaction and Societal Study by the State

Given that structural and cultural dimensions condition rather than determine, how are such influences interpreted by actors, and how are interactions played out in the next cycle of stasis or change? During the inter-war period, new actors emerged within the polity, public service, health and welfare sectors all with interests in furthering the study of society.

At the core of social science research, as of most other developments, is the notion of interest. This central component of interests is too often missing from explanation, leading to a Whiggish view of social science development.⁵⁰ It is against such a view that the current analysis stands: to reiterate, interests must be at the centre of any account. If the development of natural science within New Zealand is taken as a parallel – while this group of natural scientists had looked to government and legislation for funding since early settlement, only after 1918 did scientists become a socially effective group in seeking funding for fundamental research.⁵¹ Arguably, health and medical interests around this time were also successfully exploiting their position as a socially effective group in order to raise the profile of medical research. The 1920s saw the beginning of State and charitable funding of medical research, and legislative imperatives within the Health Act 1920 obligated the Health Department 'to promote or carry out researches and

investigations in relations to matters concerning the public health, and the prevention or treatment of disease'. The Department of Health's Annual Report (1923) stated 'very plainly that the greatest encouragement should be given to research work'.⁵²

The lessons of health and medical research interests can illuminate the development of social science research. Health had a legitimacy that had not yet accrued to those advancing social science activities, and a growing academic push for health and medical research found support from political authorities, medical interests in professional practice, and also legitimacy within the social body. As Minister of Health, Peter Fraser argued for increased research funding and the full utilisation of money that was allocated, and there was growing encouragement to undertake research from within medicine itself.⁵³ In 1935 the New Zealand branch of the British Medical Association called for increased Government funding for medical research and the establishment of the Medical Research Council in 1937 delineated boundaries around disease, illness and cure, and forms of research practice.⁵⁴

Much of this early work was carried out by the Special Schools of Medicine and Dentistry at Otago University, although during the 1930s practitioners such as H.B. Turbott received funding for projects.⁵⁵ The alliance of support from medical professionals, the University, and the State, meant the development of medical research was not tied to the fortunes of the UNZ, nor to public sector interests. Internal resources, external support, and strong networks, plus the ideational coherence of the notion of science and the investigation of disease and illness contributed to the introduction of health research. J.D. Sinclair argues that the establishment of the MRC was the result of pressure from Hercus (Otago Medical School), Watt (the Director-General of Health), and Malcolm (Otago Physiology Department). He also suggests:

Perhaps not their persuasion but at least their access was helped by the fact that the minister had been best man at the wedding of one of the future research directors.⁵⁶

However, even these smaller network relations must be seen within the broader discursive patterns and relations, and social science research development must be similarly viewed in this way.

The fate of economics allows a closer form of comparison within the social science family itself. Economics had a relatively established base within the University, having separated from history in 1885,⁵⁷ and from political economy by the early 20th century.⁵⁸ As suggested in Chapter Two, economists had been the first group of social scientists to offer their services to the central policy-making apparatus. Four economists gave evidence to the 1912 Royal Commission on the Cost of Living, but only after the 1920s did participation become consolidated and continuous, with a series of Committees reflecting the severity of issues and the expectations of State intervention. During the inter-war years political government came to mean economic government and there was a range of Committees around unemployment, expenditure and trade involved in New Zealand's recovery programme⁵⁹ and on which there was University representation. As Belshaw notes, 'The growth in emphasis on the need for intensive, authoritative enquiry was one of the most significant political phenomena during the depression'.⁶⁰ While this authoritative enquiry could have remained within the domain of the State, two motivations existed for moving economists into Government activity. First, the political desire of politicians to utilise more scientific techniques in addressing problems that appeared to evade their grasp; second, University economists themselves were willing to embrace the public sector opportunities. In relation to the insider status of economics, Fleming notes:

The acceptance of economists as legitimate players in the policy-making process depends on largely on the perceptions of them held by certain social groups.⁶¹

Economics was the exemplar of what other social sciences had to achieve if they were to secure acceptance within the State policy-making apparatus. By the 1930s there was the growing influence of economists and economic theory within Royal Commissions and investigative Commissions.⁶²

Up to 1918 the economics syllabus was extremely wide and encompassed techniques of socio-economic investigation and the role of statistics.⁶³ In the post-1918 University, the discipline of economics grew in strength and re-centred itself on a tradition of marginal analysis of production, consumption, distribution, and exchange. Coverage remained wide, including public as well as theoretical dimensions: theories of the firm and taxation, money and trade, as well as discussion of socialist theories, and State and municipal socialism. Economics occupied an economic and social space, while specific tools of

social analysis were still not fully formed and had yet to find their own particular audiences. Instead, socialised approaches, at best, found expression in existing disciplinary forms rather than through the construction of new disciplines.

By 1938, economists had gained a legitimate role in Government, the Reserve Bank drew on academics, and there was a series of prominent economic commentators writing and contributing to debates during the 1930s. This is a useful contrast to the Social Science Research Bureau that was established within the Department for Scientific and Industrial Research in 1936, and which marked the first attempt to organise Government social research activities. Despite its short life, and spectacular failure if projects are the measure,⁶⁴ the Bureau draws attention to the concept immaturity that existed during this period. It suggests that there was not yet a fully developed understanding, within either the State or the academy, of what a non-economic social research or social science could potentially contribute. The life of the Bureau points to a continued lack of funds for social science, while the decision to establish a unit outside the College environment further illuminates the constricted view of University expertise.

It was intended that that the Bureau should harness the welfare and ethical implications of social science and aim at 'promoting fundamental research (which is independent of political considerations) [and] also make a contribution to practical problems of Government'.⁶⁵ The Bureau was expected to secure funding from learned societies, foundations and local authorities, while avoiding funding from political and semi-political organisations, and any whiff of politicised disputes.⁶⁶ The Bureau illuminates the inter-war desire to use investigation and inquiry to address complex social issues and contribute to improved decision making. While the Bureau's demise is often attributed to political intervention arising from its first report on living standards,⁶⁷ its history is subject to much misunderstanding. A closer reading of SSRB archives suggests a lack of clarity around what the Bureau could achieve, and that it was also deprived of resources. The Bureau did not survive the war (folding in 1940) because survival required the very ideational circumstances that the war facilitated. The institutional and intellectual soil was thin, which prevented the Bureau laying down roots; when priorities became more pressing, it was abandoned.

Academic interests

Academics within the Colleges continued to advocate for what they identified as the proper research role of the University. These calls received some support from national developments, with the 1925 Royal Commission on University Education, for example, advocating an expanded notion of the University, stressing the necessity of a 'research spirit' (p. 77). The potential contribution that the University could make to knowledge, and the importance of breaking down misunderstanding and resentment among the community were also increasingly defended.⁶⁸ So while the University role remained practical rather than intellectual, there were some murmurs of support for it having a new role as purveyor of knowledge in addition to preparing the new servants of State scientificity.

The welfare sector was expanding and consolidating into a number of professional schisms as intervention intensified across policy fields. As a body of ideas, psychology found favour among groups including health workers, teachers, social workers, and youth workers. It emphasised the importance of empirical investigation in raising the standard of interventions. The role and education of welfare professionals after 1918 were increasingly influenced by psychology and the hopes for welfare prevention rather than solely care, cure or containment. Prevention presented greater opportunities for social science investigation and diagnosis of individuals. The popularisation of psychological terminology and concepts within professional domains had implications for College training and research services because it contributed to greater academic legitimacy around a distinct form of social science. Much of psychology's appeal was the result of its perceived immediate practical usage, the interest of the DSIR in Industrial Hygiene, for example, led it to fund Shelly and Beeby to study work processes in 1929 at the Auslebrook factory. Shelly at Canterbury and T.A. Hunter at Victoria both established clinics that offered services for children and vocational adjustment. This practical value meant that alongside State support there were also small amounts of funds forthcoming from private firms.

Compared with economics, some of the newer social sciences including politics and sociology appeared to lack a constituency or champion who valued their concepts and recognised the contribution they could make. Not until public administrative efficiency

was revitalised in the 1940s, or social science was linked to social work and social administrative 'social problems' after 1950 were adequate sponsors obtained. Indeed, it was difficult to find sponsors at this time who endorsed a notion of the 'social' sphere as apart from the individual (psychology), community (anthropology), or body (medical). For non-economic 'social science' there were difficulties in grounding the concepts and making them appear useful for practical analysis. Only in the post-war period of the discovery of social problems and the consolidation of the welfare state was this hurdle to be surmounted.

The relative position and bargaining strength of academic staff began to change during the inter-war period as academic staff increased, and some academic services became increasingly demanded by the State and public services. Internal political shifts took place in the organisation of the academic community, with greater academic representation secured through a Board of Studies. This Board continued to press for reform and facilitated the 1925 Royal Commission on University Education (1925) which explored teaching, examinations, University relations with secondary and technical institutes, research, and whether Colleges should become separate Universities. The Commission recommended a new University Council with advice through an Academic Board consisting of professors. But 'Unfortunately the new Council was very much the old Senate, and the new era in consequence difficult to distinguish from the old'.⁶⁹ Thus the Senate remained a major block to University development by preventing College interests from successfully mounting a challenge to the strictures of UNZ control. Within the University, social science, like many other subject areas, remained disadvantaged by these arrangements.

Colleges retained direct access to the Education Minister, and this situation created problems in advancing national priorities and ensuring political trade-offs remained. The Commission of 1925 recommended that special school finance be kept separate from that of other faculties, and this continued to impede the vast majority of Colleges in their fight for funds. The interests of the academic community were split and previous University developments created interests that were jealously guarded. As well as difficulties created by Colleges lobbying for funding there were also conflicts amongst the Colleges on academic matters: Canterbury blocked Auckland's attempts to introduce an engineering

school, and Otago clung to the medical jewel in its University crown. There were also divisions *within* Colleges: Otago Council in 1924 transferred funds from its arts and general account to meet the needs of medicine, fuelling a broader sense of resentment about the privileged position occupied by special schools.⁷⁰

On social science more specifically, individuals that included Hunter and John and Ernest Beaglehole saw major opportunities in the rising tide of social science.⁷¹ The Professorial Board at AUC argued for full-time social science research posts to be established so that the incumbents could conduct Government investigations, and argued these posts should be under the direction of Heads of University Departments.⁷² But disciplinary battles were also being fought within the fragile social science community, with splits emerging within Colleges amongst academic disciplines. Thus was social science opposed at Auckland College,⁷³ and apparent rivalry continued between Oxbridge-educated teachers and 'the home-grown article'.⁷⁴ For social science to gain a foothold it needed to gain ideational and material support whilst also fending off resistance from established professions (science and medicine), and internal opposition from existing academics and the system of governance.

The 1938 Committee on University Graduates and the Public Services, which arose from academic and University and student pressure to permit more direct graduate recruitment into the public service, had only equivocal public service support during this time. Public Service Commissioners expressed concern over the academic inflation of recruitment, given that between 1928 and 1938, the number of staff with specialist degree and diplomas had risen from 850 to 1,917.⁷⁵ Public servants who were organising the NZIPA as a professional grouping to legitimise its own position and pursue 'the science of administration', trod a fine line in not wishing to equate expertise with academicism and thereby undermine their own positions. There was some criticism within the public service of the establishment of the Public Administration Diploma,⁷⁶ illustrating how the 'academic' had to be carefully combined with the practical to maintain the support of senior public servants of the time.

Within the University, examinations continued to dominate and this exerted an influence on the research possibilities because intellectual discovery and the challenge to canons

conflicts with a restrictive syllabus that is marked overseas. For example, while experimental psychology was part of the University curriculum even in 1904, and industrial psychology had been introduced in the syllabus at Victoria since the 1920s, not until 1949 was it compulsory to take laboratory work as part of a course.⁷⁷ As James Driver, psychology examiner during the early inter-war period, wrote:

The chief point demanding notice is, as in previous years, the fewness of candidates taking experimental questions... psychology without due attention to the experimental side is an anachronism today.⁷⁸

The lack of scale within the New Zealand society created some difficulties in providing training and thwarted some empirical developments – including investigation into occupational safety and health in the early 1920s.⁷⁹

Summary

As a result of these interactions, the picture by 1945 was one of relatively piecemeal research developments but not a broader acceptance of such developments within public service, UNZ, or society-centred, activities. Knowledge was viewed in a highly instrumental manner, and many latent disciplines were unable to attract the support necessary to enable them to stake out intellectual territories more fully. Despite developments within psychology, and the continued growth of economics, the Colleges were only bit players in the development of social science. Social science still faced a number of incompatibilities: for research to develop certain things must exist, including a sponsor (either State or social groups) with interests in collecting empirical material of social and policy ‘encounters’. In the inter-war years, health, education, psychology, and economics all began to find patrons for their activities and to advance their positions and interests. They carved away spheres of inquiry leaving a residue for what was later to be staked by social anthropology, social administration, public administration and sociology. Whether further disciplines could develop would depend on how the object of inquiry was reconstituted, and whether support could be courted from broader social interests. While the experience of the Social Science Research Bureau reflects the increasing recognition that a non-economic investigative space existed, social science research required the stability of the welfare state and full support of new professionals that were to emerge in the post-1945 expansion.

In summary, during this period changes occurred in State demand for research services, and professionals began to see benefits from social science. Meanwhile, health and education interests were exerting their strength and pursuing their own research opportunities. But despite growing ideational support for the idea of social science research, there was a lack of research training, compounded by further colonial residues. The period of war (1939–45) was to be one of magnification, and groups were to emerge with a shared interest in demarcating a ‘social’ and policy space, and developing resources and organisational structures towards its investigation.

Chapter Eleven Notes

¹ By 1936, 68% of the New Zealand population was urban – although the figure for Maori was much lower – 17% (Levett, 1988).

² Glazer (1973, p. 67).

³ Polaschek (1958, p. 117).

⁴ Stephens (1942).

⁵ Mawson (1938).

⁶ Beaglehole, E. (1937); Lang (1951); Polaschek (1958).

⁷ Turner (1938).

⁸ Mark and Boyes (1938, p. 4).

⁹ Woodward (1938); Scott (1942).

¹⁰ Henderson (1990, p. 131).

¹¹ Henderson (1990, p. 150).

¹² F.B. Stephen in letter dated 13 June 1938 accompanying New Zealand Institute of Public Administration's response to a questionnaire issued by the Institutes' International body (NZIPA Papers, Alexander Turnbull Collection)(see also Webb, 1940).

¹³ *Report on the activities of the NZ Public Administration Society (Wellington) by 14th November 1934* (NZIPA Papers, Alexander Turnbull Collection).

¹⁴ Report of Executive Committee to first Annual Meeting of NZ Public Administration Society, 23 May 1935 (NZIPA Papers, Alexander Turnbull Collection).

¹⁵ This included Doctorates within social science conceived as epidemiology, education, anthropology: H. Turbott *Tuberculosis in the Maori: East Coast New Zealand* (1935); A.G. Butcher's (1933) *Education*; Pearl Beaglehole (1937) *The Impact of Culture: A study in ethnology*; H.D. Skinner (1938) *Culture of the Maoris of the Chatham Islands*. Records suggest two other researchers submitted doctorates but were unsuccessful: William Doig *Standards of Life of New Zealand Dairy Farms* (1945); H.G. McQueen (1948) *Vocations for Maori Youth* (see Doctorate Thesis Register 1933–58 University of New Zealand, held at National Archives, Wellington).

¹⁶ J. Beaglehole (1949, p. 248).

¹⁷ Staff/student ratio to full-time staff by 1948 (UNZ 10, Item 1184, National Archives):

Auckland University College	1:31
Victoria University College	1:33
Canterbury University College	
Arts/Science	1:27
Special School	1:15
University of Otago	
Arts/Science	1:26
Special School	1:14
Sheffield	1:13

There was less criticism of special/professional schools' teaching quality and research outputs than the other UNZ Departments (Morrell, 1969).

¹⁸ Allen et al. (1946); Wood (1946); Beaglehole, J. (1949).

¹⁹ Beardsley (1973); Gardner (1973); Popper (1976b).

²⁰ Arnold Wall (*The Press*, 8 April 1929); James Hight (*Christchurch Press*, 22 May 1935). Popper's recollections of his time at Canterbury from the later 1930s also reflect the sentiment: 'I had a desperately heavy teaching load and the University authorities not only were unhelpful, but tried actively to make difficulties for me. I was told that I should be well advised not to publish anything while in New Zealand, and that any time spent on research was a theft from the working time as lecturer for which I was being paid (1976b, p. 119).

²¹ Allen et al. (1946).

²² Parton (1979).

²³ Rochester (1990) notes that Carnegie supported libraries at Auckland (US \$25,000 for 1933–49) and Victoria (US \$25,000). Donations were withheld from Otago and Canterbury because conditions upon which they were to be granted had not been met.

²⁴ Alcorn (1992).

²⁵ NZCER (1935, p. 8).

²⁶ Campbell (1945).

²⁷ McQueen (1940, p. v); Beeby (1938).

²⁸ Galbreath (1998).

²⁹ Cawthorn Institute and Dairy Research established 1920 and 1927 respectively, and then 1936–9 ten further institutes including those dealing with crop, grass, plant, soil, wool, physics and engineering.

³⁰ See articles by Thomson and Thomson (1923) 'Scientific Research in New Zealand: Report of the Activities of the Various Organisations, Government Departments and private individuals conducting the same', and Thomson (1925) 'Scientific Research in New Zealand: Report on the Activities of the Various Government Organizations, Government Departments, and private individuals conducting research since 1st June 1923'. The Department of Internal Affairs gave £2,250 to the New Zealand Institute for research work (AJHR H-22, 1919, p. 14).

³¹ 'Organization of Scientific and Industrial Research' (AJHR H-47, 1917, p. 6).

³² Parry (1975).

³³ 'Organisation of Social Research', *Nature* (1937, p. 521).

³⁴ Dalley (1998, p. 98).

³⁵ Carter (1993, p. 124).

³⁶ Dalley (1998, p. 114).

³⁷ 'Freud had just burst upon the layman's world, and it was at last possible to talk about sex in public...' (Beeby, 1979, p. 1).

³⁸ Somerset (1938). By 1961 *Littledene* had sold 2,309 copies, more than any other NZCER publication (UNZ 10 H-4, NZCER Vol II, National Archives).

³⁹ Murphy (1935, p. 8).

⁴⁰ Crozier (2002).

⁴¹ Or as Wagner (1989) notes, 'the scientific study of social conditions beyond being socially necessary must also be cognitively possible (p. 512).

⁴² Report of Examiners (1928), *Outlines of Sociology*, T.A.Hunter, p. 44. Similar comments were frequently made about political science (UGC, item 474, National Archives).

⁴³ Lundberg (1945); Hart (1949).

⁴⁴ Beeby (1988).

⁴⁵ Archer (1979, p. 790).

⁴⁶ *The Dominion* (6 March, 1934).

⁴⁷ Booker (1983, p. 14).

⁴⁸ Of the 1937 Committee, seven were from the UNZ and six from the Public Service (Booker, 1983).

⁴⁹ See, for example, Campbell (1938).

⁵⁰ See, for example, Watson (2003) for an illustration of such an approach.

⁵¹ Hoare (1976).

⁵² In Dow (1995).

⁵³ In the year up to July 1926 only £1,982 of a £5,000 allocation was spent.

⁵⁴ Morrell (1969).

⁵⁵ Turbott (circa 1984), *The Long Locum*, Audio recording.

⁵⁶ J.D. Sinclair (1973, p. 1).

⁵⁷ See Minutes of Proceedings of Senate, Wednesday 11 March 1885 (UGC, Box 461, National Archives).

⁵⁸ Gardner (1973).

⁵⁹ Committee on Unemployment, 1928; National Expenditure Commission, 1932; Economics Committee, 1932; Tariff Commission, 1933; Commission of the Dairying Industry, 1934; Commission on Company Promotion, 1934.

⁶⁰ Belshaw (1936, p. 33).

⁶¹ Fleming (1989, p. 2).

⁶² Endres (1987); Endres and Fleming (1994).

⁶³ Examples of exam questions: "Discuss the methods of social investigation employed by either Mr C. Booth, or Mr B.S. Rowntree, or by Mr Sidney Webb". There were also questions about the use of family

budgets, the use of the census, and methodological issues about economics' possible experimental approach, and how its laws compared with those of the physical sciences (UGC, items 449–455, National Archives). For a discussion within the Australian setting see Crozier (2002).

⁶⁴ One survey was published, with data collected through interviews; a fieldwork report; and a household budget filled in by households (Doig, 1940).

⁶⁵ D.O. William's report (SSRB, Box 17, National Archives).

⁶⁶ Souter and Belshaw (2 February 1938) (SSRB, Box 9, National Archives).

⁶⁷ Cf. Shields (1974); MacKay, (1975).

⁶⁸ SPIKE (1937) 'Town and Gown', pp. 5–7.

⁶⁹ Parton (1979, p. 58).

⁷⁰ Parry (1975, p. 10); Parton (1979, p. 48).

⁷¹ Beaglehole, J.C. (1938); Beaglehole, E. (1944).

⁷² Brown (1973).

⁷³ Sinclair (1983, p. 201).

⁷⁴ Wall (1965, p. 84).

⁷⁵ Brown (1973).

⁷⁶ Barrowman (1999, p. 387, footnote 34).

⁷⁷ Brown and Fuchs (1979); Barrowman (1999, p. 388, footnote 55).

⁷⁸ Reports of Examiners, 1923, p. 9 (UGC, item 474, National Archives).

⁷⁹ Martin (1996, p. 151).

figure 11.1

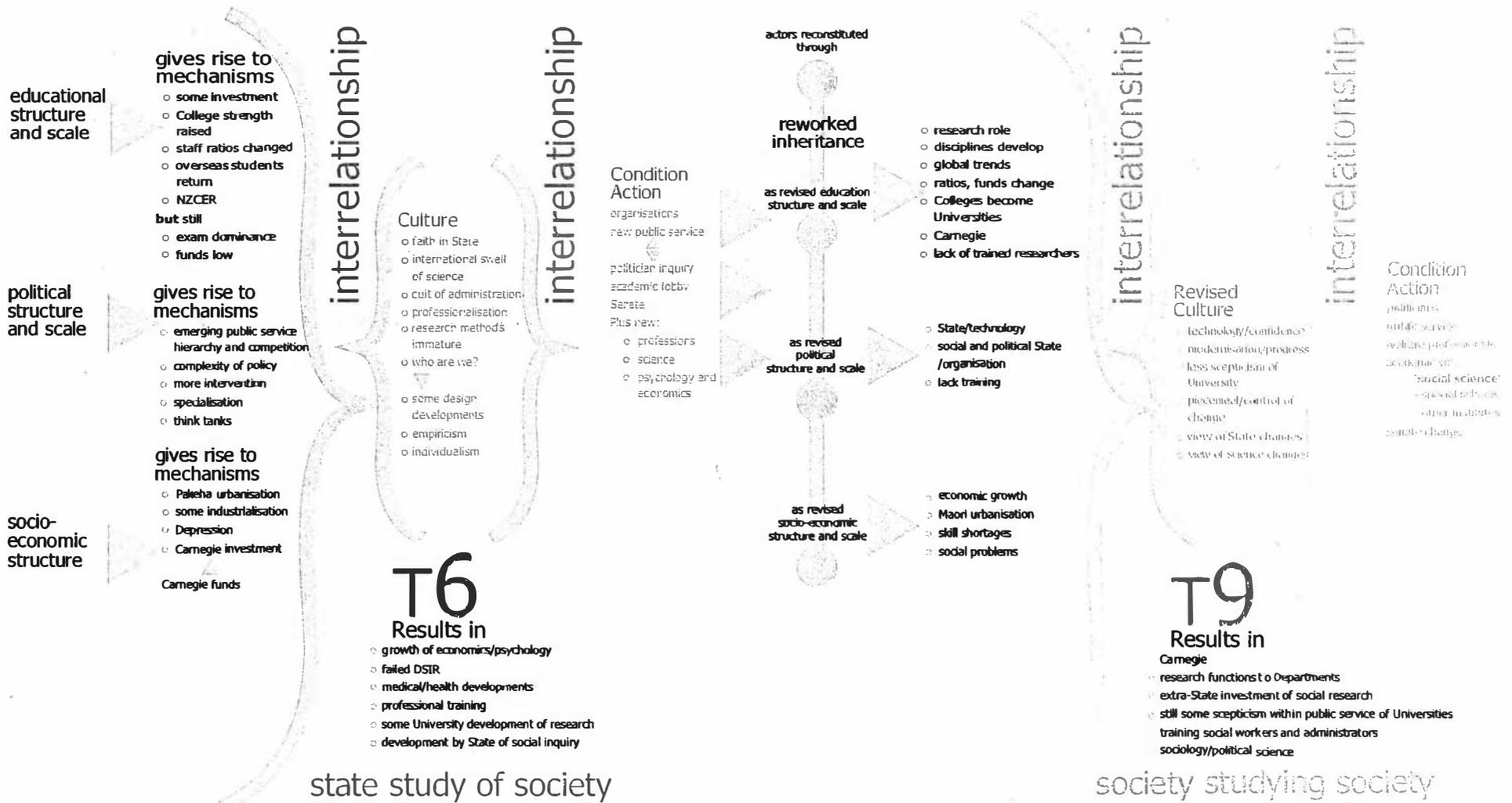
From In Search of Independent Means (Inter-War) Alternative Benefactors (Post-War)

T4

T5

T7

T8



Chapter Twelve

Alternative Benefactors

Introduction

This chapter highlights how social science research gained traction in the post-1945 era. During the 1930s a series of revised structural and cultural contexts provided opportunities for the elaboration of social science activity. But while piecemeal developments occurred during this time, structural and ideational conditions continued to militate against a fuller social science research during this period. Structural inhibitors included the numbers of College staff, the expectations of their academic role, and the role of Government agencies and professionals. Previous chapters pointed to how the governance of the UNZ and its constituent Colleges acted as a brake on development. Approaches to making social and economic policy in the pre-1945 period also placed limits on the potential contribution of social science. The war experience of 1939–45 gave rise to a number of structural and cultural changes that weakened these negative feedback loops that had impeded social science research interests. New structural changes that arose after the War included greater autonomy for Colleges/Universities in the subjects they taught, and as a result of changing expectations of the University role. Main actors were also reorganising during this period, and while ‘names’ remain the same, their influence differed significantly from the influence they had exerted in previous periods. During the 1950s, new sponsors emerged for social science research, including politicians, public servants, and professionals within the growing welfare bureaucracy. Funds were also secured from the Carnegie Corporation, thus providing some measure of alternative benefaction and increased visibility and respectability for social science research. In the cultural sphere, language, concepts and techniques for social science research enabled an expanded range of possibilities during this period.

The 1950s and 1960s is a period of social science research development conditioned by a set of complementary structural and cultural dimensions. This was a halcyon period for social science – a ‘reform coalition’¹ of political, bureaucratic and academic interests galvanised in support of social science research expansion. The belief, held by diverse

institutional interests, was that social science could satisfy the multiple hopes that were held of it. Social science proceeded to gain a sturdier organisational footing during this period; activity increased and expectations soared.

This chapter accounts more fully for the structural and cultural changes that helped condition the responses of the main players. The chapter highlights the range of interests, and alternative domestic (and international) benefactors who were willing to invest in social science – financially, politically and intellectually – during this period. In line with previous methodological discussions, the conditioning cycle from an earlier period also exerts its causal pull, and the residues of the past are re-enacted with the contemporary moment via the availability of resources and role-sets and through an enduring culture of pragmatism. In brief, the ongoing spatial and temporal consequences of colonisation continued to shape the development of social science research in New Zealand.

Re-structuring Economy, Society, Polity

The Second World War intensified State intervention in the economy and society. Increased wartime economic regulation, social controls and operational planning were seen as crucial to the eventual victory.² War contributed to the major reorganisation of Government functions, and as the peace dawned, Keynesian demand management was to accompany the consolidation of the welfare state.³ Some Government Departments established in the aftermath of the war showed an early willingness to invest in statistical and social research functions to allow them to win the peace.⁴ The success of agricultural-led exports meant economic security, leading to a blossoming consumer society and enlarged welfare space as the dividends of prosperity. No longer were social services viewed as a residual safety net but were seen instead as compatible with, and an encouragement to, economic growth and development.⁵ While Britain had its Butskellite consensus and the United States reached ‘the end of ideology’, New Zealand basked in the image of the ‘half-gallon, quarter-acre, pavlova paradise’.⁶ A new epoch was projected, one focused not on material want but on addressing ‘behavioural problems’ focused on child welfare, probation, and integration of Maori.⁷ The role of the State was reorientated into that of manager rather than an architect. In political terms, the choice was selecting the party best equipped to manage the economy and, apart from housing, consensus reigned around welfare with an accompanying narrowing of ideological debate.

Cracks in this post-war hegemony were apparent as early as the 1950s with the (re)emergence of complex social problems and the growth, for example, of discretionary benefits in the form of supplementary assistance and the Needy Families Scheme.⁸ One commentator on family policy refers to the period 1948–72 as ‘recording the history of social breakdown’.⁹ There was a rapid growth of welfare professionals: by the mid-1950s there were 125 Child Welfare Officers, 25 Probation Officers, and 53 Maori Welfare Officers in post, and increased emphasis was being placed on their training. This growth helped cement the practice of the intimate welfare-encounter, and aspirant welfare professions entered the debate over resources.

Whilst hardship and poverty continued to bubble beneath the policy agenda, a range of further social welfare issues portrayed as ‘social problems’ spilled over during the 1950s. The ageing population faced evils of poverty, poor housing and loneliness; elsewhere juvenile delinquency was seen as an emerging social ill; illegitimacy was on the rise, and the Maori ‘problem’ was constructed in fields of education, employment, income, housing, health and justice.¹⁰ The rhetoric of ‘social problems’ was linked to individualised welfare interventions, offering a deficit-driven approach of correction and resolution. While the pillars of the welfare state – education, health, family benefits, superannuation and full employment – were to withstand substantial erosion and fundamental re-examination for another two decades, the plethora of ‘social problems’ invited a more incremental tinkering and fine-tuning. One view of the complacency of the period suggests:

Historians looking back upon the period from 1945 to 1970 will find little evidence of any dynamic political intelligence at work... There was after all little need. Full employment, a comfortable standard of living, an enviable lack of social conflict in comparison with other societies persuaded the majority of us that we were on the right track.¹¹

It is this discursive articulation of ‘social problems’ and their being situated within a broader narrative of ‘progress’ that fostered opportunities for social science research. The question of how ‘social problems’ were best addressed was increasingly met by answers containing a kernel of social science concepts and techniques.

During the 1950s, New Zealand social fabric was being further stretched by ongoing urbanisation that now encompassed the Maori population as it moved from tribal homelands to cities in search of work.¹² Within this expanding urban space, 'social problems' were increasingly discovered (or uncovered) and propelled into the public gaze. This visibility was magnified by a series of vociferous press campaigns around issues of old age, delinquency and, later, poverty.¹³ Close at hand for politicians and public servants were the intellectual resources of social science, social research, social administration, and social work that promised immediate benefits in helping to reverse this incipient social malaise.

The aspirations for utilisation of social science knowledge within policy making gathered momentum during the 1950s, due in part to the growing scale of intervention and the perceived complexity of the policy scene. As the 1962 Royal Commission on the State Services suggested:

In days gone by, a well-educated politician with some leisure to study political and social problems could become as competent as most of his advisers. Nowadays, no matter how intelligent and able a Minister may be, he no longer has the time, the training, and the expertise, to master all the subjects likely to fall within his responsibility. He must depend on expert advice.¹⁴

There was growing interest in the contribution of expertise, and political endorsement of more technocratic and scientific approaches to decision making.¹⁵ A constituent of this was the belief that instinctive and pragmatic decision making must be relegated, or in the very least accompanied, by collection of data and greater attempts to understand root causes. Thus:

I do not think this country can afford to continue in ignorance of the causes of crime. There must be exact knowledge enabling appropriate action to be taken and I have decided that a research unit be established in the Department of Justice.¹⁶

Although the public service did not increase in size relative to the workforce after 1949, it did foster more specialist positions, including the development of economists and analysts within the ranks of State Departments. The range of economic techniques increased as

Cost-Benefit Analysis, output budgeting and model building found support in the heady expectations laid upon economics. The desire was for more accurate and timely forecasting – particularly as the demand management of the economy became more trying from the 1960s onwards.¹⁷ A new policy emphasis on the management of the economy and society meant the provision of strategic advice now accompanied a more routine administration, and with economic downturn there was also growing awareness of the importance of efficiency and effectiveness. Both the expectation that research had a part to play in social and economic decisions, and the requirement that senior administrators be able to interpret such information, presented opportunities for an expanded social science and within this a major University role and contribution.

The changing context helped reshape the role and interests of senior public servants. Accompanying the ascendancy of expertise and provision of information was the expectations that senior administrators would play a key role in evaluating expert views, and relate this information to the decision-making context. However, in the post-war era the public service was ill-equipped to meet the demands being placed upon it. A difficulty was perceived to exist around the number and standard of graduates entering the service in non-specialist roles. The colonial inheritance had underwritten a public service that eschewed an elite administrative division, and led to an emphasis upon coordination and administration rather than policy formation, as occurred in Britain.¹⁸ In 1950s New Zealand, views were expressed that ‘the prevailing belief in equality has hampered the introduction of classification and promotion policies which would give a marked preference to university graduates...’¹⁹ A better prepared public service was seen as a precursor to improved policy making, and within such preparation the social and policy sciences and the University were expected to play a key role.

Alongside policy administration and policy making, support for social science research also emerged from those interests involved in the delivery of welfare. The identification of a suite of ‘social problems’ saw the expansion of professions and specialists across health, education, justice, children and family services. It provided impetus to training and preparing these professionals, including basic theoretical preparation and more practical techniques of intervention. Professor Marsh at Victoria identified that welfare officers had no previous training in social research and statistics, and that a similar gap existed in

preparing the public sector. In addition to including social research techniques in the training of both sets of professionals, his broader recommendation was for increased social research activity to be taken into account when considering legislation and intervention:

there is insufficient use made of methods of social research for purposes of checking up; social service departments ought continually to be carrying out research into the problems with which they are dealing and into the methods of dealing with them.²⁰

From College to University

There was during this period an increased educational autonomy resulting from the accumulation of resources and expansion of College roles, along with the broader appeal of University education and training to non-State interests. A growth in confidence cut across a number of institutions during the 1950s and 1960s as the activities of academics gained increasing legitimation and status. Within the University system there was an increase in student numbers which, unlike the expansion of the 1920s, was matched by rising staff numbers. Despite difficulties in staff recruitment during the 1960s, there was a sharp increase in staff employed at New Zealand Universities. At Victoria University, for example, staff numbers grew from 78 in 1948, to 400 by the early 1970s. Between 1945 and 1948, student numbers increased by 46%, then remained constant until 1956, when a rapid increase occurred. Overall, student numbers doubled between 1945 and 1960. With the rise in student numbers and staff appointments came the opportunity for specialisation and disciplinary demarcation. Organisationally, there was an increased number of departments and development of new curricula, along with the accompanying paraphernalia of academic life – conferences, books and journals. A new dynamic of disciplinary tribes and territories was emerging. The University system was also raising its credibility *vis-à-vis* rivals; for example, in 1954 the DSIR reported the loss of 11 of its professional officers to professorial chairs.²¹ At Victoria University, the range of courses doubled in the 1960s, ‘partly it was because of the universities’ newly acquired autonomy, partly because of the expansion and diversification of the business of knowledge’.²² As an illustration, the study of Maori for example, from initially being located within departments of economics, history and philosophy, now became organised in divisions of anthropology, sociology, linguistics, and physical anthropology.²³ This disciplinary matrix was to bring its own distinct dynamic to the development of social science research.

Opportunities for disciplinary multiplication also faced some structural inhibitors in this period, including the centralised UNZ placing restrictions on the development of subjects, and the horse-trading among Colleges around the establishment of specific Departments.

Prior to the Second World War, the Senate had been an impediment to reforming the University system. After the War the Senate became more sympathetic to a reformed University system, recognising that voting for its own dissolution would improve the longer-term health of the New Zealand University system. In April 1955 the Senate approved powers of delegation to the governing bodies of six Colleges within the University and to its Curriculum Committee under the New Zealand University Amendment Act. This was a significant break in the relationship of College and University, and individual Colleges set about diversifying courses with the approval of this Curriculum Committee.²⁴ This flurry of activity added up to further improvements in autonomy for Universities, as they were soon to be known. They were now able not only to examine what they taught, but also to decide what they taught. This fostered a greater status for individual Colleges/ Universities and contributed to increased recognition of the importance of University self-governance. Furthermore, there was a greater sense of professional academic identity, and in 1945 a University conference adopted a scale of general salaries and a system of classifying academic staff, all of which contributed to more cohesive academic organisation.

The 1959 Hughes Parry Report on the University system was the death-knell for the UNZ²⁵. The Report identified the shortage of graduates for professions, industry, commerce and public service, stressing the need to improve productivity and raise the numbers of the professionally qualified workforce. Thus the case for University reform became connected to a series of other potential sponsors, including professionals, business and commerce. The Report criticised the predominance of part-time students and the understaffing that existed. The most crucial leverage exerted by the Report was to equate reform with national growth and prosperity, whereas previously reform was synonymous with University or academic interests. The resulting University Act of 1961 cemented the new context of greater academic autonomy, and helped unleash the development of the academic system.

State support of the University system remained high in the post-war period, rising from 56% of total income, to 89% of University income by 1960. The financial position of the UNZ was stronger in the immediate post-war period than before this time, and while total student numbers were unchanged between 1948 and 1957, the government grant increased from £44,000 to £1.59 million. A large boost to social science came in the 1950s from the injection of Carnegie resources in a \$60,000 fund for 1953–8 distributed via a UNZ Committee.²⁶ Over twenty projects were soon underway, including some studying Maori communities, and psychological studies in interpersonal and social problems, stratification, adolescents, and older people. Victoria and Auckland Colleges received the highest proportion of funding (see Chapter Two). In addition to such international benefaction for social sciences, the University during the 1950 and 1960s was also increasingly part of a growing web of international relations and networks. These included trans-Pacific Fulbright flows, Harkness and Nuffield Fellowships. Many visitors from the United States published work around New Zealand society, with studies of Maori being of particular interest.²⁷ Trans-Pacific flows were considerable with 1,654 Graduate and Senior exchanges between 1948 and 1988. Of those, two-thirds returned to their previous-held post.²⁸ The New Zealand scene was presented as a vibrant and fruitful one for social science, offering opportunities for others to learn from the New Zealand experience of race relations and small-state democracy. While visitors to New Zealand was certainly nothing new (there had been regular visitors from the 1890s and particularly during the 1930s), this wave of ‘academic’ visitors gave impetus to, and legitimated, the social scientific study of New Zealand affairs. Alongside this American influence, flows to and from Britain also continued, and Britain remained the primary source of overseas recruited staff.

Re-conceptualised Cultural Considerations

The post-war period saw new ideas and beliefs emerging, both nationally and internationally, which were to become important conditioning influences on the shape of social science. A series of public and political discourses allowed social issues and problems to be discussed in a way that was favourable to social science research gaining support. In the 1950s the notion of ‘specialist’ or ‘expert’ was becoming a more embedded discursive frame. If the post-war period did signal the end of ideology and the beginnings of a new consensus, to whom or what could politicians and the public turn for policy direction except to the newly emerging technocratic elites and professionals? Such elites,

in turn, looked to the growing body of social and political ideas that were in circulation. The pull of expertise was strong, and was presented as being synonymous with modernity and progress, and this was enough to counter earlier pragmatic traditions.

In the aftermath of war, science and technology were elevated to new heights. The confidence that emerged around technique and control of the physical and natural world flowed into the social and economic domains. Professionals within the political and welfare bureaucracies became the vanguard and carriers of this new knowledge. The period of growth and prosperity begat a view of economic and social destiny, best expressed in the tide of modernisation theory that swept the globe, and whose reach extended to New Zealand. It was anticipated that piecemeal engineering and fine-tuning would keep countries on their developmental track. Such technocratic conceptions found support in approaches to policy development and organisational forms, and social science research received a major dividend from these shifts. To doubt the power of social science was to doubt progress itself, and to fly in the face of economic and welfare destiny.

Fine-tuning was seen as being facilitated by the social sciences (including economics), which were viewed as a set of practical techniques that generated useful policy knowledge. On the heels of economic prosperity would follow social welfare success, aided by the newly emerging sciences of policy and society. A body of developing 'social research' ideas was painted as coherent, rational, value-free, and technically precise. Developments in statistical sampling and validity presented social inquiry as an objective and scientific body of knowledge.²⁹ The architects of this methodological *Zeitgeist* (including Guttman, Stouffer and Lazarsfeld) envisaged far-reaching implications for the social science curriculum:

The history of social thought will be relegated approximately to the position that the history of chemistry, physics, and biology occupies in the training of students in these fields. That is, advanced students will be expected to be familiar with the main outlines *in the light of contemporary knowledge*, in which they will first have a firm grounding.³⁰

This view of social science as the search for objective facts, when allied with a social administrative ameliorative tradition, had broad appeal. These new social and policy sciences, with their bracketing of ideology and values, promised to turn the debate into one

focused around the most efficient means. Multiple audiences bought into this Lindblomian-type vision, with its aversion to blueprint planning, and which was coherent and simple and underpinned by powerful concepts of 'facts' and techniques such as the social survey. Within these developments, disciplinary specialisation of psychology, anthropology, political science, sociology, and social administration all found ideational support.

Interaction as Extra-State Investigation of the Social

While social roles, resource distribution, and cultural properties continued to structure the research interests of groups, it is how these were played out through interaction that determined the final outcome. National and international structural and cultural influences are mediated through national systems, groups, and their interests. The changes discussed previously provided conditions that were supportive of social science research, but central was that the Senate/ UNZ was willing to embrace change and to take advantage of opportunities. Within the UNZ there was a consolidation of governance and academic interests that opened a route to reform. There was now a belief that a new range of circumstances faced the UNZ and that the institution itself had to be changed. As one retiring Senate member reflected on 25 years of contact with the University:

We cannot in the end overwork, under-pay, and expect idealism to flourish but we can in the meantime, keep alive the idea in the hope that in the end, Governments will see that research is just as necessary to our well-being in peace as in war.³¹

Chancellor Justice David Smith took a pro-active role in espousing a broader vision for the University in the post-war period. His annual lectures captured the challenges facing the UNZ role, organisation and finance.³² As Parton notes of the 'reforming Chancellor', the annual address Smith made to the Senate:

Present[ed] problems and possible solutions to the mainly lay body which dominated the University scene. Reform was no longer a field occupied solely by frustrated academics. They now had allies in high places.³³

Smith sowed the seeds for the eventual demise of the Senate. In cultivating a broader vision for the New Zealand University system, the Chancellor took a particular interest in the scale and funding of research activity. This echoed discontent that had long existed within Colleges, but was previously smothered at the Senate level. Smith seized upon

what he believed was a central place for research in University life, and conducted a survey of research activity:

... the public, the Senate, the Academic Board and the governing bodies of the constituent Colleges must all be convinced that research should be a vital element in the life of each constituent College.³⁴

The Chancellor's annual address was sent to Members of Parliament, heads of State Departments, judges, editors, and leaders of organisations, and this contributed to a growing sense of the University issue being a *national* issue, rather than a selfish concern of insular academics.

Academics themselves continued to agitate for changes. Following the war, the Academic Board called for greater freedom, a research committee, and research funding, and these were approved by the Senate of November 1946. Sources of research funding came from both a Government grant to College Councils and a newly established £10,000 Research Fund, administered by a Senate-appointed Research Committee. Administration of research thus became a core role of the Senate, and with a sub-committee to oversee the funding and progress of projects, the profile and legitimacy of research was raised, not least within the Senate itself. Despite changes in research funding all was not well within the Colleges and among academic staff.³⁵ The Academic Board continued to call for broader change, 'the time has come when serious consideration should be given to the establishment of four separate Universities in the Dominion in place of the existing system'.³⁶ In the time between the end of the war and the Hughes Parry Report of 1959, the University had gone some way towards expanding its role, assisted by increased appointment, external resources, internal funds, and a broader legitimacy and support among sections of New Zealand professionals. Important restrictions on University growth still existed, however, in the 1950s, including the fact that the main educational emphasis was upon primary and (from 1955) secondary education. With the 1959 Royal Commission calling for more graduates, a predominantly full-time student population, greater emphasis upon science and technology, increased salaries, and more research, the University system was finally overhauled. The 1960s saw major expansion of University activities³⁷ and the unleashing of the social sciences.

Developments within the public service and welfare services offered greater opportunities for social science research to gain a footing during the 1950s. There were criticisms of the ways in which policy was developed and delivered, and leading academics pointed to the lack of research around public administration, with any research that was undertaken being descriptive and wide-ranging instead of being analytical and focused.³⁸ Despite the School of Public Administration at Victoria being advanced as the place from which to conduct research within public administration, the public service was unwilling to concede completely the role of research and training to the Colleges. A Course in Executive Management offered by the Institutes of Public Administration and Management, taught by university teachers and public servants, was available in 1952. In 1952 the Institute of Public Administration inaugurated a 'Professional Examination in Public Administration' aimed at employees of central and local government, and this was pitched between the senior examination course and Victoria's Diploma. While the examination had little success, its very establishment suggests that the University was not yet seen as the sole provider of social science training. Similarly, during the mid-1960s the Institute of Public Administration continued to explore what its potential research role could be. Pockets of scepticism still existed about the University being synonymous with expertise and capable of meeting the demands placed upon it. As Campbell, for example, noted:

In New Zealand very many of the people best qualified and experienced to speak about economic and social problems are in Government service and few of them are outside.³⁹

Despite such interests, the strategic use of social science research within policy making and calls to improve the preparation of public servants were main routes that led back to the University.

Emerging social science interests

A two-year Diploma of Social Science, taught from within Victoria's School of Social Science from 1950 onwards, offered social work training and drew large number of its students from Child Welfare, Vocational Guidance, Justice, and Maori Affairs. The Diploma included a stage-one course in Contemporary Social Problems, and stage-two courses in Contemporary Social Problems, Statistics and Methods of Social Research, and Social Administration. Between 1950 and 1954 there were 64 enrolments, 48 from the State and 16 private employees. State employees were bonded for a five-year period,⁴⁰

thus ensuring a cadre of senior staff with both interests in and knowledge of social science research techniques. Throughout the period, there was continued growth of social welfare professionals, and the 1960s saw the establishment of a social work professional body, a journal and regular meetings.

As well as providing the first academic course of social work education, the School of Social Science established an academic social administrative tradition.⁴¹ The social work course negotiated the path between a social reform agenda and more individualised, therapeutic interventions. In its early years the School was dominated by a staff that was largely British educated: Marsh, Minn, Robb, Spinley, Robertson,⁴² and this reinforced the social welfare tradition of inquiry. To counter accusations of 'trade training' the School sought to win academic acceptance through research and publication activities.⁴³ Staff within the School embarked on a research programme that included surveys of 'aged' in regional centres (Wellington, 1951; Auckland, 1952; Christchurch, 1953; Dunedin, 1954; Taranaki, 1957); foster homes used by Child Welfare Division; Porirua Mental Hospital; and hostel accommodation for adolescent girls. Increased Government intervention provided a growing range of policy spheres, institutions and 'problems' upon which social research could focus inquiry. Within the School at Victoria, a melding of investigative research traditions was evident: the influence of anthropology, interest in studies of Maori, along with a survey tradition and a commitment to produce information that would assist the State. The School was in high demand and Marsh noted the potential overloading of the School in researching social problems:

There is no other group available in the field of social welfare than the School. This has led Government and others to seek the School's help more than has been necessary in the case of most other University groups. If the University is to satisfy the community's demands, staff time must be increased for it.⁴⁴

As well as the development of social science research within the training curriculum of professionals, there was also during this period the development of sub-disciplines of social science. Increasingly, autonomous areas of knowledge were coming on-stream, with specialist curricula, concepts and broader theoretical positions, separate qualifications, and new staff. The loosening of Senate control encouraged the discipline of sociology and its

teaching began during the late 1950s with Canterbury using growing international interest and the volume of overseas research as a partial justification.⁴⁵ At Victoria, papers in 'social problems' had always been part of the Social Work Diploma and in the 1960s sociology papers increased to eight, including three at Stage III.⁴⁶ The study of political science was also developing popular appeal: the VUC Political Society was established in 1947, and there were 257 Political Science enrolments in 1947, and an MA from 1950.⁴⁷ The building of a politics discipline was evident in the founding of the *Journal of Political Science* in 1948 (becoming *Political Science* in 1951). These expansions – of the University system and of the social sciences – were mirrored at the international level,⁴⁸ causing New Zealand to experience some problems in filling academic positions – of 152 positions advertised in 1965, 44% went unfilled.⁴⁹

In addition to the difficulties in recruitment of staff, there were further brakes upon the development of social science research within the University system. The Senate Social Science Committee had recommended that a new research committee be established upon the dissolution of the UNZ, with a specialist sub-committee administering ring-fenced funds for social science. The full Senate declined the recommendation and social science was left to compete with the full range of disciplines for its resources.⁵⁰ Between 1964 and 1969 social science received an average of 9.1 per cent of all the funds allocated through the new University Grants Committee (UGC).⁵¹ The UGC came under fire, with some institutions and individuals calling for more humanities /social science interests to be on the interviewing sub-committee. So University stock was on the rise but it is arguable that social science was under-valued by interests within the University, and even viewed with suspicion by some competing disciplines. The precise implications of this for particular disciplines varied given that each College had its own departmental patterns, historical rivalries and trade-offs as a result of scale and institutional configuration. Consequently, while inter-College rivalry was placating, there was still internal rivalry to contend with at each College/University. A focus on specific interests and rivalries would illuminate why anthropology, for example, was based at Auckland, and why social science/social work gained a foothold at Victoria.⁵² It would also explain the blocks to particular developments; for example, Keith Sinclair suggests Auckland had a deep-rooted scepticism to political science and psychology which were blocked until 1957, and sociology until the late 1960s.⁵³

In addition to the broader conditioning effects of disciplinary dynamics, there are particular dynamics within each discipline itself. In the case of sociology, for example, some suggest that the major universities holding out were the more established ones with an ingrained Oxbridge and Scottish conservatism.⁵⁴ In support of such a view, they identify the average age of those serving on professorial boards, and the place, level and recency of training as important considerations. As the inaugural Professor of Sociology at Auckland wrote:

Representatives of longer-established disciplines were frequently hostile to the development of autonomous sociology courses and proposals to establish staff appointments were the object of sometimes acrimonious dispute.⁵⁵

Other accounts of sociology perceived little hostility from academic traditionalists, and no blocks in the country's distinctive value patterns and national character, seeing instead only encouragement and support.⁵⁶ Such accounts are deficient, however, in viewing support in isolation and ignoring the broader social and political structures and cultural conditions. Take one such personal account that explains the rise of sociology at Canterbury:

The decision to introduce sociology as a subject at Canterbury was made with the Professor of Psychology over a cup of tea one afternoon in 1957.⁵⁷

Whilst the account may be accurate – the individual in question should know because he was there – it is superficial and misleading because it ignores a broader and deeper conditioning.⁵⁸ To understand the patchwork of social science disciplines or the development of local initiatives does require the exploration of local circumstances, particular advocates and opponents, but this must always be done within the broader structural and cultural conditioning.

The size of New Zealand served to restrict disciplinary fragmentation and helped shape academic organisation. Despite the growing acceptance by State interests and professions, the social science community at the beginning of the 1950s was relatively small, comprising a handful of individuals at most.⁵⁹ In contrast, the United States had a broader and more specialised base of academic disciplines (psychology, sociology, economics, anthropology, and political science) from as early as 1914, which were then consolidated in the inter-war period.⁶⁰ As well as differences of scale, the New Zealand system experienced some centrifugal shaping and central control, in contrast to the competitive and decentralised American system.

External interests

To this point there has been little discussion of the external actors and interests that supported the expansion of social science research. During the 1950s and 1960s the intellectual stock of Colleges and Universities was on the rise, and the University began to realign in developing a greater research role. A number of groups (public sector, professionals) allied with the Universities and academics in support of this venture. From being a marginal concern, social science became a major currency, trading on the perceived value it had for policy making and its potential for an array of welfare professionals involved in assessment, administration and delivery.

The State embrace of social science research resulted in piecemeal research activity during the 1950s. Embryonic research functions were gradually added to the pre-existing information and statistical functions within Departments such as Justice, Labour and Education. The 1958 Joint Committee on Youth Offending drew together a range of departmental interests in seeking to understand the causes and cures of juvenile offending, and was serviced by a research unit. The focus of these research activities was strongly instrumentalist, and in the early 1950s both the Child Welfare Division and Probation Service sought to build rigorous case records to assist with a better understanding of the problems they faced. The guiding principle was one of personal pathology – research on ‘social problems’ was synonymous with individuals and families, focusing variously on work motivation, behavioural impacts on health, and frameworks drawing heavily upon deficit theory. The methodological orientation of the work was strongly positivist. Studies in justice and welfare utilised existing administrative data, and embarked upon the beginnings of a social survey tradition. By the end of the 1960s there were research units within Education (Research and Planning Unit; Curriculum Planning Unit; Child Welfare Research Unit; and host to the Research Unit of Young Offenders); a research section within Justice; a research division within Labour, and a Manpower Planning Unit that serviced the National Development Conference. There were also three qualified research officers in the Social Security Department; and an official research unit in the Ministry of Works which was home to Housing Construction Division.⁶¹

Departments developed their own particular research focus depending upon their delivery focus – Child Welfare emphasised causation and treatments, whilst the activities of the Social Security Department were driven by a tradition of national policy, administration and utilisation of case records. The development of better administrative systems and data collection across all Departments made the scale of ‘social problems’ increasingly transparent – those entering courts were counted and their characteristics described; similarly, those in juvenile detention or in receipt of welfare benefits were the focus of quantitative studies. Increasingly, attention focused on the cause and cures of such social ills, but despite the vision of establishing ‘causal links’ (for matters such as offending, indigence, and educational failure) aspirations outran the reality – which was a handful of staff engaged in descriptive surveys.⁶² There are strong parallels with the emergence of justice research within Britain at around the same time. The Home Office Research and Planning Unit from 1957 expressed interest in causes and effects, focusing particularly upon penal treatment and juvenile delinquency, and offering a brand of inquiry that was ‘pragmatic, interdisciplinary, positivist, correctional and reformist’.⁶³

The developments that took place within Government Departments also presented corresponding opportunities for Colleges/Universities. At Victoria there were discussions as early as 1965 about the utility of a survey research unit that would serve government and other public (i.e non-commercial) interests. Whilst this proposal received support of the National Research Advisory Committee, the Minister of Health and Social Services rejected the bid, citing the need to contain expenses during the increasingly difficult times of the late 1960s.⁶⁴

Government Departments also sought to stimulate University interest in policy-based activities, by identifying topics of interest from the mid-1960s. Within Government itself, pressing Ministerial demands and requirements to service the Royal Commissions that were in progress meant some diversion of Departmental research energy. In addition, the desire to employ and establish social research capacity within Government Departments was countered by a lack of adequate training and preparation for research staff.⁶⁵ Within housing, for example, the importance of linking research to policy and conducting more sociological studies was stressed,⁶⁶ but the lack of suitably qualified individuals was seen as a bottleneck on developments. Many research roles up to the end of the 1960s

continued to be filled by experienced administrators rather than by trained social researchers,⁶⁷ and training frequently occurred on the job⁶⁸ with research or analyst roles frequently enmeshed with that of routine administration.⁶⁹

By the end of the 1960s the uncoordinated manner of research developments is evident, with diverse activities being undertaken without a clear overarching shape or system. Indeed:

Explanation involves making sense of this myriad of episodes. To theorize about interaction is to relate these episodic events to a set of more general relations which underlie them and account for their patterning.⁷⁰

There are developments within Government Departments, within the Special Schools that focused upon professional preparation, within embryonic disciplines and research units, and developing within extra-social spaces with the founding of the Society for Research on Women in 1966. Some strategic and coordination issues were also becoming more apparent during this period. The 1962 Royal Commission on State Services had proposed the establishment of a research body to advise Government, resulting in the National Research Advisory Council Act 1963, and broad discussion about how to stimulate research capacity, training and utilisation. With the NRAC focused upon natural and physical science, a debate got underway during the 1960s around Government funding and responsibility for social science. By 1968 an overarching body was required, and the diverse and multiple developments were seen as inadequate to meet the challenges ahead. Interests looked enviously at the British Social Science Research Council, established in the wake of the Heyworth Committee,⁷¹ and calls mounted within New Zealand for a more strategic response.

Summary

The development of one cycle structurally conditions the ensuing one, and in summarising the chapter, the next cycle is already in motion. While social science research increased during the 1950s and 1960s, it did so on the basis of its advocates promising a good return on all the hopes and 'funds' that were being invested in it. But as the 1970s dawned, such promissory notes were becoming increasingly debased, and new groups of research interests sought to revise the terms of trade and set their own rates of exchange. In summarising, developments of this period were around the study of social welfare, rather

than the scientific study of society. There were murmurs of the philosophical, ethical, and political difficulties around social science, all of which were precursors of the anti-expert backlash that would surface at the end of the 1960s. There was also the emergence of new actors during the period as fresh disciplinary groupings formed and strengthened.

State support for social science was premised upon it delivering empiricist knowledge and unlocking causality for 'social problems'. Social science research was viewed in a handmaiden role of technique, and underpinned theoretically by a functionalism. With emphasis shifting to system dysfunctions, empirical social science was increasingly called upon to assist with practical problem solving. Gouldner's comments on sociology have broader relevance here:

the liberal technologies of sociology have become the market researchers of the Welfare State and the agents of a new managerial sociology.⁷²

Whether social science research could deliver to its sponsors was increasingly in question. Correspondingly, social science interests within the Universities were using extra resources (financial, legitimisation) to tread a more theoretical path and pursue *disciplinary* goals. Up to the 1970s there was little tradition of critical empiricism, nor any developed theoreticism, but this was soon to change as the taken-for-granted of the 1950s and 1960s was put under greater scrutiny.

With the expansion of disciplines, a latent rift that had been masked during the expansion of the 1950s became more open as disciplines gathered strength and began to pursue their own goals. Social science research interests within New Zealand began to identify separate agendas from those of the State which were more structural and theoretically informed:

the solution to our unsolved problems will not be found by 'administrative tidying-up'. The answers to our important questions on welfare will not be provided by a study of the routine statistics of the 'work done' by Government Departments.⁷³

State and disciplinary interests were pulling the 'reform coalition' apart in the struggle to assert their vision and achieve goals. The size of New Zealand, and its relatively small policy and academic communities forced these interests to co-exist for some time, whereas

in the larger academic and policy communities of Britain and the United States they could be despatched to different 'worlds'. But even within the confines of New Zealand society these tensions could only be covered for a short time: substantive tensions were becoming evident and with the full articulation of differing views of social science these would be fully apparent. State patronage of social science research meant inquiries were a function/production of social and economic currents and an aid to the containment of social problems. This had been a strategy that had occurred in a number of other countries by the early 1960s:

The rapid increase in demand for social-science knowledge occurred during a period in which the disciplines in most European countries were not yet fully recognized, established, and consolidated in academic terms. In such a situation political demands served as external support for social scientists trying to reach that stage.⁷⁴

But with an academic consolidation, there was a move from State relations and an attempt to assert disciplinary autonomy and improve their intellectual stock on what were non-governmental markets. Increased attention to researching society by interests outside the State, particularly the Universities, created tensions – given previous University-State relations. At heart lay a struggle to define and diagnose the social and the intensification of such developments would soon prise apart the reform alliance that had previously held.

Chapter Twelve Notes

¹ Wagner and Wollmann (1986).

² Cf. Baker (1965). In Britain the story was similar, with social science on the one hand and social surveys on the other gaining credibility during the war (Whitehead, 1985; Cornish and Clarke, 1987). See also Alpert (1973) for expansion of social science in the United States in the post-war era.

³ The short-lived Organisation for National Development (1945) had three aims: full and efficient employment; utilise resources to raise standard of living; orderly development of institutions and towns. AJHR, H-11A, 1946, p. 106 – outlines State responsibility for full employment.

⁴ For example, the National Employment Service appointed a Senior Research Officer in 1946, and the Department of Labour and Employment that replaced it the next year continued these developments. By 1948 there were 35 staff within Surveys, Statistics and Returns, and 13 in Research Section. The period also sees the beginning of more regular (quarterly) labour surveys. Again the situation was similar in Britain with the 1946 Report of the Committee on the Provision for Social and Economic Research (Clapham Report).

⁵ Green et al. (1955, p. 3); Scott (1955a & b); Baker (1955, p. 104); Bassett (1998, p. 291).

⁶ Mitchell (1972). Cf. also the optimism that permeates the accounts of Scott (1955a & b) and Hunn (1960).

⁷ Green et al. (1954, p. 3).

⁸ Labrum (2000).

⁹ Dalley (1998).

¹⁰ Marsh (1952); McCreary and Somerset (1955); Tasker (1953); the Mazengarb Report (1954); Sears (1969); Sutherland (1940a & b); McQueen (1945); Beaglehole and Beaglehole (1946); Schwimmer (1960); Hunn (1960).

¹¹ Roberts (1974, p. 2). See also the comment by Winks (1954), a Fulbright Scholar studying in New Zealand who suggested of New Zealand complacency, 'No one, of course, cares to be criticized, but New Zealanders show this trait more than any other people the author has met' (p. ii).

¹²

	% of Maori urban	% of Maori rural	% of New Zealand urban	% of New Zealand rural
1936	17	83	68	32
1956	35	65	75	25
1966	62	38	80	20

From Levett (1988).

¹³ The Mazengarb Report resulted from previous press coverage of immoral activities in Lower Hutt. *The Auckland Star* (19 February 1962) for example talked about how the welfare state had failed the aged, 'In the midst of plenty – a substantial majority exists in poverty'. This was one of a series of articles on the 'Cold Fringe of the Welfare State'.

¹⁴ Royal Commission on the State Services in New Zealand (1962, paragraph 65, p. 27).

¹⁵ Angus (1959).

¹⁶ Marshall, Hansard, 3rd August 1957, 313: 1902.

¹⁷ Again this was similarly an Anglophone phenomenon – the number of specialist economists in Britain ballooned from 22 in 1964 to 251 in 1972 (Coats, 1978).

¹⁸ Webb (1940, p. 89).

¹⁹ Polaschek (1958, p. 127).

²⁰ Marsh (1950, p. 121).

²¹ Parton (1979, p. 217).

²² Barrowman (1999, p. 105).

²³ Oppenheim (1963, p. 86).

²⁴ Parton (1979, p. 208).

²⁵ Royal Commission on the New Zealand Universities (1959) (Hughes Parry Report).

²⁶ NZIPA, MS 1336, 8:1, letter 29/9/48 (Alexander Turnbull Collection). The Committee consisted of the Vice-Chancellor, Beaglehole, Simpkin, Belshaw, Field, and Morrell.

²⁷ See Winks (1954); Havighurst (1954); Ausubel (1961).

²⁸ McDonald et al. (1989).

²⁹ Lundberg (1945); Young (1945).

³⁰ Lundberg (1945, p. 508; italics added).

³¹ Letter from Frederick de la Mer to Vice-Chancellor (26 April 1946). (Frederick de la Mer Collection, MS 3865, Alexander Turnbull Collection.)

³² *Needs of the University* (Smith, 1946); *Problem of University Finance in New Zealand* (Smith, 1947), *Research in the University* (UNZ, 1949).

³³ Parton (1979, p. 161).

³⁴ *Needs of the University* (Smith, 1946, p. 25).

³⁵ See for example articles by Professor Ian Gordon 'Crisis in the University' and 'The University at the Crossroads', *The Listener* (1946a & b).

³⁶ Report of Academic Board on University Reform (UGC, Minutes of Senate, 1947, p. 64, item 463, National Archives).

³⁷ Numbers enrolled at University Institutions (from Bloomfield, 1984).

1935	5,101
1940	5,528
1945	8,425
1950	11,515
1955	10,851
1960	15,809
1965	22,145
1970	34,446
1975	42,436

³⁸ Scott (1959).

³⁹ Campbell (1956, p. 22).

⁴⁰ For reflections of a foundation student see Hancock (1996, p. 318).

⁴¹ McCreary (1971a & b).

⁴² Jim Robb completed his doctorate on Working-Class anti-Semitism in London, and returned to take up post at Victoria in 1954. In 1966 he became New Zealand's first Professor of Sociology.

⁴³ D.C. Marsh, Annual Report to Senate of the Special Schools (1951) (UGC, item 463, National Archives).

⁴⁴ School of Social Science, Victoria University at Wellington, Submissions to Hughes Parry (p. 53)(UGC, item 497, National Archives.)

⁴⁵ See UNZ records (UNZ 10, A Courses, a3 Arts, National Archives). The syllabus consisted of: introduction; culture; social structure and personality; social groups and collective behaviour; social stratification; communities; social institutions; social disorganization and change. An early founder recalls that in the first year of 1958:

'There were thirty-one students, a curious mixture of the competent looking for an additional unit with which to complete their degree and the incompetent who had already failed everything else' (Thompson, 1965, p. 132).

⁴⁶ Letter 11/ 7/60 Victoria University at Wellington to Registrar University of New Zealand (UNZ 10 , A Courses, a3 Arts). See also McCreary (1971b).

⁴⁷ Barrowman (1999, p. 63).

⁴⁸ Smith (1982) notes the British expansion from 212 university teachers of social science in 1938, to 7,000 by the mid-1970s.

⁴⁹ Holmes (1965).

⁵⁰ Parton (1979).

⁵¹ Representation of disciplines on URC (1962–81)(UGC)

Discipline	Man year representation	% representation
Chemistry	54	24.5
Physical	36	16.3
Agriculture	35	15.9
Engineering	22	10.0
Earth sciences	16	7.3
Education	9	4.0
Botony	10	4.5
History	5	2.3
Economics	16	7.3
Philosophy/psychology	17	7.7

⁵² McCreary (1971b); Sinclair (1983).

⁵³ Sinclair (1983, p. 202).

⁵⁴ Mols (1968); Hansen and King (1966).

⁵⁵ Timms (1971, p. 189).

⁵⁶ Thompson (1967, 1972); Robb (1996).

⁵⁷ Thompson (1965, p. 132).

⁵⁸ Miller (1987) makes a similar criticism when noting how shots being fired were not the underlying cause of events of 1848. He highlights a similar concern with deep causes.

⁵⁹ The Directory of NZ Science (Bastings, 1951) identified around a dozen anthropologists, psychologists, social scientists, mostly at Victoria, out of 1,300 scientists nationally.

⁶⁰ Bulmer (1987b & c).

⁶¹ Gibson Report (1970).

⁶² For example, Survey of Foster Home Placements by the Child Welfare Branch in 1952; Survey of Hostel Accommodation for the Borstal After Care Association, 1953. (See also MacKay, 1975.)

⁶³ Cornish and Clarke (1987, p. 170).

⁶⁴ Letter from Minister of Health and Social Services to Minister of Science (23 February, 1967, CW, 33.4.2, National Archives).

⁶⁵ The State Services School for Social Workers in 1963 offered four lectures on social research (CW, 20.1, National Archives).

⁶⁶ 'Notes on the Meaning of Research as an activity of the Housing Division' (Daish, 1966, in HD, W1552 1/24/31/1 Part 1, National Archives).

⁶⁷ (CW, 33 3.3.3, Research Reports, National Archives).

⁶⁸ Submission from Child Welfare Division to NRAC 1970, (CW, 8/10/3, National Archives).

⁶⁹ Research Programme 1968–75, Part 1 (CW, 33.2, National Archives).

⁷⁰ Archer (1979, p. 396).

⁷¹ The Heyworth Committee (1965) was established 'To review the research at present being done in the field of social studies in Government departments, universities and other institutions and to advise whether changes are needed in the arrangements for support and co-ordinating this research' (cited in Blume, 1987). It led to the creation of a Social Science Research Council and recommended increased employment of social scientists in Government.

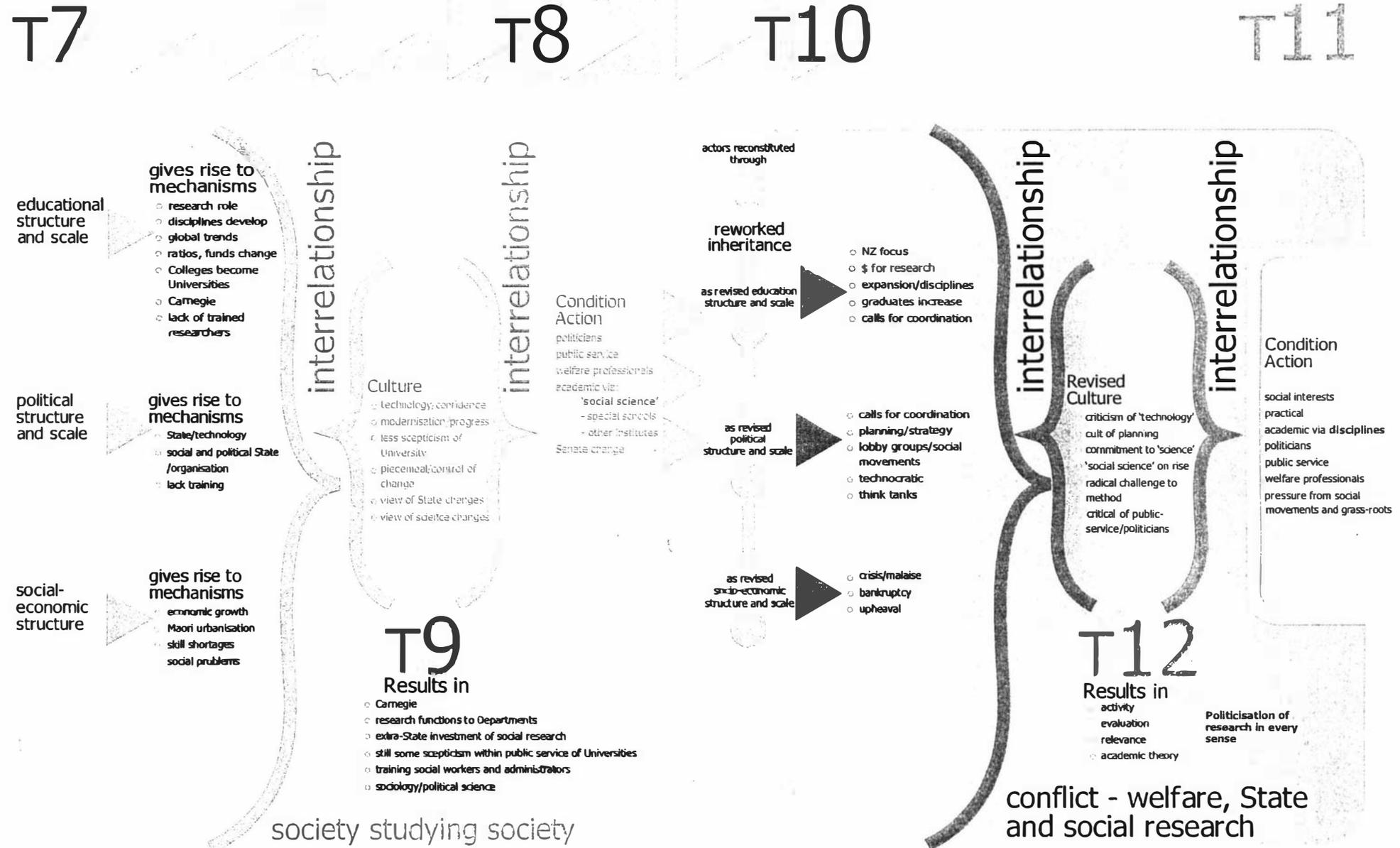
⁷² Gouldner (1970, pp. 500–1).

⁷³ Robb (1966, p. 21).

⁷⁴ Wittröck et al. (1991, p. 57).

figure 12.1

From Alternative Benefactors (Post-War) to On the Brink of Bankruptcy (1984)



Chapter Thirteen

On the Brink of Bankruptcy

Introduction

The final chapter draws the explanatory account of social science research to a close, bringing discussion up to 1984. In the previous cycle, social science research had developed under the close sponsorship of the State within individual Government Departments, and as part of the training of public servants and welfare professionals within Special Schools. By the 1970s a change in the distribution of resources and roles was underway which, along with the conditioning of values, ideas and beliefs, contributed to a revised context for social science. The period is marked by continued attempts by Universities, and within them social scientists as distinct communities of practice, to lever resources and raise autonomy. Meanwhile, State decision makers and interests within the public and welfare bureaucracies continued to express high hopes about the contribution of social science. During the previous cycle, the Universities and disciplinary groups began to emerge as new actors, seeking to assert their own range of interests. To this was added the stirring of broader social movements and grassroots activism, which themselves introduced new sets of interests to the debate around the role of social science research and the place for social knowledge within contemporary society. A site of contestation developed around the 'meaning' of social science research, as a result of the competing understandings of State and academics. A distinction was emerging between, on the one side, politicians and the public service, and those allied to the Government social science research interests (i.e. those on funding and steering bodies), and on the other side those advocating more autonomous disciplines, or even a more democratised social science. The 'reform coalition' from the previous period was increasingly split, resulting in utilitarian social research and the secession of academic social science disciplines. At root was the conflict over what knowledge, particularly academic knowledge, consisted of. The title of this final chapter alludes to the growing discontent and loss of confidence around social science research that such competing views helped to foster.

Economic, Social and Political Catallaxy

The notion of bankruptcy also points of course to the growing stagflation that was to haunt New Zealand policy makers during the 1970s. Britain's entry to the European Economic Community in 1973 saw a cold chill blow into the New Zealand economy. Whilst dark clouds had been gathering for some time, changing trade relationships, increasing oil prices and falling GDP presented relatively uncharted problems for New Zealand policy makers.¹ The 1970s was a period of economic and social upheaval: changing family patterns, rising unemployment, demands to honour the Treaty of Waitangi, a growing Pacific Island migrant population, and urban dislocation, were storms to be weathered. There was a sea change to the prevailing optimism that had existed, and academics of both left and right, along with popular currents of opinion, began to turn against the social democratic State.² The political scene was becoming more turbulent with an increasingly vociferous pressure group lobby and social movements looking to assert their interest against the State. Internationally, a similar picture was emerging; during the 1960s the United States engaged in Wars on Poverty and the Great Reform Programme, involving large-scale income and educational programmes. In Britain, the rediscovery of poverty, despite full employment and economic growth, gave cause for concern and hastened the erosion of post-war complacency. No single solution was put forward to address the malaise; instead, a multitude of liberal, social democratic, Marxist, and critical paradigms presented their competing diagnoses.

Crosscutting debates about the role of the State and the place of welfare were new discourses of civil rights and participation. Across the Western world the 1960s heralded a fundamental re-examination of institutions and a wish to revisit the assumptions and techniques upon which post-war growth and prosperity had been premised and citizenship constructed. The introduction to a collection of New Zealand essays of the later 1960s captures the air of change and uncertainty well:

There hangs over New Zealand a sense of imminent change. It is impossible to doubt that change is coming; all that can be in question is when and where it will begin and how extensive its consequences will be.³

Most critics agreed on the identification of problems, including lack of accountability, inefficiency, and poor levels of economic growth. This domestic and global unease bore two immediate responses: first, more polemic and ideologically fuelled visions. On the

right these were illustrated by the retreat to neo-classical economics, aided by monetarist and public choice additions from Chicago and Virginia respectively. From the left came a number of Marxist re-interpretations of the State crisis and routes out. But there was a second, more technocratic, response that emphasised greater scientific planning, and the necessity of expanding technocratic machinery. Under this latter interpretation, improved utilisation of expertise and techniques would help contribute to putting Government policy back on track.

In New Zealand it was the second route that seemed to hold sway and the appeal of 'scientific' planning to ministers and officials alike during this period can be identified.⁴ The interventionist tradition won out during the 1970s, and the centralised State was further reinforced by a policy of 'Think Big' and, later, a political style of Muldoonism. From the late 1960s the looming economic bankruptcy was met with a flurry of national conferences, committees, Task Forces, and Royal Commissions, and there was major reorganisation of local government, health, and welfare. The National Development Conference (1969), the Royal Commission on Accident Compensation (1967), the Royal Commission on Social Security (1972), and the Commission of Inquiry into Housing (1970), were a recognition of the trenchant difficulties of existing *policy responses* and *policy-making approaches*. The incrementalism, considered such a strength during the immediate post-war years, was usurped by the perceived need for more robust social and economic planning. A number of these Commissions and Inquiries suggested social science research be brought more towards the centre of advisory and political machinery. If the 1950s and 1960s were decades of complacency, the 1970s became one of reflecting, rethinking and reorientating.⁵ A role for external contributions to policy formulation developed, and there was growing 'think-tank' influence during the era. As Johns suggests, a number of developments began to introduce an element of competition within the State apparatus itself among Treasury, Audit Office, Prime Minister's Department, and New Zealand Planning Council.⁶ He talks of:

The increasingly complex and competitive nature of the central and administrative machine and of the increasing array of bodies and techniques available to assist Government in making its key public choices in a more informed fashion.⁷

A number of the inquiries pointed to the limited place for, and inadequate utilisation of, social science research within policy making and planning. The National Development Conference recommended the NRAC, which had previously paid scant attention to social research, review Governmental social research capacity. The result was the Gibson Report of 1970, that advocated the NRAC develop a social science arm to encourage the growth and development of research activity. The Gibson Report recognised a number of difficulties that Government needed to address around social science research, including recruitment and retention, as well as the funding, role and coordination of social scientists within Government Departments. The Inquiry into Housing also highlighted shortcomings in planning and a lack of research, which led to a National Housing Commission (October 1972) that had significant expectations of undertaking housing research. Thus, social science research was still seen as an important contributor to growth, and it was felt that with more investment, coordination and organisation, it would be able to deliver on its promises.

The pledge to social science research ran deep during the 1970s within the political machinery, with growing legislative interest around the role and best ways to organise social research activity. Explicit recognition of the value of social research was embedded in Section Four of the Department of Social Welfare Act 1971. For the first time a Department centrally involved in social policy was given specific responsibility to undertake and promote social research, something that the Department of Health had been given half a century earlier.⁸ Senior public servants highlighted the importance of exploring social needs more rigorously, and of convincing Government politicians about the value of evaluating programmes in justice, child welfare and social security.⁹ Distinctions between policy and administration, and between public servants charged with research as against a policy function, became more cemented – thus was a specialist category of ‘social scientists’ and ‘social researchers’ emerging within the public service.¹⁰ Distinctions were also increasingly made between different types of social research activity – statistics, the use of social science to explore longer-term issues, and evaluating the effectiveness of existing and recently introduced programmes.¹¹

During the 1970s it became clear that the NCD was unable to deal with the increasingly unstable internal and external environment, and the growing complexity of issues.¹² As a successor, the Task Force for Economic and Social Planning (1976) continued to highlight the need for improved social research on the effects of urbanisation and social dislocation, particularly in respect to Maori, and pointed to the need for research in fields of housing, education, crime, and community change. It also stressed the need for better evaluation of programmes. The Task Force Report provided the impetus for establishing the New Zealand Planning Council and Commission for the Future. The Planning Council had an advisory capacity and was charged with medium-term analysis and forecasting (3–5 years). The Council criticised policy entrenchment and accepted ways of doing things, noting that existing policies went unchallenged while new proposals received a thorough appraisal:

Efficient policy-making requires that both undergo systematic evaluation... Evaluating existing policies and programmes...is a fundamental part of planning, but it has often been neglected.¹³

The Commission for the Future took a longer-term view of 30 years – setting the agenda for Parliamentary and public debate.¹⁴ It was ‘to study the possibilities for the long-term economic and social development of New Zealand... setting an agenda for public discussion and debate on possible futures for New Zealand’.¹⁵ The initiatives signalled the commitment to greater cross-sectoral planning, and the desire to use an appropriate knowledge base within decision making. Both initiatives fell during the *de-quangoisation* that emerged from the mid-1980s. These initiatives and their incorporation of significant social scientific activity had the consequence of diverting energies away from Departmental attempts to foster the growth of (evaluation) research activity. That economic and social dislocation was seen to necessitate a more strategic policy process, using greater consultation and social knowledge, had knock-on effects for the public service which was increasingly operating within a complex policy environment that placed new demands upon it.¹⁶ As an interest group, the public sector were themselves receptive to the possibilities of social science that would allow better understandings of the nature of problems and the impacts of initiatives. Such support was hastened by the public sector also coming under attack during the 1970s as one of many identified causes of the economic malaise.

There were further changes which also shaped opportunities and interests for social science change, including the public service undergoing a major shift in its shape and role. Although staff ceilings had been introduced into the service from 1967 (and during the 1970s a sinking lid had been imposed), there was a growing graduate population entering the service. Among these there was a range of social science graduates who joined the more established discipline of economics.¹⁷ A push to raise the quality of public service recruits and make better use of graduates within the new policy environment took place, and was itself part of broader discussion about the contribution of graduates to New Zealand industry, commerce and society, as a way out of recession. Discussion centred on the role of not only those graduates who had generic degrees but also about the specific skills and capacities that were required, including specialist social science considerations. The 1960s had seen some expansion of specialists within Government, particularly in the discipline of economics where high hopes remained, and this push accelerated in the 1970s. Given the perceived breakdown of what had previously been seen as effective ways of working, new intellectual resources became necessary to improve the effectiveness and efficiency of economy and society. This differed radically from the previous role played by such intelligence, that of managing an assumed growth.¹⁸

There was a 'spectacular' rise in the number of social science graduates being demanded as advisers and for planning and investigative roles during this period.¹⁹ Three-quarters of 1974 social science graduates entering employment did so in public, educational or service sectors; for some branches of social science the figure was even higher.²⁰ Within the central public services there was a move away from emphasising efficient implementation of policy to analysing and researching its purpose and effects, thus contributing to the need for multidisciplinary roles that moved beyond established techniques of economics. These demands contributed to the growth of policy planning units in Departments,²¹ and to political support for University research units including the Centre for Criminology and Industrial Relations Centre at Victoria.²²

Again there were parallels with overseas where the 1968 Fulton Committee in Britain sought to move the service from a generalist one, acting intuitively (albeit with increasing social science supports), to a service that expected to intervene more, and to do so with greater accuracy. Interpretations of these events in Britain suggest that up to 1976, civil

servants and Ministers invested (or gambled) heavily, both financially and emotionally, in the promise of social science.²³

The continued expansion of welfare professionals exerted further pull upon social science during this turbulent period. Between 1974 and 1978, prison and institutional workers, bailiffs and legal and social workers increased from 2,195 to 3,052, an increase of 39%. There was a large rise in the number administering welfare benefits (and also a 27% rise of uniformed and civil police during the same period).²⁴ The expectation was that these professionals would be better qualified and trained to address the 'social problems' they faced. A number of new courses were introduced, and professional groupings themselves became increasingly organised into associations and societies.²⁵

Asserting University freedom

The Universities went about their business from the late 1960s in an increasingly complex environment. There was the expansion of the University system with an increase of institutions, higher student enrolments, more staff employed, and a diversification of courses. As one commentator suggests, increased institutional size provided opportunities for disciplinary configurations, including within fields of social science:

It was the universities' growth in size as much as an improvement in staffing ratios that enabled staff to develop some measure of specialisation in their teaching.²⁶

Across the (now six) Universities, sixty per cent of staff were New Zealand-born and disciplines were consolidating in a number of social science fields. The Universities and academic staff conducted their business in an increasingly internationalised environment of staff recruitment, visitor flows, journals and conferences. The growth of disciplinary groupings (including sociologists, political scientists, anthropologists), and a hardening of their boundaries, created incentives for those working within their parameters and whose allegiance was increasingly to a discipline that spanned national boundaries.²⁷ Internationally, the volume of journals, publications and fund of intellectual ideas was increasing exponentially. Not only did the Universities go about their business in a more global context; in addition their business itself had begun to change into one coalescing around disciplines. Such a revised environment would see reformulations of what up to then had been fairly homogenised social science research and social sciences, into discreet

disciplines. While the small size of New Zealand society would exert some constraint upon the fragmentation to prevent academics and public servants inhabiting completely 'different worlds', the previous coalition was severely tested as academics sought to assert autonomy and to labour within their disciplinary paradigms.

The system expansion of institutions and disciplines exhibited some time lag effects in its being worked through into a revised set of structural conditions. The waning of ideational support for Universities was felt more immediately, particularly when it is recalled that these institution had established only a precarious foothold in the post-war cultural context. Along with the public service, the Universities themselves faced growing criticism during the 1970s. As earlier chapters suggest, the University faced considerable obstacles in securing a position within colonial society given the prevalent structural and cultural traditions that were inherited. With a combination of University expansion *and* growing economic difficulties in the 1970s, these criticisms returned with a vengeance and questions were asked about the utility and public expenditure implications of such increases.²⁸ While one commentator notes that, 'The Universities' response was increased emphasis on the relevance of their business to the national interest',²⁹ such a response was itself problematic given the competing discourses of discipline and relevance that found support among different interests within the Universities. This conflict brought particular consequences for those who had social science interests. On one side there lay a dynamic of relevance and a commitment to instrumentalism, and on the other, an assertion of disciplinary autonomy and separation from other disciplines, and from the reach of the State.

Despite an expansion of the University system, and within it of social science disciplines, which levelled a challenge to the State's dominance of social science research, spending on social science research remained low. In 1975–76 only 1.8% of government research expenditure was on social science research (comparisons made with the UK suggest that the social science share of science budget there would, at a conservative estimate, be around 6%). Such national levels of funding, combined with small amounts of money from the Medical Research Council, Golden Kiwi Lottery, and NZCER research focused on social issues, provided relatively slim pickings for social scientists. Many social scientists saw the lack of discrete funding as a clear block to disciplinary aspirations. As a

result of increasing fiscal restraint, the UGC grant towards the end of the year up to March 1977 was reduced. But the blame for low funding for social science could not be laid solely at the door of Government, since the competing demands *within* the University for the Government grant resulted in social science research being a relatively marginal concern for the UGC.³⁰ With resource distributions for social science relative to other academic disciplines being viewed as low, social science interests seemed set to lobby for a separate fund for social science research.

Ideational Challenge and Chaos

Tensions went beyond dispute around levels of research funding and drove to the core of what particular institutions perceived their *raison d'être* to be. A range of value shifts further conditioned the activities of interests during this period. Centrally, there was a loss of confidence in the role of the State and a growing pessimism around the ability of the State to deal with social and economic problems, whether incrementally or indeed at all. There was also growing mistrust of the established political parties and system and, as mentioned earlier, the bureaucracy that served them. The belief that science and expertise would provide the guides to professional activity of the polity and welfare sectors was under attack. The culture of State deference was critiqued, and broadsides came from a range of social movements, including women, Maori, community development, as well as from within the academy. That existing political and economic arrangements had been unable to deliver the policy dividend encouraged further realigning of support within broader social movements. As a consequence of this questioning, further doubt was cast on expertise, professionals, State organisations, and their paternalistic notions of well-being and welfare. The academy added to this critique (as well as being under criticism itself), altogether lending to the period a feel of pantophobia.

Among University social scientists there was far greater sobriety around the potential contribution of social science research. There was some recognition that the stock of social science had risen beyond its real value, and individuals within the academy and public sector cautioned against overselling social science and contributing to further inflation. Indeed, Departmental officials went on record to voice concerns that the previous emphasis on social science research meant they were now holding an overvalued investment. The head of the State Service Commission, for example, warned that the

application of social science to social problems was problematic – citing the experience of Head Start in the United States.³¹ Similarly, the New Zealand Treasury, although defending the use of analysis and evaluation, warned against it being oversold.³² At a 1981 Conference, Graeme Fraser, the Chair of the NRAC Social Science Committee spoke of the exaggerated expectations of social science investment and the importance of these being dampened in order to retain the real value.³³ Underpinning these comments from the within the polity and research organisations was a call for a more diversified social science research portfolio; one that hedged its short/long-term investments and was more cautious in promising dividends within the taxing regime of methodological indeterminacy and *realpolitik*.

Methodological challenges exerted a major conditioning influence upon those backing an instrumental social science. The relationships between Government Departments and University that had arisen from the 1950s were premised on a positivist view of social science research. This was also evident with the growing State Departments of the 1950s and 1960s, with researchers addressing particular policy questions and activities within an assumed settled paradigm underpinned by functionalism. Attacks on the model of natural science, such as within the work of Kuhn and Lakatos, had implications for a social science that had modelled itself on the natural sciences.³⁴ During the 1960s and 1970s, naturalism as a basis for the study of social life, at least in the form that had sustained a generation of survey researchers and Government researchers, was increasingly assailed.³⁵ A series of critiques emerged around positivist and functionalist dominance of social science. This era was one of ‘new rules of sociological’ method, rampant phenomenology, and also a series of Marxist, cultural Marxist, feminist and critical responses, that shattered the existing complacency.

Questions of value forced their way into what was a rapidly mutating social science. A tradition of humanistic philosophy, which had been relatively absent from the New Zealand University system given its later colonial development and demands, was re-introduced into New Zealand intellectual life (it had always been part of the discussions within local societies and institutes). There was major methodological rethinking with attacks on the notion of objectivity, and the advocacy of community and grass-roots approaches to research. Thus there were the beginnings of a broader challenge to the

social relations of research production. To a post-war brand of 'Standard American Sociology' that fused empiricism and functionalism, were now added heady theoretical mixes, including a range of provocative ideas from history, politics, education and sociology: Braverman, Illich, Willis, Gouldner, Anderson, Bourdieu, Foucault, Althusser, Firestone, Hall, and Said, all of whom contributed to an expanding range of intellectual possibilities.

Such theoretical challenges and critiques were to find their way into the broader policy – research nexus. The State's own response during this time was to give increased encouragement to evaluation and effectiveness. The data that resulted from such endeavours was recognised and challenged as politically mediated³⁶ and the conceptualisation and conduct of social science research was increasingly seen as politically infused. Debates and clashes over method were also taking place within the State bureaucracy itself, including Departments and planning bodies. In the view of the Planning Council, for example:

documentation of social processes is a much more qualitative and impressionistic procedure than for economic processes. Mixing approaches creates some tension within the secretariat. Those working in established disciplines, notably economics, tend to resist the introduction of the more impressionistic social policy orientations.³⁷

This was new ground for those accustomed to viewing social science research technique as a paradigm in itself, and social science research was presented as something of an inflated, perhaps even debased, currency. One result of the rise of social activism was that calls were gathering for a more democratised research process, as well as adequate recognition of the political nature of research endeavours. In part, these represented a shift from the collection of factual evidence, to embracing more qualitative and interpretative concerns:

Finally, when we evaluate the impact of public policies, we should talk more from the perspectives of those *affected* by those policies. I think that can only be done by a subtle redefinition of the concept of merit within the public service. If it is true that the public service within the 1980s is going to rely increasingly on social research as it seeks to grapple with a wide range of social problems, then that must be complemented by an increase in qualitative skills and talents deployed within its ranks.³⁸

The combined outcome of such critiques was a fundamental re-appraisal of social science research. Whilst many previous supporters of social science research, both within the bureaucracies and based in Universities, continued to advocate empirical methods as *the* solution, others assailed the claims, assumptions and aspirations of empirical inquiry, presenting more theoretically informed alternatives.

Interaction and Conflict over the Investigation of State and Society

These structural and conditioning effects were mediated through actors including politicians and the public service, many of whom retained vested interests in social science research. To these actors were added a number of reconstituted actors, including welfare professionals and strengthened social science interests within the Universities. Joining the fray were a number of pressure groups and grass-roots movements who increasingly questioned the use of social science, and sought to open up the process of social science research to a broader collection of societal interests. The ongoing debate was therefore a multi-vocal one, including interests attempting to assert their understanding of social science research, and to shape funding, organisation and control in the light of this image. The ensuing relations between these resource distributions and interest groups would be the next phase of social science research development.

The Universities were undergoing major expansion at the beginning of the period and faced few blocks to broadening the courses and qualifications they offered. Academics faced new incentives that were increasingly generated by, and mediated through, the network of disciplinary configurations being spun at each institution. Academics demarcated themselves into more specialist curriculum, and used the growing strength and legitimacy that came from University expansion to press their disciplinary demands.³⁹ The disciplinary boundaries of ideas and roles in turn conditioned the activities of academics. Groups of academics focused on their own intellectual values that were bound up with those of the discipline of which they aligned. Within a number of social science disciplines there was a move from servicing a practical State-driven agenda, to developing their own research interests that had disciplinary drives around theory-building and audiences that were not solely State, or even primarily national.

The State and its Departmental forms continued to invest in the fruits of social science. For a number of State interests, the answer to the malaise was to instigate a more coordinated role for social science research, displacing the haphazard incrementalism that had predominated up to then. The 1970s was a heyday of rational-planning for policy, and the push for policy and programme evaluation can be situated within this trend. There was a comparable tide in the United States, where the Grand Reform programmes placed emphasis on understanding better causes and effects, leading to the beginnings of the first formal evaluation workforce. In Britain, although the beginnings of an evaluation workforce were more *ad hoc*, the Government became more systematic in its selection of research topics and, from 1973, introduced the Rothschild 'customer-contractor relationship' of research management.⁴⁰ The emphasis of New Zealand State Departments began to move in a similar direction.

Within the New Zealand political system, support for social science research was forthcoming with both major parties highlighting the need for better funding and organisation. The Labour Party in a 1972 document advocated a full suite of changes, including the introduction of a NRAC Social Science Council to oversee developments, an independent social research unit, the prioritisation of evaluation within Government Departments, full-time research positions within the Universities, as well as the stimulation of research amongst private groups and individuals.⁴¹ Norman King, National's Minister of Social Welfare, emphasised systematic planning of social welfare, which took better account of social needs, service patterns and policy results.⁴² A Social Science Committee (Committee D) was established under the auspices of the NRAC, and this Committee was to foster the introduction of the Social Science Research Fund in October 1978, which finally marked a separate fund for social science.

By the early 1980s there had been developments of programme evaluation within Government Departments – a Social Programme Evaluation Unit existed within the Department of Social Welfare, a handbook was in circulation, and the State Services Commission (SSC) was tentatively encouraging evaluation activity across a range of government agencies.⁴³ Outside the social policy arena, agencies including the New Zealand Forest Service, Ministry of Agriculture and Fisheries were attempting to evaluate the social effects of their programmes.⁴⁴ However, such developments were a far cry from

the Planning Council's vision of mandatory evaluation of existing policies and programmes. There were problems around the lack of a clear career structure for social scientists within Government Departments. A survey of Governmental social science capacity highlighted the lack of skills, difficulties of retention, and staff being engaged in a wide range of work – research and analysis, evaluation and monitoring, advice, information, and statistics.⁴⁵

Along with new policy mechanisms and approaches to the organisation of the business of Government itself, there was also the search for new institutional mechanisms to promote and coordinate social science research activities. There was a real sense that better organisation of capacity and more strategic coordination would go some considerable way to harnessing the utility of social science research and evaluation. A number of non-Government voices added to the debate for the reorganisation of social science, reflecting the shift that was underway in the relations of research production. The Social Council (one of the consultative Councils resulting from the NDC), for example, highlighted the importance of evaluating systems and exploring the effectiveness of spending on particular interventions, and the Council advocated an enlarged view of social science research compared with the earlier recommendations of the NRAC.⁴⁶ It noted a dearth of long-term research work within government, and how research frequently focused on issues that were inseparable from administrative and clerical duties.

Discussion outside the Universities, among Government Departments and broader social interests, grappled with challenges created by method and values. Social research was recognised as having a role to play in assembling a factual basis for decision making:

It involves also an appreciation of the ways in which philosophical and ideological assumptions can influence the planning of research and the ways in which the selection of research techniques can limit or otherwise influence the nature of the conclusions arrived at.⁴⁷

For the Social Development Council, therefore, research should be practical and theoretical. It also pointed to cross-departmental consideration being overlooked and how Government Department priorities were often misaligned with the pressing problems of society. Other non-Governmental interests advocated an independent Social Research Institute to evaluate existing social services, new interventions, and effects of social

policy.⁴⁸ Interests within the University called for a Centre for the study of New Zealand society to be established at a University,⁴⁹ and there were calls for an Institute of Economic Research, something the NRAC eventually decided against.⁵⁰

Overall, a struggle was underway for the control of social science research among the State, Universities, and social interests.⁵¹ This struggle reflected a series of images and expectations that were held of social science. Discussions focused on both the role of broader 'social problem'-type research (new immigrants, urban dislocation, gangs, juvenile crime, regional decline), and the absence of evaluation. Differences in views were not merely technical but consisted of fundamental divisions about the role of theory and the place of values. A range of interrelated debates developed around: Government's own social research capacity; survey research and gaps in statistical capacity; forecasting; funding and organisation of social research outside the Governmental sector; consultations; and how to bridge more effectively the research-policy divide.

An emerging theoreticism

During the 1970s a University-driven theoreticism of social science was on the rise which sought to understand both the nature of the State and society itself. Whilst this did not completely displace a handmaiden role for social science research that had developed after the Second World War, serious challenges were levelled at it. Instrumental social science research continued to seek a contribution towards better planning and immediate concerns, but more theoretical expositions questioned the utility and foundations of such activity. Up to the 1960s a unified social science interest had been apparent, but during the 1970s this separated into two camps: one coalescing around a more theoretical, critical and structural perspective, and the other gathering around a more traditionally empirical and social administrative approach. Inevitably some interests sought to straddle both,⁵² and in part the story during the latter part of the cycle concerns the outcome of these dynamics and their cross-trading over time. One example was the rift that emerged at the Sociological Association Australia and New Zealand Conference in 1972 between those emphasising the professional, academic status of sociology and those concerned with how sociology could be applied to 'policy issues'.⁵³

For sociology, one outcome was intellectual and sub-disciplinary fragmentation in a field that had previously been strongly functionalist and focused upon social dysfunction, or what policy makers defined as the contemporary 'social problems'.⁵⁴ While these traditional social administrative concerns did not dissipate, they were overlaid by a more critical academic stratum, generating alternative research interests and programmes of study. These included the nature of New Zealand urbanisation and an urban sociology that distanced itself from purely descriptive or demographic concerns.⁵⁵ Similarly, there was a broader theorisation about family and youth,⁵⁶ the re-emergence of poverty,⁵⁷ and race and ethnicity, all of which were becoming sites of contestation, both within the academy and outside.⁵⁸ There was talk of social science delivering the possibility of 'transformative knowledge',⁵⁹ illustrating how such theoretical aspirations traded far beyond their earlier notion of social research utilised for State and bureaucracy.

During the 1970s, social science as a holistic entity splintered into political science, anthropology, sociology, geography, and social administration. The nature of social science, its role, audience, and funding were increasingly open to dispute. Existing meanings and understandings were loosening, and previous coalitions that supported social science research in the 1950s and 1960s looked increasingly problematic. Within this mix was an aversion to the dominance of social administrative and survey techniques which focused on policy or welfare values, but which denied 'science' or disciplinary autonomy.⁶⁰

Despite some expressed optimism about the opportunities that existed for social science in new funding regimes and government expectations of the 1970s,⁶¹ there was a disappointing response from the University sector to calls for more policy-oriented research.⁶² Although funding priorities for University social science were around 'policy-oriented research', including evaluative research, the gulf between the academic and policy worlds seemed wide. The growth of disciplines and their interests help explain this chasm, with practical projects often failing to meet the needs of a more theory-driven system of academic social science. Advocates of direct practice and relevance suggested that too many projects verged on the 'esoteric or dilettante',⁶³ while for opponents the crucial question was posed as 'who are social scientists serving?'⁶⁴

This contestation may be situated more broadly within attempts being made by academics and disciplines to consolidate the role of the 'intellectual' within cultural life. Such a process was not confined to New Zealand, and as a commentator of the European experience noted:

If the relevant actors see the academic institution as research-oriented, this process encourages 'scientization', the development of a closed, formal 'self referential' discourse.⁶⁵

How these developments played out within the context of New Zealand is crucial. Organisationally, new disciplines centred around cores of professional identity, interests and portfolios, and shared some paradigmatic commitments of concepts, disciplinary methods and founding fathers.⁶⁶ Counterpoised to these developments were criticisms of Universities' failure to prepare social scientists appropriately in research methodology.⁶⁷ Similarly, a review of social science capacity within Government that took place towards the close of the study period suggested, 'Changes in emphasis in applied social research were not responded to by social science departments in the universities'.⁶⁸

While academics sought to academicise their contributions, in so doing they faced a lack of prior investment and scholarly tradition that left positions subject to rapid revisions and reversal. Social science research had never previously pressed for reform as a product of autonomous social movements, nor had it been scientised into a body of autonomous practice within the University. Instead, it had gone straight into being 'expert' knowledge for administration, monitoring, expansion and readjustment. Structural conditions and discourses as played out through interests of the 1970s sought to foster a very different social science. Some University social scientists, perhaps inevitably, sought to balance both interests, attempting to develop an intellectual role, whilst acknowledging the importance of applied, practical endeavours. Hedging bets may have seemed a good strategy in such a changing context. A newly appointed Professor of Sociology indicated, for example, his wish to encourage staff into policy work.⁶⁹ Similarly, some disciplines attempted to outline priorities for research of New Zealand society.⁷⁰

Summary

The period saw social science interests seek to talk across policy-focused activities and illuminate broader and longer-term understanding of social change and needs. Tensions between these over-inflated aspirations from earlier cycles were becoming more apparent, and, when combined with shallow intellectual, institutional and financial foundations, put social science on a relatively precarious footing. The final cycle suggests a dynamic that puts paid to a linear notion of social research, social science and disciplinary development, which all develop in a clear direction. Rather, there were tension and conflicts in what are perceived to be the meanings and objectives of such practices.

The policy and organisational *Blitzkrieg* that occurred after 1984 merely adds weight to the view that the notion of 'progress' is erroneous. As the balance sheet for social science research was being drawn up in the early 1980s, it was increasingly replaced by the perceived value of indicative markets. The view held so strongly at the beginning of the 1970s, namely that economic and social policy needed to be more scientific, had run its course. A backlash was developing and new interests and cultural and structural conditions were emerging. As this chapter draws to a close, there are two positions demarcated, with interests forming around them. While in many other countries these two traders retain relative autonomy, the small-scale of New Zealand institutions and society maintains them in closer contact. In turn, this has had consequences for later cycles of interaction and elaboration of social science research in New Zealand. In the post-1984 period, both instrumental and theoretical developments were to share the fate of restricted funds and legitimacy, and neither was able to resist attacks levelled at them. Social science research was losing credibility, and its meagre resources, relatively small Departments and fragmentation, to say nothing of methodological dilemmas, left it vulnerable.⁷¹ A non-empirical and non-social paradigm was on the rise, and as a theoretical notion it disclaimed the 'social', advocating instead a foundation of economic individualism.⁷² Social scientists within Government were also a relatively small and fledgling group that was prey to market forces and reorganisation. In the *Blitzkrieg*, University social scientists were to be mere spectators. But to begin such an account is to enter another cycle, one where avoiding bankruptcy would require new forms of intellectual trading, a return to 'instrumental social research' rather than theoretical 'social sciences', a new set of institutional relations and properties and, ultimately, a reworking of the inheritance.

Chapter Thirteen Notes

¹ Roper (1997).

² For a New Zealand perspective see McLauchlan (1976); for Britain see Kogan and Henkel (1983); whilst a 'global narrative' is provided by the Chicago School (Friedman and Friedman, 1980) or the Public Choice Theorists (Tullock, 1976).

³ McLeod (1968, p. 13).

⁴ Bassett (1998).

⁵ Town and Country Planning Division (1980, p. 3).

⁶ Johns (1979).

⁷ Johns (1979, p. 5).

⁸ MacKay (1975, p. 61).

⁹ Dickinson, E.B. (1972) 'Proposal for structuring the research role in the Department of Social Welfare'. Research Section, Social Security Department (CW, 33.1, National Archives).

¹⁰ In the mid-1970s the Department of Social Welfare had 13 research staff; Education 11; Labour 33; Justice 7; Committee for Joint Young Offenders 3; Housing Corporation 6; Department of Maori and Island Affairs, 1 (MacKay, 1975).

¹¹ MacKay (1975).

¹² Fischer (1981).

¹³ New Zealand Planning Council (1982, p. 21).

¹⁴ Hunn (1981).

¹⁵ New Zealand Planning Act 1977 (p. 2).

¹⁶ See Smith (1974); Henderson (1981).

¹⁷ Jones (1973, p. 51).

¹⁸ Johns (1979).

¹⁹ Brown (1983, p. 58).

²⁰ For sociology graduates this was 85%; for anthropologists, 68%; for political science graduates, 60% (Clements, 1977).

²¹ Jones (1973, p. 18).

²² Barrowman (1999, p. 205).

²³ Smith (1982).

²⁴ Williams (1980).

²⁵ The New Zealand Association of Social Workers was founded in 1964. By 1975 there were courses of social work and social administration at Victoria, Massey, Auckland and Canterbury (Clements, 1977). For a fuller account see Nash (1998).

²⁶ Williams (1980, p. 74).

²⁷ A number of disciplinary associations were established during the 1960s and 1970s: New Zealand Association of Social Anthropologists (1975); New Zealand Association of Economists (1965); Political Studies Association (1975); Sociological Association of Australia and New Zealand (1963) (Clements, 1977).

²⁸ The increase of full-time equivalent staff:

1935	200
1972	2,490
1973	2,829
1978	3,055

²⁹ Barrowman (1999, p. 116).

³⁰ The 1975 UGC funding saw 0.5% given to political and social science. For a broader social science family (social science, anthropology, politics, economics, psychology and geography,) the figure was 6.5% of the total (calculations based on Clements, 1977, p. 64).

³¹ Williams (1980).

³² Treasury (1973).

³³ Fraser in NRAC (1981, p. 6)

³⁴ Cf. Kogan and Henkel (1983); also Kuhn (1962); Lakatos (1978).

³⁵ Bohman (1991).

³⁶ Gregory (1980, p. 85), also Clements in proceedings of NRAC (1977).

³⁷ Fischer (1981, p. 19).

³⁸ Gregory (1980, p. 85; italics in original).

³⁹ By 1975, 512 academic 'social scientists' were identified, including 45 in political science, 51 in sociology, 42 in anthropology (Clements, 1977).

⁴⁰ Rothschild Report (1972).

⁴¹ Although none of these potentially costly changes were in fact introduced by 1975. See Clements (1977).

⁴² King (1975).

⁴³ Benton (1983).

⁴⁴ New Zealand Planning Council (1982, p. 22).

⁴⁵ Keir (1982).

⁴⁶ Social Council (1973); Social Development Council (1975).

⁴⁷ Social Development Council (1975 p. 4).

⁴⁸ Shields (1974).

⁴⁹ Fougere and Orbell (1974).

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- ⁵⁰ Made by Brian Easton. The NRAC decided against (Committee C, declined meeting 66, 7th June 1974 p. 16).
- ⁵¹ Fougere and Orbell (1974); Shields (1974).
- ⁵² Contributors to the 2003 Special Edition of the *New Zealand Journal of Sociology* marking the retirement of Professor Graeme Fraser comment about his dual commitments (Volume 18, Issue 1).
- ⁵³ Baldock and Lally (1974).
- ⁵⁴ Middleton (1989, p. 58).
- ⁵⁵ Johnston (1973); Kilmartin and Thorns (1978); Smith and Thorns (1978).
- ⁵⁶ Swain (1977); Koopman-Boyden (1978).
- ⁵⁷ Cf. Macrae (1977); Easton (1981).
- ⁵⁸ Vaughan (1972) including chapters by Vaughan and Awatere, and Walker; Pitt and Macpherson (1974).
- ⁵⁹ Trlin (1977).
- ⁶⁰ Cf. Easthope (1974).
- ⁶¹ NRAC (1977, 1981).
- ⁶² New Zealand Planning Council (1982, p. 50).
- ⁶³ Holmes (1981, p. 14).
- ⁶⁴ Clements (1977).
- ⁶⁵ Wagner and Wittröck (1991b, p. 333).
- ⁶⁶ Cf. Becher (1989); Keaney (2000).
- ⁶⁷ Comment from State Services Commission official, to NRAC (July 1980).
- ⁶⁸ Keir (1982, p. 17).
- ⁶⁹ Letter from Professor Graeme Fraser to E.B. Dickinson of Social Security Department 25 January 1972 (CW, 33.1.2, National Archives).
- ⁷⁰ Clements (1977) suggests New Zealand Sociological Association (1974); Political Studies (1976).
- ⁷¹ Couchman (1984).
- ⁷² See comments of Trotman (2003) around applied research interests during this time.

Chapter Fourteen

Conclusion

The broad focus of the thesis centred on the production of one facet of 'social knowledge'. The aim of inquiry was to account for the emergence of social science research in New Zealand, exploring the growth of social research, social science and individual disciplines. Tracing their development from colonisation through to 1984 in both the public service and the University system is an ambitious task, particularly given the diverse episodes which result in there being 'only themes, not a continuous story' (Archer, 1979, p. 395). The account sought to avoid description in its guises of chronology, reportage and encyclopaedism. Early chapters argued that writing history is a theoretically and empirically informed endeavour, and to this end made use of the insights of Critical Realism, and particularly the work of Margaret Archer. The challenge throughout was to retain the possibility of causal explanation, recognising the structural and cultural conditioning exerted upon agents. By placing agents and interests at the centre of the account, it has been possible to explore *who* was demanding social research knowledge – and for what *purpose*.

The thesis sought to make a fourfold contribution. In setting up the task in Section I, there was an attempt to show that a theoretically informed account is required, but that positivism, anti-positivism and post-positivism cannot deliver on the promises of explanation. Whilst the aspirations of post-positivism are retained, it is necessary to look beyond it for causal explanation and the appropriate tools.

Section II examined approaches and tools that could make sense of the problematic. To this end, a series of discussions explored what any viable framework must incorporate whilst also pointing to the deficiencies of previous frameworks. There was agreement with Giddens' identification of the importance of agency, constitutive activity, power and negotiation; however, it was argued that these could not be resolved within his schema. Instead, it was suggested that analytical validity is resolved within temporal vitality, but

that this can only be addressed by upholding viable notions of agency and structure. These discussions pointed to an engagement with Critical Realism.

In the light of these insights, Section III explores the possibilities of Critical Realism and, in particular, of Archer's morphogenesis and analytical dualism for historical practice. It suggests there are major possibilities for historical sociology by separating the people and the parts and making strong claims for the causal efficacy of agency, culture and structure. Combined with a strong ontology, a weighted reflexivity is retained by acknowledging epistemological relativism alongside judgemental rationality. In suggesting greater transparency in how this process is best achieved, the thesis offers 'Retrospective Narrative Realism' as a route for historical analysis. This involves analytical hypotheses being tested in the form of causal narratives, exploring interplay and emergence. Such *precessional* and *processional* moves involve putting up hypotheses (the analytic move), and some form of test (the narrative move). Abbott's early work on process, sequence, level, and domains of analysis is instructive here, as are the insights of Miller, Gerring and Fairburn, who emphasise testability and comparison.

Whilst there is no magic in Critical Realism, its invitation to attend to the hard work of sociology and history is one worth accepting, and it is in this spirit that Section IV tackles the explanatory task. The Section offers the first conceptually informed account of social science research in New Zealand. It avoids reportage and encyclopaedism, and respects agency while also fully exploring the place of structure and culture. The account advanced in figures (Figures 10.1, 11.1 and 12.1) and the narrative (Section IV) is the result of endless to-and-fro movements. The account that is offered has the strength of being fundamentally propositional, addressing not only *what*, but also *how*, *why*, *when*, *where* and *who*. This is achieved by centring interests and through specificity. Thus a series of social, political and professional elites face structural and cultural contexts, and these contexts are themselves shaped by actor interactions. The State (political interests, Departmental interests and the public service), the academy, (College, Senate, academics, disciplines), professionals, and social interests, all play a key role in the account.

Four cycles are offered to account for the changes in ideas and context, incorporating agency, structure and culture, contingency, and process. The account in Section IV

focuses on the educational, political, and socio-economic domains. Beginning with colonisation, the account emphasises the narrow social structure, centralised State and weakened University. In the cultural domain, there existed a cult of pragmatism, preference for action, and anti-intellectualism towards the roles of the University and State.

Structure and culture do not stay the same, interplay sees their reconstitution over time and agency is itself reformulated. In the second cycle (Chapter Eleven), welfare and professional interests are in ascendancy. The policy space is enlarged as a consequence of War, Depression, and a greater recognition of complexity. There are also value shifts around politics, economics and society, and the rise of science, intervention and conceptual transformation that included an emerging body of social science notions. In the inter-war period, health and medicine interests, along with disciplines of economics and psychology, were on the rise. Conversely, the failure of the Social Science Research Bureau can be understood in the light of the continued experience and residues of colonial structure and culture.

The third cycle that began to shape activities after 1945 saw greater autonomy for the Colleges and Universities (Chapter Twelve). Expectations of intervention soared, and the University system was also undergoing major expansion. Multiple hopes were being placed upon social science, not least on its instrumental ability to tackle 'social problems' by attending to causality and focusing on 'empiricism'. Government and College bought into this and a 'reform coalition' of politicians, public service, professionals and academics was evident. Specialism and expertise were in demand and ideas of modernisation, science, technology, and the possibility of fine-tuning were all prevalent. Carnegie Corporation funding allowed the development of social science projects which utilised the investigative tools that were gaining credence internationally. The emergence of national social science interests contributed to increased legitimacy within New Zealand. But this increase in Government research functions and incremental growth in College social science were uncoordinated and there was a lack of research training.

Finally, the fourth cycle takes discussion from the late 1960s to 1984 and highlights the emergence of disciplinary goals (Chapter Thirteen). The period witnessed political, economic and social upheaval with a plethora of commissions and inquiries. As the reform

coalition was put under increasing pressure, social knowledge itself became contested. There was a questioning of the role and purpose of social science and recognition that expectations had been oversold. Overall, there was a loss of confidence around the possible contribution of instrumental social science. An emerging theoreticism began to rub against the emphasis on relevance that was retained by political, public service and professional interests. Universities began to exercise greater power and influence, and within them autonomous social science disciplines brought new interests.

In summary, these four phases saw the following research developments: *the inheritance* – research by the State for the purposes of the State; *in search of independent means* – research by the State of broader social forms; *alternative benefactors* – research of social issues by broader social and University interests; *on the brink of bankruptcy* – a questioning of the State and social forms by social and University interests. Over these four periods it is possible to trace the emergence of social research, then social science, and then specific disciplines. These are all driven by interests, and by structural and cultural conditioning. The account is the result of upholding the commitment to the interrelations of ontology, methodology, and practice. It is offered in the spirit of ontological realism, epistemological relativism, and judgemental rationality. It awaits engagement, and is open to being further developed or surpassed.

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